Not So Simple? Financial Aid Simplification and the Impact of Pell Grants on College Enrollment

PRELIMINARY: PLEASE DO NOT CITE OR CIRCULATE

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1 Introduction

Children born to rich parents are twice as likely to attend, and three times as likely to graduate from college as children born to poor parents. The federal government devotes about $30 billion per year to close this gap by subsidizing the cost of college for low and middle-income students through the Pell grant program, and billions more on an array of other grants, tax deductions and credits, and subsidized loans. The evidence on whether these programs are effective in promoting college going for low-income students, however, is not encouraging. While studies of other aid programs have shown positive effects on enrollment, persistence, completion, and labor market outcomes, a variety of studies have found that the Pell grant program has little to no effect on college enrollment, completion, or post-graduation outcomes (Hansen 1983, Kane 1995).A prominent explanation for this paradox is that the complexity of the eligibility determination process for the Pell grant undermines its impact, leading for calls to simplify the application process (Dynarski and Scott-Clayton 2006, Bettinger, Long, Oreopoulos and Sanbonmatsu 2012).

This study attempts to shed new light on the impact of aid simplification and Pell grant receipt on college enrollment. To isolate the causal effect of simplification and Pell receipt, I exploit features of the financial aid application process intended to simplify if for lower income students that have not previously been evaluated. In particular, I study two policies that allow students with family income below a threshold ($49,999 or less in 2016-2017) to use a "simplified needs test (SNT)" to determine their financial aid eligibility, and students with family income below a lower threshold ($25,000 in 2016-2017) to receive an "automatic-zero (AZ)" expected family contribution (EFC), and thus the maximum amount of federal Pell grant aid. Students qualifying for the SNT do not need to report student or parent assets in their federal aid determination, and students qualifying for AZ need only report their parent's income and a small set of extra demographic information; information about assets, their own income, and taxes paid by themselves and their parents is not required. When the Department of Education started allowing federal student aid applicants to fill out their applications on the web, the website incorporated 'automatic skip-logic' algorithms that encourage students to avoid filling in the unneeded items on the application by allowing the student to skip the irrelevant parts of the online application. In addition to a much simpler aid application

process, the AZ rules also generate significantly higher Pell grant awards for students with family incomes just below the AZ thresholds. The amount varies from year to year as both the size of the maximum Pell award, and the level of family income used for the AZ threshold changes, with differences in average Pell awards reaching over $800 in 2010.

Using a combination of regression discontinuity and difference-in-difference methods, and a vast set of administrative data covering 13 years of (nearly) all federal aid recipients beginning their studies at a post-secondary institution, I estimate the contributions of aid simplification and increases in Pell grant aid on college enrollment. Since the probability of being eligible for a simplified aid application and the expected Pell award are both discontinuous at the AZ threshold, regression discontinuity methods estimate the combined effect of both policies on enrollment. To isolate the independent contribution of each, I leverage the fact that a group of 15 states do not allow the Department of Education to implement application simplification through skip-logic, since they use the additional financial information in their state aid eligibility formulas. In these states, any discontinuity in enrollment is driven only by the effect of the increase in Pell grant aid, and under plausible restrictions on treatment effect heterogeneity we can difference the RD estimates across states that do and do not allow simplification to estimate the effect of simplification. The richness of the data allow me to estimate these parameters separately for a number of subgroups in the data, across both different groups of students and different time periods.

I find that the AZ policy seems to have a small positive impact on dependent students' college enrollment, of about 4 percent for dependents and about half that for independent students. This effect, however, appears to have little to nothing to do with simplification of the application process. Rather, the small effect appears partly due to the impact of Pell grants, and partly due to some other aspect of the AZ policy–I offer a candidate explanation below, and suggest future avenues for investigation.

The paper proceeds as follows. The next section provides background of research on the impact of grant aid for college, and why researchers have reached a consensus around the importance of aid simplification. Following that I describe the unique data employed by the paper–a set of linked administrative records covering nearly every federal aid (e.g., Pell grant or Stafford Loan) between 2002 and 2014. With that background, section 4 presents the research design starting with details on the aid application process and the two simplification policies, and then a discussion of how those policies are used to identify the effects of simplification and Pell award eligibility on

college enrollment. Section 5 presents results illustrating the effect of the simplification policies on students' FAFSA experiences and Pell awards, followed by estimates of the impact of the AZ policy on enrollment. I then discuss estimates of the relative contributions of simplification and Pell grants to that effect. In the final section, I discuss implications of these findings for policy.

2 Background

This study assesses whether complexity in the application process might undermine the intended effects of federal investments in financial aid. This section provides an overview of research on the impact of federal and other college financial aid programs, and background on the emerging consensus around the adverse effects of complexity.

2.1 Impacts of Financial Aid on Enrollment, Completion, and Labor Market Success

The largest sources of federal support for the college attendance of low-income students are the federal Pell Grant program and the Stafford Loan Program, with annual volumes of about $30 and $75 billion, respectively.[1] Students applying for either source of aid must submit extensive information about their own and, if they are financially dependent, their parents' finances to determine their ability to pay for their own education and their need for aid. Since the 1970s, this process has been based on a common set of data elements and a uniform methodology, both codified in an application form called the Free Application for Federal Student Aid (FAFSA) in 1992. The FAFSA is long, with more questions (105 in 2015-16) than a typical tax return (Dynarski and Scott-Clayton 2006, Dynarski and Wiederspan 2012), collecting information about demographic characteristics and the number of family members in college, income, assets, taxes paid, whether a family has received means-tested benefits, experienced certain hardships (spells of homelessness or unemployment), and more. This data is used to develop an index of each student's ability to pay for college, called the expected family contribution (EFC). Students are generally eligible for a Pell Grant equal to the difference between the maximum annual Pell award and their EFC, and eligible for subsidized federal loans that cover the student's remaining 'need', defined as the difference

[1] Pell Grant expenditures were about $28.2 billion in 2015-2016, down (in real terms) 20 percent from the 2010-2011 peak during the recession. Of course, much of the dollars lent through the Stafford program are repaid, so the federal subsidy is much less than the loan volume. From this perspective, both the Pell Grant program and the American Opportunity Tax Credit represent larger federal investments.

between the total cost of college, and the sum of grant aid and the EFC. The data inputs (i.e., the questions on the FAFSA) and methodology that determine EFC are controlled by Congress through modifications to the 1965 Higher Education Act, though the Education Department can and has altered some aspects of the application process through regulation and administrative actions.

Standard models of human capital investment suggest that grant aid should increase college going by lowering the price, thereby increasing net returns (Becker 1964). Similarly, federal loans may ease credit constraints to allow more students to enroll in college. Beyond enrollment effects, both types of aid might reduce the need for students to work while pursuing a degree, which could increase the likelihood of completion and potentially improve post-enrollment outcomes like labor market earnings. While a host of empirical studies find evidence for these hypothesized effects, the literature is not uniformly supportive of the conclusion that financial aid produces benefits commensurate with its costs.

The literature on the effects of the Pell grant program have been particularly confounding for higher education researchers, with little consistent evidence of positive impacts on enrollment or student success outcomes despite its standing as the single largest source of grant support for low-income students. Early work by Hansen (1983), Kane (1995), and Manski and Wise (1983) found no evidence that the 1972 introduction of Pell grants increased low-income dependent student enrollment relative to higher income students' enrollment. On the other hand, Seftor and Turner (2002) refined Kane's methodology and finds that the introduction of Pell increased enrollment for non-traditional students in their twenties and thirties.[2] More recent work built on quasi-experimental variation in Pell eligibility, similar to the variation exploited in this paper, fails to find effects of Pell eligibility on college enrollment, school choice, or college completion (Rubin 2011, Turner 2014, Carruthers and Welch 2015, Marx and Turner 2015).[3]

Beyond the Pell program, however, other studies of college financial aid have found substantial effects on enrollment. (Dynarski 2002) finds the Social Security Student Benefit (SSSB), a college grant benefit for children of deceased SS beneficiaries that was ended in 1982, increased enrollment by about 5 percentage points per $1,000 in aid (in 1997 dollars). Studies of merit-aid program such as the Georgia HOPE Scholarship (Dynarski 2000, Cornwell, Mustard and Sridhar 2006) and the CalGrant (Kane 2003) have shown effects on enrollment of similar magnitude. More recently,

[2]Under the new rules of the 1986 Amendments to the Higher Education Act, to qualify as an independent students needed to be over 24, married, or have their own children.

[3]While other forms of aid are not the focus of this paper, a recent study by Bulman and Hoxby (2015) finds no enrollment effects of federal tax credits for education expenses.

Denning (n.d.) finds consistent evidence that reductions in community college tuition driven by expansions of "in-district" boundaries increase enrollment by roughly 5.1 percentage points per $1,000 decrease (an increase of about 20 percent relative to a baseline enrollment probability of 26.5 percentage points). Angrist, Autor, Hudson and Pallais (2016) also find effects of need based aid on enrollment and persistence in the context of a randomized evaluation of a private need-based grant.

What explains this discrepancy between the programs just described and the apparent lack of impact of the Pell grant program? Summarizing the evidence from many quasi-experimental evaluations including most cited above, Deming and Dynarski (2010) conclude that grant aid programs with simple eligibility rules and less targeted benefits (e.g., Georgia's HOPE scholarship, where eligibility is based only on having a high-school GPA over 3.0) have substantial impacts on college enrollment. Among such programs, they find typical impacts in a range of about 3 to 4 percentage points per $1000 increase in aid eligibility. Studies of programs that are more targeted–especially the federal Pell Grant program–on the other hand, appear to have small to no impacts on enrollment. Deming and Dynarski (2010) suggest the complexity of the Pell Program might undermining its intended impact.[4]

Before turning to this "complexity hypothesis," note that even if financial aid does not increase enrollment it might still have positive social returns if it improves college persistence, completion, and post-enrollment outcomes conditional on enrollment. In a randomized control trial evaluation of a $3,500 annual grant given to students already enrolled at University of Wisconsin, Goldrick-Rab, Kelchen, Harris and Benson (2016) find that grant receipt increased on-time degree completion (by nearly 30 percent relative to non-recipients), and modestly increased credit accumulation and cumulative GPA. Similarly, Castleman and Long (2016) find that in addition to its impact on enrollment the Florida Student Access Grant (FSAG) increased credit accumulation and bachelor's degree completion within 6 years by 22 percent. Bettinger, Gurantz, Kawano and Sacerdote (2016) study the CalGrant program–an unusually generous grant covering all of public school tuition and up to $9,500 of private tuition at colleges in California–and find impacts on degree completion

[4]It is worth noting that several recent studies find substantial effects for grant aid programs that do not fit Deming and Dynarski (2010)'s categorization. For example, Castleman and Long (2016) show that the Florida Student Access Grant (FSAG), a need based grant with eligibility requirements analogous to the Pell grant increases enrollment in public four-year schools by about 12 percent (3.2 percentage points relative to a mean of 26 percent). In 2000, the FSAG awarded about $1,300 (beyond their Pell award) to students whose EFC was below $1,590 (roughly a family income of $30,000). In 2016 dollars, the aid amount is about $1,800 and EFC threshold corresponds to family income of about $42,000. In its first year, knowledge of eligibility and application burden for the FSAG were probably comparable to that for the Pell grant.

and suggestive evidence that grant recipients have higher labor market earnings. These studies all illustrate that even absent an enrollment effect, policies that increase take-up of financial aid might still improve the outcomes of low-income college students.

2.2 Complexity and the Impact of Aid

How might the complexity of the eligibility rules and application process for Pell grants and other federal aid hinder college enrollment? The literature has highlighted at least two possible mechanisms: 1) *lack of transparency about eligibility, and thus college prices*: the complex formula used to determine how much aid students qualify for might cause students to be uncertain about future aid eligibility and thus overestimate the net price (tuition and fees net of financial aid) they will need to pay, and thus discourage them from seeking to attend college; and 2) *hassle costs*: conditional on making the decision to try to attend, the hassle costs of completing the FAFSA might deter students from completing the aid application and thus not be able to afford attendance.

While calls to simplify the financial aid application process and especially the FAFSA form have continued for decades, the most direct evidence that complexity of the aid process might hinder enrollment is the pioneering study by Bettinger et al. (2012). In a field experiment conducted at the tax preparation company H&R Block, Bettinger and colleagues randomly assigned low income adults to one of three treatment arms, using the financial information just collected for tax filing. The main treatment group received immediate personalized assistance preparing their FAFSA (for themselves or their dependent children) generally taking less than 10 minutes since most information was available in their tax forms, an estimate of their eligibility for federal and state aid and information about the net prices (i.e., tuition and fees net of their federal grant aid) of nearby colleges, and assistance submitting the FAFSA form to the U.S. Department of Education. Another treatment group received only the information intervention, and a control group received general information about financial aid, also given to both treatment groups.

