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Community Colleges and Upward Mobility†

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Two-year community colleges enroll nearly half of all first-time undergraduates in the United States, but to ambiguous effect: low persistence rates and the potential for diverting students from four-year institutions cast ambiguity over two-year colleges' contributions to upward mobility. This paper develops a new instrumental variables approach to identifying causal effects along multiple treatment margins, and applies it to linked education and earnings registries to disentangle the net impacts of two-year college access into two competing causal margins: significant value added for two-year entrants who otherwise would not have attended college, but negative impacts on students diverted from immediate four-year entry. (*JEL* I23, I26, I28, J24, J31)

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 outpaced modest growth in the supply of college-educated workers (Katz and Murphy 1992; Goldin and Katz 2008; Acemoglu and Autor 2011), and this modest supply 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 two-year community colleges, which enroll nearly half of

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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 two-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 two-year colleges will help extend the prospects of postsecondary attainment and upward mobility to a broader share of young Americans.

Even if expanding two-year access succeeds in increasing enrollment in two-year colleges, however, causal evidence on the efficacy of two-year colleges in ultimately boosting educational attainment and earnings is limited (Belfield and Bailey 2011, 2017; Denning 2017), and greater two-year access may even detrimentally divert college-bound students from directly entering higher-resourced four-year institutions (Clark 1960; Brint and Karabel 1989; Rouse 1995). Concerns stem from low rates of degree completion and upward transfer among two-year entrants: while 81 percent begin with the intention to earn a bachelor's degree, only 33 percent actually transfer up to a four-year institution within six years, and just 14 percent complete a bachelor's degree (Jenkins and Fink 2016). In contrast, 60 percent of students who begin college directly at a four-year institution complete a bachelor's degree over the same time frame, and substantial outcome gaps between two-year and four-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 two-year versus four-year institution, the potential for four-to-two diversion casts further ambiguity over the role of two-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 two-year colleges on student outcomes. Does two-year access boost educational attainment and earnings, on net? Are some students diverted from four-year entry, with detrimental effects? How large are the gains, if any, among two-year entrants who otherwise would not have attended any college? To answer these questions, I develop a new instrumental variables (IV) approach that nonparametrically identifies causal effects along multiple margins of treatment. Applied to the two-year college setting, this approach unlocks a decomposition of the overall net effects of two-year access into two distinct and potentially opposing treatment margins: causal value-added for new two-year entrants who otherwise would have not enrolled in any college, versus causal diversion impacts on two-year entrants who otherwise would have started directly at a four-year institution.[2]

Methodologically, this new approach overcomes a challenge in the 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

[1] Following Cohen, Brawer, and Kisker (2014), I use the terms community colleges, two-year colleges, and junior colleges interchangeably when referring to nonprofit institutions that award associate's degrees as their highest credential.

[2] This paper focuses on identifying and estimating ex post treatment effects. See Cunha, Heckman, and Navarro (2005) and Arcidiacono et al. (2020) for examples of methods to recover ex ante effects of student choices, as perceived 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.

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, Leuven, and Mogstad 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. Differencing these mean potential outcomes then delivers margin-specific treatment effects.[3] This approach generalizes the binary complier-describing logic of Imbens and Rubin (1997); Abadie (2002); and Carneiro and Lee (2009) to multiple treatments and instruments: 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, including their potential outcomes, emerge when studying compositional changes in each treatment group driven by specific instruments.[4] This separate identification approach has a straightforward implementation as a series of 2SLS regressions, replacing the usual outcome variable with outcome-by-treatment interactions and using local instrumental variation around a given evaluation point. Stratifying the estimates across different instrument evaluation points helps probe external validity of the local IV estimates, in this case examining how marginal returns evolve as two-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 link all 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 US context in their combination of breadth and depth of coverage, spanning the population of the second largest state (comprising 10.5 percent of all public K–12 students in the United States) and providing detailed information on demographics, test scores, college enrollment dynamics, degree completion, and longitudinal earnings.

Linking the student-level microdata with the locations of high school and college campuses in Texas, I identify causal effects along the multiple margins of two-year enrollment with instrumental variation in students' proximities to local two-year and four-year college campuses. Departing from most papers in the returns-to-schooling canon that employ distance instruments,[5] I control directly for detailed neighborhood-level measures of urbanization, as well as commuting zone fixed effects, to ensure that 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 one-dimensional variation in a student's distance to any type of college; instead, identification comes from varying two-year distance while holding four-year distance fixed, and varying

[3] The proposed 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.

[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.

[5] E.g. Card (1995); Rouse (1995); Kling (2001); Cameron and Taber (2004); Carneiro and Lee (2009); Carneiro, Heckman, and Vytlacil (2011); Eisenhauer, Heckman, and Vytlacil (2015); and Nybom (2017), among others.

four-year distance while holding two-year distance fixed. Encouragingly, I show that these two dimensions of conditional instrumental variation are uncorrelated with excluded student ability measures that strongly predict college choices and long-run outcomes. Given this balancing result, the 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 two-year and four-year colleges.

The results of this IV approach offer four main conclusions. First, greater access to two-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 two-year entrants are diverted from four-year entry and ultimately complete less education as a result, empirically confirming concerns over the diversion channel but with smaller magnitudes than ordinary least squares (OLS) regressions would suggest. The other two-thirds of induced two-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 two-year access particularly boosts the upward earnings mobility of students from low-income families thanks to their lower likelihood of diversion from four-year entry. Finally, stratifying the local IV estimates across the range of two-year college proximity suggests that causal impacts on marginal students do not diminish as two-year access further expands and draws deeper into the population of potential entrants.

Taken together, the methods and results of this paper contribute a quantitative causal framework for studying the long-run impacts of community college access policies, highlighting the trade-off between democratizing new students into higher education and diverting college-bound students from direct four-year entry. Since the Tennessee Promise program launched in 2014, offering recent high school graduates free tuition at all two-year community colleges in the state, Oregon, Minnesota, Kentucky, and Rhode Island have implemented similar programs, with many more 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 results of this paper suggest that broad expansions of two-year college access are likely to boost the upward mobility of students "democratized" into higher education from nonattendance, but more targeted policies that avoid significant four-year diversion may generate larger net benefits.

The empirical results build on an interdisciplinary literature studying the outcomes of two-year college students, reviewed by Kane and Rouse (1999) and Belfield and Bailey (2011, 2017).[6] Most of this literature relies on selection-on-observables

[6]The two-year versus four-year comparison also contributes more broadly to the college quality literature; see Hoxby (2009) for a review, along with Andrews, Li, and Lovenheim (2016); Mountjoy and Hickman (2021); and Black, Denning, and Rothstein (forthcoming) for recent contributions using the same administrative data source as this paper. The Peltzman (1973)-esque four-to-two diversion results in this paper are reminiscent of Cohodes and Goodman (2014), who study diversion across vertical quality tiers within the four-year sector. The greater prevalence of vocational courses in the two-year sector could alternatively cast the comparison as one between horizontally differentiated

assumptions to interpret OLS and matching results as causal. A growing set of papers relax this assumption in panel specifications with individual fixed effects (e.g., Jacobson, LaLonde, and Sullivan 2005; Jepsen, Troske, and Coomes 2014; Dynarski, Jacob, and Kreisman 2019; Stevens, Kurlaender, and Grosz 2018), but this approach necessarily focuses on older workers who have accumulated pre-enrollment earnings histories. Several recent papers exploit natural experiments that directly or indirectly influence two-year college enrollment (Denning 2017; Zimmerman 2014; Goodman, Hurwitz, and Smith 2017; Carruthers, Fox, and Jepsen 2020; Grosz 2020; Gurantz 2020; Smith, Goodman, and Hurwitz 2020; Acton 2021), but much of this variation is too recent to examine longer-run outcomes, and these papers do not offer methods to nonparametrically disentangle net impacts into separate causal effects along each treatment margin.

In estimating causal impacts of two-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 2SLS specifications.[7] As shown by Kirkeboen, Leuven, and Mogstad (2016), multivariate 2SLS estimands generally combine 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.[8] Multiple pieces of evidence point to such heterogeneity across potential two-year college entrants, motivating the development of a nonparametric identification approach that is robust to such heterogeneity.

Methodologically, the identification results contribute to the literature on identifying causal effects of multivalued treatments with IV. 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 growing set of papers have developed conditions under which margin-specific treatment effects can be identified. This paper's nonparametric separate identification approach allows the analyst to relax many of these conditions: it does not require parameterizing unobserved heterogeneity (Feller et al. 2016; Hull 2020), or imposing homogeneity in treatment effects or selection behavior across observable covariate stratifications (Hull 2018; Kline and Walters 2016), or observing individuals' preference rankings over treatment alternatives (Kirkeboen, Leuven, and Mogstad 2016).[9]

curricula, e.g., Altonji, Blom, and Meghir (2012); Kirkeboen, Leuven, and Mogstad (2016); and Bertrand, Mogstad, and Mountjoy (2021).

[7] Brand, Pfeffer, and Goldrick-Rab (2014) also study democratization and diversion in a heterogeneous treatment effects framework, relying on selection-on-observables assumptions via propensity score matching rather than IV.

[8] In online 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 (2018) 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 (2021) for a related discussion in the context of the multiple treatment arms of the Moving to Opportunity experiment.

[9] See also Lee and Salanié (2020); Rodriguez and Saltiel (2020); Galindo (2020); Wang (2020); Kamat (2021); and Arteaga (forthcoming) for recent contributions to this literature. Section IIID discusses Heckman and Pinto (2018).

My employment of multiple continuous instruments most closely resembles Heckman, Urzua, and Vytlacil (2008)—with related expositions in Heckman and Vytlacil (2007a, b)—and Lee and Salanié (2018). Heckman, Urzua, and Vytlacil (2008) rely on a multidimensional identification-at-infinity argument: they show how the challenge of identifying the effect of one treatment against a specific alternative can collapse to a binary treatment problem in the limit if sufficiently extreme instrument values are available that force the probability of choosing any of the other treatments—other than the binary comparison of interest—to be arbitrarily small. My approach does not require any such large support conditions; instead, I show how local instrument shifts that induce overlapping complier flows can identify marginal treatment effects at each point in whatever support the instruments happen to enjoy. Lee and Salanié (2018) explore identification of similar target parameters in an index model framework that generalizes the illustrative discrete choice model I use to visualize the mechanics of my approach in Section IIIC. Their framework allows for complex complier behavior, with each treatment choice governed by threshold-crossing rules involving potentially multiple unobservables crossing multiple thresholds, but generally requires nonparametrically identifying the index functions that comprise these thresholds in a first step. My approach in Section IIID bypasses this demanding empirical challenge by deriving conditions on complier behavior that allow a set of easily implemented local 2SLS regressions with modified outcome variables to recover all of the ingredients necessary for identifying margin-specific treatment effects.

Finally, while the institutional focus of this paper lies with higher education in the United States, the methodology could apply to a broad range of settings. The two-year and four-year college distance instruments play the role of prices or cost shifters, suggesting parallels to other multivalued treatment choices that depend on initial costs, including migration, occupational choice, insurance enrollment, hospital admission, 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; Kline and Walters 2016), since the task of evaluating a policy or program with readily available substitutes—here the encouragement of two-year enrollment in the presence of the four-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 obtained the substitute (four-year entry).

The remainder of the paper proceeds as follows. Section I provides institutional background on the American community college. Section II describes the linked administrative data and presents descriptive results on initial enrollment choices and outcomes. Section III discusses the identification challenges posed by multiple treatment margins and develops the nonparametric separate identification approach. Section IV discusses estimation and conducts diagnostics on the instruments. Section V presents the empirical results. Section VI concludes.

I. 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 twentieth century, 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 twentieth century was whether four-year universities should spin off their first two years of teaching to these emerging junior colleges, allowing two-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 University's David Starr Jordan, and several other prominent university presidents at the time (Cohen, Brawer, and Kisker 2014)—never materialized on a large scale in the United States, as the vast majority of colleges and universities that offer bachelor's degrees have maintained their common model of four continuous undergraduate years.[10]

The resulting functional overlap between two-year and four-year colleges helped fuel debate over a second question: should two-year colleges continue to prepare students for four-year transfer through academic coursework, or should they differentiate themselves from four-year institutions by focusing on terminal vocational training to prepare students for workforce entry? The academic transfer function remained the core mission of two-year colleges from their inception through the mid-twentieth century, despite persistent efforts from the leadership of the American Association of Junior Colleges to carve out a clear niche for two-year colleges by providing vocational training (Brint and Karabel 1989; Cohen, Brawer, and Kisker 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 degrees; several reports from the influential Carnegie Commission on Higher Education advocating more vocational emphasis at two-year colleges; and the shift in terminology from "junior" to "community" college, shedding the former connotation of subordination to four-year institutions in favor of responsiveness to community needs, including occupational education suited to local industry demand (Freeman 1976; Brint and Karabel 1989; Cohen, Brawer, and Kisker 2014). These forces coincided with a rise in the share of two-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, Brawer, and Kisker 2014).

The rise of vocational education at two-year colleges has only intensified debate over a final question, on which this paper focuses: do two-year colleges boost the

[10] 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 two-year colleges did emerge and grow dramatically over the twentieth century, Harper's own bifurcation of the University of Chicago never resulted in two standalone institutions—evoking grumbles from faculty members whom Harper had recruited to the new university with assurances that there would be no need to teach lower-division undergraduates (Boyer 2015).

upward mobility of individuals who otherwise would not participate in higher education, or do they mainly divert college-bound students away from four-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 two-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 two-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 in costs, over 70 percent of which are subsidized by local, state, and federal taxpayers (National Center for Education Statistics [NCES] 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 two-year college enrollment, atop a century of controversy over the role of community colleges in American social mobility, motivates the empirical analysis of this paper.

II. Data and Descriptive Results

A. *Data Sources*

My empirical analysis combines several restricted administrative datasets spanning the state of Texas (UT Dallas Education Research Center 2022). As the second largest US state by population, land area, and GDP, Texas comprises 9 percent of the US population and educates over 10 percent of US public K–12 students. This large populace supports a comprehensive statewide system of higher education: roughly 75 public and private four-year colleges and universities collectively enroll over 760,000 students, and roughly 57 two-year community college districts collectively enroll over 730,000 students (NCES 2015).

