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Who Benefits From Student Aid? The Economic Incidence of Tax-Based Federal Student Aid

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WHO BENEFITS FROM STUDENT AID?

THE ECONOMIC INCIDENCE OF TAX-BASED FEDERAL STUDENT AID

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Abstract: Federal benefit programs are designed to aid targeted populations. Behavioral responses to these programs may alter the incidence of their benefits, a possibility that receives less attention in the literature compared to tax incidence. I demonstrate the importance of benefit incidence analysis by showing that the intended cost reductions of tax-based federal student aid are substantially offset by institutional price increases for a sample of 4-year colleges and universities. Contrary to the goal of policymakers, I find that tax-based aid crowds out institutional aid roughly dollar-for-dollar. Unfortunately, it is not clear how institutions utilize these captured resources, so that the ultimate incidence of the programs is uncertain.

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1. Introduction

Behavioral responses to government programs may undermine their intended effects, and as a result, alter their designed welfare implications. For this reason, tax incidence, which examines where the burdens of taxation ultimately fall, receives much attention in the literature (Fullerton and Metcalf 2002; Gruber 1997; Kubik 2004). The study of benefit incidence of government programs is less common. For example, until recently the assumption that the Earned Income Tax Credit benefits recipients had never been tested. Leigh (2010) and Rothstein (2010) address this omission, and find substantial erosion of benefits for nominal recipients via reduced wages in the labor market. These results suggest that the efficacy of benefit programs depends crucially on the extent of offsetting price changes. This paper adds to the benefits incidence literature by quantifying the institutional price response to tax-based federal student aid.

Funding for federal student aid, over $660 billion between 1998 and 2006,[2] is based on the assumption that students and families claiming the programs are the economic beneficiaries. The existing literature finds that student aid increases enrollment (Dynarski 2000, 2003; Ellwood and Kane 2000; Heller 1997; Leslie and Brinkman 1987; Turner 2009), but how effectively these programs do so depends on the degree to which there are offsetting price changes. Yet, the literature examining the institutional price response to student aid is limited and generally focuses on tuition effects at the school level. The use of tuition increases to appropriate the benefits of federal student aid is referred to as the Bennett Hypothesis, named after former Education Secretary William Bennett.[3] Long (2006, 2008) and Ikenberry (1997) discuss the

[2] Expenditures are in 2006 dollars and include grants, student loans and tax-based aid (Baum and Steele 2007).
[3] Bennett (1987) made his original argument in the context of federal loan programs. Following Singell and Stone (2007) and Hoxby (1998), I use a broad interpretation of the Bennett Hypothesis that includes the appropriation of any external student aid program using tuition increases.

existing work on the Bennett Hypothesis and note that there is weak empirical evidence supporting its validity.[4]

One possible explanation for these inconclusive findings is that instead of increasing tuition, schools may appropriate the benefits of federal student aid by strategically reducing institutional grant aid. I refer to this possibility as the price-discrimination Bennett Hypothesis. Unlike tuition increases that affect all students, the reduction of institutional aid allows schools to realize financial gains from increases in federal student aid while ensuring that no student is made worse off. The strategic use of institutional aid also avoids the highly visible and unpopular process of increasing tuition.[5] Both policymakers and financial aid administrators are aware of the possibility that institutional aid will be replaced by tax-based aid. Former Education Secretary Richard Riley sent a letter to presidents of colleges and universities declaring that the goal of tax-based aid is to, "…provide additional help for families to pay for college and not simply substitute for existing sources of financial assistance" (Riley 1998). In response, some financial aid administrators pledged that students would receive the full benefits of tax-based aid (Burd 1998). However, others argued for the need to incorporate tax-based aid awards in the calculation of institutional aid. One such director noted, "…families that receive

[4] Long (2004) finds limited support for the Bennett Hypothesis in response to two tax-based aid programs. Contrary to the Bennett Hypothesis, Long (2003) and Scafidi, Rubenstein, Schwartz and Henry (2007) report price decreases in the context of the Georgia Hope Scholarship. McPherson and Shapiro (1991) and Singell and Stone (2007) report conflicting patterns of changes in tuition across public and private schools in response to federal grant programs. Singell and Stone (2007) and Rizzo and Ehrenberg (2003) report opposite findings on the effect of out-of-state tuition at public schools.

[5] This is an especially complicated process for public schools. Only 16 states give schools the authority to raise tuition, while the legislature, a state agency or a system board sets tuition in the remaining states (Mumper and Freeman 2005). McPherson and Shapiro (1998) note that there is substantial pressure to limit tuition increases at private schools, and in earlier work (1991), suggest that the goal of maintaining an economically diverse applicant pool moderates tuition increases.

$1,500 from the federal government are better off than those that don't. And I don't think that I can ignore that" (Burd 1998).[6]

Despite the awareness that institutions may decrease aid, rather than increase tuition, in response to increases in external aid, Long (2003) and McPherson and Shapiro (1991) are the only papers that explicitly raise this possibility. They document student aid incidence at the school level and reach different conclusions on whether external aid is a substitute for institutional aid.[7] Yet, the use of school-level data prevents Long (2003) and McPherson and Shapiro (1991) from determining *which* students are impacted by the institutional response. The flexibility of student-level data allows me to add to this work by addressing several related questions. First, do colleges and universities selectively lower institutional grant aid *for students that benefit from tax-based aid*? Second, how do students who experience these aid declines cope? Due to a likely time delay in benefit receipt of tax-based aid, a reduction in institutional aid may cause students to borrow more in order to offset their short-term unmet need.

To estimate student-level effects, I exploit policy-induced variation in all three tax-based aid programs, the Hope Tax Credit, the Lifetime Learning Tax Credit and the Tuition Deduction, using data from the National Center on Education Statistics. The analysis sample includes students enrolled at 190 4-year schools during the 1995-96, 1999-2000 and 2003-04 school years.[8] Enrollment at the schools in the sample represents roughly 40 percent of students enrolled in 4-year colleges and universities, although schools in the sample are relatively more selective and have larger enrollments than comparable 4-year schools nationally. I estimate the intention

[6] $1,500 is the maximum value of the Hope Tax Credit, and was the maximum tax-based aid award when the statement was made in 1998.
[7] Long (2003) reports that merit-based aid substitutes for institutional aid at private colleges in Georgia in the 1990s, whereas McPherson and Shapiro (1991) find that federal grant aid complements institutional aid for private schools in an earlier period for a nationally representative sample.
[8] The number of schools is rounded to the nearest 10 to comply with the Department of Education Institute of Education Sciences confidentiality statutes.

to treat effect of tax-based aid using instrumental variables to address the endogeneity of education spending and school fixed-effects to control for unobserved heterogeneity in student aid practices across institutions.

Contrary to the goal of policymakers, who sought to increase postsecondary access for eligible students by lowering the cost of enrollment, I find that the institutional price response substantially counteracts the intended cost savings of tax-based aid. Students appear to increase loans in response to the reduction of institutional aid, suggesting that tax-based aid falls short of an important federal aid goal to reduce student indebtedness (Burgdorf and Kostka 2006). These results imply that students eligible for tax-based aid may not be the economic beneficiaries of the programs. To determine the ultimate incidence of tax-based aid, I consider two ways in which institutions might utilize the captured resources. One, that institutions redirect aid towards students who are ineligible for tax-based aid, or two, that institutions channel the resources into other expenditures, such as capital improvements or faculty/staff salaries. Unfortunately, these results are largely uninformative so that the incidence of tax-based aid is uncertain (see Appendix A). However, I offer an important first step in establishing the incidence of tax-based aid by demonstrating that eligible students and their families are not directly benefitting from tax-based aid in the manner envisioned by policymakers. Similar unintended behaviors are found to offset the intention of policies in other contexts, including public health insurance (Cutler and Gruber 1996) and intergovernmental grants (Baicker and Gordon 2006; Gordon 2004; Hines and Thaler 1995).

The remainder of this paper proceeds as follows. Section 2 provides information on the tax-based aid. In Section 3, I discuss the institutional price response to federal student aid. Section 4 discusses the empirical specifications and results. Section 5 concludes.

2. Tax-Based Federal Student Aid

Tax-based aid programs provide a convenient natural experiment for examining the impact of federal aid on college pricing. Program implementation and changes in program generosity create discrete changes in aid for eligible students over time. In 1997, the Taxpayers' Relief Act introduced the Hope Tax Credit and the Lifetime Learning Tax Credit. In 2001, The Economic Growth and Tax Relief Reconciliation Act added a third program, the Tuition Deduction. Between 1998 and 2006 these three tax-based aid programs cost over $41 billion and were claimed by more than 54 million students and their families (Baum and Steele 2007).

While there are three tax-based aid programs, only one program may be claimed per student per year. The value of each program depends in part on educational spending. The Hope Tax Credit is equal to 100 percent of the first $1,000 and 50 percent of the next $1,000 of qualified expenses and may only be used during the first two years of undergraduate education (Internal Revenue Service [IRS] 1998). The Lifetime Learning Tax Credit covers 20 percent of qualified expenses for all years of postsecondary study for most students. Between 1998 and 2002, the qualified spending limit was $5,000, and in 2003, it increased to $10,000 (IRS 1998, 2003). The Tuition Deduction allows tax filers to deduct 100 percent of the first $3,000 of qualified education expenses,[9] and has a broader eligibility range compared to the Hope Tax Credit and Lifetime Learning Tax Credit (IRS 2002). Maag and Rohaly (2007) report that program take up is 63-74 percent, comparable to take up rates for Unemployment Insurance,

[9] The maximum deduction increased to $4,000 and the adjusted gross income eligibility range expanded in 2004. Because I use data from the 2003-04 school year, I calculate the Tuition Deduction based on the 2003 program rules, which were in place for the first half of that school year. This is done because the data do not include payment date and only expenses paid after January 1, 2004 are affected by the program changes.

Head Start and the Earned Income Tax Credit (Currie 2006).[10] Table 1 describes the programs in greater detail.

In contrast to federal grant aid, such as Pell Grants and Federal Supplemental Education Opportunity Grants that target relatively low-income students and families, tax-based aid targets middle and upper-middle class families. Figure 1 highlights several key features of the tax-based aid programs that result in middle-class targeting, showing how the maximum value of tax-based aid varies by adjusted gross income for a hypothetical joint-filing family of four. Below $20,000 of income, a family of four claiming only the standard deduction and personal exemptions will have no tax liability. Families in this income range will be unable to capitalize on tax-based aid because the Hope Tax Credit and the Lifetime Learning Tax Credit are nonrefundable and the Tuition Deduction cannot reduce taxable income below zero. As income increases beyond $20,000, tax-based aid phases in according to the marginal tax rate until tax liability is no longer binding. At higher-income levels, the value of the Hope Tax Credit and the Lifetime Learning Tax Credit are reduced due to the phase-out range, which begins around $80,000 for joint returns. The value of the Tuition Deduction is a function of the marginal tax rate. As shown in Figure 1, the Tuition Deduction increases with income due to the progressive tax rate schedule. Figure 1 also shows how the value of tax-based aid changes over time. The top panel reflects the introduction of the Hope and Lifetime Learning Tax Credits. The introduction of the Tuition Deduction and the increased generosity of the Lifetime Learning Tax Credit are visible in the lower panel of Figure 1.

[10] Long (2004) provides evidence that many parents/guardians were unaware of tax-based aid using data from the National Household Education Survey. She also reports that take-up was less than expected in the first years of the programs using National Postsecondary Student Aid Study data. However, the National Postsecondary Student Aid Study may not accurately capture program take up. More than one-third of respondents in the 1999-2000 survey replied "don't know" or "not reached/missing" when asked about tax-based aid use.

The timing of award receipt also sets tax-based aid apart from traditional forms of student aid. Benefits from tax-based aid are likely realized when tax returns are received, generally after educational expenses are paid. For example, a family that pays education costs in September and receives their tax return in April faces an eight-month delay.[11] As a result, if schools substitute tax-based aid for their own sources of aid, students will face a temporary increase in unmet financial need.

