Three Essays in Education Finance and Consumer Credit

by

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DEDICATION

This dissertation is dedicated to my family. Without their love and support, I surely never would have completed this journey. To my mother, Daryl Hershaff, thank you for your unconditional support through any and all difficulties and willingness to patiently listen anytime I needed an ear. To my father, Stuart Hershaff, thank you for ensuring my eyes were always kept on the prize. No matter what distractions life threw at me, you kept me focused on the completion of my doctoral studies. Finally, to my brother and sister, Brian and Melanie Hershaff, no matter how much time passes between visits, you make each trip feel like I’m coming home again.

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CHAPTER I

Moral Hazard and Lending: Evidence from the Federal Student Loan Market

Section 1 Introduction

Moral hazard describes the increased incentives of a party to engage in risky behavior, typically due to the presence of insurance provided by another part~~y.~~ Moral hazard on the part of the lender has been described as one of the main culprits of the U.S. mortgage default crisis (Mian & Sufi, 2009). In the face of record student loan debt and default rates,[1] commentators have drawn parallels between today's student loan market and that of the mortgage crisis (Dynarski, 2014). Even so, the role of lender moral hazard in the student loan market has been relatively ignored.

For more than fifteen years, there were two parallel systems of originating student loans. Within the Guarantee program (1965–2010),[2] banks provided their own capital and lent to borrowers at terms defined by the government. In return for participation, the government guaranteed to purchase loans at 97% to100% of the outstanding balance in the event of default. In the Direct Loan program (1994–Present), the Department of Education funds the student loan. There is no default guarantee, and estimates suggest that ED recovers approximately 82% of the outstanding balance on defaulted loans (Department of Education, 2014).

[1] That is, record default rates under the 270-day definition of default, in place since October 1998.
[2] Known as the Federal Family Educational Loan program (FFELP or FFEL program).

Moral hazard arises if the guarantee causes banks to change their behavior. Unlike the mortgage market, lender moral hazard should cause no changes in the composition of student loan borrowers, as eligibility is determined by the Higher Education Amendments and not by lender underwriting. Therefore, it is not obvious that lender moral hazard should impact borrower repayment. For example, in a simple framework of rational, utility-maximizing consumers, an individual's optimal allocation of resources between consumption, saving, and debt repayment should not be influenced by the financial repercussions to the lender given the fixed terms of the loan. However, student loan borrowers tend to underestimate the consequences of default and seem unaware of their repayment options (Lochner, Stinebrickner, & Suleymanoglu, 2013), making the conditions ripe for intervention. I hypothesize that the government guarantee causes a reduction in intervention efforts when a borrower misses payments. Because borrowers lack information on their repayment options to avoid default, this reduced effort should lead to an increase in default rates.

Guaranteed loans are not randomly allocated to students. Students select into schools and, prior to the elimination of the Guarantee program, schools selected into one of the loan programs. Between 2005 and 2007, for example, public schools made up 70% of Direct Loan schools but only 46% of Guarantee schools. Unobserved differences between students across schools would bias cross-sectional comparisons of default rates across programs.

Over time, however, some schools switch programs. If this switching is unrelated to the probability of its borrowers defaulting on their student loans, a panel data model with school fixed effects can estimate the causal impact of participating in the Direct Loan program relative to the Guarantee program, among schools that switch. I identify this impact on default rates using two sources of plausibly exogenous variation in the allocation of federal student loans.

First, investigative reports (Barnett, Julian, & Knight, 2003) and a settlement with the New York Attorney General (Ernst, 2007) describe a pattern of kickbacks and pay-for-play schemes between private lenders and financial aid officers in exchange for participating in the Guarantee program. These provide evidence that some schools switched programs for reasons that may be unrelated to their students' ability to repay debt.

Second, following the financial crash, the Health Care and Education Reconciliation Act (HCERA 2010) eliminated the Guarantee program, causing thousands of schools to switch into the Direct program by July 2010. To the extent that schools already participating in the Direct Loan program prior to HCERA form a valid control group for those schools forced to switch, I can identify the impact on default rates from school switching.[3]

To illustrate one source of variation, consider borrowers who enroll in a school that used to participate in the Guarantee program and who then switched into the Direct Loan program in 2010. Students exiting prior to 2010 will have Guarantee loans, while students first enrolling after 2010 will have Direct loans. Some cohorts will have a mix of loans from both programs. The impact of Direct Loan participation on default rates is estimated from these "switcher schools." Non-switcher schools, as well as variation in the timing of school switches, are used to control for aggregate economic conditions, while time-varying student characteristics control for changes in the composition of borrowers.

This study takes two approaches to estimating the impact of obtaining a Direct Loan relative to a Guarantee program loan on repayment outcomes. The first approach utilizes school-x-year aggregate data containing cohort default rates, choice of federal student loan program, and time-

[3] The Guarantee loan program has been discontinued from originating new loans. However, loans already originated through this program still exist and are currently being paid down by borrowers.

varying averages of student characteristics. These aggregate variables are available for more than ten years within the universe of schools participating in federal student loan programs.

The second approach utilizes two student-level surveys that track cohorts of students over five-year periods, with rich detail on the family and background characteristics of each student. The surveys contain administrative data on student loan repayment status, and assignment to either the Direct Loan or Guarantee program.

There are tradeoffs between the two approaches. The aggregate model covers 96% of student loan borrowers who exit school between 2003 and 2011. However, there is measurement error in program assignment. Specifically, individuals within a repayment cohort must be mapped to previous years of school participation in each loan program, and some students are enrolled longer than others, or attend multiple institutions.

The student-level surveys contain administrative loan repayment data, and there is no measurement error in assigning students to either Direct or Guarantee program loans. Moreover, the data allows me to identify repayment alternatives to default and control for individual borrower characteristics. The tradeoff is coverage: The student level surveys do not coincide with the policy change eliminating the Guarantee program, or the recent increase in income-based repayment plan usage. Many schools are not represented in the surveys at all, and even fewer schools are represented in both surveys.

Using a school-x-year panel of default rates and program participation at more than 4,500 schools, I confirm empirically that Direct Loan participation reduces default rates by 1.1 percentage points (12%) at less-than-four-year (LT4-year) schools. Impacts are concentrated at for-profit schools with high baseline default rates and low average enrollment. There is no discernible impact at four-year schools, although the small number of four-year for-profit schools makes estimates imprecise in that sector.

There are numerous mechanisms through which Direct Loan participation could impact default rates. More frequent contact attempts by aggressive loan servicers could lead borrowers to place a higher priority on student loan debt repayment. Alternatively, servicers with an incentive to keep loans out of default may inform borrowers about alternative repayment options. The student-level model allows me to investigate one mechanism of default reduction. I find evidence that the reduction in default rates from Direct Loan participation coincides with a corresponding increase in the use of forbearance. I note that the student-level model does not cover the time period with the recent increase in the use of income-based repayment plans.

This paper contributes to three strands of literature. First, it contributes to a body of work that discusses the role of lender moral hazard and borrower default. Second, it speaks to the sensitivity of borrower repayment to nonfinancial interventions. Finally, this research speaks to education finance policy. To my knowledge, this is the first paper to consider the role of lender moral hazard in student loan default. This study is also the first to consider the impact of the elimination of the Guarantee lender program on borrower outcomes. Within the span of a few years, the Health Care and Education Reconciliation Act (2010) shifted the ownership of hundreds of millions of dollars in student loans from the private sector to the public sector. Prior research has suggested that the Direct Loan program is less expensive to taxpayers than the Guarantee program (e.g., Lucas & Moore, 2009). To the extent that default reduction is a desirable goal, this has been a successful change in policy.

The rest of the paper is structured as follows: Section 2 discusses the relevant literature. Section 3 gives background information on student loans and describes borrower and lender incentives to avoid default. Sections 4 and 5 describe the data sources, analysis sample, empirical strategy, and results of the aggregate model. Sections 6 and 7 describe the data sources, analysis sample, empirical strategy, and results of the student-level model. The paper concludes in Section 8.

Section 2 Literature Review

Lender moral hazard has been shown to have played a major role in the U.S. mortgage default crises (Mian & Sufi, 2009). This was in part through the channel of securitization, in which loans were originated and then sold them to investors, incentivizing lenders to reduce their screening efforts (Keys, Mukherjee, Seru, & Vig, 2010). Thus, in the mortgage market, lender moral hazard led to a compositional shift in borrower quality.

To my knowledge, the student loan default literature has neglected to consider the role of the lender and the loan servicer in encouraging debt repayment. Perhaps this isn't surprising, considering that student loan eligibility is not determined by lender underwriting. Lender moral hazard should not cause compositional changes in borrower quality. Yet there is evidence from Canada that student loan borrowers are unaware of their numerous repayment options, and underestimate the consequences of default (Lochner et al., 2013). In the US, defaulted borrowers could not recall receiving information from their lenders (Loonin & McLaughlin, 2012), and could have avoided default if they had been aware of alternative repayment plans (Chopra, 2013), making conditions ripe for lender interventions.

The only paper I've seen that discussed the role of the lender in student loan default is a dissertation chapter (Akers, 2012). Akers also estimated the impact of Direct Loan participation on borrower default rates within schools that switch. However, the sample period for this paper concludes prior to HCERA and therefore misses more than 3,000 schools that switched programs due to this policy shift. In addition, the study doesn't make an argument for why schools switch programs. Akers found that Direct Loan participation raises default rates at proprietary institutions, which is consistent with the results of my study.

Numerous observational studies have identified factors associated with student loan default. Recent summaries of the literature on default include Hillman (2014), Loonin and McLaughlin (2012), and Gross et al. (2009). These studies have found that degree attainment, income and employment, institution type, race and ethnicity, age, gender, debt levels, and attitude toward debt are related to default risk. For example, the Loonin and McLaughlin survey found that 47% of defaulted borrowers felt they shouldn't have to repay their debt. Over 90% of these individuals attended for-profit schools.

However, empirical research that can identify factors causing student loan default has been scarce, and for good reason. Federal regulations have made student loans homogenous across student populations, and changes over time tend to be coincident with meaningful changes in economic conditions. For example, recent increases in federal loan limits were coincident with the collapse of the private student loan market.

Given this lack of variation, Ionescu (2009) carefully modeled borrower repayment incentives within the structure of the student loan programs. She calibrated a model to include the option of consolidating loans into an income-contingent repayment plan. The availability of the income-contingent scheme allows some borrowers to lower their payments—in particular, those with low incomes—and the model predicts a decline in default rates of 10 percentage points due to the changes. However, the model assumes that borrowers have full information.

Nonfinancial interventions by lenders have been shown to stimulate repayment. A field experiment in Uganda showed that text message reminders increased repayment rates. Many borrowers missed payments simply because they were unable to keep track of their payment schedules (Cadena & Schoar, 2011). In the United States, an intervention program from a single mortgage lender showed the effectiveness of lender communication and borrower education. Struggling borrowers were sent letters referring them to foreclosure counseling. The authors find that the counseling referral caused a 20% decline in foreclosure starts (Collins, Lam, & Herbert, 2011).

The high guarantee rate to lenders in the Guarantee program may not have given lenders sufficient incentive to expend resources in order to keep struggling borrowers out of default. In other financial markets, evidence suggests that an increased cost to the lender of loan default can incent the lender to communicate with the borrower. For example, there are higher rates of loan modification when the foreclosure process is more expensive to lenders, due to statewide laws (Collins et al., 2011).

In summary, many student loan borrowers have limited knowledge of their loan terms. In other markets, nonfinancial interventions have been used to stimulate repayment of such borrowers. However, lenders need an incentive to expend resources in order to provide these interventions. In the student loan market, the lenders in the Guarantee program likely do not have these incentives.

Section 3 Background on Student Loan Repayment and Default[4]

There are three types of federal loans students may obtain each year while enrolled: subsidized Stafford, unsubsidized Stafford, and PLUS loans.[5] Each of these loans—whether in the Direct or Guarantee program—have the same published rates, limits, and eligibility.[6] Once no longer enrolled, borrowers enter the repayment period after a six-month grace period. The default repayment schedule is a ten-year fixed-rate amortization with equal monthly payments over the life of the loan.[7] Borrowers may sign up for alternative plans that could lower required monthly payments. However, they must make an active effort to switch.

When borrowers are unable to make required payments, they may be eligible to enter temporary states of forbearance.[8] Required payments are reduced, although interest accumulates at the same rate. Entering forbearance or alternative repayment plans could increase the total cost of the loan, but allows a struggling borrower to avoid penalties associated with missed payments.[9]

Missing payments trigger a series of actions. First, borrowers may be penalized up to 6% *per month* on past-due balances. The lender may report late payments to credit bureaus, resulting in a reduced credit score for the borrower. In addition, lenders will initiate collection efforts that include letters and phone calls to delinquent borrowers.

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[4] Many statistics in this section are from Sallie Mae. Sallie Mae is the largest holder of Guarantee loans (over $100 billion, more than five times the second-largest holder) and one of just four companies since 2009 to be allocated servicing rights by ED for Direct loans.
[5] Further description in Appendix A.
[6] To clarify, a subsidized Stafford loan does not have the same interest rate as an unsubsidized Stafford loan. However, a subsidized Stafford loan originated through the Direct program has the same interest rate as a subsidized Stafford loan originated through the Guarantee program.
[7] Prior to 2006, the default repayment plan included variable-rate loans, though borrowers could consolidate to a fixed-rate loan.
[8] Deferment is a special form of forbearance in which the required monthly payment is reduced to zero. While there are different automatic qualifiers for eligibility, I will denote "deferment or forbearance" as simply "forbearance" for the remainder of this paper. https://studentaid.ed.gov/repay-loans/deferment-forbearance
[9] Alternative repayment plans include income-based or graduated repayment plans, or consolidating into an extended term of up to 30 years.

Once a borrower reaches 270 days past due, the lender has ninety days to submit a default claim. The borrower can then face a number of additional penalties—including an initial charge of up to 25% of the outstanding balance, and forced repayment via wage garnishment.[10] Entering default can also limit employment opportunities.

In the Guarantee program, lenders are required to make a well-defined minimum effort to contact at-risk borrowers in order to satisfy a default claim. These efforts include one letter each month and up to two phone call attempts per month, between the fourth and seventh month of delinquency. Lenders almost always exert this minimum effort (SLM 2012).[11]

A lender should use additional resources to keep borrowers out of default only if the expected return from doing so exceeds the expected return in default. I propose that lenders in the Guarantee program have a reduced incentive to expend such resources relative to the Direct Loan program.

First, if lenders are unable to stimulate repayment, these incentives will be a moot point. However, the actions of Sallie Mae suggest that it is able to stimulate delinquent borrowers to recover. Specifically, Sallie Mae uses considerably more resources in dealing with delinquent borrowers with private student loans (no government guarantee) than in the Guarantee program.[12]

In both the Direct and the Guarantee programs, a principal-agent problem exists. The principal in the Guarantee program is either the bank's shareholders, or investors who purchased loans on the

[10] Wages are only garnished for those earning at least thirty times the minimum wage per week. Thus, the lowest income borrowers may not be forced to make payments immediately.
[11] Sallie Mae uses an assumption that 99.5% of default claims are upheld, demonstrating that the minimum required effort to receive the government guarantee is nearly always met.
[12] These include hiring a stand-alone collections unit made up of more tenured officers, and subjecting high-risk borrowers to more frequent attempts at contact and default prevention interventions. In the Guarantee program, all delinquent borrowers receive the same treatment.

secondary market. The principal in the Direct Loan program is the Department of Education (or, by extension, the taxpayer). In each program, the agent is represented by customer service representatives servicing the loans. However, the principal in the Guarantee program has less to lose in the event of borrower default.

In default, at little cost, Guarantee lenders receive at least 97% of the loan balance. In addition, lenders collect income through late fees, and some may be hired as contractors for collection activities after default. In the Direct Loan program, the government only recovers $0.87 on net for every $1 defaulted over the life of subsidized loans. This rate falls to $0.82 and $0.74 for unsubsidized Stafford and PLUS loans respectively (Department of Education, 2014). Importantly, each of these recovery rates is meaningfully lower than 97%.

The government may have an objective function that is not simply profit maximizing. To the extent that the Department of Education cares about the repayment outcomes of student loan borrowers, or the political repercussions of high national default rates, the relative value of a defaulted loan in the Direct Loan program would be even lower compared to the Guarantee program. In other words, lenders in the Direct Loan program should have *more incentive* to intervene to avoid borrower default than even the gap in recovery rates suggests.

I model the lender's problem and use the facts described above to make predictions about intervention efforts across loan programs. A formal description can be found in Appendix B. Below, I discuss the general outline of the problem.

When a borrower becomes delinquent, the lender must make a decision whether to expend resources in an intervention. There are three types of borrowers who enter delinquency. The first type will recover regardless of an intervention, the second will default regardless of an intervention, and the third is a marginal borrower who recovers if and only if there is an intervention. For the first two types, an intervention has no value to the lender. The expected value of providing an intervention is thus a function of three components: the fraction of marginal borrowers, the difference in value between a defaulted and a recovered loan, and the cost necessary to avoid default for a marginal borrower.

I assume that the fraction of marginal borrowers is constant across loan programs. The net present value of a loan in recovery is the same across programs (because terms are essentially identical), and I assume that the cost necessary to recover a marginal borrower is the same across programs. This should hold because, in many cases, borrowers are being serviced by Sallie Mae for both Direct and Guarantee loans. The critical difference is that the value of a loan in default is greater to the lender in the Guarantee program. This leads to the prediction that Guarantee lenders have *less* incentive to expend resources for default avoidance interventions.

A comparison of the way Sallie Mae contacts delinquent borrowers across programs suggests that this prediction is true. Sallie Mae created a presentation to its investors that details both the minimum requirements in the Guarantee program to satisfy the default claim, as well as the additional resources expended above the minimum. The company expends exactly the minimum effort for the first six months of delinquency, and only increases effort starting in the seventh month (SLM, 2012).[13]

With regard to Direct Loans, I can only speak of information received from a Sallie Mae customer service representative via a personal phone call. I asked the representative if the Department of Education has specific requirements for how Sallie Mae must handle delinquent

[13] Increased efforts include phone call attempts made every 2 to 3 days.

accounts, and was told this: “Phone calls to delinquent borrowers begin at just fifteen days past due. Phone calls are made typically every day. Delinquency letters are sent out to borrowers in fifteen day increments.” When I asked if I could get this in writing, a supervisor unfortunately shut down our conversation.[14]

In summary, Guarantee program lenders have a reduced financial incentive to expend resources on default prevention relative to the Direct Loan program. These lenders have the capacity to stimulate repayment among delinquent borrowers, but choose not to do so. Finally, these incentives are born out in the actions of the agent: Even within a single servicer, delinquent Direct Loan borrowers are subject to more intervention measures than are Guarantee borrowers.

Selection into federal student loan programs is accomplished through schools, and not by individual students. Prior to the elimination of the Guarantee program, financial aid offices decided each year whether to participate in either the Direct Loan or Guarantee program, but rarely did they participate in both programs within an academic year. Between 2000 and 2009, more than 90% of schools had at least 99% of students obtain loans through a single program.[15]

Schools have switched between programs in three distinct time periods. Prior to the fall of 1994, the only program in existence was the Guarantee program. By 1997, the Direct Loan program originated one-third of new federal student loans. Legislation then restricted the Department of Education from marketing Direct loans. Investigative reports and settlements with state Attorneys General suggest patterns of bribes and kickbacks between private lenders and financial aid offices over the next decade. From 1997 to 2007, hundreds of schools switched back to the

[14] The Department of Education informed me that its contractual minimum requirements of DL servicers were set to match those in the Guarantee program, but that it could provide informal guidance, and that servicers will do “whatever we ask them to do.”
[15] In the 2009–10 academic year, many schools offered both types when the Guarantee program was coming to an end.

Guarantee program, while only a few dozen schools switched to the Direct program. Since July 2010, any schools that had been Guarantee program participants were required to switch to the Direct Loan program. These periods are detailed in Appendix A. Figure 1 displays the fraction of new federal loans originated through the Guarantee program during each academic year, along with the announcement and effective date of the policy eliminating the Guarantee program.

Section 4 School-x-Year Aggregate Model

Annual two-year cohort default rates for each school participating in federal student loan programs are published by the Department of Education, and are available online. These data allowed me to observe the proportion of students who default on their loans within two years of entering repayment. I extended the analysis to three-year cohort default rates for the available period, and the results were quite similar.[16] A more detailed explanation of cohort default rates is in Appendix A.

The predictor of interest—school-x-year participation in the Direct Loan program—comes from Department of Education Title IV reports available online starting in AY 1999–2000. These reports also provide the fraction of federal loans disbursed as subsidized, unsubsidized, or PLUS.

Time-varying school-level characteristics were provided by the Institute for College Access and Success (TICAS). The panel includes annual data on school enrollment, demographics, and graduation rates.[17] It also includes the income levels of dependent and independent students, the

[16] Three-year default rates are available starting with the 2005 cohort and officially replace two-year rates starting with the 2012 cohort.
[17] Collected from the Integrated Postsecondary Education Data System (IPEDS).

proportion of freshmen with education loans, and the average student loan amount among freshmen.[18]

The study uses repayment cohorts 2003 through 2011, in order to have at least four years of federal student loan participation data for each cohort. Cohorts with fewer than thirty borrowers are dropped because the reported default rate is a moving average of three years instead of an annual rate for those small cohorts. Many schools are dropped from the analysis, although nearly all borrowers are captured by the following sample restrictions.

The sample begins with 6,933 unique US-based schools, of which 5,941 ever report at least one default rate between 2003 and 2011. The sample size falls to 4,912 schools that have at least one year with at least thirty borrowers in a repayment cohort. Of these schools, 4,889 match to Title IV reports (in order to identify program participation) and 4,579 match to the TICAS panel of characteristics (in order to identify the institution level, e.g., a four-year school). The analysis sample is an unbalanced panel and represents 96% of the universe of borrowers in repayment during the sample period. Program impacts are identified from schools that switch programs. Table 1 displays characteristics of schools that were either Guarantee only, Direct only, or that switched over the full sample period. A detailed description of the switcher schools by time period can be found in Appendix C.

The empirical strategy used in this study is partly a result of data availability, as well as how the outcome variable and key predictors are measured. This section builds up the model slowly in order to account for identification concerns, and then data availability and measurement.

[18] Collected from the Financial Institutions Shared Assessments Program (FISAP).

In an ideal experiment, borrowers would be randomly assigned to either the Direct or Guarantee program, and the impact of Direct Loan participation could be measured by a simple comparison of the outcomes in each group.[19]

$$(1)\ y_i = \alpha + \beta Direct_i + \varepsilon_i.$$

In reality, estimating equation (1)—predicting an individual's probability of default with cross-sectional data—may lead to biased estimates of the impact of Direct Loan exposure, because factors correlated with Guarantee vs. Direct participation may be directly correlated with default likelihood. Between 2005 and 2007, for example, for-profit schools accounted for 25% of Guarantee program schools but only 13% of Direct Loan schools.

Selection into loan programs is done by the school, and some schools switch programs over time. Equation (2) adds school fixed effects to the previous equation.

$$(2)\ y_i = \alpha + \beta Direct_i + \mu_s + \varepsilon_i.$$

The coefficient on $Direct_i$ represents the estimated impact of Direct Loan participation on a borrower's likelihood of default within schools that switch programs over time. Switching isn't random, however, and almost always goes in one direction within periods. Prior to 2008, nearly all switching was into the Guarantee program; but starting in 2009, all switching was into the Direct Loan program. Trends in the composition of borrowers over these time periods could confound estimates of the impact of Direct Loan participation. To account for this, equation (3)

[19] An argument could be made for randomizing across colleges. However, the treatment from having a Direct loan only occurs months after leaving college.

adds a vector of observed student characteristics, X_i, as well as time trends within institution sectors, in order to control for changing economic conditions.[20]

$$(3)\ y_i = \alpha + \beta Direct_i + \delta X_i + \theta_{ct} + \mu_s + \varepsilon_i.$$

The critical identifying assumption is that schools switching loan programs is unrelated to any unobserved variables affecting student default probability. This assumption would be violated, for example, if more financially literate borrowers altered their matriculation or borrowing decisions based on a school's selection into the Direct Loan program. If the identifying assumption holds, an ordinary least squares model with school fixed effects can identify the impact of Direct Loan participation on default rates among schools that switched programs.[21]

Student-level data is relatively scarce, particularly through the period when the Guarantee program was eliminated. Equation (4) aggregates the previous equation to the school-x-year level, and variables included are now school-x-year population means.

$$(4)\ y_{st} = \alpha + \beta PctDirect_{st} + \delta X_{st} + \theta_{ct} + \mu_s + \varepsilon_{st}.$$

There is one key challenge to estimating this aggregate model. Specifically, cohort default rates are defined by the date in which students enter the repayment period, and a repayment cohort consists of students enrolled in a wide range of previous years. Thus, for schools that switch loan programs, it is not clear how much exposure each repayment cohort contributed to each loan program.

To account for this challenge in measurement, I estimate a distributed lag model separately for four-year and LT4-year institutions.

[20] Sector time trends assume the existence of repeated cross-sectional or panel data for this hypothetical situation.
[21] Approximately 75% of schools switch programs during the sample period, so the effects are estimated off a large majority of the college-going population.

$$(5)\ y_{st} = \alpha + \sum_{l=0}^{N}(\beta_l PctDirect_{s,t-l} + \delta_l X_{s,t-l}) + \theta_{ct} + \mu_s + \varepsilon_{st},$$

where y_{st} is the two-year cohort default rate for the university s repayment cohort of t as described above. $PctDirect_{s,t-l}$, is the proportion of federal loans disbursed through the Direct Loan program by university s, l academic years prior to the repayment cohort. For example, for the 2010 repayment cohort, $PctDirect_{s,t-0}$ is the proportion of loans disbursed through the Direct Loan program in school s during the 2009–10 academic year, while $PctDirect_{s,t-1}$ indicates the proportion disbursed in 2008–09. These values are often close to zero or one.[22] The vector $X_{s,t-l}$ contains a series of lagged school-x-year characteristics of the student population, to control for college-going trends that may alter the composition of the borrower population.[23]

The parameter of interest, $\sum_{l=0}^{N}\beta_l$, represents the estimated impact on cohort default rates if all loans in the repayment cohort were to switch from the Guarantee program to the Direct Loan program. For four-year schools *N* is chosen to be six, and *N* is three for LT4-year schools, representing approximately 150% of normative program duration.

School fixed effects μ_s accounts for time-invariant unobserved school heterogeneity that might affect default rates such as school and faculty quality, the distribution of student ability and wealth, career services, and the ability to match students to appropriate majors.

