More Credit, More Problems? Federal Student Loan Limits and Education Outcomes

Cullen F. Goenner, PhD
Professor and Faculty Fellow
Department of Economics and Finance
University of North Dakota
293 Centennial Drive Stop 8369
Grand Forks, ND 58202-8369
Phone: 701-775-3852
Email: cullen.goenner@und.edu

Chih Ming Tan, PhD
Professor and Page Endowed Chair in Applied Economics
Department of Economics and Finance
University of North Dakota
293 Centennial Drive Stop 8369
Grand Forks, ND 58202-8369
Phone: 701-775-3349
Email: chihming.tan@business.und.edu

More Credit, More Problems? Federal Student Loan Limits and Education Outcomes[*]

Cullen F. Goenner & Chih Ming Tan[#]

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Abstract

We study the treatment effects of an increase in Federal Stafford loan limits between 2006 and 2008 on the borrowing behavior, loan composition, and academic outcomes of first-time freshmen at the University of North Dakota. Our student body provides a unique opportunity to examine the effect of this change, as we are able to isolate the impact of macroeconomic fluctuations on borrowing due to the strength and stability of the state's economy during the period. Using a difference-in-difference estimation approach, freshmen are shown to substitute an increase in borrowing through Stafford loans with a partial reduction in their use of private student loans. Interestingly, student academic outcomes did not improve, and for some measures worsened, despite increased credit availability.

Keywords: Borrowing constraints; government student loans; education outcomes

JEL Codes: H52, I22, I28

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[#] Goenner & Tan: Department of Economics, College of Business and Public Administration, University of North Dakota, Gamble Hall, 293 Centennial Drive, Grand Forks, ND 58202-8369. Corresponding author (Goenner): cgoenner@business.und.edu (tel: 701-777-3353).

1. Introduction

In academic year (AY) 2014-15 the federal Stafford loan program provided more than 76 billion dollars to students in the United States to assist with the cost of college (College Board, 2015). The maximum a student is able to annually borrow from the program is equal to the minimum of a limit set by Federal statute and a student's adjusted cost of attendance. Few changes have been made to the program's limit in the 45 plus year history of the program. Between AY 1993-94 and AY 2006-07 the annual loan limit was set at $2,625 for freshmen, yet during this same time period the average cost of undergraduate tuition, fees, and room and board doubled from $6,365 to $12,796 at 4-year public institutions.[1] With the costs of college rising sharply and loan limits fixed, students increasingly found themselves maxing out their borrowing from government student loan (GSL) programs.[2] In response, many turned to private student loans to supplement their borrowing needs.[3] Private student loans, unlike Stafford loans require students to be either creditworthy themselves or have a creditworthy cosigner. For students in this period, particularly among those lacking parental support, financing college may have therefore become more of a challenge.

This paper's contribution is to examine the behavioral responses of first-time freshmen to an increase in their ability to borrow from the Stafford loan program. Specifically, we identify the impact of a change in the statutory limit on student borrowing and academic outcomes. As

[1] The loan limit for freshmen who qualified based on need (subsidized Stafford loans) has been $2,625 since January of 1987. Prior to 1993 there were no unsubsidized loans in the Stafford loan program. Cost of attendance figures are from Snyder and Dillow (2013) Digest of Education Statistics 2012 Table 381, U.S. Department of Education.

[2] The percentage of students nationwide who borrowed the annual subsidized Stafford loan program limit increased from 41% in AY 1995-96 to 51% in 2003-04, a period in which there were no changes in program limits (Wei and Skomsvold, 2011).

[3] Between AY 2002-03 and 2005-06 the volume of private-student loans more than doubled from $6.3 billion to $15.7 billion - data is from the College Board's Trends in Student Aid 2015 Table 1A.

part of the Deficit Reduction Act of 2006, the statutory limit for Stafford loans increased to $3,500 for freshmen in AY 2007-08 with the purpose of narrowing the gap between what under- and upper-classman were able to borrow (see Table 1 for historical limits).[4] A year later, in the midst of the financial crisis and recession, Congress responded to tightening credit markets and reduced access to private student loans by increasing the annual Stafford loan limit by an additional $2,000 for all undergraduate students, meaning freshmen in AY 2008-09 were able to borrow $5,500. Over this two-year period, freshmen were able to more than double their borrowing capability from the Stafford loan program.

[Insert Table 1 about here]

We use the temporal variation in the GSL limit and the cross-sectional variation of borrowing by freshmen to identify, within a difference-in-differences framework, the treatment effect of the two policy changes on freshmen at the University of North Dakota.[5] A key challenge in identifying the effect of the loan limit changes is the fact that at the time of these policy initiatives there were significant concurrent confounding shocks occurring in the macro economy from the financial crisis and Great Recession, which caused incomes and asset values to decline and the market for private student loans to collapse.[6]

By design, the estimate of the treatment effect under difference-in-differences is unable to discern between the impact of the policy change and other omitted or unobserved factors that

[4] The legislation was part of the Higher Education Reconciliation Act of 2005, which is Title 8 of the Deficit Reduction Act of 2005 [Pub. L. No. 109-171, § 8005, 120 Stat. 4, 158 (2006)]

[5] Our work contrasts with more structural approaches that employ simulated policy experiments, for example, using NSLY data (Lochner and Monge-Naranjo, 2011; Johnson, 2013) and National Education Longitudinal Survey and Beginning Postsecondary Student Longitudinal Survey data (Ionescu and Simpson, 2016).

[6] The value of private loans originated declined from $21 billion in AY 2007-08 to $10 billion in AY 2008-09 (College Board, 2015).

coincide with the timing of the treatment, which may affect the control and treatment groups differently. This poses a threat to identification if unobserved factors caused the borrowing and academic performance of our treated and control groups to respond differently to a macroeconomic shock.[7] For example, students may be more reticent to borrow if the future outlook for the economy in their region is grim, in which case borrowing in 2008 in the midst of the recession, will not only capture the effect of raising credit limits but also the effect of the poor economic outlook on one's unwillingness to borrow.[8] If the future outlook is different among our control and treatment groups then our estimate of the treatment effect may over or underestimate the true effect of the policy. These differences are likely precisely because of the unprecedented nature of the Great Recession. By using student data from the University of North Dakota, we are uniquely able to minimize the effects of macroeconomic fluctuations on our estimates of the treatment effect due to the economic stability within North Dakota during the period as a result of the shale oil boom in the state.

The estimates from our model show the increases in Stafford loan limits led to an increase in total student borrowing and a decrease in borrowing from private student loans, which indicates additional federal student loans only partially crowd-out private student loans. This finding is consistent with results derived from structural approaches; e.g., Lochner and Monge-Naranjo (2011) and Ionescu and Simpson (2016). This change in the composition of

[7] Another threat to identification may occur if the macroeconomic shock leads to different college enrollment response across treatment and control groups. Christian (2007) does find some indication that children of households that are more likely to face liquidity constraints may experience more procyclical college enrollment, but points out that the evidence is mixed. Further clouding the picture is the fact that measuring liquidity constraints is potentially complicated in practice; see, Garcia et al. (1997).

[8] Deaton (1991) demonstrates that, in the presence of liquidity constraints, if growth rates in income and real GDP are persistent, then the onset of an economic downturn signals to individuals (students) that income is expected to fall. To moderate the impact on future consumption, one should save now to offset the future effects.

credit shifts credit risk away from private student loans where parents are cosigners, to federal student loans solely in the student's name. It also appears that a typical freshmen at the University of North Dakota in AY 2007-08 was not credit constrained after the changes in loan limits, as their total borrowing increased by less than the limit. In addition, we find the effects of the policy change vary by a student's expected family contributions (EFC) to their education. Students from families with high EFC substantially increased their borrowing in both periods, relative to the baseline, whereas students with low EFC did not alter their total borrowing due to the 2007 policy change and increased their borrowing by only half the increase seen by high EFC freshmen in 2008. This finding suggests that students with fewer parental transfers were more likely to alter the composition of their borrowing away from private student loans (PSL), whereas less constrained students used the increase in borrowing capacity to fund consumption.

We also explore the consequences of easing GSL limits on the academic performance of UND students. Financially constrained students who enroll in college are likely to work more (Keane and Wolpin, 2001), potentially at the detriment of their academic outcomes. By working more, particularly if off-campus, students are less able to become integrated into the academic community and as such are less likely to receive the support needed to succeed (see Tinto, 1975 for discussion of factors influencing dropout). Also with less time available for academic activities, constrained students are more likely to enroll part-time and have a break in enrollment than their counterparts. Removing or weakening this constraint would seemingly ease these effects and improve academic outcomes, though previous evidence (Cornwell et al., 2005; Dynarski, 2008; Angrist et al., 2009) is rather mixed on this point as it relates to the effects of large scale merit based scholarship programs (see Scott-Clayton, 2011 for discussion) and increased availability of government student loans (Johnson, 2013).

We find that despite easing the financial constraints faced by students, there is no evidence to suggest that an increase in the access to GSL had a positive effect on student academic outcomes. In fact, we find the opposite. The cumulative GPA of freshmen impacted by the change in the 2007 change in policy was .16 grade points lower than their counterparts in 2006. With respect to credits completed, we find freshmen in 2007 and 2008 completed approximately 1 credit less than the 2006 cohort due to the policy changes. Finally, we did not find any evidence to suggest the increases in the access to credit had an impact on freshmen retention.

The rest of the paper is organized as follows. We provide a literature review in Section 2. Section 3 details our methodology and describes the data. Section 4 discusses our findings. Finally, Section 5 concludes.

