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 that may also serve as a financing channel for non-education activities for students whose liquidity is otherwise restricted. This paper explores this possibility, using administrative data for the universe of federal student loan borrowers, linked to tax records, to examine the short- and medium-term effects of a discontinuity in program limits—generated by the timing of a student borrower's 24[th] birthday—on household formation. We estimate an initial jump in federal support and an increase in homeownership that persists over the medium-term. In the year of the discontinuity, borrowers with higher limits also earn less but are more likely to save; however, there are no differences in subsequent years. Finally, effects on family formation (i.e., marriage and fertility) lag homeownership. Altogether, the results appear to be driven by liquidity rather than human capital or wealth effects.

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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 credit. In standard economic models, these programs encourage investment by helping to remove credit constraints from the decision to attend college and subsidizing its cost (Becker 1962; Ben-Porath 1967; Mankiw 1986, Palacios 2014; Ji 2017). However, they may also provide liquidity to students for non-education activities, which enables them to smooth consumption over the lifecycle.

Over the medium-run, the liquidity effects of these programs are theoretically ambiguous.[1] Unlike other types of credit, access does not entail credit underwriting or risk-pricing; however, like most other types, student loan balances and payment histories are reported to credit bureaus and used to calculate credit scores. These features imply two potential modes of transmission. First, there is a direct channel, whereby the government provides liquidity to those who have very little access to credit markets otherwise and who, for example, may need to meet a down payment requirement for a mortgage. Second, there is an indirect channel, whereby increased debt and any resulting imprint on credit files affect future access to credit.[2] In some states of the world— such as when liquidity is restricted only because of short credit histories or collateral constraints—access to additional resources would, on balance, *increase* medium-run spending on non-education activities, with the effect operating through either or both of these channels. In others—e.g., if an individual has high underlying credit risk—additional access would *decrease* such spending, particularly if it leads to early damage to credit scores or debt overhang (Mian and Sufi, 2011; Gorea and Midrigan, 2017).[3] Indeed, the narrative surrounding the second state 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,

[1] This discussion abstracts from potential human capital effects.
[2] This tradeoff is similar to the problem for a firm, in which 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, 2012), myopic (Benartzi and Thaler, 1995), or inattentive (Pagel, 2017).

2016). A recent *New York Times* editorial noted: "*Loan payments are keeping young people from getting on with life, delaying marriage and homeownership*."

Ultimately, which of these effects dominates is an empirical question, best answered by an experiment that randomly assigns students access to additional resources through these programs and compares their medium-run outcomes. In this paper, we attempt to approximate this experiment via a Department of Education (DoEd) policy rule that classifies undergraduates applying for financial aid as either financially 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 that they enroll are considered independent, which generally induces a higher limit.[5] Linking administrative federal student loan, grant, and tax records, we exploit the sharp discontinuity in limits that this rule generates over student borrowers' birthdates to examine the effects of access to additional resources through these programs on household formation—primarily homeownership but also earnings, savings, marriage, and fertility—in the year of the discontinuity and up to five years later. The intuition behind our regression discontinuity design (RDD) is that, among those whose 24th birthdays are in the vicinity of January 1, borrowers are effectively randomly assigned higher or lower program limits.[6]

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. Turning to our main finding, the discontinuity induces an immediate 0.5 percentage point (p.p.) jump in homeownership. This effect is extremely robust, with no evidence of effects among same-aged placebo populations that should not be affected by the policy rule. It also

[4] The use of the term "limit" throughout this paper is related to but differs from the statutory loan limits published by the DoEd. The latter refers to the maximum amount of each major type of federal loan that may be borrowed in a particular academic year as a function of a student's academic level and dependency status. 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 she faces as well as her expected family contribution. (This figure measures financial "need" according to a complex formula that weights its inputs differently according to the student's dependency status. It is a direct determinant of loan and grant eligibility used by both the DoEd and academic institutions.)

[5] The sample is comprised of borrowers attending public and private nonprofit institutions. In 2011-2012, 59 percent of undergraduates were receiving Federal support and 44 percent were over 24 years old; thus, this variation affects a substantial fraction of college students. 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.

generally persists through the medium-term, and is later echoed by an increase in family formation (i.e., marriage and fertility).

We leverage additional aspects of our design to explore whether increased liquidity, human capital, or wealth best explains these results. Namely, we examine labor market and savings responses, as well as potential sources of heterogeneity, and find the most empirical support for a liquidity explanation. First, borrowers with higher limits have marginally lower earnings in the year of the discontinuity, consistent with additional financial resources acquired through these programs modestly helping students meet expenses or discretionary spending while enrolled, though the reduction offsets little of the much larger increase in federal support. In subsequent years, estimates are indistinguishable from zero and can rule out more than modest effects, suggesting that general human capital effects are negligible.[7] Second, 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 towards education as well as these increased resources being quickly spent down (reflective of financial constraints). Then, we find that the homeownership effect is concentrated among borrowers who are particularly liquidity constrained and who see little or no change in grants. Specifically, the effects are driven by borrowers with an Expected Family Contribution (EFC) of 0 in the prior year, who tend to both be more financially constrained (as their families have relatively low income and assets) and experience smaller changes in subsidy.[8] In addition, the effects are largest, and still concentrated among borrowers with an EFC of 0, 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 an entirely novel result, which

[7] While we examine key labor market outcomes (e.g., earnings, participation), the empirical design focuses on inframarginal changes in limits. Indeed, within our sample, we find little evidence of college enrollment or completion effects. Altogether, our findings imply that any human capital effects in our setting are modest and cannot explain the main estimates. There may still be first-order human capital effects of these federal programs in other settings.

[8] Increases in subsidy are exclusively driven by decreases in EFC, but zero-EFC borrowers are already at the lower bound; thus, the increase in federal support for this group is driven by increased borrowing stemming from the higher borrowing limit.

suggests that these programs represent a crucial credit instrument for this demographic. In addition, the formulation of the analysis sample generates a novel finding with respect to enrollment, specifically that marginal loan and grant dollars raise attendance only at for-profit institutions, at least among individuals on the cusp of turning 24 years old.[9]

The results also expand several literatures. First, a body of work investigates the determinants of household formation, both generally (e.g., Paciorek, 2016) and among young adults (Bhutta, 2013; Martins and Villanueve, 2009). The most related studies in this area find roles for liquidity and the availability of credit (Campbell and Cocco, 2003; DeFusco, Johnson, and Mondragon, 2017; Gorea and Midrigan, 2017; Mian and Sufi, 2011, 2015). Other related work finds some evidence that consumer debt burdens, sometimes focusing on student loans, *reduce* household formation; to arrive at their conclusions, these studies generally attempt to compare individuals who are similar on all dimensions except for the debt that they have accumulated, a conceptually different experiment than ours (Bleemer et al., 2014 and 2017; Mezza et al., 2016; Dettling and Hsu, 2017; Chiteji, 2007).[10]

A second, related literature, motivated by the canonical permanent-income hypothesis, evaluates whether liquidity motivates consumer behaviors more generally. Within this literature, our setting is most similar to work that has examined consumer response to changes in credit availability (e.g., Deaton, 1991; Carroll, 1992; Ludvigson, 1999; Mian, Rao, and Sufi, 2013; Mondragon, 2017; Baker, 2017; Souleles, 1999; Gross and Souleles, 2002). In particular, Gross and Souleles (2002) estimate that spending is quite sensitive to credit card limits and interest rates, across the distribution but particularly among those already close to their limits, evidence that they interpret as most consistent with binding liquidity constraints but also somewhat consistent with buffer-stock models of precautionary savings.

[9] Specifically, to help validate the empirical design, we test for potential extensive margin responses, which could introduce bias into the estimates and confound their interpretation as distinct from human capital effects. Leveraging the full breadth of the tax data, we discover clear enrollment effects at for-profits but negligible effects elsewhere, consistent with the for-profit sector being uniquely adept at identifying and enrolling students that are eligible for more federal financing. In light of this evidence, the analysis sample is restricted to borrowers attending public or private nonprofit community or four-year colleges, among whom causal links can be most confidently identified. The sample is refined along some additional dimensions, described more fully in Section 3.

[10] In particular, these studies do not isolate changes in cash on hand. Mezza et al. (2016) and Bleemer et al. (2017) find negative effects of student debt instrumented by increases in tuition. Dettling and Hsu (2017) and Bleemer et al. (2014) find evidence that less advantageous credit positions affect the probability of living with parents (though Chiteji (2007) does not). We do not view our findings as being necessarily inconsistent with these studies, but rather they imply that any negative effects of debt increases within our setting are dominated by alternative channels (e.g., liquidity effects).

Finally, our results contribute to work examining federal student loan and grant programs. They reveal that these programs enable spending on important non-education activities and increase household formation. However, they also indicate that, within our context, the marginal dollar has a low return on investment: it only raises attendance within a notoriously low-return sector, and, within the much larger public and private nonprofit sectors, earnings do not increase. Avery and Turner (2012) and Looney and Yannelis (2015) describe pertinent aspects of the student loan market. Lochner and Monge-Naranjo (2016) and Palacios (2014) study the theoretical framework for student borrowing and human capital investment. Most of the empirical work investigates the determinants of take-up of these programs and their effects on human capital accumulation (e.g., Dynarski, 2003; Stinebrickner and Stinebrickner, 2008; Lochner and Monge-Naranjo, 2011; Bettinger, Long, Oreopoulos, and Sanbonmatsu, 2012; Dynarski and Scott-Clayton, 2013; Turner and Marx, 2015; Angrist et al., 2017; Cox, 2017a; Denning, 2017; Solis, 2017) . Otherwise, they examine the interplay between such programs and other forms of education financing (e.g., Lucca, Nadauld, and Shen, 2017; Amromin, Eberly, and Mondragon, 2016; Turner, 2017; Cox, 2017b).

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 restrictions, 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, and the Pell Grant Program. The reach of these programs has expanded considerably over the last 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 to 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.[11] Approximately 40% 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 tax forms.[12] 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.[13] 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 via an award letter from the college in which they are enrolled or planning to attend.

Undergraduate loans through the DL and FFEL programs are borrowed funds that must be repaid with interest. The "Stafford Loan," the main brand of such loans, features standardized

[11] While a private student lending market exists, the size of this market has always been considerably dwarfed by the federal lending programs, but 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% of that disbursement was through federal programs.

