Community Colleges and Upward Mobility

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Abstract
Two-year community colleges enroll nearly half of all first-time undergraduates, but to ambiguous effect: low persistence rates and the potential for diverting students from 4-year institutions cast ambiguity over 2-year colleges’ contributions to upward mobility. Developing a new multivariate instrumental variables approach applied to linked administrative education and earnings records, I disentangle the net impacts of 2-year college access into two competing causal margins: significant value-added for 2-year entrants who otherwise would not have attended college, but negative impacts on students diverted from 4-year entry. 2-year access particularly boosts the upward mobility of disadvantaged students, who experience less 4-year diversion.

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1 Introduction

Since 1980, the earnings gap between college and high school graduates has roughly doubled in the United States (Autor, 2014). Rising demand for skilled labor has out-paced modest growth in the supply of college-educated workers (Katz and Murphy, 1992; Goldin and Katz, 2008; Acemoglu and Autor, 2011), and this modest growth has been uneven: children from high-income families are more likely to enroll in and complete college than their low-income peers (Chetty et al., 2014), and this college gradient in family income has steepened since 1980 (Belley and Lochner, 2007; Bailey and Dynarski, 2011).

In response to these trends, a recent wave of policies aimed at broadening college access have focused on 2-year community colleges, which enroll nearly half of all college entrants and a disproportionate share of low-income students, as key arteries in increasing the flow of young Americans into higher education.\(^1\) Several states and major cities have launched free 2-year college tuition programs since 2014, with many more considering similar legislation (National Conference of State Legislatures, 2016), all in the hope that expanding access to 2-year colleges will help extend the prospects of postsecondary attainment and upward mobility to a broader share of young Americans.

Even if expanding 2-year access succeeds in increasing enrollment in 2-year colleges, however, causal evidence on the efficacy of 2-year colleges in ultimately boosting educational attainment and earnings is limited (Belfield and Bailey, 2011, 2017; Denning, 2017), and greater 2-year access may even detrimentally divert college-bound students from direct entry into higher-resourced 4-year institutions (Clark, 1960; Brint and Karabel, 1989; Rouse, 1995). Concerns stem from low observed rates of degree completion and upward transfer among 2-year entrants: while 81 percent begin with the intention to earn a bachelor’s degree, only 33 percent actually transfer up to a 4-year institution within six years, and just 14 percent complete the bachelor’s degree (Jenkins and Fink, 2016). In contrast, 60 percent of students who begin college directly at a 4-year institution complete a bachelor’s degree over the same timeframe, and sub-

\(^1\)Following Cohen et al. (2014), I refer to community colleges, 2-year colleges, and junior colleges interchangeably as accredited non-profit institutions that award associate’s degrees as their highest credential.
stantial outcome gaps between 2-year and 4-year entrants remain after adjusting for observable differences in test scores and demographic backgrounds (Reynolds, 2012). To the extent that these gaps reflect causal impacts of beginning college at a 2-year versus 4-year institution, the potential for 4-to-2 diversion casts further ambiguity over the role of 2-year colleges in promoting upward mobility.

This paper develops new econometric tools and marshals linked administrative data to explore the consequences of expanding access to 2-year colleges on student outcomes. Does 2-year access boost educational attainment and earnings, on net? Are some students diverted from 4-year entry, with detrimental effects? How large are the gains, if any, among 2-year entrants who otherwise would not have attended any college? To answer these questions, I develop a new approach to using multiple instrumental variables that nonparametrically identifies causal effects along multiple margins of treatment. Applied to the 2-year college setting, this new approach unlocks a decomposition of the overall net effects of 2-year access into two distinct and potentially opposing treatment margins: causal value-added for new 2-year entrants who otherwise would have not enrolled in any college, versus causal diversion impacts on new 2-year entrants who otherwise would have started directly at a 4-year institution.²

Methodologically, this new approach overcomes a challenge in the instrumental variables (IV) literature: standard IV methods like two-stage least squares (2SLS) do not generally recover causal effects of one alternative versus another when individuals face multiple treatment alternatives and reap heterogeneous treatment effects (Heckman and Urzua, 2010), even when researchers have access to as many instruments as treatment margins (Kirkeboen et al., 2016). In contrast, I show how a nonparametric separate identification approach, which studies instrument-induced variation in outcome-by-treatment interactions, can isolate the mean potential outcomes of instrument compliers along each distinct treatment margin, and differencing these mean potential outcomes delivers margin-specific treatment effects.³ This generalizes the binary

²This paper focuses on identifying and estimating ex post treatment effects. See Cunha et al. (2005) and Arcidiacono et al. (2014) for examples of methods to recover ex ante effects of schooling choices, as perceived by students at the point of decision, through additional data on student expectations and/or structural modeling of choice behavior. See also Manski (1993) and Dominitz and Manski (1996) for discussions of student expectation formation.

³In fact, the method can recover any features of the marginal distributions of complier potential outcomes, so estimating quantile treatment effects, for example, remains an avenue for future work.
complier-describing logic of Imbens and Rubin (1997), Abadie (2002), and Carneiro and Lee (2009) to multiple margins of treatment and instrument compliance: the data do not directly reveal which individuals are compliers, nor the treatment margin along which they would comply, but the distributional characteristics of each complier group emerge when studying compositional changes in each treatment group driven by specific instruments. Moving beyond local treatment effects, instruments that take on a range of values enable identification of causal effects for a wider range of compliers, as in the binary treatment IV setting (Heckman et al., 2010; Angrist and Fernández-Val, 2013; Brinch et al., 2017). This enables tests for different types of selection behavior, like Roy (1951)-style sorting on comparative advantage, and helps probe the external validity of the local estimates, including how marginal returns evolve as 2-year access further expands and draws deeper into the population of potential entrants.

I implement the method on longitudinal administrative data spanning the state of Texas. I individually link the population of Texas public high school students with enrollment and degree completion records at all public and private Texas colleges and universities, then further link these students with quarterly earnings records for all Texas employees from the state unemployment insurance administration. These data are unusual in the U.S. context in their combination of breadth and depth of coverage, spanning the population of the second largest state (comprising 10.5% of all public K-12 students in the U.S.) and providing detailed information on demographics, test scores, college enrollment dynamics, degree completion, and longitudinal earnings.

Linking the student-level microdata with annual geospatial measures of all high school and college locations in Texas, I identify causal effects along the multiple margins of 2-year enrollment with instrumental variation in students’ proximities to local 2-year and 4-year college campuses. Departing from most papers in the returns-to-schooling canon that employ distance instruments, I control directly for detailed neighborhood-level measures of urbanization, as well as commuting zone fixed effects, to ensure that

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Footnotes:

4. Kline and Walters (2016) consider the case of multiple treatments but only one binary instrument, showing nonparametric identification of certain complier characteristics. This paper shows how additional instruments can secure nonparametric identification along all relevant complier margins and thus yield margin-specific treatment effects.

distance comparisons come only from students who grow up in similar neighborhoods and face similar local labor markets when deciding whether and where to begin college. I also do not rely on variation in a student’s distance to any type of college; instead, identification comes from conditional variation in 2-year distance while holding 4-year distance fixed, and likewise 4-year distance while holding 2-year distance fixed. Encouragingly, I show that these two dimensions of conditional instrumental variation are empirically balanced across excluded student ability measures that strongly predict college choices and outcomes. These balanced instruments yield causal estimates that are unaffected by the inclusion of ability measures as controls, providing evidence against the concern that the IV results are spuriously driven by families with different levels of human capital systematically sorting into neighborhoods with different residual proximities to 2-year and 4-year colleges.

The results of this IV approach offer four main conclusions. First, greater access to 2-year colleges boosts educational attainment and earnings on net. Second, these net effects shroud opposing impacts along the two distinct treatment margins: roughly one third of induced 2-year entrants are diverted from 4-year entry and ultimately complete less education as a result, empirically confirming concerns over the diversion channel but with smaller magnitudes than OLS regressions would suggest. The other two thirds of induced 2-year entrants would not have otherwise attended college, and they reap significant gains in educational attainment and earnings. Third, stratifying by demographics reveals that women drive these results with effects of larger magnitude along both margins compared to men, while 2-year access particularly boosts the upward earnings mobility of students from low-income families thanks to their lower likelihood of diversion from 4-year entry. Finally, stratifying the local IV estimates across the range of 2-year college proximity suggests that causal impacts on marginal students do not diminish as 2-year access further expands and draws deeper into the population of potential entrants.6

Taken together, these results contribute clarifying empirical evidence in light of a

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6If anything, the net effects increase slightly over the observed support of 2-year proximity, implying a reverse-Roy selection pattern of higher gains accruing to those students who are less likely to enroll, but this result is not statistically precise. See Aakvik et al. (2005), Walters (2017), Kline and Walters (2016), and Cornelissen et al. (2017) for examples of reverse-Roy selection patterns into educational programs.
growing policy movement to broadly expand access to 2-year community colleges. Since the Tennessee Promise program launched in 2014, offering recent high school graduates free tuition at all 2-year community colleges in the state, Oregon, Minnesota, Kentucky, and Rhode Island have implemented similar programs, and at least ten more states are considering similar legislation. Several major metropolitan governments have launched analogous local programs, including the Chicago STAR Scholarship (launched in 2014), San Francisco’s Free City program (2017), and the Boston Bridge program (2017). The highest-profile policy of all, the Obama administration’s 2015 proposal to make two years of community college free across the country, has not yet advanced at the national level, but it continues to catalyze state and local programs and may become a component of the Democratic National Committee policy platform (Donnis, 2017). The results of this paper suggest that such universal expansions of 2-year college access may boost upward mobility among the subset of students “democratized” into higher education from non-attendance, but more targeted policies that avoid diverting college-bound students from 4-year entry may generate larger net benefits.

The empirical results build on an interdisciplinary literature studying the outcomes of 2-year college students, reviewed by Kane and Rouse (1999) and Belfield and Bailey (2011, 2017). Most of this literature relies on selection-on-observables assumptions to interpret OLS and matching results as causal. A small but growing set of papers relax this assumption in panel specifications with individual fixed effects (e.g. Jacobson et al., 2005; Jepsen et al., 2014), but this approach necessarily focuses on older workers who have accumulated pre-enrollment earnings histories. A handful of recent papers exploit natural experiments that directly or indirectly influence 2-year college enrollment (Denning, 2017; Zimmerman, 2014; Goodman et al., 2017), but these papers do...
not attempt to separately identify causal effects along each distinct treatment margin.

In estimating causal impacts of 2-year college entry along both the “democratization” and “diversion” margins through the use of multiple instruments, this paper advances the related work of Rouse (1995, 1998) and Miller (2007) by relaxing the implicit assumption of homogeneous treatment effects embedded in their multivariate two-stage least squares (2SLS) specifications. As shown by Kirkeboen et al. (2016), multivariate 2SLS estimands generally mix comparisons across multiple treatment margins and complier groups, making multivariate 2SLS estimates difficult to interpret in the general case where treatment effects vary across individuals along unobservable dimensions.\(^{10}\) A rapidly growing literature suggests that homogeneous treatment effects are the exception rather than the rule across a wide range of educational settings,\(^{11}\) which motivates the development of this paper’s generalized nonparametric identification approach.

Methodologically, the identification results contribute to the IV literature on identifying causal effects of multivalued treatments. Heckman and Urzua (2010) discuss the identification challenges inherent in settings with multiple margins of treatment, highlighting how individuals induced into a specific treatment by instrumental variation can come from multiple economically distinct alternatives. A small set of papers have developed certain conditions under which margin-specific treatment effects can be identified, including parametric restrictions on unobserved heterogeneity (Heckman and Vytlacil, 2007; Feller et al., 2016; Hull, 2018a); full-support instruments that permit a multidimensional version of identification at infinity (Heckman et al., 2008); homogeneity in treatment effects or selection behavior across observable stratification groups (Hull, 2018b; Kline and Walters, 2016); and explicit data on individual preference rankings over alternatives (Kirkeboen et al., 2016). This paper’s nonparametric

\(^{10}\) In Appendix B, I derive and decompose the multivariate 2SLS estimands corresponding to my setting, showing how they fuse multiple treatment margins and complier types into each coefficient. Kline and Walters (2016) and Hull (2018b) derive related results in the 2SLS case where a single instrument is interacted with a stratifying covariate to generate another dimension of instrumental variation. See also Pinto (2016) for a related discussion in the context of the multiple treatment arms of the Moving to Opportunity experiment.

separate identification approach with multiple instruments allows for the relaxation of many of these conditions, complementing recent work by Lee and Salanie (2018), who study identification in the context of multinomial selection models, and Heckman and Pinto (2018), who study monotonicity in unordered discrete choice models with discrete instruments.

Finally, while the institutional focus of this paper lies with higher education in the United States, the methodology developed to identify causal effects of multivalued treatments could apply to a broad range of settings. The 2-year and 4-year college distance instruments have a general interpretation as prices or cost shifters, suggesting parallels to other multivalued treatment choices that depend on initial costs, including hospital admission, insurance enrollment, occupational choice, migration, K-12 school choice, and firm location decisions, among others. This method also enhances program evaluation in settings with crowd-out or substitution bias (Heckman et al., 2000), since the task of evaluating a policy or program with readily-available substitutes—here the encouragement of 2-year enrollment in the presence of the 4-year alternative—is aided by the ability to decompose net policy impacts into distinct effects among individuals who would otherwise go untreated (no college) versus those who would have simply obtained the substitute (4-year entry).  

The remainder of the paper proceeds as follows. Section 2 provides institutional background on the American community college. Section 3 describes the linked administrative data and presents descriptive results on initial enrollment choices and outcomes. Section 4 discusses the identification challenges posed by multiple treatment margins and develops the nonparametric separate identification approach. Section 5 discusses estimation and conducts diagnostics on the instruments. Section 6 presents the empirical results. Section 7 concludes.

## 2 Institutional Background

Two-year community colleges straddle a complicated space in American higher education. From the emergence of the first “junior colleges” at the dawn of the 20th century,

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12See Kline and Walters (2016) for a parallel setting evaluating the impacts of Head Start.
through their explosive mid-century growth and modern stabilization at roughly one thousand campuses across all fifty states, debate over the proper role of this “contradictory college” (Dougherty, 1994) has continued apace, centering around three interrelated questions. The oldest, and largely resolved, question from the primordial period at the turn of the 20th century was whether 4-year universities should spin off their first two years of teaching to these emerging junior colleges, allowing 2-year college faculty to specialize in undergraduate instruction while freeing up university faculty and resources to focus on the “higher” academic pursuits of research and graduate training. This sharp bifurcation into separate junior and senior institutions, advocated by the University of Chicago’s William Rainey Harper, Stanford’s David Starr Jordan, and several other prominent university presidents at the time (Cohen et al., 2014), never materialized on a large scale in the U.S., as the vast majority of colleges and universities that offer bachelor’s degrees have maintained their common model of four continuous undergraduate years.\(^{13}\)

The resulting functional overlap between 2-year and 4-year colleges helped fuel debate over a second question: should 2-year colleges continue to prepare students for 4-year transfer through academic coursework, or should they differentiate themselves from 4-year institutions by focusing on terminal vocational training to prepare students for workforce entry? The academic transfer function remained the core mission of 2-year colleges from their inception through the mid-20th century, despite persistent efforts from the leadership of the American Association of Junior Colleges to carve out a clear niche for 2-year colleges by providing vocational training (Brint and Karabel, 1989; Cohen et al., 2014). A confluence of events in the 1960s and 1970s finally brought vocational education to the fore, including the federal Vocational Education Act of 1963, billions of dollars of subsequent vocational program funding championed by the Nixon administration, the early 1970s downturn in the wage premium to bachelor’s

\(^{13}\)William Rainey Harper did manage to separate the undergraduate experience at the nascent University of Chicago into a Junior College and a Senior College, and even pioneered the American associate’s degree as an award to students who completed the two-year Junior College curriculum (Brint and Karabel, 1989). But while separate 2-year colleges did emerge and grow dramatically over the 20th century, Harper’s own bifurcation of the University of Chicago never resulted in two standalone institutions, much to the grumbling of many senior faculty members whom Harper recruited to the new university with assurances that there would be no need to teach lower-division undergraduates (Boyer, 2015).
degrees, several reports from the influential Carnegie Commission on Higher Education advocating more vocational emphasis at 2-year colleges, and the shift in terminology from “junior” to “community” college, shedding the former connotation of subordination to 4-year institutions in favor of responsiveness to community needs, including occupational education suited to local industry demand. These forces coincided with a rise in the share of 2-year college students pursuing vocational instead of academic programs, from less than a third in 1970 to more than half in 1977 (Blackstone, 1978), and settling at rough parity today (Cohen et al., 2014).

The rise of vocational education at 2-year colleges has only intensified debate over a final question, on which this paper focuses: do 2-year colleges boost the upward mobility of individuals who otherwise would not participate in higher education, or do they mainly divert college-bound students away from 4-year institutions, perhaps to their own detriment? The early champions of the community college movement focused almost exclusively on the “democratization” effect along the extensive margin, viewing 2-year college accessibility as a cornerstone in building a higher education system that offered equal opportunity to Americans from all backgrounds (Eells, 1931; Koos, 1944). In more recent decades, concerns over the diversion channel have grown more prominent in the academic literature, with Brint and Karabel (1989) arguing in an influential book that diversion is actually the dominant function of modern 2-year colleges, and others like Grubb (1989) and Dougherty (1994) noting that both of these margins are likely at play as simultaneous features of the “contradictory” community college.

With nearly 10 million community college students in the United States annually generating over 50 billion dollars in costs, over 70 percent of which are subsidized by local, state, and federal taxpayers (National Center for Education Statistics, 2015), building a clear understanding of democratization, diversion, and their impacts on student outcomes is vital in evaluating a wide range of higher education policies that influence student decisions on whether and where to enroll in college. The recent wave of policies promoting and subsidizing 2-year college enrollment discussed in the Introduction, atop a century of controversy over the role of community colleges in American social mobility, motivates the analysis of this paper.

14 Freeman (1976); Brint and Karabel (1989); Cohen et al. (2014).
3 Data and Descriptive Results

3.1 Data Sources

My empirical analysis combines several restricted administrative datasets spanning the state of Texas. As the second largest U.S. state by population, land area, and GDP, Texas comprises 8.5 percent of the U.S. population and educates 10.5 percent of U.S. public K-12 students, and the Texas economy would rank 11th largest in the world as a sovereign nation. This large populace supports a comprehensive statewide system of higher education, with 37 public 4-year universities enrolling 637,000 students, 38 private 4-year colleges and universities enrolling 124,000 students, and 57 public 2-year community college districts enrolling 732,000 students (National Center for Education Statistics, 2015).

The analysis sample begins with student-level data from the Texas Education Agency (TEA) covering the population of Texas public high school students. I link these students to administrative records from the Texas Higher Education Coordinating Board (THECB), capturing all enrollments and degrees at all public and private Texas colleges and universities. I further link these students to individual quarterly earnings records from the Texas Workforce Commission (TWC), measuring total earnings at each job each quarter for all Texas employees subject to the state unemployment insurance system. I complement these administrative student-level records with several auxiliary school- and neighborhood-level data sources: high school characteristics from the National Center for Education Statistics (NCES) Common Core of Data, college characteristics from the Integrated Postsecondary Education Data System (IPEDS), and neighborhood characteristics from the 2000 Decennial Census measured at the tract level.