The estimated impacts of the main intervention were striking. For dependent students, college attendance increased by 8.1 percentage points, or 24 percent, relative to a control group mean of 34.2 percent. For independent students with no prior college experience, enrollment increased from 9.5 to 11.0 percentage points, an increase of 16 percent. relative to a control group. The costs of this intervention were very low: Dynarski, Hyman and Schanzenbach (2013) show the results suggest FAFSA assistance is a more cost-effective means of increasing college attendance

than other interventions targeting low-income youth by at least an order of magnitude. In contrast to the comprehensive FAFSA assistance intervention, the information-only intervention had no impacts on enrollment relative to the control group.

As noted above, the evidence from this experiment intensified calls for simplifying the application process for federal aid. In part driven by earlier evidence that many of the questions on the FAFSA form had very little impact on the targeting of aid (Dynarski and Scott-Clayton 2006), much of this conversation focused on simplifying the FAFSA form itself (Baum and Scott-Clayton 2013, Bill and Melinda Gates Foundation 2015, National Association of Student Financial Aid Administrators 2015, National College Access Network 2017). As Bettinger et al. noted in their study, however, reducing the hassle involved in completing and submitting the FAFSA is only one of several mechanisms through which their treatment might have increased enrollment. In addition, they suggest the experiment might have affected enrollment decisions by reducing anxiety about making mistakes on an official form, removing stigma about applying for need-based aid, or nudging individuals to consider college attendance. In the conclusion of their study, they comment "one key question of interest is whether our results would have occurred through form simplification alone, without face-to-face assistance (Bettinger et al. 2012, p. 1239)." It is this key question that is the focus of this study.

Below, I describe two long-standing simplifications to the FAFSA that reduce hassle costs by allowing low-income students to skip groups of financial questions when applying for aid, and how I estimate their impact on enrollment. Since the research design is dictated by features of the unique administrative data I employ for the study, I offer a brief overview of this data and then discuss details of how the simplification policies form the basis for my empirical strategy.

3 Data

This study makes use of a unique combination of federal administrative data sources. While the richness of the data allows an exceptionally detailed examination of the effect of grant aid and application simplification across multiple subgroups of individuals and post-secondary institutions, there are important limitations that guide the empirical strategy explained below. The data derive from a set of individual level data files that were assembled in 2014 to 2015 to construct the U.S. Department of Education's (ED) College Scorecard website, a repository of institution level information about typical completion, borrowing, and labor market outcomes for federally aided

students for nearly every college and university in the United States. The data represent all students receiving either federal grants or loans to attend college at the undergraduate level, based on administrative data from the National Student Loan Data System (NSLDS) used by the Federal Student Aid Agency of the U.S. Department of Education to manage federal aid. These data were merged to individual data on labor market earnings by staff in Treasury Department, and grouped into cohorts of students based on the year they first enrolled in an institution.[5]

I draw from a subset of this data that constitutes the universe of all federally-aided students starting their studies at a post-secondary institution in the United States between 2002 and 2014 (some were enrolled at different institutions in prior years). This original universe contains information on more than 40 million students. This includes roughly 2.8 to 5.8 million students each year, ranging from a low of 2.82 million in 2002, up to a high of about 5.83 million in 2010 and 2011, and 4.67 million in 2014. Note that since the underlying data are used to administer financial aid programs, only students who enroll and are (ever) aided are included in the data–I do not observe data for students who might consider, or apply for, either financial aid or college but ultimately do not enroll and receive federal aid.

From this universe of students, I make several restrictions to a) isolate students whose enrollment decisions may have been affected by the aid simplification process, and b) eliminate students for whom the minimal data elements required to use their information in the research design outlined below is not available. In particular, I drop all students who are not in their first year at the institution when they first receive federal aid (generally about 15 percent of all students across years), and then a small number of students (an additional 1 to 2 percent) who do not have data from the relevant year's FAFSA on file. Since the analyses in the paper rely on knowing the state a student lives in, financial aid type (i.e., dependent, independent with kids, or independent without kids), and their family's reported income on the FAFSA form I also drop students who have missing information on any of these key variables (less than an additional 0.5 percent).

The data I use contain a wealth of demographic information, but do not have all the administrative data stored either in ED's Central Processing System (CPS) or NSLDS data. Information exist on students' age, gender, marital status, dependent status, whether they have dependent children, their parents' education level (in four categories), family income, CPS-determined expected

[5]Technically entry dates are based on when students are first observed receiving federal aid at an institution. If the student indicates they are a sophomore, junior, etc. then the entry date is imputed (e.g., two years earlier for a student first observed as a junior). See Council of Economic Advisers (2015) for a detailed explanation of the data construction procedures, and the College Scorecard data.

family contribution (EFC), and where they attended college. These variables allow me to compute financial aid type (e.g., dependent, independent without dependents, and independent with dependents), and Pell award eligibility. Unfortunately, I do not have item level responses to detailed financial information questions from the FAFSA, information about how the FAFSA was filed (e.g., via the Web, or whether the new I.R.S. "Digital Retrieval Tool" was used), the time to complete the FAFSA, or information on tax filing or federal benefit receipt. All of these would help to more directly establish eligibility and exposure to the simplification provisions discussed below.

To fill in information on how the simplification provisions analyzed here affect the burden on FAFSA applicants, I supplement the data above with data from the 2012 National Postsecondary Student Aid Survey (NPSAS). These data are a combination of survey and administrative data on students' FAFSA and other information for a representative sample of students attending institutions that participate in federal aid programs. I rely on FAFSA data recorded in the NPSAS, which is drawn from the same administrative data sets used to produce the College Scorecard, so in this sense the NPSAS data should contain more detailed information for representative subsets of the individual students in my main data.

4 Research Design

How would enrollment be affected if the FAFSA were simplified? As several analysts have documented, the vast majority of variation in Pell grant awards can be explained with only a handful of data elements in the FAFSA, namely family income and family size (Dynarski and Scott-Clayton 2006, Dynarski and Wiederspan 2012). Based on this insight, there have been many calls to eliminate questions from the form (Council of Economic Advisers and National Economic Council 2009, Baum and Scott-Clayton 2013, Bill and Melinda Gates Foundation 2015, National Association of Student Financial Aid Administrators 2015, National College Access Network 2017), with Dynarski and Scott-Clayton (2007) proposing that grant aid be determined only by taxable income and family size so all aid information could fit on a postcard.

4.1 Simplified Needs Test and Automatic-zero EFC Policies

In fact, the FAFSA form already incorporates several simplification policies that reduce the number of financial questions that are used to determine the expected family contribution of low income aid applicants. First, for students from families with taxable income below $50,000 and meeting several

additional requirements, applicants are eligible for a "simplified needs test (SNT)" in which family net worth, estimated by the sum of the value of three types of assets held by either parents or children (for dependent students), is ignored in determining federal aid eligibility. While representing only three questions each for parents and students, these questions are considered amongst the most burdensome to report on the forms, in part because this information is unlikely to be readily available from an applicants' tax forms. Beyond the the taxable income ceiling, applicants qualify if they meet any of three additional criteria: a) anyone in the household received benefits from one of 5 federal means-tested programs (SNAP (formerly Food Stamps), Free and Reduced Price School Lunch (FRPL), Temporary Assistance for Needy Families, Supplemental Security Income, or Special Supplemental Nutrition Program for Women, Infants and Children); b) the students' parents (or the student and spouse) were eligible to file the 1040A or 1040EZ tax form (or not required to file taxes); or after 2010, c) the student's parent (or student or spouse) is a dislocated worker.

For even lower income students, the EFC formula is simplified even further. Students whose families have taxable income (parents' income for dependents, or the sum of a student and his or her spouse's income for independent students) equal or less than a threshold that varies across years–for the 2016-2017 application cycle the threshold is $25,000–and who meet one of the same three additional criteria for the SNT are automatically eligible for a zero EFC. This automatic zero EFC (AZ) policy was incorporated in the aid determination process in 1986, before the FAFSA form was first used for all types of federal aid applicants. For students who qualify for AZ, no further financial information is used for aid determination, so in addition to the asset questions skipped by the SNT, an additional 31 questions about "additional financial information" and "untaxed income" for both parents and children, as well as child earnings are not required for federal aid determination.[6] Effectively, for students qualifying for AZ the aid determination process uses approximately the same information as Dynarski and Scott-Clayton (2007)'s "grant on a postcard" proposal.[7]

Importantly, when first adopted, these policies simplified the formulae used to determine applicants' EFC, but did nothing to alter the application process for students and their families.

[6]For reference, the complete FAFSA form with all questions for 2017-18 is available here: https://fafsa.ed.gov/fotw1718/pdf/PdfFafsa17-18.pdf. The questions skipped by AZ eligible students are in subsections titled "additional financial information" and "untaxed income" for both students and parents. As explained in the text, nearly all students file their form online and do not necessarily face each of the other questions on the full form.

[7]This is not to imply the application process is nearly as simple as Dynarski and Scott-Clayton (2007) propose. There are still many non-financial questions on the FAFSA used to determine eligibility for aid and financial dependent status, and the transparency aspects of the postcard proposal are non-existent. The hassle costs of reporting financial information is, however, approximately equal.

In effect, these policies only "made things easier for the computer that processes aid applications (Dynarski and Scott-Clayton 2006)." With the advent of electronic FAFSA filing through "FAFSA on the Web" in 1997-1998, however, this started to change. ED incorporated dynamic data input screens that used "skip logic" to encourage applicants to skip blocks of questions that were irrelevant to their EFC determination. For example, if a dependent student reported parents' taxable income of less than $50,000 and that her parent filed a 1040EZ tax form, she would see a screen asking her permission to skip asset questions for both her parents and herself. In the 2017-2018 application cycle, if the same student reported parents' income of less than $25,000, she would be skipped through the entire remainder of the process to a signature page. Though it is difficult to document the timeline of the implementation of this skip logic in FOTW, and exactly how the website functioned in different application years, various memos and presentations to financial aid professionals by Federal Student Aid staff suggest that the financial information made irrelevant by either the SNT or AZ was skipped in FOTW as early as 2001-2002.[8]

Two factors limit the number of students who would otherwise qualify for the simpler application created by the SNT and AZ policies based on the eligibility criteria described above. First, students must use FOTW in order to benefit from its skip logic–the paper FAFSA form has never included information about the SNT or AZ policies, so applicants are unlikely to be aware that the relevant questions are not required of them. In the first year of FOTW (1997-1998), about 24 percent of applicants filed electronically, including under an older electronic submission system (see Appendix Table 1). By 2002 (hereafter, I refer to academic or award years by the year of the end date), 50 percent of applicants used FOTW to file, rising to 66 percent in 2004, 88 percent in 2006, 96 percent in 2008, and above 99 percent since 2010.[9]

A more important factor limiting students' access to simplification for the purposes of this paper is the fact that some states do not allow ED to implement skip logic on FOTW for aid applicants from their state. Many states grant college aid to their residents, and 15 states require FSA to ask applicants to fill out all of the FAFSA data elements since their own aid determination processes rely on these items regardless of family income. The states that do not allow FSA to implement simplification–Colorado, District of Columbia, Georgia, Hawaii, Illinois, Montana, New Jersey, New Mexico, Ohio, Oklahoma, South Carolina, Vermont, Washington, Wisconsin, and Wyoming–

[8] See, for example, https://ifap.ed.gov/presentations/attachments/02NASFAAFOTWPINFAA.pdf.

[9] These statistics are gathered from several sources, including https://ifap.ed.gov/presentations/attachments/20022003AppProcessingUpdat and FAFSA volume reports https://studentaid.ed.gov/sa/about/data-center/student/application-volume/fafsa-school-state.

are referred to as 'no-skip' states below. About one-fourth of all aid applicants come from these no-skip states. The states are diverse, and on average the characteristics of students in these states are quite similar to the characteristics of students in states that allow skip-logic.

4.2 Empirical Strategy

The crux of the research design employed in this study is to examine whether students who are just eligible for an automatic zero EFC or the SNT calculation by virtue of having family income below the relevant threshold, and thus face a simpler federal aid application, are more likely to enroll in college. This basic and transparent research design is complicated by two aspects of the available data and the federal aid determination process. First, the data contain information only on students who enroll and receive federal aid, so we do not observe data on the relevant universe of students 'at risk'–in the sense that their probability of attendance could conceivably be affected by the complexity of the application process–for attending college. Because of this limitation, I cannot distinguish between increases in enrollment, and increases in take-up of federal aid conditional on enrollment. Since the effects of simplification and higher Pell awards on enrollment are likely to be in the same direction, the estimated impacts should capture the sum of these effects, and thus an upper bound on enrollment effects. Below I continue to refer to an enrollment effect for simplicity, but strictly this includes take-up effects as well. Second, in some years there is a sizable difference in students' EFCs, and thus a large increase in the amount of Pell grant they can receive, among students whose family incomes lie just above and below the auto-zero income cutoff. This occurs since the auto-zero EFC policy essentially ignores the underlying EFC formula, and can thus result in a substantially different amount of aid eligibility. The SNT, which disregards assets but otherwise adheres to the formula, has only a small impact on Pell eligibility since few students have assets sizable enough to influence their award amount. As a result, differences in the likelihood of enrollment at the AZ income threshold may reflect both the effect of aid simplification, and the effect of increased financial aid leading to a lower cost of attendance (i.e., net price).

