Borrowing smarter or borrowing more? Investigating the effects of a natural experiment in federal loan policy

Abstract: Much of the student loan literature focuses on institutional, individual, and family-level characteristics associated with indebtedness, default, and other undesirable post-graduation outcomes; however, relatively little research examines the role that federal policy has played in student borrowing. This study addresses this gap by analyzing the effects of a change in Stafford loan limits that impacted one subgroup of borrowers. Findings consistently revealed that increases in federal loan limits did not result in students borrowing more. Instead, students utilized newly available Stafford loan dollars to substitute away from other loan sources with less favorable terms, such as private and parent PLUS loans.

Keywords: Student loans, Federal financial aid policy

JEL: I22, J24

Highlights:

- In response to federal loan limit increases, students did not increase their aggregate borrowing
- Instead, students borrowed more from the Stafford loan program and less from private loan sources and the parent PLUS loan program
- Responses to the increased loan limits did not differ across race, gender, nor family income

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1. Introduction

Today's college-goers are the most indebted students in the country's history (Baum, 2015). In fact, they are significantly more indebted than those who preceded them just a decade ago, as student borrowing increased 51% in inflation-adjusted dollars from 2001-02 to 2012-13 (College Board, 2014). Although many researchers argue that college remains a worthwhile investment for the average student (Avery & Turner, 2012; Baum, 2015; Toutkoushian, Shafiq & Trivette, 2013; Webber, 2016), others are concerned about the potential deleterious effects associated with borrowing. Recent studies suggest that borrowing may decrease a student's likelihood of owning a home (Mezza, Ringo, Sherlund & Sommer, 2016), marrying (Gicheva, 2016), enrolling in a graduate or professional degree program (Malcolm & Dowd, 2012, Zhang, 2013), pursuing a lower-paying public service profession (Rothstein & Rouse, 2011), or even taking a job with higher earnings potential (Minicozzi, 2005). These upward trends in borrowing and the potential negative post-graduation outcomes make analyzing the determinants of student loan debt accumulation an important and timely endeavor.

Considering the average costs of attending college are growing more rapidly than inflation each year at both public and private four-year institutions (College Board, 2014), popular media have attributed the concurrent rise in indebtedness to these increasing costs. However, studies suggest that increased student borrowing is a highly complex phenomenon and not solely related to increases in tuition (King, 1999; Monks, 2014; Perna, 2001). For example, Monks (2014) stated, "Policies that solely focus on tuition levels and tuition growth are overly simplistic and ignore other important factors in determining student debt" (p. 140). This point was bolstered by Chen & Wiederspan (2014), who suggested that we should engage in critical research on how specific policy changes affect a borrower's distribution of debt. While there are

a number of individual, institutional, and state-level determinants associated with debt accumulation, few researchers have examined the role of federal policy in shaping student debt. In particular, we know very little about the effects of federal legislation since the 1998 Reauthorization of the Higher Education Act (Chen & Wiederspan, 2014; Heller, 2008). This gap in the literature is particularly troubling as Congress has changed the aggregate and annual federal student loan borrowing limits five times through three different pieces of legislation since said reauthorization (Kantrowitz, 2015), with likely significant effects on student borrowing.

This study aims to contribute to filling this gap in the literature by analyzing the effect of a federal policy on students' borrowing behaviors. More specifically, this study assesses the effect of a recent policy change that was part of the Higher Education Reconciliation Act of 2005 and increased the annual federal borrowing limits as of July 2007 for freshmen and sophomore students. Given this newly available source of credit, it is feasible that affected students may have decided to increase their overall borrowing, or alternatively, may have instead borrowed more from federal sources as opposed to other less favorable sources of credit, such as private loans. As such, this specific policy change represents an ideal context to assess the role that federal policy plays in student borrowing. Notably, this change in federal policy allows for the natural identification of participants affected by a policy before and after its implementation (freshmen and sophomores), along with those who were unaffected by the change across the same time period (juniors and seniors). We capitalize upon this natural experiment through the implementation of a difference-in-differences analytic strategy (henceforth "DD"). Our identification approach uses upperclassmen as a control group for first- and second-year students affected by the policy and relies upon three nationally representative cohorts from the National Postsecondary Student Aid Study (NPSAS). The results suggest that affected students do not

increase their aggregate borrowing in response to increased federal loan limits; rather, they use these newly available funds to supplant borrowing from private loan sources and the parent PLUS loan program. These findings are consistent across a number of model specifications and robustness checks and have important policy implications.

In the next section we synthesize the previous literature on the determinants of student loan borrowing. In Section 3 we discuss the current landscape of student loans, as well as recent changes in federal student loan policy. Next, Section 4 describes the data, sample, and variables used in the study. Section 5 details the empirical strategy. The results of the study can be found in Section 6, with concluding remarks presented in Section 7.

2. Related literature

Whether conceptualized in terms of total borrowing, debt-to-earnings ratio, or default rates, numerous studies have examined the borrowing behaviors of the college-going population (see Dowd, 2008; Gross, Cekic, Hossler & Hillman, 2010; Heller, 2008; Hillman, 2015). In reviewing this literature, we identify four contexts or levels that may influence student borrowing: 1) individual and family characteristics; 2) institutional characteristics and policies; 3) state-level characteristics and policies; and 4) federal policy. At the individual and family level, researchers have investigated borrowing behaviors by demographics and characteristics such as race/ethnicity, family income, academic preparation, and college major. A number of studies that examine debt burden, such as monthly debt-to-income ratio, have found that borrowing and career outcomes vary significantly by race and income (Chen & Wiederspan, 2014; Price, 2004; Thomas, 2000, 2003). Collectively, these studies found that lower income students and underrepresented minorities are more likely to have larger debt burdens, though

Price (2004) attributes this difference to lower earnings, rather than higher levels of indebtedness. Notably, these studies only include students who graduated from college.

Beyond student-level characteristics, other studies suggest institutional characteristics are important in shaping student borrowing behavior. For example, students attending private, nonprofit four-year institutions frequently carry greater debt loads than those attending public institutions (Chen & Wiederspan, 2014; Houle, 2014; Price, 2004; Thomas, 2000, 2003). Chen and Wiederspan (2014) have also suggested that location plays an important part in borrowing, as students attending urban colleges or universities—which potentially offer more work opportunities for students while enrolled in school—had lower amounts of debt and higher probabilities of zero debt burden than students attending suburban or rural institutions. Turning to measures of institutional resources, Macy and Terry (2007) found high endowment levels are negatively associated with student debt, which aligns with prior research suggesting wealthier institutions may be better able to subsidize student costs (Winston, 1999). Alternatively, these findings may reflect the ability of wealthier institutions to successfully recruit and enroll students from higher socioeconomic means who are more likely to afford a college education without accruing debt or who have safety nets that enable repayment after college enrollment.

States' strategies for financing higher education— whether need-based or merit based—also affect student borrowing. For example, Monks (2014) found state grant aid was negatively related to student debt, with a 10% increase in state aid per student associated with a 0.5% reduction in average student debt. Similarly, González Canché (2014) examined the impact of Georgia's merit-based financial aid program, Helping Outstanding Pupils Educationally (HOPE) and its impact on student debt, finding loan default rates were significantly lower as a result of the scholarship program. Implemented in 1993, HOPE has been found to be a fundamental factor

in reducing debt accumulation across the college student population in Georgia. Chen and Wiederspan (2014) corroborate this finding, noting broad-based merit aid programs reduced overall student debt burden (4% lower than states without such programs). However, when Georgia was removed from their analysis due to its large provision of merit-based aid, the effect for all students (regardless of state of residency) fell away, leading the authors to refer to this phenomenon as the "Georgia effect." Nevertheless, more research is necessary to better understand the role that state policy plays in student borrowing.

Finally, and most relevant to the current study, several researchers have studied the effects of federal policy changes on student indebtedness. Redd (1994, 1999) found the 1992 Higher Education Act reauthorization permitted greater availability of student aid for middle- and upper-income borrowers, leading to an increase in the number of high-income borrowers and a 53% increase in federal loan debt over just three years. Drawing on data from the National Postsecondary Student Aid Survey (NPSAS), Perna (2001) showed that the increased use of federal loan dollars that occurred after the expansion of the loan program in the early 1990s was concentrated primarily among dependent undergraduate students from middle-income families. Relying upon data from a single institution, Hart and Mustafa (2008) noted students from lower-middle-income and upper-middle-income families were more likely to increase borrowing following an increase in the availability of additional Perkins loan funds. However, low-income students were more likely to borrow more from the Perkins program without increasing overall borrowing, by substituting away from higher-risk loans.