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15 Private high school students, who are not observed in this data, account for less than 5 percent of all Texas high school graduates (National Center for Education Statistics, 2015).
16 I do not observe for-profit college enrollments. In the fall of 2004, when the last of my main analysis cohorts begin to enter college, the for-profit share of enrollment at all degree-granting postsecondary institutions is only 5.1 percent (National Center for Education Statistics, 2015).
17 Excluded from the state UI system are the self-employed, independent contractors, military personnel, some federal employees, and workers in the informal sector. Stevens (2007) estimates that roughly 90 percent of the civilian labor force is captured in state UI records.
18 Census tracts delineate neighborhoods of roughly 1,200 to 8,000 people, averaging around 4,000.
One obvious limitation of any administrative data from a particular state is attrition due to outmigration. In my setting, college enrollments and earnings of Texas high school students who leave the state will not be observed. Fortunately, Texas has the lowest outmigration rate of any U.S. state, with 82 percent of all Texas-born individuals remaining in Texas as of 2012 (Aisch et al., 2014). On the college enrollment front, National Student Clearinghouse (NSC) records are available for a subset of my sample—students who graduate from high school in 2008 and 2009—allowing me to study college enrollment patterns inclusive of the small fraction of Texas high school students who do attend college out-of-state. On the earnings front, missing earnings values could represent either nonemployment or outmigration; I show below that students with non-missing earnings look nearly identical to those with missing earnings in terms of observable characteristics, suggesting that the scope for sample selection bias may be limited.

3.2 Variable Definitions

Cohorts.—The main analysis sample consists of five cohorts of Texas public high school students enrolled in 10th grade between 1998 and 2002. I will hereafter refer to their projected high school graduation years of 2000 through 2004. 2000 is the oldest cohort for which private Texas college enrollments can be observed, and 2004 is the last cohort of the Texas Assessment of Academic Skills (TAAS) testing regime, with substantial changes to the testing structure thereafter. I also make separate use of the 2008 and 2009 cohorts in the descriptive results, leveraging their National Student Clearinghouse coverage to show college enrollment patterns inclusive of students who attend out-of-state.

Covariates.—Student-level demographics are measured in the 10th grade TEA en-
rollment files and include categorical variables for gender, race/ethnicity, and eligibility for free or reduced price lunch, a proxy for economic disadvantage. To obtain a single test score measure for each student, I combine raw 10th grade math and reading scores in a one-factor model separately by cohort, then normalize this factor to within-cohort percentiles. High school-level controls are measured in the NCES Common Core of Data and include the share of students eligible for free/reduced price lunch, the NCES geographic locale code, which measures local urbanization in twelve detailed categories based on Census geospatial data, and a county variable, which I group into the 62 Texas commuting zones using the year-2000 mapping provided by the U.S. Department of Agriculture’s Economic Research Service. To control for any local influences of the oil and gas industry in Texas, I also measure the long-run share of oil and gas employment at the high school level using NAICS industry codes in the TWC workforce data. Finally, I construct an index of neighborhood quality similar to the test score measure: I combine the tract-level Census measures of median household income and percent of households under the poverty line with the high school-level percent eligible for free/reduced price lunch into a one-factor model, then normalize this neighborhood factor to within-cohort percentiles.

Treatments.—The three mutually exclusive and exhaustive treatments of interest are starting at a 2-year college, starting at a 4-year college, and not enrolling in any college. I define these by taking the first observed postsecondary enrollment, if any, starting the fall semester after projected high school graduation through the subsequent three academic years.

Instruments.—I measure proximity (in miles) to the nearest 2-year and 4-year college campuses by computing ellipsoidal distances between the coordinates of all Texas public high schools (from NCES CCD) and the coordinates of all Texas postsecondary institutions (from IPEDS), then taking minimum distances for each high school within

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23 These twelve urbanization categories are large city, midsize city, small city, large suburb, midsize suburb, small suburb, fringe town, distant town, remote town, fringe rural, distant rural, and remote rural.


25 For the very small number of students who initially enroll in both sectors simultaneously, I assign them to the sector with greater credit hours. Following Andrews et al. (2014), I ignore summer terms when defining sector of enrollment.
the 2-year sector and 4-year sector separately.\textsuperscript{26} For colleges with missing geospatial data in IPEDS, I manually collected their locations by first checking each college’s institutional profile for standalone branch campuses and location changes over my sample period, then converting those year-specific physical addresses to geocoordinates via Google Maps.

\textit{Academic outcomes}.—I study the effects of 2-year college enrollment on two main academic outcomes: bachelor’s degree completion and years of completed schooling. Bachelor’s degree completion is an indicator for appearing in the THECB public or private 4-year degree completion files within ten years of projected high school graduation. Years of completed schooling are calculated using the algorithm detailed in the footnote below.\textsuperscript{27}

\textit{Earnings}.—I measure real quarterly earnings by summing TWC earnings within each person-quarter, deflating by the quarterly U.S. consumer price index (base year 2010), winsorizing at the 99th percentile, and averaging the non-missing quarters within person over ages 28-30, which are the oldest common ages available across my analysis cohorts.\textsuperscript{28} To study earnings dynamics, I also construct similar within-person averages over ages 22-24 and 25-27, as well as an annual panel of mean quarterly earnings at each observed age.

### 3.3 Sample Construction and Summary Statistics

To construct the main analysis sample of 2000-2004 cohorts and the NSC sample of 2008-2009 cohorts, I begin with the population of 10th grade students in each cohort with valid student identifiers, covariates, and high school locations. Table 1 presents summary statistics for these base samples. Column (1) shows that 3.7 percent of

\textsuperscript{26}In determining minimum distances, I ignore small private college campuses and small extension centers that offer very limited courses and student services.

\textsuperscript{27}Years of completed schooling range from 10 to 17. To complete 10: enroll in 11th grade. To complete 11: enroll in 12th grade. 12: complete high school. 13: enroll in college with 2nd year standing, or complete a certificate, or complete the academic core requirement at a community college. 14: enroll in college with 3rd year standing, or complete an associate’s degree. 15: enroll in college with 4th year standing. 16: complete a bachelor’s degree, or enter a postsecondary program with post-baccalaureate standing, or enroll in graduate school. 17: complete any graduate degree.

\textsuperscript{28}These ages assume the student was 16 at the end of 10th grade, which is true for roughly 96 percent of students in the sample. The 2004 cohort, the youngest in the sample, is only measured over ages 28-29, since the TWC earnings data are only available through 2015.
Table 1: Summary Statistics

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<th>(2) Main cohorts 2000-2004</th>
<th>(3) Main cohorts w/out top scoring quintile</th>
<th>(4) Main cohorts w/out top scoring quintile or missing earnings</th>
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<tr>
<td>No college</td>
<td>0.318</td>
<td>0.388</td>
<td>0.433</td>
<td>0.391</td>
</tr>
<tr>
<td>Start at 2-year college</td>
<td>0.371</td>
<td>0.339</td>
<td>0.366</td>
<td>0.394</td>
</tr>
<tr>
<td>Start at 4-year college</td>
<td>0.274</td>
<td>0.273</td>
<td>0.201</td>
<td>0.215</td>
</tr>
<tr>
<td>Start at 4-year (out of state)</td>
<td>0.037</td>
<td>-</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td><strong>Instruments</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Miles to 2-year college</td>
<td>8.5 (10.1)</td>
<td>9.7 (11.4)</td>
<td>9.7 (11.5)</td>
<td>9.7 (11.5)</td>
</tr>
<tr>
<td>Miles to 4-year college</td>
<td>19.5 (18.7)</td>
<td>20.5 (19.7)</td>
<td>20.5 (19.9)</td>
<td>20.4 (19.8)</td>
</tr>
<tr>
<td><strong>Academic Outcomes</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Bachelor's degree</td>
<td>-</td>
<td>0.255</td>
<td>0.187</td>
<td>0.207</td>
</tr>
<tr>
<td>Years of schooling</td>
<td>-</td>
<td>13.1 (2.1)</td>
<td>12.8 (2.0)</td>
<td>13.0 (2.0)</td>
</tr>
<tr>
<td><strong>Earnings Outcomes</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Mean quarterly earnings</td>
<td>-</td>
<td>8,826 (5,841)</td>
<td>8,168 (5,413)</td>
<td>8,168 (5,413)</td>
</tr>
<tr>
<td>Has quarterly earnings</td>
<td>-</td>
<td>0.764</td>
<td>0.773</td>
<td>1.000</td>
</tr>
<tr>
<td><strong>Observations</strong></td>
<td>454,078</td>
<td>957,752</td>
<td>763,847</td>
<td>590,397</td>
</tr>
</tbody>
</table>

Notes: NSC cohorts are those with National Student Clearinghouse college enrollment data. The twelve NCES geographic locale categories are grouped into four values (city, suburb, town, rural) to save space. Academic outcomes are measured at age 28. Earnings outcomes are measured over ages 28-30.
students in the 2008-2009 NSC cohorts attend college outside of Texas. To mitigate any bias from outmigration in the 2000-2004 main analysis cohorts for whom out-of-state enrollments are unobserved, column (3) drops the highest ability students with test scores above the 80th percentile. Appendix Figure A.1 shows that out-of-state enrollment in the 2008-2009 NSC cohorts is concentrated among students with test scores above this threshold, and that top-scoring students are also more likely to have missing earnings in the main 2000-2004 cohorts. Appendix Table A.1 shows that after dropping top-scoring students, the proximity instruments have no effect on the small remaining share of out-of-state enrollments in the 2008-2009 NSC cohorts.

To complete the main analysis sample, column (4) of Table 1 drops the remaining students with no observed quarterly earnings over ages 28-30. Comparing columns (3) and (4) shows that students with non-missing earnings look very similar to the full sample in terms of covariates, though those with non-missing earnings are a bit more likely to have (observed) college enrollments and degrees. In Appendix Figure A.2, I project earnings on all covariates and instruments within the earnings sample, predict earnings for those with missing earnings, and plot the two densities of predicted earnings for comparison. The distributions are nearly identical, with a mean difference of just 58 dollars. These results cannot rule out differential attrition based on unobservables, but they offer some assurance that the scope for sample selection bias may be limited.

3.4 Sorting into Initial College Enrollments

Figure 1 describes how initial college enrollment choices vary across observable student characteristics in the 2008-2009 NSC cohorts. Across demographic groups and neighborhood quality deciles, the 2-year college enrollment share is remarkably constant around the grand mean of 37 percent; what differ are the outside option shares of 4-year enrollment and no college, with men, disadvantaged students, underrepresented minorities, and students from poor neighborhoods more likely to forego college altogether than enroll in a 4-year institution. The bottom panel of Figure 1 shows that the

---

29 Appendix Figure A.3 reproduces these results for the 2000-2004 main analysis cohorts. The plots are very similar up through the 80th percentile test score sample cutoff, beyond which the share of students in the 2000-2004 main analysis cohorts starting 4-year is somewhat understated, and no college overstated, due to unobserved out-of-state enrollments.
Figure 1: Sorting into College Enrollment Choices by Observable Characteristics

Notes: 2008-2009 cohorts with National Student Clearinghouse college enrollment coverage; see Appendix Figure A.3 for comparison to the main analysis cohorts for whom out-of-state enrollments are not observed. Disadvantaged is an indicator for free or reduced price lunch eligibility in 10th grade. Neighborhood quality and test score percentiles, defined in Section 3.2, are grouped into 5-unit bins.
Figure 2: Enrollment Dynamics by Sector of Initial Enrollment

Notes: Enrollment shares at age 19 are not equal to 1 due to the 3-year window in defining the sector of initial enrollment. Subsequent enrollments are not mutually exclusive at a given age; a small fraction of students enroll in both sectors simultaneously.

2-year enrollment share is hump-shaped across 10th grade test scores with a peak at the 40th percentile, though 2-year enrollment is still quite broadly distributed. 4-year college enrollment and no college enrollment, meanwhile, are strongly monotonic in test scores in opposing directions.

3.5 Enrollment and Earnings Dynamics

Figure 2 describes how the initial sector of enrollment relates to enrollment in subsequent years. The left panel conditions on initial 2-year entrants and shows that the vast majority of them stay in the 2-year sector, if any, for the first few years after entry. Enrollment in 4-year colleges among these 2-year entrants then rises and peaks around 20 percent at ages 22-23, with both 2-year and 4-year enrollment slowly trailing off thereafter. The right panel, conditioning on 4-year entrants, tells a similar story of “sticky treatment” in that the vast majority of 4-year entrants stay in the 4-year sector. Roughly 20 percent of initial 4-year entrants enroll in a 2-year college in their early 20s, either as a transfer or dual enrollment.

Figure 3 plots the raw earnings profiles associated with each initial enrollment choice. As expected, 4-year entrants overtake 2-year entrants, and 2-year entrants
Notes: Quarterly earnings are measured in real 2010 U.S. dollars and averaged within person over each age. Earnings after age 29 are only available for progressively older cohorts in the analysis sample, as the earnings data end in 2015.

Overtake those who do not enroll in any college, but differences by gender emerge: women experience this overtaking a full two years prior to men, and the raw college premiums for women are larger, especially in proportional terms. These gender differentials persist into the causal results, as shown in Section 6.

3.6 Regression Results

Turning to regression specifications that quantify outcome differences across initial enrollment choices, Table 2 presents coefficients from OLS regressions of the following form:

\[ \text{Outcome} = \alpha - \beta_{2\leftarrow 0} \mathbb{1}\{\text{No college}\} - \beta_{2\leftarrow 4} \mathbb{1}\{\text{Start 4-year}\} + \text{Controls} + \epsilon \]

Writing the specification in this form, with 2-year entry as the excluded category, immediately delivers a comparison between 2-year entry vs. no college in \( \beta_{2\leftarrow 0} \), and a comparison between 2-year entry vs. 4-year entry in \( \beta_{2\leftarrow 4} \). The control set includes dummies for each categorical covariate and cubic polynomials in each continuous covariate listed in Table 1, plus cohort fixed effects and commuting zone fixed effects.
Table 2: Raw and Controlled OLS Regressions

<table>
<thead>
<tr>
<th></th>
<th>Years of schooling</th>
<th>Bachelor’s degree</th>
<th>Quarterly earnings</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Raw</td>
<td>Controlled</td>
<td>Raw</td>
</tr>
<tr>
<td>Start 2-year vs. no college</td>
<td>1.75</td>
<td>1.48</td>
<td>0.189</td>
</tr>
<tr>
<td></td>
<td>(0.01)</td>
<td>(0.01)</td>
<td>(0.002)</td>
</tr>
<tr>
<td>Start 2-year vs. start 4-year</td>
<td>-1.58</td>
<td>-1.33</td>
<td>-0.392</td>
</tr>
<tr>
<td></td>
<td>(0.01)</td>
<td>(0.01)</td>
<td>(0.002)</td>
</tr>
<tr>
<td>( R^2 )</td>
<td>0.407</td>
<td>0.457</td>
<td>0.286</td>
</tr>
<tr>
<td>( N )</td>
<td>590,397</td>
<td>590,397</td>
<td>590,397</td>
</tr>
</tbody>
</table>

Notes: Standard errors in parentheses are clustered at the high school campus by cohort level. Academic outcomes are measured at age 28. Quarterly earnings are measured in real 2010 U.S. dollars and averaged within person over ages 28-30.

Taken at face value, the OLS results in Table 2 suggest diversion from 4-year to 2-year entry has large negative consequences on educational attainment and earnings: 2-year entrants complete 1.3 fewer years of schooling, are 36 percentage points less likely to complete a bachelor’s degree, and earn 1,400 dollars less per quarter around age 30 relative to observably similar 4-year entrants. But how much do these differences reflect causality versus selection bias? And what share of students are actually on the diversion margin when 2-year college access expands? These questions motivate the instrumental variables method developed in the next section.

4 Identification

4.1 The Methodological Challenge

Instrumental variables (IV) offer a potential solution to the problem of selection bias in college enrollment choices, since valid instrumental variation can induce otherwise similar students into different choices and thus enable causal comparisons of their subsequent outcomes. For the well-known case of binary treatment, Imbens and Angrist (1994) demonstrate that such comparisons identify a local average treatment effect (LATE) among compliers, those individuals whose choices respond to instrumental variation, under the standard IV assumptions of independence, exclusion, and mono-
Multiple margins of treatment present a challenge within this paradigm: when instruments shift individuals among more than two alternatives, the relevant counterfactuals for compliers induced into a specific alternative may be both multiple and unobserved, which hampers causal comparisons of the consequences of one choice versus another.30 To see this in the setting of 2-year community college enrollment, suppose an exogenous binary instrument $Z_2$ induces students into 2-year college entry (indicated by $D_2$) and does not otherwise affect enrollment choices or outcomes. The multiple margins of treatment in this case are the “democratization” margin and the “diversion” margin: some $Z_2$ compliers are “democratized” into higher education and would not have otherwise attended college ($2 \leftarrow 0$, read “two from zero”), while other $Z_2$ compliers would have otherwise started at a 4-year institution ($2 \leftarrow 4$, read “two from four”).

Abstracting from covariates, the standard two-stage least squares (2SLS) approach to IV would specify the following outcome and first stage equations:

$$Y = \beta_0 + \beta_2 D_2 + \epsilon$$

$$E[D_2|Z_2] = \alpha_0 + \alpha_2 Z_2,$$

where $Y$ is a student outcome (e.g. bachelor’s degree attainment or earnings) and $\beta_2$ is the coefficient of interest on the 2-year entry treatment indicator $D_2$. A straightforward argument, provided in Appendix A, shows that the 2SLS estimand $\beta_2$ represents a pooled local average treatment effect (LATE) of 2-year entry on student outcomes that combines the two distinct complier margins into a single weighted average:31

$$\beta_2 = \omega \text{LAT} E_{2 \leftarrow 0} + (1 - \omega) \text{LAT} E_{2 \leftarrow 4}$$

30See Kirkeboen et al. (2016) for a higher education setting with observable measures of these relevant counterfactuals, thanks to a centralized admissions system that requires applicants to submit rank-ordered lists of program enrollment preferences. The college application and enrollment process is far more decentralized in the United States, usually prohibiting identification of a given student’s next-preferred alternative to a given program.

31Heckman and Urzua (2010), Hull (2018b), and Kline and Walters (2016) provide related derivations, as do Angrist and Imbens (1995) for the case of ordered multivalued treatments.
The weight $\omega$ captures the share of $Z_2$ compliers who are on the 2–0 democratization margin, and this share is identified by the reduction in $Pr(\text{no college})$ induced by $Z_2$ as a fraction of the increase in $Pr(D_2)$. The distinct 2–0 democratization and 2–4 diversion treatment effects are not separately identified, however, leaving these likely opposing impacts of 2-year enrollment shrouded behind the identified net effect that pools them together.