The analysis sample begins with student-level data from the Texas Education Agency (TEA) covering the population of Texas public high school students.[11] I link these students to administrative records from the Texas Higher Education Coordinating Board (THECB), capturing all enrollments and degrees at all public

[11] 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). Nationally representative surveys show that private school students tend to come from more advantaged backgrounds: roughly 70 percent of them have parents with at least a bachelor's degree, for example, compared to 40 percent of public school students (Wang, Rathbun, and Musu 2019). Augmenting my public sample with the 5 percent of Texas students who come from private high schools would therefore slightly increase the socioeconomic profile of the sample, and in turn likely lead to slightly larger estimates of the share of students on the diversion margin between two-year and four-year entry rather than the margin between two-year and no college.

and private Texas colleges and universities.[12] 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 (UI) system.[13] I complement these administrative student-level records with several auxiliary school- and neighborhood-level data sources: high school characteristics from the NCES (2022a) Common Core of Data (CCD), college characteristics from the Integrated Postsecondary Education Data System (IPEDS) (NCES 2022b), and neighborhood characteristics from the 2000 decennial census measured at the tract level (US Census Bureau 2000b).[14]

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 US state, with 82 percent of all Texas-born individuals remaining in Texas as of 2012 (Aisch, Gebeloff, and Quealy 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.[15] On the earnings front, missing earnings values could represent either nonemployment or outmigration; I show below that students with missing earnings in my analysis sample look nearly identical to those with nonmissing earnings in terms of observable characteristics, suggesting that the scope for sample selection bias may be limited.[16]

B. *Variable Definitions*

Cohorts.—The main analysis sample consists of five cohorts of Texas public high school students enrolled in tenth grade between 1998 and 2002. I will hereafter refer to their projected high school graduation years of 2000 through 2004.[17] 2000 is the oldest cohort for whom private Texas college enrollments are observed,[18] and 2004 is the latest cohort for whom I observe earnings around age 30. I also make separate use of the 2008 and 2009 cohorts in the descriptive results, leveraging their NSC coverage to show college enrollment patterns inclusive of students who attend out of state.

[12] 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).

[13] 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.

[14] Census tracts delineate neighborhoods of roughly 1,200 to 8,000 people, averaging around 4,000.

[15] The NSC records cover 90 percent of nationwide college enrollment (Dynarski, Hemelt, and Hyman 2013).

[16] Andrews, Li, and Lovenheim (2016) and Dobbie and Fryer (2020) arrive at similar conclusions using different extracts from the same Texas administrative data as this paper.

[17] Online Appendix Table A.1 shows that the two-year and four-year college proximity instruments have no detectable influence on the probability of graduating from high school.

[18] The private Texas college enrollment data begin in Fall 2002, so the 2002 high school graduates are officially the first with complete private college enrollment coverage. Persistence rates at private colleges are quite high, however, so catching the 2001 and 2000 cohorts in their second and third years, respectively, at private colleges allows me to significantly increase the sample size with little measurement error in treatments.

Covariates.—Student-level demographics are measured in the tenth grade TEA enrollment files and include categorical variables for gender, race/ethnicity,[19] and eligibility for free or reduced price lunch (FRPL), a proxy for economic disadvantage. To obtain a single test score measure for each student, I combine raw tenth grade math and reading scores in a one-factor model separately by cohort, then normalize this factor to its within-cohort percentile.[20] High school-level controls are measured in the NCES CCD 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,[21] and a county variable, which I group into the 62 Texas commuting zones using the year-2000 mapping provided by the US Department of Agriculture's Economic Research Service (US Department of Agriculture 2012). 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 the North American Industry Classification System industry codes in the TWC workforce data. Finally, I construct an index of neighborhood quality by combining 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.[22]

Treatments.—The three mutually exclusive and exhaustive treatments of interest are starting at a two-year college, starting at a four-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.[23]

Instruments.—I measure proximity (in miles) to the nearest two-year and four-year college campuses by computing surface distances between the coordinates of Texas public high schools (from NCES CCD) and the coordinates of Texas postsecondary institutions (from IPEDS), then taking minimum distances for each high school within the two-year sector and four-year sector separately.[24] 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.

[19] Due to small shares in some descriptive statistics, Native American students are pooled with Hispanic students.

[20] The factor model, estimated with Stata's *factor* command, decomposes the test score measures $X_m, m \in \{1,2\}$, according to $X_m = b_m F + e_m$, where F is the common latent factor of interest, b_m is a loading coefficient, and e_m is homoskedastic measurement error uncorrelated with F and uncorrelated across measures.

[21] These 12 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.

[22] The estimation of the neighborhood factor model is similar to the test score model in footnote 20, with three neighborhood measures $X_m, m \in \{1,2,3\}$, instead of two test scores. High school coordinates were matched to census tracts via the US Census Bureau (2000a) API.

[23] 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, Li, and Lovenheim (2014), I ignore summer terms when defining sector of enrollment.

[24] In determining minimum distances, I ignore small private college campuses as well as small community college extension centers that offer limited courses and student services.

Academic Outcomes.—I study the effects of two-year college enrollment on two key 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 four-year degree completion files within ten years of projected high school graduation. To study time to degree, I also construct separate indicators for completing a BA by each integer year from four to ten. And to study the broad content of these bachelor's degrees, I decompose them into STEM versus non-STEM majors using the official classification of the US Department of Homeland Security (2016). Years of completed schooling, as a more continuous measure of college completion, are calculated using the algorithm detailed in the footnote below.[25]

Earnings.—I measure real quarterly earnings by summing TWC earnings within each person-quarter, deflating by the quarterly US consumer price index (CPI) (base year 2010), winsorizing at the ninety-ninth percentile, and averaging the nonmissing quarters within person over ages 28–30, which are the oldest common ages available across my analysis cohorts.[26] 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.

C. *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 tenth 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 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 eightieth percentile. Online 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. Online 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.

[25] Years of completed schooling range from 10 to 17. To complete 10: enroll in eleventh grade; to complete 11: enroll in twelfth grade; 12: complete high school; 13: enroll in college with second year standing, or complete a certificate, or complete the academic core requirement at a community college; 14: enroll in college with third year standing, or complete an associate's degree; 15: enroll in college with fourth 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.

[26] These ages assume the student was 16 at the end of tenth grade, which is true for roughly 96 percent of students in the sample. Earnings for the 2004 cohort, the youngest in the sample, are only available over ages 28–29. Quarterly CPI measures come from Federal Reserve Economic Data (Federal Reserve Bank of St. Louis 2022).

Table 1—Summary Statistics

	NSC cohorts 2008–2009	Main cohorts 2000–2004	Main cohorts w/out top scoring quintile	Main cohorts w/out top scoring quintile or missing earnings
	Mean (SD) (1)	Mean (SD) (2)	Mean (SD) (3)	Mean (SD) (4)
Covariates				
Female	0.514	0.513	0.516	0.517
White	0.456	0.529	0.477	0.464
Hispanic	0.37	0.319	0.358	0.369
Black	0.142	0.122	0.142	0.145
Asian	0.032	0.029	0.024	0.021
Free/reduced price lunch	0.395	0.311	0.354	0.359
Test score percentile	50.6	50.7	40.5	40.4
Neighborhood quality percentile	50.6	50.6	48.7	48.2
Oil/gas employment share	0.017	0.018	0.018	0.018
City	0.364	0.377	0.389	0.387
Suburb	0.276	0.256	0.243	0.242
Town	0.119	0.134	0.134	0.136
Rural	0.242	0.233	0.234	0.235
Treatments				
No college	0.318	0.388	0.433	0.391
Start at two-year college	0.371	0.339	0.366	0.394
Start at four-year college	0.274	0.273	0.201	0.215
Start at four-year (out of state)	0.037	—	—	—
Instruments				
Miles to two-year college	8.4 (10.1)	9.6 (11.4)	9.6 (11.5)	9.6 (11.5)
Miles to four-year college	18.5 (17.6)	19.3 (18.4)	19.2 (18.6)	19.2 (18.5)
Academic outcomes				
Bachelor's degree	—	0.255	0.187	0.207
Years of schooling	—	13.1 (2.1)	12.8 (2.0)	13.0 (2.0)
Earnings outcomes				
Mean quarterly earnings	—	8,825 (5,840)	8,167 (5,412)	8,167 (5,412)
Has quarterly earnings	—	0.764	0.773	1
Observations	454,137	958,645	764,497	590,862

Notes: NSC cohorts in column 1 are the high school graduating classes of 2008 and 2009, for whom NSC college enrollment data are available. The remaining columns are the main analysis cohorts of tenth graders, measured by their projected high school graduation years of 2000 to 2004. 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.

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 nonmissing earnings look very similar to the full sample in terms of covariates, though those with nonmissing earnings are a bit more likely to have (observed) college enrollments and degrees. In online 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 $60. 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.

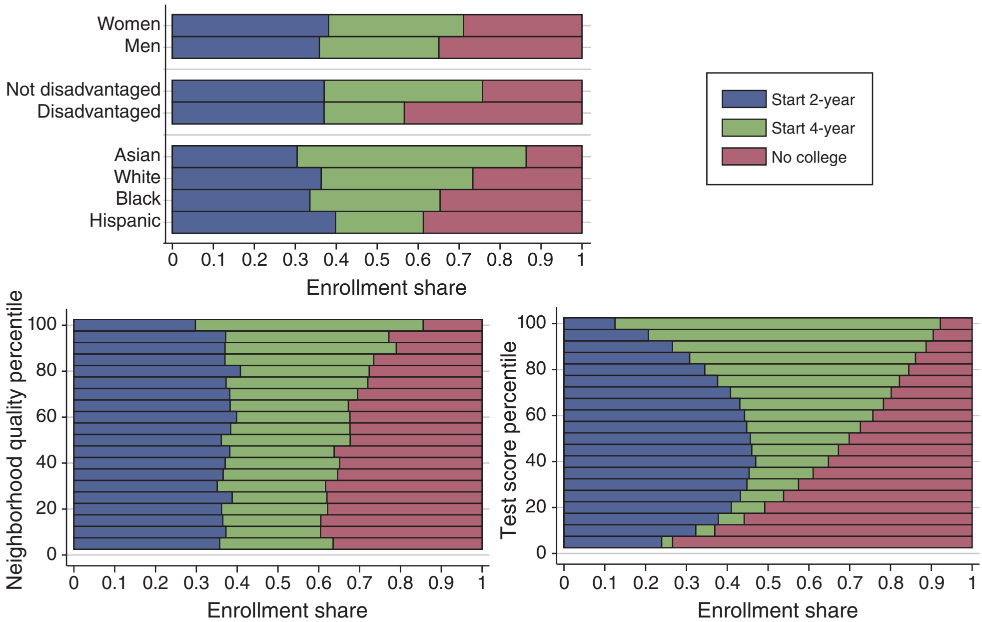

Figure 1. Sorting into College Enrollment Choices by Observable Characteristics

Notes: The 2008–2009 cohorts with NSC college enrollment coverage; see online 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 FRPL eligibility in tenth grade. Neighborhood quality and test score percentiles, defined in Section IIB, are grouped into five-unit bins.

D. *Sorting into Initial College Enrollments*

Figure 1 describes how initial college enrollment choices vary across observable student characteristics in the 2008–2009 NSC cohorts.[27] Across demographic groups and neighborhood quality deciles, the two-year college enrollment share is remarkably constant around the grand mean of 37 percent; what differ are the outside option shares of four-year enrollment and no college, with men, disadvantaged students, underrepresented minorities, and students from poor neighborhoods more likely to forgo college altogether than enroll in a four-year institution. The bottom-right panel of Figure 1 shows that the two-year enrollment share is hump-shaped across tenth grade test scores with a peak at the fortieth percentile, though two-year enrollment is still quite broadly distributed. Four-year college enrollment and no college enrollment, meanwhile, are strongly monotonic in test scores in opposing directions.

[27] Online Appendix Figure A.3 reproduces these results for the 2000–2004 main analysis cohorts. The plots are very similar up through the eightieth percentile test score sample cutoff, beyond which the share of students in the 2000–2004 main analysis cohorts starting four-year is somewhat understated, and no college overstated, due to unobserved out-of-state enrollments.

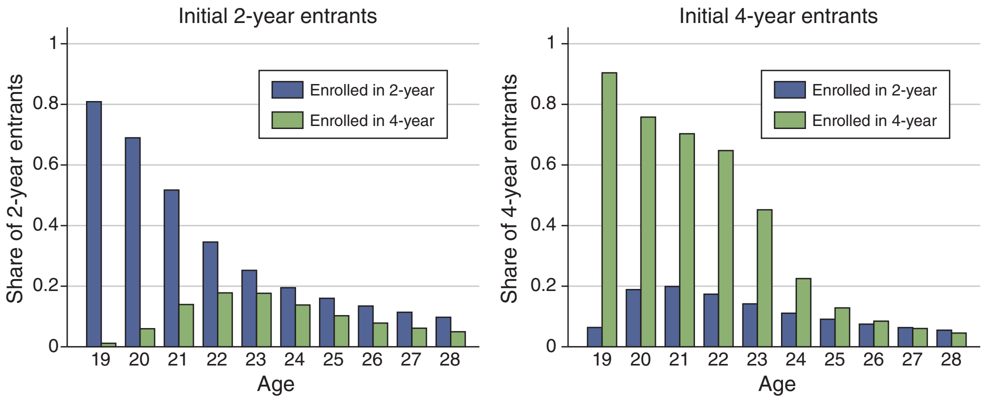

Figure 2. Enrollment Dynamics by Sector of Initial Enrollment

Notes: Enrollment shares at age 19 are not equal to one due to the three-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.

E. *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 two-year entrants and shows that the vast majority of them stay in the two-year sector, if any, for the first few years after entry. Enrollment in four-year colleges among these two-year entrants then rises and peaks around 20 percent at ages 22 to 23, with both two-year and four-year enrollment slowly trailing off thereafter. The right panel, conditioning on four-year entrants, tells a similar story of "sticky treatment" in that the vast majority of four-year entrants stay in the four-year sector. Roughly 20 percent of initial four-year entrants are enrolled in a two-year college in their early 20s, either as a transfer or dual enrollment.

Figure 3 plots the earnings profiles associated with each initial enrollment choice, controlling solely for cohort fixed effects. As expected, four-year entrants overtake two-year entrants, and two-year entrants 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. These gender differentials persist into the causal results, as shown in Section V.

F. *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:

$$(1)\quad Outcome = \alpha - \beta_{2\leftarrow 0}\mathbf{1}\{No\ college\} - \beta_{2\leftarrow 4}\mathbf{1}\{Start\ 4\text{-}year\} + Controls + \epsilon.$$

Writing the specification in this form, with two-year entry as the excluded category, immediately delivers a comparison between two-year entry versus no college in $\beta_{2\leftarrow 0}$,

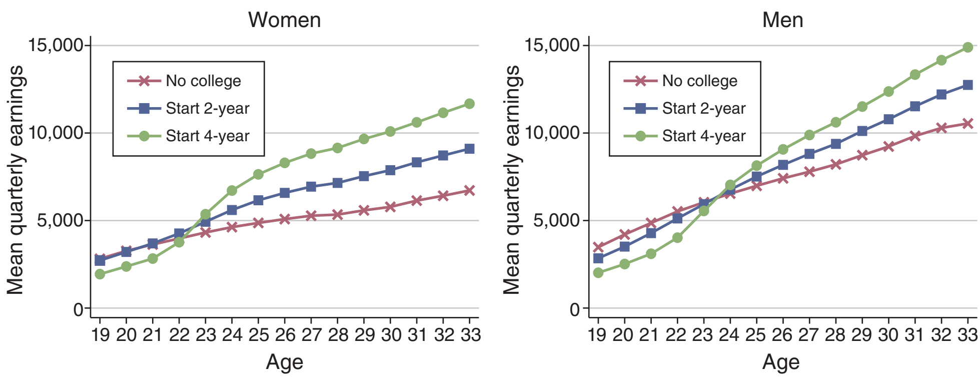

Figure 3. Earnings Profiles by Sector of Initial Enrollment

Notes: Quarterly earnings are measured in real 2010 US dollars and averaged within person at each age. Each point is the coefficient on the corresponding age dummy in a regression of earnings on age indicators and cohort indicators. Earnings after age 29 are only available for progressively older cohorts in the analysis sample, as the earnings data end in 2015.

