

How Tax Credits for Higher Education Affect College-Going¹

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Abstract

The federal tax deduction for tuition is appealing: it potentially increases investments in postsecondary education with minimal administrative cost. We assess whether it actually causes greater investments by applying regression discontinuity methods to the income cutoffs that govern eligibility for the deduction. Although a substantial share of eligible households take nearly the maximum deduction allowed, we find no evidence that the deduction has a causal effect on attending college (at all), attending full- versus part-time, attending four- versus two-year college, the instructional and other resources experienced in college, the amount paid for college, or student loans. Also, we find that a significant share of households who would be just above a cut-off manage their incomes so that they fall slightly below it. We show that this income management generates bias due to reverse causality, and we impose statistically appropriate "doughnut-holes" to avoid bias. A logical analysis suggests that the deductions' inefficacy may be due to superficial details of salience, timing, and the method of receipt. We speculate that the deductions might increase college-going if they were modified in simple ways that would not materially change their nature, administrative costs, or potential tax expenditures. We outline how such modifications could be tested.

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1 Introduction

The U.S. federal government has a somewhat bewildering array of programs that help families pay for higher education. Some of these programs, such as the Pell grant for low-income students, receive significant media attention and appear to be salient to families. Others, especially those that operate through the tax code, are less in the public eye. However, all of these programs have the goal of causing people to acquire additional higher education by relaxing liquidity constraints and/or reducing the price of college. They are usually justified with a return-on-investment argument: By causing people to attain more education than they otherwise would, society benefits because people earn more, pay sufficiently more taxes to finance the programs, and are better citizens in myriad ways. All these arguments depend, however, on the programs' having positive *causal* effects on college-going. In this paper, we investigate one of the key tax expenditures for higher education: the above-the-line deduction for tuition and fees (DTF). The DTF has features--sharp eligibility cut-offs based on household income--that make it more susceptible to causal analysis than almost any other federal aid program. Because estimates of the DTF suffer from reverse causality bias if we do not account for households' tendency to manage their incomes so as to get them slightly below the cutoffs, we produce unbiased causal estimates by applying a statistically appropriate "doughnut-hole" to each cut-off. Since we find no evidence that the DTF has a causal effect on any measure of college-going, we apply economic logic to its structure to explain the likely reasons why it is inefficacious. We suggest simple modifications to the DTF that would not change its nature or potential cost but that might make it more likely to causally increase college-going. We outline how such modifications could be tested.

It is reasonable to ask why the federal government has both grant-based and tax-based programs that support individuals' spending on higher education. Programs that operate through the tax code, like the DTF, have the advantage of extremely low paperwork and administrative costs. Form 8917, which a family files for the DTF, has only 6 questions and could take at most a few minutes to complete. In contrast, the Free Application for Federal Student Aid (FAFSA), required for the grant programs, has 105 questions and is time-consuming to complete. To help the Internal Revenue Service (IRS) administer the tax expenditures for higher education, schools issue a 1098-T for every student. But, the cost of doing this plus the IRS's costs of processing the extra lines in the tax code, even if very generously estimated, could not possibly represent more than 0.1 percent of the tax expenditures. In contrast, each college and the U.S. Department of Education maintains an office to administer federal grant aid, and cost of running these offices appears to amount to 10 percent of the total spent on grants. There are also concerns that schools commit fraud when administering grant-based aid.⁴

The negligible cost of administering tax-based aid programs like the DTF is undoubtedly an advantage, but they may have disadvantages owing to their structure. If a family pays tuition and fees with typical timing, it will receive its tax-based aid an average of 10.5 months later. This timing may mean that tax-based aid is less likely to relax liquidity constraints than grant-based aid which is timed to coordinate with tuition bills. In addition, families may fail to realize

⁴ The estimate of the cost of administering federal grant aid is based on authors' calculations. The U.S. Department of Education's budget indicates that the federal administrative cost amounts to about 4.3% of the total spent on grants. The budgets of higher education institutions suggest that their cost of administering financial aid amounts to about 5.4% of grants. For the concerns about fraud, see for instance U.S. General Accountability Office (2010).

that they are eligible for tax-based aid when they are making college-going decisions. Such non-recognition may limit the causal impact of the programs on educational attainment. Moreover, families may perceive the money from tax-based aid as *income* rather than a change in the *price* of college. If they do, the effects of the aid are likely to be minuscule. We show below that a reasonable upper bound on the *income* effect of the DTF is an increase in college attendance of a tiny 0.25 percentage points (a quarter of 1 percentage point).

In short, understanding the causal effects of the DTF is both feasible and important. If the DTF causally increases college-going, its administrative costs are so low that it might be wise to substitute tax-based for grant-based aid. If the DTF has little or no effect on college-going, its peculiar structure may explain why. Economic logic and our evidence may indicate how the DTF might be modified to retain its nature but nevertheless increase its causal effects.

We believe this paper contributes in four ways. First, the DTF is an important tax-based aid program that has received almost no evaluation.⁵ Second, because the DTF lends itself to regression discontinuity analysis and because we employ nearly ideal administrative data, our estimates of its effects are precise and bias-free under assumptions that we are able to validate well. By contrast, in even the best differences-in-differences studies of the effects of grant aid, there are concerns about whether non-aided (control) students behave as the aided students would have done had they not been given aid.⁶ There are notable exceptions including a number of studies that examine aid that is randomly assigned to students or that use a regression discontinuity design. However, these studies typically analyze a program that is much smaller than the \$4 billion per year DTF and that is more narrowly targeted.⁷ Analyzing the Pell Grant program has proven to be especially difficult because its parameters change gradually and the changes are often triggered by macroeconomic events that may have independent effects.⁸ Third, our analysis suggests that apparently superficial aspects of a program's structure--its salience, timing, the way it is presented, the way it is received--may crucially change the program's effects. Put another way, we may be able to use logic to restructure a program like the DTF to make it attain more of its intended effect *without increasing its cost*. Finally, our study is ideal for investigating manipulation of the forcing variable (income, in our case) and the use of

⁵ For analysis of the federal tax *credits* for higher education, see Long 2004, Turner 2011, Bulman and Hoxby 2015.

⁶ There is a rich literature that employs differences-in-differences and similar non-experimental methods to study the effects of college costs on college-related outcomes. We could not possibly do justice to this literature in a footnote and accordingly refer the reader to two excellent, recent reviews: Oreopoulos & Petronijevic (2013) and Barrow and Malamud (forthcoming).

⁷ Most of these studies examine an aid program that is specific to a city, state, or postsecondary institution. See, for instance, van der Klaauw (2002), Kane (2003), Linsenmeier, Rosen, and Rouse (2006), Angrist, Autor, and Pallais (2014).

⁸ See, for instance, Bettinger (2004). The Pell program does have kinks in its formula that can be potentially used for regression kink (RK) analysis. However, the RK method can be unreliable in the presence of even small amounts of curvature in the relationship between the outcome and the omitted variables (the ones that are related to the forcing variable). See Ganong and Jaeger (2014). The existing studies that use RK analysis on the Pell Grant appear to suffer (unintentionally) from this curvature problem. They report estimates and standard errors that are unlikely to be reliable, and they lack the statistical power to construct reliable permutation-based confidence intervals.

doughnut-holes in regression discontinuity analysis. Although we did not begin this study in an effort to learn about optimal doughnut-holes, our results are so precise that they can inform econometric innovation in regression discontinuity methods.

The main limitation of this study is that our estimates of the effect of the DTF are local to households with income in the vicinity of one of the eligibility thresholds. Fortunately, there are multiple thresholds so we do not rely exclusively on households in a narrow income range.⁹

In section 2 of this paper, we explain how the DTF works. Section 3 describes our data and the college-going context. Section 4 reviews the regression discontinuity method. We discuss income management and statistically appropriate doughnut-holes in section 5. In section 6, we explore how our analysis will be affected by the households' perception of the DTF. Will they see it as a change in income or a change in the price of college? In section 7, we estimate the DTF's causal effect on numerous college-related outcomes: attendance, college choice, instructional resources, tuition paid, and student loans. In section 8, we summarize our findings and use economic logic to explain why we should not be surprised that DTF has negligible effects on college-going. In section 9, we posit that simple revisions to the DTF might increase its causal effects. We propose a rigorous test of these revisions.

2. The Tax Deduction for Tuition and Fees

The DTF was enacted as part of the Economic Growth and Tax Relief Reconciliation Act of 2001 (P.L. 107-16). It remains in force today although currently it is less used because the American Opportunity Tax Credit (AOTC), a temporary higher education tax credit enacted in 2009 as part of the Economic Stimulus, is more generous for full-time undergraduate students in many circumstances.¹⁰ Since a family cannot take both the DTF and one of the higher education tax credits, tax expenditures on the DTF will remain small until 2017 when the AOTC expires. At that time, projecting from pre-AOTC costs, the DTF will be a tax expenditure of \$3 to 4 billion each year. We focus our analysis on the pre-AOTC tax years (2002 to 2008) because that period reveals the effects of the deduction in a normal year when temporary measures like the AOTC are irrelevant.

The DTF has a simple structure. A household that pays tuition and required fees for undergraduate or graduate education is eligible to deduct that payment, up to some maximum, from gross income.¹¹ The deduction is above-the-line, meaning that the households need not itemize deductions to take the DTF. Eligibility for the deduction depends on a household's modified adjusted gross income (MAGI) which is equal to total income minus all of the other

⁹ The small number of studies that examine aid that is randomly assigned usually also produce effects that are local. For instance, most of the random assignment occurs only among students who are marginal to the program along some dimension such as achievement or family income.

¹⁰ There was no DTF in the 2006 tax year, but the DTF was reinstated for 2007 and subsequent years. The Lifetime Learning tax credit can also be more generous than the DTF under certain circumstances

¹¹ The deduction is per *filer*, not per student. The household itself must spend the money for tuition and fees. If some colleges expenses are paid by a tax-free scholarship, fellowship, grant, employer assistance, or veterans' assistance, the qualifying tuition and fees are reduced commensurately.

above-the-line deductions.¹² In 2002 and 2003, joint-filing households with MAGI less than or equal to \$130,000 and single-filing households with MAGI less than or equal to \$65,000 were eligible for a \$3,000 deduction. In 2004, a two threshold system was adopted and the maximum deduction was increased. Joint filers with MAGI less than or equal to \$130,000 and single filers with MAGI less than or equal to \$65,000 were eligible for a \$4,000 deduction. Joint filers with MAGI greater than \$130,000 but less than or equal to \$160,000 and single filers with MAGI greater than \$65,000 but less than or equal to \$80,000 were eligible for a \$2,000 deduction. These parameters remain in force as of 2015. We thus have a total of six distinct income cut-offs that can be used to identify the effects of the DTF.

The DTF income thresholds are *not* the same as the thresholds for the Hope tax credit, Lifetime Learning tax credit, and AOTC. Thus, regression discontinuity-based estimates of the causal effect of the DTF are wholly unaffected by the presence of these other tax expenditures related to higher education. This is an important point.¹³

The amount by which the DTF changes a household's tax liability depends on its marginal tax rate. For instance, if a 2004 household had income of \$130,000 and tuition and fees spending of \$4,000 or more, it would be eligible for a deduction of \$4,000. Its marginal tax rate in that year would have been 28 percent, so the DTF would have reduced its taxes by \$1,120 ($=0.28 \times 4000$). If the household paid the same tuition and fees but had only \$50,000 in income, its marginal tax rate would have been only 15 percent so the DTF would reduced its taxes by only \$600.

The DTF is for expenses paid in the *tax year*. For instance, a student might pay for the spring of her freshman year in January 2007 and pay for the fall of her sophomore year in September 2007. These two payments made in 2007 would generate a deduction on the taxes due on April 15, 2008. Thus, the tax-based aid associated with the January payment would be received about 12.5 to 16.5 months after the payment was made. The aid associated with the September payment would be received about 4.5 to 8.5 months after the payment was made. On average, households realize the DTF 10.5 months after making tuition payments.

We hypothesize that if a household understands the DTF rules and anticipates how they will apply, then the household is more likely to treat the DTF as a reduction in the price of higher education (which it is) rather than as an increase in its income. (We elaborate on this hypothesis below.) Therefore, it is important to consider whether households learn the DTF rules.

If a household manually fills out Form 8917 in order to take the DTF, the income eligibility cut-offs are fairly obvious. The form states:

If the result [MAGI] is more than \$80,000 (\$160,000 if married filing jointly),
STOP. You cannot take the deduction for tuition and fees.

¹² The DTF is computed after all of the other above-the-line deductions have been subtracted from total income. These other deductions vary slightly with the tax year but are: educator expenses, moving expenses, self-employment taxes, alimony, IRA contributions, student loan interest, penalties on saving withdrawals, Archer medical savings accounts, Health Savings Accounts, self-employed health insurance, self-employed retirement accounts, and business expenses of reservists, performing artists, and certain others.

¹³ We use statistically appropriate bandwidths for our regression discontinuity analysis. Fortunately, these happen not to include the phase-out ranges for the higher education tax *credits*.

Slightly less obvious language divides filers into those who qualify for the two tiers of deductions:

Is the amount [MAGI] more than \$65,000 (\$130,000 if married filing jointly)?

Yes. Enter the smaller of [tuition and fee spending] or \$2,000. No. Enter the smaller of [tuition and fee spending] or \$4,000.

The value of the DTF may be misunderstood even by households who manually complete Form 8917 since what they transfer to their main tax form is the amount of the *deduction* (for instance, \$4,000). They may fail to understand that they need to multiply the deduction by their marginal tax rate to learn how much it reduces their taxes. Many people do not know their marginal, as opposed to their average, tax rate anyway. (Tax filing programs typically reveal average tax rates.)

If a household uses a tax preparer, the preparer may or may not acquaint it with the DTF rules. A savvy preparer might, for instance, tell a household with tuition expenses that it was just above the income threshold and would be eligible for the DTF if it invested sufficient money in an IRA.

Tax preparation software, such as TurboTax, obscure the DTF rules. Because a household with tuition and fee expenses is potentially eligible for several tax breaks, software first asks whether the household paid tuition and fees and then silently determines which, if any, tax break will benefit it most. Some software alerts a household that it is ineligible for tax-based aid even though it paid tuition and fees. At that point, the household might feel impelled to investigate the DTF rules or use trial and error (adding or subtracting income) to figure out whether it was close to a threshold. Because most software nudges filers to invest in an IRA at the end of the filing process, that nudge might also cause a household to realize that it was close to a DTF threshold.¹⁴

3. Data, Cohorts and Years, the College-Going Context, and Income Dynamics

A. Data

We use de-identified data from an IRS database that are fully accurate and maximally dense. From Form 8917, we derive the qualified spending on tuition and fees that a tax filer claims. We derive relevant variables from returns: MAGI and filing status. Using data from information returns, we compute income and filing status even for non-filers. We use variables derived from Form 1098-T (the form on which institutions report payments of tuition and fees): tuition and fee payments, whether the student is enrolled at least half-time, whether the student is enrolled in graduate studies, and scholarships and grants received by the student. These variables are available regardless of whether a student actually takes the DTF or other tax-based aid for higher education. However, in some cases, the 1098-T-derived variables may generate a less than completely accurate calculation of the DTF for which the filer is eligible.¹⁵ We derive data on

¹⁴ Davis (2002) and Turner (2012) suggest that the complexity of the higher education tax benefits may make eligible people fail to take the most advantageous benefit or to take a benefit at all.

¹⁵ Exact calculations are sometimes not possible because a scholarship can pay for qualified tuition and fees but can also pay for other expenses such as room and board. Only the part of scholarship that pays for tuition and fees should be subtracted from the payment made by the student's family, but schools can report the entire scholarship on the 1098-T.

student loan interest from Form 1098-E. For data on colleges' characteristics (two- versus four-year, college resources), we rely on the National Center for Education Statistics' Integrated Postsecondary Education Data System (IPEDS).

B. Cohorts and Years

We describe individuals by their "cohort" based on the year in which they would graduate from high school if they entered primary school according to their state's compulsory schooling laws and progressed through school strictly on time. For instance the "2004 cohort" are the individuals expected to graduate from high school in June 2004.¹⁶ We hereafter call the cohort's expected graduation year "year 0," the next year "year 1," and so on. Year 1 corresponds to the spring of freshman year and fall of sophomore year for people who progress through grade 12 strictly on time. It corresponds to the fall of freshman year for people who enter kindergarten late or otherwise graduate a year late, as about one-third of students do (Deming and Dynarski, 2008). As a factual matter, people are most likely to be enrolled in higher education in year 1, followed closely by year 0.

C. The College-Going Context

To put the DTF in context, we present relevant summary statistics in Table 1 which groups households by MAGI. The MAGI intervals are narrower near the DTF thresholds to facilitate later statistical comparisons. The table shows college attendance, tuition paid, grants received, "list" tuition and fees, and instructional resources in year 1 for the 2004 cohort. We also show student loan interest paid by them in years 1 through 7 (2005 through 2011). We focus on the 2004 cohort simply because it is similar to the cohorts who would start college in 2002 and 2003 (potentially affected by the first version of the DTF) and to the cohorts who would start college in 2004 through 2008 (potentially affected by the second version of the DTF).

Table 1 reveals a few things that help us interpret our findings. First, comparing panels a and b, we see there are many more joint filers near the income cut-offs than there are single filers. This is because single filers often have incomes too low to be near the cut-offs. Second, college attendance (at all), four-year college attendance, college tuition paid, list tuition, and instructional resources rise (nearly) monotonically with income. That is, students not only switch from no-college to four-year college as income rises but they also "upgrade" from two- to four-year college and from colleges with fewer resources to colleges with more resources. Third, grants and scholarships first fall with income (as need-based aid falls), then rise in income (reflecting the fact that upper-middle income students attend more expensive schools and are more likely to receive merit aid), and then fall in income again (because affluent households receive little aid of any kind). Fourth, student loan interest rises and then falls with income. Low-income households receive more generous need-based grants and have little borrowing capacity. Thus, loans peak for households in the \$90,000 to \$110,000 range and thereafter fall, presumably because more affluent households do not need to borrow much to finance college education.

D. Income Dynamics

The DTF income thresholds have remained the same in *nominal* dollars but the incomes of

¹⁶ We use the compulsory schooling dates for each state to identify when a child would typically start school. For instance, a state might specify that any child who is age 6 by December 31 must be enrolled in the school year that begins in September of that calendar year.

households in the vicinity of the eligibility cutoffs tended to grow, in nominal terms at least, over the years we study. Therefore, if a household with a prospective college student gets near an income cut-off, it typically crosses it a single time as its income rises. These income dynamics are relevant to regression discontinuity analysis because a household that is close to but below a threshold (eligible, in other words) in year 0 is fairly likely to cross that threshold sometime before year 4.

To see this, consider income dynamics for the 2002 cohort's households whose MAGI was \$0 to \$10,000 below \$120,000 in the year they should have graduated from high school. We deliberately examine this "placebo threshold" rather than the actual DTF threshold because we are interested in income dynamics that are unaffected by income management.

We find that households who had MAGI \$0 to \$10,000 below the placebo threshold in year 0 had a 42 percent probability of being (placebo) eligible in that year *only*. The households had a 21 percent probability of being eligible in years 0 and 1, a 10 percent probability of being eligible in years 0 through 2, and a 27 percent probability of being eligible for all four years from 0 to 3. Over years 0 through 3, only 6 percent of households cross the placebo threshold first in an upwards and later in a downwards direction.¹⁷

In short, a student who is near a DTF threshold but treated in his freshman year (the 0-1 school year) has a very solid chance of being treated in his sophomore year (the 1-2 school year), a good chance of being treated in his junior year (the 2-3 school year), but only a small chance of being treated in his senior year (the 3-4 school year). It will be useful to remember these dynamics when interpreting the regression discontinuity results.