Figure 1 illustrates the relationship between Pell award eligibility and family income in two select years and two types of students for illustration. In the federal needs analysis methodology, students are classified into one of three groups–or financial aid types–and the applicable formula, and thus the relationship between Pell eligibility and family income, is different for each group: dependent students, independent students with no dependents other than a spouse, and independent students

with dependents. Since independent students with no dependents are not eligible for AZ, I ignore them for the remainder of the paper. In the Figure, data for each student aid-type is presented in a separate row, with data for 2004 in the left column, and 2010 in the right. In each panel of Figure 1, average Pell award eligibility is presented by family income groups, where family incomes are grouped into $100 'bins', and the dashed vertical line indicates the value of the AZ cutoff in the relevant year.

Figure 1 shows that the AZ policy results in a discontinuity in the average Pell award for students on either side of the income threshold, but that this discontinuity differs both across types of students and years. For example, in 2010, dependent students whose family income makes them just eligible for AZ have Pell eligibility that is $810, or about 20 percent, higher than students whose income is just over the threshold of $30,000 in that year. In 2004, this discontinuity was significantly smaller at $224, or about 7 to 8 percent. Similarly, for independent students with dependents (third row of Figure 1), the discontinuity in Pell award amount eligibility is less than half of the difference for dependent students at $393, and in 2004 the discontinuity is only $50. As noted above, independent students with no dependents are not eligible for AZ, and in most years the estimated discontinuity in Pell eligibility is small in magnitude and statistically insignificant.

To isolate the effects of simplification versus the difference in Pell grant eligibility amounts just shown, I leverage two important sources of variation in Pell grant awards and whether students face simplification. First, as shown below, changes in the auto-zero EFC threshold and the maximum Pell award lead to differences in the discontinuity in average Pell awards in different years and for different types of students. Second, students in no-skip states do not fill out a simpler FAFSA if they qualify for AZ whereas some students in skip states do, but the Pell award eligibility formula is the same across states. Below, I describe how both sources of variation permit me to estimate the extent to which observed enrollment increases at the AZ threshold are driven by Pell awards versus simplification, again accounting for the fact that only data on enrolled students is observed.

To fix ideas, consider the following statistical model that links the probability that a student i, who is financial aid type a (dependent or independent with dependents), and lives in state group s in year y will enroll in college to whether the student is eligible for a simplified aid application, the amount of Pell grant eligibility she qualifies for, and other factors:

$$\ln P(E_{iasy}) = \beta_{1ay} S_{iasy} + \beta_{2ay} P_{iasy} + \epsilon_{iasy}. \tag{1}$$

Here, E_{iasy} is an indicator for college enrollment and S_{iasy} is the key treatment variable of interest indicating whether the student is eligible for an automatic zero EFC calculation and simplified aid application. P_{iasy} is the amount of Pell Grant aid to which the student is eligible, and ϵ_{iasy} is a vector of other observable and unobservable determinants of enrollment (e.g., family income, age, gender, other policies, etc.). We explicitly allow the effect of aid simplification to differ across years and by student financial aid type. The indicator S_{iasy} is defined equal to one if the student has family income below an AZ eligibility threshold that varies from year to year, and lives in a skip-state that does not require extra FAFSA information for students with a zero EFC.[10] Since the size of the Pell grant students are eligible for depends on their calculated EFC, there is also a discontinuity in the average Pell grant students are eligible for for students with family incomes around the auto-zero EFC income thresholds. The magnitude of this discontinuity varies across years, as the auto-zero threshold and the maximum Pell grant change over time.

Ideally, to assess the impact of the aid-simplification created by the auto-zero EFC policy one could use data on family income and eventual college enrollment for a group of students who might be considering college. In the absence of such data, I use information on the universe of students who enroll in college each year and receive federal aid. With this data, instead of assessing directly whether the conditional probability of enrollment as a function of family income is discontinuous at the auto-zero income threshold, I test whether the density of family income among enrolled students is discontinuous at the threshold. Intuitively, if simplification increases enrollment, there should be more students enrolled with family income just below the auto-zero threshold than with family income just above.[11] To see this, let the conditional density of family income given enrollment status to be $f(M|E)$, and denote the auto-zero threshold level of income as $m^{\star}$. Using Bayes' Rule, note that

$$f(M = m|E = 1) = \frac{P(E = 1|M = m)f(M = m)}{P(E = 1)}$$

so that

$$\ln f(M = m|E = 1) \quad = \quad \ln P(E = 1|M - m) + \ln f(M = m) - \ln P(E = 1).$$

[10] As described above, students or their parents must also meet one of three additional critera to qualify for AZ–we abstract from these criteria here, but consider whether the policy results in a simpler application process below.

[11] Similar approaches are employed by Turner (2014), Marx and Turner (2015) and Carruthers and Welch (2015).

Taking the difference in the limits of this expression as family income m approaches $m^\star$ from below and above then yields

$$\begin{aligned}\lim_{m\uparrow m^\star} f(M=m|E=1) - \lim_{m\downarrow m^\star} f(M=m|E=1) \quad = \quad & \left[\lim_{m\uparrow m^\star} \ln P(E|M=m) - \lim_{m\downarrow m^\star} \ln P(E|M=m)\right] \\ & - \left[\lim_{m\uparrow m^\star} \ln f(M=m) - \lim_{m\uparrow m^\star} \ln f(M=m)\right] \\ & - \left[\lim_{m\uparrow m^\star} \ln P(E=1) - \lim_{m\uparrow m^\star} \ln P(E=1)\right].\end{aligned}$$

Under the assumption that the density of family income among families who are 'at-risk' of applying for federal aid is continuous at m^*, the second bracketed terms of the expression above is zero (the third term is automatically zero) and the derivation shows that the discontinuity in the log density of income among enrolled students is equal to the discontinuity in the log of the probability of enrolling. Denoting the discontinuity in $f(M|E)$ for students in aid group a, state group s, and year y as θ^f_{asy}, we can then use equation (1) to relate the discontinuity in this density to the discontinuity in whether students are eligible for a simpler FAFSA in that state and year, the discontinuity in the average Pell grant award, and any other factors affecting enrollment and changing discontinuously at the income threshold as follows

$$\theta^f_{asy} = \beta_{1ay}\theta^S_{asy} + \beta_{2ay}\theta^P_{asy} + \theta^\epsilon_{asy}. \tag{2}$$

Here, θ^S_{asy} indicates the discontinuity in whether students face a simpler FAFSA on either side of the threshold in state group s and year y. For now I define this equal to one in skip states, and zero in non-skip states, but offer some evidence about the actual size of this discontinuity and how it might have changed over time. θ^P_{asy} is the discontinuity in the average size of the Pell grant, and θ^ϵ_{asy} represents any potential discontinuity in other unobserved influences of enrollment that affect all students.

Note that for any given student type and year, there are only two estimates of θ^S_{asy} and θ^P_{asy}, and so the model is under-identified. We can make progress in one of two ways. First, under the assumption that θ^ϵ_{asy} is equal to zero in both skip states and no-skip states, the model is just identified. Let $s = 0$ correspond to these states that do not allow simplification by skipping questions on the FAFSA, and $s = 1$ correspond to states where simplification is implemented. This

suggests intuitive estimators for our key parameters as follows:

$$\widehat{\beta_{2ay}}^{*} = \frac{\widehat{\theta}^{f}_{ay,s=0}}{\widehat{\theta}^{P}_{ay,s=0}}, \quad \text{and} \quad \widehat{\beta_{1ay}}^{*} = \left[\widehat{\theta}^{f}_{ay,s=1} + \widehat{\beta_{2ay}}(\widehat{\theta}^{P}_{ay,s=0} - \widehat{\theta}^{P}_{ay,s=1})\right] - \widehat{\theta}^{f}_{ay,s=0}. \tag{3}$$

While this approach seems reasonable, I show below that the assumption that θ^{ϵ}_{asy} is zero appears to be rejected by the data and thus estimators based on that assumption are likely to be biased.

Returning to equation 2, notice that if the parameters of interest (i.e., the effects of simplification and Pell eligibility) are constant across years (or spans of several years), and I assume that $\theta^{\epsilon}_{asy} = \theta^{\epsilon}_{ay}$, then multiple years of data can be used to estimate β_{1ay} and β_{2ay} using the estimated discontinuities in simplification and Pell eligibility. The assumption on θ^{ϵ} is that whatever other factors are changing discontinuously at the AZ threshold are common to both skip states and no-skip states. I implement this approach by using the estimated discontinuities as data in the regression model

$$\widehat{\theta}^{f}_{asy} = \beta_{1ay}\widehat{\theta}^{S}_{asy} + \beta_{2ay}\widehat{\theta}^{P}_{asy} + \theta^{\epsilon}_{ay} + \nu_{asy} \tag{4}$$

where the variables are as defined above, and ν_{asy} is an error term reflecting sampling errors of the estimated coefficients. Note in the model that θ^{ϵ}_{ay} is a parameter (on the constant term in the regression) to be estimated, and can be interpreted as the effect of all other influences on college enrollment that vary discontinuously across the AZ family income threshold. Since the discontinuity estimates are independent from one another, estimating the model using weighted least squares with weights equal to the inverse variance of θ^{f}_{asy} is equivalent to the minimum distance (or optimal GMM) estimator, and efficient. I estimate parameters separately by student type, using data for all years, and also splitting the sample of discontinuity estimates into four time periods from 2003 to 2006, 2007 to 2010, and 2011 to 2014.

4.3 Discontinuity Estimation Details

I use local linear regression methods to estimate the parameters θ^{P}_{asy}, using both conventional and robust inference methods discussed by Calonico, Cattaneo and Titiunik (2014). To estimate the discontinuities in the densities, i.e. θ^{f}_{asy}, I use the procedure proposed by McCrary (2008). In both cases I use data driven algorithms to choose the bandwidth–the 'CCT' bandwidth selector for the Pell discontinuities (Calonico et al. 2014) and the Silverman rule of thumb estimator for the density (Silverman 1986)–and present sensitivity analyses illustrating robustness to other bandwidth

choices. One complication in estimating the density of income is that the density function exhibits a high degree of heaping at multiples of $1,000. As explained further below, in order to avoid distortions to the discontinuity estimates I drop observations at these values when estimating the discontinuities. Assuming the propensity to 'heap' does not change discontinuously at the threshold, this procedure should avoid the heaping induced biases discussed by Barreca, Guldi, Lindo and Waddell (2011).

5 Results

5.1 Impact of the AZ and SNT on FAFSA Burden

To what extent to the SNT and Autozero EFC policies lead to a simpler application experience for federal aid applicants? This is a difficult question to answer with the administrative data I use, since they do not contain detailed FAFSA information on the three criteria used along with taxable income to determine eligibility for simplification, nor do they contain the specific data elements for the financial questions that can be skipped on FOTW. There are reasons to be skeptical that all students eligible for simplification actually experience a simpler FAFSA due to the SNT and AZ. For example, Dynarski and Scott-Clayton (2006) note that in the 2003-2004 National Postsecondary Student Aid Survey (NPSAS), many students whose applications were processed using the automatic-zero EFC or SNT formula report non-zero values for assets or other irrelevant questions nonetheless. As noted already, some students are required to answer questions that are irrelevant for federal aid because they live in states that do not allow FSA to skip these questions, presumably so they can be used for state aid determination. In other states this may still occur because although students are prompted to skip questions if they qualify for AZ or SNT, they may nonetheless choose to fill out those questions, and non-FOTW filers (about one-third of all filers in 2004) are unlikely to be aware of the option to skip the questions at all. To better understand whether students experience a simpler FAFSA due to SNT and AZ policies, I use the most recent version of the NPSAS data from 2011-2012 and analyze patterns of responses to specific FAFSA items around the income-eligibility criteria for each simplification policy.

Before presenting those results, it bears noting that most of the enrolled Title IV aid recipients in my sample meet the taxable income criteria for the SNT. Across all years from 2002 to 2014, between 75 and 80 percent of the enrolled, first-year aid recipients have taxable income (as reported

on their FAFSA) below $50,000. For independent students, the numbers are much higher with about 90 percent of independents with dependents, and 95 percent of independents without dependents meeting the SNT income threshold. For dependent students, about 60 to 65 percent of students had family income below $50,000.