Table 2—Raw and Controlled OLS Regressions

	Years of schooling		Bachelor's degree		Quarterly earnings	
	Raw	Controlled	Raw	Controlled	Raw	Controlled
Start two-year versus no college	1.75	1.48	0.189	0.149	1,417	1,088
	(0.01)	(0.01)	(0.002)	(0.001)	(20)	(17)
Start two-year versus start four-year	−1.58	−1.33	−0.392	−0.355	−1,660	−1,401
	(0.01)	(0.01)	(0.002)	(0.002)	(30)	(23)
R^2	0.407	0.457	0.286	0.323	0.046	0.169
Observations	590,862	590,862	590,862	590,862	590,862	590,862

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 US dollars and averaged within person over ages 28–30. Controlled specification 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.

and a comparison between two-year entry versus four-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.

Taken at face value, the OLS results in Table 2 suggest democratizing students from no college into two-year entry yields significant gains in educational achievement and earnings, while at the same time diverting students from four-year to two-year entry has large negative consequences. Compared to observably similar four-year entrants, two-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 less per quarter around age 30. But how much do these outcome disparities reflect causal consequences of different enrollment choices, versus selection bias from systematically different students making systematically different choices? And what share of students are actually democratized versus diverted when two-year college access expands? These questions motivate the IV method developed in the next section.

III. Identification

A. *The Methodological Challenge*

IV offers 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—individuals whose choices respond to instrumental variation—under the standard IV assumptions of independence, exclusion, and monotonicity.

Multiple margins of treatment present a challenge within this paradigm. When instruments shift individuals among more than two treatment states, the relevant counterfactual states for compliers induced into a specific treatment may be both multiple and unobserved, which hampers causal comparisons of the consequences of one treatment versus another.[28] To see this in the setting of two-year community college enrollment, suppose a valid instrument Z_2 induces students into two-year college entry. 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 from the counterfactual of not attending any college ($2 \leftarrow 0$, "two from zero"), while other Z_2 compliers are diverted from the counterfactual of starting directly at a four-year institution ($2 \leftarrow 4$, "two from four"). Abstracting from covariates, the standard 2SLS approach to IV would specify the following outcome and first stage equations:

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

$$D_2 = \alpha_0 + \alpha_2 Z_2 + \eta,$$

where Y is a student outcome (e.g., bachelor's degree attainment or earnings), D_2 is a binary indicator for starting college at a two-year institution, and β_2 is the coefficient of interest. Online Appendix A shows that the 2SLS estimand β_2 represents a pooled LATE of two-year entry on student outcomes that combines the two distinct complier margins into a single weighted average:[29]

$$(2) \qquad \beta_2 = \underbrace{LATE_2}_{\text{Net effect of 2-year entry}} = \omega \underbrace{LATE_{2\leftarrow 0}}_{\text{Democratization effect}} + (1-\omega)\underbrace{LATE_{2\leftarrow 4}}_{\text{Diversion effect}}.$$

The weight ω captures the share of Z_2 compliers who are on the $2 \leftarrow 0$ democratization margin, and this share is identified by the reduction in Pr(no college) induced by Z_2 as a fraction of the increase in $\Pr(D_2)$. The distinct $2 \leftarrow 0$ democratization and

[28] See Kirkeboen, Leuven, and Mogstad (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 preference lists over programs. 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.

[29] Heckman and Urzua (2010); Hull (2018); and Kline and Walters (2016) provide related derivations, as do Angrist and Imbens (1995) for the case of ordered multivalued treatments.

$2 \leftarrow 4$ diversion treatment effects are not separately identified, however, leaving these likely opposing impacts of two-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 two-year entry on all students affected by the instrument, which may correspond to policy-relevant variation like closer access to a two-year college campus, subsidized two-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 (2) 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 two-year colleges relative to their four-year counterparts, this case could potentially justify broad investment in two-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 two-year access, perhaps in favor of targeted policies towards the types of students more likely to be on the democratization margin, minimizing the mass of compliers diverted from direct four-year entry.

With two treatment margins of interest and only one instrument, the preceding 2SLS framework is fundamentally underidentified. A natural next step would be to consider multivariate 2SLS with two endogenous treatments when a second instrument is available, e.g., an exogenous Z_4 that makes starting at a four-year college more attractive. Writing the second stage equation in the same form as the OLS specification in (1), with the no-college treatment D_0 and the four-year entry treatment D_4 separately measured relative to two-year entry as the excluded category, would seem to deliver a $2 \leftarrow 0$ democratization effect via $-\beta_0$ and a $2 \leftarrow 4$ diversion effect via $-\beta_4$:

$$
\begin{aligned}
Y &= \kappa + \beta_0 D_0 + \beta_4 D_4 + \epsilon, \\
D_0 &= \alpha_0 + \alpha_2 Z_2 + \alpha_4 Z_4 + \eta_0, \qquad (3) \\
D_4 &= \gamma_0 + \gamma_2 Z_2 + \gamma_4 Z_4 + \eta_4.
\end{aligned}
$$

In fact, Kirkeboen, Leuven, and Mogstad (2016) show that the multivariate 2SLS framework in (3) does not generally identify well-defined causal parameters, even with one instrument per endogenous treatment, except in special cases like homogeneous treatment effects across all individuals. Instead, each of the 2SLS estimands β_0 and β_4 is a linear combination of multiple potential outcome comparisons mixed across distinct treatment margins and distinct complier subpopulations, as I

Table 3—Overidentification Tests of Treatment Effect Heterogeneity

	Multivariate 2SLS just-identified (1)	Multivariate 2SLS overidentified (2)	Multivariate 2SLS overidentified (3)
First stage instruments	Z_2, Z_4	$Z_2, Z_4, Z_2^2, Z_4^2, Z_2 \times Z_4$	$Z_2, Z_4, Z_2 \times X, Z_4 \times X$
Years of schooling			
2←0 democratization effect	1.77	2.47	1.81
	(0.28)	(0.21)	(0.16)
2←4 diversion effect	−1.06	−1.59	−0.98
	(0.19)	(0.15)	(0.17)
Overidentification test p-value		<0.001	<0.001
Bachelor's degree			
2←0 democratization effect	0.258	0.385	0.195
	(0.056)	(0.044)	(0.031)
2←4 diversion effect	−0.298	−0.410	−0.277
	(0.039)	(0.032)	(0.035)
Overidentification test p-value		<0.001	<0.001
Quarterly earnings			
2←0 democratization effect	973	2,222	1,432
	(827)	(642)	(482)
2←4 diversion effect	−1,906	−2,034	−4,059
	(571)	(459)	(523)
Overidentification test p-value		0.002	<0.001
Observations	590,862	590,862	590,862

Notes: The just-identified multivariate 2SLS estimates in column 1 come from a 2SLS regression of each outcome on indicators for no college and four-year entry, yielding democratization and diversion parameters relative to the omitted two-year treatment, using two-year distance (Z_2) and four-year distance (Z_4) as instruments. The overidentified multivariate 2SLS estimates in column 2 come from the same specification as 1, but with three additional instruments: two-year distance squared, four-year distance squared, and two-year distance times four-year distance. The estimates in column 3 come from the same specification as column 1, but with four additional instruments: two-year distance interacted with gender and disadvantaged status (FRPL), and four-year distance interacted with gender and disadvantaged status. All specifications include the baseline control set described in Section IVB. Standard errors in parentheses, and p-values from Hansen's J-statistic overidentification test, are clustered at the high school campus by cohort level. Academic outcomes are measured at age 28. Quarterly earnings are measured in real 2010 US dollars and averaged within person over ages 28–30.

derive explicitly in online Appendix B.[30] In a world of treatment effect heterogeneity, these linear combinations do not generally identify an average impact of one choice versus another among that treatment margin's compliers, or even economically interpretable net effects as in the single treatment model of equation (2).

Empirically, several pieces of evidence point to systematic heterogeneity in treatment effects across students on the margin of two-year college enrollment, hampering the ability of a traditional multivariate 2SLS approach like (3) to recover interpretable parameters and motivating the development of a new approach that accommodates such heterogeneity. First, Table 3 presents results from overidentification tests that reject constant treatment effects across individuals. Column 1 shows estimates of the $-\beta_0$ democratization effect and $-\beta_4$ diversion effect from the baseline 2SLS model in (3), which is just-identified under constant effects using the two instruments of a student's distance to the nearest two-year campus (Z_2)

[30] A similar result applies when interacting a single instrument with covariates in the attempt to generate additional sources of instrumental variation (Kline and Walters 2016; Hull 2018). See Pinto (2021) for a related discussion in the context of the multiple treatment arms of the Moving to Opportunity experiment.

and four-year campus (Z_4), controlling for the covariates detailed in Section IVB. Generating additional "instruments" by squaring and interacting Z_2 and Z_4 in column 2 of Table 3, and interacting Z_2 and Z_4 with covariates in column 3, yields overidentification test results that consistently reject the baseline model, suggesting that β_0 and β_4 are not constant parameters and instead vary systematically across students.[31] Second, the alternative identification approach developed in the remainder of this section can identify differences in observable characteristics across the compliers along different treatment margins. The implementation of this exercise in Table 6 shows that students democratized into college along the 2←0 treatment margin are a markedly different subpopulation compared to diverted students along the 2←4 margin, making it more difficult to assume these two distinct subpopulations would have identical, and thus interchangeable, treatment effects along a given margin, as the traditional 2SLS approach implicitly does. Finally, and most directly, the treatment effect estimates in Sections VD and VE indicate substantial heterogeneity in both democratization and diversion effects across observable student dimensions like gender and family income, making it difficult to rule out effect heterogeneity across unobserved dimensions as well.

B. *Setting up the Separate Identification Approach: Notation and Instruments*

To overcome the limitations of traditional multivariate 2SLS in the presence of effect heterogeneity, I now develop an alternative IV approach that separately identifies causal effects along multiple treatment margins while remaining robust to such heterogeneity. Let us first collect and augment the notation used throughout this section. I suppress the individual index i and implicitly condition on the control set X, detailed in Section IV. The three mutually exclusive and exhaustive treatments are $D = 2$ (start college at a two-year institution), $D = 4$ (start college directly at a four-year institution), 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. Two continuous instruments influence treatment choices: Z_2 (distance to the nearest two-year college) and Z_4 (distance to the nearest four-year college). Denote potential treatment choice as $D(z_2, z_4) \in \{0, 2, 4\}$, i.e., 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, i.e., the outcome a student would reap if exogenously assigned to treatment $D = d \in \{0, 2, 4\}$. We observe the realized outcome $Y = Y_0 D_0 + Y_2 D_2 + Y_4 D_4$. The potential outcome contrasts $Y_2 - Y_0$ (democratization effect) and $Y_2 - Y_4$ (diversion effect) are our treatment-margin-specific causal effects of interest.

[31] Of course, an alternative interpretation of the overidentification test is an assessment of instrument validity under a maintained assumption of constant treatment effects. The other pieces of evidence of heterogeneity discussed in this paragraph make such a premise difficult to maintain. Section IVB conducts instrument diagnostics that do not require first assuming constant treatment effects.

Throughout this section, I maintain the assumption of valid instrumental variation, i.e., the instruments are as good as randomly assigned and only affect outcomes through choices:

ASSUMPTION IE (Independence and Exclusion): $(Z_2, Z_4) \perp\!\!\!\perp (Y_0, Y_2, Y_4, \{D(z_2, z_4)\}_{\forall(z_2,z_4)})$.

Relative to its standard invocation in the case of a binary instrument and binary treatment, Assumption IE simply expands the instrumental variation to two continuous dimensions (Z_2, Z_4) and expands the treatment options from two to three $(D \in \{0,2,4\})$. Section IV probes this assumption empirically.

C. *Intuiting the Separate Identification Approach with an Index Model*

Instead of attempting to use these instruments to recover multiple causal differences in a single outcome equation like (3), with the undesirable consequence of mixing comparisons across distinct treatment margins and complier groups, the separate identification approach first isolates the mean potential outcomes of each distinct complier group along each treatment margin of interest. Then, with these separately identified components in hand, appropriately differencing them delivers well-defined causal effects for compliers along each margin.

In this subsection, I use a discrete choice index model to intuitively derive and visualize this separate identification approach. Section IIID generalizes the index model to a set of assumptions that deliver the same identification results, and online Appendix C provides the proofs. In the illustrative index model, individuals have latent indirect utilities for each mutually exclusive and exhaustive treatment option,

$$I_0 = 0,$$

$$I_2 = U_2 - \mu_2(Z_2),$$

$$I_4 = U_4 - \mu_4(Z_4),$$

where the utility of no college is normalized to zero. The variable U_2 is an individual's gross utility from two-year entry (relative to no college), representing unobserved individual preference heterogeneity from the econometrician's perspective: $\mu_2(Z_2)$ is the cost of two-year entry, shifted around by the continuous two-year instrument Z_2, such that $U_2 - \mu_2(Z_2)$ is the net utility of starting at a two-year college. Likewise, U_4 is unobserved preference heterogeneity for four-year entry, weighed against its cost $\mu_4(Z_4)$, such that $U_4 - \mu_4(Z_4)$ is the net utility of starting college in the four-year sector.[32] The functions $\mu_2(\cdot)$ and $\mu_4(\cdot)$

[32] In principle, one could allow $\mu_2(\cdot)$ to also depend on Z_4, and $\mu_4(\cdot)$ to also depend on Z_2. In that case, instead of working with shifts in each instrument directly to identify complier potential outcomes, as this paper does, the index functions $\mu_2(Z_2, Z_4)$ and $\mu_4(Z_2, Z_4)$ would need to be nonparametrically identified in a first step, then manipulated to induce complier flows from conditional variation in the value of each index function while holding the other fixed (e.g., Lee and Salanié 2018). My approach in Section IIID bypasses this demanding first step by deriving conditions on complier behavior that allow a series of local two stage least squares (2SLS) regressions with modified

are strictly increasing, differentiable, and homogeneous across individuals; (U_2, U_4) vary continuously with full support across $\mathbb{R}^2$, but with otherwise unrestricted functional form. Each individual chooses the alternative with the highest indirect utility, implying the treatment choice equations

$$\begin{aligned} D_0(z_2, z_4) &= \mathbf{1}[I_0 > I_2, I_0 > I_4] = \mathbf{1}[U_2 < \mu_2(z_2), U_4 < \mu_4(z_4)], \\ D_2(z_2, z_4) &= \mathbf{1}[I_2 > I_0, I_2 > I_4] \\ &= \mathbf{1}[U_2 > \mu_2(z_2), U_4 - U_2 < \mu_4(z_4) - \mu_2(z_2)], \\ D_4(z_2, z_4) &= \mathbf{1}[I_4 > I_0, I_4 > I_2] \\ &= \mathbf{1}[U_4 > \mu_4(z_4), U_4 - U_2 > \mu_4(z_4) - \mu_2(z_2)]. \end{aligned} \tag{4}$$

Figure 4 illustrates the usefulness of this structured choice model in visualizing the mechanics of the separate identification approach. First, panel A of Figure 4 shows how the treatment choice equations partition the two-dimensional space of unobserved preference heterogeneity (U_2, U_4) for a given value of the instruments (z_2, z_4). Individuals who choose $D = 0$ (no college) have low preference values for both two-year and four-year enrollment relative to their costs, while those who choose $D = 2$ (two-year entry) or $D = 4$ (four-year entry) have relatively higher values of the corresponding U_j.