4. The Regression Discontinuity Method

Our regression discontinuity analysis is based on the assumption that other factors that affect college-going change *continuously* through the income eligibility thresholds while, as already shown, the DTF changes very discontinuously. Following the standard formulation (Hahn, Todd, and van der Klaauw, 2001), we specify that the causal effect of the DTF on college outcome Y can be expressed by the following equation, where d is the distance between MAGI and the eligibility cutoff.

$$(1) \quad \lim_{d \rightarrow c^-} E[Y|d] - \lim_{d \rightarrow c^+} E[Y|d] = \beta + \lim_{d \rightarrow c^-} E[\varepsilon|d] - \lim_{d \rightarrow c^+} E[\varepsilon|d]$$

If other factors that affect college outcomes, ε , do not change discontinuously at the cutoff:

$$(2) \quad \lim_{d \rightarrow c^-} E[\varepsilon|d] - \lim_{d \rightarrow c^+} E[\varepsilon|d] = 0$$

then β , the change in the college outcome at the eligibility threshold, is the causal effect of the DTF. This implies the standard estimating equation:

$$(3) \quad Y_{it} = f(d_{it}) + \beta \cdot Eligible_{it} + \varepsilon_{it}$$

¹⁷ If a household crosses the threshold twice, it usually crosses it first with increasing income and second with decreasing income.

where i indexes potential students, h indexes tax filing households, f is a continuous function such as a polynomial, and $Eligible$ is a indicator for the household being eligible for the DTF on the basis of its MAGI. There are no time subscripts in the equation yet. This is an issue we discuss below.

We follow the recommended procedure (Lee and Lemieux 2010) and estimate results for alternative polynomials in distance and alternative bandwidths that encompass the optimal bandwidth ranges (Imbens and Kalyanaraman, 2012).

5. The Suitability of the Tax Deduction for Regression Discontinuity Analysis

Regression discontinuity methods work best in an environment where (i) a threshold in a continuous forcing variable (income) generates a large change in a policy variable (the amount of DTF for which a student qualifies); (ii) the threshold is strictly enforced; (iii) there are very dense data near the threshold for the forcing, policy, and outcome variables; (iv) other factors that might affect college-going do not change discontinuously at the threshold; (v) people do not manipulate the forcing variable near the threshold in an attempt to make themselves eligible.

Our setting easily satisfies conditions (i) through (iv). We have ideally accurate and maximally dense data. In this section we show that conditions (i) and (ii) are fulfilled. Now, however, we focus attention on condition (v) because it is crucial for what follows.

A. Income Management

To analyze the DTF using regression discontinuity, we need the forcing variable, MAGI, to be free of manipulation or "management" near the threshold. While such management may be perfectly legal, it can generate serious reverse causality bias because the only people who have an incentive to manage MAGI are those who pay tuition and fees during the tax year. Thus, 100 percent of "managers" who end up just below the cutoff have a student attending college while only a share of "non-managers" who remain above the cut-off have a student in college. For managing households, the DTF does *not* cause college attendance. Rather, the reverse is true: college attendance causes households to practice income management so that they can take the DTF. This reverse causality means that if we do not fully eliminate the effects of income management, our estimates will be biased in favor of finding that the DTF raises college-going.

Some forms of income management require the household to foresee that it will be above the cutoff in the absence of management. For instance, if a household decides to delay the receipt of some income, it must do this before December 31st. On the other hand, a household can deposit money in an IRA right up until April 15 of, say, 2004 and still have that deposit reduce its 2003 MAGI. Thus, a household that does not realize that it is above the cutoff for the DTF until it actually files its taxes could manage its income at late as the day on which it files so long it has not already exhausted its annual IRA contribution limit. Most forms of illegal evasion, such as overreporting business expenses or moving expenses, could also occur as late as tax filing day. Of course, all forms of income management impose some implicit or explicit costs on the household. Thus, the greater the DTF that a household would receive if eligible (that is, the greater the tuition and fees it paid), the greater its incentive to manage income.

The test for management of the forcing variable is whether there is displacement of the MAGI distribution from above to below the eligibility cutoff. We show such tests in Figures 1 through 6 for year 1 using income bins that are \$500 wide. (We show year 1 because it is the year in which people are most likely to be enrolled in higher education. However, similar figures for years 0, 2, 3, and 4 may be obtained from the authors.)

It is visually obvious that households manage their incomes to make themselves eligible for the DTF if they would otherwise be just above the cut-off. For instance, in Figure 1 (joint filers in 2002 and 2003 near the \$130,000 cutoff), mass is missing from the bins between \$130,001 to \$133,000. Mass is added to the added to the 127,001 to \$130,000 bins. The parallel Figure 2 for 2004 through 2008 shows similar displacement affecting, at minimum, the bins from \$129,001 through \$132,500. (Recall that, owing to the introduction of the two-tier DTF, there is less incentive to manage income in 2004 through 2008 than in 2002 and 2003.) Figure 3 shows the upper \$160,000 threshold in 2004 through 2008. Here, displacement obviously affects the bins from \$158,000 through \$162,500.

For single filers at all three thresholds (Figure 4 through 6), there is evidence of income management but only for the bins that are within \$1,500 on each side of the cut-off. Indeed, most of the displacement appears to be within \$1,000 of the cut-off. Of course, given single filers' lower incomes, a \$1,500 change in income is as consequential in percentage terms as a \$3,000 change for joint filers.

If we do not exclude the data in the region subject to income management, we will overestimate the effects of the DTF on college enrollment, the amount spent on tuition and fees, and the college resources that students experience. To avoid this reverse causality-induced bias, we must impose a doughnut-hole on both sides of each cut-off so that our estimates do not rely on the households most likely to practice income management. Currently, there is no econometric theory of optimal doughnut-holes (although one of the authors is working on this problem with an econometrician). However, the basic logic is as follows. We want to impose a doughnut-hole that is sufficiently wide to eliminate reverse causality bias. The wider the doughnut-hole, though, the more reduced is our statistical power. This is because we cannot simply widen the bandwidth as we widen the doughnut-hole. (If we widened the two in parallel, we could maintain statistical power.) The reason we cannot simply widen the bandwidth is that the quality of our model of the continuous relationship between income and college-going deteriorates as we draw observations further from the threshold. This is fundamental to the logic of regression discontinuity identification.

To choose a doughnut-hole that is sufficiently wide to eliminate bias from income management but not so wide as to eliminate statistical power, we proceed as follows. We estimate the bias in each small income range by running a local regression of density (g) on MAGI--omitting \$5,000 on either side of each cut-off.¹⁸ This gives us a prediction of what density would be in the absence of income management. Thus, we also have an estimate of the excess or missing density at each MAGI. We then take the base rate of college-going around each threshold from Table 1. (For instance, the base rate of college attendance is 80 percent for individuals from joint filing households near the \$130,000 threshold.) We assume that all of the excess and missing density is associated with individuals who have a 100 percent probability of attending college--since only they have an incentive to practice income management.

Then, for each possible doughnut-hole and each outcome, we have estimates of the bias due to income management and of the standard error of the effect of DTF eligibility. If the bias is such that it would be statistically significantly different from zero, a larger doughnut-hole is needed.

¹⁸ In practice, a local regression with a quadratic in MAGI has very high explanatory power.

For instance, using the \$130,000 threshold for the 2002 and 2003 cohorts, the estimated effect of the DTF on college attendance would be upward biased by 2.4 percentage points (0.024) with no doughnut-hole. Since the standard error is as small as 0.003 (with a quartic polynomial and \$20,000 bandwidth), this bias would be highly statistically significant and misleading. With a \$1,000 doughnut-hole, the bias is 1.6 percentage points (0.016) and the standard error is as small as 0.004 so we would again have the appearance of a statistically significant effect where there is none. With a \$2,000 doughnut-hole, the estimated bias is 1.3 percentage points and the standard error is between 0.007 and 0.018 depending on the polynomial and bandwidth. Thus, the bias would be statistically significantly different from zero in some cases. With a \$3,000 doughnut-hole, we estimate the bias to be 1 percentage point and, in all cases, the standard errors are such that this bias would not be statistically significantly different from zero. This suggests that we need a \$3,000 doughnut-hole at this threshold.

We estimate that we need \$3,000 doughnut-holes for the \$130,000 and \$160,000 joint filing thresholds. We need \$1,500 doughnut-holes for the \$65,000 and \$80,000 single filing thresholds. It is reasonable that larger doughnut-holes in absolute dollars are needed for joint filers' thresholds because the doughnut-holes they need are about the same *percentage* of income as the single filers'. For the remainder of the paper, we call the doughnut-hole we need the "base case" and use bold type face to emphasize the base case estimates in tables where we show results for a variety of doughnut-holes.¹⁹ (We could show estimates only for the base case since we think that only those estimates are free of bias. However, by showing estimates for a variety of doughnut-holes, we allow readers to gauge the extent of reverse causality bias caused by income management.) Doughnut-holes larger than the base case needlessly reduce statistical power.

Recall that MAGI is equal to total income minus the other above-the-line deductions. We therefore investigated the channels through which income management occurs. We found no evidence that other deductions, such as the IRA deduction, jump discontinuously at the MAGI threshold. This suggests that other deductions are not the primary channel for income management. However, deductions vary so greatly that they might matter a lot and still not generate compelling evidence. On the other hand, we observe displacement in total income that is similar to what we observe in MAGI. In addition, we observe that income management appears to be non-existent among households that report only wage and salary income.²⁰ Thus, we conclude that most of the income management visible in Figures 1 through 6 operates through channels such as schedule C business income, capital gains, rental income, partnership income, S corporation income, farm income, and the like.

B. Changes in Aid at the Income Eligibility Thresholds

Figures 7 through 12 show that the income eligibility thresholds for the DTF are strictly enforced and that the deductions fall by large amounts at the threshold. All the figures use year 1 data, employ \$500 wide MAGI bins, and show kernel-weighted local polynomial regressions. The

¹⁹ We allow the base case to vary with the dependent variable. We could also define a base case for each specification (polynomial, bandwidth). This might prove confusing to readers, however, so we selected a base case that applied well across specifications, for each dependent variable.

²⁰ All of the evidence mentioned in this paragraph is available from the authors. [Note to editors: possible appendix material.]

local polynomial regressions are shown in two ways: (i) using all of the observations, (ii) using all the observations except those in the base case doughnut-hole around the threshold. The figures are mirrored by Tables 2 and 3 which show regression discontinuity estimates of the effect of income eligibility on, respectively, DTF take up and the deduction amount (unconditional on take up). To aid interpretation, the final line of Table 3 shows the average deduction amount taken by households at each income eligibility threshold *conditional on taking the DTF*.

Figure 7 and Tables 2 and 3 show, that for the 2002 and 2003 cohorts, the share of joint filers who took the DTF in year 1 is 58 percent with the base case doughnut-hole. The average DTF is \$1,573 at the threshold which means that households near the threshold who were taking it were taking a deduction of \$2,646, close to the maximum allowable DTF of \$3,000 in 2002 and 2003.

Figure 8 and Tables 2 and 3 tell a similar story for the 2004 through 2008 cohorts. Joint filers' take-up of the top tier DTF rate drops by 55 percent at the lower cut-off if we use the base case doughnut-hole. The average deduction drops by \$1,023 at the lower cut-off in the base case. This means that households just below the threshold who were taking the DTF were taking a deduction \$1,498 larger than that of households just above the lower cut-off.

Figure 9 and the fourth column of Tables 2 and 3 show the upper cut-off among 2004 through 2008 cohorts who file jointly. Their take-up rate falls by 64 percent with the base year doughnut-hole. The average deduction drops by \$1,199 at the upper cut-off in the base case. This means that households just below the upper cut-off who were taking the DTF were taking a deduction of \$1,860, close to the maximum of \$2,000.

Figures 10 through 12 and the remaining columns of Tables 2 and 3, for single filers, also exhibit sharp discontinuities in the receipt of aid at the cut-offs and income management in the immediate vicinity of the cut-offs. Focusing on year 1 and our base case doughnut-holes for single filers, we estimate that 24 percent of those in the 2002 and 2003 cohorts who were just below the cut-off took the DTF and their average deduction--conditional on taking a deduction--was \$2,279. For the 2004 through 2008 cohorts, single filers' take-up of the top tier DTF fell by 19 percent at the \$65,000 cut-off and their average deduction--conditional on taking a deduction--fell by \$1,178 there. Their take-up rate fell by 30 percent at the \$80,000 cut-off and their average deduction--conditional on taking one--was by \$1,755, the maximum being \$2,000.

It is notable that single filers are less likely to take the DTF and take slightly lower deductions, when they take one, than joint filers. This is because students from single filing households tend to attend less expensive schools. They also receive more need-based grant aid so they themselves do not pay as much tuition or fees.

C. Summing Up the Usefulness of the "Experiments"

So far, we have focused on the year 1 effects of the income thresholds with the base case doughnut-hole, a quintic polynomial, and a bandwidth of \$20,000 for joint filers and \$10,000 for single filers. (Because they are about the same *share* of income at the income at the threshold, we highlight the \$20,000 bandwidth for the joint filers and the \$10,000 bandwidth for single filers.) We show this case in bold in the top row of Tables 2 and 3. We use the other rows of the tables to demonstrate a few other points. First, the estimated effects of income eligibility *are* sensitive to the width of the doughnut-hole. The effects with a zero doughnut-hole are very often statistically significantly larger than the effects with the base case doughnut-holes. This is evidence of income management. Second, although wider bandwidths substantially reduce our

standard errors, they do not much change the estimated effects of income eligibility on take-up or the deduction amount. This is not a surprise because it is visually obvious in Figures 1 through 6: The relationships between the DTF variables and MAGI tend to be flat near the income cut-offs so bandwidth is unimportant. Third, the estimated effects of eligibility on take-up and the deduction amount do not depend much on the order of the polynomial that we use. This suggests that, apart from income management and the rule-driven discontinuity, the MAGI-deduction relationship is smooth enough to be modeled well with a quintic polynomial. We do not need a higher order one.

In short, the doughnut-holes matter. The order of the polynomial and the bandwidth are less important.

Table 4 shows the effects of the income thresholds on take-up and the amount of the DTF for all of years 0 through 4. For conciseness, they show only the base case. The effects are always greatest in year 1. This is to be expected since year 1 picks up the school years in which people are most likely to enroll in postsecondary education. The effects rise from year 0 to year 1 and then fall away gradually from year 1 to year 2, year 3, and year 4. In other words, the DTF is most relevant in the years in which people are most likely to attend college.

We take away a few conclusions. First, all of the estimated effects on DTF take-up are highly statistically significant. For the base case, the t-statistics for the effect of the DTF threshold on take-up range from 43 to 80 for joint filers and range from 13 to 25 for single filers. (Recall that there are fewer single filers near the thresholds than joint filers.) Thus, we do not lack statistically strong "experiments." Second, the amount of the deduction taken by those who are just short of an income cut-off is consistently between 75 and 93 percent of the maximum allowable (\$2,000 to \$4,000 depending on the cut-off) for joint filers and 59 to 88 percent for single filers. Thus, the changes in aid experienced at the thresholds are substantial. Third, the estimated effects are insensitive to the polynomial and bandwidth but sensitive to the width of the doughnut-hole. Thus, we will only obtain plausibly causal effects of the DTF on college-going if we apply the base case doughnut-holes.

6. Anticipation and Salience: Price versus Income Effects

Although the regression discontinuity method is straightforward, the timing of DTF filing and receipt affect how we implement the method and how we interpret its results. It is most instructive to consider four cases: (i) households who always understand the DTF rules; (ii) households who are not aware of the DTF rules until they find themselves eligible for it but who thereafter understand the rules; (iii) households who never understand the DTF rules and simply accept the extra income as it comes; (iv) households who never understand the DTF rules, accept the extra income as it comes *but* expect to be income eligible in this tax year if they were eligible in the last tax year;

The only households relevant in a regression discontinuity analysis are those who are sufficiently near an eligibility threshold that they are equally likely to be on either side of the threshold so far as we could learn from their other determinants of college (observed and unobserved). Thus, our cases focus exclusively on these "relevant households."

A. Households Always Understand the Eligibility Rules

Suppose that the relevant households know the DTF rules. Certainty about their year-end incomes would nevertheless vary. A relevant household with only salary income might know with certainty months in advance whether it would be a little above or below the cutoff.

Relevant households with business income or investments might believe that they have equal probabilities of being below and above the cutoff.

Households who understand the eligibility rules should value this tax year's DTF at its *certainty equivalent*. For instance, a relevant household that is certain it will be eligible in 2003 should anticipate that its 2003 tuition and fee payments will be reduced by a percentage equal to its marginal tax rate (up to the maximum allowable deduction). A risk-neutral household that has a 50-50 chance of being eligible should realize that its 2003 tuition and payments will be reduced, in expectation, by a percentage half that size. Whatever the certainty equivalent is, it will act as a change in the *price* of college if the household understands the DTF rules.

Among relevant households who understand the DTF rules, 2003 eligibility matters for 2003 college decisions, 2004 eligibility for 2004 decisions, and so on. Past years' eligibility should be irrelevant. Thus, the regression discontinuity should be set up with year t choices as the dependent variables and year t eligibility and distance from the cutoff as the independent variables:

$$(4) \quad Y_{iht} = f(d_{ht}) + \beta \cdot \text{Eligible}_{ht} + \varepsilon_{iht}$$

where β should be interpreted as the effect of a change in the *price* of college equal to the *certainty equivalent* of the DTF.

B. Households Learn the Eligibility Rules After Once Being Eligible

Suppose that households are initially ignorant of the DTF rules but learn them once they take the DTF. In the first year these households are eligible for the DTF, they will fail to respond. However, after that year, they will start behaving like the always-knowledgeable households.

The appropriate regression discontinuity equation is the same and β should still be interpreted as the effect of a price change. Unlike in the first case, however, we expect β to depend on a household's history of receiving the DTF.

C. Households Never Understand the Eligibility Rules and Just Accept the Extra Income as it Comes

Suppose that the relevant households never learn the DTF rules. They find themselves with extra income from the DTF because they are asked, perhaps by a tax preparer or software, whether they paid tuition and fees in the previous tax year. However, they draw no conclusions about their future eligibility.

Households like this will experience a pure income effect of their realized DTF in the tax year *after* they are eligible. This income effect is likely to be very small because it is logically bounded by the income-college relationships shown in Table 1. The statistics shown in the table are almost surely upward biased indicators of the causal effect of income on college-going because wealth, parents' education, and other factors that increase college are positively correlated with income and are not controlled in the table. For instance, near the \$130,000 joint filing threshold, a \$3,000 DTF that increases a household's income by about \$1,000 (\$3,000 DTF with a 28 percent marginal tax rate) could have *at most* a 0.14 percentage point effect on college attendance and a 0.18 percentage point effect on 4-year college attendance (conditional on

attending).²¹ These effects are so tiny that they would likely be indiscernible.

Among households of the never-understanding type, the DTF should have no effect in the year in which the tuition and fees are paid. Instead, the appropriate regression discontinuity specification has the previous year's distance and eligibility as the independent variables:

$$(5) \quad Y_{iht} = g(d_{h,t-1}) + \gamma \cdot Eligible_{h,t-1} + \varepsilon_{iht}$$

and γ represents the pure income effect of the DTF.

However, if this were the only effect of the DTF--a quarter of 1 percent *at most*--it would be so small that it would be difficult to discern even in ideal data. The important lesson to take away is that the DTF's causal effect on college-going must necessarily be tiny if it runs purely through an income effect rather than a price effect.

D. Households Never Understand the Rules but Expect to Stay Eligible Once Eligible

Suppose that the relevant households never learn the DTF rules. Like the households in the previous case, they simply find themselves with extra income from the DTF. However, they--unlike the households in the previous case--thereafter expect to remain income eligible for the DTF until they find that they are not. That is, they expect to remain eligible until they have the experience of filing taxes when they *paid* tuition and fees but nevertheless cannot take the DTF.