Figure 2 presents the fraction of dependents and independents students with dependents other than their spouse (the two groups eligible for the AZ) who meet the eligibility requirements for the SNT and AZ by $1,000 bins of taxable income. For both groups of students, there are clear discontinuities in the fraction eligible for the AZ and SNT at the relevant income thresholds, shown by the pattern of hollow circles and triangles. At the AZ threshold of $31,000, the share of dependents eligible for AZ jumps from zero amongst students with taxable income just above to .71 just below. For independents the jump in the share eligible is slightly larger at .82. Recall these fractions reflect the share of students in each group that either have dislocated workers in the household, file a 1040A or 1040EZ tax form, or received one of the five federal means tested benefits. The solid markers in the figure show the share of students eligible for the SNT. Since the eligibility criteria for the AZ and SNT are the same aside from the taxable income threshold, the fact that there is no 'jump' in this fraction at the AZ threshold is one piece of evidence that students on either side of the threshold are similar. Below the SNT threshold of $49,999, the share of students eligible jumps to .5 for dependent students and .6 for independents. Though not shown in the Figure, these patterns are similar in skip and no-skip states. These figures give a sense for what fraction of students are potentially affected by the natural experiment analyzed in this paper, given these non-income related eligibility criteria.

Figure 2 shows there is a clear discontinuity in eligibility for simplification created by the SNT and AZ policies, but does not establish whether this intent translates into a simpler FAFSA for lower income students, or whether this differs across skip and no-skip states. Unfortunately it is not clear from NPSAS data whether students were ever prompted to answer particular data items. In particular, it is unclear whether skipped questions are recorded as missing or zero, and similarly unclear whether questions students 'see' but leave blank are recorded as zero or missing. With this caveat in mind, the data do reveal patterns of non-response that suggest that FOTW simplifies the FAFSA for students qualifying for AZ in skip states, but not no-skip states. Figure 3 shows the fraction of students with missing values recorded for all the relevant questions for two groups of data elements that are ignored by the AZ: additional financial information and untaxed income.[12]

[12]On the 2011-2012 FAFSA, additional financial information is collected in question 43. a-f (education credits, child

The Figure reveals that in skip states, the fraction of students not-reporting any of the items in each block of information jumps by about 20 percentage points for students whose income just qualifies them for AZ. This discontinuity is slightly larger for independent students than for dependents. The discontinuity is also slightly larger for the additional financial information questions than the untaxed income questions. To the extent that for some questions both skipped and not-skipped questions may both be recorded as either missing or zero, the estimates implied in these figures should be taken as lower bounds for the degree of simplification implemented by AZ.

Students with family income below $50,000 and meeting the other SNT criteria are not required to answer questions about their own or their parents' assets, and FOTW skips these questions for applicants from skip-states. Unfortunately, the NPSAS data that would allow an estimate the impact of this rule appears corrupt. As shown in Appendix Figure 1, for dependent students, the fraction of students with missing values reported for students' assets follows the expected pattern, jumping from about zero to .25 for students in skip states with family incomes just below the cutoff. For students in non-skip states, no such pattern is observed and the fraction missing is close to zero across all income levels. For questions on parental assets, however, the data for both dependent (see Appendix Figure 1) and independent (not shown) students behave strangely. In both groups of states there is no pattern in missing data corresponding to family income, but the fraction of students with missing parents' asset information is between 40 and 50 percent at all income levels for individuals in skip-states, whereas it is nearly zero for those in non-skip states. While I suspect the most reasonable inference from this data is that the degree of simplification due to the SNT follows the pattern for student assets, below I focus on the AZ where the NPSAS data provides clearer evidence that students experience the intended simplification.[13]

These analyses should be viewed as confirmation that the number of questions answered on the FAFSA does appear to be reduced by the SNT and AZ in skip states, and that no simplification is occuring in no-skip states. While further work is necessary to document the extent of simplification achieved by the SNT and AZ, and how this has changed over time, several factors bear keeping in mind. First, the fraction of students filing their FAFSA on the web increased from about 50 percent in 2002 to over 99 percent by 2010, and the FOTW experience and skip-logic functioning has evolved over that time as well. These factors would tend to increase the difference in simplification between

support paid, taxed combat pay, etc.) and untaxed income is collected in question 44.a-j (payments to tax-deferred pensions, child support received, etc.)

[13]In conversations with Department of Education staff involved in the production of this data, they agree with this sense of the patterns in the data, and hope to further explore this apparent error in data recording or processing.

skip and no-skip states over time. On the other hand, starting in 2010 FOTW users could use the "IRS Data Retrieval Tool (DRT)," which automatically retrieved FAFSA information available on parent and students' tax forms for those that had filed their taxes. While use of the DRT is arguably slightly more difficult than applying if one qualifies for AZ (assuming family income is known), this might tend to reduce the difference in application burden across states, since it is available to students in both skip and no-skip states. Adoption of the DRT was partial and increased gradually, however, since many applicants filed their FAFSA before filing their taxes and were therefore ineligible to use it.[14] In 2012 about 14 percent of dependent students and 24 percent of independent students (including those with no dependents) used the DRT, rising to 33 and 41 percent, respectively in 2014.[15]

I do not attempt to explicitly incorporate estimates of the fraction of students experiencing simplification in the estimates below, but the discussion above suggests several points about how to relate the reported 'intent to treat' estimates to the effects of actual simplification. At the AZ threshold, a conservative estimate from the 2011-12 NPSAS suggests about one-fifth to one-quarter of students just below the threshold experienced the simplified form. Given that FOTW participation exceeded 90 percent after 2006, this is perhaps a reasonable estimate to apply for the intervening time period. Lower usage of FOTW, and increased usage of the DRT might have led to less of a difference in experienced simplification in both earlier and later years.

5.2 AZ and Pell Eligibility

As depicted in Figure 1, the automatic zero EFC policy can generate large differences in Pell eligibility among students with family incomes close to the threshold. In the early 2000s, this difference was relatively small, since the AZ threshold was set at an income level where the strong majority of students were receiving the maximum Pell award–that is, at a level below the amount of disregarded income in the EFC formula, so that the impact of the policy on aid eligibility was small. Over time, however, Congress raised the level of the auto-zero threshold into the income range where the needs analysis formula dictates less than the maximum Pell award (that is, a positive EFC), and so students with income just below the threshold were eligible for a larger Pell

[14]The Obama Administration issued new regulations allowing 'prior-prior year' income to be used on the FAFSA starting in 2016-2017, with the goal of increasing use of the DRT.

[15]These statistics are from FAFSA Volume reports for various years, available at https://studentaid.ed.gov/sa/about/data-center/student/application-volume/fafsa-school-state, and downloaded on December 5, 2016.

grant than those just above.

Table 2 shows the estimated discontinuities in Pell eligibility across all years between 2002 and 2014, for dependent students and independent students with dependents. In the table, the columns marked "average Pell" show the average Pell award eligibility for students with incomes up to $1,000 above the AZ threshold, and the columns marked "Discontinuity" show the increase in this amount for students with family incomes just below. The discontinuity estimates differ very slightly across skip and no-skip states, but these estimates are robust to a variety of different estimation methods as should be readily apparent from the raw average plots in Figure 1. The table shows that the discontinuity in Pell amounts created by the AZ policy was relatively small–$162 (4.8 percent) for dependents and $34 (less than 1 percent) for independents–in 2002, and grew only slightly through 2006. It then jumped in 2007, and even more in 2010 when it reached a peak of $810 (19.8 percent) for dependents and $393 (8.1 percent) for independents, before falling in 2013 and 2014. For independents without kids who are not eligible for AZ (not shown in the Table), the estimated discontinuities are small and generally not statistically significantly different from zero.

What explains the variation shown in the Table? As Figure 4 shows, these changes are primarily driven by increases in the auto-zero income eligibility threshold. This threshold increased (in nominal dollars) from $13,000 in 2002, to $20,000 in 2007, to $30,000 in 2010, before being reduced by the Consolidated Appropriations Act of 2012 to $23,000 in 2013.[16] Since the underlying needs analysis formula changes much more gradually—its underlying parameters are indexed to inflation—increases in the AZ threshold tend to create a larger discontinuity in Pell awards (shown by the line of connected circles in the graph). Other factors like the amount of the maximum Pell award play a smaller role in affecting the size of the Pell grant discontinuity at the AZ threshold as well. Table 1 shows these increases in the AZ threshold expanded the fraction of aid applicants who were eligible for the simplified FAFSA application. The fraction of all students whose income qualified them for AZ changed from about 34 percent in 2002 to 52 percent in 2014. In 2014, more than one-third of dependents and nearly two-thirds of independent students with dependents had family income low-enough to qualify for AZ.

[16]This list of changes is not exhaustive, but rather highlights the years when major changes occurred.

5.3 AZ and Enrollment

How do AZ policies affect college enrollment? As explained above, I investigate this question by assessing whether there are more students enrolled in college with family incomes just below the AZ income thresholds than just above. More formally, I test for discontinuities in the (log) density of FAFSA income at the AZ threshold using the two-step approach of McCrary (2008). To isolate the effect of simplification from the effect of increased Pell award eligibility or other factors that might also be a function of AZ eligibility, I estimate this discontinuity separately in groups of states that do ("skip-states") and do-not ("no-skip states") allow FSA to implement the skip-logic that allows students to skip irrelevant FAFSA questions. Figure 5 illustrates the data underlying these analyses using data for the two financial aid types from 2010. Each panel of the Figure shows the histogram of family income by plotting the number of enrolled aid recipients in each $100 'bin' of family income–e.g. the number of students with family income between $30,001 and $30,100 is plotted in a blue circle centered over $30,050. The two vertical lines in the Figure indicate the lowest family income that qualifies a mother with two children for the maximum EITC in the relevant tax year (2008 in this case)–the relevance of this threshold is explained below–and the AZ threshold.

A complication for estimation is that there is a substantial degree of heaping in the distribution of reported FAFSA incomes. The x's in the Figure correspond to $100 bins containing an income multiple of $1,000. As can be plainly seen, these income-observations are substantially more likely than surrounding observations. To avoid biases in estimating the discontinuity in the density of family income, I omit students who report family income that is an exact multiple of $1,000 in the estimation procedure (Barreca et al. 2011).[17]

For 2010, the top left panel of Figure 5 shows a discontinuity in the log density of family income of about 0.053 (standard error of .008) in "skip-states"–meaning there are just over 5 percent more enrolled dependent students with family incomes just below the AZ threshold of $30,000 than there are students with family income just above that threshold. For independent students with dependents, the discontinuity in the log density is about the same magnitude, or .054 (.013). The panels in the right of Figure 5 show the same data for "no-skip" states, to help distinguish between a simplification story and other possible explanations. For both dependents and independent students, the discontinuities in density of family income are smaller–the estimated discontinuities are .045 and .034, respectively–than in "skip-states," but the difference is very small

[17]This procedure should not affect estimates of the discontinuity so long as there is no discontinuity in the probability of 'heaping' at the AZ threshold, though it reduces precision somewhat due to the reduction in sample.

and not statistically significant.

While statistically we can rule out continuity of the density at the AZ threshold, it is perhaps difficult for the eye to confirm the existence of this small break, and whether our procedure might spuriously estimate discontinuities at other points in the density of family income. To inform both questions, I conduct a series of placebo tests, estimating discontinuities at other points in the family income distribution where the density is expected to be continuous as suggested by Imbens and Lemieux (2008). Figure 6 helps to build intuition for this test, showing the estimated discontinuity and associated standard errors at each of 140 placebo AZ thresholds ranging from $500 to $70,000 in increments of $500 using data for dependents in skip states. The Figure shows that at most income thresholds above about $15,000, no discontinuity is found except at the actual AZ threshold of $30,000. The largest discontinuities are found in low income ranges, and in particular in the neighborhood of $12,590 where the vertical dotted line is drawn. This is the value of lowest income that qualifies a family with 2 or 3 children for the maximum EITC. The discontinuities here echo those documented by Saez (2010), who documents bunching in reported taxable income around the same point.

To formalize a permutation test that the discontinuities above are larger than might be expected under the null that the AZ policy has no impact on enrollment, I focus on the subset of placebo estimates that are unlikely affected by bunching near salient points in the tax schedule, and also by any impact of simplification around the SNT threshold. To do so, I omit all placebo estimates that are less than one one-half of the rule-of-thumb bandwidth greater than the EITC kink (which varies across years), or where half the bandwidth overlaps the SNT threshold ($50,000 in each year). Amongst the remaing set, I calculate the fraction of placebo estimates whose t-statistics are larger than the t-statistic of the estimate at the true AZ threshold.[18] In Figure 6, for dependent students only one of the 97 placebo estimates in skip states, and none of the 90 placebo estimates in no-skip states are greater than the (t-statistic of the) discontinuity estimate found, suggesting that we can reject the null of zero effect in each case (p-values of 1/97 and 0/90, or about .01 and 0.00). For independent students, the corresponding p-values are 1/79 and 5/75 (p=.06).[19]

With only one year of data and without making assumptions on treatment effect homogeneity

[18] An analogous test using the coefficient estimates yields very similar results, but in some cases results in lower p-values driven by variability in the tails of the income distribution where estimates are noisier.