Next, panel B of Figure 4 visualizes how a shift in the two-year instrument generates compliers along both the 2←0 democratization margin and the 2←4 diversion margin. A decrease in Z_2 to $z_2' < z_2$, while holding Z_4 fixed at z_4, lowers the cost of two-year enrollment to $\mu_2(z_2') < \mu_2(z_2)$. This shifts the treatment partition to the left and expands the size of the $D = 2$ region monotonically, as no individuals find two-year entry *less* attractive as its cost decreases. This expansion of two-year enrollment comes at the expense of both no college and direct four-year entry, thus inducing democratized 2←0 compliers as well as diverted 2←4 compliers in the two regions swept over by the instrument-induced shift in the partition.

Recalling equation 2, this single instrument shift is insufficient to disentangle the mean democratization effect on the outcomes of the 2←0 compliers from the mean diversion effect on the outcomes of the 2←4 compliers. The shift in Z_2 does separately identify two key ingredients of these margin-specific treatment effects, however. First, the mean potential outcome of 2←0 compliers in their no-college counterfactual (Y_0) is revealed by compositional changes in the outcomes of the $D = 0$ treatment group. To see this, note that as the treatment partition sweeps to the left in panel B of Figure 4, any change in the composition of the $D = 0$ group is entirely driven by 2←0 compliers leaving this group. Since the outcome-by-treatment interaction $YD_0 = Y_0$ when $D_0 = 1$, and $YD_0 = 0$ otherwise, the Z_2-induced change in the mean of YD_0, scaled by the mean change in D_0, is precisely the mean Y_0

outcome variables to recover all of the ingredients necessary for identifying margin-specific treatment effects. The index model in (4) is sufficient, but not necessary, for these conditions on complier behavior to hold, as shown in online Appendices D and E, respectively.

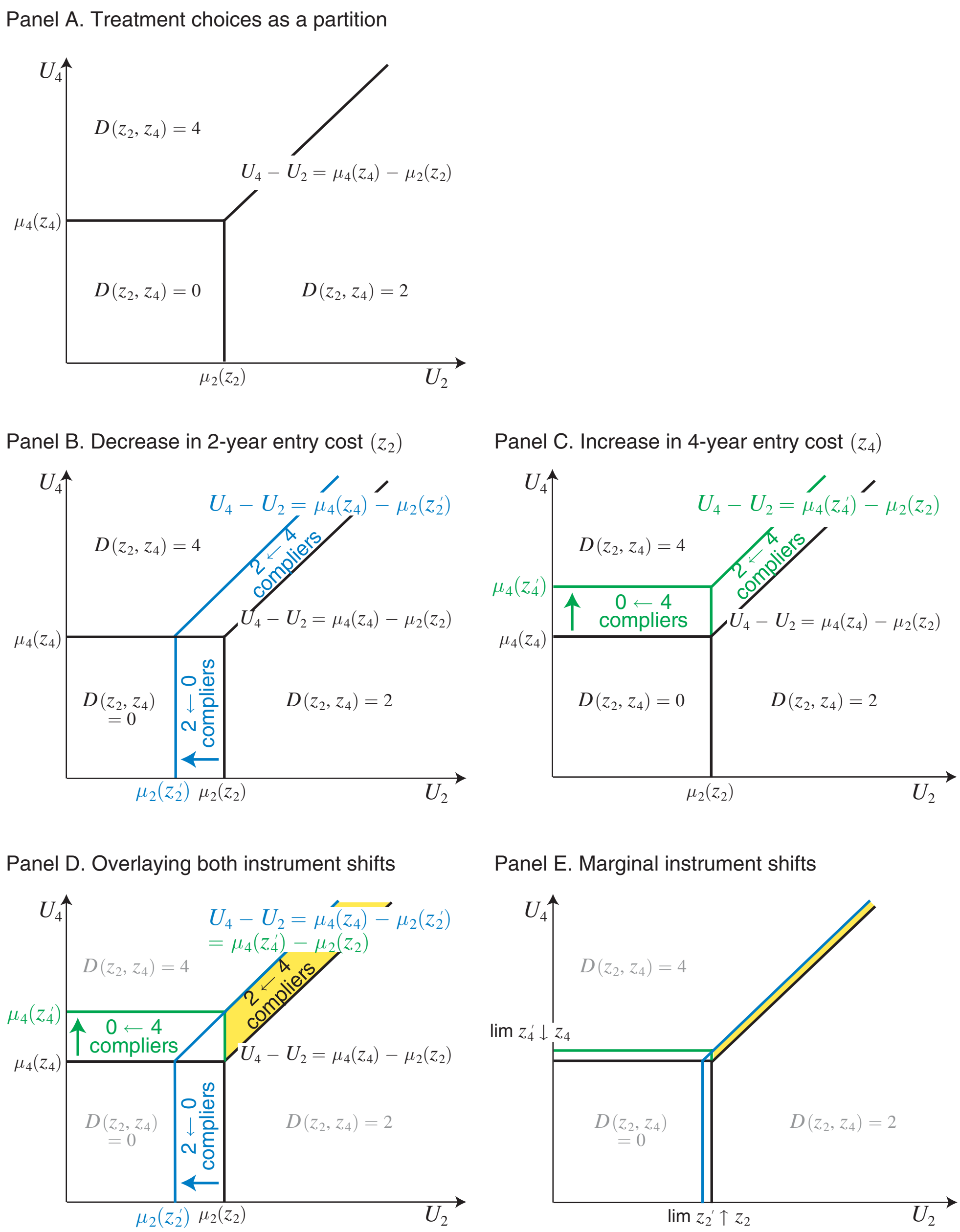

Figure 4. Visualizing the Separate Identification Approach with an Index Model

Notes: This figure visualizes the mechanics of the separate identification approach using a discrete choice index model. Panel A shows how the treatment choice equations, given a particular pair of instrument values, partition the two-dimensional space of unobserved preference heterogeneity into individuals who choose each treatment. Panel B illustrates a shift in Z_2 inducing 2←0 and 2←4 compliers, and panel C illustrates a shift in Z_4 inducing 0←4 and 2←4 compliers. Panel D overlays these two shifts to illustrate their overlapping compliers along the 2←4 diversion margin, and panel E visualizes the convergence of this overlap as the discrete instrument shifts converge to marginal shifts.

among 2⟵0 compliers, i.e., the outcome that democratized students would have realized, on average, had they not attended any college:

$$(5)\qquad \frac{E\left[YD_0 \mid z_2', z_4\right] - E\left[YD_0 \mid z_2, z_4\right]}{E\left[D_0 \mid z_2', z_4\right] - E\left[D_0 \mid z_2, z_4\right]} = E\left[Y_0 \mid 2 \leftarrow 0 \quad \text{complier w.r.t.} \quad (z_2', z_4) \leftarrow (z_2, z_4)\right].$$

Online Appendix C derives this result formally, along with the other equations in this subsection, under the weaker assumptions discussed in the next subsection. Note that estimating the Wald (1940)-esque ratio on the left side of (5) has a straightforward implementation as a local 2SLS regression of YD_0 on D_0, instrumenting for D_0 with Z_2 and conditioning on $Z_4 = z_4$. Also note that this separate identification approach can identify other features of the marginal distributions of complier potential outcomes, not simply means, by replacing Y with functions of Y, e.g., $\mathbf{1}(Y \leq y)$.

Second, by analogous logic, notice that any change in the composition of the $D = 4$ treatment group in panel B of Figure 4 is entirely driven by diverted 2⟵4 compliers leaving this group. Since $YD_4 = Y_4$ when $D_4 = 1$, and $YD_4 = 0$ otherwise, the Z_2-induced change in the mean of YD_4, scaled by the mean change in D_4, is precisely the mean Y_4 among 2⟵4 compliers, i.e., the outcome that diverted students would have realized, on average, had they started college at a four-year institution instead:

$$(6)\qquad \frac{E\left[YD_4 \mid z_2', z_4\right] - E\left[YD_4 \mid z_2, z_4\right]}{E\left[D_4 \mid z_2', z_4\right] - E\left[D_4 \mid z_2, z_4\right]} = E\left[Y_4 \mid 2 \leftarrow 4 \quad \text{complier w.r.t.} \quad (z_2', z_4) \leftarrow (z_2, z_4)\right].$$

Of course, knowing the mean potential outcome levels of Y_0 for 2⟵0 compliers and Y_4 for 2⟵4 compliers is not sufficient for identifying democratization and diversion treatment effects. We also need the mean Y_2 for each of these distinct complier groups. Panel B of Figure 4 shows why these are not separately identified by the shift in Z_2. Unlike the compositional changes in the $D = 0$ and $D = 4$ treatment groups, which were each driven by only one complier type, the compositional change in the $D = 2$ group is driven by both complier types entering this treatment group simultaneously. With only one observable quantity—the Z_2-induced change in YD_2—we cannot separately identify the two unknowns that contribute to it, i.e., the mean Y_2 of 2⟵0 compliers versus the mean Y_2 of 2⟵4 compliers:

$$(7)\qquad \begin{aligned} &E\left[YD_2 \mid z_2', z_4\right] - E\left[YD_2 \mid z_2, z_4\right] \\ &\quad = E\left[Y_2 \mid 2 \leftarrow 0 \quad \text{complier w.r.t.} \quad (z_2', z_4) \leftarrow (z_2, z_4)\right] \\ &\qquad \times \Pr\left[2 \leftarrow 0 \quad \text{complier w.r.t.} \quad (z_2', z_4) \leftarrow (z_2, z_4)\right] \\ &\qquad + E\left[Y_2 \mid 2 \leftarrow 4 \quad \text{complier w.r.t.} \quad (z_2', z_4) \leftarrow (z_2, z_4)\right] \\ &\qquad \times \Pr\left[2 \leftarrow 4 \quad \text{complier w.r.t.} \quad (z_2', z_4) \leftarrow (z_2, z_4)\right]. \end{aligned}$$

Without further assumptions, then, instrumental variation in Z_2 alone is insufficient to identify margin-specific treatment effects, which requires disentangling these mean Y_2 values for each complier type. One possible approach would be to equate these two unknown quantities by assumption, as explored by Lee and Salanié (2020); then (7) would feature only one mean potential outcome, thus identified.[33] Such a homogeneity assumption across compliers along *different* treatment margins may be reasonable in some settings, but it would be unreasonably strong in this one: Table 6 below shows that students democratized into college along the 2←0 treatment margin tend to have substantially lower test scores than diverted students along the 2←4 margin, making it difficult to assume these two distinct complier subpopulations would have equal mean values of the potential outcome level Y_2.

Instead of assuming homogeneity across compliers along different treatment margins, we can turn to partial variation in the other instrument, Z_4, which also induces compliers along the 2←4 diversion margin. Panel C of Figure 4 starts from the same baseline instrument values (z_2, z_4) as the Z_2 shift, and thus the same initial treatment partition, but visualizes a shift in Z_4 to $z_4' > z_4$ while holding Z_2 fixed at z_2. This increases the cost of four-year entry to $\mu_4(z_4') > \mu_4(z_4)$, which shifts the partition upward and makes four-year entry less attractive relative to its two alternatives of no college and two-year entry. Crucially, unlike the shift in Z_2, the Z_4-induced change in the composition of the $D = 2$ treatment group is driven solely by 2←4 compliers entering this group. The Z_4-induced change in YD_2, scaled by the change in D_2, thus isolates the mean Y_2 potential outcome of 2←4 compliers alone:

$$\text{(8)} \qquad \frac{E\big[YD_2 \,|\, z_2, z_4'\big] - E\big[YD_2 \,|\, z_2, z_4\big]}{E\big[D_2 \,|\, z_2, z_4'\big] - E\big[D_2 \,|\, z_2, z_4\big]}$$

$$= E\Big[Y_2 \,|\, 2 \leftarrow 4 \quad \text{complier w.r.t.} \quad (z_2, z_4') \leftarrow (z_2, z_4)\Big].$$

The bottom panels of Figure 4 put these pieces together to complete the visual argument. By overlaying the partial shift in Z_2 and the partial shift in Z_4, both starting from the same baseline point (z_2, z_4), panel D shows that these two different instrument shifts generate overlapping 2←4 compliers (shaded) near the margin of indifference between two-year and four-year entry, visualized in this structured choice model as the diagonal line defined by $U_4 - U_2 = \mu_4(z_4) - \mu_2(z_2)$. Panel E visualizes how the shaded complier overlap of these two instrument shifts along the 2–4 indifference margin becomes identical in the limit as discrete shifts converge to marginal shifts, highlighting the usefulness of continuous instruments in this framework.[34] Hence, we can use the marginal Z_4 shift to separately identify the mean Y_2

[33] A simple generalization of this approach would be to trace out the schedule of democratization and diversion effects implied by different hypothesized values of the difference between the two unknown means in (7).

[34] With two discrete instruments, there would be no guarantee that the 2←4 diversion compliers with respect to Z_2 and the 2←4 diversion compliers with respect to Z_4 would overlap as closely as drawn in the bottom panels of Figure 4, e.g., if the discrete shift in $\mu_4(Z_4)$ were substantially larger than the discrete shift in $\mu_2(Z_2)$. The triangular region in the center of panel D of Figure 4, moreover, shows that the 2←4 complier overlap in this model would always be imperfect with discrete shifts in Z_2 and Z_4. Identifying margin-specific LATEs with discrete instruments would thus require additional assumptions about homogeneity in potential outcomes across these different complier groups, as explored by Lee and Salanié (2020). Continuous instruments, in contrast, help narrow in on precisely the indifference margin along which 2←4 compliers with respect to Z_2 are *identical* to 2←4 compliers with respect

among marginal 2←4 compliers, which in turn can be used to back out what the mean Y_2 among marginal 2←0 compliers must be to satisfy (7). Finally, with all four mean potential outcomes separately identified via equations (5) through (8), we can difference them to form the two margin-specific treatment effects of interest: the average $Y_2 - Y_0$ democratization effect among marginal 2←0 compliers, and the average $Y_2 - Y_4$ diversion effect among marginal 2←4 compliers.

While the index model in (4) and its visualization in Figure 4 help build intuition for the identification procedure, it is more structure than necessary for disentangling margin-specific treatment effects with IV. The next subsection therefore relaxes the index model to a more general set of assumptions that deliver the same identification results, clarifying the exact restrictions on complier behavior that deliver point identification of margin-specific treatment effects in this setting.

D. *Relaxing the Index Model*

To develop the separate identification approach in a more general framework, remove the imposition of the index model in (4), leaving the potential treatment function $D(z_2, z_4)$ (and its binary variants D_0, D_2, and D_4) unstructured. Identifying margin-specific treatment effects will require two key restrictions on complier behavior for which the index model was sufficient (proven in online Appendix D) but not necessary (proven in online Appendix E).