2. Financial Constraints and Higher Education

Rising costs of higher education have left policymakers wondering whether a substantial number of students are being priced out of being able to afford college. As the direct costs of college rise, the fear is that liquidity constrained students may either defer or forgo college altogether. Constrained students may also otherwise attend a lower quality institution or pursue a two-year degree. Research (Keane and Wolpin, 2001; Carneiro and Heckman, 2002; Cameron and Taber, 2004) using the 1979 youth cohort of the National Longitudinal Survey of Youth (NLSY79) suggests financial aid programs and parental support during the early 1980s were such that borrowing constraints did not appear to matter to educational attainment, even among low income, seemingly credit constrained students. More recent data indicates income and credit constraints may play a role in education decisions even in the short-run. Belley and Lochner (2007), using Carneiro and Heckman's (2002) framework and NLSY97 data, find college aged

students in the late 1990s and early 2000s were more likely to enroll in college the higher their family income and were similarly more likely to enroll in a four-year college, when controlling for ability and family characteristics. It is possible the sharp rise in the cost of college that began in the 1990s is such that parental transfers, which are correlated with income, play a larger role in financing the higher price of education.

Working while enrolled in college was viewed (Keane and Wolpin, 2001) in the past as a substitute to parental transfers, which allowed students to cover the cost of tuition. As tuition has risen faster than income, self-financing college may become out of reach for students without transfers. In which case financial constraints and financial aid may becoming more important to academic outcomes. Brown, Scholz, and Seshardi (2012) use the number of college aged children in the home to proxy for the amount of financial aid received and find a student with no parental aid (i.e. credit constrained) and four years of sibling overlap receives an additional .4 years of schooling, relative to a child in the same family with no overlap.[9] A limitation of this result is the inability to determine whether the impact is from the receipt of additional need based grants or loans (Johnson, 2013).

Towards this end of improving academic outcomes a number of states have responded to the rising cost of college by providing significant merit-based scholarships to subsidize middle and high ability students. The treatment effect of scholarships on academic outcomes though has been mixed. In the case of Georgia's HOPE scholarship program, research indicates the scholarship increased college participation by 7 percent (Dynarski, 2000) and completion by nearly 4 percent (Dynarski, 2008). Cornwell et al. (2005) find a negative consequence of the

[9] The proxy is rough in that financial aid is based on whether other family members are actually enrolled in college and not based on the age of other dependents. Data used is from the Health and Retirement Study (HRS).

scholarship's design is students are less likely to complete a full course load, as they withdraw from courses to maintain the GPA required for the scholarship's renewal. This effect though may be overcome with additional program restrictions (see Scott-Clayton, 2011 for discussion of West Virginia's PROMISE scholarship). It is unclear whether these effects are more generalizable, as Welch et al. (2014) find there to be no effect of Tennessee's HOPE scholarship on academic outcomes (persistence, grades, credits completed) of students at two-year institutions.

Loans also play an important role in financing college, contributing 37% of total aid in AY 2014-15 (College Board, 2015). Studies of students at various universities have revealed the level of student borrowing has little to no effect on student retention (DesJardins et al., 2002 – U of Minnesota; Singell, 2004 – U of Oregon; Kerkvleit and Nowell., 2005 – Oregon State and Weber State).[10] Despite growing concern with post-college debt burdens, little is understood about what effect borrowing limits have on student academic outcomes, relative to the expansion of scholarship and grant programs. Johnson's (2013) work is a notable exception in this regard, as his simulations show even if one allowed students to borrow from the GSL program the full cost of attendance, degree completion would only increase by 2.4 percentage points and that larger improvements are achievable through tuition subsidies. Ionescu and Simpson (2016) find the effects of expanding the GSL program to be larger in their model's simulation. Their results indicate the 2008 increase in GSL limits would increase degree completion by 6.7 percentage

[10] Singell (2004) finds an exception to this in the case of subsidized loans, where a $1,000 increase results in a 4.3% increase in retention. He though finds no effect for unsubsidized loans.

points, with an equal cost subsidy increasing completion by 12.5 percentage points.[11] Data from the National Center for Education Statistics though shows the actual 2008 cohort of first-time full-time students at four-year institutions had a similar six-year completion rate (59.6%) to that of the 2006 cohort (59.2%).[12]

Students, in reality are unable to borrow on their own the full cost of attendance and in response many students have turned to private student loans (PSL) to supplement their borrowing needs. Private credit markets play an important role in the overall access to credit, as an increase in GSL limits is shown (Lochner and Monge-Naranjo, 2011to only partially crowd-out private lending, such that total student credit increases with GSL limits. In a recent study, Ionescu and Simpson (2016) build a general equilibrium model of college investment to examine the effects the 2008 increase in GSL limits had on the composition of borrowing and risk. The results from calibration of their model show that not only does expansion of GSL limits crowd out private lending, it also effects default risk of PSL by altering the pool of borrowers. Borrowers using PSL are riskier after the change in loan limits and default rates more than double. To compensate for the higher risk, interest rates on PSL rise such that welfare is lower by 0.04 percent, relative to the benchmark, after the increase in loan limits.

Private student loans are used in conjunction with and perhaps somewhat surprisingly as an alternative to federal Stafford loans. The Consumer Financial Protection Bureau and Department of Education (CFPB and Department of Education, 2012) find 54% of PSL borrowers do not use all of their Stafford loan eligibility, which implies for some students PSL

[11] The completion data Ionescu and Simpson (2016) base their simulation on are for freshmen who enroll full-time in a four year institution without delay following high school graduation. This group is more likely to complete than older students and those who enroll part time.

[12] Department of Education Digest of Education Statistics table 326.10

are either equivalent to or preferred to federal Stafford loans. At the surface, PSL and unsubsidized Stafford loans may appear to students to be similar, but they differ significantly in terms of risk.[13] While PSL and unsubsidized Stafford loans both accumulate interest that is capitalized while a student is in school, PSL loans tend to have variable interest rates whereas GSL are at a fixed-rate. This introduces interest rate risk to borrowers of PSL, such that if interest rates rise, loan repayments will also rise. Not only are borrowers of PSL exposed to more interest rate risk, but PSL loans are originated at higher interest rates than Stafford loans for everyone other than the most creditworthy borrowers (CFPB and Department of Education, 2012). It is possible higher risk and interest expense from additional PSL may lead to financial pressure on students that negatively impact their academic outcomes. Increasing GSL limits may thereby affect both the composition of borrowing and academic outcomes.

3. Model and Data

The data we use are drawn from a repeated cross section of first-time freshmen students, who enrolled at the University of North Dakota in the fall semesters of 2006, 2007, and 2008. Our sample only includes students who have submitted a Free Application for Financial Student Aid (FAFSA), which is approximately 78% of the freshmen population in each year. We exclude students who did not file for financial aid because we believe these students are not in the same position as those who do, when it comes to deciding how much to borrow.[14] In

[13] Prior to 2008 it was not uncommon for college financial aid packages to directly market both federal and private student loans originated by the same preferred lenders, which could lead to confusion.

[14] Students who file a FAFSA in as sense pay a fixed cost in terms of the time to gather the information necessary to complete the application. Other direct costs involved with student loans, such as origination fees are a fraction of the loan amount and thus variable. We also exclude from the sample, students who were either independent students or aviation majors. Aviation majors face a significantly higher and more variable cost of attendance than other freshmen at our institution. Their cost of attendance depends on the number of flight hours and particular courses enrolled in during the year.

addition, we lack key information on the family finances of those who do not file a FAFSA. Not everyone who submits a FAFSA will ultimately borrow, as students may apply in order to be considered for need based scholarships and grants. In our sample, the fraction of those who submitted a FAFSA and borrowed declined over the three-year period from 81.5% in 2006 to 76.5% in 2007, and 76.8% in 2008.

We first consider the treatment effects of the changes in Stafford loan limits on total student loan borrowing by freshmen. Freshmen receive loans from both private lenders (commercial banks and state agencies, e.g. Bank of North Dakota) and the federal government. There are three federal student loan programs for undergraduate students – Stafford loan, Perkins loan, and parent PLUS loan that make up 81%, 2%, and 17% of the GSL originated in AY 2014-15 (College Board, 2015). Since 1993 borrowing from the Stafford loan program does not depend on a student's financial need, yet students who are in need qualify for interest to be paid for by the government (subsidized) while a student is enrolled. Students with a low expected family contribution may also qualify for a Perkins loan, in which interest is subsidized.[15] Parent PLUS loans allow a parent without a negative credit history to take out a loan for their child up to the adjusted cost of attendance, where the parent is the sole signatory on the promissory note. The credit risk associated with each of the three government loan programs may vary substantially given Perkins and subsidized Stafford loans are given to those in greatest financial need, while Parent PLUS loans require a creditworthy parent. Hereafter, when we refer to an increase in the GSL limit we are implicitly referring to the change in Stafford loan limits alone. Table 2 provides a breakdown of the average borrowing amount of UND freshmen by loan

[15] The federal government provides funds to schools to originate Perkins loans. Repayment is made to the school, and not the government.

source over the period. Following the increase in GSL limits, we see the average borrowing amount of freshmen who borrowed from the program increased by $2336, and that fewer freshmen used private and PLUS loans. Overall borrowing increased by $1551, which was larger than the $936 increase in the cost of attendance (See figure 1).

[Insert Table 2 about here]

A difference-in-differences estimation strategy is applied where each cohort of freshmen is split into two groups, a treatment group and a control group. The treatment group consists of freshmen who borrowed through the Stafford loan program and as such were potentially impacted by the changes in policy, and the control group consisted of students who borrowed from other sources or who chose not to borrow. Figure 2 depicts the average borrowing by each group over the three years, with the latter two periods reflecting the policy changes. On average, freshman who borrowed from the Stafford loan program increased their total borrowing from $6,546 in AY 2006-07 to $7,255 in AY 2007-08 and $8,141 in AY 2008-09. For the control group, however, borrowing declined from $950 in AY 2006-07 to $766 in AY 2007-08 and $586 in AY 2008-09.

