Who Benefits from College Grant Aid and Why? Evidence from Texas

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Abstract

I use rich administrative data and several quasi-experiments in Texas to study which students benefit most from college grant aid and why. For "*extensive-margin*" students, grant aid *causes* enrollment in college, and therefore has potentially large benefits relative to these students' no-college counterfactual. In contrast, "*intensive-margin*" students would attend college even in the absence of additional aid, but nevertheless may benefit from additional financial support. The goal of this paper is to compare the costs and benefits of aid targeted at different groups of students and college sectors, and to understand the contributions of the intensive and extensive margins in shaping aid's overall effects. To do so, I leverage discontinuities in grant award rules which create variation in aid targeting three distinct populations: middle-income applicants to four-year colleges, low-income applicants to four-year colleges, and low-income applicants to community colleges. While these discontinuities provide exogenous variation in grant awards, I still encounter a common missing-data problem: my data contains all enrolled students, not all applicants, meaning that discontinuities in outcomes at the eligibility cutoff may conflate the causal effects of grants with compositional changes in enrolled students. I develop a bounding approach to overcome sample selection bias stemming from this missing-data problem. I find that grant aid targeted at low-income applicants to four-year colleges has large impacts on academic outcomes and students' future earnings. In sharp contrast, there is little overall effect of additional aid on academic outcomes and future earnings among middle-income applicants to four-year colleges and low-income applicants to community colleges. Across all three treatment margins, extensive-margin effects do not play a large positive role in determining the overall effects of grant aid.

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1 Introduction

Grant-based financial aid is the dominant form of financial assistance to college students in the United States. Undergraduate students receive more than $110 billion in grant aid each year from federal and state governments and college institutions (Ma and Pender 2022). This paper uses rich administrative data and several quasi-experiments in Texas to ask which students benefit most from this financial support and why.

The overall impact of grant aid on student outcomes depends on two distinct effects. For "*extensive-margin*" students, grant aid *causes* enrollment in college, and therefore has potentially large benefits relative to these students' no-college counterfactual (Autor 2014). In contrast, "*intensive-margin*" students would attend college even in the absence of additional aid, but nevertheless may benefit from additional financial support (Denning et al. 2019; Scott-Clayton 2011; Scott-Clayton and Zafar 2019). The relative importance of these two margins is likely to differ across grant programs depending on how they are targeted. For example, aid targeted to low-income community college applicants may have larger extensive-margin benefits, because these students are less likely to enroll in college without aid. In contrast, aid targeting higher-income applicants to four-year institutions may primarily have intensive-margin benefits, relieving students' financial constraints while they are enrolled in college. The goal of this paper is to compare the costs and benefits of aid targeted at different groups of students and college sectors, and to understand the contributions of the intensive and extensive margins in shaping aid's overall effects.

To achieve this goal, I develop an econometric framework that decomposes grant aid's overall effects into its separate impacts on the outcomes of extensive-margin and intensive-margin students. I apply the framework to administrative data from Texas, using discontinuities in grant award rules which create variation in aid targeting three distinct populations: middle-income students in four-year colleges, low-income students in four-year colleges, and low-income students in community colleges.[1] While these discontinuities provide exogenous variation in grant awards, I still encounter a common missing data problem: I observe all enrolled students, not all applicants, meaning that discontinuities in outcomes at the eligibility cutoff may conflate the causal effects of grants with compositional changes in enrolled students. I develop a bounding approach to overcome this missing data problem, yielding three main findings. First, grant aid targeted at low-income applicants in four-year colleges has large positive impacts on college persistence, bachelor's degree completion, and future earnings. Second, in sharp contrast, there is little overall effect of additional aid on these outcomes for middle-income students in four-year colleges and low-income students in community

[1] Following Mountjoy (2022), I use the terms "two-year colleges" and "community colleges" interchangeably to refer to nonprofit academic institutions that offer associate's degrees as their highest academic credential. I limit my analysis to public community colleges, excluding private two-year institutions.

colleges. Third, across all three treatment margins, extensive-margin effects do not play a large positive role in determining the overall effects of grant aid.

Three sources of variation in grant aid, described in Section 2, enable my analysis. The first is a financial cutoff rule used to allocate the TEXAS Grant, the state of Texas's flagship need-based grant aid program for four-year public university students.[2] The program covers the entirety of grant recipients' tuition and fees, making it unusually generous compared to other state and federal sources of need-based aid.[3] The second and third treatment margins target lower-income students in four-year public universities and two-year community colleges. Variation in aid at these margins stems from the federal government's formula for determining a student's Expected Family Contribution (EFC), previously studied in isolation by Denning et al. (2019) and Eng and Matsudaira (2021). Students at the margin of receiving an "Automatic Zero EFC" have substantially lower incomes than students at the margin of receiving a TEXAS Grant, allowing me to compare how the impacts of grant aid on student outcomes depend on the income of aid recipients.

I study these grant programs' impacts using administrative data spanning the entire state of Texas, described in Section 2.2. I link all Texas public high school graduates to enrollment, financial aid, and degree completion records from all public and private Texas colleges and universities. I further link these students to quarterly earnings records from the state's unemployment insurance system, allowing me to track students' labor market earnings both during and after college. Importantly, the data contain students' standardized test scores, allowing me to examine how the effect of grant aid on students' enrollment decisions and outcomes varies depending on their level of academic preparedness for college.

Despite the comprehensiveness of these data, I still encounter a missing-data problem, described in Section 3. While I observe outcomes for all Texas high school graduates, I only observe the running variables for my regression discontinuity (RD) analyses in the selected sample of students who enroll in college.[4] Students who enroll in college as a *result* of crossing the threshold therefore appear in my sample only on the eligible (but not on the ineligible) side of the cutoff. I show that

[2] Two dissertations have examined the TEXAS Grant using regression discontinuity analyses, finding mixed results. Villareal (2018) finds significant positive effects on TEXAS Grants' probability of graduation and imprecise impacts on future earnings using data on college entrants between 2004 and 2013. Montenegro (2020) examines a limited set of academic outcomes for later cohorts, finding reductions in loans and GPA among treated students. Relative to these findings, my paper finds null effects of the TEXAS Grant using a broader set of cohorts and more comprehensive set of academic and longer-run labor market outcomes.

[3] An exception is the Cal Grant (Bettinger et al. 2019), which provides similar levels of renewable support on the basis of need to California public college students. Large aid increases and tuition guarantees have also been studied in experimental evaluations of privately-funded aid programs (Anderson et al. 2020; Angrist et al. 2022) and state merit aid programs (Cohodes and Goodman 2014; Dynarski 2000; Scott-Clayton 2011).

[4] This issue occurs because my financial aid data come from colleges' and universities' annual submissions to state regulators. Institutions are only required to report financial aid information for enrolled students, not for the applicant pool. Similar missing-data problems are common in academic studies of higher education generally (Rothstein 2004) and of financial aid more specifically (Park and Scott-Clayton 2018; Scott-Clayton and Zafar 2019).

failing to account for this missing data problem leads to biased estimates of grant treatment effects, invalidating standard instrumental variables (IV) approaches whenever grants cause students to enroll in college.

In Section 4, I evaluate the extent of this bias in each of my three empirical settings. I use the discontinuity in the density of the running variable at each treatment margin to measure the empirical importance of the missing-data problem by measuring the impact of aid on enrollments. I find that for four-year college students, additional grant aid at the TEXAS Grant and Auto-Zero EFC margins has economically small and statistically insignificant impacts on college enrollments, implying that standard IV approaches are sufficient to measure the effects of grant aid at these treatment margins. In contrast, I find a significant and substantial enrollment effect associated with crossing the Auto-Zero EFC threshold for two-year community college applicants.[5] This enrollment effect implies that standard IV approaches will be biased due to missing data, necessitating an estimation approach that accommodates both intensive- and extensive-margin responses to aid.

In Section 5, I focus on the sources of variation in grant aid for which there is no missing-data problem, presenting RD estimates of the impact of grant aid for students enrolled in four-year colleges. For middle-income students at the margin of receiving a TEXAS Grant, I find that additional grant aid is effectively a pure transfer with no detectable impacts on marginal recipients' academic outcomes or future earnings. These null effects emerge despite a significant impact on student finances during college: "complier" students who receive TEXAS Grants at the threshold receive over $5,000 in additional first-year aid, and respond by reducing their loans and labor market earnings during college.[6] In contrast, for low-income students at the margin of receiving an Automatic Zero EFC, I find that additional grant aid has significant positive impacts on college persistence, degree attainment, and post-college earnings, replicating findings from Denning et al. (2019). These impacts emerge despite the fact that the increase in grant aid at the Auto-Zero threshold is less than one fifth of the increase in aid for TEXAS Grant compliers.

I examine the mechanisms behind these divergent effects in Section 6. Schools are given discretion to determine who among eligible students to award TEXAS Grants. Because schools must supplement state funds with institutional aid to cover TEXAS Grant recipients' tuition and fees, they face an incentive to award grants to students with high preexisting (i.e., non-TEXAS-Grant) levels of aid. Moreover, several factors may target the TEXAS Grant to *ex-ante* high-achieving students. If schools wish to maximize graduation rates among the pool of grant recipients, they may

[5] These findings accord with Denning et al. (2019), who also document enrollment impacts at the Auto-Zero EFC cutoff in Texas community colleges, but not in four-year universities. As a result, they limit attention to four-year students due to concern over bias caused by sample selection. In contrast, the framework I develop in Section 7 is able to estimate bounds on treatment effects even in the presence of enrollment effects.

[6] Similar trade-offs between grants and loans have been documented in other settings. Marx and Turner (2018) find that among City University of New York students, an additional dollar of Pell Grant aid causes a $1.80 reduction in student loans. In contrast, I find that loans fall by about half of the increase in grants.

allocate TEXAS Grants to students with high graduation probabilities rather than targeting to students with larger grant treatment effects. Furthermore, state TEXAS Grant rules give priority to students who apply early for financial aid, which may effectively target the grants to especially motivated or high-ability students. Analyzing complier characteristics at the grant threshold (Abadie 2002), I find that TEXAS Grants at the threshold are disproportionately targeted to better-funded and higher-achieving students. On average, marginal TEXAS Grant recipients enter college with over $820 more in preexisting first-year grant aid than students not chosen to receive TEXAS Grants. Moreover, even if they had not received TEXAS Grants, marginal grant recipients would be 12 percentage points more likely to graduate from college than marginal grant nonrecipients.[7] These results are consistent with schools allocating TEXAS Grants to lower their costs and ensure high graduation rates among grant recipients, rather than allocating the grants to maximize the program's overall value-added.

Section 7 shifts focus to low-income community college students at the Automatic Zero EFC threshold, where missing data due to enrollment effects invalidates standard regression discontinuity methods. Because receiving a zero EFC causes some students to enroll in college, grant recipients at the threshold consist of extensive-margin students brought into college attendance by additional aid as well as intensive-margin students who would attend college even without it. The key missing piece of data is the counterfactual outcomes of extensive-margin students, who are only observed on the eligible (but not the ineligible) side of the cutoff. I develop a proxy measure of these counterfactual outcomes using data on the B.A. completion rates and earnings of college non-attenders, allowing me to estimate the overall treatment effects of additional aid despite the missing data problem. I then present the bounding approach that allows me to decompose this overall effect into its intensive-margin and extensive-margin components.[8] I show that among "compliers" who receive zero EFCs at the threshold, extensive-margin students are heavily negatively selected, with average tenth grade test scores roughly 15 percentiles below those of intensive-margin students. Motivated and guided by this result, I tighten nonparametric bounds on treatment effects for both groups by assuming that intensive-margin compliers have weakly higher rates of B.A. completion and future earnings than extensive-margin compliers. This "mean-dominance" assumption is supported by the test score patterns identified by the data, and is consistent with students choosing college sectors based on their comparative advantage (Kirkeboen et al. 2016).

Section 8 applies the bounding method to estimate the impacts of additional grant aid on the outcomes of low-income community college students at the Automatic Zero EFC threshold. Although receiving a Zero EFC at the grant threshold corresponds to an increase of over $1,900 in

[7] I find that this difference is essentially unchanged by the introduction of merit standards to the TEXAS Grant in 2014, suggesting that schools independently value ex-ante high achieving students when awarding grants.

[8] This approach extends sample-trimming methods (Gerard et al. 2020; Horowitz and Manski 1995; Lee 2009) which use identified complier shares to place best-case and worst-case bounds on treatment effects for inframarginal program participants.

first-year grant aid, I find that this additional aid has economically small and statistically insignificant overall effects on students' B.A. completion rates and future earnings. Using the bounding method, I decompose these overall null effects into their intensive-margin and extensive-margin components. Standard no-assumptions bounds on treatment effects (Gerard et al. 2020; Lee 2009) are too wide to be informative, failing to reject either positive or negative effects of grant aid on either intensive or extensive margin students' B.A. completion or earnings at any time horizon. However, applying the mean-dominance assumption yields two key findings. First, the effect of receiving a Zero EFC on B.A. completion rates among intensive-margin compliers is weakly positive, but economically small, and does not exceed one percentage point. Although the bounds cannot rule out significant increases in B.A. completion among extensive-margin compliers, these students' small share in the overall complier population lessens their contribution in shaping grant aid's overall effects. Second, the bounds reveal offsetting effects on the early-career earnings of intensive-margin and extensive-margin students, suggesting that intensive-margin earnings effects are weakly positive and extensive margin earnings effects are weakly negative. These results imply that extensive-margin effects do not play a large positive role in determining the overall effects of grant aid for students at the Auto-Zero threshold in community colleges.

Taken together, the results indicate that additional grant aid produces large gains when targeted at low-income four-year college students, but has limited effects on middle-income four-year college students and low-income community college students. Section 9 unifies these results with a cost-benefit analysis that compares the impact of additional grant aid on student outcomes across the three treatment margins. I derive the costs and benefits of a marginal expansion of grant aid eligibility and show how these parameters can be expressed in terms of complier shares and margin-specific local average treatment effects. I find that the benefits of providing grant aid to lower-income students enrolled in four-year colleges exceed their costs. In contrast, the costs of providing grant aid to either higher-income students enrolled in four-year colleges or lower-income students enrolled in two-year colleges exceed the benefits. An implication of these results is that the current policy environment provides aid inefficiently, suggesting potentially large gains from budget-neutral reallocations of grant aid towards lower-income students enrolled in four-year colleges.

This paper contributes to a large literature studying the impact of grant aid on student outcomes (see Deming and Dynarski (2010), Dynarski and Scott-Clayton (2013), Page and Scott-Clayton (2016), and Dynarski et al. (2022) for reviews), but differs in several important ways. First, I provide a framework for decomposing the overall impact of grant aid into its extensive-margin and intensive-margin components. While many papers document that grant aid affects both enrollment and students' eventual probability of graduation (e.g., Angrist et al. 2022; Bettinger et al. 2019; Castleman and Long 2016; Fack and Grenet 2015; Lovenheim and Owens 2014), these results do not speak to whether effects are larger for extensive-margin students induced by aid to attend college or intensive-margin students who would have enrolled regardless.[9] Prevous studies have addressed this question by limiting attention to grant programs with no extensive-margin impacts on enrollment (Denning 2018; Goldrick-Rab et al. 2016),[10] often because data is only available on the enrolled population (Scott-Clayton 2011; Scott-Clayton and Zafar 2019).[11] An example is Denning et al. (2019), who study the same source of variation in aid at the Auto-Zero EFC cutoff as I do, but who restrict attention to four-year college students due to the missing-data problem caused by enrollment effects among community college students. In contrast, I recover the total effect of grant aid for this population and decompose it into its intensive-margin and extensive-margin components, finding weakly positive impacts of aid on future earnings for intensive-margin students but weakly negative impacts for students on the extensive margin.

Second, I contribute to a growing literature studying how institutional responses to grant aid policies shape their incidence and effectiveness. Turner (2017) shows that institutions respond to exogenous increases in students' Pell Grant funding by adjusting institutional aid, capturing \$0.15 of every marginal Pell Grant dollar on average. Other existing work has shown that schools price-discriminate using information reported by students during the financial aid application process (Fillmore 2023), or reduce institutional aid in response to increases in state or federal grants (Long 2004; Turner 2012). In contrast, my analysis of the TEXAS Grant shows that when institutions are given discretion in awarding state grant funds, they select recipients who are not only disproportionately cheaper to fund, but who are also more likely to graduate. Given that existing work has found that the marginal impacts of aid are higher for students with lower levels of pre-college preparedness (e.g., Angrist et al. 2022), this selection behavior by schools may limit grant aid's overall impact on their outcomes.

Lastly, this paper contributes to the methodological literature on estimating nonparametric bounds on treatment effects (Horowitz and Manski 1995; Manski 1989, 1990, 1997; Manski and Pepper 2000). The bounds I develop take Lee (2009) bounds as a foundation, and I use methodological results from Gerard et al. (2020) to implement them in the regression-discontinuity context.

[9]On enrollment impacts of aid, see (Barr 2019; Cornwell et al. 2006; Darolia 2013; Eng and Matsudaira 2021). A strand of the literature has focused on state merit-aid programs (Dynarski 2000, 2003; Sjoquist and Winters 2014), which increase the probability that students remain in their home state (Fitzpatrick and Jones 2016) but may cause students to enroll in lower-quality colleges (Cohodes and Goodman 2014). Several studies have focused on the minimum Pell Grant threshold, finding little impact on students' decisions of whether or where to enroll in college (Carruthers and Welch 2019; Marx and Turner 2018), although Bettinger (2004) finds positive impacts on persistence conditional on initial enrollment.

[10]Relatedly, Scott-Clayton and Schudde (2020) study the impact of performance standards, which affect students' ability to retain financial aid received at college entry.

[11]A notable exception is Park and Scott-Clayton (2018), who analyze the impact of the Pell Grant on the outcomes of community college students in an enrollment-selected sample. They consider two approaches: restricting attention to the subsample of community colleges with no apparent enrollment effect, and using Lee (2009)-style bounds to estimate the Pell Grant's impact on intensive-margin students (Gerard et al. 2020). The bounds I develop take these these bounds as a starting point, and tighten them with additional assumptions on the relative potential outcomes of students at different complier margins.

I show how the "no-assumptions" bounds considered in these studies can be substantially tightened by imposing assumptions on the relative potential outcomes of students at different complier margins, and show how to motivate these assumptions by separately identifying the pretreatment characteristics of different complier groups. The closest existing work is Dong (2019), who studies a similar mean-dominance assumption in an RD setting where data on outcomes is censored but the running variable is available for all units. Other work has considered similar assumptions in cases where full data on program non-participants is available (Chen et al. 2018; Huber et al. 2017). In contrast, I show how the complier characteristics required to motivate these assumptions are identified even when missing data on the running variable prevents RD estimation of enrollment effects. Other work uses parametric assumptions (Frangakis and Rubin 2002; Hull 2020; Kline and Walters 2016) or multiple instruments (Kirkeboen et al. 2016; Mountjoy 2022) to disentangle causal effects across different complier margins, but these require both access to data on untreated units and several sources of variation, which are frequently unavailable in settings where data availability depends on take-up.

2 Setting and Data

2.1 Setting and Sources of Variation

The setting for my analysis is the public higher education system in Texas.[12] The state's higher education system is the second-largest in the United States in terms of enrollment, providing a large and fairly representative setting for studying the impact of grant aid on student outcomes. During my sample period, roughly 1.4 million public undergraduate students enroll annually at one of 35 public four-year colleges or 60 public two-year community colleges and technical schools. Together, these students represent over 7 percent of overall U.S. undergraduate enrollment and roughly a tenth of all U.S. students in public college institutions during my sample period (U.S. Department of Education 2021).

Variation in Aid for Middle-Income Applicants: The TEXAS Grant

To study the impact of financial aid on the enrollments and outcomes of middle-income college applicants, I analyze the TEXAS Grant, the largest financial aid program administered by the state of Texas.[13] Students awarded TEXAS Grants receive grant aid covering the entirety of

[12]Because TEXAS Grants can only be awarded to public university students, I exclude private university students from the analysis and limit focus to the Texas public education sector. The public sector represents the vast majority of Texas college enrollments; for example, in 2017, 88% of Texas college students enrolled in public institutions (U.S. Department of Education 2021).

[13]Yearly disbursements for the TEXAS Grant range from $200 million to over $400 million during my sample period.

tuition and fees, renewable for up to five years subject to requirements on satisfactory academic performance.[14] Eligibility is limited to graduates from Texas high schools who enroll in college within 16 months of high school graduation.[15]

Eligibility for the TEXAS Grant in the initial year of college depends on a cutoff rule based on students' Expected Family Contribution (EFC). The EFC is a scalar measure of financial need assigned to students by the federal government for the purpose of administering the federal Pell Grant. To be assigned an EFC, students must complete the Federal Application for Student Aid (FAFSA). The inputs reported on the FAFSA determine each student's EFC through a complicated nonlinear formula that takes household income, assets, family size, government benefits, and other factors as inputs. Institutions are instructed to give first priority in awarding TEXAS Grants to students whose EFCs fall below a year-varying threshold determined by the state's higher education regulatory body, the Texas Higher Education Coordinating Board (THECB).[16] The threshold rule creates a discontinuity in students' probability of receiving a TEXAS Grant based on their EFC, providing the basis for my research design.

Importantly, because the TEXAS Grant is oversubscribed, not all eligible students receive TEXAS Grants. The program's rules give schools discretion to choose TEXAS Grant recipients from the pool of eligible students. However, schools are required to use institutional aid to "top up" TEXAS Grant awards for any students whose TEXAS Grant awards and other sources of grant aid do not fully cover tuition and fees.[17] As a result, schools may face an incentive to target the grant to students for whom "topping up" TEXAS Grant awards to cover full tuition and fees is less costly.

A related grant program, the Texas Educational Opportunity Grant (TEOG), provides grant aid to students at two-year community and technical colleges.[18] In practice, the programs are similar but target different academic sectors; the TEXAS Grant funds students in four-year B.A.-granting institutions, and the TEOG funds students in two-year community colleges. Both programs provide

[14] To remain eligible in future years after being awarded an initial-year TEXAS grant, students must achieve a 2.5 or higher GPA, be enrolled at least 3/4 time, and complete at least 24 semester credit hours per year in the program.

[15] This eligibility requirement describes the TEXAS Grant's "high school graduation pathway," which represents roughly 95 percent of students awarded TEXAS Grants and is the main focus of my study (Texas Higher Education Coordinating Board 2020). The remaining pathways allocate TEXAS Grants to recently-completed Associate's Degree holders, students entering college after being honorably discharged from the military, and transfer students from community colleges.

[16] Over my sample period, the priority EFC remained fixed at $4,000 until 2014. A policy change in 2014 redefined the priority EFC as 60 percent of the annual average statewide tuition and fees for four-year institutions.

[17] The eligibility rules that govern schools' allocation decisions have changed over time. For cohorts entering college prior to 2013, the main guidance issued to was to target grants based on financial need, prioritizing students with EFCs below the TEXAS Grant threshold. In contrast, for cohorts entering college in 2014 or later, the state added a set of merit-based criteria that gave priority to students with high levels of pre-college achievement. I return to this distinction in Section 6, analyzing the impact of this policy change on the composition of grant recipients and the impacts of grant aid on their outcomes.

[18] The TEXAS Grant provided funding to two-year community college students until the 2014 academic year, when part of the TEXAS Grant was reappropriated to create the TEOG as a standalone grant program.

funding covering the entirety of tuition and fees for students receiving grants, and are allocated using the same cutoff rule based on students' Expected Family Contribution (EFC). I present results in Section 4 that strongly indicate that crossing the TEXAS Grant cutoff does not influence students' enrollment decisions, limiting concerns that the confluence of these grant cutoffs cause selection bias in my estimates of the TEXAS Grant's effects. I primarily limit my focus in this paper to the TEXAS Grant, which funds a larger share of eligible students and provides larger increases in grant aid than the TEOG (Baum and Blagg 2021).

Variation in Aid for Low-Income Applicants: The Auto-Zero EFC Threshold

To study the impact of additional grant aid on the enrollments and outcomes of low-income college applicants, I analyze a federal income-based cutoff rule that determines the likelihood that a student is assigned a $0 Expected Family Contribution. Students at the margin of this "Automatic Zero EFC" threshold have substantially lower family incomes than students at the margin of receiving a TEXAS Grant; while marginal TEXAS Grant recipients have average family incomes of roughly $50,000 on average (in 2012 dollars), the Adjusted Gross Income (AGI)-based threshold for receiving an Automatic Zero EFC ranges from $20,000 to $31,000 over the 2008-2012 sample period.

Variation in grant aid at the automatic zero EFC threshold stems from an income-based cutoff in the federal government's formula for calculating the EFC. Specifically, students with family AGI below a year-specific threshold become automatically eligible to be assigned an EFC of $0, making them eligible to receive the maximum federal Pell Grant.[19] In addition, because the EFC is used as an input to determine other grant awards (including the TEXAS Grant), receiving an automatic zero EFC can substantially increase grant aid from other sources besides the Pell Grant.

2.2 Data Sources and Sample Construction

Data Sources. To conduct my analysis, I combine three sources of administrative data: high school graduation records from the Texas Education Agency (TEA), college enrollment and degree completion records from the Texas Higher Education Coordinating Board (THECB), and quarterly earnings records from the Texas Workforce Commission (TWC).[20] Together, these data sources allow me to track individual students' trajectories from high school into college and the labor market in early adulthood. I supplement these administrative records with several school- and neighborhood-level data sources: tract-level measures of economic disadvantage from the American

[19]Because the EFC depends on factors other than family AGI, it is possible both for families to have a $0 EFC despite being on the ineligible side of the threshold, and to have a nonzero EFC despite being on the eligible side.

[20]I access the data through a data-sharing agreement with the UT Austin Education Research Center (https://texaserc.utexas.edu/).

Community Survey,[21] and institution-level measures of college tuition and instructional costs from the Integrated Postsecondary Education Data System (IPEDS).

Sample Construction. The analysis sample begins with records on all Texas high school graduates between 2000 and 2018 with valid student identifiers, demographics, and test scores. I link this population to college enrollment and financial aid records from THECB, making two sample restrictions in the process. First, I restrict the enrolled sample to students who enter college within two academic years of their high school graduation. This restriction reflects the TEXAS Grant's eligibility rules, which require that students enroll in college within sixteen months of high school graduation to be eligible for a grant. I classify students' college sector (two-year, four-year, or no college) by the sector of their first enrolled institution within this two-year window.[22]

Second, I restrict the sample to students who enroll in college between the 2007 and 2017 academic years. This restriction is due to data availability: 2007 is the oldest cohort for which complete financial aid data are available for all FAFSA submitters, and 2017 is the latest cohort for which I observe five-year graduation outcomes. Observing outcomes at longer time horizons requires dropping later-entering cohorts from the sample; I observe six-year graduation rates, enrollments, and earnings for ten cohorts of entering college students (2007-2016), eight years of these outcomes for eight cohorts (2007-2014), and ten years for six cohorts (2007-2012).

Several additional restrictions are required when studying the Auto-Zero EFC cutoff. First, I restrict to cohorts entering college after 2008, as this is the first year that family AGI (the running variable) is available in the data. Following Denning et al. (2019), I exclude students whose listed family AGI is a multiple of $1,000 to avoid bias caused by bunching in the running variable. Finally, I make an additional sample restriction related to the strength of the first-stage in grant aid awards at the Auto-Zero cutoff. A policy change in 2013 significantly lowered the family income threshold for receiving a Zero EFC, targeting aid to students who already received significant financial aid packages at baseline. Appendix Figure A1 shows that as a result, the first-stage increase in first-year grant aid associated with crossing the threshold becomes statistically indistinguishable from zero from 2013 onwards. For this reason, I primarily limit attention to the 2008-2012 entry cohorts when studying the automatic zero EFC threshold, when crossing the threshold corresponded to a large increase in aid.

[21] I use 2009-2013 five-year American Community Survey estimates, matching students' high school location to census tracts.

[22] I classify the very small proportion of students who simultaneously enroll in four-year and two-year institutions as attending four-year colleges. I follow Mountjoy (2022) and Andrews et al. (2016) in ignoring summer terms when defining sector of enrollment.

2.3 Variable Definitions

Treatments. Throughout the analysis, I consider binary treatment indicators associated with crossing each grant threshold. To avoid endogeneity issues stemming from grant aid's impacts on college persistence, I define all treatment indicators based on students' first year of college enrollment. For the TEXAS Grant, treatment is an indicator for whether the student receives a TEXAS Grant in her initial year of college. For the Auto-Zero EFC cutoff, treatment is an indicator for whether the student receives a Zero EFC in her initial year of college.

Covariates. I measure student-level demographics using records from TEA graduation files. I observe categorical variables for gender, race, and ethnicity, together with a binary indicator for whether students are eligible for a free and reduced-price lunch (FRPL), a measure of economic disadvantage. To obtain a scalar measure of students' test scores, I follow Mountjoy (2022) in combining raw tenth grade math and reading scores using a one-factor model separately by cohort, and calculating percentiles of this factor within high school graduating classes.[23] Finally, I match students' geocoded high school locations to their census tracts, and merge these to tract-level measures of economic disadvantage from the ACS.

Academic Outcomes. I measure academic outcomes using THECB financial aid, enrollment, and graduation files. I measure students' total grants and total loans by summing across all sources in the financial aid data within academic years and deflate to 2019 dollars using the annual Consumer Price Index. To measure students' college persistance, I construct an indicator for whether a student remains enrolled in her institution of first college enrollment in each year following college entry. Finally, I measure B.A. completion as an indicator for whether a student appears in THECB graduation files for her initially-enrolled institution, constructed separately in each year.

Earnings. I measure yearly real earnings by summing individual earnings from all sources within academic years, Winsorizing at the 99th percentile, and deflating to 2019 dollars using the annual Consumer Price Index. I use academic rather than calendar years to align the earnings data with the financial aid and enrollment data.[24]

[23]Students who graduated from high school in Spring 2014 or earlier generally took the Texas Assessment of Knowledge and Skills (TAKS) in tenth grade, providing a single math score and a single reading score for each student. Students who graduated from high school in later years instead took the State of Texas Assessments of Academic Readiness (STAAR), which replaced general math and reading tests with subject-specific end-of-course assessments. For these cohorts, I use the end-of-course assessments for Algebra 1 and English 1 as my measures of math and reading scores. The factor model combines these raw scores by estimating $X_m = \beta_m F + \varepsilon_m$, where X_m, $m \in \{\text{math}, \text{reading}\}$ are raw math and reading scores, β_m is an estimated coefficient, F is the estimated common factor, and ε_m is an error term that by construction is uncorrelated with F and uncorrelated across subjects.

[24]For example, I construct earnings for Academic Year 2015 by summing quarterly earnings records for quarters 3 and 4 of calendar year 2014 and quarters 1 and 2 of calendar year 2015.

2.4 Descriptive Statistics

Table 1 reports summary statistics for students near the three sources of variation in grant aid. Students near the TEXAS Grant threshold are substantially economically advantaged compared to students near the margin of receiving an Automatic Zero EFC. Near the TEXAS Grant threshold, students are roughly half as likely to have received a free or reduced-price lunch in high school, attended high schools with substantially lower poverty rates, and are substantially less likely to be nonwhite. Students near the TEXAS Grant threshold also have higher measures of pre-college preparedness compared to students near the margin of receiving an Automatic Zero EFC, with high school test scores that are six percentiles higher on average. The samples also differ considerably in terms of their baseline financial support; students on the ineligible side of the Auto-Zero cutoff receive substantially higher baseline grant awards than students on the ineligible side of the TEXAS Grant cutoff, while the latter group takes out substantially higher amounts in first-year loans.

3 Empirical Framework

This section lays the groundwork for my empirical analysis, describing the empirical challenge of identifying grant treatment effects in selected data when grants affect enrollment.

3.1 Setup and Target Parameters

Let D be a binary indicator for whether a college applicant is offered a grant. Assume that each applicant has a financial running variable k, and let $Z = \mathbf{1}\{k \leq k^*\}$ denote whether an applicant's running variable falls below an exogenous cutoff k^*. The potential grant treatments $D_z(k)$ denote whether the applicant receives a grant, depending on whether k falls on the eligible $(z = 1)$ or ineligible $(z = 0)$ side of the cutoff. Finally, Y_1 and Y_0 denote potential outcomes, such as graduation or earnings, if the applicant does or does not receive a grant. The relationship between observed and potential outcomes is $Y = DY_1 + (1 - D)Y_0$.

Throughout the paper, I make two standard fuzzy regression-discontinuity assumptions:

Assumption 1 (Grant Monotonicity)**.** $D_1(k^*) \geq D_0(k^*)$ *for all applicants.*

Assumption 2 (Continuity)**.** $\mathbb{E}[Y_d \mid k]$ *and* $\mathbb{E}[D_z \mid k]$ *are continuous at* $k = k^*$ *for* $z \in \{0, 1\}$ *and* $d \in \{0, 1\}$.

Assumption 1 is a monotonicity condition, imposing that crossing the grant threshold weakly increases an applicant's chance of receiving a grant offer. Assumption 2 imposes that the potential outcome and treatment assignment functions are continuous at the grant threshold, so that crossing the threshold *only* affects students' outcomes through its impact on grant awards.

Table 1: Summary Statistics

	TEXAS Grant (4-Year Enrollees)		Auto-Zero EFC (4-Year Enrollees)		Auto-Zero EFC (2-Year Enrollees)	
	Ineligible (1)	Eligible (2)	Ineligible (3)	Eligible (4)	Ineligible (5)	Eligible (6)
Panel A. Demographics						
Female	0.57	0.56	0.57	0.56	0.54	0.54
Asian	0.06	0.06	0.06	0.07	0.02	0.02
Black	0.18	0.16	0.22	0.23	0.17	0.16
Hispanic	0.39	0.34	0.50	0.44	0.60	0.55
White	0.35	0.42	0.21	0.26	0.20	0.25
Free/reduced price lunch	0.34	0.22	0.63	0.51	0.67	0.57
Test score percentile	67.61	69.43	61.89	62.95	38.35	40.72
Poverty Rate	0.17 (0.12)	0.16 (0.12)	0.21 (0.13)	0.20 (0.13)	0.21 (0.13)	0.20 (0.13)
Unemployment Rate	0.07 (0.04)	0.07 (0.04)	0.08 (0.05)	0.07 (0.05)	0.07 (0.04)	0.07 (0.04)
SNAP Receipt Rate	0.14 (0.11)	0.13 (0.11)	0.18 (0.13)	0.16 (0.13)	0.19 (0.13)	0.17 (0.12)
Panel B. Binary Treatments						
Any TEXAS Grant	0.62	0.06	0.72	0.64	0.38	0.36
Zero EFC	0	0	0.87	0.18	0.90	0.29
Panel C. Financial Aid						
Total Grant Aid	9,262	4,746	13,031	11,330	5,837	4,855
	(5,314)	(4,807)	(5,449)	(5,662)	(2,910)	(2,905)
TEXAS Grant	3,624	349	4,328	3,911	694	660
	(3,046)	(1,463)	(3,012)	(3,137)	(971)	(962)
Pell Grant	2,108	269	5,469	4,077	4,546	3,558
	(1,047)	(515)	(1,537)	(1,807)	(1,931)	(1,930)
HB3015 Set-Asides	523	1,005	480	467		
	(1,268)	(1,706)	(1,273)	(1,229)		
Total Loans	4,702	6,597	3,076	3,877	502	604
	(4,657)	(6,106)	(3,682)	(4,174)	(1,453)	(1,622)
Observations	39,123	28,107	23,704	20,841	34,645	27,992
	67,230		44,545		62,637	
Cohorts	2007-2017		2008-2012		2008-2012	

Notes: Table reports summary statistics for the main estimation samples. TEXAS Grant observations in columns 1 and 2 are students enrolled in four-year public TEXAS universities with incoming Expected Family Contribution (EFC) within $2,000 of the TEXAS Grant cutoff. Auto-Zero EFC observations in columns 3 through 6 are students enrolled in four-year (columns 3 and 4) and two-year (columns 5 and 6) colleges with incoming family Adjusted Gross Income (AGI) within $10,000 of the automatic zero EFC threshold. The sample is restricted to students who graduate from a public Texas high school within two years prior to first college enrollment. Poverty rate, unemployment rate, and SNAP receipt rate are measured by students' home census tract, as measured by the location of the high school from which they graduated. Dollar values are in 2019 dollars.

I show in Appendix C.1 that under these assumptions, the fuzzy RD estimand at the cutoff identifies the following local average treatment effect (LATE):

$$
\begin{aligned}
\frac{\lim_{r\uparrow k^*}\mathbb{E}[Y \mid k]-\lim_{r\downarrow k^*}\mathbb{E}[Y \mid k]}{\lim_{r\uparrow k^*}\mathbb{E}[D \mid k]-\lim_{r\downarrow k^*}\mathbb{E}[D \mid k]} &= \mathbb{E}\left[Y_1-Y_0 \mid D_1>D_0, k=k^*\right] \\
&\equiv LATE
\end{aligned} \tag{1}
$$

The LATE in (1) is the first target parameter. It describes the grant's overall impacts on the outcomes of *compliers*: college applicants who receive a grant if and only if they cross to the eligible side of the grant threshold ($D_1 > D_0$). This overall treatment effect captures the returns of grant aid for students on the margin of aid receipt, and is thus relevant for evaluating the costs and benefits of slightly increasing the threshold for grant eligibility.