Among households of this type, the DTF should act like a change in the price of college in the year *after* the household is eligible. The DTF should have no effect in the actual year in which tuition and fees are paid. Thus, the appropriate regression discontinuity specification is same as in the previous case but γ now represents the effect of a perceived change in the price of college. However, it is not a classic price effect because, as we saw in the income dynamics section, the household is fairly likely *not* to receive the discount it expects. It will eventually need to reconcile its budget, and it is difficult to predict how it will then respond. Thus, just as in the case of non-salient taxes, we cannot say with confidence how the household will react (Chetty, Looney, and Kroft 2009).

E. The Specifications We Use

Households may be of all the types described above--some may be knowledgeable about the tax code and others not. All the types may practice income management but only those who understand the DTF will use practices that require foresight. Households who do not understand can only use management methods available at the time of filing.

We first estimate the all-in-the-same-tax-year regression (equation 4) with a doughnut-hole:

$$(6) \quad Y_{iht} = f(d_{ht}) + \beta \cdot Eligible_{ht} + \varepsilon_{iht}$$

$$\{h : r < d_{ht} < b\}$$

where r is the radius of the doughnut-hole (for instance, \$3,000 on either side of the cut-off) and b is the bandwidth (for instance, \$20,000 on either side of the cut-off). This regression produces the best evidence on the causal effects of the DTF if households understand the rules.

We then estimate a regression that allows for households who vary in their understanding

²¹ To see this, observe the statistics in Table 1a on either side of the \$130,000 threshold. Then observe that $(80.8\% - 79.4\%)/10 = 0.14\%$ and that $(72\% - 70.2\%)/10 = 0.18\%$.

and anticipation of the DTF:

$$(7) \quad Y_{iht} = f(d_{ht}) + g(d_{h,t-1}) + \alpha \cdot Eligible_{ht} + \gamma \cdot Eligible_{h,t-1} + \varepsilon_{iht}$$

where g is a polynomial in the previous tax year's MAGI distance from the cutoff. We estimate this equation using only the data within the bandwidth but outside the doughnut-hole in *both* years t and $t-1$:

$$\{h : r < d_{ht} < b \wedge r < d_{h,t-1} < b\}$$

We must impose the doughnut-hole on both years because we do not know which type each household is, and this is thus the only way to exclude bias due to income management.²²

7. The Effects of the Deduction for Tuition and Fees

A. Differentiating Visually between Causal Effects and Income Management

Before examining figures based on actual data, it is useful to illustrate how one would differentiate visually between a causal effect of the DTF and income management. Figure 13 shows what a causal effect of the DTF on college attendance would look like in its left panel. Its right panel shows what income management would look like.

At the eligibility threshold, a household's potential DTF falls and this may exert a price or income effect. Either of these effects could cause college attendance to fall. Economic theory indicates a downward shift in the level of attendance. Crucially, this shift should affect a wide array of households above the cut-off, *not merely households in the doughnut-hole*. Thus, we obtain a picture like that shown in the left panel, where the income-attendance relationship exhibits a vertical shift downwards at the threshold but otherwise maintains its shape.

In contrast, income management would produce an upward "flick" of the income-attendance relationship just before the threshold and a downward flick just after the threshold. The flicks would be contained in the doughnut-hole. These flicks are the symptom of households who would contain a college student in any case managing their incomes to gain DTF eligibility. Apart from these flicks (and outside the doughnut hole), the income-attendance relationship would appear as though the threshold did not exist.

Of course, a figure might show evidence of both a causal effect (the downwards shift) and income management (the flicks).

B. The Current Year Effects of the Deduction for Tuition and Fees

In this subsection, we focus on year 1 and the all-in-the-same-tax-year specification (6). That is, we focus on the specification that assumes families understand the DTF well enough for it to exert a price effect. Since, however, we are examining year 1 effects, about half of the families eligible for the DTF will have been eligible in the previous year (year 0). Thus, even if families

²² We considered a specification that also included $t-2$ eligibility, but there are two objections. First, there does not seem to be a reasonable model of expectations in which $t-2$ experience matters *in addition to* $t-1$ experience. We showed in the income dynamics section that, for the relevant households, the $t-2$ experience is not an accurate guide to current year eligibility for the DTF. Second, if our specification were to include $t-2$ eligibility, we would need to restrict our data to those that are within the bandwidth but outside the doughnut-hole in years t , $t-1$, and $t-2$. Given the evidence we reported on income dynamics, this would be an unusual set of households.

have to experience the DTF in a previous year to understand it as a price effect, we may find evidence of all-in-the-same-tax-year effects.

Figures 14 through 19 and Tables 5 and 6 present the effects of the DTF on attending postsecondary school (at all) and on attending four-year college (conditional on attending). Our definition of postsecondary attendance (at all) is whether a person is a student for whom "qualified" tuition and fees were paid or billed. That is, our definition is aligned with the rules about the attendance for which a DTF could be taken. As in the previous figures, we employ \$500 MAGI bins and show kernel-weighted local polynomial regressions using (i) all of the observations, (ii) the observations except those in the base case doughnut-holes. We show other outcomes, years, and specifications in subsequent tables.

Figures 14 through 19 provide no visual evidence that the DTF affects either attendance or four-year college. On the other hand, Figures 14 through 19 do provide clear visual evidence of income management. The flicks are small but visible especially in the four-year figures for married filers. For instance, Figures 14(b) and 15(b) make it obvious that households with students attending four-year college manage their incomes to get *just* below the income threshold. The visual evidence indicates that these managing households would have had incomes above the threshold but within \$3,000 of it. It is harder to see the flicks in the figures for single filers because all effects are smaller for them, but close inspection suggests that they manage income too.

Of course, visual evidence is not precise so we now turn to regression estimates. The top row of Table 5 shows the effects of the DTF on attendance in our base case. Without exception, the estimated effects are not statistically significantly different from zero. The standard errors are such that we cannot rule out very small effects such as a 1 percentage point effect. However, we can rule out effects of 2 percentage points.

Indeed, examining all the rows of Table 5, which show estimates from various specifications, we see a consistent story. If we ignore income management and set the doughnut-hole to \$0, the DTF apparently raises attendance and four-year college. However, this apparent effect is certainly reverse causality bias because it disappears as the doughnut-hole increases from \$0 (implausible) to the base case that eliminates the effects of income management. In other words, any apparent effect is pure bias.

Although the estimates are sensitive to the width of the doughnut-hole, they are not notably sensitive to the polynomial or bandwidth. This is not surprising because, as shown in the figures, the relationships are quite smooth apart from the flicks in the doughnut-holes. In estimates not shown but available, we can show that increasing the bandwidth merely generates more precisely estimated null effects.

Table 6 repeats the exercise for four-year college attendance, conditional on attending at all. For the base case (top row), the estimated effects of the DTF on four-year college are never statistically significantly different from zero. The standard errors are such that we cannot rule out small effects of about 2 percentage points for joint filers (3 to 4 percentage points for single filers). The remaining rows of Table 6 tell the same story as Table 5: Income management generates estimates that are substantially upward biased if we apply a \$0 doughnut-hole. Thus, a naive analysis might suggest that the DTF has a causal effect on college-going. This suggestion disappears as soon as we apply doughnut-holes that plausibly eliminate reverse causality bias.

Table 7 shows the base case regression discontinuity results for years 0 through 4 for joint and single filers. Broadly, these confirm the year 1 results: A few estimates suggest that

the DTF has a statistically significant causal effect on attending (at all), but the ratio of statistically significant estimates to all estimates is about what we would expect to occur if the true effect were zero given 90 and 95 percent confidence intervals. There are no estimates that suggest that the DTF causes students to attend four-year college (conditional on attending).

Table 8 shows the base case regression discontinuity effects of DTF eligibility on further college-related outcomes. There is little or no evidence that the DTF has a statistically significant causal effect on attending full-time (conditional on attending), on two-year college (conditional on attending), the instructional spending of the college attended, the core student-related expenditure of the college attended (an indicator of the school's resources), the "sticker price" tuition of the college attended (an indicator of how expensive it is for a full-pay student), the tuition actually paid (net of grant aid), or grants and scholarships received.

Interestingly, the DTF also does not statistically significant affect the interest paid on student loans in years 1 through 7. (For instance, we sum interest paid in 2003 through 2009 for the 2002 cohort.) This lack of an effect on interest paid suggests that when families receive the DTF, it does not reduce their student debt. Of course, the DTF unambiguously increases the family's income so it must increase consumption, increase saving, or reduce debt other than student loans (credit card debt, for instance).²³

C. Effects of Taking the Deduction for Tuition and Fees in the Current and Previous Year

We found no evidence that the DTF affects college-going outcomes in the year in which it is taken. There are several possible explanations, some of which we consider only in the next section. Here, however, let us consider whether families fail to understand the deduction well enough to recognize that it changes the price of college in the year in which it actually does change the price. Families who receive the DTF in year $t-1$ may act as though they qualify for a discounted price of college in year t --even though this is not the way the DTF works. If this is so, estimates of equation (7) should show them reacting to lagged DTF eligibility.

Estimates of the effect of lagged eligibility may also show families reacting to the DTF as an increase in income--income that arrives in year t although it is a function of year $t-1$'s DTF. Recall, though, that unless raw correlations are *downward* biased (highly implausible), Table 1 indicates that such income effects would be so small as probably to be indiscernable: A \$3,000 DTF that increases a household's income by about \$1,000 (\$3,000 DTF with a 28 percent marginal tax rate) could have *at most* a 0.14 percentage point effect on college attendance and a 0.18 percentage point effect on 4-year college attendance (conditional on attending).

Because it requires a household's income to have been in the vicinity of a DTF threshold in two subsequent years, we have fewer observations to estimate the lagged specification given by (7). Therefore, we focus on a \$20,000 bandwidth even for single filers. We also focus on the \$65,000 single and \$130,000 joint thresholds because, by combining all available years across the two DTF regimes, we can include four cohorts: 2002, 2003, 2004, 2007.²⁴ Finally, we focus on years 0, 1, and 2 in which people are most likely to attend college. (Results available from

²³ Note to the editors. We could check mortgage debt but have not done so yet.

²⁴ Recall that there was no DTF in the 2006 tax year. Thus, both the 2005 and 2006 cohorts must be omitted from the specification (7) with lagged eligibility. Estimates for the \$80,000 and \$160,000 thresholds can employ only the 2004 and 2007 cohorts. As a result, those estimates, while similar, have standard errors that are larger than desirable. These results are available from the authors.

the authors show similar effects for years 3 and 4.

Table 9 provides no indication that lagged or current eligibility for the DTF affects college attendance (at all), attending full-time (conditional on attending), attending four-year college (conditional on attending), attending two-year college (conditional on attending), the instructional spending of the college attended, the core student-related expenditure of the college attended, the sticker price of the college attended, the tuition actually paid, grants and scholarships received, or the interest paid on student loans in years 1 through 7. None of the estimated effects is statistically significantly different from zero. However, because the standard errors are larger with the lagged specification, the effects that we cannot rule out are slightly larger. For instance, we cannot rule out attendance effects of 2 percentage points for joint filers or 3 percentage points for single filers.

8 Discussion

A. Our Findings, Summarized

After testing a broad array of possibly affected outcomes using a method, regression discontinuity, that imposes minimal assumptions, we find no evidence that the DTF affects college-going. This conclusion is robust to choices about the polynomial and bandwidth used for the regression discontinuity specification.

We find that the DTF is taken up by a substantial share of households and that they take deductions close to the maximum allowed. They accurately apply the DTF rules to the MAGI they report to the Internal Revenue Service. As a result, many households that are paying tuition and fees *do* have higher net-of-tax incomes as a result of the DTF. They are spending or saving this income in some way, but it is evidently some way that does not affect college-going more than negligibly.

We find that households near the income eligibility cut-off for the DTF manage their MAGI so as to push it slightly below the cut-off. This income management need not be illegal evasion: Various legal forms of avoidance could generate the same result. Moreover, although income management is a nuisance for implementation of the regression discontinuity method, its implications for tax revenue and tax fairness are small. Most of the households who are practicing income management would have MAGI just above the cut-off (within \$2,000 or so) and end up reporting MAGI just below the cut-off (within \$2,000 or so). Put another way, the reason that we exercise considerable vigilance about income management--by imposing appropriate doughnut-holes--is that we wish to avoid producing estimates that suffer from reverse causality bias. If we were not concerned with producing causal estimates and were only concerned about lost tax revenue, such vigilance would make little difference.

B. What Explains the Tax Deduction's Lack of Effect on College-Going?

One explanation for the DTF's lack of effect on college-going is that households in the vicinity of the income cut-offs may be insensitive to the price of postsecondary education. That is, the elasticity of their college-going behaviors with respect to the price may be so low that it is indistinguishable from zero. This explanation is not implausible because families near the cut-offs are fairly affluent and may therefore be insensitive to prices.²⁵ Moreover, the vast majority

²⁵ Table 1 indicates that the eligibility cut-off occurs at the 82th percentile of income among joint filers with a 17 year-old in year -1 and the 89th percentile among single filers with a 17 year-old in year -1.

of the cost of college for most students is the *opportunity cost*, not tuition and fees. Perhaps we would not be surprised if we learned that a \$1,000 decrease in the opportunity cost of college produced a negligible change in college-going among people from households near the income cut-offs. If so, we should also not be surprised to learn that a DTF worth \$1,000 produced a negligible change in college-going among people from the same households.

However, it is possible that these households *are* sensitive to the price of college but that the DTF is structured in a way that makes it inefficacious. This possibility is especially interesting because the DTF's structure could be altered, without changing its potential cost, to make it more likely to achieve its intended, causal effects.

To see this, consider four interrelated aspects of the DTF that may make it inefficacious: the lack of salience of the DTF rules, the timing with which households become aware of the deduction for which they are eligible, the timing with which the deduction is received, and aspects of the DTF that make it likely to be perceived as additional income rather than a change in the price of college.

As noted earlier, the DTF rules are not obvious to a household that uses a tax preparer or tax preparation software. Even a filer who makes manual use of the tax forms would need to understand both the DTF's rules and the rules of the higher education tax credits (in other words, develop an abstract understanding of the formulae that underlie both form 8917 and form 8863). Otherwise, even a household that had no difficulty predicting its year-end MAGI and modified tax liability (which matters for the tax credits) would be unable to predict the DTF for which it was eligible.

This likely lack of salience *matters* because of the DTF's timing. Consider the time line for a typical prospective student who applies to college for the first time. In the third quarter of year $t-1$ or early in the first quarter of year t , she applies to colleges. Also in the first quarter of year t , she completes the Free Application for Federal Student Aid. In April of year t , she learns which schools have admitted her and what financial aid packages she has been offered. In May of year t , she accepts an admissions offer. This is also when she makes decisions about whether to take out loans and/or accept a work-study job. Some schools require a deposit on tuition and fees at this time. In August or September of year t , she pays tuition and fees for the fall term. In December of year t or in the first quarter of $t+1$, she pays tuition and fees for the spring term(s). Finally, by April 15 of year $t+1$, she or--more likely--her parents file their taxes. When they do so, they realize the deduction or, at a minimum, appreciate its income implications.

In short, the prospective student makes all of her college-going decisions--where to apply, which college to accept, which financial aid package to take, what loans to assume--10 to 18 months before the deduction is realized. Thus, if she fails to predict the DTF well in advance, her decisions may be unaffected or affected in some way that produces a negligible causal effect.

Moreover, even if the student is expert at predicting the DTF, she or her parents may be liquidity constrained at the times when tuition and fees are due. Since the deduction will not have been realized at those times, the DTF will not relax such liquidity constraints.²⁶ Its future receipt will not affect college choices. Thus, even a household that fully understands the DTF may act as though it did not exist.

²⁶ In theory, a household that accurately predicts its receipt of a DTF could petition its employer(s) to reduce its withholding, thereby realizing the DTF before it files. However, such changes in withholding are extremely rare.

Finally, the DTF seems designed to convince families that it is an increase in parents' income rather than a decrease in the price of the student's college. It is computed on separate forms (or in a series of rule-obscuring frames if a tax preparer or software is used), transferred to one of a series of deduction lines on form 1040, eventually multiplied by the household's tax rate, and ends up reducing the taxes paid by the filer, who is usually not the student. A family *might* make all of the connections and recognize that the DTF is actually a discount on the price of college, not an increase in net-of-tax income. However, it is doubtful whether the ordinary family is so savvy. We have already seen that if the family perceives the DTF to be a change in income rather than price, the effect on college-going is likely to be negligible.

C. The Tax Credits' Analogous Issues and Lack of Effect on College-Going

In previous work (Bulman and Hoxby 2015), we demonstrate that the tax credits for higher education--which cost as much as \$25 billion a year--also have little or no effect on college-going. Moreover, some estimates in that work are "local" to households with modest incomes: married joint filers with \$25,000 to \$50,000, for instance. The tax credits appear not to affect their college-going, but it seems implausible that such households are simply too affluent to be sensitive to the price of college.

While the credits differ from the DTF (especially because they are available even to low income households with little or no tax liability), the credits share the features likely to make the DTF inefficacious. Specifically, the credits may lack salience. Households probably learn about their eligibility for the credits long after having made college-related decisions. Households need to pay tuition and fees an average of 10.5 months before receiving the credits. The manner in which the credits are computed and received makes them likely to be perceived as income rather than as a change in the price of college.

If it is their *shared* features that make the credits and DTF inefficacious, we should not be surprised that both programs, despite their distinctions, have little or no effect on college-going.

9. Simple revisions to the DTF might increase its causal effects

To illustrate the point that relatively arbitrary features of the DTF may explain its inefficacy, we propose a simple thought experiment. Suppose that instead of the DTF being based on tuition and fees and MAGI in year t , it were based on tuition and fees in year t (the tax year) and MAGI in the year in which a person was age 17. Suppose also that schools could file to receive the DTF directly from government.

In this case, the DTF would work as follows. After a household filed taxes in the year in which its child was age 17, it would receive a notice saying that its child's price of college would be discounted by its tax rate up to a total discount equal to the maximum DTF. This discount might apply in up to a total of four school years out of the next seven.²⁷ Thus, a joint filing household with MAGI of \$120,000 would be notified that its child would receive a 28 percent discount off tuition and fees up to a maximum discount of \$1,120 (28 percent of \$4,000). The

²⁷ These constraints could be varied depending on whether Congress intends the DTF to support graduate as well as undergraduate education. Because some children progress slowly through elementary and secondary school or take a "gap year" between high school and college, one would not want to constrain people to take their DTF in--say--only four possible school years.

household would receive this notice at about the same time of year as it would receive news about the financial aid for which its child would qualify.²⁸ Suppose moreover that the household could show this notice to a college when its tuition was due. At that point, the college could collect the discounted tuition and file a claim with the Treasury for the amount of the discount. Since the discount formula would be *predetermined*, there would be little reason why the Treasury could not send the funds to the college quickly--just as the U.S. Department of Education sends Pell grants to colleges quickly. (Pell grants are also based on *current* tuition and a *predetermined* formula. Pell grants are paid directly to colleges just as the DTF would be.) Households could receive annual reminder notices of their DTF eligibility until the four school years of use or seven school years of eligibility were exhausted, whichever came sooner.

This simple modification would make the DTF much more salient. It would ensure that most people knew about the DTF when making key college-going decisions. Indeed, they would know about the DTF at the same time they learned about other financial aid. The DTF would reduce payments at the time they were due, thereby relaxing liquidity constraints. The DTF would likely be perceived as a change in the price, much as a coupon is perceived. In other words, if the *structure* of the DTF makes it inefficacious, the modification might make it efficacious. If households fail to respond to the DTF because they are truly insensitive to the price of college, the modification would make no difference.

The modified DTF could be made approximately budget neutral in a prospective sense. That is, the thresholds could be set to make each household eligible for the same expected discount. However, it is not obvious why the current structure of the DTF should be privileged in some way. That is, it is unclear why the income of a person's household at age 18 is a *better* measure of need for college support than the income of a person's household at age 17.