[Insert Figure 1 about here]

[Insert Figure 2 about here]

The regression model of student loan borrowing (y) is specified as follows,

$$y = \alpha + \gamma Treat + \tau_{2007} + \tau_{2008} + \delta_1 (Treat \times 2007) + \delta_2 (Treat \times 2008) + X\beta + \varepsilon \qquad (1)$$

A group indicator variable is included to control for baseline differences between the control and treatment groups prior to the policy changes and is captured by θ. Similarly, we add indicator

variables for each of the years in which the policy changes were implemented to control for differences in borrowing over time that influence both groups similarly. These effects are captured by the two coefficients for τ and reflect the impact relative to 2006, i.e. prior to the changes in policy. The coefficients for the interaction terms between the treatment group and time periods (δ_1 and δ_2) captures the treatment effects of the policy changes on borrowing. A vector (X) of student characteristics and controls is added that includes gender, minority status, whether the student is a ND/MN resident, age, family size, and high school grade point average, as well as other factors that influence the borrowing decision which we discuss below. Further we allow the error term to be non i.i.d. by clustering the standard errors by a student's high school. Summary statistics of these characteristics by control and treatment group for each year appear in Table 3.

[Insert Table 3 about here]

One concern from Table 3 is that the treatment and control group characteristics appear to be very different. The two groups may therefore potentially, for example, respond differently to changes in policies and macroeconomic conditions, based on unobservable conditions that are occurring at the same time as the increase in Stafford loan limits. To address this issue, we implement difference-in-differences matching estimation using the propensity score, as a robustness check to all of our baseline exercises discussed in the results section (Section 4). Heckman et al. (1997) and Heckman et al. (1998) find that implementing matching in difference-in-differences results in substantial reductions in evaluation bias in the estimation of treatment effects.

Specifically, we match freshmen from the treatment and control groups that have similar observed characteristics. A separate propensity score is used to match observations from AY

2006-07 to those in AY 2007-08 and similarly between AY 2006-07 and AY 2008-09. Each score is estimated via a logit model of the probability of borrowing from the Stafford loan program, where we condition on the same controls used in our baseline specification. Using kernel density weights on the common support, we estimate the difference-in-differences using the matched samples, where standard errors were bootstrapped using 100 iterations. In each matched sample we find that the covariates are balanced between students in the treated and control groups.

We now discuss some control variables that influence student's borrowing decisions. Influencing the decision to borrow and how much to borrow is the difference between the cost of attendance and the amount of grants and family transfers a student receives. Grants consist of funds provided to students by institutions and federal and state governments, which typically do not need to be repaid. In some cases, such as the federal Pell Grant, aid is based on financial need, whereas in others it is based on merit, or a combination of both. Institutional support from scholarships is an important tool used by universities themselves to attract students with various targeted characteristics, such as academic ability, athletic ability, ethnic diversity, and socio-economic backgrounds. Our dataset includes information on the dollar amount of the various scholarships and grants students receive, but we are unable to directly observe the amount of family transfers.

Family transfers consist of cash contributions to a student's education by family members. To award need based federal aid, the federal government uses a complicated formula and data provided on a student's FAFSA to estimate what a student and their family are reasonably expected to contribute to their son or daughter's education. A dependent student's expected family contribution (EFC) is based primarily on family income, family assets, number

of family members, and number of family members in college. In our analysis we use a non-linear measure, which indicates whether the family's EFC is either between five and ten thousand dollars, between ten and fifteen thousand dollars, or over fifteen thousand dollars. A family EFC less than five thousand dollars serves as our baseline group. Parents though are under no obligation to provide their EFC. To supplement the data we have on the family's EFC, we consider a number of factors that may influence parents' actual contributions. Students with high ability, as measured by ACT scores, are more likely to have parents who invest in their child early in life, and are thus more likely to provide for their child's investment in higher education. First-generation college students may be more likely to have parents who underinvest in their son or daughter's education and thus they are more reliant on borrowing to finance their education. We also posit here that one may be able to ascertain parents' willingness to support their child's education by a student's earnings. Students who work a large number of hours, thus earning more income while in school demonstrate a great deal of financial independence. This independence is brought upon by either an unwillingness of students to seek additional transfers from their parents, or an inability of parents to contribute transfers. In either case, the more students earn the less likely they are to receive transfers and the more they may need to borrow.

Students borrow out of financial need, but their need may be dependent on their perception of the costs and returns to their education and level of financial literacy. We have no way of directly measuring either student perceptions or financial literacy, so we use their family socio-economic status, as measured by family income for a proxy. The control variables for academic ability and first-generation status may also influence literacy, with financial literacy increasing with ability and parental education. We would expect more financially literate individuals to borrow less, *ceteris paribus*. Our model of borrowing also controls for whether

students are residents of North Dakota and Minnesota. Due to tuition reciprocity agreements between North Dakota and Minnesota, students from these two states have a significantly lower cost of attendance than students from other states. We also include whether a student graduated from one of the sixteen high schools located thirty or less miles to our University, where the idea is these students face fewer expenses due to the proximity of their hometowns.[16] It is also possible that proximity to our institution increases parents' awareness of the likelihood of their child to attend our institution and their need to provide financial support. Our final control, serves as an indirect measure of student motivation and interest. During the recruitment process, our institution kept track of the number of times in which students on their own initiated contact with the institution. We consider whether the level of student engagement as captured by contact count may influence borrowing behavior.

A potential concern is whether there is a relationship between the timing of the policy changes and unobserved factors that affect borrowing, which would introduce a bias into our estimates of the treatment effect. If students knew they would not be financially supported by their parents, independent of income and EFC, they may systematically defer attending college in 2006 and decide to enroll in either 2007 or 2008, when their individual borrowing capacity increased. In this case, we would see a strong response in borrowing by freshmen during the policy years that was caused by an unobservable factor, lack of family support, and not driven by increased access to credit. If this were the case, one would expect the age of students who are borrowing to increase due to their deferral of college, and more students would borrow. As noted above, during the policy years freshmen were less likely to apply for aid, and those who

[16] Card (2001) provides discussion how proximity to college can serve as an instrumental variable for the level of schooling.

did were less likely to borrow. Further, the average age of borrowers was 19.6 years in each of the three periods. It does not appear that deferrals due to borrowing considerations had an impact on our sample over time.

Another concern is whether the economic recession that began at the end of 2007 may have impacted some students at our institution differently than others based on unobserved characteristics. Students at the University of North Dakota are relatively homogenous, with 50% of the freshmen cohort being drawn from North Dakota and 44% from Minnesota.[17] Drawn primarily from our region, freshmen and their families we argue were largely isolated from the macroeconomic shock that hit the rest of the economy during the recession. Figure 3a depicts the seasonally adjusted monthly unemployment rate for North Dakota relative to the United States. From the figure, it is quite evident that North Dakota's unemployment rate was relatively unaffected by the recession, when at the same time unemployment nationwide would reach 10% in October of 2009. Similarly, if we examine Figure 3b one will note the average annual real GDP growth during calendar years 2006-2009 was 4.2% in North Dakota compared to .23% for the country.

Another key economic factor during the recession, was the impact the financial crisis had on housing prices. While housing prices in the United States began their decline in the third quarter of 2007 and continued through the first quarter of 2011, housing prices in North Dakota rose steadily through most of the period as seen in Figure 3c. Our institution was financially strong during this period, due to a substantial state budget surplus. As a result, the price of tuition and fees for residents of North Dakota and Minnesota rose from $6,130 in 2006 by only

[17] The campus of the University of North Dakota is located in Grand Forks and is approximately three miles from the border with Minnesota.

$383 in 2007 and another $213 in 2008. To address any potential for there to be a heterogeneous effect between students from North Dakota and Minnesota, we produce a robustness check where we examine whether the treatment effect is unique to residents of North Dakota.

[Insert Figure 3 about here]

4. Results and Discussion

4.1 Impact on Borrowing Behavior

We first discuss findings on the impact on the Stafford loan limits on borrowing behavior; specifically, total borrowing, and also the composition of loans. We start by describing our findings for total borrowing by students. For total borrowing, our baseline specification of equation 1 excludes two of the variables discussed above. It is possible that the increase in Stafford loan limits influenced the number of first-generation students who enrolled, which could create endogeneity issues. Similarly, students who made more contact with our institution prior to enrollment, may have been more likely to learn about and take advantage of the changes in financial aid.

The baseline difference-in-differences results appear in column 1 of Table 4 with standard errors clustered by high school. In 2007 Stafford loan limits for freshmen increased by $875 and our estimate of the treatment effect indicates freshmen increased their borrowing by $924, relative to the 2006 cohort, with the result significant at the 1% level. The 2008 policy change increased Stafford limits by $2,875, relative to 2006, and borrowing increased by $1,887, with the result also significant at the 1% level. In column 2 of Table 4 we report results from differences-in-differences with matching to check the robustness of these estimates. With matching we find the 2007 change in policy increased borrowing by $1001, and the change in

2008 had an effect of $1,897, which are both in line with our baseline difference-in-differences results. It would seem our estimates of total borrowing are quite robust. The balance between our covariates we achieved by using matching on the propensity score is shown in Table 5.[18]

[Insert Table 4 about here]

[Insert Table 5 about here]

Returning to the effects of the controls, our baseline estimates show that for every $1,000 in additional scholarships received, freshmen borrowed $242 less, when controlling for other factors. Students with expected family contributions (EFC) between $5,000 and $10,000 borrow on average $798 more than those with parental EFCs less than $5,000. It appears the ability to borrow is important to this group to pay for college. For the two higher EFC groups, there is no impact on borrowing behavior. Increasing ability, as measured by a 1 standard deviation in ACT score, decreases borrowing by $100. Not surprisingly, residents of Minnesota and North Dakota who face lower tuition than residents from other states, borrowed $1,499 less than students without reciprocity. Further we find students who graduated from a high school in these two states within 30 miles of our institution borrowed $777 less. The marginal effects of the other variables were small, where statistically significant.