Intensive and Extensive Margin Responses Importantly, the overall effect in (1) summarizes several distinct mechanisms through which grant aid affects student outcomes. In particular, while some college applicants would enroll in college *regardless* of whether they receive additional aid at the threshold, others would *only* enroll in college if they receive aid. The overall LATE is a weighted average of these intensive-margin and extensive-margin responses to receiving grant aid.

To see this, let S denote whether a college applicant ultimately decides to enroll in college in the upcoming school year. The applicant's potential enrollment status is S_1 if she receives a grant and S_0 if she does not. As I show in Appendix C.2, stratifying the sample using these potential enrollments yields the following decomposition:[25]

$$
\begin{aligned}
LATE = {} & \pi_{IM}\mathbb{E}[Y_1-Y_0 \mid \overbrace{S_1=S_0=1, D_1>D_0}^{\text{Intensive-Margin Compliers}}] + \pi_{EM}\mathbb{E}[Y_1-Y_0 \mid \overbrace{S_1=1, S_0=0, D_1>D_0}^{\text{Extensive-Margin Compliers}}] \\
& + \pi_{NT}\mathbb{E}[Y_1-Y_0 \mid \underbrace{S_1=S_0=0, D_1>D_0}_{\text{Enrollment Never-Takers}}] + \pi_{DF}\mathbb{E}[Y_1-Y_0 \mid \underbrace{S_1=0, S_0=1, D_1>D_0}_{\text{Enrollment Defiers}}]
\end{aligned} \tag{2}
$$

The decomposition in (2) divides the population of compliers into four types, defined by how applicants' enrollment choices depend on grant offers. The first two groups are *intensive-margin compliers* and *extensive-margin compliers.* Intensive-margin compliers receive grants as a result of crossing the threshold, but would attend college regardless of whether they are awarded additional aid ($S_1 = S_0 = 1$). In contrast, extensive-margin compliers are *induced* by additional aid to enroll in college ($S_1 > S_0$). The population shares of these two types are π_{IM} and π_{EM}.

[25] Purely to reduce notation in this section, I remove the condition $k = k^*$ in expressions of conditional expectations, defining the binary instrument Z as falling on the "barely-eligible" and "barely-ineligible" side of the cutoff. I refer to switching from $Z = 0$ to $Z = 1$ as "crossing the threshold."

The second two groups are enrollment never-takers and enrollment defiers. Enrollment never-takers do not enroll in college regardless of whether they receive additional aid at the cutoff ($S_1 = S_0 = 0$), while enrollment defiers are induced by additional aid to *not* enroll in college, but otherwise would have enrolled ($S_1 = 0, S_0 = 1$). The population shares of these types are π_{NT} and π_{DF}.

To make the decomposition in (2) empirically tractable, I impose two additional assumptions concerning enrollment never-takers and enrollment defiers:

Assumption 3 (No Impact on Enrollment Never-Takers). $\mathbb{E}[Y_1 - Y_0 \mid S_1 = S_0 = 0, D_1 > D_0] = 0.$

Assumption 4 (No Enrollment Defiers). $(S_1 \geq S_0) \mid k = k^*$ *for all applicants.*

Assumption 3 states that merely being offered a grant cannot affect an applicant's outcomes if she foregoes college regardless of her grant status ($S_1 = S_0 = 0$). This assumption is motivated by the structure of almost all college grant programs, which are offered as an in-kind subsidy that lowers the price of college but cannot be used for other purposes. As a result, enrollment never-takers drop out from Equation (2).

Assumption 4 states that there are no enrollment defiers, imposing that additional grant aid weakly increases the probability that a student enrolls in college. Importantly, while this assumption is likely to hold for grant programs that are targeted at a particular college sector or institution,[26] it may not hold in general for all grant programs. In particular, if D represents a source of grant aid that can be used by students at institutions outside the scope of the data, then treatment may cause some students to leave the sample as a result of crossing to the eligible side of the threshold. This is a particular concern for the Auto-Zero EFC cutoff, which increases students' federal Pell Grant aid at any institution (including out-of-state schools) and therefore may cause students to leave the sample.[27]

Under these assumptions, the LATE in (2) summarizes the impact of grant aid on two sets of applicants: *"intensive-margin"* compliers who attend college regardless of whether they receive a grant at the cutoff, and *"extensive-margin"* compliers who are induced by grant offers to enroll:

$$LATE = LATE_{IM} \times \pi_{IM} + LATE_{EM} \times \pi_{EM} \tag{3}$$

where $LATE_{IM} = \mathbb{E}[Y_1 - Y_0 \mid S_1 = S_0 = 1, D_1 > D_0]$ denotes aid's impact on intensive-margin compliers, and $LATE_{EM} = \mathbb{E}[Y_1 - Y_0 \mid S_1 > S_0, D_1 > D_0]$ denotes impacts for extensive-margin compliers. The sample shares $\pi_{IM} = P(S_1 = S_0 = 1 \mid D_1 > D_0)$ and $\pi_{EM} = P(S_1 > S_0 \mid D_1 > D_0)$ describe the shares of students who fall into the two groups.

[26] Examples of such programs include UT-Austin's Longhorn Opportunity Scholars program and Texas A&M's Century Scholars Program, studied by (Andrews et al. 2020).

[27] I return to this concern in Section 4, showing that a small number of community college students (namely, those with high test scores) are enrollment defiers who leave the sample as a result of crossing the Auto-Zero EFC threshold and therefore must be excluded from the analysis.

The margin-specific treatment effects $LATE_{IM}$ and $LATE_{EM}$ are important target parameters, describing how grant aid affects students on the intensive and extensive margin of college attendance. Many possible combinations of $LATE_{IM}$ and $LATE_{EM}$ could produce the overall $LATE$ identified in (1). The difference between these component "subLATEs" has important policy implications for how grants should be targeted. On the one hand, if $LATE_{EM}$ is larger than $LATE_{IM}$, then the overall impact of grant aid is larger for students who attend college as a result of receiving funds. This case would suggest large benefits from policies that target grant aid towards college students who are unlikely to attend college otherwise (Dynarski et al. 2021). On the other hand, if extensive-margin compliers forego labor-market earnings to attend college or are unlikely to complete college at high rates, then $LATE_{IM}$ may exceed $LATE_{EM}$, justifying grant programs that are primarily targeted at intensive-margin students.

However, $LATE_{IM}$ and $LATE_{EM}$ are not separately point identified even with data on the full population of college applicants. The reason is a fundamental underidentification problem that arises in any setting with multiple treatment margins but only one instrument: though it is possible to identify the overall $LATE$ and the shares of both complier types in (3), the data do not reveal who among the compliers is an intensive-margin complier and who is an extensive-margin complier.[28] To address this problem, I develop a bounding approach in Section 7 that delivers nonparametric bounds on $LATE_{IM}$ and $LATE_{EM}$.

3.2 The Missing Data Problem

A further challenge arises when data on the running variable is only available for enrolled students. In this case, extensive-margin compliers will appear in the dataset *only* on the eligible side of the cutoff, as grant awards resulting from crossing the grant threshold cause this population to enroll. As a result, the populations to either side of the cutoff are no longer directly comparable, biasing standard IV approaches to estimating the impact of grant aid on outcomes. I show in Appendix C.3 that failing to account for this compositional bias results in IV estimates that fail to recover a LATE for any group. When grants cause some students to enroll, the limiting Wald estimand at

[28] In fact, when there are multiple treatment margins, standard IV methods such as two-stage least squares do not recover margin-specific treatment effects even when there are as many instruments as treatment margins (Kirkeboen et al. 2016). However, Mountjoy (2022) shows that with two treatment margins and two continuous instruments, it is possible to recover margin-specific treatment effects using a nonparametric separate-identification approach.

the cutoff in enrollment-selected data becomes:

$$\begin{aligned}
&\frac{\lim_{k\uparrow k^*}\mathbb{E}[Y \mid S=1,k]-\lim_{r\downarrow k}\mathbb{E}[Y \mid S=1,k]}{\lim_{k\uparrow k^*}\mathbb{E}[D \mid S=1,k]-\lim_{k\downarrow k^*}\mathbb{E}[D \mid S=1,k]} \\
&\quad = LATE_{IM}\times\underbrace{\left(\frac{\pi_{IM}^-}{\pi_{IM}^-+\pi_{EM}^-(1+\bar{D}^+)}\right)}_{\text{Sample Size Bias}} \\
&\qquad + \underbrace{\left(\mathbb{E}[Y_1 \mid \text{EM Complier}, k^*]-\lim_{k\downarrow k^*}\mathbb{E}[Y \mid S=1,k]\right)\times\left(\frac{\pi_{EM}^-}{\pi_{IM}^-+\pi_{EM}^-(1+\bar{D}^+)}\right)}_{\text{Compositional Bias}}
\end{aligned} \tag{4}$$

where $\pi_{IM}^- = P(S_1 = S_0 = 1, D_1 > D_0 \mid S_1 = 1)$ and $\pi_{EM}^- = P(S_1 > S_0, D_1 > D_0 \mid S_1 = 1)$ denote the share of intensive- and extensive-margin compliers directly to the left (barely-eligible) side of the cutoff in the enrollment-selected sample. The term $\bar{D}^+ = \lim_{k\downarrow k^*}\mathbb{E}[D \mid S = 1, k]$ denotes the share of students in the enrollment-selected sample who are "always-takers," receiving grants despite being on the right (barely-ineligible) side of the threshold.[29]

Equation (4) shows that when grants cause some students to enroll in college, a naïve fuzzy RD recovers a biased estimate of $LATE_{IM}$, the treatment effect for intensive-margin compliers. This bias enters the limiting Wald estimand in two ways. The first term is the sample size bias, which arises because the sample on the eligible side of the cutoff becomes larger due to the entry of extensive-margin compliers. The second term is the compositional bias, which depends on $\mathbb{E}[Y_1 \mid \text{EM Complier}]$, the mean treated potential outcome of extensive-margin compliers. The sign of this bias term depends on how extensive-margin compliers' treated potential outcomes differ from the mean potential outcome across all students on the ineligible side of the cutoff. Unfortunately, even if this difference is zero, the presence of *any* enrollment effect will cause the sample size bias to be nonzero, leading the selected-sample Wald estimand to be biased downwards relative to $LATE_{IM}$. Equation (4) therefore recovers $LATE_{IM}$ if and only if there are no enrollment effects so that π_{EM}^- equals zero.

Of course, one solution to this identification challenge is to restrict attention to grant aid programs that have no impact on student enrollment. Indeed, this is the approach taken by many RD studies of grant aid using selected data, which first establish the absence of enrollment effects by showing continuity of the density of the running variable at the grant threshold (Denning et al. 2019; Scott-Clayton and Zafar 2019). Because all compliers are intensive-margin compliers in this case, meaning that $\pi_{EM}^- = 0$, both bias terms in (4) disappear. As a result, the fuzzy RD estimand

[29] Note that because the entry of extensive-margin compliers increases the sample size on the eligible side of the cutoff, it is no longer the case that the sample shares of various complier types are equal approaching the cutoff from both sides. For this reason, I use the notation k^{*-} and k^{*+} to denote limits of the cutoff from the left (eligible) and right (ineligible) sides.

is a valid estimator of $LATE_{IM}$ whenever grant programs do not cause marginal students to enroll.

However, the fact that enrollment-selected data is ubiquitous in higher education (Rothstein 2004), together with the fact that many grant aid programs have impacts on enrollment (Dynarski 2000), suggests the need for an alternative approach that delivers valid estimates of treatment effects even in the presence of the selection bias caused by enrollment effects. Before developing such an approach in Section 7, I first evaluate the extent of this enrollment bias in my empirical settings.

4 How Empirically Important is the Missing Data Problem?

The foregoing analysis shows that when grant aid programs have enrollment effects, standard RD estimation in a selected sample of enrolled students will be biased due to missing data on students induced by grant aid to enroll in college. In this section, I evaluate the magnitude of this missing-data problem in my empirical settings by estimating the enrollment effects of grant aid at each of the three treatment margins I study.

4.1 Measurement and Testing

With data on all college applicants, it would be possible to directly observe changes in enrollments across the cutoff, making it possible to measure grants' enrollment effects using an RD estimator with an indicator for college enrollment as the outcome variable.[30] Such an estimator is not possible in my setting because my dataset is itself conditioned on college enrollment. However, if the increase in grant aid at the cutoff causes some studendts to enter the sample, then the density of the running variable will jump discontinuously at the cutoff. As a result, a standard regression discontinuity manipulation test (e.g., McCrary 2008) provides a useful diagnostic test for evaluating whether standard IV is appropriate for estimating grant impacts.

4.2 Estimates of Enrollment Effects

4.2.1 Enrollment Effects among Four-Year College Applicants

Figures 1a and 1b plot estimates of the density of the running variable at the TEXAS Grant and Auto Zero thresholds for students in four-year colleges. Panel (a) shows the density of the EFC for students near the TEXAS Grant threshold, and Panel (b) shows the density of AGI for students near the Auto-Zero threshold. To make the density estimates comparable, I normalize the y-axis

[30] In fact, with data on all college applicants and a grant system targeted towards a single schooling option (e.g., four-year schooling), variants of Abadie (2002)-style regressions identify not only the overall share of extensive-margin compliers but the distribution of their origin locations. This distribution is described in Angrist et al. (2022) and Abdulkadiroğlu et al. (2017) as the "distribution of counterfactual destinies."

in each figure so that 1 equals the point estimate of the density on the right (barely-ineligible) side of the cutoff. Each figure plots a scaled histogram of the running variable for each cutoff at regularly-spaced bins,[31] together with estimated densities and bias-corrected confidence intervals constructed using the method of Cattaneo et al. (2018).

The estimates in Figures 1a and 1b indicate that neither the TEXAS Grant nor the Automatic Zero EFC threshold has a significant effect on student enrollments in four-year schools. While an enrollment effect would cause the density on the left (barely-eligible) side of the cutoff to be higher than the density on the right, there is no evidence of such a jump in either sample. Appendix Tables B1 and B2 confirm this visual evidence, showing results from formal McCrary (2008) and Cattaneo et al. (2018) tests of the null hypothesis of equality at the cutoff. I cannot reject that the density is continuous across a wide variety of estimation bandwidths and subsamples; for example, the estimated (McCrary 2008) difference in densities at the TEXAS Grant cutoff is exactly zero with a standard error of (0.23), and the estimated difference at the Auto-Zero cutoff is -0.018 with a standard error of (0.028).

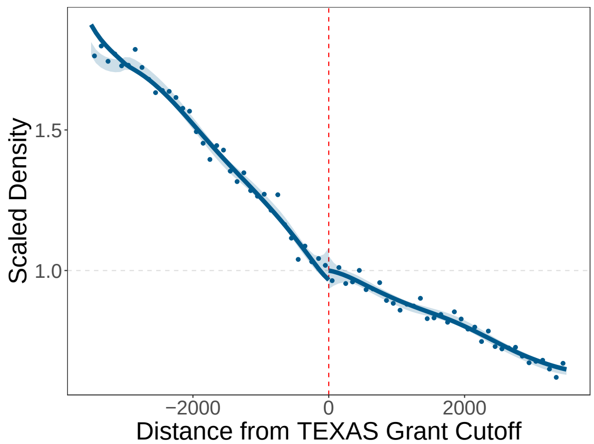
A second heuristic test for enrollment effects is to examine whether the bivariate relationship between students' predetermined characteristics and the running variable jumps discontinuously at the cutoff. Appendix Figures A2 and A3 plot these relationships and report RD estimates at the threshold for a wide variety of predetermined covariates at the TEXAS Grant and Auto-Zero cutoffs in four-year schools. I find no evidence of a statistically significant discontinuity in any student characteristic at either the TEXAS Grant or the Auto-Zero thresholds for students attending four-year colleges, indicating that the composition of students does not meaningfully change across either cutoff.

I also find no evidence that the increase in grant funding at the TEXAS Grant threshold causes four-year students to "upgrade" their schooling choices, e.g., by attending more expensive or higher-quality schools. Appendix Table B4 reports regression discontinuity estimates where the outcomes are means of institutional characteristics, measured at the entry cohort-by-institution level. If TEXAS Grant recipients change their enrollment choices, then cohort-by-school characteristics should be discontinuous at the grant threshold. I find no evidence of any such discontinuity in the mean demographic characteristics, standardized test scores, or mean financial aid awards of students' peers across the threshold. These results hold even when restricting the sample to students whose application records indicate acceptance to more than one college (columns (3) and (4)), arguably the population most likely to respond to the TEXAS Grant by changing their enrollment behavior. Overall, the results indicate that at the margins of receiving a TEXAS Grant or an Automatic Zero EFC, the increase in grant aid at the threshold does not cause students to enroll in four-year colleges or change their enrollments conditional on this sector.

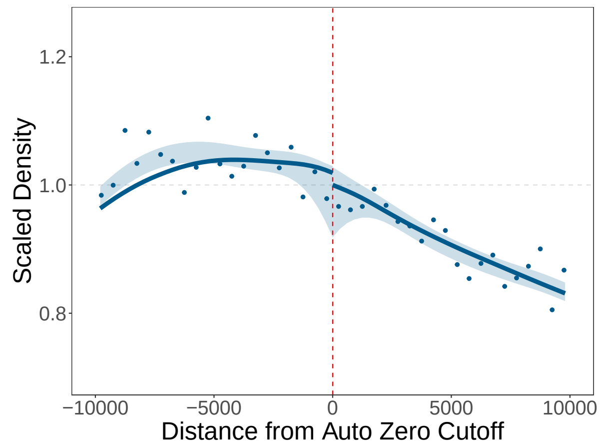
[31] The binwidth for Panel (a) is \$100 of EFC, and the binwidth for Panels (b) is \$1,000 of AGI.

Figure 1: Enrollment Effects at the TEXAS Grant and Auto-Zero Cutoffs

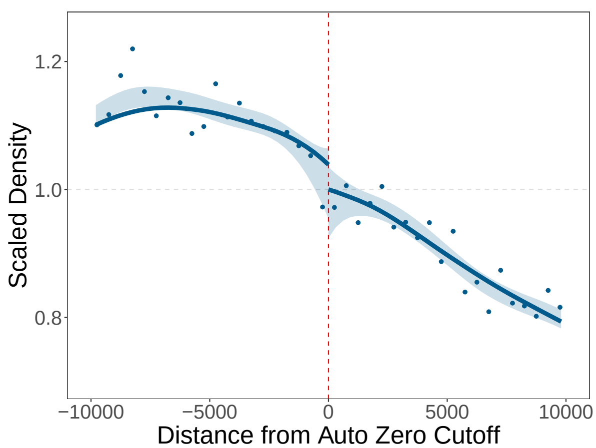
(a) Density: TEXAS Grant (4-Year Enrollees)



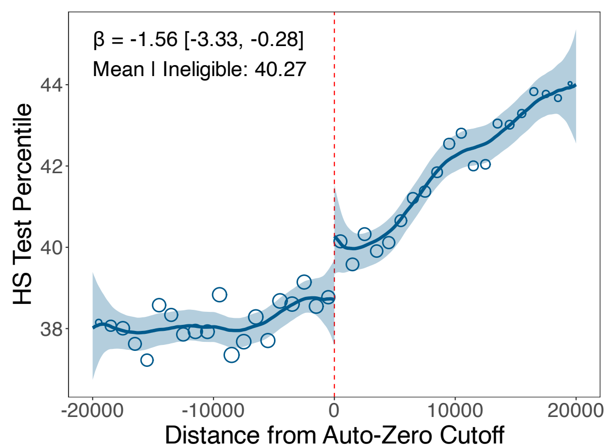
(b) Density: Auto-Zero (4-Year Enrollees)



(c) Density: Auto-Zero Cutoff (2-Year Enrollees)



(d) RD: Tenth Grade Test Scores (2-Year Enrollees)



Notes: Panels (a) through (c) show the density of the running variable in a neighborhood of the assignment threshold for all three estimation samples. Panel (a) shows the density of four-year college enrollees' Expected Family Contribution (EFC) in a neighborhood of the TEXAS Grant cutoff. Panels (b) and (c) show the density of students' incoming family Adjusted Gross Income (AGI) in a neighborhood of the Auto-Zero EFC threshold, with Panel (b) showing four-year enrollees and Panel (c) showing two-year enrollees. Note that the running variables differ across the programs due to different assignment rules. The y-axis in Panels (a) through (c) is normalized so that 1 equals the density point estimate on the right (ineligible) side of the cutoff. The points show histogram estimates of the running variable. The smoothed fits to either side of the cutoff are local-linear estimates of the density and the blue shaded regions are 95 percent bias-corrected confidence intervals produced using the method of Cattaneo et al. (2018). Panel (d) plots the bivariate relationship between two-year community college students' entering family AGI and their tenth grade test score percentile. The circles show raw means by $1,000 AGI bins. The lines and shaded regions on either side of the threshold are local-linear fits with 95 percent bias-corrected confidence intervals produced using the method of Calonico et al. (2014). The annotation reports the estimated discontinuity at the cutoff together with the estimated fit approaching the threshold from the right (ineligible) side. The plots are shown for the main estimation samples: the 2007-2017 entering cohorts for the TEXAS Grant, and the 2008-2012 entering cohorts for the Auto-Zero threshold.

4.3 Enrollment Effects at the Auto-Zero Threshold in Two-Year Colleges

Figures 1c and 1d investigate whether crossing the Auto-Zero EFC threshold causes students to enroll in two-year community colleges. In contrast to the results in four-year schools, and consistent with the findings of Denning et al. (2019), I find evidence that students respond to receiving a zero EFC by changing their enrollment behavior. Figure 1c shows visual evidence of a jump in the number of students attending community colleges on the left (barely-eligible) side of the threshold, consistent with an effect of zero-EFC eligibility on students' enrollment behavior. Figure 1d reinforces this conclusion, plotting students' average tenth-grade test scores in a window of the Auto-Zero cutoff. Students on the barely-eligible side of the Auto-Zero threshold have mean test scores that are roughly 1.5 percentiles lower than students on the barely-ineligible side, and the difference is significant at the 95 percent level. Appendix Figure A4 shows that similar discontinuities appear in many predetermined covariates: two-year community college students on the barely-eligible side of the threshold are less likely to be white and have higher home-tract poverty and SNAP benefit receipt rates than students on the barely-ineligible side, as measured by the location of their high schools. Taken together, the results indicate that standard fuzzy RD methods applied across the Auto-Zero threshold in two-year schools will not yield valid estimates of treatment effects.

4.3.1 Testing for the Presence of Defiers

Importantly, the patterns documented in Figures 1c and 1d do not by themselves prove that the compositional changes at the cutoff are entirely due to the entry of students who otherwise would not have enrolled in college. Indeed, because a $0 EFC qualifies students for the maximum federal Pell Grant, it is possible that some students may *leave* the sample as a result of crossing the cutoff, for example by enrolling in four-year or private institutions. Students of this behavior type are "enrollment defiers" in terms of the enrollment monotonicity assumption (Assumption 4) and represent a threat to identification. If some students leave the sample as a result of crossing to the eligible side of the threshold, then regression-discontinuity estimates across the cutoff will conflate the causal impacts of grant aid programs with compositional changes caused by the departure of enrollment defiers from the sample. Moreover, even the number of choice defiers is not identified due to the fact that other students (the extensive-margin compliers) enter the sample as a result of treatment. The overall positive enrollment effect in Figure 1c could therefore be caused by either small or large numbers of defiers leaving the sample, as long as the offsetting number of extensive-margin compliers is slightly larger.

However, if defiers exist, then the compositional changes in Figure 1d offer a clue as to their likely composition. Average test scores fall at the cutoff, suggesting that defiers who leave the sample at the cutoff are higher-achieving students than students who remain. Appendix Figure A5 finds support for this hypothesis, plotting the density of the running variable separately by students' pre-college achievement. Figure A5a shows positive enrollment effects among the roughly 90 percent of community college students who score below the 80th percentile on standardized tests among their high school graduating class. In contrast, Figure A5b shows a significant negative enrollment effect among students who score in the top quintile of their high school graduating class on standardized tests. This discontinuity is statistically significant; Appendix Table B3 shows that the p-value associated with the McCrary (2008) test of equality at the cutoff is 0.028. Moreover, as Appendix Figure A6 shows, this defier behavior appears limited to top-scoring students; the discontinuities among all other quintiles are consistent with at least weakly positive enrollment effects.

As a result of these patterns, the effect of financial aid on the outcomes of top-scoring community college students at the Auto-Zero cutoff is not identified. I therefore remove these students from the sample, dropping roughly 10 percent of community college students within my estimation window of the Auto-Zero cutoff. I assume that defiers do not exist among the remaining students, so that the enrollment monotonicity assumption (Assumption 4) holds in the remaining sample.

Taking Stock

Taken together, the results in this section suggest that standard IV methods are sufficient to study the impacts of the TEXAS Grant and Auto-Zero cutoffs among four-year college students. Figure 1 implies that at these treatment margins, *all* compliers are intensive-margin compliers who would attend four-year colleges even without additional grant aid. As a result, neither bias term in (4) applies, so that a standard fuzzy RD recovers valid estiamtes of $LATE_{IM}$. I therefore proceed in Section 5 with standard fuzzy regression-discontinuity methods in order to estimate these grants' impacts on intensive-margin compliers.

In contrast, the results in Figures 1c and 1d suggest that standard IV methods applied to the 2-year Auto-Zero EFC cutoff will fail to recover a valid LATE for any group of compliers. At this margin, there exist extensive-margin compliers who enter the sample as a result of crossing the Auto-Zero EFC threshold, as shown in Figure A5a. The entry of these students into the sample will cause standard IV to recover a biased estimate of $LATE_{IM}$ by (4), suggesting the need for an alternative framework which recovers valid estimates of treatment effects even in the presence of this missing-data problem. I return to this issue in Section 7, developing a nonparametric identification argument that separately develops bounds on treatment effects for extensive-margin and intensive-margin compliers.

4.4 Implementation

The previous sections indicate that for programs without enrollment effects —- namely, the TEXAS Grant and the Auto-Zero EFC cutoff in four-year schools —- the populations to either side of the cutoff consist entirely of intensive-margin students and are therefore comparable using a standard fuzzy RD design. The estimand of interest is:

$$\beta_{IV} = \frac{\lim_{r\uparrow k^*} \mathbb{E}[Y \mid k = r] - \lim_{r\downarrow k^*} \mathbb{E}[Y \mid k = r]}{\lim_{r\uparrow k^*} \mathbb{E}[D \mid k = r] - \lim_{r\downarrow k^*} \mathbb{E}[D \mid k = r]}$$

where Y is an outcome such as graduation, and where D is an indicator for the grant treatment.[32] I follow standard nonparametric practice to estimate β_{IV} by taking the ratio of two RD estimators:

$$\hat{\beta}_{IV} = \frac{\hat{\mu}_{Y-} - \hat{\mu}_{Y+}}{\hat{\mu}_{D-} - \hat{\mu}_{D+}}$$

where $(\hat{\mu}_{Y-}, \hat{\mu}_{Y+}, \hat{\mu}_{D-}, \hat{\mu}_{D+})$ are local linear estimates of the conditional expectations of Y and D given k, estimated separately approaching the cutoff from the left and the right.[33]

I also report results from specifications that include an additively separable linear term in a vector of covariates $\boldsymbol{X}$ (Calonico, Cattaneo, Farrell, and Titiunik 2019). Reassuringly, adding covariates does not significantly change the IV point estimate for any outcome I consider. However, including covariates slightly improves the precision of the estimates. I implement the estimators by choosing the MSE–optimal bandwidth separately for each outcome according to Calonico, Cattaneo and Farrell (2019). Practically, the optimal bandwidth is almost always between \$750 and \$900 of EFC for models with covariates and between \$900 and \$1200 of EFC for models without covariates.

5 The Impacts of the TEXAS Grant on Student Outcomes

5.1 First Stage: The TEXAS Grant Cutoff and Grant Aid Awards

I first show that crossing the EFC threshold results in a substantial jump in the probability of receiving a TEXAS Grant. Panel (a) of Figure 2 shows the increase in TEXAS Grants at the cutoff among students entering public four-year Texas colleges during my sample period. The running

[32] In the case of the TEXAS Grant, the treatment an indicator for whether the student receives a TEXAS Grant in their first year of college. In the case of the auto zero EFC cutoff, the treatment is an indicator for whether the student receives a zero EFC.

[33] Formally, for $W \in \{Y, D\}$, the estimators are given by $\hat{\mu}_{Y-} = \alpha_{W-}(k^*, h)$ and $\hat{\mu}_{Y+} = \alpha_{W+}(k^*, h)$, where $\alpha_{W-}(k^*, h)$ and $\alpha_{W-}(k^*, h)$ come from the solutions to the following kernel-weighted least squares problems:

$$\begin{pmatrix} \alpha_{W-}(k,h) \\ \beta_{W-}(k,h) \end{pmatrix} = \arg\min_{\alpha,\beta} \sum_{i=1}^{N} \mathbf{1}\{k \leq k^*\} \left(W_i - \alpha - \beta(k_i - k^*)\right)^2 K_h(k_i - k)$$

$$\begin{pmatrix} \alpha_{W+}(k,h) \\ \beta_{W+}(k,h) \end{pmatrix} = \arg\min_{\alpha,\beta} \sum_{i=1}^{N} \mathbf{1}\{k > k^*\} \left(W_i - \alpha - \beta(k_i - k^*)\right)^2 K_h(k_i - k)$$

where (W_i, k_i) denote data on W and k for observation i, and where K_h is a triangular kernel with bandwidth h.

variable is students' EFC in the year of college entry, which determines whether students are eligible for TEXAS Grants in their first year of college.

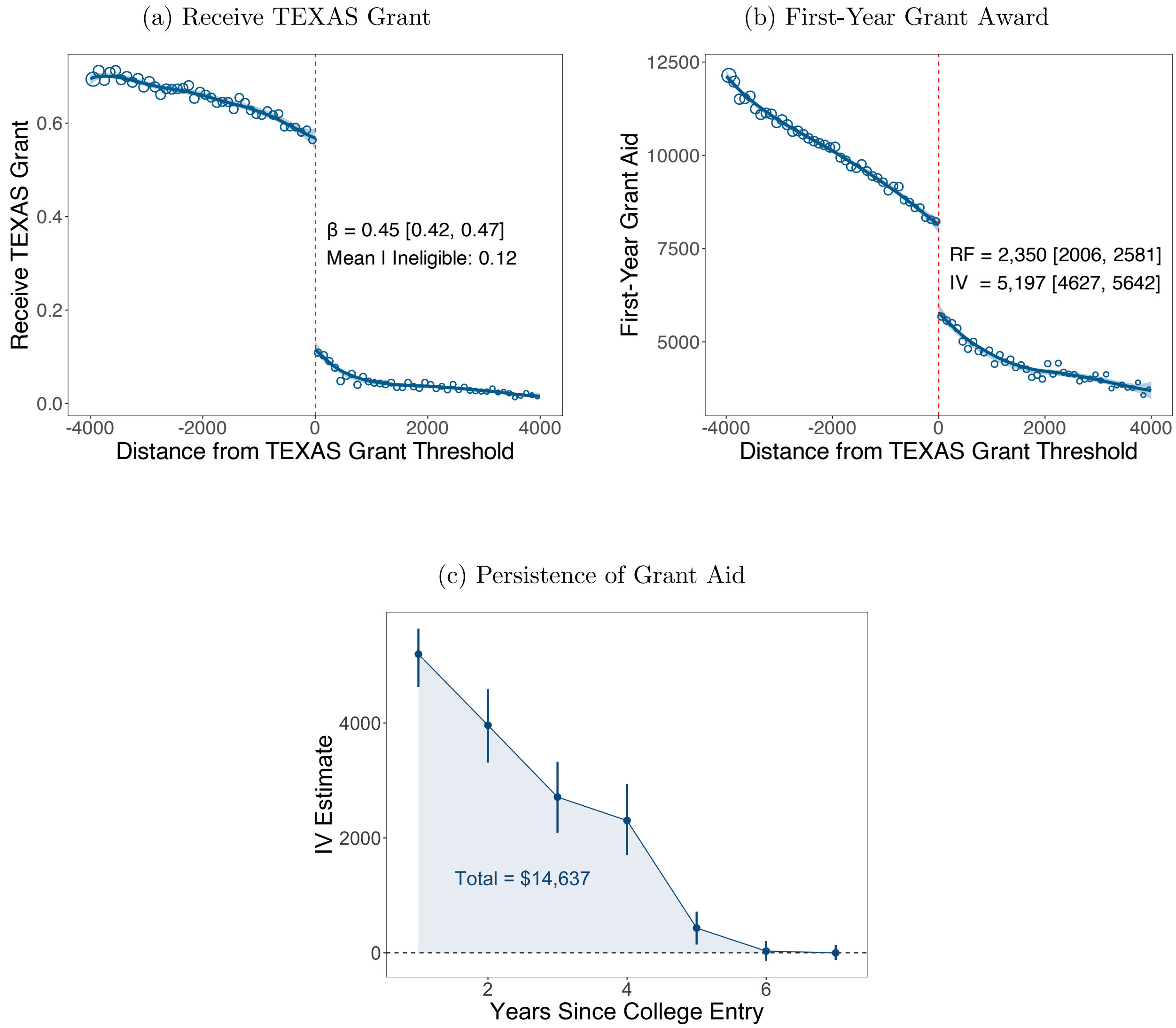
The figure documents a large jump in initial-year TEXAS Grants at the cutoff. The proportion of students receiving TEXAS Grants in their first year of college jumps by 45 percentage points, from 11.6 percent of barely-ineligible students receiving grants on the right of the cutoff to 56.8 percent of barely-eligible students receiving grants on the left. Importantly, compliance with the grant assignment rule is imperfect: roughly half of barely-eligible students do not receive TEXAS Grants despite being on the eligible side of the threshold, and 12 percent of financially-ineligible students receive TEXAS Grants anyway. There are two main reasons for this imperfect treatment assignment. First, the grants are rationed; there are not sufficient funds available to award TEXAS Grants to all students who are eligible on the basis of their EFC. Second, schools are afforded substantial discretion in determining which students are awarded grants, and are allowed to award TEXAS Grants even to students whose EFCs fall above the grant assignment threshold. In practice, Figure 2a shows that this discretion is used relatively sparingly, and crossing the threshold still confers a large increase in the probability of receiving a TEXAS Grant.

Figures 2b and 2c show that receiving a TEXAS Grant corresponds to a substantial and persistent increase in students' grant aid packages. Figure 2b plots the bivariate relationship between students' first-year grant aid awards and their EFC in the first year of college. Students on the barely-eligible side of the threshold receive $2,350 more in grant aid, on average, than barely-ineligible students. The corresponding IV estimate for the increase in first-year grant aid among compliers is $5,197. Figure 2c shows that this increase in grant aid is persistent. The figure plots IV estimates of the increase in grant aid for compliers separately in each year after college. In constructing the figure, I assign $0 in grant awards to students who exit the college sample; as a result, a note of caution is warranted in interpreting Figure 2c, because the plotted estimates incorporate any causal effect of the TEXAS Grant on students' probability of remaining in college. Nevertheless, the impact of receiving an initial-year grant on average future aid receipts is significant; for compliers, an initial-year TEXAS Grant increases average aid received over the next 6 years of college by $14,637 on average.

5.2 Impacts on Persistence and Graduation

Figure 3 reports estimates of the impact of the TEXAS Grant on students' persistence in college and graduation probability. Figure 3a displays the reduced-form graphical relationship between students' Expected Family Contribution and the probability that students re-enroll in college for a second year. The figure shows that despite the large increase in average grant aid awards at the cutoff, there is no statistically significant change in the probability of second-year reenrollment at the threshold. The reduced-form RD estimate at the cutoff is a relatively precise zero (confidence

Figure 2: First Stage at the TEXAS Grant Threshold



Notes: This figure plots the bivariate relationship between students' grant aid awards and their Expected Family Contribution (EFC) in a window of the TEXAS Grant Cutoff. Panel (a) shows the instrumental variables first stage. The outcome is an indicator for whether the student receives a TEXAS Grant in their initial year of college. The plot annotation reports the estimated jump in initial-year TEXAS Grants at the threshold, together with the mean fraction of students on the barely-ineligible (right) side of the cutoff who do not receive grants. Panel (b) plots the relationship between total first-year grant aid awards, defined as the sum of all grant aid sources, and students' entry-year EFCs. The plot annotation reports reduced-form and IV estimates of the change in grant aid at the cutoff. Panel (c) reports IV estimates from year-by-year regressions, where the outcome is the total grant aid award received by students in each year following college entry. The error bars reflect 95 percent bias-corrected confidence intervals (Calonico et al. 2014), clustered at the institution-by-entry-cohort level. Note that because students who leave college are assigned grants of zero, the estimates in Panel (c) summarize the TEXAS Grant's impacts on both grant aid and college reenrollment.

interval $[-0.02, 0.02]$), and I am able to rule out that the overall rate of reenrollment changes by more than 2 percentage points at the cutoff.