[12] 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.

[13] 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. Regression analyses using the restricted-access 2007-8 and 2011-2 NPSAS reveal a precisely-estimated negative relationship between EFC and freshman year state and institutional aid: -0.016 (.002). Interactions between the policy rule we leverage in this study and access to/receipt of other forms of financial aid could violate the exclusion restriction necessary to generate 2SLS estimates of the effects of federal student loan and grants on household formation.

terms, a congressionally set interest rate, and a statutory limit.[14] Besides these features, compared to 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.[15] 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 Appendix Table A.2. 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 3.76%, 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. A Pell Grant-eligible student 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.[16] In addition, for a subset of students, it

[14] 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.

[15] 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.

[16] While not the focus of this study, 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. 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 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. Within our analysis, the estimated decrease in PLUS loans among financially independent

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 that are at least 24 years old by the end of the calendar year that they enroll are automatically considered financially independent.[17] 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.[18] In a nationally representative DoEd survey of undergraduates in 2011-2012, 59 percent were receiving Federal support and 44 percent were over 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 actually not today's norm: according to a separate DoEd survey of students that completed a B.A. in 2007-2008, the average time to degree was 6 years, and nearly 40 percent of recipients took more than 5 years. Also within the 2011-2012 survey, 23 percent of undergraduates and 55 percent of Stafford Loan borrowers borrowed at their "individual limit." This rate is reflective of behaviors within each financial dependency group. Specifically, among financially dependent undergraduates, 24 percent borrowed at this limit (pointing to a binding constraint, which is then relaxed by the policy variation), and among financially independent undergraduates, 21 percent did.

B. Data

The analysis relies upon 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. 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

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students represents only a portion of the increase in Stafford Loans, which we interpret as a shift in who bears the burden of a debt load that would have been acquired in either state of the world.

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

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

through the Title IV programs. It includes student loan records from 1969 to the present, 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 make use of 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 2016 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 earnings data from W-2 information returns (filed by employers), enrollment data from 1098-T information returns (filed by colleges), mortgage data from 1098 information returns (filed by lending institutions), and interest and dividend investment income data—to measure savings—from 1099 information returns filed by, for example, depository institutions and asset management companies and which are mandatory for any investment income exceeding $10. We also separately observe marital status from filing form 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 the Form 1040 and other 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 some assumptions described below, we can use this setting to identify the causal effect of higher limits on the federal resources that borrowers receive and household formation. We estimate the following regression discontinuity design (RDD) equation:

$$Y_{it} = \beta_0 + \beta_1 1[D > \bar{D}]_{it} + \beta_2 D_{it} + \beta_3 (1[D > \bar{D}] * D)_{it} + \beta_4 D_{it}^2 + \beta_5 (1[D > \bar{D}] * D^2)_{it} + \gamma_t + \varepsilon_{it}$$

where i and t indexes borrowers and cohorts, respectively. Y 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).[19] $1[D > \bar{D}]$ 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 appendix, we include 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.

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, must 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 through January 1 – which we use to inform our sample restrictions and help validate our design in the next section.

B. Sample Construction and an Exploration of Extensive Margin Responses

The analysis examines federal student loan borrowers who will turn 24 years old within the academic year during which they are borrowing and who had taken student loans in a prior

[19] 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 $t=-2$. Somewhat unsurprisingly, we find null effects in $t=-1$.

academic year.[20] The sample includes individuals who meet these criteria from 1999 to 2016. The rationale for this focus is threefold. One, in general, RDD relies on a comparison between individuals who were exposed to the policy rule and those who were nearly exposed. In theory, for our setting, any one on the cusp of turning 24 years old would qualify, but as many people in that age range do not attend college, centering our analyses around those that do will increase the chance that we have ample statistical power to detect effects. Two, students that borrowed in prior years are less likely to be induced into borrowing by the policy rule, which helps us meet key identification criteria.[21] (This concern is described more fully below and examined thoroughly.) Three, this focus allows us to have more complete records—spanning the union of the tax and student loan data—for our entire sample.

Next, we consider whether we need additional restrictions in order to be able to analyze a sample free of sample selection bias. The primary concern is that the higher limits available to independent students could affect college enrollment decisions or, because we focus on borrowers, the take-up of student loans. Though responses along either of these margins are interesting and may have important policy implications, they could result in additional mass on one side of the discontinuity, a violation of the RDD identification requirement that the distribution be smooth over the range of study.

We consider extensive margin effects by sector. To do so, we first leverage the wider sample that can be formed from the tax data and examine the smoothness of the distribution of individuals in college – i.e., whether the policy rule appears to influence *attendance behavior* on either side of the discontinuity – with both visual and regression-based evidence. Figure 1 shows enrollment effects by sector and indicates that any extensive margin effects appear to be concentrated within the for-profit sector.[22] Table A.3 confirms the visual evidence in a regression

[20] The sample excludes first-year students. We also drop any borrowers deemed financially independent in *prior* years to further increase the share of our sample influenced by the discontinuity, but without introducing selection bias. We later demonstrate in a placebo analysis that borrowers considered independent in a prior year exhibit no statistical difference in outcomes through the threshold.

[21] We find that those who did not borrow in prior years are indeed more likely to borrow in response to higher limits, but there is no evidence that they are more likely to enroll in college.

[22] These differences may imply that for-profit institutions are better at targeting and enrolling students that 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, Yuchtman, Abulafi, Katz, and Goldin, 2016; Cellini and Turner, 2016) and that for-profit institutions better capture federal aid (Cellini and Goldin, 2014; Lucca,

framework matching our main specification. While this finding is of independent interest, to satisfy the continuity assumption, we remove borrowers attending for-profit colleges from our sample.

We make two final sample restrictions for the main analysis, both of which we perturb in our robustness section to demonstrate that they do not materially affect the results. First, following (Denning, 2017; Yannelis, 2016), we drop individuals born within three days of January 1. Dropping this "donut hole" avoids concerns about the retiming of birth due to tax incentives (Schanda and Conlin, 1999, Turner, 2014).[23] Second, to ensure that the group formed from borrowers eligible for higher limits is comparable to the one from those who are not, we use the Calonico, Cattaneo, and Titiunik (2014) method to select the optimal bandwidth of birthdates to include, which leads us to restrict the main sample to those born within 50 days of January 1.[24] Figure 2 plots the density of borrowers for the final sample, subject to all of the above restrictions, which, together with the results of a McCrary test (McCrary, 2008), indicate that the distribution is smooth in the vicinity of the threshold.[25]

C. Sample Description and Balance

Table 1 presents summary statistics, and Appendix Table A.1 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, within 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.

Homeownership increases considerably as the sample ages, from 5 percent in the year of the discontinuity to 34 percent five years later. 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

Nadauld, and Shen, 2017; Turner, 2014). 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; Seftor and Turner, 2002; Turner, 2014; 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.)

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

[24] In the robustness checks in the appendix, we compute the optimal bandwidth using a linear specification.

[25] Figure A.1 shows that the density remains smooth for more refined borrower bins as well.

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, which likely reflects declining enrollment. Note that this pattern is, more generally, consistent with lifecycle earnings profiles. Labor force participation rates, defined as the presence of earnings, essentially do not change with age: around 90% of borrowers have some earnings in each period.

The RDD identification assumption implies that borrowers' predetermined characteristics will be similar across the threshold. The intuition is that, if the policy cutoff approximates random assignment of borrowers, then borrowers should appear to be similar on each side. Table 2 presents the results of a formal balance test of prior observations of the main lifecycle outcomes we examine and academic outcomes (measured two years prior to the year of the discontinuity), as well as demographics, using the main RDD estimating equation. In general, estimates are not statistically significant, lending credence to the validity of our design. Figure A.2 plots corresponding visual evidence that prior observations of outcomes, two years prior to the discontinuity year, are continuous in the vicinity of the threshold.

IV. Main Results

A. Effects on Federal Student Loans and Grants

Figure 3 depicts receipt of federal student loans and grants within the academic year of the discontinuity, cumulatively and by category, around the dependency threshold. The figures are constructed similarly to those prior, with means of each outcome plotted in twenty-day birthdate bins. But, in stark comparison to the earlier figures, there are clear breaks around the threshold in each figure, visual evidence that the policy rule substantially influences the federal support that borrowers receive.[26]

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. All coefficients are

[26] Table A.9 presents estimates incorporating potential effects on Parent PLUS borrowing. The results indicate that the policy rule induces an about \$325 decrease in such loans, less than 20% of the increase in Stafford Loan borrowing, suggestive of 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 slight shift in payment responsibility from parents to their children.

precisely estimated.[27] 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. Finally, column (5) showcases that the combined effect on loans and grants is quite substantial—nearly $3,300—implying about a 40% jump relative to borrowers born after the cutoff.

B. Effects on Homeownership by Year

Figure 4 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.

Table 4 presents the corresponding regression results. In general, the estimates indicate that homeownership is significantly higher among those with higher limits. They imply an immediate effect of about 0.5 p.p. in the year of the discontinuity and a more than 1 p.p. effect over the medium-term.[28]

C. Robustness and Placebo Estimates

Figure 5 shows that the estimated effects on homeownership are 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 throughout. Tables A.10 and A.3 show that the results are robust to adding borrowers within the “donut hole” back to the sample and that, the point estimates are stable to using a linear spline rather than a quadratic (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

[27] Figure A.3 shows that these estimates are not sensitive to varying the bandwidth.
[28] 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.

bandwidth, with the estimated optimal bandwidth pointing to a smaller bandwidth than with the quadratic specification.

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 columns are the results 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.

Figure A.5 demonstrates that the results pass a relabeling permutation test, a different placebo analysis that re-estimates effects varying the threshold. Estimates using thresholds far away from the true threshold are generally not statistically significant, while those using thresholds nearer to the true threshold generally are and constitute the largest magnitudes. The results indicate that the main findings are not driven by non-linearities 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 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.[29]

[29] See Campbell and Cocco (2003), Mian and Sufi (2011), and Gorea and Midrigan (2017) for a discussion of liquidity constraints in the mortgage market, and Mian, Rao, and Sufi (2013) and Souleles (1999) for a discussion of liquidity constraints and consumption.