3. Institutional Pricing Behavior

While most authors agree that the standard profit maximization model does not fit colleges and universities (Clotfelter 1999; Winston 1999) there is not a consensus in the literature on the objective function for institutions of higher learning.[12] Despite this uncertainty, it is possible to infer the general institutional price response to tax-based aid. Suppose that schools have an optimal input allocation in equilibrium that includes student enrollment.[13] Other inputs are likely to include items such as research support, student services, faculty and staff salaries and physical capital. The introduction of tax-based aid distorts the equilibrium allocation by increasing education demand among eligible students. In response, colleges and universities will act to capture the financial gains from tax-based aid and redistribute it optimally across inputs.

Competitive pressures of the market determine the extent to which colleges and universities may capture the financial benefits of tax-based aid. If there is perfect competition then schools will not be able to reduce grant aid. To the extent that institutions exert some

[11] Tax filers could smooth the impact of the credit by adjusting their withholdings in earlier periods. However, this requires a high level of sophistication, and it is likely that most returns realize the benefits as a lump sum after education costs are paid.
[12] James (1978, 1990) and Long (2003) argue that prestige maximization incorporates many of the important goals of colleges and universities.
[13] Several authors suggest that enrollment is a key input in the objective function (Hoxby 2000; James 1990; McPherson and Shapiro 1991; Rothschild and White 1995).

degree of market power they will be able to realize financial gains by lowering grant aid for students that benefit from tax-based aid. There are several potential sources of market power for colleges and universities.[14] First, colleges and universities may be considered differentiated products. Second, unlike profit maximizing firms, schools select the purchasers of their product (Clotfelter 1999; Rothschild and White 1995; Winston 1999). This aspect allows some schools to maintain excess enrollment demand in equilibrium. Third, information asymmetry may give pricing power to schools. Entering students do not observe the counterfactual level of aid that would have been offered in the absence of the institutional response. Therefore, colleges and universities have the ability to increase the net-price that they receive in a manner that may avoid backlash from parents and students. While some continuing students do have a history of aid that may serve as a basis for comparison with their current aid offer, institutions may still exercise pricing power over continuing students due to the costs associated with transferring schools. Transferring students are likely to incur financial costs in addition to academic costs, as credit hours may not be entirely transferable.

In practice, financial aid administrators can determine eligibility and award size for tax-based aid from the Free Application for Federal Student Aid (FAFSA). Given this information, administrators can substitute tax-based aid for other sources of aid. Such a response should not be driven by a mechanical relationship between the receipt of tax-based aid and an increase the expected family contribution. The FAFSA explicitly collects information on tax-based aid in order to prevent these programs from being clawed-back via the expected family contribution

[14] The Department of Justice alleged anticompetitive behavior among private colleges in the late 1980s and early 1990s (Jaschik 1991; Netz 1999). While Hoxby (2000) shows evidence that schools named in the antitrust case were not colluding to increase tuition or to decrease the total amount of grant aid awarded, she does find evidence that the distribution of institutional grant aid across students changed within these schools following the lawsuit.

calculation.[15] McPherson and Shapiro (1998) speculate that institutional aid may be reduced dollar-for-dollar in response to tax-based aid. In contrast, it is unlikely that tax-based aid will have a large impact on Pell Grants and federal campus-based aid, due to limited overlap in eligibility with these programs. Maag and Rohaly (2007) estimate that tax returns with income of at least $40,000 receive about 65-70 percent of the total expenditures for the tax credit programs while Mercer (2005) notes that 90 percent of families claiming Pell Grants have income less than $40,000.

Ultimately, determining the scope of the institutional price response to tax-based aid is an empirical question. Figure 2 provides some suggestive evidence that financial aid administrators at 4-year colleges and universities responded to tax-based aid by lowering institutional grant aid for eligible students. This figure shows changes in tax-based aid and changes in institutional grant aid by income between the 1999-2000 and 1995-96 school years (top panel) and the 2003-04 and 1999-2000 school years (bottom panel). A decrease in grant aid for students realizing an increase in tax-based aid is consistent with the price-discrimination Bennett Hypothesis, although the changes in aid are unconditional on school or student characteristics and are relatively noisy.

4. Empirical Strategy and Results

4.1. Analysis Sample

To explore the institutional price response to tax-based aid, I use data from the National Postsecondary Aid Study (NPSAS) published by the National Center for Education Statistics. These data provide student-level information on financial aid, student and parent characteristics, and institutional detail. Using samples from the 1995-96, 1999-2000 and 2003-04 school years, I include 190 4-year schools with roughly 74,280 undergraduate students aged 18-24 in the

[15] See Worksheet B in the 1999-2000 FAFSA or Worksheet C in the 2003-04 FAFSA.

primary analysis sample.[16] To construct this sample I limit the data in two ways. First, I drop students with invalid grade level responses.[17] The value of tax-based aid depends on grade level (the Hope Tax Credit is available only during the first two years of college), so including observations with missing grade level information adds measurement error. I also limit the sample to the 190 schools that appear in each of the NPSAS files, ensuring that the sample of schools is balanced over time.[18] As a robustness check, I also analyze an unbalanced panel of 350 4-year schools that appear in the 1995-96 school year and at least one of the later school years (1999-2000, 2003-04).[19]

The NPSAS does not include information on the value of tax-based aid, or reliable information on program use. To address this shortcoming of the data, I estimate the value of the tax-based aid in the following way. First, I use IRS rules (1998, 2002, 2003) to define the formulas for the Hope Tax Credit, the Lifetime Learning Tax Credit and the Tuition Deduction, which depend on education spending, adjusted gross income and taxes owed. The NPSAS contains data on family income and education spending, defined as tuition minus student aid. In calculating taxes owed, I assume that only the standard deduction and personal exemptions are claimed. Using these values of income, education spending, and taxes owed, I apply the tax-based aid formulas to estimate the value of each of the three programs for a given student.

[16] Both the number of schools and the sample size are rounded to the nearest 10 to comply with the Statistical Standards Program of the U.S. Department of Education Institute of Education Sciences restricted use data protocol.
[17] Invalid grade level responses include both observations with missing information and those that skipped the survey question. Roughly 5 percent of students have invalid grade level responses.
[18] In the 1995-96 NPSAS sample there are roughly 440 4-year schools.
[19] The number of schools is rounded to the nearest 10 to comply with the Department of Education Institute of Education Sciences confidentiality statutes.

Students can claim at most one program per year, so for students that are eligible for multiple programs, I assign the program with the largest value.[20]

Figure 3 shows the average value of the eligible tax-based aid award by adjusted gross income for the 1999-2000 and 2003-04 school years. Cross-sectional variation in the subsidy arises from differences in qualified education spending, differences in adjusted gross income, differences in tax filing status and from program rules that define eligibility. These sources of variation are evident across the panels in Figure 3. For example, the decrease in value around $40,000 corresponds to the phase out range for non-joint returns whereas the decrease in value around $80,000 corresponds to the phase out for joint returns. Time-series variation comes from the enactment of the tax-credit programs in 1998, and from the increase in the value of the Lifetime Learning Tax Credit and introduction of the Tuition Deduction between the 1999-2000 and 2003-04 school years.

For public (private) schools in the sample, average tax-based aid eligibility is $586 ($634) compared to $480 nationally in the 1999-2000 school year and $682 ($869) compared to $540 nationally in the 2003-04 school year.[21] Omitting 2-year schools from the sample is likely to result in larger tax-based aid relative to the national average.[22] Table 2 shows the mean values of various measures of student aid, including tax-based aid, and student demographic variables by institution type for each of the three school years. (All dollar amounts in Table 2 are in 2003 dollars.) The value of tax-based aid is comparable across public and private school students in the sample because the maximum qualified spending of the tax-based aid programs is relatively

[20] Analyzing individual tax return data from the IRS, Turner (2010) finds that not all taxpayers select the single tax-based aid program that offers the largest value. However, the dollar amount of the loss incurred from these selections is small, so the effect on the estimated value of tax-based in this work should be minimal.
[21] National average for tax-based aid from Trends in Student Aid 2004, The College Board, Table 7.
[22] The price response by 2-year schools is likely to be different than that of 4-year schools, as 2-year schools generally offer less aid and therefore have less scope for the price-discrimination Bennett Hypothesis. Unfortunately, I am unable to consider 2-year schools separately due to sample size considerations. Omitting 2-year institutions removes an important component of the national postsecondary education market.

low, so that additional education spending by private school students does not increase the value of the tax-based aid award.

Enrollment at schools in the analysis sample represents roughly 40 percent of national 4-year college enrollment during the analysis period. Table 3 shows both aggregate national enrollment and total enrollment for schools in the analysis sample. Broken down by school type, enrollment at 4-year public schools in the sample is roughly 45 percent of 4-year public enrollment nationally and enrollment at 4-year private schools in the sample is about 25 percent of national 4-year private enrollment. However, schools in the sample are on average larger than comparable schools nationally. In the 2003-04 school year, median enrollment in the sample at public (private) institutions is about 24,000 (12,500) compared to roughly 10,000 (2,000) nationally.[23] The sample of schools is also relatively more selective than comparable 4-year schools nationally. In the sample, the share of public (private) schools that are most or very selective is 27 (60), moderately selective 65 (36), and minimally selective and open admissions is 8 (4). Nationally, these shares for public (private) schools were 14 (38), 59 (41) and 27 (21) in the 2003-04 school year.[24] Baum and Steele (2007) show that more selective schools offer more institutional aid, suggesting greater scope for the price-discrimination Bennett Hypothesis. As a result, the institutional response for the sample of schools considered here is likely to represent an upper bound of the price-discrimination Bennett Hypothesis.

4.2. Measuring the Price-Discrimination Bennett Hypothesis

[23] I measure average school size using enrollment and number of institutions from Tables 168 and 243 from the Digest of Education Statistics 2005, National Center for Education Statistics.
[24] National selectivity from the 2003-04 NPSAS and includes 4-year not-for-profit schools.

The price-discrimination Bennett Hypothesis predicts that institutions reduce institutional aid to capture the financial benefits of tax-based aid. To explore this possibility, I exploit policy-induced variation in the value of tax-based aid using Equation (1):

$$(1) \qquad InstitutionalAid_{ijt} = \beta_1 TBA_{it}\left(S_{ijt}, I_{it}, \tau_{it}\right) + \beta_2 X_{ijt} + \alpha_j + \varepsilon_{ijt}$$

where *i, j, t* index individuals, schools and years respectively. The key independent variable is tax-based (*TBA*), which is a function of education spending (*S*), adjusted gross income (*I*) and taxes (τ) as described in Section 4.1. β_1 measures the impact of eligibility for one dollar of tax-based aid on institutional aid. An estimate of -1 for β_1 implies dollar-for-dollar aid substitution, while $\beta_1 < 0$ is consistent with the price-discrimination Bennett Hypothesis.

A primary concern with ordinary least squares (OLS) estimation of Equation (1) is the possibility that tax-based aid and institutional aid are jointly determined, because tax-based aid is a function of education spending. Holding all else equal, if spending is below the programs' limits, an increase in education costs, such as a reduction in institutional aid, will increase the value of tax-based aid. Figure 4 highlights this program feature, showing the values of the three tax-based aid programs as functions of qualified education spending. To address this source of bias, I instrument for tax-based aid using two separate approaches. In each case, I generate an instrument by calculating the value of the tax-based aid using a plausibly exogenous value of education spending in order to isolate policy-induced variation in tax-based aid eligibility.[25]

In the first approach, I instrument using the value of tax-based aid calculated at the programs' spending limits. As shown in Figure 4, the value of tax-based aid is constant for

[25] Wooldridge (2002) refers to this as a generated instrument, and shows that estimates using generated instruments are consistent and reach valid inferences. This approach is similar to Dahl and Lochner (2008) who replace an endogenous input in the Earned Income Tax Credit schedule to isolate policy-induced variation. It is also similar to Hoxby and Kuziemko (2004) who use pre-period school district characteristics in the contemporaneous school aid formula to isolate policy-induced variation from a school finance equalization in Texas and refer to their approach as simulated instrumental variables following Currie and Gruber (1996).

qualified spending that exceeds these limits, so that this instrument is unaffected by an institutional response that increases spending. Equation (2) gives the first-stage equation:

$$(2) \qquad TBA_{it}\left(S_{ijt}, I_{it}, \tau_{it}\right) = \gamma_1 TBA_{it}\left(S_{\max t}, I_{it}, \tau_{it}\right) + \gamma_2 X_{ijt} + \alpha_j + \varepsilon_{ijt}$$

where $S_{\max t}$ is the maximum spending limit in year *t*.