In many settings, the pooled net effect is a parameter of interest in its own right; here, $LATE_2$ captures the aggregate impact of 2-year entry on all students induced by the instrument, which may correspond to policy-relevant variation like closer access to a 2-year college campus, subsidized 2-year tuition, etc. Decomposing the net effect into its potentially opposing impacts on students from each margin, however, allows for a more comprehensive assessment of the impacts and potential unintended consequences of such policies. Equation (1) reveals that diverse combinations of democratization and diversion effects could all yield the same net effect, with very different policy implications. To take two illustrative cases, consider that the same positive $LATE_2$ value could be generated from a moderately positive $LATE_{2 \leftarrow 0}$ plus a zero $LATE_{2 \leftarrow 4}$, or alternatively a large positive $LATE_{2 \leftarrow 0}$ plus a large negative $LATE_{2 \leftarrow 4}$. The first case features modest average gains for democratized students and zero average impact on diverted students; in light of lower costs at 2-year colleges relative to their 4-year counterparts, this case could potentially justify broad investment in 2-year college access as a cost-effective engine of upward mobility. The second case features large average gains for democratized students but large average losses for diverted students; this case would demand caution in broadly expanding 2-year access, perhaps in favor of targeted policies towards the types of students likely to be on the democratization margin while encouraging students with 4-year ambitions to start directly in that sector.

With two treatment margins of interest and only one instrument, the preceding 2SLS framework is fundamentally underidentified, so a natural next step is to consider
multivariate 2SLS when a second instrument is available, e.g. $Z_4$:

\[
    Y = \beta_0 + \beta_2 D_2 + \beta_4 D_4 + \epsilon
\]

\[
    E[D_2 | Z_2, Z_4] = \alpha_0 + \alpha_2 Z_2 + \alpha_4 Z_4
\]

\[
    E[D_4 | Z_2, Z_4] = \gamma_0 + \gamma_2 Z_2 + \gamma_4 Z_4
\]

Kirkeboen et al. (2016) consider this case and show that even with one instrument per endogenous treatment, the multivariate 2SLS framework does not generally recover causal effects of one treatment versus another for any relevant population, except in special cases like constant treatment effects across all individuals. Instead, the 2SLS estimands $\beta_2$ and $\beta_4$ mix together multiple treatment margins across multiple complier subpopulations to yield weighted averages that generally do not correspond to a simple net effect as in Equation (1), making them difficult to interpret as treatment effects for any well-defined population along any well-defined treatment margin. In Appendix B, I derive and decompose the multivariate 2SLS estimands corresponding to my econometric framework in Section 4.2, showing how they fuse all treatment margins into each coefficient.\(^{32}\)

The remainder of this section develops an alternative separate identification approach that overcomes the limitations of multivariate 2SLS in the presence of multiple treatment margins, allowing for a decomposition of the net effect of 2-year college access into its distinct democratization and diversion components.

### 4.2 Setup and Assumptions

To set up the identification arguments, begin with notation. The three mutually exclusive and exhaustive discrete treatments are $D = 2$ (start at a 2-year college), $D = 4$ (start at a 4-year college), and $D = 0$ (no college). Define $D_2$, $D_4$, $D_0$ as the binary indicators corresponding to each treatment, noting that $D_2 + D_4 + D_0 = 1$ for a given individual. We are interested in identifying causal effects of these initial choices on outcomes $Y$ using two continuous instruments: $Z_2$ (distance to the nearest 2-year col-

\(^{32}\)A similar result applies when interacting a single instrument with covariates in the attempt to generate additional sources of instrumental variation (Hull, 2018b; Kline and Walters, 2016). See Pinto (2016) for a related discussion in the context of the Moving to Opportunity experiment.
lege) and \( Z_4 \) (distance to the nearest 4-year college). To simplify notation, suppress the individual index \( i \) and implicitly condition on the control set \( X \) throughout.

Denote potential treatment choice as \( D(z_2, z_4) \in \{0, 2, 4\} \): this is the enrollment choice a student would make if exogenously assigned to instrument values \( (Z_2, Z_4) = (z_2, z_4) \). Define the binary indicators \( D_0(z_2, z_4), D_2(z_2, z_4), D_4(z_2, z_4) \) analogously. The potential outcomes associated with these treatments are \( Y_0, Y_2, Y_4 \), indicating the outcome a student would reap if exogenously assigned to treatment \( D = d \in \{0, 2, 4\} \). Realized outcomes are thus \( Y = Y_0D_0 + Y_2D_2 + Y_4D_4 \), and the margin-specific treatment effects of interest are \( Y_2 - Y_0 \) (democratization) and \( Y_2 - Y_4 \) (diversion).

The following assumptions put the necessary structure on these counterfactual objects to secure identification from the observed data \( (Y, D, Z_2, Z_4) \).

**A1. Independence and Exclusion:**

\[
(Z_2, Z_4) \perp \!\!\!\!\!\! \perp (Y_0, Y_2, Y_4, \{D(z_2, z_4)\}_{\forall (z_2, z_4)}).
\]

First, the standard IV assumption of independence and exclusion maintains its binary intuition in this multivariate setting: the instruments are as good as randomly assigned relative to potential outcomes and potential treatment choices, conditional on the implicit control set \( X \). Relative to the binary case, A1 is simply expanded to allow for multiple instruments \( (Z = (Z_2, Z_4)) \) and multiple unordered treatments \( (D \in \{0, 2, 4\}) \).

**A2. Marginal Unordered Monotonicity:**

For all \( z_2, z_2', z_4 \) with \( z_2' < z_2 \) and holding \( z_4 \) fixed: \( D_2(z_2', z_4) \geq D_2(z_2, z_4) \), \( D_0(z_2', z_4) \leq D_0(z_2, z_4) \), and \( D_4(z_2', z_4) \leq D_4(z_2, z_4) \) for all individuals.

For all \( z_4, z_4', z_2 \) with \( z_4' < z_4 \) and holding \( z_2 \) fixed: \( D_4(z_2, z_4') \geq D_4(z_2, z_4) \), \( D_0(z_2, z_4') \leq D_0(z_2, z_4) \), and \( D_2(z_2, z_4') \leq D_2(z_2, z_4) \) for all individuals.

Next, marginal unordered monotonicity generalizes the standard monotonicity assumption from the binary case by again allowing for multiple instruments and multiple unordered treatments. The binary intuition of “no defiers” carries over to this multivariate setting: each instrument shift renders each treatment either weakly more
Figure 4: Marginal Unordered Monotonicity

Notes: This figure visualizes the complier flows permitted by Assumption A2. “Z₂” denotes a marginal decrease in Z₂ while holding Z₄ fixed, and “Z₄” denotes a marginal decrease in Z₄ while holding Z₂ fixed.

attractive for all individuals or weakly less attractive for all individuals, ruling out simultaneous flows of compliers both into and out of a given treatment in response to a given instrument shift. As Figure 4 illustrates, however, this does not limit each instrument to only inducing one type of complier: closer 2-year proximity (z′₂ < z₂) induces both 2–0 and 2–4 compliers by rendering 2-year entry weakly more attractive (D₂(z′₂, z₄) ≥ D₂(z₂, z₄)) at the expense of no college (D₀(z′₂, z₄) ≤ D₀(z₂, z₄)) and 4-year entry (D₄(z′₂, z₄) ≤ D₄(z₂, z₄)). Likewise, closer 4-year proximity (z′₄ < z₄) induces both 4–0 and 4–2 compliers by rendering 4-year entry more attractive (D₄(z₂, z′₄) ≥ D₄(z₂, z₄)) at the expense of no college (D₀(z₂, z′₄) ≤ D₀(z₂, z₄)) and 2-year entry (D₂(z₂, z₄) ≤ D₂(z₂, z₄)).

Since the treatment values D ∈ {0, 2, 4} need not be ordered in any uniform way across individuals, A2 is closely related to the “unordered monotonicity” assumption of Heckman and Pinto (2018), but is weaker in that it only concerns marginal instrumental variation in Z₂ while holding Z₄ fixed, and likewise Z₄ holding Z₂ fixed, and is

33One potential violation would be an option value channel that causes 4-year proximity to induce 2–0 compliers: the future prospect of upward transfer may inspire some non-college individuals into 2-year entry. Empirically, however, the first stage relationship between 4-year proximity and the probability of not attending college turns out to be quite small; this limits any influence of such an option value channel, since the mass of any 2–0 compliers with respect to 4-year proximity would be bounded above by this small first stage share. A different type of violation could arise in the presence of capacity constraints, whereby an instrument-induced switch of one student out of a given sector allows another student to switch into that sector. This type of behavior is notationally ruled out by the exclusion of other students’ instrument values in potential choices, and is empirically unlikely in this setting given that all 2-year colleges and most of the 4-year institutions attended by marginal students have non-selective admission practices.
thus agnostic about complier flows when both $Z_2$ and $Z_4$ change value simultaneously. Heckman and Pinto (2018) consider the case where unordered monotonicity holds across any shift in the value of a discrete instrument $Z$; by considering each combination of $(Z_2, Z_4)$ in the present setting as a distinct value of $Z$, the relaxation to "marginal" unordered monotonicity arises by limiting the scope of the assumption to the subset of shifts in $Z = (Z_2, Z_4)$ in which only one element of the vector changes value. To demonstrate the importance of this relaxation, consider that a simultaneous decrease in both 2-year distance ($Z_2$) and 4-year distance ($Z_4$) would likely induce not only 2–0 compliers and 4–0 compliers, but also compliers between 2 and 4 (with the direction between 2 and 4 depending on the relative magnitude of the 2-year vs. 4-year distance reduction). This dual instrument shift would thus violate unordered monotonicity by inducing some students into a given treatment choice at the same time that others are induced out of it, leading to underidentification from too many distinct complier groups generating too few observable quantities.\footnote{If $D$ were binary instead of multinomial, this marginal monotonicity assumption would similarly relax the more stringent monotonicity condition of Imbens and Angrist (1994), who require $D(z') \leq D(z)$ for all individuals, or $D(z') \geq D(z)$ for all individuals, for any shift in $Z$ from $z$ to $z'$, which would include shifts in which both elements of the vector $Z = (Z_2, Z_4)$ change value simultaneously. See Mogstad et al. (2018b) for a discussion of marginal monotonicity in the binary treatment case.}

\section*{A3. Comparable Compliers:}

For all $(z_2, z_4)$:

$$\lim_{z_2' \uparrow z_2} E[Y_2|D(z_2', z_4) = 2, D(z_2, z_4) = 4] = \lim_{z_4' \downarrow z_4} E[Y_2|D(z_2, z_4') = 2, D(z_2, z_4) = 4].$$

Finally, the assumption of comparable compliers draws a connection between the two types of diversion compliers on the margin between 2-year and 4-year entry. From a common initial instrument value of $(z_2, z_4)$, the left side of the equation in A3 involves those compliers who would switch to 2 from 4 in response to a marginal decrease in $z_2$ holding $z_4$ fixed ($D(z_2', z_4) = 2, D(z_2, z_4) = 4$ for $z_2'$ less than but arbitrarily close to $z_2$), and the right side involves those compliers who would make the same switch to 2 from 4 in response to a marginal increase in $z_4$ holding $z_2$ fixed ($D(z_2, z_4') = 2, D(z_2, z_4) = 4$ for $z_4'$ greater than but arbitrarily close to $z_4$). Intuitively, imagine two students who...
are initially the same distances from the nearest 2-year and 4-year colleges, both of whom are planning to start college at a 4-year institution. The student on the left side of the equation in A3 would switch to 2-year entry if the nearest 2-year campus were slightly closer, and the student on the right side of the equation would also switch to 2-year entry if the nearest 4-year campus were slightly farther away. What the equality in Assumption A3 requires is that these two types of 2–4 diversion compliers, who are both on the margin of indifference between 2-year and 4-year entry at the same initial instrument value \((z_2, z_4)\), do not differ systematically in terms of their mean potential 2-year outcomes \((Y_2)\).

As shown below, this assumption is actually a necessary implication of a standard selection model, and it emerges as a generalized index sufficiency condition: marginal shifts in \(z_2\) versus \(z_4\) from the same initial point \((z_2, z_4)\) both shift the relative attractiveness of \(D_2\) versus \(D_4\), which is governed by an index. Shifting this index traces out the same group of marginal 2–4 compliers regardless of which instrument caused the shift, so these identical complier groups automatically have equal mean potential 2-year outcomes. On the empirical side, Assumption A3 is not directly testable, but a useful diagnostic akin to a balancing check involves comparing the means of observable, predetermined predictors of outcomes across these two marginal complier groups, which are separately identified in the framework developed below in Section 4.3 by replacing the unobservable \(Y_2\) with an observable covariate \(X\). Table 3 below conducts this check with 10th grade standardized test scores, which strongly predict educational and labor market outcomes, and finds that the two diversion complier groups are statistically identical.

To interpret these assumptions through the lens of economic behavior, consider that both marginal unordered monotonicity (A2) and comparable compliers (A3) are implied by a weakly separable selection model with choice-specific instruments.\(^{35}\) Suppose

\(^{35}\)Vytlacil (2002) proves an equivalence result between the standard IV assumptions of independence, exclusion, and monotonicity and a weakly separable selection model for the binary IV case, and Heckman and Pinto (2018) extend this logic to multivariate treatment settings in which unordered monotonicity holds across all pairwise instrument comparisons. In my weaker marginal unordered monotonicity setting, the selection model presented here is sufficient for Assumptions A1-A3 to hold; fleshing out necessary conditions of a potentially more general selection model is an avenue for future work.
individuals have latent indirect utilities for each choice given by

\[ I_0 = 0 \]
\[ I_2 = U_2 - \mu_2(Z_2) \]
\[ I_4 = U_4 - \mu_4(Z_4), \]

where the utility of no college is normalized to zero, \( U_2 \) is unobserved preference heterogeneity for 2-year enrollment, \( U_4 \) is unobserved preference heterogeneity for 4-year enrollment, and the \( \mu_j() \) functions represent the costs of each alternative. Note that \( \mu_2() \) and \( \mu_4() \) need not be the same function, allowing for different disutilities of 2-year and 4-year distance; the key restrictions are that these functions are weakly separable from unobserved heterogeneity and that each instrument is specific to each choice.\(^{36}\) Independence and exclusion (A1) in this selection model translates to \((Z_2, Z_4) \perp \perp (U_2, U_4, Y_0, Y_2, Y_4)\), while selection can arise through relationships between unobserved preferences and potential outcomes: \((U_2, U_4)\) and \((Y_0, Y_2, Y_4)\) can depend on each other in unrestricted ways. To complete the model, individuals simply choose the alternative with the highest indirect utility, implying the choice equations

\[ D_0(z_2, z_4) = 1[U_2 < \mu_2(z_2), U_4 < \mu_4(z_4)] \]
\[ D_2(z_2, z_4) = 1[U_2 > \mu_2(z_2), U_4 - U_2 < \mu_4(z_4) - \mu_2(z_2)] \]
\[ D_4(z_2, z_4) = 1[U_4 > \mu_4(z_4), U_4 - U_2 > \mu_4(z_4) - \mu_2(z_2)]. \]

Figure 5 visualizes how this selection model generates A2 and A3 as necessary implications, and Appendix C provides the formal proofs. The first panel of Figure 5 shows how the choice equations partition the two-dimensional space of unobserved preference heterogeneity \((U_2, U_4)\) for a given value of the instruments: individuals who choose \( D = 0 \) have low preference values for both 2-year and 4-year enrollment, while those

---

\(^{36}\)The index/cost function \( \mu_2() \) could depend on \( Z_4 \), and \( \mu_4() \) on \( Z_2 \), as long as each index is able to vary while fixing the other; then identification would proceed by first identifying these index functions (e.g. Matzkin, 1993), then exploiting conditional variation in the index values rather than the instruments directly as this paper does. See Cameron and Heckman (1998) for an application of this logic in a dynamic discrete choice framework, and Lee and Salanie (2018) for a recent application to multivalued treatments with multiple dimensions of unobserved heterogeneity.
Notes: This figure illustrates how the selection model in Section 4.2 generates Assumption A2 (marginal unordered monotonicity) and Assumption A3 (comparable compliers). The top left panel shows how the choice equations, given a particular pair of instrument values, partition the two-dimensional space of unobserved preference heterogeneity. The top right panel illustrates a shift in $Z_2$ inducing 0–4 and 2–4 compliers; the bottom left panel illustrates a shift in $Z_4$ inducing 0–4 and 2–4 compliers; and the final panel overlays these two shifts to illustrate their comparable compliers along the 2–4 diversion margin.
who choose \( D = 2 \) or \( D = 4 \) have relatively higher values of the corresponding \( U_j \). The second and third panels visualize marginal unordered monotonicity (A2): a marginal decrease in \( Z_2 \) (holding \( Z_4 \) fixed) increases the attractiveness of 2-year enrollment at the expense of no college and 4-year enrollment, thus inducing 2–0 and 2–4 compliers, while a marginal increase in \( Z_4 \) (holding \( Z_2 \) fixed) decreases the attractiveness of 4-year enrollment at the gain of no college and 2-year enrollment, thus inducing 0–4 and 2–4 compliers. Finally, comparable compliers (A3) emerge in the fourth panel by overlaying these two instrument shifts: both \( Z_2 \) and \( Z_4 \) induce 2–4 compliers, and these two marginal complier groups have the same \((U_2, U_4)\) values. (The small inner triangle in the fourth panel of Figure 5 in which the 2–4 \( Z_2 \) and \( Z_4 \) compliers do not overlap vanishes as these visually discrete shifts converge to marginal shifts.) This is a sufficient condition for equal mean potential 2-year outcomes of these two complier groups. The selection model thus provides a useful illustration of the logic of Assumptions A2 and A3 but is not necessary for implementing the following nonparametric identification results.

### 4.3 Identification Results

I separately identify treatment effects along the 2–0 democratization margin and 2–4 diversion margin by first isolating the mean potential outcomes of compliers along each margin, then taking differences of these potential outcomes to form treatment effects.\(^{37}\) The logic begins by decomposing the reduced form with respect to \( Z_2 \), using the fact that \( D_0 + D_2 + D_4 = 1 \):

\[
E[Y|Z_2, Z_4] = E[YD_0 + YD_2 + YD_4|Z_2, Z_4]
\]

\[
\frac{\partial E[Y|Z_2, Z_4]}{\partial Z_2} = \frac{\partial E[YD_0|Z_2, Z_4]}{\partial Z_2} + \frac{\partial E[YD_2|Z_2, Z_4]}{\partial Z_2} + \frac{\partial E[YD_4|Z_2, Z_4]}{\partial Z_2}
\]

Since \( YD_0 = Y_0 \) when \( D_0 = 1 \) and \( YD_0 = 0 \) otherwise, instrument-induced changes in \( E[YD_0|Z_2, Z_4] \) reveal information about \( Y_0 \) among compliers switching into or out of \( D_0 \). By marginal unordered monotonicity (A2), changes in \( D_0 \) with respect to \( Z_2 \)

\(^{37}\)Appendix D provides the formal proofs for the results in this section.
are driven by 2–0 compliers, so changes in $E[YD_0|Z_2, Z_4]$ with respect to $Z_2$ reveal information about $Y_0$ for these 2–0 compliers:

$$\frac{\partial E[YD_0|Z_2, Z_4]}{\partial Z_2} = E[Y_0|2–0 \text{ complier at (}Z_2, Z_4\text{)]} \frac{\partial E[D_0|Z_2, Z_4]}{\partial Z_2},$$

where $E[Y_0|2–0 \text{ complier at (}Z_2, Z_4\text{)]}$ is shorthand for $\lim_{z_2' \uparrow z_2} E[Y_0|D(z_2', z_4) = 2, D(z_2, z_4) = 0]$ evaluated at a given instrument point $(Z_2, Z_4) = (z_2, z_4)$. Since the derivatives $\frac{\partial E[YD_0|Z_2, Z_4]}{\partial Z_2}$ and $\frac{\partial E[D_0|Z_2, Z_4]}{\partial Z_2}$ are directly identified from data, their ratio identifies $E[Y_0|2–0 \text{ complier at (}Z_2, Z_4\text{)]}$ at all points in the instrument support at which the first stage derivative $\frac{\partial E[D_0|Z_2, Z_4]}{\partial Z_2}$ is nonzero.