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.[30] Note that the composition of the sample is prima facie evidence of the last channel, as student borrowers are presumably liquidity constrained on some dimension. 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 examine earnings and labor force participation. The first two columns of Figure 6 presents these outcomes within 3 representative years, the year of the discontinuity, three years later, and five years later.[31] 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 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.[32] Indeed, the estimates are precise enough to rule out a 0.4

[30] 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.

[31] 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.

[32] Table A.7 analyzes college completion rates, defined cumulatively for each period, and finds no difference at any period over the horizon we examine. This pattern is consistent with other findings in this section.

percentage point increase in participation and 1 percent increase in earnings.[33] 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%.

Are these results surprising? While standard economic models assume student loans help financially constrained individuals make costly educational investments that improve their labor market outcomes (Becker, 1962; Palacios, 2014; Lochner and Monge-Naranjo, 2011), 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 (Bettinger et al., 2016). Furthermore, our experimental setting examines *marginal* dollars made available to student borrowers, among whom there is no evidence of extensive margin (attendance) effects.[34] 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. For 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 the \$3,000 increase in grants and loans we estimate 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, often, verifying an established savings pattern. If borrowers are liquidity constrained, they may be unable to meet these basic requirements. Indeed, even though the typical down payment

[33] Table A.5 presents alternative specifications for these outcomes and the results remain similar.

[34] Denning (2017) finds that access to more aid accelerates completion and some evidence that it reduces earnings, but not participation, in the year of the discontinuity. He finds no effect on earnings in the subsequent year or on completion overall. His design analyzes the effect of the same policy rule we study on all students at four-year colleges in Texas. He restricts the sample to college seniors to examine completion. Stinebrickner and Stinebrickner (2008) find that when college itself is not costly, credit constraints do not hinder completion.

requirement for a young adult is low, in a survey of student borrowers, among those without a mortgage, 21 percent of respondents list not having funds for a down payment as the rationale (Navient, 2015).[35]

The last column of Figure 6 graphs saving rates for the 3 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 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.[36] 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 towards 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 while not shown, no increase in investment income in *any* year (a proxy for the amount of savings), those increased resources still appear to be spent quite rapidly, consistent with binding credit constraints.

C. Results by EFC

To try to explicitly disentangle liquidity from wealth, we leverage a bright line rule that the DoEd uses to determine an applicant's EFC. This split also helps isolate the effects of loans and grants. 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 are relatively liquidity constrained and those who experience a relatively large wealth effect. First, borrowers with zero EFC tend to come from lower-income households with fewer assets.[37] In addition, this same

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[35] Student borrowers are often first time home buyers, and, compared to the national average, purchase relatively inexpensive houses, whereby the average mortgage amount is $101,822 (Navient, 2015).

[36] 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. Results available upon request.

[37] 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).

group would very likely be eligible for the maximum allowable subsidy as financially 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.[38]

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 zero EFC sample is clearly driving our main results, with the estimates among this group implying an economically significant increase in homeownership in the years of and after the discontinuity.[39] Note that all of the coefficients are smaller and none are statistically significant for the positive EFC sample. Altogether, responsiveness appears to be concentrated within the group that is eligible for *less* additional subsidy, favoring the liquidity explanation over the wealth one.[40]

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.6 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,

[38] Moreover, responsiveness within the zero EFC group is less likely to be driven by human capital effects (at least those that stem from an increase in subsidy).

[39] Table A.10 repeats the analysis for the outcomes explored in this section and shows that the effect on earnings in the year of the discontinuity is concentrated in the *positive* EFC sample, further evidence that human capital effects do not drive our main estimates.

[40] 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.3 shows the effects on loans and grants are pronounced across all school types.

we would expect to see the largest responses when conditions are tightest (i.e., beginning in 2007).

We begin by splitting the sample into whether the year of the discontinuity occurred before or after the contraction of credit, by splitting borrowers enrolled through 2006 and borrowers enrolled in 2007 onward.[41] 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 and academic level to resemble borrowers in the earlier period. The results are quite similar to those before, with the estimated effect on homeownership still highly significant.

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.

Altogether, the entire set of results from our analysis of mechanisms indicates that the main estimates are driven by borrowers having access to increased liquidity rather than by human capital or wealth effects. 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.

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 and which, like homeownership rates, some

[41] We do not include results for later years as, for many years in the post period, data are unavailable. Further, for some borrowers assigned to the prior period in this exercise, later outcomes may actually be observed during the recession, which would make their interpretation difficult.

commentators have expressed are *depressed* due to high student debt balances.[42] Figure 7 shows these outcomes for 3 representative years. Note that any differences in the year of the discontinuity are very small; however, they evolve over time and are reasonably stark by the end of the medium-term.

The regression-adjusted estimates in Table 9 largely corroborate the graphical evidence.[43] Neither effect is statistically significant in the year of the discontinuity, though over the medium term, both effects grow. Indeed, it appears both marriage and fertility lag the initial homeownership effect, such that by the middle of the horizon, borrowers with higher limits are 1 percentage point more likely to be married and more likely to have had children as well.

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 extension in borrowing limits and an increase in subsidy, effects appear to be driven by the former. Altogether, 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.

Our findings also 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 generally do not affect earnings, labor force participation, or college completion, calling into question how

[42] Moreover, it may be that homeownership itself has real effects on these outcomes (Sodini, Vestman, and von Lilienfeld-Toal, 2017).

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.[44]

Finally, the 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 really be the first-order consideration. As our study illuminates, separate from whether student borrowers have seen positive net returns from the education that they financed with loans, policymakers must also consider whether they extracted liquidity from these loans to finance non-education spending that ultimately left them better off as well. That said, our particular context points to potentially negative net returns from the small amount of additional educational investment being financed with loans.

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, and exhibit differential responsiveness in the outcomes we consider (Cellini and Turner, 2016). 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 as well as consider such effects when assessing the welfare consequences of these programs.

[44] That said, the human capital benefits of higher limits may be being 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, 2016; 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, 2014). Finally, education may have unobservable consumption value (Lazear, 1977) and generate externalities (Moretti, 2004a and 2004b), which could generate human capital effects beyond those that we consider.

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Figure 1: Sample Construction & Testing for Enrollment Effects

Notes: This figure shows mean number of students in twenty day bins of the assignment variable in the year of the discontinuity. The outcome is denoted above each estimate. The top left panel shows all borrowers, while the top right panel shows for-profit borrowers. The bottom left panel shows borrowers at public institutions, while the bottom right panel shows borrowers at private non-profit institutions. The assignment variable is the number of days from turning 24 in the calendar year of the discontinuity. All data comes from IRS tax data. Enrollment data is from 1098-T forms.

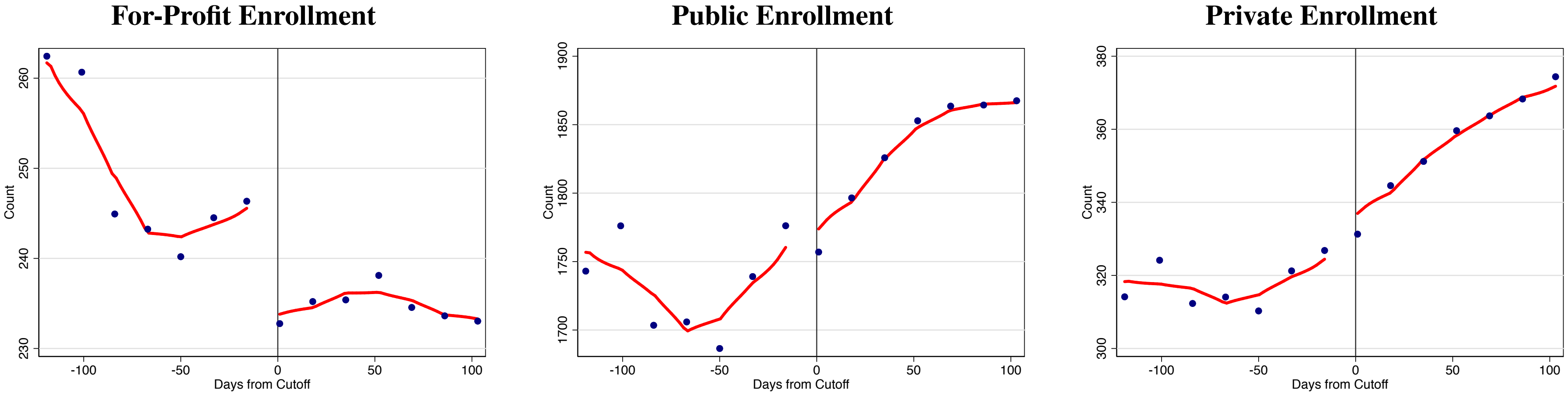

Figure 2: Density of Borrowers

Notes: This figure shows density of the assignment variable, in bins of 20 days from 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 comes from the NSLDS matched to IRS tax data.