Sector-x-cohort fixed effects θ_{ct} captures unobserved heterogeneity across years that affect default rates, estimated separately by institutional sector. This could include economic conditions

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[22] As a check for model sensitivity, $PctDirect_{s,t-l}$ is replaced with binary indicators for Direct Loan participation (having at least 90% of disbursed loans in that academic year being sourced through the Direct Loan program) and indicators for having "mixed" participation (between 10% and 90% of loans disbursed through the Direct Loan program). Results are very similar.

[23] The full variable list is detailed in Appendix C.

and legislative changes to the student loan system. An idiosyncratic error term, ε_{st}, is unrelated to Direct Loan participation. Standard errors are clustered at the school level.

Section 5 School-x-Year Model Results

The model was estimated separately for four-year and LT4-year colleges to allow for the differences in lags necessary to map repayment cohorts to annual program participation at the school, and because some covariates (e.g. loan size, and the fraction of participants submitting ACT/SAT scores) likely have a different relationship with default across school types. For each institutional level, I ran two specifications to test the sensitivity of the results. The first includes only school and sector-x-cohort fixed effects, along with the participation indicators. The second specification includes lagged characteristics of the student population to control for time-varying changes in the composition of borrowers.

Table 2 displays the estimated parameter of interest, $\sum_{l=0}^{N} \beta_l$, along with the average default rate for schools participating in the Guarantee program over the sample period 2003–2011.[24] The estimate $\sum_{l=0}^{N} \beta_l$ is statistically significant and negative for LT4-year colleges. The point estimate for the effect of Direct Loan participation on the cohort default rate for four-year colleges is negative, but small and statistically insignificant once characteristics of the student population are controlled for. Default rates for four-year colleges are already quite low relative to less-than-four-year colleges.

Direct Loan participation is estimated to reduce cohort default rates by 1.1 percentage points (12%) for LT4-year colleges. The estimate is statistically significant, meaningful, and stable with the inclusion of school-level covariates.

[24] The individual lag terms are highly collinear: Even among the schools that switch, most don't switch repeatedly.

I hypothesize that Direct Loan participation plays a role in reducing default rates due to lender incentives to provide information to struggling borrowers. High-information borrowers—as well as those borrowers who are not at risk of missing student loan payments—should not be affected by the treatment. I therefore expect differential impacts across schools with different types of students. Specifically, I expect impacts of Direct Loan participation to be driven by schools with high default rates, and by students with relatively low information on default alternatives.

To get at this, I use the 2004/09 Beginning Postsecondary Students survey to identify which school types have students least likely to repay on time (either in default or forbearance), and among those students, which ones are the most frequently in default. Using only students with Guarantee loans so as not to confound the treatment, Table 3 shows that one-year and two-year for-profit schools have the most borrowers in either default or forbearance. In addition, these schools have the highest rate of borrowers in default conditional on being in either default or forbearance. For example, four-year private schools in August 2009 have just 13% of students not paying back on time, and of that 13%, just over one quarter have defaulted. At the same time, one-year for-profit schools have 40% of students not paying back student loans on time, and of that 40%, 58% have defaulted. Thus, students at LT4-year for-profit schools should have the greatest opportunity to be impacted by default avoidance interventions.

Explicit modeling suggests this is true. I re-estimate equation (5) separately for each sector, and high/low-default schools within sector. Within a sector, a school is assigned as being in the lowest 50% or highest 50% default rate based on its earliest default rate in the sample period.[25] I fully interact the lagged coefficients of interest with quantile assignments. Estimates displayed

[25] Specifically, default rates from 2002 (pre-sample) are used. Schools without a reported default rate in 2002 are assigned to their default rate quantile based on their first year in the sample. I split at the median to preserve power.

are the sum of these interaction terms for each sector and quantile. As expected, estimates are small and statistically insignificant for four-year private and public schools, and I do not display those results here.

Table 4 displays estimated heterogeneous impacts of Direct Loan participation across different types of schools. There are large, statistically significant estimated impacts for LT4-year for-profit schools with relatively high baseline default rates. Direct Loan participation at these schools reduces default rates by 3.0 percentage points (25%). There is no statistically significant impact on schools with relatively low baseline default rates across any sector, nor are there significant impacts at public schools. While many students attend four-year for-profit schools, in the sample there are less than 200 unique schools in this sector, leading to imprecise and sensitive estimates.[26]

Larger schools may have an increased opportunity to provide information to students. Specifically, there have been reports of for-profit schools with high default rates expending meaningful financial resources to keep their students out of default, in return for maintaining their status to accept federal loan and grant dollars for tuition (Kirkham, 2012).

I re-estimate equation (5), limiting the sample to high-default LT4-year for-profit schools and interacting program participation indicators with enrollment groups.[27] Table 5 suggests impacts are of greater magnitude among these high-default for-profit schools with relatively small enrollment. The estimated difference in impact among high/low-enrollment schools is 3.6 percentage points, which is meaningful and statistically significant ($p = 0.02$).

[26] Estimates within each sector are available upon request.
[27] Splitting by highest and lowest 50% average undergraduate enrollment over 2003–2011.

Three-year cohort default rates are available beginning with the 2005 cohort and will replace two-year rates starting with the 2012 cohort. Notably, trial three-year cohort default rates for the 2005–2008 cohorts were first created and published in 2011, so they were less subject to manipulation. Tables 6 and 7 replicate Tables 4 and 5 respectively, using three-year default rates. Direct Loan participation reduces default by 24% at high-baseline-default LT4-year for-profit schools.[28] There is no statistical difference along the enrollment margin among these high-default schools. This is consistent with suggestions that larger schools may simply have delayed default for their students until just past the two-year cohort default rate measurement window, although these estimates are imprecise.

Finally, because the variation in school switching is quite different in the two time periods, I re-estimate Equation (5) separately for the high-default, low-enrollment LT4-year for-profit schools in the cohorts 2003–2007 and 2007–2011. While the point estimates are larger in the later period, the differences are not statistically different from each other.[29]

Estimated impacts of Direct Loan participation are concentrated in a subset of schools: namely, one-year and two-year for-profit schools with high baseline default rates and relatively low enrollment. I ran a series of internal validity tests on this sample of schools and describe the results here, while the details of the tests are in Appendix C.

First, I re-estimated Equation (5) on this sample but include school-specific time trends. Both the standard errors and magnitudes increase, but the results hold—suggesting that the reduction in default rates is not due to linear time trends.

[28] The estimated reduction is 7% at low default schools, which is not statistically significant. The 4.3 percentage point gap between the two estimated impacts (high and low default schools) is significant ($p < 0.01$).
[29] Results are available, but not displayed here.

Next, I estimated the impact of program participation in an event study framework. Importantly, this framework requires program switching to go in one direction, so I separately studied the impact of switching into the Guarantee program during the voluntary switching period, as well as the impact of switching into the Direct program during the involuntary switching period. This split sharply reduces power, and so estimates are quite imprecise. Results from the event study suggest that the distributed lag model may understate the impact of Direct Loan participation in the voluntary switching period and overstate impacts in the involuntary period, but are likely a wash when combined.

Finally, as a check on whether changes in unobserved student characteristics may be related to Direct Loan participation, I tested whether Direct Loan participation predicts changes in observed characteristics. Specifically, I simultaneously re-estimated Equation (5), replacing the dependent variable with eight time-varying characteristics of the student population within schools, using a multiple-equation regression model.[30] Among these eight characteristics, program participation is not significant at the five percent level for any of them. At the ten percent level, Direct Loan participation predicts lower graduation rates and a smaller fraction of loans disbursed are subsidized. Because degree attainment is an important predictor of default, the former relationship likely understates the estimated impact of Direct Loan participation on default rates.[31]

[30] Variables chosen to minimize missing observations and to represent a breadth of characteristics.

[31] The latter relationship could be a result of either relatively fewer low-income students taking out loans, or due to increases in the use of unsubsidized loans. Note that unsubsidized loan limits increased in 2009; increased unsubsidized loans and constant subsidized loan use results in a reduced fraction of subsidized loans disbursed.

Section 6 Student-Level Model

The Beginning Postsecondary Students (BPS) surveys follow nationally representative samples of first-time postsecondary students in 1995–96 and 2003–04.[32] In addition to a rich set of student, school, and family characteristics, the BPS matches students to the National Student Loan Data Systems (NSLDS), which provides a snapshot of the loan balance, repayment status, and whether the loan is being serviced in the Direct or Guarantee program. A refreshed loan status as of August 2011 is forthcoming, but not available at this time.

The BPS 1996 and 2004 surveys track 9,130 and 15,160 students starting college. [33] The analysis sample consists of those students (a) who were surveyed in both the initial and follow-up periods, (b) who are not foreign born, (c) who have not attended graduate school, and (d) who have been out of college for at least two years when the student loan repayment status was extracted in August 2001 and August 2009.

The latter restriction (d) was chosen to give students a non-trivial opportunity to default. It takes at least fifteen months for students to default following enrollment. This includes the six-month grace period and the 270 days past-due in order to default. The lender has an additional ninety days in which to submit claims, which could increase the time period from enrollment to default to eighteen months—even if the borrower never made a single payment.[34]

By construction, students who take five or more years to graduate will not have enough time to default within the time frame of the survey. In light of this—as well as results from the aggregate

[32] Ninety-one percent of the sample population was successfully located in the BPS 2004/09, and of these, 90% at least partially completed the survey, yielding a response rate of 82%.
[33] Unweighted counts rounded to the nearest 10, as required.
[34] As an alternative, I extended the sample to include all students who would have entered repayment within the 2008 (2000) repayment cohort window in the 2004 (1996) BPS. For the most part, this adds students who were last enrolled in December 2008 (2000). Results are virtually identical.

model suggesting that there is no discernible impact of Direct Loan participation on students at four-year schools—I removed students at four-year schools from the analysis sample, and focused on students at one-year and two-year institutions. This leaves me with 710 students from the 1996 survey and 1,780 students from the 2004 survey, all from 780 unique one-year and two-year schools. Approximately 50% of the remaining one-year and two-year schools are in the for-profit sector.[35]

A more technical description of the student-level sample can be found in Appendix D. This includes statistics describing the analysis sample, assignment of students to a single "main" school, and program participation within schools during the sample period.

I estimate the causal impact of exposure to the Direct Loan program using a student-level model, which builds slightly on Equation (3). In order to increase power, and because there is no measurement error in program assignment, I combined one- and two-year schools and estimated

$$(6)\ y_i = \alpha + \beta_1 OnlyDirect_i + \beta_2 HasBoth_i + \ \delta Stu_i + \gamma Sch_{st} + \theta_{ct} + \mu_s + \varepsilon_i,$$

where y_i is an indicator for the repayment status of student *i*. For one series of specifications, this includes whether any of a student's loans (i) have ever been in default or (ii) are currently in forbearance. The data do not allow identification of loans previously in forbearance but now in good standing. For students with multiple loans, the outcomes separately denote whether at least one of the loans is in default or forbearance.[36] There is one observation per student.

The predictors of interest $OnlyDirect_i$ and $HasBoth_i$ are mutually exclusive indicators for students having only Direct Loans, or students with both Direct and Guarantee Loans,

[35] Unweighted counts rounded to the nearest 10 as required.
[36] Results are similar when using a mutually exclusive set of outcomes.

respectively. The excluded category is students with only Guarantee loans. Students could have both types of loans if they attended multiple schools, or if the school they attended switched programs while enrolled. Transfer students are controlled for in one specification and excluded in another for sensitivity analysis. Results are similar in both instances. The coefficient of interest β_1 represents the estimated impact on the outcome if a borrower switched from having only Guarantee loans to having only Direct loans. The estimated coefficient β_2 represents the impact of "split servicing."[37]

The vector Stu_{it} consists of individual student characteristics, and Sch_{st} is a vector of time-varying school characteristics. The full list of controls is detailed in Appendix D.

School fixed effects μ_s account for time-invariant unobserved school heterogeneity, as in the school-level model. For students attending multiple schools, each student is assigned to a single "main school" based on the series of decision rules also described in Appendix D.

BPS sector-x-cohort fixed effects θ_{ct} capture unobserved heterogeneity across years. Cohort fixed effects in the aggregate model link students who exit school in the same year, but link students who begin school in the same year in the student-level model. For sensitivity analysis, I assigned students to cohorts based on their last enrollment date. Results are virtually identical. Standard errors are clustered at the school level.

Section 7 Student-Level Model Results

Table 8 displays the estimated impacts of exposure to the Direct Loan program on the conditional probability of student default (top panel) and the use of forbearance (lower panel) for

[37] So-called split servicing is currently a contentious issue. ED recently removed defaults from the cohort default rates for students with both loan types, citing "difficulties" students face in this situation (Baker, 2014).

students in LT4-year schools. Coefficients predicting default use and forbearance use were estimated separately. Students may have multiple loans, so the outcomes are not mutually exclusive.[38]

Each of the five columns represents a specification with additional covariates. The first column includes only the program participation variables, and school and sector-x-survey year fixed effects. The second column adds variables characterizing the student loans. Column 3 adds individual student-level characteristics, Column 4 adds institution characteristics, and Column 5 adds indicators for the student's experience while in college—such as major, and degree attainment.[39]

Table 8 suggests that having only Direct Loans increases the likelihood of having a loan in forbearance by 89%, with a corresponding drop in default rates of nearly two-thirds the magnitude. Having both types of loans meaningfully increases the use of both default and forbearance, and reflects the recent concerns that students with split servicing are facing repayment difficulties.

Table 9 supports the aggregate-model results on heterogeneous treatment effects by sector. Specifically, the reduction in default rates is driven by the for-profit sector for students with only Direct Loans. Students with split servicing continue to see increases in default rates across sectors. Splitting the LT4-year for-profit sector into high/low baseline default rate schools leads to a sharp drop in power. The standard errors are quite large, and there is no statistically significant difference in estimates at the high/low baseline default schools within the sector. As a result, I limited the analysis of heterogeneous treatment effects to the sector level.

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[38] Results using mutually exclusive outcomes tell the same story.
[39] Described in more detail in Appendix D, section entitled "building up the model."

Because the BPS has only time-invariant outcomes for each student, I was unable to estimate linear time trends or use an event study analysis (as I do in the aggregate model). As a test for whether changes in unobserved characteristics may be correlated with changes in Direct Loan participation over time, I re-estimated equation (6) in a multiple-equation regression model, selecting eleven student-level characteristics with relatively few missing observations as dependent variables. Students with only Direct Loans correlated with increased rates of their having parents with at least a BA, and decreased rates of the student having a GED, although it is not a statistically significant predictor of the other nine variables. This suggests a correlation between more advantaged borrowers and Direct Loan use within this limited sample, which could overstate the results if there are other, unobserved, changing characteristics unrelated to the controls operating in the same direction.

Section 8 Conclusions

Broadly, this study suggests that lender incentives and moral hazard can impact borrower repayment patterns. Policymakers concerned with student loan default, home foreclosures, and auto repossessions may be able to reduce these rates by reducing the net value to lenders of these outcomes. For example, requiring mortgage servicers to verify loan ownership and borrower missed payments—instead of "robo-signing" foreclosure documents—could lead to lower rates of foreclosure. This may be a cheaper intervention than subsidizing principal reductions, or encouraging other loan modifications.

More narrowly, this study provides evidence that participation in the Direct Loan program reduces cohort default rates at one- and two-year for-profit schools. The student-level analysis provides evidence of a mechanism: Students are more likely to use forbearance in order to avoid default in the Direct Loan program relative to the Guarantee program. The impacts are driven by schools with students who have been the least likely to repay their loans on time and the most likely to enter default relative to forbearance, providing support for my hypothesis that borrowers in the Direct Loan program will be subjected to more default prevention interventions. Subject to data availability, future research into the relationship between lender incentives and the use of income-based repayment plans, instead of forbearance, would make a worthwhile endeavor.

From a student loan policy perspective, this study suggests that the Direct Loan program may be better for struggling borrowers than the Guarantee program. Combined with reports suggesting that the Direct Loan program is cheaper for taxpayers under a broad range of assumptions (Lucas & Moore, 2009) and that forbearance use reduces the likelihood of entering default (Price, 2001), this study suggests that federal student loan programs should not return to a Guarantee system without meaningful modifications, particularly to the high guarantee rate.

The Guarantee program is no longer available today, but the current administration of the Direct Loan program is unlikely to be optimal. Indeed, the Deputy Secretary of the Treasury recently questioned the effectiveness of student loan servicing in the Direct Loan program, asking why seven million borrowers were in default on federal loans when they could be enrolled in income-based repayments, or obtaining loan modifications (Raskin, 2014).

Recent research suggests that financial incentives in the Direct Loan program are still not strong enough to entice servicers to provide more than the minimum required effort to communicate with borrowers and keep them out of default (Fink & Zullo, 2014), although servicer communication in the Direct Loan program is higher than in the Guarantee program. The results of this study suggest that further aligning the incentives of student loan servicers with borrower repayment—or raising their minimum required effort—should continue to reduce default rates.

Providing information to struggling borrowers may not be so easy. While private lenders are required to make regular attempts to contact borrowers prior to default, surveys suggest that few borrowers recall such contact. One area of promise might be to provide information at the time of loan signing and again when exiting school, and then require borrowers to make an *active decision* on their choice of loan repayment plan.

The default repayment plan for student loan borrowers is a ten-year fixed-rate amortization, although borrowers are eligible for alternative repayment plans which could reduce required monthly payments in exchange for a longer term. Borrowers with steeper earnings profiles may be better off in alternatives such as the graduated or income-based repayment plan. Yet, research suggests that default plans are often sticky, even when suboptimal (Choi, Laibson, Madrian, & Metrick, 2004).

Given this, policymakers may be able to improve federal student loan programs and reduce default rates by providing information to borrowers about their repayment options prior to entering repayment, and then ask borrowers to make an active decision about which repayment plan to choose. Further, the loan program can make it easier for struggling borrowers to enroll in forbearance. For example, borrowers who are receiving unemployment benefits could automatically be enrolled in forbearance, but left with the option to continue making payments in order to reduce the payment of lifetime interest.

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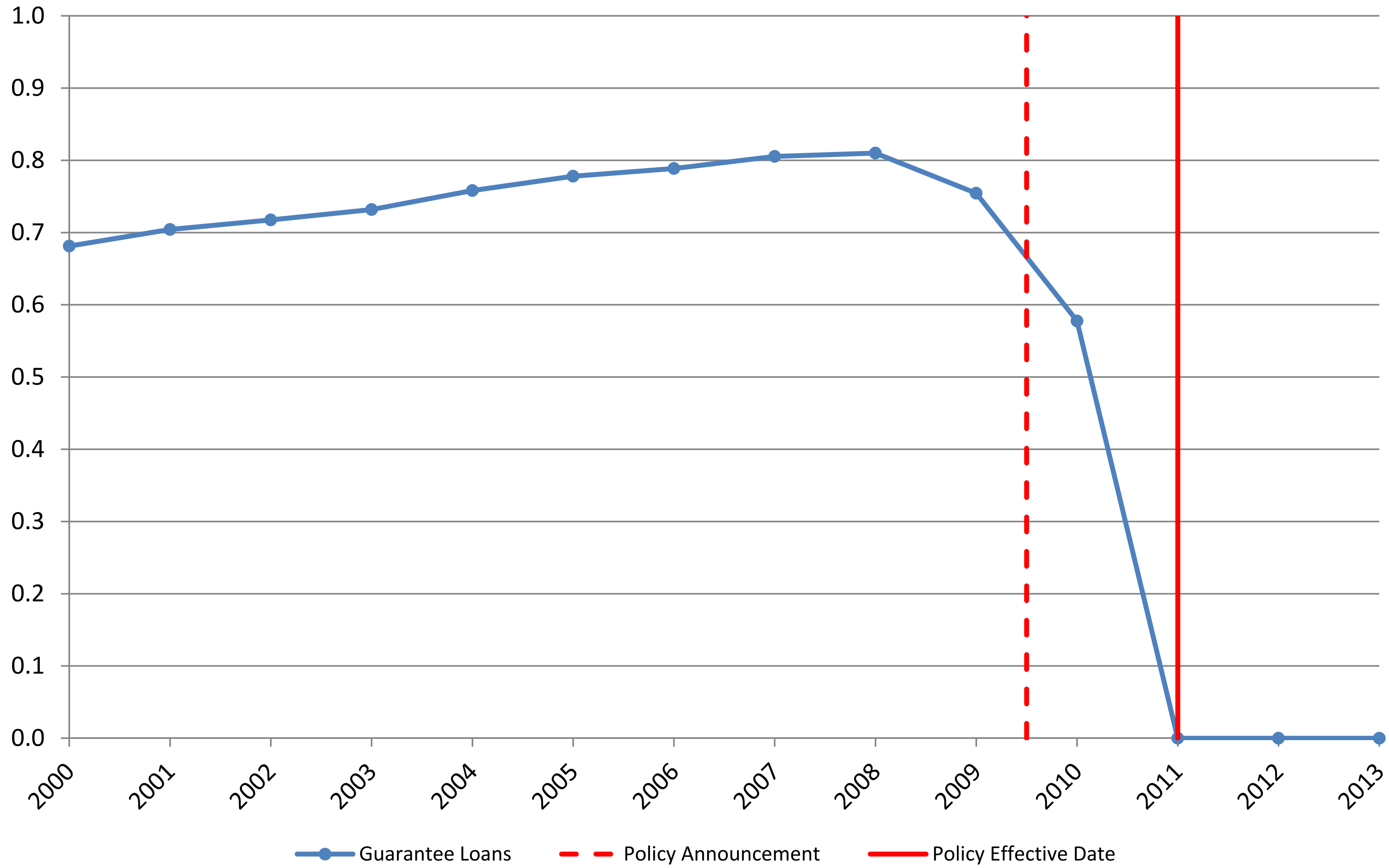

Figure I.1. Dollar-Weighted Fraction of New Federal Student Loans Originated through the Guarantee Program each Academic Year. Spring-semester years are displayed on the x-axis. E.g. 2002 represents the 2001-02 academic year.

Table I.1. Characteristics of Schools by Program Participation: 2000-2011.

	Guarantee Only	Direct Only	Switch
Unique Schools	267	1,068	3,239
School-x-Year Observations	1,610	8,407	36,087
Avg Annual Loan Recipients	1,005	3,537	2,912
Average Annual Default Rate	9.0	7.2	6.9
Avg Freshman Loan Amount	4,786	4,663	4,725
Pct Years Direct Only	0.00	0.95	0.14
Pct Years Guarantee Only	0.95	0.00	0.82
Pct Years Mixed Programs	0.03	0.06	0.05
Four-Year Public Schools	0.02	0.24	0.14
Four-Year Private Schools	0.12	0.15	0.33
Four-Year For-Profit Schools	0.04	0.03	0.05
Two-Year Public Schools	0.22	0.10	0.22
Two-Year For-Profit Schools	0.26	0.15	0.12
One-Year For-Profit Schools	0.21	0.31	0.10
Other Sector	0.13	0.02	0.06
Undergrad Enrollment	1,825	5,401	4,950
Pct Female	0.64	0.69	0.63
Pct Asian	0.04	0.04	0.04
Pct Black	0.21	0.18	0.14
Pct Hispanic	0.12	0.12	0.09
Pct Independent	0.64	0.45	0.44

Notes: Schools are assigned to the Direct or Guarantee program if at least 90% of loans within a year are disbursed through that program. A school is defined as a Switch school if it is assigned as a participant in each program for at least one year during the period. Guarantee Only schools must not have offered federal loans in 2011. Five schools (not shown) have one observation only and have mixed program participation.

Table I.2. Estimated Impact of School Participation in the Direct Loan Program on Cohort Default Rates for Repayment Cohorts 2003-2011.

	Less-than-Four-Year Schools	Four-Year Schools
Sector * Year + School Fixed Effects	-1.459***	-0.668**
	(0.401)	(0.322)
+ Time-Varying School Characteristics	-1.065***	-0.220
	(0.392)	(0.297)
Average Default Rate: 2003-2011	9.1	4.5
Observations	18,639	16,826
R-Squared	0.679	0.835

Notes: Each estimate is from a separate regression estimating equation (1) on 2-year cohort default rates. Standard errors are clustered at the school level and displayed in parentheses. Average default rate displayed is among Guarantee program schools. R-squared and observations are from second specification. * Statistically significant at the 10% level; ** Statistically significant at the 5% level; *** Statistically significant at the 1% level.

Table I.3. Federal Student Loan Repayment Status in 2009 among Borrowers with Guarantee Program Loans.

	Fraction Loans in Default or Forbearance	Default Share of Loans in Default or Forbearance
Four-Year Public Schools	19%	25%
Four-Year Private Schools	13%	27%
Four-Year For-Profit Schools	25%	44%
Two-Year Public Schools	32%	44%
Two-Year For-Profit Schools	34%	66%
One-Year For-Profit Schools	40%	58%

Notes: Student Loan program participation based on originating source for loans in 2003-04 only. Fraction loans in default or forbearance is a share of loans ever reaching repayment, including paid in full, deferment or forbearance, and default. The numerator includes those in deferment, forbearance, and default.

Table I.4. Heterogeneous Impacts of Direct Loan Participation by Baseline Default Rates.

		Below Median Default Rate	Above Median Default Rate
Less-than-Four-Year Public Schools	Coefficient	-0.347	0.206
	Standard Error	(0.771)	(0.777)
	Unique Schools	423	394
	Mean Borrowers	706	658
	Mean Default Rate	8.1	11.3
Less-than-Four-Year For-Profit Schools	Coefficient	-0.270	-3.026***
	Standard Error	(0.608)	(0.842)
	Unique Schools	830	712
	Mean Borrowers	331	432
	Mean Default Rate	7.0	11.9

Notes: Unique schools and mean borrowers are unweighted over the sample period within group. Each specification is estimated separately by sector. Schools are assigned to top/bottom 50% default rates by their earliest default rate in the sample. Heterogeneous impacts are estimated by fully interacting lag terms with group indicators, and estimates presented are the sum of lag terms. Sectors displayed have at least 200 unique schools in the sample. Standard errors are displayed in parentheses. * Statistically significant at the 10% level; ** Statistically significant at the 5% level; *** Statistically significant at the 1% level.

Table I.5. Heterogeneous Impacts of Direct Loan Participation by Average Undergraduate Enrollment Size among High Default Less-than-Four-Year For-Profit Schools.

		Below Median Default Rate	Above Median Default Rate
Less-than-Four-Year For-Profit Schools	Coefficient	-3.303**	0.294
	Standard Error	(1.407)	(0.953)
	Unique Schools	358	358
	Mean Borrowers	111	682
	Mean Default Rate	11.2	12.3

Notes: Each specification is estimated separately by sector. Schools are assigned to top/bottom 50% by average enrollment over the period. Heterogeneous impacts are estimated by fully interacting lag terms with group indicators, and estimates presented are the sum of the lag terms. The sample includes for-profit schools in the top 50% baseline default rate only. Standard errors are clustered at the school level and displayed in parentheses. * Statistically significant at the 10% level; ** Statistically significant at the 5% level; *** Statistically significant at the 1% level.