While the aforementioned studies explored the varying determinants of student borrowing, more research is necessary to ensure an empirically robust examination of the ways in which federal policy shapes student borrowing behaviors. Although informative, most prior

research relies upon naïve estimations, often using ordinary least squares or logit models. Dowd (2008) suggests a need for better methodological approaches in studying the interactions between college choice, expectations, and student finance. She calls for "new statistical techniques" in order to "disentangle these complex interactions and complex relationships" between student indebtedness and policy (p. 233). Moreover, despite the previous literature suggesting borrowing behaviors differ significantly by race and socioeconomic status, there is a significant lack of information on the differential effects of financial aid policies on student borrowing by income status and race. Thus, the present study aims to fill these gaps, using nationally representative data, a natural experiment setting, and a quasi-experimental research design to examine how a change in federal loan policy affects student borrowing.

3. Federal policy and student borrowing

In this section, we provide a brief discussion of the context and typology of student loans in the United States. We follow with a description of recent federal loan policy changes, turning specifically to the 2007-08 policy change and potential ways in which students may have reacted to such a change.

Avery and Turner (2012) identify five primary loan types that undergraduate students utilize to offset costs within the U.S. higher education system. Four of these loan types are offered through the federal government (subsidized Stafford loans, unsubsidized Stafford loans, Perkins loans, and parent PLUS loans). The Perkins and subsidized Stafford loans are need-based, and, as such, can only be awarded to students with unmet financial need. The remaining federal programs (unsubsidized Stafford and parent PLUS loans), as well as the fifth loan source, private loans, can be used to pay any additional costs of attendance, including cost-of-living expenses (e.g., books, supplies, etc.). One might expect, and economic theory would predict, that

borrowers would exhaust loan funds from programs with the highest government subsidies and most favorable repayment terms prior to utilizing unsubsidized and/or private loan sources. However, this borrowing behavior is not consistently supported by prior research (Avery & Turner, 2012). In fact, Kantrowitz (2009) estimates that as many as a quarter of undergraduate borrowers take out private loans without using any Stafford loan dollars.

3.1 Changes to federal financial aid policy

In this section, we summarize the most recent changes in federal policy taking place from the 1990s through 2010. After this presentation, we focus on the policy change this study analyzed. The impetus for recent changes in federal higher education policy has been the expansion of federal financial aid programs to the middle class. This expansion began with the 1992 reauthorization of the Higher Education Act, through which Congress not only increased annual loan limits for the subsidized Stafford loan program but also created the unsubsidized Stafford loan. Redd (1999) found that these increased loan limits and the introduction of the new non-need-based loan program resulted in students from upper-income families increasing their borrowing. Subsequent changes to federal programs include the introduction of Academic Competitiveness Grants (ACG) and National Science and Mathematics Access to Retain Talent (SMART) Grants as a result of the Higher Education Reconciliation Act of 2005, both designed to increase and support Pell-eligible students majoring in mathematics, technology, science, engineering, and related fields. In 2007-08, the annual Stafford loan limit was again increased by $875 for dependent freshmen and $1,000 for dependent sophomores, while juniors and seniors experienced no change in their annual or aggregate borrowing limits. Finally, in response to the economic recession in 2008, Congress quickly passed a bill, which went into effect in July 2008,

and allowed students to borrow an additional $2,000 in unsubsidized Stafford loans. The bill also increased the aggregate loan limits for the unsubsidized Stafford loan program.

More recent changes to higher education policy have focused on increasing the availability of information for students and families. The 2008 Higher Education Opportunity Act required all colleges and universities participating in federal financial aid programs to provide a net price calculator on their websites by October 2010, in order to help prospective students and families estimate their financial aid awards and bottom line costs. Additional tools such as the *College Navigator* website and the *College Scorecard* have provided even more detailed and pertinent data to consumers, though recent research suggests that these tools may be disproportionately used by already advantaged students (Hurwitz & Smith, 2016). Ultimately, the federal government has made a concerted effort to provide tools focusing on the provision of standardized metrics to allow students and families to compare estimated costs across multiple institutions. Whether these information-based interventions have a substantial effect or not remains unresolved.

The present study investigates the impact of the 2007-08 changes on Stafford loan limits on student borrowing behavior. As described earlier, these changes were part of the Higher Education Reconciliation Act of 2005 and increased the annual federal borrowing limits effective July 2007. We limit our analyses to these increases in order to avoid the confounding effects of the multiple policy changes mentioned above, such as the significant changes to the student loan program occurring in the early 1990s and some less significant changes that went into effect in July 2008. Moreover, we investigate this time period to avoid conflating the effects of our policy with the Great Recession, which was in full effect by the fall of 2008 (The Economist, 2013).

3.2 Potential student responses to changes in loan limits

Given that the Stafford loan limit increases did not prevent affected students from borrowing from other sources, it is plausible that the total amount borrowed by said participants increased overall. There is some debate about the role that credit constraints play in higher education (Brown, Scholz & Seshardi, 2012; Ionescu & Simpson, 2014; Lochner & Monge-Naranjo, 2011), but one might expect aggregate borrowing to increase if a significant number of students are, in fact, credit constrained. For this overall increase in annual borrowing to have taken place, two conditions must have been met: (a) affected participants' borrowing amounts from the Stafford program should have increased and (b) other non-Stafford loan borrowing amounts from different sources (e.g., parent PLUS, private & Perkins loans) should have also remained at least constant.

A second plausible scenario is that total borrowing across all loan programs would have remained unchanged, given the following conditions: (a) affected participants' borrowing amounts from the Stafford program increased and (b) other non-Stafford loan borrowing amounts from different sources decreased. In other words, those students who are not credit constrained may respond to the policy by utilizing the newfound federal dollars to borrow less from loans without government subsidies or higher interest rates. Given these two possibilities, it remains uncertain whether students are borrowing more annually as a result of the policy or if students are using the additional Stafford loan dollars to supplant borrowing from other sources. Extending this hypothesis, the increase in Stafford loan limits may incentivize some students to work fewer hours while enrolled, so they can focus more on academic pursuits. In fact, recent quasi-experimental evidence suggests that students work fewer hours per week when receiving

scholarship monies (DesJardins, McCall, Ott & Kim, 2010), although whether students respond in similar ways when receiving additional loan dollars remains unknown.

While we are primarily concerned with how the increased Stafford loan limits affected aggregate borrowing and borrowing across the major loan types, there are, of course, other margins that may have been affected by this policy change. For instance, the availability of additional Stafford loan dollars may have impacted the extensive margin, resulting in students who would have otherwise foregone higher education to choose to enroll. Furthermore, even if the policy change did not cause new students to enroll, it may have affected where students decided to enroll (e.g., four vs. two-year institutions, public vs. private four-year institutions, etc.). Because our estimation strategy relies upon cross-cohort comparisons, it is possible that the underlying student composition across cohorts may be systematically different. As such, we conduct a series of robustness checks and falsification tests that suggest the policy change in question is causing the variation in the outcomes of interest.

4. Data

To investigate the effects of increased annual loan limits, we utilize three cross-sections of NPSAS: 1999-00, 2003-04, and 2007-08. NPSAS is administered by the National Center for Education Statistics (NCES) approximately every four years and is comprised of a nationally representative group of students across all postsecondary sectors. The primary advantage of NPSAS is that it includes data from the U.S. Department of Education's central database for federal loans: the National Student Loan Database System (NSLDS). Because all data on federal borrowing is taken directly from this administrative database, our data set includes official, non-self-reported data on all federal borrowing in each time period. Moreover, each iteration of NPSAS includes detailed information on other types of financial aid, background characteristics

reported by students, families, and their respective colleges, and institutional characteristics from the Integrated Postsecondary Education Data System, collectively allowing for the inclusion of a rich set of covariates.

Our dependent variables of interest include: the annual amounts borrowed through the Stafford loan programs, monies borrowed through the parent PLUS program, Perkins loan totals, self-reported private loan totals, annual total borrowing, and the self-reported number of weekly hours worked. The independent variable of interest is enrollment year in college (freshman, sophomore, junior, senior), as we examine how borrowing changed by enrollment year following the policy implementation. Because the number of years a student is enrolled in college can differ from the enrollment year used for federal financial aid eligibility, we use the latter to define a student's academic standing and to ensure that the class-level annual borrowing limits are appropriately defined. We include multiple control variables in our models to account for differences in student borrowing behaviors, as suggested by prior research, including gender, race, family income, and first-generation college student status. Although endogenous to our model of behavior, we show that including college major, net price, and college grade point average do not change our inferences.