There might be three additional advantages of a DTF based on age 17 household income. First, prospective students would be able to plan their college education based on a certain discount rather than an uncertain one that could disappear under circumstances beyond the student's control. Second, the Treasury and tax preparers could easily make DTF "forecasters" available to households who are filing their taxes for the year when their child was age 16. Like the FAFSA4caster, such tools would provide students and their parents with an early estimate of their eligibility. Third, a measure based on age 17 household income would reduce families' incentives to "game" the dependent/independent classification in an effort to obtain more generous financial aid.

The only clear disadvantage to a DTF based on age 17 household income is that it would give families additional incentive to manage their MAGI so as to get just below the eligibility threshold. (The incentive would be greater because more years of eligibility would be determined at one time.) Such income management could easily be eliminated by making the DTF phase out rather than sharply cut-off at a threshold. There is no evidence that households manage income to obtain the tax credits for higher education which phase out rather than sharply cut off (Bulman and Hoxby 2015).

If economic logic suggests that modifying the structure of the DTF might make it more

²⁸ This would typically be true if the child were born in the first three quarters of the year and had progressed in school in an on-time manner. If the child had an fourth quarter birthday and/or had progressed in school with a year's delay, the family would learn of its DTF eligibility one year ahead of its child receiving financial aid information.

efficacious, the same logic would suggest parallel modifications for the larger program of tax credits for higher education.

It would not be impossible to test rigorously whether the DTF's and credits' inefficacy is due to their peculiar features or households' insensitivity to the price of college. A randomized controlled trial that varied eligibility and notification in the manner described above (while holding constant all other DTF rules) might elucidate how households interact with the tax expenditures for higher education. Since the DTF and credits potentially support higher education investments with minimal administrative cost, understanding them better might not only clarify household behavior but might be useful for policy.

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Figure 1

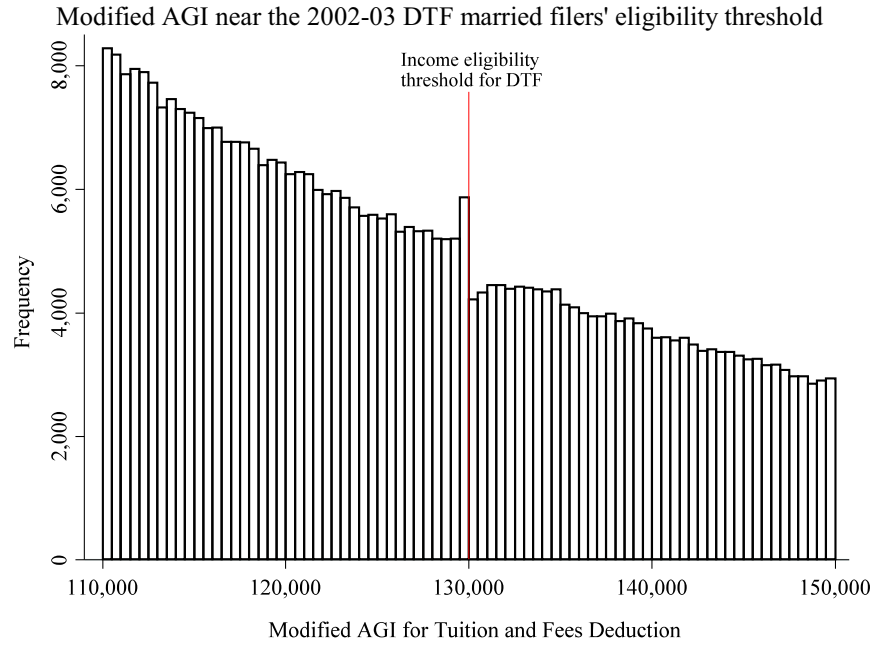
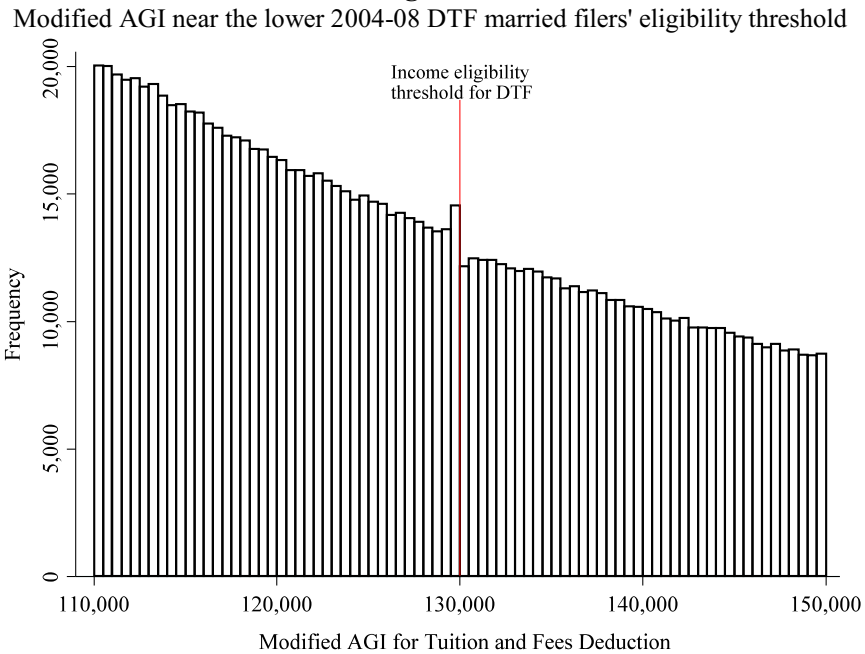


Figure 2



Notes: Figures are histograms of modified adjusted gross income in \$500 bins in year 1 (2002 and 2003 or 2004 through 2008) among married joint filers who had a child who would have graduated from high school in year 0 if he or she had progressed through secondary school on time. Source: De-identified tax data.

Figure 3

Modified AGI near the upper 2004-08 DTF married filers' eligibility threshold

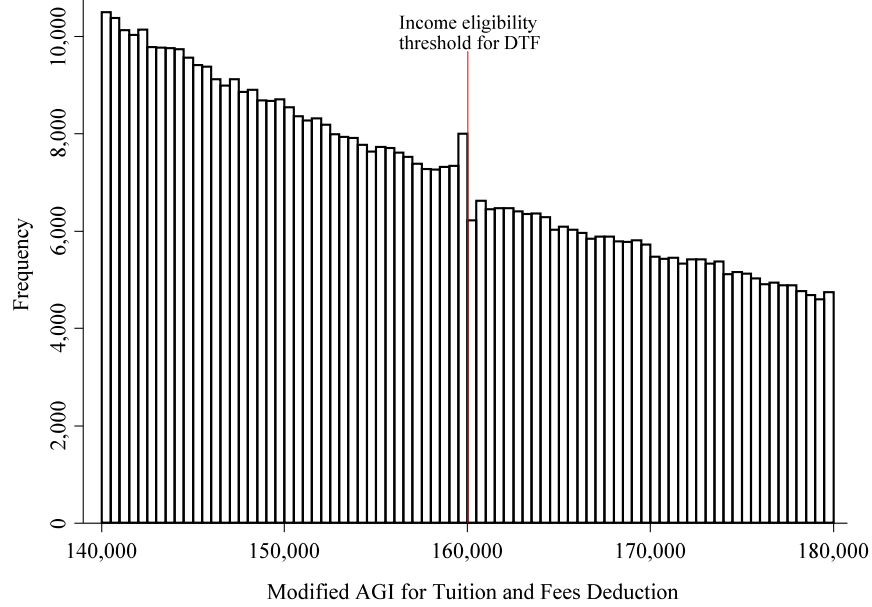
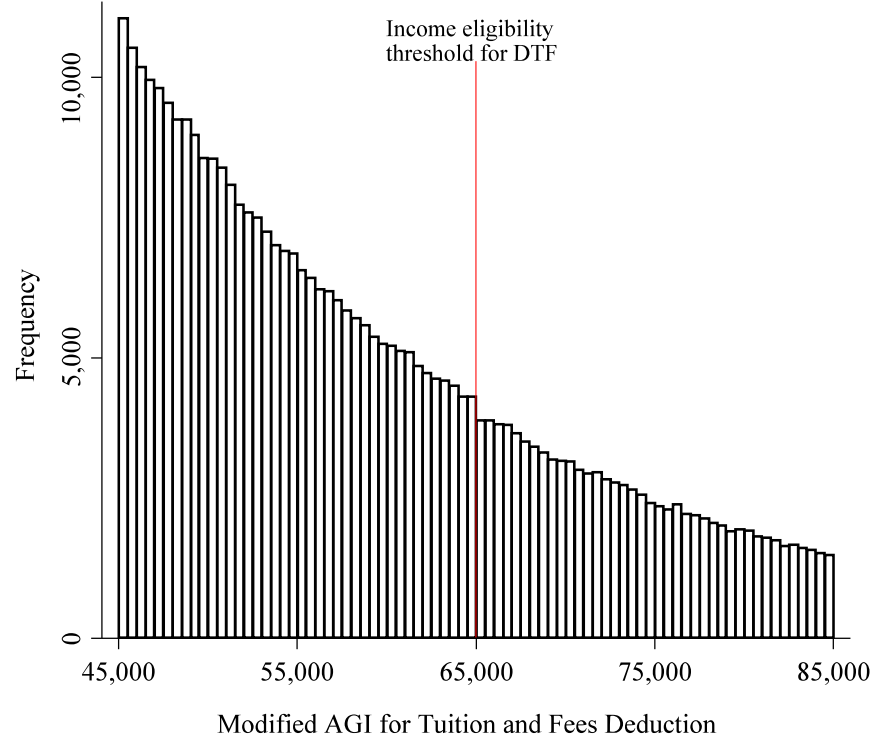


Figure 4

Modified AGI near the 2002-03 DTF single filers' eligibility threshold



Notes: Figures are histograms of modified adjusted gross income in \$500 bins in year 1 (2002 and 2003 or 2004 through 2008) among married joint or single filers who had a child who would have graduated from high school in year 0 if he or she had progressed through secondary school on time. Source: de-identified tax data.

Figure 5

Modified AGI near the lower 2004-08 DTF single filers' eligibility threshold

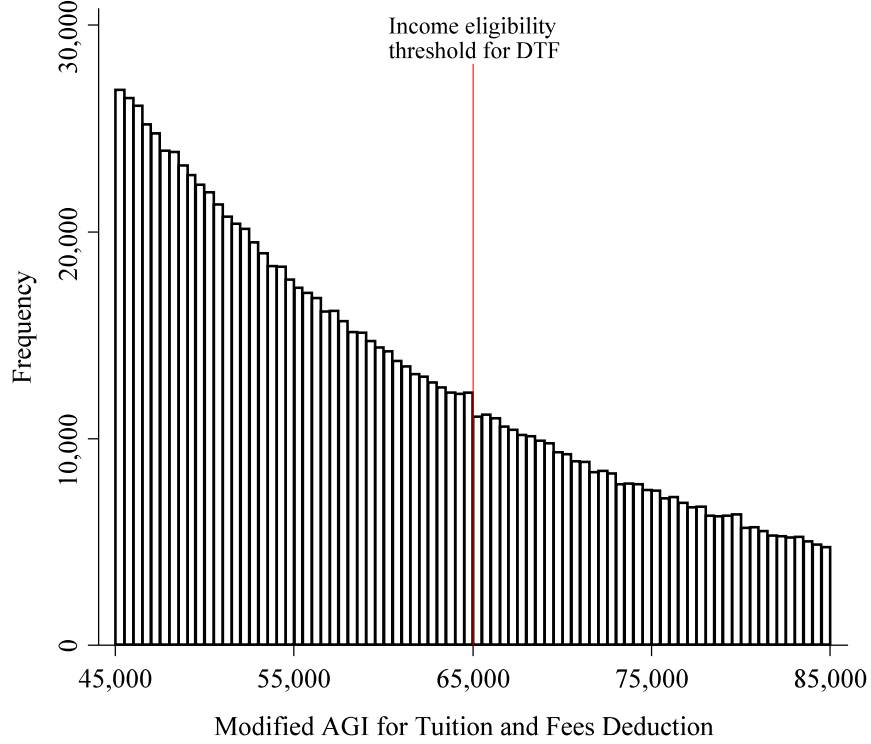
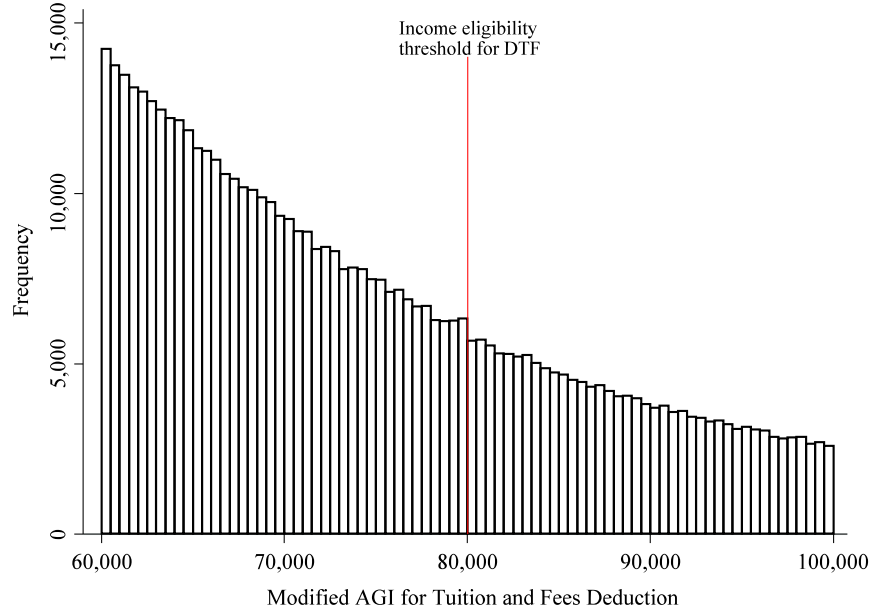


Figure 6

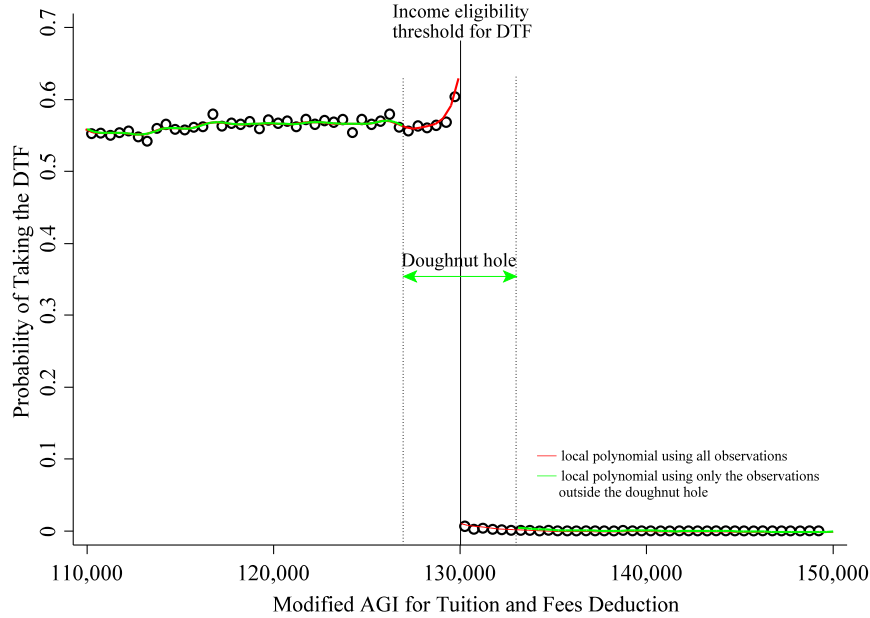
Modified AGI near the upper 2004-08 DTF single filers' eligibility threshold



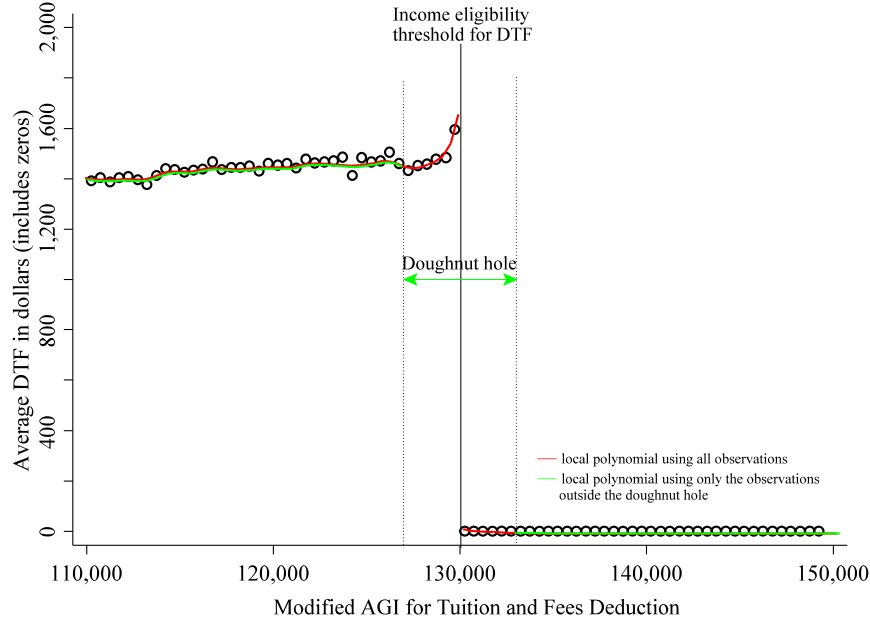
Notes: Figures are histograms of modified adjusted gross income in \$500 bins in year 1 (2004 through 2008) among single filers who had a child who would have graduated from high school in year 0 if he or she had progressed through secondary school on time. Source: de-identified tax data.

