A Day Late and a Dollar Short: Liquidity and Household Formation among Student Borrowers*

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Abstract

The federal government encourages human capital investment through lending and grant programs, but resources from these programs may also finance non-education activities for liquidity-constrained students. To explore this possibility, we use administrative data for federal student borrowers linked to tax records and a sharp discontinuity generated by the timing of a student's 24th birthday, which induces a jump in federal support. We estimate a persistent increase in homeownership, with larger effects among those most financially constrained, and lagged marriage and fertility effects. Analysis of earnings, savings, and heterogeneity strongly favors liquidity—over human capital or wealth—in explaining the results.

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I. Introduction

The federal government expends considerable resources to encourage human capital investment. Indeed, the majority of U.S. undergraduate tuition is financed by federal sources, with over $75 billion in student loans and grants disbursed in 2016 alone. Moreover, the $1.2 trillion currently owed in federally guaranteed student debt exceeds the cumulative amount outstanding from any other source of non-mortgage household debt. In standard economic models, these programs encourage investment by helping remove credit constraints from the decision to attend college and subsidizing its cost (Becker, 1962; Ben-Porath, 1967; Mankiw, 1986; Palacios, 2014). However, they may also provide liquidity to students for important non-education activities—for example, by helping homebuyers finance upfront payments required to obtain a mortgage—facilitating consumption-smoothing at a point in the lifecycle when credit is generally scarce.[1]

Still, the net effect of these programs on liquidity is ambiguous, as taking on student debt today might impair the availability of other credit in the future.[2] Specifically, student loan balances and payment histories are used to calculate credit scores and in other aspects of credit underwriting, which could decrease spending on non-education activities, particularly if such borrowing leads to early damage to credit scores or debt overhang (Mian and Sufi, 2011; Gorea and Midrigan, 2017).[3] Indeed, this possibility has dominated the public discussion of these programs, which has often attributed relatively low levels of household formation among young adults to high levels of student debt (e.g., CEA, 2016). A recent *New York Times* (2017) editorial noted: "Loan payments are keeping young people from getting on with life, delaying marriage and homeownership."

Ultimately, how these programs affect liquidity is an empirical question, best answered by an experiment that randomly assigns students access to additional resources and compares their early-lifecycle outcomes. In this paper, we approximate this experiment via a Department of Education (DoEd) policy rule that classifies undergraduates applying for financial aid as either financially

[1] Unlike other types of credit, access to federal student loans does not entail credit underwriting or risk-pricing.
[2] This discussion abstracts from potential effects on liquidity from investment in higher education, which we demonstrate are minimal within our setting. Note that a similar tension has been examined from the firm's perspective, wherein relaxing firm credit constraints can spur investment but high resulting levels of debt can reduce investment later on through a debt overhang channel (Albuquerque and Hopenhayn, 2004; Myers, 1977; Whited, 1992).
[3] These effects may be amplified if potential recipients are debt averse (Caetano, Palacios, and Patrinos, 2011), myopic (Benartzi and Thaler, 1995), or inattentive (Pagel, forthcoming). Note that, for borrowers who meet their student loan obligations, credit availability could be enhanced or expanded via the establishment of a strong credit record, in which case spending on non-education activities may *increase*.

dependent or financially independent, a key determinant of the amount of federal support that they can receive (i.e., their federal loan and grant "limit"), based on the timing of their 24th birthday.[4] Specifically, those who are at least 24 years old in the calendar year they enroll are considered independent; thus, students whose 24th birthdays fall just before January 1 generally face a higher limit than their classmates whose birthdays fall immediately after that date. A substantial fraction of college students are likely affected by this rule.[5]

The analysis links administrative federal student loan, grant, and tax records and uses a regression discontinuity design (RDD) to credibly estimate the effects of access to additional resources through these programs on household formation. Exploiting the sharp cutoff in limits this rule generates over student borrowers' birthdates to approximate random assignment, we primarily examine homeownership—but also earnings, savings, marriage, and fertility—in the year of the discontinuity and up to five years later.[6] Several features of our design help isolate the direct effects of resources from those that could stem from spending on human capital, which we confirm with an extensive analysis of potential mechanisms.[7]

We first estimate that, in the year of discontinuity, borrowers with higher limits receive about 40 percent more federal loans and grants than their peers, which is driven by increased borrowing. We then find that the discontinuity induces an immediate 0.5 percentage point (p.p.) jump in homeownership, evidence that these programs help student borrowers finance important non-education spending.[8] This effect persists through the medium run, consistent with a net positive

[4] The use of the term "limit" throughout this paper is related to but differs from the statutory loan limits published by the DoEd. Our use accords with the credit market definition of the term and refers to the maximum cumulative loan and grant amount that a given student may receive. Within our experiment, this limit can vary with the full set of particular statutory limits faced as well as a student's calculated financial need.

[5] Completing college within four years of high school graduation has become much less commonplace over time, while borrowing for college has become more so. In 2011–2012, 44 percent of undergraduates were at least 24 years old and 59 percent were receiving federal support. These points are discussed further in Section 2.

[6] The identifying assumption generates testable implications, which we are able to validate. Namely, the density of borrowers with respect to the assignment variable is continuous in the vicinity of the discontinuity, and predetermined variables evolve smoothly in this region as well.

[7] The design exploits marginal changes in limits for borrowers attending public and private nonprofit colleges who had previously taken out loans, among whom there is no evidence of enrollment effects. We later more fully describe the rationale for these sample criteria and show the results are similar in samples that relax them.

[8] Purchasing a home generally entails upfront payments, usually to meet a down payment requirement (the percentage of the home that must be paid at settlement) and closing costs (the costs associated with processing the paperwork to buy a house). These costs may represent an impediment for first-time homebuyers, who tend to earn less, purchase less expensive houses, and finance their home purchases with Federal Housing Administration (FHA) loans, which offer lower upfront costs at the expense of higher downstream costs (NAR, 2016; FHFA, 2013). (The minimum down payment for an FHA loan is 3.5 percent of the purchase price, far lower than the 20 percent usually recommended for

effect on liquidity over this horizon. These estimates are robust, with no evidence of effects among same-aged placebo populations that should not be affected by the policy rule. The effect is also echoed in subsequent years by an increase in family formation as measured by marriage and fertility. Altogether, our results underscore the contribution of early access to credit to lifecycle outcomes.

In an analysis of mechanisms, we examine labor market and savings responses, as well as potential sources of heterogeneity, and demonstrate that increased liquidity best explains our results. Consistent with our design, we first find very little evidence that the marginal dollar in our setting is financing or improving human capital. Borrowers have marginally lower earnings in the year of the discontinuity, in line with additional financial resources helping students meet expenses or bolster discretionary spending. However, estimates in all subsequent years—i.e., through the year they turn 30 years old—are indistinguishable from zero and can rule out even small effects. We also find no effect on enrollment intensity or attainment, even in the short run. Furthermore, even though they earn less, borrowers with higher limits are 2.5 percentage points more likely to save (i.e., have a savings or investment account) in the year of the discontinuity, with no discernible differences in subsequent years, consistent with an immediate increase in resources that are not allocated toward education but are otherwise quickly spent down (reflective of financial constraints). We then show that the results appear to be driven by liquidity (as opposed to increased wealth), as effects are largest among borrowers who are particularly financially constrained and who see little or no change in grants. In addition, the effects are largest, and still concentrated among these borrowers, after access to other forms of credit tightened during the Great Recession.

To our knowledge, there has been no work exploring whether young adults extract liquidity from the federal student loan and grant programs. Thus, our core finding that higher program limits facilitate spending on important non-education activities is a novel result and suggests that these programs represent a crucial credit instrument for this demographic that enables consumption-smoothing. While, to demonstrate this function, our study isolates a setting in which student loans and grants are not financing education, the liquidity benefits of these programs could easily extend to contexts in which they do.

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a conventional home mortgage loan.) According to Navient (2015), 21 percent of young adults without a mortgage list not having funds for a down payment as the rationale.

Our study adds to several literatures. First, we make several contributions to work examining federal student loan and grant programs. Within this literature, Avery and Turner (2012) and Looney and Yannelis (2015) describe pertinent aspects of the student loan market. Other recent work motivates borrowing within the context of human capital investment (e.g., Lochner and Monge-Naranjo, 2011 and 2016; Dearden et al., 2008; Palacios, 2014; Stinebrickner and Stinebrickner, 2008) and examines the interplay between the federal programs and other forms of education financing (e.g., Amromin, Eberly, and Mondragon, 2016; Lucca, Nadauld, and Shen, 2017; Turner, 2017; Cox, 2017). Otherwise, most of the empirical work investigates the effects of these and similar programs on human capital (e.g., Angrist et al., 2017; Solis, 2017; Marx and Turner, 2017; Castleman and Long, 2016; Deming and Dynarski (2010) review prior work), with few focusing on federal student loans and none examining their effects on later labor market outcomes (or housing, saving, and family formation).[9] Our main finding represents an unexplored benefit of these programs. Further, we are the first to observe loan and grant records for all U.S. student borrowers and to augment these records with corresponding tax records. The scope of this new dataset, together with our regression discontinuity design, makes our setting well-powered to not only credibly identify the liquidity effects we are after, but also obtain the first precise, nationally representative estimates of the effects of raising program limits more generally. With respect to human capital, our findings suggest that the marginal financial aid dollar later in the education trajectory has a low return on investment; still, the net increase in homeownership pushes back against concerns that increased student borrowing reduces homeownership or that students recklessly over-borrow.[10]

Second, our study makes a novel contribution to the literature linking credit conditions to real activity, which has gained considerable momentum since the financial crisis. In particular, our results add to the empirical micro evidence on whether housing choices reflect credit availability, revealing how relatively low levels of liquidity required for home purchases appear to be out of reach for many potential young homeowners. Several prior studies in this area have emphasized the role of credit availability in housing market dynamics more generally (e.g., Kaplan, Mitman,

[9] We provide suggestive evidence our estimates are not driven by grants. Recent studies examining federal student loans within certain schools or states have found effects on some earlier, predominantly schooling-related outcomes (Marx and Turner, 2015; Denning, 2018).

[10] In formulating our sample, we discover enrollment effects of higher program limits only at for-profit colleges, where returns are notoriously low (Deming, Goldin, and Katz, 2012). Within the much larger public and private nonprofit sectors, we find no effect on later earnings.

and Violante, 2017; DeFusco, Johnson, and Mondragon, 2017; Appel and Nickerson, 2017; Mian and Sufi, 2015; Campbell and Cocco, 2003). Separate work investigates potential determinants of low household formation among young adults (Paciorek, 2016; Bhutta, 2015; Martins and Villanueva, 2009; Kaplan, 2012; Bleemer et al., 2014 and 2017; Mezza et al., 2016; Dettling and Hsu, 2017; Berger, Turner, and Zwick, 2016; Chiteji, 2007). A few of these studies find evidence that student debt (and/or other consumer debt) reduces formation.[11] Other related findings imply down payment requirements are a particular impediment for this demographic (Engelhardt, 1996; Fuster and Zafar, 2016) and that the policy environment may favor older groups (Floetotto, Kirker, and Stroebel, 2016).

Our results also add to a related literature with roots in the canonical permanent-income hypothesis, which evaluates whether credit availability and housing wealth affect consumer spending (e.g., Deaton, 1991; Carroll, 1992; Ludvigson, 1999; Souleles, 1999; Gross and Souleles, 2002; Melzer, 2011; Mian and Sufi, 2011; Mian, Rao, and Sufi, 2013; Kaplan, Mitman, and, Violante, 2016; Aladangady, 2017; Mondragon, 2017; Gorea and Midrigan, 2017; Olafsson and Pagel, 2018; Pagel, 2018; Baker, 2018). In particular, Gross and Souleles (2002) find that spending is quite sensitive to credit card limits and interest rates. Relatedly, work examining effects of predictable changes in income on spending finds evidence consistent with liquidity constraints (e.g., Johnson, Parker, and Souleles, 2006; Parker et al., 2013). We find that making additional resources available through the federal loan and grant programs begins a process that increases homeownership and, subsequently, family formation, suggesting that many young adults are liquidity constrained and that these programs facilitate consumption smoothing. These findings stress the importance of models that incorporate borrowing constraints early in the lifecycle. Further, to reach our main conclusions, we find the effects are stronger among borrowers from low-income families and when credit conditions were relatively tight, which would also favor more nuanced models that allow for heterogeneous responses among agents and over the business cycle.

[11] Two studies strive to compare individuals who are similar on all dimensions except for their student debt, using tuition as an instrument (Mezza et al., 2016; Bleemer et al., 2017). Within our setting, any negative effects of student debt on liquidity appear to be dominated by the positive effect of the limit increase. In related work, Dettling and Hsu (2017) and Bleemer et al. (2014) find evidence that the probability of living with one's parents reflects one's credit position (though Chiteji (2007) does not).

The rest of this paper is organized as follows. Section II describes the policy environment and data sources. Section III discusses the identification strategy, sample construction, and balance tests. Section IV presents the main estimates and examines robustness. Section V describes and evaluates mechanisms. Section VI examines family formation. Section VII concludes.

II. Research Design

A. Institutional Background

The majority of U.S. undergraduate tuition is financed by federal sources through programs established under Title IV of the Higher Education Act of 1965. The largest of these programs, and the focus of our study, are the two major student lending programs, the Federal Direct Loan (DL) Program and the (now-defunct) Federal Family Education Loan (FFEL) Program, as well as the Pell Grant Program. The reach of these programs has expanded considerably over the past several decades, driven primarily by rising college enrollments and attendance costs: In the 2015–2016 academic year, more than 7.1 million undergraduates received a loan and 7.6 million received a grant, compared with 4.3 million and 3.9 million undergraduates, respectively, in the 2000–2001 academic year. In addition, as reliance on these programs has grown, student debt has become an increasingly important component of household balance sheets: According to credit bureau data, there were 43 million individuals with student debt in 2014 (almost double the amount from a decade prior), with an average balance of about $27,000.[12] Approximately 40 percent of households headed by an individual under the age of 35 years old have a student loan (Navient, 2015). In this section, we describe aspects of these programs relevant for our study, highlighting the dimensions along which financial dependency status, per the DoEd definition, can influence the loan and grant amounts for which a student is eligible.

To receive financial assistance through the Title IV programs, students must first be deemed eligible according to a standardized application, the Free Application for Federal Student Aid (FAFSA). The FAFSA collects the demographic, asset, and income information of students and their households pertaining to the calendar year prior to enrollment, much of which is available on

[12] While a private market for student lending exists, the size of this market has always been considerably dwarfed by the federal lending programs—even more so as the credit market began to experience increased regulatory scrutiny. According to The College Board's *Trends in Student Aid* report, in the 2011–2012 academic year nearly $80 billion was disbursed in student loans, and 92 percent of that disbursement was through federal programs.

tax forms.[13] These data are entered into a complex nonlinear formula to compute a student's EFC, the dollar amount that the federal government determines a family can contribute to college expenses in the coming year.[14] The inputs and weights of this formula vary with a student's dependency status; for example, parents' assets and income are key elements of a dependent student's EFC calculation and are not included in an independent student's calculation, which, as a result, often yields a lower EFC. The EFC is subtracted from the cost of attendance (COA) of the college to determine the student's "financial need." Students learn of the types and amounts of federal assistance for which they are eligible in a given academic year via an award letter from the college in which they are enrolled or planning to attend, which is generally sent the prior spring. Continuing students must reapply each year.

Undergraduate loans through the DL and FFEL programs are borrowed funds that must be repaid with interest. The "Stafford Loan," the main type of such loans, features standardized terms, a congressionally set interest rate, and a statutory limit.[15] Besides these features, compared with other forms of credit, including educational loans made through the private sector, Stafford Loans can be made to any student who meets the basic eligibility criteria for federal financial aid programs, even those with thin or adverse credit histories. Stafford Loans come in two varieties: subsidized loans, which are need-based, and unsubsidized loans, which are not. For subsidized loans, interest that accrues early in the life of the loan (e.g., while borrowers are in school) is paid by the government.[16] For both loan types, borrowing is subject to statutory annual limits: The limit for subsidized loans varies with academic level while the cumulative (i.e., subsidized and unsubsidized) limit varies with both academic level and dependency status. Limits over time are shown in Table A.1. As a general rule, all else equal, independent students may borrow more than dependent students. For example, in 2016–2017, when the interest rate on new Stafford Loans was

[13] Students generally wait until after the prior year's tax returns are filed to complete the FAFSA. A fraction of FAFSA applications are audited by the DoEd, and the IRS verifies income.

[14] While the majority of financial aid is distributed through federal programs (College Board, 2015), the EFC is often a factor in institutional and state aid determinations as well. Interactions between the policy rule we leverage in this study and access to other forms of college financing would not bias our main estimates. Regression analyses using the restricted-access 2007–2008 and 2011–2012 National Postsecondary Student Aid Study (NPSAS) reveal a precisely estimated negative relationship between EFC and state and institutional aid: -0.016 (.002).

[15] Prior to 2010, when the FFEL program was eliminated by the Health Care and Education Reconciliation Act of 2010, both the DL and FFEL programs issued Stafford Loans. Stafford Loans are backed by the government, though DLs are financed through direct federal funds and FFELs through private capital. Either program could disburse both subsidized and unsubsidized Stafford Loans, subject to the same loan limits.

[16] Interest rates are set by Congress for both loan types, such that most student borrowers receive a more favorable rate than the market would generally offer them.