In addition to the control variables from NPSAS, we include data from two other sources: a) the U.S. Department of Housing and Urban Development (HUD) and b) Barron's Admissions Competitive Index. We utilize annual fair market rent data from HUD to account for the geographical differences in cost of living. Additionally, controlling for Barron's selectivity allows us to capture variation among different tiers of institutional selectivity, which, as Monks (2014) demonstrated, is associated with lower cumulative debt.

For our analyses, we focus on undergraduate dependent students (those directly affected by the policy), and further limit our sample to U.S. citizens who applied for federal financial aid and attended a four-year institution full-time. Across all three cohorts, our final sample includes 46,440 observations,[1] representing approximately 8 million students.

5. Empirical strategy

The natural experiment setting and repeated cross-section structure of the data allow for the implementation of a DD estimation strategy. In the DD approach, at least two groups are observed across two time periods (T_0, T_1), with T_0 representing the pre-policy change and T_1 as the post-policy change. These two groups are comprised of students who were either affected or were not affected by the policy change. Since the policy was implemented in July 2007, all dependent freshmen and sophomore participants in the 2007-08 survey were exposed to the effects of this policy, and thus represent our treatment group (Tr=1) in the implementation time (T_1). Juniors and seniors were not eligible to take part in this policy change and, therefore, constitute the control group (Tr=0). Treatment and control status does not change across time, allowing for the estimation of the DD as a collection of conditional means for the outcome of interest. Since we can identify Tr_i at T_1 and T_0, the DD is estimated as follows:

$$DD=[E(Y|Tr=1,T_1)- E(Y|Tr=1,T_0)] - [E(Y|Tr=0,T_1)- E(Y| Tr=0,T_0)], \quad (1)$$

where Y is the outcome of interest across time and treatment statuses. The regression-based form of the equation is

$$Y = \beta_0 + \beta_1 Tr + \beta_2 T_i + \beta_3 (Tr_i * T_i) + \beta_j X_j + u_i \quad (2)$$

where β_3 is the coefficient that captures the DD from equation (1). The coefficient of interest may be affected by other important predictors and control variables, as well as by unobserved

[1] Per NCES guidelines, all sample sizes are rounded to the nearest 10.

state characteristics, all of which are accounted for in β_j. As mentioned, the data set built to estimate the DD models was taken from three cross-sections, which are then pooled. Accounting for survey design effects across three iterations of NPSAS complicates our estimation strategy, as we cannot simply apply a Taylor linearization procedure or a balanced repeated replication approach to account for the two-stage sampling design. Instead, we follow a similar approach to Domina (2012) by clustering our heteroscedasticity robust standard errors at the primary sampling unit (postsecondary institution) by year, while still making use of the weights provided by NPSAS to account for non-random panel attrition and the oversampling of underrepresented populations.

Because the greatest threat to identification in a pooled DD is a contemporaneous change in the treatment group that coincides with the implementation of the policy being investigated, we take a number of approaches to ensure we are not conflating the effects of the policy with changes in the composition of the sample. One strategy we employ is to estimate a fully interacted version of equation (2), as suggested by Duflo (2004). Through this approach, we interact each covariate with dummy indicators for the second and third time periods to account for the changes in the composition of the control and treatment groups across the three cross-sections of data. Additional strategies to account for threats to the internal validity of our research design are discussed in Section 6.2.

To answer our research questions, we first estimated equation (2) with Stafford borrowing (including subsidized and unsubsidized loans) as the outcome of interest. If β_3 results in a positive point estimate, freshmen and sophomores did borrow more from the Stafford loan program in 2007-08, which we argue was a result of the policy change. Examining our other outcomes, we first estimated models with total borrowing (e.g., Stafford, Perkins, parent PLUS,

and private borrowing combined) as the outcome of interest and then estimated models for each additional borrowing source as well as for work hours. If the coefficient of interest was statistically significant in the total borrowing models, the treated students did borrow more after the policy implementation. However, if β_3 was negligible or if the effect is not statistically significant, students did not borrow more in the aggregate and the increases in Stafford borrowing will be met with concurrent decreases in borrowing from other loan sources.

6. Results

Descriptive statistics for our sample, provided in Table 1, include all dependent variables and control variables, aggregated by treatment status (freshmen/sophomores vs. juniors/seniors), as well as pre- and post-policy periods. All dollar amounts are presented in 2007 constant dollars.

INSERT TABLE 1 HERE

Reviewing Table 1, we see that Whites comprise the majority of the sample across all three cohorts. Moreover, women outnumber men, the majority of students attend college in their state of residence, and net prices increased over the years examined. Many of our covariates remained relatively constant across the pre- and post-treatment periods, with family income and first-generation status remaining virtually unchanged. Examining our dependent variables, total borrowing increased across both the treatment and control groups with underclassmen increasing their borrowing by about 24% and upperclassmen borrowing about 15% more in the aggregate. Both groups also borrowed significantly more from private loan sources and the parent PLUS program in the latter time period. Finally, first-year students and sophomores borrowed more from the Stafford loan program, while upperclassmen decreased their borrowing over time.

Table 2 provides the treatment effects for each of our six outcomes of interest without covariates. We find aggregate annual borrowing in the Stafford loan program increased by $974, on average, as a result of the policy change, while private and parent PLUS borrowing both declined significantly (by $242 and $399, respectively), Perkins borrowing declined marginally ($51), but the policy had no effect on the amount of hours worked. Finally, we ran models with all loan types aggregated into one outcome variable to determine if total student borrowing increased as a result of the policy change. Our analysis revealed no statistically significant effect of the policy on aggregate student borrowing. Additionally, the magnitude of the coefficient, $283, did not approach the increases in loan limits.

INSERT TABLE 2 HERE

The results from Table 3, which control for demographic characteristics, tell a similar story, with slightly more precise estimates. The models in columns 1-6 include pre-treatment control variables, including cohort effects, while the subsequent columns present the fully interacted models, which also include state-year fixed effects. The results are remarkably consistent across both specifications, with none of the DD coefficients becoming statistically distinguishable from one another. Again, we find no evidence to suggest that students altered their work hours in response to the new policy.

INSERT TABLE 3 HERE

Concerned that omitted variables may be accounting for some of these cross-cohort changes, we include an additional set of covariates in Table 4. While some of these variables are likely to be jointly determined with a student's borrowing decision, we wanted to ensure other unobserved factors were not accounting for the changes we are attributing to the increased loan limits. Some of these additional variables include county-level rent estimates from HUD to

partially control for costs-of-living, residency status while enrolled, and a measure of net price (tuition and fees less grant aid) to account for the increases in price over the duration of our study. In reviewing these results, the increases in borrowing from the Stafford loan program are similar to the decreases in borrowing across the other loan types. Moreover, as in our prior specifications, the change in total borrowing remained statistically indistinguishable from zero.

INSERT TABLE 4 HERE

6.1 Heterogeneous effects

Prior research has noted that borrowing may vary significantly by race and income (Chen & Wiederspan, 2014; Price, 2004; Thomas, 2000, 2003). As displayed in Table 5, we find little evidence that students are responding differentially to the change in loan limits. White and Black students are primarily driving the increases in borrowing through the Stafford loan program, with Asian and Hispanic students borrowing considerably less. Testing for the differences in coefficients across subpopulations using Chow tests confirms that Whites did borrow more through the Stafford loan program than their Asian and Hispanic counterparts; however, there are no statistically distinguishable differences among other groups. Moreover, Chow tests comparing each subgroup to their counterpart (e.g., male to female, first-generation to non-first-generation, etc.) resulted in no statistically significant differences for each of the loan types, except those previously mentioned.[2]

INSERT TABLE 5 HERE

6.2 Robustness tests

We performed a number of robustness checks to confirm our main findings. First, DD relies upon the assumption that the external shock resulting from the policy change is the main

[2] Results available upon request.

factor affecting the variation in the outcome of interest across affected participants. Thus, in the absence of this policy change, the outcomes of both treated and control participants should have remained unchanged. To corroborate this assumption, we first examined the parallel trends across treatment and control groups for each outcome of interest. While the parallel trends assumption cannot be formally tested, we provide those figures for reference (see Figure 1).