Figure 3b shows results for BA completion, plotting the relationship between students' EFC and the probability of graduating within six years of college entry. I do not find evidence that the TEXAS Grant singificantly increases graduation rates at the cutoff; the reduced-form point estimate is positive at 1 percentage point, but the confidence interval rules out increases in graduation rates of more than 3 percentage points or declines of more than 2 percentage points. Importantly, this null result is not because baseline graduation rates are too high for any program to improve them; the figure shows that just over half of barely-ineligible students graduate. The results therefore indicate that for middle-income students at the margin of receiving a TEXAS Grant, additional grant aid is not a key determining factor in whether students complete college.

Figures 3c and 3d examine effects on persistence and graduation at different time horizons. The figures plot IV estimates of the impact of receiving a first-year TEXAS Grant on the probability of reenrollment and graduation, with the corresponding point estimates reported in Appendix Table B8. Though the point estimates are small and positive in most years, they are not statistically significant at any time horizon. I conclude that the increase in financial support associated with crossing the TEXAS Grant threshold does not cause a significant increase in students' probability of reenrolling or graduating from college.

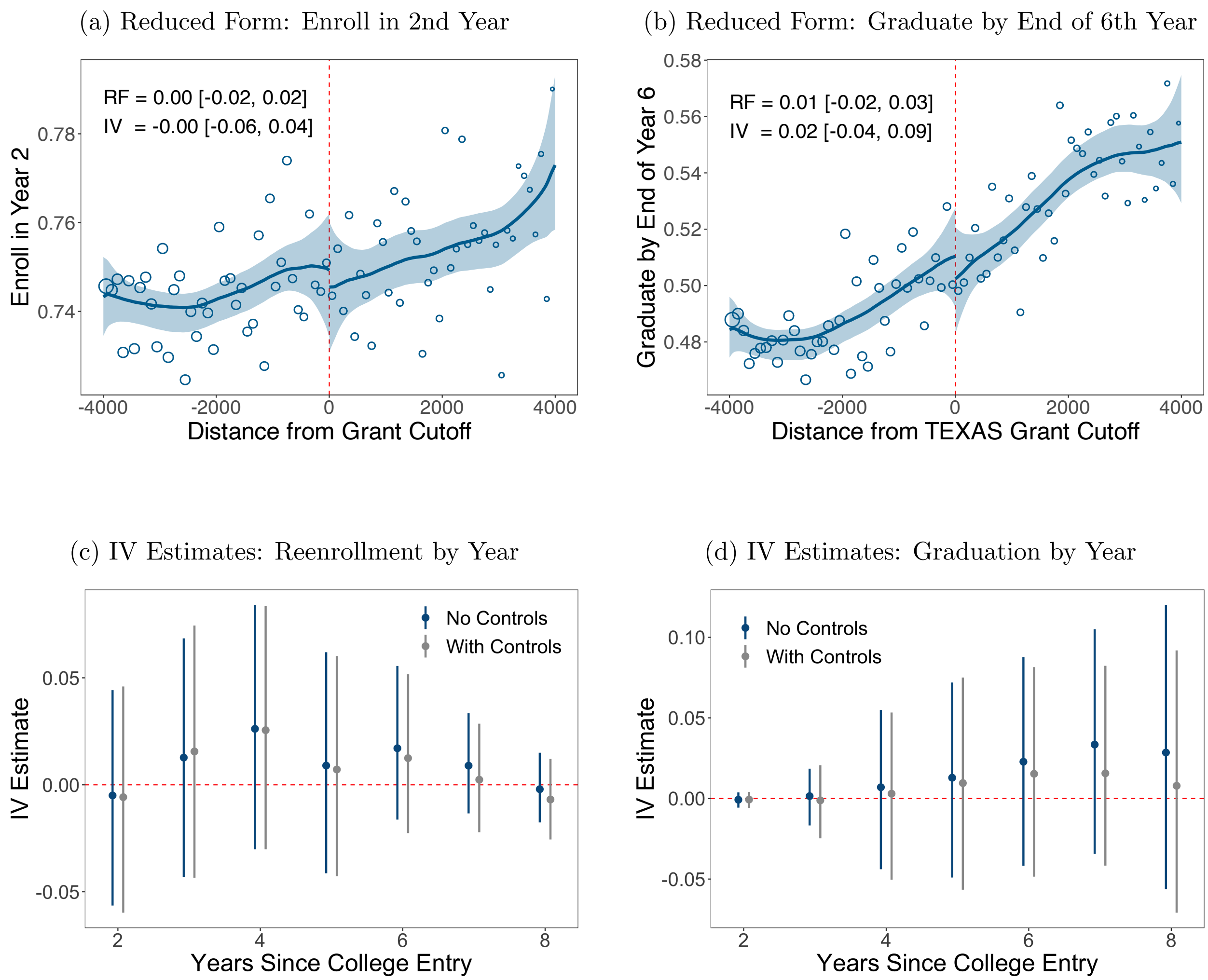
In addition to these results, I also find no evidence that the TEXAS Grant has a significant effect on shorter-run measures of student success in college. Appendix Figure A7 examines the impact of the TEXAS Grant on complier students' course completion. The results show that there is no significant effect of the TEXAS Grant on the number of credits completed by complier students, indicating that additional grant aid does not induce students awarded TEXAS Grants to complete significantly greater numbers of credit hours.

Taken together, the results indicate that the increase in grant aid at the threshold has no impact on the average postsecondary outcomes of TEXAS Grant recipients. Importantly, this result applies only to *marginal* recipients of the TEXAS Grant who receive funds as a causal result of crossing the grant's eligibility threshold, and may not be representative of the TEXAS Grant's impacts on lower-income students with EFCs well below the grant cutoff. In Section 5.3, I investigate the possibility that reallocations of grant aid to lower-income students would produce gains in overall student outcomes through a comparison to the Auto-Zero EFC threshold in four-year schools.

5.2.1 Impacts on Loans and Earnings

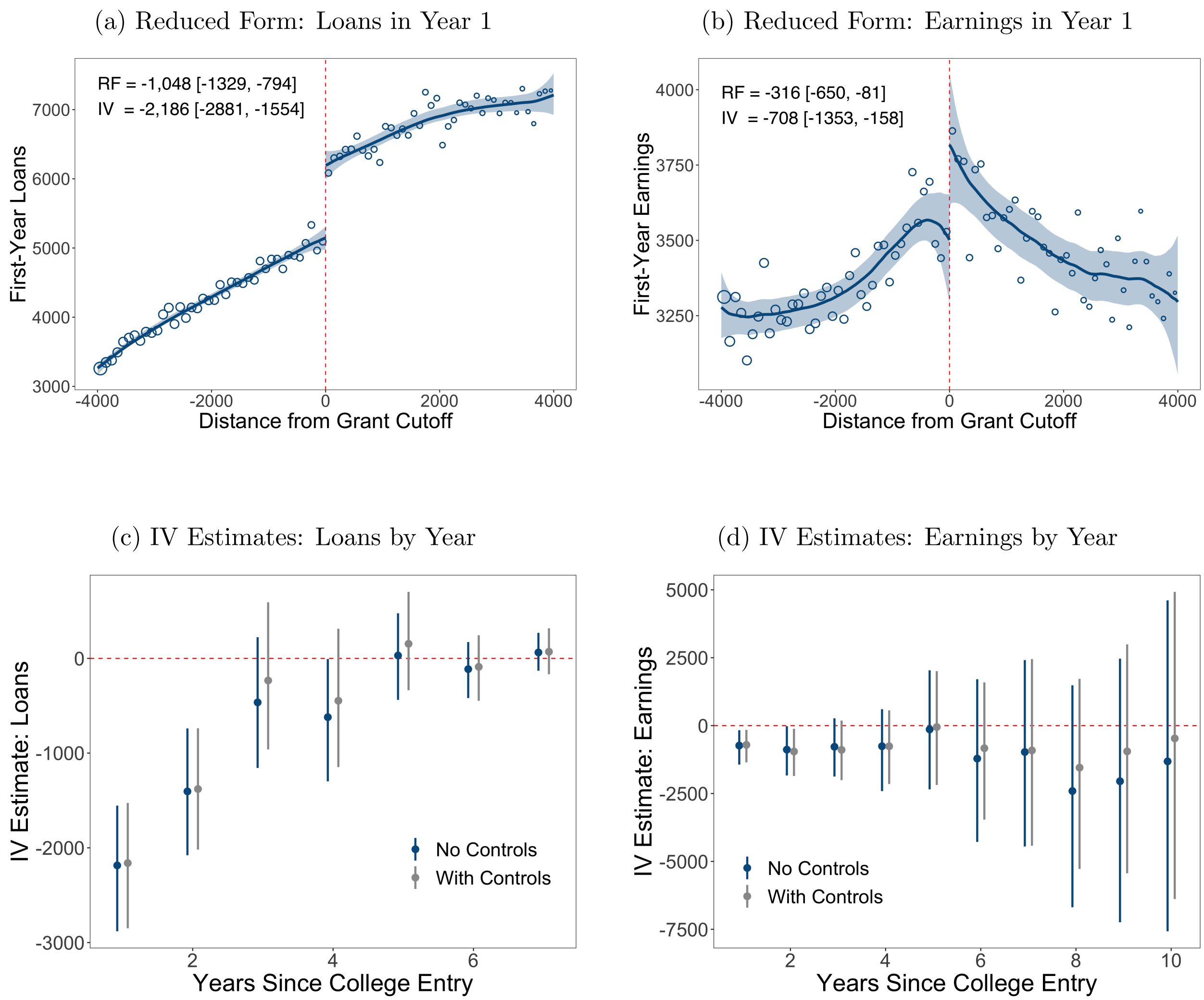
Figure 4 reports estimates of the TEXAS Grant's impacts on students' labor market earnings and student loans. The increase in TEXAS Grants at the cutoff corresponds with a sharp decrease in student loans and earnings during college, shown in Figures 4a and 4b. Students on the barely-

Figure 3: Effects of the TEXAS Grant on Persistence and Graduation



Notes: This figure shows estimates of the impacts of the TEXAS Grant on students' reenrollment in college and probability of graduation. Panel (a) shows the reduced-form relationship between students' Expected Family Contribution (EFC) and the probability of completing a BA degree at their initially-enrolled institution within 6 academic years of entry. I pool all entry cohorts and normalize the running variable so that zero equals the TEXAS Grant's EFC cutoff. The points show unconditional means by $100-wide EFC bins, and the lines to either side of the cutoff are local-linear fits with bias-corrected confidence intervals following Calonico et al. (2014). The annotation reports reduced-form and IV estimates of the discontinuity at the cutoff. Panel (b) shows IV estimates of the discontinuity at the cutoff at several estimation horizons. Results through 4 years are for the 2007-2017 entry cohorts The last entry year for results at 5, 6, 7, and 8 years is 2016, 2015, 2014, and 2013, respectively. The error bars reflect 95 percent bias-corrected confidence intervals, clustered at the institution-by-entry-cohort level.

Figure 4: Effects of the TEXAS Grant on Loans and Earnings



(a) Reduced Form: Loans in Year 1

(b) Reduced Form: Earnings in Year 1

(c) IV Estimates: Loans by Year

(d) IV Estimates: Earnings by Year

Notes: This figure shows estimates of the impacts of the TEXAS Grant on enrollment persistence Panel (a) shows the reduced-form relationship between students' Expected Family Contribution (EFC) and the probability of completing a BA degree at their initially-enrolled institution within 6 academic years of entry. The points show unconditional means by $100-wide EFC bins, and the lines to either side of the cutoff are local-linear fits with bias-corrected confidence intervals following Calonico et al. (2014). The annotation reports reduced-form and IV estimates of the discontinuity at the cutoff. Panel (b) shows IV estimates of the discontinuity at the cutoff at several estimation horizons. Results through 4 years are for the 2007-2017 entry cohorts The last entry year for results at 5, 6, 7, and 8 years is 2016, 2015, 2014, and 2013, respectively.

eligible side of the TEXAS Grant threshold take out over $\$1,000$ less in average first-year loans than students on the barely-ineligible side. Scaling this reduced-form estimate by the first stage implies that compliers reduce their first-year loans by over $\$2,100$ in response to receiving a TEXAS Grant award. Panel (b) documents a similar effect on complier students' earnings during college. Students on the barely-eligible side of the cutoff reduce their earnings by $300, on average, relative to students on the barely-ineligible side. The corresponding IV estimates indicate a $700 decrease in first-year earnings among grant compliers.

Panels (c) and (d) investigate the persistence of these effects, plotting IV estimates of the TEXAS Grant's impact on compliers' yearly borrowing and labor market earnings.[34] The grant's impact on compliers' loans and earnings is persistent, with point estimates remaining negative and significant through the second year of college. This persistence reflects the fact that TEXAS Grant recipients can renew their grants for additional years, subject to satisfactory academic progress requirements. Estimates of loan impacts fall towards zero over time, in part reflecting the fact that many students do not remain in college. Estimates on earnings appear more persistently negative, although they become imprecise in later years.

5.3 Comparison: Impacts of Automatic Zero EFC Eligibility

The results in Section 5 indicate that for middle-income students at the margin of receiving a TEXAS Grant, additional grant aid is not an influential factor in determining post secondary outcomes. In this section, I compare these null effects against estimates of the impact of additional financial support on student outcomes for four-year college students at the Automatic Zero EFC Cutoff.

5.3.1 First Stage: The Auto-Zero EFC Cutoff and Grant Aid

Figure 5 describes the impact of crossing the automatic zero EFC threshold on the grant aid awards of four-year college students. Figure 5a shows how crossing the Automatic Zero EFC threshold affects whether students receive a zero EFC in their first year of college, which I use as the definition of binary treatment to analyze the Automatic Zero EFC cutoff. The x axis plots students' family Adjusted Gross Income in $1,000 bins, and the y axis plots the proportion of four-year college students whose EFC is zero in the entering year. The figure shows that the probability of being assigned a $0 EFC jumps by 49 percentage points at the threshold. Because other inputs into the EFC formula affect whether a student receives a $0 EFC, a substantial number of students receive zero EFCs despite being on the barely-ineligible side of the threshold; likewise, a small proportion of students receive nonzero EFCs despite being on the barely-eligible side. Nevertheless, the jump in $0 EFC assignments results in a substantial increase in students' first-year grant aid awards, as

[34]To make the figures comparable, I code students who exit college as taking out zero loans.

shown in Figure 5b. Students on the barely-eligible side of the threshold receive an additional $800 of grant aid, on average, than students on the barely-ineligible side. The instrumental-variables estimates show that compliers who receive zero EFCs as a result of crossing the auto-zero threshold receive an additional $1,800 in aid, on average, as a result of crossing the threshold.

Figure 5c shows that this increase in aid is much smaller than the increase for compliers at the TEXAS Grant threshold. The figure plots yearly IV coefficients of the increase in grant aid for compliers at the TEXAS Grant and Auto-Zero thresholds, plotted separately by grant program.[35] The point estimates at $x = 1$ show that the $1,800 increase in first-year aid for Auto-Zero compliers is only a third of the $5,200 increase for TEXAS Grant compliers. This difference persists over time. Summing the yearly estimates reveals that while TEXAS Grant compliers receive an additional $14,600 in aid over the six years following college entry, the increase for compliers at the Automatic Zero EFC threshold is only a quarter as large, amounting to roughly $3,400 in aid.

Figure 5d shows analogous yearly IV estimates where the outcome is students' yearly loans. The point estimates at $x = 1$ show that Auto-Zero EFC compliers reduce their first-year loans by roughly half as much as TEXAS Grant compliers. Notably, the offsetting reduction in loans is roughly half of the first-year change in aid for both programs, suggesting similar elasticities of loan originations to grant aid for both populations. However, in contrast to the persistently negative impact of the TEXAS Grant on student loans, students who receive an Auto-Zero EFC as a result of crossing the threshold take out *greater* average loan burdens in later years of college as a result. Such an effect would arise if treated students become more likely to remain in college. I investigate this possibility in the next subsection by comparing the impacts of the TEXAS Grant and the Zero-EFC policy on students' college persistence and graduation probability.

5.3.2 Grant Awards and Student Outcomes at the Auto-Zero EFC Threshold

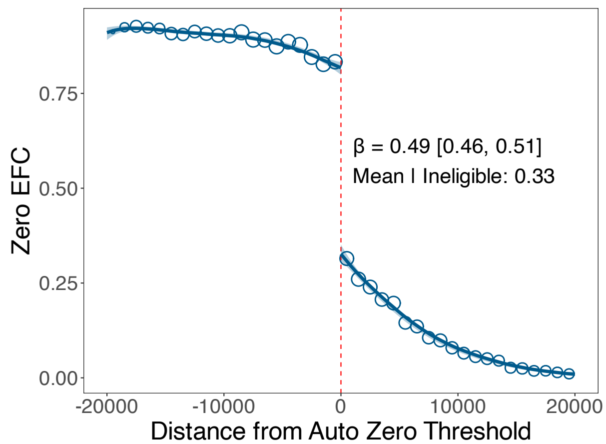
Figure 6 plots IV estimates of the impact of financial support on students' academic outcomes and later-life earnings at the TEXAS Grant and Auto-Zero thresholds. To make the estimates comparable in terms of aid's impact on outcomes, I estimate models where the first stage is measured in $1,000s of grant aid rather than a binary indicator for treatment.[36]

[35] In constructing the figure, I code students who drop out of college as receiving $0 in grant aid to avoid bias due to differential attrition. Estimates in later years may therefore incorporate any potential impacts of grant aid on the probability that students remain in college.

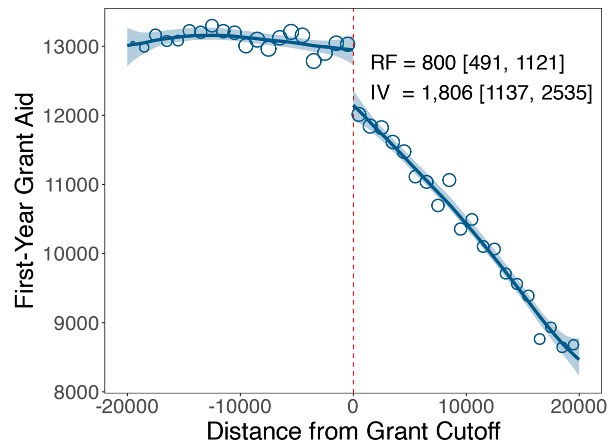
[36] This normalization effectively assumes that the TEXAS Grant and the Auto-Zero EFC cutoff affect students' grant aid awards only through the channel of increased financial support. This assumption would be violated if qualifying for either grant program gave students access to nonfinancial resources that potentially impacted their college performance. This assumption seems reasonable in the case of the Automatic Zero EFC threshold, as students' EFCs are calculated by the federal government (not by colleges) and are primarily used by colleges as an input into financial aid awards. My conversations with financial aid administrators indicate that the TEXAS Grant also acts solely as a subsidy that reduces the price of college. Note also that a similar assumption is inherent in MVPF calculations which scale the benefits of grant programs by their fiscal net costs.

Figure 5: First Stage at the Auto-Zero Threshold in Four-Year Schools

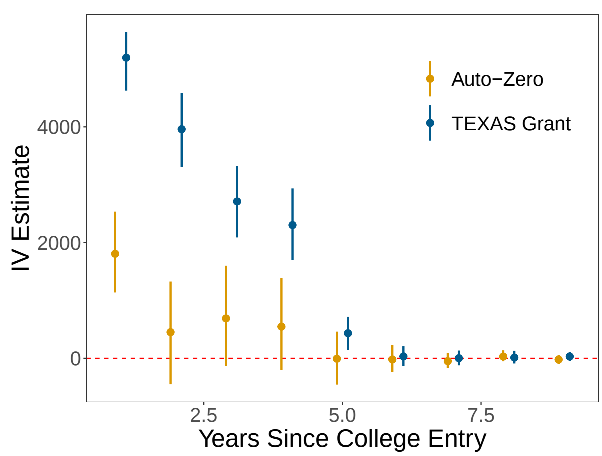
(a) Zero EFC at Auto-Zero Cutoff



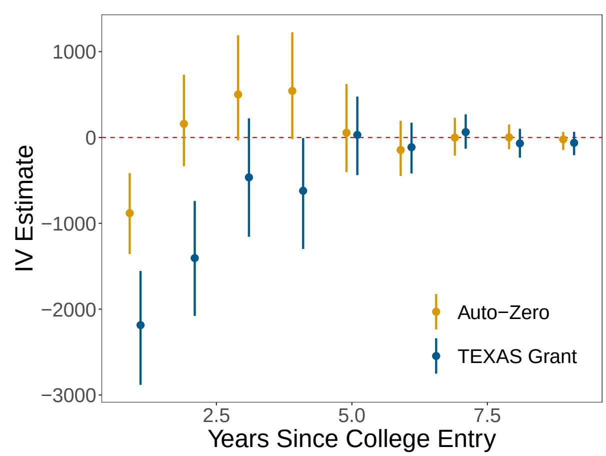
(b) First-Year Grant Aid at Auto-Zero Cutoff



(c) IV Estimates: Yearly Grants for Compliers



(d) IV Estimates: Yearly Loans for Compliers



Notes: This figure summarizes the increase in grant aid stemming from crossing the Automatic Zero EFC threshold, and compares this increase to the change in grant aid at the TEXAS Grant cutoff. Panel (a) reports the fraction of four-year college students who have a zero EFC by their family adjusted gross income (AGI). The plot pools all years of data for the 2008-2012 entering cohorts, and the x axis is normalized within each year so that zero equals the federal Auto-Zero EFC threshold. Panel (b) is similar, but instead plots students' first-year grant aid awards by their incoming AGI. I plot unrestricted means of both outcomes in \$1,000 bins and include estimated local linear regression lines and 95% bias-corrected confidence intervals on each side of the cutoff. Panels (c) and (d) plot instrumental variables estimates of the effect of receiving a TEXAS Grant or an initial-year Zero EFC on students' yearly grant aid (Panel c) and loans (Panel d). The first stage for each program is a binary indicator for whether the student is "treated" by receiving an initial-year TEXAS Grant or an initial-year zero EFC, respectively. In constructing the figures, I define grants and loans as \$0 for students who do not appear in the college sample to avoid attrition bias; note, however, that these grant and loan impacts therefore incorporate the effects of each grant program on students' college persistence. The error bars reflect 95 percent bias-corrected confidence intervals estimated using the method of (Calonico et al. 2014) and clustered at the institution-by-entry-cohort level.

Comparing the estimates reveals that the impact of additional grant aid on student outcomes is starkly different across the two thresholds. Figure 6 shows impacts for college persistence, measured as an indicator for reenrollment. While an additional $1,000 of grant aid at the TEXAS Grant cutoff has a precise null effect on students' probability of college reenrollment, an equivalent increase for poorer students at the margin of the Auto-Zero EFC cutoff increases the probability of reenrollment by 3.4 percentage points in year 3 and 3.9 percentage points in year 4. The point estimate for reenrollment in year 1 is positive, at 1 percentage point, but is statistically imprecise.

This increase in reenrollment probabilities corresponds to large increases in graduation for compliers at the margin of receiving an automatic zero EFC. Figure 6b shows that while TEXAS Grant compliers are not significantly more likely to complete a B.A. at any time horizon, an additional $1,000 of grant aid for compliers at the Auto-Zero cutoff increases graduation rates by 2.9 percentage points by the end of the fourth year after college entry, 3.8 percentage points by the end of the fifth year, and 4.8 percentage points by the end of the sixth. These effects are large; for example, the six-year graduation increase of 4.8 percentage points is a 12 percent increase over the mean 6-year graduation rate of 41% on the barely-ineligible side of the cutoff.

Finally, Figure 6c provides suggestive evidence that these increases in persistence and graduation probability affect students' earnings trajectories. The figure plots the IV impact of an additional $1,000 of financial support on students' earnings, estimated separately by year. The figure has two main takeaways. I find that the increase in aid for compliers at the Auto-Zero threshold lowers labor market earnings during college. Notably, the elasticity of this earnings response to first-year grant aid is larger for Auto-Zero compliers than it is for compliers at the margin at the TEXAS Grant cutoff, although the difference between these coefficients is not statistically significant. Second, I find evidence that this initial decline in earnings is compensated by increased earnings starting six years after college entry. While the coefficients are not statistically significant, they suggest that the impact of financial support on the graduation rates of Auto-Zero compliers spills over into their labor market earnings after college.

Overall, these results align with the findings of Denning et al. (2019) (DMT), who use the same data source to study the impact of crossing the Auto-Zero threshold on the outcomes of four-year college students in Texas. However, my results differ from theirs in some notable respects. First, I find that Auto-Zero EFC compliers' earnings fall during college, while DMT do not find any evidence of this temporary earnings decline. Second, I find slight differences in the estimated impacts on persistence and graduation, finding (in contrast to DMT) that students are more likely to persist until the fourth year of college, and finding slightly smaller impacts than theirs on the probability of graduating within 5 years. These differences may stem from differences in sample composition; I use one additional year of data (2012) compared to DMT, and the starting point for constructing my sample is the set of all recent Texas public high school graduates rather than the full set of all first-time-in-college dependent students (including non-Texas residents). Overall, however, I find robust positive impacts of crossing the Auto-Zero threshold on the outcomes of compliers in four-year schools.

Additionally, my results may shed insight on the mechanism through which financial aid affects the outcomes of low-income students. A recent study by Eng and Matsudaira (2021) re-visits the Auto-Zero EFC threshold using the universe of federal aid recipients (rather than the selected sample of Texas students), finding much smaller effects of crossing the Automatic Zero EFC cutoff on students' probability of graduating from their initially-enrolled institution. For example, their IV estimates indicate that an additional $1,000 of aid at the threshold corresponds to a statistically imprecise 0.8 percentage point increase in the probability of BA degree completion within 6 years,[37] in contrast to my and DMT's estimate of roughly 5 percentage points. In considering the discrepancy between these results and the findings of DMT, they conjecture that qualifying for an Auto-Zero EFC may cause large outcome gains for Texas students because a zero EFC also increases the probability of receiving a TEXAS Grant, which — in contrast to the federal Pell grant — provides a guarantee of financial support covering the entirety of tuition and fees for up to five years after students' initial college enrollment. The Automatic Zero EFC policy may therefore be especially effective in improving the outcomes of TEXAS students because it coresponds to the *persistent* increase in grant aid documented in Figure 5c. This persistent increase is likely entirely due to the impact of the TEXAS Grant, because an initial-year zero EFC is not automatically renewed in future years.[38] In contrast, an initial-year TEXAS Grant is renewable regardless of most changes in students' financial situations, yielding a persistent increase in grant aid over time. Overall, the results suggest that the renewal-guarantee aspect of the TEXAS Grant, and not the Pell Grant alone, is important in shaping the outcomes of students near the Auto-Zero EFC threshold.

6 Mechanisms: Who Gets a TEXAS Grant?

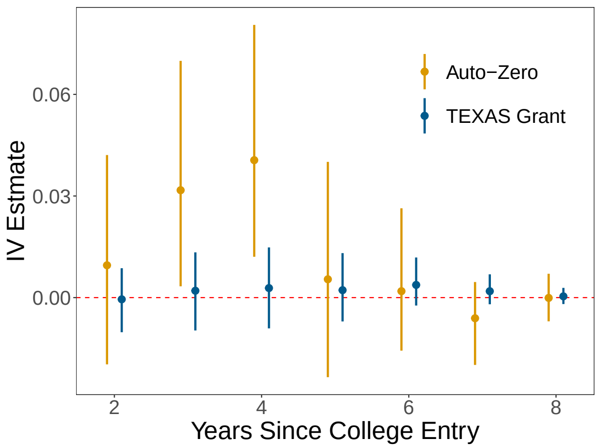
The previous sections show that for compliers at the TEXAS Grant threshold, a substantial increase in grant funding does not correspond to significant increases in enrollment, graduation, or earnings. This section examines possible mechanisms for this result by analyzing the characteristics of students who receive grants at the threshold. Which students receive grants as a result of crossing the threshold, and how do they compare to students who do not receive grants despite being financially eligible? I find that schools are substantially more likely to give grants to two subpopulations of

[37] See Eng and Matsudaira (2021), Table 3.

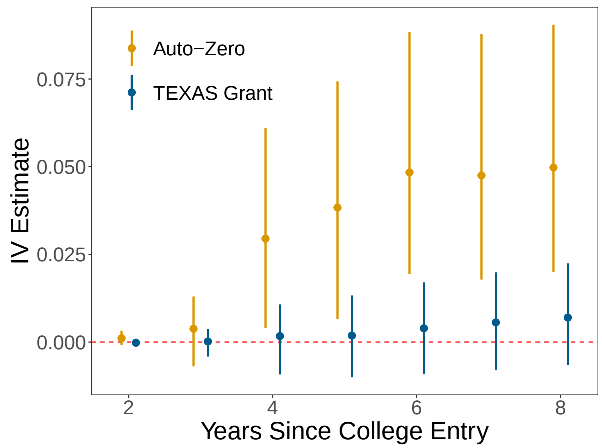
[38] In fact, if students' yearly family AGI incorporates an element of random noise, then students who barely receive a zero EFC in the initial year of college have probability zero of being on the barely-eligible side of the threshold in any future year. This is likely to especially hold in the samples used in my paper and DMT, which remove students who report AGI multiples of $1,000 due to bias concerns due to bunching.

Figure 6: IV Effects of $1,000 of Grant Aid at the TEXAS Grant and Auto Zero Cutoffs in Four-Year Schools

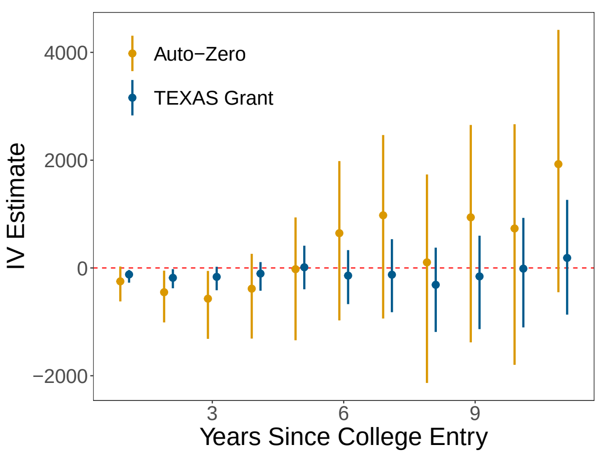
(a) Persistence



(b) Graduation



(c) Earnings



Notes: This figure compares the impact of an additional $1,000 of grant aid on two groups of students: middle-income students at the TEXAS Grant threshold, and lower-income students at the Auto-Zero AGI threshold. Each point estimate and 95% confidence interval reports the results of a separate instrumental variables regression, where the first stage is first-year grant aid measured in thousands of dollars. In all figures, the x axis is the number of years since the student entered college. The outcomes in Panels (a), (b), and (c), respectively, are graduation, persistence, and earnings at the given time horizon. The results for the TEXAS Grant are estimated on the 2007-2017 entering cohorts, and the results for the Auto-Zero cutoff are estimated on the 2008-2012 entering cohorts.

students: those who are likely to graduate, and those who would receive substantial amounts of grant-based financial aid even if not awarded TEXAS Grants.

Measurement. To characterize how grants are allocated at the TEXAS Grant cutoff, I analyze the mean untreated potential outcomes of compliers at the threshold. Abadie (2002) shows that this mean is identified by:

$$\frac{\lim_{k\uparrow k^*}\mathbb{E}[Y(1-D)\mid k]-\lim_{k\downarrow k^*}\mathbb{E}[Y(1-D)\mid k]}{\lim_{k\uparrow k^*}\mathbb{E}[(1-D)\mid k]-\lim_{k\downarrow k^*}\mathbb{E}[(1-D)\mid k]}=\mathbb{E}[Y_{1,0}\mid D_1>D_0,k=k^*] \tag{5}$$

For example, if Y denotes graduation, then (5) identifies the mean graduation rate that TEXAS Grant compliers at the threshold would have achieved if they had *not* been awarded TEXAS Grants.

I compare this mean to the mean outcomes of "never-takers," who are not awarded TEXAS Grants despite being on the barely-eligible side of the threshold:[39]

$$\lim_{k\uparrow k^*}\mathbb{E}[Y\mid D=0,k]=\mathbb{E}[Y_{1,0}\mid D_0=D_1=0,k=k^*] \tag{6}$$

Together, (5) and (6) measure how students who are awarded grants as a result of crossing the threshold (compliers) compare to students who are not awarded grants despite being barely eligible (never-takers).

Factors influencing grant allocations at the threshold. Two mechanisms may contribute to observed differences between compliers and never-takers. First, schools have discretion in allocating TEXAS Grants, meaning that the observed pattern of grant awards at the threshold is influenced by schools' preferences in deciding grant funding. In particular, because schools are required to fully cover tuition and fees for TEXAS Grant recipients, they may face an incentive to allocate the grant to students with high levels of aid at baseline. Second, the process of applying for a TEXAS Grant may advantage high-achieving students. In particular, schools are instructed to give priority to students who submit their FAFSA forms by a "priority deadline."[40] If students who apply by the state's deadline have higher academic ability on average, then this system of grant allocation may effectively target grants to students with higher *ex-ante* levels of achievement.

[39] In addition to compliers and never-takers, 12 percent of students at the cutoff are "always-takers" who receive grants despite being on the ineligible side of the threshold. Because always-takers are never observed in the untreated state, it is not possible to identify their mean untreated potential outcomes.

[40] During my sample period, the priority deadline is January 15 of the academic year prior to a student's year of college entry.

6.1 Targeting on Ability

Figures 7a and 7b report estimates of Equations (5) and (6), examining how schools target TEXAS Grants at the threshold by students' *ex-ante* ability. The results indicate that schools target TEXAS Grants at the threshold to high-ability students. Figure 7a shows results for BA completion, showing that compliers would complete BA degrees at far higher rates than never-takers, even if they had not been awarded TEXAS Grants. For example, six years after entering college, the difference in mean untreated graduation rates between these groups is 12 percentage points. Figure 7b shows evidence of a similar pattern in students' post-college earnings. The mean untreated potential earnings of compliers, measured six to eight years after college entry, are several thousand dollars higher than those of never-takers who are not awarded TEXAS Grants. Taken together, the results imply that TEXAS Grants at the threshold are disproportionately targeted to students who would graduate at higher rates and have higher post-college labor-market earnings *even if* they had not received TEXAS Grants, compared to students who are not selected to receive grants.

Figure 7c confirms these patterns, reporting estimates of Equations (5) and (6) where outcomes are replaced with students' tenth grade test scores. Because predetermined characteristics are not affected by TEXAS Grants, it is also possible to examine the characteristics of "always-taker" students who receive grants despite falling on the ineligible side of the threshold.[41] The figure indicates that compliers' test scores are more than three percentiles higher, on average, than the test scores of never-taker students. The test scores of always-takers fall between the complier and never-taker estimates. The results indicate that schools target on preexisting academic ability within the set of students for whom crossing the threshold is required to receive a grant aid award.[42]

Importantly, these targeting patterns favoring high-ability students are the *opposite* of how \$0 EFCs are distributed to eligible students at the Auto-Zero cutoff. Appendix Figure A8 compares the test scores of compliers, never-takers, and always-takers at the Auto-Zero EFC cutoff, where treatment is defined as an indicator for receiving a zero EFC. In sharp contrast to the targeting patterns at the TEXAS Grant threshold, the mean test scores of compliers at the Auto-Zero cutoff are 7 percentage points lower than the mean test scores of never-takers. The results indicate that students assigned zero EFCs at the threshold are less academically prepared for college than

[41] The mean test score for always-takers is identified by $\lim_{k \downarrow k^*} \mathbb{E}[X \mid D = 1, k] = \mathbb{E}[X \mid D_0 = D_1 = 1, k = k^*]$.

[42] One possibility is that these selection patterns are due to merit standards, which since the 2014 academic year have been part of the TEXAS Grant's design. Although only four out of the eleven cohorts in my sample enter college after this policy change, it is in theory possible that the observed disproportionate allocation of grants towards high-achieving students is a reflection of state-administered merit criteria rather than a consequence of schools' preferences in distributing grant aid awards. Appendix Figure A10 provides evidence against this possibility, showing that school's revealed preference for awarding TEXAS Grants to high-achieving students predates the 2014 introduction of merit standards to the TEXAS Grant, and for some outcomes (e.g., five-year graduation rates) appears to favor ex-ante high-achieving students even more strongly.

students receiving nonzero EFCs despite falling on the eligible side of the threshold.[43] Combined with the high impacts on these students' outcomes documented in Section 5, these patterns are consistent with marginal impacts of grant aid being higher for students with lower *ex-ante* ability (Angrist et al. 2022).

6.2 Targeting on Preexisting Aid

Because schools are required to supplement TEXAS Grants with institutional aid to fully cover grant recipients' tuition and fees, they may face an incentive to disproportionately award TEXAS Grants to students with high preexisting aid awards.[44] Figure 7d compares the mean untreated potential aid awards of TEXAS Grant compliers (Equation 5) against the aid awards of never-takers at the grant threshold (Equation 6). The results show that schools disproportionately award TEXAS Grants to students with high preexisting levels of grant-based financial aid. Even if TEXAS Grant compliers at the threshold did not receive TEXAS Grants, they would still enter college with roughly \$820 more in first-year aid than never-taker students. The results indicate that schools disproportionately target marginal TEXAS Grants to *ex-ante* higher-funded students.