The results above focused on the impact GSL limits had on total borrowing. Also of interest is whether the change in Stafford loan limits had an impact by crowding out borrowing from private lenders. The baseline difference-in-differences results in column 3 of Table 4 use the same treatment and control groups as our baseline model and consider the impact on borrowing from private lenders. The treatment effect of the 2007 policy change was to reduce

[18] In the interest of space, we only report the results for the balancing check for the Total Borrowing exercise. In all reported exercises below which include difference-in-differences with matching, similar balancing checks were done. In all cases, the matched samples exhibited covariate balance. These results are available from the authors upon request.

private loans by $425 relative to the 2006 cohort, which was significant at the 5% level. For 2008, borrowing from private lenders declined by $564 relative to the 2006 cohort and was significant at the 1% level. In each case the decrease in private borrowing was only partially crowded out by the changes in GSL limits. Using matching we find (column 4 Table 4) the policy change in 2007 had a slightly smaller impact on loan composition ($360) than our baseline model, though the effect was still significant at the 10% level. The estimate of the 2008 treatment effect was similar to the baseline difference-in-differences, and the result was significant at the 5% level. It appears the increase in GSL limits at most partially crowded out private borrowing, and may have had a negligible effect.

In sum, we find that the policy change did in fact have an impact on students' borrowing behavior. Students increased total borrowing after the change in loan limits and there was a partial reduction in private borrowing. These results are consistent with the findings of Ionescu and Simpson's (2016) policy experiments, whereby they use aggregate level data from several national surveys to calibrate a general equilibrium model and examine the effects of the 2008 change in the GSL program. Their results indicate the higher GSL limit increases average government student loan debt by $1432 and decreases private student loan debt by $1072. The changes in borrowing in Ionescu and Simpson's (2016) model are primarily driven by the composition of who enrolls in college – students with low ability, low expected family contributions, and bad credit are all much more likely to enroll following the change in policy and are likely to borrow more from government sources. In their model, students with a high expected family contribution borrow a similar amount following the change, whereas we find conditional on family contributions and other factors, borrowing rises. We explore whether there are heterogeneous effects of family contributions further in Section 4.2.2.

4.1.1 Falsification test and robustness checks

We conduct a number of robustness checks to evaluate the sensitivity of our results for both total borrowing and private student loan borrowing. In the first we consider a falsification test, in which we split our original control group that consisted of non-Stafford borrowers and non-borrowers into a control group of non-borrowers and a fake treatment group of non-Stafford borrowers. If our revised treatment group is indeed fake in the sense of not being affected by the policy, then we would expect our estimates of the treatment effect to not be significantly different than zero. Our results (Appendix 1, columns 1 and 4) confirm this, as both years' treatment effects are not significantly different than zero under the falsification test for both of the outcome variables (total borrowing and private student loan borrowing).

We also examined several different comparison groups, to determine whether the estimates were sensitive to construction of the control groups. Column 2 in Appendix 1, reports results on total borrowing from using a treatment group consisting of Stafford borrowers, and a control group of non-borrowers. Our estimates of the treatment effect are slightly smaller than our baseline model, with an estimated impact of $814 due to the 2007 policy change and $1667 due to the 2008 policy change. Both estimates are statistically significant at the 1% level. Using non-Stafford borrowers alone as our control group, we find in column 3 of Appendix 1 the effect of the 2007 policy change to be significantly reduced and not statistically significant. The impact of the 2008 policy change ($1,604) though is in line with our other results and is significant at the 5% level.

For the case of private student loan borrowing, when we use a treatment group made up of Stafford loan borrowers and a control group of non-borrowers, we find (column 5, Appendix 1) slightly larger impacts than the baseline results. The 2007 policy change reduced private

borrowing by $484 and the 2008 change by $658. When we compare the same treatment group, with the control group that consists of non-Stafford borrowers, the effects are again negative ($535 and $521, respectively), but are not significant due to the substantial standard errors (column 6, Appendix 1).

To further test the sensitivity of our baseline model for total borrowing, we use our baseline model's control and treatment groups and add to the specification the first-generation and contact count variables. The results in column 1 of Table 6 are similar to our baseline findings, with first-generation freshmen borrowing on average $384 more than similar counterparts and contact count not having a significant effect. Adding other demographic factors, such as gender, ethnicity, family size, age, and high school grade point average were found not to be statistically significant and did not impact our results.[19] A potential concern is whether freshmen who enrolled in 2008, may have chosen to enroll at a lower cost state school such as ours, only as a result of the credit crunch. If this were the case, our 2008 sample may differ in unobserved ways.

To discern whether freshmen in 2008 were "settling" in their choice to attend UND, we use the order in which students list where to send their ACT scores as a proxy for their preferences. Students generally take their ACT during their junior year, therefore their preference sorting was established prior to the recession. In both 2006 and 2008 freshmen cohorts, 61% of students indicated UND was their first choice, with similar mean ordering between the two years. UND's position in the ordering of ACT scores was found not to effect borrower behavior, when added to the baseline model. The effect of the 2007 policy change on total borrowing was diminished to

[19] These results are available upon request.

$623, while the 2008 change of $1,652 was similar to our other findings. These estimates appear in column 2 of Table 6. [Insert Table 6 about here]

4.2 Interpretation of findings for Total Borrowing and Composition

4.2.1 North Dakota

To interpret our findings on the effects of the policy change on total borrowing and the composition of borrowing, we first explore whether the observed treatment effects varied in magnitude between North Dakota residents and students from other states. While our school draws students primarily from the region, it is possible that the strength of North Dakota's economy could have impacted residents differently than non-residents in unobserved ways. To evaluate the effect we add five variables to the specification in equation 2 that interact a North Dakota residency indicator with the treatment group, the year effects, and the treatment effects.

$$\begin{aligned} y = {} & \alpha + \theta Treat + \gamma ND + \tau_{2007} + \tau_{2008} + \delta_0 \left(ND \times Treat \right) + \delta_1 \left(Treat \times 2007 \right) + \\ & \delta_2 \left(Treat \times 2008 \right) + \delta_3 \left(ND \times 2007 \right) + \delta_4 \left(ND \times 2008 \right) + \delta_5 \left(ND \times Treat \times 2007 \right) + \\ & \delta_6 \left(ND \times Treat \times 2008 \right) + X\beta + \varepsilon \end{aligned} \tag{2}$$

The results in column 1 of Table 7 show that the effect of the 2007 policy change is to increase total borrowing by $1378 and there was no statistically significant difference in the effect between North Dakotans and residents of other states. Further the policy effect of the 2008 change indicated borrowing increased by $2039, again without a significant difference in the effect based on residency. With respect to the composition of borrowing, we find there to be no statistically significant effect of the 2007 policy change on private student loan borrowing, yet the 2008 policy led to a decrease in private loans of $829. In neither case was there evidence of a differential impact due to residency from the policy change.

[Insert Table 7 about here]

4.2.2 Mechanism: need or availability of easy credit

From a policy perspective, a concern with the expansion of the GSL program is whether the increase in borrowing by freshmen is on the basis of need or the availability of easy credit. If students borrowed on the basis of need, one would expect a stronger response from the policy change of those who are most in need. Without data on parental transfers, we are unable to directly measure a student's financial need, but we might expect students whose parents have a high EFC to have lower need (Ionescu and Simpson, 2016). Given unsubsidized Stafford loans are not need based it is possible the change in policy affected borrowers who were liquidity constrained due to need, differently than those who were not. Here we identify students with financial need to have an EFC less than $23,000, which places them below the upper quartile of the student body and is approximately the cost of attending our institution.

To identify the effect of need on the impact of the policy changes, we add five variables to the specification that interact the EFC indicator with the treatment group, the year effects, and the treatment effects.

$$\begin{aligned} y = {} & \alpha + \theta Treat + \gamma EFC + \tau_{2007} + \tau_{2008} + \delta_0 (EFC' \ Treat) + \delta_1 (Treat' \ 2007) + \\ & \delta_2 (Treat' \ 2008) + \delta_3 (EFC' \ 2007) + \delta_4 (EFC' \ 2008) + \delta_5 (EFC' \ Treat' \ 2007) + \\ & \delta_6 (EFC' \ Treat' \ 2008) + X\beta + \varepsilon \end{aligned} \quad (3)$$

The treatment effect of the 2007 policy change on those with need (low EFC) is captured by coefficients $\delta_1 + \delta_5$ and the effect on those without need is given by δ_1. Similarly, the treatment effect of the 2008 policy change is given by $\delta_2 + \delta_6$ and δ_2.

The results from Table 7 indicate the policy changes affected high and low EFC freshmen borrowers differently, with surprisingly a larger impact on overall borrowing among high EFC borrowers. For the 2007 policy change the treatment effect on low EFC households is equal to

$334, which is not statistically different than zero. For 2008, low EFC household's borrowed an additional $1,489 as a result of the policy change. The impact was larger among high EFC students as total borrowing rose by $2,701 and $2,859 due to the 2007 and 2008 policy changes. Students with the least need as indicated by potential family support responded the most strongly to changes in Stafford loan limits. This is quite different from Ionescu and Simpson's (2016) results, where they find students with low EFC respond more strongly by borrowing $223 more, relative to students with high EFC that borrow $79 more. We believe the larger effect we find may be a result of parents with high EFC providing insufficient transfters to meet their student's desired consumption level. Given most students who borrow from the GSL program borrow the statutory limit, it is not surprising borrowing for these students rises similar to the change in limits. It may also be the case that some parents choose to provide transfers only equal to the net cost of attendance after deducting what there student may borrow from the GSL program.

Examining the impact on private loans (Table 7, columns 3 and 4) we discover another interesting finding as it relates to private borrowing. Among low EFC students, the change in 2007 loan limits had no statistically measureable impact on their overall borrowing, but with respect to private borrowing we see there is a reduction of private borrowing by $597, which is statistically significant at the 5% level (p-value .03). Similarly, in 2008 low EFC students reduced their private borrowing by $614 (p-value .02). Based on these results it appears low EFC students used the additional GSL credit to adjust their composition of borrowing away from private loans, whereas unconstrained students took advantage of easy access to credit to increase consumption.[20]

[20] Keane and Wolpin (2001) also find that relaxing borrowing constraints induces students to work less and consume more while in college.