INSERT FIGURE 1 HERE

Second, we estimated a placebo test for each of the models, as suggested by Bertrand, Duflo, and Mullainathan (2004). In the placebo test framework, researchers remove all observations occurring after the real policy change took place and artificially establish a new time for a false policy implementation prior to the actual policy implementation. The treatment and control statuses of the students are maintained, while the coefficient of interest is created using the false time for policy implementation. In this study, the time observations corresponding to 1999-2000 and 2003-04 were measured before the policy change occurred in 2007. Accordingly, we omitted the students sampled during 2007-08 academic year, instead setting 1999 as T_0 and 2003 as T_1. Freshmen and sophomores remained in the treated group, while juniors and seniors continued to constitute the control group. Within this framework, the coefficient of interest (β_3) shown in equation (2) remains the same, but in the placebo test the coefficient should either be non-significant or have the opposite sign compared to the sign observed in the original DD model. If the results from the placebo test mirror the magnitude and direction of β_3 found in the real DD, then the significance associated with the policy change was merely the result of prior trends in the data or factors unaccounted for in the model and not the result of the policy change itself. Placebo tests for each of our outcomes resulted in insignificant

coefficients for those models in which there was a significant effect to begin with. These analyses can be found in Appendix Table 1.

A change in the composition of either group across the three cohorts can present a threat to the internal validity of our research design. One way to test for the presence of such changes is to run a model similar to equation (2) with the selected covariates as the outcomes (Duflo, 2004). Should the DD estimate result in a statistically significant coefficient, we may be concerned that changes in the composition of the sample may instead be responsible for the effects we are attributing to the change in policy. Our results show that the distribution of racial groups, first-generation students, and women did not change between the pre- and post-policy implementation years. Moreover, it appears that first- and second-year students did not enroll in more expensive nor more selective institutions after the policy went into effect. These results, found in Appendix Table 2, add credence to our conclusion that the change in annual loan limits is leading to the observed changes in borrowing.

Although there are little to no changes in the observable characteristics that coincide with the policy, there may be unobservable characteristics that vary over time and, as such, are not captured by our analytic approach. One such concern may arise if students are induced to enroll in the four-year sector as a result of the policy. If these students differ in unobservable ways from pervious cohorts of first-year students, then we may be naively attributing the changes in borrowing behavior to the increase in loan limits, as opposed to a different type of student enrolling as a result of the increased limits. A novel way of testing this theory is to remove first-time entrants from our analyses and see if the results still hold. By excluding new entrants from the treatment group, we attempt to mitigate some concerns about selection. In Column 2 of Table 6 we see that Stafford borrowing remains relatively constant after excluding new entrants.

However, aggregate borrowing, which bordered on statistical significance across many of the previous model specifications, increases in magnitude by nearly $500 and becomes statistically significant at the .05 level. Nevertheless, this amount accounts for only half of the increase in annual loan limits for sophomore students therefore not constituting a significant concern in terms of overall debt burden among affected sophomore participants.

INSERT TABLE 6 HERE

Another way to address concerns regarding extensive margin of attendance is to estimate a lower bound for the effect of the policy. Little evidence exists on the demand elasticity of loans in the United States, but we know that the demand elasticity of grants is approximately 5% for each $1,000 decrease in net price (Dynarski, 2002, 2003; Leslie & Brinkman, 1987). While it seems unlikely that loans would have a similar effect, we can proceed with another thought experiment, assuming that 5% is the upper bound on the elasticity of student loans. Therefore, in this robustness test, we assume that a proportion of students were enticed to enroll in a college or university as a result of the policy, remove them from the sample, and then rerun the same analyses. As this policy increased the availability of Stafford loans by approximately $1,000, we remove the 5% of new entrants who borrowed the most from the Stafford loan program and rerun our preferred model specification. In Table 6, Column 3, we see that dropping these students who may have enrolled as a result of the increased loan limits does not change our inferences.

Our final robustness test investigates whether a change in the number of parents who were deemed ineligible to receive PLUS loans may be driving our results. As discussed in Section 3, students whose parents are unable to receive a PLUS loan due to an adverse credit history are then eligible to receive additional Stafford loan dollars. As long as the distribution of

students taking advantage of this policy did not change across our pre- and post-policy implementation years, then these students should have no effect on our estimates. To test this proposition, we remove those students from our analyses. The results in Column 4 of Table 6 suggest that this subgroup of students still do not borrow more in the aggregate as a result of the annual loan limit increase.

6.3 Limitations

Despite attempts to reduce bias, this study is limited in three ways. First, our analysis was restricted by the availability and clarity of the data. While NPSAS provides a nationally representative group of students, incomplete tracking data on individual students remain. For example, students who attended more than one institution, whether due to institutional transfer or dual enrollment, were treated as if they only attended the first institution.

We were unable to include students from two-year institutions in our study for several reasons. First, two-year students lack an equivalent control group akin to juniors and seniors at four-year institutions that did not receive an increase in their Stafford loan limit. Second, approximately ten percent of community colleges do not participate in Title IV loan programs (TICAS, 2014). Thus, our findings would not be applicable to all students attending two-year schools and determining which students in our sample are eligible for Title IV loans in each of the three cross-sections would be impossible. As two-year institutions enroll a significant proportion of underrepresented minority, first-generation, and low-income students, it is possible that students' borrowing experiences at these institutions may vary with respect to our findings from the four-year sector.

Finally, while NPSAS includes the most detailed information available on federal borrowing behavior, we are unable to account for other forms of borrowing that students may

undertake. For instance, students may be informally borrowing money from family members or friends. Earlier iterations of NPSAS attempted to account for this behavior through the questionnaire, but changes in the composition of relevant questions, coupled with non-response, make it a difficult variable to include in our models. Furthermore, parents may be borrowing to help pay for college expenses in more ways than just through the PLUS program, including home equity loans or less secure forms of debt such as credit cards. In fact, students themselves may be relying upon credit cards to fund their college costs. While NPSAS includes questions pertaining to credit card use, the changes in questions related to credit card use over the span of our sample make it difficult to include as a variable in our study.

7. Conclusion

Overall, our findings suggest that changes to federal loan limits do affect student borrowing. We find that although aggregate annual borrowing did not increase as a result of these federal policy changes, families replaced parent PLUS and private borrowing with the newly available Stafford loan dollars. Therefore, students and families did not borrow more as a result of the policy, but instead, substituted away from higher cost and higher risk and costlier credit options.

This behavior, while empirically interesting, also has broader implications for America's higher education funding model, particularly given the rhetoric surrounding the current student loan debate. Our results suggest students may be exercising greater self-control or perhaps even greater financial literacy than some (King, 1999; Redd, 1999) suggest. In light of these findings, policymakers should contemplate raising federal loan limits, particularly for those students likely to substitute away from private loans with less favorable terms. Unlike many other higher education finance issues, legislators have a distinct policy lever available that can affect specific

borrowing behaviors of dependent undergraduate students. Thus, the heterogeneous response and its potential deleterious effects across various student populations (e.g., graduate students, independent students) could be avoided. That said, policymakers must weigh the obvious benefit of providing lower-risk credit to students with the potential for postsecondary institutions to capture those readily available loan dollars through tuition and fee increases.

In attempting to explain the null findings for our hypothesis regarding number of hours worked, we posit that the variation in substitution effects may result from a difference in how students allocate their funds. With respect to the amount of funding received and delivery mechanism, federal loans are remarkably akin to private loans and, therefore, may simply represent an equivalent or comparable good with lower costs. As such, loan money, regardless of source, may be utilized to pay for costs of attendance, such as tuition, fees, and housing, whereas funds generated from work hours might be used for consumption or leisurely activities. Regardless of the availability of federal loan dollars, students may view work hours as an independent source of funding outside of financial aid, opting to continue to work to pay for smaller, everyday expenses. There is evidence from the behavioral economics literature to support such a conclusion (e.g., mental accounting from Thaler, 1999), but our data does not allow for the testing of such a hypothesis.

In conclusion, this study aimed to advance our understanding of federal financial aid policies and their potential impacts on student borrowing. A specific federal government policy shifted students' borrowing behaviors by enticing them to borrow less from costlier loan options and rely more heavily on Stafford loans. While this study focused on the effects of the Stafford loan limit increase that occurred in 2007, several questions remain for future research. For example, what are the long-term consequences of raising federal loan limits, such as delinquency

and default rates, as well as student attrition and graduation? Moreover, will institutions respond to these increased loan limits by subsequently increasing their tuition and fees, as some researchers have suggested? Finally, are our findings generalizable to the two-year sector or financially independent undergraduates? These are important factors to consider in future analyses of student borrowing, but remain outside the scope of this study. In light of growing trends in student debt burden and the upcoming Higher Education Act reauthorization, it is critical to understand the implications of federal policies on student borrowing behaviors. Evidence from our study suggests that students do borrow more from the Stafford loan program when limits are increased; however, they use these newfound monies to borrow less from private loan sources and the parent PLUS program, rather than increasing their total borrowing.