[19] Ganong and Jäger (2017) show in the context of a regression kink design that under certain conditions the distribution of placebo discontinuity estimates corresponds to the exact distribution of possible estimates under the sharp null hypothesis of no effect of AZ on enrollment, and the proportions noted here represent the one-sided p-value of a test of that null hypothesis.

across types of students, our analysis would stop here. Using only 2010 data, following the estimators defined in equation 3 above the implied estimate of β_1 is .0075 (.014), and the estimate of β_2, expressed per \$1000 increase in Pell eligibility, is .055 (.015). As explained above, interpreting these parameters causally requires the assumption that θ^{ϵ}_{asy} is equal to zero–in other words, no other factors that affect enrollment change discontinuously at the AZ threshold. If the parameters of interest are stable over time, however, then data from more years can be used to relax this assumption, and gain efficiency by combining parameter estimates across years.

Table 3 presents discontinuity estimates for the log density of family income for the even years in my sample period from 2002 to 2014 (odd numbered years are omitted for economy in presentation). The second column also shows the Pell discontinuity estimates from Table 2 for reference. The next column shows the rule-of-thumb bandwidth used to estimate the main discontinuity estimates, presented under the column labeled 100% BW, and to check sensitivity estimates at two smaller bandwidths equal to 75 and 50 percent of the rule of thumb bandwidth are also shown. The top panel of the Table shows results for depedent students, while the bottom reports results for independents with dependents.

Several patterns in these estimates stand out. First, the estimated jump in the number of students with incomes just below the AZ thresholds is positive in nearly every case, and the handful of negative point estimates are all statistically insignificant. It seems the AZ policy consistently results in more, though the magnitude is small, students being enrolled in college: across all years and both groups of states there are 4.1 percent more dependent students, and 1.7 percent more independents with dependents, enrolled with family income just below the AZ cutoff than just above. Second, the jump in the number of students just qualifying for AZ is not uniformly larger in skip states compared to no skip states, casting some doubt on theory that simplification is driving the AZ effect. negative estimates are always statistically insignificant–varies across years, with little evidence of a secular increase over time, or pattern that follows the business cycle. Finally, while estimates of the estimated discontinuity in the density of family income vary across years, little pattern is evident, and in particular the magnitudes are not strongly correlated with the magnitude of the Pell discontinuities in the second column of the Table.

In Table 4, I present the results of the permutation test described above for each of the main estimates in Table 3, including the odd year estimates. For dependent students most of the state-year estimates look significant in light of the test, with only 4 of the 26 estimates yielding a p-value

above .1. For independent students the estimation procedure looks substantially less reliable with 16 of the 26 estimates yielding a p-value above .1. The more robust results appear concentrated in the last four to five years in the sample, so we treat estimates from that set of years as more reliable in the discussion of results below.

5.4 Simplification versus Pell Grant Effects

To synthesize the pattern of results shown in Table 3, I use a minimum distance estimator described in equation 4 above to assess the relative importance of simplification, Pell award eligibility, and other factors on college enrollment (and take-up of federal aid). The intuition for this approach and its resuls can be seen in Figure 7, which graphs the "data" used to estimate the effects of Pell and simplification on enrollment: the yearly discontinuities in log enrollment and Pell eligibility, separately for students in states that do and do not allow FSA to skip FAFSA questions for students who qualify for AZ. The $\times$'s in the Figure show the data for states with no simplification for AZ students. If the discontinuity in enrollment were due solely to a linear effect of Pell eligibility, we would expect these points to cluster around a ray extending from the origin, whose slope was equal to β_2. In fact, the line of best fit (the dashed line) has a positive intercept: in other words, our model predicts a an increase in enrollment of about 4 percent due to AZ even with no increase in Pell or a simplified FAFSA. The $\circ$'s in the Figure show the estimates for simplification states. Our estimate of the impact of simplification is based on the average vertical difference in the enrollment discontinuities in each year, after adjusting for the small differences in Pell award increases between the two state groups. This is shown by the gap between the best fit lines for skip and no-skip states, which is very small.

I report estimates of the parameters of the fitted model depicted in Figure 7 (that is, he parameters for equation 4 above) in Table 5, separately for dependent and independent students, and splitting the sample into three separate four-year time periods. The estimates for the effect of simplification (i.e., the intent to treat) on enrollment are uniformly small and not statistically significant in any time period, and for either group of students. For dependent students the point estimates are negative with the exception of the 2003 to 2006 time period. Across all years, the estimated effect is -.0071, and the 95 percent confidence interval is consistent with at most an effect of an increase in enrollment of 1.2 percent. Recalling the evidence on the impacts of AZ on simplifying the FAFSA which suggest a difference in the fraction experiencing simplification of between

one-fourth and one-fifth, this suggests a treatment effect on simplification of at most 6 percent, but again the point estimate is negative and very small. For independent students the point estimate of the intent to treat effect using all years is again very small, but slightly noisier with an upper bound of the 95 percent confidence interval of about 2 percent. Again, since these estimates reflects the combined impact on both enrollment and take-up of federal aid conditional on enrollment they should be viewed as an upper bound on the enrollment effect.

Estimates of the effect of Pell are expressed as the effect of an increase in Pell eligibility of $1,000 (that is, the coefficient is multiplied by 1000) to facilitate interpretation and comparison to other studies. Using data from across all years, the estimated effect for dependents is .028 (.019) and the effect for independents is .056 (.042). Comparing this estimate with the literature requires an estimate of the 'baseline' fraction of individuals 'at-risk' individuals who enroll in college. In this study that fraction is likely to be quite high, as presumably the only set of students on the margin of being influenced by their Pell eligibility here are students who submit applications for federal aid (since most students are unlikely able to precisely predict their aid given their income). If this baseline fraction were as high as 90 percent, the implied percentage point increase in enrollment would be about 2.5 for dependents–slightly on the low side of estimates in the literature–and about twice that for independent students though the estimates are noisy and the confidence intervals overlap zero in both cases.

Interestingly, the data suggest a role for some unobserved factor(s) that change discontinuously at the AZ threshold, especially for dependent students. For dependents, across all years the estimated θ^{ϵ} is .034 (.011). For independent students, the estimate is smaller in magnitude at .010 (.010) and insignificant, though larger (.028) and on the margin of statistical significance in 2011 to 2014, when the discontinuity in the family income density estimates are more reliable. It is not readily apparent what factors might be producing this effect, but I offer a hypothesis below.

6 Differences by subgroups and sectors

Table 6 presents similar results using all years of data from 2003 to 2014 for two sets of student subgroups. The top two rows show results for men and women applicants separately. For dependent students, the results are qualitatively and quantitatively very similar to the full sample results shown above. For independents, the magnitude of the point estimate for men is much higher for the effect of Pell eligibility on enrollment, but the results are not sufficiently precise to statistically

distinguish them. The bottom two rows show results for first generation (defined as not having a college graduate parent) students versus non-first generation students. Here the results are again too noisy to permit precise comparisons.

Table 7 presents results by college sector. The estimated effect of simplification varies very little across sectors, with negative point estimates that are small in magnitude and not statistically different from zero in each sector. The resuls for Pell vary slightly across sectors, but the estimates are too imprecise to make strong inferences about whether Pell might have different effects on enrollment for students likely to enroll in each sector.[20]

As discussed above, this study relies on regression discontinuity methods to estimate a reduced form effect of qualifying for an automatic-zero expected family contribution on the aid application, and difference-in-differences methods to parse this effect into components due to aid simplification and increased grant aid. Identification in the regression discontinuity part of the analysis requires that the conditional expections of the potential outcomes, or the unobserved determinants of enrollment, are continuous at the auto-zero threshold (Hahn, Todd and Klaauw 2001). In practice, this assumption is usually tested by examining whether the conditional expectations of predetermined covariates display discontinuities at the threshold, or by testing for potential manipulation of the assignment variable (family income in the present case) by testing whether its density is continuous at the threshold (Lee 2008, McCrary 2008, Lee and Lemieux 2010). Since the data used for this study are all conditional on the enrollment outcome–that is, only enrolled students are observed–these standard tests are not appropriate. For example, differences in the fraction female at the discontinuity could be consistent with either bias, for example driven by differential application probabilities among women near the auto-zero family income threshold, or with treatment effect heterogeneity, if women's enrollment decisions are more or less sensitive to grant aid or simplified application procedures.

7 Discussion

There are two important and novel findings that emerge from the analyses above. The first is that, for dependent students, the auto-zero simplification policy appears to increase the number of enrolled federal aid recipients significantly. While the magnitude varies across years, there is

[20] In a companion paper, Eng and Matsudaira (2017) show complementary evidence that college choices are not affected by increases in Pell eligibility.

a consistent and positive effect of qualifying for AZ on the likelihood of enrollment of about 4 percent. The second finding is that although the intent of the AZ policy is to simplify the FAFSA for lower-income students, it appears that the simplification aspect of AZ has very little if any impact on enrollment or take-up of aid conditional on enrollment. A third finding consistent with other research findings is that I find a small effect of Pell on enrollment, though sometimes it is insignificant (Marx and Turner 2015, Rubin 2011, Carruthers and Welch 2015). Rather than FAFSA simplification or increased Pell grant eligibility, it appears the effect of AZ is driven by some unobserved factor triggered by having income below the AZ threshold that is common across states.

While this is speculative, one possibility that has recently received attention is Pell verification (The Institute for College Access and Success 2016). After applicants apply for aid, ED uses a risk-model to identify applicants to select to have their financial information verified. If selected, applicants must provide documentation for their information, and anecdotally many students fail to do so and either do not receive aid or do not attend college as a result. Unfortunately little public information exists about the model ED uses to select students for verification, but it is conceivable that since AZ applicants submit less information to ED, they may be less likely to be selected for verification simply because there are fewer variables that might be used to identify anomalous values.

More work is necessary to understand the mechanisms at work behind the AZ effect, but the lack of an effect found for the simplification effected through the policy recommends rethinking some of the emphasis of recent FAFSA simplification proposals. In particular, the results of this study suggest that the active ingredient in the Bettinger et al. (2012) experiment may have had more to do with the personal assistance or nudging people to consider college, rather than easing the burden of filling in the FAFSA application per se. Proposals such as that of Baum and Scott-Clayton (2013) to set aside a percentage of the Pell Program's budget to fund college coaching, counseling and other support services in applications seem wise in this light, and deserve more attention.

The results of this paper should not be taken as evidence that simplifying the FAFSA form is not wise. While the enrollment effects of such a policy seem small, there may be other benefits that justify its small costs in terms of lost targeting efficiency (Dynarski and Scott-Clayton 2006, Dynarski and Wiederspan 2012). These might include paving the way for more clarity about college costs, for example through look-up tables for Pell eligibility, reduced processing costs including a

reduced need for verification of financial information, and so on. This transparency could well enable outreach efforts or other interventions for low-income students that could be important in increasing the likelihood they enroll in college. It does seem, however, that researchers looking for ways of increasing college enrollment for low income Americans need to look beyond (only) tweaks to the set of questions asked, and that hassle costs associated with the FAFSA are unlikely to be an important reason behind the Pell Grant Program's apparent lack of impact on student enrollments and success.