7 Nonparametric Bounds on Grant Treatment Effects

So far, the paper has shown that the impact of grant aid on student outcomes differs substantially across the TEXAS Grant and Auto-Zero EFC cutoffs for students in four-year schools. The remaining task is to estimate the impact of grant aid on two-year students at the Auto-Zero cutoff. However, this task is made more difficult by the existence of a significant enrollment effect at the grant assignment threshold, meaning that standard IV methods applied at the threshold do not recover a valid LATE for any population.

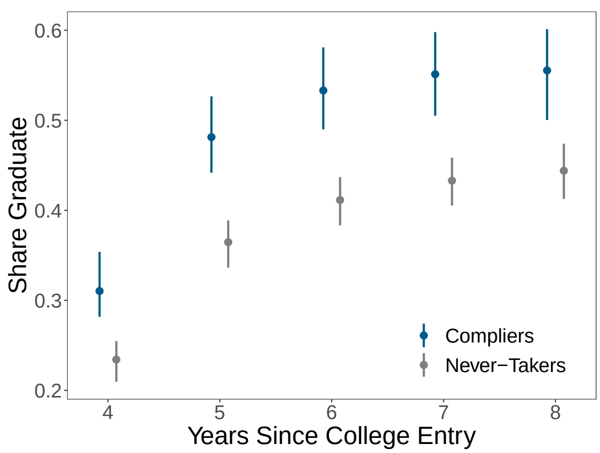
To deal with this challenge, this section develops a bounding approach extending Lee (2009) and Gerard et al. (2020). I extend their framework by showing how an additional mean-dominance assumption motivated by the relative pretreatment characteristics of extensive-margin and intensive-margin compliers yields substantially tighter bounds than the "no-assumptions" bounds derived by Gerard et al. (2020) and considered in prior work on grant impacts (Park and Scott-Clayton 2018).

[43] This pattern is consistent with the non-AGI criteria for receiving a zero EFC. In addition to crossing the Auto-Zero threshold, students' families either must have been eligible to file a 1040A or 1040EZ tax return or have received means-tested benefits in the previous year to receive an Automatic Zero EFC. "Never-taker" families who do not meet these criteria are likely to have higher assets, which may result in higher academic preparedness among their children.

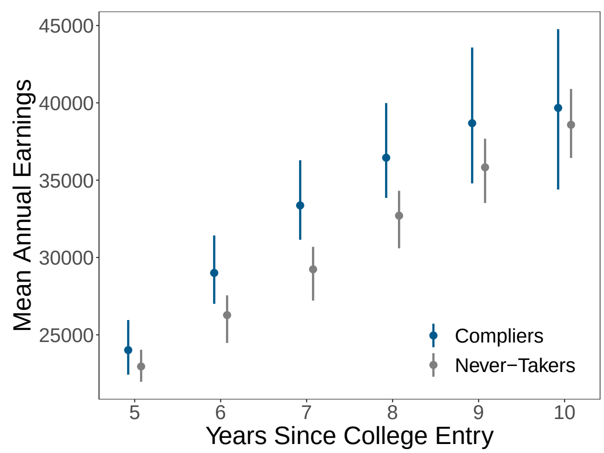
[44] The idea that this incentive shapes schools' allocation decisions has been widely conjectured in descriptions of the TEXAS Grant (Andrews and Stange 2019; Denning et al. 2019), but to my knowledge has not been empirically documented.

Figure 7: Selection at the TEXAS Grant Threshold

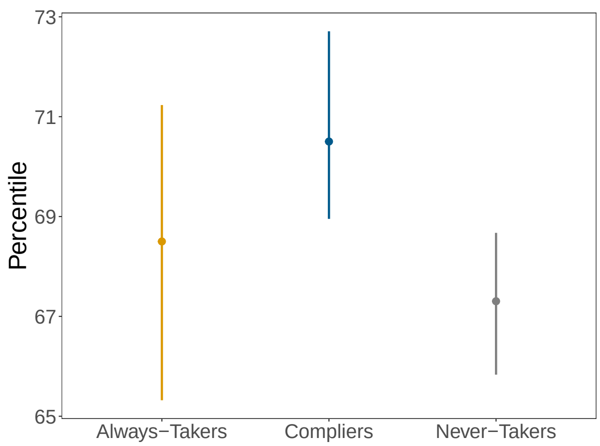
(a) Graduation



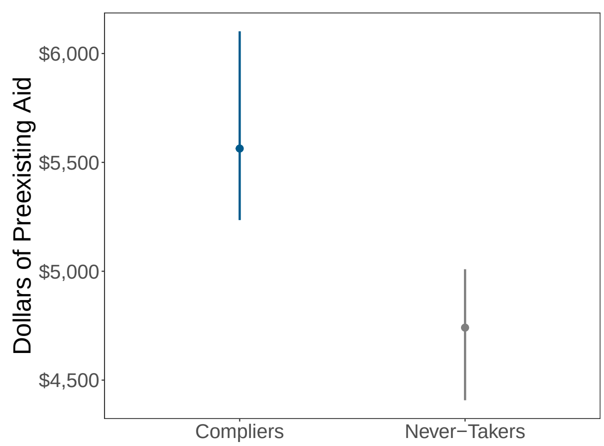
(b) Post-College Earnings



(c) Selection by 10th Grade Test Scores



(d) Selection by Preexisting Grant Aid



Notes: This figure shows estimates of graduation probabilities (Panel (a)) and post-college earnings (Panel (b)) by the complier group of students at the TEXAS Grant cutoff. "Always-takers" are students who receive TEXAS Grants despite being on the barely-ineligible side of the threshold. "Compliers" are students who receive TEXAS Grants as a causal result of crossing the threshold. "Never-takers" are students who do not receive TEXAS Grants despite being on the barely-eligible side of the threshold. Panel (c) shows the mean tenth grade test score percentile of compliers, always-takers, and never-takers at the threshold. Panel (d) shows the mean baseline grant awards of compliers and never-takers at the TEXAS Grant threshold, defined as the amount of grant aid these students would receive if not awarded a TEXAS Grant. Counterfactual grant amounts for always-takers cannot be estimated, because all always-takers at the threshold receive TEXAS Grants. The point estimates and confidence intervals at each time horizon are constructed by separately estimating equations (5) through (6) for each outcome and time horizon; see main text for details. The error bars reflect 95 percent bias-corrected confidence intervals estimated using the method of (Calonico et al. 2014).

7.1 Target Parameters

The first target parameter is the *overall* effect of grant aid for community college students at the Auto-Zero EFC threshold. I denote this quantity by $LATE^* = \mathbb{E}[Y_1 - Y_0 \mid S_1 = 1, D_1 > D_0, k = k^*]$, which expresses the impact of grant aid on the total population of *enrolled compliers*: students who receive grants by crossing the threshold ($D_1 > D_0$), and who enroll in college in the treated state ($S_1 = 1$).

I then decompose this overall effect into its intensive-margin and extensive-margin components. Appendix C.4 shows that $LATE^*$ can be expressed as the sum of grant aid's intensive-margin and extensive-margin effects:

$$LATE^* = \underbrace{(1-\omega)LATE_{IM}}_{\text{Intensive-Margin Effect}} + \underbrace{\omega LATE_{EM}}_{\text{Extensive-Margin Effect}} \tag{7}$$

where the weight $\omega = P(S_1 > S_0 \mid S_1 = 1, D_1 > D_0)$ denotes the proportion of enrolled compliers who are extensive-margin compliers. The terms $(1 - \omega)LATE_{IM}$ and $\omega LATE_{EM}$ express the contributions of the intensive and extensive margins in shaping the overall impact of grant aid on the outcomes of enrolled compliers. These contributions depend on the intensive-margin and extensive-margin treatment effects of aid, given by:

$$\begin{aligned}
LATE_{IM} &\equiv \mathbb{E}[Y_1 - Y_0 \mid \underbrace{S_1 = S_0 = 1, D_1 > D_0}_{\text{IM Complier}}, k = k^*] \\
&= \bar{Y}_1^{\text{IM}} - \bar{Y}_0^{\text{IM}} \\
LATE_{EM} &\equiv \mathbb{E}[Y_1 - Y_0 \mid \underbrace{S_1 > S_0, D_1 > D_0}_{\text{EM Complier}}, k = k^*] \\
&= \bar{Y}_1^{\text{EM}} - \bar{Y}_0^{\text{EM}}
\end{aligned} \tag{8}$$

The remainder of this section lays out how I estimate the overall effect of grant aid at the threshold ($LATE^*$), and develops an approach that allows me to separately bound the intensive-margin and extensive-margin components $(1 - \omega)LATE_{IM}$ and $\omega LATE_{EM}$.

7.2 Identifying the Mass of Extensive-Margin Compliers

The first step in deriving the target parameters is to identify the share of extensive-margin compliers among all enrolled compliers, denoted ω in Equation (7). Appendix D shows that the mass of extensive-margin compliers on the barely-eligible side of the cutoff is identified by the discontinuity in the density of the running variable at the threshold:

$$\pi^{-}_{\text{EM}} = P(D_1 > D_0, S_1 > S_0 \mid S = 1, k = k^{*-}) = \frac{f_{S=1}(k^{*-}) - f_{S=1}(k^{*+})}{f_{S=1}(k^{*-})} \tag{9}$$

where $f_{S=1}(k)$ is the density of the running variable in the selected sample of enrolled students, and where the notation k^{*-} and k^{*+} denotes limits approaching the cutoff from the left and right sides.

It is also possible to identify the *total* mass of enrolled compliers on the barely-eligible side of the threshold. Appendix D shows that this total complier share is recovered by:

$$\begin{aligned} \pi^{-}_{\text{Complier}} &= P(S_1 = 1, D_1 > D_0 \mid S = 1, k = k^{*-}) \\ &= \bar{D}^{-} - \frac{f_{S=1}(k^{*-})}{f_{S=1}(k^{*+})} \bar{D}^{+} \end{aligned} \tag{10}$$

where $\bar{D}^{-} \equiv \lim_{r \uparrow k^*} \mathbb{E}[G \mid S = 1, k = r]$ and $\bar{D}^{+} \equiv \lim_{r \downarrow k^*} \mathbb{E}[G \mid S = 1, k = r]$ denote the proportion of students with zero EFCs on the left and right of the cutoff in the sample of enrolled students. Equation (10) is analogous to the standard Wald first stage, but contains an additional sample-size correction term $(f_{S=1}(k^{*-})/f_{S=1}(k^{*+}))$ which adjusts for the fact that the left side of the cutoff includes additional students (the extensive-margin compliers) who are not present to the right of the cutoff.

Because (9) identifies the mass of extensive margin compliers and (10) identifies the mass of all compliers, the share of extensive-margin compliers among all compliers is identified by Bayes' rule:

$$\begin{aligned} \omega \equiv \frac{\pi^{-}_{\text{EM}}}{\pi^{-}_{\text{Complier}}} &= P(S_1 > S_0 \mid D_1 > D_0, S_1 = 1, k = k^{*-}) \\ &= P(\text{EM Complier} \mid \text{Complier}, k = k^{*-}) \end{aligned} \tag{11}$$

The share ω measures the share of extensive-margin compliers among the overall enrolled complier population on the barely-eligible side of the cutoff.

Table 2 reports estimates of complier shares obtained by estimating Equations (9) through (11). The first row shows the estimate of π^{-}_{EM}, the overall mass of extensive-margin compliers on the barely-eligible side of cutoff. The point estimate indicates that 5.2 percent of the population on the barely-eligible side of the cutoff consists of extensive-margin compliers. The second row reports estimates from the modified first-stage regression in (10), estimating that intensive and extensive margin compliers make up 39.8 percent of the population on the barely-eligible side of the cutoff. Dividing these two numbers yields the estimate that 13.1% of the complier population consists of extensive-margin compliers.

Table 2: Estimates of Complier Shares

Quantity	Symbol	Derivation	Estimate
EM Compliers on Eligible Side $P(D_1 > D_0, S_1 > S_0 \mid S_1 = 1, k = k^{*-})$	π^-_{EM}	Eqn. (9)	0.052 (0.028)
Compliers on Eligible Side $P(D_1 > D_0 \mid S_1 = 1, k = k^{*-})$	π^-_{Complier}	Eqn. (10)	0.398 (0.019)
EM Share of Compliers on Eligible Side $P(S_1 > S_0 \mid D_1 > D_0, S_1 = 1, k = k^{*-})$	ω	$\left(\pi^-_{\text{EM}}/\pi^-_{\text{Complier}}\right)$	0.131 (0.066)

Notes: This table reports estimates of complier shares for the population of students on the eligible side of the Automatic Zero EFC threshold in two-year community colleges. The first row reports the share of all students on the barely-eligible side of the cutoff who are extensive-margin compliers. The second row reports the share of all students on the barely-eligible side of the cutoff who are compliers (either extensive-margin or intensive-margin). The third row takes the ratio of these two terms to report the share of all compliers who are extensive-margin compliers. Bootstrap standard errors are in parentheses, where the optimal bandwidth for density estimation is chosen separately within each of 500 bootstrap replications.

7.3 Step 2: Identifying Potential Outcome Means

The second step is to identify the potential outcome means which make up $LATE^*$, $LATE_{IM}$, and $LATE_{EM}$. Note first that by plugging (8) into (7), it is possible to express $LATE^*$ as:

$$LATE^* = \bar{Y}_1^{\text{Complier}} - (1-\omega)\bar{Y}_0^{\text{IM}} - \omega\bar{Y}_0^{\text{EM}} \tag{12}$$

where $\bar{Y}_1^{\text{Complier}}$ represents the mean potential outcome in the treated state among *all* enrolled compliers, including both intensive-margin and extensive-margin compliers:

$$\bar{Y}_1^{\text{Complier}} = (1-\omega)\bar{Y}_1^{\text{IM}} + \omega\bar{Y}_1^{\text{EM}} \tag{13}$$

and where $\omega = P(S_1 > S_0 \mid S_1 = 1, D_1 > D_0, k = k^{*-})$ is the extensive-margin share among compliers, identified by (11). Equation (12) shows that to identify $LATE^*$, it is sufficient to identify $\bar{Y}_0^{\text{IM}}$, $\bar{Y}_1^{\text{Complier}}$, and $\bar{Y}_0^{\text{EM}}$. I address each potential outcome in turn.

Identifying $\bar{Y}_0^{\text{IM}}$ and $\bar{Y}_1^{\text{Complier}}$

Despite the missing-data challenge, $\bar{Y}_0^{\text{IM}}$ — the mean untreated potential outcome of intensive-margin compliers in their no-grant counterfactual — is still nonparametrically point identified.

Appendix D.2 shows that this counterfactual mean potential outcome is given by:

$$\bar{Y}_0^{\mathrm{IM}} = \mathbb{E}[Y_0 \mid S_1 = S_0 = 1, D_1 > D_0, k = k^*] = \frac{\bar{Y}_0^+ - \bar{Y}_0^- \kappa_0}{1 - \kappa_0} \tag{14}$$

where $\bar{Y}_0^+ = \lim_{k \downarrow k^*} \mathbb{E}[Y \mid S = 1, D = 0, k]$ and $\bar{Y}_0^- = \lim_{k \uparrow k^*} \mathbb{E}[Y \mid S = 1, D = 0, k]$ are limits of the mean outcomes of untreated students approaching the cutoff from the right and left, and where $\kappa_0 = \frac{f_{S=1}(k^{*-})}{f_{S=1}(k^{*+})} \frac{(1-\bar{D}^-)}{(1-\bar{D}^+)}$ is a modified first stage ratio that corrects for the fact that there is a greater mass of students on the eligible side of the cutoff. The adjustment term κ_0 adapts the complier-describing logic of Abadie (2002) to a case where treatment leads some individuals to enter the sample. The intuition is the same: as intensive-margin compliers become treated by crossing the threshold, the change in outcomes among *un*treated units identifies the intensive-margin compliers' counterfactual potential outcomes in the untreated state.[45] It is straightforward to verify that if there are no enrollment effects, then $f_{S=1}(k^{*-}) = f_{S=1}(k^{*+})$ and (14) collapses to a standard Abadie (2002)-style Wald estimand where the numerator is a treatment-by-outcome interaction.

Appendix D.2 shows that a similar argument identifies the pooled mean potential outcomes for treated intensive-margin and extensive-margin compliers:

$$\bar{Y}_1^{\mathrm{Complier}} = \mathbb{E}[Y_1 \mid S_1 = 1, D_1 > D_0, k = k^*] = \frac{\bar{Y}_1^- - \bar{Y}_1^+ \kappa_1}{1 - \kappa_1} \tag{15}$$

where $\bar{Y}_1^-$ and $\bar{Y}_1^+$ are limits of the mean outcomes of treated students approaching the cutoff from the left and right, and where $\kappa_1 = \frac{f_{S=1}(k^{*+})}{f_{S=1}(k^{*-})} \frac{\bar{D}^+}{\bar{D}^-}$.

Identifying $\bar{Y}_0^{\mathrm{EM}}$

The final ingredient required to identify $LATE^*$ is $\bar{Y}_0^{\mathrm{EM}}$, the mean untreated potential outcome of extensive-margin compliers on the barely-ineligible side of the cutoff. Because I only observe the running varaible for enrolled students, and because extensive-margin compliers only enroll in college if they are treated, I cannot precisely estimate $\bar{Y}_0^{\mathrm{EM}}$ due to the missing-data problem.

I address this problem by constructing a noisy measure of $\bar{Y}_0^{\mathrm{EM}}$. Although my data do not contain the running variable for students who do not attend college, the data contain outcomes, including eventual B.A. completion and earnings, for all high school graduates. I therefore estimate

[45] Equation (14) is a special case of a more general result proven in Gerard et al. (2020). Their paper derives bounds on treatment effects of "non-manipulated" units in RD designs when there are also "manipulated" units on one side of the cutoff. In this paper, the "non-manipulated" units correspond to intensive-margin compliers and the "manipulated" units correspond to extensive-margin compliers. In contrast to their paper, which assumes that manipulated units can be either treated or untreated, the exclusion restriction in Assumption 2 implies that all extensive-margin compliers are treated, entering the 2-year sample if and only if they receive a zero EFC as a result of crossing the grant assignment threshold. This requirement is key to the point-identification results in this subsection.

$\bar{Y}_0^{\text{EM}}$ by taking means of outcomes over the population of high school graduates who do not enroll in college within two years of graduating from high school.[46]

Combining this proxy measure of $\bar{Y}_0^{\text{EM}}$ with the point-identified values of $\bar{Y}_1^{\text{Complier}}$ and $\bar{Y}_0^{\text{IM}}$ from Equations (14) and (15) yields estimates of $LATE^*$ by Equation (12). In the next subsection, I describe the bounding approach that allows me to decompose this overall treatment effect into its intensive-margin and extensive-margin components.

7.4 Step 3: Bounds on Intensive-Margin and Extensive-Margin Effects

The remaining task is to decompose $LATE^*$ into its component intensive-margin and extensive-margin effects, $(1-\omega)LATE_{IM}$ and $\omega LATE_{EM}$. Because the weight ω and the counterfactual mean potential outcomes $\bar{Y}_0^{\text{IM}}$ and $\bar{Y}_0^{\text{EM}}$ are identified, the last remaining ingredients required to identify $(1-\omega)LATE_{IM}$ and $\omega LATE_{EM}$ are the mean treated potential outcomes $\bar{Y}_1^{\text{IM}}$ and $\bar{Y}_1^{\text{EM}}$.

Note that $\bar{Y}_1^{\text{Complier}}$ identifies a weighted average of $\bar{Y}_1^{\text{IM}}$ and $\bar{Y}_1^{\text{EM}}$ by (13). This expression is at the heart of the identification challenge. Comparisons across the grant threshold are able to identify $\bar{Y}_1^{\text{Complier}}$, but not its component parts. Despite this identification challenge, it is possible to obtain meaningful bounds on $\bar{Y}_1^{\text{IM}}$ and $\bar{Y}_1^{\text{EM}}$, and therefore on the intensive-margin and extensive-margin effects $(1-\omega)LATE_{IM}$ and $\omega LATE_{EM}$, given our knowledge of the complier shares.

7.4.1 Worst-Case Bounds

The starting point for bounding $\bar{Y}_1^{\text{IM}}$ and $\bar{Y}_1^{\text{EM}}$ is to consider "worst-case" bounds (Gerard et al. 2020; Lee 2009). These bounds are built from two identified quantities: the number of extensive margin compliers ω, and the *distribution* of outcomes among the pooled set of all enrolled compliers at the threshold.[47] The key intuition, first developed by Lee (2009), is that no sample of size $(1-\omega)$ drawn from the complier population can have a lower mean treated outcome than the lowest $(1-\omega)$ fraction of this distribution. Hence, the mean outcome among this lowest fraction is a "worst-case" bound for $\bar{Y}_1^{\text{IM}}$. A similar argument obtains an upper bound for $\bar{Y}_1^{\text{IM}}$ by assuming that intensive-margin compliers have the highest $(1-\omega)$ outcomes in the distibution of complier outcomes.

[46] Because students at the margin of receiving an Automatic Zero EFC are poorer than the overall population of non-college-attending high school graduates, this proxy measure of $\bar{Y}_0^{\text{EM}}$ is likely to be higher than the true counterfactual mean outcomes of extensive-margin compliers at the cutoff. This, in turn, leads to estimates of $LATE^*$ that are likely smaller than estimates based on the true value of $\bar{Y}_0^{\text{EM}}$. However, because $\bar{Y}_0^{\text{EM}}$ is weighted by the extensive-margin complier share ω in the calculation of $LATE^*$ in (12), the overall bias in $LATE^*$ stemming from bias in $\bar{Y}_0^{\text{EM}}$ is likely to be small.

[47] Gerard et al. (2020) show that this distribution is identified by replacing the outcome in Equation (15) with $\mathbf{1}\{Y \leq y\}$, where y is a value in the support of the distribution of Y:

$$\begin{aligned}\frac{\lim_{k\uparrow k^*}\mathbb{E}[Y \mid G=1,k]-\kappa_1\lim_{k\downarrow k^*}\mathbb{E}[Y \mid G=1,k]}{1-\kappa_1} &= \mathbb{E}[\mathbf{1}\{Y_{1,1}\leq y\} \mid S_1=1, D_1>D_0, k=k^*] \\ &= H(y \mid S_1=1, D_1>D_0, k=k^*)\end{aligned}$$

where $H(y \mid S_1=1, D_1>D_0, k=k^*)$ is the CDF of Y among units with $(S_1=1, D_1>D_0, k=k^*)$, evaluated at y.

To formalize this intuition, let $H_{S_1=1,D_1>D_0,k=k^*}(y)$ denote the distribution of outcomes among enrolled compliers at the threshold. Let y_ω and $y_{(1-\omega)}$ denote the ω-quantile and $(1-\omega)$-quantile of $H_{S_1=1,D_1>D_0,k=k^*}(y)$. Then $\bar{Y}_1^{\text{IM}}$ is bounded by:

$$\mathbb{E}[Y_1 \mid S_1 = 1, D_1 > D_0, Y_{1,1} \leq y_{1-\omega}] \leq \bar{Y}_1^{\text{IM}} \leq \mathbb{E}[Y_{1,1} \mid S_1 = 1, D_1 > D_0, Y_{1,1} \geq y_\omega] \tag{16}$$

Of course, each bound on $\bar{Y}_1^{\text{IM}}$ also implies a bound on $\bar{Y}_1^{\text{EM}}$ because the weighted average of these two terms must add up to the overall complier mean $\left(\bar{Y}_1^{\text{Complier}}\right)$ by Equation (13). Intuitively, if the intensive-margin compliers have the lowest (highest) $(1-\omega)$ outcomes among all compliers, then the extensive-margin compliers must have the remaining highest (lowest) ω share of outcomes. The bounds in (16) therefore yield the following no-assumptions bounds on $\bar{Y}_{1,1}^{\text{EM}}$:

$$\mathbb{E}[Y_1 \mid S_1 = 1, D_1 > D_0, Y_{1,1} \leq y_\omega] \leq \bar{Y}_1^{\text{EM}} \leq \mathbb{E}[Y_1 \mid S_1 = 1, D_1 > D_0, Y_{1,1} \geq y_{(1-\omega)}]$$

7.4.2 Mean Dominance

We can tighten these bounds by adding assumptions on the relationship between potential outcomes and students' complier types. The first assumption is that mean treated outcomes among intensive-margin compliers are weakly larger than mean treated outcomes among extensive-margin compliers:

Assumption 5 (Mean Dominance)**.** $\bar{Y}_1^{IM} \geq \bar{Y}_1^{EM}$*.*

Assumption 5 states that intensive-margin compliers, who would have attended college even without having received a zero EFC, perform at least as well as extensive-margin compliers brought into college attendance by receiving a zero EFC.[48] Such a restriction is consistent with the notion that students sort into college options based on their comparative advantage (Kirkeboen et al. 2016).

Assumption 5 tightens bounds on treatment effects by raising the lower bound on $\bar{Y}_1^{\text{IM}}$ and lowering the upper bound on $\bar{Y}_1^{\text{EM}}$. Specifically, because Y_1^{IM} and $\bar{Y}_1^{\text{EM}}$ must have $\bar{Y}_1^{\text{Complier}}$ as their weighted average, the constraint that $\bar{Y}_1^{\text{IM}} \geq \bar{Y}_1^{\text{EM}}$ implies that the lower bound on $\bar{Y}_1^{\text{IM}}$

[48] This assumption is a related to the Monotone Treatment Selection (MTS) restriction first introduced by Manski and Pepper (2000), but is not identical. Standard applications of MTS make assumptions about the potential outcomes of units who take up treatment versus the potential outcomes of units who do not. For example, considering the "treatment" of attending a community college, a standard MTS assumption in my setting might be:

$$\mathbb{E}[Y_1 \mid S = 1] \geq \mathbb{E}[Y_1 \mid S = 0]$$

which would state that the potential outcomes of students who select into two-year schooling are different from the potential outcomes of students who do not select into two-year schooling. Assumption (5) is slightly different from such a condition because the populations on both sides of the equation $Y_1^{\text{IM}} \geq Y_1^{\text{EM}}$ enroll in two-year schooling if they receive a zero EFC ($S_1 = 1$). Rather than restricting expected values of treated outcomes $Y_{1,1}$ based on the observed college choice under grant assignment (S_1), Assumption (5) instead restricts potential outcomes based on the *counterfactual* college choice S_0 that students would make if they did not receive grants.

and the upper bound on $\bar{Y}_1^{\text{EM}}$ both become $\bar{Y}_1^{\text{Complier}}$, the pooled treated mean potential outcome among all compliers.[49]

7.4.3 Motivating the Mean Dominance Assumption with Identified Characteristics

Importantly, we can motivate the mean dominance assumption by comparing the pretreatment characteristics of extensive-margin and intensive-margin compliers. Even though the mean treated potential outcomes $\bar{Y}_1^{\text{IM}}$ and $\bar{Y}_1^{\text{EM}}$ are not separately point identified, the pretreatment characteristics of students of both complier types are separately identified. Replacing Y in Equation (13) with a predetermined characteristic X results in:

$$\bar{X}^{\text{Complier}} = (1-\omega)\bar{X}^{\text{IM}} + \omega\bar{X}^{\text{EM}} \tag{17}$$

where the loss of the d subscript reflects the fact that predetermined characteristics do not depend on treatment. Because $\bar{X}^{\text{Complier}}$ is identified by (15) and $\bar{X}^{\text{IM}}$ is identified by (14), it is possible to solve (17) for $\bar{X}^{\text{EM}}$, obtaining the mean covariate values of extensive-margin compliers.

Appendix Figure A9 shows estimates of mean tenth-grade test scores for intensive-margin and extensive-margin compliers at the Auto-Zero EFC Cutoff among students enrolled in two-year colleges. The point estimates indicate that extensive-margin compliers are heavily negatively selected, with high school test scores more than 20 percentiles lower than those of intensive-margin compliers. However, the estimates are imprecise, owing to the fact that extensive-margin compliers represent a small fraction of the complier population.[50] The results indicate that extensive-margin compliers enter college with far lower average levels of ability. As a result, they lend credence to the mean-dominance assumption, which assumes that these students have weakly lower average graduation probabilities and post-college earnings than intensive-margin compliers.

[49] To see this, note that because $\bar{Y}_1^{\text{IM}}$ and $\bar{Y}_1^{\text{EM}}$ must average to $\bar{Y}_1^{\text{Complier}}$ by (13), it cannot be the case that $Y_1^{\text{IM}} < \bar{Y}_1^{\text{Complier}}$ and $\bar{Y}_1^{\text{IM}} > \bar{Y}_1^{\text{EM}}$, because the average of a number (Y_1^{IM}) with a smaller number (Y_1^{EM}) cannot produce a larger number ($\bar{Y}_1^{\text{Complier}}$).

[50] This imprecision arises because $\bar{X}^{\text{EM}}$ is backed out of (17) by:

$$\bar{X}^{\text{EM}} = \frac{\bar{X}^{\text{Complier}} - (1-\omega)\bar{X}^{\text{IM}}}{\omega}$$

In some bootstrap replications, the estimate of ω is small, leading to the conclusion that the (large) overall decline in test scores is caused by the entry of a very small proportion of the population. This fact leads to standard errors for $\bar{X}^{\text{EM}}$ with a lower bound below zero, which is lower than permissible for a variable defined as a 0-100 percentile. The upper end of the 90 percent confidence interval for the test scores of EM compliers includes the level for IM compliers; nevertheless, the results suggest that extensive-margin compliers are negatively selected and provide motivation for the mean-dominance assumption introduced in Section 7.4.

7.4.4 From Bounds on Treated Outcomes to Bounds on LATEs

Finally, I convert bounds on $\bar{Y}_1^{\text{IM}}$ and $\bar{Y}_1^{\text{EM}}$ into bounds on $LATE_{IM}$ and $LATE_{EM}$ by subtracting the counterfactual means, $\bar{Y}_0^{\text{IM}}$ (for intensive-margin compliers) and $\bar{Y}_0^{\text{EM}}$ (for extensive-margin compliers). Because the extensive-margin complier share ω is point-identified, the resulting bounds on $LATE_{IM}$ and $LATE_{EM}$ immediately yield bounds on the intensive-margin and extensive-margin effects $(1-\omega)LATE_{IM}$ and $\omega LATE_{EM}$.

7.5 Implementation

Estimating the bounds for discrete outcomes requires six ingredients: estimates of the density to the left and right side of the cutoff $(f_{S=1}(k^{*-}))$ and $(f_{S=1}(k^{*+}))$, as well as limits of the proportion of treated students approaching the cutoff from the left and right $(\bar{D}^-, \bar{D}^+)$ and of four limits of treatment-by-outcome interactions $(\bar{Y}_1^+, \bar{Y}_1^-, \bar{Y}_0^+, \bar{Y}_0^-)$. I estimate the densities using the CDF-based estimator of Cattaneo et al. (2018) and estimate limiting quantities of outcomes at the cutoff using local-linear regressions with optimal bandwidth set using the method of Calonico et al. (2014).

8 Results: Nonparametric Bounds on Grant Treatment Effects

8.1 Overall Effects of Grant Aid

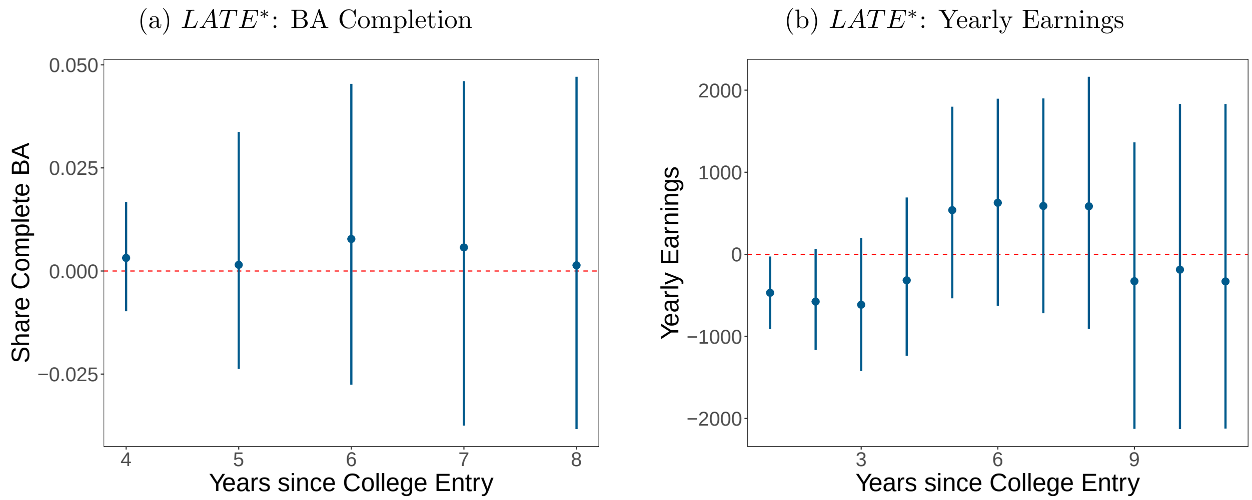
Figure 8 plots estimates of $LATE^*$, the overall effect of receiving a zero EFC on the academic outcomes and earnings trajectories of community college students at the Auto-Zero EFC cutoff. Appendix Tables B6 and B7 report the corresponding point estimates, together with estimates of the extensive-margin complier share ω and component potential outcomes $\bar{Y}_1^{\text{Complier}}$, $\bar{Y}_0^{\text{IM}}$, and $\bar{Y}_0^{\text{EM}}$ that comprise $LATE^*$ by equation (12).

Figure 8a shows results for BA completion. The results indicate that the overall impact of grant aid on B.A. completion among intensive-margin compliers and extensive-margin compliers is economically small and statistically indistinguishable from zero. Importantly, this effect arises despite the fact that extensive-margin compliers, who are brought into community college attendance as a result of being assigned a zero EFC, make up 13.1% of the complier population. Appendix Table B6 shows that the estimated counterfactual B.A. completion rate for extensive-margin compliers is very small,[51] meaning that the overall treatment effects for all compliers are small despite the fact that the extensive-margin effect $\omega LATE_{EM}$ is bounded below almost at zero.

Figure 8b examines overall impacts on yearly earnings. The point estimates indicate that zero-EFC recipients' earnings fall by roughly \$470 in the year of college entry, with point estimates remaining negative through the fourth year after college enrollment. Estimated impacts on

[51] for example, the estimate of $\bar{Y}_0^{\text{EM}}$ for six-year B.A. completion is 0.007.

Figure 8: Overall Effects of Aid at the Auto-Zero Threshold in Two-Year Community Colleges



Notes: This figure shows estimates of the overall local average treatment effect of receiving a Zero EFC on BA Completion (Panel (a)) and yearly earnings (Panel (b)) for students at the Auto-Zero cutoff in two-year community colleges. The figure reports estimates of the overall $LATE^*$ defined in Equation (12), representing the pooled effect on both intensive-margin and extensive-margin compliers. See main text for details. Appendix Tables B6 and B7 report point estimates and confidence intervals for the reported overall $LATE^*$ estimates and their component parts.

earnings are positive five to seven years after entry, but are not statistically significant, and are smaller in magnitude than estimated impacts for low-income four-year students reported in Figure 6. Taken together, the results suggest that receiving an Auto-Zero EFC lowers average earnings in the short term among the pooled set of intensive-margin and extensive-margin compliers, but does not produce significant longer-run earnings gains.

Importantly, these null overall effects could reflect offsetting effects for intensive-margin and extensive-margin compliers. Take earnings as an example. Because extensive-margin compliers are induced by aid to enroll in college, earnings impacts on this group may be negative as they forego full-time work in the labor market. In contrast, if intensive-margin compliers exit community colleges more quickly as the result of aid, the impacts of this group's medium-run earnings may be positive as they re-enter the labor market. The next two sections investigate this possibility, presenting bounds on grant aid's intensive-margin and extensive-margin impacts.

8.2 Bounds on Treatment Effects for Intensive-Margin and Extensive-Margin Compliers

Impacts on B.A. Completion. Figures 9a and 9b report bounds on the treated potential outcomes of intensive-margin and extensive-margin compliers, $\bar{Y}_1^{\mathrm{IM}}$ and $\bar{Y}_1^{\mathrm{EM}}$. The outcome is an indicator for whether a student completes a BA degree within 6 years of initial college entry. Figure 9a shows no-assumptions bounds. The black downward-sloping line plots the linear relationship between $\bar{Y}_1^{\text{IM}}$ and $\bar{Y}_1^{\text{EM}}$ specified by Equation (13), showing all possible combinations of these two unknown quantities that are consistent with the share of extensive-margin compliers ω and the pooled treated potential outcome $\bar{Y}_1^{\text{Complier}}$.[52] The extrema of the line, and their projections onto the x and y axes, correspond to the worst-case Lee (2009) bounds described in Equation (16). For example, the lower-right point of the line describes the case where no intensive-margin compliers graduate, which by (13) implies a 45 percent graduation rate among extensive-margin compliers. The upper-left point describes the opposite scenario in which no extensive-margin compliers graduate, corresponding to a 7 percent graduation rate among intensive-margin compliers. Importantly, the bounds contain the counterfactual mean potential outcome for intensive-margin compliers, shown by the blue horizontal dotted line. As a result, it is impossible to rule out without further assumptions whether treatment effects for intensive-margin compliers are positive or negative.