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Table 1: Summary Statistics of 4-year Dependent Undergraduates

	Pre-Period						Post-Period					
	Freshman & Sophomore			Junior & Senior			Freshman & Sophomore			Junior & Senior		
	Obs.	*Mean*	*Std. Dev.*	*Obs.*	*Mean*	*Std. Dev.*	*Obs.*	*Mean*	*Std. Dev.*	*Obs.*	*Mean*	*Std. Dev.*
Dependent Variables												
Total Loans	13,170	5542.232	6325.537	9,320	7133.103	6241.010	9,300	6945.007	7617.096	14,640	8253.331	7688.582
Stafford Loans	13,170	2673.318	2133.329	9,320	4827.469	3108.920	9,300	2781.233	2185.872	14,640	3960.992	2675.831
Private Loans	13,170	801.686	3028.702	9,320	870.308	3085.938	9,300	2045.761	4934.892	14,640	2356.009	5015.106
PLUS Loans	13,170	1729.599	4737.794	9,320	1193.400	3847.689	9,300	1869.491	5148.444	14,640	1732.566	4990.264
Perkins Loans	13,170	337.629	899.653	9,320	241.926	806.827	9,300	248.523	749.175	14,640	203.764	709.038
Work Hours	12,430	13.404	13.177	8,360	17.018	13.171	9,300	13.010	13.398	14,640	16.303	13.449
Background Variables												
White	13,170	0.727	0.446	9,320	0.746	0.435	9,300	0.687	0.464	14,640	0.713	0.452
Black	13,170	0.118	0.322	9,320	0.102	0.302	9,300	0.124	0.330	14,640	0.097	0.296
Latino	13,170	0.074	0.261	9,320	0.070	0.255	9,300	0.104	0.305	14,640	0.095	0.293
Asian	13,170	0.045	0.208	9,320	0.049	0.217	9,300	0.049	0.216	14,640	0.064	0.245
Native American	13,170	0.006	0.077	9,320	0.006	0.076	9,300	0.006	0.080	14,640	0.004	0.066
Other Race	13,170	0.010	0.101	9,320	0.011	0.105	9,300	0.003	0.051	14,640	0.002	0.040
Multiracial	13,170	0.020	0.141	9,320	0.016	0.124	9,300	0.027	0.162	14,640	0.025	0.156
Female	13,170	0.553	0.497	9,320	0.574	0.495	9,300	0.545	0.498	14,640	0.569	0.495
High Income	13,170	0.262	0.440	9,320	0.260	0.439	9,300	0.275	0.447	14,640	0.275	0.447
Low Income	13,170	0.231	0.422	9,320	0.234	0.423	9,300	0.258	0.438	14,640	0.251	0.434
Continuous Income	13,170	79881.680	57695.180	9,320	78560.830	55150.060	9,300	79293.160	59933.920	14,640	79799.820	57322.110
First Generation	13,170	0.477	0.499	9,320	0.468	0.499	9,300	0.477	0.499	14,640	0.466	0.499
Same State	13,170	0.771	0.420	9,320	0.778	0.415	9,300	0.823	0.382	14,640	0.838	0.368
College GPA	13,170	2.829	0.748	9,320	3.016	0.623	9,300	2.898	0.725	14,640	3.129	0.544
Institution Variables												
Public	13,170	0.635	0.481	9,320	0.632	0.482	9,300	0.638	0.481	14,640	0.660	0.474
Private Non-Profit	13,170	0.344	0.475	9,320	0.358	0.480	9,300	0.335	0.472	14,640	0.330	0.470
For-Profit	13,170	0.015	0.123	9,320	0.007	0.083	9,300	0.025	0.155	14,640	0.010	0.098
Highly Selective	13,170	0.170	0.376	9,320	0.201	0.401	9,300	0.137	0.343	14,640	0.182	0.385
Moderately Selective	13,170	0.727	0.445	9,320	0.729	0.445	9,300	0.737	0.440	14,640	0.744	0.437
Less Selective	13,170	0.103	0.303	9,320	0.070	0.255	9,300	0.127	0.333	14,640	0.075	0.263
Net Price	11,820	6113.450	6863.822	8,450	6635.623	6868.145	7,890	6947.160	7778.560	12,730	7219.920	7636.851
On Campus	13,170	0.612	0.487	9,320	0.373	0.484	9,300	0.589	0.492	14,640	0.342	0.474
Off Campus	13,170	0.176	0.381	9,320	0.430	0.495	9,300	0.168	0.374	14,640	0.420	0.494
Off Campus w/Parents	13,170	0.180	0.384	9,320	0.164	0.370	9,300	0.157	0.364	14,640	0.153	0.360
Rent	12,990	774.125	235.724	9,210	780.476	233.519	9,250	779.847	244.039	14,580	791.528	248.709

Notes: Means are weighted by NPSAS sample weights. All dollar amounts are in 2007 constant dollars. Per NCES guidelines, unweighted observations are rounded to 10. Institutional selectivity is determined using Barron's 2008 Admissions Competitive Index.

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Table 1: Summary Statistics of 4-year Dependent Undergraduates

	Pre-Period						Post-Period					
	Freshman & Sophomore			Junior & Senior			Freshman & Sophomore			Junior & Senior		
	Obs.	*Mean*	*Std. Dev.*	*Obs.*	*Mean*	*Std. Dev.*	*Obs.*	*Mean*	*Std. Dev.*	*Obs.*	*Mean*	*Std. Dev.*
Dependent Variables												
Total Loans	13,170	5542.232	6325.537	9,320	7133.103	6241.010	9,300	6945.007	7617.096	14,640	8253.331	7688.582
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Notes: Means are weighted by NPSAS sample weights. All dollar amounts are in 2007 constant dollars. Per NCES guidelines, unweighted observations are rounded to 10. Institutional selectivity is determined using Barron's 2008 Admissions Competitive Index.

Table 1: Summary Statistics of 4-year Dependent Undergraduates

	Pre-Period						Post-Period					
	Freshman & Sophomore			Junior & Senior			Freshman & Sophomore			Junior & Senior		
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Institution Variables												
Public	13,170	0.635	0.481	9,320	0.632	0.482	9,300	0.638	0.481	14,640	0.660	0.474
Private Non-Profit	13,170	0.344	0.475	9,320	0.358	0.480	9,300	0.335	0.472	14,640	0.330	0.470
For-Profit	13,170	0.015	0.123	9,320	0.007	0.083	9,300	0.025	0.155	14,640	0.010	0.098
Highly Selective	13,170	0.170	0.376	9,320	0.201	0.401	9,300	0.137	0.343	14,640	0.182	0.385
Moderately Selective	13,170	0.727	0.445	9,320	0.729	0.445	9,300	0.737	0.440	14,640	0.744	0.437
Less Selective	13,170	0.103	0.303	9,320	0.070	0.255	9,300	0.127	0.333	14,640	0.075	0.263
Net Price	11,820	6113.450	6863.822	8,450	6635.623	6868.145	7,890	6947.160	7778.560	12,730	7219.920	7636.851
On Campus	13,170	0.612	0.487	9,320	0.373	0.484	9,300	0.589	0.492	14,640	0.342	0.474
Off Campus	13,170	0.176	0.381	9,320	0.430	0.495	9,300	0.168	0.374	14,640	0.420	0.494
Off Campus w/Parents	13,170	0.180	0.384	9,320	0.164	0.370	9,300	0.157	0.364	14,640	0.153	0.360
Rent	12,990	774.125	235.724	9,210	780.476	233.519	9,250	779.847	244.039	14,580	791.528	248.709

Notes: Means are weighted by NPSAS sample weights. All dollar amounts are in 2007 constant dollars. Per NCES guidelines, unweighted observations are rounded to 10. Institutional selectivity is determined using Barron's 2008 Admissions Competitive Index.

Table 2: Diff-in-Diff Estimates

	(1)	(2)	(3)	(4)	(5)	(6)
	Stafford loans	*Private loans*	*PLUS loans*	*Perkins loans*	*Total loans*	*Work hours*
Post	-866.477***	1,485.700***	539.166***	-38.162*	1,120.228***	-0.715*
	(70.510)	(111.419)	(106.298)	(18.806)	(193.813)	(0.300)
Treated	-2,154.151***	-68.622	536.199***	95.704***	-1,590.871***	-3.614***
	(48.077)	(50.763)	(69.486)	(15.850)	(108.154)	(0.239)
Treated*Post	974.391***	-241.626*	-399.274**	-50.945*	282.547	0.320
	(67.689)	(117.351)	(127.947)	(22.124)	(188.831)	(0.351)
Constant	4,827.469***	870.308***	1,193.400***	241.926***	7,133.103***	17.018***
	(49.342)	(46.589)	(51.261)	(12.591)	(99.081)	(0.210)
Observations	46,440	46,440	46,440	46,440	46,440	44,740
R-squared	0.117	0.028	0.003	0.004	0.020	0.017

Notes: Regressions are weighted by NPSAS sample weights. Standard errors are adjusted for heteroskedasticity and correlation within institution-year cells (*** p<0.001, ** p<0.01, * p<0.05). All dollar amounts are in 2007 constant dollars.