Table I.6. Heterogeneous Impacts of Direct Loan Participation on Three-Year Cohort Default Rates by Baseline Default Rates.

		Low 50% Default Rate	High 50% Default Rate
Less-than-Four-Year Public Schools	Coefficient	-1.114	0.839
	Standard Error	(1.044)	(1.218)
	Unique Schools	423	394
	Mean Borrowers	706	658
	Mean Default Rate	14.4	18.6
Less-than-Four-Year For-Profit Schools	Coefficient	-1.272	-5.562***
	Standard Error	(0.970)	(1.376)
	Unique Schools	830	712
	Mean Borrowers	331	432
	Mean Default Rate	16.4	23.7

Notes: Unique schools and mean borrowers are unweighted means over the sample period within group. Each specification is estimated separately by sector. Schools are assigned to top/bottom 50% default rates by their earliest default rate in the sample. Heterogeneous impacts are estimated by fully interacting lag terms with group indicators, and estimates presented are the sum of the coefficients on the lag terms. Sectors displayed have at least 200 unique schools in the sample. Standard errors are displayed in parentheses. * Statistically significant at the 10% level; ** Statistically significant at the 5% level; *** Statistically significant at the 1% level.

Table I.7. Heterogeneous Impacts of Direct Loan Participation on Three-Year Cohort Default Rates among High-Default Less-than-Four-Year For-Profit Schools.

		Low 50% Enrollment	High 50% Enrollment
Less-than-Four-Year For-Profit Schools	Coefficient	-5.303**	-2.509
	Standard Error	(2.572)	(1.661)
	Unique Schools	332	331
	Mean Borrowers	117	730
	Mean Default Rate	20.8	25.4

Notes: Unique schools and mean borrowers are unweighted means from 2005-2011 within each group. Each specification is estimated separately by sector. Schools are assigned to top/bottom 50% default rates by their earliest default rate in the sample. Heterogeneous impacts are estimated by fully interacting lag terms with group indicators, and estimates presented are the sum of the coefficients on the lag terms. Sectors displayed have at least 200 unique schools in the sample. Standard errors are displayed in parentheses.
* Statistically significant at the 10% level; ** Statistically significant at the 5% level; *** Statistically significant at the 1% level.

Table I.8. Linear Probability Model Separately Predicting Default and Forbearance. Stacked 1996 and 2004 BPS Surveys. Less-than-Four-Year Institutions Only.

			Outcome is Any Loans in Default			Outcome Mean
Has Only Direct Loans	-0.080	-0.126**	-0.095*	-0.077	-0.062	0.27
	(0.055)	(0.055)	(0.053)	(0.057)	(0.057)	
Has Both Direct and Guarantee Loans	0.232***	0.231***	0.206***	0.219***	0.211***	
	(0.049)	(0.049)	(0.048)	(0.052)	(0.053)	
			Outcome is Any Loans in Forbearance			
Has Only Direct Loans	0.092**	0.087**	0.084**	0.111**	0.098**	0.11
	(0.039)	(0.039)	(0.040)	(0.043)	(0.044)	
Has Both Direct and Guarantee Loans	0.151***	0.106***	0.096***	0.107***	0.101**	
	(0.034)	(0.035)	(0.036)	(0.040)	(0.040)	
Observations	2488	2488	2488	2385	2385	
R-squared	0.487	0.516	0.570	0.575	0.589	
School + Cohort Fixed Effects	X	X	X	X	X	
+Loan-specific variables		X	X	X	X	
+Student Characteristics			X	X	X	
+Institution Characteristics				X	X	
+Educational Experience					X	

Notes: Coefficients were estimated separately for each of the two outcomes. Each observation uses the survey weight associated with being observed in both the initial interview and final follow-up period. R-squared estimates are for Default specifications. Standard errors are displayed in parentheses. Outcome mean represents fraction in default or forbearance among Guarantee loan borrowers.
* Statistically significant at the 10% level; ** Statistically significant at the 5% level; *** Statistically significant at the 1% level.

Table I.9. Heterogeneous Impacts of Direct Loan Participation by Sector.

	Less-than-Four-Year Public Schools		Less-than-Four-Year For-Profit Schools	
	Any Default	Any Forbearance	Any Default	Any Forbearance
Has Only Direct Loans	0.066	0.076	-0.201*	0.095
	(0.075)	(0.067)	(0.104)	(0.076)
Has Both Direct and Guarantee Loans	0.199**	0.035	0.246***	0.148***
	(0.083)	(0.074)	(0.078)	(0.057)
Observations	899	899	1221	1221
R-squared	0.788	0.680	0.529	0.438

Notes : Each observation uses the survey weight associated with being observed in both the initial interview and final follow-up period. Standard errors are displayed in parentheses. * Statistically significant at the 10% level; ** Statistically significant at the 5% level; *** Statistically significant at the 1% level.

CHAPTER II

Estimating the Causal Impact of the CARD Act on Credit Card Ownership among Young Borrowers

Section 1 Introduction

The Credit Card Accountability Responsibility and Disclosure Act of 2009 (CARD Act, or the Act) was enacted in order to "establish fair and transparent practices related to the extension of credit" (Consumer Financial Protection Bureau, 2013). One of the four key provisions of the Act was to restrict credit access among individuals under the age of 21, unless they could prove an independent ability to repay debt. The provision was passed in part due to misleading reports that college students were amassing excessive credit card debt (Hawkins, 2012).

Restricting credit access among young adults may have good or bad long-term consequences. For example, if young borrowers are myopic or have low financial literacy, they may incur large debt burdens before understanding how to make optimal financial decisions. Restricting access to credit could help ensure that these borrowers are not overburdened with debt before understanding the long-term costs of paying for consumption on credit.

However, responsible card holders may begin building a credit history while young, which could positively affect interest rates and access to auto or home mortgage debt products later in life. Having a "thin file" (lacking a credit history necessary for accurate credit scoring) is one of the most important negative predictors of homeownership, even while controlling for credit impairments and income constraints (Calem, Firestone, & Wachter, 2010). Moreover, customers without a credit history are routinely denied access to traditional forms of credit (Cheney, 2008) and may shift into inferior substitutes (Zinman, 2010).

Four years after the enactment of the CARD Act, the impact of this provision on the access of young adults to credit is not clear. First, if credit card companies already required that individuals to be able to independently repay debt, then the Act's age restrictions would be redundant and would not affect credit access. However, anecdotes suggest that lenders actively targeted college students in the pre-Act period. Many incoming undergraduates would have very little personal income, suggesting that income requirements were likely uncommon.

In addition, surveys have suggested that (a) credit card companies still market products to students, (b) having an independent ability to pay debt is not always verified, and (c) in some cases, student loans are being used to demonstrate income (Hawkins, 2012). These loopholes suggest that savvy consumers are able to bypass the Act's restrictions, which could lead to heterogeneous treatment effects across certain segments of the population.

A Consumer Financial Protection Bureau report displays a reduction in new credit card ownership among 21-year-olds after passage of the Act, but new card ownership among this group had been steadily declining since 2007, and then began rising (slowly) since 2010 (Consumer Financial Protection Bureau, 2013). The descriptive statistics are unable to distinguish the impact of the Act on card ownership from general economic effects.

One recent working paper uses a difference-in-differences model to control for changes in the economy, and estimates an 8 percentage point (15%) decline in card ownership among 20-year-olds due to the Act (Debaut, Ghent, & Kudlyak, 2014).[1] This estimate is based on a sample that may not be representative of the majority of affected individuals, which I discuss in more detail later in this paper.

This study asks two questions. (a) How much do the age restrictions of the CARD Act reduce credit card ownership among individuals affected by the Act? (b) How do the impacts of the law vary across different segments of the population? Individuals affected by the CARD Act are still under age 25 by the end of 2014. As these cohorts age, this study will serve as a stepping stone to estimate the impact of reducing credit card access on longer-term outcomes: for example, credit scores, delinquency rates, and prices faced in other debt markets.

Identification is complicated by the financial crash of 2008–2009, which saw a sharp reduction in credit card ownership among young adults who would eventually be affected by the Act. In order to disentangle the effects of the recession from the Act on card ownership, I control for the unemployment rate and other economic conditions that individuals faced at each age in which they were most likely to obtain a credit card for the first time. I provide evidence that there exists a similar pre-trend in credit card ownership between young adults aged 21–24, and those aged 18–20, once these economic conditions are accounted for.

With little evidence of a violation in the parallel trends assumption, I identify the impacts of the Act on card ownership by estimating a difference-in-differences model. I compare the fraction of individuals with a credit card within public-use micro areas (pumas) among affected versus unaffected cohorts, before and after the Act's enactment.[2] In order to deal with potential anticipation effects in the nine months between the announcement and effective date, I re-

[1] Relative to 22-year-olds from December 2008 to December 2010.
[2] A description of pumas may be found on the Census website: https://www.census.gov/geo/reference/puma.html

estimate the model and drop the year 2009 from the sample. The estimated impact on card ownership is even greater, suggesting that anticipatory responses to the Act may not be meaningfully present among this population.

I find evidence that the Act reduces credit card ownership among young adults, and that the effect of the Act lingers even after individuals turn 21 years of age. Specifically, the Act reduces credit card ownership by 39%— from 11.3% to 6.8%— among individuals 18–20 years old. Further, individuals who had been affected by the Act still observe a 22% reduction in credit card ownership—from 24.6% to 19.1%—even after turning 21 years old, when the age restrictions are no longer binding. Estimated effects are greatest among cohorts who were the youngest (specifically, under age 18) when the Act was first passed. The effects of the Act are greater in pumas with a relatively high concentration of low-income households.

Broadly, this study contributes to a literature discussing the impact of restricting credit access. More narrowly, this study is one of a number of studies to estimate the impact of various components of the CARD Act. To my knowledge, this is the second paper attempting to quantify the effect of the Act's age restrictions, and the first to estimate this impact over the entire population of affected consumers.

The rest of the paper is structured as follows. Section 2 provides background detail on the CARD Act. Section 3 discusses the contribution of this study to the literature. Section 4 describes the data and sample, while section 5 discusses the identification strategy and empirical methods. Section 6 discusses the results, and the paper concludes in section 7.

Section 2 Background on the CARD Act

The House of Representatives began work on passing a "Bill of Rights" for credit card holders in early 2008. The bill, titled the Credit Card Accountability Responsibility and Disclosure Act, eventually passed both the House and Senate and was signed into law on May 22, 2009. Most provisions, including the age-based restrictions, went into effect nine months later on February 22, 2010.

The CARD Act has four key components: Consumer Protections, Enhanced Consumer Disclosures, Protection of Young Consumers, and Gift Cards. Consumer Protections establishes limits on penalties, fees, and interest rate increases, and requires a 45-day advance notice of interest rate increases. The Enhanced Consumer Disclosures provision provides consumers with explicit calculation of the additional costs of paying only the minimum payment each period, as well as disclosing late payment deadlines and penalties related to missing payments. The Gift Cards component removes dormancy, inactivity, and service fees from gift cards, and restricts expiration dates.[3]

The Protection of Young Consumers component is the focus of this study. Within this component, there are five subcomponents, the first of which is the most relevant. First, credit card companies are prohibited from issuing a credit card to individuals under 21 years of age, unless that individual meets one of two exceptions: (a) the applicant has an over-21 cosigner,[4] or (b) the applicant submits an application with proof of an independent ability to repay debt obligations resulting from the extension of credit. Second, this component restricts under-21

[3] The full law can be found here: http://www.gpo.gov/fdsys/pkg/BILLS-111hr627enr/pdf/BILLS-111hr627enr.pdf.
[4] Explicit guidance confirms that this does not mean family members or guardians only. Any individual 21 years or older may act as a cosigner.

consumers from receiving pre-screened credit offers. Third, parental approval is required for any increases in credit limits for co-signed cards where the parent is jointly liable.[5]

The final subcomponents are specific to college students. The fourth subcomponent requires colleges to disclose the terms of any agreements with credit card companies, and prohibits credit card companies from offering tangible gifts to induce applications for credit on college campuses. Finally, the fifth subcomponent requires creditors to submit reports to regulators on all terms and agreements of college affinity cards.

Section 3 Contribution to the Literature

Broadly, this study contributes to a literature discussing the effectiveness of financial regulation and the impact of restricting credit access. Restricting credit access should have meaningfully heterogeneous treatment affects across the population. Some individuals may be made worse off without access to credit. For example, Zinman (2010) found that regulations on payday lending reduces access to payday lending, but may have shifted consumers to inferior products such as the use of overdraft on checking accounts. Moreover, consumers use credit cards as a standard way to smooth consumption (e.g., Dey, 2005), so restricting access to credit could lead to reduced lifetime utility.

Other consumers may be made better off without access to credit cards. Specifically, some consumers may be myopic or lack the cognitive ability to optimize among their financial options, and learning from past mistakes or from others may be difficult (Campbell, Jackson, Madrian, & Tufano, 2011). In these circumstances, financial regulation may improve lifetime welfare.

[5] This same restriction holds for whoever is the cosigner. The term “parent” is emphasized in the law.

A few studies have used the recent implementation of the CARD Act to measure the impact of regulating the credit card industry. Agarwal, Chomsisengphet, Mahoney, and Stroebel (2013) analyzed two components of the CARD Act. The study found that a regulation limiting the fees banks may charge customers reduces total fees consumers faced by more than 20%, with the largest treatment effects among the highest credit-risk consumers. In addition, the study found that disclosing to consumers the cost of paying the minimum monthly payment led to a small, but statistically significant, increase in repayment rates. Finally, the study found that these impacts exist with no evidence of rising interest rates or reduced credit limits.

Jambulapati and Stavins (2014) tested whether the CARD Act regulations led credit card issuers to close unprofitable accounts. They found some evidence in Equifax data of an increase in account closures after the Federal Reserve Board adopted the initial regulatory rules, but not after the Act became effective. However, evidence from the Consumer Finance Monthly surveys suggests that the bulk of closures were initiated from the consumer and not the card issuer, consistent with the Agarwal et al. (2013) finding that the Act did not meaningfully reduce credit access.

This study estimates the impact of another component of the CARD Act: restricting credit card access to young adults. To my knowledge, just two papers have evaluated this component. First, Hawkins (2012) surveys approximately 500 students (not randomly) in post-CARD years at the University of Houston and Baylor University, with a response rate of nearly 90%. He uncovered evidence that students have continued to be marketed by credit card companies, that under-21 students have been able to access credit, and that some students have been able to easily bypass the income requirements by reporting student loans as independent income.

In addition, Hawkins stressed that the reports most typically cited as evidence of rising debt levels among students in Congressional arguments for the law are misleading. Specifically, the Sallie Mae and Nellie Mae series of studies draw from a population of students who have applied for private student loans (e.g., Sallie Mae, 2009). While Sallie Mae (2009) reports credit card ownership among undergraduates as high as 84% in April 2008, I find that credit card ownership among 18–21-year-olds prior to the Act is less than 25%.[6]

The age restrictions of the CARD Act were included in the law due to concerns that college-age young adults are unable to make optimal financial decisions regarding credit card debt. Debaut et al. (2014) challenged this assumption and, using the CARD Act to provide exogenous variation for age of entry into the credit markets, found evidence that borrowers who obtain a credit card at a young age (under 21) are no more likely to have serious credit delinquencies than those borrowers who first obtain a credit card at age 21.

As a first stage to test the impact of the law, the study found that the age restrictions of the Act reduce credit access among affected individuals by 8 percentage points (15%). However, the sample comprising this estimate may not be representative of the population of affected consumers.

Specifically, the study used the Equifax/Consumer Credit Panel, which tracks a 5% random sample of individuals with entry and exit since 1999, and is updated quarterly.[7] In order to exist in the Equifax universe, an individual must have a credit history. Individuals obtain a credit history when lenders report the repayment status of debt to credit bureaus. Credit card debt and

[6] Author's calculations using the Equifax/Consumer Credit Panel. This is a sample of all individuals and not just college students, who may be more likely to hold credit cards.
[7] The CCP also tracks debt at the household level, linking all individuals who live at the same address as those who make the initial 5% sample. This study, and Debaut et al., use the base sample to avoid using students who are considered in the same "household" but in reality just share a dormitory address.

installment loans are reported, but items such as utility bills, cable or cell phone bills, and rent are not reported, and therefore do not initiate credit histories with Equifax.

Thus, the population from which the sample is drawn is endogenous to the CARD Act itself: Individuals affected by the Act may not have a credit history and will not show up in the data. A standard difference-in-difference model which does not account for sample endogeneity would fail to consider the affected individuals who do not have a credit history in the post-Act period, which would lead to upwardly biased estimates. The authors accounted for this issue by comparing relative changes in credit card ownership among 20-year-olds versus 22-year-olds in 2008 and 2010, conditional on having a credit history by age 18 (thus, prior to the Act being announced).[8]

The impact of the CARD Act on this sample is likely to meaningfully differ from the average impact on the affected population for a number of reasons. First, it ignores the impact on individuals who were 19 years of age or younger in 2010, focusing instead on a single cohort of individuals of age 20, as of Dec 31, 2010. However, in the pre-Act period, many individuals who desired a credit card already obtained one by age 20, therefore reducing the impact of the law on this cohort.[9]

In addition, the restriction of having a credit history by age 18 has two implications. First, this reduces the sample of 20- and 22-year-olds *within the Equifax universe* by nearly 70%. Second, it ensures that all members of the sample had a credit history prior to the Act, making sample members even more likely to have already had a credit card in their own name prior to the Act. All of this leads to a sample where, for example, 53% of their 20-year-olds in 2008 have a credit

[8] The authors excluded the year 2009 due to concerns that affected borrowers responded to the law by obtaining credit in the period between the announcement and the effective date.
[9] See Figure 1, discussed in detail below.

card, which compares to my estimate of just 27% of the population of 20-year-olds in 2008 having a credit card.

This study builds on Debaut et al. (2014) to provide a more complete estimate of the impact of the CARD Act's age restrictions on credit card ownership. First, I combine the Equifax data with population data from the U.S. Census. This allows for estimates that represent the impact on card ownership for cohorts of each individual age within the entire U.S., instead of those with a prior credit history. Second, I demonstrate that I can satisfy the parallel trends condition necessary to produce unbiased estimates in a difference-in-differences model, once I control for the statewide economic conditions that individuals faced at each age where they are most likely to first obtain credit (ages 18–21). I then estimate heterogeneous treatment effects based on the age when first affected by the Act, as well as across demographic characteristics (e.g., the fraction of low-income households) of the puma.

Section 4 Data and Sample

Observations are at the age-year-puma level. The sample includes individuals aged 18–24 in each year from 2006 through 2013. The outcome of interest is the fraction of individuals within an age-year-puma that have a credit card in their own name, and controls include the annual statewide economic conditions that cohorts faced at each age.

Individual-level credit card ownership data comes from the Equifax Consumer Credit Panel (CCP) under the management of the Federal Reserve System.[10] Individuals are assigned to census tracts based on the addresses reported with the credit accounts. For college students, it is

[10] This data is detailed in Lee and van der Klaauw (2010).

possible that some may use their school addresses while other students use their permanent family address.[11] Both age and credit ownership are determined as of December 31 each year.

I've been granted access to the CCP, aggregated to the age-year-puma level for 18–24-year-olds from 2006–2013. The denominator of interest is the annual population count within an age-year-puma, which is generated from publicly available population estimates provided by the U.S. Census Bureau.[12] The Census only provides population counts for five-year age buckets at the county level in non-Census years (e.g., the number of individuals between the ages of 16 and 20 in county X in the year 2009).[13] I combine population counts by individual age from the 2010 Census with these five-year age buckets, along with a county-to-puma mapping, to estimate the population counts of each individual age in all years within each puma. This is detailed in Appendix A.

I use three economic measures to control for the economic conditions and the lending environment faced by each cohort. These include annual statewide unemployment rates and gross domestic product, which are publicly available from the Bureau of Labor Statistics. In addition, the Federal Reserve Bank of New York publishes annual per-capita outstanding mortgage balances for each state, which I use as a control for the lending environment within the state. From these data, I calculate annual statewide growth rates of per-capital mortgage balances and GDP.

[11] To address this possibility, I re-estimate the models at the state level instead of the puma level, and the results are comparable.
[12] College students are assigned to the address at which they spend the most time residing for both the Census and American Community Survey: http://www.census.gov/population/www/cen2010/resid_rules/resid_rules.html (Census) and https://askacs.census.gov/faq.php?id=5000&faqId=915 (ACS).
[13] Intercensal estimates at each individual age are available at the state and national level, but not at a geography smaller than the state. I requested this from Census and was turned down.

Individuals affected by the Act may still obtain credit by demonstrating sufficient income to repay debt. College students may be limited by income constraints while in school; however, they may also have the savviness to navigate the application process. I expect the Act to have differential effects across populations with varying income or college enrollment. I obtain time-invariant puma-level statistics from the American Community Survey five-year summary files (2006–2010), including the fraction of households with less than $25,000 annual income and the proportion of 18–24-year-olds enrolled in college. These statistics are publicly available for download through the Data Ferret tool provided by the U.S. Census Bureau.

Table 1 summarizes the relevant statistics of the sample. There are 980 pumas which have 18–24-year-olds in the Equifax data during the sample period.[14] The Equifax population—those with a credit history—sharply decreases from 2008–2012. This demonstrates the importance of controlling for sample endogeneity. From 2010–2012, the fraction of 18–24-year-olds in Equifax with a credit card increased from 37% to 45%, but this was due to the shrinking proportion of individuals with a credit history. The overall fraction of the population with a credit card was actually decreasing through this period from 21% to 16%. Finally, this table demonstrates the credit card usage of young adults in the pre-CARD period. While this data cannot differentiate college students, it shows that less than 30% of 18–24-year-olds had a credit card in 2008. This is sharply lower than the 84% of undergraduates with a credit card reported in Sallie Mae (2009).

The economic controls reflect aggregate statewide annual measures that are not age specific. The table shows unemployment rates rapidly rising through 2009, peaking in 2010, and slowly falling over the rest of the period. Per-capita GDP growth was negative in 2008 and 2009 before

[14] The eight pumas that drop out in 2012–2013 represent just 0.1% of the Equifax population from 2006–2011. There are just over 2,000 pumas in the United States.

returning to small, positive levels. Mortgage growth was explosive in 2006 and 2007, but has been negative since 2009.

The controls demonstrate a wide variety of factors in the economy: GDP growth reflects the economic contractions in 2008–2009. The high unemployment rates show that the economy was improving since 2010, but still weak in some areas. The mortgage balances reflect weak lending since 2009 after incredibly loose conditions in the early years of the sample.

Section 5 Identification and Methods

In this section, I discuss threats to identification and concerns over measurement in the data. I slowly build up the model to resolve these issues.

Table 2 displays the pattern and intensity of how cohorts have been affected by the CARD Act. Individuals who are under 21 years old as of February 2010 are affected by the age restrictions of the Act. However, these cohorts don't remain forever restricted from obtaining credit cards. The red arrow displays a cohort over time as its members age. Individuals born in 1990 are 20 years old by year-end 2010. These individuals were first affected by the CARD Act at age 20. The following year, in 2011, this cohort is 21 years old. They are no longer affected by the Act, but they had been previously (for one year).

The birth cohort of 1992 was 18 years of age in 2010, and so they faced the age restrictions for three years, from 2010–2012. In 2013, this cohort was 21 years old and no longer affected by the Act, but they had been affected for nearly all the years in which they would have most likely obtained a pre-Act credit card. This variance in treatment intensity guides my estimation strategy.

Credit card use has fallen among young adults after the implementation of the CARD Act. Figure 1 displays the fraction of individuals with a card in their own name, by birth cohort, as they age from 18 to 23. Those cohorts affected by the Act display a depression in credit card use from ages 18–20, with relative increases after turning 21 when the Act is no longer binding. If cohorts would have followed the same trajectory absent the Act, then a difference-in-differences model presented in Equation (1) with borrower-level data could estimate the causal impact of the Act on the credit card use of affected groups.[15]

$$(1)\ HasCreditCard_{it} = \alpha + \beta Under21_{it} * PostAct_t + \theta_i + \varphi_t + X_{it} + \varepsilon_{it}.$$

$HasCreditCard_{it}$ is a binary indicator that equals one if individual *i* in year *t* has a credit card. The variables θ_i and φ_t represent age and year fixed effects, X_{it} is a vector of individual characteristics that could affect the likelihood of having a credit card such as family income, and $Under21_{it} * PostAct_t$ represents being under 21 in the post-CARD Act period. Finally, β represents the causal impact of the Act on the likelihood that an individual has a credit card under the standard difference-in-differences assumptions of parallel trends, and no confounding treatments concurrent with the passage of the Act. Equifax data provides only the individual's birth year, so age is measured each year as of December 31st.[16]

Borrower-level data is restricted, although I have been granted license to use Equifax data aggregated to the age-puma-year level, and so I aggregate Equation (1) to this level. As shown in Figure 1, older individuals are much more likely to have a credit card, even before the CARD Act. To reflect that a five percentage point drop is a larger percentage drop for 18–19-year-olds

[15] Below, I discuss the potential effect of individuals responding in anticipation of the passage of the Act.
[16] Individuals with birthdays late in the calendar year may be identified as unaffected by the age restrictions of the Act, yet were affected for much of the year. This should bias estimates toward zero, and so the true magnitude of the effect of the Act may be greater than estimated.

than for 21–22-year-olds (with a much higher baseline of credit card ownership), I model the natural log of the fraction of credit card ownership within an age-puma-year, as in Equation (2).

$$(2)\ LnPctCreditCard_{apt} = \alpha + \beta Under21_a * PostAct_t + \rho_p + \theta_a + \varphi_t + X_{apt} + \varepsilon_{apt}.$$

Here, $LnPctCreditCard_{apt}$ represents the natural log of the fraction of individuals of age a in puma p in year t that have a credit card in their own name.[17] The variables ρ_p, θ_a, and φ_t represent puma, age, and year fixed effects respectively, while being "treated" is represented by $Under21_a * PostAct_t$ —that is, individuals between the ages of 18 and 20 in the post-CARD Act sample period 2010–2013. The vector X_{apt} consists of characteristics describing individuals of age a in puma p and year t, such as the statewide unemployment rate faced when the cohort was 18 years old.