Figure 9b adds the Mean-Dominance assumption, imposing the condition that intensive-margin compliers fare weakly better than extensive-margin compliers in terms of their graduation rates ($\bar{Y}_1^{\text{IM}} \geq \bar{Y}_1^{\text{EM}}$). The assumption is represented by the 45-degree line, defined by ($\bar{Y}_1^{\text{IM}} = \bar{Y}_1^{\text{EM}}$). The remaining permissible combinations of $(\bar{Y}_1^{\text{EM}}, \bar{Y}_1^{\text{IM}})$ lie above the 45-degree line's intersection with the black line. The assumption considerably narrows the bounds for both groups. The lower bound for intensive-margin compliers and the upper bound for extensive-margin compliers becomes 6 percent, which is the mean treated graduation rate among all compliers $\bar{Y}_1^{\text{Complier}}$. Notably, imposing this assumption places the lower bound for $\bar{Y}_1^{\text{IM}}$ above the counterfactual mean potential outcome for intensive-margin compliers, meaning that the lower bound for $LATE_{IM}$ becomes positive.

Figures 9c and 9d present the resulting bounds on $LATE_{IM}$ and $LATE_{EM}$ under the mean-dominance assumption, obtained by subtracting the point-identified counterfactual means $\bar{Y}_0^{\text{IM}}$ and $\bar{Y}_0^{\text{EM}}$ from the upper and lower bounds shown in Panel (b). Figure 9c indicates that receiving a Zero EFC has limited effects on the graduation probabilities of intensive-margin compliers. The bounded ranges for $LATE_{IM}$ are positive at the four year and six year horizons, but are economically small; at the six-year horizon, the estimates suggest that recieiving a zero EFC improves intensive-margin compliers' rates of 6-year B.A. completion by no more than 1 percentage point. The bounds for extensive-margin compliers, shown in Figure 9d, are large and include zero at every time horizon. However, note that the linear relationship between $\bar{Y}_1^{\text{IM}}$ and $\bar{Y}_1^{\text{EM}}$ in (13) implies a relationship between the intensive-margin and extensive-margin bounds. If the intensive-margin compliers are at the lower bound for $LATE_{IM}$, then Equation (13) implies that extensive-margin compliers must be

[52] Specifically, the equation for the black downward-sloping line is obtained by rearranging (13):

$$\bar{Y}_1^{\text{IM}} = \frac{\bar{Y}_1^{\text{Complier}}}{1-\omega} - \frac{\omega}{1-\omega}\bar{Y}_1^{\text{EM}}$$

The line has slope $(-\omega/(1-\omega))$ and y-intercept $\bar{Y}_1^{\text{Complier}}/(1-\omega)$. Any permissible combination of $(\bar{Y}_1^{\text{EM}}, \bar{Y}_1^{\text{IM}})$ must lie somewhere on this line.

at their upper bound for $LATE_{EM}$. Moreover, the vast majority of the bounded range for $LATE_{EM}$ is above zero, implying that the only case in which treatment effects for this population are not positive is one where intensive-margin compliers achieve nearly the upper bound of $LATE_{IM}$. Overall, the results are consistent with null effects on the graduation rates of intensive-margin compliers, together with plausible evidence that being brought into college attendance by receiving an automatic zero EFC improves extensive-margin compliers' graduation outcomes.

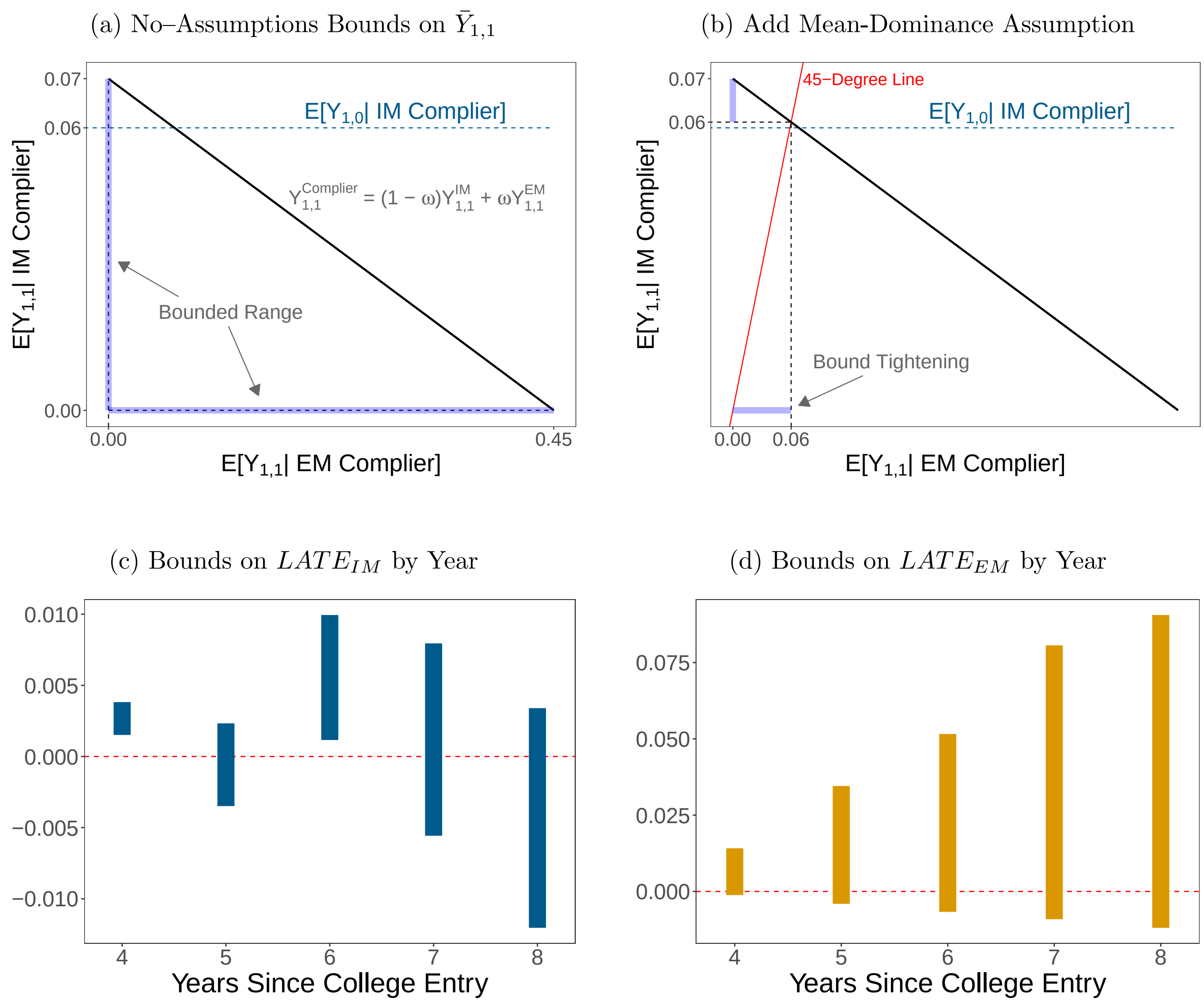
The intuition for these results can be summarized as follows. A naïve reduced-form RD across the cutoff shows that 6-year graduation rates fall slightly across the threshold; Appendix Figure A11 shows that the overall graduation rate at the cutoff exhibits a small and statistically insignificant negative jump, corresponding to an reduced-form point estimate of -0.01. However, this negative jump combines two forces: treatment effects of the grant, and compositional changes due to the entry of extensive-margin compliers on the left of the cutoff. These forces may act in opposite directions, e.g. if the grant improves B.A. completion rates but new students entering the sample on the eligible side of the cutoff are less likely to graduate. If one imposes this belief upon the data, as in Figure 9b, then one must also believe that the treatment of a zero EFC causes small and statistically insignificant increases in BA completion rates among intensive margin compliers, ruling out that a zero EFC harms intensive-margin students' outcomes. Such a result is obfuscated by the no assumptions bounds in Figure 9a, which do not make any assumptions about the relative potential outcomes of different complier groups and thus cannot rule out that a Zero EFC is so harmful to intensive-margin compliers that it reduces their rates of B.A. completion to zero.

8.3 Bounds on the Intensive-Margin and Extensive-Margin Effects of Aid

The previous section reported bounds on $LATE_{IM}$ and $LATE_{EM}$, the effects of receiving a Zero EFC on the outcomes of intensive-margin and extensive-margin compliers. These ingredients allow me to decompose grant aid's overall effect into its intensive-margin and extensive-margin components $(1-\omega)LATE_{IM}$ and $\omega LATE_{EM}$. Figure 10 reports bounds on these components, again using the mean dominance assumption to impose that the mean treated outcomes of intensive-margin compliers are weakly higher than those of extensive-margin compliers.

Figure 10a reports bounds on intensive-margin and extensive-margin earnings effects. The left panel shows bounds on the extensive-margin effect $\omega LATE_{EM}$, and the right panel shows bounds on the intensive-margin effect $(1-\omega)LATE_{IM}$. The figure shows two extreme possibilities for how the intensive-margin and extensive-margin earnings effects combine to form the overall effects of aid. At one extreme, represented by the upper bound for the extensive-margin effect and the lower bound for the intensive-margin effect, neither the extensive-margin impact nor the intensive-margin impact of the Auto-Zero EFC policy is large. At the other extreme, the overall small effect of grant aid on students' earnings trajectories reflects large positive intensive-margin effects, offset by large

Figure 9: Bounds on $LATE_{IM}$ and $LATE_{EM}$: B.A. Completion



Notes: This figure shows how the Mean-Dominance assumption tightens nonparametric bounds on treatment effects. Panels (a) and (b) show bounds on treated potential outcomes for extensive-margin compliers (on the x-axis) and intensive-margin compliers (on the y-axis), where the outcome is an indicator for completing a BA degree within 6 years of college entry. The blue shaded regions show the bounded area for each complier group. The blue dotted line shows the counterfactual mean outcome for intensive-margin compliers, denoted $\bar{Y}_0^{IM}$ in the main text. Panel (a) shows "no-assumptions" (e.g., (Lee 2009)) bounds. Panel (b) shows how the Mean Dominance assumption (Assumption 5) tightens bounds by assuming that intensive-margin compliers have weakly higher outcomes than extensive-margin compliers (represented by the red 45-degree line). Panels (c) and (d) apply the mean-dominance assumption in order to estimate the effects of receiving a Zero EFC on the graduation probability of intensive-margin compliers (Panel (c)) and extensive-margin compliers (Panel (d)). The point estimates corresponding to the bounds shown in the figures are reported in Appendix Table B10.

negative extensive-margin effects. Importantly, the bounds rule out either large positive extensive-margin effects or large negative intensive-margin effects, suggesting that extensive-margin effects do not play a large positive role in determining the overall effects of grant aid on the future earnings of students at this threshold.

Figure 10b reports bounds on intensive-margin and extensive-margin graduation effects. The bounds imply that neither the extensive-margin effect nor the intensive-margin effect on graduation is large. The extensive-margin component is limited because even though the impact of grant aid on these students' graduation rates ($LATE_{EM}$) can be large, these students make up a small share (ω) of the overall complier population, which lowers their contribution ($\omega LATE_{EM}$) in making up the overall treatment effect. In contrast, the intensive-margin component is limited because while intensive-margin compliers make up most of the complier population, they do not see large increases in graduation probability from receiving a Zero EFC.

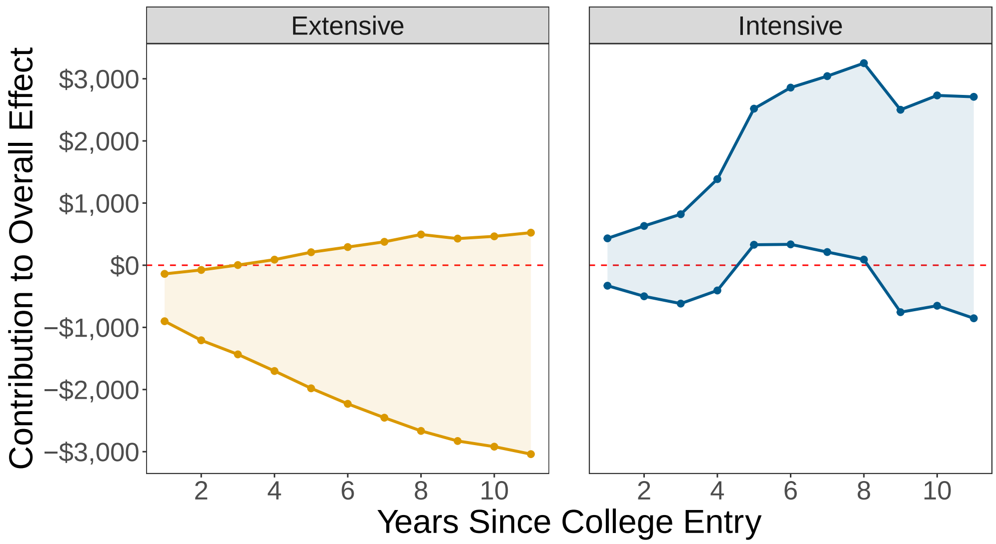
Taken together, the results suggest that the extensive-margin channel does not play a large positive role in shaping the overall impacts of aid on the earnings of community college students on the margin of receiving an Automatic Zero EFC. This limited extensive-margin effect is the result of two factors. First, the extensive-margin share of compliers is small, limiting the contribution of these students to shaping the overall effect of grant aid on students' outcomes. Second, the earnings gains among these students are somewhat limited under the mean-dominance assumption, which imposes that intensive-margin compliers have weakly higher post-college earnings. These results arise despite the fact that receiving a zero EFC may have large impacts on the B.A. completion rates of extensive-margin compliers, as shown in Figure 9d. The results are consistent with earnings losses for extensive-margin compliers despite these B.A. completion effects, as they exit the labor market to attend college.

9 Costs and Benefits of Grant Aid

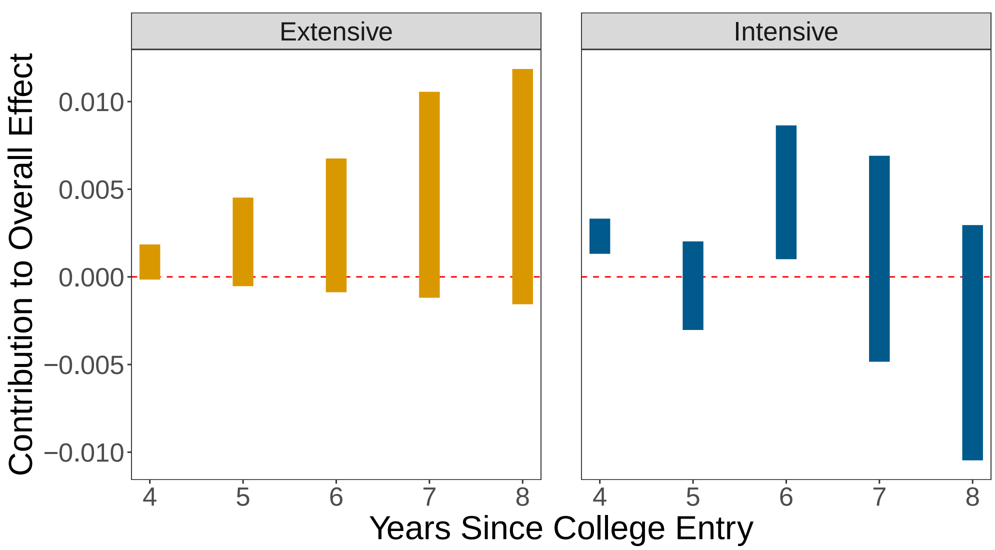
In this section, I unify the analyses of the TEXAS Grant and the Automatic Zero EFC policies by comparing the relative costs and benefits of providing additional grant aid at each threshold.

Figure 10: Extensive- and Intensive-Margin Effects of Aid under Mean Dominance Assumption

(a) Bounds on $\omega LATE_{EM}$ and $(1-\omega)LATE_{IM}$: Earnings



(b) Bounds on $\omega LATE_{EM}$ and $(1-\omega)LATE_{IM}$: BA Completion



Notes: This figure reports bounds on the intensive-margin and extensive-margin effects of grant aid on the outcomes of students at the Auto-Zero cutoff in two-year community colleges. Panel (a) shows bounds for effects on yearly earnings, and Panel (b) shows bounds for effects on BA completion. The left panel of each figure shows results for extensive-margin compliers, and the right panel shows results for intensive-margin compliers. The bounds are constructed under the the mean dominance assumption (Assumption 5); see main text for details. The figure represents two extremes on the jointly bounded outcomes for the two complier groups. One extreme corresponds to the upper bound for extensive-margin compliers and the lower bound for intensive-margin compliers. The other extreme corresponds to the lower bound for extensive-margin compliers and the upper bound for intensive-margin compliers.

9.1 Marginal Benefits and Costs

I model average per-student costs and benefits as a function of discounted yearly grants, loans, earnings, and costs per enrolled student:

$$
\begin{aligned}
B &= \sum_{t=1}^{T} \beta^{t-1} \mathbb{E}[\text{Grants}_t + \text{Loans}_t + (1-\tau)\text{Earnings}_t - \mathbf{1}\{\text{Enrolled}\}_t \times \delta_{\text{tuition}}] \\
C &= \sum_{t=1}^{T} \beta^{t-1} \mathbb{E}[\text{Grants}_t + \text{Loans}_t - \tau\text{Earnings}_t + \mathbf{1}\{\text{Enrolled}\}_t \times \delta_{\text{instruction}}]
\end{aligned}
\tag{18}
$$

where β is the discount factor, τ is the tax rate on earnings, and δ_{tuition} and $\delta_{\text{instruction}}$ denote per-student tuition expenses and instructional costs.[53] Equation (18) states that benefits per enrolled student are the present discounted value of grants, loans, and after-tax earnings, net of tuition costs incurred in every enrolled year. Costs to the government are the discounted sum of grants, loans, and instructional expenditures per enrolled student, net of tax receipts.[54]

Appendix E shows that the marginal benefit of slightly expanding a grant program by raising the grant threshold k^* can be expressed in terms of overall LATEs for enrolled compliers:

$$
\begin{aligned}
\frac{\partial B}{\partial k^*} = \Bigg[& \sum_{t=1}^{T} \beta^{t-1} \left(LATE^*(\text{Grants}_t) + LATE^*(\text{Loans}_t) + (1-\tau) LATE^*(\text{Earnings}_t) \right) \\
& - \underbrace{\omega \times \delta_{\text{tuition}}}_{\substack{\text{First-Year} \\ \text{Enrollment Effect} \\ \text{(EM Compliers)}}} \underbrace{- \delta_{\text{tuition}} \times \sum_{t=2}^{T} LATE^*(\text{Enrolled}_t)}_{\substack{\text{Later-Year Enrollment Effects} \\ \text{(All Compliers)}}} \Bigg] \times \underbrace{P(S_1 = 1, D_1 > D_0 \mid k^*) f(k^*)}_{\text{Mass of Enrolled Compliers}}
\end{aligned}
\tag{19}
$$

where $LATE^*(Y_t) = \mathbb{E}[Y_{1,1} - Y_{S_0,0} \mid S_1 = 1, D_1 > D_0, k = k^*]$ is the overall effect of the grant treatment for outcome Y_t, and where $\omega = P(S_1 > S_0 \mid S_1 = 1, D_1 > D_0, k = k^*)$ is the fraction of enrolled compliers at the threshold who are extensive-margin compliers. Equation (19) shows that the marginal benefits of a grant expansion depend on the grant's overall impacts on students' grants, loans, earnings, and enrollment trajectories. The term $(\omega \times \delta_{\text{tuition}})$ denotes first-year tuition for extensive-margin compliers who enroll in college as a result of receiving a grant. Intensive-margin compliers do not incur this negative benefit, because they enroll at $t = 1$ regardless of whether they are treated. In later years, the negative benefits from tuition incorporate the effect of treatment on college persistence for both extensive-margin and intensive-margin compliers.

[53] I use $\beta = 0.95$ and a constant tax rate of $\tau = 0.33$ to calculate marginal costs and benefits.

[54] I abstract away from transfers between federal, state, and local governments and model the costs to a unitary public sector, which provides public education and provides subsidies to students through grants and loans.

Similarly, the marginal cost of raising the grant threshold is:

$$\begin{aligned}\frac{\partial C}{\partial k^*} = \Bigg[&\sum_{t=1}^{T} \beta^{t-1} \left(LATE^*(\text{Grants}_t) + LATE^*(\text{Loans}_t) - \tau LATE^*(\text{Earnings}_t) \right) \\ &+ \delta_{\text{instruction}} \left(\omega + \sum_{t=2}^{T} LATE^*(\text{Enrolled}_t) \right) \Bigg] \times \underbrace{P(S_1 = 1, D_1 > D_0 \mid k^*) f(k^*)}_{\text{Mass of Enrolled Compliers}}\end{aligned} \tag{20}$$

where again, the instructional costs term incorporates the first-year enrollment response of extensive-margin compliers.

Importantly, the marginal cost and benefit terms do not require separately estimating $LATE_{IM}$ and $LATE_{EM}$. Instead, they depend on $LATE^* = (1-\omega)LATE_{IM} + \omega LATE_{EM}$, which pools the effects on both complier groups. As a result, while the bounds in Section 7 are useful for separately considering the grant's impacts on the outcomes of intensive-margin and extensive-margin compliers, they are not necessary for evaluating the overall costs and benefits of the grant expansion. Instead, the point-identified ingredients $\bar{Y}_1^{\text{Complier}}$, $\bar{Y}_0^{\text{IM}}$, and $\bar{Y}_0^{\text{EM}}$, together with the extensive-margin complier share ω, are sufficient to characterize the $LATE^*$ terms by (12). This result underscores the usefulness of the selection-corrected potential outcomes identified in Section 7. Even if the analyst only observes data on enrolled students, it is possible to estimate the costs and benefits of grant expansions given that a noisy measure of $\bar{Y}_0^{\text{EM}}$, the counterfactual mean outcomes among extensive-margin compliers, is available.

9.2 The MVPF of a Grant Expansion

Taking the ratio of (19) and (20) yields the MVPF (Hendren and Sprung-Keyser 2020) of a marginal expansion of the grant threshold k^*:

$$\begin{aligned}&\text{MVPF}_{k^*} \\ &= \frac{\sum_{t=1}^{T} \beta^{t-1} \left(\Delta_t^*(\text{Grant}) + \Delta_t^*(\text{Loan}) + (1-\tau)\Delta_t^*(\text{Earn}) \right) + \delta_{\text{tuition}} \times \left(1 + \sum_{t=2}^{T} \Delta_t^*(\text{Enroll}) \right)}{\sum_{t=1}^{T} \beta^{t-1} \left(\Delta_t^*(\text{Grant}) + \Delta_t^*(\text{Loan}) - \tau\Delta_t^*(\text{Earn}) \right) + \delta_{\text{instructional}} \times \left(1 + \sum_{t=2}^{T} \Delta_t^*(\text{Enroll}) \right)}\end{aligned} \tag{21}$$

where $\Delta_t^*(Y_t)$ is shorthand for $LATE^*(Y_t)$. Equation (21) measures the value of the grant program net of the fiscal externality from potential increases in government tax revenue owing to the grant's treatment effect on students' lifetime earnings.

9.3 Implementation

For students enrolled in four-year colleges, the results in Section 4 indicate that neither the TEXAS Grant nor the increase in grant aid at the Auto-Zero threshold impacts student enrollment. In this case, there are no extensive-margin compliers, so that $\omega = 0$ and $LATE^* = LATE_{IM}$, simplifying the marginal cost and benefit formulas. I further assume (following the results in Section 5) that the TEXAS Grant has no meaningful impact on students' reenrollment probabilities, meaning the TEXAS Grant's costs and benefits boil down to its intensive-margin effects on grants, earnings, and loans. In contrast, the increase in aid at the Auto-Zero threshold has positive impacts on initial enrollment among students in two-year community colleges. For students at this treatment margin, I calculate yearly treatment effects in terms of the combined $LATE^*$ identified by Equation (12).

9.4 Estimates of Costs and Benefits

Table 3 reports estimates of the overall costs and benefits of grant aid for students at the three program margins: middle-income four-year students at the margin of the TEXAS Grant, and low-income four-year and community college students at the margin of the Automatic Zero EFC cutoff. There are several key takeaways. First, for the TEXAS Grant and the Automatic Zero EFC cutoff in two-year colleges, the benefits of grant aid do not exceed the costs. For the TEXAS Grant, this effect arises because the grant fails to produce meaningful gains in later-life earnings that make up for the cost of providing grant aid. Column (1) shows that TEXAS Grant recipients receive a total discounted sum of roughly \$13,700 in additional grant aid in the years following receiivng a TEXAS Grant, causing a $\$2,000$ decline in earnings and a roughly $\$4,400$ decline in loans. As a result, the grant represents a net transfer of $\$7,700$ to TEXAS Grant compliers. The government saves money on the loans that would have been taken out by compliers, but loses roughly \$1,000 in tax revenues that would have otherwise been collected from the compliers' labor market earnings, corresponding to a net cost of roughly $\$11,000$. Put together, I estimate that the MVPF of a marginal increase in the TEXAS Grant cutoff is 0.7, indicating benefits to compliers that are less than the grant's costs.

Column (3) shows that for the Automatic Zero EFC cutoff in two-year schools, the comparison of costs and benefits is even less favorable. The entry of extensive-margin compliers into the sample lowers benefits and increases costs due to increases in tuition and institutional expenditures. In addition, because the grant increase at the cutoff does not produce overall earnings gains for two-year students, the government does not recoup the costs of providing grant aid to students at the margin. Overall, while the benefits are positive for students at the margin of grant reciept, they are overwhelmed by the cost increases, producing a MVPF of roughly 0.3.

In contrast, Column (2) shows that for the Automatic Zero EFC cutoff in four-year schools, the benefits of providing grant aid are more than double the costs. The main reason for this difference is the grant's positive impact on students' lifetime earnings, which far outweigh increases in students' tuition costs stemming from the grant's impact on college persistence. The goverment recoups some of these earnings increases as tax revenue, lowering the overall costs. While this estimate indicates a positive MVPF from expenditures of public funds on four-year college students at the margin of the Auto-Zero cutoff, it is smaller than the infinite MVPF estimated by Denning et al. (2019), who find that the increase in grant aid to students is likely to be fully recouped by the government within 10 years. The reason for this discrepancy is that my cost-benefit framework includes impacts on persistence, which lowers benefits for students (by increasing tuition) and increases costs to the government (by increasing institutional expenditures).

Overall, the results indicate that budget-neutral reallocations of funding to students at the Auto-Zero cutoff in four-year schools, financed by reductions in funding at the other two treatment margins, are likely to be welfare enhancing. Among four-year college students, such a reallocation would imply aggregate benefits from lowering the TEXAS Grant threshold, targeting grant aid towards lower-income students at the margin of receiving a Zero EFC rather than funding comparatively higher-income students at the margin of receiving a TEXAS Grant. Among the overall population of lower-income students at the margin of receiving an Automatic Zero EFC, the results suggest benefits from reallocating grant aid from the two-year to the four-year sector. The results suggest that despite the fact that while receiving Zero EFC has substantial impacts on two-year college enrollment, the overall gains in earnings and B.A. completion among compliers are not sufficiently large to justify spending public funds on this population instead of on four-year students.

10 Conclusion

The goal of this paper was to compare the costs and benefits of grant aid targeted at different groups of students and college sectors, and to understand the contributions of the intensive and extensive margins in shaping aid's overall effects. To achieve this goal, I developed an economic framework that decomposed the overall impact of grant aid into its separate impacts on the outcomes of "extensive-margin" and "intensive-margin" students. While extensive-margin students are induced by grant aid to enroll in college, intensive-margin students would attend college even without additional aid, but nevertheless may benefit from additional financial support while enrolled.

I applied the framework to administrative data from Texas, where several discontinuities in grant aid award rules create variation in aid targeting three distinct populations of students: middle-income students in four-year colleges, low-income students in four-year colleges, and low-income students in community colleges. I analyzed grant aid's impact at these margins using variation from income-based cutoff rules provided by two programs. The first is the TEXAS Grant, the state

Table 3: Costs and Benefits of Grant Programs

	TEXAS Grant (1)	Auto-Zero (4-Year) (2)	Auto-Zero (2-Year) (3)
Panel A. Benefits			
After-Tax Earnings	-2,088	2,653	-451
Grants	13,733	3,270	2,748
Loans	-4,447	105	81
Tuition and Fees	0	-409	-739
Total Discounted Benefits	7,700	7,779	1,761
Panel B. Costs			
Tax Receipts	1,029	-1,307	222
Grants	13,733	3,270	2,748
Loans	-4,447	105	93
Institutional Expenditures	0	1,642	2,958
Total Discounted Costs	11,070	3,255	6,237
MVPF	0.70	2.39	0.28
BCR	0.30	2.92	1.70

Notes: This table reports estimates of the costs and benefits of the TEXAS Grant and the increase in grant aid associated with crossing the Automatic Zero EFC threshold. Benefits to students are defined as the sum of after-tax earnings, grant receipts, loan awards, and changes in tuition, defined as the change in the probability of reenrollment multiplied by statewide average tuition and fees. Costs are defined as grants, loans, and expenditures net of tax revenues, where changes in expenditures are calculated as enrollment effects multiplied by statewide average institutional costs per full-time-enrolled student. Expenditure is calculated as average statewide four-year institutional expenditures per full-time enrolled student, calculated separately for 2-year and 4-year students using 2008-2012 IPEDS data and weighted by effects on enrollment. Tuition is calculated similarly using average statewide tuition and fees in 2-year and 4-year schools. I use a constant tax rate of 0.33 to calculate after-tax earnings and tax receipts. I use a deadweight loss of $\phi = 0.5$ to calculate the benefit-cost ratio.

of Texas's flagship need-based financial aid program for four-year public university students. The second is a discontinuity in the federal government's formula that influences whether a student receives a zero Expected Family Contribution (EFC). Analyzing the impact of aid at these margins required overcoming a missing-data problem: because the data contain all enrolled students rather than all applicants, discontinuities in outcomes at the eligibility threshold may conflate the causal effects of grants with compositional changes in enrolled students. I developed a bounding approach to overcome this missing-data problem, yielding three main conclusions. First, grant aid targeted at low-income applicants to four-year colleges has large positive impaccts on college persistence, bachelor's degree completion, and future earnings. Second, there is little if any overall effect of additional aid on these outcomes for middle-income students in four-year colleges and low-income students in community colleges. Third, across all three treatment margins studied, the extensive-margin impacts of grant aid on overall outcomes is small.

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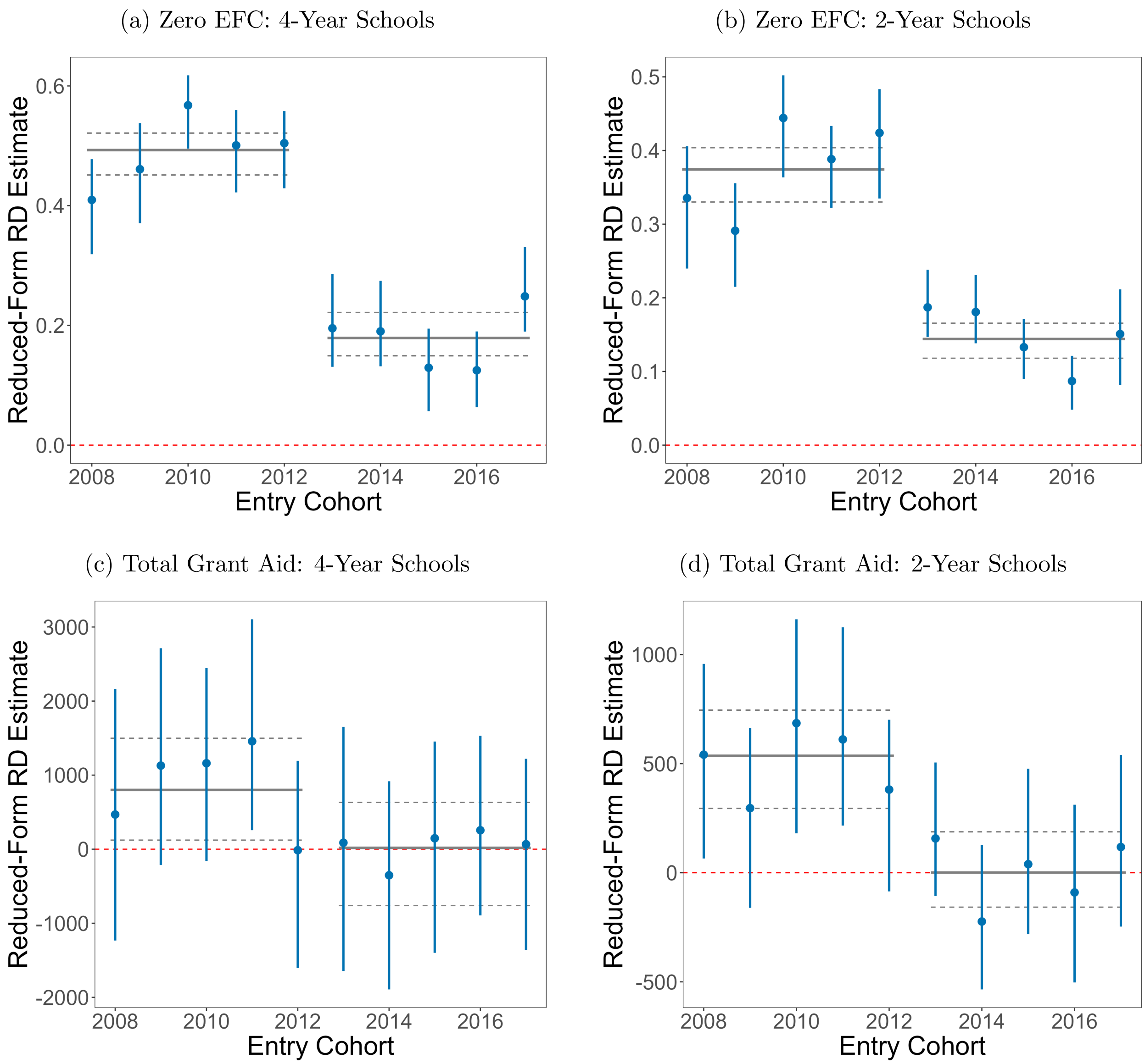
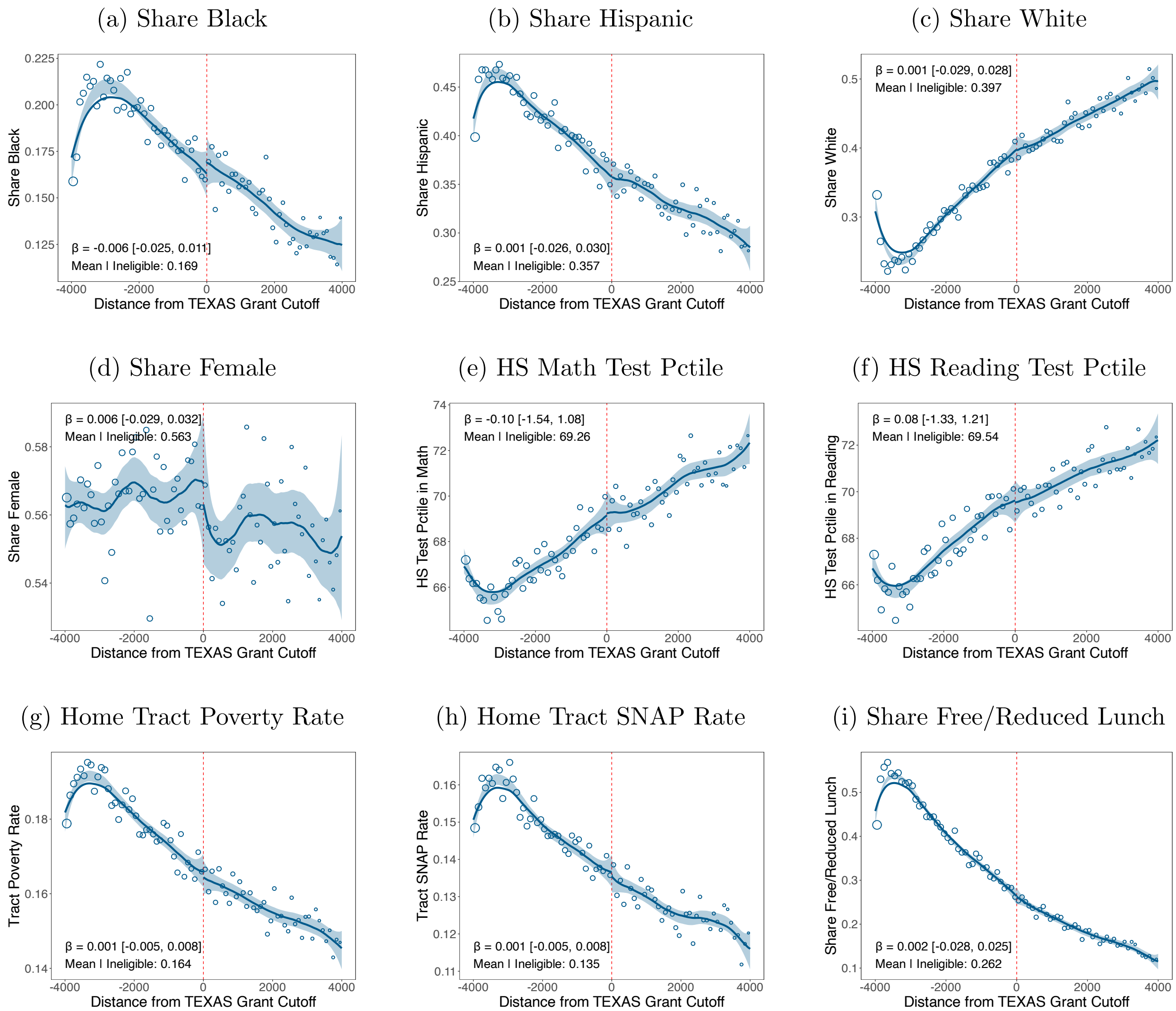
A Additional Figures