Figure 7

(a) Taking the DTF and modified AGI near 2003-04 married eligibility threshold



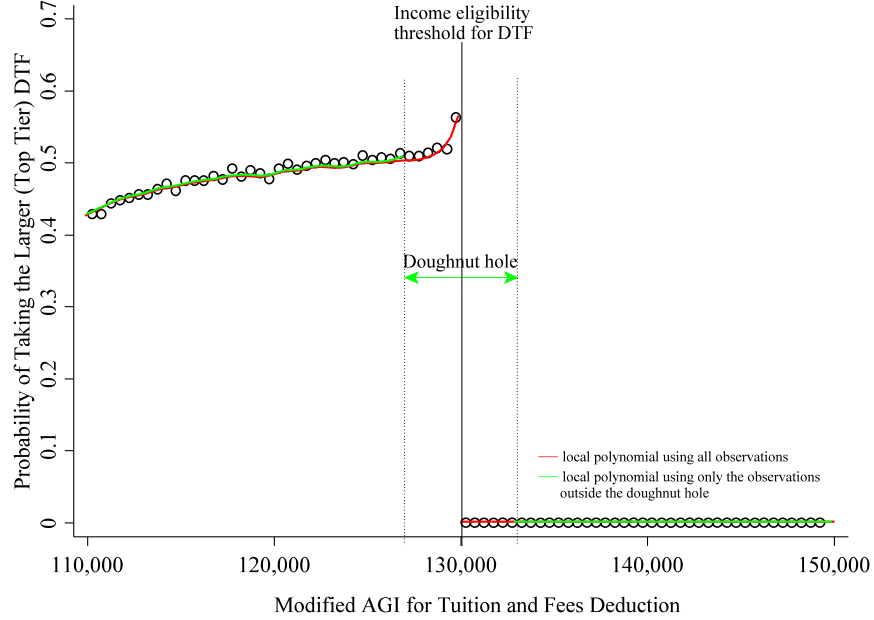
(b) Average DTF and modified AGI near 2003-04 married eligibility threshold



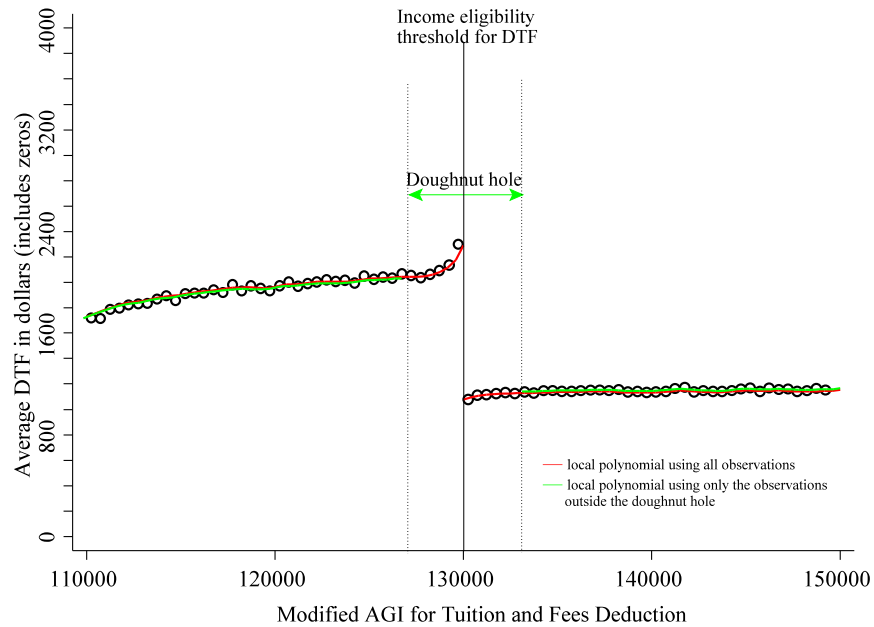
Notes: Figures show the probability of taking the DTF and average DTF in dollars as a function of modified adjusted gross income in year 1 (2002 and 2003) among married joint filers who had a child who would have graduated from high school in year 0 if he or she had progressed through secondary school on time. Average DTF includes zeros (non-takers). Each dot summarizes data in a \$500 interval. The figures also show smoothed values from gaussian kernel-weighted local polynomial regressions (degree 1, bandwidth 500) that include all observations or only those outside the doughnut hole. Source: de-identified tax data.

Figure 8

(a) Taking DTF and modified AGI at lower 2004-08 married eligibility threshold



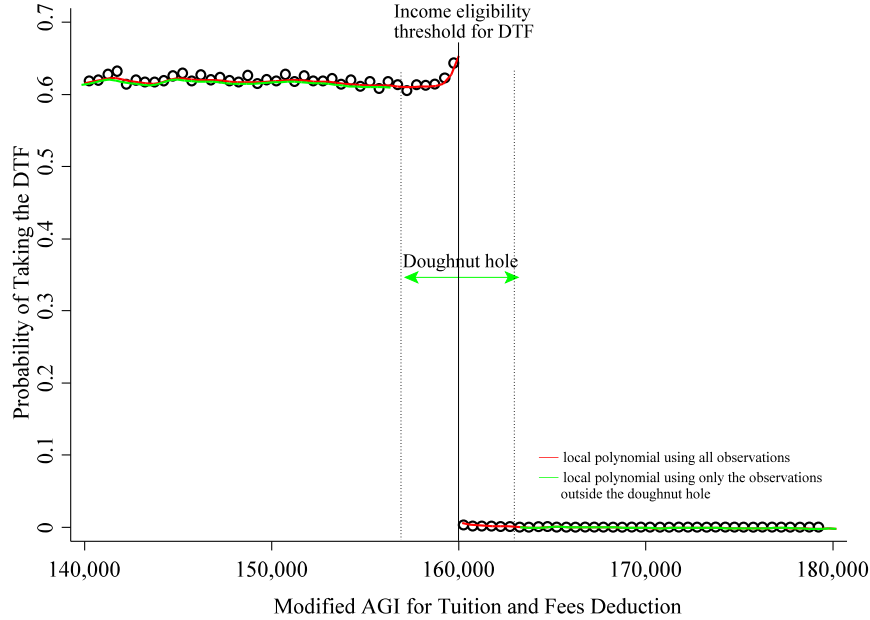
(b) Average DTF and modified AGI at lower 2004-08 married eligibility threshold



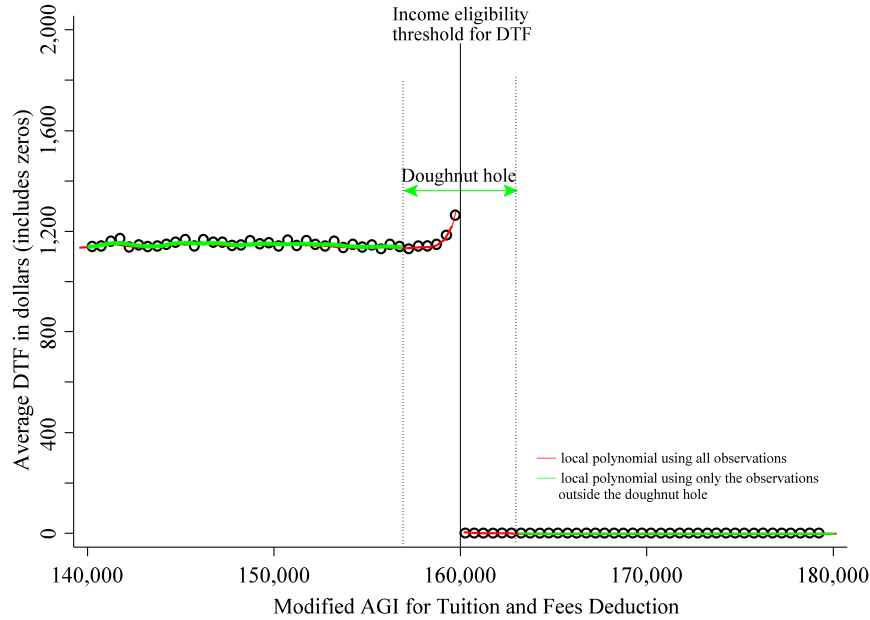
Notes: Figures show the probability of taking the DTF and average DTF in dollars as a function of modified adjusted gross income in year 1 (2004 through 2008) among married joint filers who had a child who would have graduated from high school in year 0 if he or she had progressed through secondary school on time. Average DTF includes zeros (non-takers). Each dot summarizes data in a \$500 interval. The figures also show smoothed values from gaussian kernel-weighted local polynomial regressions (degree 1, bandwidth 500) that include all observations or only those outside the doughnut hole. Source: de-identified tax data.

Figure 9

(a) Taking DTF and modified AGI at upper 2004-08 married eligibility threshold



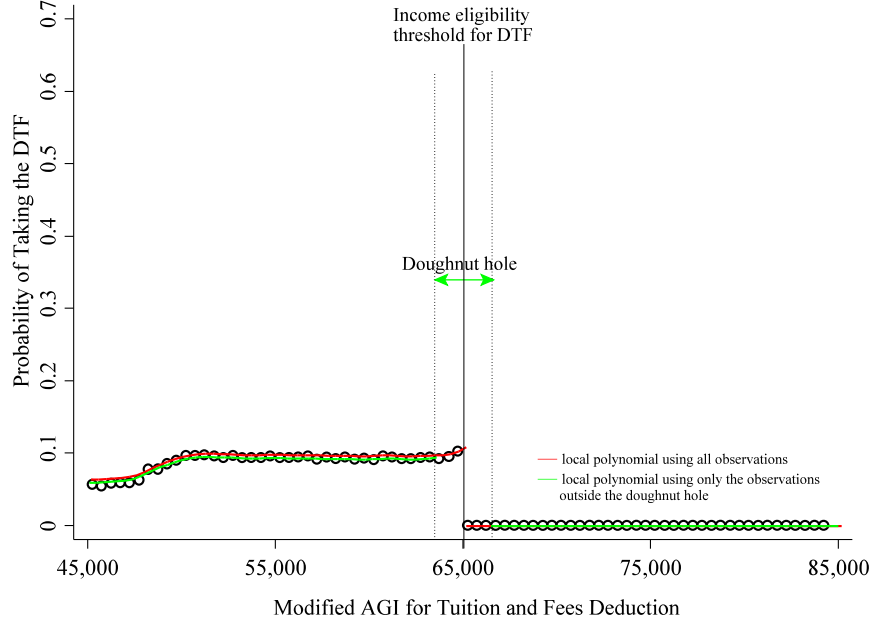
(b) Average DTF & modified AGI at upper 2004-08 married eligibility threshold



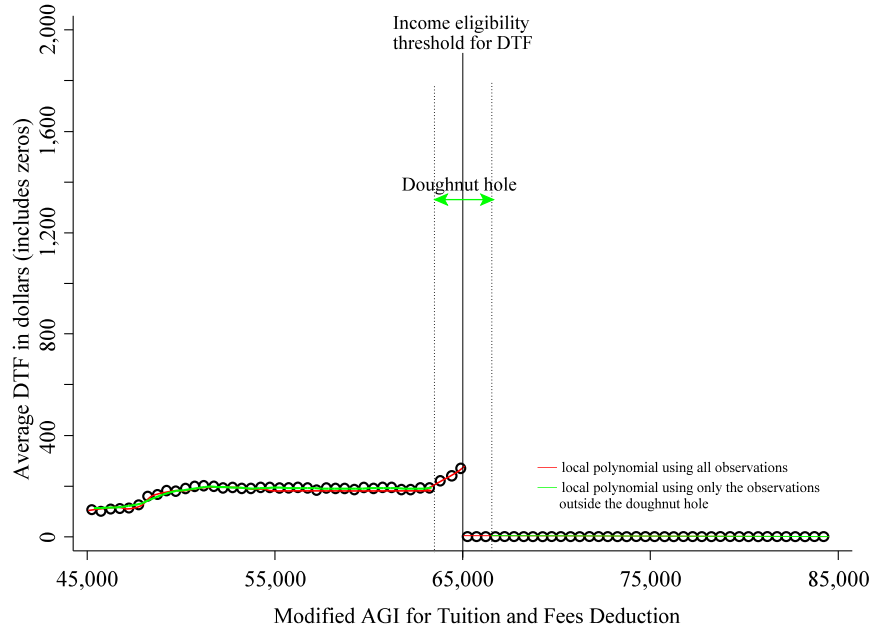
Notes: Figures show the probability of taking the DTF and average DTF in dollars as a function of modified adjusted gross income in year 1 (2004 through 2008) among married joint filers who had a child who would have graduated from high school in year 1 if he or she had progressed through secondary school on time. Average DTF includes zeros (non-takers). Each dot summarizes data in a \$500 interval. The figures also show smoothed values from gaussian kernel-weighted local polynomial regressions (degree 1, bandwidth 500) that include all observations or only those outside the doughnut hole. Source: de-identified tax data.

Figure 10

(a) Taking DTF and modified AGI at 2002-03 single eligibility threshold



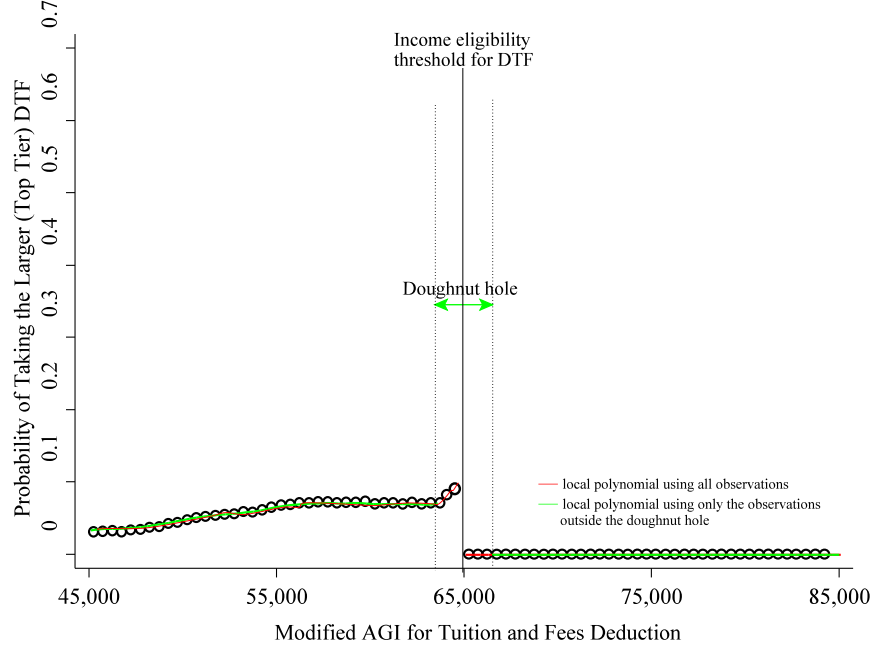
(b) Average DTF and modified AGI at 2002-03 single eligibility threshold



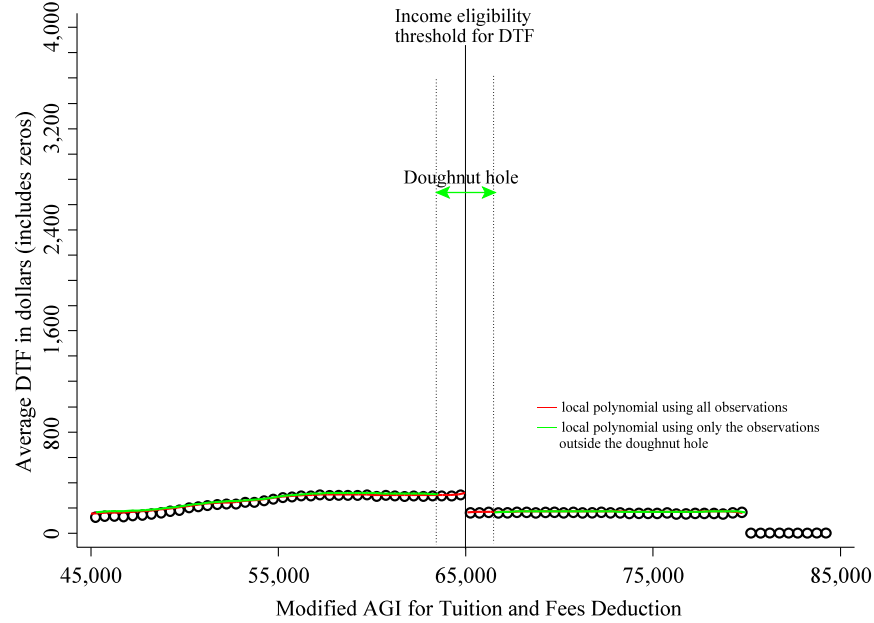
Notes: Figures show the probability of taking the DTF and average DTF in dollars as a function of modified adjusted gross income in year 1 (2002 and 2003) among single filers who had a child who would have graduated from high school in year 0 if he or she had progressed through secondary school on time. Average DTF includes zeros (non-takers). Each dot summarizes data in a \$500 interval. The figures also show smoothed values from gaussian kernel-weighted local polynomial regressions (degree 1, bandwidth 500) that include all observations or only those outside the doughnut hole. Source: de-identified tax data.

Figure 11

(a) Taking DTF and modified AGI at lower 2004-08 single eligibility threshold



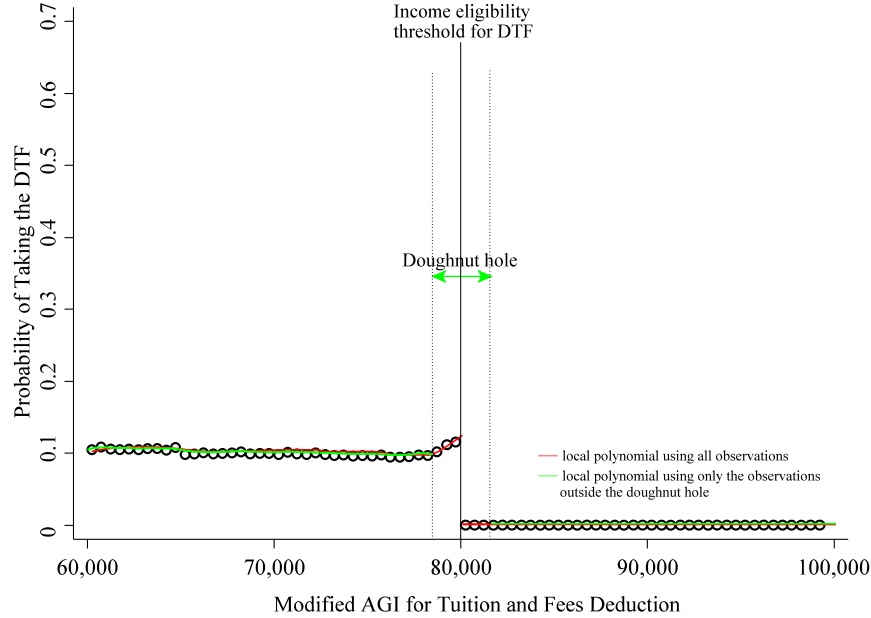
(b) Average DTF and modified AGI at lower 2004-08 single eligibility threshold



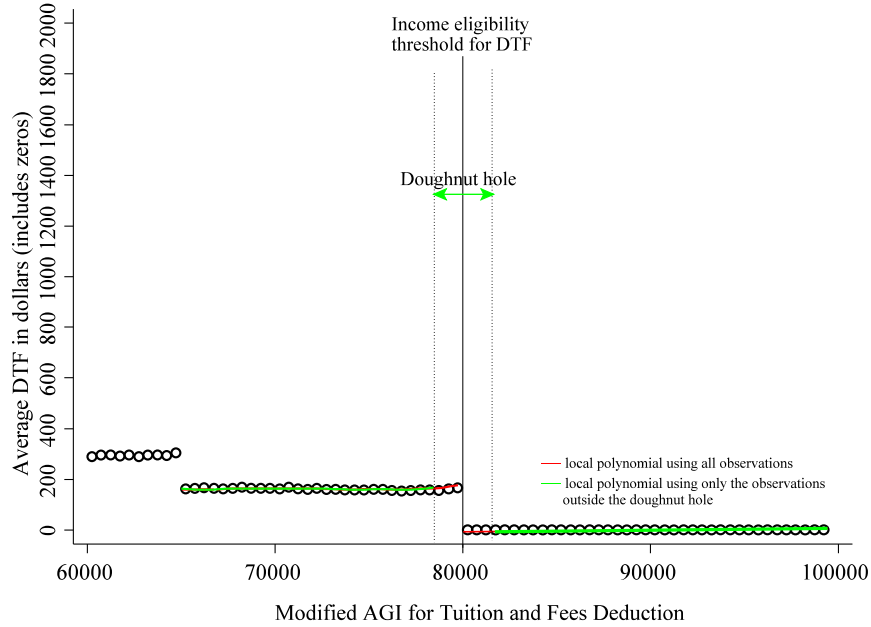
Notes: Figures show the probability of taking the DTF and average DTF in dollars as a function of modified adjusted gross income in year 1 (2004 through 2008) among single filers who had a child who would have graduated from high school in year 0 if he or she had progressed through secondary school on time. Average DTF includes zeros (non-takers). Each dot summarizes data in a \$500 interval. The figures also show smoothed values from gaussian kernel-weighted local polynomial regressions (degree 1, bandwidth 500) that include all observations or only those outside the doughnut hole. Source: de-identified tax data.

Figure 12

Taking DTF and modified AGI at upper 2004-08 single eligibility threshold



(b) Average DTF and modified AGI at upper 2004-08 single eligibility threshold



Notes: Figures show the probability of taking the DTF and average DTF in dollars as a function of modified adjusted gross income in year 1 (2004 through 2008) among single filers who had a child who would have graduated from high school in year 0 if he or she had progressed through secondary school on time. Average DTF includes zeros (non-takers). Each dot summarizes data in a \$500 interval. The figures also show smoothed values from gaussian kernel-weighted local polynomial regressions (degree 1, bandwidth 500) that include all observations or only those outside the doughnut hole. Source: de-identified tax data.