The CARD Act was passed in the wake of the 2008–09 financial crash, during which time lenders tightened access to credit. This tightening of credit likely had greater effects on credit card access among young adults who hadn't already obtained credit. Indeed, Figure 2 shows that credit card ownership among 18–20-year-olds was already decreasing relative to older cohorts throughout 2008 and 2009. This is a violation of the parallel trends assumption needed for identification in a difference-in-differences framework.

I argue that the differential effect of the recession on under-21 borrowers can be controlled for, using time-varying statewide economic variables. Data provided by the Board of Governors (Figure 3) shows that financial institutions have tightened lending standards with regard to credit cards during recessionary periods. Figure 1 showed that most credit card holders first receive a card between ages18 and 21. Putting these together suggests that the age of initial credit

[17] Co-signed credit cards are disregarded.

ownership should be affected by the economic conditions present when an individual is between the ages of 18 and 21.

In equation (3), I regress the natural log of the fraction of individuals with a credit card within an age-puma-year on age and puma fixed effects, including only the pre-CARD years 2006–2009. I also include a number of statewide economic variables that individuals faced at each age. I then calculate the average residual by year for individuals over and under the age of 21. I exclude year fixed effects so that I may predict out of sample from 2010–2013.

$$(3)\ LnPctCreditCard_{apt} = \alpha + \rho_p + \theta_a + EconControls_{apt} + \varepsilon_{apt}.$$

The variable $EconControls_{apt}$ includes the state unemployment rate, the growth rate of per-capita GDP, and the growth rate of per-capita mortgage debt that each cohort faced at each age from 18 to 24, as well as interactions among the rates faced between the ages of 18 and 21.[18] Pumas do not cross state boundaries. All cohorts face the same economic condition within a state and year; however, the age at which they face these conditions varies. For example, California had a 12.35% unemployment rate in 2010. The 1990 birth cohort residing in California receives this value for "unemployment rate at age 20," while the 1992 birth cohort receives this value for "unemployment rate at age 18." I detail the precise main effects and interaction terms in Appendix A.

Figure 4 shows that the inclusion of economic controls almost completely eliminates the pre-Act difference in trends among those older or under the age of 21. This is necessary to satisfy the identification assumptions of a difference-in-differences model.[19] I then build on Equation (3) to

[18] These age-specific values are set to zero if the cohort hasn't reached that age by the observation year.
[19] The residuals gap between the two groups narrows in the final years of the sample. By 2013, most individuals between the ages of 21 and 24 had been previously affected by the CARD Act when younger.

estimate the causal impact of the CARD Act on the likelihood of young adults having a credit card in their own name.

$$(4)\ LnPctCreditCard_{apt} = \alpha + \beta * EverAffected_{at} + \rho_p + \theta_a + \varphi_t + EconControls_{apt} + \varepsilon_{apt.}$$

The variable $EverAffected_{at}$ is an indicator for a cohort having ever been affected by the Act through year *t*, regardless of age in the observation year. Specifically, cohorts born in 1990 or later in years starting in 2010 are "Ever Affected." Individuals born in 1990 would be no older than 20 years old by Dec 31, 2010 and thus no older than 20 years old in Feb 2010 when the Act went into effect.

The variables ρ_p, θ_a, and φ_t represent puma, age, and year fixed effects respectively. Standard errors are clustered at the birthyear · puma level to reflect that 21-year-olds in 2012 were the same 20-year-olds in 2011, and so on. Because credit ownership is typically maintained over time, cohorts with a high error term in one year are likely to have a high error term in later years.

It is unlikely that the reduction in credit card access due to the Act when the Act is binding (i.e., an individual is under 21) is equal to the impact when the Act is no longer binding. Equation (5) separates out the effect for individuals currently and previously affected by the Act.

$$(5)\ LnPctCreditCard_{apt} = \alpha + \beta * CurrentlyAffected_{at} + \gamma * PreviouslyAffected_{at} + \rho_p + \theta_a + \varphi_t + EconControls_{apt} + \varepsilon_{apt}.$$

The variable $CurrentlyAffected_{at}$ takes on the value of 1 for cohorts who are under age 21 in year *t* in the post-CARD years 2010–2013, whereas $PreviouslyAffected_{at}$ takes on a value of 1 for cohorts who are now at least 21 years old, but who had been previously affected by the

CARD Act. Specifically, these will be individuals born in 1990 or later but 21 years old, or older, in year t.

Affected individuals who were relatively older when the Act was first enacted had more opportunities to obtain credit prior to the Act's passage. Equation (6) distinguishes the currently affected and previously affected variables by the cohorts' age as of Dec 31, 2010—the initial age at which the cohort was affected by the Act. Among cohorts currently affected in the post-CARD years, there are cohorts that were ages 20, 19, 18, and under-18 when the Act passed.[20] Cohorts previously affected but now at least 21 years old within the sample years were ages 20, 19, and 18 when the Act passed.[21]

$$(6)\ LnPctCreditCard_{apt} = \alpha + \sum_{i=17}^{20} \beta_i * CurrentlyAffected_{ati} + \sum_{i=17}^{20} \gamma_i * PreviouslyAffected_{ati} + \rho_p + \theta_a + \varphi_t + EconControls_{apt} + \varepsilon_{apt}\ .$$

The variable $CurrentlyAffected_{ati}$ takes on a value of 1 for cohorts of age a in year t who were initially affected by the Act at age i and are still under age 21, whereas $PreviouslyAffected_{ati}$ takes on a value of 1 for cohorts of age a in year t who were initially affected by the Act at age i and are at least 21 years old in year t.

The Act allows individuals to obtain credit by independently verifying an ability to repay debt. Though, Hawkins (2012) provides evidence that college students have obtained credit by using student loans and family income to bypass weak verification procedures. This process may take

[20] Under-18 is referenced as 17 in the summation operator in Equation (6).
[21] Cohorts under age 18 in 2010 all remain under age 21 through the final year of the sample in 2013.

consumer savviness and financial literacy which would be associated with higher educated individuals. I expect that individuals with higher income and financial literacy would have the easiest time obtaining credit.

On the other hand, individuals from wealthier households may have credit-worthy parents willing to co-sign for credit cards (which do not show up as an “owned” credit card in this sample) or provide intra-household loans. Individuals from lower-income households may have greater need for credit and may be more willing to put in effort to bypass the Act so as to avoid costlier alternatives such as reducing consumption or using payday loans.

I split pumas into high and low levels of poverty and education. Specifically, I label pumas as being in the top and bottom 50% fraction of low-income households (those earning less than $25,000 annually) and the top and bottom 50% fraction of adults aged 25 or older with at least a bachelor’s degree. I then fully interact the predictors of interest from equations (4) and (5) with indicators for being in the high or low fraction low-income pumas. In addition, I interact the poverty and education measures and re-estimate the models with these four mutually exclusive categories of high and low rates of poverty and degree attainment.

Finally, Debaut et al. (2014) found evidence of anticipation effects in their sample: underage individuals obtaining credit cards earlier than they might have, absent the Act. This phenomenon could bias estimates of Act’s impact, because underage individuals would have a particularly high error term in the pre-period, leading to a steep estimated decline in the post-period.

To account for this, I re-estimate models one through six excluding the year 2009, and find that all the results hold, and that the estimates are comparable. There may be less evidence of anticipatory effects in my sample for two reasons. First, I include individuals younger than age

20 in the pre-Act years so that the cohorts who anticipate the Act are still under 21 after the Act passes. For these additional cohorts, the treated group has a high error term in both the pre- and post-Act periods. In addition, I use estimates representative of the entire population, and not just those with a credit history by age 18. Those individuals who entered the credit markets by age 18 may be particularly likely to have been aware of the CARD Act and responded accordingly.

Section 6 Results

Table 3 displays the impact of the CARD Act on credit card ownership among the affected population. The first two columns of Table 3 display the impact of being ever affected by the age restrictions of the CARD Act, as estimated in Equation (4). Column 1 excludes economic controls while Column 2 includes $EconControls_{apt}$ and is the preferred specification. Affected cohorts saw an overall reduction of 0.55 log points after the enactment of the Act. Economic controls explain 26% of this, leaving the CARD Act having reduced credit card ownership by an estimated 0.404 log points.

The outcome is in log points, which can be difficult to interpret. For each affected group, I calculate the baseline mean[22] and convert the coefficient estimates–from the specifications that include economic controls–into estimated reductions off the baseline mean due to the Act. The first row of columns 4 and 5 shows that the CARD Act led to a 9 percentage point decline in credit card ownership among those ever affected by the Act. At baseline, 27% of individuals of ages that would be affected by the Act during the sample (ages 18-23) had a credit card.

Columns 3 and 4 of Table 3 display the impact of being currently or previously affected by the Act as estimated in Equation (5). Column 4 includes economic controls in the specification. In

[22] Specifically, I calculate the mean fraction with a credit card among individuals of the same age as each affected group using years 2006-2009.

Column 4, we see that the impact of the Act is, not surprisingly, concentrated on those cohorts still affected in the observation year. The impact of the Act on currently affected cohorts is a reduction of 0.498 log points. There is a smaller, but meaningful impact on those previously affected but now over 21 years old. The Act reduces card ownership among this group by 0.252 log points. This translates into a 7 percentage point and 8 percentage point decline off baseline means of 18% and 35% respectively for the currently and previously affected groups.

Columns 5 and 6 of Table 3 display the impact of being currently or previously affected by the Act, varying by the cohort's age as of Dec 31, 2010, as estimated in Equation (6). Column 6 shows results inclusive of economic controls. Within both currently and previously affected cohorts, there is a monotonic relationship between the estimated impact of the CARD Act on card ownership and the age of the cohort when the Act was enacted. Specifically, cohorts who were youngest when the Act was passed witness the greatest reductions in credit card ownership due to the Act. This is expected, as older cohorts had a greater opportunity to obtain credit prior to its passage.

Individuals who were under-18, 18, 19, and 20 as of Dec 2010 observe a decrease of 0.768, 0.604, 0.474, and 0.348 log points respectively. This translates into a reduction in the fraction of individuals in the cohort with a credit card of 53%, 45%, 37%, and 29% respectively (dividing the estimated reduction by the baseline mean), while currently affected by the Act. These reductions vary from the baseline mean values of 18%, 18, 23%, and 28% respectively.

Individuals who were aged 18, 19, and 20 as of Dec 2010, but who are age 21 or older in year *t*, observe a decrease of 0.580, 0.409, and 0.246 log points respectively. This translates into decreases of 44%, 34%, and 22% from the baseline percent of individuals with a credit card: 33%, 34%, and 36% respectively.

It may seem surprising that individuals who were 18 when the Act passed observe a very similar estimated impact of the Act when currently affected or previously affected. However, this cohort has only one period when it is labeled as previously affected—in 2013, when 21 years of age as of December 31st. Many individuals in this cohort may have only just turned 21 for a few months by the end of 2013. In addition, this cohort had a larger "dosage" of the Act than any other group of 21-year-olds in the sample.

Table 4 displays the estimated coefficients and predicted percentage declines obtained from re-estimating Equation (5) and interacting the predictors of interest with variables reflecting the demographic composition of the puma. Column 1 of Table 4 shows the impact of being currently and previously affected by the Act across pumas with a high or low fraction of low-income households.

In pumas with an above-average fraction of low-income households (Row 2, "Above Median Pct Poverty"), individuals currently affected by the Act see a reduction in credit card ownership of 0.56 log points. Columns 4 and 5 show that this represents a decline of 0.065 (43%) from a baseline mean of 0.152. In those same high-poverty pumas, individuals previously affected by the Act see a reduction in credit card ownership of 0.301 log points. This represents a decline of 0.082 (26%) from a baseline of 0.317. In low-poverty pumas, individuals currently affected by the Act observe a 35% decline in credit card ownership off a baseline of 0.201. In the same low-poverty pumas, individuals previously affected by the Act face a 16% reduction in credit card ownership from a baseline of 0.392.

Column 2 of Table 4 displays the results of estimating Equation (5), with separate estimated effects across pumas with high or low fraction of adults over age 25 who have attained at least a B.A. degree. Individuals from pumas with relatively lower degree attainment face a steeper decline in credit card ownership due to the Act. Though, the differential impact across education level is not nearly as great as the difference across poverty levels.

For the final specification, I assign pumas to the interaction of high/low poverty rates and high/low rates of degree attainment.[23] Column 3 of Table 4 demonstrates again that the impact of the CARD Act is more varied across poverty than education rates. The percentage reduction in credit card ownership due to the Act is greater (and the baseline smaller) in high poverty areas – regardless of educational achievement – than in low poverty areas for both currently and previously affected individuals.

Table 5 displays the results from re-estimating Equations (4) through (6) but excluding the year 2009 in case of anticipatory effects. I find that the point estimates without economic controls are mostly unchanged, while the estimates with economic controls become even larger in magnitude. Perhaps this is because 2009 was a year in which there was both a sharp economic downturn as well as a sharp decrease in credit card ownership among young people. Therefore, removing this year removes a lot of variation in card ownership due to economic conditions.

Section 7 Conclusions

During the years 2010 through 2013, the age restrictions of the CARD Act have affected more than 25 million individuals between the ages of 18 and 20. The Act was passed in part due to misleading statistics on credit card use among undergraduates. I estimate that, absent the Act,

[23] The high poverty/low B.A. and low poverty/high B.A. groups each have a 37% share of pumas. The high poverty/high B.A. and low poverty/low B.A. each have a 13% share.

only 15%–20% of 18–20-year-olds would have had a credit card in their own name during the years 2010–2013.

While the intention of the Act was to safeguard young adults from becoming over indebted, it is possible the Act could have adverse long-term consequences due to having a thin credit file later in life. Loopholes in the Act allow for some individuals under the age of 21 to obtain credit, and one survey suggests these loopholes are being utilized.

I find evidence that the CARD Act reduced credit card access by nearly 50% for individuals who were 18 years old or younger when the Act first went into effect, and there are lingering reductions in credit card ownership even after affected individuals turn 21. The impact is smaller for cohorts who were either 19 or 20 years old when the Act passed, likely because many of these individuals had already obtained credit cards prior to the Act's effective date.

Post-CARD, obtaining credit while under the age of 21 requires a certain level of consumer savviness, in addition to being able to demonstrate an ability to repay debt. I find that the reduction in card ownership among young adults is greater in areas with a high fraction of low-income households. This situation has the potential to be particularly harmful. Specifically, individuals with the least amount of income (and likely wealth) are now left without a common form of temporary debt financing. This could lead low-income individuals to substitute toward more costly forms of debt, such as payday lending.

In ongoing research, I am looking to see how consumers have responded to this reduction in credit risk. I have preliminary evidence that individuals from low-income households have substituted into *alternative financial services*, which includes payday lending.

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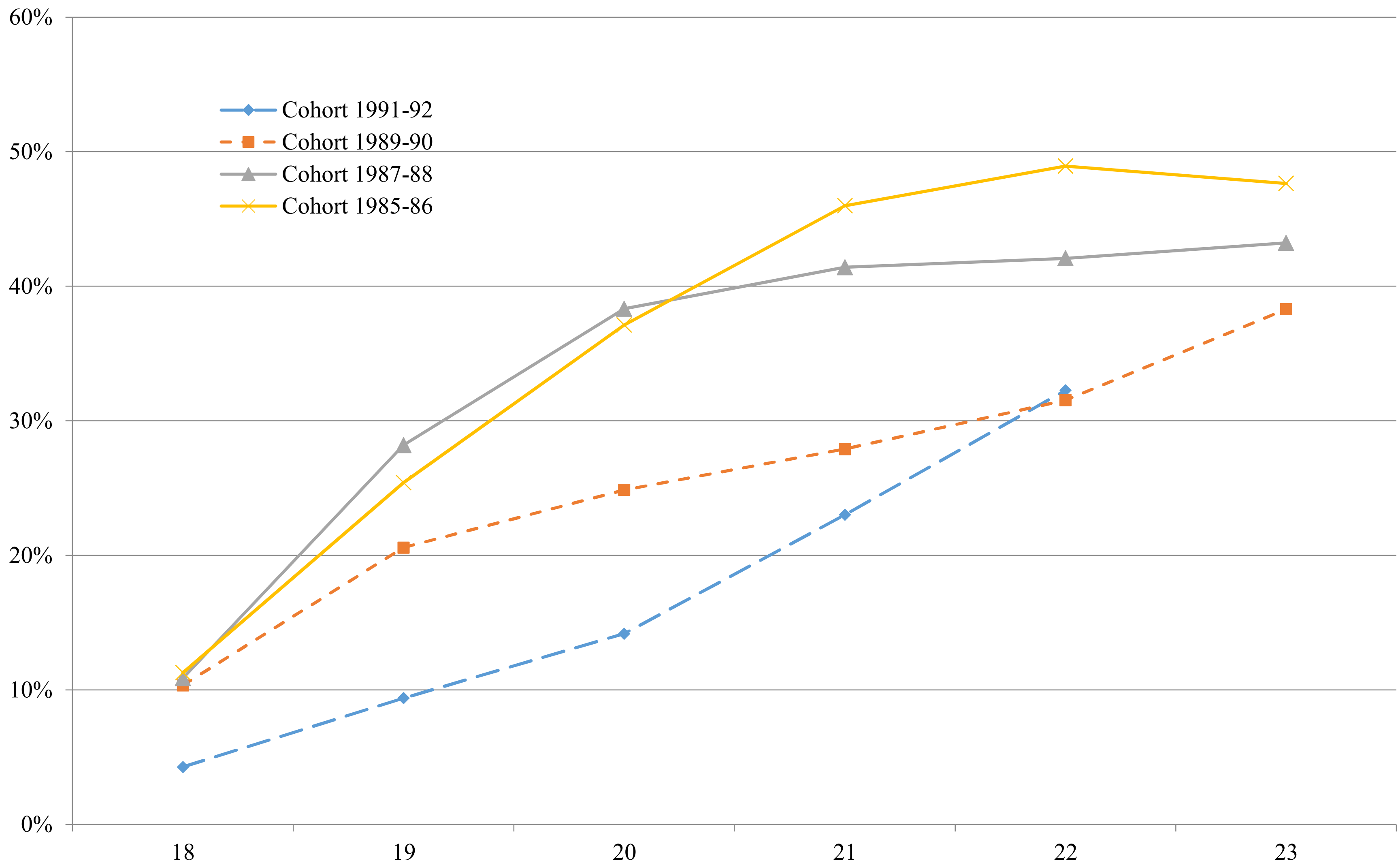

Figure II.1. Fraction of Individuals Aged 18–23 in the US with a Credit Card in Their Own Name. Lines represent birth year cohorts. Cohorts born in 1991−1992 were aged 18−19 when the CARD Act went into effect in 2010 and are the most restricted by the Act. Cohorts born in 1989−1990 were aged 20−21 by Dec 31, 2010 and may have been moderately restricted. Cohorts born in 1985−1988 were unaffected by the age restrictions of the CARD Act. Data is from the Equifax/Consumer Credit Panel aggregated by age and year to the national level.

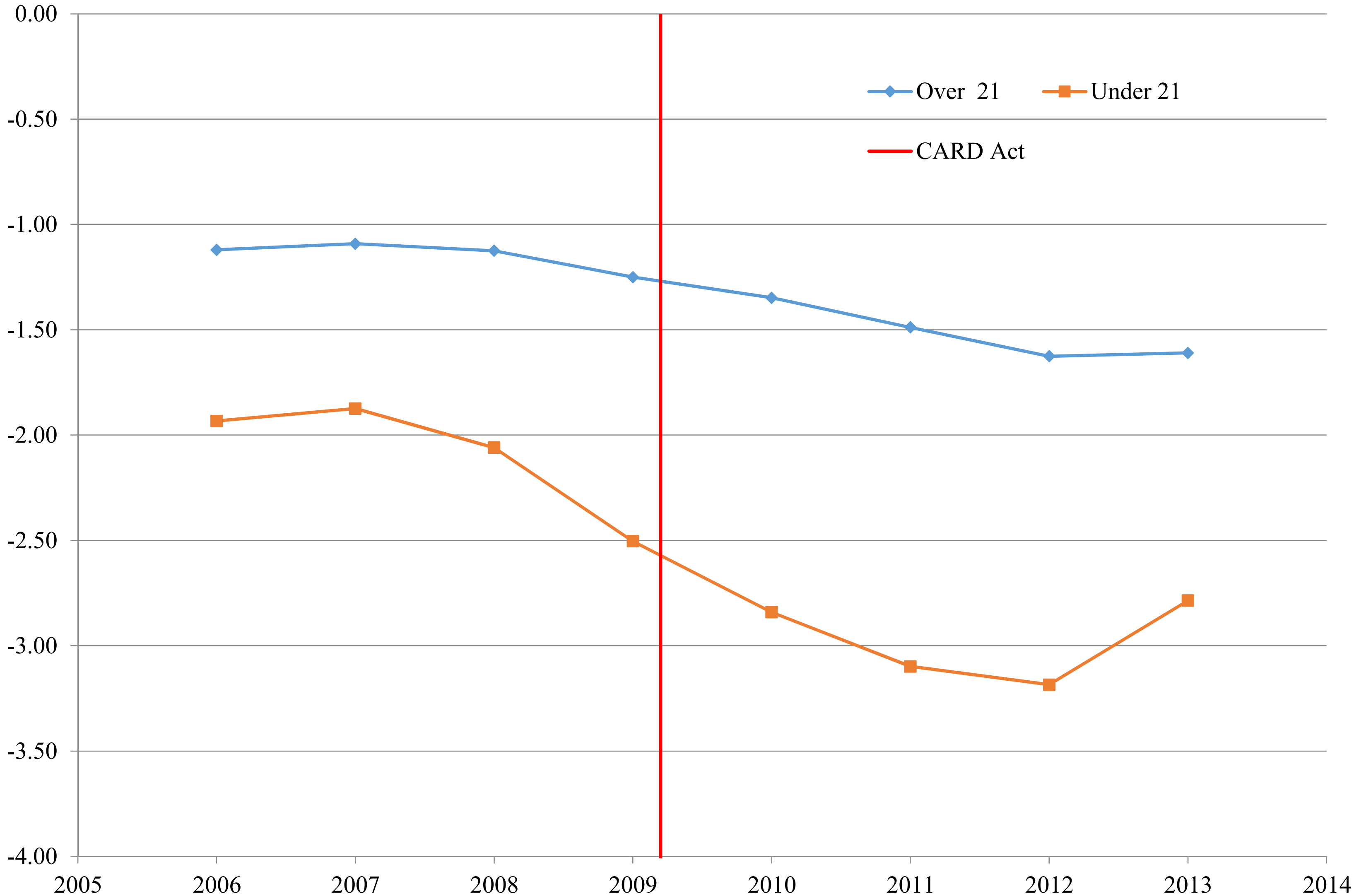

Figure II.2. Natural Log of the Fraction of Individuals in the US with a Credit Card in Their Own Name as of December 31st of Each Year. The vertical red line represents the effective date of the CARD Act (Febuary 2010). The "Over 21" line represents individuals aged 21–24 in each year. "Under 21" represents individuals aged18–20. Data comes from the Equifax/Consumer Credit Panel aggregated by age and year to the national level.

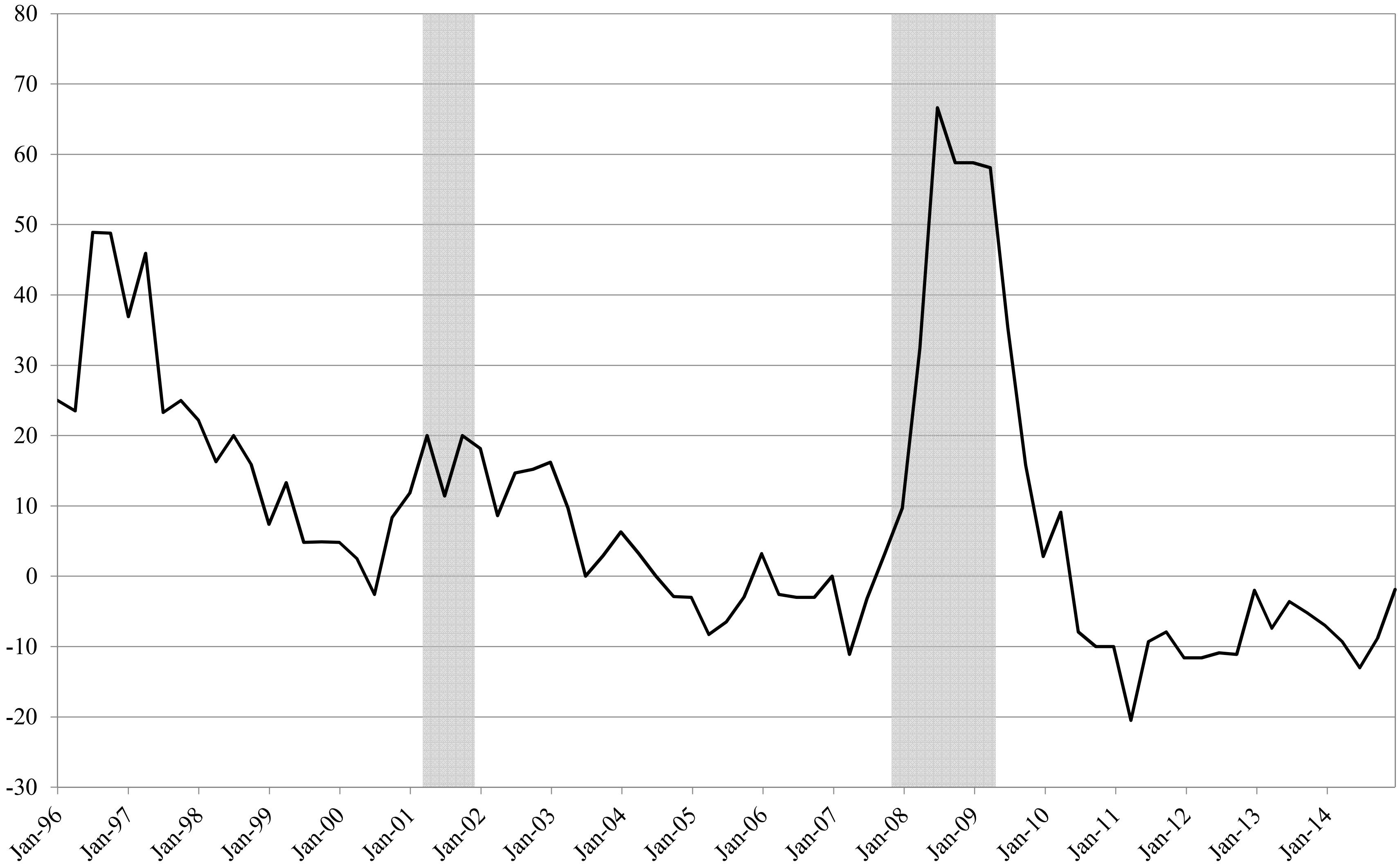

Figure II.3. Net Percentage of Domestic Banks Tightening Lending Standards on Consumer Loans and Credit Cards. Shaded areas indicate US recessions. Reported net fractions equal the fraction of banks that reported having tightened lending standards over the past three months minus the fraction of banks that reported having eased standards. Data comes from the Federal Reserve Senior Loan Officer Opinion Survey on Bank Lending Practices. This figure was downloaded from research.stlouisfed.org.