As a second approach, I use an instrument that includes variation in education spending by student characteristics for each school. This instrument relies on a plausibly exogenous level of spending that is based on the determinants of spending in the 1995-96 school year. A key assumption of this approach is that spending in 1995-96 is unaffected by the endogenous response expected after the enactment of tax-based aid. To construct this instrument, I first estimate spending in the 1995-96 school year as a function of student income and demographic characteristics for each school *j*, using Equation (3).

$$(3) \qquad S_i = \lambda_{j1} black_i + \lambda_{j2} hispanic_i + \lambda_{j3} income_i + \lambda_{j4} age_i + \lambda_{j5} female_i + \lambda_{j6} dependent_i + \varepsilon_i$$

Next, I use the parameter estimates from Equation (3) to predict qualified spending for students in later periods at the same school (in real terms).[26] From this simulation, I calculate the instrument as the value of tax-based aid that a given student would have received if the pattern of qualified spending were held constant from the 1995-96 school year. Equation (4) gives the first stage equation.

$$(4) \qquad TBA_{it}\left(S_{ijt}, I_{it}, \tau_{it}\right) = \gamma_1 TBA_{it}\left(\hat{S}_{ijt}, I_{it}, \tau_{it}\right) + \gamma_2 X_{ijt} + \alpha_j + \varepsilon_{ijt}$$

$TBA_{it}\left(\hat{S}_{ijt}, I_{it}, \tau_{it}\right)$ is the subsidy based on simulated qualified spending $\hat{S}_{ijt}$ defined by Equation (3). This instrument contains variation at the individual-year level that results from program

[26] I use the CPI-U to adjust dollar amounts to 2003 dollars. I also used both the Higher Education Cost Adjustment published by the State Higher Education Executive Officers Association and the Higher Education Price Index reported in Lingenfelter, L'Orange, Winter, and Wright (2004) to adjust qualified spending, and the results were similar.

rules and differences in income, and also includes variation in spending both across schools and within schools based on income-demographic groups.

A key assumption of the estimation strategy is that the tax-based aid variable is not simply identified from an underlying relationship between institutional aid and income. To guard against this possibility, I flexibly control for income in X_{ijt} using a cubic spline function with five knots. Figure 5 shows the relationship between institutional aid and income in the 1995-96 school year using a similar spline function. For public schools, Figure 5 suggests that the preexisting relationship between institutional aid and income is substantively different than the relationship between tax-based aid and income shown in Figure 3. For private schools, where the underlying relationship between income and institutional aid is non-linear, this implication is less clear. However, for both public and private schools, the estimated effect of tax-based aid is identified, in part, from the differences in functional forms shown in Figures 3 and 5. To test the robustness of the estimates, I consider different income controls, including alternate spline function specifications and higher-order polynomial functions of income. I also control for the key determinants used in calculating tax liability, including the amount of taxes owed, family composition, number of family members and dependency status.

As control variables, I include sources of aid that the financial aid administrator is likely to treat as given when making institutional aid decisions. These include Pell Grants, federal campus-based aid and state aid. I also include school-year averages of these variables to address the possibility that changes in these programs for other students at a given school affect the institutional response to tax-based aid. Another assumption of the identification strategy is that these other sources of aid are relatively constant during the analysis period. Average changes in Pell Grants, campus-based aid and state aid for eligible students are small compared to the

average change in tax-based aid, suggesting that this assumption holds.[27] As a robustness check of the baseline results, I remove students who may have experienced changes in state aid based on the timing of state level policy changes. I discuss these results in Section 4.5.

To address time effects, I include indicator variables for the 1999-2000 and 2003-04 school years, and I allow for different time trends based on institutional selectivity by interacting year indicator variables with variables for selectivity. I also include controls for student, parent and institutional characteristics that may affect the receipt and value of institutional aid such as student race, age, gender, parent/guardian education and Census division of residence. To control for unobserved heterogeneity in student aid practices across schools, I include school fixed effects. I also cluster the standard errors at the school level, to allow for arbitrary correlation in the error terms between observations, both across different students and across different school years, at a given school.[28]

4.3. Baseline Results for the Price-Discrimination Bennett Hypothesis

The estimates imply that colleges and universities substantially offset the intended cost reduction of tax-based aid by reducing institutional grant aid. Panel A of Table 4 presents the baseline results. Columns (1)-(3) show the OLS, maximum spending IV and simulated spending IV results for public schools, while these results for private schools appear in Columns (4)-(6). For public schools, I cannot rule out nearly complete crowd out of institutional aid. The OLS estimate of the effect of an additional $1.00 of tax-based aid implies a reduction of $0.83 in institutional grant aid, while the maximum spending and simulated spending IV results suggest a

[27] For example, between the 1995-96 and 1999-2000 school years the average changes for eligible students were: Pell Grants ($46), campus-based aid (-$81), state aid ($14), tax-based aid ($754).
[28] Kezdi (2004) shows that cluster robust standard errors allow for accurate inference when the number of clusters exceeds 50.

reduction of $0.89 and $0.82. The reduction of institutional grant aid is also substantial at private schools, and I cannot reject an effect equal to that found for public schools. For private institutions, the OLS estimate implies a $1.20 reduction in grant aid per $1.00 of tax-based aid while the IV results suggest a reduction of $0.91 using the maximum spending IV and $0.69 using the simulated spending IV.

The instruments perform well among both public and private school students in Panel A of Table 4. The strength of the instruments is a result of the limited scope for endogeneity, occurring only when actual spending is less than the programs' limits (see Figure 4).[29] To the extent that education spending is endogenous, OLS will overestimate the impact of tax-based aid because education spending and institutional grant aid are negatively related and education spending and tax-based aid are positively related over the endogenous range of spending. Removing this source of bias will decrease the absolute value of the OLS estimates. For private schools, the OLS results are larger in magnitude compared to the IV estimates consistent with this interpretation, although the OLS estimate is not significantly different from the IV results. The results for public schools do not fit this pattern, as the OLS estimate is not larger than the IV estimates. However, I cannot reject a cluster robust test of endogeneity (at the 1 percent level) for either public schools or private schools in Panel A.[30] Therefore, I rely on the IV estimates to address the endogeneity of education spending.

The effect of tax-based aid in Panel A is the average of the institutional response for students eligible for tax-based aid and for ineligible students. However, the institutional

[29] For example, using the maximum spending IV the first-stage regression holds as an identity for students with qualified spending at or above the programs' limits. Roughly 60 percent of public school students and 80 percent of private school students have spending in this range.

[30] I test for endogeneity by calculating the C statistic, defined as the difference in two Sargan statistics. In the case of one endogenous regressor and the null hypothesis of exogeneity, this statistic is distributed chi-square (1). Hayashi (2000) shows that this statistic is equivalent to the Hausman test under conditional homoskedasticity.

response is likely to vary across these student types. The price-discrimination Bennett Hypothesis predicts that colleges and universities will reduce institutional aid to offset the benefits of tax-based aid for eligible students. One possible use for the captured institutional aid is to redirect it towards ineligible students. In this case, the results in Panel A represent the average of institutional aid decreases for eligible students and institutional aid increases for ineligible students. To the extent that this type of aid redistribution occurs, Panel A may overstate the institutional response.[31] To explore this possibility, I limit the sample to eligible students by removing students who would never be eligible for tax-based aid based on the 2003-04 program rules in Panel B of Table 4. In Panel B, I cannot reject a dollar-for-dollar reduction in institutional grant aid for both public and private schools using either of the two instruments. Compared to Panel A, the estimated effects in Panel B are larger, which suggests that colleges and universities do not substantively redirect the captured aid towards ineligible students. In Appendix A, I explore further the possibility that institutions redirect the captured aid to students ineligible for tax-based aid, or towards other institutional expenditures. Unfortunately, these results are inconclusive.

4.4. Further Results

In the baseline results, institutional grant aid includes both non need-based and need-based institutional grants. As Table 5 shows, institutions reduce both of these components in response to tax-based aid.[32] Non need-based aid includes merit-based aid as well as other grant aid

[31] If schools redistribute the captured aid towards ineligible students then the estimated effect of tax-based aid will be more negative on the entire sample compared to the sample of eligible students. To the extent that substantive redistribution occurs, including ineligible students will increase the intercept and will result in a more negative relationship between institutional grant aid and tax-based aid. (Eligible students outnumber ineligible students by roughly 5:1 so that redistributing a large share of the captured aid implies a large increase for ineligible students.)
[32] A small number of observations do not distinguish between need-based and non need-based aid and are not included in Panels A or B of Table 5.

awarded for circumstances not related to financial need. Due to data limitations, I cannot separately consider merit-based aid.[33] The larger reduction in non need-based aid, relative to need-based aid, may be the result of greater discretion financial aid administrators have in awarding non need-based grant aid. The reduction in need-based aid may reflect the belief by financial aid administrators that tax-based aid increases student ability to pay (McPherson and Shapiro 1998). However, differences in non need-based and need-based aid should be interpreted with caution. The definition of what constitutes need-based aid varies across schools (Baum and Lapovsky 2006) and may even change over time for a given school.

Students appear to finance the institutional aid reduction, in part, through increased student loans.[34] These results appear in Panel C of Table 5. Increased borrowing may result from the short-term increase in unmet need in the period after paying education costs but before receipt of tax-based aid. Total loan amounts may also increase if program take up is less than complete, as the results here represent the intention-to-treat effect of tax-based aid. Total loan amounts, including federal Stafford loans and private loans, are estimated to increase $0.42-$0.46 per $1.00 of tax-based aid at public schools using the IV estimates. At private schools, the IV estimates imply an increase of $0.37-$0.43. Although not reported in Table 5, a breakdown of loan types suggests the majority of increased borrowing is from subsidized Stafford loans, the most favorable loan option.[35]

[33] Merit-based aid is not included as a separate category for the 1995-96 school year in the NPSAS.
[34] These loan effects may be counteracted if continuing students use their tax-based aid to finance education in subsequent years and therefore reduce their borrowing. To test this possibility, I estimated the effect of tax-based aid on total loans for first-year students and students in their second year and beyond separately. Equal borrowing effects across years could not be ruled out. Instead of reducing loans in subsequent years, students and families may use their tax-based aid to finance consumption or pay back loans from previous years. Just under one-third of respondents in the Survey of Consumers report that they will "mostly save" their tax refunds from 2008 and 2001 (Shapiro and Slemrod 2003, 2009).
[35] In the case of dependent students, institutional aid reductions and increases in student loan amounts suggest an intra-family transfer. The institutional grant in the student's name is replaced with tax-based aid for the parent and a loan in the student's name.

4.5. Robustness Checks of Price-Discrimination Bennett Hypothesis Results

In Table 6, I demonstrate the robustness of the baseline results in Table 4 in several ways. (To simplify the discussion, I report only the IV results in Table 6, although as in Tables 4 and 5 the corresponding OLS results are similar in magnitude.) First, I address the possibility that changes in state level policies bias the results. Second, I show that the results are not sensitive to the omission of student and family controls. Third, I show that the substantive reduction in institutional grant aid persists using an unbalanced panel of schools. The results in Table 4 are also robust to alternate income controls, including cubic and linear splines with up to 7 knots (the baseline specification uses a cubic spline with 5 knots), and to higher-order polynomial functions of income (not shown). I also find little evidence that there is substantial heterogeneity in the institutional response based on grade level, by institutional selectivity, or by student quality. These results appear in Appendix B.

During the analysis period, several states enacted changes to race-based admission and aid policies. Additionally, several states initiated merit-based aid programs during this period. Concurrent changes in state-level policies could bias the estimated effect of tax-based aid if schools respond to changes in state-based aid programs.[36] Although not reported, I find little evidence that institutional aid responded to changes in state aid in the analysis sample. One reason for this finding may be the difficulty in determining which students experience increases in state aid. Changes to state-based programs could still bias the effect of tax-based aid by altering the composition of enrolled students. To further explore these possible sources of bias, I estimate the effect of tax-based aid after removing states that enacted substantive policy changes.

[36] The average increase in state aid for eligible students is $110 in states enacting merit-based aid compared to $8 for students in the remaining states between the 1999-2000 and 1995-96 school years.