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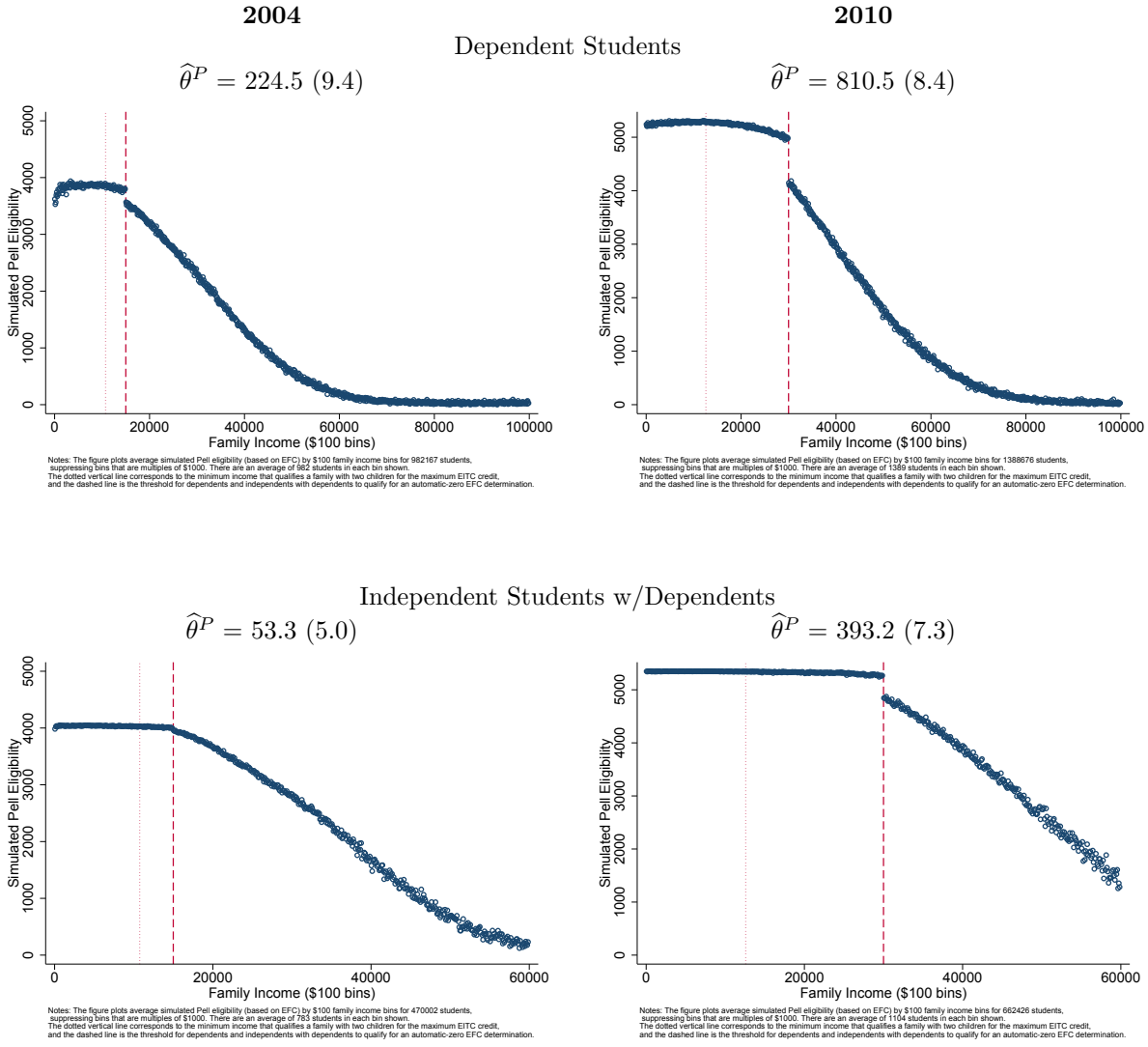
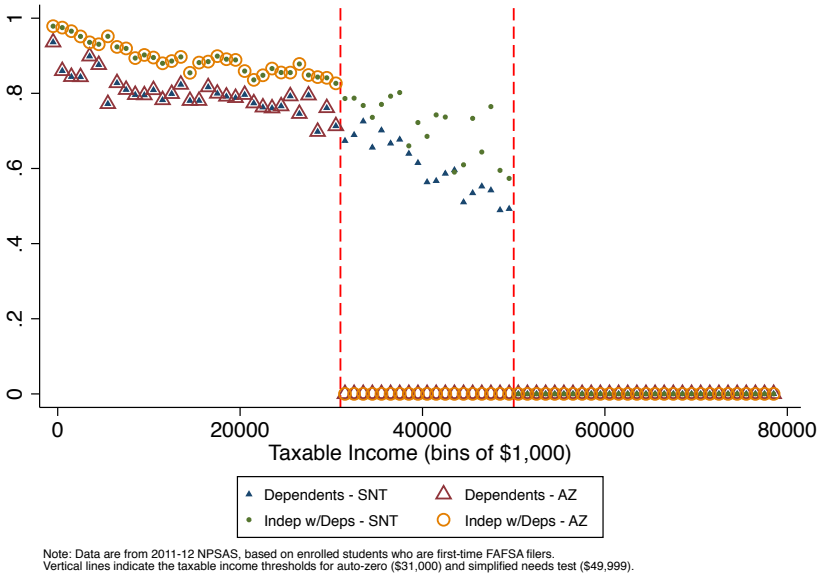
Figure 1:
Simulated Pell Award Eligibility by Family Income and Aid Type in States Allowing Simplification, 2004 and 2010



Notes: All figures use student data in states that allow skip-logic to be implemented in the FAFSA on the Web. Discontinuities in average Pell amounts shown in the figure are estimated using local linear regression methods using equal 'rule-of-thumb' bandwidths (varying from about $2500 to $6700 across different years and student-aid types) on each side of the discontinuity and conventional standard error estimates. See the tables for alternative estimates using the bias-correction and robust inference methods proposed by Calonico et al. (2014).

Figure 2:
Eligibility for SNT and AZ by Taxable Income, 2011-12

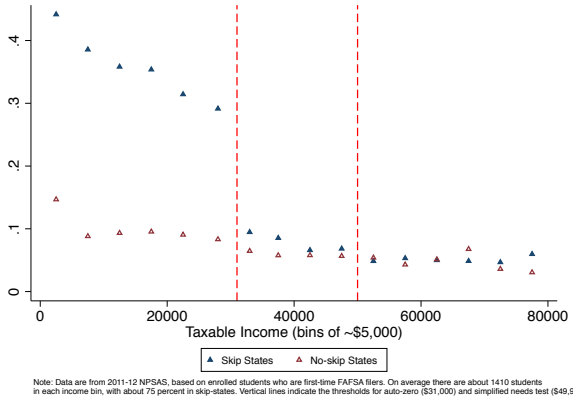


Notes: The figure plots eligibilit for the SNT (solid markers) and AZ (hollow markers) for dependent students (triangles) and independent students with dependents (circles) for students grouped by $1,000 bins of family income. Aside from taxable income, eligibility for both tests is based on whether students meet one of three additional criteria. See the text for details.

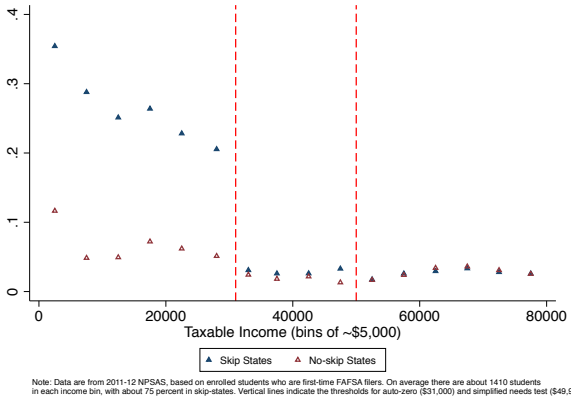
Figure 3:
No Information Reported for FAFSA Questions by Taxable Income and State Group, 2011-12

Dependent Students

Additional Financial Information

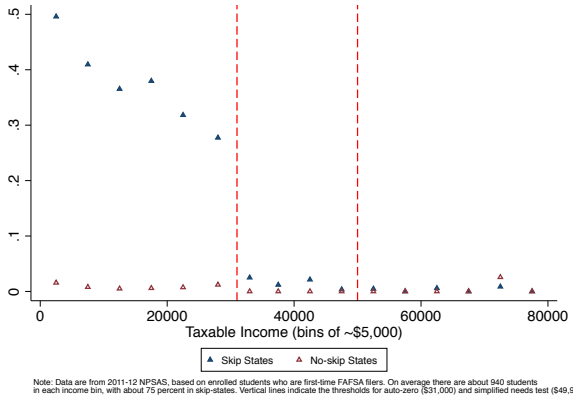


Untaxed Income

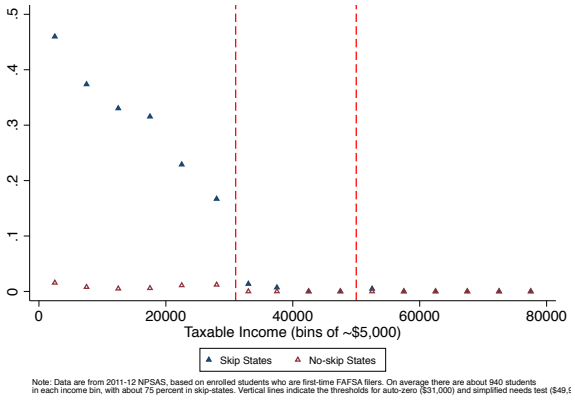


Independent Students w/Dependents

Additional Financial Information

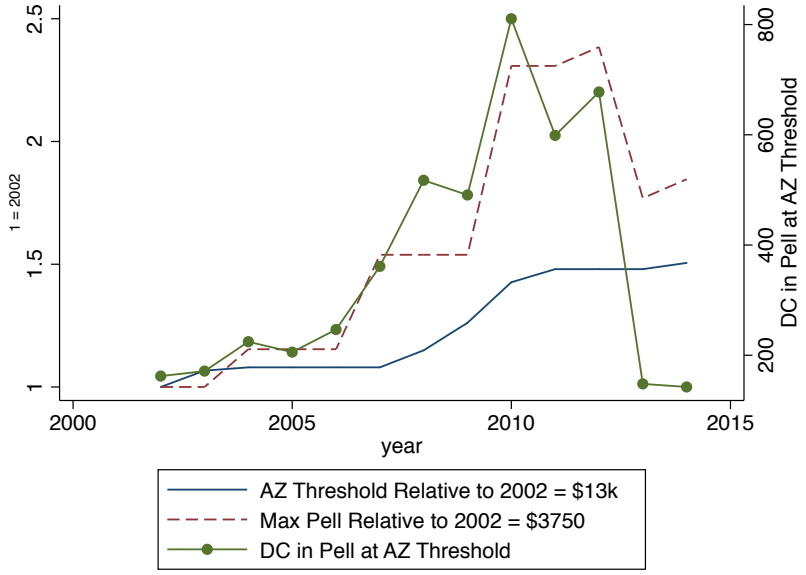


Untaxed Income



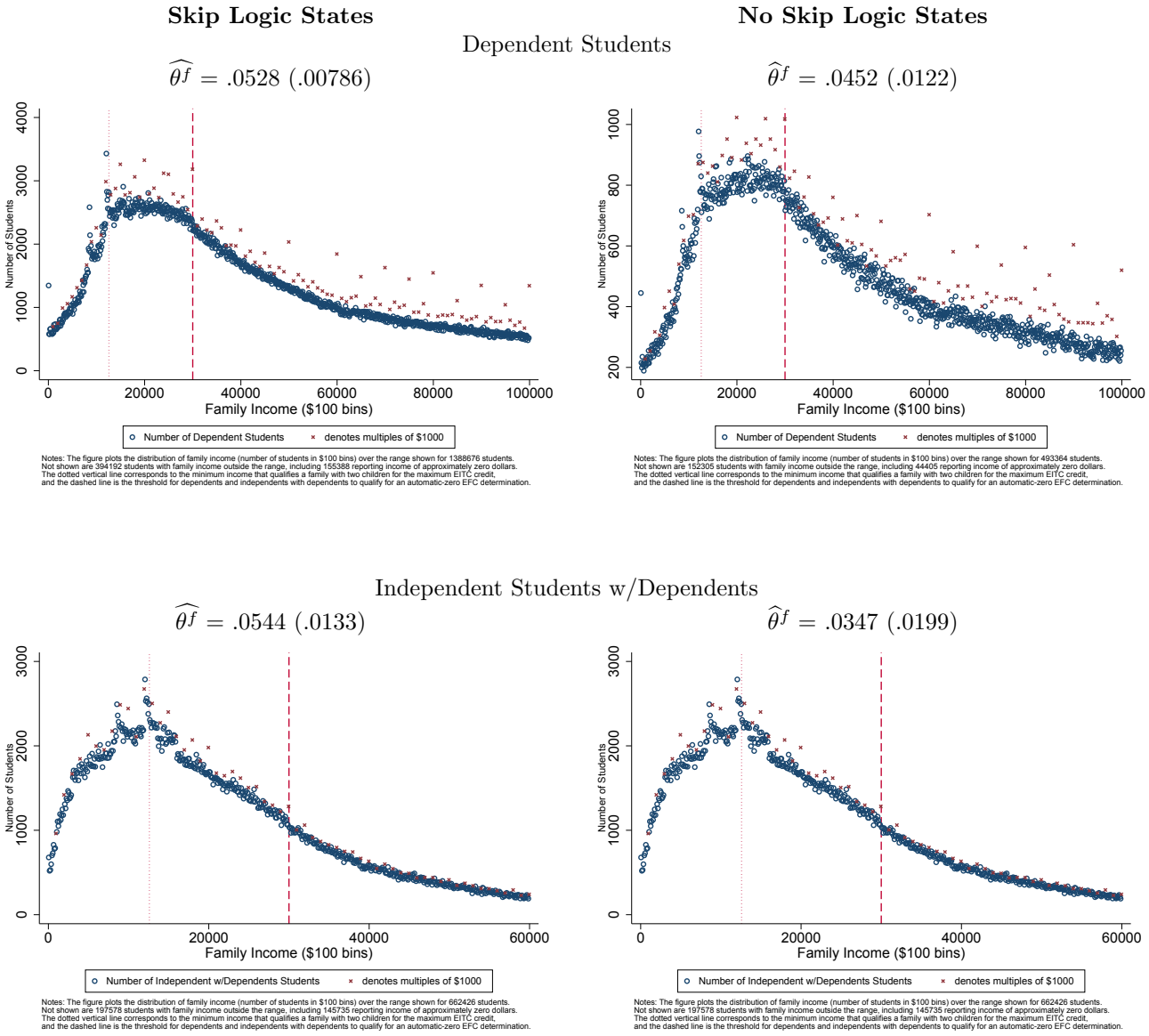
Notes: Data are from the 2011-2012 NPSAS. See the text for discussion.