Figure A1: Strength of the First-Stage Relationship at the Auto-Zero Cutoff by Year



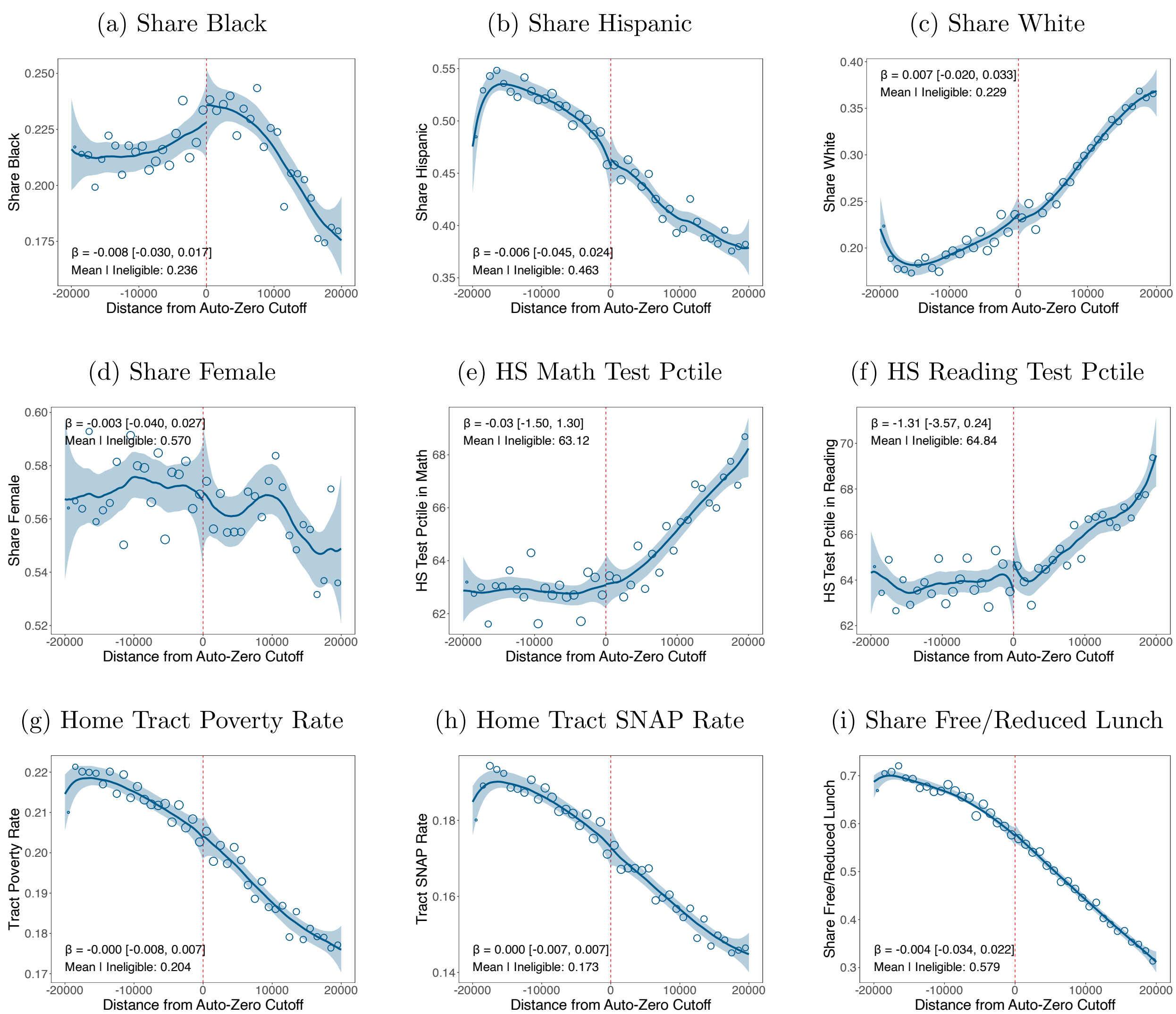
Notes: This figure shows regression discontinuity estimates of the effect of crossing the Auto-Zero EFC cutoff on receipt of a Zero EFC (Panels A and B) and students' first-year grant aid awards (Panels B and C). Each point estimate and 95 percent bias-corrected confidence interval (Calonico et al. 2014) is the result of a separate regression discontinuity estimate. The gray solid and dotted horizontal lines show estimates from regression discontinuity estimations that pool the 2008-2012 and 2013-2017 samples.

Figure A2: Covariate Balance: TEXAS Grant



Notes: This figure shows individual characteristics by students' Expected Family Contribution (EFC) in students' entering year of college, for students within $4,000 of the TEXAS Grant threshold. All individual characteristics are obtained from high school graduation data and measured prior to college entry. The sample consists of students who enter college between the 2007 and 2017 academic years. In each graph, I plot unrestricted means within $100-wide EFC bins and predicted means from local linear regressions estimated separately on each side of the cutoff. The blue regions represent 95 percent confidence intervals of the regression lines. The annotation reports the estimated RD coefficient at the cutoff, together with the mean value of the characteristic on the right (ineligible) side of the cutoff.

Figure A3: Covariate Balance: Auto-Zero Cutoff in Four-Year Schools



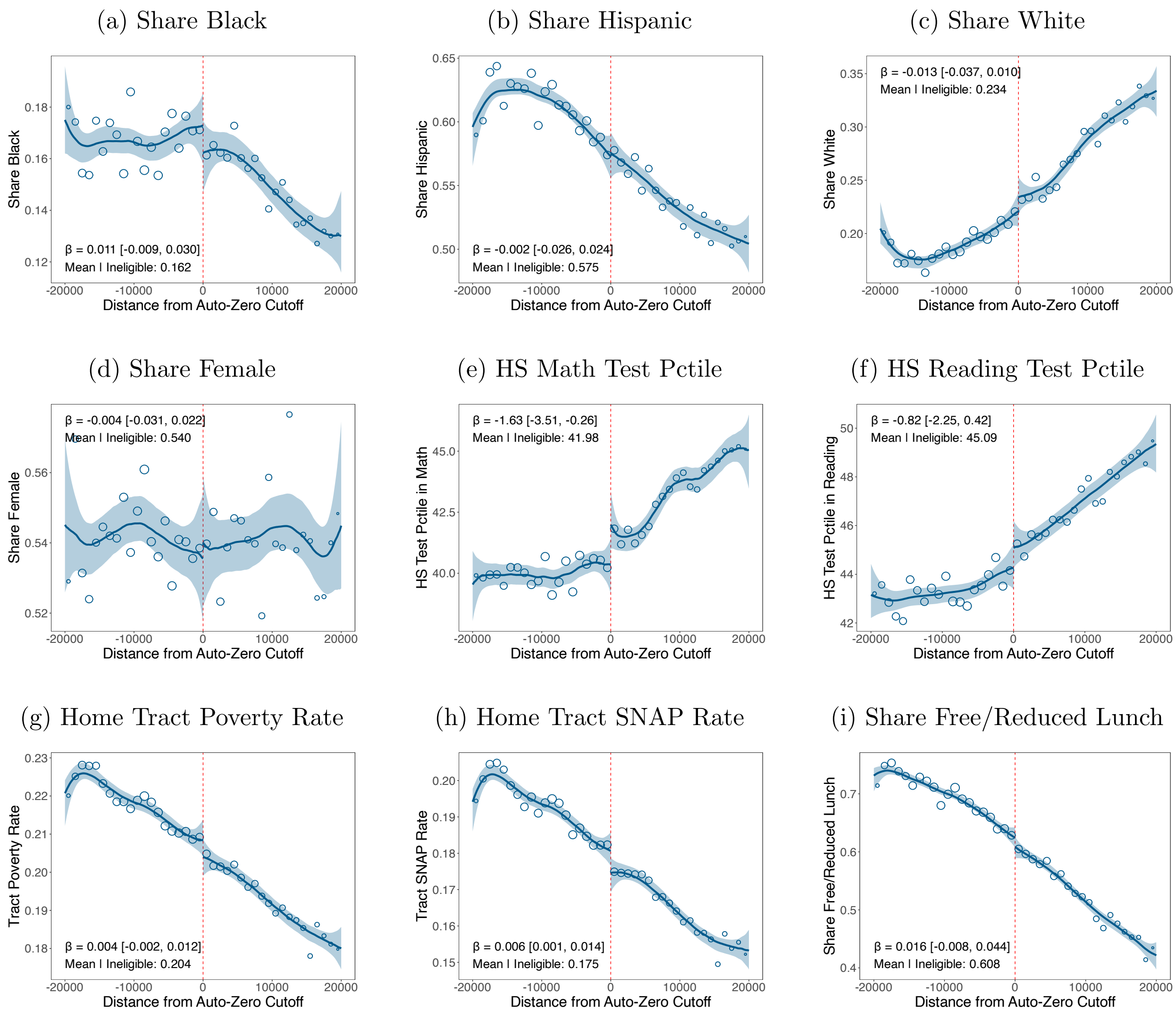
(a) Share Black (b) Share Hispanic (c) Share White

(d) Share Female (e) HS Math Test Pctile (f) HS Reading Test Pctile

(g) Home Tract Poverty Rate (h) Home Tract SNAP Rate (i) Share Free/Reduced Lunch

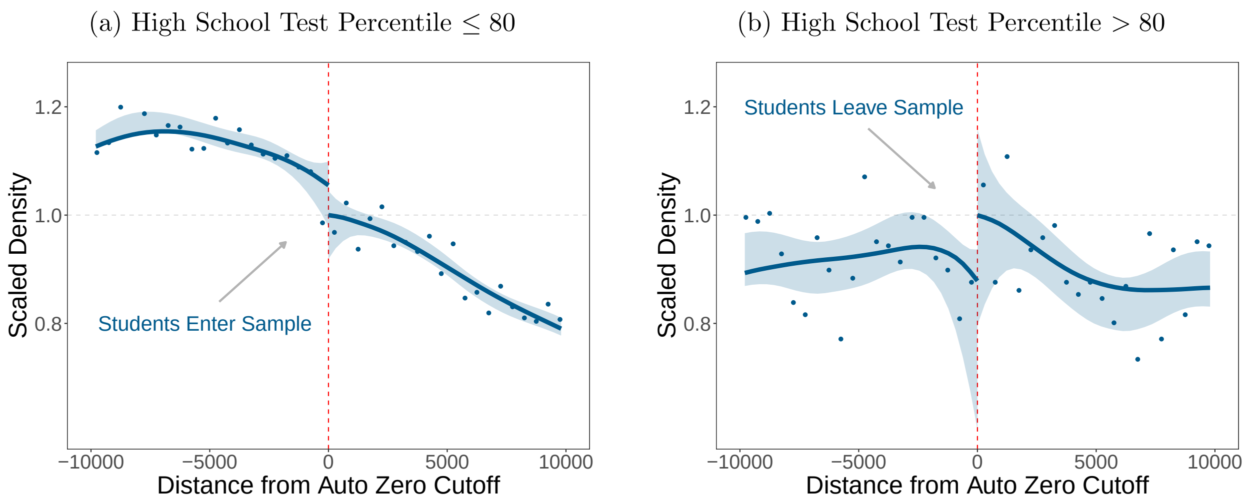
Notes: This figure shows individual characteristics by students' family Adjusted Gross Income (AGI), for students within \$20,000 of the Auto Zero threshold in four-year schools. Family AGI is measured in students' entering year of college. All individual characteristics are obtained from high school graduation data and measured prior to college entry. The sample consists of students who enter college between the 2007 and 2012 academic years. In each graph, I plot unrestricted means within \$1000-wide AGI bins and predicted means from local linear regressions estimated separately on each side of the cutoff. The blue regions represent 95 percent confidence intervals of the regression lines. The annotation reports the estimated RD coefficient at the cutoff, together with the mean value of the characteristic on the right (ineligible) side of the cutoff.

Figure A4: Covariate Balance: Auto-Zero Cutoff in Two-Year Schools



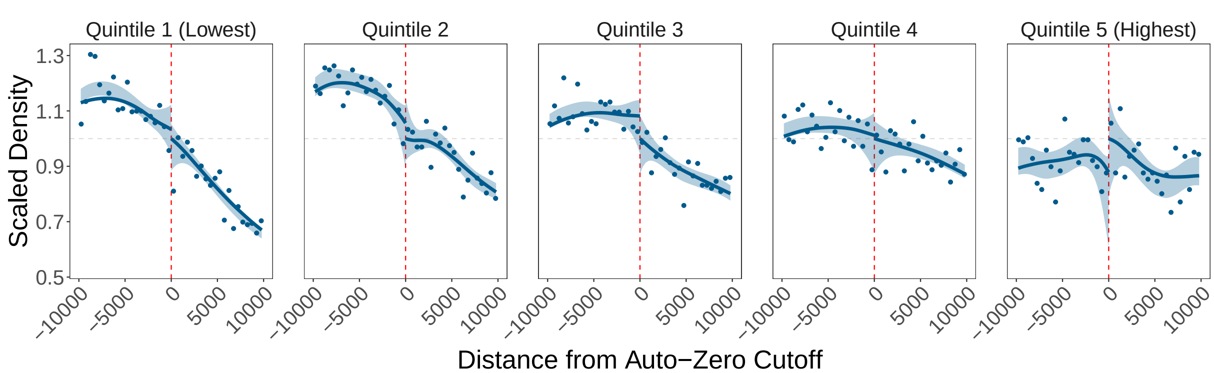
Notes: This figure shows individual characteristics by students' family Adjusted Gross Income (AGI), for students within $20,000 of the Auto Zero threshold in two-year schools. Family AGI is measured in students' entering year of college. All individual characteristics are obtained from high school graduation data and measured prior to college entry. The sample consists of students who enter college between the 2007 and 2012 academic years. In each graph, I plot unrestricted means within $1000-wide AGI bins and predicted means from local linear regressions estimated separately on each side of the cutoff. The blue regions represent 95 percent confidence intervals of the regression lines. The annotation reports the estimated RD coefficient at the cutoff, together with the mean value of the characteristic on the right (ineligible) side of the cutoff.

Figure A5: Defier Behavior among High-Test-Score Community College Students



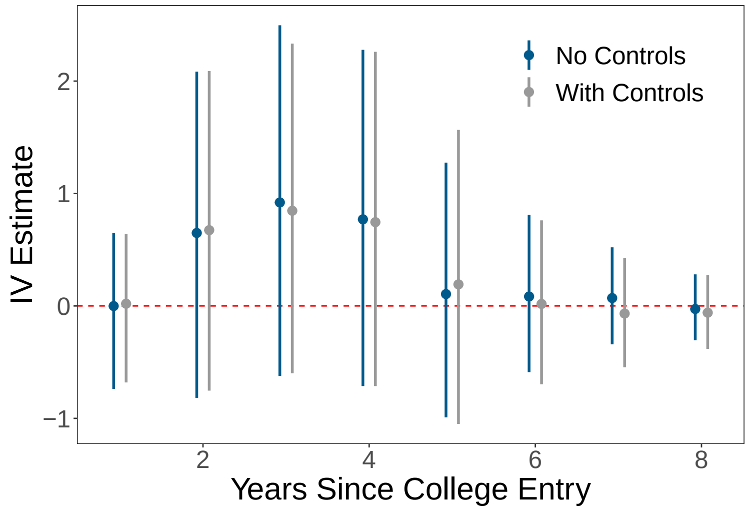
Notes: The figure plots density estimates of students in two-year community colleges in a window of the Automatic Zero EFC cutoff. Panel (a) plots the density for students whose high school test scores are below the 80th percentile of the test score distribution of their graduating class. Panel (b) plots the density for students whose test scores are in the top 20 percent of their high school graduating class. The densities are scaled so that 1 equals the point estimate of the density to the right of the threshold. The blue dots show scaled histogram estimates of the data in \$1,000 AGI bins. The blue fits and 95% confidence intervals are constructed using the method of Cattaneo et al. (2018) at the MSE-optimal bandwidth.

Figure A6: Density Estimates by Quintile: Auto-Zero Cutoff in Two-Year Schools



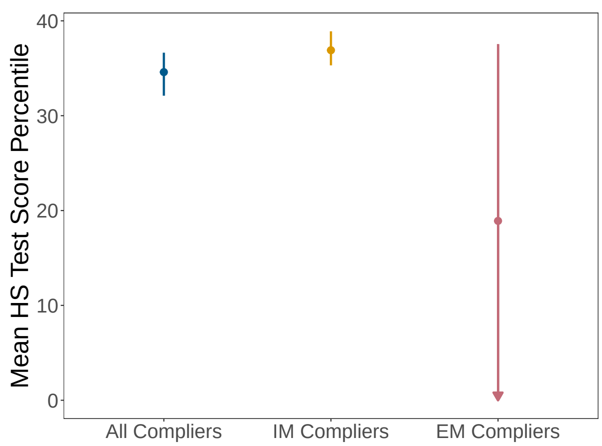
Notes: This figure plots the density of students' family Adjusted Gross Income (AGI) in a neighborhood of the Auto Zero cutoff for the sample of community college students. Each panel plots the density separately by a different quintile of students' high school test scores, with the lowest-scoring students on the left and the highest-scoring students on the right. The y axis is scaled so that 1 equals the density point estimate to the right of the cutoff. The dots plot scaled histogram estimates in $\$1,000$ AGI bins, and the solid lines and shaded regions are density estimates and bias-corrected 95% confidence intervals using the method of Cattaneo et al. (2018). The sample is all students entering between the 2008 and 2012 academic years.

Figure A7: Effects of the TEXAS Grant on Course Completion



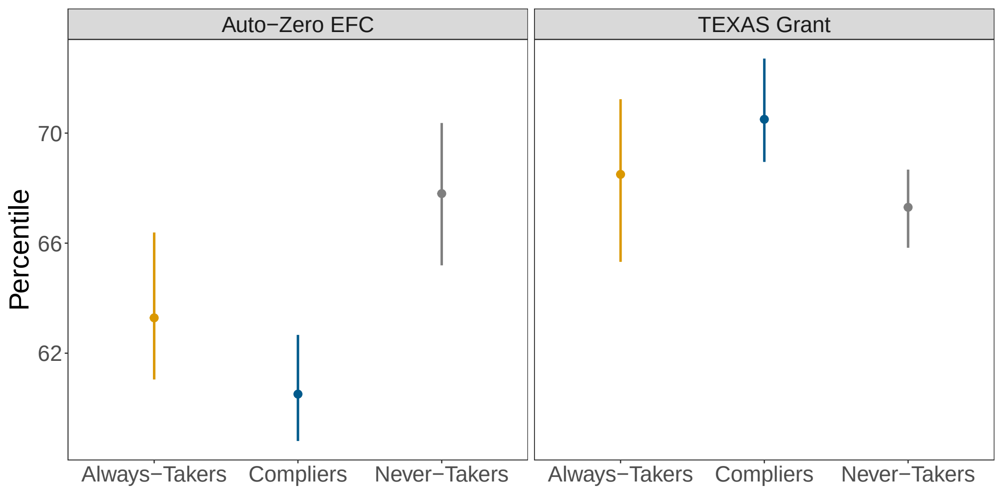
Notes: This figure shows regression discontinuity estimates of the impact of crossing the TEXAS Grant threshold on the credit completion of compliers. The x axis measures years since the student entered college, and the outcome variable at each time horizon is the number of credits completed in the given year. The figure shows point estimates and 95 percent bias-corrected confidence intervals constructed using the method of Calonico et al. (2014). The gray estimates report covariate-adjusted estimates following Calonico, Cattaneo and Farrell (2019).

Figure A9: Extensive-Margin Compliers are Negatively Selected



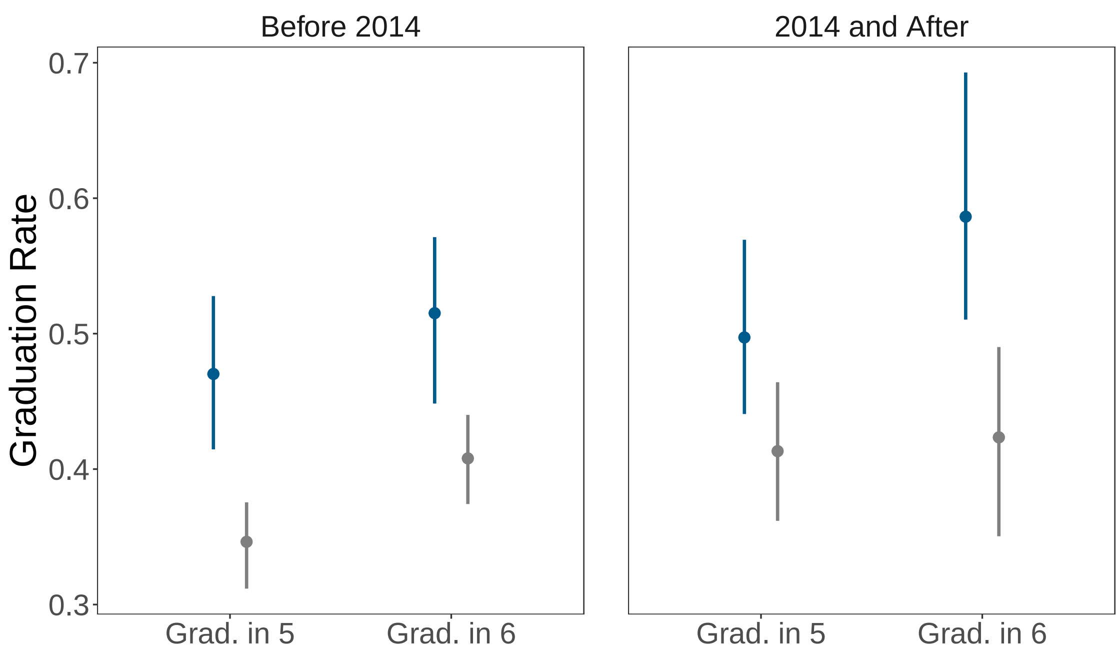
Notes: This figure reports estimates of the characteristics of extensive-margin and intensive-margin compliers at the threshold for Auto-Zero EFC assignment in two-year community colleges. The point estimates are constructed by estimating the analogue to (17) by the analogy principle, and the standard errors are constructed by taking the 5th and 95th percentiles of the estimated parameters from 1,000 bootstrap replications of the entire estimation procedure. The figure truncates the lower confidence interval for mean test scores among extensive-margin compliers; because the overall extensive-margin complier share is very small in some bootstrap replications, the lower bound on the confidence interval falls below zero despite the fact that test score percentile is bounded between 0 and 100.

Figure A8: Targeting of the TEXAS Grant and the Auto-Zero Policy at the Grant Threshold



Notes: This figure shows the characteristics of compliers, always-takers, and never-takers at the TEXAS Grant and Auto-Zero EFC thresholds. The binary treatment for the Auto-Zero cutoff is an indicator for whether a student receives a Zero EFC, and the binary treatment for the TEXAS Grant cutoff is whether a student receives an initial-year TEXAS Grant. "Always-Takers" are students who receive the treatment despite being on the barely-ineligible side of the cutoff, "Never-Takers" are students who do not receive the treatment despite being on the barely-eligible side, and "Compliers" are students who receive the treatment as a result of crossing the grant threshold. The point estimates and confidence intervals are constructed by separately estimating equations (5) and (6) in the main text. The error bars reflect 95 percent bias-corrected confidence intervals estimated using the method of Calonico et al. (2014).

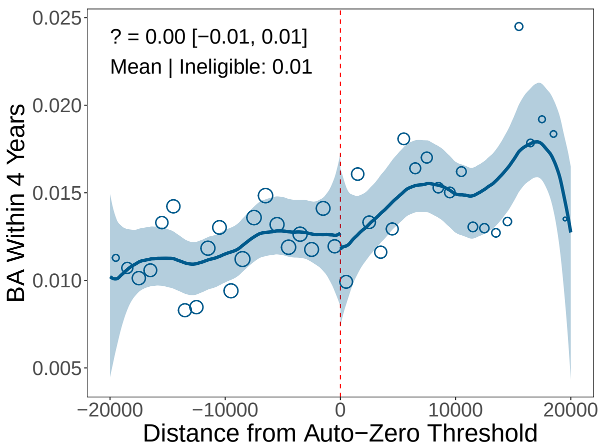
Figure A10: Grant Allocation at the Cutoff Before and After the 2014 Introduction of Merit Standards



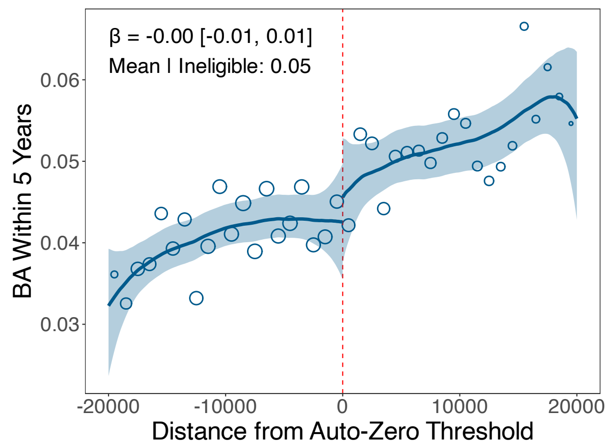
Notes: This figure shows the relative graduation rates of TEXAS Grant compliers and never-takers, separately for students who enter college in 2013 or prior (in the left panel) and for students who enter college in 2014 or later (in the right panel). The error bars are bias-corrected 95 percent confidence intervals following the method of (Calonico et al. 2014).

Figure A11: Raw Comparisons of BA Completion Rates at the Auto-Zero Cutoff in Community Colleges

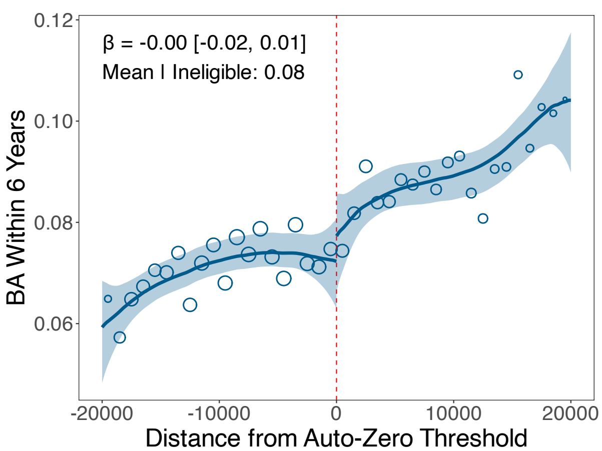
(a) BA Within 4 Years



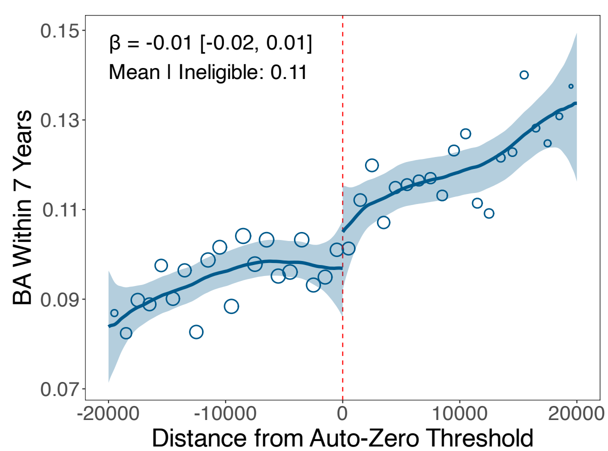
(b) BA Within 5 Years



(c) BA Within 6 Years



(d) BA Within 7 Years



(e) BA Within 8 Years

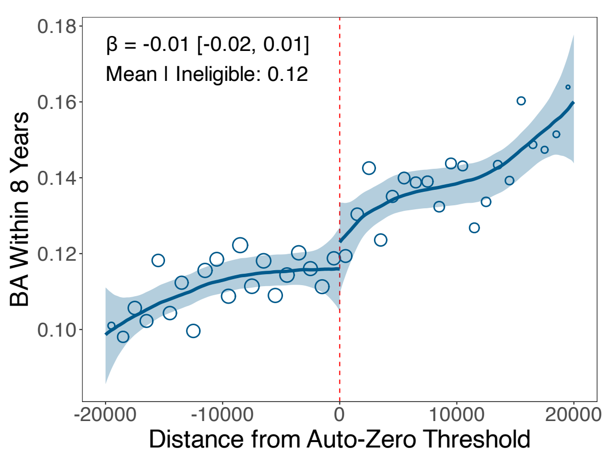
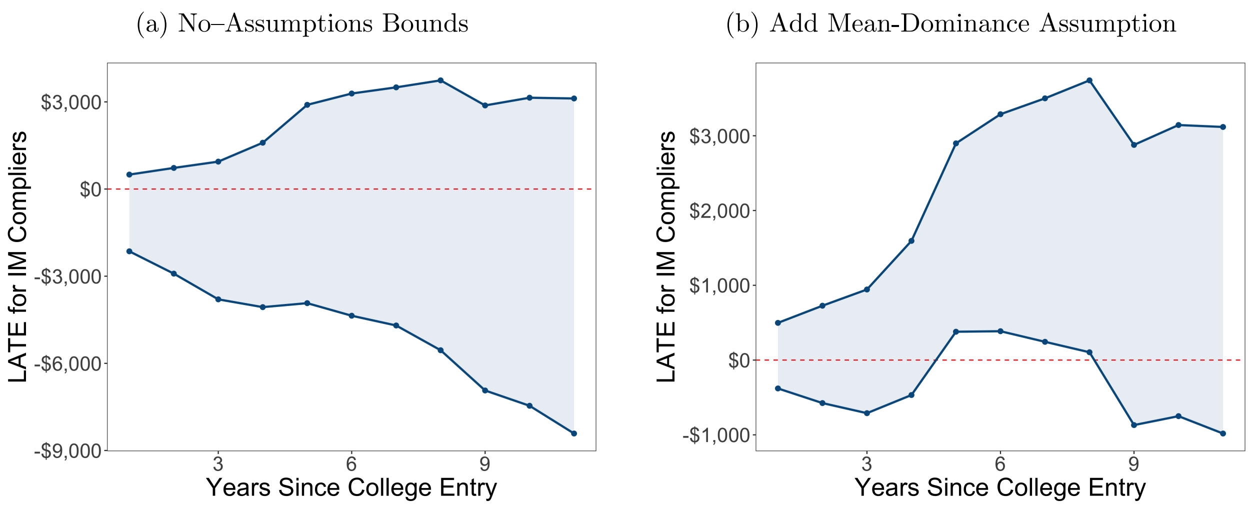


Figure A12: Bounds on Earnings Effects: Intensive-Margin Compliers



Notes: This figure reports bounds on the causal effect of receiving a zero EFC on the earnings trajectories of intensive-margin compliers at the Auto-Zero EFC cutoff in two-year community colleges. Panel (a) shows no-assumptions bounds. Panel (b) introduces the mean-dominance assumption, which raises the lower bound on treatment effects for intensive-margin compliers. Panel (c) adds monotone treatment response assumptions which restrict the grant's impact on earnings to be negative in the first four years after college entry and positive in subsequent years. Panel (d) adds positive monotone treatment response in later years for extensive-margin compliers, which results in tighter upper bounds on treatment effects for intensive-margin compliers.

B Additional Tables

Table B1: Density Tests at the TEXAS Grant Cutoff

		Cattaneo et al. (2018) Test				McCrary (2008) Test		
	N	BW$_{left}$	BW$_{right}$	Diff (s.e.)	p	BW	Diff (s.e.)	p
Optimal Bandwidth	146172	863.4	863.4	0.018 (0.049)	0.72	1185.9	0.000 (0.023)	0.98
By Bandwidth								
BW = 400	146172	400.0	400.0	0.038 (0.072)	0.17	400.0	-0.066 (0.039)	0.095
BW = 800	146172	800.0	800.0	0.008 (0.051)	0.96	800.0	-0.032 (0.028)	0.25
BW = 1200	146172	1200.0	1200.0	0.051 (0.042)	0.97	1200.0	0.001 (0.023)	0.97
Entry 2007–2012	82927	880.4	880.4	0.004 (0.066)	0.71	1397.3	-0.018 (0.027)	0.5
Entry 2013–2017	63245	1726.1	1726.1	0.065 (0.052)	0.09	1152.1	0.040 (0.037)	0.27
By Subsample								
Women	82086	1850.3	1850.3	-0.020 (0.045)	0.75	1558.5	-0.026 (0.027)	0.33
Men	64086	853.3	853.3	0.068 (0.074)	0.98	1059.2	0.019 (0.037)	0.6
Asian	10235	2862.0	2862.0	0.068 (0.099)	0.92	1200.9	-0.081 (0.089)	0.36
Black	25775	902.3	902.3	0.045 (0.112)	0.21	1226.1	0.043 (0.055)	0.44
Hispanic	57242	2921.6	2921.6	0.043 (0.042)	0.24	1096.7	-0.001 (0.040)	0.98
White	50755	1007.8	1007.8	0.075 (0.083)	0.72	1334.7	-0.007 (0.034)	0.83
Test Quintile = 1 (Lowest)	5440	1247.0	1247.0	0.091 (0.201)	0.87	1398.3	0.064 (0.115)	0.58
Test Quintile = 2	15753	1526.6	1526.6	0.014 (0.107)	0.07	1040.0	-0.117 (0.079)	0.14
Test Quintile = 3	27807	1827.1	1827.1	0.060 (0.078)	0.07	965.0	0.086 (0.060)	0.15
Test Quintile = 4	41310	1148.4	1148.4	0.040 (0.081)	0.89	1102.4	-0.024 (0.044)	0.58
Test Quintile = 5 (Highest)	55862	989.5	989.5	0.061 (0.076)	0.78	1343.3	-0.010 (0.034)	0.76

Notes: This table reports results from formal tests of equality of the density of Expected Family Contribution (EFC) across the TEXAS Grant cutoff. The estimation sample is students entering from 2007-2017 with EFC within $\$4,000$ of the cutoff. Columns 3 through 6 report estimates of the test of Cattaneo et al. (2018), and columns 7 through 9 report esitmates of the test of McCrary (2008). Except for tests by bandwidth, the bandwidths for each test are optimally chosen according to each test's selection procedure. Columns 5 and 8 report estimates and standard errors of the discontinuity in the estimated density at the threshold, and columns 6 and 9 report p-values from Wald tests of equality at the cutoff.