Unordered Partial Monotonicity.—The first of these restrictions is a generalization of instrument monotonicity to multiple instruments and multiple treatments:

ASSUMPTION UPM (Unordered Partial Monotonicity): *For all z_2, z_2', z_4 with $z_2' < z_2$ and holding z_4 fixed,*

$$D_2(z_2', z_4) \geq D_2(z_2, z_4),\, D_0(z_2', z_4) \leq D_0(z_2, z_4), \text{ and } D_4(z_2', z_4) \leq D_4(z_2, z_4)$$

for all individuals, with each inequality holding strictly for at least some individuals.

For all z_4, z_4', z_2 with $z_4' < z_4$ and holding z_2 fixed,

$$D_4(z_2, z_4') \geq D_4(z_2, z_4),\, D_0(z_2, z_4') \leq D_0(z_2, z_4) \text{ and } D_2(z_2, z_4') \leq D_2(z_2, z_4)$$

for all individuals, with each inequality holding strictly for at least some individuals.

Assumption UPM retains the intuition of "no defiers" from the binary case: each instrument shift renders each treatment either weakly more attractive for all individuals, or weakly less attractive for all individuals, thus ruling out simultaneous flows of different individuals both into and out of a given treatment

to Z_4 in this structured discrete choice model, as visualized in panel E of Figure 4, foreshadowing the generalized "comparable compliers" condition in the next subsection.

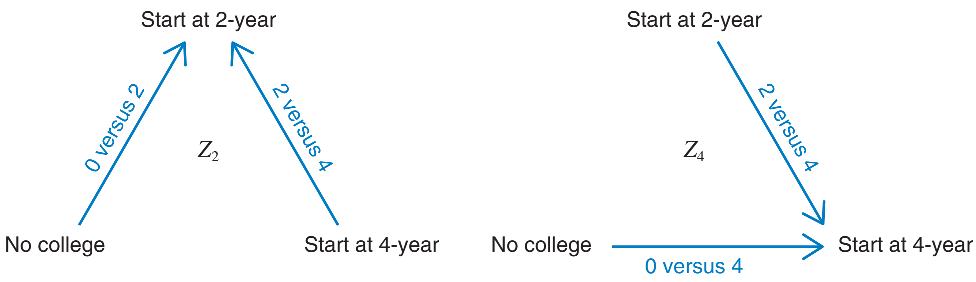

Figure 5. Unordered Partial Monotonicity

Notes: This figure visualizes the complier flows permitted by Assumption UPM. The variable "Z_2" denotes a marginal decrease in Z_2 while holding Z_4 fixed, and "Z_4" denotes a marginal decrease in Z_4 while holding Z_2 fixed.

in response to a given instrument shift.[35] As Figure 5 illustrates, however, this does not limit each instrument to only inducing one type of complier: closer two-year proximity$(z_2' < z_2)$ induces both 2←0 and 2←4 compliers by rendering two-year entry weakly more attractive $(D_2(z_2', z_4) \geq D_2(z_2, z_4))$ at the expense of no college $(D_0(z_2', z_4) \leq D_0(z_2, z_4))$ and four-year entry $(D_4(z_2', z_4) \leq D_4(z_2, z_4))$. Likewise, closer four-year proximity $(z_4' < z_4)$ induces both 4←0 and 4←2 compliers by rendering four-year entry more attractive $(D_4(z_2, z_4') \geq D_4(z_2, z_4))$ at the expense of no college $(D_0(z_2, z_4') \leq D_0(z_2, z_4))$ and two-year entry $(D_2(z_2, z_4') \leq D_2(z_2, z_4))$.[36]

Notice that these complier flows are the same as those induced by the more structured index model in (4); online Appendix E shows that Assumption UPM is strictly weaker, however, by considering a nonseparable index model that generalizes (4) and no longer satisfies the two-dimensional visualization in Figure 4, but still satisfies Assumption UPM. To summarize, one can generalize (4) to allow for individual-level heterogeneity in instrument sensitivity, i.e., allowing $\mu_2(\cdot)$ and $\mu_4(\cdot)$ to vary across individuals. As long as this heterogeneity is such that $\mu_2(\cdot)$ and $\mu_4(\cdot)$ are still strictly increasing in their arguments for a given individual, then instrument responses still run in the same directions those induced by (4), thus continuing to satisfy Assumption UPM.

[35] Assumption UPM also embeds an instrument relevance condition by requiring that each instrument shift induce at least some individuals to switch into or out of each treatment.

[36] One potential violation of Assumption UPM would be an option value channel that causes four-year proximity to induce 2←0 compliers: the future prospect of upward transfer may inspire some noncollege individuals into two-year entry. Empirically, however, the first stage relationship between four-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 four-year proximity would be bounded above by this small first stage share. Online Appendix Table A.1, moreover, shows that neither two-year proximity nor four-year proximity influence the probability of graduating from high school, consistent with limited forward-looking behavior among instrument compliers. A different type of violation of Assumption UPM 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 the potential treatment functions. It is also empirically unlikely in this setting given that all two-year colleges are open enrollment, and the types of four-year institutions attended by marginal students tend to be open enrollment or have very inclusive admission policies.

Since the multiple treatments in this setting need not be ordered in any uniform way across individuals, Assumption UPM is closely related to the "unordered monotonicity" assumption of Heckman and Pinto (2018). Assumption UPM is weaker, however, in that it only concerns *partial* instrumental variation in Z_2 while holding Z_4 fixed, and likewise Z_4 holding Z_2 fixed, and is thus agnostic about complier flows when both Z_2 and Z_4 change value simultaneously.[37] 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, my relaxation to unordered "partial" monotonicity limits 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 shift in two-year distance $(z_2' < z_2)$ and four-year distance $(z_4' > z_4)$—which could be visualized in the structured index model of Figure 4 as a single shift in the partition to the northwest—would induce not only 2←0 compliers and 2←4 compliers, but also 0←4 compliers. Such behavior would violate Heckman and Pinto's (2018) unordered monotonicity condition by inducing some students into a given treatment (in this example, $D = 0$) at the same time that others are induced out of it. Assumption UPM, meanwhile, is fully consistent with these patterns, given its more limited imposition of unordered monotonicity on partial instrument shifts only.[38]

The payoff from Assumption UPM (in tandem with Assumption IE) is that it allows us to separately identify the mean potential outcomes of instrument compliers along each treatment margin, despite our inability to identify which specific individuals in the data actually comprise these complier groups. Online Appendix C derives the key identification results in equations (5) through (8) under solely Assumptions UPM and IE, not the more structured discrete choice index model used to illustrate the intuition of the method.

Comparable Compliers.—At first glance, the use of the second instrument, Z_4, to produce the result in equation (8) might appear to be the deus ex machina that disentangles the two mean potential outcomes of interest in (7), immediately allowing us to form separate treatment effects for democratization and diversion compliers. In the end, it will fulfill this role, but not without a final assumption and a sharper scope. Closer inspection of (8) shows that it actually involves a slightly different conditioning set than its counterpart in (7): notice that (8) involves 2←4 compliers with respect to the partial shift in Z_4, $(z_2, z_4') \leftarrow (z_2, z_4)$, whereas its counterpart in (7) involves 2←4 compliers with respect to the partial shift in Z_2, $(z_2', z_4) \leftarrow (z_2, z_4)$. To be clear, both of these conditioning sets are comprised of students who would choose $D = 4$ when assigned to the base instrument value (z_2, z_4), and would switch to $D = 2$ in response to two-year college becoming relatively more accessible than

[37] On the other hand, while Assumption UPM is written to apply to discrete or continuous instruments, some of the identification results in this section exploit marginal shifts in continuous instruments, which is an important way in which this approach is more demanding than Heckman and Pinto's (2018) discrete instrument framework.

[38] If D were binary instead of multinomial, partial monotonicity 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 multiple elements of the vector Z change value simultaneously. See Mogstad, Torgovitsky, and Walters (2021) for further discussion of partial monotonicity in the binary treatment case.

four-year college. The difference is simply which instrument caused the relative change: in one case, Z_2 decreased while holding Z_4 fixed, while in the other, Z_4 increased while holding Z_2 fixed.

As visualized in panel D of Figure 4 and discussed in footnote 34, these two groups of 2⟵4 compliers need not be identical subpopulations when the shifts in Z_2 and Z_4 are discrete, since their overlap may be imperfect. Narrowing in on marginal shifts in Z_2 and Z_4, however, as visualized in panel E of Figure 4, did isolate exactly the same subpopulation of marginal 2⟵4 compliers in the index model. The structure in (4) is thus sufficient for (8) to disentangle (7) as the shifts in Z_2 and Z_4 become arbitrarily small, since the two conditioning sets of 2⟵4 compliers involved become identical in the limit. As with Assumption UPM, however, the structure in (4) is not necessary: the nonseparable model in online Appendix E generalizes (4) but still features identical 2⟵4 compliers with respect to marginal shifts in Z_2 and Z_4. The final assumption below therefore generalizes this "identical compliers" feature of the index model, stripping it down to the minimal requirement that 2⟵4 compliers with respect to marginal shifts in Z_2 and Z_4 from the same base value have comparable mean Y_2 potential outcomes:

ASSUMPTION CC (Comparable Compliers): *For all base values* (z_2, z_4),

$$\lim_{z_2' \uparrow z_2} E\left[Y_2 \mid D(z_2', z_4) = 2, D(z_2, z_4) = 4\right]$$

$$= \lim_{z_4' \downarrow z_4} E\left\{Y_2 \mid D(z_2, z_4') = 2, D(z_2, z_4) = 4.\right.$$

To understand the content of Assumption CC, consider all students who live in a given location, and thus face the same distance costs to the nearest two-year and four-year options (z_2, z_4). A subset of these students are planning to start college at a four-year institution, given these distances $(D(z_2, z_4) = 4)$. The left side of the equation in Assumption CC considers the further subset of such four-year entrants who would switch to two-year entry if the nearest two-year campus were slightly closer $(D(z_2', z_4) = 2)$. The right side of Assumption CC also considers four-year entrants at (z_2, z_4) who would switch to two-year entry, if the nearest four-year campus were slightly farther $(D(z_2, z_4') = 2)$. In the index model of (4), these two sets of students would perfectly coincide, and would therefore necessarily have the same mean Y_2. Assumption CC preserves this equality of mean potential outcomes, but does not require that it result from a structured index model like (4), and does not require that the two sets of compliers comprise the same common set of individuals.

To emphasize that the only difference between the two sets of students involved in Assumption CC is the particular instrument used to induce their switch from four to 2, another way to write the assumption is

$$E\left[Y_2 \mid \text{marginal 2–4 complier w.r.t. } Z_2 \text{ at } (z_2, z_4)\right]$$

$$= E\left[Y_2 \mid \text{marginal 2–4 complier w.r.t. } Z_4 \text{ at } (z_2, z_4)\right].$$

This formulation helps clarify what Assumption CC does *not* impose. First, it does not make any claims of comparability between compliers along different treatment margins: Assumption CC solely concerns diversion compliers, i.e., students along the 2–4 treatment margin, leaving them free to differ systematically from democratization compliers along the 2–0 margin. Second, Assumption CC does not require comparability between 2–4 diversion compliers induced by instrument shifts of large and differing magnitudes: its scope is limited to infinitesimal changes in Z_2 and Z_4. Finally, Assumption CC does not require comparability between 2–4 diversion compliers who face different initial costs of two-year and four-year entry: it only considers individuals who share the same base value of (z_2, z_4) and are on the margin of indifference between D_2 and D_4 given this cost schedule.

Assumption CC is automatically satisfied in a broad class of index models, including (4) and even its nonseparable generalization in online Appendix E. In the unstructured framework of this subsection, however, Assumption CC does not quite come for free; online Appendix F offers a counterexample of a nonseparable model that satisfies Assumptions IE and UPM but not CC. Intuitively, imagine that some (but not all) of the students who are on the margin between two-year and four-year entry at a given (z_2, z_4) point are completely insensitive to two-year distance, but still sensitive to four-year distance. Then these students will not be 2–4 compliers with respect to Z_2, but will be 2–4 compliers with respect to Z_4, breaking the exact overlap of the two complier groups involved in Assumption CC. If, furthermore, these Z_2-insensitive students are systematically different from the other marginal 2–4 students at (z_2, z_4) in terms of their mean potential outcomes, then Assumption CC may not hold.

Fortunately, it is straightforward to test for such differences empirically using observable student covariates. If (Z_2, Z_4) are as good as randomly assigned relative to some predetermined student characteristic W, then we can replace Y with W in (6) to identify

$$\text{(9)} \qquad \frac{E\big[WD_4 \,|\, z_2', z_4\big] - E\big[WD_4 \,|\, z_2, z_4\big]}{E\big[D_4 \,|\, z_2', z_4\big] - E\big[D_4 \,|\, z_2, z_4\big]}$$

$$= E\Big[W \,|\, 2 \leftarrow 4 \quad \text{complier w.r.t.} \quad (z_2', z_4) \leftarrow (z_2, z_4)\Big],$$

and likewise replace Y with W in (8) to identify

$$\text{(10)} \qquad \frac{E\big[WD_2 \,|\, z_2, z_4'\big] - E\big[WD_2 \,|\, z_2, z_4\big]}{E\big[D_2 \,|\, z_2, z_4'\big] - E\big[D_2 \,|\, z_2, z_4\big]}$$

$$= E\Big[W \,|\, 2 \leftarrow 4 \quad \text{complier w.r.t.} \quad (z_2, z_4') \leftarrow (z_2, z_4)\Big],$$

then compare the two for marginal shifts in Z_2 and Z_4. This exercise does not require Assumption CC, since (6) and (8) result from Assumptions IE and UPM alone.[39] It therefore offers a useful empirical diagnostic for assessing the validity

[39]The framework in this section is therefore overidentified in the sense that if equality of (9) and (10) is rejected, one could weaken Assumption CC to only hold conditional on W.

of Assumption CC in any given setting.[40] Table 6 below conducts this exercise with tenth grade standardized test scores, which are strong predictors of student outcomes, and finds that marginal 2⟵4 compliers with respect to Z_2 are statistically identical to marginal 2⟵4 compliers with respect to Z_4, bolstering the credibility of Assumption CC in this setting.