Table 3: Diff-in-Diff Estimates w/Pre-Treatment Controls

	(1) *Stafford loans*	(2) *Private loans*	(3) *PLUS loans*	(4) *Perkins loans*	(5) *Total loans*	(6) *Work hours*	(7) *Stafford loans*	(8) *Private loans*	(9) *PLUS loans*	(10) *Perkins loans*	(11) *Total loans*	(12) *Work hours*
Black	798.228***	-142.659	246.751*	-72.293***	830.027***	-0.485	920.545***	151.624*	329.128	-32.930	1,065.119***	-0.453
	(78.304)	(79.796)	(102.316)	(21.357)	(165.282)	(0.300)	(140.306)	(74.320)	(169.401)	(41.968)	(243.776)	(0.730
Hispanic	-317.125***	-38.250	51.645	-80.571***	-384.302	0.792*	-185.306	80.229	-2.728	3.524	-104.281	0.194
	(79.716)	(134.052)	(101.920)	(21.588)	(217.268)	(0.329)	(205.108)	(185.441)	(210.924)	(54.386)	(380.551)	(0.801
Asian	-723.164***	-462.914***	-35.686	33.917	1,187.847***	-3.376***	-291.481	-176.215	-243.750	93.411	-618.035	-4.247**
	(78.894)	(105.918)	(144.206)	(29.876)	(212.582)	(0.403)	(174.548)	(133.340)	(264.442)	(82.335)	(347.082)	(0.902
Native Am.	-412.530	-501.032*	908.084***	-85.356	1,907.001***	-0.522	-184.704	123.197	-401.477	-34.968	-497.952	-1.775
	(225.132)	(199.175)	(246.553)	(51.790)	(451.193)	(1.077)	(596.798)	(425.973)	(597.726)	(142.795)	(1,064.973)	(2.762
Other	-514.943**	153.007	-35.084	70.783	-326.238	0.235	-858.215*	-66.901	31.743	194.663	-698.709	-2.506
	(191.151)	(332.407)	(297.647)	(71.931)	(520.869)	(0.990)	(357.297)	(262.582)	(595.585)	(151.754)	(782.378)	(1.368
Multiracial	10.684	-87.489	168.820	68.639	160.654	-1.150*	-66.614	42.367	-265.063	-12.850	-302.159	0.430
	(121.180)	(147.309)	(189.259)	(40.897)	(304.287)	(0.536)	(243.538)	(214.623)	(310.596)	(88.177)	(513.214)	(1.333
Female	7.857	-123.024*	-96.971	7.113	-205.025*	0.840***	37.222	-26.928	-92.288	24.812	-57.182	0.328
	(32.686)	(58.276)	(62.101)	(10.702)	(100.768)	(0.172)	(70.099)	(64.812)	(100.431)	(22.269)	(147.958)	(0.379
Family Income	-14.537*	118.050***	261.922***	-54.388***	311.046***	-0.490***	-7.295	38.599**	211.540***	-59.668***	183.175***	-0.465**
	(6.449)	(9.323)	(11.480)	(2.403)	(17.275)	(0.032)	(13.532)	(12.272)	(19.219)	(4.514)	(27.405)	(0.074
First-Gen	325.209***	314.528***	-176.724**	-43.170***	419.842***	2.043***	325.421***	164.351*	-197.041*	-21.929	270.801	2.211**
	(32.972)	(65.764)	(61.100)	(11.351)	(104.091)	(0.180)	(67.616)	(69.850)	(90.342)	(24.180)	(148.275)	(0.390
2003-04	-158.169*	365.403***	290.652***	46.591*	544.477**	0.733*	-740.438	-113.719	-289.913	-28.244	-1,172.314	1.400
	(64.773)	(67.936)	(87.727)	(22.201)	(166.495)	(0.312)	(512.106)	(256.506)	(809.380)	(103.326)	(1,204.892)	(2.856
2007-08	-929.238***	1,631.166***	548.556***	19.839	1,270.324***	0.041	-1,066.965**	-133.527	-745.244	-167.821*	-2,113.557*	-0.871
	(75.615)	(108.870)	(110.056)	(21.968)	(198.431)	(0.337)	(402.182)	(332.677)	(755.240)	(79.972)	(1,061.629)	(2.680
Treated	2,172.769***	-69.558	533.256***	97.977***	1,611.093***	-3.624***	2,154.297***	-69.561	550.410***	98.594***	,574.854***	-3.715**
	(47.084)	(49.915)	(67.650)	(15.937)	(105.749)	(0.234)	(46.103)	(48.936)	(65.581)	(15.416)	(100.163)	(0.234
Treated* Post	960.207***	-236.466*	-375.373**	-55.137*	293.232	0.235	940.076***	268.882*	-380.223**	-55.992**	234.979	0.289
	(66.125)	(115.744)	(125.943)	(22.111)	(183.772)	(0.343)	(63.566)	(111.747)	(124.753)	(21.492)	(175.116)	(0.337
Constant	4,835.102***	-111.335	504.405***	584.646***	4,804.008***	18.418***	4,784.752***	-95.053	488.617***	587.947***	4,789.028***	18.422*
	(76.828)	(91.074)	(111.186)	(26.809)	(187.806)	(0.383)	(77.450)	(93.210)	(112.278)	(27.510)	(191.277)	(0.385
Time-Varying Covariates	N	N	N	N	N	N	Y	Y	Y	Y	Y	Y
State FEs	N	N	N	N	N	N	Y	Y	Y	Y	Y	Y
Observations	46,440	46,440	46,440	46,440	46,440	44,740	46,440	46,440	46,440	46,440	46,440	44,740
R-squared	0.138	0.038	0.032	0.037	0.040	0.043	0.168	0.072	0.051	0.068	0.078	0.060

Notes: Regressions are weighted by NPSAS sample weights. Standard errors are adjusted for heteroskedasticity and correlation within institution-year cells (*** p<0.001, **

p<0.01, * p<0.05). All dollar amounts are in 2007 constant dollars. Estimates with time-varying covariates include each covariate interacted with time dummies for 2003-04 and 2007-08, including state fixed-effects.