Panel A of Table 6 reports the results after removing three states (CA, MI, TX)[37] that experienced major changes to race-based policies. The results obtained on this limited sample are similar to those reported in Table 4, and suggest that large-scale changes to race-based admissions and aid policies in three populous states do not affect the baseline results. Using sample splits based on minority status and also on gender, I also find that the effect of tax-based aid on institutional aid is similar across these groups of students (not shown). In Panel B of Table 6, I remove 10 states (FL, KY, LA, MD, MI, NV, NM, SC, TN, WV) that enacted merit-based aid programs during the analysis period.[38] The similarity of the results from this limited sample, compared to the baseline results, imply that the introduction of merit-based aid in these states does not impact the baseline results.

If tax-based aid affects the sample of enrolled students, then the estimated effect of tax-based aid may also reflect compositional changes. To the extent that students sort into schools based on the lowest net price (into schools that offset their tax-based aid the least), the bias from a changing composition of enrolled students works against finding a substantive effect. Alternatively, if students select different schools than they would have absent tax-based aid and if these schools are less likely to offer less institutional aid, then the bias is towards negative one. I do not find evidence that tax-based aid affects the composition of enrolled students using a school-year level analysis that estimates the effect of tax-based aid on the share of eligible students (not shown). To further explore the possibility that a changing composition of enrolled students impacts the estimated effect of tax-based aid, I estimate Equation (1) after omitting the

[37] In 1996 the Fifth Circuit Court of Appeals handed down the *Hopwood v. University of Texas* decision and California passed Proposition 209. Also in 1996, a lawsuit was filed in Michigan challenging race-based admissions. In 2003, after several lower court rulings and appeals in this lawsuit, *Grutter v. Bollinger,* the Supreme Court ruled that race could be used as a "plus factor" in the admission decision, but it can't be the only factor schools consider, and schools can't have a quota system for race.

[38] See Dynarski (2004) Table 2.1. I also tried removing an additional three states that enacted merit-based aid programs in an earlier period (AR, GA, MS) and the results were similar to those in Panel B.

following controls: student demographic variables (race, age gender, dependency status); parent characteristics (education level, marital status); and family size. If the effect of these control variables is changing over time due to compositional effects of tax-based aid, then omitting them from the analysis may lead to a different estimated effect of tax-based aid. The similarity of the results without the control variables in Panel C of Table 6, compared to the baseline results, suggests that the impact of the observable control variables is not changing over time in a way that is correlated with tax-based aid. This may be interpreted as evidence that compositional effects do not substantively impact the baseline results.

I also find a substantial reduction in institutional grant aid using an unbalanced panel of 4-year schools as shown in Panel D of Table 6. The unbalanced panel includes institutions that appear in the NPSAS in the 1995-96 school year and at least one of the later school years (1999-2000, 2003-04).[39] Enrollment in these schools represents a considerable share of national 4-year enrollment: 57 percent in the 1995-96 school year, 53 percent in the 1999-2000 school year and 48 percent in the 2003-04 school year. In Panel D, the impact of tax-based aid is identified from either the introduction of the programs (schools that appear in 1995-96 and 1999-200 only), the total change in the programs (schools that appear in 1995-96 and 2003-04 only), or from the introduction and expansion of tax-based aid (schools that appear in all three school years). In contrast, for each school in the balanced panel, the institutional response is identified from both the introduction and expansion of tax-based aid.

IV. Conclusion

I demonstrate the importance of benefit incidence analysis by showing that the intended cost reductions of tax-based federal student aid are substantially counteracted by reductions in

[39] The number of public (private) schools in the unbalanced panel (rounded to the nearest 10) is 200 (150) for 1995-96, 170 (110) for 1999-2000 and 140 (100) for 2003-04.

institutional grant aid for a sample of 4-year colleges and universities. I find that students at these schools cope with this reduction of institutional support by increasing student loan amounts. Together, these findings imply that students eligible for tax-based aid are not directly benefitting from the programs in the sense of realizing a lower cost of postsecondary attendance. Rather, they may be evidence that institutions and the student loan industry realize financial gains from tax-based aid at the expense of eligible students. Given that schools in the primary analysis sample represent about 25 percent of all students enrolled at degree-granting postsecondary institutions, the results here imply that a substantial share of total tax-based aid expenditures do not reduce costs for students and their families. In aggregate, more than 37 million students claimed one of the programs during the analysis period for a total tax expenditure of $29 billion (Baum and Steele 2007).

The results here suggest several areas for future work. First, it is unclear how institutions utilize the captured resources. The two possibilities considered here, that schools redirect the captured aid towards students ineligible for tax-based aid, or into other institutional expenditures are inconclusive. Clarifying this aspect of the institutional response is a necessary step for determining the ultimate incidence of tax-based aid. Eligible students may partially benefit from tax-based aid if institutions devote the captured resources to increasing education quality, providing student services, or other expenditures valued by students.

Second, the results here do not characterize the response of all postsecondary institutions. While enrollment at schools in the sample represents roughly 40 percent of national 4-year enrollment, schools in the sample have larger enrollments and are more selective than 4-year schools nationally. The sample also excludes students enrolled at 2-year institutions. Less selective 4-year schools and 2-year schools generally offer less institutional aid compared to

more selective 4-year schools, so that the effects found here are likely to be an upper bound of the institutional price response. Understanding the pricing behavior of the remaining school types is an important avenue for future research.

Third, the results suggest that institutions may also offset the intended cost reduction of other direct student aid programs targeting middle-income students and families, such as the recently enacted American Opportunity Tax Credit. Yet, it is not clear if the crowd out of institutional aid similarly undermines traditional student aid programs targeting lower-income students. To the extent that the reduction of institutional grant aid holds for other forms of student aid, previous studies may have underestimated the price sensitivity of postsecondary enrollment by presuming greater than achieved cost reductions. More generally, the results underscore the need for consideration of benefit incidence in the context of other government benefit programs.

NOT FOR PUBLICATION

Appendix

A1. Institutional Aid Redistribution

A1.1 Measuring Institutional Aid Redistribution

An increase in institutional aid for ineligible students may be evidence that schools redistribute institutional aid. I consider this possibility by estimating how much of the total institutional aid withheld from eligible students is redistributed to ineligible students using Equation (A1).

$$\text{(A1)} \quad InstitutionalAid_{ijt} = \pi_1 \left(\frac{\sum_{i \in j}^{Enrollment_t} TBA_{it}\left(S_{ijt}, I_{it}, \tau_{it}\right)}{IneligibleEnrollment_{jt}} \right) + \pi_2 X_{ijt} + \alpha_j + \varepsilon_{ijt}$$

X_{ijt} contains similar controls as in Equation (1). The key independent variable is $\left(\frac{\sum_{i \in j}^{Enrollment_t} TBA_{it}\left(S_{ijt}, I_{it}, \tau_{it}\right)}{IneligibleEnrollment_{jt}} \right)$, the total value of tax-based aid, per ineligible student, received at school *j* in year *t*. It represents the total amount of institutional aid available for redistribution if schools reduce institutional aid dollar-for-dollar with tax-based aid. The parameter π_1 measures the share of this total that is redistributed. A positive estimate for π_1 in Equation (A1) combined with a negative estimate of β_1 in Equation (1) is consistent with the redistribution of institutional aid away from eligible students towards ineligible students. Complete redistribution is implied by $\pi_1 = 1$ and $\beta_1 = -1$.

Total tax-based aid per ineligible student may be endogenous in Equation (A1) for several reasons. First, similar to the case of eligible students, qualified spending may be endogenous. School-wide changes in institutional aid could affect both the tax-based aid of

eligible students, through qualified spending, and the institutional aid of ineligible students. Second, enrollment may be affected by institutional aid redistribution, so that the number of ineligible students and the total value of tax-based aid at a given school may both be endogenous. To address these concerns, I exploit the timing of tax-based aid implementation to generate plausibly exogenous instruments. In place of contemporaneous values of spending and enrollment, I use values from the 1995-96 school year. This approach isolates the policy-induced variation in the tax-based aid function while holding fixed the composition of students from the 1995-96 school year. Spending and enrollment in this year should be free from the endogenous responses expected in later periods. Paralleling the approach used in the text, I also estimate the total value of tax-based aid based on maximum spending. Equation (A2) gives the first-stage regression using the IV based on actual spending in the 1995-96 school year:

$$\text{(A2)} \qquad \left(\frac{\sum_{i \in j}^{Enrollment_t} TBA_{it}\left(S_{ijt}, I_{it}, \tau_{it}\right)}{IneligibleEnrollment_{jt}} \right) = \gamma_1 \left(\frac{\sum_{i \in j}^{Enrollment_{96}} TBA_{it}\left(S_{ij96}, I_{i96}, \tau_{i96}\right)}{IneligibleEnrollment_{j96}} \right) + \gamma_2 X_{ijt} + \alpha_j + \eta_{ijt}$$

where $\left(\frac{\sum_{i \in j}^{Enrollment_{96}} TBA_{it}\left(S_{ij96}, I_{i96}, \tau_{i96}\right)}{IneligibleEnrollment_{j96}} \right)$ is the total value of the tax-based aid received at school *j* in year *t* based on the enrollment characteristics in the 1995-96 school year.

I also explored the possibility that schools translate large total tax-based aid receipt into increased expenditures in other categories. Unfortunately, the NPSAS has little information on other types of expenditures and changes in accounting practices during the analysis period make expenditures from other sources difficult to compare across years (Budack 2000; IPEDS Data Center; Wellman, Desrochers and Lenihan 2008). For categories that may be comparable, I

linked the NPSAS data to expenditure data from the Integrated Postsecondary Education Data System (IPEDS) from the National Council of Education Statistics at the school-year level to estimate if schools translate large total tax-based aid receipt into increases in other expenditures. These estimates are imprecise, and combined with the data quality concerns, offer little insight into this possibility.

A1.2. Institutional Aid Redistribution Results

The redistributive results for ineligible students are largely inconclusive because the approach suffers from weak instruments. As shown in Panel A of Table A1, neither of the instruments perform well in the first stage. (F-tests on the restriction that the instrument is zero in the first stage range from 2.01 to 6.52.) The weakness of the instrument may be the result of the limited sources of variation in total tax-based aid at the school-year level after including both school fixed effects and flexible time controls. In Panel B, I replace the time controls (year indicator variables and interactions of year indicators with indicator variables for institutional selectivity) with a squared time trend. When this step is taken, the instrument performs better in the first stage, although it is still weak for private schools. (Making this replacement has no effect on the baseline results reported in Table 4.) At public schools, the estimates in Panel B suggest at most a modest amount of redistribution. A necessary condition for complete redistribution, $\pi_1 = 1$, is unlikely in this case. However, as these results are not robust to the flexible time controls in Panel A, the redistributive consequences are unclear even for public schools. Results using a sample of only low-income ineligible students or only high-income ineligible students are also inconclusive.

B1. Heterogeneity in the Price-Discrimination Bennett Hypothesis by Grade Level, Institutional Selectivity and Student Ability

Both entering and continuing students experience nearly complete crowd out of institutional grant aid. Table B1 reports the results for first year students and students in years two through five separately. For both public and private schools, the estimated reduction in grant aid is larger for first year students compared to continuing students. Entering students have no history of institutional aid offers, and therefore do not observe an aid offer that may represent their counterfactual level of aid. This information asymmetry may provide schools with increased pricing power for entering students relative to continuing students, some of whom have a history of institutional aid offers. However, the differences across first year students in Panel A and continuing students in Panel B are not substantively large, nor are they statistically significant, so that this implication is unclear. One explanation for the similar effects across grade levels is the transaction costs associated with transferring schools. As a result of these costs, both academic and financial, schools may also exert pricing power over continuing students.

The reduction of institutional grant aid holds for both more selective and less selective institutions. Panel A of Table B2 shows the results based on institutional selectivity.[40] The point estimates suggest that the reduction in institutional aid is larger at more selective institutions, compared to less selective ones. This pattern may reflect the market structure for selective schools. If more selective institutions have fewer direct competitors and/or larger excess demand for enrollment, compared to less selective institutions, then the price response should be relatively larger at more selective institutions. Due to sample size considerations, I combine

[40] Institutional selectivity is defined by NPSAS categories. Most selective includes "most" and "very" selective, while less selective includes "moderately" and "minimally" selective as well as "open admissions." There are very few "open admissions" schools and the results are the same if these schools are removed.

public and private schools when considering institutional selectivity.[41] However, because relatively more private schools are in the most selective category, the comparison across institutional selectivity may also be partially attributable to the differences in public and private school types.