Figure 13

Stylized illustration of a causal effect of the DTF (left panel) versus pure income management (right panel)

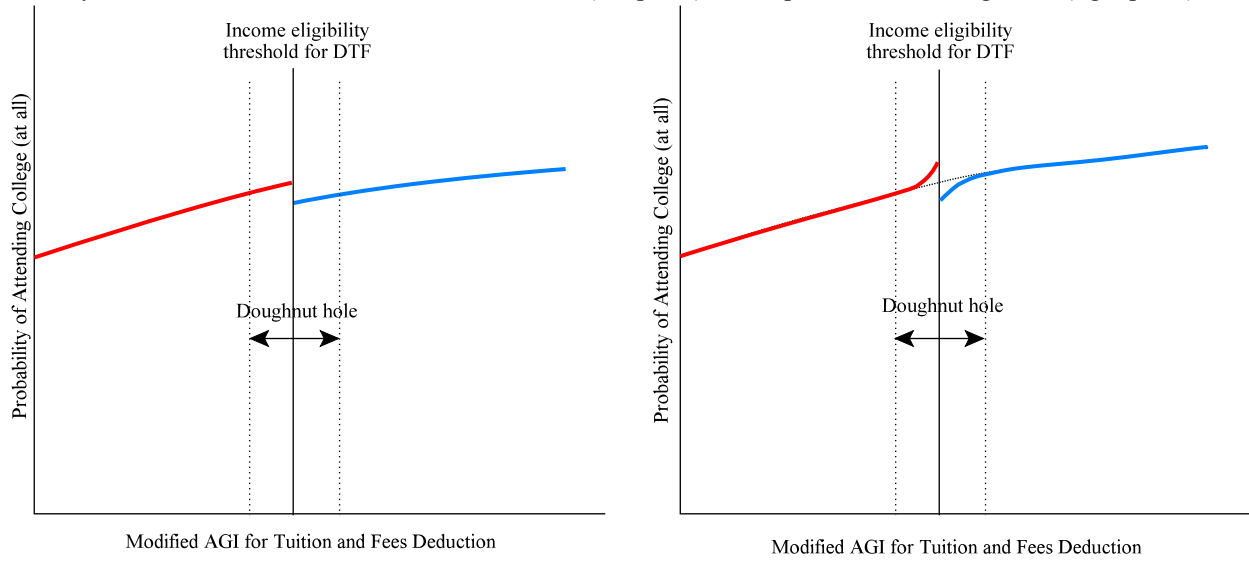
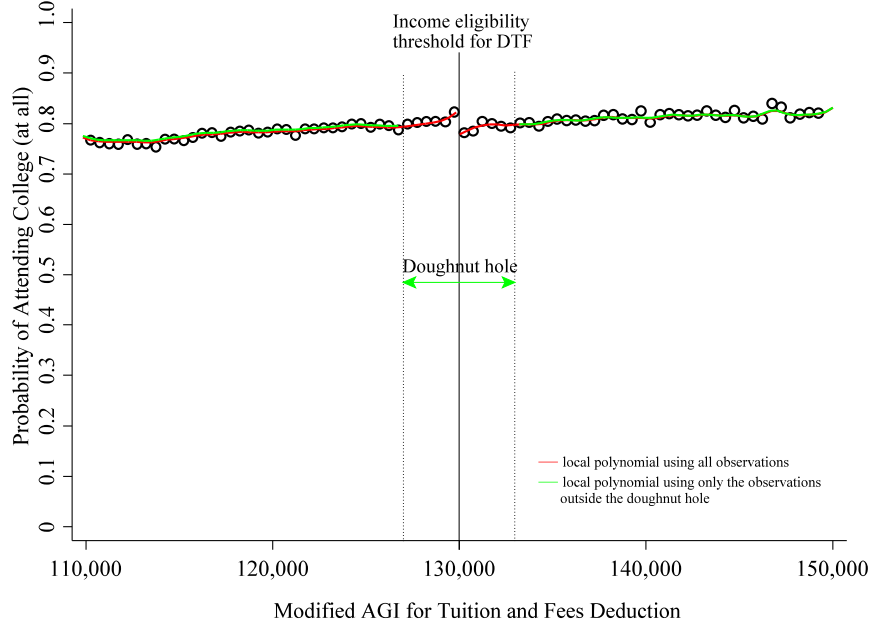
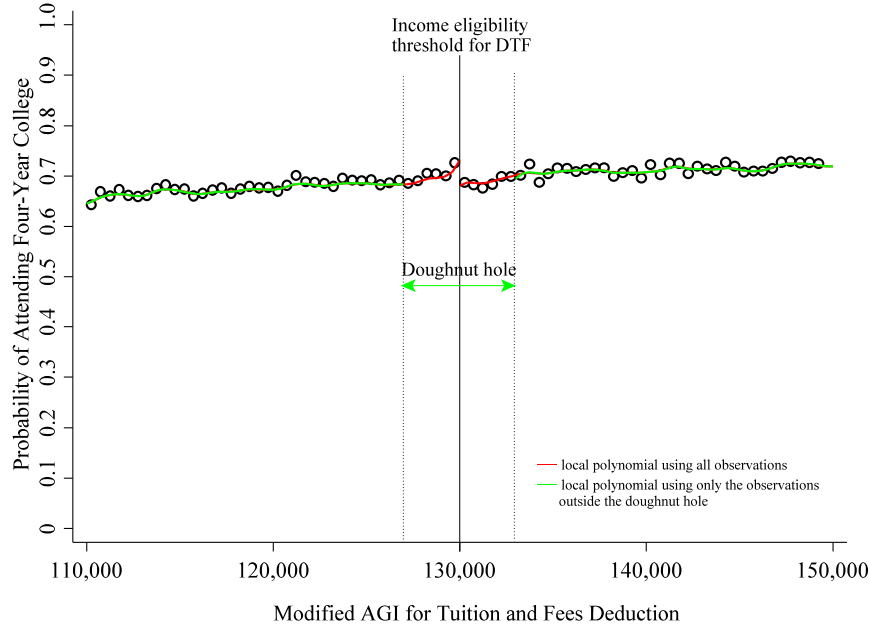


Figure 14

(a) Attending college and modified AGI at 2002-03 married eligibility threshold



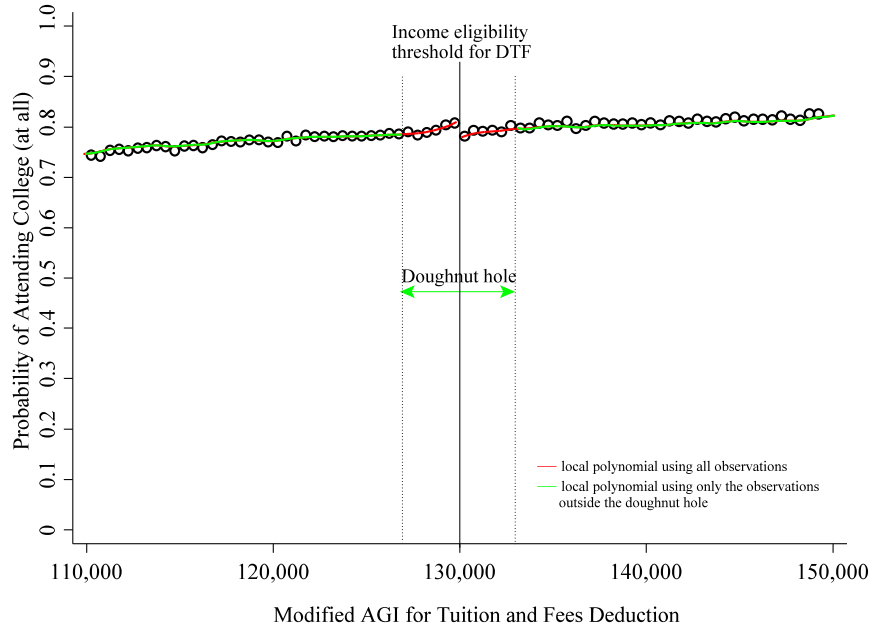
(b) Four-year college and modified AGI at 2003-04 married eligibility threshold



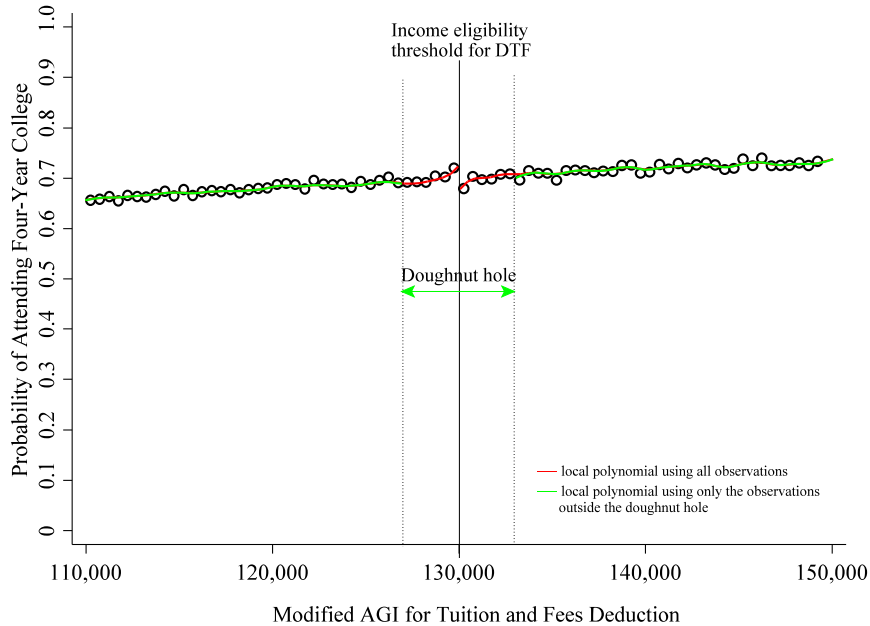
Notes: Figures show the probability of attending college (at all) and attending four-year college (conditional on attending at all) as a function of modified adjusted gross income in year 1 (2002 and 2003) among married joint filers who had a child who would have graduated from high school in year 0 if he or she had progressed through secondary school on time. Each dot summarizes data in a \$500 interval. The figures also show smoothed values from gaussian kernel-weighted local polynomial regressions (degree 1, bandwidth 500) that include all observations or only those outside the doughnut hole. Source: de-identified tax data.

Figure 15

(a) Attending college (at all) and modified AGI at the lower 2004-08 married eligibility threshold



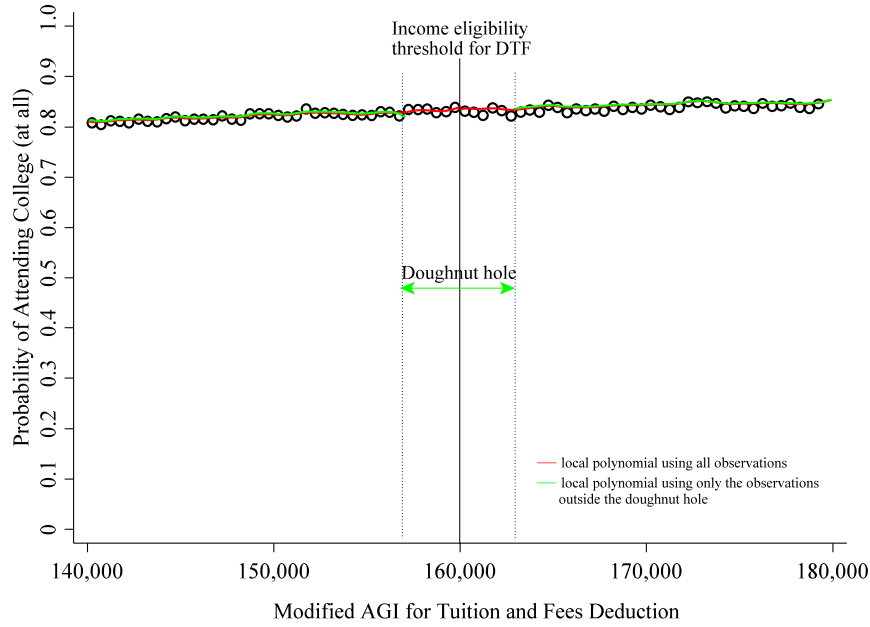
(b) Attending four-year college and modified AGI at the lower 2004-08 married eligibility threshold



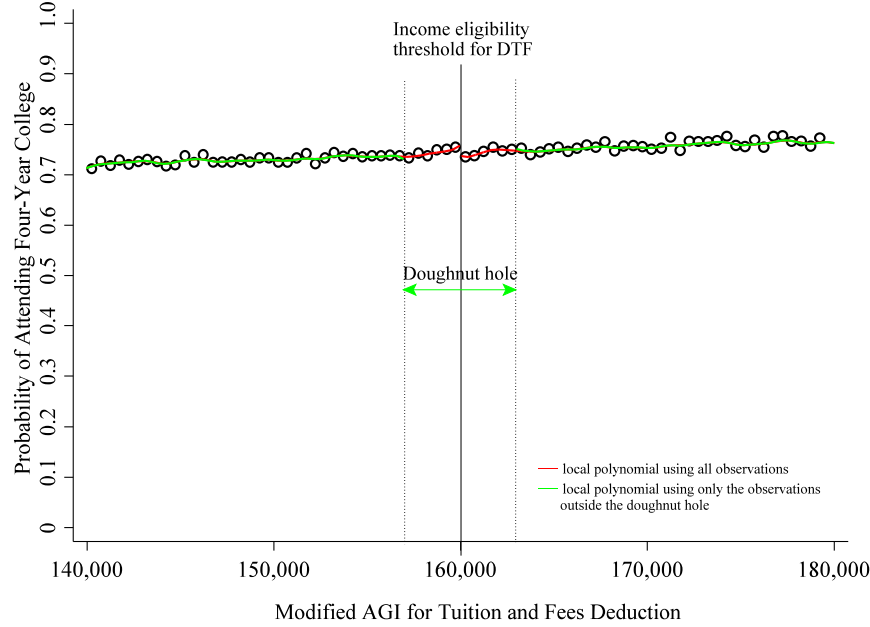
Notes: Figures show the probability of attending college (at all) and attending four-year college (conditional on attending at all) as a function of modified adjusted gross income in year 1 (2004 through 2008) among married joint filers who had a child who would have graduated from high school in year 0 if he or she had progressed through secondary school on time. Each dot summarizes data in a \$500 interval. The figures also show smoothed values from gaussian kernel-weighted local polynomial regressions (degree 1, bandwidth 500) that include all observations or only those outside the doughnut hole. Source: de-identified tax data.

Figure 16

(a) Attending college (at all) and modified AGI at the upper 2004-08 married eligibility threshold



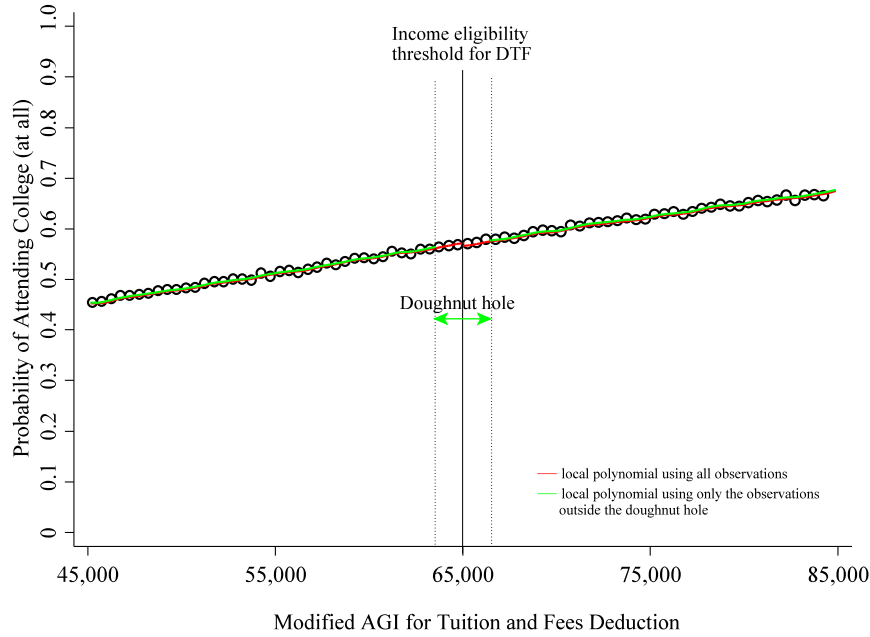
(b) Four-year college and modified AGI at the upper 2004-08 married eligibility threshold



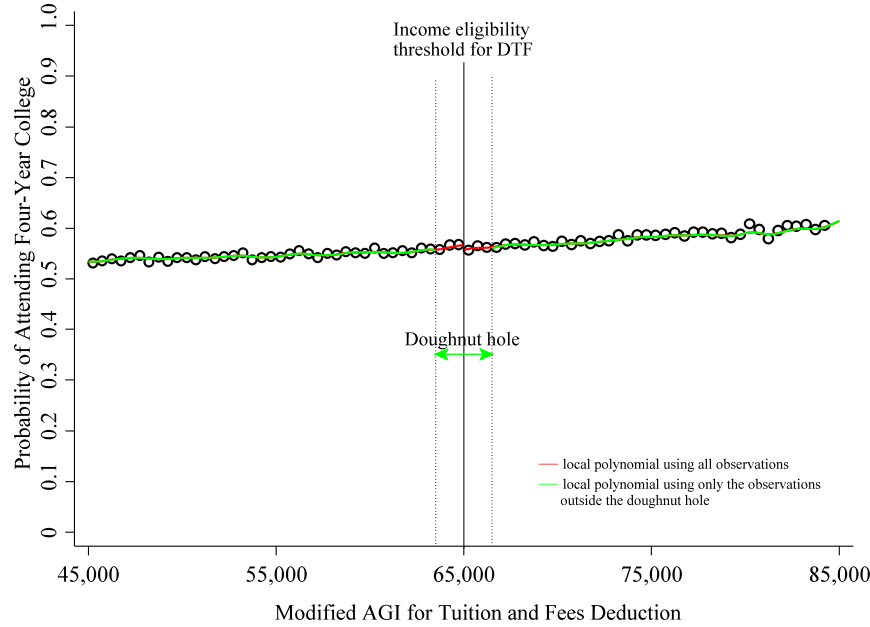
Notes: Figures show the probability of attending college (at all) and attending four-year college (conditional on attending at all) as a function of modified adjusted gross income in year 1 (2004 through 2008) among married joint filers who had a child who would have graduated from high school in year 0 if he or she had progressed through secondary school on time. Each dot summarizes data in a \$500 interval. The figures also show smoothed values from gaussian kernel-weighted local polynomial regressions (degree 1, bandwidth 500) that include all observations or only those outside the doughnut hole. Source: de-identified tax data.

