NBER WORKING PAPER SERIES

ATTRITION FROM ADMINISTRATIVE DATA:
PROBLEMS AND SOLUTIONS WITH AN APPLICATION TO POSTSECONDARY EDUCATION

Andrew Foote
Kevin M. Stange

Working Paper 30232
http://www.nber.org/papers/w30232

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
July 2022

Any opinions and conclusions expressed herein are those of the authors and do not represent the views of the U.S. Census Bureau or the National Bureau of Economic Research. All results have been approved for disclosure with DRB Requests #DRB-B0007-CED-20181029, #DRB-B0033-CED-20190318, #CBDRB-FY20-051, #CBDRB-FY22-211, #CBDRB-FY22-CED006-0022. We thank numerous colleagues and seminar participants for helpful discussions and comments. The authors have no financial support or interests to disclose.

Attrition from Administrative Data: Problems and Solutions with an Application to Postsecondary Education
Andrew Foote and Kevin M. Stange
NBER Working Paper No. 30232
July 2022
JEL No. I26,J31,J61

ABSTRACT

This paper examines the bias arising from individuals' migration from administrative outcome data, with a focus on the labor market consequences of postsecondary education. We find that out-of-state migration is particularly problematic for high-earners, flagship graduates, and certain majors. Consequently, the effect of graduating from a flagship university is 10% higher than one would estimate using in-state earnings exclusively, though the extent of bias differs substantially across contexts. The impact of obtaining a 2-year CTE credential is also understated, as are earnings differences across majors. Approaches to testing for and bounding this bias are considered.

Andrew Foote
U.S. Census Bureau
4600 Silverhill Road
Suitland, MD 20746
andrew.foote@census.gov

Kevin M. Stange
Gerald R. Ford School of Public Policy
University of Michigan
5236 Weill Hall
735 South State Street
Ann Arbor, MI 48109
and NBER
kstange@umich.edu

I. Introduction

There has been a big shift from the use of survey data to administrative data in social science generally (Penner & Dodge, 2019) and in economics and education in particular (Chetty 2012; Figlio et al 2017). Relative to survey data, administrative data has several benefits including larger samples, lower cost, less measurement error and more extensive longitudinal follow-up (Card, Chetty, Feldstein, Saez, 2010; Figlio et al 2017). An important aspect of this longitudinal follow-up is that administrative data generally have much higher response rates and lower attrition than survey data since inclusion is generally not an active decision that sample participants must make. Prior work has found that survey non-response and attrition can generate considerable bias (Lillard, Smith and Welch, 1986; Bollinger, Hirsch, Hokayem, Ziliak, 2019), as can sample selection more generally (Lee, 2009). Many view the administrative data revolution as eliminating, or at least mitigating, much of this non-response and attrition bias.

However, much of the administrative data used comes from administrative units at a subnational level, such as cities, school districts, counties, and states. Researchers have used such sources to study educational outcomes, criminal justice outcomes, health care utilization, earnings, and participation in social insurance programs such as SNAP or unemployment insurance (UI). The use of such subnational administrative data can introduce bias if study participants are mobile across jurisdictions.[1] For instance, researchers can usually not distinguish whether someone absent from state administrative earnings data is truly not working or is working in another state. The same problem potentially arises even when using national administrative data (e.g. tax records); many OECD countries have double-digit emigration rates for high-skilled workers. (OECD, 2015)

In this paper we examine the bias arising when using administrative data to measure program outcomes in the presence of attrition. We illustrate the problem with the case of estimating the labor market consequences of college choices in the US, a growing literature that has made extensive use of state-level administrative earnings data collected to administer unemployment insurance. The key challenge is that earnings are not observed if participants move out of state

[1] Merging administrative data across domains can also potentially introduce bias from cases that cannot be uniquely merged for various reasons, such as name changes, misspellings, or lack of unique identifiers. This source of bias is also potentially present when using national administrative data sources. We ignore this issue in this paper.

or are self-employed, so researchers are unable to distinguish non-employment from interstate mobility or self-employment. This could be problematic if migration is affected by the treatment under study, which is likely given that migration differs with college attainment (Malamud and Wozniak, 2012), with financial aid receipt (Fitzpatrick and Jones, 2016), and across majors (Ransom, 2016). Conzelmann et al (2022a) find that more than 30% of recent college graduates are living and working in a different state than where they graduated, with this rate exceeding 50% for the most selective 4-year institutions. Our analysis is made possible by a special link between education records from many four-year and two-year institutions in five states and the U.S. Census Longitudinal Employer-Household Dynamics (LEHD) data, which combines UI earnings records from all states and the District of Columbia. Information on self-employment income from Non-Employer Business Register is also linked. Thus we can validate analyses that use in-state (subnational) UI earnings records with those using national records. This novel data allows us to answer four questions: (1) How significant is out-of-state migration for recent college graduates? (2) Does migration differ across the earnings distribution? (3) Does it impact estimates of earnings effects of college selectivity, major, and attainment? (4) In what settings should researchers worry about it and how should they address it?