The reduction of institutional grant aid holds for both high and low ability students. Using SAT scores to determine student ability, I define above (below) average students as those with combined math and verbal SAT scores that are above (below) the average scores at their school in a given school year.[42] Unfortunately, in the NPSAS many student records do not include valid SAT scores. Roughly 55 percent of students in the analysis sample have valid scores, and I am forced to condition the sample to these students in order to explore student ability implications.[43] As shown in Panel B of Table B2, I cannot reject the possibility that tax-based aid is substantively offset for both high ability and low ability students. While the point estimates imply a larger effect for higher ability students, the differences across high and low ability students are not significant.

[41] Kezdi (2004) shows that cluster robust standard errors allow for accurate inference when the number of clusters exceeds 50. Given the distribution of selectivity across school types, I am forced to combine public and private schools to create school quality groups that meet this requirement.
[42] I also tried different cuts of the data, using the 75th and 25th percentiles to define three student quality groups. The results from this analysis are similar to the results reported in the text.
[43] Using a specification similar to Equation (1) but with an indicator variable for valid SAT scores as the dependent variable, I find no evidence of a substantive or significant relationship between having valid SAT scores and tax-based aid, suggesting that tax-based aid does not affect SAT reporting. Missing SAT scores appears to be distributed evenly across grade level and public and private school types.

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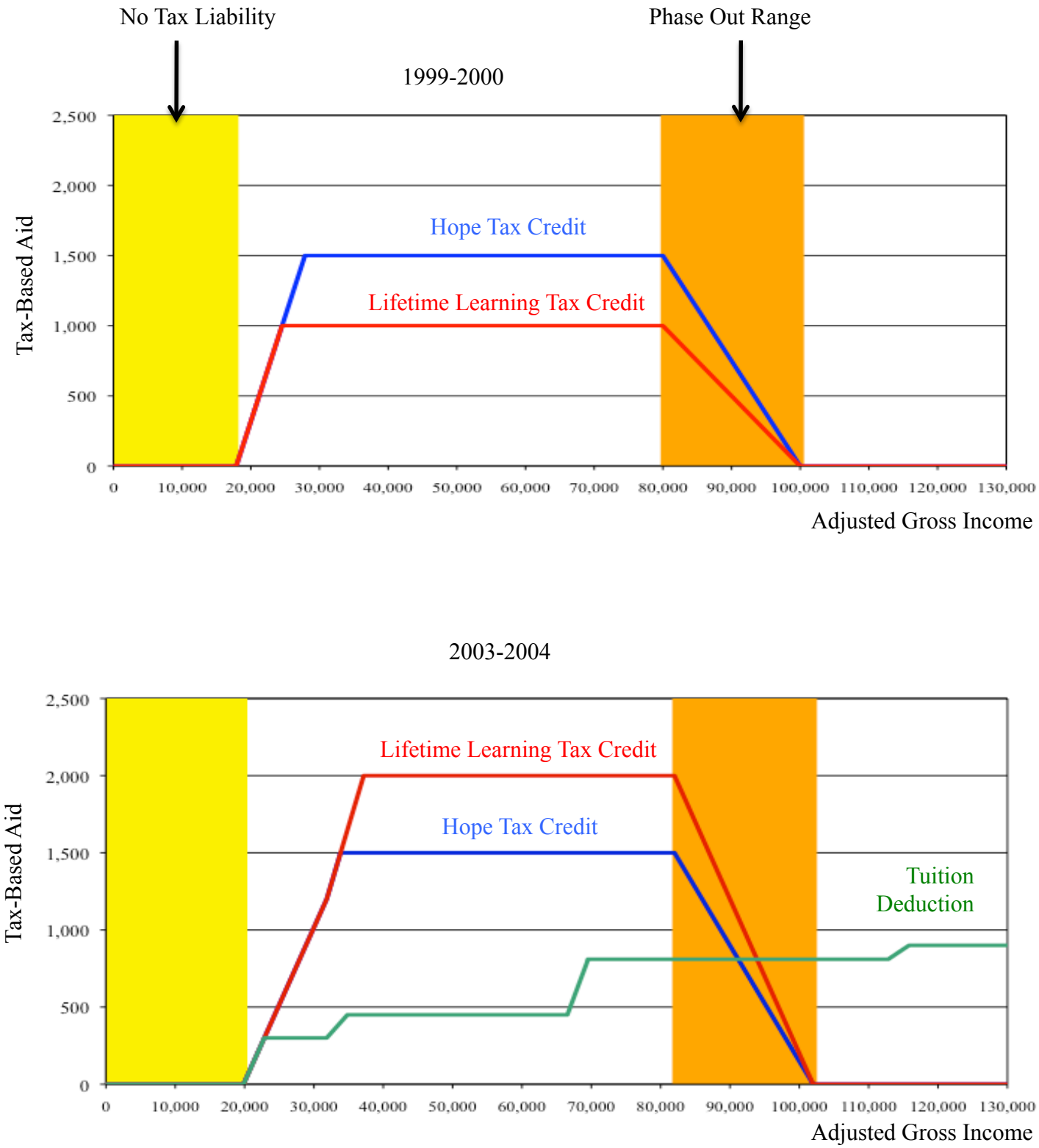


Figure 1

Maximum Tax-Based Aid for Joint-Filing Married Family of Four with one College Student

Notes: Tax liability is estimated using only the standard deduction and personal exemptions.
See Section 2 for a description of tax-based aid.

Changes 1999-2000 and 1995-96 School Years

Changes 2003-04 and 1999-2000 School Years

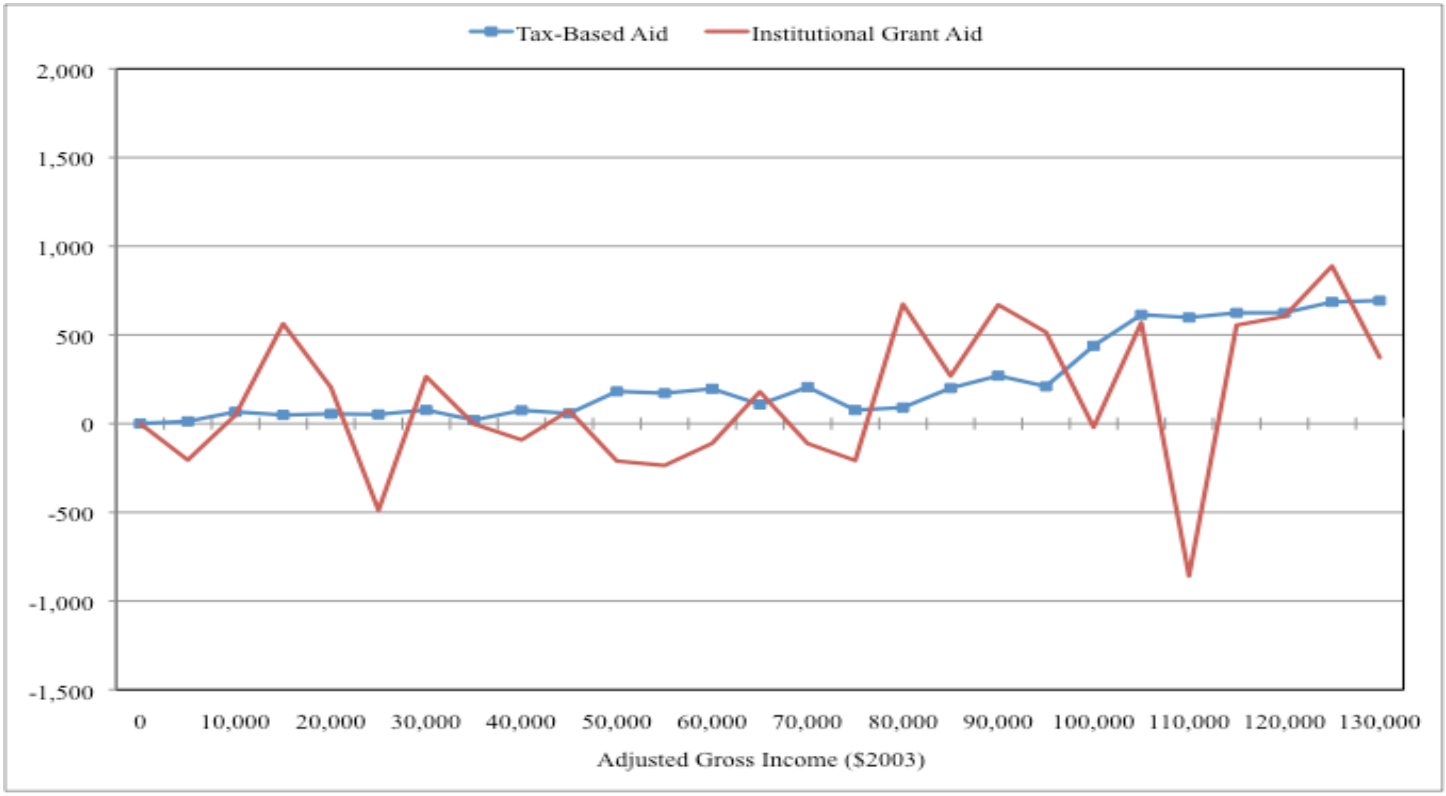
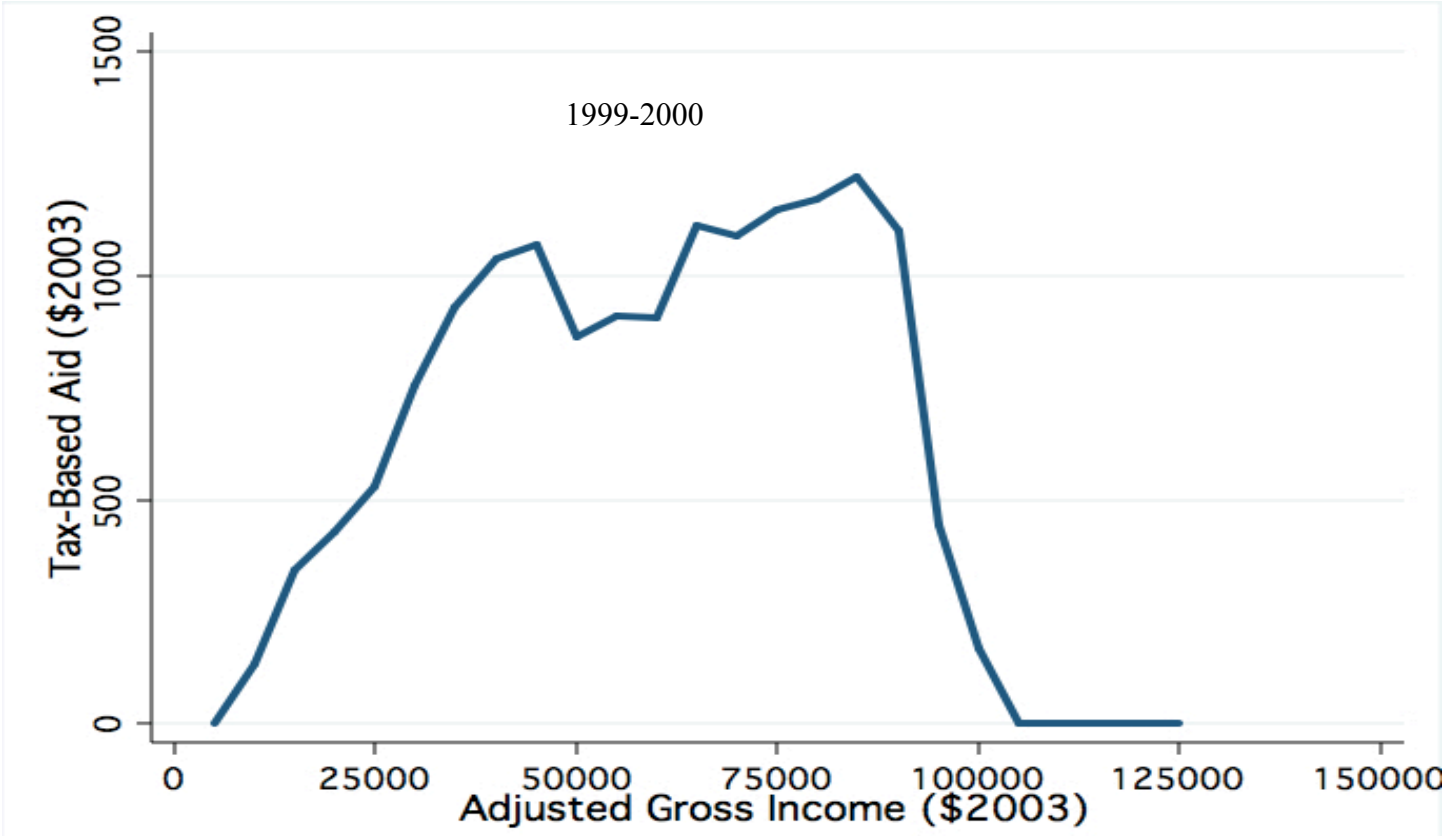


Figure 2

Changes in Tax-Based Aid and Institutional Grant Aid by Adjusted Gross Income

Notes: The value of tax-based aid is calculated from program rules (IRS 970) and income and education spending data from the NPSAS. Institutional aid is from the NPSAS data. See Section 4.1 for details.