Likewise, changes in $D_4$ with respect to $Z_2$ are driven by 2–4 compliers, so changes in $E[YD_4|Z_2, Z_4]$ with respect to $Z_2$ reveal information about $Y_4$ for these 2–4 compliers:

$$\frac{\partial E[YD_4|Z_2, Z_4]}{\partial Z_2} = E[Y_4|2–4 \text{ complier at (}Z_2, Z_4\text{)]} \frac{\partial E[D_4|Z_2, Z_4]}{\partial Z_2}.$$

The story is more complicated for $E[YD_4|Z_2, Z_4]$, since changes in $D_2$ with respect to $Z_2$ are driven by both 2–0 compliers and 2–4 compliers:

$$\frac{\partial E[YD_2|Z_2, Z_4]}{\partial Z_2} = E[Y_2|2–0 \text{ complier at (}Z_2, Z_4\text{)]} \left( -\frac{\partial E[D_0|Z_2, Z_4]}{\partial Z_2} \right) + E[Y_2|2–4 \text{ complier at (}Z_2, Z_4\text{)]} \left( -\frac{\partial E[D_4|Z_2, Z_4]}{\partial Z_2} \right)$$

(2)

Instrumental variation in $Z_2$ alone is therefore insufficient to identify margin-specific treatment effects, since the mean $Y_2$ potential outcomes for both complier margins are pooled together.

The key to disentangling these margins lies with $Z_4$: since changes in $D_2$ with respect to $Z_4$ are driven only by 2–4 compliers, we have

$$\frac{\partial E[YD_2|Z_2, Z_4]}{\partial Z_4} = E[Y_2|2–4 \text{ complier at (}Z_2, Z_4\text{)]} \frac{\partial E[D_2|Z_2, Z_4]}{\partial Z_4}.$$
tion into the pooled expression in Equation (2) disentangles \( E[Y_2|2–0 \text{ complier at } (Z_2, Z_4)] \) from \( E[Y_2|2–4 \text{ complier at } (Z_2, Z_4)] \), since every other piece of the pooled expression is identified.\(^{38}\)

Putting all these pieces together, this separate identification approach secures all of the mean potential outcomes necessary for forming the margin-specific treatment effects of interest:

\[
E[Y_0|2–0 \text{ complier at } (Z_2, Z_4)] = \frac{\partial E[YD_0|Z_2, Z_4]}{\partial Z_2} \frac{\partial E[D_0|Z_2, Z_4]}{\partial Z_2} \\
E[Y_4|2–4 \text{ complier at } (Z_2, Z_4)] = \frac{\partial E[YD_4|Z_2, Z_4]}{\partial Z_4} \frac{\partial E[D_4|Z_2, Z_4]}{\partial Z_4} \\
E[Y_2|2–4 \text{ complier at } (Z_2, Z_4)] = \frac{\partial E[YD_2|Z_2, Z_4]}{\partial Z_2} - \frac{\partial E[YD_0|Z_2, Z_4]}{\partial Z_2} \frac{\partial E[D_0|Z_2, Z_4]}{\partial Z_2} \\
E[Y_2|2–0 \text{ complier at } (Z_2, Z_4)] = \frac{\partial E[YD_2|Z_2, Z_4]}{\partial Z_2} - \frac{\partial E[YD_0|Z_2, Z_4]}{\partial Z_2} \frac{\partial E[D_0|Z_2, Z_4]}{\partial Z_2}
\]

\[
E[Y_2|2–0 \text{ complier at } (Z_2, Z_4)] - E[Y_0|2–0 \text{ complier at } (Z_2, Z_4)] \\
= E[Y_2 - Y_0|2–0 \text{ complier at } (Z_2, Z_4)] \\
= MTE_{2–0}(Z_2, Z_4)
\]

\[
E[Y_2|2–4 \text{ complier at } (Z_2, Z_4)] - E[Y_4|2–4 \text{ complier at } (Z_2, Z_4)] \\
= E[Y_2 - Y_4|2–4 \text{ complier at } (Z_2, Z_4)] \\
= MTE_{2–4}(Z_2, Z_4).
\]

These marginal treatment effects (MTEs) are simply the continuous instrument ana-

\(^{38}\)Instead of assuming comparable compliers (A3) directly, an alternative approach could gauge the sensitivity of the estimates to departures from A3 by exploring how different the mean potential 2-year outcomes of \( Z_2 \) versus \( Z_4 \) compliers along the 2–4 diversion margin would have to be to meaningfully change the estimated treatment effects. Since \( Z_2 \) and \( Z_4 \) diversion compliers in this setting are statistically indistinguishable in terms of test scores (Table 3), this approach is not pursued here, but may be useful in applying the method to other settings where A3 is empirically suspect, or to characterize bounds on treatment effects implied by certain types of relaxations of A3.
logues to discrete LATEs. After identifying these MTEs across the empirical support of the instruments, any discrete LATE of interest within the instrument support can be formed by integrating the corresponding MTE over the relevant discrete instrument shift (Heckman and Vytlacil, 2005).39

Finally, the net effect of 2-year entry, which pools 2–0 and 2–4 compliers together into a single weighted average effect, is identified by the local instrumental variables estimand (Heckman and Vytlacil, 1999) involving \( Y, D_2, \) and \( Z_2, \)

\[
MTE_2(Z_2, Z_4) = \frac{\partial E[Y|Z_2, Z_4]}{\partial Z_2} \frac{\partial E[D_2|Z_2, Z_4]}{\partial Z_2},
\]

and the share of 2–0 compliers (i.e. the weight \( \omega \) in the weighted average effect) is identified by

\[
\omega(Z_2, Z_4) = -\frac{\partial E[D_0|Z_2, Z_4]}{\partial Z_2} \frac{\partial E[D_2|Z_2, Z_4]}{\partial Z_2}.
\]

Bringing these identification results together, we arrive at the decomposition of interest:

\[
MTE_2 = \omega MTE_{2\rightarrow 0} + (1 - \omega) MTE_{2\rightarrow 4},
\]

where each component is separately identified at each point in the empirical support of the instruments at which the first-stage derivatives are nonzero.

Note that this separate identification approach has potential extensions along several fronts. First, the method can identify any distributional feature of complier potential outcomes, not simply means, by replacing \( Y \) with appropriate indicator functions of \( Y, \) e.g. \( 1(Y \leq y) \) for potential outcome CDF recovery.41 Second, replacing \( Y \) with a covariate \( X \) allows for identification of complier characteristics. This exercise does not require assuming comparable compliers (A3), and thus provides an empirical check.

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39 The methods of Mogstad et al. (2018a), generalized to the multivalued treatment setting, could also be used to learn about parameters of interest that are only partially identified without larger instrument support, like average treatment effects (ATEs).

40 Recall the discrete analogue \( LATE_2 \) from Equation (1); \( MTE_2 \) is simply the limit of \( LATE_2 \) as the size of the instrument shift from \( z_2 \) to \( z_2' \) becomes arbitrarily small.

41 This extends the logic of Imbens and Rubin (1997), Abadie (2002), and Carneiro and Lee (2009) to multiple treatment margins and that of Kline and Walters (2016) to multiple instruments.
on the assumption as discussed above; Table 3 below conducts this check with 10th grade test scores. Third, this separate identification approach could be applied to discrete instruments under certain conditions. If the comparable compliers assumption (A3) holds across a pair of element-wise discrete instrument shifts from a common base point, \((z_2, z_4) \rightarrow (z_2', z_4)\) and \((z_2, z_4) \rightarrow (z_2, z_4')\), then point identification is secured by replacing derivatives with discrete differences in the above framework. As just discussed, A3 can be probed empirically by comparing pre-determined observable characteristics across the two 2–4 diversion complier groups; if such a test reveals differences, information about the potential outcomes of 2–4 compliers who respond to one discrete instrument shift may still be somewhat informative about the potential outcomes of 2–4 compliers who respond to the other discrete shift, which could lead to a partial identification approach, or sensitivity analysis to different assumptions about the degree of similarity between these two complier groups, or parametric modeling of the relationship between choice behavior and potential outcomes. Finally, the logic of the separate identification approach extends to \(N_D > 3\) treatment alternatives and \(N_Z > 2\) instruments with generalized versions of marginal unordered monotonicity (A2) and comparable compliers (A3), as long as enough instruments are available to generate sufficient overlap in complier flows to identify potential outcome differences along each treatment margin of interest. Formalizing the generalized conditions for arbitrary \(N_D\) and \(N_Z\) remains an avenue for future work.

5 Estimation and Instrument Diagnostics

5.1 Locally Linear Specification

All of the quantities of interest in Equation (3) are composed of ratios of partial derivatives of the conditional expectations of \(\{D_0, D_2, D_4, Y, YD_0, YD_2, YD_4\}\) with respect to the instruments \((Z_2, Z_4)\). These partial derivatives can be consistently estimated as local slopes in two-dimensional locally linear regressions, evaluated at each point in the empirical support of \((Z_2, Z_4)\).

In many empirical applications, the instruments may only satisfy Assumptions A1-A3 conditional on a control set \(X\). All of the preceding identification and estimation
arguments still apply after conditioning on each \( X = x \), but the curse of dimensionality quickly sets in as \( X \) becomes high-dimensional, and \( X \) may also include continuous variables, as it does in my setting. To reduce this dimensionality problem, I estimate flexible locally linear conditional expectation specifications around \((z_2, z_4)\) evaluation points in which the variables in the control set \( X \) enter additively but with coefficients that are allowed to vary arbitrarily across different \((z_2, z_4)\) evaluation points. Formally, for a given variable \( W \in \{D_0, D_2, D_4, Y, YD_0, YD_2, YD_4\} \), the estimated coefficients at each \((z_2, z_4)\) evaluation point solve a kernel-weighted least squares problem:

\[
\begin{pmatrix}
\hat{\beta}_W^{D_0}(z_2, z_4) \\
\hat{\beta}_W^{D_2}(z_2, z_4) \\
\hat{\beta}_W^{D_4}(z_2, z_4) \\
\hat{\beta}_W^x(z_2, z_4)
\end{pmatrix} = \underset{\beta_0, \beta_2, \beta_4, \beta_4}{\text{argmin}} \sum_{i=1}^{N} K\left(\frac{Z_{2i} - z_2}{h}, \frac{Z_{4i} - z_4}{h}\right) \left(W_i - \beta_0 - \beta_2 Z_{2i} - \beta_4 Z_{4i} - X_i' \beta_4\right)^2,
\]

where \(K()\) is a two-dimensional kernel with bandwidth \(h\). This specification represents a flexible compromise between a fully nonparametric specification across all dimensions of \((Z_2, Z_4, X)\) and a restrictive globally linear specification that would constrain the \(\beta\) slope coefficients to remain constant across all \((z_2, z_4)\) evaluation points.

Forming the potential outcome and treatment effect estimates then proceeds by the analogy principle, plugging in the local slope coefficients \(\hat{\beta}_W^{D_2}(z_2, z_4)\) and \(\hat{\beta}_W^{D_4}(z_2, z_4)\) in place of the local partial derivatives \(\frac{\partial E[W|Z_2=z_2, Z_4=z_4]}{\partial Z_2}\) and \(\frac{\partial E[W|Z_2=z_2, Z_4=z_4]}{\partial Z_4}\) involved in each expression, e.g.

\[
\hat{E}[Y|2-0 \text{ complier at } (z_2, z_4)] = \frac{\hat{\beta}_W^{YD_0}(z_2, z_4)}{\hat{\beta}_W^{D_0}(z_2, z_4)}.
\]

These plug-in estimators deliver numerically equivalent estimates to a modified version of single-treatment 2SLS: continuing the example above, we would arrive at the same estimate of \(\hat{E}[Y|2-0 \text{ complier at } (z_2, z_4)]\) through a 2SLS regression of \(YD_0\) on \(D_0\) instrumented with \(Z_2\), controlling linearly for \(Z_4\) and \(X\), and using data in a kernel-weighted region around the evaluation point \((z_2, z_4)\).

---

42See Hastie and Tibshirani (1993) for further discussion of this type of varying coefficient model.
5.2 Implementation

In my setting, 2-year and 4-year college proximities may be related to the quality of a student’s neighborhood, the degree of urbanization of the neighborhood, and local labor market opportunities, all of which may directly influence enrollment choices and outcomes through confounding channels. To purge the proximity instruments of these relationships, I include in the control set $X$ a cubic polynomial in the neighborhood quality index described in Section 3.2, indicators for the 12 neighborhood-level NCES urbanization locale codes (e.g. midsize city, large suburb, fringe town, remote rural, etc.), and indicators for each of the 62 commuting zones in Texas to soak up unobserved characteristics of each local labor market. I also include basic demographics (cohort, gender, race, and free/reduced price lunch eligibility) and a cubic in the long-run high-school level oil and gas employment share to account for any local influences of the Texas oil and gas industry. Note that this control set does not include ability measures; I exclude these in order to conduct balancing tests and robustness checks below.

I use a two-dimensional Epanechnikov (parabolic) kernel with 40-mile bandwidth to weight observations in the locally linear regressions, and I report the main results evaluated at the mean values of $(Z_2, Z_4)$. The selection results evaluate estimates across the empirical support of $Z_2$ in 5-mile increments, holding $Z_4$ at its mean. Inference is conducted via block bootstrap at each evaluation point with clusters at the high school campus by cohort level, which corresponds to the level at which the instruments vary and allows for arbitrary error correlations among students in the same high school class.

5.3 Instrument Diagnostics

Table 3 presents diagnostic results on the 2-year and 4-year proximity instruments. The first column conducts a balancing test by regressing the excluded 10th grade test score measure on the instruments and controls using the main specification. The results show no relationship between distances and test scores: the coefficient of $-0.001$ on $Z_2$,

---

Table 5 below shows that the results are not sensitive to deviations around this bandwidth value.

Table 5 below compares the estimated standard errors when bootstrapping at the individual level versus clustering at the high school campus by cohort level.

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Table 3: Instrument Diagnostics: Balance, First Stages, Reduced Forms, and Comparable Compliers

<table>
<thead>
<tr>
<th>Test score percentile (0 to 1)</th>
<th>Start at 2-year college</th>
<th>Start at 4-year college</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Z₂:</strong> 2-year distance (miles/10)</td>
<td>-0.001 (0.001)</td>
<td>-0.043 (0.003)</td>
</tr>
<tr>
<td><strong>Z₄:</strong> 4-year distance (miles/10)</td>
<td>0.0005 (0.001)</td>
<td>0.019 (0.002)</td>
</tr>
<tr>
<td>( R^2 )</td>
<td>0.101</td>
<td>0.027</td>
</tr>
<tr>
<td>( N )</td>
<td>556,726</td>
<td>556,726</td>
</tr>
</tbody>
</table>

Baseline controls  ✓ ✓ ✓ ✓ ✓
Test score control ✓ ✓ ✓ ✓ ✓

<table>
<thead>
<tr>
<th>Years of schooling</th>
<th>Bachelor’s degree</th>
<th>Quarterly earnings</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Z₂:</strong> 2-year distance (miles/10)</td>
<td>-0.040 (0.011)</td>
<td>-0.004 (0.002)</td>
</tr>
<tr>
<td></td>
<td>-0.037 (0.009)</td>
<td>-0.004 (0.002)</td>
</tr>
<tr>
<td><strong>Z₄:</strong> 4-year distance (miles/10)</td>
<td>-0.052 (0.008)</td>
<td>-0.011 (0.002)</td>
</tr>
<tr>
<td></td>
<td>-0.055 (0.007)</td>
<td>-0.012 (0.001)</td>
</tr>
<tr>
<td>( R^2 )</td>
<td>0.093</td>
<td>0.075</td>
</tr>
<tr>
<td>( N )</td>
<td>556,726</td>
<td>556,726</td>
</tr>
</tbody>
</table>

Baseline controls  ✓ ✓ ✓ ✓ ✓ ✓
Test score control ✓ ✓ ✓ ✓ ✓ ✓

Comparable compliers check:

Mean test score percentile

| 2-4 complier w.r.t. \( Z₂ \) | 53.4 (2.77) |
| 2-4 complier w.r.t. \( Z₄ \) | 54.1 (2.79) |

Notes: Locally weighted observations: 556,726. Test score percentiles in the top panel are measured from 0 to 1 for comparison to first stage coefficients, so 0.001 is one-tenth of one percentile. Test score percentiles in the bottom panel and elsewhere are measured from 1 to 100. Standard errors in parentheses are block bootstrapped at the high school campus by cohort level. All estimates are evaluated at the mean values of the instruments. Academic outcomes are measured at age 28. Quarterly earnings are measured in real 2010 U.S. dollars and averaged within person over ages 28-30.
Figure 6: Mean Earnings by Test Score Percentile

Notes: Quarterly earnings are measured in real 2010 U.S. dollars and averaged within person over ages 28-30. Test score percentile construction is described in Section 3.2.

which is measured in 10-mile units, means that moving 10 miles further from a 2-year college is associated with an insignificant decrease of just one-tenth of one percentile in the test score distribution. Likewise with respect to $Z_4$, moving 10 miles further from a 4-year college is associated with an insignificant increase of five hundredths of one percentile in the test score distribution. Since this evidence is only meaningful if test scores are related to choices and outcomes, recall Figure 1 showing strong sorting patterns into initial college enrollment choices across the test score distribution, and see Figure 6 for the tight relationship between test scores and earnings: average quarterly earnings around age 30 more than double across the range of the test score distribution.

The next several columns of Table 3 show the first stage and reduced form estimates. Each pair of sub-columns compares the results across exclusion and inclusion of the test score control, entering as a cubic polynomial. Given the balance result in the first column, we should expect these estimates to remain robust to test score inclusion, and this result is borne out across all of the first stages and reduced forms.\textsuperscript{45} The large gains in $R^2$ that result from test score inclusion provide further evidence that test scores strongly predict choices and outcomes. Table 3 also shows the statistical

\textsuperscript{45}Table 5 below also confirms robustness of the main results to the test score control.
Figure 7: Conditional Distributions of Estimated 2-Year Entry Propensity Scores

Notes: The left panel plots the distribution of the estimated propensity score of 2-year entry as a function of 2-year distance, estimated via the locally linear specification described in Section 5.1 and evaluated at the mean values of 4-year distance and the control set described in Section 5.2. The right panel plots the distribution of the estimated propensity score of 2-year entry as a function of 4-year distance, evaluated at the mean values of 2-year distance and the control set.

precision of the first stage estimates and their intuitive signs: 2-year distance decreases 2-year entry but increases 4-year entry, while 4-year distance decreases 4-year entry but increases 2-year entry. Figure 7 plots the estimated propensity score distributions of 2-year entry with respect to 2-year distance (left panel) and 4-year distance (right panel), evaluated at the mean values of the other distance dimension and the controls.