Table B2: Density Tests at the Auto-Zero Cutoff in 4-Year Schools

		Cattaneo et al. (2018) Test				McCrary (2008) Test		
	N	BW_{left}	BW_{right}	Diff (s.e.)	p	BW	Diff (s.e.)	p
Optimal Bandwidth	80077	19999.0	19999.0	-0.042 (0.007)	0.72	5292.4	-0.018 (0.028)	0.52
By Bandwidth								
BW = 1000	80077	1000.0	1000.0	-0.001 (0.031)	0.33	1000.0	-0.044 (0.066)	0.51
BW = 3000	80077	3000.0	3000.0	-0.001 (0.018)	0.70	3000.0	-0.019 (0.038)	0.61
BW = 5000	80077	5000.0	5000.0	-0.001 (0.014)	0.78	5000.0	-0.018 (0.029)	0.55
BW = 7000	80077	7000.0	7000.0	-0.005 (0.012)	0.99	7000.0	-0.018 (0.025)	0.47
BW = 9000	80077	9000.0	9000.0	-0.007 (0.010)	0.87	9000.0	-0.021 (0.022)	0.33
By Subsample								
Women	45332	5186.9	5186.9	0.002 (0.018)	0.47	6018.4	-0.016 (0.035)	0.65
Men	34745	19999.0	19999.0	-0.035 (0.011)	0.44	4747.5	-0.025 (0.046)	0.58
Asian	5137	13573.2	13573.2	0.021 (0.034)	0.36	6188.3	-0.028 (0.102)	0.78
Black	17258	6085.9	6085.9	0.020 (0.029)	0.41	6767.5	0.014 (0.052)	0.78
Hispanic	37373	18468.3	18468.3	-0.056 (0.011)	0.98	4857.7	-0.002 (0.044)	0.96
White	19607	5237.7	5237.7	-0.005 (0.027)	0.37	6525.3	-0.046 (0.053)	0.38
Test Quintile = 1 (Lowest)	4645	10432.6	10432.6	0.013 (0.042)	0.78	7195.2	-0.073 (0.101)	0.47
Test Quintile = 2	11309	5218.5	5218.5	0.015 (0.037)	0.69	5028.1	0.001 (0.076)	0.99
Test Quintile = 3	16861	6203.6	6203.6	-0.011 (0.028)	0.53	5375.2	-0.074 (0.060)	0.22
Test Quintile = 4	21933	8190.1	8190.1	0.013 (0.021)	0.24	6130.2	0.025 (0.050)	0.61
Test Quintile = 5 (Highest)	25329	6825.5	6825.5	-0.030 (0.021)	0.59	8503.2	-0.034 (0.041)	0.4

Notes: This table reports results from formal tests of equality of the density of family Adjusted Gross Income (AGI) across the Auto-Zero cutoff in 4-year schools. The estimation sample is students entering from 2008-2012 with AGI within $\$20,000$ of the cutoff. Columns 3 through 6 report estimates of the test of Cattaneo et al. (2018), and columns 7 through 9 report esitmates of the test of McCrary (2008). Except for tests by bandwidth, the bandwidths for each test are optimally chosen according to each test's selection procedure. Columns 5 and 8 report estimates and standard errors of the discontinuity in the estimated density at the threshold, and columns 6 and 9 report p-values from Wald tests of equality at the cutoff.

Table B3: Density Tests at the Auto-Zero Cutoff in 2-Year Schools

		Cattaneo et al. (2018) Test				McCrary (2008) Test		
	N	BW_{left}	BW_{right}	Diff (s.e.)	p	BW	Diff (s.e.)	p
Optimal Bandwidth	113896	5402.5	5402.5	-0.009	0.46	4398.5	-0.006	0.83
				(0.011)			(0.027)	
By Bandwidth								
BW = 1000	113896	1000.0	1000.0	0.013	0.21	1000.0	0.059	0.3
				(0.026)			(0.057)	
BW = 3000	113896	3000.0	3000.0	-0.006	0.71	3000.0	0.007	0.84
				(0.015)			(0.032)	
BW = 5000	113896	5000.0	5000.0	-0.009	0.54	5000.0	-0.008	0.75
				(0.012)			(0.025)	
BW = 7000	113896	7000.0	7000.0	-0.011	0.54	7000.0	-0.019	0.36
				(0.010)			(0.021)	
BW = 9000	113896	9000.0	9000.0	-0.012	0.29	9000.0	-0.024	0.2
				(0.009)			(0.018)	
By Subsample								
Women	61645	4714.3	4714.3	-0.010	0.61	6268.4	-0.008	0.8
				(0.017)			(0.030)	
Men	52251	5040.3	5040.3	-0.007	0.70	4393.6	-0.029	0.46
				(0.017)			(0.039)	
Asian	2759	7745.6	7745.6	-0.073	0.04	5354.6	-0.399	0.01
				(0.061)			(0.156)	
Black	18192	8071.6	8071.6	-0.028	0.67	5388.8	-0.073	0.21
				(0.024)			(0.058)	
Hispanic	66034	5680.3	5680.3	-0.009	0.59	4475.4	0.001	0.98
				(0.015)			(0.035)	
White	26006	4656.4	4656.4	0.003	0.95	5374.0	0.043	0.39
				(0.025)			(0.050)	
Test Quintile = 1 (Lowest)	29987	8399.7	8399.7	-0.009	0.32	4631.4	-0.084	0.096
				(0.018)			(0.050)	
Test Quintile = 2	31141	14171.5	14171.5	-0.026	0.74	6094.0	-0.010	0.82
				(0.013)			(0.043)	
Test Quintile = 3	25515	6301.4	6301.4	-0.016	0.67	6443.6	-0.047	0.29
				(0.023)			(0.045)	
Test Quintile = 4	18073	10862.2	10862.2	-0.005	0.78	5036.2	0.030	0.64
				(0.020)			(0.063)	
Test Quintile = 5 (Highest)	9180	5749.2	5749.2	0.040	0.10	5591.6	0.183	0.028
				(0.038)			(0.083)	

Notes: This table reports results from formal tests of equality of the density of family Adjusted Gross Income (AGI) across the Auto-Zero cutoff in 2-year schools. The estimation sample is students entering from 2008-2012 with AGI within $\$20,000$ of the cutoff. Columns 3 through 6 report estimates of the test of Cattaneo et al. (2018), and columns 7 through 9 report esitmates of the test of McCrary (2008). Except for tests by bandwidth, the bandwidths for each test are optimally chosen according to each test's selection procedure. Columns 5 and 8 report estimates and standard errors of the discontinuity in the estimated density at the threshold, and columns 6 and 9 report p-values from Wald tests of equality at the cutoff.

Table B4: RD Estimates: Institutional Characteristics

	All Students		> 1 Acceptance	
	RF (1)	IV (2)	RF (3)	IV (4)
Mean Share Asian	-0.00	-0.00	-0.00	-0.00
	[-0.00, 0.00]	[-0.01, 0.01]	[-0.01, 0.01]	[-0.02, 0.01]
Mean Share Black	-0.01	-0.02	-0.00	-0.01
	[-0.02, 0.00]	[-0.04, 0.00]	[-0.02, 0.01]	[-0.04, 0.02]
Mean Share Hispanic	0.01	0.01	0.00	-0.00
	[-0.00, 0.02]	[-0.02, 0.04]	[-0.02, 0.02]	[-0.05, 0.04]
Mean Share White	0.00	0.01	0.00	0.02
	[-0.01, 0.01]	[-0.02, 0.03]	[-0.01, 0.03]	[-0.02, 0.06]
Mean Share Female	0.00	0.00	-0.00	-0.00
	[-0.00, 0.00]	[-0.01, 0.01]	[-0.01, 0.00]	[-0.02, 0.01]
Mean Incoming SAT Score	0.35	2.58	-0.12	1.58
	[-5.63, 6.07]	[-10.75, 15.42]	[-9.47, 10.19]	[-19.40, 23.65]
Mean Parent AGI	-778.66	-646.13	-654.96	-1358.60
	[-3302, 1539]	[-7261, 6186]	[-4743, 4558]	[-12358, 10719]
Mean Share w/ TEXAS Grant	0.00	0.01	0.00	0.00
	[-0.00, 0.01]	[-0.01, 0.02]	[-0.01, 0.01]	[-0.03, 0.03]
Mean Total Grant Aid	82.82	47.40	184.29	116.21
	[-227, 371]	[-731, 778]	[-246, 614]	[-982, 1149]
Mean Total Loans	12.50	149.45	257.96	722.10
	[-321, 280]	[-745, 1074]	[-254, 919]	[-628, 2306]
Mean TEXAS Grant	77.65	155.77	44.37	69.33
	[-18, 182]	[-58, 395]	[-93, 183]	[-296, 412]
Mean Pell Grant	44.42	-3.55	34.70	-132.53
	[-82, 165]	[-309, 299]	[-154, 188]	[-678, 265]

Notes: This table reports estimates of changes in mean institutional characteristics among four-year college students across the TEXAS Grant cutoff. $N = 160,270$ for regressions on all students and $N = 55,025$ for regressions on students with > 1 acceptance. The outcome in each regression is the mean of the given characteristic within a student's institution and entering cohort. Columns (1) and (2) report results for the full population of college entrants between the 2007 and 2017 academic years. Columns (3) and (4) restrict the sample to students whose application records indicate more than one acceptance to a Texas public four-year college. Each estimation uses the MSE-optimal bandwidth calculated separately for each outcome using the method of Calonico et al. (2014).

Table B5: Estimated Bounds for Potential Outcomes by Complier Group, 6-Year BA Completion

Complier Margin	Intensive	Extensive
	(1)	(2)
Panel A. Untreated Potential Outcomes		
Untreated Mean, IM Compliers	0.057	
	(0.015)	
Untreated Mean, EM Compliers		0.007
		(Assumed)
Panel B. Treated Potential Outcomes		
Pooled Treated Mean, All Compliers	0.058	
	(0.017)	
Panel C. Bounds on Treated Potential Outcomes by Group		
Upper Bound, No Assumptions	0.067 (0.020)	0.445 (0.277)
Lower Bound, No Assumptions	0.000 (0.018)	0.000 (0.000)
Upper Bound, Mean Dominance	0.067 (0.020)	0.058 (0.017)
Lower Bound, Mean Dominance	0.058 (0.017)	0.000 (0.000)
Upper Bound, Mean Dominance + MTR	0.066 (0.019)	0.058 (0.031)
Lower Bound, Mean Dominance + MTR	0.058 (0.013)	0.007 (0.000)
Panel D. Bounds on LATEs by Complier Group		
Upper Bound, No Assumptions	0.010 (0.026)	0.439 (0.277)
Lower Bound, No Assumptions	-0.057 (0.023)	-0.007 (0.000)
Upper Bound, Mean Dominance	0.010 (0.026)	0.052 (0.017)
Lower Bound, Mean Dominance	0.001 (0.023)	-0.007 (0.000)
Upper Bound, Mean Dominance + MTR	0.009 (0.025)	0.052 (0.031)
Lower Bound, Mean Dominance + MTR	0.001 (0.015)	0.000 (0.000)
Share of Compliers	0.869	0.131

Notes: This table shows estimates of bounds on the effect of receiving a Zero EFC on students' probability of B.A. completion within six years of initial college entry. Panel A reports the mean B.A. completion rates of untreated intensive-margin compliers (Column (1)), estimated by Equation (14), and untreated extensive-margin compliers (Column (2)), estimated by taking the mean 6-year B.A. completion rate among all non-college-attenders in my sample. Panel B reports the pooled B.A. completion rates of treated intensive-margin and extensive-margin compliers, etsimated by Equation (15). Panel C reports upper and lower bounds on $\bar{Y}_1^{\mathrm{IM}}$ and $\bar{Y}_1^{\mathrm{EM}}$, the treated mean potential outcomes of intensive-margin and extensive-margin compliers. Panel D subtracts the counterfactual means in panel A from these estimates to arrive at bounds on $LATE_{IM}$ and $LATE_{EM}$. See main text for additional details.

Table B6: Overall BA Completion Effects, Community College Students at Auto-Zero EFC Cutoff

	$LATE^*$	=	$\bar{Y}_1^{\text{Complier}}$	−	$(1-\omega)$	$\bar{Y}_0^{\text{IM}}$	−	ω	$\bar{Y}_0^{\text{EM}}$
Grad. w/in:	Overall LATE		Treated Mean, All Compliers		Share IM Compliers	Counterfactual, IM Compliers		Share EM Compliers	Counterfactual, EM Compliers
4 Years	0.003		0.015		0.869	0.014		0.131	0.001
	(0.008)		(0.007)		(0.066)	(0.004)		(0.066)	(—)
5 Years	0.001		0.039		0.869	0.042		0.131	0.004
	(0.017)		(0.013)		(0.066)	(0.011)		(0.066)	(—)
6 Years	0.008		0.058		0.869	0.057		0.131	0.007
	(0.022)		(0.017)		(0.066)	(0.015)		(0.066)	(—)
7 Years	0.006		0.090		0.869	0.095		0.131	0.009
	(0.027)		(0.019)		(0.066)	(0.017)		(0.066)	(—)
8 Years	0.001		0.102		0.869	0.115		0.131	0.012
	(0.027)		(0.020)		(0.066)	(0.018)		(0.066)	(—)

Notes: This figure reports estimates of the overall effect of receiving a Zero EFC on the B.A. completion rates of community-college applicants at the Auto-Zero EFC threshold. The first column of the table reports the overall $LATE^*$ defined by Equation (7), and the remaining columns decompose this effect into complier shares and potential outcomes following Equation (12). Standard errors come from 1,000 bootstrap replications of the entire estimation procedure. Standard errors are not reported for $\bar{Y}_0^{\text{EM}}$, because this counterfactual value is assumed constant following the proxy assumption described in Section 7.

Table B7: Overall Earnings Effects, Community College Students at Auto-Zero EFC Cutoff

	$LATE^*$	=	$\bar{Y}_1^{\text{Complier}}$	−	$(1-\omega)$	$\bar{Y}_0^{\text{IM}}$	−	ω	$\bar{Y}_0^{\text{EM}}$
Years Post College Entry	Overall LATE		Treated Mean, All Compliers		Share IM Compliers	Counterfactual, IM Compliers		Share EM Compliers	Counterfactual, EM Compliers
1 Year	-469		5,822		0.869	6,201		0.131	6,884
	(285)		(236)		(0.066)	(182)		(0.066)	(—)
2 Years	-576		8,644		0.869	9,219		0.131	9,226
	(372)		(307)		(0.066)	(228)		(0.066)	(—)
3 Years	-614		10,980		0.869	11,691		0.131	10,958
	(480)		(409)		(0.066)	(303)		(0.066)	(—)
4 Years	-316		13,685		0.869	14,153		0.131	12,999
	(611)		(515)		(0.066)	(384)		(0.066)	(—)
5 Years	538		16,723		0.869	16,344		0.131	15,127
	(703)		(602)		(0.066)	(431)		(0.066)	(—)
6 Years	628		19,265		0.869	18,879		0.131	17,032
	(783)		(668)		(0.066)	(487)		(0.066)	(—)
7 Years	589		21,615		0.869	21,371		0.131	18,738
	(836)		(711)		(0.066)	(548)		(0.066)	(—)
8 Years	585		24,138		0.869	24,034		0.131	20,360
	(966)		(851)		(0.066)	(617)		(0.066)	(—)
9 Years	-327		24,874		0.869	25,743		0.131	21,600
	(1,050)		(899)		(0.066)	(675)		(0.066)	(—)
10 Years	-187		25,846		0.869	26,596		0.131	22,296
	(1,185)		(988)		(0.066)	(740)		(0.066)	(—)
11 Years	-330		27,215		0.869	28,197		0.131	23,215
	(1,244)		(1,056)		(0.066)	(749)		(0.066)	(—)

Notes: This figure reports estimates of the overall effect of receiving a Zero EFC on the yearly earnings of community-college applicants at the Auto-Zero EFC threshold. The first column of the table reports the overall $LATE^*$ defined by Equation (7), and the remaining columns decompose this effect into complier shares and potential outcomes following Equation (12). Standard errors come from 1,000 bootstrap replications of the entire estimation procedure. Standard errors are not reported for $\bar{Y}_0^{\text{EM}}$, because this counterfactual value is assumed constant following the proxy assumption described in Section 7.

Table B8: IV Estimates of the Impact of the TEXAS Grant on Persistence and Graduation

X =	2	3	4	5	6	7	8
A. Reenrollment in Year X							
TEXAS Grant	-0.01	0.01	0.01	0.01	0.02	0.01	0.00
	[-0.06, 0.05]	[-0.05, 0.06]	[-0.04, 0.07]	[-0.04, 0.06]	[-0.01, 0.05]	[-0.01, 0.03]	[-0.01, 0.01]
Mean | Ineligible	0.42	0.24	0.15	0.16	0.09	0.03	0.01
Observations	146,172	146,172	146,172	146,172	146,172	146,172	146,172
Obs. w/in Bandwidth	27,273	31,119	29,455	30,399	27,063	26,935	26,420
Panel B. Graduation by End of Year X							
TEXAS Grant	-0.00	-0.00	0.00	0.01	0.02	0.02	0.01
	[-0.01, 0.00]	[-0.02, 0.02]	[-0.05, 0.05]	[-0.06, 0.08]	[-0.05, 0.08]	[-0.04, 0.08]	[-0.07, 0.09]
Mean | Ineligible	-0.00	0.00	-0.06	0.00	0.04	0.05	0.07
Observations	146,172	146,172	146,172	133,480	126,714	112,927	99,246
Obs. w/in Bandwidth	40,982	25,981	27,580	21,910	25,463	33,234	21,766

Notes: This table reports estimates of the impacts of the TEXAS Grant on students' probability of renrollment in college (Panel A) and graduation (Panel B). Each point estimate comes from a separate fuzzy RD specification across the TEXAS Grant threshold, where the first stage is an indicator for receiving a TEXAS Grant in the first year of college. The columns show outcomes measured at different time horizons, measured from the year of college entry. Each estimation uses the MSE-optimal bandwidth calculated using the method of Calonico et al. (2014).

Table B9: IV Estimates of the Impact of the TEXAS Grant on Loans and Earnings

X =	1	2	3	4	5	6	7
Panel B. Loans in Year X							
TEXAS Grant	-2,186	-1,404	-465	-621	30	-114	63
	[-2881, -1554]	[-2078, -739]	[-1157, 224]	[-1299, -7]	[-438, 476]	[-419, 172]	[-130, 270]
Mean | Ineligible	6195	4693	4338	3798	1672	569	232
Observations	146,172	146,172	146,172	146,172	146,172	133,480	126,714
Obs. w/in Bandwidth	27,793	30,514	31,755	33,491	34,420	31,003	31,949
Panel B. Earnings in Year X							
TEXAS Grant	-734	-886	-779	-759	-140	-1,215	-972
	[-1434, -169]	[-1836, -22]	[-1875, 269]	[-2411, 605]	[-2347, 2035]	[-4284, 1708]	[-4450, 2413]
Mean | Ineligible	6189.16	10188.52	13256.89	14630.12	14981.33	17190.14	19380.95
Observations	146,172	146,172	146,172	146,172	146,172	146,172	133,480
Obs. w/in Bandwidth	25,523	25,604	28,474	22,868	26,081	23,118	23,405

Notes: This table reports estimates of the impacts of the TEXAS Grant on students' student loans (Panel A) and labor market earnings (Panel B). Each point estimate comes from a separate fuzzy RD specification across the TEXAS Grant threshold, where the first stage is an indicator for receiving a TEXAS Grant in the first year of college. The columns show outcomes measured at different time horizons, measured from the year of college entry. Each estimation uses the MSE-optimal bandwidth calculated using the method of Calonico et al. (2014).

Table B10: Bounds on BA Completion Effects by Horizon for Intensive-Margin and Extensive-Margin Compliers

		$LATE$ for IM Compliers			$LATE$ for EM Compliers		
BA Within:	Bound	No-Assm.	MD	MD + MTR	No-Assm.	MD	MD + MTR
4 Years	Upper	0.004 (0.009)	0.004 (0.009)	0.004 (0.009)	0.116 (0.255)	0.014 (0.007)	0.014 (0.009)
	Lower	-0.014 (0.006)	0.002 (0.008)	0.002 (0.005)	-0.001 (0.000)	-0.001 (0.000)	0.000 (0.000)
5 Years	Upper	0.002 (0.020)	0.002 (0.020)	0.002 (0.019)	0.291 (NA)	0.035 (0.013)	0.011 (0.023)
	Lower	-0.042 (0.015)	-0.003 (0.018)	0.000 (0.011)	-0.004 (0.000)	-0.004 (0.000)	0.000 (0.000)
6 Years	Upper	0.010 (0.026)	0.010 (0.026)	0.009 (0.025)	0.439 (0.277)	0.052 (0.017)	0.052 (0.031)
	Lower	-0.057 (0.023)	0.001 (0.023)	0.001 (0.015)	-0.007 (0.000)	-0.007 (0.000)	0.000 (0.000)
7 Years	Upper	0.008 (0.031)	0.008 (0.031)	0.007 (0.030)	0.676 (0.242)	0.081 (0.019)	0.044 (0.045)
	Lower	-0.095 (0.034)	-0.006 (0.028)	0.000 (0.015)	-0.009 (0.000)	-0.009 (0.000)	0.000 (0.000)
8 Years	Upper	0.003 (0.030)	0.003 (0.030)	0.002 (0.030)	0.771 (0.215)	0.091 (0.020)	0.011 (0.050)
	Lower	-0.115 (0.040)	-0.012 (0.027)	0.000 (0.014)	-0.012 (0.000)	-0.012 (0.000)	0.000 (0.000)

Notes: This table shows estimates of bounds on the effects of receiving a Zero EFC on the graduation probability of intensive-margin compliers and extensive-margin compliers at the Auto-Zero EFC threshold in two-year community colleges. Extensive-margin compliers are students who enter the sample as a result of treatment, and intensive-margin compliers are students who would remain in two-year colleges even without the treatment of receiving a zero EFC. Each row reports results for a separate outcome, defined as an indicator for completing any BA degree within the specified number of years. The "no-assumptions" columns report no-assumptions bounds, while the "MD" and "MD+MTR" bounds add mean-dominance and monotone treatment response assumptions; see main text for details. Standard errors are from $1,000$ bootstrap replications of the entire estimation procedure, following the bootstrap procedure for RD designs with manipulation of Gerard et al. (2020).

C Identification with Data on All Applicants

C.1 Proof of Equation (1)

Let $Y_{s,g}$ denote the potential outcomes of an individual who chooses schooling option $s \in \{0,1\}$ and receives grant status $g \in \{0,1\}$. Realized schooling choices S_g are a function of grant receipt. Finally, let $Z = \mathbf{1}\{k \leq k^*\}$ be an indicator for whether the student's EFC falls below the a grant threshold, and let $D_z \in \{0,1\}$ represent the student's potential grant status as a function of $z \in \{0,1\}$.

Equation 1 states that

$$\frac{\lim_{r\uparrow k^*} \mathbb{E}[Y \mid k=r] - \lim_{r\downarrow k^*} \mathbb{E}[Y \mid k=r]}{\lim_{r\uparrow k^*} \mathbb{E}[D \mid k=r] - \lim_{r\downarrow k^*} \mathbb{E}[D \mid k=r]} = \mathbb{E}[Y_{S_1,1} - Y_{S_0,0} \mid D_1 = 1, D_0 = 0, k = k^*]$$

To see this, note that we can write realized outcomes Y as:[55]

$$\begin{aligned} Y &= Y_{S_1,1} D + Y_{S_0,0}(1-D) \\ &= D\left(Y_{S_1,1} - Y_{S_0,0}\right) + Y_{S_0,0} \end{aligned}$$

The numerator of the limiting Wald Estimand is therefore:

$$\begin{aligned} &\lim_{r\uparrow k^*} \mathbb{E}[Y \mid k=r] - \lim_{r\downarrow k^*} \mathbb{E}[Y \mid k=r] \\ &= \lim_{r\uparrow k^*} \mathbb{E}\big[D(Y_{S_1,1} - Y_{S_0,0})\big] - \lim_{r\downarrow k^*} \mathbb{E}\big[D(Y_{S_1,1} - Y_{S_0,0})\big] \\ &\quad + \underbrace{\lim_{r\uparrow k^*} \mathbb{E}[Y_{S_0,0}] - \lim_{r\downarrow k^*} \mathbb{E}[Y_{S_0,0}]}_{=0} \end{aligned}$$

[55] To see this, start with potential outcomes:

$$Y = S(Y_{1,1}D + Y_{1,0}(1-D)) + (1-S)(Y_{0,1}D + Y_{0,0}(1-D))$$

Plugging in $S = DS_1 + (1-D)S_0$ and rearranging gives:

$$Y = D(S_1 Y_{1,1} + (1-S_1)Y_{0,1}) + (1-D)(S_0 Y_{1,0} - Y_{0,0}(1-S_0))$$

which immediately yields the result.

where the canceled term comes from continuity of potential outcomes at the cutoff (Assumption 2). We can expand the first term of the numerator as:

$$\begin{aligned}
&\lim_{r\uparrow k^*} \mathbb{E}\big[G(Y_{S_1,1} - Y_{S_0,0})\big] \\
&= \lim_{r\uparrow k^*} \mathbb{E}[Y_{S_1,1} - Y_{S_0,0} \mid T_g = C, k = r]P(T_g = C \mid k = r) \\
&\quad + \lim_{r\uparrow k^*} \mathbb{E}[Y_{S_1,1} - Y_{S_0,0} \mid T_g = AT, k = r]P(T_g = AT \mid k = r) \\
&= \mathbb{E}[Y_{S_1,1} - Y_{S_0,0} \mid T_g = C, k = k^*]P(T_g = C \mid k = k^*) \\
&\quad + \mathbb{E}[Y_{S_1,1} - Y_{S_0,0} \mid T_g = AT, k = k^*]P(T_g = AT \mid k = k^*)
\end{aligned}$$

where $T_g = C$ is notational shorthand for $(D_1 = 1, D_0 = 0)$ (grant compliers) and $T_g = AT$ stands for $(D_1 = D_0 = 1)$ (grant always-takers), and where the last equality again invokes the assumed continuity of the potential outcome and type composition functions at the cutoff. By a similar argument, the second term of the numerator is:

$$\begin{aligned}
&\lim_{r\downarrow k^*} \mathbb{E}\big[D(Y_{S_1,1} - Y_{S_0,0})\big] \\
&= \lim_{r\downarrow k^*} \mathbb{E}[Y_{S_1,1} - Y_{S_0,0} \mid T_g = AT, k = r]P(T_g = AT \mid k = r) \\
&= \mathbb{E}[Y_{S_1,1} - Y_{S_0,0} \mid T_g = AT, k = k^*]P(T_g = AT, k = k^*)
\end{aligned}$$

It follows that the numerator of the limiting Wald Estimand is:

$$\begin{aligned}
&\lim_{r\uparrow k^*} \mathbb{E}[Y \mid k = r] - \lim_{r\downarrow k^*} \mathbb{E}[Y \mid k = r] \\
&= \mathbb{E}[Y_{S_1,1} - Y_{S_0,0} \mid T_g = C, k = k^*]P(T_g = C \mid k = k^*)
\end{aligned}$$

By a similar argument, the denominator of the limiting Wald Estimand is given by:

$$\lim_{r\uparrow k^*} \mathbb{E}[G \mid k = r] - \lim_{r\downarrow k^*} \mathbb{E}[G \mid k = r] = P(T_g = C \mid k = k^*) \tag{22}$$

Together, these imply that:

$$\frac{\lim_{r\uparrow k^*} \mathbb{E}[Y \mid k = r] - \lim_{r\downarrow k^*} \mathbb{E}[Y \mid k = r]}{\lim_{r\uparrow k^*} \mathbb{E}[G \mid k = r] - \lim_{r\downarrow k^*} \mathbb{E}[G \mid k = r]} = \mathbb{E}[Y_{S_1,1} - Y_{S_0,0} \mid T_g = C, k = k^*]$$

which is the expression in Equation 1.

C.2 Decomposition into subLATEs (Equation 3)

To derive Equation 3, breaking down (1) into subLATEs, we use the law of total expectation to write:

$$
\begin{aligned}
&\mathbb{E}[Y_{S_1,1} - Y_{S_0,0} \mid T_g = C, k = k^*] \\
&= \mathbb{E}[Y_{1,1} - Y_{S_0,0} \mid S_1 = 1, T_g = C, k = k^*] P(S_1 = 1 \mid T_g = C, k = k^*) \\
&\quad + \mathbb{E}[Y_{0,1} - Y_{S_0,0} \mid S_1 = 0, T_g = C, k = k^*] P(S_1 = 0 \mid T_g = C, k = k^*) \\
&= \mathbb{E}[Y_{1,1} - Y_{S_0,0} \mid S_1 = 1, T_g = C, k = k^*] P(S_1 = 1 \mid T_g = C, k = k^*) \\
&\quad + \mathbb{E}[Y_{0,1} - Y_{0,0} \mid S_1 = 0, T_g = C, k = k^*] P(S_1 = 0 \mid T_g = C, k = k^*) \\
&= \mathbb{E}[Y_{1,1} - Y_{S_0,0} \mid S_1 = 1, T_g = C, k = k^*] P(S_1 = 1 \mid T_g = C, k = k^*) \\
&= \mathbb{E}[Y_{1,1} - Y_{1,0} \mid S_1 = S_0 = 1, T_g = C, k = k^*] P(S_1 = S_0 = 1 \mid T_g = C, k = k^*) \\
&\quad + \mathbb{E}[Y_{1,1} - Y_0 \mid S_1 > S_0, T_g = C, k = k^*] P(S_1 > S_0 \mid T_g = C, k = k^*)
\end{aligned}
$$

which is Equation 3. The first equality above comes directly from the law of total expectation. The second equality comes from Assumptions (4) (Enrollment Monotonicity). The third equality comes from Assumption (3), which states that $Y_{0,1} = Y_{0,0} = 0$. The final equality comes from the law of total probability.

C.3 Fuzzy RD in Enrolled Data is Biased (Equation 4)

Consider the selected-data Wald estimand:

$$
\frac{\lim_{k\uparrow k^*} \mathbb{E}[Y \mid S = 1, k] - \lim_{r\downarrow k} \mathbb{E}[Y \mid S = 1, k]}{\lim_{k\uparrow k^*} \mathbb{E}[D \mid S = 1, k] - \lim_{k\downarrow k^*} \mathbb{E}[D \mid S = 1, k]}
$$

First, consider the terms in the numerator. Using Assumptions 1 and 4, we can expand the limit of $\mathbb{E}[Y \mid S = 1, k]$ approaching the cutoff from the left (eligible) side into mean potential outcomes for each complier stratum:

$$
\begin{aligned}
&\lim_{k\uparrow k^*}\mathbb{E}[Y \mid S=1,k] \\
&\quad= \mathbb{E}[Y_{1,1} \mid D_1 > D_0, S_1 = S_0 = 1, S=1, k=k^*]P(D_1 > D_0, S_1 = S_0 = 1 \mid k = k^{*-}, S=1) \\
&\qquad+ \mathbb{E}[Y_{1,1} \mid D_1 = D_0 = 1, S_1 = S_0 = 1, S=1, k=k^*]P(D_1 = D_0 = 1, S_1 = S_0 = 1 \mid k = k^{*-}, S=1) \\
&\qquad+ \mathbb{E}[Y_{1,1} \mid D_1 > D_0, S_1 > S_0, S=1, k=k^*]P(D_1 > D_0, S_1 > S_0 \mid k = k^{*-}, S=1) \\
&\qquad+ \mathbb{E}[Y_{1,1} \mid D_1 = D_0 = 1, S_1 > S_0, S=1, k=k^*]P(D_1 = D_0 = 1, S_1 > S_0 \mid k = k^{*-}, S=1) \\
&\qquad+ \mathbb{E}[Y_{1,0} \mid D_1 = D_0 = 0, S_1 = S_0 = 1, S=1, k=k^*]P(D_1 = D_0 = 0, S_1 = S_0 = 1 \mid k = k^{*-}, S=1)
\end{aligned} \tag{23}
$$

By a similar expansion, we can write $\lim_{k\downarrow k^*}\mathbb{E}[Y \mid S=1,k^*]$ as:

$$
\begin{aligned}
&\lim_{k\downarrow k^*}\mathbb{E}[Y \mid S=1,k] \\
&\quad= \mathbb{E}[Y_{1,0} \mid D_1 > D_0, S_1 = S_0 = 1, S=1, k=k^*]P(D_1 > D_0, S_1 = S_0 = 1 \mid k = k^{*+}, S=1) \\
&\qquad+ \mathbb{E}[Y_{1,1} \mid D_1 = D_0 = 1, S_1 = S_0 = 1, S=1, k=k^*]P(D_1 = D_0 = 1, S_1 = S_0 = 1 \mid k = k^{*+}, S=1) \\
&\qquad+ \mathbb{E}[Y_{1,1} \mid D_1 = D_0 = 1, S_1 > S_0, S=1, k=k^*]P(D_1 = D_0 = 1, S_1 > S_0 \mid k = k^{*+}, S=1) \\
&\qquad+ \mathbb{E}[Y_{1,0} \mid D_1 = D_0 = 0, S_1 = S_0 = 1, S=1, k=k^*]P(D_1 = D_0 = 0, S_1 = S_0 = 1 \mid k = k^{*+}, S=1)
\end{aligned} \tag{24}
$$

As discussed in Appendix D, the notation k^{*+} and k^{*-} indicates that limits of the sample shares in enrollment-selected data are not equal at the cutoff. The reason is that extensive-margin compliers are present on the left (eligible) side of the cutoff but not on the right. To relate these shares to each other, note that by Bayes' rule we can write:

$$
P(W \mid k = k^{*+}) = \frac{f_{S=1}(k^{*+} \mid W)P(W \mid S=1)}{f_{S=1}(k^{*+})} \qquad P(W \mid k = k^{*-}) = \frac{f_{S=1}(k^{*-} \mid W)P(W \mid S=1)}{f_{S=1}(k^{*-})}
$$

where $f_{S=1}(k)$ is the density of the running variable in enrollment-selected data, and where W denotes a complier stratum defined by values of D_1, D_0, S_1, and S_0. Rearranging gives:

$$
P(W \mid k = k^{*-}, S=1) = \frac{f_{S=1}(k^{*+})}{f_{S=1}(k^{*-})}P(W \mid k = k^{*+}, S=1) \tag{25}
$$

Using (25) to re-write (23) and subtracting (24) yields, after some algebra:

$$\begin{aligned}
&\lim_{k\uparrow k^*}\mathbb{E}[Y \mid S=1,k]-\lim_{k\downarrow k^*}\mathbb{E}[Y \mid S=1,k]\\
&=\underbrace{\mathbb{E}[Y_{1,1}-Y_{1,0} \mid D_1>D_0, S_1=S_0=1, S=1, k=k^*]}_{LATE_{IM}}\underbrace{P(D_1>D_0, S_1=S_0=1 \mid S=1, k=k^*)}_{\pi^-_{IM}}\\
&\quad+\mathbb{E}[Y_{1,1} \mid \underbrace{D_1>D_0, S_1>S_0}_{\text{EM Complier}}, S_1, k=k^*]\underbrace{P(D_1>D_0, S_1>S_0 \mid k=k^{*-}, S=1)}_{\pi^-_{EM}}\\
&\quad-\underbrace{\left(1-\frac{f_{S=1}(k^{*+})}{f_{S=1}(k^{*-})}\right)}_{(*)}\left(\lim_{k\downarrow k^*}\mathbb{E}[Y \mid S=1,k]\right)
\end{aligned}$$

In Appendix D.1.1 I prove that the starred term $(*)$ equals $\pi^-_{EM}=P(D_1>D_0, S_1>S_0 \mid k=k^{*-}, S=1)$. Using this result, the numerator becomes:

$$LATE_{IM}\pi^-_{IM}+\left(\mathbb{E}[Y_{1,1} \mid \text{EM Complier}]-\lim_{k\downarrow k^*}\mathbb{E}[Y \mid S=1,k]\right)\pi^-_{EM}$$

Now consider the denominator, $\lim_{k\uparrow k^*}\mathbb{E}[D \mid S=1,k]-\lim_{k\downarrow k^*}\mathbb{E}[D \mid S=1,k]$. Writing $\bar{D}^+$ and $\bar{D}^-$ as shorthand for these limits, note that we can write:

$$\bar{D}^+-\bar{D}^-=\underbrace{\left(\bar{D}^--\frac{f_{S=1}(k^{*+})}{f_{S=1}(k^{*-})}\bar{D}^-\right)}_{(\dagger)}-\underbrace{\left(1-\frac{f_{S=1}(k^{*+})}{f_{S=1}(k^{*-})}\right)}_{(*)}\bar{D}^+ \tag{26}$$

Appendix D.1 shows that the term $(\dagger)$ equals π^-_{IM}, and the term $(*)$ equals π^-_{EM}. As a result, we have:

$$\bar{D}^+-\bar{D}^-=\pi^-_{IM}-\pi^-_{EM}(1+\bar{D}^+) \tag{27}$$

Dividing (26) by (27) yields Equation (4) in the main text, completing the proof.