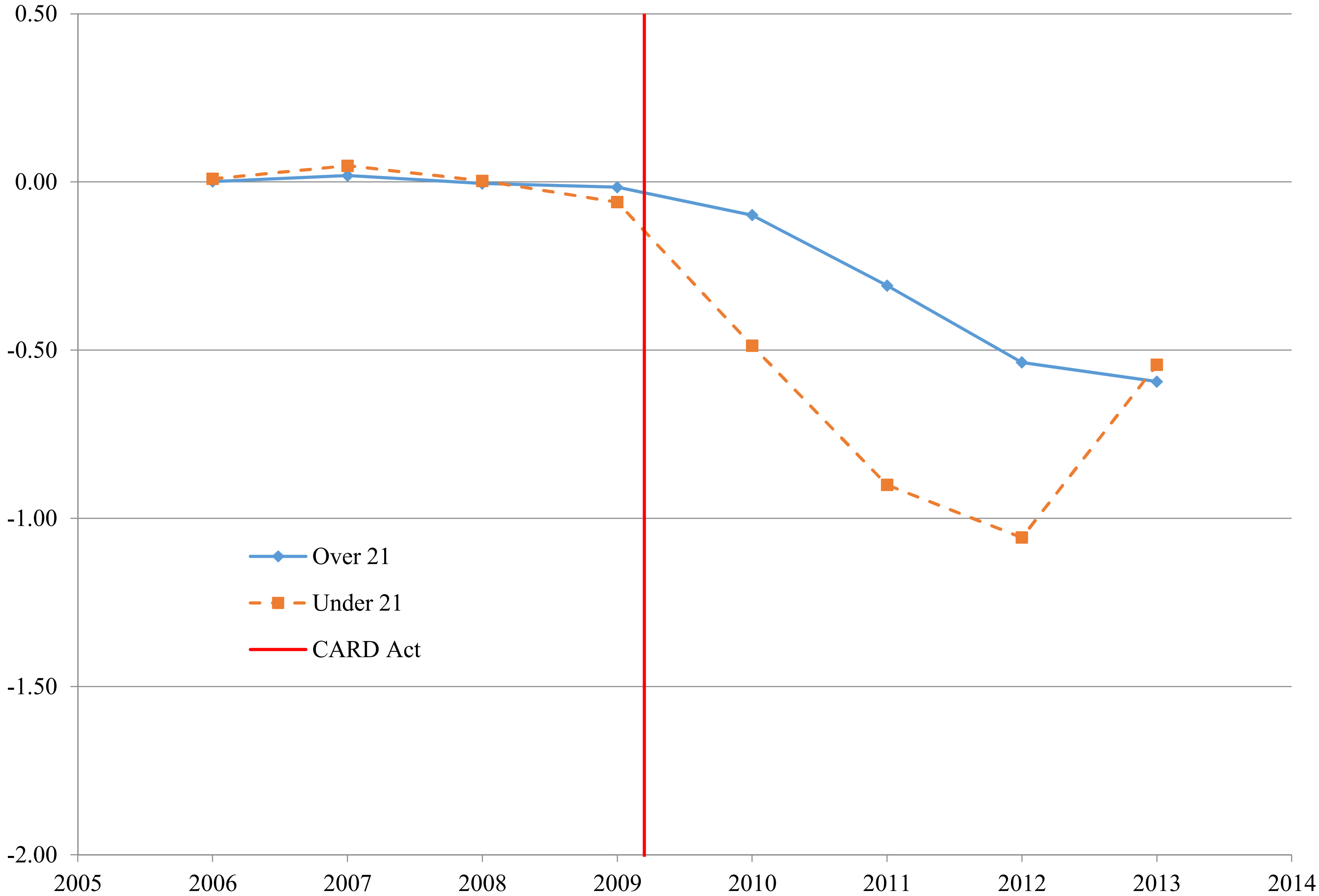

Figure II.4. Regression-Adjusted Natural Log of the Fraction of Individuals in the US with a Credit Card in Their Own Name as of December 31st of Each Year. Plotted are residuals from a regression on puma and age fixed effects and flexible controls for the economic conditions individuals faced at each age using years 2006 through 2009 and predicting out of sample for 2010–2013. The vertical red line represents the effective date of the CARD Act (February 2010). "Over 21" includes individuals aged 21–24 while "Under 21" includes those aged 18-20.

Table II.1. Summary Statistics of Credit Card Ownership and Economic Conditions Facing 18–24 Year Olds.

	2006	2007	2008	2009	2010	2011	2012	2013
Number of Pumas	980	980	980	980	980	980	972	972
5% Sample Equifax Population	779,244	774,972	747,254	712,462	676,642	595,069	514,477	660,592
5% Sample Puma Population	1,467,233	1,469,880	1,483,446	1,506,228	1,528,370	1,539,843	1,516,998	1,548,797
Fraction Equifax Population with Credit Card	47%	49%	48%	41%	37%	36%	45%	38%
Fraction Total Population with Credit Card	29%	30%	28%	24%	21%	18%	16%	18%
Mean Statewide Unemployment Rate	4.64	4.61	5.81	9.26	9.65	8.99	8.11	7.39
Mean Per-Capita State GDP	49,065	49,356	48,607	46,896	47,517	47,882	48,737	49,250
Mean Growth in Per-Capita State GDP	1.7%	0.4%	-1.7%	-3.9%	1.3%	0.8%	1.7%	1.1%
Mean Per-Capita Outstanding Mortgage Balances	33,810	37,749	39,015	37,322	35,491	34,684	33,665	32,047
Mean Growth in Per-Capita Mortgage Balances	13.6%	11.4%	4.0%	-3.7%	-4.3%	-1.8%	-2.6%	-4.7%

Notes: Equifax and credit card ownership data come from the Equifax/Consumer Credit Panel under management of the Federal Reserve Bank of New York. Puma population is estimated using county-level intercensal estimates and a county-puma mapping from the Missouri Census Data Center. Unemployment and GDP statistics are from the Bureau of Labor Statistics. Statewide per-capita mortgage balances are provided by the Federal Reserve Bank of New York. Means are weighted by the population of the puma in each year. Unemployment, GDP, and mortgage data are at the state-year level and are not age specific.

Table II.2. Intensity of the CARD Act Age Restrictions for Individuals of Each Age over Time.

Age	2008	2009	2010	2011	2012	2013
18	No	No	Affected at Age 18 and Still Affected	Affected by Age 18 and Still Affected	Affected by Age 18 and Still Affected	Affected by Age 18 and Still Affected
19	No	No	Affected at Age 19 and Still Affected	Affected at Age 18 and Still Affected	Affected by Age 18 and Still Affected	Affected by Age 18 and Still Affected
20	No	No	Affected at Age 20 and Still Affected	Affected at Age 19 and Still Affected	Affected at Age 18 and Still Affected	Affected by Age 18 and Still Affected
21	No	No	No	Was Affected at Age 20	Was Affected at Age 19	Was Affected at Age 18
22	No	No	No	No	Was Affected at Age 20	Was Affected at Age 19
23	No	No	No	No	No	Was Affected at Age 20

Notes : The table represents the exposure to the CARD Act for individuals of a particular age each year. The red arrow represents a cohort: Individuals aged 18 in 2009 are then age 19 in 2010 and so forth. Green boxes represent groups unaffected by the Act through that year. Orange boxes (top right rectangle) are groups restricted by the Act in that year. Yellow boxes (bottom right triangle) are cohorts that were previously restricted by the Act, but now of an age where the restrictions are no longer binding. Text denotes "dosage": the age at which first affected, if ever, by the Act through that year.

Table II.3. Causal Impact of the CARD Act on the Logged Fraction of Individuals with a Credit Card.

	(1)	(2)	(3)	(4)	(5)	(6)	Baseline Mean Fraction with a Credit Card	Estimated Reduction due to the CARD Act
Ever Affected (Year >= 2010 · Birth Cohort >= 1990)	-0.550***	-0.404***					0.27	0.09
	(0.006)	(0.012)						
Currently Affected (Under 21 · Ever Affected)			-0.759***	-0.498***			0.18	0.07
			(0.007)	(0.012)				
Previously Affected (Over 21 · Ever Affected)			-0.294***	-0.252***			0.35	0.08
			(0.006)	(0.013)				
Currently Affected · Birth Cohort >= 1993 (Under 18 as of 12/31/2010)					-0.923***	-0.768***	0.18	0.09
					(0.011)	(0.019)		
Currently Affected · Birth Cohort = 1992 (Age 18 as of 12/31/2010)					-0.829***	-0.604***	0.18	0.08
					(0.010)	(0.017)		
Currently Affected · Birth Cohort = 1991 (Age 19 as of 12/31/2010)					-0.669***	-0.474***	0.23	0.09
					(0.010)	(0.014)		
Currently Affected · Birth Cohort = 1990 (Age 20 as of 12/31/2010)					-0.413***	-0.348***	0.28	0.08
					(0.008)	(0.017)		
Previously Affected · Birth Cohort = 1992 (Age 18 as of 12/31/2010)					-0.672***	-0.580***	0.33	0.14
					(0.012)	(0.020)		
Previously Affected · Birth Cohort = 1991 (Age 19 as of 12/31/2010)					-0.506***	-0.409***	0.34	0.11
					(0.008)	(0.016)		
Previously Affected · Birth Cohort = 1990 (Age 20 as of 12/31/2010)					-0.278***	-0.246***	0.36	0.08
					(0.006)	(0.014)		
Observations	126,750	126,750	126,750	126,750	126,750	126,750		
R-squared	0.861	0.879	0.870	0.880	0.874	0.881		
Economic Controls		X		X		X		

Notes : Observations are at the puma-year-age level. Outcome is the natural log of the fraction of individuals with a credit card for years 2006–2013 based on year-end credit bureau data. All regressions include fixed effects for puma, age, and year. Birth cohort of 1990 is the oldest "full cohort" that was affected by the Act as no individuals were age 21 as of Feb 2010. Individuals born in 1993 do not reach age 21 during the sample period, which is why there is no "Previously Affected" result for that cohort. Robust standard errors are clustered at the puma-birthyear level and are presented in parentheses. Observations are weighted by the population count within each puma-year-age cell. The baseline mean represents the mean fraction of individuals with a credit card among individuals of the same age in the pre-Act period. *** p < 0.01, ** p < 0.05, * p < 0.1

Table II.4. Heterogenous Impacts of the CARD Act by Pre-CARD Demographic Characteristics of the Puma.

	(1)	(2)	(3)	Baseline Mean Fraction with a Credit Card	Estimated Reduction due to the CARD Act
Currently Affected · Below Median Pct Poverty	-0.425***			0.201	0.070
	(0.014)				
Currently Affected · Above Median Pct Poverty	-0.560***			0.152	0.065
	(0.014)				
Previously Affected · Below Median Pct Poverty	-0.178***			0.392	0.064
	(0.014)				
Previously Affected · Above Median Pct Poverty	-0.301***			0.317	0.082
	(0.014)				
Currently Affected · Below Median Pct B.A.		-0.522***		0.161	0.065
		(0.014)			
Currently Affected · Above Median Pct B.A.		-0.477***		0.193	0.073
		(0.014)			
Previously Affected · Below Median Pct B.A.		-0.274***		0.322	0.077
		(0.014)			
Previously Affected · Above Median Pct B.A.		-0.233***		0.388	0.081
		(0.014)			
Currently Affected · High Poverty-Low B.A.			-0.544***	0.149	0.063
			(0.036)		
Currently Affected · High Poverty-High B.A.			-0.598***	0.160	0.072
			(0.036)		
Currently Affected · Low Poverty-Low B.A.			-0.438***	0.193	0.068
			(0.041)		
Currently Affected · Low Poverty-High B.A.			-0.419***	0.204	0.070
			(0.042)		
Previously Affected · High Poverty-Low B.A.			-0.289***	0.304	0.076
			(0.036)		
Previously Affected · High Poverty-High B.A.			-0.326***	0.354	0.098
			(0.036)		
Previously Affected · Low Poverty-Low B.A.			-0.186***	0.373	0.063
			(0.035)		
Previously Affected · Low Poverty-High B.A.			-0.175***	0.399	0.064
			(0.036)		
Observations	126,750	126,750	126,750		
R-squared	0.881	0.880	0.881		
Economic Controls	X	X	X		

Notes : Observations at the puma-year-age level. Outcome is the natural log of the fraction of individuals with a credit card for years 2006–2013 based on year-end credit bureau data. All regressions include flexible economic controls and fixed effects for puma, age, and year. Robust standard errors are clustered at the puma-birthyear level and are presented in parentheses. Observations are weighted by the population count within each puma-year-age cell. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table II.5. Causal Impact of the CARD Act on the Logged Fraction of Individuals with a Credit Card. Excludes Year 2009.

	(1)	(2)	(3)	(4)	(5)	(6)
Ever Affected (Year >= 2010 · Birth Cohort >= 1990)	-0.579***	-0.540***				
	(0.006)	(0.017)				
Currently Affected (Under 21 · Ever Affected)			-0.859***	-0.752***		
			(0.008)	(0.018)		
Previously Affected (Over 21 · Ever Affected)			-0.287***	-0.418***		
			(0.007)	(0.016)		
Currently Affected · Birth Cohort >= 1993 (Under 18 as of 12/31/2010)					-1.047***	-1.167***
					(0.011)	(0.024)
Currently Affected · Birth Cohort = 1992 (Age 18 as of 12/31/2010)					-0.934***	-0.974***
					(0.010)	(0.023)
Currently Affected · Birth Cohort = 1991 (Age 19 as of 12/31/2010)					-0.753***	-0.795***
					(0.010)	(0.021)
Currently Affected · Birth Cohort = 1990 (Age 20 as of 12/31/2010)					-0.478***	-0.510***
					(0.009)	(0.021)
Previously Affected · Birth Cohort = 1992 (Age 18 as of 12/31/2010)					-0.692***	-0.891***
					(0.012)	(0.023)
Previously Affected · Birth Cohort = 1991 (Age 19 as of 12/31/2010)					-0.523***	-0.683***
					(0.008)	(0.020)
Previously Affected · Birth Cohort = 1990 (Age 20 as of 12/31/2010)					-0.290***	-0.403***
					(0.006)	(0.016)
Observations	110,634	110,634	110,634	110,634	110,634	110,634
R-squared	0.859	0.878	0.872	0.879	0.878	0.882
Unemployment and GDP Controls		X		X		X

Notes : Observations at the puma-year-age level. Outcome is the natural log of the fraction of individuals with a credit card for years 2006-2008, 2010-2013 based on year-end credit bureau data. All regressions include fixed effects for puma, age, and year. Birth cohort of 1990 is the oldest "full cohort" that was affected by the Act as no individuals were age 21 as of Feb 2010. Individuals born in 1993 do not reach age 21 during the sample period, which is why there is no "Previously Affected" result for that cohort. Robust standard errors are clustered at the puma-birthyear level and are presented in parentheses. Observations are weighted by the population count within each puma-year-age cell. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

CHAPTER III

Choosing Delinquency? Ranking Student Loans on the Repayment Hierarchy

Section 1 Introduction

Student loan cohort default rates have been increasing steadily since the graduating cohort of 2005, and the 2010 graduating cohort had reached the highest level under the modern definition of default.[1] Student loan delinquency rates have climbed steadily since 2003 even as delinquency rates in other debt markets have fallen since 2010.[2] These increasing rates have attracted attention in the media, and among policymakers and researchers seeking to understand why borrowers are not repaying their student loans.

Recent surveys of the student loan default literature have suggested a number of factors related to default and delinquency, mostly related to student and school characteristics. However, there are very few studies that consider student loan repayment within a model of consumer choice. Each period, households face choices on where to allocate their income and wealth: to consumption, for savings, and to repayment of other debts including auto loans, mortgages, and credit card balances. In addition, the relative incentives to repay certain debts can change over time with market conditions. For example, variable interest rates, home prices, and knowledge of the consequences of non-repayment could potentially influence the consumer's choice.

[1] http://www2.ed.gov/offices/OSFAP/defaultmanagement/defaultrates.html
[2] http://www.newyorkfed.org/research/national_economy/householdcredit/DistrictReport_Q22013.pdf

This study demonstrates how the debt repayment hierarchy can be observed in the publicly available Survey of Consumer Finances, and offers preliminary evidence for understanding these repayment preferences. The evidence suggests that consumers prioritize student loan repayment below other installment debts, including auto and home loans. First, when consumers have multiple loans and are delinquent on some—but not all—of their debts, households overwhelmingly choose to miss student loan payments. In addition, when households have just a single loan, there is evidence to suggest that, relative to consumption, the repayment of student loans has a lower priority than home mortgage or vehicle loans. We cannot observe credit card delinquency in the data.

Repayment prioritization should be important for a number of reasons. From a policy perspective, all student loans originated since July 2010 are Direct Loans owned by the Department of Education. Non-repayment of student loans in favor of private, non-education loans shifts credit risk from private lenders to taxpayers. In addition, policies that alter parameters of the student loan market may have consequences in other debt markets. For example, changing the consequences for non-repayment of student loans may alter repayment preferences, shifting delinquencies to and from other debt markets.

From an economic perspective, we may be concerned that consumers are not behaving optimally. Recent surveys suggest that borrowers in default are mostly unaware of the consequences of default, and unaware of alternative repayment options such as income-based repayment (e.g., Loonin & McLaughlin, 2012). Theory suggests that borrowers weigh the benefits and consequences of consumption, saving, and repayment of debt, allocating resources across the three during each period. A rational consumer who underestimates the consequences of non-repayment of student loans may thus sub-optimally allocate financial resources.

This study has two research questions:

1. How do consumers prioritize education loans relative to other installment loans?
2. How can this prioritization be explained through observed loan characteristics?

The rest of the paper is set up as follows: Section 2 is a review of the relevant literature. Section 3 describes the data, construction of key variables, and the analysis sample. Section 4 displays stylized facts from the data, which demonstrate the repayment prioritization. Section 5 describes the repayment prioritization model. Section 6 describes the results, and Section 7 concludes the paper.

Section 2 Contribution to the Literature

This study contributes to multiple threads of the academic literature related to consumer credit. First, there are very few studies explicitly modeling the consumer's decision-making process regarding delinquency. Cohen-Cole and Morse (2010) used credit bureau data to isolate households who have both a credit card and a home mortgage and miss a payment in exactly one type of debt. They modeled the event of households missing a home mortgage payment while making credit card payments on time, and found that liquidity concerns dominate this decision. Specifically, households who have credit card balances close to the limit will choose to miss home mortgage payments while paying down credit card balances, even when there is equity in the home. The results are counter to industry expectations that consumers will pay their mortgages first when there is positive home equity.

TransUnion (2012) displays summary statistics on the repayment prioritization over time of home mortgages versus auto loans. This study also isolated consumers who are delinquent on exactly one type of debt, and found that the repayment priority of consumers who had historically favored home mortgages shifted to auto loans during the U.S. mortgage default crisis, and then shifted back toward mortgages as home prices stabilized since the 2008-09 recession.

Ionescu and Ionescu (2015) have a recent working paper modeling the decision of consumers to choose default—a more severe option than delinquency—on either student loans or credit cards, but not both. They found evidence of cross-market effects: Credit card debt amplifies the likelihood of defaulting on student loans. The authors argued that this is due to differences in bankruptcy regulations and consumers' desire for liquidity: Financially constrained borrowers find it optimal to default on student loans in order to maintain access to the credit card market.

A second literature thread examines consumer repayment of individual debts, with a particular emphasis on student loan default. Most of the studies on student loan default are correlation studies, identifying factors that are related to default. For example, Gross, Cekic, Hossler, and Hillman (2009) and Loonin and McLaughlin (2012) detailed a number of characteristics often found to be related to student loan default, such as educational experience (attainment, degree, major choice), student characteristics at college entry, institutional characteristics, employment and earnings, student debt levels, and attitudes toward student loan debt. In a causal paper on student loan default, Hershaff (2015) demonstrated that increasing financial incentives to the lender to keep borrowers out of default reduced borrower default rates, especially among borrowers at for-profit colleges.

A number of studies have looked at the causes and correlates of default in other debt markets. Elul, Souleles, Chomsisengphet, Glennon, and Hunt (2010) showed that illiquidity and negative equity "trigger" mortgage default. Keys, Mukherjee, Seru, and Vig (2010) demonstrated that the ease of securitization reduced lender screening, which increased default rates. Guiso, Sapienza, and Zingales (2013) found that in March 2009, 26% of defaults were "strategic,"—due to negative home equity—which increased to 35% by September 2010. Agarwal, Ambrose, and Chomsisengphet (2008) found that auto loan defaults are related to weak credit, age of the car, and the loan-to-value ratio of the vehicle.

This study first demonstrates that households typically place student loans below home mortgage and vehicle debt on the repayment hierarchy. Relative loan age is an important predictor of the repayment decision: Younger loans are more likely to be repaid on schedule. Increased equity in the home is correlated with lower mortgage delinquency rates, but there is no evidence that equity in a vehicle affects the probability of vehicle loan repayment.

Section 3 Data and Analysis Sample

Data for this study comes from the 2001–2010 public versions of the Survey of Consumer Finances. The SCF has existed in its current form (with some survey questions added or dropped over the years) since 1989, and is updated every three years. The 2001–2007 surveys each have approximately 4,500 unique households, while the 2010 survey has closer to 6,500 households. The surveys form a repeated cross section, but not a panel. Over time, many of the questions related to debt, income, and assets are repeated, but to different households.

The unit of analysis is the *Primary Economic Unit* (PEU), which consists of all financially interdependent members of the household. Income, debts, and assets are aggregated across all members of the PEU. This means that specific debts (such as student loans) are not tied to an individual member of the household and that individual's reported income. It is the respondent's choice whether to include college students who may not be living full-time at home—and their student loan debt—in the PEU.[3]

Each survey consists of thousands of questions related to household demographics, net worth and assets, annual income, debt levels, and the terms and status of debt repayment. There is no geographic information available in the public surveys.[4] Many financial products (e.g., bonds and commercial real estate) are held by a small number of households, so the surveys oversample high-net-worth households who might hold these products.

To account for non-response and allow for a distribution of imputed values, multiple imputation methods are used, and each household is represented by five implicates in the survey. In addition, for privacy purposes, some known variables were given random error across implicates. Sample weights are provided to make each survey representative of the U.S. population.

The outcome of interest is whether a borrower is behind schedule on a loan payment for each type of installment loan within the household. Because this is a cross section, I cannot identify loans that have already been paid off in full, or loans that were previously delinquent but are now being paid back on schedule. There are two steps to determining whether a loan is behind schedule.

First, the household is asked about the repayment status for each individual loan. The answers can be (a) ahead of schedule, (b) on schedule, (c) behind schedule, or (d) missing/not applicable. I consider a loan to be “in repayment” if the respondent has provided a non-missing repayment status. A loan can have a missing repayment status if the borrower does not have to make payments. While only 2% of mortgage loans and 5% of vehicle loans have a missing repayment

[3] From email correspondence with the Federal Reserve Board.
[4] Not even the Census region is available to researchers outside the SCF group within the Board.

status, approximately 50% of reported student loans have a missing status.[5] The repayment status question could capture in-school deferral, economic hardship deferral, or forbearance, among possibly other respondent interpretations of the question.

Second, among those in repayment, I consider an individual loan to be delinquent if the repayment status is provided as (c) behind schedule. Households may have multiple loans within a single debt type; for example, a household may have more than one student loan. A household is considered delinquent on student loans if at least one student loan is reported behind schedule.

The repayment status is defined similarly for mortgage and vehicle loans. Interest rates and loan age are defined as the balance-weighted average of all loans within that debt type. For this analysis, mortgage loans are defined as loans backed by the principal residence. Vehicle loans include any installment loan for personally owned cars or trucks, as well as "other owned vehicles" such as an RV, a motorcycle, or a boat. A household is considered delinquent on mortgage or vehicle loans if at least one mortgage or vehicle loan is behind schedule.

For some households, one survey implicate may have different values than another implicate, even for binary indicators. 2.8% of households that have at least one implicate with a student loan have at least one implicate that says it does not have a student loan. This falls to 0.5% and 0.2% for mortgage and vehicle loans.[6]

I restricted the sample to households where the respondent's age is less than 45. I did this to reduce the possibility of (a) adult children living at home, and (b) a PEU consisting of education

[5] This figure approximately matches the current proportion of student loan borrowers *in repayment* from the Federal Reserve Bank of New York's Consumer Credit Panel. It also matches ED's 2014 reported 47% of student loans in repayment retrieved May 18, 2015: https://studentaid.ed.gov/sa/about/data-center/student/portfolio.

[6] I re-estimate the model excluding all households with an inter-implicate conflict on either having an education, vehicle, or mortgage loan in repayment or it being paid on-schedule. Results are similar.

debt for the children and vehicle or home mortgage debt for the parents. For such households, it is reasonable to conclude that the decision to miss student loan payments may be the child's and not the parents'. This restriction comes at a cost: Over the past ten years, approximately 25%–35% of student loan balances have been held by individuals aged 40 or older.[7]

This study presents three distinct components that describe the repayment hierarchy of installment debt. The first follows the methodology of Cohen-Cole and Morse (2010) and Transunion (2012): Households with multiple debts which become delinquent on one debt while paying another on schedule reveal a repayment preference across debt types.

The second component isolates households with exactly one type of loan in repayment. While these households cannot reveal a repayment preference across debt types, each household must prioritize debt repayment relative to consumption. Within each debt type, I studied the debt burden of delinquent households relative to those households currently on schedule. The debt burden of student loans is defined as total monthly student loan payments divided by average monthly income. The debt burden of mortgage and vehicle loans is defined similarly.

Finally, the third component explains the prioritization gap through observed loan characteristics. I transform the data into loan type-level observations, and estimate the probability of delinquency for each loan. Observed loan characteristics explain one third of the repayment prioritization of mortgages over student loans, and one quarter of the prioritization of vehicle loans over student loans.

[7] http://www.newyorkfed.org/regional/Brown_presentation_GWU_2013Q2.pdf

Table 1 provides statistics on the prevalence of installment loan debt among households. Column 1 displays unweighted household counts for each category, and Column 2 displays the unweighted number of households with the respondent under age 45.