Putting the Pieces Together: Margin-Specific Marginal Treatment Effects.—Assumptions IE, UPM, and CC together deliver all the ingredients required for identifying margin-specific treatment effects. To match the marginal scope of Assumption CC, narrow the discrete instrument differences in (5) to partial derivatives by making the change in Z_2 arbitrarily small:

$$\begin{aligned}(11)\qquad &\lim_{z_2'\to z_2}\frac{E\big[YD_0\,|\,z_2',z_4\big]-E\big[YD_0\,|\,z_2,z_4\big]}{E\big[D_0\,|\,z_2',z_4\big]-E\big[D_0\,|\,z_2,z_4\big]}\\ &=\lim_{z_2'\to z_2}\frac{\dfrac{E\big[YD_0\,|\,z_2',z_4\big]-E\big[YD_0\,|\,z_2,z_4\big]}{z_2'-z_2}}{\dfrac{E\big[D_0\,|\,z_2',z_4\big]-E\big[D_0\,|\,z_2,z_4\big]}{z_2'-z_2}}=\frac{\dfrac{\partial E\big[YD_0\,|\,z_2,z_4\big]}{\partial Z_2}}{\dfrac{\partial E\big[D_0\,|\,z_2,z_4\big]}{\partial Z_2}}\\ &=\lim_{z_2'\to z_2}E\big[Y_0\,|\,D(z_2',z_4)=2,D(z_2,z_4)=0\big]\\ &\equiv E\big[Y_0\,|\,\text{marginal 2–0 complier w.r.t. } Z_2 \text{ at } (z_2,z_4)\big].\end{aligned}$$

Likewise, the marginal version of (6) is

$$(12)\qquad \frac{\dfrac{\partial E\big[YD_4\,|\,z_2,z_4\big]}{\partial Z_2}}{\dfrac{\partial E\big[D_4\,|\,z_2,z_4\big]}{\partial Z_2}}=E\big[Y_4\,|\,\text{marginal 2–4 complier w.r.t. } Z_2 \text{ at } (z_2,z_4)\big],$$

the marginal version of (7) is

$$\begin{aligned}(13)\qquad &\frac{\partial E\big[YD_2\,|\,z_2,z_4\big]}{\partial Z_2}\\ &=E\big[Y_2\,|\,\text{marginal 2–0 complier w.r.t. } Z_2 \text{ at } (z_2,z_4)\big]\left(-\frac{\partial E\big[D_0\,|\,z_2,z_4\big]}{\partial Z_2}\right)\\ &\quad+E\big[Y_2\,|\,\text{marginal 2–4 complier w.r.t. } Z_2 \text{ at } (z_2,z_4)\big]\left(-\frac{\partial E\big[D_4\,|\,z_2,z_4\big]}{\partial Z_2}\right),\end{aligned}$$

[40] This test also does not require the shifts in Z_2 and Z_4 to be infinitesimal; if continuous instruments are unavailable, one could try to justify a discrete version of Assumption CC (or suggest a rejection of it) by comparing the observable characteristics of 2⟵4 compliers with respect to discrete shifts in Z_2 versus Z_4, subject to the discussion in footnote 34.

and the marginal version of (8) is

$$\text{(14)} \qquad \frac{\dfrac{\partial E[YD_2 \mid z_2, z_4]}{\partial Z_4}}{\dfrac{\partial E[D_2 \mid z_2, z_4]}{\partial Z_4}} = E\big[Y_2 \mid \text{marginal 2–4 complier w.r.t. } Z_4 \text{ at } (z_2, z_4)\big].$$

Applying Assumption CC to (12) and (14) therefore yields

$$\frac{\dfrac{\partial E[YD_2 \mid z_2, z_4]}{\partial Z_4}}{\dfrac{\partial E[D_2 \mid z_2, z_4]}{\partial Z_4}} - \frac{\dfrac{\partial E[YD_4 \mid z_2, z_4]}{\partial Z_2}}{\dfrac{\partial E[D_4 \mid z_2, z_4]}{\partial Z_2}} = E\big[Y_2 - Y_4 \mid \text{marginal 2–4 complier at } (z_2, z_4)\big]$$

$$\equiv MTE_{2\leftarrow 4}(z_2, z_4),$$

and applying it to (13) and (14), in tandem with (11), yields

$$\frac{\dfrac{\partial E[YD_2 \mid z_2, z_4]}{\partial Z_2}}{-\dfrac{\partial E[D_0 \mid z_2, z_4]}{\partial Z_2}} - \frac{\dfrac{\partial E[YD_2 \mid z_2, z_4]}{\partial Z_4}}{\dfrac{\partial E[D_2 \mid z_2, z_4]}{\partial Z_4}} \frac{\dfrac{\partial E[D_4 \mid z_2, z_4]}{\partial Z_2}}{\dfrac{\partial E[D_0 \mid z_2, z_4]}{\partial Z_2}} - \frac{\dfrac{\partial E[YD_0 \mid z_2, z_4]}{\partial Z_2}}{\dfrac{\partial E[D_0 \mid z_2, z_4]}{\partial Z_2}}$$

$$= E\big[Y_2 - Y_0 \mid \text{marginal 2–0 complier at } (z_2, z_4)\big]$$

$$\equiv MTE_{2\leftarrow 0}(z_2, z_4).$$

These margin-specific marginal treatment effects, $MTE_{2\leftarrow 0}(z_2, z_4)$ and $MTE_{2\leftarrow 4}(z_2, z_4)$, are simply the continuous instrument analogues to the discrete "sub-LATEs" from equation (2). After identifying $MTE_{2\leftarrow 0}(z_2, z_4)$ and $MTE_{2\leftarrow 4}(z_2, z_4)$ at each (z_2, z_4) evaluation point in the support of the instruments, any discrete sub-LATE of interest within the instrument support can be formed by integrating the relevant MTE over the domain of the discrete instrument shift of interest (Heckman and Vytlacil 2005).

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

$$MTE_2(z_2, z_4) = \frac{\dfrac{\partial E[Y \mid z_2, z_4]}{\partial Z_2}}{\dfrac{\partial E[D_2 \mid z_2, z_4]}{\partial Z_2}},$$

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

$$\omega(z_2, z_4) = \frac{-\dfrac{\partial E[D_0 \mid z_2, z_4]}{\partial Z_2}}{\dfrac{\partial E[D_2 \mid z_2, z_4]}{\partial Z_2}}.$$

Putting all of these separately identified pieces together delivers the decomposition of interest:

$$(15)\qquad \underbrace{MTE_2}_{\text{Net effect of 2-year entry}} = \omega \underbrace{MTE_{2\leftarrow 0}}_{\text{Democratization effect}} + (1-\omega)\underbrace{MTE_{2\leftarrow 4}}_{\text{Diversion effect}},$$

where each component is identified at each point in the instrument support with nonzero first-stage derivatives.

IV. Estimation and Instrument Diagnostics

A. *Locally Linear Specification*

All of the ingredients that go into equation (15) are 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), evaluated at a given point in the support of (Z_2, Z_4). In many empirical applications, the instruments may only satisfy Assumptions IE, UPM, and CC conditional on a control set X. All of the preceding arguments still apply after conditioning on each $X = x$, but the curse of dimensionality quickly sets in as X becomes high-dimensional and includes continuous variables, as it does in my setting.

To reduce this dimensionality problem, I assume that the conditional expectations of interest are well approximated by a specification that is locally linear in the instruments and globally linear in the control set X. That is, for a given variable $T \in \{D_0, D_2, D_4, Y, YD_0, YD_2, YD_4\}$, the estimated coefficients at each (z_2, z_4) evaluation point solve a kernel-weighted least squares problem:

$$\begin{pmatrix} \hat{\beta}_2^T(z_2, z_4) \\ \hat{\beta}_4^T(z_2, z_4) \\ \hat{\beta}_x^T \end{pmatrix} = \underset{\beta_2, \beta_4, \beta_x}{\arg\min} \sum_{i=1}^{N} K\left(\frac{Z_{2i} - z_2}{h}, \frac{Z_{4i} - z_4}{h}\right)(T_i - \beta_2 Z_{2i} - \beta_4 Z_{4i} - X_i' \beta_x)^2,$$

where $K(\cdot)$ is a two-dimensional kernel with bandwidth h and X includes a constant. This specification offers a flexible compromise between two extremes: fully nonparametric estimation across all dimensions of (Z_2, Z_4, X), which is infeasible in this setting, and a globally linear specification that would constrain the β_2 and β_4 slope coefficients to remain constant across all (z_2, z_4) evaluation points, which is a common but strong restriction in IV estimation.

Forming the potential outcome and treatment effect estimates then proceeds by the analogy principle, plugging in the local slope coefficients $\hat{\beta}_2^T(z_2, z_4)$ and $\hat{\beta}_4^T(z_2, z_4)$ in place of the local partial derivatives $\frac{\partial E[T \mid Z_2 = z_2, Z_4 = z_4]}{\partial Z_2}$ and $\frac{\partial E[T \mid Z_2 = z_2, Z_4 = z_4]}{\partial Z_4}$ involved in each expression, e.g.

$$\hat{E}\left[Y_0 \mid \text{marginal 2–0 complier at } (z_2, z_4)\right] = \frac{\hat{\beta}_2^{YD_0}(z_2, z_4)}{\hat{\beta}_2^{D_0}(z_2, z_4)}.$$

These plug-in estimators are numerically equivalent to 2SLS estimators with modified outcome variables: continuing the example above, we would arrive at the same estimate of $\hat{E}[Y_0 \,|\, \text{marginal 2–0 complier at } (z_2, z_4)]$ through a 2SLS regression of YD_0 on D_0 instrumented with Z_2, controlling linearly for Z_4 and X in a local kernel-weighted region around the evaluation point (z_2, z_4).

I use a two-dimensional Epanechnikov (parabolic) kernel with 40-mile bandwidth to weight observations in the locally linear regressions; Table 8 below shows that the point estimates are similar when using smaller bandwidths but are less precisely estimated. I report the main results evaluated at the mean values of (Z_2, Z_4), while Section VB explores heterogeneity and selection patterns across different instrument evaluation points. 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.

B. *Instrument Diagnostics*

In my setting, two-year and four-year college proximity are unlikely to satisfy Assumption IE unconditionally, given that different types of families with high school aged students sort systematically into different types of locations. The first three columns of Table 4 confirm this: regressing two-year distance (top panel) and four-year distance (bottom panel) on individual student covariates in the main evaluation sample shows that higher ability students tend to live slightly farther away from each type of college, while minority and low-income students tend to live closer.

One could reasonably argue, however, that different types of families do not sort into locations on the basis of two-year and four-year college distances per se. Instead, families sort on more fundamental factors like local labor markets, preferences for different levels of urbanization, and neighborhood quality, which themselves happen to correlate with college distances. The remaining columns of Table 4 show that controlling for these "neighborhood fundamentals" tends to substantially weaken, and in many cases statistically eliminate, the relationships between college distances and student covariates. Column 6 controls for each of the 62 commuting zones in Texas and the neighborhood-level oil and gas employment share to proxy for local labor markets, column 7 adds controls for each of the 12 neighborhood-level NCES urbanization locale codes (e.g., midsize city, large suburb, fringe town, remote rural, etc.), and column 8 adds a cubic in the neighborhood quality index constructed in Section IIB. The lack of any conditional relationship between distances and the test score measure in the first row of column 8 in each panel in Table 4 is especially important given the remarkably strong power of this ability measure to predict college enrollment choices, as illustrated in Figure 1, and longer-run outcomes like earnings and years of completed schooling, as illustrated in online Appendix Figure A.4.

A few of the other covariates in Table 4, however, retain economically small but statistically significant relationships with two-year and/or four-year distance after controlling for neighborhood fundamentals. Table 5 therefore explores the robustness of the first stage and reduced form estimates, as well as the ability balance result,

TABLE 4—INSTRUMENT DIAGNOSTICS: RAW AND CONTROLLED RELATIONSHIPS WITH STUDENT COVARIATES

	Dependent variable: Z_2, miles to the nearest two-year college							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Relationships with two-year distance								
Test score percentile (0 to 1)	0.990				−0.435	−0.297	−0.067	−0.035
	(0.119)				(0.086)	(0.070)	(0.058)	(0.057)
Hispanic		−2.594			−1.525	−0.630	−0.457	−0.478
		(0.150)			(0.130)	(0.097)	(0.085)	(0.080)
Black		−3.282			−2.324	−0.185	0.050	0.023
		(0.136)			(0.128)	(0.103)	(0.089)	(0.087)
Asian		−4.215			−2.931	−1.106	−0.364	−0.325
		(0.150)			(0.152)	(0.117)	(0.089)	(0.089)
Disadvantaged (FRPL)			−0.873		0.319	0.215	−0.015	−0.033
			(0.107)		(0.073)	(0.059)	(0.050)	(0.040)
Z_4, miles to nearest four-year college				0.307	0.293	0.234	0.061	0.061
				(0.011)	(0.011)	(0.013)	(0.015)	(0.015)
R^2	0.001	0.039	0.003	0.203	0.218	0.463	0.603	0.604
	Dependent variable: Z_4, miles to the nearest four-year college							
Panel B. Relationships with four-year distance								
Test score percentile (0 to 1)	1.900				0.002	−0.280	−0.081	0.017
	(0.197)				(0.140)	(0.109)	(0.100)	(0.098)
Hispanic		−4.342			−2.869	−0.735	−0.373	−0.602
		(0.222)			(0.197)	(0.126)	(0.116)	(0.107)
Black		−3.795			−1.828	−0.423	0.199	0.053
		(0.243)			(0.226)	(0.161)	(0.145)	(0.145)
Asian		−4.600			−2.004	−1.262	−0.248	−0.194
		(0.261)			(0.265)	(0.182)	(0.166)	(0.157)
Disadvantaged (FRPL)			−1.548		0.324	0.501	0.424	0.248
			(0.165)		(0.110)	(0.085)	(0.084)	(0.063)
Z_2, miles to nearest two-year college				0.662	0.632	0.416	0.123	0.122
				(0.021)	(0.021)	(0.023)	(0.029)	(0.029)
R^2	0.002	0.037	0.005	0.203	0.216	0.556	0.628	0.632
Local labor market controls						✓	✓	✓
Urbanization controls							✓	✓
Neighborhood quality controls								✓
Observations	565,687	565,687	565,687	565,687	565,687	565,687	565,687	565,687

Notes: Test score percentiles in the top row of each panel are measured from 0 to 1 for readability. Standard errors in parentheses are clustered at the high school campus by cohort level. Observations are locally weighted around the mean values of the instruments to match the exact sample as the main causal effect estimates in Section V. Column 5 controls solely for gender and cohort year. Column 6 controls for each of the 62 commuting zones in Texas and a cubic polynomial in the neighborhood-level oil and gas employment share. Column 7 adds controls for each of the 12 neighborhood-level NCES urbanization locale codes (e.g., midsize city, large suburb, fringe town, remote rural, etc.). Column 8 adds a cubic polynomial in the neighborhood quality index constructed in Section IIB.

to the inclusion of these covariates as additional controls. The first four columns of Table 5 proceed in the same fashion as the last four columns of Table 4, beginning with uncontrolled regressions of each panel's dependent variable on two-year distance and four-year distance (now divided by 10 for more readable coefficients), and then sequentially adding the baseline controls for local labor markets, neighborhood urbanization, and neighborhood quality. While the first stage coefficients in panel B are fairly stable across all specifications, the changes in the reduced form coefficients in panel C across columns 1 through 4 demonstrate the importance of controlling for neighborhood fundamentals. Adding student demographic controls in