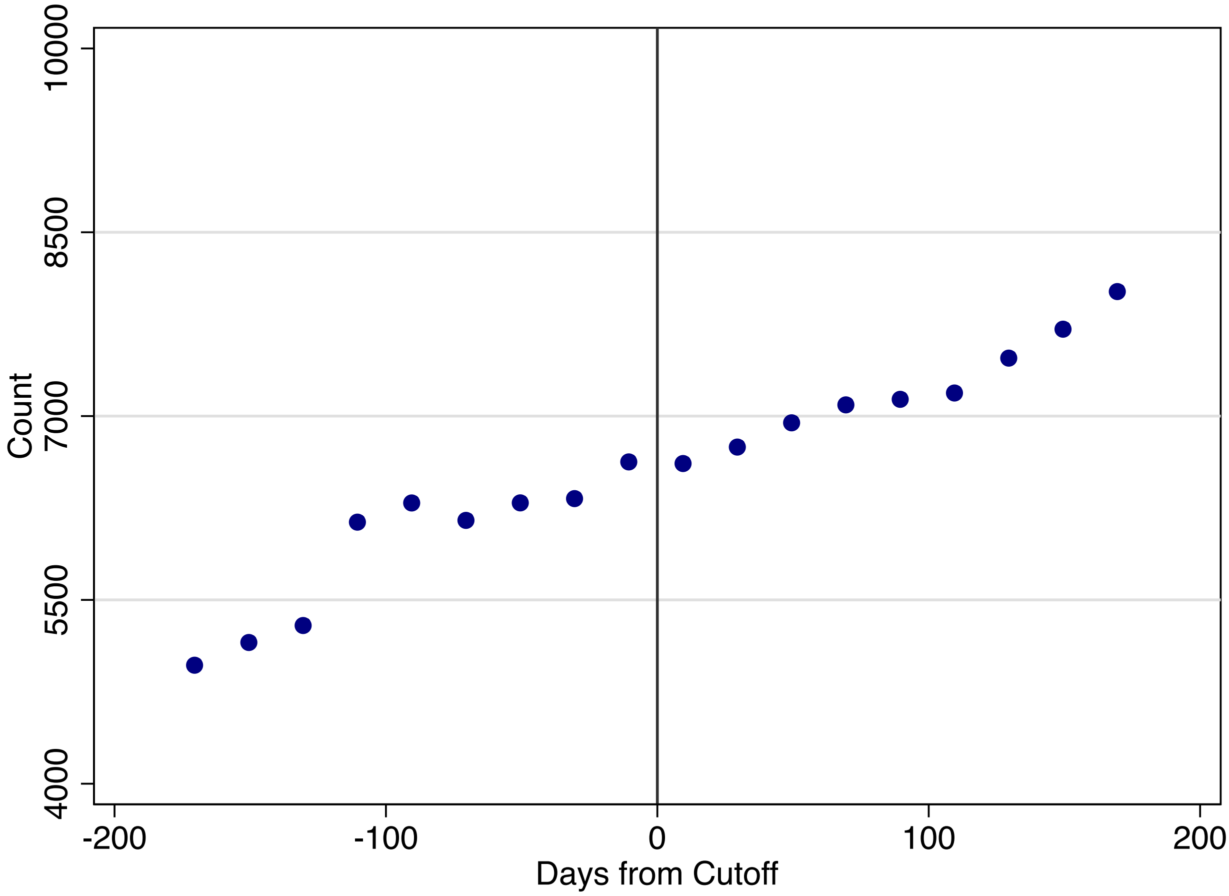

Figure 3: Effect of Limit Increase on Federal Loans and Grants

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 comes from the NSLDS matched to IRS tax data.

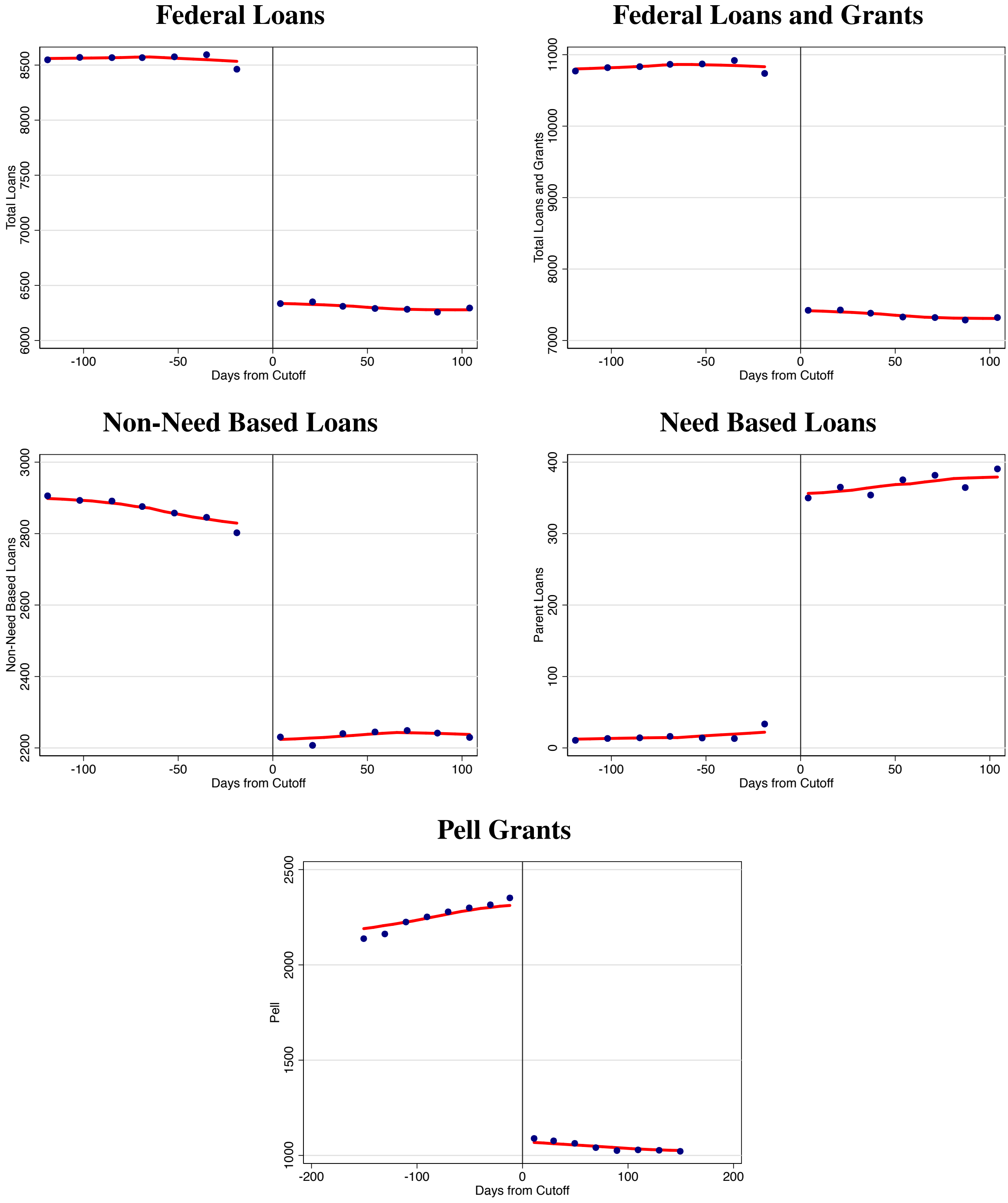

Figure 4: Homeownership

Notes: This figure shows mean home ownership in twenty day bins of the assignment variable. The dependent variable is an indicator denoting whether an individual or their spouse has a mortgage. The assignment variable is the number of days from turning 24 in the calendar year of the discontinuity. All data comes from the NSLDS matched to IRS tax data.

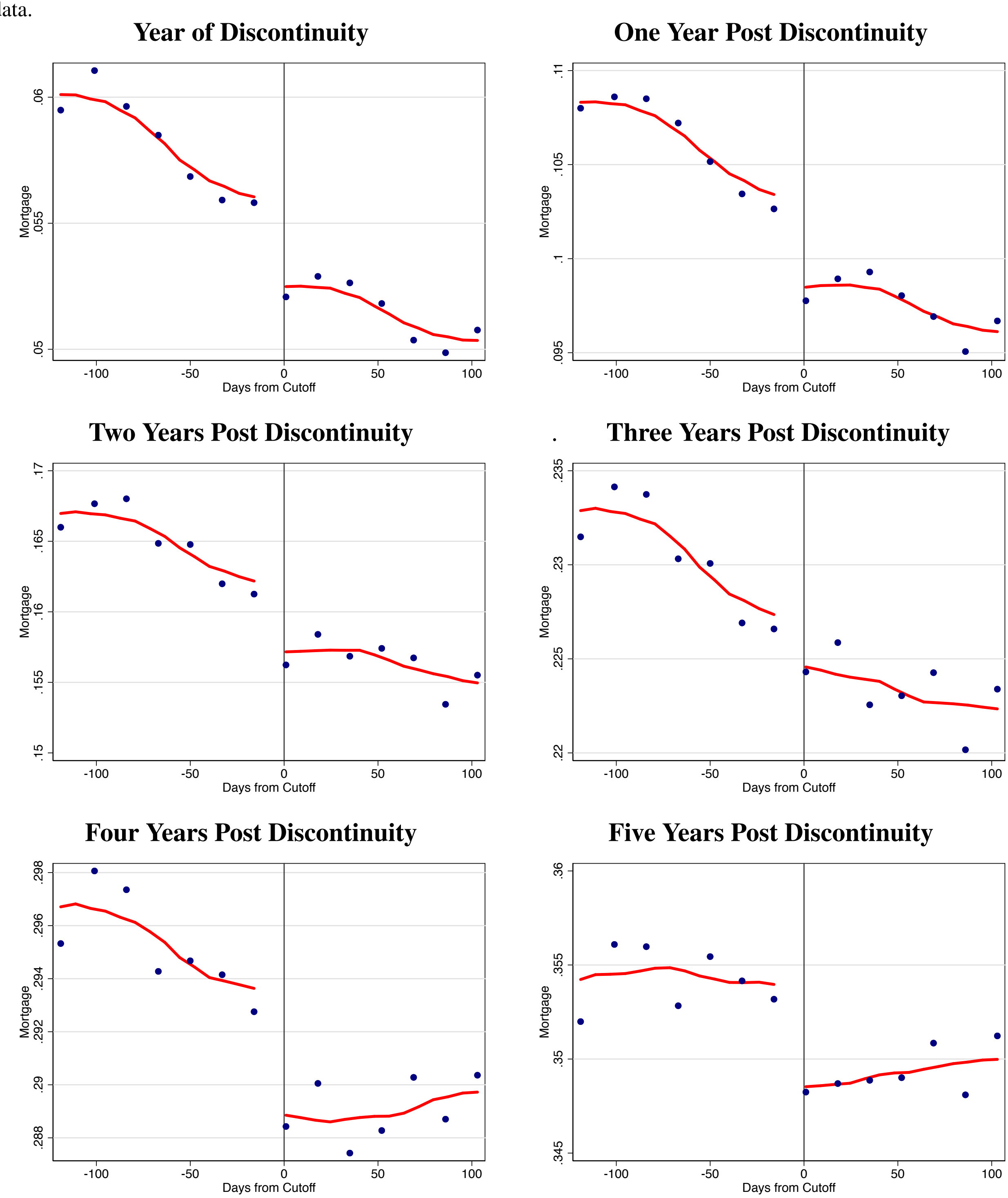

Figure 5: Alternative Bandwidth for Homeownership

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 comes from the NSLDS matched to IRS tax data.