Figure 4:
Discontinuity in Pell Award Eligibility at the Auto-zero EFC Threshold for Dependent Students, the Auto-zero threshold, and the maximum Pell grant award, 2002 to 2014



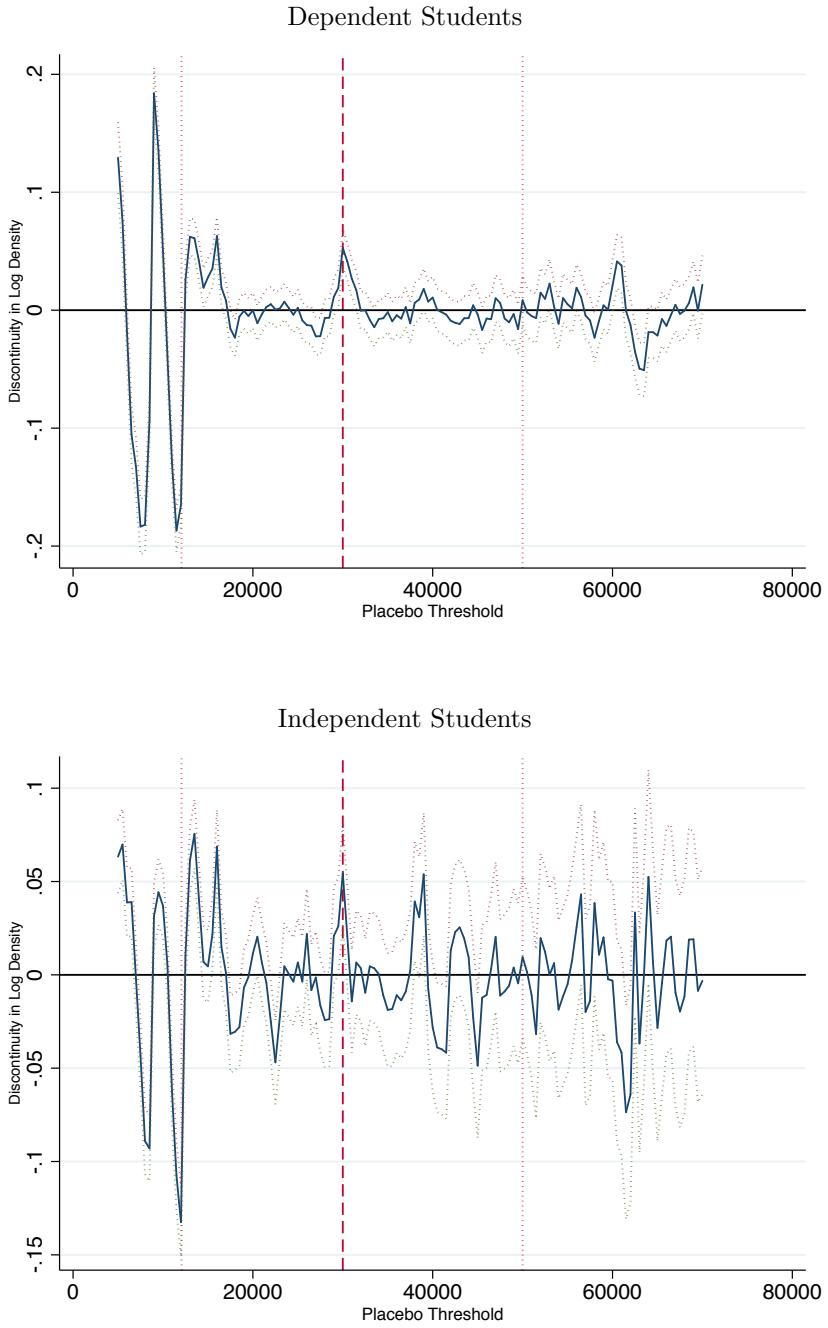
Notes: The value of the auto-zero EFC threshold and the maximum Pell award amount are shown relative to their respective value in 2002. All values are in current dollars.

Figure 5:
Density of Family Income by Aid Type by Whether States Allow Simplification in 2010



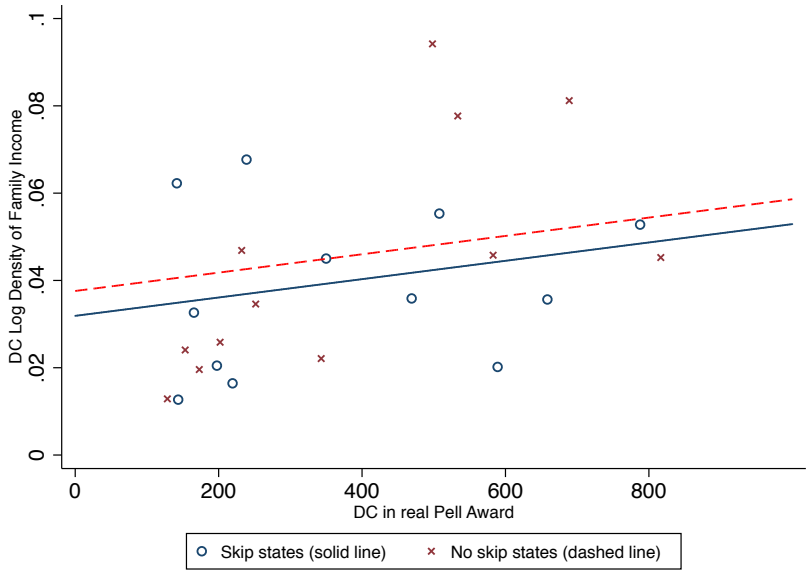
Notes: Figures in the left column use student data in states that allow skip-logic to be implemented in the FAFSA on the Web, whereas figures on the left are based on students in the 15 states that do not permit FAFSA questions to be skipped. Discontinuities and standard errors are estimated using the procedure proposed by McCrary (2008), using 'rule-of-thumb' bandwidths. See tables for alternative estimates using different bandwidths.

Figure 6:
Test for Discontinuities at Placebo Thresholds: Students in Simplification States in 2010



Notes: Placebo discontinuities in the density are estimated using the rule of thumb bandwidth using McCrary's (2008) proposed procedure under placebo AZ thresholds between $5,000 and $70,000, at $500 intervals. The solid line represents these discontinuity estimates, and the dotted lines denote the 95% confidence bands around the estimate. The first dotted line indicates the first kink of the EITC schedule (where the maximum credit is reached) for families with 2 or more children. The dashed line corresponds to the AZ threshold in 2010.

Figure 7:
Simplification and Pell Effects on Enrollment: Dependent Students 2003-2014



Notes: The markers in the figure plot estimates of the discontinuities in the log density of enrollment by by the discontinuity in Pell awards in each year, separately for states that do and do not allow FAFSA simplification (i.e., skip vs. non-skip states). The regression lines show the minimum distance fit of the statistical model (3) explained in the text.

Table 1:
Descriptive Statistics for Select Variables and Years

	All	Dependents	Independents w/o kids	Independents w/kids
			2002	
Family Income	32088.4	44415.5	14446.2	18320.9
	36587.7	42167.9	18584.6	19557.5
% Below AZ Threshold	.337	.195	.596	.46
	.473	.396	.491	.498
EFC	4479.4	6540.3	3372.4	988.7
	8828	10639.4	6166.3	3177
% Living in Skip-Logic States	.741	.738	.747	.744
	.438	.44	.435	.436
% Mom College Grad	.302	.366	.253	.202
	.459	.482	.434	.402
Age	24.1	19.6	29.2	30.1
	7.60	1.4	8.70	8
% Female	.619	.558	.535	.8
	.486	.497	.499	.4
Ever receive federal loan	.694	.759	.705	.557
	.461	.428	.456	.497
Ever receive Pell Grant	.731	.64	.71	.931
	.443	.48	.454	.253
Observations	2664310	1474954	466498	722858
			2014	
Family Income	37841.5	52897.6	14969.9	23945.6
	48684.5	57839.7	21113.1	27129.7
% Below AZ Threshold	.516	.358	.796	.632
	.5	.479	.403	.482
EFC	4554	6967.9	2777.4	849.2
	10810.5	13457	6240.3	3710.2
% Living in Skip-Logic States	.738	.741	.74	.732
	.44	.438	.439	.443
% Mom College Grad	.368	.436	.309	.27
	.482	.496	.462	.444
Age	25	19.7	30.7	31.9
	8.5	1.4	9.9	8.4
female	.606	.562	.519	.767
	.489	.496	.5	.423
Ever receive federal loan	.656	.648	.674	.66
	.475	.478	.469	.474
Ever receive Pell Grant	.759	.682	.773	.911
	.428	.466	.419	.285
Observations	4548294	2466160	913348	1168786

Note: Entries in the labeled rows show means of each variable for the subgroup indicated in the columsn. The values below the labeled rows are standard deviations.

Table 2:
Pell Discontinuity Estimates at the AZ Threshold

Year	Dependents		Independents w/Dependents	
	Ave. Pell	Discontinuity	Ave. Pell	Discontinuity
2002	3345.4	162.1	3679.7	33.7
		(9.4)		(4.2)
2003	3546.8	171.1	3920.7	31.4
		(9.8)		(4.9)
2004	3498.6	224.5	3932	53.3
		(9.4)		(5)
2005	3520.5	205.6	3930.8	55.8
		(8.4)		(5.3)
2006	3478.7	246.6	3909	77.5
		(8.4)		(5.9)
2007	3155.5	361.2	3602.4	234.3
		(13.2)		(6.7)
2008	3494	517.4	3965.7	242.7
		(8.1)		(5)
2009	3959.8	490.7	4439.1	200.7
		(8.1)		(4.8)
2010	4083.8	810.5	4822.6	393.3
		(8.4)		(7.3)
2011	4482	598.9	5238.9	199.1
		(8.4)		(6.2)
2012	4390.7	677.7	5315.3	142.9
		(8.8)		(6.3)
2013	5228.7	148	5522.1	3.8
		(7.3)		(3.2)
2014	5345	142.4	5620.6	3.5
		(8.1)		(3.2)

Note: Entries in the labeled rows show discontinuity estimates in the simulated Pell grant award in each year using information from all states for the relevant year. Standard errors are in parentheses.

Table 3:
Discontinuities in the Density of Family Income at the AZ Threshold in Simplification and Non-simplification States in Select Years

		Simplification States				No Simplification States			
Year	Pell	ROT BW	100% BW	70% BW	50% BW	ROT BW	100% BW	75% BW	50% BW
					Dependents				
2002	162	6853	.0639	.038	.04	7800	.0492	.0586	.0572
	(9.4)		(.009)	(.0105)	(.0105)		(.0161)	(.0187)	(.0231)
2004	225	6602	.0164	.0075	.0166	7710	.0469	.0472	.0682
	(9.4)		(.0092)	(.0106)	(.0106)		(.0156)	(.0181)	(.0222)
2006	247	6048	.0677	.0599	.0668	6784	.0346	.0118	.0068
	(8.4)		(.0091)	(.0105)	(.0105)		(.016)	(.0186)	(.0226)
2008	517	6917	.0553	.0536	.0538	8354	.0942	.0999	.1163
	(8.1)		(.0083)	(.0096)	(.0096)		(.0136)	(.0157)	(.0193)
2010	811	6743	.0528	.0518	.0474	8336	.0452	.0417	.0456
	(8.4)		(.0079)	(.0091)	(.0091)		(.0122)	(.0141)	(.0173)
2012	678	6905	.0356	.0225	-.0009	8823	.0812	.0636	.0331
	(8.8)		(.0078)	(.009)	(.009)		(.0119)	(.0138)	(.0169)
2014	142	6969	.0623	.0671	.0604	8373	.0129	.0249	.0212
	(8.1)		(.0074)	(.0086)	(.0086)		(.012)	(.0139)	(.0171)
					Independents w/Dependents				
2002	34	4768	.0033	.0048	.0257	5736	.0135	.0043	.0091
	(4.2)		(.0115)	(.0133)	(.0133)		(.0181)	(.0209)	(.0256)
2004	53	5470	.0043	.0265	.0413	6104	.0075	.0211	.0195
	(5)		(.011)	(.0126)	(.0126)		(.0176)	(.0203)	(.0249)
2006	77	5203	.0091	.0176	.0188	5847	.008	.0167	.0468
	(5.9)		(.0107)	(.0123)	(.0123)		(.0169)	(.0196)	(.0239)
2008	243	5200	.0145	.0199	.03	5883	-.0105	-.0044	-.0141
	(5)		(.0111)	(.0128)	(.0128)		(.0173)	(.02)	(.0244)
2010	393	5147	.0544	.0592	.06	6269	.0347	.041	.0625
	(7.3)		(.0133)	(.0153)	(.0153)		(.0199)	(.0229)	(.0281)
2012	143	5526	.0154	.0115	.0088	6384	.0537	.0486	.0514
	(6.3)		(.0121)	(.014)	(.014)		(.0186)	(.0215)	(.0262)
2014	3	5175	.0282	.0167	.0014	5717	-.0011	-.0013	-.0119
	(3.2)		(.0114)	(.0132)	(.0132)		(.0181)	(.021)	(.0256)

Note: Discontinuity estimates are based on the procedure and programs developed by McCrary (2008). ROT BW denotes the "rule of thumb" bandwidth (Silverman 1986). For robustness, discontinuity estimates are presented using the ROT bandwidth, and two smaller bandwidths that are 75 and 50 percent as large. Estimates based on local linear regressions using the data in Figure 1 (i.e., log of the number of observations in each family income bin) yield similar results.