Table 4: Diff-in-Diff Estimates with Additional Controls

	(1) *Stafford*	(2) *Private*	(3) *PLUS*	(4) *Perkins*	(5) *Total loans*	(6) *Work hours*
Black	761.556***	-179.523	107.029	-29.256	659.806**	-0.655
	(141.671)	(92.035)	(171.373)	(43.063)	(221.570)	(0.754)
Hispanic	-160.878	68.148	57.020	30.318	-5.392	-1.161
	(184.511)	(195.038)	(228.929)	(52.870)	(367.985)	(0.871)
Asian	-138.940	-205.080	-183.472	57.336	-470.156	-4.148***
	(188.557)	(149.093)	(283.834)	(78.663)	(362.892)	(0.940)
Native American	-566.912	217.080	-799.173	-88.984	-1,237.989	-2.834
	(464.066)	(465.063)	(465.454)	(145.259)	(970.463)	(2.948)
Other	-803.561*	-144.684	-114.327	203.729	-858.844	-3.985**
	(338.766)	(297.395)	(617.791)	(154.129)	(786.401)	(1.278)
Multiracial	-96.627	53.581	-349.231	-50.842	-443.119	1.558
	(237.446)	(221.647)	(315.145)	(90.819)	(486.139)	(1.362)
Female	-2.652	-3.382	-91.555	21.209	-76.380	0.900*
	(75.176)	(71.977)	(108.797)	(24.675)	(150.767)	(0.384)
Family Income	-41.091**	-0.160	91.504***	-65.435***	-15.183	-0.288***
	(14.844)	(13.924)	(20.646)	(5.229)	(30.214)	(0.083)
First-Generation	380.021***	249.454**	-51.180	33.853	612.148***	1.190**
	(69.518)	(76.430)	(95.384)	(23.944)	(148.334)	(0.380)
Public	3,583.237***	367.699	-1,996.983	-86.373	-5,298.894***	2.749
	(762.822)	(295.601)	(1,064.707)	(92.532)	(905.705)	(4.090)
Less Selective	-2.797	147.603	321.046	-376.093***	89.759	5.030***
	(180.981)	(180.610)	(234.887)	(64.116)	(419.135)	(0.815)
Moderately Selective	356.621***	58.417	108.237	-281.147***	242.128	2.823***
	(102.546)	(113.322)	(172.154)	(47.123)	(269.734)	(0.495)
Net Price	0.047***	0.043***	0.140***	-0.004	0.226***	0.000
	(0.009)	(0.011)	(0.016)	(0.003)	(0.024)	(0.000)
Same State	-53.374	86.121	-404.653*	-139.475***	-511.381*	0.807
	(93.011)	(116.825)	(159.551)	(38.638)	(237.107)	(0.451)
College GPA	-315.166***	-33.758	-77.319	-19.396	-445.639***	-1.611***
	(50.347)	(54.568)	(75.638)	(17.512)	(99.582)	(0.282)
Off Campus	141.645	-28.070	-298.510*	-23.011	-207.947	4.782***
	(85.915)	(80.567)	(116.942)	(27.194)	(171.770)	(0.458)
Off Campus w/Parents	-607.881***	-163.852	638.145***	-144.953***	-1,554.831***	7.672***
	(100.932)	(86.371)	(133.555)	(28.020)	(197.850)	(0.605)
Rent	-0.273	0.312	0.731	0.146	0.917	0.004*
	(0.316)	(0.317)	(0.497)	(0.108)	(0.734)	(0.002)
2003-04	-886.688	2,404.019**	-763.557	251.601	1,005.375	10.367
	(1,005.041)	(816.000)	(1,545.933)	(243.256)	(1,793.895)	(5.692)
2007-08	-1,854.345	4,563.753**	-2,768.266	-98.046	-156.904	8.796
	(969.432)	(1,470.665)	(1,658.483)	(220.248)	(2,028.770)	(5.694)
Treated	2,117.437***	-57.729	488.627***	93.107***	-1,593.432***	-2.923***
	(52.239)	(54.517)	(72.543)	(17.389)	(106.384)	(0.271)
Treated* Post	850.280***	-429.494***	-429.123**	-45.950	-54.287	0.220
	(72.272)	(125.563)	(141.254)	(23.710)	(192.131)	(0.389)
Constant	8,979.099***	-786.626	2,731.923	964.406***	11,888.801***	7.857
	(876.955)	(542.562)	(1,405.399)	(169.275)	(1,429.296)	(5.073)

Time-Varying	Y	Y	Y	Y	Y	Y
State FEs	Y	Y	Y	Y	Y	Y
College Major FEs	Y	Y	Y	Y	Y	Y
Observations	40,370	40,370	40,370	40,370	40,370	38,800
R-squared	0.224	0.151	0.105	0.123	0.220	0.106

Notes: Regressions are weighted by NPSAS sample weights. Standard errors are adjusted for heteroskedasticity and correlation within institution-year cells (*** p<0.001, ** p<0.01, * p<0.05). All dollar amounts are in 2007 constant dollars. All estimates include covariates interacted with time dummies for 2003-04 and 2007-08, including state fixed-effects.

Table 5: Heterogeneous Effects of Increased Loan Limits

	(1) *First-Generation*	(2) *White*	(3) *Asian*	(4) *Black*	(5) *Hispanic*	(6) *Male*	(7) *Female*	(8) *Low-Income*	(9) *High-Income*
	Outcome = Total Borrowing								
Treated*Post	329.107	237.123	-88.080	393.637	344.376	301.324	214.228	106.490	75.441
	(241.628)	(214.380)	(692.557)	(460.684)	(599.833)	(276.860)	(220.078)	(247.240)	(393.559)
Observations	22,410	32,940	2,770	5,190	3,940	19,890	26,550	13,460	11,440
R-Squared	0.112	0.081	0.112	0.132	0.151	0.081	0.089	0.095	0.081
	Outcome = Stafford Borrowing								
Treated*Post	932.504***	1,047.847***	451.509	728.566***	564.426*	943.381***	936.434***	707.360***	1,165.698***
	(89.943)	(69.004)	(284.337)	(214.630)	(240.315)	(98.441)	(80.754)	(131.427)	(116.907)
Observations	22,410	32,940	2,770	5,190	3,940	19,890	26,550	13,460	11,440
R-Squared	0.174	0.183	0.184	0.162	0.178	0.170	0.177	0.172	0.214
	Outcome = Private Borrowing								
Treated*Post	-343.494	-336.301*	-198.826	138.415	-23.909	-42.344	-429.591**	-170.160	-215.886
	(182.279)	(141.285)	(343.422)	(260.817)	(399.268)	(206.612)	(136.005)	(145.272)	(221.080)
Observations	22,410	32,940	2,770	5,190	3,940	19,890	26,550	13,460	11,440
R-Squared	0.092	0.078	0.107	0.126	0.104	0.077	0.079	0.054	0.081
	Outcome = PLUS Borrowing								
Treated*Post	-215.323	-417.735**	-188.356	-407.991	-176.031	-580.697**	-213.754	-386.629**	-861.363**
	(141.942)	(155.759)	(435.986)	(306.178)	(348.433)	(198.289)	(146.931)	(117.390)	(309.098)
Observations	22,410	32,940	2,770	5,190	3,940	19,890	26,550	13,460	11,440
R-Squared	0.068	0.048	0.106	0.114	0.130	0.055	0.059	0.040	0.043
	Outcome = Perkins Borrowing								
Treated*Post	-44.580	-56.689*	-152.407	-65.353	-20.110	-19.016	-78.862**	-44.080	-13.008
	(29.389)	(24.045)	(100.684)	(57.934)	(67.025)	(31.300)	(26.301)	(47.845)	(28.519)
Observations	22,410	32,940	2,770	5,190	3,940	19,890	26,550	13,460	11,440
R-Squared	0.074	0.076	0.126	0.111	0.125	0.076	0.071	0.077	0.062

Notes: Regressions are weighted by NPSAS sample weights. Standard errors are adjusted for heteroskedasticity and correlation within institution-year cells (*** p<0.001, ** p<0.01, * p<0.05). All dollar amounts are in 2007 constant dollars. While not shown here, estimates account for time-varying pre-treatment covariates from Table 3 and state-year fixed-effects.

Table 6: Robustness Tests

	(1) *Preferred Specification*	(2) *No New Entrants*	(3) *Remove Top 5%*	(4) *Parent PLUS Rejects*
	Outcome = Total Borrowing			
Treated*Post	234.979	498.553*	53.083	-155.285
	(175.116)	(222.166)	(176.747)	(198.300)
Observations	46,440	32,630	45,970	33,250
R-Squared	0.078	0.081	0.079	0.148
	Outcome = Stafford Borrowing			
Treated*Post	940.076***	860.403***	787.899***	395.200***
	(63.566)	(75.779)	(63.254)	(71.357)
Observations	46,440	32,630	45,970	33,250
R-Squared	0.168	0.128	0.175	0.312
	Outcome = Private Borrowing			
Treated*Post	-268.882*	-103.778	-328.012**	-300.332*
	(111.747)	(151.314)	(113.143)	(119.648)
Observations	46,440	32,630	45,970	33,250
R-Squared	0.072	0.083	0.072	0.089
	Outcome = PLUS Borrowing			
Treated*Post	-380.223**	-207.810	-338.251**	-226.679
	(124.753)	(150.644)	(126.236)	(142.500)
Observations	46,440	32,630	45,970	33,250
R-Squared	0.051	0.050	0.051	0.056
	Outcome = Perkins Borrowing			
Treated*Post	-55.992**	-50.262	-68.553**	-23.475
	(21.492)	(26.421)	(21.382)	(24.929)
Observations	46,440	32,630	45,970	33,250
R-Squared	0.068	0.066	0.068	0.061

Notes: Regressions are weighted by NPSAS sample weights. Standard errors are adjusted for heteroskedasticity and correlation within institution-year cells (*** p<0.001, ** p<0.01, * p<0.05). All dollar amounts are in 2007 constant dollars. Estimates include time-varying covariates with each covariate interacted with time dummies for 2003-04 and 2007-08, including state fixed-effects.