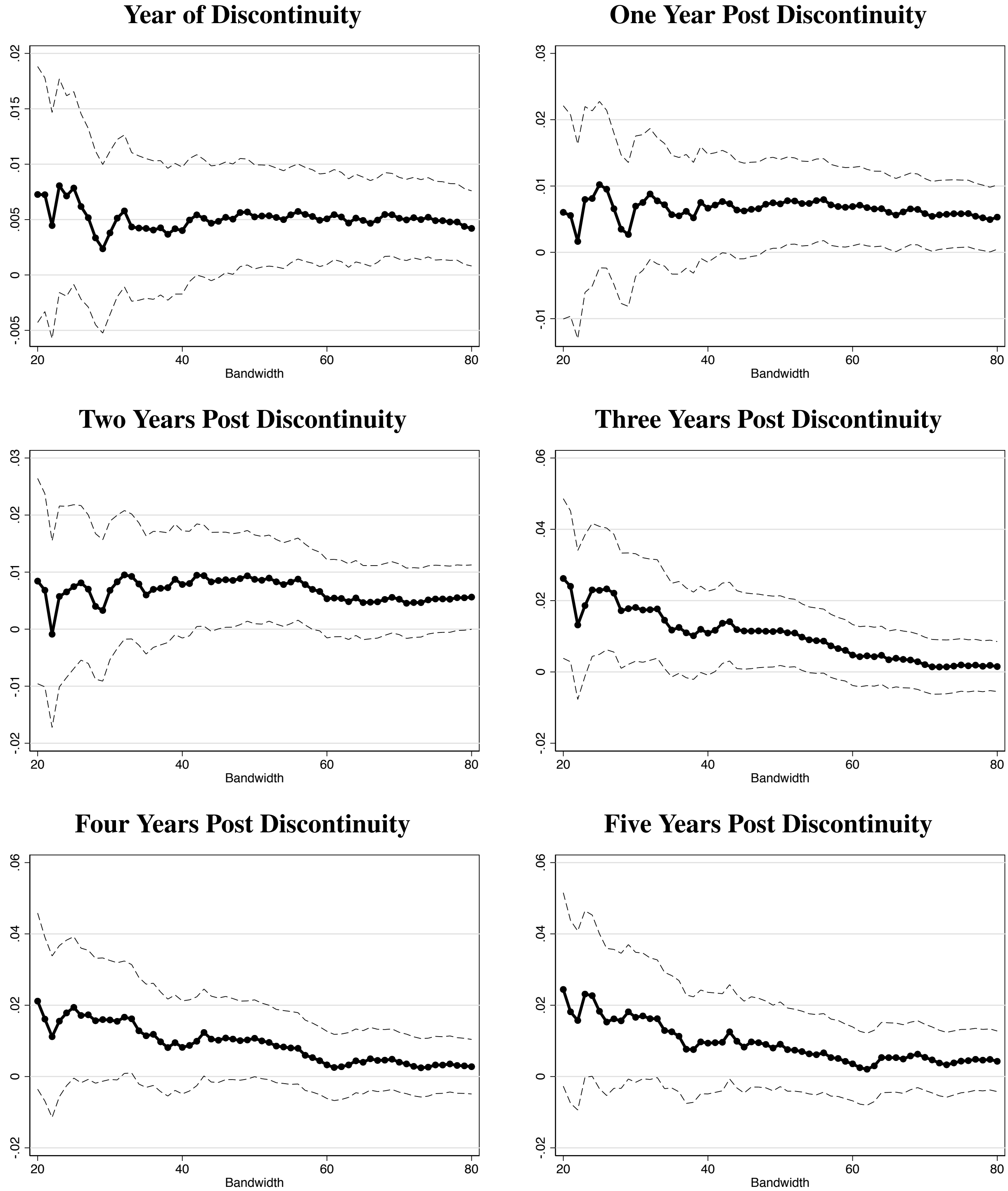

Figure 6: Human Capital and Savings

Notes: This figure shows mean outcomes in twenty 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 an indicator denoting whether earnings are reported on behalf of an individual. The dependent variable in the second column is earnings. The dependent variable in the third column is an indicator of whether an individual or their spouse has a bank or income bearing account. 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 comes from the NSLDS matched to IRS tax data.

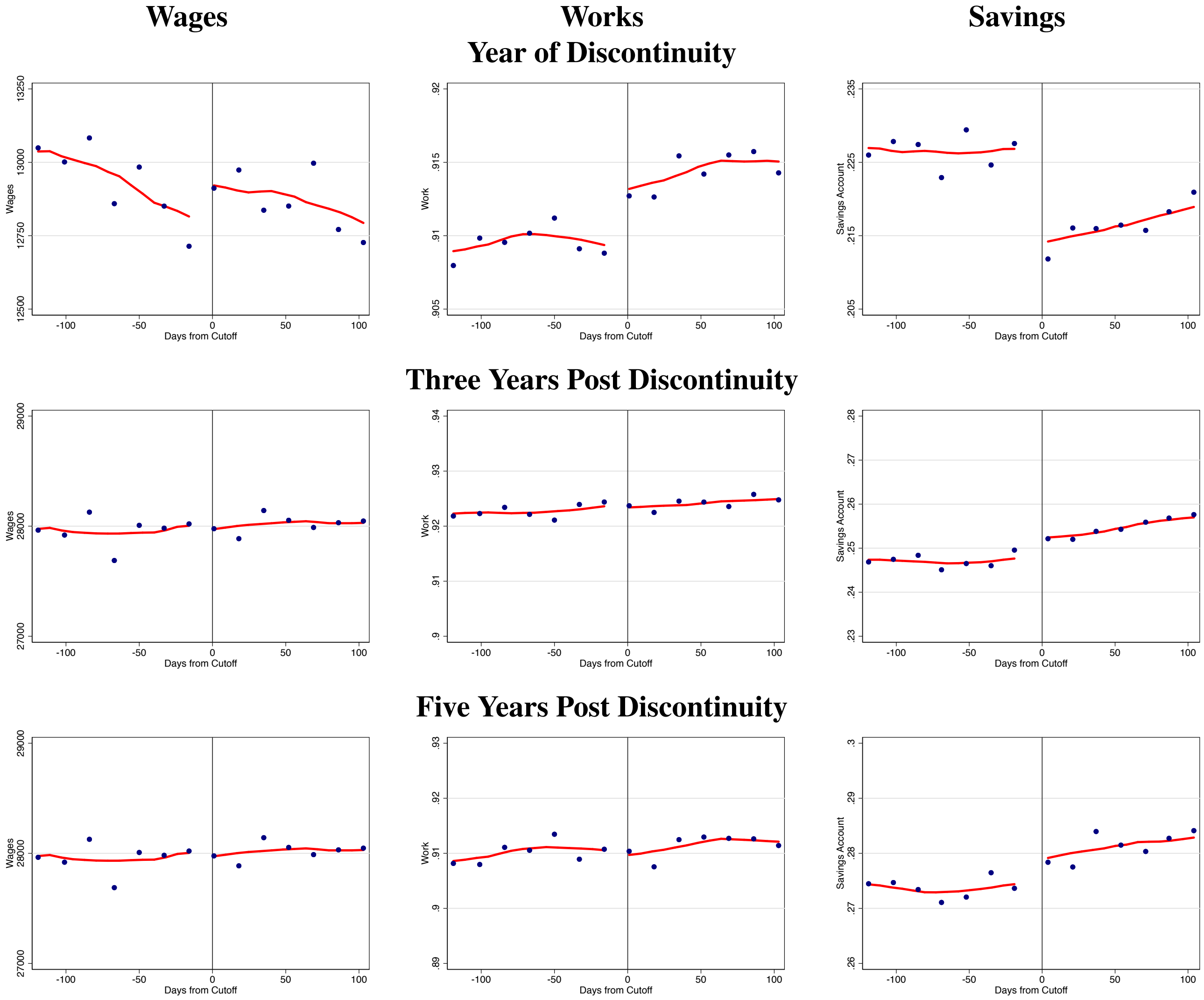

Figure 7: Marriage and Children

Notes: This figure shows mean marriage and children in twenty 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 or their spouse claims dependent children. The outcome is denoted above each estimate. Children refers to the mean number of children in the household. The assignment variable is the number of days from turning 24 in the calendar year of the discontinuity. All data comes from the NSLDS matched to IRS tax data.