Table 5—Instrument Diagnostics: Balance, First Stages, and Reduced Forms

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Balance test						
Dependent variable: test score percentile (0 to 1)						
Z_2: miles/10	0.005	−0.003	0.001	0.002	−0.001	
	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	
Z_4: miles/10	0.007	−0.002	−0.002	0.001	0.000	
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	
R^2	0.002	0.019	0.024	0.039	0.101	
Panel B. First stages						
Dependent variable: D_2 (start at two-year college)						
Z_2: miles/10	−0.042	−0.042	−0.043	−0.044	−0.043	−0.043
	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)
Z_4: miles/10	0.026	0.023	0.017	0.019	0.020	0.020
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
R^2	0.004	0.013	0.017	0.019	0.027	0.038
Dependent variable: D_4 (start at four-year college)						
Z_2: miles/10	0.014	0.006	0.016	0.019	0.015	0.015
	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)
Z_4: miles/10	−0.025	−0.035	−0.028	−0.024	−0.026	−0.026
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
R^2	0.003	0.019	0.021	0.031	0.069	0.157
Panel C. Reduced forms						
Dependent variable: years of schooling						
Z_2: miles/10	−0.035	−0.101	−0.038	−0.025	−0.040	−0.037
	(0.011)	(0.015)	(0.016)	(0.013)	(0.011)	(0.009)
Z_4: miles/10	−0.020	−0.101	−0.081	−0.051	−0.055	−0.056
	(0.009)	(0.010)	(0.011)	(0.009)	(0.008)	(0.007)
R^2	0.000	0.012	0.015	0.038	0.093	0.223
Dependent variable: bachelor's degree						
Z_2: miles/10	−0.005	−0.018	−0.004	−0.001	−0.005	−0.004
	(0.002)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)
Z_4: miles/10	−0.006	−0.022	−0.017	−0.011	−0.012	−0.012
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
R^2	0.000	0.012	0.014	0.034	0.074	0.151
Dependent variable: quarterly earnings						
Z_2: miles/10	13.9	−103.2	−43.2	−25.8	−30.8	−26.6
	(29.3)	(25.7)	(28.2)	(23.5)	(20.5)	(20.1)
Z_4: miles/10	139.5	−126.3	−137.9	−97.4	−87.8	−89.4
	(21.7)	(19.6)	(20.8)	(18.2)	(15.3)	(14.4)
R^2	0.001	0.029	0.031	0.038	0.105	0.142
Local labor market controls		✓	✓	✓	✓	✓
Urbanization controls			✓	✓	✓	✓
Neighborhood quality controls				✓	✓	✓
Student demographic controls					✓	✓
Test score controls						✓
Observations	565,687	565,687	565,687	565,687	565,687	565,687

Notes: Test score percentiles are measured from 0 to 1 for comparing coefficient magnitudes to specifications with binary outcomes. Standard errors in parentheses are clustered at the high school campus by cohort level. Observations are locally weighted around the mean values of the instruments to match the exact sample as the main causal effect estimates in Section V. Academic outcomes are measured at age 28. Quarterly earnings are measured in real 2010 US dollars and averaged within person over ages 28–30. Column 2 controls for each of the 62 commuting zones in Texas and a cubic polynomial in the neighborhood-level oil and gas employment share. Column 3 adds controls for each of the 12 neighborhood-level NCES urbanization locale codes (e.g., midsize city, large suburb, fringe town, remote rural, etc.). Column 4 adds a cubic polynomial in the neighborhood quality index constructed in Section IIB. Column 5 adds categorical controls for gender, free/reduced price lunch status, race, and cohort. Column 6 adds a cubic polynomial in test score percentile.

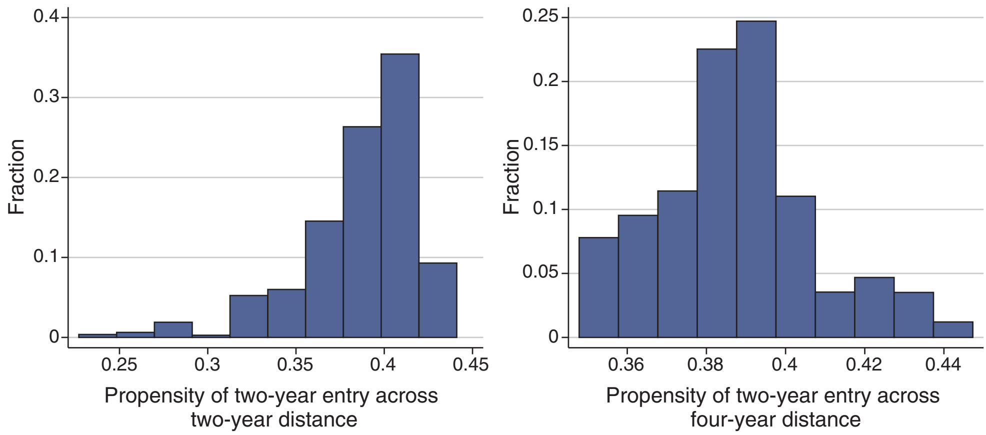

Figure 6. Conditional Distributions of Estimated Two-Year Entry Propensity Scores

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

column 5, however, does not affect the estimates as much, despite the large increases in R^2. Finally, the estimates change little (besides another large increase in R^2) when tacking on the highly predictive test score control in column 6, which corroborates the balance results in panel A.

Taken together, these diagnostics suggest that controls for local labor markets, urbanization, and neighborhood quality help purge the raw distance instruments of confounding relationships with student potential, with only a supporting role played by controls for individual student demographics. I therefore proceed with the control set defined by column 5 in Table 5, leaving the highly predictive test score measure as an excluded covariate available for describing compliers in Table 6 and validating the main results in Table 8.

The first stage estimates in Panel B of Table 5 also align with Assumption UPM. The coefficients on two-year distance, multiplied by -1, show that decreasing Z_2 while holding Z_4 fixed increases two-year entry at the expense of both four-year entry and nonenrollment.[41] Likewise, the coefficients on four-year distance show that increasing Z_4 while holding Z_2 fixed increases two-year entry and nonenrollment at the expense of four-year entry. Figure 6 further shows that the distance instruments induce meaningful variation in enrollment behavior by plotting the estimated propensity score distributions of two-year entry with respect to two-year distance (left panel) and four-year distance (right panel), evaluated at the mean values of the other distance dimension and the controls.

Finally, Table 6 empirically probes the validity of Assumption CC by describing the observable abilities of each complier type. The upper row in both columns shows

[41] The coefficients on D_0 are excluded from Table 5 to save space, but are easily inferred given $D_0 = 1 - D_2 - D_4$.

Table 6—Describing Compliers

Comparable compliers: Compliers along the *same* treatment margin, induced by *different* instruments	Mean test score percentile	Distinct compliers: Compliers along *different* treatment margins, induced by the *same* instrument	Mean test score percentile
2–4 complier with respect to Z_2	53.6 (2.7)	2–4 complier with respect to Z_2	53.6 (2.7)
2–4 complier with respect to Z_4	51.7 (2.4)	2–0 complier with respect to Z_2	32.4 (2.5)

Notes: Locally weighted observations: 565,687. Test score percentiles in this table 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.

that 2–4 diversion compliers with respect to Z_2 tend to come from the middle of the high school ability distribution, with a mean test score percentile of 53.6. The bottom row in the left column reports the mean test score percentile among 2–4 diversion compliers with respect to Z_4, which is independently identified without making any assumption about the similarity of these two groups. In fact, the two complier means are statistically indistinguishable, lending empirical credence to Assumption CC's requirement that 2–4 diversion compliers with respect to Z_2 and Z_4 have equal mean Y_2 potential outcomes. In contrast, the right column of Table 6 shows that democratization compliers along the 2–0 treatment margin tend to have much lower high school test scores than 2–4 diversion compliers, reinforcing the motivation for the separate identification approach and its accommodation of unrestricted heterogeneity across compliers along *different* treatment margins.

V. Results

A. *Main Results*

Table 7 presents the main results.[42] The first column shows the net effect of two-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, two-year college access boosts educational attainment and earnings: for the "average" complier induced into two-year entry by closer access, completed schooling increases by roughly one year, bachelor's degree attainment increases by 10 percentage points, and earnings per quarter around age 30 increase by an imprecisely estimated \$700, a 9 percent gain over the mean.

The next four columns of Table 7 decompose these net effects into the two potentially opposing channels of democratization and diversion. Roughly two-thirds (0.657) of compliers would not have otherwise attended college, and these democratized two-year entrants experience significant gains in all outcomes compared to

[42] Main estimates are evaluated at the mean values of the instruments. Section VB below explores heterogeneity in the local estimates across the range of two-year college proximity. Averaging the entire two-dimensional grid of estimates across the empirical distribution of the instruments yields average marginal treatment effects that are similar to the reported MTE estimates evaluated at the mean.

Table 7—Causal Effect Estimates of Enrolling in a Two-Year College

	MTE_2 =	ω	$MTE_{2\leftarrow 0}$	$+ (1 - \omega)$	$MTE_{2\leftarrow 4}$	$H_0: MTE_{2\leftarrow 0} = MTE_{2\leftarrow 4}$
	Net effect	Democratization share	Democratization effect	Diversion share	Diversion effect	Test of equal effects across treatment margins
Years of schooling	0.92	0.657	1.74	0.343	−0.66	$p < 0.001$
	(0.24)	(0.049)	(0.19)	(0.049)	(0.34)	
Bachelor's degree	0.104	0.657	0.265	0.343	−0.204	$p < 0.001$
	(0.048)	(0.049)	(0.036)	(0.049)	(0.082)	
Quarterly earnings	711	0.657	1,522	0.343	−844	$p = 0.044$
	(485)	(0.049)	(641)	(0.049)	(857)	

Notes: Locally weighted observations: 565,687. 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 US dollars and averaged within person over ages 28–30.

their counterfactual of not attending any college. They complete 1.7 more years of schooling, and are 27 percentage points more likely to earn a bachelor's degree. Online Appendix Table A.2 shows that very few democratized students are able to transfer to a four-year institution and complete a BA within four years of college entry, but the democratization effect rises steadily as these students are given a progressively longer observation window to transfer and eventually complete degrees. Online Appendix Table A.2 also shows that nearly all of these degrees are in non-STEM fields, with the democratization effect on completing a BA in STEM just 2 percentage points, in line with the fact that the average democratization complier is only at the thirty-second percentile of the high school test score distribution (Table 6). Moving to earnings, the average student democratized into two-year entry from nonenrollment earns about \$1,500 more per quarter around age 30. This is an 18 percent premium over the mean, implying a 10 percent average return to each of the additional 1.74 years of college induced by democratization. Considering that the net tuition price of attending a two-year college for the average student in the United States is approximately zero after grant aid,[43] this represents a healthy private return to two-year entry along the democratization margin. From a social perspective, the average cost of educating a full-time community college student is roughly \$10,000 per year of enrollment, so it takes less than two years of (undiscounted) higher earnings to recoup each year of upfront social investment.

Diverted students, on the other hand, make up the other third (0.343) of compliers, and they end up with lower average outcomes as a result of starting college at a two-year instead of a four-year institution. Diverted students complete roughly two-thirds of a year less of total education, and are 20 percentage points less likely to complete a bachelor's degree relative to their counterfactual of starting directly at a four-year institution, leading to a negative but statistically imprecise impact on earnings around age 30. Online Appendix Table A.2 shows that the magnitude of the BA effect nearly doubles when moving from a six-year observation window to a ten-year observation window, and diversion has little impact on bachelor's degree completion in STEM fields. Thus, a large fraction of the BA degrees lost to diversion

[43] Net price statistics throughout this section come from College Board (2018).

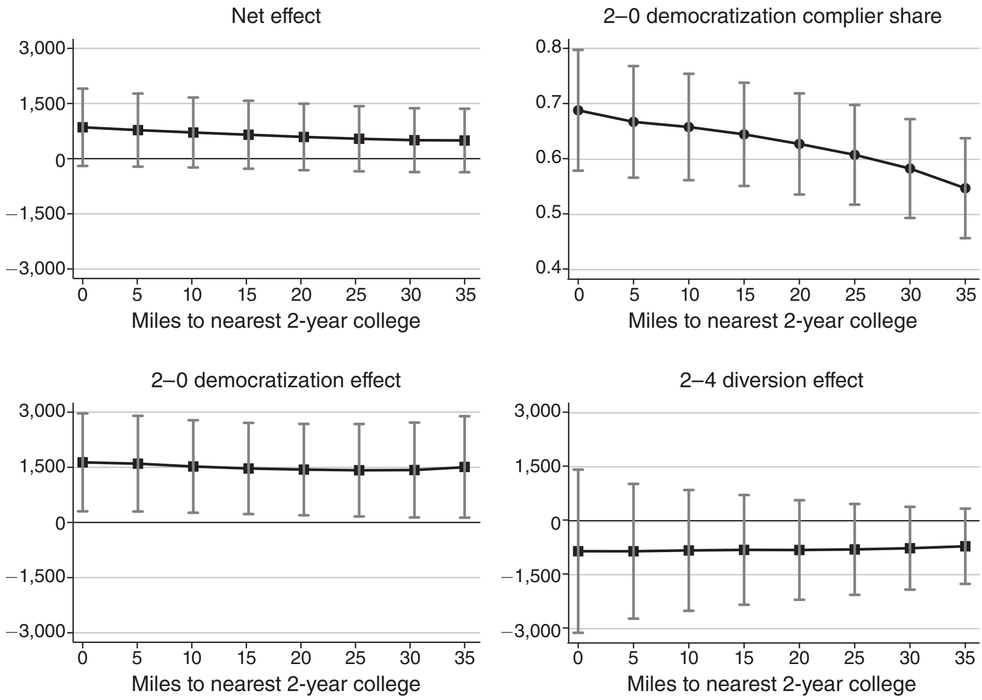

Figure 7. Selection Patterns and External Validity

Notes: Each estimate is evaluated at a given two-year distance value holding four-year distance fixed at its mean. Quarterly earnings are measured in real 2010 US dollars and averaged within person over ages 28–30. Ninety-five percent confidence intervals are estimated via block bootstrap at the high school campus by cohort level.

come from students who would have had long time to degrees even after starting directly in the four-year sector, and very few of these students would have majored in STEM.[44] Still, taking the imprecise earnings estimate at face value, a prospective student along the diversion margin comparing the average net tuition price of two-year entry (∼\$0 per year) to the average net tuition price of public four-year entry (∼\$3,500 per year) would have to severely discount the future to rationalize losing roughly \$800 in earnings every quarter in exchange for the upfront savings of ∼\$3,500 per year enrolled in a two-year instead of a four-year institution. The social calculation is quite similar, given that annual educational expenditures per student at public community colleges (∼\$10,000) and public baccalaureate institutions (∼\$13,500) also differ by about \$3,500.

B. *Selection Patterns and External Validity*

To explore selection patterns and probe the external validity of the local IV estimates, Figure 7 stratifies each component of the MTE decomposition in (15)

[44] Further exploration of the mechanisms driving diversion effects remains an important avenue for future work. See Monaghan and Attewell (2015) for a propensity score matching approach that suggests the importance of credit loss during two-year to four-year transfer, and Xu (2019) for evidence suggesting adverse impacts of two-year colleges' greater reliance on part-time adjunct faculty relative to four-year institutions.

across the available support of two-year college distance. The estimates at the mean two-year distance of roughly ten miles correspond to the main results in Table 7, while moving from high to low values of two-year distance (right to left on the horizontal axis) simulates progressive expansions of two-year college access. The MTE estimates along both the democratization margin (bottom left panel) and the diversion margin (bottom right panel) are flat across the empirical support of two-year proximity. If anything, the net effect of two-year entry (top left panel) increases slightly as two-year distance decreases over this range, driven not by changes in the margin-specific marginal treatment effects, but rather by a changing composition of marginal compliers: as two-year distance decreases from right to left, the share of compliers who are on the $2 \leftarrow 0$ democratization margin increases (top right panel). The large confidence intervals preclude a precise conclusion, but such a pattern suggests that as two-year access progressively expands, the net returns slightly increase thanks to a growing share of compliers democratized into higher education from non-enrollment rather than diverted from four-year college entry.