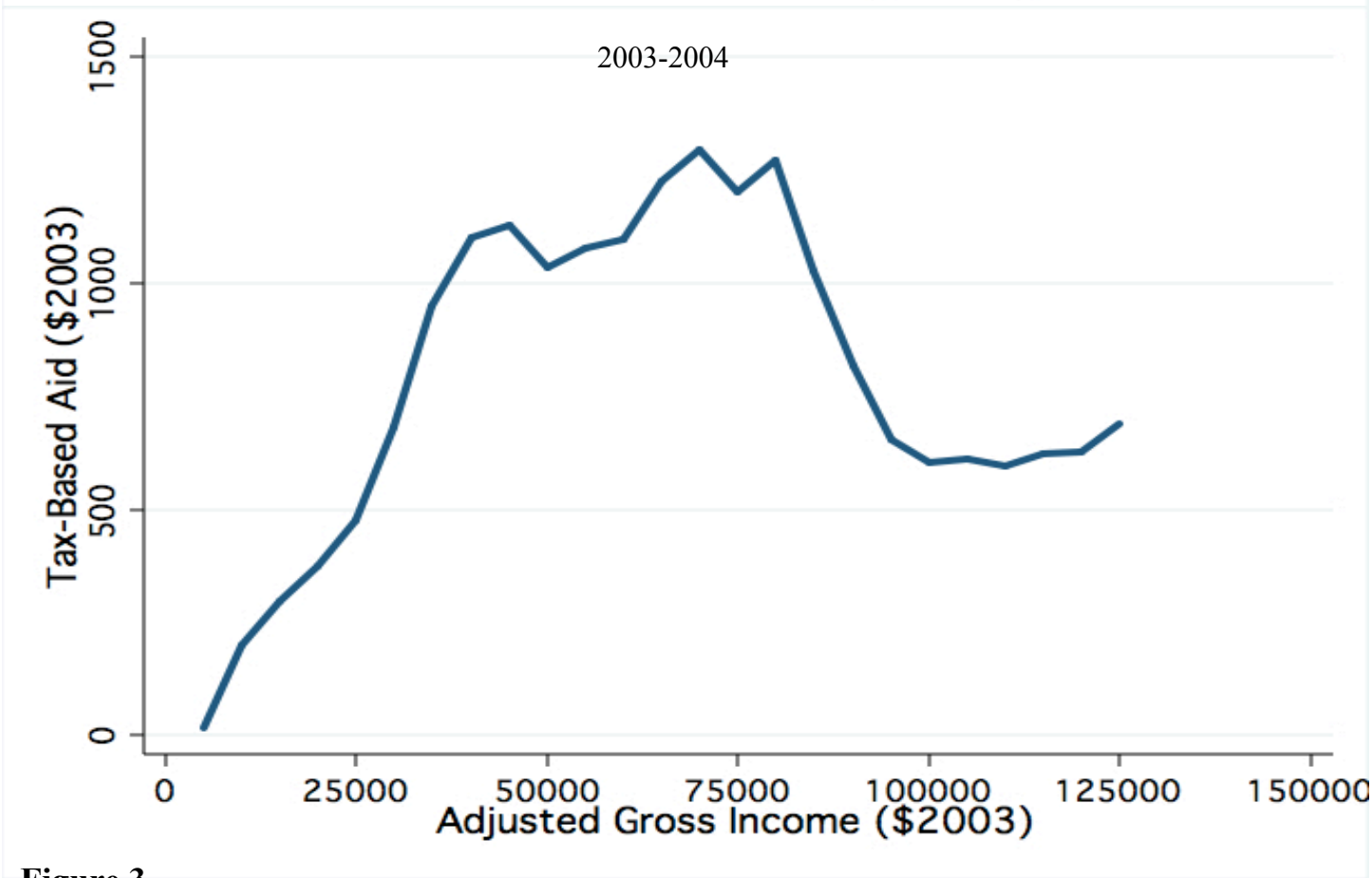


Figure 3

Average Tax-Based Aid Eligibility by Adjusted Gross Income

Notes: The value of tax-based aid is calculated from program rules (IRS 970) and income and education spending data from the NPSAS. See Section 4.1 for details.

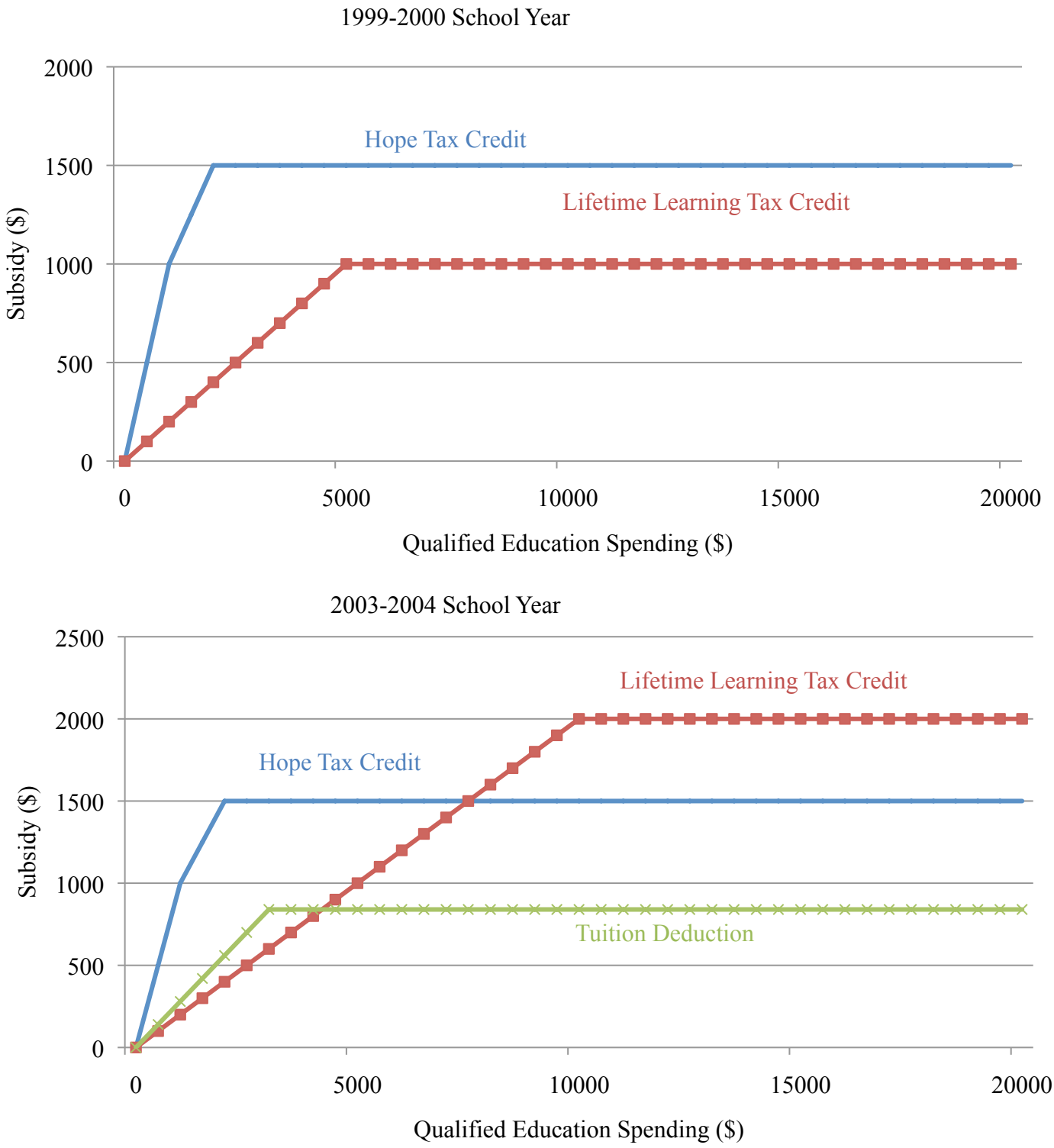
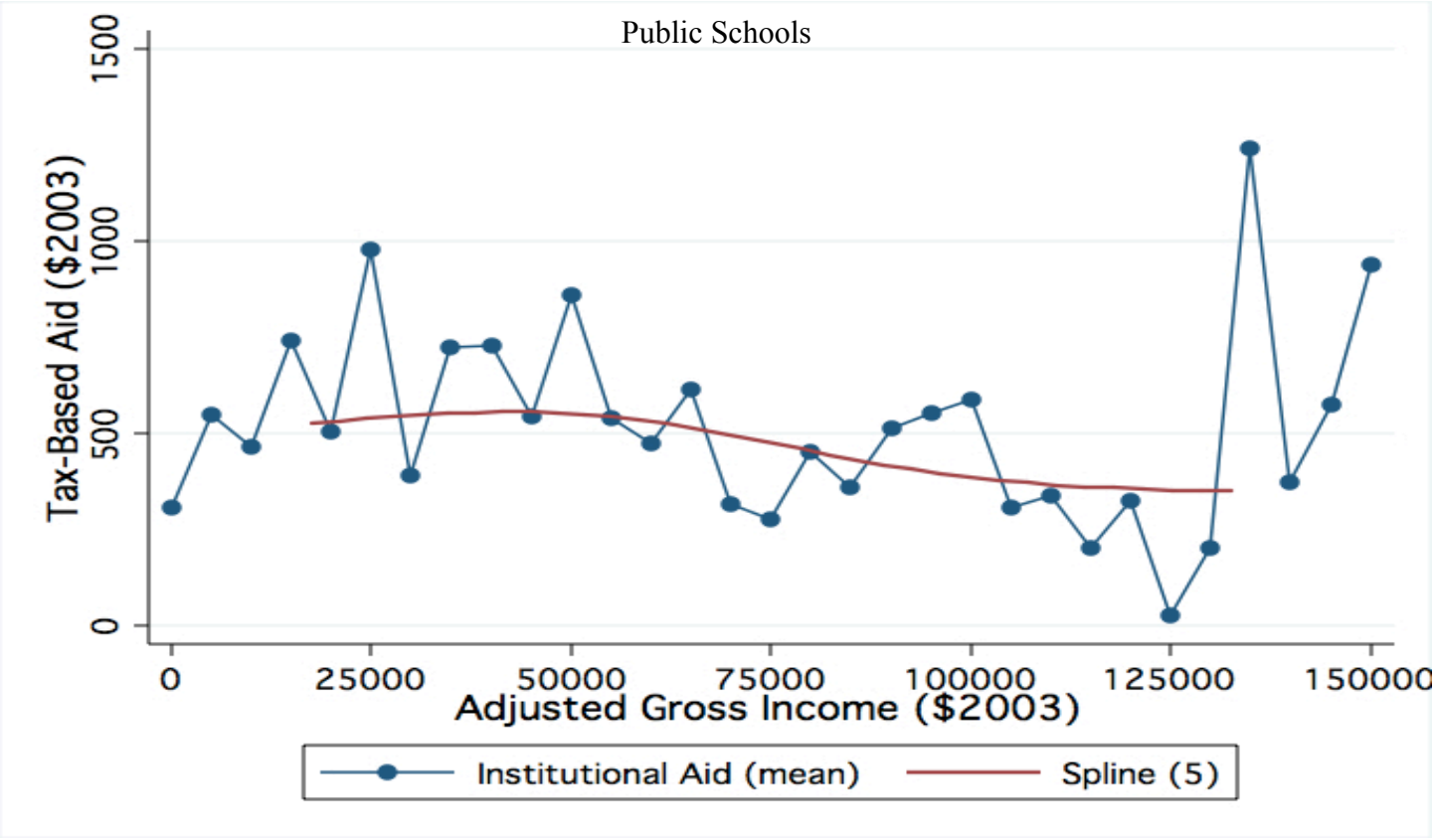


Figure 4

Value of Tax-Based Aid by Education Spending

The value of the programs also depend on taxes owed. The values shown here assume a tax liability as least as large as the subsidy. The value of the Tuition Deduction also depends on the marginal tax rate. The value shown uses a 28-percent marginal rate.