4.3 Impact on Student Outcomes

We next turn our attention to whether the changes in GSL limits had an impact on student outcomes. The three outcomes we consider are a student's cumulative grade point average (GPA), their credits completed at the end of their freshmen year, and whether they failed to return for their sophomore year. The latter measure is often referred to as a student stopping out. A similar difference-in-differences framework is used as before, with our treatment group consisting of freshmen who borrow from the Stafford loan program and the control group consisting of non-Stafford borrowers and non-borrowers. Figure 4 depicts the mean for each performance measure and group before and after the change in policy.

[Insert Figure 4 about here]

For our baseline specification of student outcomes we include the same variables used in the baseline model of borrowing and add control variables for a student's high school GPA, and whether they are female, or a minority student. Table 8 column 1 displays the results of our baseline difference-in-differences model for a student's cumulative grade point average. The treatment effect of the 2007 change in GSL limits is to reduce on average a student's GPA by .16 grade points relative to the GPAs of the 2006 cohort. What this result implies is freshmen, who had a greater ability to borrow from the GSL program due to the policy change, performed academically worse than their counterparts the year before.[21] The impact of the 2008 policy change, while negative is not found to be significant at the 10% level (p-value .14). Results (column 2, Table 8) using difference-in-differences with propensity score matching showed the

[21] One possible reason for this finding may be that student study behavior is primarily determined by the student's predetermined non-cognitive traits such as conscientiousness and future orientation and not by access to financial aid; see, Delaney et al. (2013).

impact of the 2007 policy change to be slightly larger in magnitude, reducing GPA by .18 grade points, with no significant effect in 2008.

This result contrasts with experimental findings on the effects of direct financial incentives for good grades. For example, Angrist et al. (2009) find that such incentives have persistent multi-year effects on academic performance for women. Scott-Clayton (2011), examining the PROMISE program in West Virginia, also find that financial incentives linked to minimum GPA scores and course load have significant impacts on academic outcomes. The suggestion is that, if improving academic performance is a policy goal, then, blunt policy instruments like raising loan limits are likely to be far less effective at fulfilling these goals than targeted financial incentives.

Demographic factors and parental income also had a larger impact on student performance than borrowing. The results indicated female students perform significantly better and minority students significantly worse by a magnitude of .29 grade points. Increasing a family's EFC also played an important role, as academic performance steadily increased for each of the groups.

[Insert Table 8 about here.]

With respect to credits completed the results of our baseline model in column 3 of Table 8 suggest both the 2007 and 2008 policy changes had a negative impact on student outcomes, relative to the 2006 cohort. The treatment effect of the 2007 policy change was to reduce the average number of credits completed by 1.1, which is quite substantial given the average number completed is approximately 26. The same sized effect is found due to the 2008 policy change, with both of the effects statistically significant at the 10% level.

Demographics again played a role in credits completed, with women completing on average 1.8 more credits and minorities nearly 3 credits (1 course) less per year than other freshmen. When we use the differences-in-differences matching estimator the estimate of the treatment effects are slightly larger in magnitude than the baseline model. The 2007 policy change decreased credits completed by 1.2, which was significant at the 10% level. For the 2008 estimate, the treatment effect is to reduce credits completed by 1.35, which was significant at the 5% level. These estimates appear in column 4 of Table 8. In our final analysis we examine whether the changes in GSL limits had an impact on freshmen retention. A binary indicator is our dependent variable, which takes a value of one if a student did not re-enroll for the start of their sophomore year (stop out). Twenty percent of the 2006 freshmen cohort did not return for their sophomore year. Of the 2007 cohort, sixteen percent of freshmen did not return, and 20% did not return in 2008. The results of our baseline model estimated using logistic regression indicate in column 5 of Table 8 the policy changes that eased borrowing constraints did not have an impact on whether freshmen were retained, a finding supported by Stinebrickner and Stinebrickner (2008) for students at Berea College, Kerkvliet and Nowell (2005) for students at Weber State University and Oregon State University, and Singell (2004) for students at the University of Oregon. Similarly, the use of a matched sample (column 6) did not uncover evidence of an impact due to either policy change.

Our results are also largely consistent with DesJardins et al. (2002) who examine data from the mid-1980's on 4800 students from the University of Minnesota. DesJardins et al. do find that loan availability reduced the probability of stop out, but the magnitude of the effect was small and substantially smaller than those for other forms of financial aid.

4.3.1 Falsification test and robustness checks

Similar to our analysis of the impact of the policy on total borrowing and composition, we considered a number of falsification tests and robustness checks. A falsification test in which the treatment group consisted of non-Stafford borrowers and the control group was non-borrowers showed no significant finding for the false treatment for all 3 student outcome variables (as expected).[22]

Further robustness checks using the separate control groups (non-borrowers & non-Stafford borrowers) are also provided to evaluate the sensitivity of our results to changes in the control group. In both instances, we find evidence that the 2007 policy change lowered academic performance as measured by GPA. Using the non-borrowers as our control group the treatment effect is -.14, whereas for the control group of non-Stafford borrowers the effect is -.29. With regard to credit completion, with non-borrowers as our control group, the estimate of the treatment effect is -.81, which is not statistically significant (p-value .23). For the 2008 estimate the result is -1.12 and significant at the 10% level. When non-Stafford borrowers are used as the control group the treatment effect of the 2007 change is magnified, with an estimate of -2.99 that is statistically significant at the 5% level. Hence, again, we find that the effect of the policy is to lower academic achievement. The estimated effect of the 2008 policy is similar to our baseline results at .97, but the large standard error results in an insignificant finding (p-value .50). Finally, employing alternative control groups did not result in any supporting evidence to suggest that changes in policy impacted freshmen retention.[23] We therefore conclude that our baseline results for student outcomes are robust to changes in the control group.

[22] These estimates appear in columns 7-15 of the appendix.

[23] These results and those for the control group split appear in Appendix 1.

A potential concern one might have is whether the lower performance in GPA and credit completion witnessed in 2007 was a result in a shift of major choices by freshmen Stafford borrowers to more challenging majors. In an attempt to control for this we add to our baseline model a variable indicating whether a student was a science, technology, engineering, or math (STEM) major, given the potential such students may enroll in more challenging courses their freshmen year. We also added to this specification our first-generation and contact count measures.

Major choice did not appear to have an impact on GPA as the effect was not statistically significant and our estimates (Table 9, column 1) of the treatment effect were unchanged. First-generation students performed slightly worse, though the marginal effect while statistically significant was negligible at .06 grade points. Increasing contact counts by 1 standard error (4.6 contacts) improved one's GPA by .12 grade points.

For the case of credit completion, each of the three variables (contact counts, first-generation status, and STEM major) added to the baseline specification were significant at the 10% level. STEM majors were likely to complete ½ less credits per year than other majors, while first-generation college students completed .65 credits less and each additional contact increased credits completed by .20. The estimates of the treatment effects were similar to those we found in the baseline model, with effects significant at the 10% level. With respect to retention, we did not find that major choice had an impact, though first generation students were more likely to stop out.

[Insert Table 9 about here]

5. Conclusion

In this paper, we consider the effects of changes in Stafford loan limits in 2007 and 2008 compared to the benchmark year of 2006 on the behavior and outcomes of first-time freshmen students at the University of North Dakota. The North Dakota setting is of interest because, while the rest of the country was entering the historic Great Recession in 2007 and 2008, the state of North Dakota enjoyed economic growth that was consistent with full employment. The North Dakota context, therefore, allowed us to isolate the effects of the policy changes from the confounding influence of a concurrent economic shock of historic proportions.

We find that, relative to the benchmark 2006 cohort, the increase in Stafford loan limits led to an increase in total borrowing comparable to 65-100% of the total increase in loan limits. There is evidence that the increase in Stafford loan limits led to at most partial crowding out of borrowing from private lenders, as some students substituted Federal loans for private loans. However, in this case, the treatment effect also exhibited substantial heterogeneity with students who expect low family contributions using the additional loan access to substitute for private loans while financially unconstrained students used the additional credit simply to increase consumption. In terms of student academic outcomes, the impact of the increase in Stafford loan limits was largely negative in terms of GPA scores. Hence, while this policy may have led to some welfare improvements for poorer students who would otherwise have had to accept potentially less favorable terms from private lenders, the benefits of the policy have to be weighed against the finding that the overall set of outcomes from this policy largely do not conform to its intended goals.

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Table 1: Stafford Loan Program Maximum Annual Limits, AY 1993-94 to Present

Academic Year	Freshmen	Sophomores	Juniors	Seniors
AY 1993-94 to AY 2006-08	$2,625	$3,500	$5,500	$5,500
AY 2007-08	$3,500	$4,500	$5,500	$5,500
AY 2008-09 to present	$5,500	$6,500	$7,500	$7,500

Table 2: Average UND Freshman Borrowing by Loan Source

	AY 2006-07	AY 2007-08	AY 2007-08
Stafford Loan	$2,556.69	$3,347.49	$4,892.54
	78.5%	74.0%	73.5%
Perkins Loan	$2,019.13	$2,368.90	$2,312.55
	22.6%	26%	22.9%
Private Loan	$7,276.43	$7,240.55	$7,021.14
	16.5%	10.6%	8.8%
Parent PLUS Loan	$6,513.20	$7,259.36	$6,594.57
	10.5%	7.3%	5.9%
State Loan Program	$6,712.60	$6,633.08	$6,433.64
	14.1%	18.1%	15.5%
Total Borrowing	$6,439.14	$7,211.20	$7,989.87
	82.1%	77.6%	76.7%

The average dollar amount is among freshmen who borrowed from a particular loan program or who borrowed from any source in the case of total borrowing. The percentage represents the fraction of freshmen FAFSA filers who borrowed from a particular program or at all.