C.4 Decomposition of $LATE^*$ (Equation 7)

By Equation (1), we have $LATE=\mathbb{E}[Y_{S_1,1}-Y_{S_0,0} \mid D_1>D_0, k=k^*]$. We may expand this LATE into two cases, defined by whether students enroll in the treated state ($S_1=1$):

$$\begin{aligned}
LATE&=LATE^*\times P(S_1=1 \mid D_1>D_0, k=k^*)\\
&\quad+\underbrace{E[Y_{0,1}-Y_{0,0} \mid S_1=0, D_1>D_0, k=k^*]}_{(*)}P(S_1=0 \mid D_1>D_0, k=k^*)
\end{aligned}$$

where $LATE^* = \mathbb{E}[Y_{1,1} - Y_{S_0,0} \mid S_1 = 1, D_1 > D_0, k = k^*]$ is the local average treatment effect on "enrolled compliers": students who receive grants as a result of crossing the grant threshold, and who enroll in the treated state. The starred term $(*)$ is zero by Assumption 3, which states that grants only affect outcomes if students enroll. The decomposition in Equation (7) immediately follows by expanding $LATE^*$ into:

$$\begin{aligned} LATE^* &= \mathbb{E}[Y_{1,1} - Y_{1,0} \mid S_1 = S_0 = 1, D_1 > D_0, k = k] P(S_0 = 1 \mid S_1 = 1, D_1 > D_0, k = k^*) \\ &\quad + \mathbb{E}[Y_{1,1} - Y_0 \mid S_1 > S_0, D_1 > D_0, k = k] P(S_0 = 0 \mid S_1 = 1, D_1 > D_0, k = k^*) \\ &= LATE_{IM} \times (1 - \omega) + LATE_{EM} \times \omega \end{aligned}$$

D Identification with Selected Data

This appendix shows how to derive important quantities with data subject to sample selection. I first show point-identification of the following sample shares:

$$\begin{aligned} P(\text{Treated Complier} \mid S = 1, k = k^{*-}) &\equiv P(S_1 = 1, D_1 > D_0 \mid S = 1, k = k^{*-}) \\ P(\text{IM Complier} \mid S = 1, k = k^{*-}) &\equiv P(S_1 = S_0 = 1, D_1 > D_0 \mid S = 1, k = k^{*-}) \\ P(\text{EM Complier} \mid S = 1, k = k^{*-}) &\equiv P(S_1 > S_0, D_1 > D_0 \mid S = 1, k = k^{*-}) \end{aligned}$$

where the notation $k = k^{*-}$ denotes limits of the relevant quantity approaching the cutoff from the left. Then I show how to point-identify the following potential outcome means:

$$\begin{aligned} \mathbb{E}[Y_{1,1} \mid \text{Treated Complier}] &\equiv \mathbb{E}[Y_{1,1} \mid S_1 = 1, D_1 > D_0, k = k^*] \\ \mathbb{E}[Y_{1,0} \mid \text{IM Complier}] &\equiv \mathbb{E}[Y_{1,0} \mid S_1 = 1, S_0 = 1, D_1 > D_0, k = k^*] \end{aligned}$$

The selection problem is that the running variable is not available for the $S = 0$ sample. As a result, it is not possible to derive the full-sample analogues of sample shares. Instead, I show how to derive sample shares in the selected sample of four-year students immediately to the left of the cutoff, and show how the data are still sufficient to point-identify certain potential outcome means in the full sample.

The identification proofs in this section measure selection behavior using the densities of the running variable k in various samples. Throughout, I assume that the full-sample distribution of k, written $f(k)$, is continuous at the cutoff k^*. However, this full-sample distribution is not observed because data is missing for students who forego college. Instead, we observe the distribution of the running variable in selected samples:

$$f(k) = \underbrace{f_{S=1}(k)}_{\text{observed}} \times P(S=1) + \underbrace{f_0(k)}_{\text{observed}} \times P(S=0)$$

where $f_s(k) = f(k \mid S = s)$.

As a result of this selection problem, while Assumption (2) ensured continuity of potential outcomes and potential college choices in the full sample, this continuity does not necessarily hold in selected samples, as crossing the threshold leads some students to leave one sample and enter another. The proofs in this appendix leverage the fact that discontinuities in the densities of k selected samples in fact *measure* these selection responses to grant receipt. Throughout, I use the notation k^{*+} and k^{*-} to denote limits of the relevant quantity approaching the cutoff from the right and the left.

Some of these identification arguments extend proofs originally derived by Gerard et al. (2020), who derive bounds for treatment effects in regression discontinuity designs subject to manipulation. In contrast to their framework, which derives bounds for non-manipulated units in a setting where some units on the treated side of the cutoff are manipulated, my framework assumes that the full-sample distribution of the running variable is not manipulated, but uses a similar limiting-densities argument to address sample selection.[56]

D.1 Point-identification of population shares from densities

Identifying the mass of treated compliers

To identify $P(\text{Treated Complier} \mid S = 1, k = k^{*-}) \equiv P(S_1 = 1, D_1 > D_0 \mid S = 1, k = k^{*-})$, let $\bar{D}^+$ and $\bar{D}^-$ denote the share of treated students to the left and right of the cutoff in the enrolled sample:

$$\bar{D}^+ \equiv \lim_{k \downarrow k^*} \mathbb{E}[G \mid S = 1, Z = 0] = \mathbb{E}[G \mid S = 1, k = k^{*+}]$$
$$\bar{D}^- \equiv \lim_{k \uparrow k^*} \mathbb{E}[G \mid S = 1, Z = 1] = \mathbb{E}[G \mid S = 1, k = k^{*-}]$$

Let $f_{S=1}(k) = f(k \mid S = 1)$ denote the density of EFC conditional on enrollment. Let $f_{S=1}(k^{*-})$ and $f_{S=1}(k^{*+})$ denote the limits of this density approaching the cutoff from the left and right. To the right of the cutoff we have $D = D_0$, and to the left we have $D = D_1$.

[56] Here is a mapping between concepts in Gerard et al. (2020) (GRR) and this paper. The full sample in GRR is analogous to the $S = 1$ sample in my paper, and the $M = 1$ group in their paper corresponds to the extensive margin compliers ($S_1 > S_0, D_1 > D_0$) in this paper. Furthermore, in the notation of their paper, $\tau_0 = 0$ in my setting because there are no "manipulated" untreated units to the left of the cutoff; students only enter the enrolled sample as a result of being awarded a grant.

All grant recipients to the right of the cutoff are grant always-takers, allowing us to expand $\bar{D}^+$ as follows:

$$\begin{aligned}
\bar{D}^+ &\equiv \lim_{k \downarrow k^*} \mathbb{E}[D \mid S = 1] \\
&= P(D_0 = 1 \mid S = 1, k = k^{*+}) \\
&= P(D_0 = D_1 = 1 \mid S = 1, k = k^{*+}) \\
&= P(D_0 = D_1 = 1, S_1 = 1 \mid S = 1, k = k^{*+}) \\
&= \frac{f_{S=1}(k^{*+} \mid S_1 = 1, D_1 = D_0 = 1) P(S_1 = 1, D_1 = D_0 = 1 \mid S = 1)}{f_{S=1}(k^{*+})}
\end{aligned} \tag{28}$$

where the third equation comes from the grant monotonicity assumption, the fourth equation is by the law of total probability, and the fifth equation is by Bayes' theorem.

In contrast, grant recipients to the left of the cutoff are a mix of grant always-takers and grant compliers:

$$\begin{aligned}
\bar{D}^- &\equiv \lim_{k \uparrow k^*} \mathbb{E}[G \mid S = 1] \\
&= P(D_1 = 1 \mid S = 1, k = k^{*-}) \\
&= P(D_1 > D_0 \mid S = 1, k = k^{*-}) + P(D_1 = D_0 = 1 \mid S = 1, k = k^{*-}) \\
&= P(D_1 > D_0, S_1 = 1 \mid S = 1, k = k^{*-}) + P(D_1 = D_0 = 1, S_1 = 1 \mid S = 1, k = k^{*-}) \\
&= \frac{1}{f_{S=1}(k^{*-})} \Big(f_{S=1}(k^{*-} \mid S_1 = 1, D_1 > D_0) P(S_1 = 1, D_1 > D_0 \mid S = 1) \\
&\qquad\qquad + f_{S=1}(k^{*-} \mid S_1 = 1, D_1 = D_0 = 1) P(S_1 = 1, D_1 = D_0 = 1 \mid S = 1) \Big)
\end{aligned} \tag{29}$$

Combining and rearranging (28) and (29) yields:

$$\begin{aligned}
\bar{D}^- - \frac{f_{S=1}(k^{*+})}{f_{S=1}(k^{*-})} \bar{D}^+ &= \frac{f_{S=1}(k^{*-} \mid S_1 = 1, D_1 > D_0) P(S_1 = 1, D_1 > D_0 \mid S = 1)}{f_{S=1}(k^{*-})} \\
&= P(S_1 = 1, D_1 > D_0 \mid S = 1, k = k^{*-}) \\
&\equiv P(\text{Treated Complier} \mid S = 1, k = k^{*-})
\end{aligned} \tag{30}$$

where the first equation uses the fact that $f_{S=1}(k^{*-} \mid S_1 = 1, D_1 = D_0 = 1) = f_{S=1}(k^{*+} \mid S_1 = 1, D_1 = D_0 = 1)$,[57] and the second equation comes from Bayes' theorem.

[57] To see this, use Bayes' theorem to write:

$$\begin{aligned}
f_{S=1}(k^{*-} \mid S_1 = 1, D_1 = D_0 = 1) &= \frac{\overbrace{P(S = 1 \mid S_1 = 1, D_1 = D_0 = 1)}^{=1} \overbrace{f(k^{*-} \mid S_1 = 1, D_1 = D_0 = 1)}^{=f(k^* \mid S_1 = 1, D_1 = D_0 = 1)}}{P(S = 1 \mid S_1 = 1, D_1 = D_0 = 1)} \\
&= \frac{f(k^* \mid S_1 = 1, D_1 = D_0 = 1)}{P(S = 1 \mid S_1 = 1, D_1 = D_0 = 1)}
\end{aligned}$$

Equation (30) identifies the fraction of the four-year sample directly to the left of the cutoff who are grant compliers. We can expand this population into the two complier margins:

$$\begin{aligned} P(\text{Treated Complier} \mid S=1, k=k^{*-}) &\equiv P(S_1=1, D_1>D_0 \mid S=1, k=k^{*-}) \\ &= P(S_1=S_0=1, D_1>D_0 \mid S=1, k=k^{*-}) \\ &\quad + P(S_1>S_0, D_1>D_0 \mid S=1, k=k^{*-}) \\ &= P(\text{IM Complier} \mid S=1, k=k^{*-}) \\ &\quad + P(\text{EM Complier} \mid S=1, k=k^{*-}) \end{aligned} \tag{31}$$

D.1.1 Identifying the mass of extensive-margin compliers

To identify $P(\text{EM Complier} \mid S=1, k=k^{*-})$, note that under Assumptions 1 and 4, we can break the enrolled sample on the right (ineligible) side of the cutoff into four subpopulations defined by possible combinations of (S_0, S_1, D_0, D_1):[58]

$$\begin{aligned} f_{S=1}(k^{*+}) = &\overbrace{f_{S=1}(k^{*+} \mid S_1>S_0, D_1>D_0)}^{=0} P(S_1>S_0, D_1>D_0 \mid S=1) \\ &+ f_{S=1}(k^{*+} \mid S_0=S_1=1, D_1>D_0) P(S_1=S_0=1, D_1>D_0 \mid S=1) \\ &+ f_{S=1}(k^{*+} \mid S_0=S_1=1, D_1=D_0=0) P(S_0=S_1=1, D_1=D_0=0 \mid S=1) \\ &+ f_{S=1}(k^{*+} \mid S_1=1, D_1=D_0=1) P(D_1=D_0=1 \mid S=1) \end{aligned}$$

where $f_{S=1}(k^{*+} \mid S_1>S_0, D_1>D_0)=0$ because extensive-margin grant compliers only enter the four-year sample if they receive grants, which only happens if they are on the left of the cutoff.

where the second equation comes from Assumption (2), as $f(k^{*-} \mid S_1=1, D_1=D_0=1)$ is defined on the full sample, not on a selected sample. Finally, note that we would get the an equal result expanding $f_{S=1}(k^{*+} \mid S_1=1, D_1=D_0=1)$ the same way.

[58] They are:

- $D_1>D_0, S_1>S_0$: Extensive-margin grant compliers. These students receive a grant if and only if they cross the grant threshold ($D_1>D_0$), and attend four-year schools if and only if they receive a grant ($S_1>S_0$). For this reason, they appear only on the left side of the cutoff in the $S=1$ sample, not on the right.
- $D_1>D_0, S_0=S_1=1$: Intensive-margin grant compliers. These students do not receive a grant because they are on the right of the threshold, but would receive a grant if moved to the left of the threshold ($D_1>D_0$). However, they attend four-year college despite not being offered a grant ($S_0=S_1=1$).
- $D_1=D_0=0, S_0=S_1=1$: Intensive-margin grant never-takers. These students do not receive a grant regardless of whether they cross the threshold ($D_0=D_1=0$). However, they attend four-year college regardless of grant receipt ($S_1=S_0=1$).
- $D_1=D_0=1, S_1=1$: Intensive-margin grant always-takers. These students receive a grant despite being on the "wrong" side of the cutoff ($D_0=1$), and enroll in college ($S_1=1$). Note that this population can be broken down further into students with $S_0=1$ and students with $S_0\neq 1$, but this decomposition is not necessary for the proof.

The expansion of $f_{S=1}(k^{*-})$ incorporates extensive-margin grant compliers:

$$\begin{aligned} f_{S=1}(k^{*-}) = {} & f_{S=1}(k^{*-} \mid S_1 > S_0, D_1 > D_0) P(S_1 > S_0, D_1 > D_0 \mid S = 1) \\ & + f_{S=1}(k^{*-} \mid S_0 = S_1 = 1, D_1 > D_0) P(S_1 = S_0 = 1, D_1 > D_0 \mid S = 1) \\ & + f_{S=1}(k^{*-} \mid S_0 = S_1 = 1, D_1 = D_0 = 0) P(S_0 = S_1 = 1, D_1 = D_0 = 0 \mid S = 1) \\ & + f_{S=1}(k^{*-} \mid S_1 = 1, D_1 = D_0 = 1) P(D_1 = D_0 = 1 \mid S = 1) \end{aligned}$$

Letting W denote groups of students, note that $f(k^{*-} \mid W, S = 1) = f(k^{*+} \mid W, S = 1)$ for each of the principal strata $W \in \{(S_0 = S_1 = 1, D_1 > D_0), (S_0 = S_1 = 1, D_1 = D_0 = 0), (S_1 = 1, D_1 = D_0 = 1)\}$. To see this, note that by Bayes' theorem, we can write both $f(k^{*+})$ and $f(k^{*-})$ as:

$$f(k^{*(\text{sgn})} \mid W, S = 1) = \frac{\overbrace{P(S = 1 \mid W, k = k^{*(\text{sgn})})}^{=1} \overbrace{f(k^{*(\text{sgn})} \mid W)}^{=f(k^*|W)}}{P(S = 1 \mid W)} = \frac{f(k^* \mid W)}{P(S = 1 \mid W)}$$

which holds for both (sgn) $\in \{+, -\}$ and shows that $f(k^{*+} \mid W, S = 1) = f(k^{*-} \mid W, S = 1)$ for all $W \in \{(S_0 = S_1 = 1, D_1 > D_0), (S_0 = S_1 = 1, D_1 = D_0 = 0), (S_1 = 1, D_1 = D_0 = 1)\}$. The same equality does not hold for $W = (S_1 > S_0, D_1 > D_0)$, because $P(S = 1 \mid S_1 > S_0, D_1 > D_0, k = k^{*-}) = 0$ while $P(S = 1 \mid S_1 > S_0, D_1 > D_0, k = k^{*-}) = 1$ due to the fact that extensive-margin compliers appear only to the left of the cutoff.

As a result, differencing the conditional densities in the enrolled sample across the cutoff yields:

$$f_{S=1}(k^{*-}) - f_{S=1}(k^{*+}) = f_{S=1}(k^{*-} \mid S_1 > S_0, D_1 > D_0) P(S_1 > S_0, D_1 > D_0 \mid S = 1)$$

It follows from Bayes' theorem that:

$$\begin{aligned} \frac{f_{S=1}(k^{*-}) - f_{S=1}(k^{*+})}{f_{S=1}(k^{*-})} &= P(S_1 > S_0, D_1 > D_0 \mid S = 1, k = k^{*-}) \\ &= P\left(\text{EM Complier} \mid S = 1, k = k^{*-}\right) \end{aligned} \tag{32}$$

which identifies the mass of extensive-margin compliers in the enrolled sample to the left of the cutoff.

Identifying the mass of intensive-margin compliers

Combining (31) and (32) immediately yields the mass of intensive-margin compliers:

$$P(\text{IM Complier} \mid S=1, k=k^{*-}) = \underbrace{\left(\bar{D}^{-} - \frac{f_{S=1}(k^{*+})}{f_{S=1}(k^{*-})}\bar{D}^{+}\right)}_{\text{Treated Compliers}} - \underbrace{\frac{f_{S=1}(k^{*-}) - f_{S=1}(k^{*+})}{f_{S=1}(k^{*-})}}_{\text{EM Compliers}} \tag{33}$$

With the sample shares in hand, it remains to identify the counterfactual means.

D.2 Point-Identification of Counterfactual Means

We wish to identify:

- $\mathbb{E}[Y_{1,0} \mid S_1 = 1, S_0 = 1, D_1 > D_0, k = k^*]$
- $\mathbb{E}[Y_{1,1} \mid S_1 = 1, D_1 > D_0, k = k^*]$

Both sides of the cutoff contain grant-receiving and non-grant-receiving students ($D \in \{0, 1\}$). We therefore observe four conditional mean outcomes directly from the data:

$$\begin{aligned}
\bar{Y}_{1,1}^{+} &\equiv \lim_{k \downarrow k^*} \mathbb{E}[Y \mid S = 1, D = 1, Z = 0] \\
\bar{Y}_{1,0}^{+} &\equiv \lim_{k \downarrow k^*} \mathbb{E}[Y \mid S = 1, D = 0, Z = 0] \\
\bar{Y}_{1,1}^{-} &\equiv \lim_{k \uparrow k^*} \mathbb{E}[Y \mid S = 1, D = 1, Z = 1] \\
\bar{Y}_{1,0}^{-} &\equiv \lim_{k \uparrow k^*} \mathbb{E}[Y \mid S = 1, D = 0, Z = 1]
\end{aligned}$$

as well as the share of treated students to the left and right of the cutoff:

$$\begin{aligned}
\bar{D}^{+} &\equiv \lim_{k \downarrow k^*} \mathbb{E}[D \mid S = 1, Z = 0] = \mathbb{E}[D \mid S = 1, k = k^{*+}] \\
\bar{D}^{-} &\equiv \lim_{k \uparrow k^*} \mathbb{E}[D \mid S = 1, Z = 1] = \mathbb{E}[D \mid S = 1, k = k^{*-}]
\end{aligned}$$

and the limiting densities $f_{S=1}(k^{*+})$ and $f_{S=1}(k^{*+})$ of the running variable on either side of the cutoff.

First, note that grant monotonicity (Assumption 1) implies that crossing the grant threshold can only increase the probability that a student receives a grant. Thus, all of the units to the right of the threshold who receive grants ($D = 1, Z = 0$) are grant always-takers, and all of the units to the left of the threshold who do not receive grants ($D = 0, Z = 1$) are grant never-takers. Futhermore, any student who enrolls not receiving a grant ($S = 1, D = 0$) must have ($S_1 = S_0 = 1$) by Assumption 4. Therefore $\bar{Y}_{1,0}^{-}$ and $\bar{Y}_{1,1}^{+}$ identify potential outcomes for enrolled grant never-takers and enrolled grant always-takers:

$$\begin{aligned}
\bar{Y}_{1,0}^{-} &\equiv \lim_{k \uparrow k^*} \mathbb{E}[Y \mid S=1, D=0, Z=1] \\
&= \lim_{k \uparrow k^*} \mathbb{E}[Y_{1,0} \mid S=1, D=0, Z=1] \\
&= \lim_{k \uparrow k^*} \mathbb{E}[Y_{1,0} \mid S_0=1, D_1=0] \\
&= \mathbb{E}[Y_{1,0} \mid S_0=1, D_1=D_0=0, k=k^*] \qquad \text{(by continuity and grant monotonicity)} \\
&= \mathbb{E}[Y_{1,0} \mid S_0=S_1=1, D_1=D_0=0, k=k^*] \qquad \text{(by enrollment monotonicity)}
\end{aligned}$$

and:

$$\begin{aligned}
\bar{Y}_{1,1}^{+} &\equiv \lim_{k \downarrow k^*} \mathbb{E}[Y \mid S=1, D=1, Z=0] \\
&= \lim_{k \downarrow k^*} \mathbb{E}[Y_{1,1} \mid S=1, D=1, Z=0] \\
&= \lim_{k \downarrow k^*} \mathbb{E}[Y_{1,1} \mid S_1=1, D_0=1] \\
&= \mathbb{E}[Y_{1,1} \mid S_1=1, D_1=D_0=1, k=k^*] \qquad \text{(by continuity and grant monotonicity)}
\end{aligned}$$

In contrast, untreated mean outcomes for four-year students to the right of the cutoff are a weighted average across grant compliers and grant never-takers:

$$\begin{aligned}
\bar{Y}_{1,0}^{+} &\equiv \lim_{k \downarrow k^*} \mathbb{E}[Y \mid S=1, D=0, Z=0] \\
&= \lim_{k \downarrow k^*} \mathbb{E}[Y_{1,0} \mid S_0=1, D_0=0, Z=0] \\
&= \mathbb{E}[Y_{1,0} \mid S_0=S_1=1, D_0=D_1=0, k=k^*] P(D_1=0 \mid S_0=1, D_0=0, S=1, k=k^{*+}) \\
&\quad + \mathbb{E}[Y_{1,0} \mid S_0=S_1=1, D_1>D_0, k=k^*] P(D_1=1 \mid S_0=1, D_0=0, S=1, k=k^{*+}) \\
&= \bar{Y}_{1,0}^{-} P(D_1=D_0=0 \mid S_0=1, D_0=0, S=1, k=k^{*+}) \\
&\quad + \mathbb{E}[Y_{1,0} \mid S_0=S_1=1, D_1>D_0, k=k^*] P(D_1>D_0 \mid S_0=1, D_0=0, S=1, k=k^{*+}) \\
&= \bar{Y}_{1,0}^{-} (1 - P(D_1>D_0 \mid S_0=1, D_0=0, S=1, k=k^{*+})) \\
&\quad + \mathbb{E}[Y_{1,0} \mid S_0=S_1=1, D_1>D_0, k=k^*] P(D_1>D_0 \mid S_0=1, D_0=0, S=1, k=k^{*+})
\end{aligned}$$

where the fourth equation used grant monotonicity. This implies that untreated mean outcomes for intensive-margin grant compliers are:

$$\begin{aligned}
&\mathbb{E}[Y_{1,0} \mid S_0 = S_1 = 1, D_1 > D_0, k = k^*] \\
&\quad = \frac{\bar{Y}^+_{1,0} - \bar{Y}^-_{1,0}(1 - P(D_1 > D_0 \mid S_0 = 1, D_0 = 0, S = 1, k = k^*))}{P(D_1 > D_0 \mid S_0 = 1, D_0 = 0, S = 1, k = k^*)}
\end{aligned} \tag{34}$$

Similarly, treated mean outcomes to the left of the cutoff are a weighted average of grant compliers and grant always-takers:

$$\begin{aligned}
\bar{Y}^-_{1,1} &\equiv \lim_{k \uparrow k^*} \mathbb{E}[Y \mid S = 1, D = 1, Z = 1] \\
&= \lim_{k \uparrow k^*} \mathbb{E}[Y_{1,1} \mid S_1 = 1, D_1 = 1, Z = 1] \\
&= \mathbb{E}[Y_{1,1} \mid S_1 = 1, D_1 = D_0 = 1, k = k^*] P(D_0 = 1 \mid S_1 = 1, D_1 = 1, S = 1, k = k^*) \\
&\quad + \mathbb{E}[Y_{1,1} \mid S_1 = 1, D_1 > D_0, k = k^*] P(D_0 = 0 \mid S_1 = 1, D_1 = 1, S = 1, k = k^*) \\
&= \bar{Y}^+_{1,1}(1 - P(D_1 > D_0 \mid S_1 = 1, D_1 = 1, S = 1, k = k^*)) \\
&\quad + \mathbb{E}[Y_{1,1} \mid S_1 = 1, D_1 > D_0, k = k^*] P(D_1 > D_0 \mid S_1 = 1, D_1 = 1, S = 1, k = k^*)
\end{aligned}$$

Implying that treated mean outcomes for grant compliers are:

$$\begin{aligned}
&\mathbb{E}[Y_{1,1} \mid S_1 = 1, D_1 > D_0, k = k^*] \\
&\quad = \frac{\bar{Y}^-_{1,1} - \bar{Y}^+_{1,1}(1 - P(D_1 > D_0 \mid S_1 = 1, D_1 = 1, S = 1, k = k^*))}{P(D_1 > D_0 \mid S_1 = 1, D_1 = 1, S = 1, k = k^*)}
\end{aligned} \tag{35}$$

It remains to identify $P(D_1 > D_0 \mid S_0 = 1, D_0 = 0, k = k^*)$ and $P(D_1 > D_0 \mid S_1 = 1, D_1 = 1, S = 1, k = k^*)$. These probabilities are identified by the treated shares to either side of the cutoff $\bar{D}^+$ and $\bar{D}^-$, together with the limiting EFC densities $f(k^{*+})$ and $f(k^{*-})$. To identify $P(D_1 > D_0 \mid S_0 = 1, D_0 = 0, S = 1, k = k^{*+})$, note first that we can write:

$$\begin{aligned}
(1 - \bar{D}^+) &\equiv \lim_{k \downarrow k^*} \mathbb{E}[(1 - D) \mid S = 1] \\
&= P(D_0 = 0 \mid S = 1, k = k^{*+}) \\
&= P(D_1 > D_0 \mid S = 1, k = k^{*+}) + P(D_1 = D_0 = 0 \mid S = 1, k = k^{*+}) \\
&= P(D_1 > D_0, S_1 = S_0 = 1 \mid S = 1, k = k^{*+}) \\
&\quad + P(D_1 = D_0 = 0, S_1 = S_0 = 1 \mid S = 1, k = k^{*+}) \\
&= \frac{1}{f_{S=1}(k^{*+})} \Big(f_{S=1}(k^{*+} \mid S_1 = S_0 = 1, D_1 > D_0) P(S_1 = S_0 = 1, D_1 > D_0 \mid S = 1) \\
&\qquad + f_{S=1}(k^{*+} \mid S_1 = S_0 = 1, D_1 = D_0 = 0) P(S_1 = S_0 = 1, D_1 = D_0 = 0 \mid S = 1) \Big)
\end{aligned}$$

where the third equality comes from expanding into the $D_1 = 1$ and $D_1 = 0$ cases, and the fourth equality comes from recognizing that $D_0 = 0$ and $S = 1$ imply $S_0 = S_1 = 1$.[59] The fifth equality is a direct application of Bayes' theorem.

We can use an analogous argument to write:

$$(1 - \bar{D}^-) = \frac{1}{f_{S=1}(k^{*-})} f_{S=1}(k^{*-} \mid S_1 = S_0 = 1, D_1 = D_0 = 0) P(S_1 = S_0 = 1, D_1 = D_0 = 0 \mid S = 1)$$

Re-arranging these two results gives us:

$$\begin{aligned}
(1 - \bar{D}^+) - \frac{f_{S=1}(k^{*-})}{f_{S=1}(k^{*+})}(1 - \bar{D}^-) &= \frac{f_{S=1}(k^{*+} \mid S_1 = S_0 = 1, D_1 > D_0) P(S_1 = S_0 = 1, D_1 > D_0 \mid S = 1)}{f_{S=1}(k^{*+})} \\
&= P(S_1 = S_0 = 1, D_1 > D_0 \mid S = 1, k = k^{*+}) \\
&= P(S_1 = S_0 = 1, D_1 > D_0 \mid D_0 = 0, S = 1, k = k^{*+}) P(D_0 = 0 \mid S = 1, k = k^{*+}) \\
&\quad + \underbrace{P(S_1 = S_0 = 1, D_1 > D_0 \mid D_0 = 1, S = 1, k = k^{*+})}_{=0} P(D_0 = 1 \mid S = 1, k = k^{*+})
\end{aligned}$$

where the first equation comes from the fact that $f_{S=1}(k^{*-} \mid S_1 = S_0 = 1, D_1 = D_0 = 0) = f_{S=1}(k^{*+} \mid S_1 = S_0 = 1, D_1 = D_0 = 0)$ by Assumption 2. The second equation again comes from Bayes' theorem, and the third comes from the law of total probability. Further dividing both sides by $(1 - \bar{D}^+) = P(D_0 = 0 \mid S = 1, k = k^*)$ gives:

$$\begin{aligned}
1 - \frac{f_{S=1}(k^{*-})}{f_{S=1}(k^{*+})} \frac{(1 - \bar{D}^-)}{(1 - \bar{D}^+)} &= P(S_1 = S_0 = 1, D_1 > D_0 \mid D_0 = 0, S = 1, k = k^{*+}) \\
&= P(S_0 = 1, D_1 > D_0 \mid S_0 = 1, D_0 = 0, S = 1, k = k^{*+}) \\
&= P(D_1 > D_0 \mid S_0 = 1, D_0 = 0, S = 1, k = k^{*+})
\end{aligned}$$

where the second equality comes from Assumption 4 and from the fact that $(D_0 = 0, S = 1, k = k^{*+})$ imply $S_0 = 1$.

[59] To see this, expand one of the terms into the $S_0 = 1$ and $S_0 \neq 1$ cases:

$$P(D_1 > D_0 \mid S = 1, k = k^{*+}) = P(D_1 > D_0, S_0 = 1 \mid S = 1, k = k^{*+}) + \underbrace{P(D_1 > D_0, S_0 \neq 1 \mid S = 1, k = k^{*+})}_{=0}$$

where the second term is zero because ($k = k^{*+}$ implies $D = D_0$), ($D_1 > D_0$ implies $D_0 = 0$), and therefore $S = S_0 = 1$. A similar argument applies to the $P(D_1 = D_0 \mid S = 1, k = k^{*+})$ term.

The derivation of $P(D_0 = 0 \mid S_1 = 1, D_1 = 1, S = 1, k = k^{*-})$ proceeds similarly. Starting with Equation (30) we have:

$$\begin{aligned}
\bar{D}^- - \frac{f_{S=1}(k^{*+})}{f_{S=1}(k^{*-})}\bar{D}^+ &= P(D_1 > D_0, S_1 = 1 \mid S = 1, k = k^{*-}) \\
&= P(D_1 > D_0, S_1 = 1 \mid D_1 = 1, S = 1, k = k^{*-})P(D_1 = 1 \mid S = 1, k = k^{*-}) \\
&\quad + \underbrace{P(D_1 > D_0, S_1 = 1 \mid D_1 = 0, S = 1, k = k^{*-})}_{=0} P(D_1 = 0 \mid S = 1, k = k^{*-})
\end{aligned}$$

Dividing both sides by $\bar{D}^- = P(D_1 = 1 \mid S = 1, k = k^{*-})$ gives:

$$\begin{aligned}
1 - \frac{f_{S=1}(k^{*+})}{f_{S=1}(k^{*-})}\frac{\bar{D}^+}{\bar{D}^-} &= P(D_1 > D_0, S_1 = 1 \mid D_1 = 1, S = 1, k = k^{*-}) \\
&= P(D_1 > D_0, S_1 = 1 \mid S_1 = 1, D_1 = 1, S = 1, k = k^{*-}) \\
&= P(D_1 > D_0 \mid S_1 = 1, D_1 = 1, S = 1, k = k^{*-})
\end{aligned}$$

where the second equation comes from the fact that $(D_1 = 1, S = 1, k = k^{*-})$ imply $S_1 = 1$.

Armed with expressions for $P(D_1 > D_0 \mid S_0 = 1, D_0 = 0, S = 1, k = k^{*+})$ and $P(D_1 > D_0 \mid S_1 = 1, D_1 = 1, S = 1, k = k^{*-})$ in terms of observable quantities, we can now write Equations (34) and (35) as:

$$\begin{aligned}
\mathbb{E}[Y_{1,0} \mid S_0 = S_1 = 1, D_1 > D_0, k = k^*] &= \frac{\bar{Y}^+_{1,0} - \bar{Y}^-_{1,0}\kappa_0}{1 - \kappa_0} \\
\mathbb{E}[Y_{1,1} \mid S_1 = 1, D_1 > D_0, k = k^*] &= \frac{\bar{Y}^-_{1,1} - \bar{Y}^+_{1,1}\kappa_1}{1 - \kappa_1}
\end{aligned}$$

where:

$$\kappa_0 = \frac{f_{S=1}(k^{*-})}{f_{S=1}(k^{*+})}\frac{(1 - \bar{D}^-)}{(1 - \bar{D}^+)} \qquad\qquad \kappa_1 = \frac{f_{S=1}(k^{*+})}{f_{S=1}(k^{*-})}\frac{\bar{D}^+}{\bar{D}^-}$$

which completes the proof.

E Derivation of Marginal Cost and Benefit Formulas

Start with the definitions in Section 9.1:

$$B = \sum_{t=1}^{T} \beta^{t-1} \mathbb{E}[\text{Grants}_t + \text{Loans}_t + (1-\tau)\text{Earnings}_t - \mathbf{1}\{\text{Enrolled}\}_t \times \delta_{\text{tuition}}]$$

$$C = \sum_{t=1}^{T} \beta^{t-1} \mathbb{E}[\text{Grants}_t + \text{Loans}_t - \tau\text{Earnings}_t + \mathbf{1}\{\text{Enrolled}\}_t \times \delta_{\text{instruction}}]$$

Taking $\partial B/\partial k^*$ or $\partial C/\partial k^*$ boils down to deriving $\partial \mathbb{E}[Y_t]/\partial k^*$, where Y_t is an outcome in $\{\text{Grants}_t, \text{Loans}_t, \text{Earnings}_t, \mathbf{1}\{\text{Enrolled}\}_t\}$. To take this derivative, note first that $\mathbb{E}[Y_t]$ can be written:

$$\begin{aligned}
\mathbb{E}[Y_t] = \mathbb{E}[Y_t(S(D(Z)), D(Z))] &= \int_{\underline{k}}^{\bar{k}} \mathbb{E}[Y_t(S(D(Z)), D(Z)) \mid k] f(k) dk \\
&= \int_{\underline{k}}^{k^*} \mathbb{E}[Y_t(S(D_1), D_1) \mid k] f(k) dk + \int_{k^*}^{\bar{k}} \mathbb{E}[Y_t(S(D_0), D_0) \mid k] f(k) dk
\end{aligned}$$

where the last equality comes from the law of iterated expectations. It follows by the Fundamental Theorem of Calculus that:

$$\begin{aligned}
\frac{\partial \mathbb{E}[Y_t]}{\partial k^*} &= \mathbb{E}[Y_t(S(D_1), D_1) - Y_t(S(D_0), D_0) \mid k^*] f(k^*) \\
&= \mathbb{E}[Y_t(S(D_1), D_1) - Y_t(S(D_0), D_0) \mid D_1 > D_0, k^*] P(D_1 > D_0 \mid k^*) f(k^*) \\
&= \underbrace{\mathbb{E}[Y_t(S_1, 1) - Y_t(S_0, 0) \mid D_1 > D_0, k^*]}_{LATE} \cdot P(D_1 > D_0 \mid k^*) \cdot f(k^*) \\
&= \underbrace{\mathbb{E}[Y_t(S_1, 1) - Y_t(S_0, 0) \mid S_1 = 1, D_1 > D_0, k^*]}_{LATE^*} \cdot P(S_1 = 1, D_1 > D_0 \mid k^*) \cdot f(k^*)
\end{aligned}$$

where the second equality comes from the fact that the difference inside the expectation can only be nonzero if $D_1 > D_0$, and where the third equality comes from Assumption 1 (Grant Monotonicity). The fourth equality comes from the law of total expectation and from Assumptions 3 and 4 (Partial Exclusion and Enrollment Monotonicity), which require that if $S_1 = 0$ then $S_0 = 0$, implying $Y_{0,1} = Y_{0,0} = Y_0$, so that the LATE for units with $S_1 = 0$ is zero.

Applying this result to each outcome and plugging the result back into the expression for B gives:

$$\frac{\partial B}{\partial k^*} = \Bigg[\sum_{t=1}^{T} \beta^{t-1} \Big(LATE^*(\text{Grants}_t) + LATE^*(\text{Loans}_t) + (1-\tau) LATE^*(\text{Earnings}_t) + \delta_{\text{tuition}} \times LATE^*(\mathbf{1}\{\text{Enrolled}\}_t)\Big)\Bigg] \times P(S_1 = 1, D_1 > D_0 \mid k^*) f(k^*)$$

Now consider the enrollment term. At $t = 1$, the enrollment indicator is equal to S, the student's initial enrollment choice. We can expand $LATE^*(\text{Enrolled}_t)$ into:

$$LATE^*(\text{Enrolled}_t) = (1-\omega) LATE_{IM}(\text{Enrolled}_t) + \omega LATE_{EM}(\text{Enrolled}_t)$$

where $\omega = P(S_1 > S_0 \mid S_1 = 1, D_1 > D_0, k = k^*)$ is the share of extensive-margin compliers among all enrolled compliers. At $t = 1$, we have by definition that $LATE_{IM}(\text{Enrolled}_1) = 0$ and $LATE_{EM}(\text{Enrolled}_1) = 1$, because intensive-margin compliers are students who would have enrolled in college regardless of treatment and extensive-margin compliers are brought into initial college enrollment by the treatment. This allows us to finally write:

$$\frac{\partial B}{\partial k^*} = \Bigg[\sum_{t=1}^{T} \beta^{t-1} \left(LATE^*(\text{Grants}_t) + LATE^*(\text{Loans}_t) + (1-\tau) LATE^*(\text{Earnings}_t)\right) + \left(\omega + \sum_{t=2}^{T} \beta^{t-1} LATE^*(\mathbf{1}\{\text{Enrolled}\}_t)\right) \times \delta_{\text{tuition}}\Bigg] \times P(S_1 = 1, D_1 > D_0 \mid k^*) f(k^*)$$

The proof for $\partial C / \partial k^*$ proceeds similarly.