C. *Robustness Checks and Comparisons to Other Approaches*

Table 8 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 7 for comparison. Column 2 shows how the standard errors change when bootstrapping at the individual student level rather than clustering at the high school campus by cohort level. Column 3 adds a cubic polynomial in 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 9 compares the main estimates to those resulting from other identification approaches. Column 1 transposes the main IV estimates from Table 7. Column 2 estimates the controlled OLS specification from Section IIF 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 two-year and four-year entrants bias the OLS diversion estimates towards larger negative magnitudes. Column 3 presents results from the multivariate 2SLS specification corresponding to this setting: a 2SLS regression of outcomes on indicators for no college and four-year entry, yielding democratization versus diversion comparisons relative to the omitted two-year treatment, using the same instruments, control set, and local observation weighting as the separate IV approach in column 1. As shown in online 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 individuals, the estimands of the separate 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.

Table 8—Causal Effect Estimates: Robustness Checks

	Baseline specification (1)	No clustering (2)	Add test score control (3)	Bandwidth 35 miles (4)	Bandwidth 30 miles (5)
2–0 democratization share	0.657	0.657	0.646	0.652	0.655
	(0.049)	(0.022)	(0.046)	(0.052)	(0.054)
Years of schooling					
2–0 democratization effect	1.74	1.74	1.72	1.73	1.71
	(0.19)	(0.13)	(0.18)	(0.20)	(0.23)
2–4 diversion effect	−0.66	−0.66	−0.73	−0.72	−0.76
	(0.34)	(0.19)	(0.30)	(0.36)	(0.37)
Bachelor's degree					
2–0 democratization effect	0.265	0.265	0.263	0.262	0.264
	(0.036)	(0.024)	(0.037)	(0.039)	(0.039)
2–4 diversion effect	−0.204	−0.204	−0.219	−0.213	−0.213
	(0.082)	(0.051)	(0.073)	(0.085)	(0.090)
Quarterly earnings					
2–0 democratization effect	1,522	1,522	1,508	1,741	1,843
	(641)	(438)	(655)	(683)	(612)
2–4 diversion effect	−844	−844	−1,017	−914	−1,094
	(857)	(573)	(776)	(907)	(1,043)
Observations	565,687	565,687	565,687	554,775	540,722

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 US dollars and averaged within person over ages 28–30.

Table 9—Causal Effect Estimates: Comparisons to Other Approaches

	Baseline specification (1)	Controlled OLS (2)	Multivariate 2SLS (3)
Years of schooling			
2–0 democratization effect	1.74	1.49	2.23
	(0.19)	(0.01)	(0.21)
2–4 diversion effect	−0.66	−1.34	−1.58
	(0.34)	(0.01)	(0.16)
Bachelor's degree			
2–0 democratization effect	0.265	0.150	0.357
	(0.036)	(0.001)	(0.041)
2–4 diversion effect	−0.204	−0.357	−0.381
	(0.082)	(0.002)	(0.035)
Quarterly earnings			
2–0 democratization effect	1,522	1,072	2,518
	(641)	(18)	(621)
2–4 diversion effect	−844	−1,441	−2,755
	(857)	(24)	(511)
Observations	565,687	565,687	565,687

Notes: Column 1 reproduces the main estimates from Table 7. OLS estimates in column 2 come from the same specification as the controlled OLS regressions described in Section IIF and presented in Table 2, but now locally weighted in the same way as the main IV estimates in column 1 to ensure identical samples across the columns, i.e., with an Epanechnikov (parabolic) kernel with 40-mile bandwidth around the mean values of the instruments. Analogously, multivariate 2SLS estimates in column 3 come from the same specification as the just-identified multivariate 2SLS specification described at the end of Section IIIA and presented in column 1 of Table 3, but now locally weighted in the same way as the main IV estimates in column 1 to ensure identical samples across the columns, i.e., with an Epanechnikov (parabolic) kernel with 40-mile bandwidth around the mean values of the instruments. 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 US dollars and averaged within person over ages 28–30.

Table 10—Causal Effect Estimates: Women versus Men

	MTE_2	=	ω	$MTE_{2\leftarrow 0}$	+	$(1-\omega)$	$MTE_{2\leftarrow 4}$
	Net effect		Democratization share	Democratization effect		Diversion share	Diversion effect
Panel A. Women							
Years of schooling	1.03		0.651	2.07		0.349	−0.91
	(0.24)		(0.049)	(0.20)		(0.049)	(0.36)
Bachelor's degree	0.120		0.651	0.331		0.349	−0.274
	(0.050)		(0.049)	(0.042)		(0.049)	(0.090)
Quarterly earnings	1,517		0.651	2,921		0.349	−1,102
	(455)		(0.049)	(529)		(0.049)	(887)
Panel B. Men							
Years of schooling	0.79		0.666	1.32		0.334	−0.28
	(0.32)		(0.065)	(0.30)		(0.065)	(0.53)
Bachelor's degree	0.085		0.666	0.174		0.334	−0.092
	(0.061)		(0.065)	(0.053)		(0.065)	(0.125)
Quarterly earnings	−103		0.666	61		0.334	−431
	(903)		(0.065)	(1,239)		(0.065)	(1,422)

Notes: Locally weighted observations: 292,631 (women), 273,056 (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 US dollars and averaged within person over ages 28–30.

D. *Heterogeneity by Gender*

Table 10 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. For women, large gains in educational attainment and significant earnings returns to two-year entry accrue to 2←0 democratization compliers who otherwise would not have attended college, consistent with the large OLS literature documenting a female premium in the returns to two-year college enrollment relative to nonattendance (Belfield and Bailey 2011, 2017). Diverted women, meanwhile, experience significant losses in educational attainment and a (imprecise) decline in earnings relative to their four-year entry counterfactual.

To gauge the evolution of male and female earnings effects across the early-career life cycle, 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 two-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 two-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

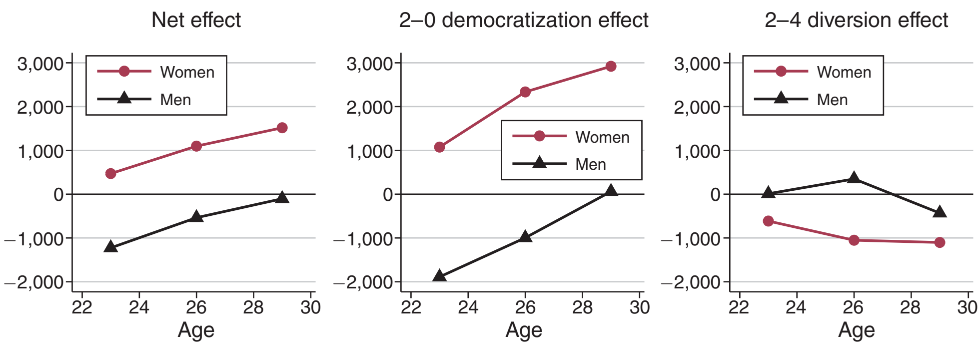

Figure 8. Earnings Effect Profiles by Gender

Notes: This figure plots marginal treatment effect estimates of two-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.

effects over at least the next decade.[45] In contrast, the rightmost panel of Figure 8 shows no strong age trend for either gender in the earnings effects along the 2←4 diversion margin.

E. *Impacts on Disadvantaged Students and Implications for Upward Mobility*

Table 11 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 two-year rather than four-year institutions and are likely the most sensitive to policies that reduce two-year entry costs. The second column of Table 11 shows that when two-year access expands, disadvantaged students are overwhelmingly on the 2←0 democratization margin: 80 percent of disadvantaged students who are induced into two-year entry thanks to closer access would not have otherwise attended any college, leaving just 20 percent who are diverted from immediate four-year entry.

The results in the third column of Table 11 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 two-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 two-year colleges

[45] 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, remains an important avenue for future work.

Table 11—Causal Effect Estimates: Disadvantaged Students

	MTE_2	$=$	ω	$MTE_{2\leftarrow 0}$	$+$	$(1-\omega)$	$MTE_{2\leftarrow 4}$
	Net effect		Democratization share	Democratization effect		Diversion share	Diversion effect
Years of schooling	0.79		0.802	1.01		0.198	−0.10
	(0.25)		(0.059)	(0.21)		(0.059)	(0.85)
Bachelor's degree	0.074		0.802	0.097		0.198	−0.018
	(0.042)		(0.059)	(0.031)		(0.059)	(0.206)
Quarterly earnings	1,396		0.802	1,832		0.198	−363
	(685)		(0.059)	(785)		(0.059)	(2,503)

Notes: Locally weighted observations: 200,140. Disadvantaged is an indicator for free or reduced price lunch eligibility in tenth 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 US dollars and averaged within person over ages 28–30.

may confer meaningful earnings benefits through other channels, and identifying these specific channels remains an important avenue for future work.

F. *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 forecast the consequences of policies that increase students' physical access to two-year colleges, like building new campuses in previously underserved areas. With additional assumptions of external validity, these estimates may also help forecast the impacts of a wider range of two-year access policies, like tuition subsidies or targeted outreach programs. I use the IV estimates to conduct two policy simulations as illustrations of such forecasts. First, I remain within the college proximity setting and explore the implications of marginally decreasing two-year college distance on earnings gaps by gender and by family income. Second, under the additional assumption of external validity of the distance-based estimates, I quantify how the magnitudes, and even signs, of net effects across different two-year access policies depend crucially on what fraction of new two-year entrants are diverted from four-year college entry.

To carry out the first simulation, consider a decrease in two-year college distance resulting from building a new local two-year campus. How does this affect local earnings gaps between men and women, and between students from low-income and higher-income backgrounds?[46] The expected reduced form effect on quarterly earnings among women is a net increase of \$7.66 per mile reduction in two-year distance, while men experience roughly no change in earnings (an insignificant increase of \$0.37); hence, expanding access to two-year colleges by decreasing distance serves to reduce the overall gender earnings gap, with larger reductions expected for larger decreases in distance. The larger reduced form effect for women is driven by both a

[46] For ease of exposition, all estimates used in the policy simulations in this section are evaluated from the starting point of mean two-year and four-year distances, corresponding to the results in previous sections.

larger first stage effect (two-year entry is more responsive to two-year distance for women than men) and a larger MTE (marginal women induced into two-year entry earn a bigger return than marginal men). Similarly, reducing distance to two-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 $6.64 per mile reduction in two-year distance, while higher-income students gain only about $2 per mile reduced. Like the gender gap, this reduction in the family income gap is driven both by a larger first stage effect for low-income students and a larger marginal treatment effect.

To illustrate the second type of policy simulation, note that the net effects of other policies that expand two-year college access, beyond proximity changes, are also likely comprised of a share-weighted average of the democratization effect among 2⟵0 compliers (new two-year entrants who otherwise would not have enrolled in college) and the diversion effect among 2⟵4 compliers (new two-year entrants who otherwise would have started at four-year institutions). Assuming that the main democratization and diversion treatment effect estimates from Table 7 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 from initial policy-induced changes in enrollment shares without waiting years to observe long-run outcomes.[47]

Figure 9 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 7, 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 7, since all 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 percent diversion share corresponding to marginal reductions in two-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 7. Another policy example is the Tennessee Promise Program, which (as discussed in the introduction) began offering free two-year college tuition to recent Tennessee high school graduates in 2014 and has since spurred a huge number of similar programs across the United States. According to annual enrollment numbers from the Tennessee Higher Education Commission (2018), the post-policy decrease in four-year enrollment among recent high school graduates divided by the increase in two-year enrollment suggests a diversion share around 30 percent, which applied to Figure 9 would forecast similar long-run net effects as the distance variation used in this paper.

Importantly, moving further rightward on the x-axes of Figure 9 suggests the net effects of two-year access policies begin to turn negative as the diversion share

[47] Specifically, as discussed in Section IIIA, the diversion share is identified by the policy-induced reduction in four-year enrollment as a fraction of the policy-induced increase in two-year enrollment.

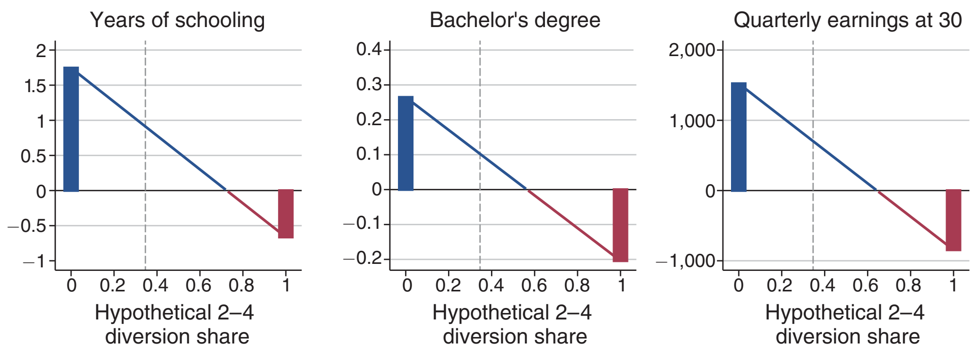

FIGURE 9. NET EFFECTS OF HYPOTHETICAL POLICIES

Notes: This figure plots the net effects of hypothetical two-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 7 are externally valid. A diversion share of zero yields a hypothetical net effect equal to the democratization effect from Table 7; a diversion share of one yields a net effect equal to the diversion effect from Table 7; 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 percent diversion share corresponding to marginal reductions in two-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 7.

reaches roughly 70 percent, 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. More generally, however, Figure 9 offers a useful framework for thinking quantitatively about the long-run impacts of community college access policies, highlighting the trade-off between democratizing new students into higher education and diverting college-bound students from direct four-year entry.

VI. Conclusion

Policymakers often look to two-year community colleges as policy levers for extending higher education to a broader share of Americans. This paper has empirically explored the consequences of expanding access to two-year colleges among students of traditional college-going age, highlighting the trade-off between attracting new students into higher education and diverting those already bound for college away from immediate four-year enrollment.

Decomposing the net impacts of two-year college access into effects along these two distinct enrollment margins presents a methodological challenge, since standard IV 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 two-year and four-year college proximities net of controls for local labor markets, neighborhood urbanization, and neighborhood quality. 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 two-year versus four-year proximity has empirical support through equal mean test scores across these two complier groups.

The empirical results indicate that broadly expanding access to two-year colleges does boost educational attainment and earnings on net, but decomposing these net effects reveals substantial heterogeneity along several dimensions: students diverted from four-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 two-year entry with little offsetting diversion. Taken together, these results suggest that broad expansions of two-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 equal or greater net impacts with fewer unintended consequences.

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