Finally, the bottom panel of Table 3 conducts an empirical check on the validity of the comparable compliers assumption (A3). The upper row reports the estimated mean test score percentile among 2–4 compliers with respect to $Z_2$, while the lower row reports the separately-identified mean test score percentile among 2–4 compliers with respect to $Z_4$. The two complier means are statistically indistinguishable, which lends credence towards the comparable compliers assumption of equal mean 2-year potential outcomes between these two groups given the strong predictive power of test scores on outcomes.
Table 4: Causal Effect Estimates

<table>
<thead>
<tr>
<th></th>
<th>MTE₂ = \omega MTE₂₀ + (1 - \omega) MTE₂⁻⁴</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Net effect</td>
</tr>
<tr>
<td>Years of schooling</td>
<td>0.931 0.654</td>
</tr>
<tr>
<td>Bachelor’s degree</td>
<td>0.105 0.654</td>
</tr>
<tr>
<td>Quarterly earnings</td>
<td>701 0.654 1,337 0.346 -499</td>
</tr>
</tbody>
</table>

Notes: Locally weighted observations: 556,726. All estimates are evaluated at the mean values of the instruments. Standard errors in parentheses are block bootstrapped at the high school campus by cohort level. Complier shares are the same across outcomes due to common first stage equations. Academic outcomes are measured at age 28. Quarterly earnings are measured in real 2010 U.S. dollars and averaged within person over ages 28-30.

6 Results

6.1 Main Results

Table 4 presents the main results. The first column shows the net effect of 2-year entry on each outcome, which pools the effects on 2–0 democratization compliers and 2–4 diversion compliers into a single weighted average. On net, 2-year college access boosts educational attainment and earnings: for the “average” complier induced into 2-year entry by closer access, completed schooling increases by roughly one year, bachelor’s degree attainment increases by 10.5 percentage points, and earnings per quarter around age 30 increase by a marginally significant 701 dollars. These extra earnings correspond to a 10.3 percent premium relative to the pooled counterfactual complier potential outcome mean of 6,835 dollars per quarter.

The next four columns of Table 4 decompose these net effects into the two poten-

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46Main estimates are evaluated at the mean values of the instruments. Section 6.5 below explores heterogeneity in the local estimates across the range of 2-year college proximity. Averaging the entire two-dimensional grid of estimates across the empirical distribution of the instruments yields average marginal treatment effects (AMTEs) that are similar to the reported MTE estimates evaluated at the mean.
entially opposing channels of democratization and diversion. Roughly two-thirds (0.654) of compliers would not have otherwise attended college, and these democratized 2-year entrants experience significant gains in all outcomes compared to their counterfactual of not attending any college. They complete 1.7 more years of schooling, are 26 percentage points more likely to earn a bachelor’s degree, and earn 1,337 dollars more per quarter around age 30 relative to never enrolling in college, which corresponds to a 21.8 percent earnings premium over their counterfactual potential outcome mean of 6,141 dollars per quarter. Considering that the net tuition price of attending a 2-year college for the average student in the U.S. is approximately zero after grant aid, this represents a healthy private return to 2-year entry along the democratization margin; from a social perspective, the average cost of educating a full-time community college student is roughly 10 thousand dollars per year of enrollment, so it takes only about $10,000/(1,337 \times 4) \approx 2$ years of (undiscounted) higher earnings to recoup each year of upfront social investment.

Diverted students, on the other hand, make up the other third (0.346) of compliers, and they end up with lower average outcomes as a result of starting college at a 2-year instead of a 4-year institution. Diverted students complete roughly half a year less of total education and are 18 percentage points less likely to complete a bachelor’s degree relative to their counterfactual of starting directly at a 4-year institution, leading to a negative but statistically imprecise impact on earnings around age 30. A prospective student along the diversion margin comparing the average net tuition price of 2-year entry ($\sim$0 dollars per year) to the average net tuition price of public 4-year entry ($\sim$3,500 dollars per year) would have to severely discount the future to rationalize perpetual earnings losses in exchange for the upfront private savings of roughly 3,500 dollars per year enrolled in a 2-year instead of a 4-year institution; the social calculation is quite similar given a similar difference of roughly 3,500 dollars per year in educational expenditures per student between public community colleges ($\sim$10,000 dollars) and public baccalaureate institutions ($\sim$13,500).

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47 Net price statistics throughout this section come from College Board (2018).
48 Exploration of the mechanisms driving these negative diversion effects remains an avenue for future work. See Monaghan and Attewell (2015) for a propensity score matching approach that suggests the importance of credit loss during 2-year to 4-year transfer, and Xu (2018) for evidence suggesting adverse impacts of 2-year colleges’ greater reliance on part-time adjunct faculty relative to 4-year institutions.
# Table 5: Causal Effect Estimates: Robustness Checks

<table>
<thead>
<tr>
<th></th>
<th>(1) Baseline specification</th>
<th>(2) No clustering</th>
<th>(3) Add test score control</th>
<th>(4) Bandwidth 35 miles</th>
<th>(5) Bandwidth 30 miles</th>
</tr>
</thead>
<tbody>
<tr>
<td>2-0 Democratization share</td>
<td>.654 (0.048)</td>
<td>.654 (0.023)</td>
<td>.638 (0.047)</td>
<td>.649 (0.058)</td>
<td>.655 (0.053)</td>
</tr>
<tr>
<td>Years of schooling</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>2-0 Democratization effect</td>
<td>1.72 (0.201)</td>
<td>1.72 (0.137)</td>
<td>1.69 (0.196)</td>
<td>1.69 (0.210)</td>
<td>1.67 (0.222)</td>
</tr>
<tr>
<td>2-4 Diversion effect</td>
<td>-.557 (0.359)</td>
<td>-.557 (0.198)</td>
<td>-.647 (0.320)</td>
<td>-.617 (0.373)</td>
<td>-.639 (0.448)</td>
</tr>
<tr>
<td>Bachelor’s degree</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>2-0 Democratization effect</td>
<td>.257 (0.036)</td>
<td>.257 (0.026)</td>
<td>.256 (0.037)</td>
<td>.252 (0.038)</td>
<td>.252 (0.040)</td>
</tr>
<tr>
<td>2-4 Diversion effect</td>
<td>-.182 (0.088)</td>
<td>-.182 (0.048)</td>
<td>-.202 (0.080)</td>
<td>-.192 (0.087)</td>
<td>-.192 (0.106)</td>
</tr>
<tr>
<td>Quarterly earnings</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>2-0 Democratization effect</td>
<td>1,337 (731)</td>
<td>1,337 (398)</td>
<td>1,302 (726)</td>
<td>1,546 (625)</td>
<td>1,665 (822)</td>
</tr>
<tr>
<td>2-4 Diversion effect</td>
<td>-499 (874)</td>
<td>-499 (619)</td>
<td>-713 (804)</td>
<td>-553 (882)</td>
<td>-770 (1,050)</td>
</tr>
<tr>
<td>Locally weighted obs.</td>
<td>556,726</td>
<td>556,726</td>
<td>556,726</td>
<td>542,279</td>
<td>526,714</td>
</tr>
</tbody>
</table>

Notes: All estimates are evaluated at the mean values of the instruments. Standard errors in parentheses are block bootstrapped at the high school campus by cohort level, except in column (2), where bootstrapping is at the individual student level. Academic outcomes are measured at age 28. Quarterly earnings are measured in real 2010 U.S. dollars and averaged within person over ages 28-30.

## 6.2 Robustness Checks and Comparisons to Other Approaches

Table 5 conducts several robustness checks to probe the sensitivity of the results to alternative specifications. Column (1) transposes the baseline point estimates and standard errors from Table 4 for comparison. Column (2) shows how the standard errors decrease when bootstrapping at the individual student level rather than clustering at the high school campus by cohort level. Column (3) adds the excluded test score measure to the control set, column (4) decreases the local regression bandwidth from 40 to 35 miles, and column (5) further reduces it to 30 miles; none of these alternative specifications lead to meaningful changes in the estimates.
Table 6 compares the main estimates to those resulting from other identification
approaches. Column (1) transposes the main nonparametric IV estimates from Ta-
ble 4. Column (2) estimates the controlled OLS specification from Section 3.6 using
the same local observation weighting as the main IV specification in column (1). The
main diversion IV estimates are meaningfully smaller in magnitude than those implied
by controlled OLS, suggesting that unobserved differences between 2-year and 4-year
entrants bias the OLS diversion estimates towards larger negative magnitudes.\footnote{Differences between the IV and OLS estimates could also be driven by treatment effect heterogeneity.} Column (3) presents results from the multivariate 2SLS specification corresponding to this
setting: a 2SLS regression of outcomes on indicators for no college and 4-year entry,
yielding democratization versus diversion comparisons relative to the omitted 2-year
treatment, using the same instruments, control set, and local observation weighting as
the main nonparametric IV approach in column (1). As shown in Appendix B, the
multivariate 2SLS estimands fuse together multiple treatment margins across multiple
complier subpopulations, making them difficult to interpret when treatment effects are
heterogeneous. Under the strong assumption of constant treatment effects across in-
dividuals, the estimands of the nonparametric IV approach and the multivariate 2SLS
approach would coincide; instead, the substantial differences in the estimates between
columns (1) and (3) provide evidence against this homogeneity assumption.

6.3 Heterogeneity by Gender

Table 7 stratifies the main results by gender. Men and women have nearly identical
complier shares along each enrollment margin, but the similarities end there: women
drive the main results with effects of larger magnitude than men for every outcome along
every margin. While men experience positive gains in educational attainment along
the 2–0 democratization margin, their 2–4 diversion losses are small and insignificant,
and male earnings appear relatively unaffected by 2-year entry along both margins.
Women, meanwhile, experience large gains in educational attainment and significant
earnings returns to 2-year entry among 2–0 democratization compliers who otherwise
would not have attended college, consistent with the large OLS literature documenting
### Table 6: Causal Effect Estimates: Comparisons to Other Approaches

<table>
<thead>
<tr>
<th></th>
<th>(1) Nonparametric IV approach</th>
<th>(2) Controlled OLS</th>
<th>(3) Multivariate 2SLS</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Years of schooling</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>2-0 Democratization effect</td>
<td>1.72</td>
<td>1.49</td>
<td>2.21</td>
</tr>
<tr>
<td></td>
<td>(.201)</td>
<td>(.01)</td>
<td>(.208)</td>
</tr>
<tr>
<td>2-4 Diversion effect</td>
<td>-.557</td>
<td>-1.34</td>
<td>-1.49</td>
</tr>
<tr>
<td></td>
<td>(.359)</td>
<td>(.01)</td>
<td>(.171)</td>
</tr>
<tr>
<td><strong>Bachelor’s degree</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>2-0 Democratization effect</td>
<td>.257</td>
<td>.150</td>
<td>.350</td>
</tr>
<tr>
<td></td>
<td>(.036)</td>
<td>(.001)</td>
<td>(.041)</td>
</tr>
<tr>
<td>2-4 Diversion effect</td>
<td>-.182</td>
<td>-.356</td>
<td>-.358</td>
</tr>
<tr>
<td></td>
<td>(.088)</td>
<td>(.002)</td>
<td>(.036)</td>
</tr>
<tr>
<td><strong>Quarterly earnings</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>2-0 Democratization effect</td>
<td>1,337</td>
<td>1,068</td>
<td>2,456</td>
</tr>
<tr>
<td></td>
<td>(731)</td>
<td>(18)</td>
<td>(617)</td>
</tr>
<tr>
<td>2-4 Diversion effect</td>
<td>-499</td>
<td>-1,440</td>
<td>-2,610</td>
</tr>
<tr>
<td></td>
<td>(874)</td>
<td>(25)</td>
<td>(531)</td>
</tr>
<tr>
<td><strong>Observations</strong></td>
<td>556,726</td>
<td>556,726</td>
<td>556,726</td>
</tr>
</tbody>
</table>

Notes: Column (1) reproduces the main estimates from Table 4. Estimates in column (2) come from the controlled OLS regressions described in Section 3.6 and presented in Table 2, but now weighted by the same local weighting scheme as the main IV estimates in column (1) to ensure matched samples across the columns. Multivariate 2SLS in column (3) is a two stage least squares regression of outcomes on indicators for no college and 4-year entry, yielding democratization and diversion parameters relative to the omitted 2-year treatment, using the same instruments, control set, and local observation weighting as the nonparametric IV approach in column (1). Standard errors in parentheses are block bootstrapped at the high school campus by cohort level. Academic outcomes are measured at age 28. Quarterly earnings are measured in real 2010 U.S. dollars and averaged within person over ages 28-30.
Table 7: Causal Effect Estimates: Women vs. Men

<table>
<thead>
<tr>
<th></th>
<th>Women</th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>$MTE^2$ = $\omega MTE_{2\rightarrow 0}$ + $(1 - \omega) MTE_{2\rightarrow 4}$</td>
<td>$\omega$</td>
<td>Democratization share</td>
<td>Democratization effect</td>
<td>Diversion share</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Net effect</td>
<td>Democratization share</td>
<td>Democratization effect</td>
<td>Diversion share</td>
</tr>
<tr>
<td>Years of schooling</td>
<td>1.05 (.265)</td>
<td>.650 (.048)</td>
<td>2.04 (.239)</td>
<td>.350 (.048)</td>
<td>-.772 (.400)</td>
</tr>
<tr>
<td>Bachelor’s degree</td>
<td>.124 (.053)</td>
<td>.650 (.048)</td>
<td>.324 (.042)</td>
<td>.350 (.048)</td>
<td>-.247 (.098)</td>
</tr>
<tr>
<td>Quarterly earnings</td>
<td>1.587 (479)</td>
<td>.650 (.048)</td>
<td>2.863 (580)</td>
<td>.350 (048)</td>
<td>-779 (955)</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th></th>
<th>Men</th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>$MTE^2$ = $\omega MTE_{2\rightarrow 0}$ + $(1 - \omega) MTE_{2\rightarrow 4}$</td>
<td>$\omega$</td>
<td>Democratization share</td>
<td>Democratization effect</td>
<td>Diversion share</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Net effect</td>
<td>Democratization share</td>
<td>Democratization effect</td>
<td>Diversion share</td>
</tr>
<tr>
<td>Years of schooling</td>
<td>.785 (.339)</td>
<td>.659 (.063)</td>
<td>1.30 (.318)</td>
<td>.341 (.063)</td>
<td>-.211 (.514)</td>
</tr>
<tr>
<td>Bachelor’s degree</td>
<td>.083 (.062)</td>
<td>.659 (.063)</td>
<td>.166 (.054)</td>
<td>.341 (.063)</td>
<td>-.078 (.130)</td>
</tr>
<tr>
<td>Quarterly earnings</td>
<td>-216 (929)</td>
<td>.659 (.063)</td>
<td>-320 (1,386)</td>
<td>.341 (.063)</td>
<td>-15 (1,210)</td>
</tr>
</tbody>
</table>

Notes: Locally weighted observations: 287,979 (women), 268,747 (men). All estimates are evaluated at the mean values of the instruments. Standard errors in parentheses are block bootstrapped at the high school campus by cohort level. Complier shares are the same across outcomes due to common first stage equations. Academic outcomes are measured at age 28. Quarterly earnings are measured in real 2010 U.S. dollars and averaged within person over ages 28-30.
Figure 8: Earnings Effect Profiles by Gender

![Graph showing earnings effect profiles by gender](image)

Notes: This figure plots marginal treatment effect estimates of 2-year entry on quarterly earnings averaged within three different age windows: 22-24, 25-27, and 28-30. All estimates are evaluated at the mean values of the instruments.

A female premium in the returns to 2-year college enrollment relative to nonattendance (Belfield and Bailey, 2011, 2017). Diverted women, on the other hand, experience significant losses in educational attainment and a modestly large (though imprecise) decline in earnings relative to their 4-year entry counterfactual.

To gauge the evolution of male and female earnings effects across the early-career lifecycle, Figure 8 estimates mean quarterly earnings effects separately across the three age windows of 22-24, 25-27, and 28-30 (pooled for greater precision), then plots these estimates to yield dynamic effect profiles by gender. The left and middle panels provide context for the roughly zero earnings effects around age 30 for men on net and along the 2-0 democratization margin, showing that these null effects are actually preceded by negative returns at earlier ages: men on the margin between 2-year entry and no college who do enroll end up taking their entire 20s to overtake the earnings of those who do not enroll. Extrapolating from these profiles suggests that marginal men will start to reap positive returns to 2-year entry in their 30s, while women already begin experiencing positive effects on net and along the democratization margin in the early 20s and enjoy steadily increasing effects over at least the next decade.\(^{50}\)

\(^{50}\)Recall similar gender differences in the raw earnings profiles of Figure 3. Exploring the mechanisms behind these gender differentials, including mediation through field of study and occupational choice,
Table 8: Causal Effect Estimates: Disadvantaged Students

<table>
<thead>
<tr>
<th></th>
<th>$MTE_{2\rightarrow 0}$</th>
<th>$MTE_{2\rightarrow 4}$</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>$\omega$</td>
<td>$1 - \omega$</td>
</tr>
<tr>
<td>Net effect</td>
<td>$\delta$</td>
<td>$\beta$</td>
</tr>
<tr>
<td>Democratization share</td>
<td>$\delta - \beta$</td>
<td>$\delta - \beta$</td>
</tr>
<tr>
<td>Democratization effect</td>
<td>$\delta - \beta$</td>
<td>$\delta - \beta$</td>
</tr>
<tr>
<td>Diversion share</td>
<td>$\delta - \beta$</td>
<td>$\delta - \beta$</td>
</tr>
<tr>
<td>Diversion effect</td>
<td>$\delta - \beta$</td>
<td>$\delta - \beta$</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th></th>
<th>Years of schooling</th>
<th>Bachelor’s degree</th>
<th>Quarterly earnings</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>.859 (.262)</td>
<td>.084 (.046)</td>
<td>1,373 (687)</td>
</tr>
<tr>
<td></td>
<td>.807 (.056)</td>
<td>.807 (.056)</td>
<td>.807 (.056)</td>
</tr>
<tr>
<td></td>
<td>1.04 (.220)</td>
<td>.100 (.038)</td>
<td>1,730 (769)</td>
</tr>
<tr>
<td></td>
<td>.193 (.056)</td>
<td>.193 (.056)</td>
<td>.193 (.056)</td>
</tr>
<tr>
<td></td>
<td>.101 (.982)</td>
<td>.017 (.241)</td>
<td>-122 (2,816)</td>
</tr>
</tbody>
</table>

Notes: Locally weighted observations: 197,260. Disadvantaged is an indicator for free or reduced price lunch eligibility in 10th grade. All estimates are evaluated at the mean values of the instruments. Standard errors in parentheses are block bootstrapped at the high school campus by cohort level. Complier shares are the same across outcomes due to common first stage equations. Academic outcomes are measured at age 28. Quarterly earnings are measured in real 2010 U.S. dollars and averaged within person over ages 28-30.

the diversion margin, the rightmost panel of Figure 8 shows that the weak earnings effects along the 2–4 diversion margin do not have a detectable age trend, offering no evidence that diversion impacts will grow larger, at least across these early career ages currently observable in the data.