Figure 17

(a) Attending college and modified AGI at the 2002-03 single eligibility threshold



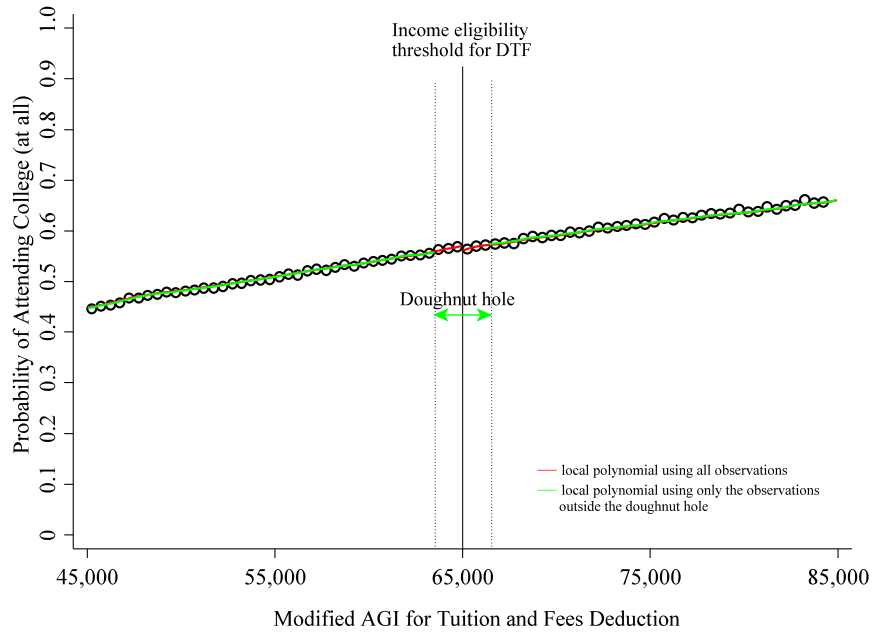
(b) Four-year college and modified AGI at the 2002-03 single eligibility threshold



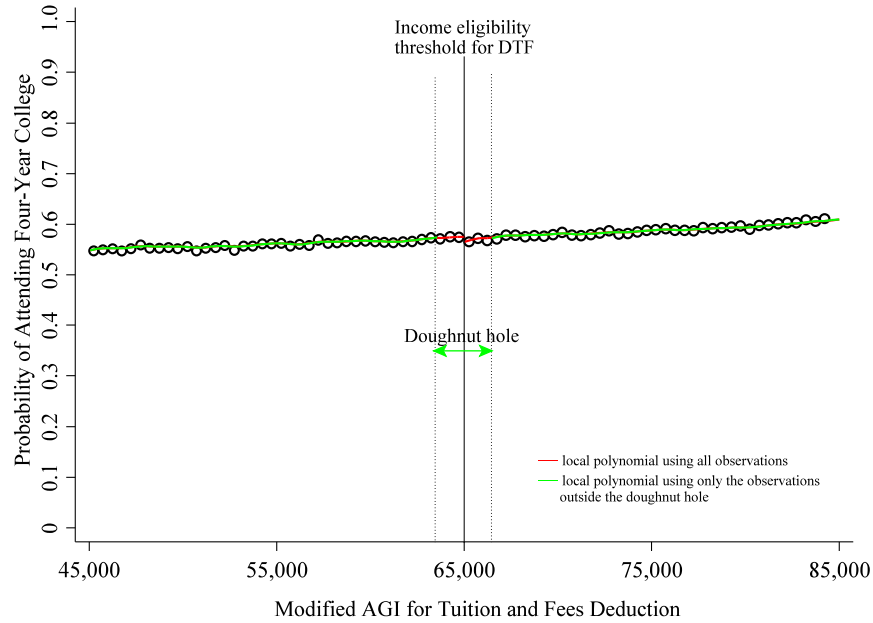
Notes: Figures show the probability of attending college (at all) and attending four-year college (conditional on attending at all) as a function of modified adjusted gross income in year 1 (2002 and 2003) among single filers who had a child who would have graduated from high school in year 0 if he or she had progressed through secondary school on time. Each dot summarizes data in a \$500 interval. The figures also show smoothed values from gaussian kernel-weighted local polynomial regressions (degree 1, bandwidth 500) that include all observations or only those outside the doughnut hole. Source: de-identified tax data.

Figure 18

(a) Attending college and modified AGI at the lower 2004-08 single eligibility threshold



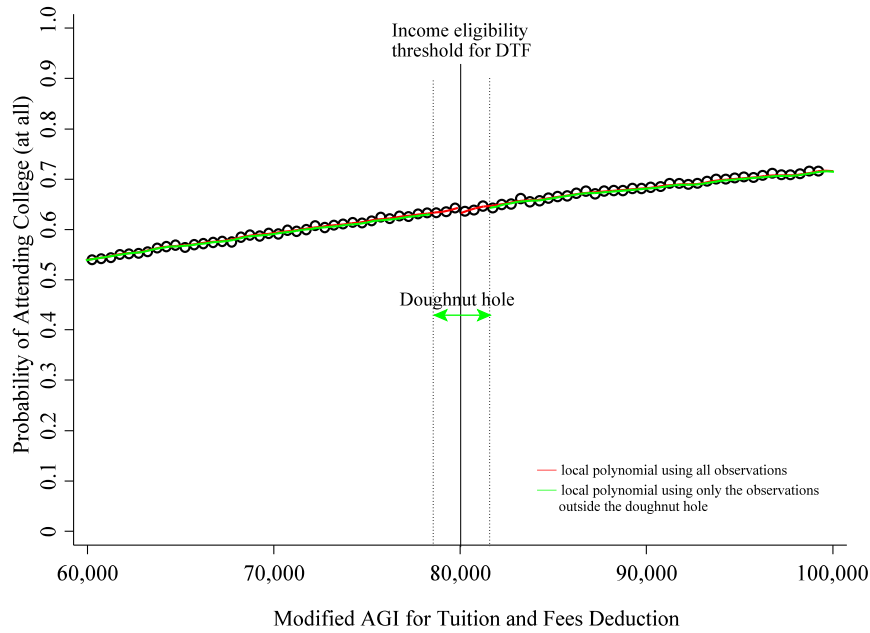
(b) Four-year college and modified AGI at the lower 2004-08 single eligibility threshold



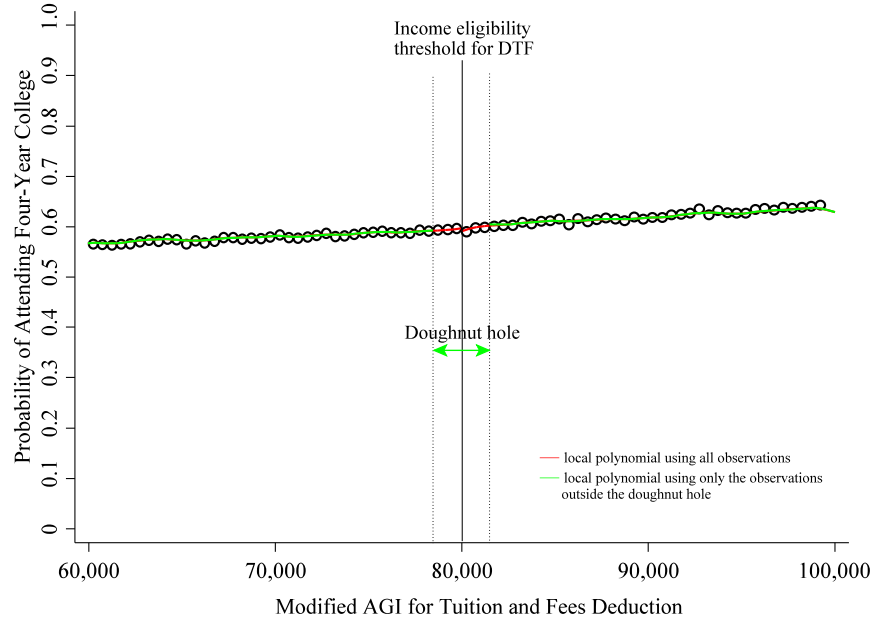
Notes: Figures show the probability of attending college (at all) and attending four-year college (conditional on attending at all) as a function of modified adjusted gross income in year 1 (2004 through 2008) among single filers who had a child who would have graduated from high school in year 0 if he or she had progressed through secondary school on time. Each dot summarizes data in a \$500 interval. The figures also show smoothed values from gaussian kernel-weighted local polynomial regressions (degree 1, bandwidth 500) that include all observations or only those outside the doughnut hole. Source: de-identified tax data.

Figure 19

(a) Attending college and modified AGI at the upper 2004-08 single eligibility threshold



(b) Four-year college and modified AGI at the upper 2004-08 single eligibility threshold



Notes: Figures show the probability of attending college (at all) and attending four-year college (conditional on attending at all) as a function of modified adjusted gross income in year 1 (2004 through 2008) among single filers who had a child who would have graduated from high school in year 0 if he or she had progressed through secondary school on time. Each dot summarizes data in a \$500 interval. The figures also show smoothed values from gaussian kernel-weighted local polynomial regressions (degree 1, bandwidth 500) that include all observations or only those outside the doughnut hole. Source: de-identified tax data.

Table 1a
College-Related Outcomes for the 2004 Cohort from Joint Filing Households
all outcomes are for year 1 except as noted

All outcomes below for those who enroll in at least some postsecondary education										
Modified Adjusted Gross Income	Number of Households	Attend Post-secondary at All	Attend at Least Half-Time	Attend a Four-Year College	Tuition and Fees Paid (\$)	Grants and Scholarships Received (\$)	"List" Tuition and Fees	Instructional Spending Per Student	Core Educational Resources	Interest Paid on Student Loans through year 7 (2011)
\$0-25k	223,253	32.0%	86.4%	54.5%	8,829	6,397	6,314	6,033	14,731	778
\$20-45k	298,369	40.1%	87.6%	54.4%	7,961	5,417	6,429	5,962	14,527	959
\$45-55k	174,107	48.6%	88.8%	55.6%	7,801	4,269	6,590	5,974	14,564	1,171
\$55-65k	183,033	54.5%	89.8%	57.0%	7,867	3,850	6,796	6,003	14,644	1,302
\$65-70k	90,682	58.8%	89.9%	58.2%	8,404	3,883	6,876	6,107	14,895	1,390
\$70-75k	88,492	61.4%	90.4%	59.0%	8,033	3,418	7,032	6,122	14,951	1,444
\$75-80k	85,350	63.5%	90.7%	59.7%	8,617	3,825	7,092	6,236	15,220	1,494
\$80-90k	155,395	67.5%	90.9%	61.7%	8,746	3,534	7,319	6,365	15,546	1,519
\$90-110k	236,533	72.7%	91.4%	64.7%	10,332	3,640	7,682	6,621	16,182	1,548
\$110-120k	83,681	77.0%	91.6%	68.1%	11,024	3,574	8,174	7,012	17,125	1,521
\$120-130k	66,986	79.4%	91.9%	70.2%	13,236	3,908	8,566	7,253	17,753	1,491
\$130-140k	51,764	80.8%	91.9%	72.0%	15,169	4,171	8,842	7,525	18,426	1,457
\$140-150k	41,564	82.1%	92.0%	74.2%	15,399	3,426	9,280	7,790	19,050	1,384
\$150-160k	33,680	83.2%	92.4%	75.3%	16,461	3,228	9,627	8,064	19,732	1,337
\$160-170k	26,518	83.7%	92.1%	76.3%	17,067	3,222	9,896	8,231	20,168	1,242
\$170-180k	21,843	84.9%	92.4%	78.0%	17,562	3,354	9,996	8,410	20,586	1,161
\$180k +	189,049	87.4%	92.1%	84.2%	27,031	2,637	12,765	10,212	25,096	709

Notes follow at bottom of Table 1b.

Table 1b
College-Related Outcomes for the 2004 Cohort from Single Filing Households
all outcomes are for year 1 except as noted

All outcomes below are conditional on some postsecondary attendance

Modified Adjusted Gross Income	Number of Households	Attend Post-secondary at All	Attend at Least Half-Time	Attend a Four-Year College	Tuition and Fees Paid (\$)	Grants and Scholarships Received (\$)	"List" Tuition and Fees (\$)	Instructional Spending Per Student (\$)	Core Educational Resources (\$)	Interest Paid on Student Loans through year 7 (\$)
\$0-25k	535,248	24.8%	84.8%	53.1%	6,878	5,211	5,958	5,537	13,499	715
\$25-45k	371,594	35.4%	86.6%	55.0%	7,247	4,738	6,337	5,740	14,007	956
\$45-55k	98,366	46.0%	88.5%	56.4%	7,939	4,226	6,778	6,167	15,055	1,136
\$55-65k	63,631	52.1%	88.8%	57.6%	8,659	4,317	7,083	6,217	15,238	1,202
\$65-70k	22,487	56.0%	89.2%	58.5%	9,512	4,354	7,313	6,388	15,701	1,219
\$70-75k	17,933	58.7%	89.0%	57.9%	10,874	3,497	7,210	6,542	15,965	1,198
\$75-80k	14,153	60.2%	89.7%	60.1%	11,513	5,377	7,645	6,692	16,369	1,209
\$80-90k	19,903	62.4%	89.7%	61.8%	11,124	4,596	7,930	6,934	16,946	1,270
\$90-110k	21,514	66.5%	89.4%	62.9%	14,514	4,547	8,211	7,285	17,860	1,208
\$110-120k	5,963	69.6%	90.5%	65.4%	14,998	4,959	8,855	7,425	18,194	1,078
\$120-130k	4,320	72.5%	90.2%	65.2%	15,190	3,117	8,809	7,670	18,866	1,063
\$130-140k	3,253	72.7%	90.6%	69.3%	15,279	5,409	9,485	8,133	19,861	1,148
\$140-150k	2,411	74.1%	91.0%	69.2%	15,703	4,150	9,714	8,072	19,800	1,009
\$150-160k	2,091	74.2%	90.9%	69.7%	15,853	2,556	9,480	8,043	19,747	1,032
\$160-170k	1,571	75.9%	91.6%	71.2%	16,825	3,957	9,643	8,326	20,344	938
\$170-180k	1,337	74.7%	92.7%	70.5%	19,536	3,463	10,273	8,503	20,803	981
\$180k +	12,502	79.5%	91.2%	77.0%	19,205	2,394	11,785	9,642	23,606	647

Notes follow on the next page.

Notes: The 2004 cohort is the group of students who would be expected to graduate from high school in June 2004. A person is associated with a joint (single) filing household if, when he is age 17 (and thus not independent) his household files jointly (singly). We continue to associate each person with his age 17 household for the purpose of classification owing to the fact that, after age 17, filing status is endogenous to the person's enrollment in postsecondary school. Year 1 is the first full tax year after a person would be expected to graduate from high school if he started elementary school on time, according to his state's compulsory schooling laws, and progressed through school on schedule (with no retention in grade). Year 1 is the year in which people are most likely to be enrolled in postsecondary school. For on-time students, it corresponds to the spring of freshman year and fall of sophomore year in college.
Source: Authors' calculations based on de-identified tax data.

Table 2
Effect of the DTF Threshold on Deduction Take-up in Year 1

			2002 & 2003 cohorts	2004 through 2008 cohorts		2002 & 2003 cohorts	2004 through 2008 cohorts	
			\$130,000 threshold	\$130,000 threshold (take larger)	\$160,000 threshold	\$65,000 threshold	\$65,000 threshold (take larger)	\$80,000 threshold
<i>base case (unlikely to be biased by income management)</i>			0.584*** (0.010)	0.550*** (0.006)	0.640*** (0.008)	0.238*** (0.013)	0.192*** (0.007)	0.296*** (0.011)
<u>Other cases</u>								
doughnut- hole	poly- nomial	band- width						
\$0	quintic	\$20,000	0.586 (0.004)	0.560 (0.002)	0.648 (0.003)	0.238 (0.004)	0.187 (0.002)	0.301 (0.003)
\$1,000 (500single)	quintic	\$20,000	0.573 (0.005)	0.549 (0.003)	0.643 (0.004)	0.226 (0.004)	0.183 (0.002)	0.297 (0.004)
\$2,000 (1000single)	quintic	\$20,000	0.576 (0.007)	0.547 (0.004)	0.642 (0.006)	0.219 (0.005)	0.181 (0.002)	0.294 (0.005)
\$3,000 (1500single)	quintic	\$20,000	0.584*** (0.010)	0.550*** (0.006)	0.640*** (0.008)	0.216*** (0.005)	0.177*** (0.002)	0.295*** (0.005)
\$0	quintic	\$10,000	0.595 (0.005)	0.570 (0.003)	0.657 (0.004)	0.258 (0.005)	0.198 (0.003)	0.308 (0.005)
\$1,000 (500single)	quintic	\$10,000	0.567 (0.010)	0.551 (0.006)	0.652 (0.008)	0.242 (0.007)	0.194 (0.004)	0.303 (0.006)
\$2,000 (1000single)	quintic	\$10,000	0.562 (0.018)	0.542 (0.011)	0.661 (0.014)	0.232 (0.009)	0.197 (0.005)	0.295 (0.008)
\$3,000 (1500single)	quintic	\$10,000	0.593*** (0.035)	0.550*** (0.022)	0.680*** (0.028)	0.238*** (0.013)	0.192*** (0.007)	0.296*** (0.011)
\$0	quartic	\$20,000	0.587 (0.003)	0.551 (0.002)	0.647 (0.002)	0.233 (0.003)	0.204 (0.001)	0.298 (0.003)
\$1,000 (500single)	quartic	\$20,000	0.581 (0.004)	0.543 (0.002)	0.644 (0.003)	0.225 (0.003)	0.205 (0.002)	0.295 (0.003)
\$2,000 (1000single)	quartic	\$20,000	0.584 (0.005)	0.540 (0.003)	0.644 (0.004)	0.220 (0.004)	0.208 (0.002)	0.293 (0.004)
\$3,000 (1500single)	quartic	\$20,000	0.591*** (0.006)	0.540*** (0.004)	0.644*** (0.005)	0.219*** (0.004)	0.210*** (0.002)	0.293*** (0.004)
\$0	quartic	\$10,000	0.589 (0.005)	0.562 (0.003)	0.651 (0.004)	0.251 (0.005)	0.193 (0.002)	0.301 (0.004)
\$1,000 (500single)	quartic	\$10,000	0.571 (0.007)	0.547 (0.004)	0.645 (0.005)	0.239 (0.005)	0.189 (0.003)	0.296 (0.005)
\$2,000 (1000single)	quartic	\$10,000	0.573 (0.010)	0.542 (0.006)	0.645 (0.008)	0.233 (0.007)	0.188 (0.003)	0.290 (0.006)
\$3,000 (1500single)	quartic	\$10,000	0.589*** (0.016)	0.545*** (0.010)	0.644*** (0.013)	0.236*** (0.008)	0.184*** (0.004)	0.290*** (0.007)

Notes follow on the next page.

Notes: The table shows regression discontinuity-based estimates of the effect of various income eligibility cut-offs on take-up of the DTF. Standard errors are in parentheses. The base case is shown in the first row and in bold. *** indicates that an estimate is statistically significantly different from zero at the 0.01 level. We do not show asterisks on estimates that employ doughnut-holes smaller than the base case because we know that those estimates suffer from reverse causality bias due to income management. Because there are two tiers of income eligibility in 2004 through 2008, the estimates in the columns headed "take larger" refer to the households taking up the larger deduction for which they qualify based on being just below an income cut-off.

Source: Authors' calculations based on de-identified tax data.

Table 3
Effect of the DTF Threshold on the Deduction Amount in Year 1 (unconditional on taking it up)

			2002 & 2003 cohorts	2004 through 2008 cohorts		2002 & 2003 cohorts	2004 through 2008 cohorts	
			\$130,000 threshold	\$130,000 threshold (take larger)	\$160,000 threshold	\$65,000 threshold	\$65,000 threshold (take larger)	\$80,000 threshold
<i>base case (unlikely biased by income management)</i>			1573*** (35)	1023*** (24)	1199*** (15)	570*** (31)	361*** (27)	524*** (21)
<u>Other cases</u>								
doughnut	poly-	band-						
\$0	quintic	\$20,000	1594	1103	1225	578	372	620
\$1,000	quintic	\$20,000	1547	1039	1213	554	365	636
\$2,000	quintic	\$20,000	1538	1018	1207	537	366	657
\$3,000	quintic	\$20,000	1573***	1023***	1199***	532***	361***	694***
\$0	quintic	\$10,000	1641	1168	1247	619	389	549
\$1,000	quintic	\$10,000	1560	1068	1236	586	377	538
\$2,000	quintic	\$10,000	1511	1019	1252	558	382	519
\$3,000	quintic	\$10,000	1657***	1080***	1269***	570***	361***	524***
\$0	quartic	\$20,000	1584	1064	1220	554	449	543
\$1,000	quartic	\$20,000	1554	1012	1211	536	459	540
\$2,000	quartic	\$20,000	1552	993	1207	522	475	539
\$3,000	quartic	\$20,000	1572***	986***	1203***	517***	489***	542***
\$0	quartic	\$10,000	1606	1132	1233	604	369	539
\$1,000	quartic	\$10,000	1540	1057	1218	581	355	530
\$2,000	quartic	\$10,000	1512	1033	1213	565	349	518
\$3,000	quartic	\$10,000	1558***	1057***	1199***	572***	329***	520***
<i>change in deduction amount at</i>			2646	1498	1860	2279	1178	1755

Notes follow on the next page.