Rows 2–5 relate to the first component of the study: the delinquency choice of households with multiple loans in repayment. Among households with a respondent under age 45, there are 160 households that have both an education loan and a mortgage (but not a vehicle) loan, and 238 households with an education loan, home mortgage, and vehicle loan. This combines to make 398 unique households with both an education loan and a home mortgage in repayment.

Among households with a respondent under age 45, there are 166 households with an education loan and a vehicle loan (but not a mortgage) and 238 households with all three. Combined, this makes 404 unique households with both a vehicle and education loan in repayment. The 238 households with all three loan types have both loan pairs. Altogether, there are 564 unique, eligible households that have an education loan and either a mortgage or vehicle loan, or both. The age restriction reduces the sample by about one third.

The next three rows relate to the second component of this study. Among eligible households, there are 212 single-loan households with a student loan, 1,403 single-loan households with a home mortgage, and 931 single-loan households with a vehicle loan.[8]

Section 4 Stylized Facts[9]

This section provides summary evidence of the relative prioritization of debt repayment among households.

[8] These do not consider whether a household has an "other" installment loan such as a home improvement loan or commercial real estate loan.
[9] Summary statistics are calculated by using all implicates and dividing the sample weights by five.

Table 2 further describes the sample making up the loan-pairs component of the study. It begins with the sample of 564 households that have an education loan, and either a mortgage or vehicle loan, or both, in repayment. Of these, there are 398 (404) households that have a student loan and a mortgage (vehicle) loan in repayment, and just 42 (51) of these households are delinquent in exactly one loan of the pair. Some households have both loan pairs. Overall, among households with student loans, there are 70 unique households that reveal a repayment preference within at least one loan pair.

Table 3 displays the repayment choices facing all households with multiple loans in repayment. For each loan pair, the household may be on schedule with both loans, behind schedule with both loans, or on schedule with one but not the other (and vice versa). That makes four mutually exclusive categories. Concentrating on those with student loans, the table displays the proportion of households in each of the four repayment categories separately: households with student loans and mortgages, and households with student loans and vehicle loans. As noted, some households fall into both loan pair groups.

The top row of Table 3 displays the repayment status of households with both a student loan and a mortgage in repayment. Of these households, 89% are repaying both loans on time, and 1% of are behind schedule on both loans. This leaves 10% of households with a student loan and a mortgage in one of the “choice” categories. Either the household is behind on the student loan and on-schedule with the mortgage, or vice versa. Here, there is a greater than 4:1 ratio of households that chose to be delinquent on the student loan relative to those choosing to be delinquent on the home mortgage. Households overwhelmingly pay mortgage debt over student loans, even though mortgage payments are much greater (see Table 7).

The second row displays the repayment status of households with both a student loan and a vehicle loan. Here, 12% of households with both of these loan types are choice households, and there is a 3:1 ratio of households that chose to be delinquent on student loans relative to those choosing to be delinquent on vehicle loans.[10]

I divide column 3 by the sum of columns 3 and 4, and obtain a statistic describing the frequency of student loan delinquency when households are delinquent on exactly one loan in the pair. For example, when consumers repay exactly one debt on time among the mortgage-education loan pair, they choose to repay home mortgage loans 81% of the time. Table 4 displays this statistic for each loan pair over time.

Within each survey year, there are a very small number of loan pairs with exactly one loan in delinquency. While I do not make any claims over time trends, this table demonstrates that the repayment hierarchy is not driven by a single survey year. Across surveys, the smallest percent of households choosing student loan delinquency over mortgage delinquency was 75%, while the smallest percent of households choosing student loan delinquency over vehicle delinquency was 67%.

Approximately 45% of households that are delinquent on one installment loan have no other installment loans in repayment.[11] While these single-loan households cannot prioritize debt repayment across installment loans, each still allocates resources between consumption and debt repayment, and thus must prioritize debt repayment relative to consumption. To my knowledge,

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[10] I replicate this without the under-45 age restriction and display results in appendix Table A1. The repayment ratios of mortgage to education loans and vehicle to education loans are each approximately 4:1.
[11] Author's calculation among the analysis sample.

this paper is the first to study the relative prioritization of installment debts among single-loan households.[12]

To investigate, I compare the monthly debt burden of households behind schedule on payments to those on schedule for each loan type. For student loans, home mortgages, and vehicle loans separately, I define the debt burden of each loan type as $\frac{monthly\ loan\ payment}{(annual\ income/12)}$.

Debt burden is difficult to compare across debt types. Households with only student loan debt are likely much different than households with only mortgage debt. Further, apartment rental costs are difficult to compare to home mortgage debt, as are public transportation costs relative to vehicle loans.

Within each debt type, I calculate the debt burden for all single-loan households and identify the fraction of borrowers that are paying behind schedule at each level of debt burden. Figure 1 displays the relationship between debt burden and delinquency rates within single-loan households for each of the three loan types studied.

Delinquency rates among single-loan households with education loans are meaningfully higher than rates among single-loan households with vehicle or mortgage debt. In addition, there is a fairly high rate of delinquent households with very low levels of education debt relative to income. Recall that these households by construction have no other installment loans that they need to pay off. Yet, approximately 15% of single-loan households with education debt burdens less than 5% of total income are delinquent on their loans.

[12] A conversation with the Fair Isaac Corporation suggested that this is a measure of interest within the credit scoring industry.

The fraction of delinquent households rises rapidly and immediately with debt burden among households with education loans, especially compared to those with mortgage debt. Among single-loan households with mortgage debt, the delinquency rate rises very slowly with debt burden until debt burden reaches 55%, after which it rapidly increases. In other words, mortgage debt holders typically have to reach a threshold where mortgage debt becomes overwhelming relative to income before they begin missing payments. Education debt holders simply miss more payments as debt burden increases at all levels of debt burden.

Vehicle debt shows a mixed story among these single-loan households. Like mortgage debt holders, households with very low debt burdens due to vehicle loans have very low delinquency rates, as expected. Yet, similar to education debt holders, the delinquency rates rise rapidly and immediately as debt burden rises. In summary, single-loan households with education debt are much more likely to miss student loan payments at all levels of debt burden, including very low ones. Delinquency rates on education and vehicle loans rise rapidly with debt burden, while delinquency rates on mortgages rise rapidly only after reaching a relatively high threshold.

Moving forward, I combine the single-loan households with the multiple-loan households and use observed loan characteristics to explain the repayment prioritization of student loans.

Section 5 Model the Repayment Decision within Households

I transformed the data so that observations are at the household-x-loan type level.[13] Equation (1) estimates a linear probability model of loan delinquency. A household with a mortgage loan, a student loan, and a vehicle loan would have a separate observation for each loan type.

$$(1)\ BehindSchedule_{hl} = \alpha + \beta LoanType_{l} + \gamma LoanChars_{hi} + \theta_{h} + \varepsilon_{hl}.$$

[13] Actually, it is at the household-implicate-loan type level, as all five implicates are included.

The variable $BehindSchedule_{hl}$ is an indicator for whether household h reports that at least one loan of type l is being paid behind schedule, and $LoanType_l$ is a vector consisting of student loan and vehicle loan dummy variables. Mortgage loans make up the excluded category.

The vector $LoanChars_{hl}$ consists of loan characteristics that vary across households. It includes the dollar-weighted interest rate and loan age, and aggregate loan payments within each loan type for the household. The vector also includes the equity in the asset and an indicator for having zero or negative equity. Equity is defined as the fraction of the asset not covered by the loan, and takes on a negative value when the loan amount exceeds the value of the asset.[14]

These latter two variables are assigned a value of zero for student loans, and estimated with separate coefficients for vehicle and mortgage loans. Because of the recourse property of loans, the equity variables are estimated separately by loan type. Vehicle loans often have recourse, which means that lenders can not only confiscate the collateral upon delinquency, but can sue the consumer for the gap between the value of the asset and the loan. Mortgages typically are non-recourse loans, and so "walking away" from a mortgage on a property with zero equity can be a strategic decision. θ_h is a vector of household fixed effects, and ε_{hl} is an error term unrelated to loan type. Standard errors are clustered at the household level and account for multiple imputations. I choose not to use survey weights, so that each household is counted equally, and is not weighted by wealth.[15]

[14] Specifically, Equity = $1-\left(\frac{loan\ amount}{asset\ value}\right)$.

[15] There are also technical reasons for not using weights. The program provided by the Federal Reserve to account for multiple imputations for standard error calculations has trouble dealing with regressions that use both survey weights and a large number of fixed effects to be absorbed. To test the implications, I re-estimate the model using all five implicates unweighted, the first implicate unweighted, and the first implicate weighted. The results, displayed in appendix Table A3, are nearly identical using each method, suggesting that weights do not affect the results.

Table 6 displays the mean value of the loan characteristics entering the model. On average, education loans are 4.1 years old, compared to 3.1 years for mortgages and 1.7 years for vehicle loans. The interest rate on student loans is typically less than those of vehicle and mortgage loans, and the payment is meaningfully smaller. The average monthly payment on mortgages is more than $1,600 compared to $460 for vehicles and $240 for student loans.

Households with mortgage loans have, on average, 34% equity in their home. This means that the loan size averages 66% of the home value. Just 7% of home mortgage loans are greater than the value of the home (zero equity). Those with vehicle loans have, on average, 44% equity, and 10% of vehicle loans are greater than the value of the asset. Approximately 50% of households with a student loan have a mortgage loan, and 50% have a vehicle loan. However, just 12% of households with a mortgage have a student loan, and 14% of households with a vehicle loan also have an education loan.

The model is limited by the data available. Ideally, it would include factors related to the consequences of non-repayment, information on loans fully paid off, a history of repayment status, an accurate linking of debts and income to individuals within the household, and more precise information on the length and severity of delinquency.

Section 6 Results

Table 7 displays regression estimates building up to equation (1). Column 1 displays the regression with only household- and loan-type fixed effects. The delinquency rate on student loans is predicted to be 5.4 percentage points higher than that of mortgages, and 7.2 percentage points higher than vehicle loans. Columns 2 through 5 display the results from separately adding linear terms related to equity, payment amount, interest rate, and loan age to the regression. The equity and loan age variables are the only ones that reduced the estimated coefficient on student loans by themselves.

Column 6 displays the results when the entire vector of linear loan characteristics is included. Controlling for these observed loan characteristics, the delinquency rate on student loans is predicted to be 3.6 percentage points higher than mortgages, and 5.4 percentage points higher than vehicle loans.

As expected, equity in the asset is negatively related to delinquency. Loan age is also predictive of delinquency: Older loans are more likely to be behind schedule on payments. The indicator for zero equity is surprising: It is small and not statistically significant for home mortgages, and it is negative and significant at the 10% level for vehicle loans.

Column 7 includes squared terms for each of the continuous variables: equity of each loan type, loan age, payment amount, and the interest rate.[16] Including these terms further reduces the estimated coefficient on the student loan indicator to 0.036, while the indicator on vehicle loans is 0.018 and is not statistically significant from zero. In sum, observed loan characteristics reduced the "student loan effect" on delinquency probability by 33% relative to mortgages, and by 25% relative to vehicle loans. Linear terms account for nearly all of the model's explanation of the student loan effect on repayment prioritization.

I replicate the analysis including households with a respondent age over 45 years old and display results in Appendix Table A2. The results are similar. In addition, I re-estimate the model using a single implicate (instead of all five) and using survey weights. The coefficients, displayed in Appendix Table A3 are similar in both magnitude and statistical significance.

[16] Additional higher order terms dramatically increased standard errors of estimates. The coefficients on the quadratic terms are not displayed in the table in order to save space.

Section 7 Conclusions

This paper provides evidence that households place a low priority on student loan repayment relative to other debts. When households are able to pay some—but not all—of their debts, they overwhelmingly choose to miss their student loan payments. This suggests that changes to the student loan system that encourage repayment could reduce student loan delinquencies, while possibly increasing delinquencies in other debt markets.

The small sample counts and cross-sectional nature of the data make it difficult to determine empirically why households are making these decisions, although theory can guide us. Prior research suggests that equity, liquidity, credit scores, and consequences of non-repayment matter for unilateral debt repayment. It seems reasonable to conclude that such factors would enter a consumer's decision to prioritize one debt over another.

While there is evidence that households are prioritizing student loans below other debts, the evidence isn't perfectly clear on which factors drive this decision. However, some observations can be made. First, home equity matters both for predicting delinquency and for explaining the student loan effect. Households with more ownership of the asset are less likely to fall behind in their payments and risk repossession by the lender.

Loan age also matters for predicting delinquency and explaining the student loan effect. Households are more likely to miss payments on older loans, all else equal. This could reflect psychological factors such as individuals being more "attached" to newer assets, and therefore prioritizing the debt behind those assets.

There are some unexpected observations. The sign on interest rates is near zero and statistically insignificant. To minimize interest paid, consumers should be paying off loans with higher interest rates first, although this may be mitigated by the loan amount. The coefficient on payment amount is statistically significant, but very small in magnitude: A $1,000 increase in payment amount increases the expected probability of delinquency by 1 percentage point. While household fixed effects control for some ability to pay measures such as income and wealth, I expected that payment size would be more positively correlated with delinquency rates because the payment of larger loan amounts may be very difficult for income-constrained households.

One might expect a number of other factors to influence the decision-making of borrowers, yet these are difficult to capture in the currently available data. Households may face varying consequences of non-repayment, depending on their individual loan terms. Households may also have internal probabilities on their own likelihood of bankruptcy, or future income streams. They may also have varying degrees of knowledge about the consequences of default.

With respect to student loan repayment, an ideal data set would combine the college experience, debt, and demographic variables available in the popular National Center for Education Statistics surveys with a fuller set of income, debt, and repayment patterns over time. NCES data sets do not allow us to observe whether students are delinquent on either their student loans or any other debts. The Survey of Consumer Finances and the Equifax/Consumer Credit Panel under management of the Federal Reserve Bank of New York contain rich information on household debt and repayment, but scant information on the educational experiences and demographics of student loan borrowers.

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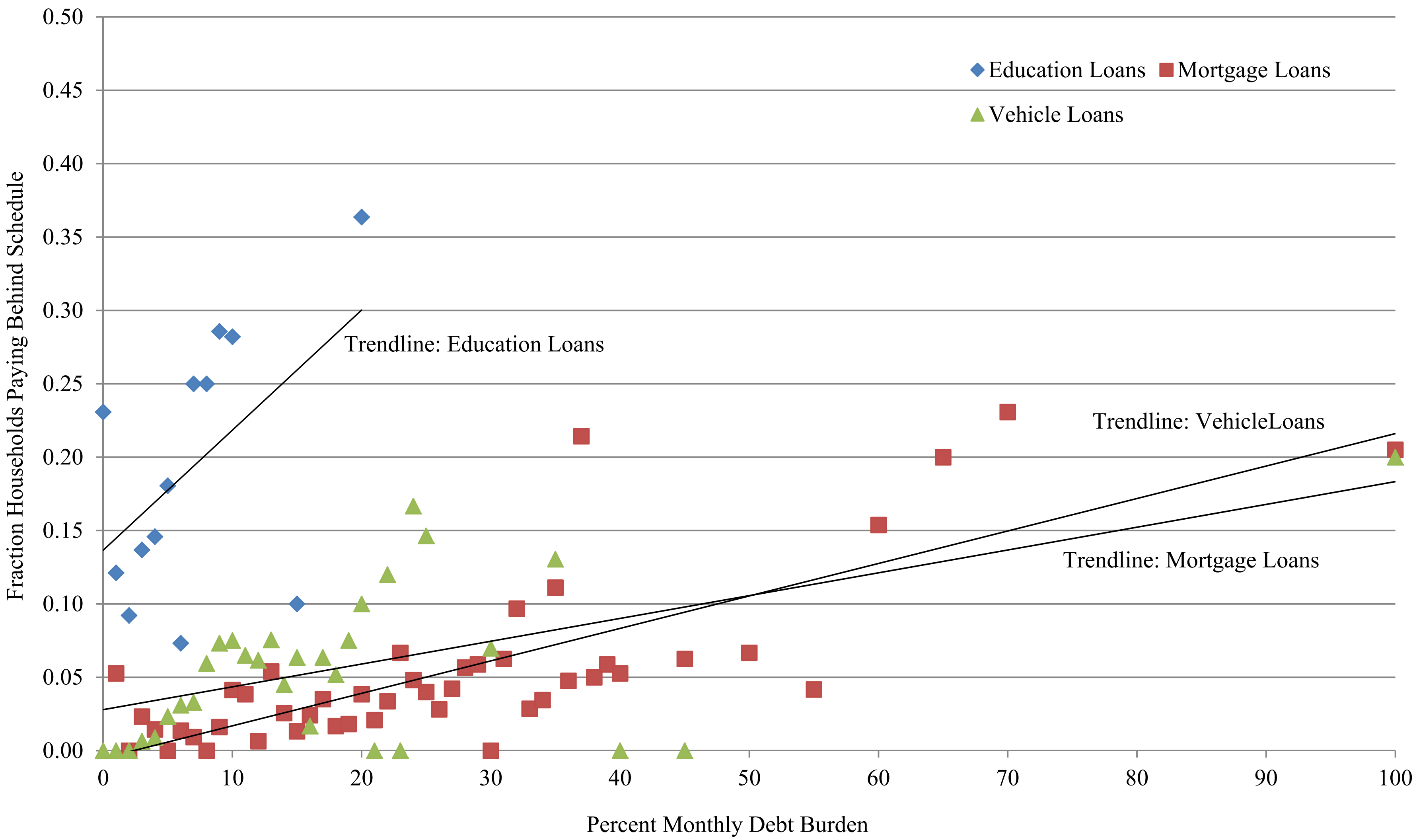

Figure III.1. Fraction of Single-Loan Households Paying Behind Schedule by Debt Burden. Data from the 2001-2010 Surveys of Consumer Finance. Sample includes households with exactly one installment loan in repayment and with respondent age under 45 years old. Only the first survey implicate is used. Within each loan type, debt burden is defined as the monthly debt payment divided by monthly income. Debt burden bins start at one percentage points wide and increase to five points as counts diminish. Reported are the fraction of households paying behind schedule within each bin. Minimum 10 households per bin.

Table III.1. Prevalence of Installment Loans among Households.

	Unweighted Count of Households	Restricted to Respondents Under Age 45
Number of Households	19,653	7,658
Households with a Student Loan and Home Mortgage Only in Repayment	270	160
Households with a Student Loan and Vehicle Loan Only in Repayment	217	166
Households with a Home Mortgage and Vehicle Loan Only in Repayment	2,913	1,358
Households with a Student Loan, Home Mortgage, and Vehicle Loan in Repayment	370	238
Single-Loan Households with a Student Loan in Repayment	270	212
Single-Loan Households with a Home Mortgage in Repayment	3,909	1,403
Single-Loan Households with a Vehicle Loan in Repayment	1,741	931

Notes: Data comes from the 2001–2010 Surveys of Consumer Finances. Household counts are unweighted. "In repayment" is defined as having a non-missing response to the survey question asking if the household is ahead of schedule, on schedule, or behind schedule on the loan. Loans other than home mortgage, vehicle, and student loans are disregarded. The first implicate was used for these calculations. The "age 45" restriction considers the age of the survey respondent only.

Table III.2. Households which Reveal a Repayment Preference across Two Installment Loans.

	Unweighted Number of Households
Has a Student Loan and either Mortgage or Vehicle Loan in Repayment	564
Has a Student Loan and Mortgage in Repayment	398
Has a Student Loan and Vehicle Loan in Repayment	404
Has a Student Loan and Mortgage in Repayment and Exactly One is Behind Schedule	42
Has a Student Loan and Vehicle Loan in Repayment and Exactly One is Behind Schedule	51
Has either of above Two Categories	70

Notes: Data comes from the 2001–2010 Surveys of Consumer Finances. Household counts are unweighted. Limited to households with a survey respondent no older than age 45 and with at least two types of loans in repayment. "In repayment" is defined as having a non-missing response to the survey question asking if the household is ahead of schedule, on schedule, or behind schedule on the loan. "Behind Schedule" is defined as paying at least one loan within a loan type behind schedule for that same survey question. Loans other than home mortgage, vehicle, and student loans are disregarded. The first implicate was used for these calculations.

Table III.3. Delinquency Rates Conditional on Having a Student Loan and Another Loan in Repayment.

			Households Facing Delinquency Choice		
Other Loan in Repayment	Unweighted Count of Households	On Schedule Both Loans	Behind Schedule on at least one Student Loan but not Other Loan	Behind Schedule in at least one Other Loan but not Student Loans	Behind Schedule Both Loans
Mortgage	398	88.6%	8.4%	2.0%	1.0%
Vehicle Loan	404	85.4%	9.3%	3.1%	2.2%

Notes: Data comes from the 2001–2010 Surveys of Consumer Finances. Household counts are unweighted. Limited to households with a survey respondent no older than age 45 and with at least two types of loans in repayment. "In repayment" is defined as having a non-missing response to the survey question asking if the household is ahead of schedule, on schedule, or behind schedule on the loan. "Behind Schedule" is defined as paying at least one loan within a loan type behind schedule for that same survey question. Loans other than home mortgage, vehicle, and student loans are disregarded. The first implicate was used in these calculations. Means are unweighted.

Table III.4. Repayment Prioritization among Loan Pairs over Time.

	2001	2004	2007	2010	Total
Repay Mortgage but not Student Loan	89%	78%	86%	75%	81%
Repay Vehicle but not Student Loan	85%	67%	88%	67%	75%

Notes: Data comes from the 2001–2010 Surveys of Consumer Finances. Limited to households with a survey respondent no older than age 45 and with at least two types of loans in repayment. The sample consists of all households delinquent in exactly one debt among the loan pair. The reported value represents those sample households behind schedule on the student loan. The first implicate was used in these calculations, and annual means were calculated without survey weights. The "Total" column is a weighted average (by households) across the survey years.

Table III.5. Delinquency Rates of Single-Loan-Type Households.

	Number of Households	Delinquency Rate on Single Installment Loan
Single-Loan Households with a Student Loan in Repayment	212	28.8%
Single-Loan Households with a Home Mortgage Loan in Repayment	1,403	3.1%
Single-Loan Households with a Vehicle Loan in Repayment	931	7.1%

Notes: Data comes from the 2001–2010 Surveys of Consumer Finances. Limited to households with respondent age no older than 45. The sample consists of all age-eligible households with just one loan type among student loan, mortgage, and vehicle loans in repayment. "In repayment" is defined as having a non-missing response to the survey question asking if the household is ahead of schedule, on schedule, or behind schedule on the loan. Delinquency is defined as being behind schedule on at least one loan within each loan type. Household counts are unweighted. All imputations were used in these calculations. Percentages are unweighted.

Table III.6. Summary Statistics of Loan-Level Model Covariates.

	Education	Mortgage	Vehicle
Unweighted Number of Households with Loan of Each Type	794	3,441	2,799
Mean Loan Age (Years)	4.1	3.1	1.7
Mean Loan Interest Rate (Percentage Points)	5.7	6.3	8.2
Mean Payment Amount ($100s)	2.4	16.3	4.6
Mean Equity in Home	0.00	0.34	0.00
Fraction Households with Zero Equity in Home	0.00	0.07	0.00
Mean Equity in Vehicle	0.00	0.00	0.44
Fraction Households with Zero Equity in Vehicle	0.00	0.00	0.10
Has Two Loan Types	0.41	0.44	0.54
Has Three Loan Types	0.30	0.07	0.09
Has an Education Loan	1.00	0.12	0.14
Has a Mortgage Loan	0.50	1.00	0.57
Has a Vehicle Loan	0.51	0.46	1.00

Notes: Data comes from the 2001–2010 Surveys of Consumer Finances. Household counts are unweighted. Limited to households with a survey respondent no older than age 45 and at least one student loan, vehicle loan, or mortgage in repayment. "In repayment" is defined as having a non-missing response to the survey question asking if the household is ahead of schedule, on schedule, or behind schedule on the loan. Loans other than home mortgage, vehicle, and student loans are disregarded. All survey implicates are used in these calculations.

Table III.7. Loan-Level Model Probability of Delinquency.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Education Loan Indicator	0.054***	0.050***	0.063***	0.054***	0.043***	0.037**	0.036**
	(0.010)	(0.014)	(0.011)	(0.011)	(0.011)	(0.015)	(0.016)
Vehicle Loan Indicator	-0.018***	-0.012	-0.011	-0.017***	-0.009	-0.001	-0.018
	(0.006)	(0.017)	(0.007)	(0.006)	(0.007)	(0.018)	(0.021)
Fraction Equity in Home		-0.019				-0.049*	-0.176***
		(0.027)				(0.028)	(0.046)
Fraction Equity in Vehicle		-0.012				-0.015	0.023
		(0.025)				(0.025)	(0.063)
Has Zero Equity in Home		0.017				0.004	-0.046
		(0.026)				(0.026)	(0.030)
Has Zero Equity in Vehicle		-0.069*				-0.070*	-0.049
		(0.036)				(0.036)	(0.047)
Payment Amount ($100s)			0.001*			0.001**	0.002***
			0.000			0.000	(0.001)
Interest Rate (Percentage Points)				0.000		-0.001	0.003
				(0.002)		(0.002)	(0.005)
Loan Age (Years)					0.007***	0.008***	-0.001
					(0.002)	(0.002)	(0.004)
Constant	0.055***	0.061***	0.044***	0.057***	0.036***	0.037**	0.035
	(0.004)	(0.010)	(0.007)	(0.011)	(0.006)	(0.016)	(0.024)
Household Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Squared Terms of Continuous Variables	No	No	No	No	No	No	Yes
Unique Households	4,867	4,867	4,867	4,867	4,867	4,867	4,867
Estimated Adjusted R-squared	0.81	0.82	0.81	0.81	0.82	0.82	0.82

Notes : Data comes from the 2001–2010 Surveys of Consumer Finances. Observations are at the household-imputation-loan level. Mortgage Loan indicator is the excluded category. All imputations are used in these regressions. Standard errors are clustered at the household level and account for multiple imputation. Estimated R-Squared comes from a least squares regression using a single implicate. Survey weights are not used.

APPENDICES

APPENDIX I.A

Background Information on Student Loans

Section 1 Description of Federal Loan Types

There are three types of federal loans students may obtain while enrolled. Subsidized Stafford loans are means tested and interest is paid by the government while the student is enrolled. Unsubsidized Stafford loans are not means tested. Payments are not required while the borrower is enrolled, although interest accumulates once the loan is disbursed. Stafford loans have annual loan limits, often well below the cost of attendance. PLUS loans can be made to parents and graduate students to cover any remaining costs not covered by grants or Stafford loans. These are unsubsidized. Each of these loans have the same published rates, limits, and eligibility, whether in the Direct or Guarantee program.