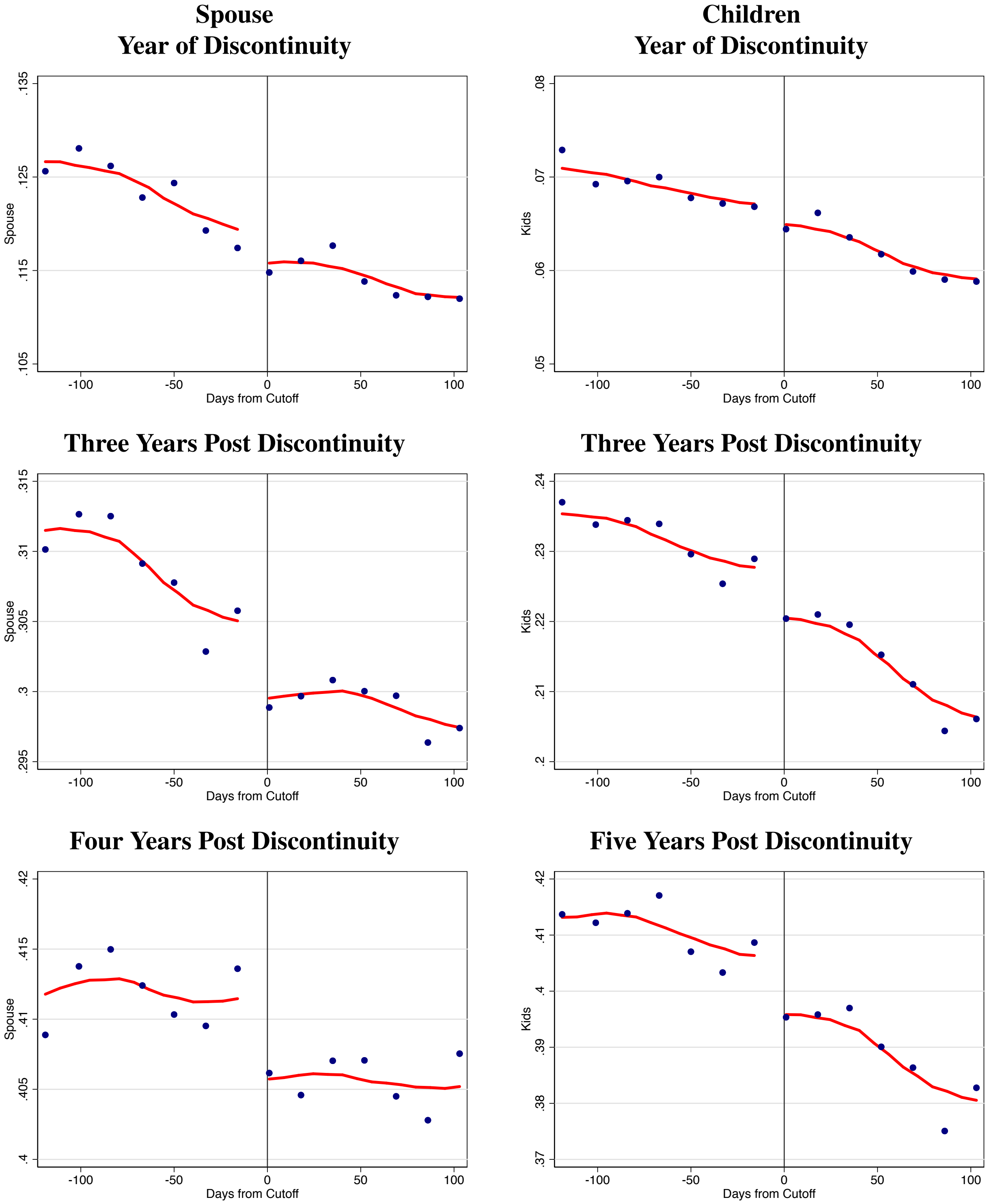

Table 1: Summary Statistics

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

		Mean	SD	Observations
Loans		6,532.81	3,717.83	464,008
Need Based		3,732.43	2,298.47	464,008
Non-Need Based		2,617.68	2,774.03	464,008
Pell Grants		1,766.54	1,933.30	464,008
Earnings				
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
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
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*	0.06	0.28	464,008
	3	0.22	0.52	426,478
	5	0.40	0.70	352,446

Table 2: Predetermined Covariates

Notes: This table shows regression discontinuity estimates of predetermined covariates. 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. Standard errors are clustered at the day from cutoff level. All data comes from the NSLDS matched to IRS tax data. $*p < .1$, ** $p < .05$, *** $p < .01$.

	(1)	(2)	(3)
	Mortgage $_{t\text{-}2}$	Savings $_{t\text{-}2}$	Family Income $_{t\text{-}2}$
Above Cutoff	.0015612	.0015762	.0096111
	(.0015204)	(.0029899)	(.0149399)
Observations	464,008	464,008	211,353
	Works $_{t\text{-}2}$	Wages $_{t\text{-}2}$	Children $_{t\text{-}2}$
Above Cutoff	0002253	313.645	-.0023549
	(.0034927)	(208.1957)	(.0038721)
Observations	464,008	464,008	464,008
	US Citizen	Gender	Borrowed $_{t\text{-}2}$
Above Cutoff	.0009799	.0110338*	.0225669
	(.0029496)	(.005637)	(.0233396)
Observations	464,008	464,008	464,008
	Acad. Level $_{t\text{-}2}$	Public $_{t\text{-}2}$	Spouse $_{t\text{-}2}$
Above Cutoff	-.0106153	.0049528	-.0013308
	(.0101294)	(.0044997)	(.0110233)
Observations	464,008	464,008	464,008
	Independent $_{t\text{-}2}$	Zero EFC $_{t\text{-}2}$	FourYear $_{t\text{-}2}$
Above Cutoff	.004094	.0003425	-.0048781
	(.0044181)	(.005113)	(.0030946)
Observations	464,008	464,008	464,008

Table 3: Effect of Limit Increase on Federal Loans and Grants

Notes: This table shows regression discontinuity estimates of federal loan and grant receipt in the academic year of the discontinuity. 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 comes 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
	(1)	(2)	(3)	(4)	(5)
Above Cutoff	1,892.918***	1,230.531***	662.38702***	1,332.817***	3,275.697***
	(44.110813)	(39.539291)	(48.298885)	(24.41938)	(58.28166)
Observations	464,008	464,008	464,008	464,008	464,008

Table 4: Homeownership 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 or their spouse has a mortgage. 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 comes 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, for groups that were unaffected by the discontinuity. The top panel repeats the analysis for borrowers who took out loans in other years, but not in the year that they turn 24. The bottom panel shows individuals who filed the FAFSA as independents before turning 24, but otherwise fit our sample selection criteria. The dependent variable is an indicator denoting whether an individual or their spouse has a mortgage. 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 comes 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 a bank or income bearing account. 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 comes from the NSLDS matched to IRS tax data. $*p < .1$, ** $p < .05$, *** $p < .01$.

	Wages	Work	Savings
	(1)	(2)	(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	464,008	464,008	464,008
		Five Years Later	
Above Cutoff	-323.4143	-.0008361	-.0019679
	(338.6448)	(.0037454)	(.0031079)
Observations	388,518	388,518	388,518

Table 7: Homeownership by Zero EFC

Notes: This table shows regression discontinuity estimates in the year of the discontinuity, two years after the year of the discontinuity and four years after the discontinuity, broken down by a zero EFC in the year prior to enrollment. Zero EFC status affects grant versus loan availability. The dependent variable is an indicator denoting whether an individual or their spouse has a mortgage. 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 comes 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	.0040354	0.0006216	0.0038612
	(.0047483)	(0.0094582)	(0.0134629)	(0.0027431)	(0.0054393)	(0.0066761)
		Effect on Loans and Grants			Effect on Loans and Grants	
	Total Loans	Need Based Loans	Pell Grants	Total Loans	Need Based Loans	Pell Grants
Above Cutoff	1,502.633***	293.69675***	233.2187***	1,510.088***	1,358.6099***	1,777.483***
	(56.67731)	(53.968742)	(36.62447)	(38.21965)	(34.299011)	(34.72785)
Observations	83,989	83,989	83,989	116,310	116,310	116,310

Table 8: Availability of Credit: Borrower Outcomes 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. The dependent variable is an indicator denoting whether an individual or their spouse has a mortgage. 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 comes from the NSLDS matched to IRS tax data. $*p < .1$, $** \ p < .05$, $*** \ p < .01$.

	(1)	(2)	(3)	(4)	(5)
	Before 2006	After 2007	After 2007 (Weighted)	After 2007 (Zero EFC)	After 2007 (Positive EFC)
	Year of Discontinuity	Year of Discontinuity	Year of Discontinuity	Year of Discontinuity	Year of Discontinuity
	Mortgage	Mortgage	Mortgage	Mortgage	Mortgage
Above Cutoff	.0006096	.0094545***	.009019***	.0156198***	.0058351
	(.0035807)	(.0031376)	(.003171)	(.0057289)	(.0045034)
Observations	221,513	242,495	242,495	242,495	242,495

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 or their spouse has children. 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 comes 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	464,008
	Five Years Later	
	Married	Children
Above Cutoff	.0138455**	.0130425**
	(.0065783)	(.0061593)
Observations	352,446	388,518

Table A.1: 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
Earnings	Labor earnings.	W-2 Information Returns
Works	Indicator of whether labor earnings are reported.	W-2 Information Returns
Enrollment	Indicator of whether borrower is enrolled in year.	1098-T Information Returns
Mortgage	Indicator of whether borrower or spouse has a mortgage.	1098 Information Returns
Spouse	Indicator of whether borrower is married.	Form 1040
Savings	Indicator of whether borrower has an interest bearing account.	1099 Information Returns
Children	Cumulative birth of a child.	Social Security Card Applications

Table A.2: 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

	Financial Dependency Status			
Level	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.3: Enrollment

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

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

Table A.4: Robustness of Homeownership Results

Notes: The top column shows results in the year of the discontinuity, using a linear rather than a quadratic spline. The dependent variable is an indicator denoting whether an individual or their spouse has a mortgage. Standard errors are clustered at the day from cutoff level. All data comes 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	394,661	394,661	394,661	330,380	305,030	299,749

Table A.5: Human Capital Outcomes Robustness

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, one year after the year of the discontinuity and three 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 an interest bearing account. 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 comes 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	-183.1405**	122.9001	-105.8653
	(77.35224)	(162.6355)	(385.3459)
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	-0.0009366	0.0015374	0.0047247
	(.0018326)	(.0017449)	(.0044967)
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.6: 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, one year after the year of the discontinuity and three 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 or their spouse report children. 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 comes 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

Table A.7: 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 or their spouse completes a four year 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 comes from the NSLDS matched to IRS tax data. *$p < .1$, ** $p < .05$, *** $p < .01$.

	(1)	(2)	(3)	(4)	(5)	(6)
	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	0.0040502	0.0025154	0.0082822	0.0097979	0.0082979	0.007468
	(.005307)	(.0057882)	(.0057902)	(.0001757)	(.0060532)	(.0064243)
Observations	464,008	464,008	464,008	426,478	388,518	352,446

Table A.8: Effects of Limit Increase on Loans and Grants by 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 non-profit schools granting four-year degrees, and the third column presents results for non-profit 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 comes from the NSLDS matched to IRS tax data. *$p < .1$, ** $p < .05$, *** $p < .01$.

	Public (1)	Private (2)	Comm. College (3)
Total Loans	1618.046***	1436.707***	958.6834***
	(30.55355)	(74.73764)	(71.12733)
Total Grants	1466.327***	1355.804***	853.7033***
	(20.80598)	(29.71102)	(50.87597)
Observations	337,745	94,157	32,106

Table A.9: Effect of Limit Increase on Parent Borrowing

Notes: This table shows regression discontinuity estimates on federal parent PLUS loans each year. Each variable is denoted above the estimates. The first column shows total student borrowing. The column in the middle shows total household borrowing including parent borrowing. 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 comes from the NSLDS matched to IRS tax data. *$p < .1$, ** $p < .05$, *** $p < .01$.