6.4 Impacts on Disadvantaged Students and Implications for Upward Mobility

Table 8 limits the sample to disadvantaged students, as measured by eligibility for subsidized meals in high school. Low-income students are a key constituency in policy debates over community colleges, since they are disproportionately more likely to enroll in 2-year rather than 4-year institutions and are likely the most sensitive to policies that reduce 2-year entry costs. The second column of Table 8 shows that when 2-year access expands, disadvantaged students are overwhelmingly on the 2–0 democratization margin: 81 percent of disadvantaged students who are induced into 2-year entry thanks to closer access would not have otherwise attended any college, while just 19 percent remains an important avenue for future work.
are diverted from immediate 4-year entry.

The results in the third column of Table 8 show that disadvantaged students “democratized” into higher education along the 2–0 margin experience smaller-than-average gains in educational attainment, but slightly larger-than-average earnings returns. This suggests that 2-year college enrollment may involve other labor market benefits for disadvantaged students beyond modest increases in formal educational attainment, such as better access to employer networks, short course sequences teaching readily-employable skills, and improved job matching. Taken together, these results suggest that boosting the upward earnings mobility of disadvantaged youth need not require large increases in years of formal postsecondary schooling or a narrow focus on bachelor’s degree attainment; simply attracting more disadvantaged students into 2-year colleges may confer meaningful earnings benefits through other channels, and identifying these specific channels remains an important avenue for future work.51

6.5 Selection Patterns

Stratifying the results across the range of 2-year proximity permits exploration of selection patterns and helps probe the external validity of the local IV estimates. Since the separate identification approach directly identifies complier mean potential outcomes along each treatment margin, Figure 9 first plots these mean potential outcome estimates in the top panel, stratifying across the range of 2-year distance while holding 4-year distance fixed at its mean.52 As 2-year distance increases, the mean potential earnings of marginally-induced compliers increase along all treatment margins. This implies 2-year entrants are positively selected on potential outcome levels, since the individuals who are enticed into 2-year entry by a marginal decrease in 2-year distance while still remaining far away from the nearest campus must be those with the greatest unobserved preference for 2-year entry, and these eager compliers also happen to have

51 Since the geographic accessibility of 2-year colleges is a characteristic of a local neighborhood, these results relate to the active literature studying the relationship between neighborhoods and the upward mobility of low-income youth, e.g. Kling et al. (2007), Chetty et al. (2016), and Chetty and Hendren (2016).

52 For the binary IV case, Huber and Mellace (2014) and Kitagawa (2015) develop specification tests from necessary restrictions on potential outcome distributions. This logic could be extended to the case of multiple treatments and instruments but is not pursued here.
Figure 9: Selection Patterns

Notes: Each estimate is evaluated at a given 2-year distance value holding 4-year distance fixed at its mean. Quarterly earnings are measured in real 2010 U.S. dollars and averaged within person over ages 28-30. 90 percent confidence intervals are estimated via block bootstrap at the high school campus by cohort level.
the highest mean potential outcomes. The ranking of mean potential outcome levels across treatments and complier groups also yields an intuitive pattern: the 4-year outcome for compliers along the 2–4 diversion margin dominates all others, followed by the 2-year outcome for these 2–4 compliers, which lies almost entirely above the 2-year outcome for compliers along the 2–0 democratization margin, all of which strongly dominate the no-college outcome for 2–0 compliers. This ranking makes sense in light of a mean test score percentile of 54 for compliers along the 2–4 diversion margin compared to 32 for compliers along the 2–0 democratization margin.

The middle panel of Figure 9 takes differences of the potential outcomes in the top panel to form the marginal treatment effects of interest. The estimates at the mean 2-year distance of 10 miles correspond to the main results in Table 4, while stratifying across other 2-year proximity values permits exploration of selection-on-gains patterns and external validity. The 90 percent confidence intervals cannot reject that the effects along all margins are flat across the empirical support of 2-year proximity, though the first stage estimates in Table 3 imply that this 35-mile range only spans a 15 percentage point change in the propensity score of 2-year entry—far from full support across the unit interval of potential propensity score variation. If anything, the net effect of 2-year entry increases slightly as 2-year distance decreases over this range, i.e. as 2-year access further increases. This is not driven by meaningful slopes in the 2–0 or 2–4 marginal treatment effects, but rather by an increasing share (as 2-year distance decreases to the left) of compliers who are on the 2–0 margin, as shown in the bottom panel of Figure 9. The large confidence intervals preclude a precise conclusion, but such a pattern would imply reverse-Roy negative selection on net gains driven primarily by a change in the composition of marginal compliers: as 2-year access further expands, the net returns to marginal 2-year entrants increase because a greater share of them are “democratized” into higher education rather than diverted from 4-year college entry.53

53See Aakvik et al. (2005), Walters (2017), Kline and Walters (2016), and Cornelissen et al. (2017) for evidence of reverse-Roy selection patterns into educational programs.
6.6 Policy Simulations

All of the preceding IV estimates are specific to compliers who would change their initial college enrollment behavior in response to changes in college proximities. These estimates can help readily quantify the consequences of policies that increase students’ physical access to 2-year colleges, like building new community college campuses; with additional assumptions of external validity, these estimates may also help forecast the impacts of a wider range of policies that expand access to 2-year colleges, like tuition subsidies or targeted outreach programs. I use the IV estimates to conduct two policy simulations as illustrations towards these ends. First, I remain within the college proximity setting and explore the implications of marginally decreasing 2-year college distance on earnings gaps by gender and by family income. Second, under the strong additional assumption that my main treatment effect estimates in Table 4 have external validity in other policy settings, I quantify how the magnitude and even sign of the net effects of a wide range of 2-year access policies depend on what fraction of policy-induced 2-year entrants are diverted from 4-year college entry.

To carry out the first simulation, consider a decrease in 2-year college distance resulting from building a new local 2-year campus: how does this affect local earnings gaps between men and women, and between students from low-income and higher-income backgrounds? The expected reduced form effect on quarterly earnings among women is a net increase of 7.90 dollars per mile reduction in 2-year distance, while men experience roughly no change in earnings (an insignificant decrease of 78 cents); hence, expanding access to 2-year colleges by decreasing distance serves to reduce the overall gender earnings gap, with larger reductions expected for larger decreases in distance. Since the reduced form effect is the product of the first stage effect and the marginal treatment effect, a natural decomposition arises: the larger reduced form effect for women is driven by both a larger first stage effect (2-year entry is more responsive to 2-year distance for women than men) and a larger marginal treatment effect (marginal women induced into 2-year entry earn a bigger return than marginal men). Quantifying this decomposition, since the male first stage is roughly 25% smaller than the female

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54 For ease of exposition, all estimates used in the policy simulations in this section are evaluated from the starting point of mean 2-year and 4-year distances, corresponding to the results in previous sections.
first stage, if women had the same 2-year entry responsiveness to 2-year distance as men, the reduction in the gender earnings gap would be 25% smaller.

Similar logic suggests that reducing distance to 2-year colleges decreases the overall earnings gap between low-income students (those eligible for subsidized meals in high school) and their higher-income counterparts. The marginal reduced form effect on quarterly earnings among low-income students is a net increase of 6.55 dollars per mile reduction in 2-year distance, while higher-income students gain only 2.01 dollars per mile reduced. And again, this reduction in the earnings gap is driven both by a larger first stage effect for low-income students and a larger marginal treatment effect: the earnings gap reduction would be 20% smaller if low-income students had the same responsiveness to 2-year distance as higher-income students.

To carry out the second policy simulation, note that the net effects of other hypothetical policies that expand 2-year college access are also likely comprised of a share-weighted average of the democratization effect among 2-0 compliers (new 2-year entrants who otherwise would not have enrolled in college) and the diversion effect among 2-4 compliers (new 2-year entrants who otherwise would have started at 4-year institutions), assuming that marginal unordered monotonicity (A2) continues to hold, i.e. that such policies make 2-year entry weakly more attractive at the expense of 4-year entry and not attending any college. Under the strong additional assumption that the main democratization and diversion treatment effect estimates from Table 4 are externally valid with respect to other policy changes, the net effects of such policies on student outcomes can be forecasted given information on the share of policy compliers who are on the 2-4 diversion margin, which can be calculated solely from initial policy-induced changes in enrollment shares.55

Figure 10 visualizes the entire range of net effects forecasted for different hypothetical policies, with each policy indexed by its diversion share. A hypothetical policy with a diversion share of zero yields a net effect equal to the democratization effect from Table 4, since all compliers would be along the democratization margin; a diversion share of one yields a net effect equal to the diversion effect from Table 4, since all com-

55Specifically, as discussed in Section 4.1, the diversion share is identified by the policy-induced reduction in 4-year enrollment as a fraction of the policy-induced increase in 2-year enrollment.
Notes: This figure plots the net effects of hypothetical 2-year college access policies indexed by their share of compliers who are along the 2-4 diversion margin, assuming the main 2-0 democratization and 2-4 diversion effect estimates in Table 4 are externally valid. A diversion share of zero yields a hypothetical net effect equal to the democratization effect from Table 4; a diversion share of one yields a net effect equal to the diversion effect from Table 4; and every hypothetical policy in between is simply a convex combination of these two effects, weighted by the diversion share. The dashed vertical line marks the 35% diversion share corresponding to marginal reductions in 2-year college distance, such that the y-axis value at the intersection of the dashed line and the diagonal solid line equals the net effect estimate reported in Table 4. Another policy example is the Tennessee Promise Program, which (as discussed in the Introduction) began offering free 2-year college tuition to recent Tennessee high school graduates in 2014. Figure 11 plots the raw time series patterns of public 2-year and 4-year college enrollment around the implementation of the program: the raw post-policy decrease in 4-year enrollment divided by the increase in 2-year enrollment suggests a diversion share of roughly 50%, which applied to Figure 10 forecasts positive net effects on the educational attainment and earnings of induced 2-year entrants, though smaller than the net effects from reducing 2-year college distance and likely substantially smaller than presumably less diversionary policies like targeted outreach programs to low-income compliers would be along the diversion margin; and every hypothetical policy in between is simply a convex combination of these two effects, with the policy’s diversion share serving as the weight. To take the setting of this paper as an example, the dashed vertical line marks the 35% diversion share corresponding to marginal reductions in 2-year college distance, such that the y-axis value at the intersection of the dashed line and the diagonal solid line equals the net effect estimate reported in Table 4. Another policy example is the Tennessee Promise Program, which (as discussed in the Introduction) began offering free 2-year college tuition to recent Tennessee high school graduates in 2014. Figure 11 plots the raw time series patterns of public 2-year and 4-year college enrollment around the implementation of the program: the raw post-policy decrease in 4-year enrollment divided by the increase in 2-year enrollment suggests a diversion share of roughly 50%, which applied to Figure 10 forecasts positive net effects on the educational attainment and earnings of induced 2-year entrants, though smaller than the net effects from reducing 2-year college distance and likely substantially smaller than presumably less diversionary policies like targeted outreach programs to low-income
Figure 11: College Enrollment Patterns and the Tennessee Promise Program

![Graph showing college enrollment patterns and the Tennessee Promise Program](image)

Notes: This figure plots enrollment shares in public 2-year and public 4-year colleges among recent high school graduates in Tennessee around the implementation of the Tennessee Promise Program. Data source: Tennessee Higher Education Commission.

students. Moving further rightward on the x-axes of Figure 10 suggests the net effects of 2-year access policies begin to turn negative as the diversion share reaches roughly .75, with bachelor’s degree completion turning negative even sooner. These hypothetical net effects are inherently speculative and come with many caveats, since they rely on strong assumptions of external validity, but they offer a first pass at forecasts of the net impacts of other types of 2-year access policies and provide a framework for quantifying the tradeoff between democratizing new students into higher education and diverting college-bound students from 4-year entry.

7 Conclusion

In light of slowing college attainment and rising inequality across education levels in the United States, policymakers are increasingly looking to 2-year community colleges as key policy levers in extending higher education to a broader share of American youth. This paper has empirically explored the consequences of expanding access to 2-year colleges, mindful of the potential tradeoff between attracting new students into higher
education and diverting those already bound for college away from 4-year enrollment.

Decomposing the net impacts of 2-year college access into effects along these two distinct enrollment margins presents a methodological challenge, since standard instrumental variables methods are not generally equipped to disentangle such effects. I show how a separate identification approach, guided by the flows of different compliers to different instruments, can secure identification of causal effects along these distinct complier margins. I apply the method using linked administrative data spanning the state of Texas, leveraging instrumental variation in 2-year and 4-year college proximities net of controls for neighborhood quality, neighborhood urbanization, and fixed effects for each local labor market in Texas. I verify that this residual proximity variation is balanced across excluded test scores that strongly predict enrollment choices and outcomes, and I show that the assumption of comparable compliers along the diversion margin with respect to marginal shifts in 2-year versus 4-year proximity has empirical support through equal mean test scores across these two complier groups.

The empirical results suggest that expanding access to 2-year colleges does boost the aggregate educational attainment and earnings of new 2-year entrants, but decomposing these net effects reveals substantial heterogeneity along several dimensions: students diverted from 4-year entry face lower outcomes, those who would not have otherwise attended college experience large gains, women experience larger effects along both margins compared to men, and disadvantaged students reap large earnings returns to 2-year entry with little offsetting diversion. Taken together, these results suggest that broad expansions of 2-year college access have different implications for the upward mobility of different types of students, leaving open the potential for more targeted policies to achieve greater net impacts with fewer unintended consequences.
References


Appendix: For Online Publication

A Proof of Binary 2SLS Decomposition

This appendix section derives the binary two-stage least squares (2SLS) decomposition in Equation (1) from Section 4.1, showing that 2SLS estimates a weighted average of local average treatment effects along the 2–0 (2-year entry vs. no college) and 2–4 (2-year entry vs. 4-year entry) complier margins.\textsuperscript{56} Recall the 2SLS specification:

\[ Y = \beta_0 + \beta_2 D_2 + \epsilon \]

\[ E[D_2|Z_2] = \alpha_0 + \alpha_2 Z_2, \]

where \( Y \) is a student outcome, \( D_2 \) is an indicator for 2-year college entry, and \( Z_2 \) is an exogenous and excludable binary instrument that induces students into 2-year entry from the alternative treatments of no college (\( D_0 \)) and 4-year entry (\( D_4 \)). In this system, \( \beta_2 \) is the familiar Wald (1940) estimand:

\[ \beta_2 = \frac{E[Y|Z_2 = 1] - E[Y|Z_2 = 0]}{E[D_2|Z_2 = 1] - E[D_2|Z_2 = 0]}. \]

Decompose \( E[Y|Z_2 = 1] \) in the numerator using the fact that \( Y = Y_0 D_0 + Y_2 D_2 + Y_4 D_4 \), where \( Y_j \) is the potential outcome associated with treatment \( j \in \{0, 2, 4\} \):

\[ E[Y|Z_2 = 1] = E[Y_0 D_0 + Y_2 D_2 + Y_4 D_4|Z_2 = 1] \]

\[ = E[Y_0 D_0|Z_2 = 1] + E[Y_2 D_2|Z_2 = 1] + E[Y_4 D_4|Z_2 = 1] \]

\[ = E[Y_0|D_0 = 1, Z_2 = 1]Pr(D_0 = 1|Z_2 = 1) + E[Y_2|D_2 = 1, Z_2 = 1]Pr(D_2 = 1|Z_2 = 1) + E[Y_4|D_4 = 1, Z_2 = 1]Pr(D_4 = 1|Z_2 = 1). \]

Letting \( D(z_2) \in \{0, 2, 4\} \) denote the potential choice an individual would make if exogenously assigned to \( Z_2 = z_2 \in \{0, 1\} \), by instrument independence and exclusion this

\textsuperscript{56}Heckman and Urzua (2010), Hull (2018b), and Kline and Walters (2016) provide related derivations, as do Angrist and Imbens (1995) for the case of ordered multivalued treatments.
becomes

$$E[Y|Z_2 = 1] = E[Y_0|D(1) = 0]Pr(D(1) = 0)$$
$$+ E[Y_2|D(1) = 2]Pr(D(1) = 2)$$
$$+ E[Y_4|D(1) = 4]Pr(D(1) = 4).$$

The monotonicity assumption that $Z_2$ induces students into $D_2$ from $D_0$ and $D_4$ permits
the following five complier groups: \{ $D(0) = 0, D(1) = 0$ \}, \{ $D(0) = 0, D(1) = 2$ \},
\{ $D(0) = 2, D(1) = 2$ \}, \{ $D(0) = 4, D(1) = 4$ \}, and \{ $D(0) = 4, D(1) = 2$ \}. Hence we
can further decompose:

$$E[Y|Z_2 = 1] = E[Y_0|D(0) = 0, D(1) = 0]Pr(D(0) = 0, D(1) = 0)$$
$$+ E[Y_2|D(0) = 0, D(1) = 2]Pr(D(0) = 0, D(1) = 2)$$
$$+ E[Y_2|D(0) = 2, D(1) = 2]Pr(D(0) = 2, D(1) = 2)$$
$$+ E[Y_2|D(0) = 4, D(1) = 2]Pr(D(0) = 4, D(1) = 2)$$
$$+ E[Y_4|D(0) = 4, D(1) = 4]Pr(D(0) = 4, D(1) = 4).$$

By analogous arguments, decompose $E[Y|Z_2 = 0]$ into

$$E[Y|Z_2 = 0] = E[Y_0|D(0) = 0, D(1) = 0]Pr(D(0) = 0, D(1) = 0)$$
$$+ E[Y_2|D(0) = 0, D(1) = 2]Pr(D(0) = 0, D(1) = 2)$$
$$+ E[Y_2|D(0) = 2, D(1) = 2]Pr(D(0) = 2, D(1) = 2)$$
$$+ E[Y_2|D(0) = 4, D(1) = 2]Pr(D(0) = 4, D(1) = 2)$$
$$+ E[Y_4|D(0) = 4, D(1) = 4]Pr(D(0) = 4, D(1) = 4).$$

Subtracting $E[Y|Z_2 = 1] - E[Y|Z_2 = 0]$ eliminates the always-taker and never-taker
groups, leaving only the instrument compliers:

\[ E[Y|Z_2 = 1] - E[Y|Z_2 = 0] = E[Y_2|D(0) = 0, D(1) = 2]Pr(D(0) = 0, D(1) = 2) \]
\[ - E[Y_0|D(0) = 0, D(1) = 2]Pr(D(0) = 0, D(1) = 2) + E[Y_2|D(0) = 4, D(1) = 2]Pr(D(0) = 4, D(1) = 2) \]
\[ - E[Y_4|D(0) = 4, D(1) = 2]Pr(D(0) = 4, D(1) = 2) = E[Y_2 - Y_0|D(0) = 0, D(1) = 2]Pr(D(0) = 0, D(1) = 2) \]
\[ + E[Y_2 - Y_4|D(0) = 4, D(1) = 2]Pr(D(0) = 4, D(1) = 2) . \]