3.76 percent, dependent undergraduates in their third-year and above could borrow up to $7,500, while independent undergraduates at the same level could borrow up to $12,500, with no more than $5,500 in subsidized loans in either instance.

Pell Grants are need-based grants to low- and middle-income undergraduate students. The size of the award is a function of a student's financial need, the statutory limit for the maximum grant that can be awarded in a given year (e.g., $5,815 for 2016–2017), and the student's anticipated enrollment intensity in the coming year. As with loans, Pell Grant–eligible students can apply the funds to school costs, receive the grant directly, or combine these methods.

In sum, dependency status influences the level of loans and grants that a student can receive in a given year along several dimensions. Most directly, it determines the total dollar amount a student may borrow through the Stafford Loan programs.[17] In addition, for a subset of students, it affects the formula that determines financial need and thus can alter the maximum amount of Pell Grant and subsidized loans for which a student is eligible. Key for our design, undergraduate students who are at least 24 years old by the end of the calendar year they enroll are automatically considered financially independent.[18] This policy rule creates a situation where students with very similar age profiles, who are born a few days apart, face very sharp differences in limits.

Many students are potentially exposed to this policy rule.[19] In a nationally representative DoEd survey of undergraduates in 2011–2012, 59 percent were receiving federal support and 44 percent were at least 24 years old. While the latter figure may seem high within a framework that assumes undergraduates complete their degree within four years of graduating high school, such a framework is not today's norm: According to a separate DoEd survey of students who completed a B.A. in 2007–2008, the average time to degree was six years, and nearly 40 percent of recipients took more than five years. Also within the 2011–2012 survey, 23 percent of undergraduates and

[17] Parent PLUS loans are another brand of Title IV undergraduate-level loans that may be influenced by dependency status as, by definition, they are only available to dependent students. We later explore effects on PLUS borrowing. The annual volume of lending through the Parent PLUS loan program is about one-fifth that of the Stafford Loan program, with the key differences being that, for Parent PLUS loans, a student's parent, rather than the student, commits to repaying the loan, the parent's credit history is taken into account in determining eligibility, the interest rate is generally several percentage points higher, and parents can borrow up to the cost of attendance (which includes living expenses) less other financial aid.

[18] Other relevant factors include the student's active duty or veteran status and family circumstances concerning marriage, own dependents, emancipation, homelessness, and foster care.

[19] For statistics related to borrowing behaviors at the individual limit, see the NCES Stats in Brief. For statistics related to age ranges and federal support of undergraduates, see the NCES Digest of Education Statistics. For statistics related to time to degree, see the NCES Web Tables, table 2.8.

55 percent of Stafford Loan recipients borrowed at their "individual limit." This rate is reflective of behaviors within both financial dependency groups. Specifically, among financially dependent undergraduates, 24 percent borrowed at their limit (pointing to a binding constraint, which is then relaxed by the policy variation), and among financially independent undergraduates, 21 percent borrowed at their limit.

B. Data

The analysis relies on a linkage between two administrative data sources: 1) the DoEd's National Student Loan Data System (NSLDS) and 2) individual tax records filed with the Internal Revenue Service (IRS). The reliance on administrative data minimizes concerns regarding sample selection, attrition, and measurement error.

The NSLDS is a large administrative database containing the enrollment and federal student loan and grant records for the full universe of individuals that receive financial assistance through the Title IV programs. It includes student loan records from 1969 to 2014, with newly originated loans reported to the system within 30 days of disbursement, and assembles data from a variety of sources, including schools, guaranty agencies, loan servicers, and DoEd programs, to assess loan eligibility, track disbursement of loans, and monitor the repayment status of loans. For this study, we use detailed information pertaining to loans (e.g., balances, counts, subsidized/unsubsidized/Parent PLUS), financial aid applications (e.g., family income, date of birth), Pell Grant disbursements, and enrollment.

The IRS data cover the full universe of individuals with tax records from 1999 and 2015 and describe information from their tax returns as well as from mandatory third-party reporting on their behalf by employers and institutions, including schools. To conduct our analysis, we use mortgage data from 1098 information returns (filed by lending institutions), earnings data from W-2 information returns (filed by employers), enrollment data from 1098-T information returns (filed by colleges), and interest and dividend income data—to measure savings—from 1099 information returns (filed by financial institutions).[20] We also separately observe marital status from filing form

[20] Our main outcome is measured by Form 1098, which is a required filing for any lender that receives at least $600 of mortgage interest during a calendar year, and, according to the form instructions, only one "payer of record" may be designated. Thus, within our design, "homeownership" is defined based on whether the individual or spouse has been designated on such a form. Note that neither the balance test nor the analysis of family formation is consistent with possible marriage effects driving effects on homeownership. Other information returns have similar reporting

1040 (filed by individuals and households) and births of new children from Social Security Card applications.

Finally, we use outcomes measured in prior years and available demographics (citizenship, gender, family income) in the IRS data to test for balance in the research design. Many of these variables are reported on tax forms, though gender and citizenship come from social security records.

III. Empirical Strategy

A. Estimating Equation

There can be a large difference in the amount of loans and grants for which borrowers are eligible based on whether they are deemed financially dependent or independent by the DoEd guidelines. This difference implies a sharp discontinuity in limits among those on the cusp of turning 24 years old whose birthday falls on either side of January 1. Under assumptions described below, we can use this setting to identify the causal effect of higher limits on the federal resources borrowers receive and on household formation. We estimate the following regression discontinuity design (RDD) equation:

$$Y_{it} = \beta_0 + \beta_1 1[D > \bar{D}]_{it} + \sum_{j=1}^{2} \{\delta_j D_{it}^j + \varphi_j 1[D > \bar{D}]_{it} * D_{it}^j\} + \gamma_t + \varepsilon_{it}$$

where i and t index borrowers and cohorts, respectively. Y_{it} is an outcome over a particular horizon (e.g., total grants and loans in the year of the discontinuity, homeownership in the year of the discontinuity, homeownership two years after the discontinuity).[21] $1[D > \bar{D}]$ is an indicator for whether the borrower is 24 years old as of December 31 and thus potentially eligible for higher limits. D is a borrower's age in days, centered to equal 0 for those with a January 1 birthday, included as a quadratic polynomial that is fully interacted with $1[D > \bar{D}]$. In our main specification, we follow Gelman and Imbens (2014) and use a second-order polynomial. In the

requirements; for example, W-2s are mandatory to file if labor earnings are at least $600, and 1099-INT and 1099-DIV are mandatory for interest or dividend income of at least $10 (though a significant number are filed that report less income).

[21] Technically, because the discontinuity in loans and grants occurs over an academic year but our outcomes are measured on the calendar year level, *t* indexes cohorts by calendar year, whereby *t=0* refers to the calendar year that coincides with the ending of the academic year relevant for the policy rule. Outcomes measured "in the year of the discontinuity" refer to *t=0* outcomes. Note that as *t=-1* outcomes could partially reflect the policy variation (during the fall semester), when we examine prior characteristics, we consider those observed in *t=-2*.

appendix, we include only a first-order polynomial and show that our results are not sensitive to this choice. The term γ_t is a cohort effect, and standard errors are clustered on the assignment variable, D. We use the Calonico, Cattaneo, and Titiunik (2014) method to select the optimal bandwidth of birthdates, which leads us to focus on those born within 50 days of January 1.

β_1 identifies causal effects if the RDD continuity assumption is met—namely, the conditional distribution of unobserved determinants of borrower outcomes is continuous in the vicinity of the January 1 threshold. If so, borrowers are as good as randomly assigned to their limits, and we may attribute any differences in outcomes across the threshold to the discontinuous increase in limits generated by the policy rule. While the continuity assumption is not directly testable, for it to hold, all other factors, including those that could be determinants of our outcomes, should be smooth in the vicinity of the threshold, and there should be no evidence of borrower sorting. More formally, the assumption generates two testable implications—(1) the conditional distribution of borrowers' predetermined characteristics should be continuous through January 1, and (2) the conditional density of borrowers should be continuous as well—which we use to help validate our design in the next section.

B. Sample Construction, Including an Exploration of Enrollment Responses and Balance

The analysis examines federal student loan borrowers who turned 24 years old within an academic year between 1998-1999 and 2012-2013 (inclusive). While this focus helps ensure ample statistical power to detect effects, to appear in this sample, individuals must both attend college and receive a student loan, which could theoretically be influenced by the policy rule. If so, comparisons on either side of the cutoff would not isolate effects of liquidity from human capital nor would they necessarily be valid. Somewhat alleviating this concern is the fact that, as noted earlier, potential borrowers apply for financial assistance before they enroll, a clear indication of intent to enroll and some signal of interest in borrowing in the coming academic year. The remainder of this section conducts an empirical examination of enrollment responses to the policy rule in a wider sample, motivates sample restrictions that help us identify the effects we are after, and concludes with a battery of validity tests on the final sample—namely, an analysis of smoothness in the density, sample selection, and balance of predetermined variables. The robustness section later describes estimates from samples that relax our sampling criteria.

The possibility that higher limits available to independent students affect college enrollment is a first-order policy question that, to our knowledge, has not yet been answered. That said, if they do, our main analysis would be unable to distill the effects of liquidity from potential human capital effects. Furthermore, individuals induced to attend college by higher limits may be more liquidity constrained than (or otherwise different from) those who attend in the lower limit state, raising the concern that their inclusion would violate the RDD identification requirement that potential outcomes be continuous through the treatment threshold. To explore this possibility, we leverage the wider sample that can be formed from the tax data and examine the smoothness of the distribution of all college students in each sector—i.e., whether the policy rule appears to discontinuously influence *attendance behavior*—with both visual and regression-based evidence.[22] Figure A.1 graphs enrollment by sector and indicates that any attendance effects appear to be concentrated within the for-profit sector.[23] Table A.2 confirms the visual evidence in a regression framework matching our main specification. While this finding is of independent interest, for ease of interpretation and to help meet key identification criteria, the main analysis excludes borrowers attending for-profit colleges. At the end of this section, we show there is no evidence of sorting on academic level, year, or sector within our final sample.

We impose several additional sample restrictions based on prior-year factors. Most notably, to reduce the likelihood that belonging to our sample is induced by the policy rule, we limit the sample to borrowers who had taken out loans in a prior academic year, whose decision to borrow in a subsequent year is unlikely to be driven by a marginal increase in limits.[24] (Again, these

[22] For this analysis, we use Form 1098-T data, mandatory information returns filed on students' behalf by their postsecondary schools.
[23] These differences may imply that for-profit institutions are better at targeting and enrolling students who can receive more federal financing or perhaps that students attending for-profits are relatively more aware of or sensitive to their limits. This finding, discussed in more depth in the conclusion, is worthy of further study, as recent work has shown that despite relatively high costs, the labor market return from a for-profit education is at or below that from peer institutions (Deming, Katz, and Goldin, 2012; Deming et al., 2016; Cellini and Turner, 2016) and that for-profit institutions better capture federal aid (Cellini and Goldin, 2014; Lucca, Nadauld, and Shen, 2017; Turner, 2017). Otherwise, the evidence is consistent with studies that have generally not found effects of marginal increases in federal grant and loan programs on enrollment (Kane, 1995; Turner, 2017; Marx and Turner, 2015). Note that while Bettinger et al. (2012) found large enrollment effects of filling out the FAFSA, all students in the sample have already filled out the FAFSA and thus were ostensibly planning to enroll.
[24] In other words, individuals who both applied for financial assistance for the coming academic year and were already on the intensive margin of borrowing in a prior year are unlikely to be induced by marginal changes in limits into the take-up of student loans. Along a similar vein, such individuals are unlikely to be induced to enroll in school. One additional advantage of limiting the sample to those who have taken out loans before is that, by omitting borrowers that rarely take out loans, estimates will be more representative of a habitual borrower's experience.

borrowers would have already applied for financial assistance for the coming year.) In addition, this group's *level* of borrowing may be particularly sensitive to their limits, likely enhancing our statistical power to detect effects. To further increase statistical power, we also drop any borrower deemed financially independent in a prior year, who would thus generally be unaffected by the policy rule.[25] Following Yannelis (2016) and Denning (2018), the analysis excludes individuals born within three days of January 1. Dropping this "donut hole" avoids concerns about the retiming of birth due to tax incentives (Dickert-Conlin and Chandra, 1999; Turner, 2017).[26] We then take this final sample and subject it to a battery of validity tests below.

Figure 1 plots the density of borrowers, which, together with the results of a McCrary (2008) test, indicates that the distribution is smooth in the vicinity of the threshold and suggests that potential outcomes are as well.[27] Table A.3 probes selection from another angle: leveraging wider samples we can form from our data, we examine the possibility that individuals on one side of the discontinuity are differentially likely to appear in our sample. Using first the full universe of individuals within the relevant age groups in the tax data, we find no evidence of differential selection, with the 95 percent confidence interval ruling out an increase greater than 0.006 p.p. (about 2.36 percent relative to the control group mean). To increase statistical power, we limit this universe using the prior-period characteristics that help form our final sample – namely, we exclude individuals without prior student loans or who had been previously deemed financially independent. Here, we obtain an *extremely* precise zero, where we can rule out differences greater than 0.2 p.p. (about 0.75 percent relative to the control group mean). Across all of these tests, we find no evidence of sorting around the threshold or differential selection into the sample.

The RDD identification assumption also implies that borrowers' predetermined characteristics will be similar across the threshold. The intuition is that, if the policy cutoff approximates random assignment, then borrowers should appear to be similar on each side. Table 1 presents the results of a formal balance test of prior observations of federal loans and grants and the lifecycle outcomes we examine, as well as academic outcomes and demographics, using the RDD estimating equation. In general, estimates are not statistically significant (out of 18, only one is marginally significant),

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[25] We later demonstrate in a placebo analysis that this group exhibits no statistical difference in outcomes through the threshold.

[26] A related potential concern is that kindergarten entrance cutoffs are correlated with January 1; however, the vast majority of state cutoffs are in September and October (NCES). Further, we show in same-aged placebo samples there is no effect on our outcomes of interest.

[27] Figure A.2 shows the density remains smooth for wider borrower bins used for other figures in this study as well.

lending credence to the validity of our design. Figure A.3 plots corresponding visual evidence that prior observations of outcomes are continuous in the vicinity of the threshold. Finally, consistent with the analysis above, Table 1 presents estimated effects on postsecondary institution type (e.g., four-year versus two-year, public versus private) and borrower academic level and finds no evidence of sorting.

C. Sample Description

Table 2 presents summary statistics, and Table A.4 describes the main analysis variables. The average student borrower in our sample borrows about $6,500 in Stafford Loans in the year of the discontinuity, of which a small majority is in need-based loans. The average Pell Grant is substantially lower, about $1,800, reflecting a combination of the more restrictive eligibility criteria and a lower annual limit. Finally, over 60 percent of students are in their fourth or fifth academic year.

The homeownership rate in the year of the discontinuity is 5 percent, and it doubles by the next year. These rates are broadly consistent with overall formation rates estimated over a similar period—namely, between 1999 and 2011, Bhutta (2015) finds that, on average, 6 percent of 20-25 year olds became homeowners within the next two years. Homeownership then increases considerably as the sample ages, reaching 34 percent five years later. Separately, while not shown, compared to all student borrowers, including those who finished their undergraduate schooling at earlier ages, those in our sample unsurprisingly have lower initial rates of homeownership. These rates fully converge within a few years after the discontinuity year.

Within our sample, the fraction of married individuals quadruples over the same window, from 11 percent to 40 percent, and the fertility rate more than sextuples. Still, a majority of individuals do not have children at the end of the study horizon. The saving rate, on the other hand, inches up steadily over the horizon: In the year of discontinuity, just over 20 percent of the sample saves compared with just over 25 percent five years later. Earnings jump after the year of the discontinuity and continue to increase as the sample ages, consistent with lifecycle earnings profiles. Labor force participation rates, defined as the presence of earnings, essentially do not change with age: Around 90 percent of borrowers have some earnings in each period.

IV. Main Results

A. Effects on Federal Student Loans and Grants

Figure 2 depicts receipt of federal student loans and grants within the academic year of the discontinuity, cumulatively and by category, around the dependency threshold. The figure is constructed similarly to those prior, with means of each outcome plotted in 20-day birthdate bins (overlaid with kernel weighted local polynomial regressions). But, in stark comparison with the earlier figures, there are clear breaks around the threshold for each outcome, visual evidence that the policy rule substantially influences the federal support that borrowers receive.

Table 3 confirms this visual evidence in the regression framework, whereby each cell contains an estimate of β_1 with the outcome denoted by the column header.[28] Specifically, the first column indicates that those who are 24 years old within the calendar year, on average, borrow about \$1,900 dollars in additional loans. Columns (2) and (3) indicate that the bulk of the increase is in need-based loans, which jump nearly twice as much as non-need-based loans. Column (4) indicates that the policy rule also leads to an additional \$1,300 in Pell Grants, on average. Column (5) showcases that the combined effect on loans and grants is quite substantial—nearly \$3,300—implying about a 40 percent jump relative to borrowers born after the cutoff within our data. Finally, the last columns indicate that the estimated effect on total loans and grants over time is very close to the estimate for the year of the discontinuity, which, important for the interpretation of our other estimates, implies that the liquidity differences induced by the rule persist and do not reflect loan and grant dollars that would otherwise be taken being pulled forward to an earlier period.