Section 2 School Switching and Selection into Federal Student Loan Programs

Changes to federal accounting rules in the early 1990s—along with estimates that a direct lending program may be cheaper to taxpayers—led to the roll-out of the Direct Loan program in 1994. Early surveys by the Office of Postsecondary Education report relatively high satisfaction in the Direct Program, as financial aid officers only had to work with a single lender (OPE, 1996). Market share in the Direct Program increased rapidly and peaked at 33% of new loan volume in 1997.

However, a new Congress opposed to the Direct Loan program passed regulations, effective in 1997, prohibiting the Department of Education from marketing the Direct Loan program. Over the next ten years, the Guarantee program steadily regained market share. Investigations by *U.S. News & World Report* (2003), whistle blowing by small private lenders (2006), and settlements and reports by the Attorney General's Office of New York (2007) disclosed a pattern of kickbacks and inducements by private lenders to financial aid offices to return to the Guarantee program and obtain placement at the top of preferred lender lists. By 2007, just 19% of newly originated loans were disbursed through the Direct Loan program.

These practices came to light just prior to turmoil in the financial markets beginning in 2008. During this financial crisis, many lenders voluntarily exited the federal student loan market as yields plummeted and the cost of capital rose. New federal regulations passed in 2009 eliminated the Guarantee program and as of July 1, 2010, all newly disbursed federal loans must be originated through the Direct Loan program.

Section 3 Cohort Default Rates Defined

The repayment cohorts are defined by the end of the fiscal year which runs from October 1 through September 30. A student graduating in May 2008 who uses the six-month grace period will enter repayment in November 2008. Students entering repayment between Oct 1, 2008 and Sep 30, 2009 make up the denominator of the 2009 cohort default rate. Students who default by the end of the next fiscal year enter the numerator of the cohort default rate. The numerator of this ratio includes all students in the cohort who default on *any of their federal loans* by September 30, 2010, even if the defaulted loan was disbursed through another institution.

The Information for Financial Aid Professionals (IFAP) website within the Department of Education produces a handbook that describes cohort default rates, appeals, and exceptions in great detail. The latest version can be found at: http://www.ifap.ed.gov/cdrguidelenderga/3YearFY11CohortDefaultRateGuideGALender.html

APPENDIX I.B

Modeling Lender Incentives for Interventions

When a borrower misses a payment and becomes one step closer to default, lenders must decide whether it is worth expending resources in an intervention attempt to move the borrower back to on-time repayment. Moreover, because of the principal-agent framework, the lender will create a general intervention policy that agents (collection officers) can follow.

Delinquent borrowers can be classified into three mutually exclusive categories: those who will recover regardless of whether the lender provides an intervention, those who will default regardless of an intervention, and the marginal borrowers (or compliers) who recover if and only if there is an intervention performed. For analytical ease, suppose the fraction of each borrower type is fixed across the Direct and Guarantee program, and each type has a probability of occurring in the population denoted by $\alpha_1, \alpha_2,$ and α_3 respectively.

We can imagine this holds for the entire population of delinquent borrowers, or within buckets of observed characteristics (e.g., borrowers who attended a one-year school and missed a payment in the first year after exiting college). Lenders should choose to provide an intervention if and only if the expected value of intervention exceeds the cost of the intervention (beyond the minimum effort required to satisfy the default guarantee claim).

I define:

$V_R \equiv$ Net present value of a loan in repayment.

$V_D \equiv$ Net present value of a loan in default.

$C \equiv$ Cost of an intervention required to bring borrower to recovery.

Because type 1 borrowers always recover into repayment, type 2 borrowers always default, and type 3 borrowers recover only in an intervention, we can denote:

$\mathrm{E[V|Intervention]} = \alpha_1 \mathrm{V_R} + \alpha_2 \mathrm{V_D} + \alpha_3 \mathrm{V_R} - C$ as the expected value of an intervention, and

$\mathrm{E[V|Min\ Effort]} = \alpha_1 \mathrm{V_R} + \alpha_2 \mathrm{V_D} + \alpha_3 \mathrm{V_D}$ as the expected value of performing the minimum effort.

Therefore, the lender performs an intervention if $\alpha_3(V_R - V_D) \geq C$.

Then there exists a threshold C^*, which makes the lender indifferent between exerting the minimum effort and performing the intervention.

I assume that α_3 is fixed across the Direct and Guarantee programs. Moreover, I claim $\alpha_3 > 0$ because Sallie Mae is willing to exert additional intervention resources to delinquent private loan borrowers, as discussed earlier. Then $\frac{\partial C^*}{\partial V_R} > 0$ and $\frac{\partial C^*}{\partial V_D} < 0$.

Because V_R is simply the net present value of a loan in repayment, this value is also constant across the Direct and Guarantee loan programs. But, in the Guarantee program, $V_D \geq 0.97 * Balance$ due to the default guarantee, the additional income the lender may collect through late fees, and the chance of being hired as a collection agent. In the Direct Loan program, the expected value of a loan in default is $V_D \leq 0.87 * Balance$, according to the Department of Education's budget justifications. Because $V_D^{DL} < V_D^{Guar}$ and $\frac{\partial C^*}{\partial V_D} < 0$, this leads to the prediction $C_{DL}^* > C_{Guar}^*$.

In other words, there is an increased willingness to provide an intervention in the Direct Loan program relative to the Guarantee program. If I assume that there are different intervention rules for different buckets of borrowers based on observed characteristics, then Direct Loan lenders will be willing to intervene for more of these buckets of borrowers.

APPENDIX I.C

Technical Details of the School-x-Year Model

Estimated impacts on default rates due to Direct Loan participation are driven by those schools switching programs throughout the sample period. How representative are these schools? First, the analysis sample covers 96% of borrowers entering repayment between 2003 and 2011. Table C1 displays statistics of schools across programs in the two distinct periods: 2000–2008, and 2008–2011. Table 1 in the main paper displayed the characteristics of schools across program participation for the entire sample.

Switching programs was not very prevalent during much of the sample period. Between 2000 and 2008, just 7% of schools participated in both programs.[1] Among schools participating in both programs, these schools spent on average 4.4 years in the Guarantee program, 3.7 years in the Direct program, and just under 1 year participating in both programs within the same year (typically as a transition between programs). During this period from 2000 to 2008, 75% of schools were in the Guarantee program every year, and 18% of schools were in the Direct program every year.

In order to have participated in both programs during the 2000–2008 period, the school would have had to first switch into Direct Loans in the late 1990s and then switch back to the Guarantee program. Thus, it is not surprising that those schools resemble a blend of the Direct-only and the Guarantee-only schools. For example, one-year for-profit schools make up 28% of the Direct-

[1] This includes the 24 schools that were “mixed only” throughout the sample.

only schools, 10% of the Guarantee-only schools, and 17% of Switcher schools. Four-year private schools make up 16% of Direct-only schools, 33% of Guarantee-only schools, and 25% of Switcher schools.

The pattern of loan program switching began to shift toward the Direct Loan program in 2008. This was coincident with both negative publicity and regulations surrounding the kickbacks in the Guarantee program, as well as the voluntary exiting of many student lenders from the market. Among schools which participated in both programs between 2008 and 2011, 2.4 years were spent in the Guarantee program, 1.2 years in the Direct Loan program, and 0.4 years participating in both. (Many schools were mixed in 2009–10 only.) Twenty-six percent of the schools were in the Direct program during the entire "involuntary switching" period. Schools participating in both programs during the involuntary switching period overwhelmingly come from schools that were Guarantee-only during the voluntary switching period 2000–2008.

Time-varying student characteristics used as controls include

- racial composition,
- the fraction of independent students,
- the 75th percentile SAT math,
- SAT reading,
- ACT composite scores,
- the proportion taking the SAT and ACT,
- the fraction graduating within 150% of program duration,
- logged cost of attendance,
- graduate and undergraduate enrollment,

- the proportion of freshmen taking out loans,
- the average student loan amount,
- the proportions of independent and dependent students classified as "low income,"
- the proportion of total federal loans disbursed which are parent PLUS, grad PLUS, or subsidized Stafford, and
- the fraction of students obtaining Pell grants.

While the cohort default rates exclude PLUS loans, the existence of these loans may help describe the student population as well as the level of parental support. Subsidized Stafford loans provide another measure of low-income student loan borrowers. These loans also influence the financial incentives of lenders, as interest on subsidized loans is paid by the government when a struggling borrower is in forbearance.

TICAS data covers the academic years ending 2001, and 2004–2012. Data for 2002 and 2003 are estimated using linear interpolation methods for those schools with data from both 2001 and later years only. In order to match the years available in the Title IV reports, this study uses the 2001 values in the TICAS panel as a best estimate for the 2000 values.[2, 3]

Section 1 Event Study Framework

Estimating the impact of Direct Loan participation on default rates in an event study framework allows for the observation of non-linear pre-trends. This methodology is complicated by the fact that program switching goes in both directions over the sample period. I can get around this,

[2] Approximately 20% of schools had no data available in 2001 and so linear interpolation wasn't used. As a test of sensitivity, the model was estimated both with missing data for these schools in 2000–2003 as well as using 2004 values for each of these years. Results are similar in magnitude and significance.

[3] Imputation flags are set to unity for those variables with initially missing data. Missing value indicators are set to unity (and variable values set to zero) for any variables which remain missing after imputations.

however, by splitting the data into the two distinct periods and dropping the small number of schools that switched from Guarantee to Direct in the earlier period. This sharply reduces power, which is why results are reported in the appendices, and why the distributed lag model is the preferred estimation strategy.

I estimated equation (7) below using both an unbalanced and balanced panel. First, the sample is limited to the high-baseline default, low-enrollment one-year and two-year for-profit schools. The results from the aggregate model suggest the impacts of Direct Loan participation are concentrated among these schools. To increase what limited power is available, I combined these two sectors.

Next, I estimated an unbalanced panel using all available years for non-switcher schools, but for switcher schools I used only the years prior to the final switch to the Direct Loan program. I also dropped all schools that switched multiple times prior to the final involuntary switch to the Direct Loan program, leaving the sample with twenty-one switcher schools. This allowed me to observe the estimated impact of switching voluntarily from the Direct to the Guarantee program.

$$(7)\ y_{st} = \alpha + \sum_{i=-4}^{i=3} switch_{s,t+i} + \sum_{l=0}^{n} \delta_l X_{s,t-l} + \theta_{ct} + \mu_s + \varepsilon_{st}.$$

All variables are defined as in Equation (1), but $switch_{s,t+i}$ represents binary indicators for whether the school switched from Direct to Guarantee in period $t+i$. The excluded category is represented by $switch_{s,t-4}$. Counts become scarce among switcher schools beyond (i=-4) and (i=+3).[4] Thus, for this unbalanced panel only, I combined (i=-8) through (i=-4) into the (i=-4) category, and combined (i=+3) through (i=+8) into the (i=+3) category. Even so, the confidence intervals sharply increase around (i=+3).

[4] Each of these categories would have the indicator turned on for fewer than 10 observations.

Figure C1 shows a sharp downward trend in default rates among switcher schools leading into the switch toward the Guarantee program in the voluntary switching period, followed by a sharp increase in default rates following the switch. This suggests that results from the distributed lag model may understate the impact of the Direct Loan program.

I re-estimated equation (7) using a balanced panel. I included schools that switched between 2003 and 2007, which allows for these schools to have at least three years of pre/post switch data. To be included in the balanced sample, all schools must have observations from (t-3) through (t+3). Unfortunately, among the high-default, low-enrollment schools, there are just three switcher schools meeting these criteria. As a result, I instead used the sample of high-default schools with either high or low average enrollment over the period, increasing the number of switcher schools to twelve in this balanced panel.

Figure C2 displays the results. The reversal in trends is not nearly as sharp as in the unbalanced panel, but there still seems to be a negative trend in default rates leading up to the voluntary switch into the Guarantee program. Again, this suggests that the main results are somewhat understated.

In order to obtain the impact of "involuntary switching" I then estimated the same model on the second period, defining switcher schools as those who switched into the Direct Loan program between 2009 and 2011. I estimated the model using both a balanced and unbalanced panel among the high-default, low-enrollment one-year and two-year for-profit schools.

The unbalanced panel includes 136 switcher schools, and includes observations from (t-5) through (t+1). I excluded (t+2) because that would be estimated from just one cohort (2009 switchers). Counts are large enough that I did not need to combine periods, as in the voluntary switching period.

Figure C3 displays the results of the event study estimating the impact of switching into the Direct Loan program. There is a small downward trend in default rates, suggesting that the estimated results may be somewhat overstated. But there is still a large gap between the trend and estimated (t+0) coefficient. The (t+1) coefficient is only estimated from the 2009 and 2010 switchers (missing more than half of the switcher schools) and thus the confidence interval increases sharply at this point.

The balanced panel uses all available years for non-switchers, but restricts switcher schools to those that have at least three pre-periods and the initial switch period. (Note that the bulk of switchers will not have a *t+1* period, because 2011 is the last available repayment cohort.) This reduced the number of switcher schools to 71, and there are just 12 observations where the (t+1) indicator is turned on.

Figure C4 displays the results. The results are similar to the unbalanced panel. Again, there seems to be a small downward trend, but the estimate at (t+0) does not seem to be explained by the trend in coefficients starting from the excluded category. The confidence interval is large at all points and doubles in size at (t+1), so I do not display that coefficient.

To summarize, the event study method is hampered by the fact that program switching goes in both directions. I split the sample and focused on each switching period separately. In the earlier period, there seems to be a slight downward trend in default rates, suggesting that the aggregate results may understate the impact of the Direct Loan program. In the later period, the downward trend suggests that results may be overstating the impact of the Direct Loan program. In none of the cases does a pre-trend in estimated coefficients seem to explain the bulk of the post-period estimated impact of Direct Loan participation.

Section 2 Does Direct Loan Participation Predict Changes in Observed Student Characteristics?

I re-estimated equation (5), replacing default rates on the left-hand side with eight observed characteristics that have relatively few missing observations. These include the fraction of students who are black, Hispanic, or female. It also includes graduation rates (within 150% of expected time-to-completion), the fraction of undergrads enrolled fulltime, the average freshman loan size, the fraction of freshmen with any student loans, and the fraction of annual loans disbursed as subsidized Stafford loans. The analysis sample is made up of high-baseline-default, low-enrollment one-year and two-year for-profit schools, as described in the paper. The only change from equation (5) is that I dropped contemporaneous time-varying student characteristics. For example, I included a one-year lag of fraction black but exclude (t-0) fraction black. For sensitivity, I also removed all time-varying student characteristics, and the results are similar.

I utilized a multivariate regression framework and simultaneously estimated the model with each dependent variable. Table C2 displays $\sum_{l=0}^{n} \beta_l$, the sum of estimated coefficients on the lagged program participation indicators. At the five percent level, Direct Loan participation does not predict significant differences in any of the dependent variables. At the ten percent level, Direct Loan participation predicts a lower graduation rate and a reduced fraction of subsidized loans. The relationship between Direct Loan participation and reduced graduation rates should meaningfully understate the impact of participation on default rates, as attainment is consistently found to be an important predictor of default.

APPENDIX I.D

Technical Details of the Student-Level Model

Section 1 Description of Analysis Sample in Student-Level Model

Table D1 displays summary statistics comparing the full sample to the analysis sample for each time period, as well as the combined surveys, excluding all students at four-year schools. The analysis sample in both years is restricted to student loan holders. The average loan (conditional on having one) is smaller for the analysis group and has a greater proportion of female, African American, and independent students than the full sample. The students have lower incomes in the analysis sample. There is a sharp decline in the proportion of students attending two-year public schools, along with a corresponding increase in the proportion of students attending one-year and two-year for-profit schools. This is due to the restriction requiring at least one loan disbursed.

Section 2 Assignment to Single "Main" School

Students may attend multiple schools yet they can only be assigned to one school. For both this analysis, and for published cohort default rates, schools are responsible for the performance of their students—regardless of whether defaulted loans were disbursed at another school. In this analysis, students are assigned to a single "main" school so that we may include school fixed effects. The main school is defined according to the following hierarchy. Once one of these rules is met, the main school can no longer be affected by subsequent rules.

First, for students attending just one institution, that is their assigned school. Second, students are assigned the school at which they earn their highest degree. Third, students are assigned the school at which they are enrolled full time for the most number of months. Fourth, students are assigned the school at which they are enrolled part time for the most number of months. Fifth, students are assigned the school with the highest level attended (e.g., a two-year school over a one-year school if no degree was earned). Finally, students are assigned to the last school attended if none of the above rules led to a match.

Because students may attend multiple schools but only be assigned to one school, it is possible in the analysis sample for a school to appear to participate in both student loan programs, even if it only participated in one. For example, suppose a student obtains a Direct Loan from one community college, transfers to a second community college participating in the Guarantee program, and earns a degree from the second school. The second college would be the main school, and thus that school is assigned students with loans from both programs.

Section 3 Within-School Variation in Program Participation

For the following table, school participation in the Direct Loan (or Guarantee) program is defined as having at least one student assigned to that school having a loan disbursed through the Direct Loan program, regardless from which school that student obtained the loan.

Table D2 displays how these main schools represent students with loans across both programs. Specifically, there are 160 schools that have at least one student in the analysis sample with loans from each student loan program. There are 850 students represented at these schools, comprising approximately one-third of the population sample.

Section 4 Variable List for Student and School Controls

The vector Stu_{it} consists of individual student characteristics, including indicators for race, gender, parental education, degree attainment, major, transfer status, debt, and the number of months between the last enrollment date and the repayment status date. Employment status and earnings in the final follow-up period could be endogenous predictors, but are controlled for in an extended specification. The results are similar.

The vector Sch_{st} consists of time-varying school characteristics including sector, racial composition, total costs net of grants, published total cost of attendance, and Barron's selectivity obtained directly from BPS. This vector includes fraction female, low-income dependent, low-income independent, undergraduates attending fulltime, freshman taking out loans, 150% cohort graduation rates, graduate and undergraduate enrollment, freshman retention rates, and the average freshman loan amount from the TICAS panel.[5]

Section 5 Building up the Student-Level Model

The second column adds loan-specific variables: dollars disbursed, fixed effects for number of loans, and loan age (months in repayment). Months in repayment is defined as the number of months between the last month enrolled and August of the final survey year (2001 or 2009). Column 3 adds student characteristics: high school performance and test scores, race and ethnicity, gender, and parental characteristics including highest education and income. There are also indicators for dependency status, marital status, whether any dependents, and number of dependent children. Column 4 adds institution characteristics: sector (combination of institution level and control), Barron's selectivity rating, and others from TICAS and the Department of

[5] Values from 2004 in the panel are assigned to BPS 2004 observations, while values from 2001 in the panel are assigned to the BPS 1996 observations.

Education as already described. Some of these values vary within schools over time, and do not fall out with the inclusion of school fixed effects. Column 5 adds indicators for college experience: attainment, major, fixed effects for number of schools attended, number of months enrolled full-time and part-time, and college grades.

Section 6 Testing the Identifying Assumption

As described in the main paper, student-level outcomes are time-invariant. As such, I could not test the internal validity of the model using the event study analysis, or include linear time trends. I was able to test whether Direct Loan participation predicts observed characteristics, as a test for the existence of unobserved changes in the student population concurrent with Direct Loan use. As reported in Section 7.3, Table D3 displays the results of the multiple-equation regression model. Each dependent variable was predicted simultaneously in order to account for multiple hypothesis testing. Of the eleven observed characteristics chosen (because there are relatively few missing variables), Direct Loan participation predicts an increase in the likelihood that a student's parent has at least a BA, and a reduced likelihood that the student earned a GED. This suggests that, within the sample population contained in the BPS, Direct Loan participation is correlated with a somewhat more advantaged population.

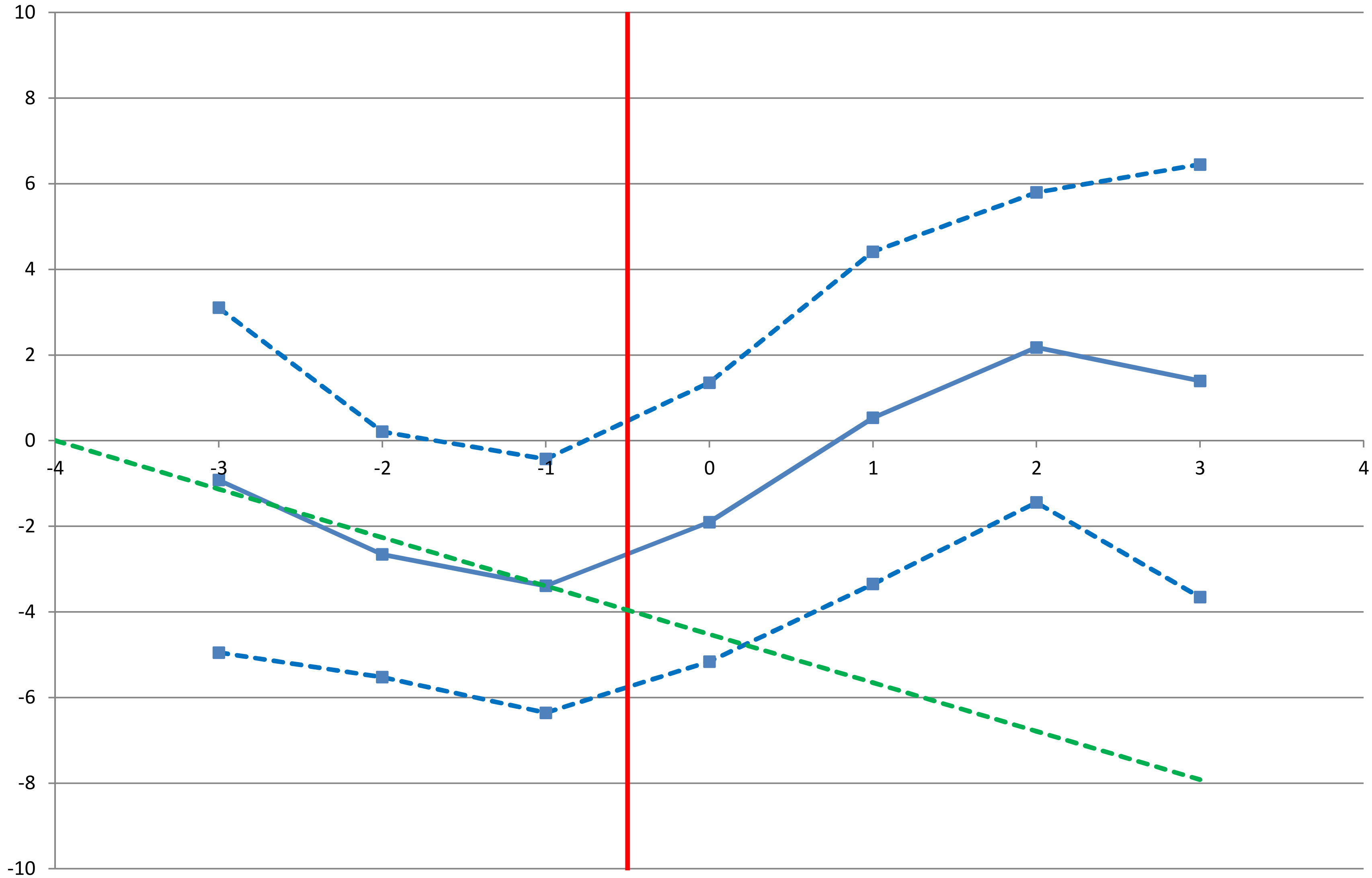

Figure I.C1. Coefficients from Event Study Model Estimating the Impact of Switching from the Direct to Guarantee Program. Unbalanced Panel. The sample includes only high-default, low-enrollment less-than-four-year for-profit schools switching from the Direct to Guarantee program. Excluded group is (t-4) which combines all pre-periods (t-4) through (t-8). (t+3) combines (t+3) through (t+8) because counts are small in these ranges.