To identify the two complier probabilities \( Pr(D(0) = 0, D(1) = 2) \) and \( Pr(D(0) = 4, D(1) = 2) \), recall from above that independence and monotonicity of the instrument imply

\[ Pr(D_0|Z_2 = 0) = Pr(D(0) = 0) = Pr(D(0) = 0, D(1) = 0) + Pr(D(0) = 0, D(1) = 2) \]
\[ Pr(D_0|Z_2 = 1) = Pr(D(1) = 0) = Pr(D(0) = 0, D(1) = 0) \]
\[ \implies Pr(D_0|Z_2 = 0) - Pr(D_0|Z_2 = 1) = Pr(D(0) = 0, D(1) = 2) \]

\[ Pr(D_4|Z_2 = 0) = Pr(D(0) = 4) = Pr(D(0) = 4, D(1) = 4) + Pr(D(0) = 4, D(1) = 2) \]
\[ Pr(D_4|Z_2 = 1) = Pr(D(1) = 4) = Pr(D(0) = 4, D(1) = 4) \]
\[ \implies Pr(D_4|Z_2 = 0) - Pr(D_4|Z_2 = 1) = Pr(D(0) = 4, D(1) = 2) . \]

This yields

\[ E[Y|Z_2 = 1] - E[Y|Z_2 = 0] = E[Y_2 - Y_0|D(0) = 0, D(1) = 2](E[D_0|Z_2 = 0] - E[D_0|Z_2 = 1]) \]
\[ + E[Y_2 - Y_4|D(0) = 4, D(1) = 2](E[D_4|Z_2 = 0] - E[D_4|Z_2 = 1]) , \]
and plugging this back into the Wald expression yields the result:

\[
\beta_2 = \frac{E[Y|Z_2 = 1] - E[Y|Z_2 = 0]}{E[D_2|Z_2 = 1] - E[D_2|Z_2 = 0]}
\]

\[
= \frac{E[Y_2 - Y_0|D(0) = 0, D(1) = 2][E[D_0|Z_2 = 0] - E[D_0|Z_2 = 1]]}{E[D_2|Z_2 = 1] - E[D_2|Z_2 = 0]}
\]

\[
+ \frac{E[Y_2 - Y_4|D(0) = 4, D(1) = 2][E[D_4|Z_2 = 0] - E[D_4|Z_2 = 1]]}{E[D_2|Z_2 = 1] - E[D_2|Z_2 = 0]}
\]

\[
= \omega E[Y_2 - Y_0|D(0) = 0, D(1) = 2] + (1 - \omega) E[Y_2 - Y_4|D(0) = 4, D(1) = 2]
\]

\[
= \omega \text{LATE}_{2\leftarrow 0} + (1 - \omega) \text{LATE}_{2\leftarrow 4},
\]

where the weights

\[
\omega = \frac{E[D_0|Z_2 = 1] - E[D_0|Z_2 = 0]}{E[D_2|Z_2 = 1] - E[D_2|Z_2 = 0]}, \quad (1 - \omega) = \frac{E[D_4|Z_2 = 1] - E[D_4|Z_2 = 0]}{E[D_2|Z_2 = 1] - E[D_2|Z_2 = 0]}
\]

result from the fact that \(D_0 + D_2 + D_4 = 1\).

\[\square\]

**B What Does Multivariate 2SLS Identify?**

This appendix section derives and decomposes the multivariate two-stage least squares (2SLS) estimands corresponding to the econometric setting in Section 4.2. To that end, assume A1-A3 (independence & exclusion, marginal unordered monotonicity, and comparable compliers), implicitly condition on \(X\), and consider a local region around an evaluation point \((z_2, z_4)\).

For maximum comparability to the parameters of interest in Section 4.2, consider

\[\text{See Kirkeboen et al. (2016) for a related derivation involving discrete instruments, a less restrictive monotonicity condition, and no comparable compliers assumption, which yields more complicated estimands due to additional margins of instrument compliance. See also Kline and Walters (2016) and Hull (2018b) for related derivations involving one binary instrument interacted with a stratifying covariate.}\]
the local multivariate 2SLS specification

\[
Y = \gamma + \beta_0 D_0 + \beta_4 D_4 + \epsilon
\]

\[
E[D_0|Z] = \alpha_0^0 + \alpha_2^0 Z_2 + \alpha_4^4 Z_4
\]

\[
E[D_4|Z] = \alpha_4^0 + \alpha_2^2 Z_2 + \alpha_4^4 Z_4
\]

where \( Z \equiv (Z_2, Z_4) \). I exclude \( D_2 = 1 \) as the reference treatment case to yield a \( 2 \leftarrow 0 \) comparison in \(-\beta_0\) and a \( 2 \leftarrow 4 \) comparison in \(-\beta_4\). These are the relevant estimands to compare to my \( MTE_{2 \leftarrow 0} \) and \( MTE_{2 \leftarrow 4} \), respectively.

To begin the derivation, plug the first stage conditional expectations into the reduced form:

\[
E[Y|Z] = \gamma + \beta_0 (\alpha_0^0 + \alpha_2^0 Z_2 + \alpha_4^4 Z_4) + \beta_4 (\alpha_4^0 + \alpha_2^2 Z_2 + \alpha_4^4 Z_4) + E[\epsilon|Z]
\]

\[
= \gamma + \beta_0 \alpha_0^0 + \beta_4 \alpha_4^0 + (\beta_0 \alpha_2^0 + \beta_4 \alpha_2^2) Z_2 + (\beta_0 \alpha_4^0 + \beta_4 \alpha_4^4) Z_4
\]

\[
= \alpha_0^0 + \alpha_2^2 Z_2 + \alpha_4^4 Z_4
\]

where \( E[\epsilon|Z] = 0 \) by A1. Note that

\[
\begin{pmatrix}
\alpha_2^2 \\
\alpha_4^4
\end{pmatrix}
= \begin{pmatrix}
\alpha_0^2 & \alpha_4^2 \\
\alpha_0^4 & \alpha_4^4
\end{pmatrix}
\begin{pmatrix}
\beta_0 \\
\beta_4
\end{pmatrix},
\]

so we can solve for \( \beta_0 \) and \( \beta_4 \) as

\[
\begin{pmatrix}
\beta_0 \\
\beta_4
\end{pmatrix}
= \begin{pmatrix}
\alpha_0^2 & \alpha_4^2 \\
\alpha_0^4 & \alpha_4^4
\end{pmatrix}^{-1}
\begin{pmatrix}
\alpha_2^2 \\
\alpha_4^4
\end{pmatrix}
\begin{pmatrix}
\beta_0 \\
\beta_4
\end{pmatrix}
\]

\[
= \frac{1}{\alpha_0^2 \alpha_4^4 - \alpha_0^4 \alpha_4^2}
\begin{pmatrix}
\alpha_4^2 & -\alpha_2^2 \\
-\alpha_0^4 & \alpha_0^2
\end{pmatrix}
\begin{pmatrix}
\alpha_2^2 \\
\alpha_4^4
\end{pmatrix}
\]

\[
\beta_0 = \frac{\alpha_0^2 \alpha_2^2 - \alpha_0^4 \alpha_4^2}{\alpha_0^2 \alpha_4^4 - \alpha_0^4 \alpha_4^2}
\]

\[
\beta_4 = \frac{\alpha_0^2 \alpha_2^4 - \alpha_0^4 \alpha_4^2}{\alpha_0^2 \alpha_4^4 - \alpha_0^4 \alpha_4^2}
\]
Using the complier mean potential outcome identification results of Section 4.3 implies

\[ \alpha_y^2 = \frac{\partial E[Y|Z]}{\partial Z_2} = \frac{\partial E[YD_0|Z]}{\partial Z_2} + \frac{\partial E[YD_2|Z]}{\partial Z_2} + \frac{\partial E[YD_4|Z]}{\partial Z_2} \]

\[ = E[Y_0|2-0] \frac{\partial E[D_0|Z]}{\partial Z_2} + E[Y_2|2-0] \left( - \frac{\partial E[D_0|Z]}{\partial Z_2} \right) \]

\[ + E[Y_2|2-4] \left( - \frac{\partial E[D_4|Z]}{\partial Z_2} \right) + E[Y_4|2-4] \frac{\partial E[D_4|Z]}{\partial Z_2} \]

\[ = - \frac{\partial E[D_0|Z]}{\partial Z_2} (E[Y_2|2-0] - E[Y_0|2-0]) - \frac{\partial E[D_4|Z]}{\partial Z_2} (E[Y_2|2-4] - E[Y_4|2-4]) \]

\[ = -\alpha_0^2 \text{MTE}_{2_{-0}} - \alpha_4^2 \text{MTE}_{2_{-4}} \]

where dependence of these quantities on the local evaluation point \((z_2, z_4)\) is suppressed and \(E[Y_0|2-0]\), for example, is shorthand for

\[ E[Y_0|2-0 \text{ complier w.r.t. } Z_2 \text{ at } (z_2, z_4)] = \lim_{z_2' \uparrow z_2} E[Y_0|D(z_2', z_4) = 2, D(z_2, z_4) = 0]. \]

Likewise with respect to \(Z_4\),

\[ \alpha_y^4 = \frac{\partial E[Y|Z]}{\partial Z_4} = \frac{\partial E[YD_0|Z]}{\partial Z_4} + \frac{\partial E[YD_2|Z]}{\partial Z_4} + \frac{\partial E[YD_4|Z]}{\partial Z_4} \]

\[ = E[Y_0|4-0] \frac{\partial E[D_0|Z]}{\partial Z_4} + E[Y_2|2-4 \text{ w.r.t. } Z_4] \frac{\partial E[D_4|Z]}{\partial Z_4} \]

\[ + E[Y_4|4-0] \left( - \frac{\partial E[D_0|Z]}{\partial Z_4} \right) + E[Y_4|2-4 \text{ w.r.t. } Z_4] \left( - \frac{\partial E[D_4|Z]}{\partial Z_4} \right) \]

where \(E[Y_0|4-0]\), for example, is shorthand for

\[ E[Y_0|4-0 \text{ complier w.r.t. } Z_4 \text{ at } (z_2, z_4)] = \lim_{z_4' \uparrow z_4} E[Y_0|D(z_2, z_4') = 4, D(z_2, z_4) = 0]. \]

By comparable compliers (A3), we can equate \(E[Y_2|2-4 \text{ w.r.t. } Z_4] = E[Y_2|2-4 \text{ w.r.t. } Z_2]\) at a given instrument evaluation point and thus write this mean complier potential outcome in shorthand as \(E[Y_2|2-4]\). Assumption A3 in the main text is silent about the relationship between \(E[Y_4|2-4 \text{ w.r.t. } Z_4]\) and \(E[Y_4|2-4 \text{ w.r.t. } Z_2]\), however, since no restrictions are needed on these \(Y_4\) potential outcomes to secure identification of
the desired treatment effects in the separate identification approach of this paper. To simplify the 2SLS decomposition, however, let us make a slightly stronger comparable compliers assumption and equate these mean \( Y \) potential outcomes across the \( Z_2 \) and \( Z_4 \) 2–4 complier groups, in addition to \( Y_2 \). Hence we equate \( E[Y_4|2–4 \text{ w.r.t. } Z_4] = E[Y_4|2–4 \text{ w.r.t. } Z_2] \equiv E[Y_4|2–4] \), which simplifies the expression for \( \alpha_4^4 \) to

\[
\alpha_4 = E[Y_0|4–0]\frac{\partial E[D_0|Z]}{\partial Z_4} + E[Y_2|2–4]\frac{\partial E[D_2|Z]}{\partial Z_4} + E[Y_4|4–0]\left(-\frac{\partial E[D_0|Z]}{\partial Z_4}\right) + E[Y_4|2–4]\left(-\frac{\partial E[D_2|Z]}{\partial Z_4}\right)
\]

\[
= -\frac{\partial E[D_0|Z]}{\partial Z_4}(E[Y_4|4–0] - E[Y_0|4–0]) + \frac{\partial E[D_2|Z]}{\partial Z_4}(E[Y_2|2–4] - E[Y_4|2–4])
\]

\[
= -\alpha_0^4 MTE_{4\leftarrow 0} - (\alpha_0^4 + \alpha_4^4)MTE_{2\leftarrow 4},
\]

again suppressing dependence on the local evaluation point \((z_2, z_4)\) and using the fact that

\[
\frac{\partial E[D_2|Z]}{\partial Z_4} = \frac{\partial E[1 - D_0 - D_4|Z]}{\partial Z_4} = -\frac{\partial E[D_0|Z]}{\partial Z_4} - \frac{\partial E[D_4|Z]}{\partial Z_4} = -\alpha_0^4 - \alpha_4^4.
\]

Plugging these results into the expressions above for \( \beta_0 \) and \( \beta_4 \) yields:

\[
\beta_0 = \frac{\alpha_4^4(-\alpha_0^4 MTE_{2\leftarrow 0} - \alpha_4^2 MTE_{2\leftarrow 4}) - \alpha_4^2(-\alpha_0^4 MTE_{4\leftarrow 0} - (\alpha_0^4 + \alpha_4^4)MTE_{2\leftarrow 4})}{\alpha_0^2\alpha_4^4 - \alpha_4^2\alpha_0^4}
\]

\[
= -\frac{\alpha_0^4\alpha_4^2MTE_{2\leftarrow 0} - \alpha_4^2\alpha_0^4(MTE_{4\leftarrow 0} + MTE_{2\leftarrow 4})}{\alpha_0^2\alpha_4^4 - \alpha_4^2\alpha_0^4},
\]

\[
\beta_4 = \frac{\alpha_0^2(-\alpha_0^4 MTE_{4\leftarrow 0} - (\alpha_0^4 + \alpha_4^4)MTE_{2\leftarrow 4}) - \alpha_4^4(-\alpha_0^4 MTE_{2\leftarrow 0} - \alpha_4^2 MTE_{2\leftarrow 4})}{\alpha_0^2\alpha_4^4 - \alpha_4^2\alpha_0^4}
\]

\[
= -\frac{(\alpha_0^2\alpha_4^4 - \alpha_4^2\alpha_0^4)MTE_{2\leftarrow 4} + (-\alpha_0^2\alpha_0^4)(MTE_{2\leftarrow 0} - MTE_{4\leftarrow 0})}{\alpha_0^2\alpha_4^4 - \alpha_4^2\alpha_0^4}.
\]

Finally, defining the weights

\[
\theta_0 = \frac{\alpha_0^2\alpha_4^4}{\alpha_0^2\alpha_4^4 - \alpha_4^2\alpha_0^4}, \quad \theta_4 = \frac{\alpha_0^2\alpha_4^4 - \alpha_4^2\alpha_0^4 + \alpha_0^2\alpha_0^4}{\alpha_0^2\alpha_4^4 - \alpha_4^2\alpha_0^4}
\]
yields the main result of this appendix section:

\[-\beta_0 = \theta_0 MTE_{2\leftarrow 0} + (1 - \theta_0)(MTE_{4\leftarrow 0} + MTE_{2\leftarrow 4})\]

\[-\beta_4 = \theta_4 MTE_{2\leftarrow 4} + (1 - \theta_4)(MTE_{2\leftarrow 0} - MTE_{4\leftarrow 0}).\]

Each local multivariate 2SLS estimand in this setting is thus a weighted average of the treatment effect for compliers along the treatment margin of interest and a biasing term involving effects for compliers along the other two treatment margins. In the special case of constant treatment effects across all individuals, note that \(MTE_{4\leftarrow 0} + MTE_{2\leftarrow 4} = (Y_4 - Y_0) + (Y_2 - Y_4) = Y_2 - Y_0\) and \(MTE_{2\leftarrow 0} - MTE_{4\leftarrow 0} = (Y_2 - Y_0) - (Y_4 - Y_0) = Y_2 - Y_4\), which confirms that 2SLS identifies the effects of interest in the absence of effect heterogeneity. With heterogeneous effects, however, \(MTE_{4\leftarrow 0} + MTE_{2\leftarrow 4} \neq MTE_{2\leftarrow 0}\) and \(MTE_{2\leftarrow 0} - MTE_{4\leftarrow 0} \neq MTE_{2\leftarrow 4}\) in general, leading to multivariate 2SLS estimands that generally do not recover a well-defined treatment effect for any well-defined complier population.

\[\square\]

C Selection Model Proofs

This appendix section shows how the selection model in Section 4.2 generates Assumption A2 (marginal unordered monotonicity) and Assumption A3 (comparable compliers) as necessary implications. Recall the choice equations

\[D_0(z_2, z_4) = 1[U_2 < \mu_2(z_2), U_4 < \mu_4(z_4)]\]

\[D_2(z_2, z_4) = 1[U_2 > \mu_2(z_2), U_4 - U_2 < \mu_4(z_4) - \mu_2(z_2)]\]

\[D_4(z_2, z_4) = 1[U_4 > \mu_4(z_4), U_4 - U_2 > \mu_4(z_4) - \mu_2(z_2)].\]
Letting \( f_{X,Y}(u,v) \) denote the joint density of random variables \((X,Y)\) evaluated at \((u,v)\), the three choice probabilities are thus

\[
Pr(D_0(z_2, z_4) = 1) = \int_{-\infty}^{\mu_2(z_2)} \int_{-\infty}^{\mu_4(z_4)} f_{U_2,U_4}(u,v) dv du \\
Pr(D_2(z_2, z_4) = 1) = \int_{-\infty}^{\mu_4(z_4)-\mu_2(z_2)} \int_{\mu_2(z_2)}^{\infty} f_{U_4-U_2,U_2}(u,v) dv du \\
Pr(D_4(z_2, z_4) = 1) = \int_{\mu_4(z_4)-\mu_2(z_2)}^{\infty} \int_{\mu_4(z_4)}^{\infty} f_{U_4-U_2,U_4}(u,v) dv du.
\]

Consider a marginal change in \( Z_2 \), corresponding to the top right panel of Figure 5. We have

\[
\frac{\partial Pr(D_2(z_2, z_4) = 1)}{\partial z_2} = \frac{\partial}{\partial z_2} \int_{-\infty}^{\mu_4(z_4)-\mu_2(z_2)} \int_{\mu_2(z_2)}^{\infty} f_{U_4-U_2,U_2}(u,v) dv du \\
= -\mu_2'(z_2) \int_{-\infty}^{\mu_4(z_4)-\mu_2(z_2)} f_{U_4-U_2,U_2}(u,\mu_2(z_2)) du - \mu_2'(z_2) \int_{\mu_2(z_2)}^{\infty} f_{U_4-U_2,U_2}(\mu_4(z_4)-\mu_2(z_2), v) dv \\
= -\mu_2'(z_2) \int_{-\infty}^{\mu_4(z_4)} f_{U_4,U_2}(u,\mu_2(z_2)) du - \mu_2'(z_2) \int_{\mu_2(z_2)}^{\infty} f_{U_4-U_2,U_2}(\mu_4(z_4)-\mu_2(z_2)) dv. \tag{4}
\]

The first term of line (4) is a line integral tracing out the set of indifferent individuals along the border between \( D(z_2, z_4) = 0 \) and \( D(z_2, z_4) = 2 \), i.e. marginal 2–0 compliers, since setting \( U_2 \) equal to \( \mu_2(z_2) \) implies \( I_2 = U_2 - \mu_2(z_2) = 0 = I_0 \). This margin of indifference is visualized in Figure 5 as the vertical line in the partition in the top left panel. The second term of line (4) traces out the set of indifferent individuals along the border between \( D(z_2, z_4) = 4 \) and \( D(z_2, z_4) = 2 \), i.e. marginal 2–4 compliers, since setting \( U_4 - U_2 \) equal to \( \mu_4(z_4) - \mu_2(z_2) \) implies \( I_4 - I_2 = U_4 - \mu_4(z_4) - (U_2 - \mu_2(z_2)) = 0 \). This margin of indifference is visualized in Figure 5 as the diagonal line in the partition in the top left panel. Hence, as visualized in the top right panel of Figure 5, a marginal shift in \( Z_2 \) induces 2–0 and 2–4 compliers and thus generates marginal unordered monotonicity (A2) in \( Z_2 \).