Table A.5 examines whether financial support from other sources interacts with the policy rule, analyzing both Parent PLUS borrowing records for the main sample and the restricted-access 2011–2012 National Postsecondary Student Aid Study (NPSAS). The NPSAS enables a more comprehensive look at how students pay for college—specifically, broader borrowing and family resource measures that include private student loans and direct resources from families, respectively—albeit in a much smaller sample.[29] Altogether, the analysis implies, even taking into

[28] Figure A.4 shows that these estimates are not sensitive to specifications with linear splines.

[29] Reflecting this more limited sample size and other data constraints, the NPSAS analysis examines student borrowers who turn 24 years old within a full year of the discontinuity and uses birth month, instead of birthdate, as the running variable. Given these differences, estimates from the linear rather than quadratic specification may be preferable, though we present both in the table.

account other major sources of college financing, the policy rule makes considerably more resources available to marginally older student borrowers.[30]

B. Effects on Homeownership by Year

Figure 3 presents the main analysis graphically, plotting homeownership rates in each year up to 5 years after the discontinuity. Each graph reveals a sharp drop in homeownership to the right of the discontinuity across the full horizon, signifying an increase in homeownership among borrowers with higher limits. Contrary to much of the public discourse, increased access to loan dollars appears to be commensurate with *more* homeownership. Note that otherwise homeownership rates often slope downward over the distribution, indicating that older borrowers are more likely to own homes.

Table 4 presents the corresponding regression results. In general, the estimates reveal that higher program limits induce homeownership. More specifically, they indicate that, even though homeownership rates were not statistically different in prior years (i.e., t-2 and t-1), those who experience a limit increase are about 0.5 p.p. more likely to own a home in the year of the discontinuity and more than 1 p.p. more likely to own a home over the medium run.[31]

C. Robustness and Placebo Estimates

Figure 4 shows that the estimated effects on homeownership are broadly robust to varying the bandwidth between 20 and 80 days around the threshold. It plots point estimates, as well as a 95 percent confidence interval, for each specified bandwidth. Estimates are quite stable within reasonable deviations from the optimal bandwidth.[32] Table A.6 relaxes each of our sample

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[30] Within the main sample the policy rule induces a mechanical decrease in Parent PLUS loans, as they are only available to the parents of dependents. However, this decrease constitutes less than half the increase in Stafford Loan borrowing, which suggests partial substitution between these loan types when terms change. As these loans require a commitment from a student's parent, the policy rule not only induces an increase in overall borrowing but also a shift in debt obligation from parents to their children. Within the NPSAS, we validate we can broadly reproduce our Table 3 estimates using NPSAS measures and then find that taking into account private lending has little influence on our student borrowing estimate and that cumulative family support is unchanged over the discontinuity. If anything, increased direct resources from families offset the reduction in Parent PLUS borrowing. Note that in the tax code, children can be claimed as dependents if they are under 24 and enrolled in college; thus, there may also be a transfer of tax benefits from, for example, education tax credits from parents to their children.

[31] By five years after the discontinuity, we have lost nearly 25 percent of the sample, and the coefficient is no longer statistically significant; nonetheless, the magnitude remains in line with the other estimates.

[32] In the later years (where we also have less power), the estimates decrease in magnitude and become less significant at larger bandwidths. This occurs out of the range of the optimal bandwidth and is unsurprising, as it puts more stress

restrictions, first separately and then cumulatively, and shows that the results are robust to wider sampling criteria—namely, including borrowers attending for-profit colleges, first-time borrowers, and borrowers within the "donut hole." Table A.7 shows the results are robust to the inclusion of the full set of predetermined covariates from Table 1 as controls.

Table A.8 shows that the point estimates are stable to using a linear spline rather than a quadratic one (though some precision is lost for later years as we lose sample). Further, Figure A.5 highlights that the point estimates using a linear spline are not particularly sensitive to bandwidth, with the estimated optimal bandwidth pointing to a smaller bandwidth than with the quadratic specification (namely, approximately 40 days, as is presented in Table A.8).

Table 5 presents results of placebo analyses of same-aged groups whose behaviors should not be influenced by the policy to confirm that the effects are driven by the policy rule versus other elements of the design. The first row examines individuals who borrowed in other years but not the year of the discontinuity and thus were unaffected by the policy rule. In no column is the result statistically significant, and the zeroes are precisely estimated. The second row examines borrowers who were financially independent prior to the year of the discontinuity and who therefore should not experience differential changes in their limits that coincide with the policy rule. Again, the analysis generates no evidence of any effect. Additionally, we can generally reject the null that the pooled placebo estimates are the same as our main estimates.

Figure A.6 demonstrates that the results pass a relabeling permutation test, a different placebo analysis that re-estimates effects varying the threshold. Estimates using thresholds away from the true threshold are generally not statistically significant and are lower in magnitude. The results indicate that the main findings are not driven by nonlinearities in the density of the assignment variable.

V. Mechanisms

There are several, non-mutually exclusive channels through which limit increases could lead to an increase in homeownership. First, they may influence investment in human capital, which could be complementary to household formation or generate differences in labor outcomes that

on the running variable polynomial and exploits less of the sharp discontinuity. Said another way, we would not expect a RDD to be very reliable with large bandwidths, especially where there may be increasing nonlinearity in the running variable.

influence housing choices. Second, there may be direct wealth effects, whereby additional subsidies induce increased spending on housing. Finally, they may help alleviate liquidity constraints, either directly by making more dollars available immediately or indirectly by helping borrowers establish a credit history and raise their credit scores, which would increase future access to other types of credit.

In this section, we probe these mechanisms and show that human capital and wealth effects do not drive our results, instead finding empirical support for a liquidity channel.[33] The analysis begins with an examination of outcomes that speak to these mechanisms and demonstrates, both empirically and logically, that human capital effects are negligible. The remainder decomposes the sample to investigate heterogeneity and shows that results are concentrated among groups for whom the limit increase primarily represents a shock to their liquidity rather than wealth.

A. Human Capital Effects

To explore human capital effects, we primarily examine earnings and labor force participation. The first two columns of Figure 5 present these outcomes within three representative years, the year of the discontinuity, three years later, and five years later.[34] There appears to be an immediate jump in earnings at the threshold, implying that borrowers eligible for higher limits earn less in the year of the discontinuity. In subsequent years, earnings and labor force participation appear to evolve smoothly.

Table 6 presents the regression estimates for these outcomes. In the year of the discontinuity, there is a marginally significant earnings decrease of about $240 but a precise zero effect on participation. Note that the earnings decrease is consistent with both a reduction in labor supply and an easing of liquidity constraints, though the earnings reduction is an order of magnitude

[33] An information channel that stems from experience managing debt could also help generate our results. Such experience may increase familiarity with repayment options and the credit market more generally, which could influence a borrower's likelihood of taking out a home mortgage later in life. Related work has found large effects of information on the debt behavior of young adults, e.g., Rooij, Lusardi, and Alessie (2011); Brown, Grigsby, van der Klaauw, Wenand, and Zafar (2016); and Liberman (2016). Still, it is not entirely clear why marginal dollars of debt would be particularly influential in this regard; moreover, to the extent an information channel is operating, because some of the effect on homeownership occurs contemporaneous to the increase in limits, experience itself cannot fully explain our results.

[34] For the remainder of our analyses, we streamline the presentation of results in this manner though the same patterns hold over the years we omit. In particular, there are no statistical differences in either earnings or savings in the year following the year of the discontinuity, which is most consistent with a liquidity channel (Table A.9).

smaller than the increase in loans and grants. More importantly, higher limits do not appear to translate into additional human capital, as there are no discernible effects on earnings or participation in subsequent years. Indeed, the estimates are precise enough to rule out a 0.4 percentage point increase in participation and 1 percent increase in earnings.[35] While earnings measured in the first years after the discontinuity may not fully reflect the long-term human capital effects of higher limits, by five years after the discontinuity, borrowers are 30 years old, an age at which earnings is highly correlated with the lifetime earnings profile; at this age, we continue to recover a precisely estimated null effect that rules out a 1 percent increase (Mincer, 1974; Chetty et al., 2011). Furthermore, assuming lifetime earnings for college graduates of approximately $2.1 million (College Board, 2016) and even an extremely conservative zero discount rate, our estimates can rule out a lifetime earnings increase of $21,000, suggesting an annual rate of return on marginal grant and loan dollars of less than 5 percent. Finally, in the appendix, we do not find effects on more education-centric human capital outcomes, such as completion or enrollment intensity.[36]

Are these results surprising? While, as noted in the introduction, standard economic models assume student loans help financially constrained individuals make costly educational investments that improve their labor market outcomes, within the empirical literature, even taking into account studies that allow for extensive margin responses, there is, thus far, little evidence that aid affects long-term earnings (e.g., Bettinger et al., 2016). Furthermore, our setting examines *marginal* dollars made available to student borrowers late in their educational trajectory, among whom there is no evidence of extensive margin (attendance) effects.[37] Thus, higher limits would predominantly influence labor outcomes via increases in attainment. While such effects in our environment appear to be negligible, even assuming a large effect, the expected impact on earnings is still small.[38] For

[35] Table A.10 demonstrates that results derived from a linear spline are similar but even more precisely estimated (i.e., can rule out an earnings increase greater than 0.5 p.p.).
[36] Table A.11 analyzes college completion rates, defined cumulatively for each period, and finds no difference at any period over the horizon we examine, overall or splitting the sample by EFC (i.e., according to how otherwise constrained a student borrower is, which is discussed later). There are also no short-run effects on enrollment or enrollment intensity (Table A.9).
[37] This is partially by construction, as we omit students at for-profit institutions for whom there is an enrollment effect, although this only applies to a small fraction of students.
[38] Denning (2018) analyzes students at four-year colleges in Texas and, while he estimates that seniors with access to more aid under the same policy rule we study are about 1¾ p.p. more likely to complete college one year earlier, this result is consistent with our earnings results (as a pull-forward, but net zero, effect of that size would imply a negligible effect on post-college earnings). Further, he similarly estimates an initial reduction in earnings but no effect in the

example, assume an additional $1,000 leads to a 4 p.p. increase in completion, an estimate on the higher end of the range from the empirical literature (Dynarski, 2003), which was derived from increases in grant aid only (resulting in substantial price effects). If college completion leads to a 15 percent annual increase in earnings, another estimate from the higher end of the empirical literature (CEA, 2016), we would expect our estimated $3,000 increase in grants and loans to increase earnings by, at most, 2 percent. Our estimates are precise enough to rule out such effects.

B. Effects on Savings

We next examine saving behavior as a potential indicator that liquidity drives the homeownership effect. Securing a mortgage generally requires making a down payment and might also entail reserve requirements and/or verifying an established savings pattern. If borrowers are liquidity constrained, they may be unable to meet these basic requirements. Still, a *persistent* increase in saving would not be consistent with liquidity constraints. Further, evidence of increased saving would provide additional evidence—beyond the lack of an effect on enrollment and other education-related outcomes—that marginal student loans and grant dollars are not being (fully) allocated toward education spending.

The last column of Figure 5 graphs saving rates for the three representative years. There appears to be a very large difference in the year of the discontinuity that disappears in subsequent years. Indeed, turning to the regression-adjusted estimates in the last column of Table 6, we find that borrowers with higher limits are 2.5 percentage points more likely to save in the year of the discontinuity, with no evidence of an effect in subsequent years, including the year following the year of the discontinuity. We have shown that borrowers with higher limits experience earnings reductions in the year of the discontinuity, so a coincident jump in the saving rate presumably reflects resources acquired through the increased federal support not being immediately allocated toward education, which are then being set aside for future use, placing both the documentation of an account and a down payment within reach. Further, given the rapid decline in the effect after the enrollment year and no increase in investment income in *any* year in the sample period (a proxy

subsequent year, arguably the most likely time an earnings gain would be evident in his setting given the completion dynamics. In separate work, Stinebrickner and Stinebrickner (2008) find that when college is free (i.e., loans are not financing education), credit constraints have only a small influence on completion.

for the amount of savings), those increased resources still appear to be spent quite rapidly, consistent with binding credit constraints.[39]

C. Results by EFC

Next, we try to disentangle liquidity from wealth. Note that the composition of the sample is prima facie evidence of a liquidity channel, as student borrowers are presumably liquidity constrained on some dimension. But, to try to explicitly make this distinction, we first leverage a bright line rule that the DoEd uses to determine an applicant's EFC, which helps isolate the effects of borrowed resources from subsidies. While a lower EFC typically leads to increased grant and loan awards, and there are separate EFC formulas for financially dependent and independent applicants, those whose "family income" passes below a certain threshold are automatically assigned a zero EFC.

This rule can be used to split the sample into two groups, those who had an EFC of zero in the prior year and those who had an EFC above zero. Borrowers with zero EFC tend to come from lower-income households with fewer assets, and thus would presumably be relatively liquidity constrained.[40] In addition, this same group would very likely be eligible for the maximum allowable subsidy even as financial dependents and thus would be less exposed to a wealth shock in the year of the discontinuity, no matter which side of the threshold their birthdate falls. In other words, relative to the positive EFC group, the zero EFC group primarily experiences higher borrowing limits under the rule.

Table 7 splits the sample accordingly. In the bottom row, we see that, as expected, borrowing increases substantially for both groups, but the increase in need-based loans and grants for the positive EFC group are each nearly a full order of magnitude larger, whereas most of the effect in the zero EFC group stems from an increase in non-need-based loans. Turning to homeownership, we see that the effect is clearly largest in the zero EFC sample, with the estimates among this group implying an economically significant increase in homeownership in the years of and after the

[39] We examined but did not detect a statistically meaningful difference in the amount of interest and dividend income (a proxy for the level of savings) in any of the years we consider. Effects from the year of, and year following, the discontinuity are presented in Table A.9 (as are the effects on the presence of savings).

[40] Borrowers from lower-income families are more likely to face credit constraints, both because their parents may be unable to cosign on loans and because they may be unable to rely on family resources for funding or collateral (Souleles, 2000; Johnson, Parker, and Souleles, 2006; Sun and Yannelis, 2016).

discontinuity. Note that all of the coefficients for the positive EFC sample are smaller, none are statistically significant, and the estimates between the groups statistically differ when pooling across all years of analysis. Altogether, responsiveness appears to be concentrated within the group that is eligible for *less* additional subsidy, favoring the liquidity explanation over the wealth one.[41]

D. Results Before and After the Great Recession

To further examine the role of liquidity, we split the sample into two time periods, leveraging the large change in lending conditions before and after the Great Recession. Specifically, prior to 2007, underwriting standards were relatively lax and credit was more widely available (Keys et al., 2008; Keys, Seru, and Vig, 2012). The effects of the crisis permeated the entire credit market. Fostel and Geanakoplos (2016) note that the average down payment for subprime home loans went from approximately 3 percent in the first quarter of 2006 to 16 percent in the first quarter of 2008. Figure A.7 shows that the fraction of banks tightening lending standards for consumer loans sharply increased in 2007. Finally, household balance sheets and local labor market conditions suffered as well. Given these factors, if liquidity effects are driving our results, we would expect to see the largest responses when conditions are tightest (i.e., beginning in 2007).[42]

We begin by splitting the sample into whether the year of the discontinuity occurred before or after the contraction of credit, dividing borrowers enrolled through 2006 and borrowers enrolled in 2007 onward.[43] The left-hand columns of Table 8 indicate that the homeownership effect is an order of magnitude larger and only statistically significant during the crisis, consistent with a liquidity explanation. Still, a potential concern is that the composition of borrowers changed between the two periods, which could be driving the differences in our estimates (Looney and Yannelis, 2015). To address this concern, we repeat the analysis for borrowers enrolled in 2007 onward, reweighting the sample by demographics to resemble borrowers in the earlier period. The results are quite similar to those before, with the estimated effect on homeownership still highly significant.

[41] As further evidence, in Table A.12, we split the sample by school type and find effects are largest among borrowers attending public universities and two-year colleges, which tend to draw students from lower-income backgrounds. Table A.13 shows that the effects on loans and grants are pronounced across all school types.
[42] Housing prices also decreased between these two periods, which might compound any effect on liquidity stemming from changing credit conditions (which includes non-mortgage forms of credit, such as private student loans).
[43] We do not include results for later years, as for some borrowers assigned to the prior period in this exercise, later outcomes will be observed during the recession, which would make their interpretation difficult. Further, data become unavailable the further out we go for cohorts in the post period. Nonetheless, the results are similar in later years.

Finally, to further explore a possible liquidity channel, we split the recessionary sample into EFC groups under the same premise as the last EFC exercise—i.e., that those with zero EFC are more constrained and primarily experience increases in their borrowing limits, with relatively little increase in their subsidy. The final columns imply that, when credit conditions were extraordinarily tight, the response is still concentrated within this group.

In sum, our analysis of mechanisms indicates that the main estimates are driven by increased liquidity rather than by human capital or wealth effects. Further, while not shown, there is also no evidence of differences in debt cancellation measured by Form 1099-C filings (e.g., debt discharge, bankruptcy) over the full study horizon, consistent with these early career liquidity injections being net beneficial to student borrowers over the horizon we examine. In addition, homeownership itself may offer a future source of liquidity. Our findings imply that, on balance, limit increases help alleviate financial constraints that young adults face and make them better able to smooth consumption between time periods.[44]

VI. Family Formation

We round out the analysis by examining marriage and fertility rates to probe whether the increases in homeownership we detect point to more general increases in household formation, which may similarly entail upfront fixed costs. As with homeownership rates, some commentators have expressed concerns that family formation is *depressed* among young college graduates due to high student debt balances.[45] Figure 6 shows these outcomes for the three representative years. Note that any differences in the year of the discontinuity are very small; however, they grow over time and are reasonably stark by the end of the medium run.