Appendix Table 1: Placebo Test

	(1) *Stafford loans*	(2) *Private loans*	(3) *PLUS loans*	(4) *Perkins loans*	(5) *Total loans*	(6) *Work hours*
Black	921.953***	-155.776*	333.215*	-32.318	1,067.074***	-0.496
	(140.568)	(74.559)	(169.416)	(42.029)	(243.697)	(0.730)
Hispanic	-185.032	79.421	-1.933	3.643	-103.901	0.192
	(205.097)	(185.952)	(211.117)	(54.379)	(380.687)	(0.801)
Asian	-291.503	-176.152	-243.812	93.402	-618.065	-4.217***
	(174.958)	(133.423)	(264.689)	(82.576)	(347.695)	(0.901)
Native American	-183.434	119.451	-397.790	-34.416	-496.188	-1.807
	(596.819)	(424.716)	(598.874)	(142.647)	(1,065.663)	(2.728)
Other	-858.390*	-66.384	31.235	194.587	-698.953	-2.498
	(357.805)	(262.753)	(595.658)	(152.036)	(783.095)	(1.373)
Multiracial	-64.242	35.372	-258.176	-11.818	-298.864	0.380
	(243.251)	(214.820)	(310.353)	(88.390)	(513.493)	(1.331)
Female	36.754	-25.547	-93.649	24.608	-57.833	0.344
	(70.178)	(64.687)	(100.363)	(22.328)	(147.926)	(0.379)
Income	-7.182	38.265**	211.868***	-59.618***	183.332***	-0.467***
	(13.534)	(12.249)	(19.183)	(4.527)	(27.393)	(0.074)
First-Generation	326.296***	161.770*	-194.501*	-21.549	272.016	2.189***
	(67.708)	(69.868)	(90.423)	(24.186)	(148.461)	(0.390)
2003-04	-769.016	-29.425	-372.896	-40.672	-1,212.010	1.978
	(521.009)	(263.331)	(810.079)	(103.843)	(1,214.166)	(2.841)
Treated	2,181.775***	11.485	470.624***	86.645***	1,613.021***	-3.085***
	(71.259)	(63.537)	(95.837)	(23.691)	(147.183)	(0.373)
Treated*Post	49.410	-145.734	143.468	21.487	68.630	-1.025*
	(93.478)	(96.005)	(131.272)	(31.198)	(200.892)	(0.477)
Constant	4,986.084***	229.002	254.288	546.572***	6,015.946***	17.951***
	(372.163)	(175.957)	(702.229)	(60.482)	(954.097)	(2.286)
Time-Varying Covariates	Y	Y	Y	Y	Y	Y
State FEs	Y	Y	Y	Y	Y	Y
Observations	22,490	22,490	22,490	22,490	22,490	20,790
R-squared	0.189	0.034	0.054	0.072	0.071	0.062

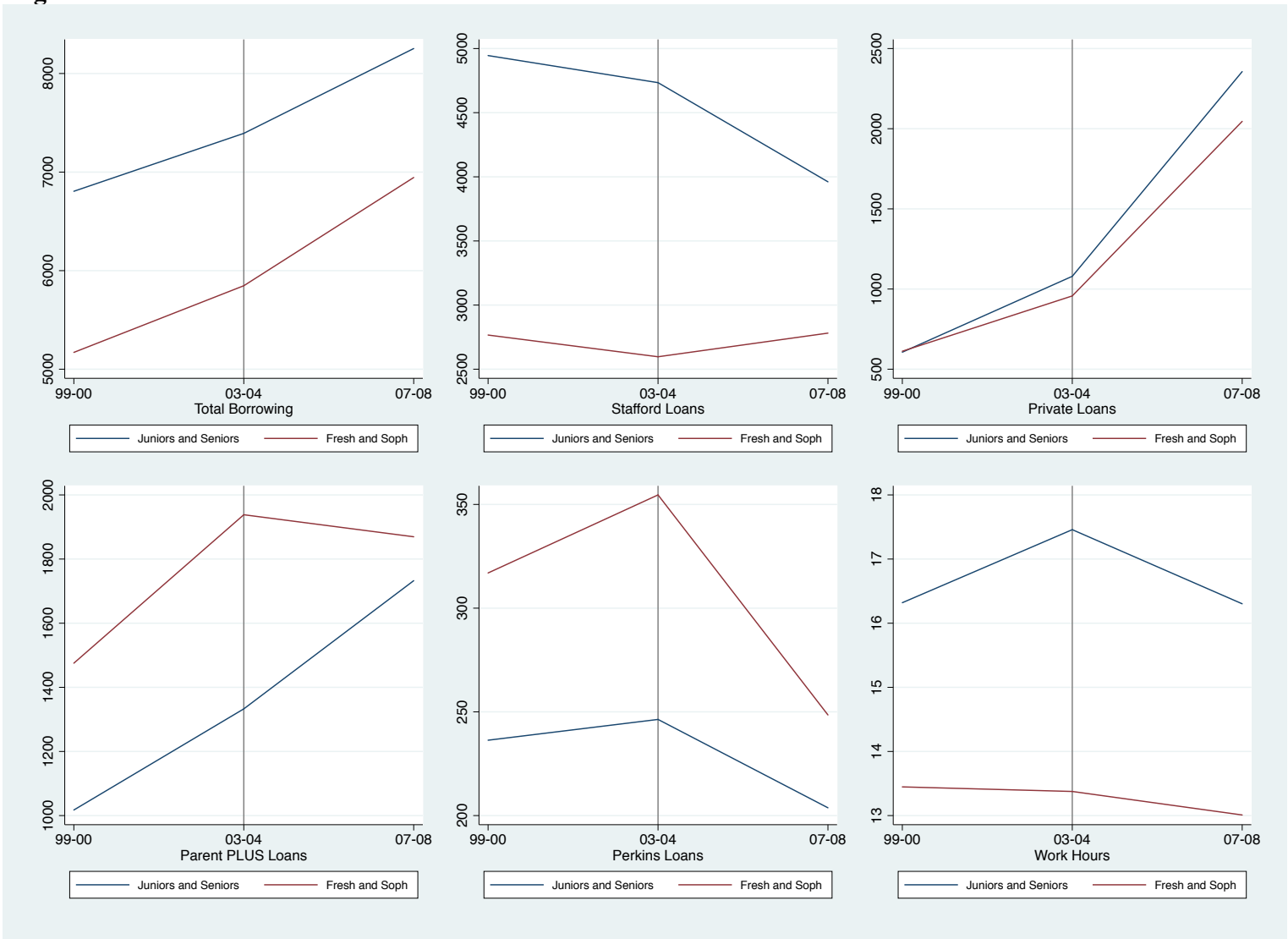
Notes: Regressions are weighted by NPSAS sample weights. Standard errors are adjusted for heteroskedasticity and correlation within institution-year cells (*** p<0.001, ** p<0.01, * p<0.05). All dollar amounts are in 2007 constant dollars. Estimates only include sample members from 1999-2000 and 2003-04 with 2003-04 serving as the placebo post-implementation year. Estimates include each pre-treatment covariate interacted with time dummies for 2003-04, including state fixed-effects.

Appendix Table 2: Falsification Test

	(1) *Family Income*	(2) *Black*	(3) *Hispanic*	(4) *First-Generation*	(5) *Highly Selective*	(6) *Moderately Selective*	(7) *Less Selective*	(8) *Female*	(9) *Net Price*
2003-04	0.399***	-0.004	0.015	-0.023*	-0.020	0.026	-0.006	-0.008	467.258
	(0.067)	(0.016)	(0.008)	(0.011)	(0.023)	(0.027)	(0.016)	(0.012)	(318.946)
2007-08	0.793***	-0.007	0.033***	-0.015	-0.031	0.029	0.001	-0.009	844.457*
	(0.071)	(0.013)	(0.009)	(0.012)	(0.028)	(0.030)	(0.014)	(0.011)	(331.117)
Treated	0.004	0.016*	0.004	0.009	-0.031***	-0.001	0.032***	-0.020*	-519.545***
	(0.049)	(0.007)	(0.004)	(0.009)	(0.007)	(0.008)	(0.006)	(0.008)	(128.383)
Treated*Post	-0.109	0.011	0.006	0.002	-0.014	-0.006	0.019	-0.003	246.786
	(0.075)	(0.009)	(0.008)	(0.013)	(0.012)	(0.015)	(0.011)	(0.013)	(221.398)
Constant	6.077***	0.104***	0.062***	0.481***	0.212***	0.714***	0.074***	0.578***	6,375.462***
	(0.053)	(0.011)	(0.006)	(0.009)	(0.018)	(0.019)	(0.011)	(0.009)	(211.028)
Observations	46,440	46,440	46,440	46,440	46,440	46,440	46,440	46,440	40,890
R-squared	0.011	0.001	0.003	0.000	0.004	0.001	0.006	0.001	0.004

Notes: Regressions are weighted by NPSAS sample weights. Standard errors are adjusted for heteroskedasticity and correlation within institution-year cells (*** p<0.001, ** p<0.01, * p<0.05). Net price is in 2007 constant dollars.

Figure 1: Trends in outcomes of interest



Notes: Means are weighted by NPSAS sample weights. 2007-08 is the policy implementation year.