Table 3: Data Summary of Means

	AY 2006-07			AY 2007-08			AY 2008-09		
Variable	Control	Treatment		Control	Treatment		Control	Treatment	
Close Proximity	0.15	0.17		0.24	0.17	**	0.13	0.17	
ACT score	24.40	22.82	**	24.75	23.32	**	24.47	23.07	**
First Generation	0.20	0.23		0.17	0.22	*	0.15	0.21	**
Number of Contacts	3.13	2.83	**	6.59	6.24	*	5.12	4.55	**
ND/MN Resident	0.94	0.95		0.93	0.94		0.92	0.92	
Parental Income ($1000)	90.36	78.36	**	94.73	79.32	**	114.66	84.44	**
Student Income ($1000)	2.56	2.95		2.49	3.02	**	3.13	3.42	
Scholarships Received	2.64	1.76	**	3.01	1.97	**	3.16	2.09	**
EFC <= 5K	0.19	0.25	**	0.20	0.25	*	0.15	0.24	**
5K < EFC <= 10K	0.13	0.21	**	0.12	0.22	**	0.11	0.18	**
10K < EFC <= 15K	0.12	0.19	**	0.14	0.16		0.08	0.18	**
15K < EFC	0.57	0.34	**	0.55	0.36	**	0.67	0.39	**
Female	0.51	0.55		0.55	0.52		0.55	0.53	
Minority	0.05	0.03		0.03	0.04		0.06	0.04	
Age	19.58	19.61		19.59	19.60		19.57	19.58	
Family Size	4.06	4.02		4.02	4.04		4.09	4.00	
High School GPA	3.49	3.40	**	3.55	3.40	**	3.53	3.37	**
ACT Ordering	1.76	1.70		1.61	1.56		1.74	1.64	
STEM major	0.32	0.19	**	0.27	0.22	**	0.28	0.24	*
Observations	215	787		241	686		275	757	

Note: Two sample t-test of control and treatment group difference in means with equal variances

ACT ordering is subject to missing values, which reduces the number of observations below those reported

*, **, Statistically different from zero at the 10%, and 5% level

Table 4: Impact on Freshmen Borrowing

	(1)	(2)	(3)	(4)
Close Proximity	-777.4331***		-265.4437***	
	(166.2623)		(87.3604)	
ACT Score	-100.0509***		-22.3277*	
	(17.3159)		(12.0544)	
	-		-	
ND/MN Resident	1499.3342***		1100.1749***	
	(360.1793)		(303.7341)	
Parental Income ($1000)	-1.0489		-0.0830	
	(1.7787)		(1.0192)	
Student Income ($1000)	38.0159***		36.9942**	
	(13.5798)		(16.1932)	
Scholarships ($1000)	-241.9485***		-125.8891***	
	(27.2412)		(18.6068)	
5K < EFC <= 10K	798.2903***		185.1204	
	(219.6029)		(164.9868)	
10K < EFC <= 15K	173.4033		-46.5374	
	(245.2607)		(195.7624)	
15K < EFC	-127.4002		-102.2696	
	(267.6556)		(181.7709)	
Treatment Group	5063.4598***		956.5673***	
	(222.6733)		(179.8774)	
Treatment Effect (2007)	923.5275***	1000.621***	-424.5820*	-360.379*
	(289.9606)	(278.600)	(216.4603)	(204.597)
Treatment Effect (2008)	1886.7703***	1897.217***	-564.3414***	-594.616***
	(284.9976)	(314.541)	(210.0276)	(190.203)
Observations	2961	1915; 2000	2961	1915 ; 2000
Adjusted R-squared	0.422	.313; .375	0.062	0.026 ; 0.033

Note: The dependent variable in columns (1-2) is total borrowing, i.e. federal government and private student loans. Columns (3-4) examine the effects on borrowing with private student loans. Estimates in columns (2) and (4) are from kernel weighted matching on the propensity score using separate 2006-2007 and 2006, 2008 samples. Robust standard errors clustered by high school appear in parentheses in columns (1) and (3) with bootstrapped cluster robust standard errors using 100 iterations appearing in columns (2) and (4). *, **, *** indicate statistically different from zero at the 10%, 5%, and 1% level.

Table 5: Covariate Balancing Check

	Control	Treatment	Difference	P-value
Close Proximity	0.160	0.159	0.000	0.995
ACT score	22.912	22.856	-0.056	0.786
ND/MN Resident	0.937	0.943	0.007	0.648
Parental Income ($1000)	80.568	78.252	-2.316	0.525
Student Income ($1000)	2.679	2.749	0.070	0.690
Scholarships Received	1.674	1.748	0.074	0.604
5K < EFC <= 10K	0.204	0.213	0.009	0.721
10K < EFC <= 15K	0.196	0.201	0.005	0.841
15K < EFC	0.355	0.335	-0.020	0.493

The difference in mean values for the control and treatment groups after kernel weighted propensity score matching.

Table 6: Alternative Borrowing Specifications

	(1)	(2)
Close Proximity	-800.1149***	-714.4018***
	(180.2326)	(184.5730)
ACT Score	-97.3459***	-95.5926***
	(18.0432)	(20.1120)
ND/MN Resident	-1487.8711***	-1525.5963***
	(360.4873)	(440.2001)
Parental Income ($1000)	-0.8578	-0.4121
	(1.7664)	(2.0638)
Student Income ($1000)	36.5930***	22.1408
	(13.6435)	(18.8646)
Scholarships ($1000)	-243.6133***	-248.7996***
	(27.2462)	(30.4847)
5K < EFC <= 10K	813.6268***	660.7092***
	(221.7068)	(245.4580)
10K < EFC <= 15K	205.4620	110.3075
	(249.2747)	(261.3664)
15K < EFC	-82.8199	-292.1443
	(262.1774)	(286.7618)
Treatment Group	5059.3993***	5130.1955***
	(221.4566)	(215.5067)
Treatment Effect (2007)	912.2610***	623.6836**
	(285.2150)	(299.4062)
Treatment Effect (2008)	1875.8095***	1652.1942***
	(283.5472)	(319.7239)
First Generation	383.7967*	
	(203.9214)	
Number of Contacts	-10.5964	
	(38.0087)	
ACT Ordering		64.7255
		(77.6039)
Observations	2961	2246
Adjusted R-squared	0.423	0.422

Note: Column (1) adds to the baseline model the potentially endogenous regressors, first generation college student and number of contacts. A measure of institution preference (ACT ordering) is added to column (2) to check for robustness. The dependent variable is total borrowing. Robust standard errors clustered by high school appear in parentheses. *, **, *** indicate statistically different from zero at the 10%, 5%, and 1% level.

Table 7: Mechanisms Influencing Borrowing

	(1)	(2)	(3)	(4)
Close Proximity	-996.8650***	-30.6568	-782.7700***	-272.1303***
	(175.4471)	(92.3318)	(170.9466)	(87.7653)
ACT Score	-102.3984***	-15.8212	-90.1301***	-21.1498*
	(17.8296)	(11.7982)	(16.6947)	(11.7056)
ND Resident	308.9028	-52.7790	-1468.5430***	-1092.6515***
	(324.3798)	(224.2462)	(360.7992)	(304.4270)
Parental Income ($1000)	-0.8072	-0.3136	1.1613	0.3869
	(1.8912)	(1.0316)	(1.6342)	(0.9437)
Student Income ($1000)	34.3341**	38.4990**	44.3648***	38.8991**
	(13.6023)	(15.8754)	(12.7368)	(16.7457)
Scholarships ($1000)	-245.3169***	-104.2780***	-269.4561***	-127.9675***
	(28.1699)	(17.6275)	(23.7263)	(15.6397)
5K < EFC <= 10K	726.5955***	261.1365		
	(222.6817)	(165.6396)		
10K < EFC <= 15K	122.3627	67.1198		
	(250.4653)	(192.3038)		
15K < EFC	-175.1981	11.1815		
	(277.2815)	(183.1643)		
Treatment Group	5078.6486***	1506.3107***	4147.6737***	702.0451***
	(387.7456)	(299.5814)	(437.0607)	(260.8204)
ND Resident	308.9028	-52.7790		
	(324.3798)	(224.2462)		
EFC			593.0093	421.7457*
			(393.7781)	(243.9187)
Treatment Effect (2007)	1377.6925***	-194.9773	2700.8391***	259.5936
	(418.2850)	(342.6701)	(599.4012)	(356.6395)
Treatment Effect (2008)	2038.7918***	-828.5263**	2858.6430***	-197.6120
	(441.8334)	(347.5382)	(568.4619)	(351.9176)
Treatment Group	-97.9435	-972.3665***	1135.9338**	260.8403
	(450.3645)	(348.1236)	(518.3327)	(331.1014)
T.E. Interaction (2007)	-803.9256	-458.4037	-2367.0008***	-857.3051**
	(559.3998)	(405.0698)	(698.2371)	(430.4582)
T.E. Interaction (2008)	-244.2204	390.6297	-1370.4611**	-417.1630
	(588.8266)	(397.1251)	(679.0777)	(439.5526)
Observations	2961	2961	2961	2961
Adjusted R-squared	0.416	0.073	0.424	0.063

Note: The dependent variable in columns (1) and (3) is total borrowing and in (2) and (4) private student loan borrowing. Columns (1-2) explore whether the treatment effect varies by whether one is a North Dakota resident (interaction effect). For columns (3-4), the treatment effect is allowed to vary by expected family contribution (EFC). Robust standard errors clustered by high school appear in parentheses. *, **, *** indicate statistically different from zero at the 10%, 5%, and 1% level.