The regression-adjusted estimates in Table 9 largely corroborate the graphical evidence. Neither effect is statistically significant in the year of the discontinuity, though both effects grow over the medium run.[46] Indeed, it appears that both marriage and fertility lag the initial homeownership effect, such that, by the middle of the horizon, borrowers with higher limits are more likely to be married and more likely to have had children. (Note that while marriage, unlike

[44] While Yannelis (2016) finds that increased student borrowing can induce student loan default, within our setting, any adverse consequences for liquidity from this channel appear to be dominated by other more liquidity-beneficial channels (e.g., direct resource effects), which may partly owe to the exclusion of for-profit borrowers from our sample.
[45] Moreover, it may be that homeownership itself has real effects on these outcomes (Sodini, Vestman, and von Lilienfeld-Toal, 2017).
[46] Table A.14 demonstrates that results are similar when using a linear spline.

other outcomes, is only observed among those who file their taxes, the filing rate is very high and there is no evidence of differential filing by the discontinuity.)

VII. Concluding Remarks

We find that additional access to federal student loans and grants increases homeownership and family formation among student borrowers, with little effect on their human capital. Effects are concentrated among those from low-income households and with lower levels of family wealth, as well as during periods when other forms of credit were relatively unavailable. While the policy rule induces both an increase in borrowing limits and an increase in subsidies, effects appear to be driven by the former. Our results demonstrate that these programs, particularly the student lending program, serve an important credit function, enhancing the liquidity of an otherwise highly constrained group (i.e., young adults). This implication adds dimensionality to the popular narrative around the student loan program—challenging the belief that, outside of potential human capital benefits, it largely hurts or delays household formation—and a new consideration to cost-benefit analyses of student aid programs more generally. While data limitations prevent the examination of a longer horizon, these patterns suggest the effect persists into older ages. Altogether, our results highlight the influential role early access to credit plays in an individual's life course.

To help with interpretation, our estimates can be combined to yield a suggestive estimate of the effect of additional student loan and grant *dollars* on homeownership. In particular, scaling the homeownership estimate by the estimated effect on federal student loans and grants implies that an additional $10,000, on average, raises homeownership by 2.4 p.p.[47] Note that $10,000 would cover the full cost due at signing for an FHA mortgage for the median-priced home among young buyers between 2011 and 2012 (NAR, 2013).[48] We can compare this estimate to those implied by

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[47] A causal interpretation of this scaled estimate requires an assumption—namely, that homeownership is only influenced by the policy rule through its effect on federal student loans and grants—that may not hold. If higher federal student loan and grant limits crowd out (in) other forms of college financing, the effect of the marginal financial aid dollar is *larger (smaller)* than this calculation implies.

[48] Specifically, among surveyed buyers under 32 years old (79 percent of which were first-time homebuyers), the median home price was $165,000, with one-third of this group spending less than $125,000. The costs due at signing are calculated assuming a down payment and closing costs of 3.5 percent and 2.5 percent of the purchase price, respectively. The less-than-$10,000 amount due at signing implied by these figures is likely lower in our sample; namely, the median amount of mortgage interest we can observe from borrowers in the first couple years is $3,000 to $4,000, which is consistent with a lower average home price than $165,000.

Bulman et al. (2017), which examines lottery wins among parents of post-adolescent children. This study, which generates estimates of the effect of resources, draws upon older age ranges and already-formed families. Unsurprisingly, our effects are larger, though not extremely so: Their estimates imply a 0.8 p.p. increase in homeownership for a range in which the mean win was about \$15,000 and a 4 p.p. increase for a range in which the mean win was about \$50,000.[49]

Our results underscore the importance of understanding the origins of student debt when assessing its implications for the economy. While the potential financial stress that such debt poses on young households has dominated the discussion, the net returns from the activities that the original loans were used to finance should be the first-order consideration. As our study illuminates, in addition to whether student borrowers have seen positive net returns from the education that they financed with loans, policymakers must also consider whether they separately extracted liquidity from these loans to finance non-education spending that ultimately left them better off as well. Indeed, this benefit likely generalizes to other students beyond the particular group we study, including those whose human capital decisions are sensitive to program resources (so long as there is still some crowd out of education spending). Future research should consider such effects when assessing the welfare consequences of these programs. That said, our particular context points to potentially negative net returns from the small amount of additional educational investment being financed with loans, which was concentrated within for-profit colleges.

Increasing homeownership has been a central policy goal in the United States, often motivated by potential consumption benefits and positive social externalities of owning a home. The federal government spends at least \$70 billion a year on the mortgage interest deduction under the auspices that it will encourage homeownership, even though in present day, the majority of spending finances intensive margin housing decisions with few such benefits (e.g., Glaeser and Shapiro, 2003) and the hazard rate into homeownership among young people has been declining (Bhutta, 2015). Policies that more efficiently stimulate homeownership may thus be of interest.

[49] Alternatively, we can compare it to a recent estimate from the housing literature. Berger, Turner, and Zwick (2016) examine the First-Time Homebuyer Credit (FTHC), a temporary tax credit for new homebuyers intended to stimulate home purchases between 2008 and 2010. Their study analyzes a policy targeted toward homeownership but draws upon age ranges and cohorts similar to those we examine; however, it entails broader price effects on the housing market that will dampen the individual partial-equilibrium effects. They find that the more generous phases of the FTHC—during which the *maximum* credit was \$8,000—induced as many as 546,000 home sales, which, based on our calculations, implies a 2.3 p.p. increase in homeownership. (The denominator for this calculation is the number of tax returns filed by 26–35 year olds in 2008 published by the IRS Statistics of Income in July 2010.)

Finally, our findings have implications for models of human capital formation that allow prospective students to be financially constrained. These models are a major rationale for student lending programs, which theoretically enable costly educational investments associated with positive net labor market returns. With the exception of a *negative* effect on earnings within the year of the discontinuity, our estimates imply that higher borrowing limits, at least later in the education trajectory, do not affect earnings or labor force participation, calling into question how policymakers should motivate proposed increases in limits. Moreover, effects on enrollment are concentrated within a notoriously low return sector. Thus, our results are not particularly consistent with financial constraints stymieing optimal investment in education, at least along the margins we consider.[50]

Nonetheless, we caution that our estimates represent the effects of marginal changes in program limits among a specific population of student borrowers, those who turn 24 years old while enrolled. This population may tend to graduate over longer horizons or enter late. Large changes in limits or changes that affect other populations could generate different effects. For example, enrollment and completion effects could be much more substantial during earlier lifecycle phases when educational investments are potentially more sensitive. Furthermore, it is possible that, while higher limits, on balance, alleviate constraints over the horizon we study, higher debt levels may play a role in exacerbating liquidity constraints later in life. Future research should examine liquidity effects of the federal student loan and grant programs within other populations and environments and on other spending outcomes.

[50] That said, any human capital benefits of higher limits could be getting offset by another underlying process. Debt may pose a drag on labor outcomes (Liberman, 2016; Dobbie et al., 2016; Herkenhoff, 2013) or disincentivize work in a manner similar to income taxation (Bernstein, 2016; Donaldson, Piacentino, and Thakor, forthcoming; Mondragon, 2017). Or, schools may price discriminate and raise tuition in lockstep with loan and grant limits (Cellini and Goldin, 2014; Lucca, Nadauld, and Shen, 2017; Turner, 2017). Finally, education may generate externalities (Moretti, 2004), which could entail human capital effects beyond those that we consider.

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Figure 1: Density of Borrowers

Notes: This figure shows number of borrowers by the assignment variable, in bins of ten days from dependency cutoff. The figure plots the final analytical sample, which, as discussed in the text, excludes borrowers in the donut hole. The assignment variable is the number of days from turning 24 in the calendar year of the discontinuity. The McCrary (2008) test statistic is .1052 (.4956). All data come from the NSLDS matched to IRS tax data.

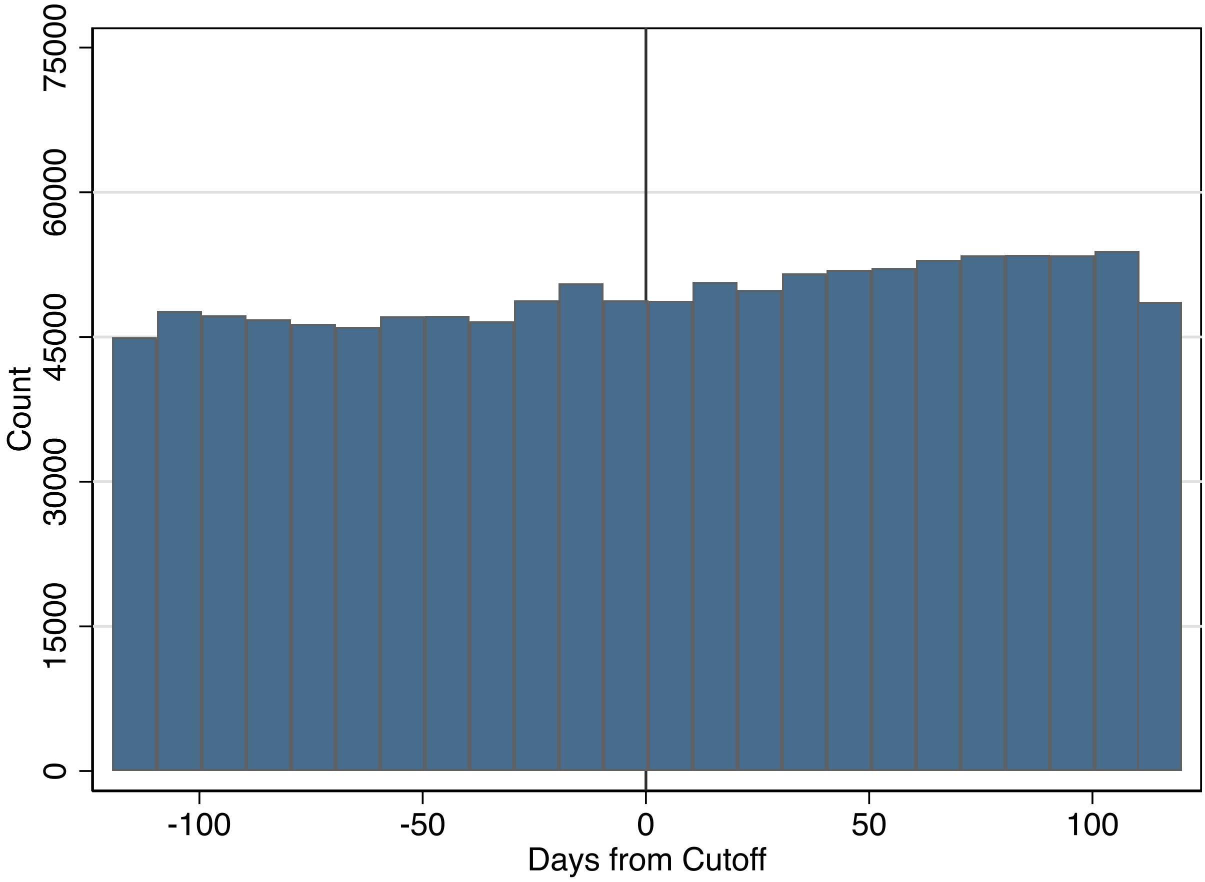

Figure 2: Federal Loans and Grants

Notes: This figure shows mean estimates of the limit increase on loans and grants in the year of the discontinuity in 20-day bins of the assignment variable, broken down by the different type of loan or grant. The outcome is denoted above each estimate. The assignment variable is the number of days from turning 24 in the calendar year of the discontinuity. All data come from the NSLDS matched to IRS tax data.

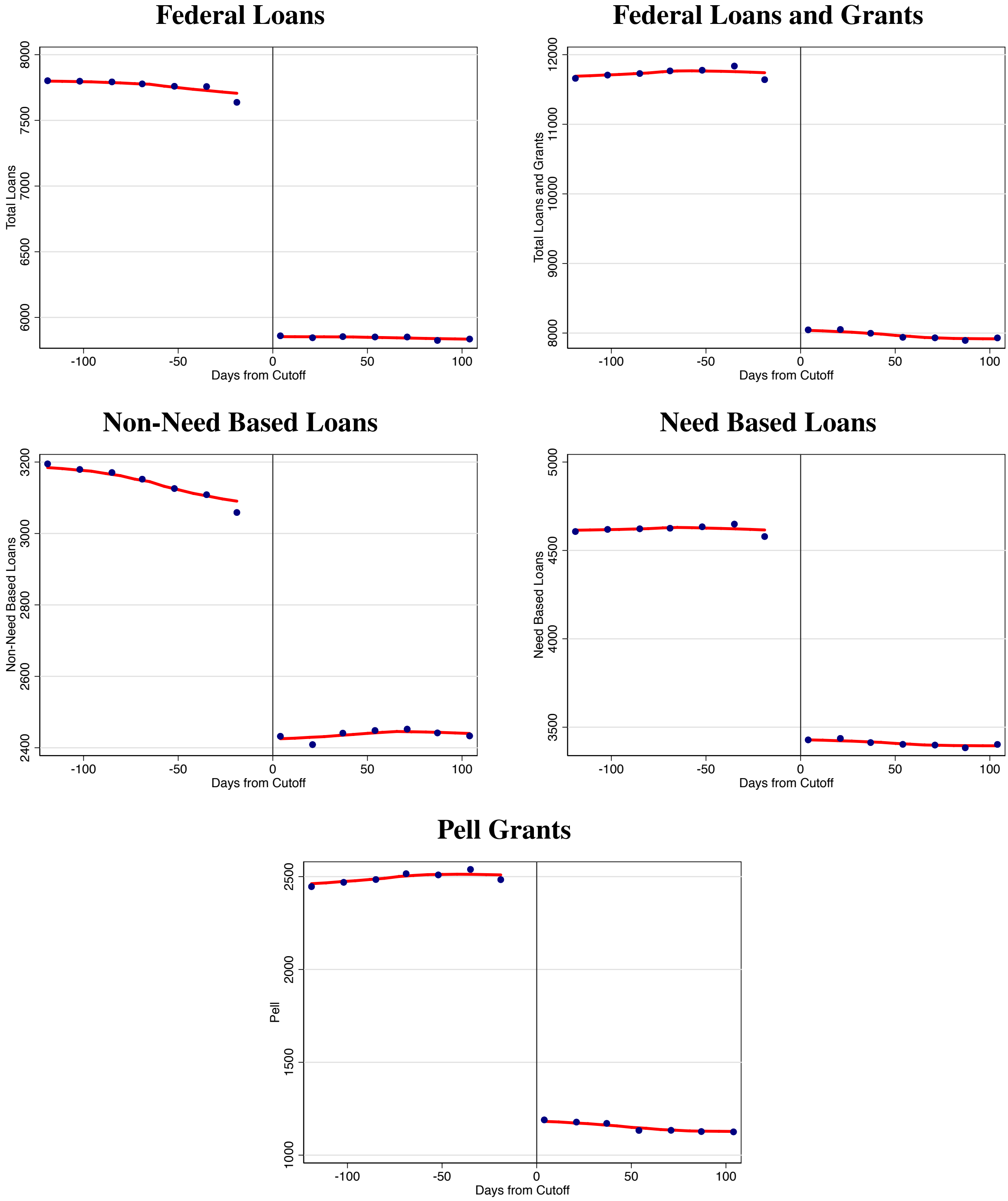

Figure 3: Homeownership

Notes: This figure shows mean home ownership in 20-day bins of the assignment variable, defined by the presence of a mortgage. The assignment variable is the number of days from turning 24 in the calendar year of the discontinuity. All data come from the NSLDS matched to IRS tax data.