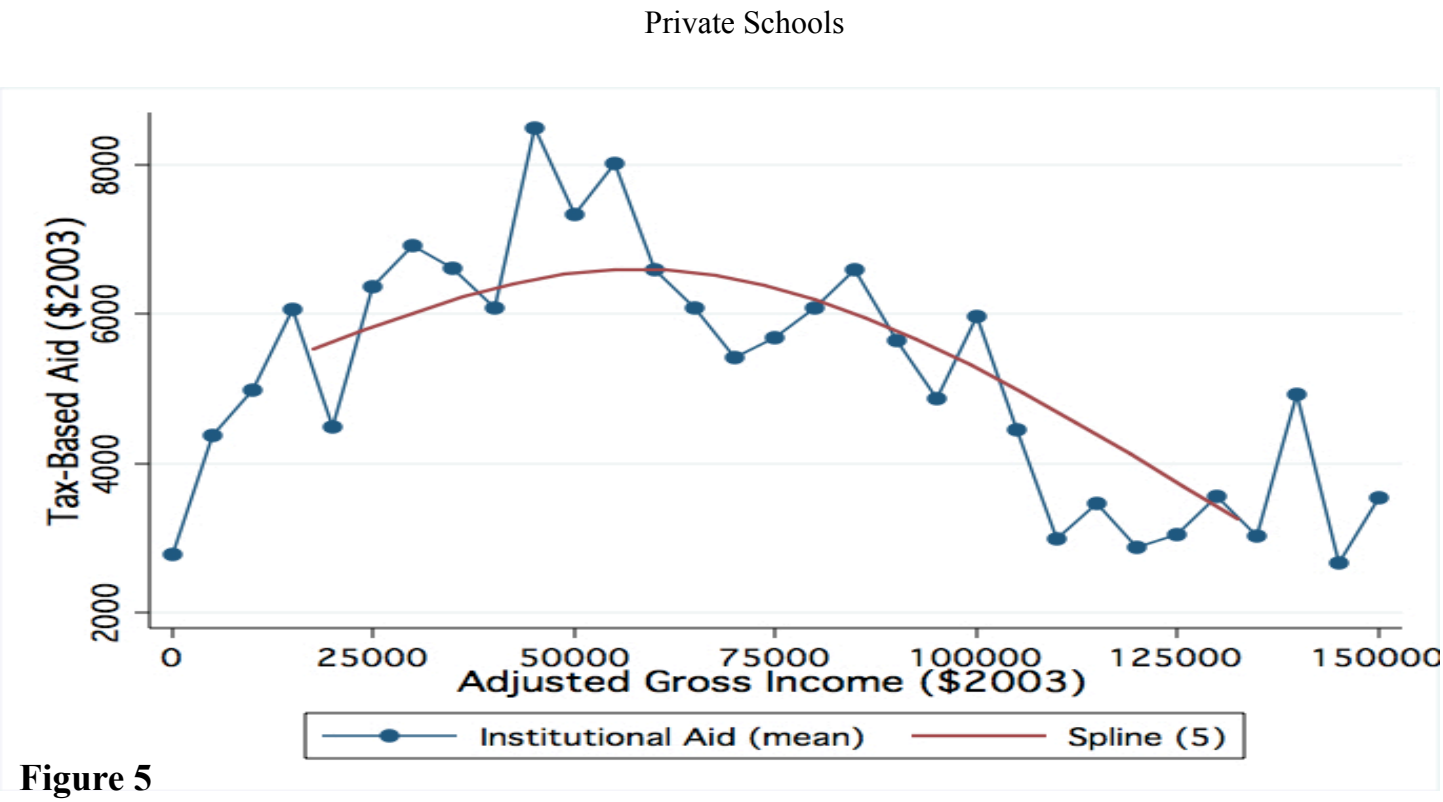


Figure 5

Institutional Aid by Income Group, 1995-96

Notes: These figures show average institutional aid, and the fitted values using a cubic spline function with five knots. This is the same spline function used in Equation (1) that estimates institutional aid effects at the student level. Income groups are based on $5,000 increments with the top group including all observations with income $150,000 or more.

Table 1

Tax-Based Aid Program Details, 1998-2003

	Hope Tax Credit	Lifetime Learning Tax Credit	Tution and Fees Deduction
Expenses Covered	Tuition and required fees at an educational instituion eligible for Department of Education student aid programs. Expenses covered do not include medical expenses, room and board, transportation, insurance and are net of scholarships, Pell Grants or any other tax free funds used to pay education expenses.		
Adjusted Gross Income Eligibility	1998-2001: Full credits for single (joint) returns less than $40,000 ($80,000). Credits linearly phased out for single (joint) returns until $50,000 ($100,000). 2002: Limits changed to $41,000 ($82,000) and $51,000 ($102,000) for single (joint) returns. 2003: Limits changed to $83,000 and $103,000 for joint returns.		Single filers with less than $65,000. Married couples must file a joint return less than $130,000.
Amount	100 percent of first $1,000 plus 50 percent of the next $1,000 of qualified education spending. Max credit $1,500 per student.	1998-2002: 20 percent of first $5,000. Max credit $1,000 per return. 2003: 20 percent of first $10,000. Max credit $2,000 per return.	100 percent of first $3,000 of qualified education spending per return. Value to student/family depends on marginal tax rate.
Recipient Eligibility	Only available for two tax years for students in the first two years of postsecondary education. Must be enrolled at least half-time, pursuing a degree or credential and student cannot have a felony drug conviction.	Undergraduate, graduate, vocational education and job skills programs. Available for an indefinite number of years. Lack of a felony drug conviction does not apply.	
Start Date	January 1, 1998	July 1, 1998	Janury 1, 2002

Source: IRS Publication 970 "Tax Benefits for Education" Various Years.

Table 2

Means of Student Aid and Demographic Data by School Year and Institutional Control

School Year	*1995-1996*		*1999-2000*		*2003-2004*	
School Type	Public	Private	Public	Private	Public	Private
Tuition and Student Aid						
Tuition	3,752	16,588	3,998	17,553	5,115	20,174
Institutional Aid	550	5,667	643	6,287	807	6,242
Tax-Based Aid*	0	0	586	634	682	869
Federal Grant Aid	1,291	2,171	1,208	2,310	1,884	3,246
Federal Campus-Based Aid	435	1,518	315	1,262	468	1,325
State Aid	409	855	480	845	640	911
Student and Family Characteristics						
Family Income	50,892	61,723	57,395	68,108	63,297	75,791
Dependent Student	84.33	90.58	79.61	87.16	85.72	92.31
Age	20.40	20.06	21.46	21.01	20.69	20.16
Black	11.49	9.68	10.94	12.86	11.81	9.58
Hispanic	6.58	9.29	7.75	9.35	7.29	10.37
Female	54.68	53.26	59.12	59.13	56.42	57.46
Student Married	4.45	3.45	4.30	3.47	2.11	1.18
Median School Enrollment	23,746	12,707	22,082	11,529	23,755	12,403
Number of Schools	120	70	120	70	120	70

Data from the National Postsecondary Student Aid Study (NPSAS) 1995-96, 1999-2000 and 2003-04.

*Tax-Based Aid is calculated as the value of the eligible award. By definition it is equal to zero for the 1995-96 school year. See Section 4.1 for details.

The number of schools is rounded to the nearest 10 to comply with the Department of Education confidentiality statutes.

All dollar amounts are in 2003 dollars.

Table 3

National Enrollment and Enrollment in Analysis Sample

School Year	*1995-1996*	*1999-2000*	*2003-2004*
National Enrollment			
4-year Public	5,806,036	5,969,950	6,649,441
4-year Private	2,998,157	3,228,575	3,767,806
Total 4-year	8,804,193	9,198,525	10,417,247
2-year Public	5,314,463	5,339,449	6,209,257
2-year Private	248,864	253,250	284,977
Total 2-year & 4-year	14,367,520	14,791,224	16,911,481
Enrollment in Institutions in the Analysis Sample			
4-year Public	2,571,280	2,704,230	3,146,390
4-year Private	784,610	793,810	929,390
Total 4-year	3,355,890	3,498,040	4,075,780
Ratio of Sample Enrollment to National Enrollment			
Sample/National 4-year	38	38	39
Public Sample/National 4-year Public	44	45	47
Private Sample/National 4-year Private	26	25	25
Total Sample/Total National 2-year & 4-year	23	24	24

National enrollment from the Digest of Education Statistics: 2009, Table 190, National Center for Education Statistics and includes students enrolled at institutions granting at least an associate's degree and whose students are eligible for Title IV federal aid. Sample enrollment calculated from the annual enrollment for the schools included in the primary analysis sample from the National Postsecondary Student Aid Study and is rounded to the nearest 10 to comply with the U.S. Department of Education confidentiality statutes.

Table 4

Estimated Tax-Based Aid Effect on Institutional Grant Aid

	(1)	(2)	(3)	(4)	(5)	(6)
School Type	Public	Public	Public	Private	Private	Private
Estimator	OLS	Maximum Spending IV	Simulated Spending IV	OLS	Maximum Spending IV	Simulated Spending IV
			Panel A: Entire Sample of Students			
Tax-Based Aid	-0.828	-0.894	-0.819	-1.203	-0.905	-0.685
	[0.075]	[0.083]	[0.089]	[0.228]	[0.235]	[0.278]
R^2	0.081	0.081	0.081	0.188	0.187	0.186
Sample Size	51,800	51,800	51,800	22,480	22,480	22,480
Number of Schools	120	120	120	70	70	70
First Stage Results						
F (instrument)		8,343	2,003		7,847	1,985
Partial R^2		0.80	0.58		0.91	0.77
C-statistic		9.23	4.35		16.33	10.84
			Panel B: Eligible Student Sample			
Tax-Based Aid	-1.035	-1.158	-1.081	-1.846	-1.449	-1.202
	[0.091]	[0.103]	[0.112]	[0.311]	[0.317]	[0.371]
R^2	0.109	0.108	0.109	0.189	0.188	0.187
Sample Size	41,210	41,210	41,210	17,350	17,350	17,350
Number of Schools	120	120	120	70	70	70
First Stage Results						
F (instrument)		6,403	1,188		6,155	886
Partial R^2		0.75	0.52		0.87	0.69
C-statistic		16.99	1.25		16.34	9.09

Sample sizes and the number of schools are rounded to the nearest 10 to comply with the Department of Education confidentiality statutes.
Panel A includes the entire sample of students. Panel B limits the sample to students who would be eligible for tax-based aid based on 2003-04 program rules.
The estimates use tax-based aid based on maximum spending or on simulated spending as the instrument. See Section 4.2 for details.
The F-test is for the restriction that the excluded instrument is zero.
The C-statistic is distributed chi-squared (1) under the null hypothesis that all regressors are exogenous.
Control variables include school fixed effects, student characteristics (race, age, gender, dependency status), family income (cubic spline), parent/guardian controls (education and marital status), family controls (size, home Census Division), time (year indicator variables and interactions of year indicators with indicators for institutional selectivity).
Controls are also included for other forms of aid (Pell Grants, federal campus-based aid, state aid) at both the student level and as the school-year average values.
Standard errors, clustered at the school level, are reported in brackets.