Table 8: Impact on Student Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
High School GPA	0.0023*		0.0094		-0.9090***	
	(0.0012)		(0.0086)		(0.1245)	
Female	0.2926***		1.8221***		0.1105	
	(0.0245)		(0.1949)		(0.1138)	
Minority Student	-0.2924***		-2.9316***		0.3994	
	(0.0779)		(0.5610)		(0.2494)	
Close Proximity	-0.0928***		-1.4665***		0.0924	
	(0.0336)		(0.2581)		(0.0907)	
ACT Score	0.0647***		0.3628***		0.0232	
	(0.0038)		(0.0318)		(0.0158)	
ND/MN Resident	0.0070		-0.6724		-0.1798	
	(0.0513)		(0.4488)		(0.1961)	
Parental Income ($1000)	0.0007**		0.0047**		-0.0007	
	(0.0003)		(0.0020)		(0.0011)	
Student Income ($1000)	0.0035		0.0450***		0.0144*	
	(0.0026)		(0.0159)		(0.0080)	
Scholarships ($1000)	0.0557***		0.3879***		-0.0939***	
	(0.0062)		(0.0525)		(0.0285)	
5K < EFC <= 10K	0.1696***		1.7155***		-0.4845***	
	(0.0512)		(0.3837)		(0.1830)	
10K < EFC <= 15K	0.2171***		1.6950***		-0.4014**	
	(0.0491)		(0.4454)		(0.1713)	
15K < EFC	0.2437***		1.8860***		-0.5806***	
	(0.0542)		(0.4234)		(0.1758)	
Treatment Group	0.0157		0.1708		0.3376	
	(0.0522)		(0.4866)		(0.2292)	
Treatment Effect (2007)	-0.1632**	-.181***	-1.0598*	-1.210*	0.4497	0.042
	(0.0681)	(.087)	(0.5769)	(.694)	(0.3444)	(.032)
Treatment Effect (2008)	-0.1111	-.124	-1.1135*	-1.352**	-0.2434	-0.018
	(0.0749)	(.126)	(0.5721)	(.611)	(0.2790)	(0.035)
Observations	2920	1876 ; 1972	2920	1876 ; 1972	2920	1876 ; 1972
Adjusted R-squared	0.220	0.020 ; 0.015	0.141	0.010 ; 0.013	0.053	.013 ; .004

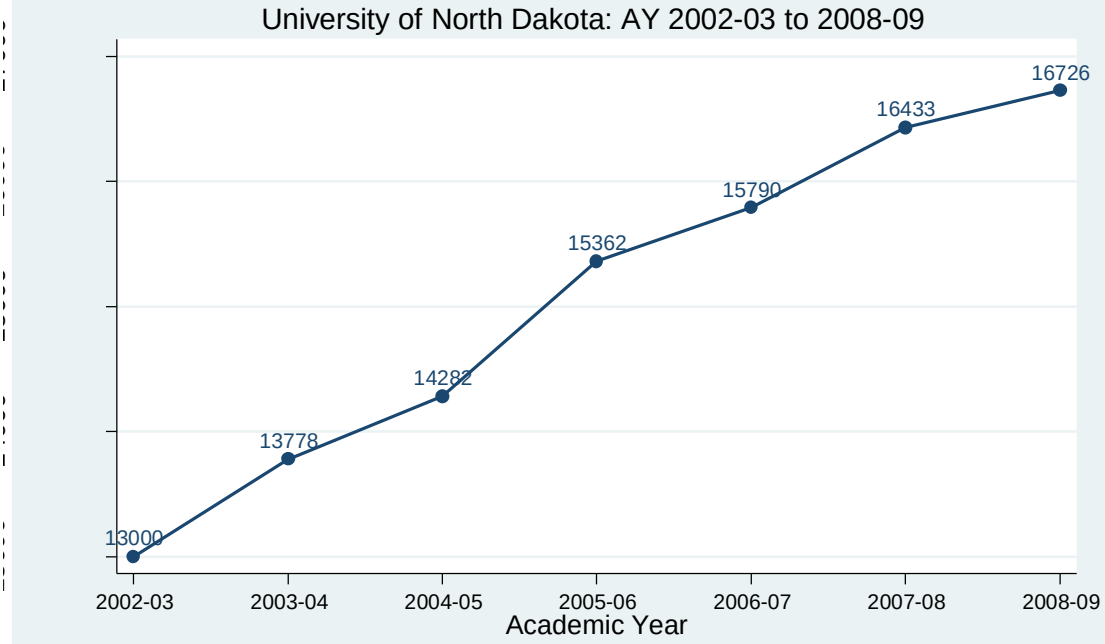
Note: The dependent variable in columns (1-2) represents a student's cumulative GPA at the end of their freshmen year, columns (3-4) represents credits completed after freshmen year, and columns (5-6) is an indicator variable for whether a student re-enrolls for their sophomore year. The estimates in columns (2, 4, and 6) are from kernel weighted matching on the propensity score using separate 2006-2007 and 2006, 2008 samples. Robust standard errors clustered by high school appear in parentheses in columns (1, 3, and 5) with bootstrapped cluster robust standard errors using 100 iterations appearing in columns (2, 4, and 6). *, **, *** indicate statistically different from zero at the 10%, 5%, and 1% level.

Table 9: Alternative Student Outcome Specifications

	(1)	(2)	(3)
High School GPA	0.0023**	0.0094	-0.9173***
	(0.0012)	(0.0084)	(0.1251)
Female	0.2771***	1.6613***	0.1484
	(0.0249)	(0.1952)	(0.1101)
Minority Student	-0.2762***	-2.8009***	0.3747
	(0.0793)	(0.5728)	(0.2541)
Close Proximity	-0.0643**	-1.2709***	0.0862
	(0.0326)	(0.2676)	(0.0928)
ACT Score	0.0633***	0.3588***	0.0206
	(0.0040)	(0.0341)	(0.0162)
ND/MN Resident	0.0154	-0.6196	-0.1662
	(0.0508)	(0.4418)	(0.1996)
Parental Income ($1000)	0.0006**	0.0044**	-0.0005
	(0.0003)	(0.0020)	(0.0010)
Student Income ($1000)	0.0039	0.0489***	0.0136*
	(0.0025)	(0.0156)	(0.0082)
Scholarships ($1000)	0.0543***	0.3780***	-0.0959***
	(0.0061)	(0.0524)	(0.0286)
5K < EFC <= 10K	0.1594***	1.6403***	-0.4828***
	(0.0512)	(0.3832)	(0.1855)
10K < EFC <= 15K	0.2004***	1.5581***	-0.3790**
	(0.0498)	(0.4432)	(0.1726)
15K < EFC	0.2210***	1.7022***	-0.5518***
	(0.0543)	(0.4228)	(0.1776)
Treatment Group	0.0147	0.1464	0.3497
	(0.0521)	(0.4869)	(0.2275)
Treatment Effect (2007)	-0.1557**	-0.9867*	0.4293
	(0.0666)	(0.5772)	(0.3390)
Treatment Effect (2008)	-0.1016	-1.0238*	-0.2594
	(0.0741)	(0.5735)	(0.2767)
Stem Major	-0.0343	-0.5041*	0.1996
	(0.0324)	(0.2624)	(0.1243)
First Generation	-0.0662**	-0.6456**	0.2957***
	(0.0300)	(0.2710)	(0.1136)
Number of Contacts	0.0265***	0.1954***	-0.0103
	(0.0071)	(0.0596)	(0.0269)
Observations	2920	2920	2920
Adjusted R-squared	0.225	0.146	0.056

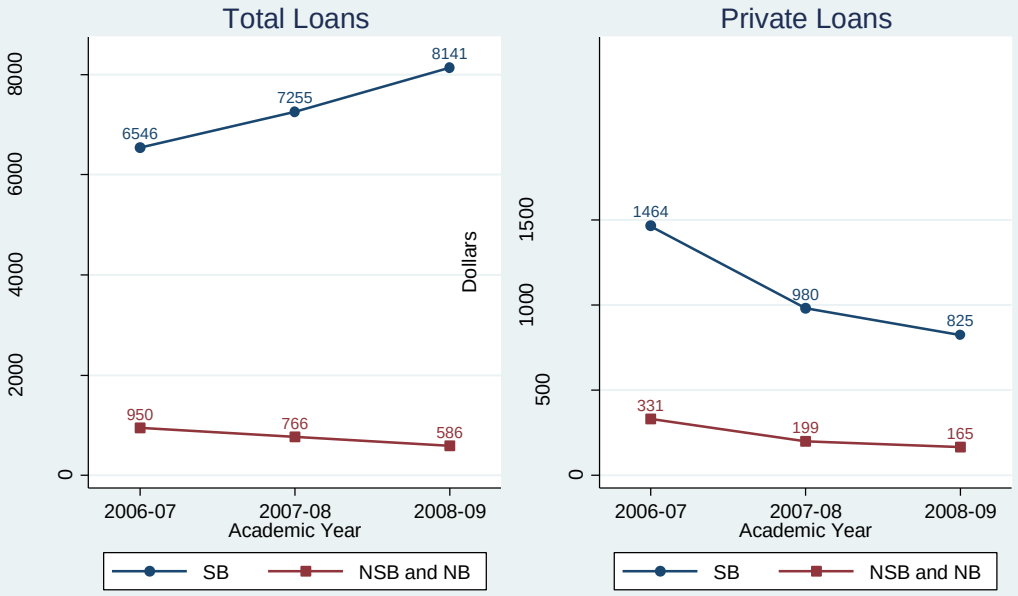
Note: The dependent variable in column (1) represents a student's cumulative GPA at the end of their freshmen year, column (2) represents credits completed after freshmen year, and column (3) is an indicator variable for whether a student re-enrolls for their sophomore year. Each specification adds to the baseline model the potentially endogenous regressors, first generation college student and number of contacts. Robust standard errors clustered by high school appear in parentheses. *, **, *** indicate statistically different from zero at the 10%, 5%, and 1% level.