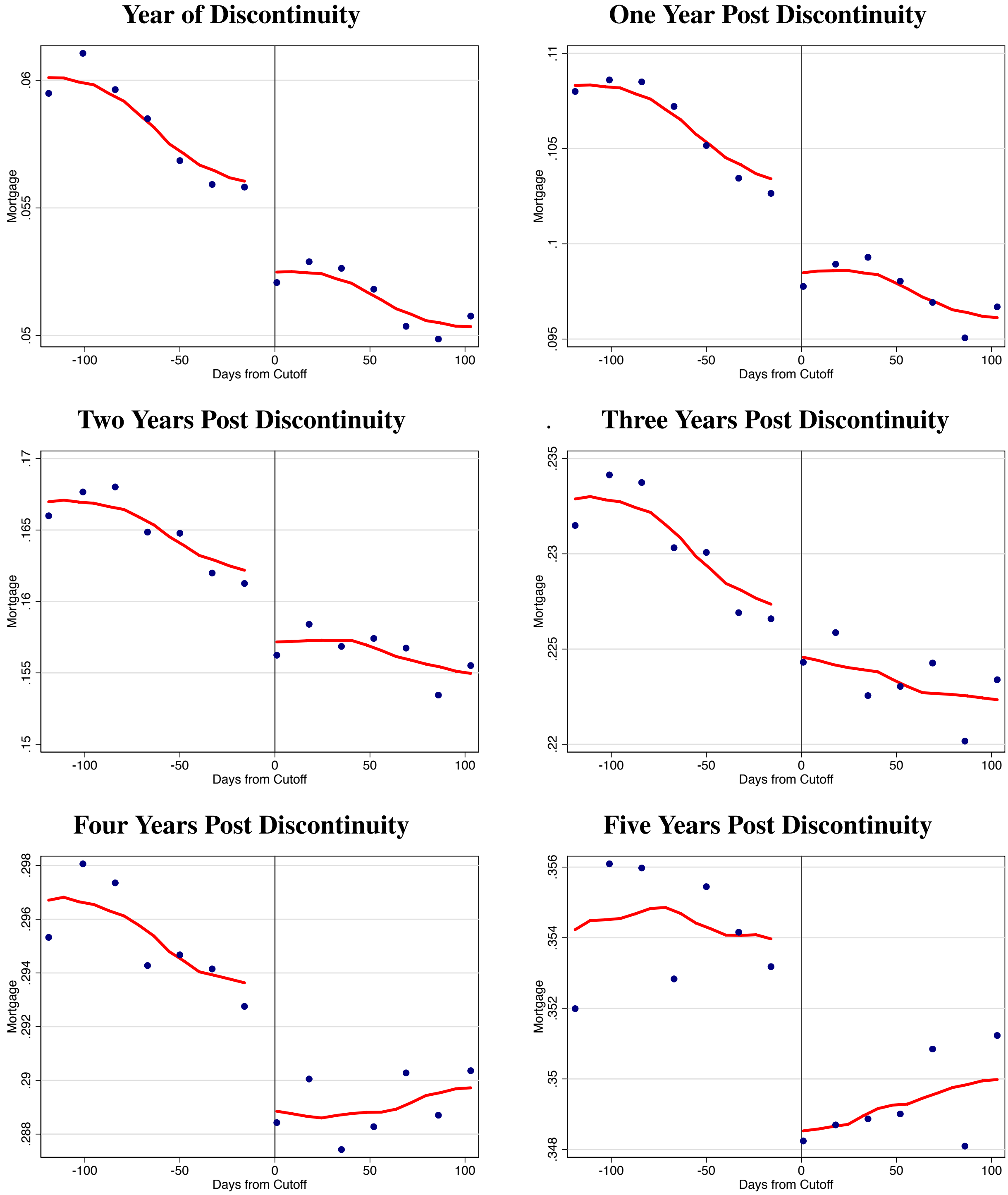

Figure 4: Alternative Bandwidth for Homeownership

Notes: This figure shows point estimates and a 95% confidence interval from the regression discontinuity design, varying bandwidth. The horizontal axis denotes the assigned bandwidth. Specifications include a quadratic spline of the assignment variable, which is the number of days from turning 24 in the calendar year of the discontinuity. Standard errors are clustered at the day from cutoff level. All data come from the NSLDS matched to IRS tax data.

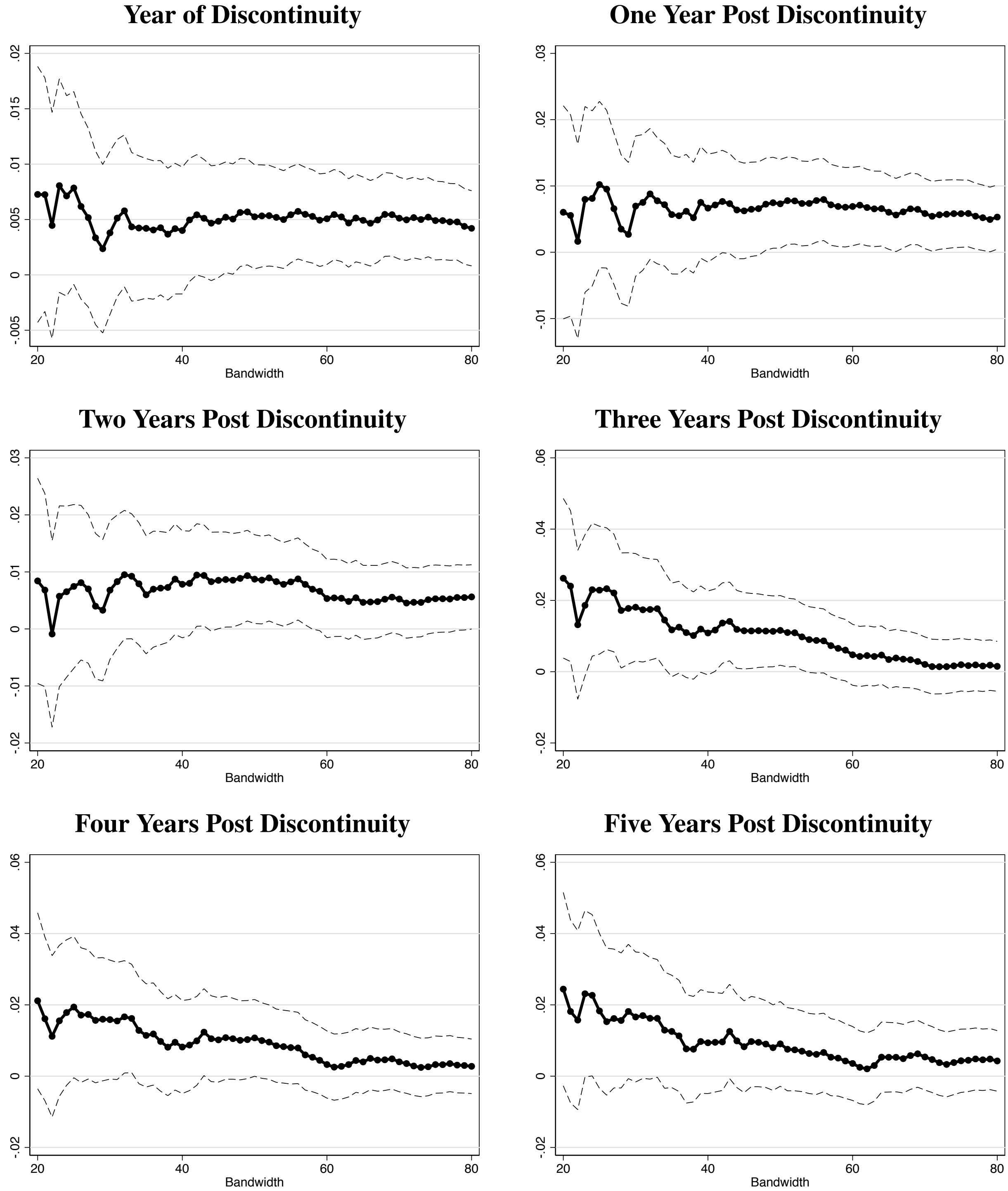

Figure 5: Human Capital and Savings

Notes: This figure shows mean outcomes in 20-day bins of the assignment variable in the year of the discontinuity, three years after the year of the discontinuity, and five years after the year of the discontinuity. The dependent variable in the first column is wage earnings. The dependent variable in the second column is an indicator denoting whether wage earnings are reported on behalf of an individual. The dependent variable in the third column is an indicator of whether an individual has interest or dividend income. The outcome is denoted above each estimate. The assignment variable is the number of days from turning 24 in the calendar year of the discontinuity. All data come from the NSLDS matched to IRS tax data.

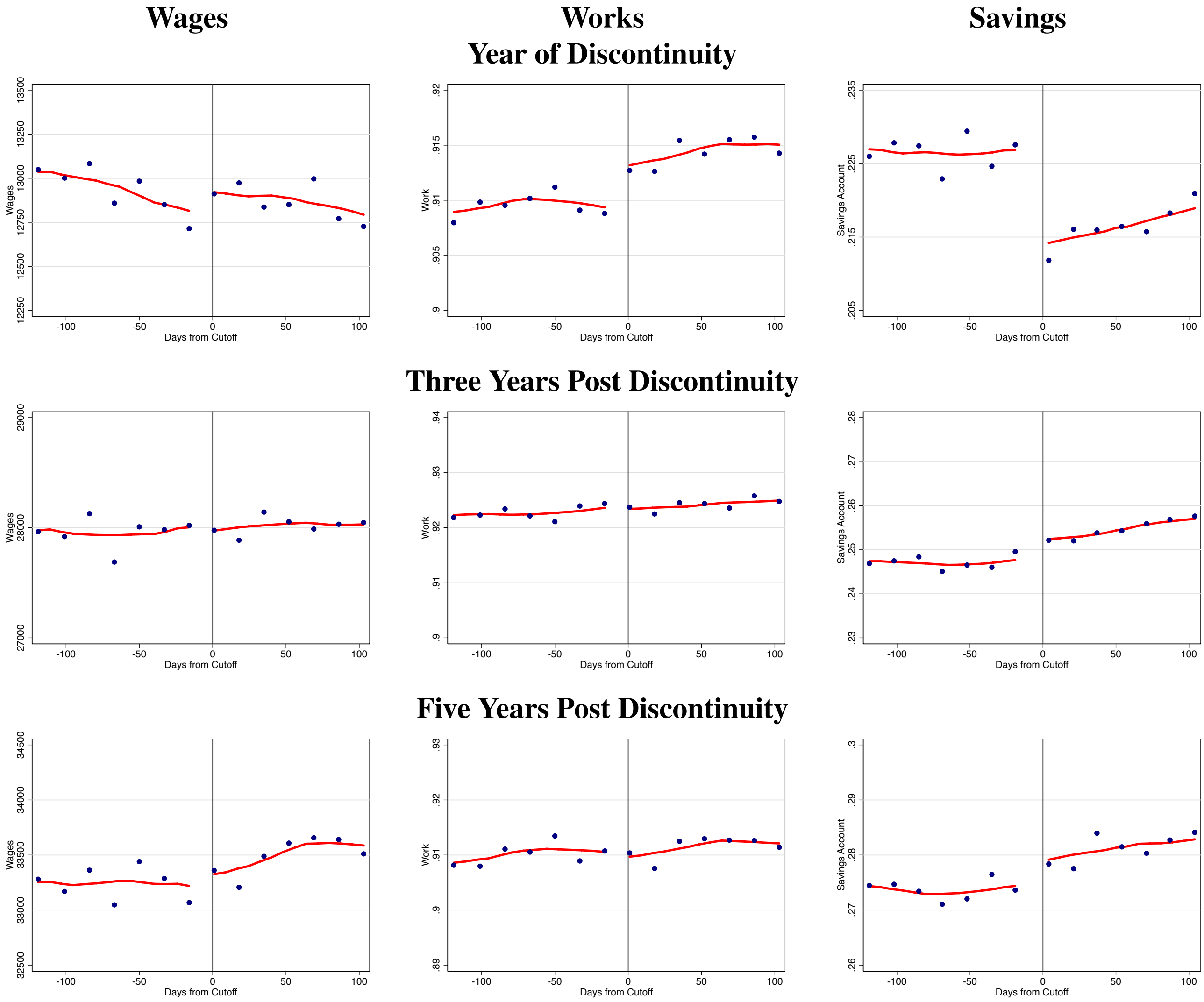

Figure 6: Marriage and Children

Notes: This figure shows mean marriage and children in 20-day bins of the assignment variable. The dependent variable in the first column is an indicator denoting whether an individual is married. The dependent variable in the second column is an indicator of whether an individual has had a child. The outcome is denoted above each estimate. "Children" refers to an indicator of whether a borrower has had any children by that year. The assignment variable is the number of days from turning 24 in the calendar year of the discontinuity. All data come from the NSLDS matched to IRS tax data.

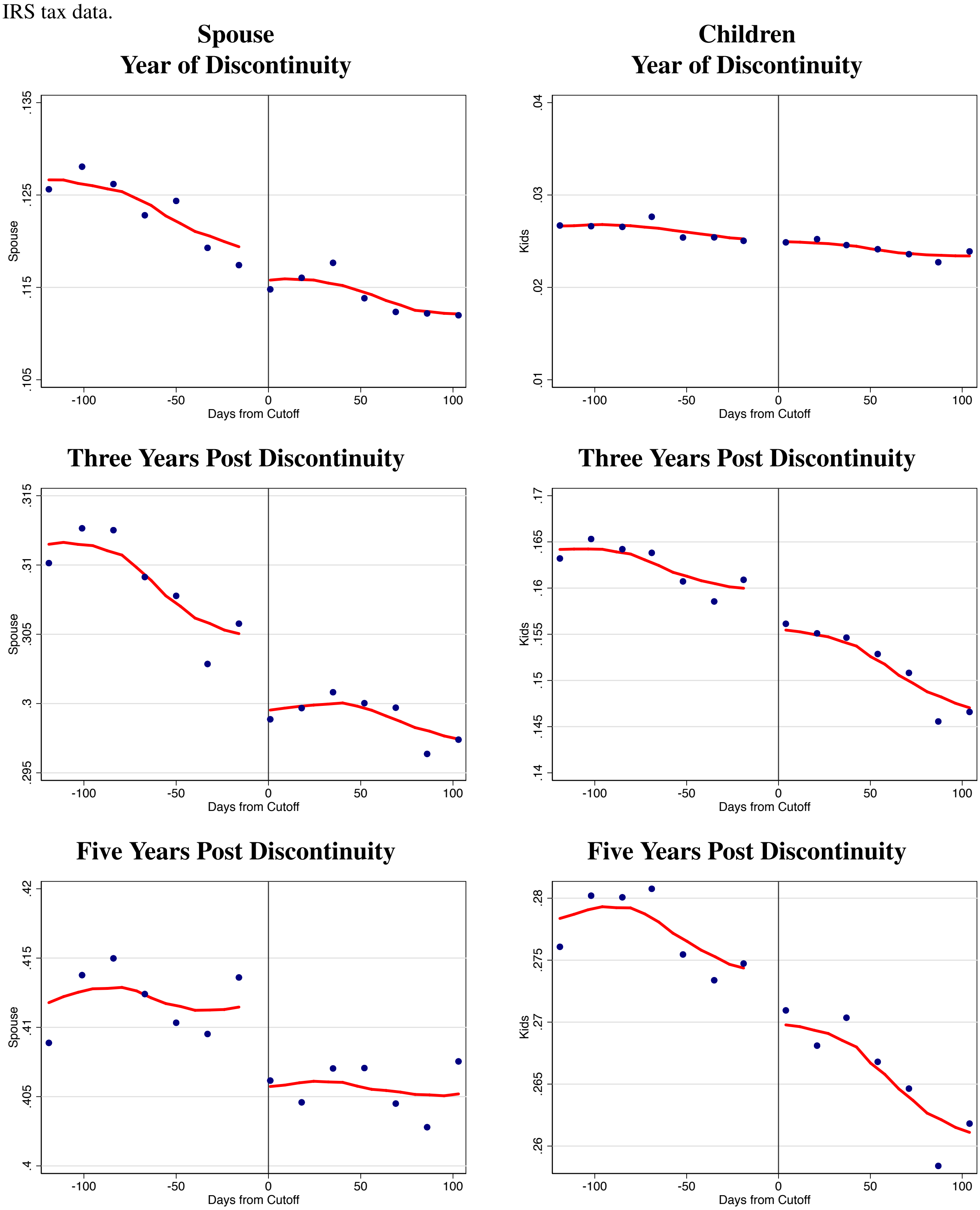

Table 1: Predetermined Covariates

Notes: This table shows regression discontinuity estimates of predetermined outcomes and covariates. Each variable is denoted above the estimates. In general, as time-varying tax variables pertain to calendar years and t-1 measures may partially reflect treatment, balance over such variables is tested using t-2 measures; time-varying education variables pertain to academic years and thus balance over such variables is tested using the prior academic year. (Academic level, four-year, and public enrollment, over which the primary concern would be sorting, are tested using measures in t.) The analysis of whether the borrower filed the FAFSA as an independent before adds those borrowers back in the sample. Although not entirely exogenous, calendar year outcomes are also insignificant in t-1. For example, the estimate on mortgages is -0.0003 with a standard error of 0.0018. Specifications include a quadratic spline of the assignment variable, which is the number of days from turning 24 in the calendar year of the discontinuity. Standard errors are clustered at the day from cutoff level. All data come from the NSLDS matched to IRS tax data. *$p < .1$, ** $p < .05$, *** $p < .01$.