Notes: The table shows regression discontinuity-based estimates of the effect of various income eligibility cut-offs on the amount of the DTF unconditional on taking it. Standard errors are in parentheses. The base case is shown in the first row and in bold. *** indicates that an estimate is statistically significantly different from zero at the 0.01 level. We do not show asterisks on estimates that employ doughnut-holes smaller than the base case because we know that those estimates suffer from reverse causality bias due to income management. The bottom row shows the change in the deduction amount at the cut-off for those households that take the DTF. These numbers are useful for understanding the degree to which households are taking the maximum possible DTF.

Source: Authors' calculations based on de-identified tax data.

Table 4
Effect of the DTF Threshold on Deduction Take-up and Amount in Years 0 through 4, Base Case

	2002 & 2003 cohorts	\$130,000 threshold	2004 through 2008 cohorts	\$130,000 threshold	\$160,000 threshold	2002 & 2003 cohorts	\$65,000 threshold	2004 through 2008 cohorts	\$65,000 threshold	\$80,000 threshold
Take-up the deduction in Year 0	0.511*** (0.010)	0.464*** (0.006)	0.566*** (0.008)	0.216*** (0.011)	0.151*** (0.006)	0.243*** (0.010)				
Take-up the deduction in Year 1	0.584*** (0.010)	0.550*** (0.006)	0.640*** (0.008)	0.238*** (0.013)	0.192*** (0.007)	0.296*** (0.011)				
Take-up the deduction in Year 2	0.547*** (0.010)	0.542*** (0.006)	0.624*** (0.008)	0.234*** (0.013)	0.196*** (0.007)	0.271*** (0.011)				
Take-up the deduction in Year 3	0.511*** (0.010)	0.509*** (0.006)	0.576*** (0.008)	0.205*** (0.012)	0.158*** (0.006)	0.244*** (0.011)				
Take-up the deduction in Year 4	0.440*** (0.010)	0.447*** (0.006)	0.503*** (0.008)	0.191*** (0.012)	0.137*** (0.006)	0.204*** (0.010)				
Deduction amount (unconditional on attending) in Year 0	1308*** (32)	856*** (24)	1035*** (15)	499*** (29)	273*** (25)	432*** (18)				
Deduction amount (unconditional on attending) in Year 1	1573*** (35)	1023*** (24)	1199*** (15)	570*** (31)	361*** (27)	524*** (21)				
Deduction amount (unconditional on attending) in Year 2	1487*** (28)	1033*** (25)	1178*** (15)	597*** (32)	405*** (27)	490*** (20)				
Deduction amount (unconditional on attending) in Year 3	1385*** (28)	997*** (25)	1082*** (16)	521*** (31)	278*** (31)	449*** (20)				
Deduction amount (unconditional on attending) in Year 4	1189*** (28)	839*** (25)	953*** (16)	483*** (30)	264*** (26)	373*** (19)				

Notes: The table shows regression discontinuity-based estimates of the effect of various income eligibility cut-offs on take-up of the DTF and the amount of the DTF unconditional on taking it. Standard errors are in parentheses. *** indicates that an estimate is statistically significantly different from zero at the 0.01 level. Only base case estimates, which are unlikely to suffer from reverse causality bias, are shown. (See text and Tables 2 and 3 for the base case specification.)

Source: Authors' calculations based on de-identified tax data.

Table 5
Effect of the DTF Threshold on College Attendance in Year 1

			joint filing households			single filing households		
			2002 & 2003 cohorts	2004 through 2008 cohorts		2002 & 2003 cohorts	2004 through 2008 cohorts	
			\$130,000 threshold	\$130,000 threshold	\$160,000 threshold	\$65,000 threshold	\$65,000 threshold	\$80,000 threshold
<i>base case (unlikely to be biased by income management)</i>			-0.007 (0.009)	-0.003 (0.006)	0.000 (0.008)	-0.005 (0.007)	0.005 (0.004)	-0.000 (0.006)
Other cases								
doughnut hole	polyno mial	band- width						
\$0	quintic	\$20,000	0.019 (0.004)	0.012 (0.003)	0.007 (0.003)	0.005 (0.005)	0.012 (0.003)	0.005 (0.005)
\$1,000 (500single)	quintic	\$20,000	0.005 (0.005)	0.003 (0.003)	0.007 (0.004)	-0.002 (0.006)	0.009 (0.004)	0.001 (0.005)
\$2,000 (1000single)	quintic	\$20,000	0.005 (0.007)	0.001 (0.004)	0.009 (0.006)	-0.005 (0.007)	0.005 (0.004)	-0.000 (0.006)
\$3,000 (1500single)	quintic	\$20,000	-0.007 (0.009)	-0.003 (0.006)	0.000 (0.008)	-0.008 (0.008)	0.005 (0.005)	0.002 (0.007)
\$0	quintic	\$10,000	0.031 (0.006)	0.024 (0.004)	0.009 (0.005)	0.015 (0.008)	0.019 (0.005)	0.012 (0.006)
\$1,000 (500single)	quintic	\$10,000	0.007 (0.010)	0.014 (0.006)	0.011 (0.008)	0.003 (0.010)	0.017 (0.006)	0.005 (0.008)
\$2,000 (1000single)	quintic	\$10,000	0.014 (0.018)	0.019 (0.012)	0.031 (0.015)	-0.000 (0.014)	0.010 (0.008)	0.003 (0.011)
\$3,000 (1500single)	quintic	\$10,000	-0.043 (0.037)	0.019 (0.023)	0.008 (0.029)	-0.008 (0.018)	0.014 (0.011)	0.015 (0.015)
\$0	quartic	\$20,000	0.019 (0.003)	0.006 (0.002)	0.008 (0.003)	0.002 (0.005)	0.004 (0.003)	0.005 (0.004)
\$1,000 (500single)	quartic	\$20,000	0.010 (0.004)	-0.002 (0.002)	0.008 (0.003)	-0.004 (0.005)	0.001 (0.003)	0.002 (0.004)
\$2,000 (1000single)	quartic	\$20,000	0.013 (0.005)	-0.005 (0.003)	0.010 (0.004)	-0.005 (0.006)	-0.003 (0.003)	0.001 (0.005)
\$3,000 (1500single)	quartic	\$20,000	0.008 (0.006)	-0.009** (0.004)	0.006 (0.005)	-0.007 (0.006)	-0.004 (0.004)	0.003 (0.005)
\$0	quartic	\$10,000	0.024 (0.005)	0.017 (0.003)	0.008 (0.004)	0.012 (0.007)	0.013 (0.004)	0.007 (0.005)
\$1,000 (500single)	quartic	\$10,000	0.007 (0.007)	0.006 (0.004)	0.008 (0.006)	0.004 (0.008)	0.009 (0.005)	0.000 (0.007)
\$2,000 (1000single)	quartic	\$10,000	0.011 (0.011)	0.003 (0.007)	0.013 (0.009)	0.003 (0.010)	0.003 (0.006)	-0.002 (0.008)
\$3,000 (1500single)	quartic	\$10,000	-0.010 (0.017)	-0.005 (0.011)	-0.005 (0.014)	0.001 (0.011)	0.003 (0.007)	0.002 (0.010)

Notes follow on next page.

Notes: The table shows regression discontinuity-based estimates of the effect of various income eligibility cut-offs on college attendance (at all). Standard errors are in parentheses. The base case is shown in the first row and in bold. ***, **, or * indicates that an estimate is statistically significantly different from zero at the 0.01, 0.05, or 0.10 level. We do not show asterisks on estimates that employ doughnut-holes smaller than the base case because we know that those estimates suffer from reverse causality bias due to income management.

Source: Authors' calculations based on de-identified tax data.

Table 6
Effect of the DTF Threshold on Attending *Four-Year* College (conditional on attending at all) in Year 1

			2002 & 2003 cohorts	2004 through 2008 cohorts		2002 & 2003 cohorts	2004 through 2008 cohorts	
			\$130,000 threshold	\$130,000 threshold	\$160,000 threshold	\$65,000 threshold	\$65,000 threshold	\$80,000 threshold
<i>base case (unlikely to be biased by income management)</i>			-0.003 (0.010)	-0.002 (0.006)	0.001 (0.008)	0.007 (0.011)	0.002 (0.007)	0.003 (0.009)
<u>Other cases</u>								
doughnut hole	poly-nomial	band-width						
\$0	quintic	\$20,000	0.027 (0.006)	0.014 (0.004)	0.008 (0.005)	0.009 (0.009)	0.008 (0.005)	0.006 (0.007)
\$1,000 (500single)	quintic	\$20,000	0.019 (0.008)	0.004 (0.005)	-0.000 (0.006)	0.006 (0.010)	0.007 (0.006)	0.003 (0.008)
\$2,000 (1000single)	quintic	\$20,000	-0.003 (0.010)	-0.002 (0.006)	0.001 (0.008)	0.007 (0.011)	0.002 (0.007)	0.003 (0.009)
\$3,000 (1500single)	quintic	\$20,000	-0.002 (0.015)	0.009 (0.009)	0.006 (0.011)	-0.005 (0.013)	-0.004 (0.008)	-0.005 (0.010)
\$0	quintic	\$10,000	0.048 (0.009)	0.022 (0.005)	0.015 (0.007)	0.017 (0.012)	0.013 (0.007)	0.015 (0.010)
\$1,000 (500single)	quintic	\$10,000	0.053 (0.015)	0.003 (0.009)	-0.004 (0.012)	0.016 (0.016)	0.012 (0.010)	0.012 (0.013)
\$2,000 (1000single)	quintic	\$10,000	0.009 (0.028)	-0.030 (0.017)	-0.015 (0.022)	0.025 (0.022)	0.003 (0.013)	0.016 (0.017)
\$3,000 (1500single)	quintic	\$10,000	0.009 (0.057)	-0.003 (0.034)	-0.032 (0.043)	-0.004 (0.029)	-0.021 (0.017)	-0.002 (0.023)
\$0	quartic	\$20,000	0.010 (0.006)	0.010 (0.003)	0.006 (0.004)	0.005 (0.007)	0.006 (0.004)	0.001 (0.006)
\$1,000 (500single)	quartic	\$20,000	-0.007 (0.008)	0.003 (0.004)	-0.001 (0.005)	0.003 (0.008)	0.005 (0.005)	-0.003 (0.006)
\$2,000 (1000single)	quartic	\$20,000	-0.008 (0.010)	-0.001 (0.004)	0.000 (0.006)	0.002 (0.009)	0.002 (0.005)	-0.004 (0.007)
\$3,000 (1500single)	quartic	\$20,000	0.004 (0.007)	0.006 (0.006)	0.003 (0.007)	-0.006 (0.010)	-0.003 (0.006)	-0.010 (0.008)
\$0	quartic	\$10,000	0.037 (0.007)	0.019 (0.004)	0.009 (0.006)	0.013 (0.010)	0.009 (0.006)	0.016 (0.008)
\$1,000 (500single)	quartic	\$10,000	0.033 (0.011)	0.007 (0.006)	-0.006 (0.008)	0.011 (0.012)	0.007 (0.007)	0.015 (0.010)
\$2,000 (1000single)	quartic	\$10,000	-0.005 (0.027)	-0.004 (0.010)	-0.010 (0.013)	0.014 (0.015)	0.000 (0.009)	0.019 (0.012)
\$3,000 (1500single)	quartic	\$10,000	0.018*** (0.005)	0.021 (0.016)	-0.009 (0.020)	-0.004 (0.018)	-0.013 (0.011)	0.012 (0.015)

Notes follow on next page.

Notes: The table shows regression discontinuity-based estimates of the effect of various income eligibility cut-offs on four-year college attendance (conditional on attending at all). Standard errors are in parentheses. The base case is shown in the first row and in bold. ***, **, or * indicates that an estimate is statistically significantly different from zero at the 0.01, 0.05, or 0.10 level. We do not show asterisks on estimates that employ doughnut-holes smaller than the base case because we know that those estimates suffer from reverse causality bias due to income management.

Source: Authors' calculations based on de-identified tax data.

Table 7
Effect of the DTF Threshold on Attending College and Four-Year College in Years 0 through 4, Base Case

	2002 & 2003 cohorts \$130,000 threshold	2004 through 2008 cohorts \$130,000 threshold	2004 through 2008 cohorts \$160,000 threshold	2002 & 2003 cohorts \$65,000 threshold	2004 through 2008 cohorts \$65,000 threshold	2004 through 2008 cohorts \$80,000 threshold
Attending college (at all) in Year 0	-0.004 (0.012)	0.016** (0.007)	0.010 (0.010)	0.021 (0.017)	0.020* (0.012)	0.008 (0.015)
Attending college (at all) in Year 1	-0.007 (0.010)	-0.003 (0.006)	0.000 (0.008)	-0.008 (0.018)	0.014 (0.011)	0.015 (0.015)
Attending college (at all) in Year 2	0.001 (0.010)	0.010 (0.006)	0.015* (0.008)	0.026 (0.019)	0.008 (0.011)	0.008 (0.015)
Attending college (at all) in Year 3	-0.013 (0.011)	-0.000 (0.007)	0.005 (0.009)	0.012 (0.019)	-0.011 (0.012)	0.009 (0.016)
Attending college (at all) in Year 4	0.034** (0.012)	-0.001 (0.007)	0.002 (0.010)	0.016 (0.019)	0.007 (0.012)	-0.002 (0.016)
Four-year college (conditional on attending at all) in Year 0	0.009 (0.017)	0.006 (0.008)	-0.008 (0.013)	0.039 (0.034)	0.019 (0.020)	-0.007 (0.026)
Four-year college (conditional on attending at all) in Year 1	-0.002 (0.014)	0.009 (0.009)	0.006 (0.011)	-0.004 (0.029)	-0.021 (0.017)	-0.002 (0.023)
Four-year college (conditional on attending at all) in Year 2	0.011 (0.015)	-0.000 (0.009)	-0.011 (0.011)	0.030 (0.030)	0.026 (0.018)	0.008 (0.023)
Four-year college (conditional on attending at all) in Year 3	0.012 (0.015)	-0.000 (0.009)	-0.008 (0.011)	-0.020 (0.030)	-0.026 (0.019)	-0.017 (0.023)
Four-year college (conditional on attending at all) in Year 4	0.006 (0.015)	0.004 (0.009)	0.019 (0.012)	0.026 (0.031)	0.019 (0.019)	0.023 (0.025)

Notes: The table shows regression discontinuity-based estimates of the effect of various income eligibility cut-offs on college attendance (at all) and attending four-year college (conditional on attending). Standard errors are in parentheses. Only base case estimates, which are unlikely to suffer from reverse causality bias, are shown. (See text and Tables 5 and 6 for the base case specification.) ***, **, or * indicates that an estimate is statistically significantly different from zero at the 0.01, 0.05, or 0.10 level.

Source: Authors' calculations based on de-identified tax data.

Table 8
Effect of the DTF Threshold on Various College-Related Outcomes, Year 1, Base Case
all outcomes are conditional on attending college at all

	2002 & 2003 cohorts \$130,000 threshold	2004 through 2008 cohorts (see notes) \$130,000 threshold	\$160,000 threshold	2002 & 2003 cohorts \$65,000 threshold	2004 through 2008 cohorts (see notes) \$65,000 threshold	\$80,000 threshold
Attending college full-time in Year 1	-0.014 (0.011)	-0.004 (0.007)	0.001 (0.009)	-0.016 (0.018)	0.006 (0.011)	0.002 (0.016)
Two-year college in Year 1	-0.000 (0.015)	-0.009 (0.009)	-0.007 (0.011)	0.004 (0.029)	0.023 (0.017)	0.006 (0.023)
Instructional spending of the college attended in Year 1	-50 (247)	139 (98)	119 (143)	-144 (424)	-214 (168)	-192 (233)
Core educational spending of the college attended in Year 1	-73 (476)	215 (178)	177 (256)	-449 (845)	-642 (307)	-11 (425)
"Sticker price" tuition and fees of the college attended in Year 1	-271 (350)	209 (181)	289 (259)	309 (595)	-284 (309)	-36 (430)
Total tuition paid in Year 1	149** (65)	171 (152)	79 (221)	-10 (107)	-151 (256)	234 (360)
Total grants and scholarships in Year 1	72 (90)	24 (84)	53 (113)	239 (193)	-249 (175)	45 (228)
Student loan interest paid in Years 1 through 7	29 (74)	-56 (87)	-62 (122)	41 (89)	-30 (100)	-230 (157)

Notes: The table shows regression discontinuity-based estimates of the effect of various income eligibility cut-offs on college-related outcomes. Standard errors are in parentheses. Only base case estimates, which are unlikely to suffer from reverse causality bias, are shown. (See text and Tables 5 and 6 for the base case specification.) ***, **, or * indicates that an estimate is statistically significantly different from zero at the 0.01, 0.05, or 0.10 level. Only the 2004 and 2005 cohorts are used for the estimates of student loan interest paid in years 1 through 7.

Source: Authors' calculations based on de-identified tax data.

Table 9
Effect of Current and Lagged DTF Eligibility on Attending College and Four-Year College
\$20,000 Bandwidth, Quintic Polynomial

	2002-2008 cohorts \$130,000 threshold	2002-2008 cohorts \$65,000 threshold	2004 & 2007 cohorts \$160,000 threshold	2004-2008 cohorts \$80,000 threshold
<i>Attending college at all</i>				
Eligible for DTF in current year (year 1)	0.003 (0.008)	0.002 (0.016)	0.028* (0.016)	0.012 (0.032)
Eligible for DTF in prior year (year 0)	-0.002 (0.009)	0.008 (0.017)	0.011 (0.016)	-0.016 (0.033)
Eligible for DTF in current year (year 2)	0.012 (0.009)	0.027 (0.017)	-0.000 (0.016)	0.005 (0.033)
Eligible for DTF in prior year (year 1)	-0.013 (0.009)	0.007 (0.017)	0.008 (0.017)	-0.007 (0.034)
<i>Four-year college attendance</i>				
Eligible for DTF in current year (year 1)	0.010 (0.013)	-0.025 (0.025)	0.011 (0.023)	-0.057 (0.048)
Eligible for DTF in prior year (year 0)	0.006 (0.013)	0.031 (0.026)	-0.028 (0.024)	-0.008 (0.049)
Eligible for DTF in current year (year 2)	0.011 (0.013)	-0.012 (0.026)	-0.004 (0.023)	0.004 (0.049)
Eligible for DTF in prior year (year 1)	0.009 (0.013)	-0.030 (0.027)	-0.014 (0.024)	-0.003 (0.050)

Notes: The table shows regression discontinuity-based estimates of the effect of various income eligibility cut-offs on college attendance (at all) and attending four-year college (conditional on attending). The specification that includes current year and lagged income eligibility is employed: equation (7) in the text. Standard errors are in parentheses. Only estimates that employ the base case doughnut holes, which are unlikely to suffer from reverse causality bias, are shown. (See text for further information on the base case doughnut holes.) ***, **, or * indicates that an estimate is statistically significantly different from zero at the 0.01, 0.05, or 0.10 level.

Source: Authors' calculations based on de-identified tax data.