	Federal Loans	Total Minus Par.	PLUS Loans
	(1)	(2)	(3)
Above Cutoff	1892.918***	1459.802***	-325.7071***
	(44.110813)	(33.70696)	(16.87936)
Observations	454,043	454,043	454,043

Table A.10: Earnings by EFC

Notes: This table shows regression discontinuity estimates in the year of the discontinuity, three years after the discontinuity and five years after the discontinuity, split by zero EFC. 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. 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 comes from the NSLDS matched to IRS tax data. $*p < .1$, ** $p < .05$, *** $p < .01$.

		Zero	Positive
		EFC	
		(1)	(2)
Earnings			
Year	*0*	-332.6337	-1263.775***
		(223.4768)	(422.0726)
	3	807.999	-526.8018
		(583.9875)	(329.3132)
	5	673.9861	-292.361
		(508.5177)	(225.2129)
Work			
Year	*0*	.0100955	-.0053298
		(.0081865)	(.0038368)
	3	-.0030152	-.0017693
		(.0063842)	(.0018361)
	5	.0033093	-.000816
		(.0066125)	(.0019663)
Observations		78,763	277,973

Table A.11: Main Homeownership Results with Donut Hole

Notes: This table shows regression discontinuity estimates in the year of the discontinuity, two years after the year of the discontinuity and four years after the year of the discontinuity, including the donut hole excluded in the main analysis. The dependent variable is listed above each column. 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 comes from the NSLDS matched to IRS tax data. $*p < .1$, $** \ p < .05$, $*** \ p < .01$.

	(1)	(2)	(3)	(4)	(5)	(6)
	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)	(.0005302)	(.0003784)
Observations	494,218	494,218	494,218	454,250	413,868	375,386

Table A.12: Main Homeownership Results by 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 non-profit schools granting four-year degrees, and the third column presents results for non-profit 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 comes from the NSLDS matched to IRS tax data. $*p < .1$, ** $p < .05$, *** $p < .01$.

		Public	Private	Comm. College
		(1)	(2)	(3)
Mortgage				
Year	*0*	0.003878**	-0.0014334	0.01434**
		(0.0015935)	(0.0032048)	(0.0064617)
	1	.0051635*	.001726	.0143631
		(.0027335)	(.0056978)	(.0116005)
	2	0.0072544	0.0088431	0.0216966
		(0.0045175)	(.0088708)	(0.0167339)
	3	.0103724*	.0089952	.0297693
		(.0056664)	(.0101475)	(.0199613)
	4	0.0117386*	0.0012834	0.0284013
		(0.0062921)	(0.0119438)	(0.0225903)
	5	.0095257	.0085219	.0073505
		(.0070388)	(.0129165)	(.0269552)
Observations		337,745	94,157	32,106

Figure A.1: Density of Borrowers

Notes: This figure shows number of borrowers by the assignment variable, in bins of 9 days from dependency cutoff. The assignment variable is the number of days from turning 24 in the calendar year of the discontinuity. All data comes from the NSLDS matched to IRS tax data.

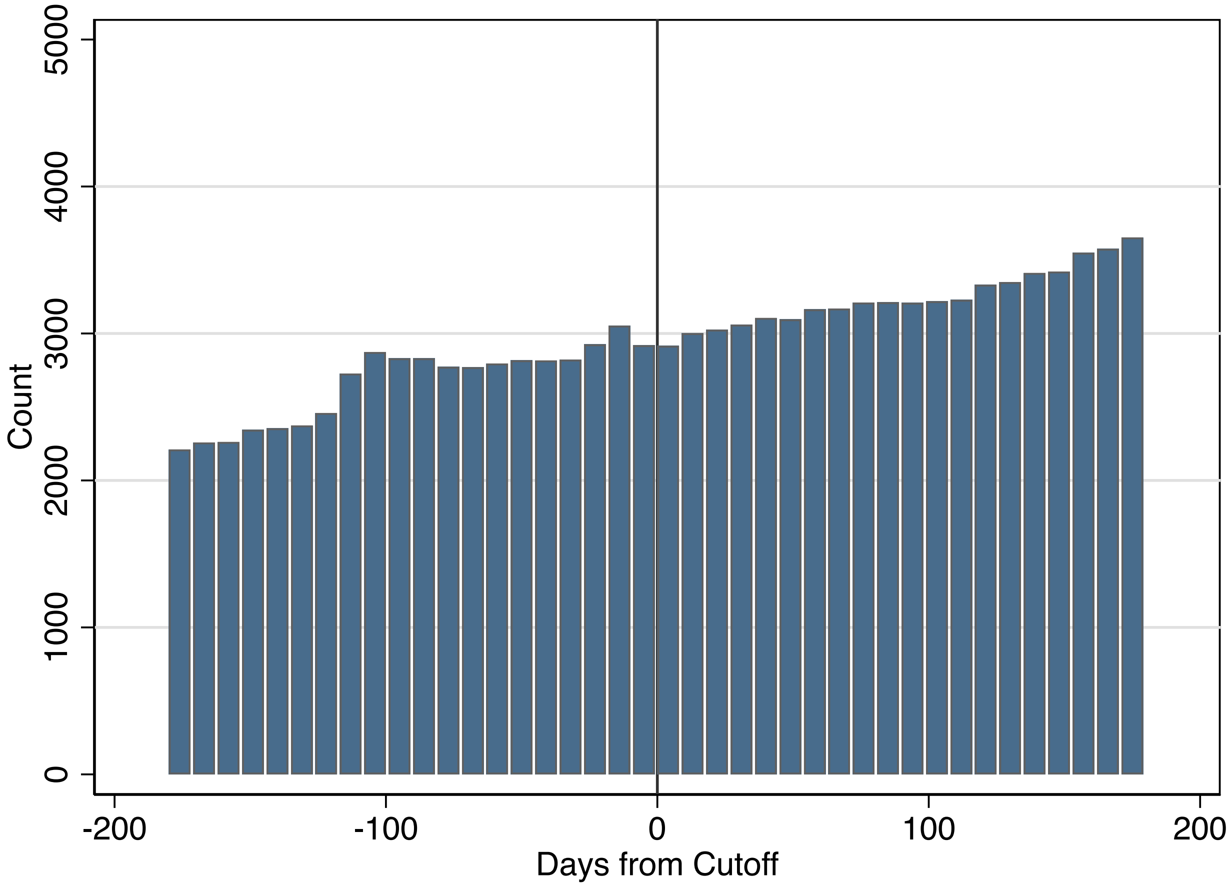

Figure A.2: Predetermined Variables Along Cutoff

Notes: This figure shows mean predetermined covariates in twenty 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 comes from the NSLDS matched to IRS tax data.

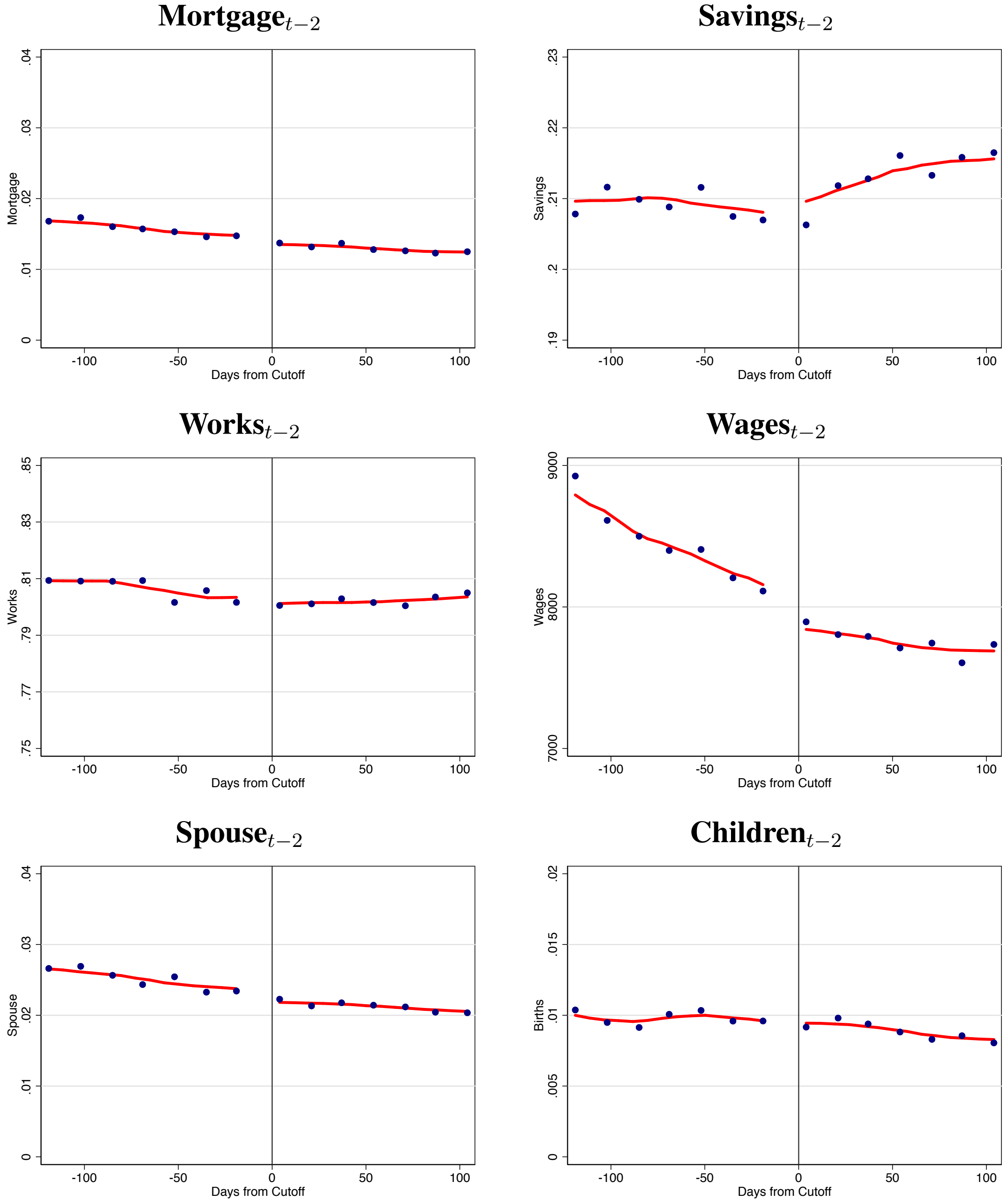

Figure A.3: Alternative Bandwidth for Effect of Limit Increase on Loans and Grants

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 comes from the NSLDS matched to IRS tax data.