	(1) Need Based Federal Loans	(2) Non-Need Based Federal Loans	(3) Pell Grants
Above Cutoff	-14.71448	75.12886	-56.74685
	(94.59582)	(74.07803)	(86.43317)
	Mortgage	Savings	Family Income
Above Cutoff	.0015612	.0015762	.0096111
	(.0015204)	(.0029899)	(.0149399)
	Works	Wages	Children
Above Cutoff	0002253	313.645	-.0023549
	(.0034927)	(208.1957)	(.0038721)
	US Citizen	Gender	Borrowed
Above Cutoff	.0009799	.0110338*	.0225669
	(.0029496)	(.005637)	(.0233396)
	Acad. Year	Public	Spouse
Above Cutoff	-.0106153	.0049528	-.0013308
	(.0101294)	(.0044997)	(.0110233)
	Independent	Zero EFC	FourYear
Above Cutoff	.004094	.0003425	-.0048781
	(.0044181)	(.005113)	(.0030946)

Table 2: Summary Statistics

Notes: This table shows summary statistics. Each variable is listed in the left-most column, along with the mean, standard deviation, and number of observations. The year after the discontinuity is listed beneath outcome variables. All data come from the NSLDS matched to IRS tax data

		Mean	SD	Observations
Loans		6,777.65	3,635.31	464,008
Need Based		4,019.61	2,528.30	464,008
Non-Need Based		2,758.04	2,886.84	464,008
Pell Grants		1,842.21	1,967.78	464,008
Mortgage				
Year	*0*	0.05	0.23	464,008
	1	0.10	0.30	464,008
	2	0.16	0.36	464,008
	3	0.22	0.41	426,478
	4	0.29	0.45	388,518
	5	0.34	0.48	352,446
Wages				
Year	*0*	12,944.08	12,105.31	464,008
	3	28,140.90	30,181.70	426,478
	5	33,380.50	28,057.64	352,446
Works				
Year	*0*	0.91	0.29	464,008
	3	0.92	0.27	426,478
	5	0.90	0.29	352,446
Savings				
Year	*0*	0.21	0.41	464,008
	3	0.24	0.43	426,478
	5	0.27	0.44	352,446
Spouse				
Year	*0*	0.11	0.32	464,008
	3	0.30	0.46	426,478
	5	0.40	0.49	352,446
Children				
Year	*0*	0.02	0.28	464,008
	3	0.16	0.52	426,478
	5	0.27	0.70	352,446
				Frequency
Academic				
Year	*2*			15.78
	3			22.36
	4			49.10
	5			12.76

Table 3: Federal Loans and Grants

Notes: This table shows regression discontinuity estimates of federal loan and grant receipt in the academic year of the discontinuity, except for the last column which shows total loans and grants over time. Each variable is denoted above the estimates. Specifications include a quadratic spline of the assignment variable, which is the number of days from turning 24 in the calendar year of the discontinuity. All data come from the NSLDS matched to IRS tax data. $*p < .1$, ** $p < .05$, *** $p < .01$.

	Federal Loans and Pell Grants					
	Total Federal Loans	Need Based Federal Loans	Non-Need Based Federal Loans	Pell Grants	Total Loans and Grants	Cumulative Loans and Grants
	(1)	(2)	(3)	(4)	(5)	(6)
Above Cutoff	1,892.918***	1,230.531***	662.38702***	1,332.817***	3,275.697***	3,401.733***
	(44.111)	(39.539)	(48.299)	(24.419)	(58.282)	(230.960)
Observations	464,008	464,008	464,008	464,008	464,008	464,008

Table 4: Homeownership

Notes: This table shows regression discontinuity estimates in the year of the discontinuity and later years. Specifications include a quadratic spline of the assignment variable, which is the number of days from turning 24 in the calendar year of the discontinuity. Standard errors are clustered at the day from cutoff level. All data come from the NSLDS matched to IRS tax data. $*p < .1$, $**\ p < .05$, $***\ p < .01$.

	(1) In Year of Discontinuity	(2) Year After Discontinuity	(3) Two Years After Discontinuity	(4) Three Years After Discontinuity	(5) Four Years After Discontinuity	(6) Five Years After Discontinuity
Above Cutoff	.005245**	.0073083**	.0087283**	0.01159**	.0107327**	.0090582
	(.002384)	(.003398)	(.003952)	(.004962)	(0.005468)	(0.006028)
Observations	464,008	464,008	464,008	426,478	388,518	352,446

Table 5: Homeownership in Placebo Samples

Notes: This table shows regression discontinuity estimates in the year of the discontinuity and later years for groups that were unaffected by the discontinuity. The top panel repeats the analysis for a random sample of individuals who took out loans in other years, but not in the year that they turn 24. The bottom panel shows borrowers who filed the FAFSA as independents before turning 24, but otherwise fit our sample selection criteria. Specifications include a quadratic spline of the assignment variable, which is the number of days from turning 24 in the calendar year of the discontinuity. Standard errors are clustered at the day from cutoff level. All data come from the NSLDS matched to IRS tax data. $*p < .1$, $**\ p < .05$, $***\ p < .01$.

	Placebo Tests					
	(1)	(2)	(3)	(4)	(5)	(6)
	Only Borrowed in Other Years					
	Discontinuity Year	One Year Later	Two Years Later	Three Years Later	Four Years Later	Five Years Later
Above Cutoff	-.0024258	.0016761	.0020467	.0025766	.004295	.0031148
	(.0031762)	(.0033849)	(.003683)	(.0041489)	(.0044732)	(.004696)
Observations	714,617	714,617	714,617	664,287	614,685	566,176
	Already Independent					
	Discontinuity Year	One Year Later	Two Years Later	Three Years Later	Four Years Later	Five Years Later
Above Cutoff	-.0007755	-.008914	-.00091	-.0033734	.0013456	.0016249
	(.0086665)	(.0094771)	(.0104252)	(.0111949)	(.0123789)	(.012209)
Observations	124,811	124,811	124,811	112,856	101,491	91,290

Table 6: Human Capital and Savings

Notes: This table shows regression discontinuity estimates in the year of the discontinuity, three years after the year of the discontinuity, and five years after the year of the discontinuity. The dependent variable in the first column is earnings. The dependent variable in the second column is an indicator denoting whether an individual reports any earnings. The dependent variable in the third column is an indicator denoting whether a borrower has interest or dividend income. Specifications include a quadratic spline of the assignment variable, which is the number of days from turning 24 in the calendar year of the discontinuity. Standard errors are clustered at the day from cutoff level. All data come from the NSLDS matched to IRS tax data. $*p < .1$, $**\ p < .05$, $***\ p < .01$.

	Wages (1)	Works (2)	Savings (3)
		Year of Discontinuity	
Above Cutoff	-240.2872*	-.0027539	.0248975***
	(130.9886)	(.0030782)	(.0048689)
Observations	464,008	464,008	464,008
		Three Years Later	
Above Cutoff	-234.3991	-.0018542	-.0055291
	(275.9309)	(.0030866)	(.0050734)
Observations	426,478	426,478	426,478
		Five Years Later	
Above Cutoff	-323.4143	-.0008361	-.0019679
	(338.6448)	(.0037454)	(.0031079)
Observations	352,446	352,446	352,446

Table 7: Homeownership by Zero EFC

Notes: This table shows regression discontinuity estimates in the year of the discontinuity, three years after the year of the discontinuity, and five years after the discontinuity, broken down by a zero or nonzero EFC in the prior year. Zero EFC status reflects differences in family income and wealth and affects grant versus loan availability. About 15% of the sample did not file a FAFSA in the prior year and are excluded from this analysis; the effects on this group are insignificant. The bottom row shows the effect on loans and grants. Specifications include a quadratic spline of the assignment variable, which is the number of days from turning 24 in the calendar year of the discontinuity. Standard errors are clustered at the day from cutoff level. All data come from the NSLDS matched to IRS tax data. $*p < .1$, $**\ p < .05$, $***\ p < .01$.

	(1)	(2)	(3)	(4)	(5)	(6)
		Zero EFC			EFC>0	
	Year of Discontinuity	Three Years After Discontinuity	Five Years After Discontinuity	Year of Discontinuity	Three Years After Discontinuity	Five Years After Discontinuity
Above Cutoff	.0087687*	0.0304815***	0.0190697	.004752	0.0082183	0.0073099
	(.0047483)	(0.0094582)	(0.0134629)	(0.0031791)	(0.0062889)	(0.0066985)
		Effect on Loans and Grants			Effect on Loans and Grants	
	Non-Need Loans	Need Based Loans	Pell Grants	Non-Need Loans	Need Based Loans	Pell Grants
Above Cutoff	1,431.417***	295.528***	209.211***	536.6336***	1,482.192***	1,849.567***
	(71.72867)	(56.49265)	(65.30761)	(55.79867)	(43.86403)	(48.09954)
Observations	90,690	78,038	53,593	302,837	280,232	237,670

Table 8: Availability of Credit: Homeownership by Time Period

Notes: This table shows regression discontinuity estimates in the year of the discontinuity, before and after 2006. The first column shows outcomes in 2006 and earlier years, while the second and third columns show outcomes after and including 2007. The third column reweighs the sample by demographics and academic level. Specifications include a quadratic spline of the assignment variable, which is the number of days from turning 24 in the calendar year of the discontinuity. Standard errors are clustered at the day from cutoff level. All data come from the NSLDS matched to IRS tax data. $*p < .1$, ** $p < .05$, *** $p < .01$.

	(1)	(2)	(3)	(4)	(5)
	2006 and Earlier	2007 and Later	2007 and Later (Weighted)	2007 and Later (Zero EFC)	2007 and Later (Positive EFC)
	Year of Discontinuity	Year of Discontinuity	Year of Discontinuity	Year of Discontinuity	Year of Discontinuity
Above Cutoff	.0006096	.0094545***	.009019***	.0156198***	.0058351
	(.0035807)	(.0031376)	(.003171)	(.0057289)	(.0045034)
Observations	221,513	242,495	242,495	62,940	149,370

Table 9: Family Formation Outcomes

Notes: This table shows regression discontinuity estimates in the year of the discontinuity, three years after the year of the discontinuity, and five years after the year of the discontinuity. The dependent variable in the first column is whether a borrower is married. The dependent variable in the second column is an indicator of whether an individual has had children by the year indicated. Specifications include a quadratic spline of the assignment variable, which is the number of days from turning 24 in the calendar year of the discontinuity. Standard errors are clustered at the day from cutoff level. All data come from the NSLDS matched to IRS tax data. $*p < .1$, ** $p < .05$, *** $p < .01$.

	(1)	(2)
	Year of Discontinuity	
	Married	Children
Above Cutoff	.0004481	.0018403
	(.003638)	(.0015059)
Observations	464,008	464,008
	Three Years Later	
	Married	Children
Above Cutoff	.0115825**	.0073205*
	(.0055916)	(.0041148)
Observations	426,478	426,478
	Five Years Later	
	Married	Children
Above Cutoff	.0138455**	.0130425**
	(.0065783)	(.0061593)
Observations	388,518	388,518

Figure A.1: Sample Construction & Testing for Enrollment Effects

Notes: This figure shows the mean number of students in 20-day bins of the assignment variable in the year of the discontinuity. The outcome is denoted above each estimate. The left panel shows for-profit borrowers, the middle panel shows borrowers at public institutions, and the right panel shows borrowers at private nonprofit institutions. The assignment variable is the number of days from turning 24 in the calendar year of the discontinuity. All data come from IRS tax data. Enrollment data are from 1098-T forms.

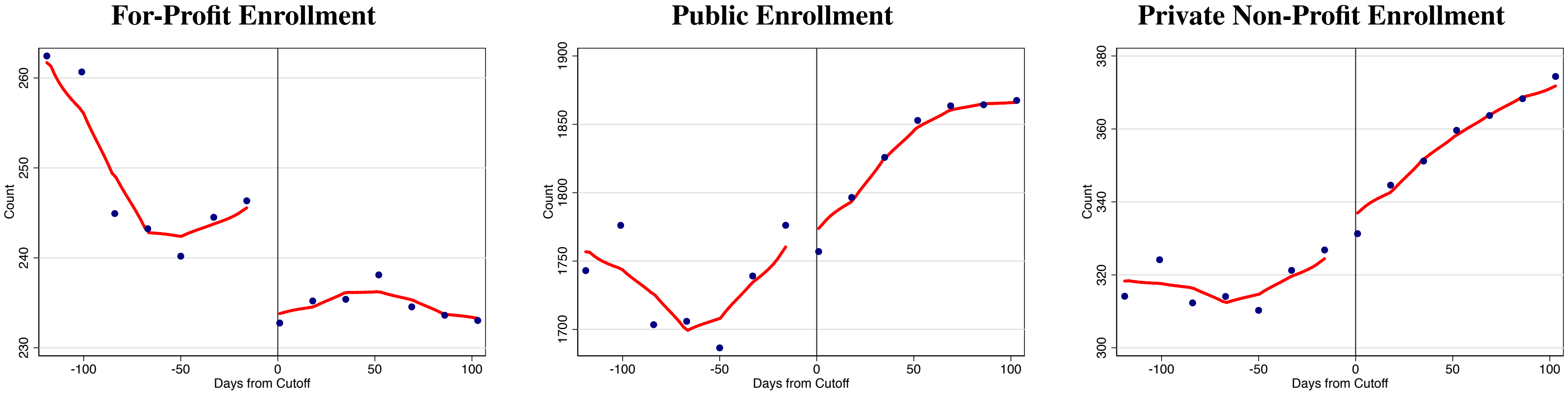

Figure A.2: Density of Borrowers

Notes: This figure shows density of the assignment variable, in bins of 20 days from the dependency cutoff, for the final analysis sample. The assignment variable is the number of days from turning 24 in the calendar year of the discontinuity. Borrowers in the donut hole are excluded, as discussed in the text. The McCrary (2008) test statistic is .1052 (.4956). All data come from the NSLDS matched to IRS tax data.

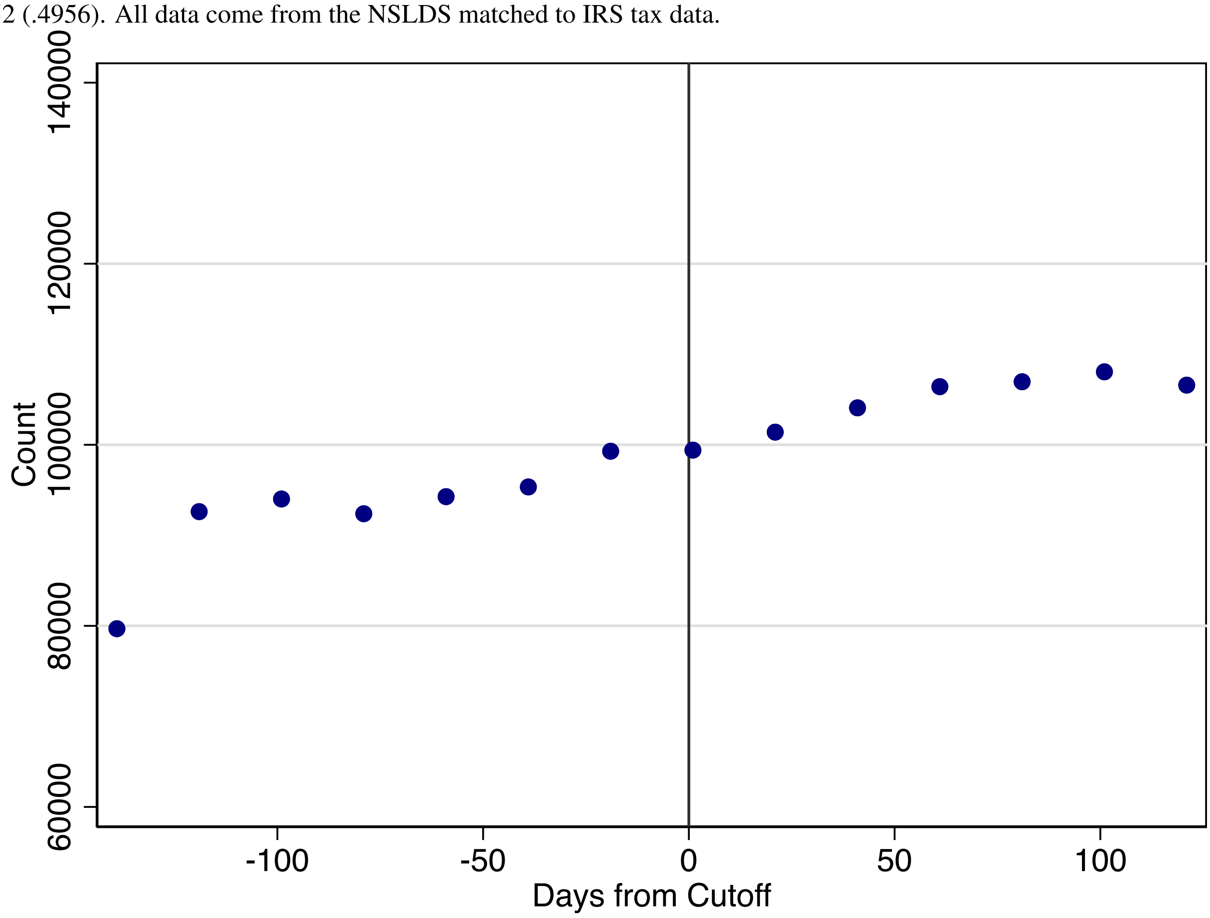

Figure A.3: Predetermined Variables Along Cutoff

Notes: This figure shows mean outcome variables in the pre-period in 20-day bins of the assignment variable. The outcome is denoted above each estimate. The assignment variable is the number of days from turning 24 in the calendar year of the discontinuity. All data come from the NSLDS matched to IRS tax data.