Table 5

Estimated Tax-Based Aid Effect on Non Need-Based and Need-Based Institutional Grant Aid and Student Loan Amounts

	(1)	(2)	(3)	(4)	(5)	(6)
School Type	Public	Public	Public	Private	Private	Private
Estimator	OLS	Maximum Spending IV	Simulated Spending IV	OLS	Maximum Spending IV	Simulated Spending IV
Panel A: Non Need-Based Institutional Grant Aid						
Tax-Based Aid	-0.510	-0.551	-0.553	-0.690	-0.512	-0.469
	[0.059]	[0.066]	[0.068]	[0.177]	[0.182]	[0.184]
R^2	0.055	0.055	0.055	0.069	0.069	0.069
Sample Size	51,600	51,600	51,600	22,240	22,240	22,240
Number of Schools	120	120	120	70	70	70
First Stage Results						
F (instrument)		8,321	1,997		7,847	2,017
Partial R^2		0.80	0.58		0.91	0.77
C-statistic		7.90	4.50		11.32	12.09
Panel B: Need-Based Institutional Grant Aid						
Tax-Based Aid	-0.264	-0.286	-0.233	-0.456	-0.339	-0.322
	[0.045]	[0.050]	[0.049]	[0.180]	[0.195]	[0.202]
R^2	0.073	0.073	0.073	0.203	0.202	0.202
Sample Size	51,600	51,600	51,600	22,240	22,240	22,240
Number of Schools	120	120	120	70	70	70
First Stage Results						
F (instrument)		8,321	1,997		7,846	2,017
Partial R^2		0.80	0.58		0.91	0.77
C-statistic		3.95	3.94		5.45	6.71
Panel C: Student Loan Amounts						
Tax-Based Aid	0.556	0.416	0.464	0.515	0.365	0.426
	[0.079]	[0.086]	[0.114]	[0.127]	[0.123]	[0.142]
R^2	0.147	0.147	0.147	0.112	0.112	0.112
Sample Size	51,800	51,800	51,800	22,480	22,480	22,480
Number of Schools	120	120	120	70	70	70
First Stage Results						
F (instrument)		7,756	1,863		7,934	2,050
Partial R^2		0.79	0.57		0.91	0.77
C-statistic		12.07	2.88		11.64	4.66

Sample sizes and the number of schools are rounded to the nearest 10 to comply with the Department of Education confidentiality statutes.

The estimates use tax-based aid based on maximum spending or on simulated spending as the instrument. See Section 4.2 for details.

The F-test is for the restriction that the excluded instrument is zero.

The C-statistic is distributed chi-squared (1) under the null hypothesis that all regressors are exogenous.

Control variables include school fixed effects, student characteristics (race, age, gender, dependency status), family income (cubic spline), parent/guardian controls (education and marital status), family controls (size, home Census Division), time (year indicator variablesand interactions of year indicators with indicators for institutional selectivity).

Controls are also included for other forms of aid (Pell Grants, federal campus-based aid, state aid) at both the student level and as the school-year average value. Standard errors, clustered at the school level, are reported in brackets.

Table 6

Robustness Checks of Tax-Based Aid Effect on Institutional Grant Aid

	(1)	(2)	(3)	(4)
School Type	Public	Public	Private	Private
Instrument	Maximum Spending	Simulated Spending	Maximum Spending	Simulated Spending
Panel A: Remove States with Changing Race-Based Aid/Admission Policies				
Tax-Based Aid	-0.859	-0.828	-0.909	-0.768
	[0.095]	[0.100]	[0.260]	[0.288]
R^2	0.081	0.082	0.186	0.185
Sample Size	43,370	43,370	19,060	19,060
Number of Schools	110	110	70	70
F (instrument)	6,537	1,969	8,249	2,033
Panel B: Remove States Introducing Merit-Based Aid				
Tax-Based Aid	-0.924	-0.832	-0.866	-0.646
	[0.095]	[0.103]	[0.229]	[0.275]
R^2	0.089	0.089	0.188	0.187
Sample Size	43,030	43,030	19,130	19,130
Number of Schools	120	120	70	70
F (instrument)	7,864	1,630	6,131	1,883
Panel C: Remove Parent/Family Controls				
Tax-Based Aid	-0.869	-0.811	-1.022	-0.846
	[0.081]	[0.086]	[0.243]	[0.293]
R^2	0.063	0.063	0.155	0.154
Sample Size	51,780	51,780	22,480	22,480
Number of Schools	120	120	70	70
F (instrument)	8,471	2,380	9,367	2,053
Panel D: Unbalanced Panel of Schools				
Tax-Based Aid	-0.801	-0.728	-0.766	-0.510
	[0.071]	[0.073]	[0.203]	[0.248]
R^2	0.081	0.081	0.172	0.132
Sample Size	71,860	71,860	37,960	37,960
Number of Schools	200	200	150	150
F (instrument)	8,526	3,002	7,070	3,230

Sample sizes and the number of schools are rounded to the nearest 10 to comply with the Department of Education confidentiality statutes.

Panel A removes students from three states (CA, MI and TX).

Panel B removes students from 10 states (FL, KY, LA, MD, MI, NV, NM, SC, TN, WV).

Panel C removes student demographic varaibles and parent/family controls from the primary (balanced panel) sample.

Panel D uses an unbalanced panel of schools that appear in the 1995-96 school year and one of the later (1999-2000, 2003-04) school years.

The F-test is for the restriction that the excluded instrument is zero.

Control variables include school fixed effects, student characteristics (race, age, gender, dependency status), family income (cubic spline), parent/guardian controls (education and marital status), family controls (size, home Census Division), time (year indicator variables and interactions of year indicators with indicators for institutional selectivity). Controls are also included for other forms of aid (Pell Grants, federal campus-based aid, state aid) at both the student level and as the school-year average value.

Standard errors, clustered at the school level, are reported in brackets.

Table A1

Institutional Aid Redistribution for Ineligible Students

School Type	(1) Public	(2) Public	(3) Private	(4) Private
Instrument	Maximum Spending	Simulated Spending	Maximum Spending	Simulated Spending
		Panel A: Flexible Time Controls		
Total Tax-Based Aid	0.147	0.141	1.963	1.751
per Ineligible Student	[0.119]	[0.102]	[1.608]	[1.329]
R^2	0.038	0.033	0.217	0.211
F (instrument)	3.85	6.52	2.03	2.01
Sample Size	10,590	10,590	5,130	5,130
Number of Schools	120	120	70	70
		Panel B: Time Trend Controls		
Total Tax-Based Aid	0.066	0.056	0.737	0.647
per Ineligible Student	[0.067]	[0.044]	[0.415]	[0.348]
R^2	0.031	0.033	0.115	0.137
F (instrument)	11.29	24.61	5.88	5.05
Sample Size	10,590	10,590	5,130	5,130
Number of Schools	120	120	70	70

Sample sizes and the number of schools are rounded to the nearest 10 to comply with the Department of Education confidentiality statutes.
The sample is limited to students who would be ineligible for tax-based aid based on 2003-04 program rules.
Panel A includes year indicator variables and interactions of year indicators with indicator variables for institutional selectivity.
Panel B uses a squared time trend.
The instrument uses enrollment from the 1995-96 school year and spending from either the 1995-96 school year (simulated spending), or from program limits (maximum spending). See Appendix for details. The F-test is for the restriction that the excluded instrument is zero.
Control variables include school fixed effects, student characteristics (race, age, gender, dependency status), family income (cubic spline), parent/guardian controls (education and marital status), family controls (size, home Census Division), time (year indicator variables and interactions of year indicators with indicators for institutional selectivity).
Controls are also included for other forms of aid (Pell Grants, federal campus-based aid, state aid) at both the student level and as the school-year average value.
Standard errors, clustered at the school level, are reported in brackets.

Table B1

Estimated Tax-Based Aid Effect on Institutional Grant Aid by Grade Level

	(1)	(2)	(3)	(4)	(5)	(6)
School Type	Public	Public	Public	Private	Private	Private
Estimator	OLS	Maximum Spending IV	Simulated Spending IV	OLS	Maximum Spending IV	Simulated Spending IV
			Panel A: Entering Students (1st year)			
Tax-Based Aid	-0.935	-0.976	-0.934	-1.597	-1.039	-1.018
	[0.129]	[0.139]	[0.159]	[0.254]	[0.288]	[0.321]
R^2	0.092	0.092	0.092	0.188	0.187	0.186
Sample Size	11,180	11,180	11,180	5,440	5,440	5,440
Number of Schools	120	120	120	70	70	70
First Stage Results						
F (instrument)		4,890	1,750		3,514	755
Partial R^2		0.81	0.63		0.90	0.74
C-statistic		12.49	9.77		16.97	6.82
			Panel B: Continuing Students (2nd-5th years)			
Tax-Based Aid	-0.798	-0.866	-0.789	-1.147	-0.918	-0.594
	[0.076]	[0.083]	[0.089]	[0.271]	[0.277]	[0.316]
R^2	0.083	0.083	0.083	0.191	0.191	0.191
Sample Size	40,620	40,620	40,620	17,040	17,040	17,040
Number of Schools	120	120	120	70	70	70
First Stage Results						
F (instrument)		7,108	1,628		7,708	2,329
Partial R^2		0.79	0.56		0.91	0.78
C-statistic		7.29	3.88		9.18	11.92

Sample sizes and the number of schools are rounded to the nearest 10 to comply with the Department of Education confidentiality statutes.
Panel A includes only 1st year (entering) students. Panel B includes students enrolled in 2nd-5th years (continuing students).
The estimates use tax-based aid based on maximum spending or on simulated spending as the instrument. See Section 4.2 for details.
The F-test is for the restriction that the excluded instrument is zero.
The C-statistic is distributed chi-squared (1) under the null hypothesis that all regressors are exogenous.
Control variables include school fixed effects, student characteristics (race, age, gender, dependency status), family income (cubic spline), parent/guardian controls (education and marital status), family controls (size, home Census Division), time (year indicator variables and interactions of year indicators with indicators for institutional selectivity).
Controls are also included for other forms of aid (Pell Grants, federal campus-based aid, state aid) at both the student level and as the school-year average values.
Standard errors, clustered at the school level, are reported in brackets.

Table B2

Estimated Tax-Based Aid Effect on Institutional Grant Aid by Institutional Selectivity and Student Ability

	(1)	(2)	(3)	(4)	(5)	(6)
School Type	Public & Private	Public & Private	Public & Private	Public & Private	Public & Private	Public & Private
Estimator	OLS	Maximum Spending IV	Simulated Spending IV	OLS	Maximum Spending IV	Simulated Spending IV
	Panel A: Effects by Institutional Selectivity					
	More Selective	More Selective	More Selective	Less Selective	Less Selective	Less Selective
Tax-Based Aid	-1.205	-1.149	-0.825	-0.591	-0.613	-0.464
	[0.170]	[0.173]	[0.216]	[0.094]	[0.104]	[0.129]
R^2	0.164	0.164		0.060	0.060	
Sample Size	27,040	27,040	27,040	47,240	47,240	47,240
Number of Schools	70	70	70	120	120	120
First Stage Results						
F (instrument)		2,730	2,499		6,785	1,782
Partial R^2		0.87	0.72		0.80	0.60
C-statistic		2.26	8.05		3.44	5.53
	Panel B: Effects by Student Ability					
	Above Average	Above Average	Above Average	Below Average	Below Average	Below Average
Tax-Based Aid	-1.064	-1.108	-0.871	-0.809	-0.809	-0.550
	[0.165]	[0.166]	[0.200]	[0.139]	[0.140]	[0.174]
R^2	0.060	0.060		0.107	0.107	
Sample Size	20,710	20,710	20,710	20,340	20,340	20,340
Number of Schools	180	180	180	180	180	180
First Stage Results						
F (instrument)		5,980	2,246		4,535	1,632
Partial R^2		0/86	0.72		0.84	0.65
C-statistic		1.16	6.56		0.66	4.39

Sample sizes and the number of schools are rounded to the nearest 10 to comply with the Department of Education confidentiality statutes.

In Panel A, institutional selectivity is defined by NPSAS categories. In Panel B, student quality is determined by SAT scores. See Appendix B1 for details.

The F-test is for the restriction that the excluded instrument is zero. Standard errors, clustered at the school level, are reported in brackets.

The C-statistic is distributed chi-squared (1) under the null hypothesis that all regressors are exogenous.

Control variables include school fixed effects, student characteristics (race, age, gender, dependency status), family income (cubic spline), parent/guardian controls (education and marital status), family controls (size, home Census Division), time (year indicator variables and interactions of year indicators with indicators for public institutions, and other forms of aid (Pell Grants, federal campus-based aid, state aid) at both the student level and as the school-year average value.