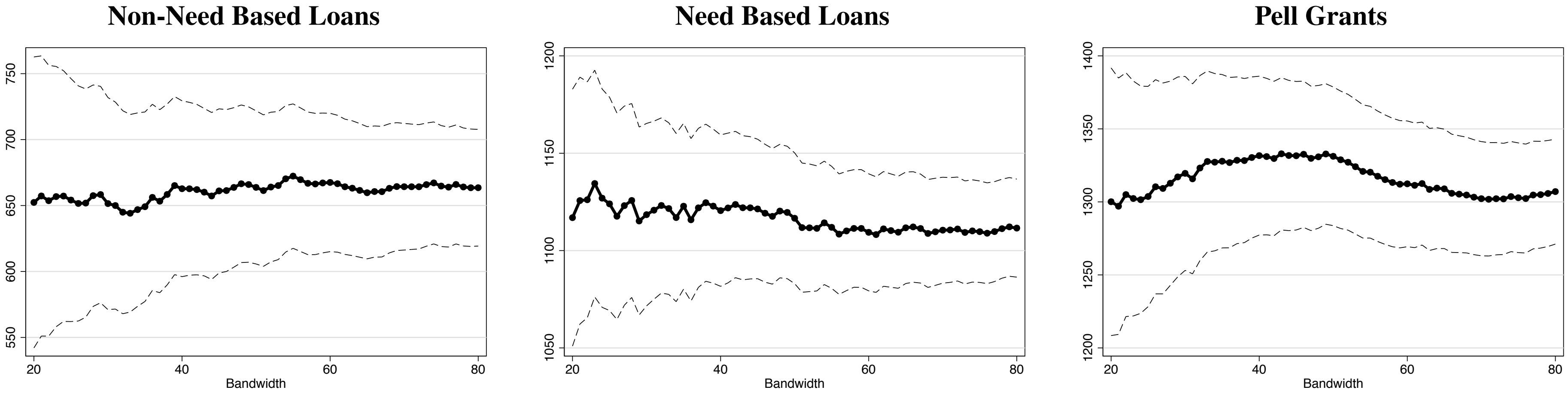

Figure A.4: Alternative Bandwidth for Homeownership Results Using Linear Spline

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 comes from the NSLDS matched to IRS tax data.

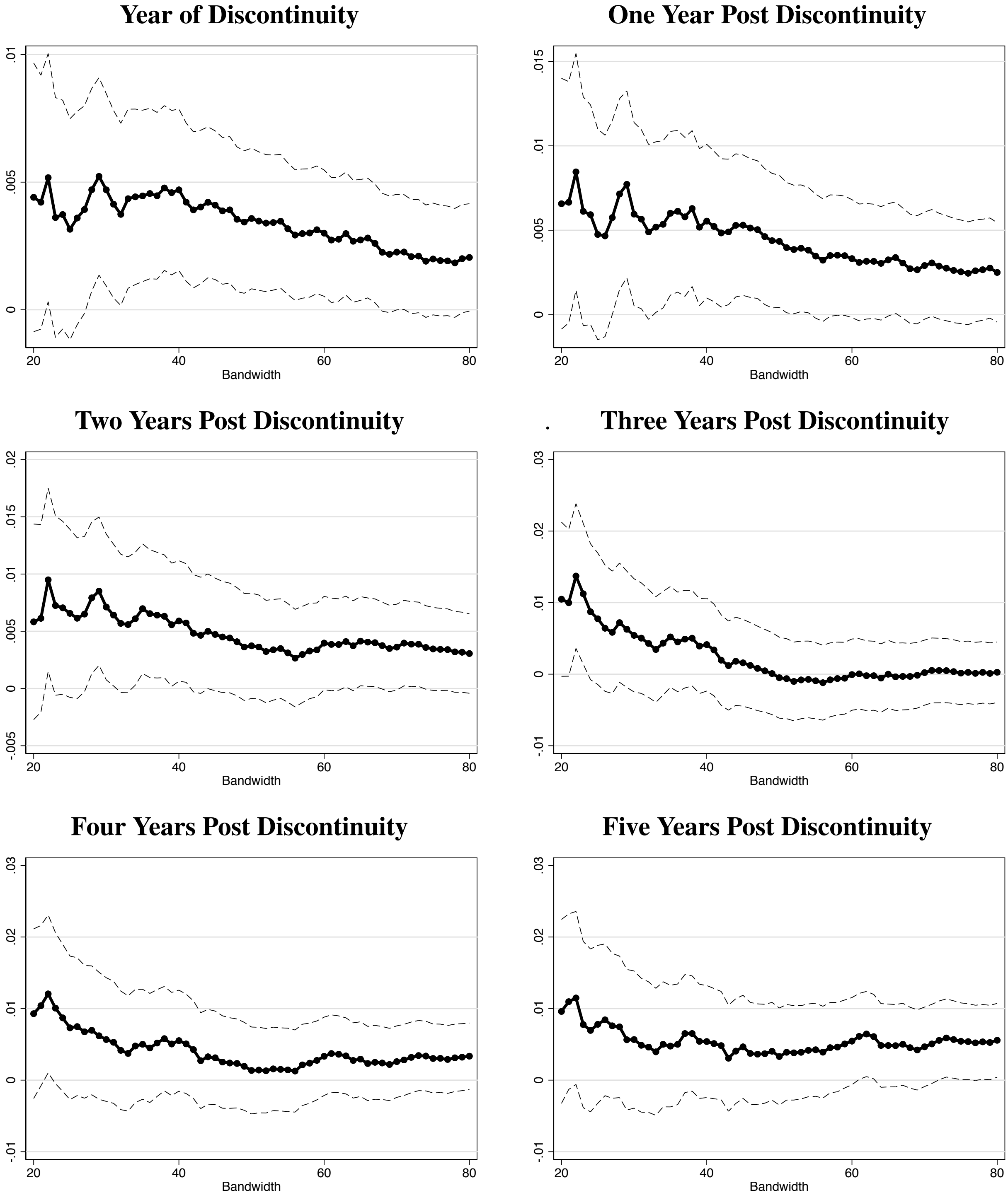

Figure A.5: Placebo Cutoffs: Mortgages

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 turning 24 in the calendar year of the discontinuity. Standard errors are clustered at the day from cutoff level. All data comes from the NSLDS matched to IRS tax data.

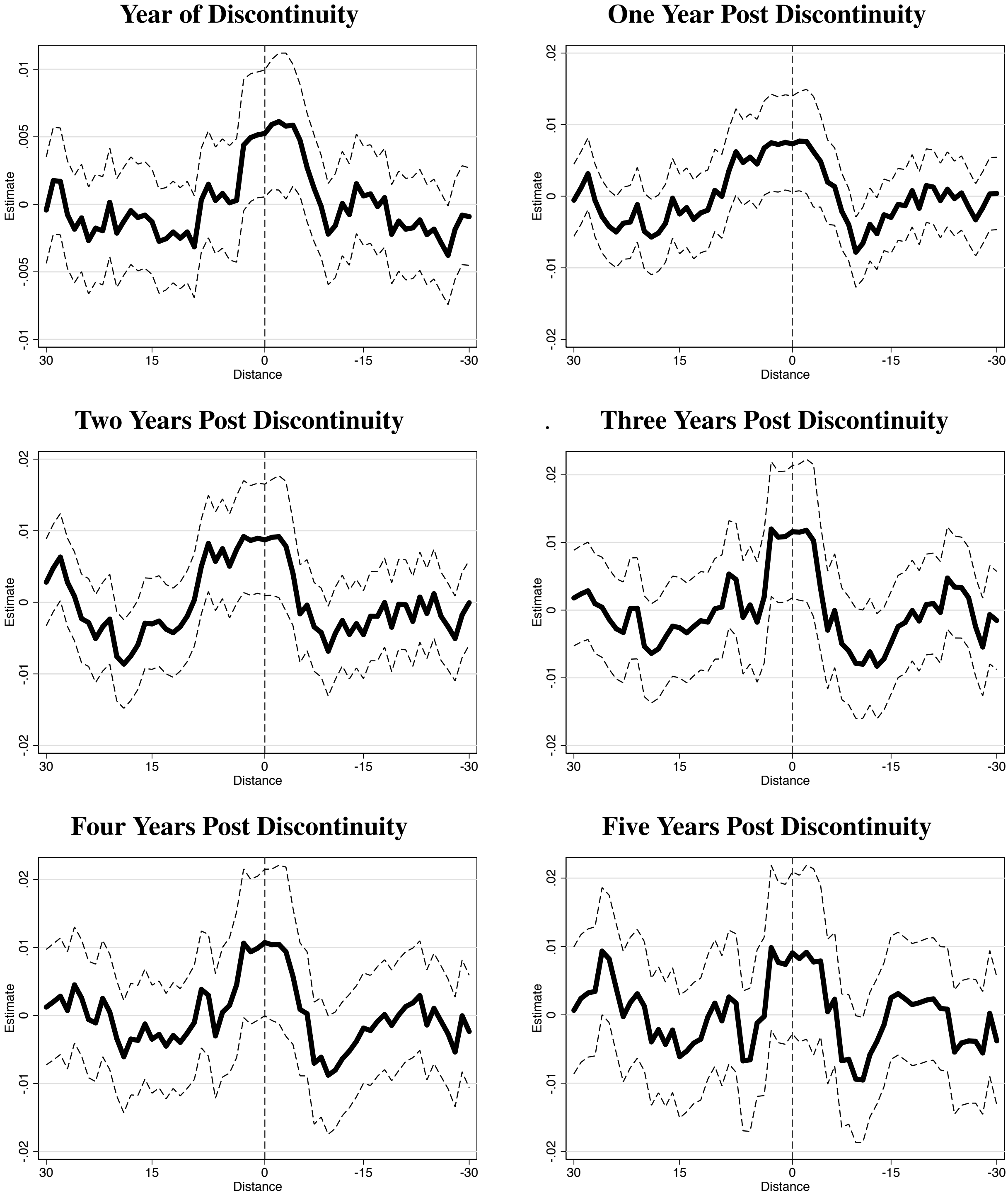

Figure A.6: Fraction of Banks Tightening Consumer Credit

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