Prior work has either focused on workers with in-state earnings (Hoekstra, 2010; Andrew, Li, Lovenheim, 2016; Andrews and Stange, 2019; Altonji and Zimmerman 2018) or set non-matched workers as having zero earnings, often in conjunction with a bounding exercise (Denning, Marx, Turner, 2019). These researchers have not been able to directly test the validity of these approaches. Our work is most directly related to two studies that assess migration-related bias from using UI administrative data. Scott-Clayton and Wen (2017) use the NLSY97 to demonstrate how estimates of the earnings effect of college attainment are affected when using only earnings records for students that remain in state. They find that out-of-state migration tends to attenuate the earnings premium of a college degree. When constructing institution-specific earnings outcomes contained in the College Scorecard, Council of Economic Advisors (2017) compare estimates derived from the full universe of IRS tax records with those using in-state employee records only, to approximate the restrictions of state UI earnings records. They find that migration bias overstates the average earnings of graduates from low-earning colleges, understates that from high-earning colleges, and is larger when out-migration rates are higher. Our paper complements these by examining a much larger administrative data sample than the NLSY and by using the state administrative data that most researchers and states have access to (in contrast to IRS tax data). We also begin to assess several tests and corrections that researchers have proposed to address the problem.

Our approach is also in the spirit of work that validates survey with administrative data. Bollinger Hirsch Hokayem and Ziliak (2019) validate Current Population Survey (CPS) earnings variables using Social Security administrative records, given the large non-response in the former. Barnow and Greenberg (2015) compare various social experiments using both survey and administrative earnings data. Britton, Shephard, and Vignoles (2018) compare labor market outcomes in the UK Labour Force Survey to administrative records, finding substantial differences between the two. The differences result in different conclusions about important labor market phenomena, including the gender wage gap, the returns to education, and the extent of earnings inequality. We also contribute to the broader literature proposing solutions to various forms of selection bias (Lee, 2008), attrition bias (Grogger, 2013), and non-response bias (Behaghel, Crepon, Gurgand, LeBarbanchon (2015; 2009).

We find that migration out of state is considerable, approaching 30% even among graduates attached to the labor force. Furthermore, it is not ignorable, as mobility is higher for students at the higher end of the earnings distribution, for certain majors, and for certain institutions. Monte Carlo analysis suggests that a key factor is the relative treatment effect of the flagship institution on out-of-state vs. in-state earnings. Migration-related bias occurs if attending a selective institution or graduating with a STEM major increases out-of-state earnings more than in-state earnings. Surprisingly, this is true even if migration is exogenous. Bias is zero when treatment has a similar effect on in-state and out-of-state earnings, even if migration is endogenous.

We illustrate the practical consequences of this migration-induced bias using three applications. First, we show that graduates of the most selective four-year schools in a state who are working earn $12,970 (0.17 log points) more per year than non-flagship graduates, though in-state earnings UI records alone understate this premium by 10% (0.02 log points). The inclusion of modest controls for selection into a flagship does not mitigate this problem. The extent of bias differs quite a bit by local context – states with substantial outmigration differences between their most selective colleges and less selective colleges experience greater levels of bias. Next, we examine differences in earnings across college majors. In-state earnings mischaracterize the earnings premium associated with different majors due to differential out-of-state migration. For instance, the earnings premium of Engineering relative to Psychology is understated by 0.11 log points in the first year after graduation. Finally, we find that the earnings gain associated with obtaining a degree in Career Technical Education (CTE) at a public two-year college is 0.01 log point higher than in-state earnings would suggest. Encouragingly, self-employment – which is not captured in state administrative datasets –does not appear to be a substantial source of bias in any of our applications