Table 4:
Permutation Tests of Discontinuity in (Log) Family Income Density Fraction of placebo t-statistics greater than t-statistic at true AZ income cutoff

Year	Dependents		Independents w/Dependents	
	Skip States	No-Skip States	Skip States	No-Skip States
2002	0/96	0/93	29/83	17/78
2003	0/92	16/92	1/82	54/78
2004	5/93	1/91	27/81	27/77
2005	4/96	7/92	62/81	17/76
2006	0/96	4/95	16/80	24/76
2007	0/95	7/93	0/53	0/53
2008	0/96	0/86	12/78	49/73
2009	0/97	0/89	50/78	42/76
2010	1/97	0/90	1/79	5/75
2011	2/95	1/89	0/50	0/50
2012	7/94	0/88	9/50	1/50
2013	18/93	9/85	6/74	10/70
2014	3/92	13/88	7/49	21/48

Note: For each year, student type, and state-group, the discontinuity in the log density of family income is estimated at each of 140 (120) placebo thresholds between $500 and $70000 ($60,000) using the rule-of-thumb bandwidth for dependent (independents). Omitting all placebo estimates that are within .5 bandwidths of either the income value at the first kink-point of the EITC or $50,000, the table reports the fraction of the t-statistics of placebo estimates that are greater than the t-statistic for the estimate at the true AZ income threshold.

Table 5:
Estimates of the Effect of Simplification and Pell Eligibility on Enrollment by Time Period and Student Type

	Dependents			Independents w/Dependents		
	Simplification	Pell	Other	Simplification	Pell	Other
Period	$\widehat{\beta_1}$	$\widehat{\beta_2}$	$\widehat{\theta^\epsilon}$	$\widehat{\beta_1}$	$\widehat{\beta_2}$	$\widehat{\theta^\epsilon}$
2003-2006	.002	.3726	-.0459	.0017	-.1094	.0102
	(.0164)	(.2715)	(.0589)	(.0109)	(.3096)	(.0192)
2007-2010	-.0126	.0149	.0516	.0038	.2558	-.0494
	(.0155)	(.0423)	(.0268)	(.0168)	(.1145)	(.0321)
2011-2014	-.0081	.0146	.0355	-.0029	.0759	.0284
	(.0208)	(.0388)	(.0231)	(.0158)	(.0846)	(.0151)
2003-2014	-.0071	.0284	.0341	.0007	.0561	.0104
	(.0095)	(.019)	(.0112)	(.0099)	(.0421)	(.01)
Robust years only	-.0071	.0284	.0341	.0076	-.0554	.0053
	(.0095)	(.019)	(.0112)	(.0077)	(.0425)	(.0078)

Note: Entries reflect minimum distance estimates of the parameters in each column, separately by time period. The row marked 'robust years only' restricts the analysis to data for years in which the p-values from the permutation test for no impact of the AZ policy on enrollment are below 0.2. As shown in Table 4, this restricts the years used for independent students to 2007, and 2010 to 2013. Standard errors are in parentheses.

Table 6:
Estimates of the Effect of Simplification and Pell Eligibility on Enrollment for Subgroups, All Years (2003-2014)

	Dependents			Independents w/Dependents		
	Simplification	Pell	Other	Simplification	Pell	Other
Period	$\widehat{\beta_1}$	$\widehat{\beta_2}$	$\widehat{\theta^\epsilon}$	$\widehat{\beta_1}$	$\widehat{\beta_2}$	$\widehat{\theta^\epsilon}$
Female	-.0017	.0226	.0324	-.0021	.0502	.0108
	(.0101)	(.0204)	(.012)	(.0106)	(.0424)	(.0107)
Male	-.0061	.0382	.0332	.0127	.1221	-.0009
	(.0103)	(.0201)	(.0119)	(.0109)	(.0574)	(.0108)
First Generation	-.0046	.0326	.0352	.0018	.0367	.0111
	(.0112)	(.0232)	(.013)	(.011)	(.0481)	(.0111)
Not First Generation	-.0067	.0112	.0403	-.0001	.0705	.0112
	(.0114)	(.0228)	(.0137)	(.0143)	(.0573)	(.0142)

Note: Entries reflect minimum distance estimates of the parameters in each column, based on all years from 2003 to 2014. Note that 'First Generation' here refers to students who report neither parent having completed college on their FAFSA form. See text for details. Standard errors are in parentheses.

Table 7:
Estimates of the Effect of Simplification and Pell Eligibility on Enrollment across Higher Education Sectors, All Years (2003-2014)

	Dependents			Independents w/Dependents		
	Simplification	Pell	Other	Simplification	Pell	Other
Period	$\widehat{\beta_1}$	$\widehat{\beta_2}$	$\widehat{\theta^\epsilon}$	$\widehat{\beta_1}$	$\widehat{\beta_2}$	$\widehat{\theta^\epsilon}$
All	-.0071	.0284	.0341	.0007	.0561	.0104
	(.0095)	(.019)	(.0112)	(.0099)	(.0421)	(.01)
Public 2-3 Year	-.0016	.0469	.0321	-.005	.1052	.0139
	(.0121)	(.0235)	(.0138)	(.0128)	(.052)	(.0127)
Public 4+ Year	-.0018	.0032	.0414	.0012	.0392	.0178
	(.0112)	(.0224)	(.0131)	(.0178)	(.0658)	(.0176)
Private 4+ Year	-.0146	.0521	.0253	.0309	.1234	-.0225
	(.0147)	(.0323)	(.0177)	(.0225)	(.0813)	(.0223)
Proprietary <2 Year	-.0082	.0427	.0502	.0103	.1039	.0023
	(.0283)	(.0621)	(.0334)	(.0297)	(.1636)	(.0287)
Proprietary 2-3 Year	-.0139	.0084	.0476	.0051	.1089	-.0011
	(.0234)	(.0454)	(.0271)	(.0227)	(.1114)	(.0222)
Proprietary 4+ Year	.0137	.0349	.0227	-.0041	-.0499	.0292
	(.0208)	(.0384)	(.0278)	(.0177)	(.0741)	(.0182)

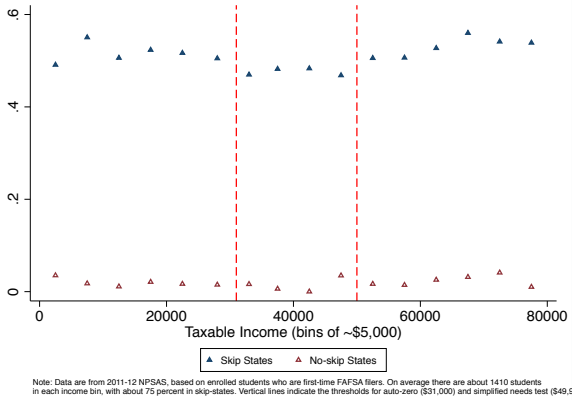
Note: Entries reflect minimum distance estimates of the parameters in each column, based on all years from 2003 to 2014. See text for details. Standard errors are in parentheses.

Appendix 1

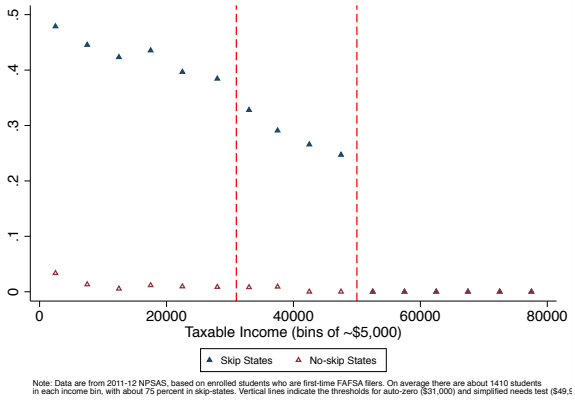
Appendix Figure 1:
No Information Reported for FAFSA Asset Questions by Taxable Income and State Group, 2011-12

Dependent Students

Parent's Assets



Student's Assets



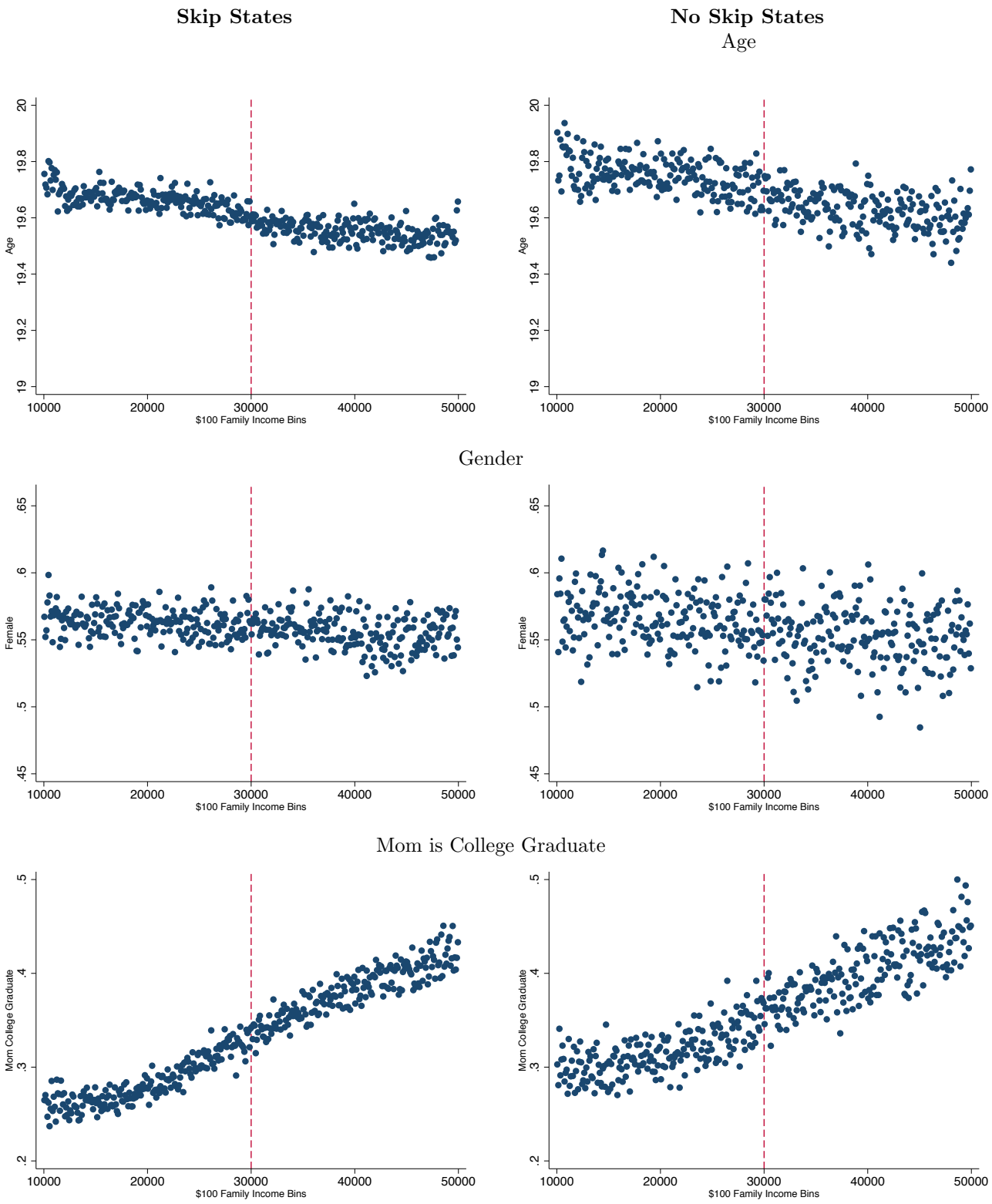
Notes: Data are from the 2011-2012 NPSAS. See the text for discussion.

Appendix Table 1:
Percent of All FAFSA Filers Using FAFSA on the Web

Year	Percent
1997-98	20
1998-99	24
1999-00	32
2000-01	40
2001-02	50
2002-03	60
2003-04	66
2004-05	77
2005-06	88
2007-08	96
2009-10	99.4
2011-12	99.6
2013-14	99.8

Notes: Data are from https://ifap.ed.gov/presentations/attachments/20022003AppProcessingUpdate.pdf (includes both renewals and first) for 1998-2003, assorted IFAP documents from 2004-2006, and IFAP FAFSA Volume Reports for 2008 and onward.

Appendix Figure 2:
Selected Covariates Near the AZ Threshold, 2010 Skip States



Notes: The value of the auto-zero EFC threshold is indicated with the vertical dashed line.