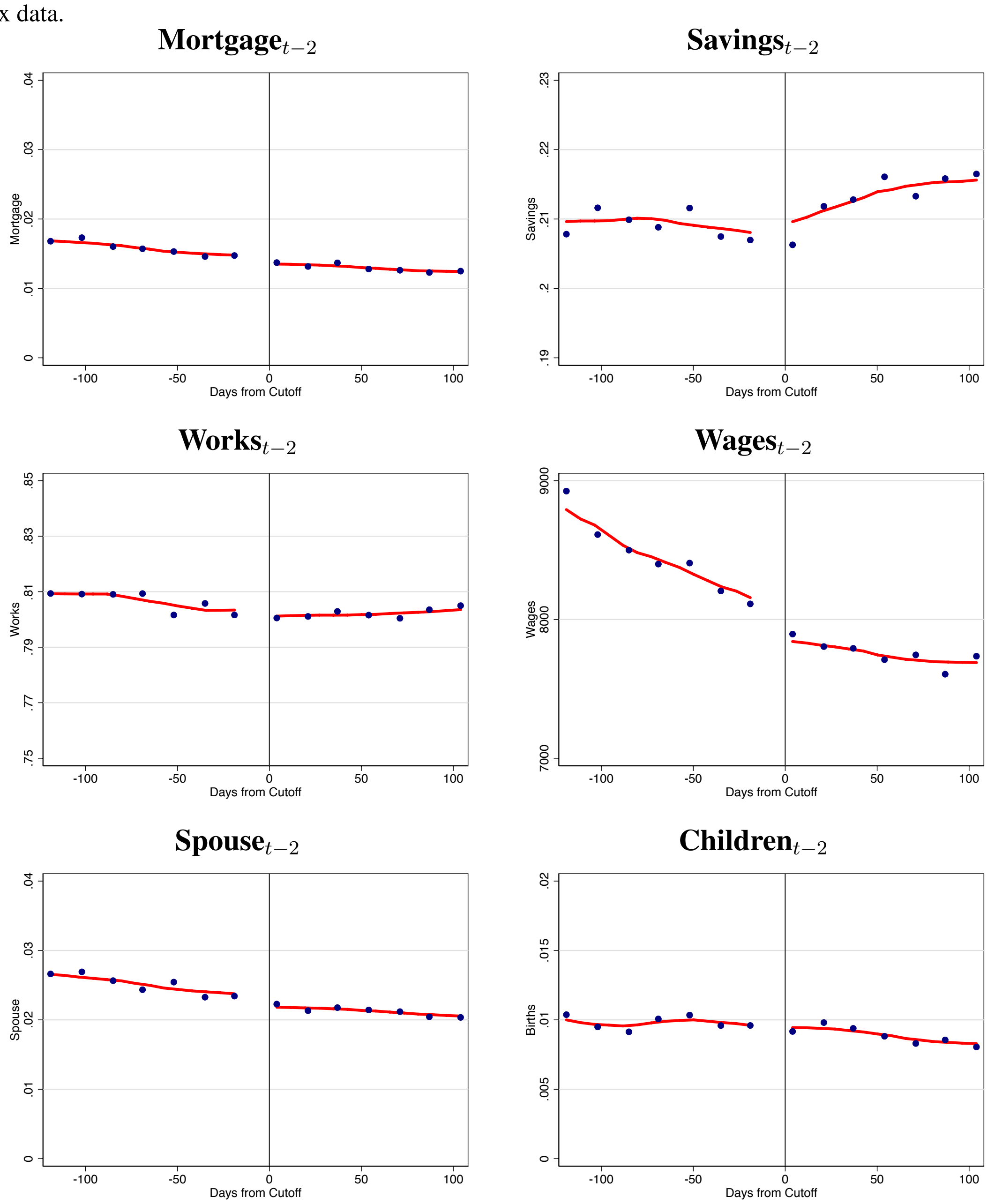

Figure A.4: Alternative Bandwidth for Effect of Limit Increase on Loans and Grants

Notes: This figure shows point estimates for the effect of the limit increase on loans and grants and a 95% confidence interval from the regression discontinuity design, varying bandwidth. The horizontal axis denotes the assigned bandwidth. The outcome is denoted above each estimate. Specifications include a linear spline of the assignment variable, which is the number of days from turning 24 in the calendar year of the discontinuity. Standard errors are clustered at the day from cutoff level. All data come from the NSLDS matched to IRS tax data.

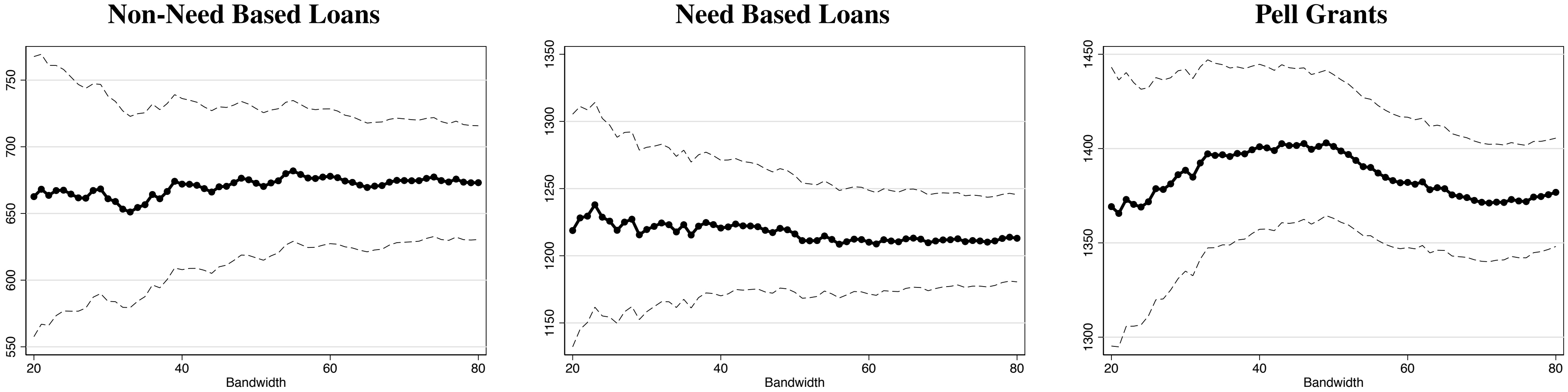

Figure A.5: Alternative Bandwidth for Homeownership Results Using Linear Spline

Notes: This figure shows point estimates and a 95% confidence interval from the regression discontinuity design, varying bandwidth. The horizontal axis denotes the assigned bandwidth. Specifications include a linear spline of the assignment variable, which is the number of days from turning 24 in the calendar year of the discontinuity. Standard errors are clustered at the day from cutoff level. All data come from the NSLDS matched to IRS tax data.

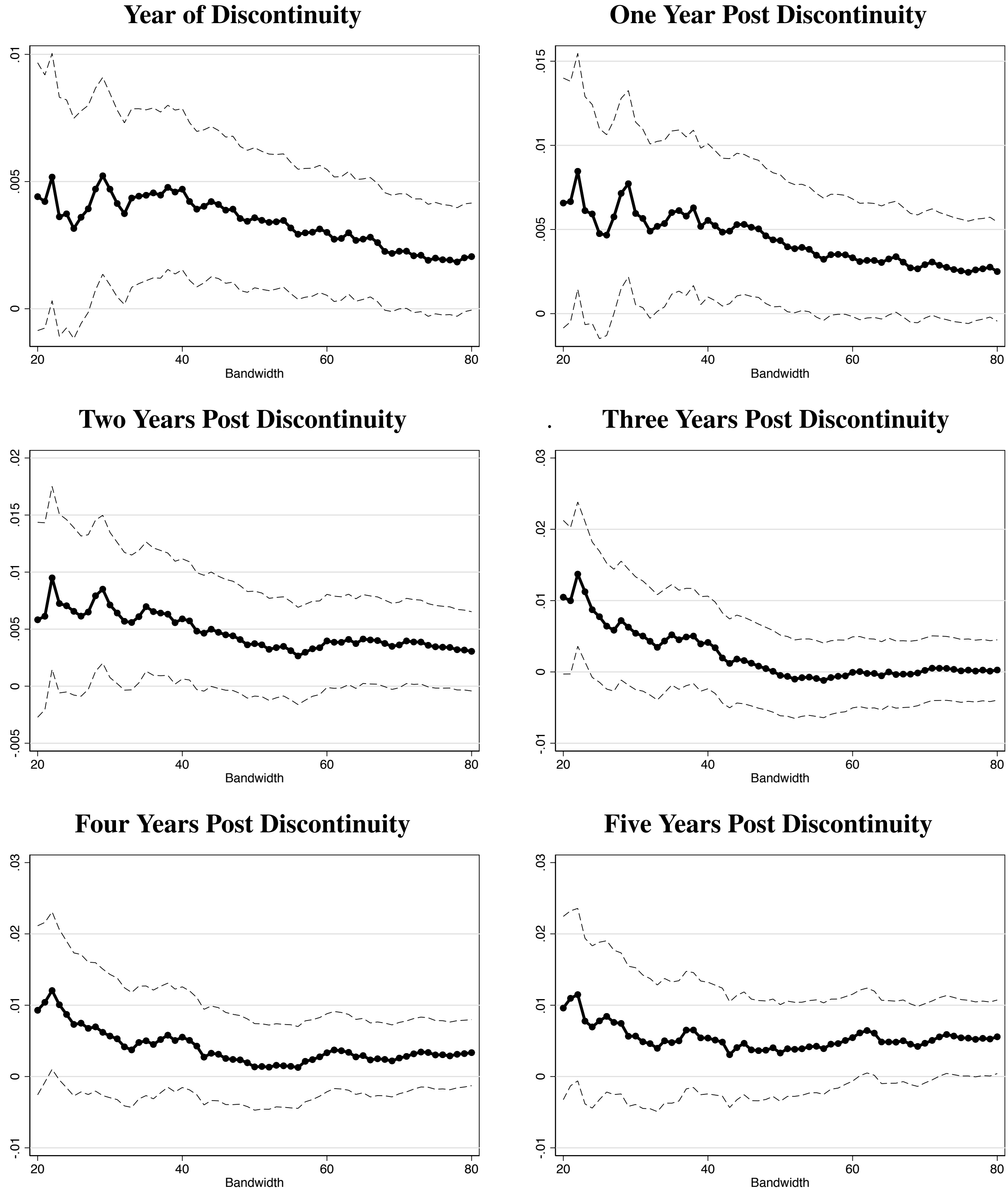

Figure A.6: Placebo Cutoffs: Mortgages

Notes: This figure shows point estimates and a 95% confidence interval from the regression discontinuity design, varying the assigned cutoff. The horizontal axis denotes the assigned cutoff. The outcomes is mortgages in the year listed above each panel. Specifications include a quadratic spline of the assignment variable, which is the number of days from the assigned cutoff. Given the donut hole nature of our design, the placebo discontinuities within three days of the actual discontinuity have the same cutoff but slightly different bandwidths. Standard errors are clustered at the day from cutoff level. All data come from the NSLDS matched to IRS tax data.

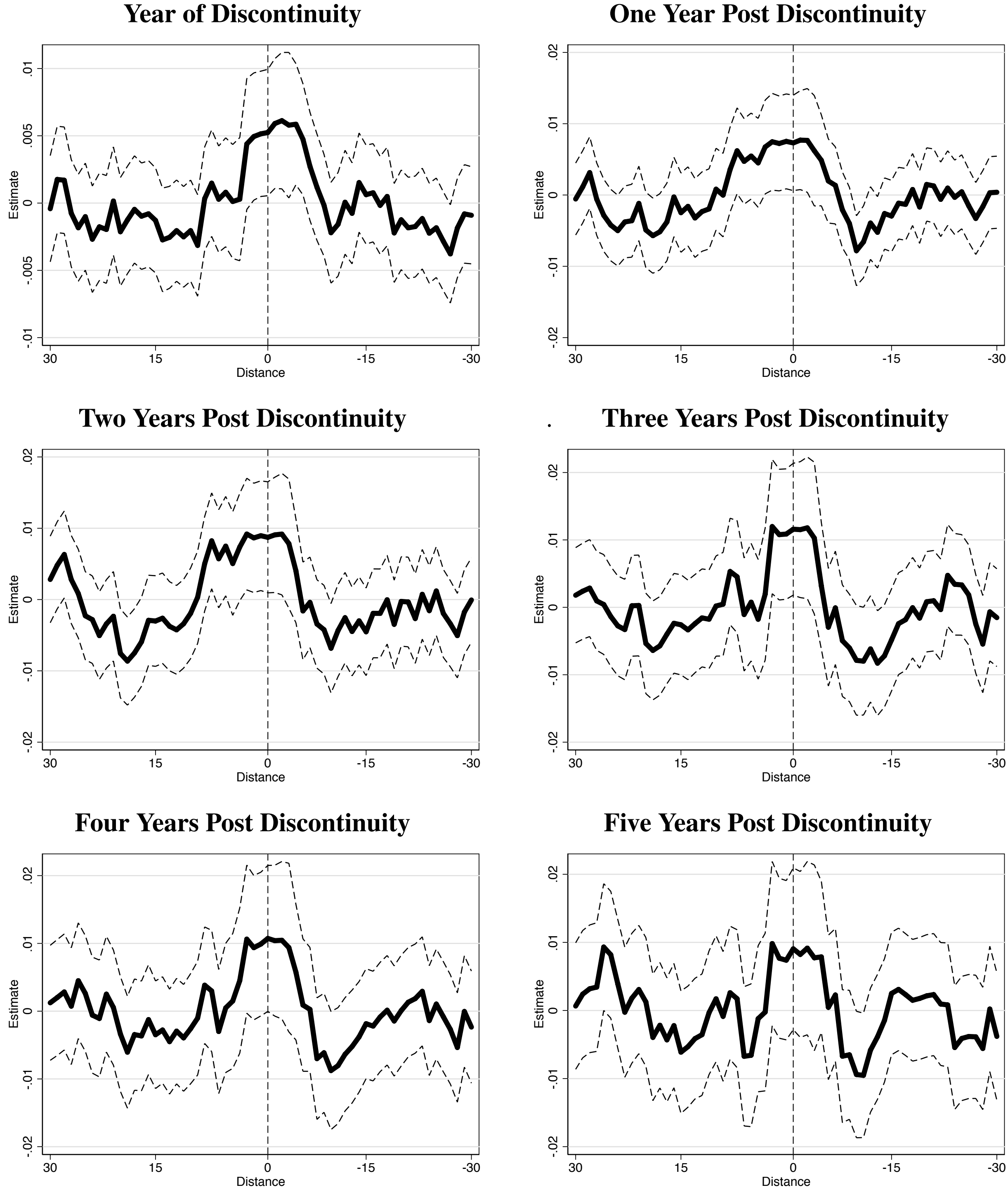

Figure A.7: Fraction of Banks Tightening Consumer Credit

Notes: This figure shows the net percentage of banks tightening standards for consumer loans and credit cards each quarter between 2006 and 2010. The source is the Federal Reserve Bank of St. Louis.

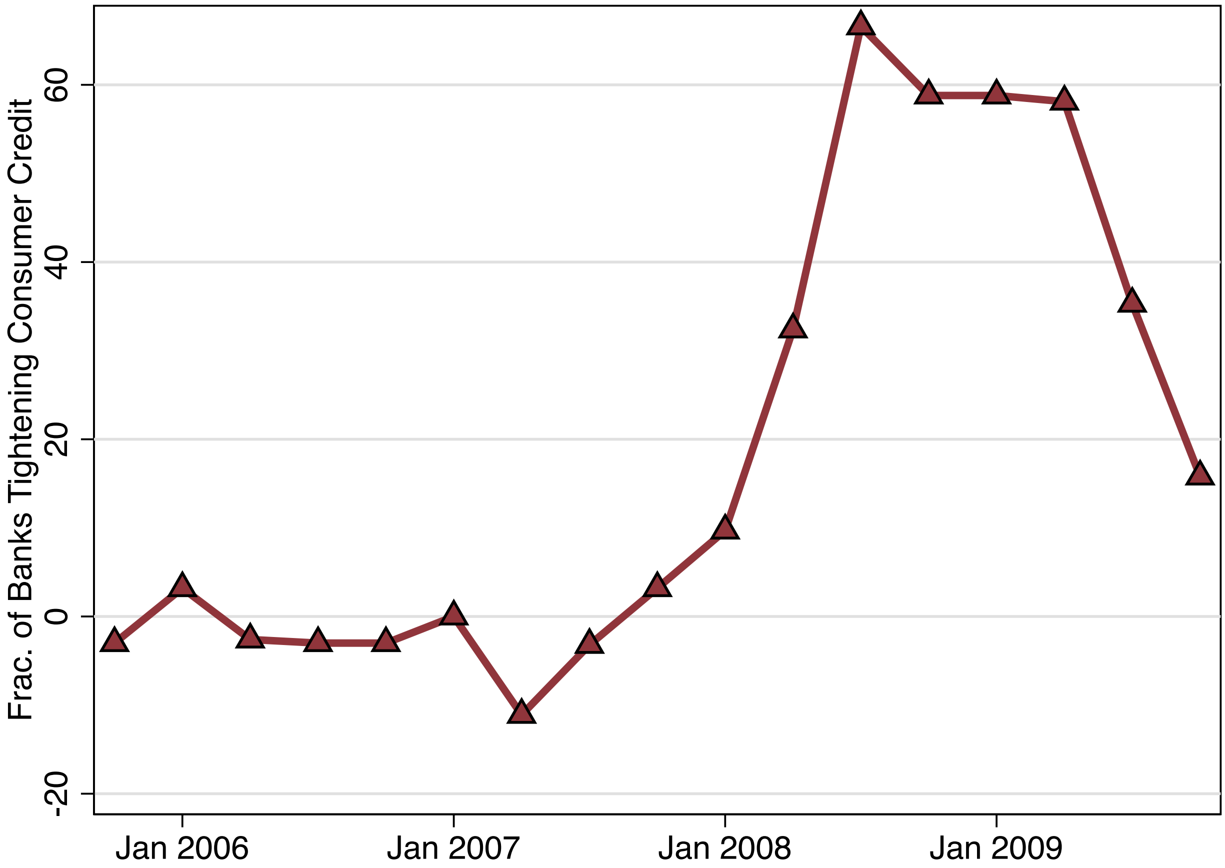

Table A.1: Borrowing Limits for Federal Student Loan Programs

Notes: The table describes the statutory limits for the federal student loan programs since 1994 for need-based and non-need-based loans by dependency status and academic level.