Considering a marginal change in \( Z_4 \), corresponding to the bottom left panel of
Figure 5, we have

$$\frac{\partial \Pr(D_4(z_2, z_4) = 1)}{\partial z_4} = \frac{\partial}{\partial z_4} \int_{\mu_4(z_4) - \mu_2(z_2)}^{\infty} \int_{\mu_4(z_4)}^{\infty} f_{U_4 - U_2, U_4}(u, v) dv du$$

$$= -\mu'_4(z_4) \int_{\mu_4(z_4) - \mu_2(z_2)}^{\infty} f_{U_2, U_4}(u, \mu_4(z_4)) du - \mu'_4(z_4) \int_{\mu_4(z_4)}^{\infty} f_{U_4 - U_2, U_4}(\mu_4(z_4) - \mu_2(z_2), v) dv$$

$$= -\mu'_4(z_4) \int_{\mu_2(z_2)}^{\infty} f_{U_2, U_4}(u, \mu_4(z_4)) du - \mu'_4(z_4) \int_{\mu_4(z_4)}^{\infty} f_{U_4 - U_2, U_4}(\mu_4(z_4) - \mu_2(z_2), v) dv. \quad (5)$$

Referring again to the partition in Figure 5, the first integral of line (5) traces out the set of indifferent individuals along the border between $D(z_2, z_4) = 0$ and $D(z_2, z_4) = 4$, i.e. marginal 0–4 compliers. The second term of line (5) traces out the set of indifferent individuals along the border between $D(z_2, z_4) = 2$ and $D(z_2, z_4) = 4$, i.e. marginal 2–4 compliers. Hence, as visualized in the bottom left panel of Figure 5, a marginal shift in $Z_4$ induces 0–4 and 2–4 compliers and thus generates marginal unordered monotonicity (A2) in $Z_4$.

Comparable compliers (A3) emerge by the fact that the second integral in line (4) and the second integral in line (5) both trace out the same exact group of 2–4 compliers along the margin of indifference between $D(z_2, z_4) = 4$ and $D(z_2, z_4) = 2$. A simple change of variables confirms this: with $U_4 - U_2$ fixed at $\mu_4(z_4) - \mu_2(z_2)$ in both integral expressions, we can write $U_2 = U_4 - \mu_4(z_4) + \mu_2(z_2)$, and plugging this transformation into the second integral in line (4) yields the second integral in line (5). The weights $-\mu'_2(z_2)$ and $-\mu'_4(z_4)$ outside the integrals may differ, but this is only to allow for differential marginal disutility of 2-year versus 4-year distance; the identical integrals confirm that both the $Z_2$ shift and the $Z_4$ shift involve the same group of compliers along the margin of indifference between 2-year and 4-year entry. This is a sufficient condition for generating comparable compliers (A3), since the marginal 2–4 compliers w.r.t. $Z_2$ and the marginal 2–4 compliers w.r.t. $Z_4$ in this selection model not only have equal mean $Y_2$ potential outcomes but are in fact the same set of individuals:

$$\lim_{z'_2 \uparrow z_2} E[Y_2 | D(z'_2, z_4) = 2, D(z_2, z_4) = 4] = E[Y_2 | U_4 - U_2 = \mu_4(z_4) - \mu_2(z_2), U_2 > \mu_2(z_2)]$$

$$= E[Y_2 | U_4 - U_2 = \mu_4(z_4) - \mu_2(z_2), U_4 > \mu_4(z_4)]$$

$$= \lim_{z'_4 \uparrow z_4} E[Y_2 | D(z_2, z'_4) = 2, D(z_2, z_4) = 4]. \quad \Box$$

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D Main Identification Proofs

This appendix section proves the main identification results of Section 4.3. From a given evaluation point \((\mathbb{Z}_2, \mathbb{Z}_4) = (z_2, z_4)\), consider a marginal decrease in \(Z_2\) from \(z_2\) to \(z'_2\) (while holding \(z_4\) fixed); the opposite direction proceeds analogously. By marginal unordered monotonicity (A2), this shift in \(Z_2\) induces \(2\leftarrow 0\) and \(2\leftarrow 4\) compliers. Changes in \(D_0\) with respect to this shift therefore must be driven by \(2\leftarrow 0\) compliers, and changes in \(D_4\) must be driven by \(2\leftarrow 4\) compliers. Using the shorthand \(E[YD_0|Z_2 = z_2, Z_4 = z_4] \equiv E[YD_0|z_2, z_4]\), we have

\[
E[YD_0|z_2, z_4] = E[Y_0|D_0 = 1, z_2, z_4]Pr(D_0 = 1|z_2, z_4)
= E[Y_0|D(z_2, z_4) = 0, z_2, z_4]Pr(D(z_2, z_4) = 0)
= E[Y_0|D(z_2, z_4) = 0]Pr(D(z_2, z_4) = 0)
\]

where the third and fourth lines are due to independence and exclusion of the instruments (A1). Decomposing the mass of individuals with \(D(z_2, z_4) = 0\) into the complier groups allowed by marginal unordered monotonicity (A2) yields

\[
E[Y_0|D(z_2, z_4) = 0]Pr(D(z_2, z_4) = 0) = [E[Y_0|D(z_2, z_4) = 0, D(z'_2, z_4) = 0]Pr(D(z'_2, z_4) = 0|D(z_2, z_4) = 0)
+ E[Y_0|D(z_2, z_4) = 0, D(z'_2, z_4) = 2]Pr(D(z'_2, z_4) = 2|D(z_2, z_4) = 0)] Pr(D(z_2, z_4) = 0)
= E[Y_0|D(z_2, z_4) = 0, D(z'_2, z_4) = 0]Pr(D(z_2, z_4) = 0, D(z'_2, z_4) = 0)
+ E[Y_0|D(z_2, z_4) = 0, D(z'_2, z_4) = 2]Pr(D(z_2, z_4) = 0, D(z'_2, z_4) = 2).
\]

Likewise, the permitted complier groups with \(D(z'_2, z_4) = 0\) yield

\[
E[YD_0|z'_2, z_4] = E[Y_0|D(z'_2, z_4) = 0]Pr(D(z'_2, z_4) = 0)
= E[Y_0|D(z_2, z_4) = 0, D(z'_2, z_4) = 0]Pr(D(z_2, z_4) = 0, D(z'_2, z_4) = 0)
\]

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where again the third line is due to A2. Taking the difference of these two expressions gives

\[
E[YD_0|z_2, z_4] - E[YD_0|z'_2, z_4] \\
= E[Y_0|D(z_2, z_4) = 0, D(z'_2, z_4) = 2] Pr(D(z_2, z_4) = 0) - Pr(D(z'_2, z_4) = 0)
\]

where the third line is due to A2: the change in the probability of \( D_0 \) with respect to this instrument shift is driven by, and identifies the mass of, 2–0 compliers. Taking limits to convert differences to derivatives,

\[
\frac{\partial E[YD_0|z_2, z_4]}{\partial Z_2} = \lim_{z'_2 \to z_2} \frac{E[YD_0|z_2, z_4] - E[YD_0|z'_2, z_4]}{z_2 - z'_2}
\]

\[
= \lim_{z'_2 \to z_2} E[Y_0|D(z_2, z_4) = 0, D(z'_2, z_4) = 2] \frac{Pr(D(z_2, z_4) = 0) - Pr(D(z'_2, z_4) = 0)}{z_2 - z'_2}
\]

\[
= E[Y_0|2–0 complier at (z_2, z_4)] \frac{\partial Pr(D(z_2, z_4) = 0)}{\partial Z_2}
\]

\[
= E[Y_0|2–0 complier at (z_2, z_4)] \frac{\partial E[D_0|z_2, z_4]}{\partial Z_2}.
\]

Rearranging identifies the first potential outcome of interest:

\[
\frac{\partial E[YD_0|z_2, z_4]}{\partial Z_2} \quad = E[Y_0|2–0 complier at (z_2, z_4)].
\]

We can proceed analogously for \( D_4 \):

\[
E[YD_4|z_2, z_4] \\
= E[Y_4|D(z_2, z_4) = 4] Pr(D(z_2, z_4) = 4)
\]

\[
= [E[Y_4|D(z_2, z_4) = 4, D(z'_2, z_4) = 4] Pr(D(z'_2, z_4) = 4|D(z_2, z_4) = 4)] Pr(D(z_2, z_4) = 4)
\]

\[
= E[Y_4|D(z_2, z_4) = 4, D(z'_2, z_4) = 4] Pr(D(z_2, z_4) = 4, D(z'_2, z_4) = 4)
\]

\[
+ E[Y_4|D(z_2, z_4) = 4, D(z'_2, z_4) = 2] Pr(D(z_2, z_4) = 4, D(z'_2, z_4) = 2)
\]

\[
\]
\[ E[YD_4 | z_2', z_4] \]
\[ = E[Y_4 | D(z_2', z_4) = 4 | Pr(D(z_2', z_4) = 4) \]
\[ = E[Y_4 | D(z_2, z_4) = 4 | D(z_2', z_4) = 4 | Pr(D(z_2, z_4) = 4, D(z_2', z_4) = 4) \]

\[ E[YD_4 | z_2, z_4] - E[YD_4 | z_2', z_4] \]
\[ = E[Y_4 | D(z_2, z_4) = 4, D(z_2', z_4) = 2 | Pr(D(z_2, z_4) = 4, D(z_2', z_4) = 2) \]
\[ = E[Y_4 | D(z_2, z_4) = 4, D(z_2', z_4) = 2 | Pr(D(z_2, z_4) = 4) - Pr(D(z_2', z_4) = 4) \]

\[ \frac{\partial E[YD_4 | z_2, z_4]}{\partial Z_2} = \lim_{z_2' \to z_2} \frac{E[YD_4 | z_2, z_4] - E[YD_4 | z_2', z_4]}{z_2 - z_2'} \]
\[ = \lim_{z_2' \to z_2} E[Y_4 | D(z_2, z_4) = 4, D(z_2', z_4) = 2 | Pr(D(z_2, z_4) = 4) - Pr(D(z_2', z_4) = 4) \]
\[ = E[Y_4 | 2–4 complier at (z_2, z_4)] \frac{\partial E[D_4 | z_2, z_4]}{\partial Z_2} \].

Therefore
\[ \frac{\partial E[YD_4 | z_2, z_4]}{\partial Z_2} = E[Y_4 | 2–4 complier at (z_2, z_4)] \]

Turning to \( D_2 \),
\[ E[YD_2 | z_2, z_4] \]
\[ = E[Y_2 | D(z_2, z_4) = 2 | Pr(D(z_2, z_4) = 2) \]
\[ = E[Y_2 | D(z_2, z_4) = 2, D(z_2', z_4) = 2 | Pr(D(z_2', z_4) = 2) \]

The pooled expression arises from the fact that changes in \( D_2 \) with respect to \( z_2 \to z_2' \)
are driven by both 2←0 and 2←4 compliers:

\[ E[YD_2|z_2', z_4] \]
\[ = E[Y_2|D(z_2', z_4) = 2] Pr(D(z_2', z_4) = 2) \]
\[ = E[Y_2|D(z_2, z_4) = 2, D(z_2', z_4) = 2] Pr(D(z_2, z_4) = 2, D(z_2', z_4) = 2) \]
\[ + E[Y_2|D(z_2, z_4) = 0, D(z_2', z_4) = 2] Pr(D(z_2, z_4) = 0, D(z_2', z_4) = 2) \]
\[ + E[Y_2|D(z_2, z_4) = 4, D(z_2', z_4) = 2] Pr(D(z_2, z_4) = 4, D(z_2', z_4) = 2) \]

Hence

\[ E[YD_2|z_2, z_4] = E[YD_2|z_2', z_4] \]
\[ = E[Y_2|D(z_2, z_4) = 0, D(z_2', z_4) = 2] Pr(D(z_2, z_4) = 0, D(z_2', z_4) = 2) \]
\[ + E[Y_2|D(z_2, z_4) = 4, D(z_2', z_4) = 2] Pr(D(z_2, z_4) = 4, D(z_2', z_4) = 2) \]
\[ = E[Y_2|D(z_2, z_4) = 0, D(z_2', z_4) = 2] [Pr(D(z_2, z_4) = 0) - Pr(D(z_2', z_4) = 0)] \]
\[ + E[Y_2|D(z_2, z_4) = 4, D(z_2', z_4) = 2] [Pr(D(z_2, z_4) = 4) - Pr(D(z_2', z_4) = 4)] \]

\[ \frac{\partial E[YD_2|z_2, z_4]}{\partial Z_2} = \lim_{z_2' \to z_2} \frac{E[YD_2|z_2, z_4] - E[YD_2|z_2', z_4]}{z_2 - z_2'} \]
\[ = \lim_{z_2' \to z_2} \left[ E[Y_2|D(z_2, z_4) = 0, D(z_2', z_4) = 2] \frac{Pr(D(z_2, z_4) = 0) - Pr(D(z_2', z_4) = 0)}{z_2 - z_2'} \right. \]
\[ + E[Y_2|D(z_2, z_4) = 4, D(z_2', z_4) = 2] \frac{Pr(D(z_2, z_4) = 4) - Pr(D(z_2', z_4) = 4)}{z_2 - z_2'} \]
\[ = E[Y_2|2\leftrightarrow0 \text{ complier at } (z_2, z_4)] \frac{\partial E[D_0|z_2, z_4]}{\partial Z_2} \]
\[ + E[Y_2|2\leftrightarrow4 \text{ complier at } (z_2, z_4)] \frac{\partial E[D_4|z_2, z_4]}{\partial Z_2}. \]

Finally, we turn to the comparable compliers induced by \( Z_4 \). From the same evaluation point \((Z_2, Z_4) = (z_2, z_4)\), consider a marginal increase in \( Z_4 \) from \( z_4 \) to \( z_4' \) (while holding \( z_2 \) fixed); the opposite direction proceeds analogously. By marginal unordered monotonicity (A2), this shift in \( Z_4 \) induces 2←4 and 0←4 compliers. Changes in \( D_2 \)
with respect to this shift therefore must only involve 2→4 compliers:

\[ E[YD_2|z_2, z_4] = \begin{cases} \frac{1}{2} |Pr(D(z_2, z_4) = 2) \end{cases} \]

\[ = E[Y_2|D(z_2, z_4) = 2] \]

\[ = E[Y_2|D(z_2, z_4) = 2, D(z_2, z'_4) = 2] \]

\[ E[YD_2|z_2, z'_4] = \begin{cases} \frac{1}{2} \frac{1}{2} \end{cases} \]

\[ = E[Y_2|D(z_2, z'_4) = 2] \]

\[ = E[Y_2|D(z_2, z'_4) = 2, D(z_2, z_4) = 2, D(z_2, z'_4) = 2] \]

\[ + E[Y_2|D(z_2, z_4) = 4, D(z_2, z'_4) = 2] \]

\[ + \begin{cases} \frac{1}{2} \frac{1}{2} \end{cases} \]

\[ \frac{1}{2} |Pr(D(z_2, z_4) = 2) \]

\[ = E[Y_2|2–4 complier at (z_2, z_4)] \cdot \frac{\partial E[D_2|z_2, z_4]}{\partial Z_4}. \]

This mean potential outcome among marginal 2–4 compliers w.r.t. \( Z_4 \) is equal to that among marginal 2–4 compliers w.r.t. \( Z_2 \) by A3 (comparable compliers). Therefore

\[ \frac{\partial E[YD_2|z_2, z_4]}{\partial Z_4} = \frac{\partial E[D_2|z_2, z_4]}{\partial Z_4} = E[Y_2|2–4 complier at (z_2, z_4)], \]

and plugging this identified potential outcome into the pooled \( Z_2 \) expression yields all of the mean complier potential outcomes of interest.

\[ \square \]
Figure A.1: Out-of-State Enrollment and Missing Earnings among Top Scorers

Notes: The top panel of this figure plots the share of students within each 10th grade test score percentile (defined in Section 3.2) who enroll in college outside of Texas using the 2008-2009 cohorts with National Student Clearinghouse college enrollment coverage. The bottom panel plots the share of students within each test score percentile who have no Texas quarterly earnings records over ages 28-30 using the 2000-2004 main analysis cohorts.
Figure A.2: Predicted Earnings Are Similar for Students with Observed and Missing Earnings

Notes: This figure plots the distributions of predicted mean quarterly earnings over ages 28-30 for students with and without observed earnings. Earnings are first projected on all covariates and instruments in Table 1 in the sample with valid earnings, then predicted in the full sample and plotted by earnings status.
Figure A.3: Sorting into College Enrollment Choices by Observables, 2000-2004 Analysis Cohorts

Notes: Disadvantaged is an indicator for free or reduced price lunch eligibility in 10th grade. Neighborhood quality and test score percentiles, defined in Section 3.2, are grouped into 5-unit bins.
Table A.1: High School Graduation and Out-of-State College Enrollment

<table>
<thead>
<tr>
<th></th>
<th>Graduate from high school</th>
<th>Enroll in college out-of-state</th>
</tr>
</thead>
<tbody>
<tr>
<td>Z2: 2-year distance (miles/10)</td>
<td>0.0013 (0.0012)</td>
<td>-0.000001 (0.0006)</td>
</tr>
<tr>
<td>Z4: 4-year distance (miles/10)</td>
<td>0.0001 (0.0001)</td>
<td>-0.0011 (0.0007)</td>
</tr>
<tr>
<td>R²</td>
<td>0.019</td>
<td>0.017</td>
</tr>
<tr>
<td>N</td>
<td>590,397</td>
<td>362,013</td>
</tr>
</tbody>
</table>

Sample: Baseline controls ✓ Main analysis cohorts ✓ NSC cohorts ✓

Notes: NSC cohorts are those with National Student Clearinghouse college enrollment data. Standard errors in parentheses are clustered at the high school campus by cohort level. High school graduation is measured cumulatively through eight years after 10th grade. Out-of-state college enrollment is measured within two years of projected high school graduation due to NSC data availability.