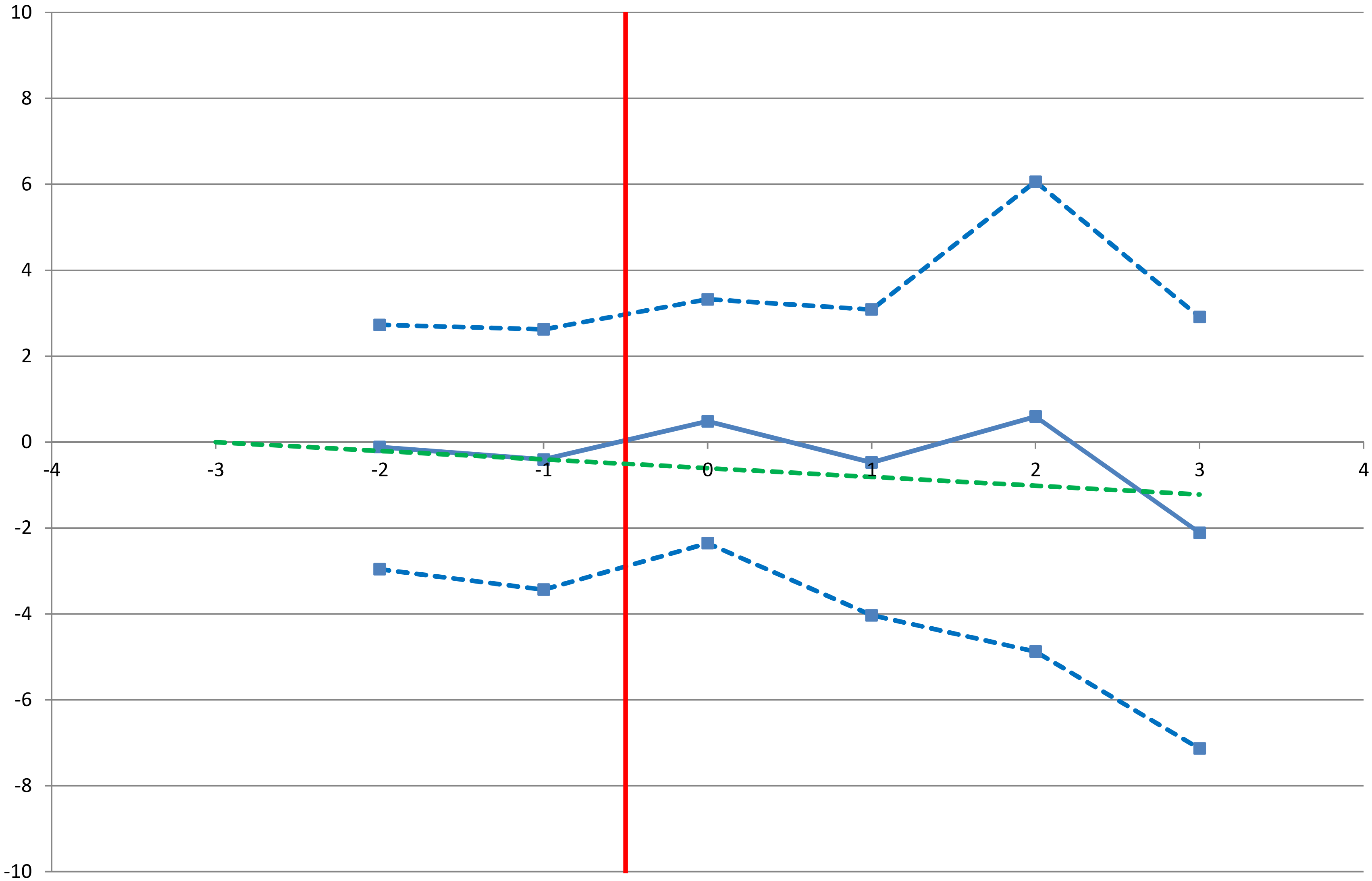

Figure I.C2. Coefficients from Event Study Model Estimating the Impact of Switching from the Direct to Guarantee Program. Balanced Panel. The sample includes only high-default, low-enrollment less-than-four-year for-profit schools switching from the Direct to Guarantee program. Schools of all enrollment levels are included for power. Excluded group is (t-3). Among switcher schools, keep only observations between (t-3) and (t+3). All others are dropped. All

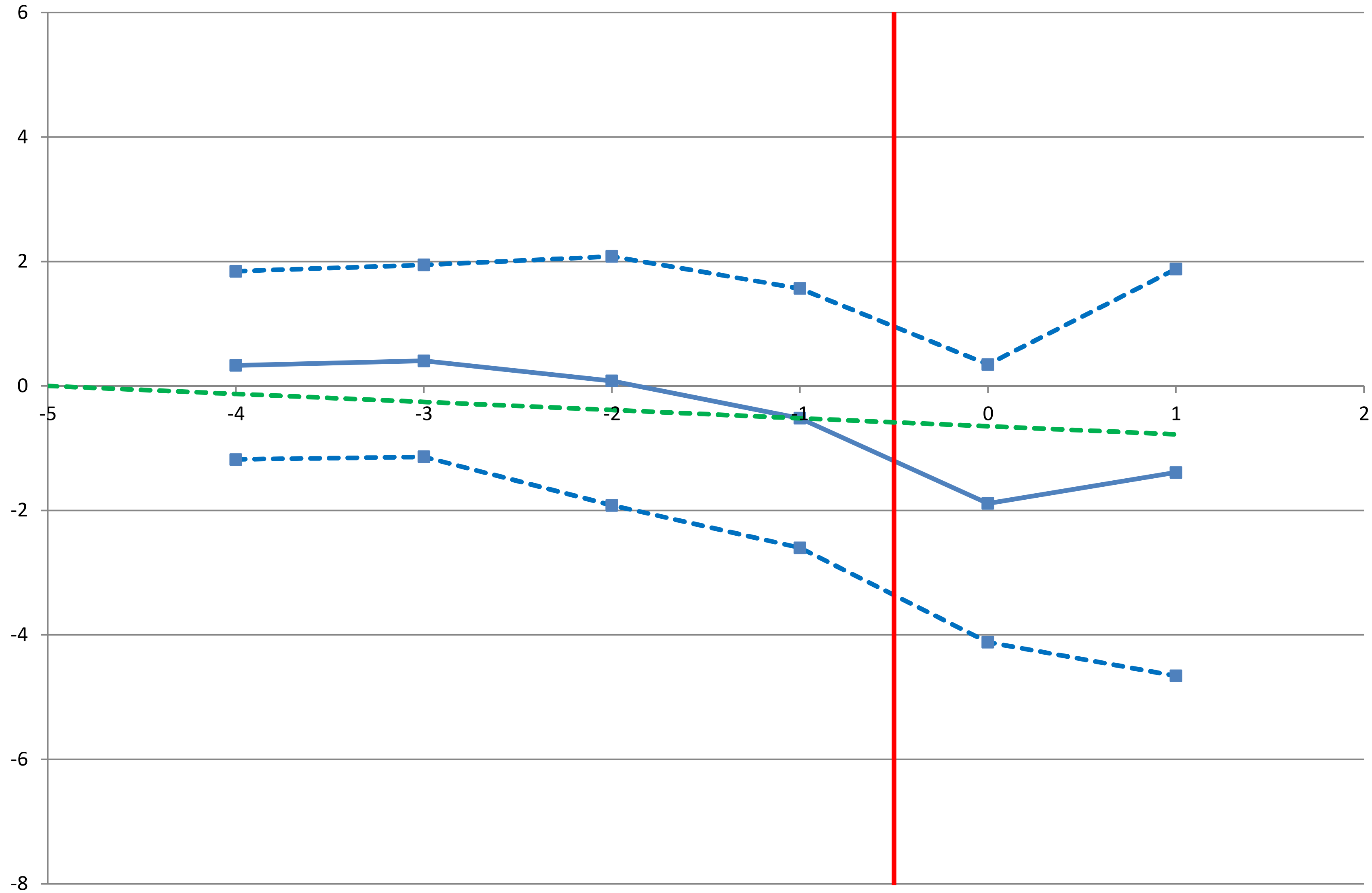

Figure I.C3. Coefficients from Event Study Model Estimating the Impact of Switching from the Guarantee to Direct Program. Unbalanced Panel. The sample includes only high-default, low-enrollment less-than-four-year for-profit schools switching from the Guarantee to Direct program after 2009. Excluded group is (t-5). All observations (t-6) and earlier for switcher schools are dropped.

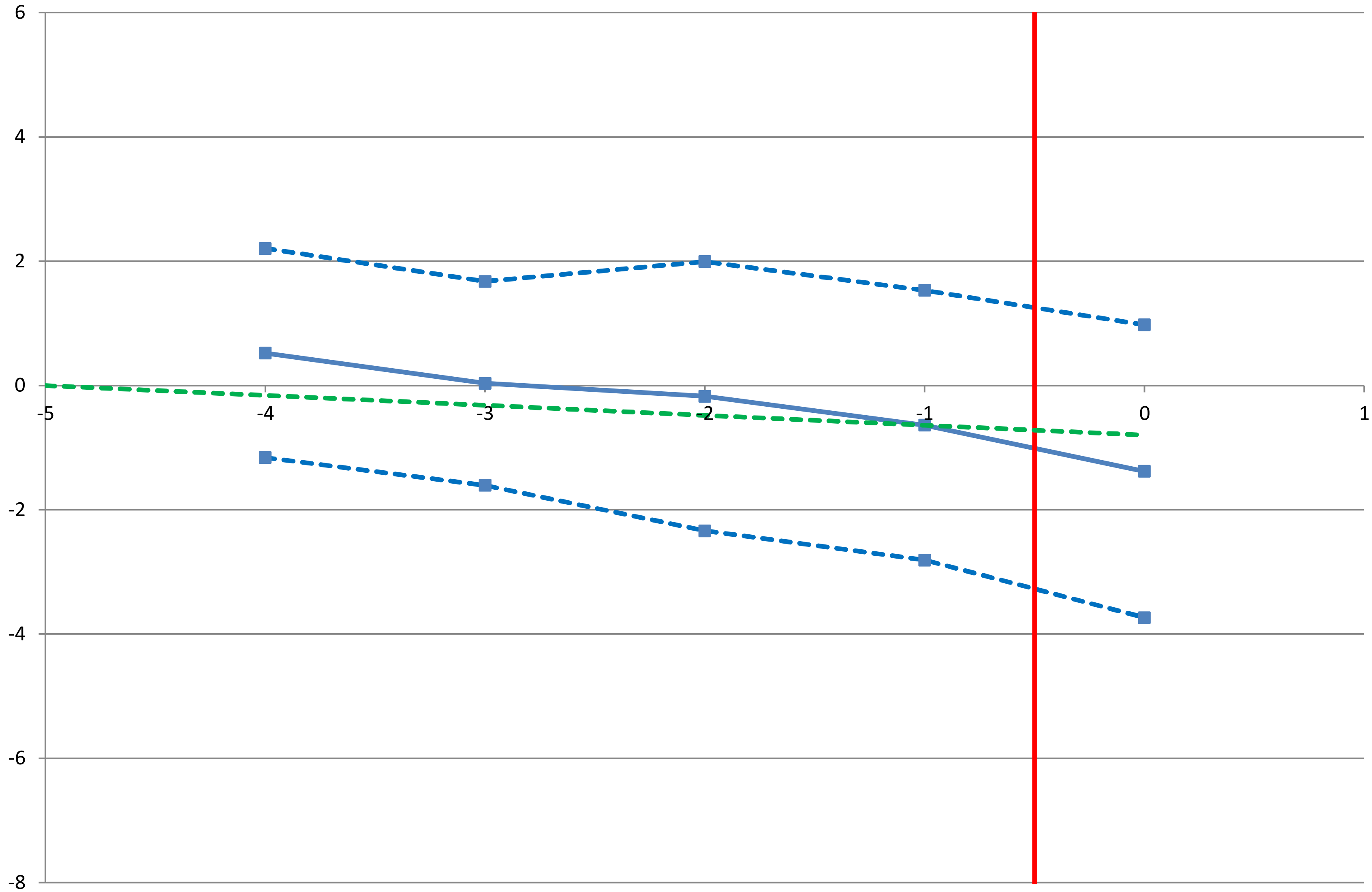

Figure I.C4. Coefficients from Event Study Model Estimating the Impact of Switching from the Guarantee to Direct Program. Balanced Panel. The sample includes only high-default, low-enrollment less-than-four-year for-profit schools switching from the Guarantee to Direct program after 2009. (t-5) is excluded. Switchers only included for observations (t-5) through (t+0). All years of non-switchers are included.

Table I.C1. Characteristics of Schools across Federal Student Loan Programs.

	2000-2008			2008-2011		
	Guarantee Only	Direct Only	Switch	Guarantee Only	Direct Only	Switch
Unique Schools	3,167	782	271	138	1,164	3,091
Avg Annual Loan Recipients	2,314	3,187	2,970	1,500	3,654	3,994
Avg Annual Default Rate	6.2	6.2	7.4	11.5	9.2	8.6
Avg Loan Amount	4,124	3,989	4,284	6,220	6,075	6,141
Pct Years Direct Only	0.00	0.96	0.41	0.00	0.93	0.29
Pct Years Guarantee Only	0.99	0.00	0.49	0.86	0.00	0.61
Pct Years Mixed Programs	0.01	0.04	0.10	0.07	0.07	0.09
Four-Year Public Schools	0.13	0.26	0.16	0.02	0.18	0.14
Four-Year Private Schools	0.33	0.16	0.25	0.10	0.13	0.32
Four-Year For-Profit Schools	0.04	0.03	0.05	0.04	0.03	0.05
Two-Year Public Schools	0.22	0.11	0.19	0.29	0.09	0.22
Two-Year For-Profit Schools	0.12	0.15	0.17	0.28	0.17	0.11
One-Year For-Profit Schools	0.10	0.28	0.17	0.16	0.38	0.10
Other Sector	0.06	0.02	0.02	0.12	0.03	0.06
Undergrad Enrollment	4,554	5,583	5,456	2,747	4,559	5,429
Pct Female	0.63	0.68	0.62	0.66	0.72	0.63
Pct Asian	0.04	0.04	0.03	0.03	0.04	0.04
Pct Black	0.13	0.17	0.20	0.21	0.20	0.15
Pct Hispanic	0.09	0.11	0.09	0.13	0.14	0.10
Pct Independent	0.45	0.45	0.45	0.62	0.47	0.44

Notes: Schools are assigned to the Direct or Guarantee program if at least 90% of loans within a year are disbursed through that program. A school is defined as a Switch school if it is assigned as a participant in each program for at least one year during the period. Guarantee Only schools must not have offered federal loans in 2011. There were 24 and 12 schools in the first and second periods respectively that offered both loan types for all years in the sample.

Table I.C2. Test of Interval Validity in Aggregate Model: Does Direct Loan Participation Predict Changes in Observed Student Characteristics?

	Less-than-Four-Year Schools
Fraction Freshman with Loans	0.032
	(0.038)
Avg Freshman Loan Size	594.1
	(377.8)
Pct Subsidized Loans	-0.020*
	(0.012)
Pct Black	-0.010
	(0.012)
Pct Hispanic	0.014
	(0.009)
Pct Female	-0.012
	(0.012)
Graduation Rates (150% of Expected Time to Degree)	-0.054*
	(0.032)
Pct Full-time (Undergraduates only)	-0.018
	(0.018)

Notes : Each dependent variable is predicted simultaneously using equation (5) on the sample of high-default, low-enrollment less-than-four-year for-profit schools. Values displayed are the sum of the estimated coefficients on lagged program participation. Contemporaneous student characteristics are dropped as independent variables. Standard errors are clustered at the school level. * Statistically significant at the 10% level; ** Statistically significant at the 5% level; *** Statistically significant at the 1% level.

Table I.D1. Summary Statistics for Student-Level Model.

	BPS 1996		BPS 2004		Both Survey Years	
	Full Sample	Analysis Sample	Full Sample	Analysis Sample	Full Sample	Analysis Sample
Student Characteristics						
Students	2,600	710	7,050	1,780	9,650	2,490
Has a loan	0.50	1.00	0.49	1.00	0.49	1.00
Only Direct	0.13	0.14	0.14	0.14	0.14	0.14
Only Guarantee	0.75	0.77	0.76	0.80	0.76	0.79
Has Both Loans	0.12	0.09	0.10	0.06	0.10	0.07
Total Loan Amount	8,119	7,309	11,872	10,797	10,835	9,802
Female	0.60	0.64	0.61	0.64	0.61	0.64
Asian	0.04	0.01	0.04	0.04	0.04	0.03
Black	0.15	0.17	0.20	0.22	0.19	0.21
Hispanic	0.18	0.15	0.17	0.17	0.17	0.16
Independent	0.40	0.47	0.42	0.50	0.42	0.49
Income (independent)	18,142	14,388	24,349	20,334	22,824	18,797
Income (Dependent)	43,290	39,798	52,198	46,324	49,819	44,488
School Characteristics						
Unique Schools	840	310	1,130	540	1,620	780
2-Year Public	0.52	0.29	0.67	0.40	0.63	0.37
2-Year For Profit	0.13	0.24	0.08	0.19	0.09	0.21
1-Year For Profit	0.21	0.32	0.15	0.31	0.16	0.31
Other Sector	0.14	0.14	0.11	0.10	0.12	0.11

Notes: Summary statistics from the Beginning Postsecondary Students 1996/2001 and 2004/2009 surveys. Sample weights are not used to create group means. Students at four-year schools are excluded from both "full" and "analysis" samples for comparability. Unweighted counts are rounded to the nearest 10 as required by the Institute for Education Sciences.

Table I.D2. Loan Holdings of Students within Main School.

	Unique Schools	Student Counts
Has Only Students with Guarantee Loans	520	1450
Has Only Students with Direct Loans	90	180
Has Students with Both Loans	160	850
Total	770	2480

Notes: Counts are derived from National Student Loan Data System extracts in 2001 and 2009. Loan holdings are of students enrolled at each assigned "main" university, regardless of which school enrolled when loan taken out. Main schools are defined by the IPEDS Unit ID. Unweighted counts are rounded to the nearest 10 as required by the Institute for Education Sciences.

Table I.D3. Test of Interval Validity in Student-Level Model: Does Direct Loan Participation Predict Changes in Observed Student Characteristics?

	Parent has at least a B.A.	Student has a GED	Female	Independent Student	Veteran	Has any dependents	Married	Single Parent	Black	Hispanic	Age
Has Only Direct Loans	0.186*	-0.289**	0.062	-0.015	-0.018	-0.131	-0.130	-0.214	-0.184	0.124	-0.820
	(0.095)	(0.121)	(0.108)	(0.134)	(0.043)	(0.136)	(0.143)	(0.134)	(0.118)	(0.109)	(2.512)
Has Both Direct and Guarantee Loans	0.131*	-0.080	0.033	0.129	-0.011	-0.164*	-0.111	0.099	-0.040	-0.026	0.572
	(0.068)	(0.087)	(0.077)	(0.096)	(0.031)	(0.098)	(0.103)	(0.096)	(0.085)	(0.078)	(1.802)
Observations	580	580	580	580	580	580	580	580	580	580	580
R-squared	0.362	0.278	0.588	0.352	0.205	0.322	0.254	0.309	0.432	0.528	0.299

Notes : Dependent variables (column headers) are predicted simultaneously using equation (6) on the sample of less-than-four-year for-profit schools. Contemporaneous student characteristics are dropped as independent variables. Each observation uses the survey weight associated with being observed in both the initial interview and final follow-up period. Standard errors are clustered at the school level. Displayed observations are unweighted and rounded to 10 as required by IES. * Statistically significant at the 10% level; ** Statistically significant at the 5% level; *** Statistically significant at the 1% level.

APPENDIX II.A

Technical Data Appendix

Section 1 Population Estimates by Age-Year-Puma

I seek to obtain the number of individuals at each age within a year and a puma. I use three key sources of data to estimate these population counts. First, the Census provides intercensal population estimates at the county level for five-year age buckets (e.g., the number of individuals between the ages of 16 and 20 in County X in year T). The Census will not provide this data for individual ages, although it exists.

Second, I estimate the proportion of individuals at each individual age within the five-year buckets at each year and county (e.g., 18-year-olds make up 22% of 16–20-year-olds in County X in year T). I use the 2010 decennial Census to obtain counts of individual ages for each county in 2010. I then assume no net differences in mobility by age to estimate the population counts for each individual age in prior years. That is, the number of 19-year-olds in County X in 2010 would be equal to the number of 18-year-olds in County X in 2009. I use these estimated population counts to calculate the desired proportions within each age bucket. I multiply these estimated proportions (e.g., the proportion of 16–20-year-olds that are age 18) by the official county-level population counts for the 5-year age buckets to approximate the number of individuals for each age-year-county.

Finally, I use data from the Missouri Census Data Center to first map counties to 2012 puma definitions, and then to map 2012 puma to 2000 puma definitions (in use through 2011). These are the population counts used in the final denominators to calculate the fraction of individuals within a puma-year-age that have a credit card.

Section 2 Components of the $EconControls_{apt}$ Vector

Starting with the statewide unemployment rate, I assign each age-year-puma observation a main effect for the statewide unemployment rate present when that cohort was between the ages of 18 and 24. If the age-year-puma hasn't reached a certain age by the observation year, then the main effect for all older ages is set to zero. For example, individuals who are 21 years old in 2010 will be assigned a value for the unemployment rate they faced at age 21 (in 2010), age 20 (in 2009), age 19 (in 2008), and age 18 (in 2007). The main effects for the unemployment rate faced at ages 22, 23, and 24 are all set to zero for that observation year.

In addition, I create a series of interaction terms. If *age18ur* represents the unemployment rate faced at age 18, the interaction terms include *age18ur·age19ur, age18ur·age19ur·age20ur, age18ur·age19ur·age20ur·age21ur, age19ur·age20ur, age19ur·age20ur·age21ur*, and *age20ur·age21ur*. I replicate these main effects and interaction terms for growth in per-capita state GDP and growth in per-capita mortgage balances.

APPENDIX III.A

Robustness to Sample Age Restrictions and Survey Weights

Section 1 Robustness to Sample Age Restrictions

Households with adult children may report debt and income across the PEU that isn't actually shared by the household. For example, adult children may be responsible for their own student loans while the parents are responsible for the home mortgage as well as the bulk of household income and assets. To avoid this, I have restricted the sample to households with a respondent age of 45 years old or younger.

This restriction results in approximately one-third of national student loan debt being unaccounted for, according to statistics from the Federal Reserve Bank of New York. I recreate Tables 3 and 7 removing the age restriction from the sample and display the results in Tables A1 and A2. From Table 3, 81% and 75% of "choice households" were delinquent on the education loan among the education-mortgage and education-vehicle loan pairs respectively. Inclusive of all ages, Table A1 shows that 79% and 80% of choice households were delinquent on the education loan among the education-mortgage and education-vehicle loan pairs respectively.

Table A2 displays the coefficients from re-estimating Model 1 as displayed in Table 7. I find that observed characteristics explain 20% of the repayment hierarchy between mortgage and education loans and 24% of the hierarchy between vehicle and education loans. The higher-order terms appear to reduce the explanatory power of the observed characteristics, perhaps highlighting the disconnect between the characteristics of loans for which the parents or children are responsible.

Section 2 Robustness to Survey Weights

The Federal Reserve Board provides a program to calculate the appropriate standard errors when using all five implicates in a regression model. However, while the stata program allows the use of the *svy* command, it does not allow for the use of explicit regression weights. In addition, including thousands of households fixed effects with *svy* requires more computational resources than I have access to. To get around this, I exclude sample weights in the main analysis.

In order to identify the effects of excluding weights, I re-estimate the model using just the first implicate for each household. I no longer need the provided program to calculate the appropriate standard errors under multiple imputation and instead estimate a fixed-effects model with and without survey weights. I display the estimated coefficients from the preferred specification in the paper as well as using just one implicate with and without weights in Appendix Table A3.

The coefficients and standard errors are fairly close in the two unweighted models. The coefficient of interest on the education loan indicator is reduced from 0.054 to 0.036 using all implicates from 0.054 to 0.034 using just the first implicate. In the weighted model, the coefficient is reduced from 0.056 to 0.036. This suggests that I would obtain a very similar pattern of coefficient estimates had I been able to use survey weights with all five implicates.

Table III.A1. Delinquency Rates Conditional on Having a Student Loan and Another Loan in Repayment. Includes All Ages.

			Households Facing Delinquency Choice		
Other Loan in Repayment	Unweighted Count of Households	On Schedule Both Loans	Behind Schedule on at least one Student Loan but not Other Loan	Behind Schedule in at least one Other Loan but not Student Loans	Behind Schedule Both Loans
Mortgage	640	89.3%	7.6%	2.0%	1.1%
Vehicle Loan	587	85.1%	10.1%	2.5%	2.3%

Notes: Data comes from the 2001–2010 Surveys of Consumer Finances. Household counts are unweighted. Limited to households with at least two types of loans in repayment, but no restrictions on age of household respondent. "In repayment" is defined as having a non-missing response to the survey question asking if the household is ahead of schedule, on schedule, or behind schedule on the loan. "Behind Schedule" is defined as paying at least one loan within a loan type behind schedule for that same survey question. Loans other than home mortgage, vehicle, and student loans are disregarded. All survey implicates are used in these calculations. Means are unweighted.

Table III.A2. Loan-Level Model Probability of Delinquency. Includes All Ages.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Education Loan Indicator	0.060***	0.047***	0.067***	0.060***	0.058***	0.044***	0.048***
	(0.008)	(0.010)	(0.008)	(0.008)	(0.008)	(0.011)	(0.012)
Vehicle Loan Indicator	-0.015***	-0.020	-0.009**	-0.015***	-0.009**	-0.015	-0.014
	(0.004)	(0.012)	(0.005)	(0.004)	(0.005)	(0.012)	(0.016)
Fraction Equity in Home		-0.045***				-0.070***	-0.113***
		(0.016)				(0.017)	(0.025)
Fraction Equity in Vehicle		-0.013				-0.014	-0.002
		(0.017)				(0.018)	(0.049)
Has Zero Equity in Home		0.040**				0.029	0.007
		(0.019)				(0.019)	(0.020)
Has Zero Equity in Vehicle		-0.052*				-0.053*	-0.047
		(0.027)				(0.027)	(0.037)
Payment Amount ($100s)			0.001**			0.001***	0.002***
			0.000			0.000	0.000
Interest Rate (Percentage Points)				0.001		0.001	0.000
				(0.001)		(0.001)	(0.002)
Loan Age (Years)					0.003***	0.004***	0.007***
					(0.001)	(0.001)	(0.002)
Constant	0.043***	0.060***	0.033***	0.039***	0.032***	0.037***	0.032**
	(0.002)	(0.008)	(0.005)	(0.007)	(0.004)	(0.011)	(0.014)
Household Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Squared Terms of Continuous Variables	No	No	No	No	No	No	Yes
Unique Households	11,021	11,021	11,021	11,021	11,021	11,021	11,021
Estimated Adjusted R-squared	0.30	0.31	0.30	0.30	0.30	0.31	0.31

Notes : Data comes from the 2001–2010 Surveys of Consumer Finances. Observations are at the household-imputation-loan level. Mortgage Loan indicator is the excluded category. All imputations are used in these regressions. Standard errors are clustered at the household level and account for multiple imputation. Estimated R-Squared comes from a least squares regression using a single implicate. Survey weights are not used.

Table III.A3. Loan-Level Model Probability of Delinquency. Implication of Excluding Survey Weights.

	Unweighted (5 implicates)		Unweighted (1 implicate)		Weighted (1 implicate)	
Education Loan Indicator	0.054***	0.036**	0.054***	0.034**	0.057***	0.036
	(0.010)	(0.016)	(0.010)	(0.016)	(0.019)	(0.028)
Vehicle Loan Indicator	-0.018***	-0.018	-0.018***	-0.021	-0.016**	-0.015
	(0.006)	(0.021)	(0.006)	(0.018)	(0.007)	(0.026)
Fraction Equity in Home		-0.176***		-0.185***		-0.207**
		(0.046)		(0.044)		(0.103)
Fraction Equity in Vehicle		0.023		0.026		0.006
		(0.063)		(0.048)		(0.073)
Has Zero Equity in Home		-0.046		-0.044		-0.050
		(0.030)		(0.029)		(0.053)
Has Zero Equity in Vehicle		-0.049		-0.047		-0.065
		(0.047)		(0.033)		(0.056)
Payment Amount ($100s)		0.002***		0.002***		0.002*
		(0.001)		(0.001)		(0.001)
Interest Rate (Percentage Points)		0.003		0.001		0.001
		(0.005)		(0.003)		(0.005)
Loan Age (Years)		-0.001		-0.001		0.001
		(0.004)		(0.003)		(0.006)
Constant	0.055***	0.035	0.055***	0.045**	0.055***	0.042
	(0.004)	(0.024)	(0.004)	(0.020)	(0.004)	(0.030)
Household Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Squared Terms of Continuous Variables	No	Yes	No	Yes	No	Yes
Unique Households	4,867	4,867	4,867	4,867	4,867	4,867
Estimated Adjusted R-squared	0.81	0.82	0.815	0.820	0.818	0.824

Notes : Data comes from the 2001–2010 Surveys of Consumer Finances. Observations are at the household-imputation-loan level. Mortgage Loan indicator is the excluded category. Standard errors are clustered at the household level and account for multiple imputation for the first two columns. The last four columns use the first implicate only. R-Squared for the first two columns comes from a least squares regression using a single implicate. Survey weights are used only in the last two columns.