Figure 1: Cost of Attendance



Source: National Center for Education Statistics - IPEDS

Figure 2: Borrowing by Group - Pre/Post Policy Changes

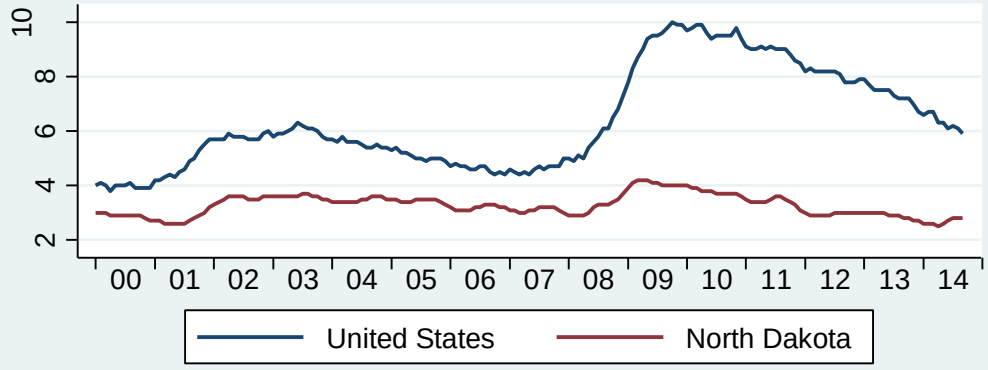


Treatment Group: SB Stafford Borrower
Control Group: NSB Non-Stafford Borrower and NB Non-Borrower

Figure 3: Macroeconomic Conditions

Unemployment Rates: 2000-2014

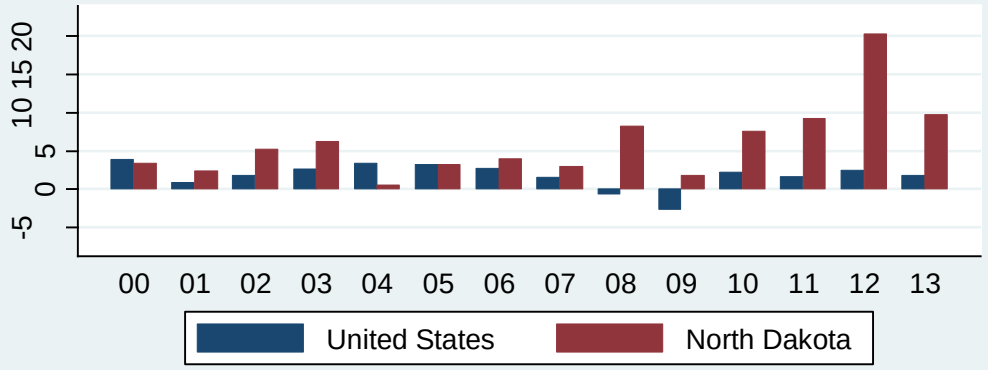
Seasonally Adjusted



Source: US Bureau of Labor Statistics - Local Area Unemployment Statistics

Real GDP Growth Rate: 2000-2013

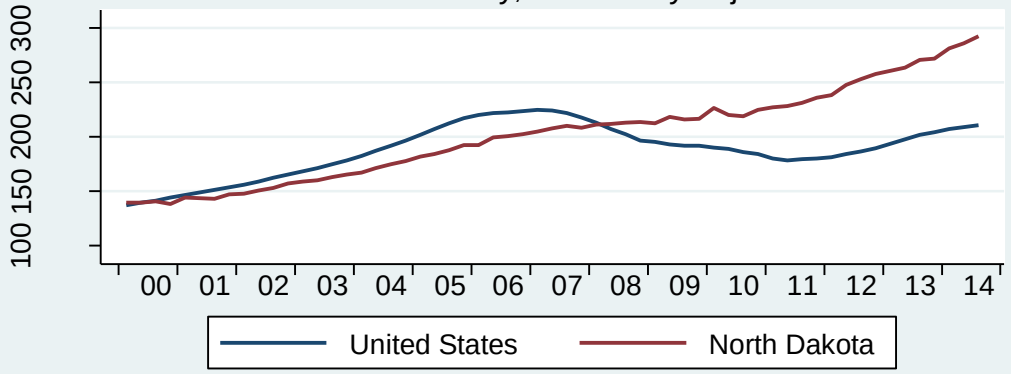
2009 Chained Dollars



Source: U.S. Bureau of Economic Analysis

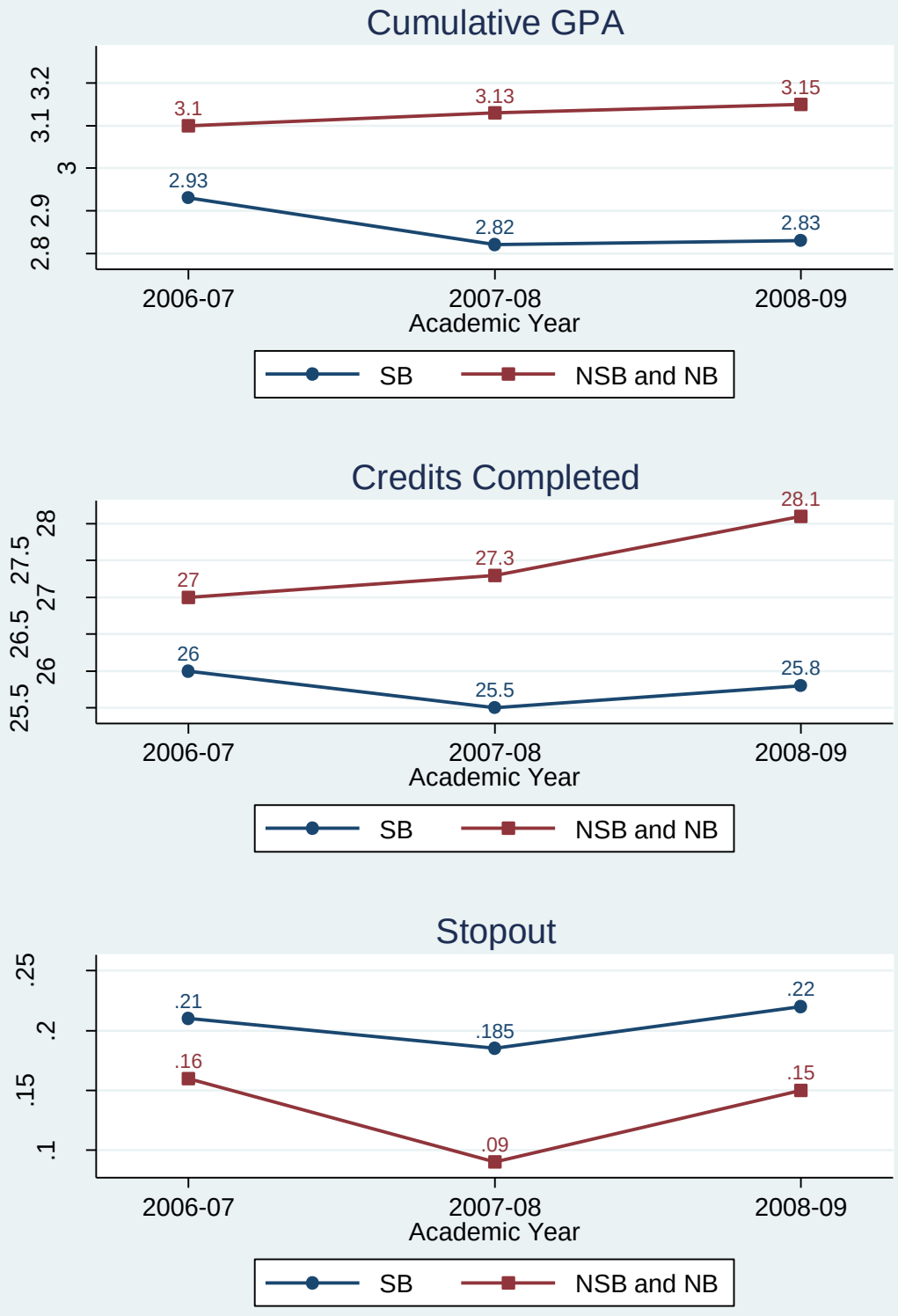
Housing Price Indices: 2000-2014

Purchases Only, Seasonally Adjusted



Source: Federal Housing Finance Agency

Figure 4: Performance by Group - Pre/Post Policy Changes



Treatment Group: SB = Stafford Borrower
Control Group: NSB = Non-Stafford Borrower and NB = Non-Borrower

Appendix 1

	(1)	(2)	(3)	(4)	(5)	(6)
Treatment Effect (2007)	-181.3766	814.2703***	249.9315	-300.4249	-483.6487***	-534.9307
	(845.2961)	(225.2226)	(842.2559)	(825.4584)	(164.0246)	(857.0417)
Treatment Effect (2008)	-400.1990	1667.4445***	1604.4480**	-354.0901	-658.1280***	-520.9063
	(775.0265)	(220.1783)	(701.6741)	(801.6633)	(155.6028)	(811.6816)
Observations	731	2858	2333	731	2858	2333
Adjusted R-squared	0.687	0.463	0.145	0.160	0.071	0.062

	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Treatment Effect (2007)	0.0525	-0.1413**	-0.2891**	1.3783	-0.7287	-2.9874**	-0.3735	0.3928	0.8620
	(0.1146)	(0.0713)	(0.1417)	(1.1375)	(0.6720)	(1.2314)	(0.9010)	(0.3514)	(0.8956)
Treatment Effect (2008)	-0.0458	-0.1123	-0.0709	-0.1721	-1.0769*	-0.9697	0.3192	-0.1668	-0.5772
	(0.1910)	(0.0806)	(0.1792)	(1.4699)	(0.6107)	(1.4462)	(0.6453)	(0.3008)	(0.6301)
Observations	720	2821	2299	720	2821	2299	720	2821	2299
Adjusted R-squared	0.409	0.216	0.204	0.276	0.140	0.126	0.080	0.051	0.044

Notes: The results reported here are the treatment effects of the baseline specifications using alternative control and treatment groups. The dependent variable in columns (1-3) is total borrowing, i.e. federal government and private student loans and in columns (4-6) is private student loan borrowing. Panel b provides analysis of freshmen GPA (columns 7-9), credits completed (columns 10-12), and stop out (columns 13-15). Each specification used the baseline controls included in the previous models, with complete estimates available upon request. We conduct a falsification test (columns 1, 4, 7, 10, and 13) for our baseline specifications where the treatment group includes non-Stafford loan borrowers and the control group is non-borrowers. The sensitivity of our estimates is examined in the other columns. Columns (2, 5, 8, 11, and 14) use a treatment group consisting of Stafford borrowers and a control group of non-borrowers, while columns (3,6, 9, 12, 15) use a treatment group of Stafford borrowers and a control group of non-Stafford borrowers. Robust standard errors clustered by high school appear in parentheses. *, **, *** indicate statistically different from zero at the 10%, 5%, and 1% level.