Recent Stafford Loan Limits

Level	Financial Dependency Status: Dependent		Independent	
	Subsidized	Cumulative (Subsidized and Unsubsidized)	Subsidized	Cumulative (Subsidized and Unsubsidized)
	2008-Present			
First Year	$3,500	$5,500	$3,500	$9,500
Second Year	$4,500	$6,500	$4,500	$10,500
Third Year and Above	$5,500	$7,500	$5,500	$12,500
Lifetime	$23,000	$31,000	$23,000	$57,500
	2007-2008			
First Year	$3,500	$3,500	$3,500	$7,500
Second Year	$4,500	$4,500	$4,500	$8,500
Third Year and Above	$5,500	$5,500	$5,500	$10,500
Lifetime	$23,000	$23,000	$23,000	$46,000
	1994-2007			
First Year	$2,625	$2,625	$2,625	$6,625
Second Year	$3,500	$3,500	$3,500	$7,500
Third Year and Above	$5,500	$5,500	$5,500	$10,500
Lifetime	$23,000	$23,000	$23,000	$46,000

Table A.2: Enrollment

Notes: This table shows estimates of the effect of the limit increase on log enrollment. Institution type is denoted above each panel. Data has been collapsed to the day from cutoff. The dependent variable in each specification is the log number of individuals enrolled. Standard errors are clustered at the day from cutoff level. All data come from the NSLDS matched to IRS tax data.

	For-Profit	Public	Private
	(1)	(2)	(3)
Above Cutoff	.0725355***	.0222045	.0225835
	(.0150903)	(.0267475)	(.0277363)
Observations	200	200	200

Table A.3: Testing for Sample Selection

Notes: This table shows regression discontinuity estimates of whether there is any differential selection into being in our sample. The left columns examine selection from tax records for the full U.S. population who turn 24 years old within 50 days of January 1 over our sample period. The right columns examine a subset of this population, excluding those without prior student loans or previously deemed financially independent. Specifications include a polynomial spline of the assignment variable, which is the number of days from turning 24 in the calendar year of the discontinuity. Quadratic or linear spline is noted beneath each specification. Standard errors are clustered at the day from cutoff level. All data come from the NSLDS matched to IRS tax data. $*p < .1$, ** $p < .05$, *** $p < .01$.

	(1)	(2)	(3)	(4)
	Total US Population		Restricted US Population	
Above Cutoff	0.000208	-.000041	-.0017977	-.0030713
	(.0001845)	(0.0003244)	(.0017016)	(.002978)
Polynomial	First	Second	First	Second
Mean	0.0246019		0.3052416	
Observations	19,393,125		1,551,357	

Table A.4: Analysis Variable Descriptions

Notes: This table describes the main analysis variables. The first column presents the variable name. The second column presents a descriptions of the variable. The third column presents the source of the variable. All loan, grant, and earning amounts refer to annual amounts.

Variable	Description	Source
Federal Loans	Total federal Direct and FFEL loans.	NSLDS
Non-Need Based Federal Loans	Total unsubsidized federal Direct and FFEL loans.	NSLDS
Need Based Federal Loans	Total subsidized federal Direct and FFEL loans.	NSLDS
Parent Loans	Total federal PLUS loans.	NSLDS
Pell Grants	Total Pell Grants.	NSLDS
Assignment	Number of days from turning 24 in the calendar year enrolled.	NSLDS
Mortgage	Presence of mortgage interest.	1098 Information Returns
Wages	Labor earnings.	W-2 Information Returns
Works	Presence of labor earnings.	W-2 Information Returns
Enrollment	Indicator of college enrollment.	1098-T Information Returns
Spouse	Indicator of whether married.	Form 1040
Savings	Presence of interest or dividend income.	1099 Information Returns
Children	Indicator of any children.	Social Security Card Applications

Table A.5: Other Sources of College Financing

Notes: This table shows regression discontinuity estimates on federal loans, Pell grants, federal parent PLUS loans and survey based measures of parental support, grants and borrowing. Each variable is denoted above the estimates. The first row shows outcomes from the main analysis sample, the second row shows outcomes from the NPSAS using a first order polynomial spline, while the third row shows outcomes from the NPSAS using a second order polynomial spline. Data in the first row three columns comes from administrative records. Data in the second three columns comes from survey measures in the NPSAS. $*p < .1$, $**\ p < .05$, $***\ p < .01$.

	(1)	(2)	(3)	(4)	(5)	(6)
	Administrative Records			Survey Measures		
	Total Federal Loans	Pell Grants	Parent Plus	Family Support (non-PLUS)	Cumulative Loans (less private)	Cumulative Loans (incl. private)
Main Analysis Sample	1892.918***	1332.817***	-864.9811***	-	-	-
N=464,008	(44.111)	(24.419)	(36.11433)	-	-	-
NPSAS Sample, 1st Order	1617.408***	1184.21***	-745.7251**	717.6914	1622.158**	1810.837*
N=1,217	(497.8358)	(258.7707)	(291.9965)	(503.9739)	(647.9044)	(1052.87)
NPSAS Sample, 2nd Order	1987.136***	1235.694***	-232.6972	843.6764	2295.075***	2691.484*
N=1,217	(640.1389)	(439.8177)	(445.3329)	(507.3981)	(728.4946)	(1412.145)

Table A.6: Homeownership Results Including Borrowers Excluded from Main Sample

Notes: This table shows regression discontinuity estimates in the year of the discontinuity and later years, relaxing restrictions taken in the analysis sample. The top panel includes the donut hole excluded in the main analysis. The second panel includes borrowers at for-profit colleges. The third panel includes borrowers who had no prior loans or were in their first year. The final panel relaxes all three restrictions and includes all borrowers. Specifications include a quadratic spline of the assignment variable, which is the number of days from turning 24 in the calendar year of the discontinuity. Standard errors are clustered at the day from cutoff level. All data come from the NSLDS matched to IRS tax data. $^{*}p < .1$, $^{**}\ p < .05$, $^{***}\ p < .01$.

	(1)	(2)	(3)	(4)	(5)	(6)
	Include Donut Hole					
	In Year of Discontinuity	Year After Discontinuity	Two Years After Discontinuity	Three Years After Discontinuity	Four Years After Discontinuity	Five Years After Discontinuity
Above Cutoff	.0050224***	.0072145***	.0114504***	.013719***	.0121326***	.009263**
	(.0001194)	(.0002376)	(.0005971)	(.0006919)	(0.0005302)	(0.0003784)
Observations	494,218	494,218	494,218	454,250	413,868	375,386
	Include For-Profits					
	Discontinuity	Discontinuity	Discontinuity	Discontinuity	Discontinuity	Discontinuity
Above Cutoff	.0044233*	.006627 **	.0077805**	.0111825**	.0097381*	.008282
	(.0023208)	(.0032517)	(.0038558)	(.0047388)	(.0052027)	(.0057826)
Observations	515,855	515,855	515,855	473,825	430,958	389,679
	Include No Prior Loans					
	In Year of Discontinuity	Year After Discontinuity	Two Years After Discontinuity	Three Years After Discontinuity	Four Years After Discontinuity	Five Years After Discontinuity
Above Cutoff	.0025324	.0057616**	.0084839**	.0122136***	.0100768**	.0084209*
	(.0021418)	(.0027603)	(.0034366)	(.0042491)	(.0046514)	(.0050528)
Observations	655,006	655,006	655,006	602,895	548,765	496,855
	Relax All Restrictions					
	In Year of Discontinuity	Year After Discontinuity	Two Years After Discontinuity	Three Years After Discontinuity	Four Years After Discontinuity	Five Years After Discontinuity
Above Cutoff	.0033181**	.0064727***	.0096369***	.0121118***	.0114786***	.0112692***
	(.0014277)	(.001815)	(.0023015)	(.0027475)	(.0032212)	(.0035509)
Observations	899,514	899,514	899,514	830,421	756,993	683,298

Table A.7: Homeownership with Controls

Notes: This table shows regression discontinuity estimates in the year of the discontinuity and later years, including control variables. Control variables include lagged borrowing, earnings, marital status, children, homeownership and contemporaneous gender, citizenship status and school type. Specifications include a quadratic spline of the assignment variable, which is the number of days from turning 24 in the calendar year of the discontinuity. Standard errors are clustered at the day from cutoff level. All data come from the NSLDS matched to IRS tax data. $*p < .1$, $**\ p < .05$, $***\ p < .01$.

	(1) In Year of Discontinuity	(2) Year After Discontinuity	(3) Two Years After Discontinuity	(4) Three Years After Discontinuity	(5) Four Years After Discontinuity	(6) Five Years After Discontinuity
Above Cutoff	.005179***	.0069087**	.0080355**	.0108852**	.009732*	.0079784
	(.0018136)	(.0030654)	(.0037968)	(.0048442)	(.0054011)	(.0059744)
Observations	464,008	464,008	464,008	426,478	388,518	352,446

Table A.8: Robustness of Homeownership Results to Linear Spline

Notes: The top column shows results in the year of the discontinuity, using a linear rather than a quadratic spline. Standard errors are clustered at the day from cutoff level. All data come from the NSLDS matched to IRS tax data. $*p < .1$, $**\ p < .05$, $***\ p < .01$.

	(1) In Year of Discontinuity	(2) Year After Discontinuity	(3) Two Years After Discontinuity	(4) Three Years After Discontinuity	(5) Four Years After Discontinuity	(6) Five Years After Discontinuity
Above Cutoff	0.004700***	0.005541**	0.0058992**	0.0041345	0.0055215	0.0053953
	(0.001607)	(0.002310)	(0.0026716)	(0.0032946)	(0.0035828)	(0.0039755)
Observations	364,451	364,451	364,451	334,896	305,030	276,809

Table A.9: Human Capital and Savings Over the Near Term

Notes: This table presents near-term human capital and savings outcomes. The dependent variable is listed above each specification. The first column presents results in the year of the discontinuity, and the second column presents results one year later. Specifications include a quadratic spline of the assignment variable, which is the number of days from turning 24 in the calendar year of the discontinuity. Standard errors are clustered at the day from cutoff level. All data come from the NSLDS matched to IRS tax data. $*p < .1$, ** $p < .05$, *** $p < .01$.

	(1) In Year of Discontinuity	(2) Year After Discontinuity
	Wages	
Above Cutoff	-240.2872*	-92.09363
	(130.9886)	(178.074)
	Works	
Above Cutoff	-0.0027539	0.001999
	(0.0030782)	(0.002867)
	Enroll	
Above Cutoff	-0.002054	0.002841
	(0.0044338)	(0.00549)
	Enroll at Least Half Time	
Above Cutoff	-0.0015745	0.006798
	(0.0049517)	(0.0054893)
	Savings	
Above Cutoff	0.0248975***	0.007189
	(0.0048689)	(0.00492)
	Amount of Savings	
Above Cutoff	-0.9384397	0.240176
	(1.928757)	(1.966781)
Observations	464,008	464,008

Table A.10: Human Capital and Savings Robustness to Linear Spline

Notes: This table presents alternative specifications for the main labor market outcome results. The table shows regression discontinuity estimates in the year of the discontinuity, three years after the discontinuity and five years after the discontinuity. The dependent variable in the top panel is earnings. The dependent variable in the middle panel is an indicator denoting whether an individual reports any earnings. The dependent variable in the bottom panel is whether a borrower has interest or dividend income. Specifications include a linear spline of the assignment variable, which is the number of days from turning 24 in the calendar year of the discontinuity. Standard errors are clustered at the day from cutoff level. All data come from the NSLDS matched to IRS tax data. $*p < .1$, ** $p < .05$, *** $p < .01$.

	Wages		
	(1)	(2)	(3)
	In Year of Discontinuity	Three Years After Discontinuity	Five Years After Discontinuity
Above Cutoff	-322.0075***	3.165113	-301.6173
	(86.28787)	(202.5779)	(252.9594)
Observations	364,451	334,896	276,809
	Work		
	(1)	(2)	(3)
	In Year of Discontinuity	Three Years After Discontinuity	Five Years After Discontinuity
Above Cutoff	-0.0042647**	0.0006177	0.0018731
	(.0021228)	(.002096)	(.0024387)
Observations	364,451	334,896	276,809
	Savings		
	(1)	(2)	(3)
	In Year of Discontinuity	Three Years After Discontinuity	Five Years After Discontinuity
Above Cutoff	0.0185142***	-0.0022659	-0.0040763
	(.0030781)	(.0033147)	(.0036196)
Observations	364,451	334,896	276,809

Table A.11: Completion Estimates

Notes: This table shows regression discontinuity estimates in the year of the discontinuity and later years. The dependent variable is an indicator denoting whether an individual completes a degree. Specifications include a quadratic spline of the assignment variable, which is the number of days from turning 24 in the calendar year of the discontinuity. Standard errors are clustered at the day from cutoff level. All data come from the NSLDS matched to IRS tax data. $*p < .1$, $**\ p < .05$, $***\ p < .01$.

	(1) In Year of Discontinuity	(2) Year After Discontinuity	(3) Two Years After Discontinuity	(4) Three Years After Discontinuity	(5) Four Years After Discontinuity	(6) Five Years After Discontinuity
Above Cutoff	0.0028051	0.0021568	0.0065377	0.0080338	0.0066127	0.0060678
	(0.0053655)	(0.0058244)	(0.0057768)	(0.0059684)	(0.0060088)	(0.0063801)
Observations	464,008	464,008	464,008	426,478	388,518	352,446

	Zero EFC			EFC>0		
	Year of Discontinuity	Three Years After Discontinuity	Five Years After Discontinuity	Year of Discontinuity	Three Years After Discontinuity	Five Years After Discontinuity
Above Cutoff	-0.0148086	0.0117859	0.0124152	0.0094213	0.0091234	0.0062582
	(0.0128633)	(0.015788)	(0.0181966)	(0.0067621)	(0.0068237)	(0.0073181)
Observations	83,989	83,989	83,989	116,310	116,310	116,310

Table A.12: Homeownership by School Sector

Notes: This table presents the main results, by school type. The first column presents results for public schools granting four-year degrees, the second column presents results for private nonprofit schools granting four-year degrees, and the third column presents results for nonprofit schools granting two year degrees, Specifications include a quadratic spline of the assignment variable, which is the number of days from turning 24 in the calendar year of the discontinuity. Standard errors are clustered at the day from cutoff level. All data come from the NSLDS matched to IRS tax data. $*p < .1$, $** p < .05$, $*** p < .01$.

		Public	Private	Comm. College
		(1)	(2)	(3)
Mortgage				
Year	*0*	.0051635*	.0013525	.0161729
		(.0027335)	(.0056301)	(.0118298)
	1	.0054716	.008545	.0211923
		(.0037503)	(.0073645)	(.0144806)
	2	.0072544	.0076952	.0258244
		(.0045175)	(.008854)	(.0170883)
	3	.0103724*	.0087086	.0315956
		(.0056664)	(.0101194)	(.0203247)
	4	.0117386*	-.0005293	.0354505
		(0.0062921)	(.0118479)	(.02282453)
	5	.0095257	.0051272	.0182377
		(.0070388)	(.0127207)	(.0272335)
Observations		337,745	94,157	32,106

Table A.13: Federal Loans and Grants by School Sector

Notes: This table presents limit increase results by school type. The first column presents results for public schools granting four-year degrees, the second column presents results for private nonprofit schools granting four-year degrees, and the third column presents results for nonprofit schools granting two-year degrees, Specifications include a quadratic spline of the assignment variable, which is the number of days from turning 24 in the calendar year of the discontinuity. Standard errors are clustered at the day from cutoff level. All data come from the NSLDS matched to IRS tax data. $*p < .1$, ** $p < .05$, *** $p < .01$.

	Public	Private	Comm. College
	(1)	(2)	(3)
Total Loans	1,866.153***	2,365.588***	1,010.127***
	(49.12869)	(99.82655)	(123.0574)
Total Grants	1,427.308***	1,398.384***	905.9641***
	(35.48191)	(52.55398)	(88.82216)
Observations	337,745	94,157	32,106

Table A.14: Family Formation Outcomes Robustness

Notes: This table presents alternative specifications for the main family formation results. The table shows regression discontinuity estimates in the year of the discontinuity, three years after the year of the discontinuity and five years after the discontinuity. The dependent variable in the top panel is an indicator of whether a borrower is married. The dependent variable in the second column is an indicator denoting whether an individual has had any children by that year. Specifications include a linear spline of the assignment variable, which is the number of days from turning 24 in the calendar year of the discontinuity. Standard errors are clustered at the day from cutoff level. All data come from the NSLDS matched to IRS tax data. $*p < .1$, ** $p < .05$, *** $p < .01$.

	Marriage		
	(1)	(2)	(3)
	In Year of Discontinuity	Three Years After Discontinuity	Five Years After Discontinuity
Above Cutoff	-0.0019367	0.006721*	0.0098406**
	(.0019192)	(.003609)	(.0043012)
Observations	364,451	334,896	276,809

	Children		
	(1)	(2)	(3)
	In Year of Discontinuity	Three Years After Discontinuity	Five Years After Discontinuity
Above Cutoff	0.00024714	0.00443186*	0.00841002**
	(.00099316)	(.00262697)	(.00403502)
Observations	364,451	334,896	276,809