We offer four practical lessons for researchers using such data. First, bias is reduced when the sample is conditioned on having positive observed earnings. Doing so changes the target of estimation to a parameter that does not fully capture the consequences of treatment (and could be subject to standard Heckman-like selection), but this drawback may be the lesser of two evils compared to erroneously assuming that movers are not working. Second, a sensible test of the potential for bias is whether the rate of in-state earnings being observed differs between treatment and control groups. In both our flagship and college major analyses, bias is larger in settings with greater differential rates of observed in-state earnings. However, this test is not definitive of the presence of bias. We also show in our setting that similar levels of missingness can produce quite different levels of earnings bias. Our Monte Carlo analysis shows that even having migration unrelated to treatment is not sufficient to rule out bias. Nor is a relationship between moving and actual earnings necessarily evidence of bias. The key factor is whether migration is correlated with earnings differentially with the treatment under study. This is inherently not testable, though supplemental data might be suggestive if available. Third, the Lee (2007) bounding approach is likely inappropriate in this setting given the failure of the monotonicity assumption and bounds are wide and uninformative. Finally, self-employment is not a major source of differential attrition or bias in the three applications we examined. Analysts should focus on differential migration as the most problematic confounder.

Our analysis also has two implications for states. First, states do not retain many of their highest-paid workers, which is a goal of many state merit-aid programs. Second, earnings estimates published by state higher education boards and made available to students will understate earnings differences between programs (institutions and fields) due to systematic differences in rates of out-migration. Published earnings records for smaller states with high rates of out-migration will be particularly misleading.[2] States also need to pay attention to the timing of outcome measurement; longer-term earnings outcomes are more pertinent to welfare and are pushed in use of performance measures by states because they are more stable (Miniya and Scott Clayton, 2018). The extent of attrition bias may differ over different time horizons as well.

This paper proceeds as follows. In the next section, we provide a selective review of recent work that uses administrative records to estimate treatment effects. We focus on earnings outcomes used to measure the effect of postsecondary choices and treatments. Section III describes our data and samples. Section IV presents descriptive evidence on whether attrition from state administrative data is ignorable. In Section V we test for bias in three applications that commonly use state administrative earnings records: college selectivity, college major, and CTE credential attainment. In Section VI we discuss the tests and bounding approaches used in the literature and evaluate the performance of common bounding techniques. In Section VII we briefly describe Monte Carlo simulations of a simple model that illuminates the conditions that give rise to biased estimates of treatment effects, permitting us to speak to settings more general than our specific empirical examples. Finally, section VIII concludes.

II. The Use of Sub-National Administrative Earnings Data

Administrative earnings data has been used extensively by researchers to study the effects of various choices and treatments in higher education.[3] Such data has permitted researchers to estimate the labor market effects of college quality (Hoekstra, 2010; Andrews, Li, Lovenheim; 2016; Minaya and Scott-Clayton, 2018; Cunha and Miller, 2014), college attendance (Zimmerman, 2014; Turner, 2014; Ost, Pan, Webber, 2018); degrees (Jepsen, Troske, Coomes, 2014; Engborn and Moser, 2017), financial aid (Denning, Marx, Turner, 2019; Carlson et. al, 2020) and major or program of study (Bakkes, Holzer, Valez, 2015; Stevens, Kurlaender, Grosz, 2019; Altonji and Zimmerman, 2019; Andrews and Stange, 2019). Outside the US, researchers have exploited institutional features to credibly estimate the earnings effects of field and program

[2] One caveat is that states themselves may be particularly interested in the earnings of graduates that remain in state, since this has important tax revenue implications. Earnings of graduates who leave the state may be less relevant.
[3] Appendix Table A1 lists recent examples. We focus on work related to higher education, but examples in other contexts are numerous. Recent studies of high school curriculum (Brunner, Dougherty, Ross, 2021), displaced workers (Lachowska, Mas, & Woodbury, 2018), housing demolitions (Chyn, 2018), incarceration, (Mueller-Smith, 2018), and foster care (Doyle, 2013) all use administrative data from one state to measure outcomes.

(Hastings, Neilson, Zimmerman, 2013; Kirkeboen, Leuven, Mogstad, 2016; Belfield et al, 2018; Böckerman, Haapanen, Jepsen, 2019). Furthermore, many U.S. states have begun publishing interactive tools that allow students to see the consequences of college and major choices, matching postsecondary records to in-state earnings records.[4]

All these studies may be subject to bias due to migration or attrition into non-covered employment.[5] The overall five-year cross-state migration rate in the U.S. is approximately 9%, though this is likely higher for young college graduates. Furthermore, there is quite a bit of variation across states, suggesting that the potential for bias likely differs across states. Outside the US, rates of emigration from OECD countries is high, particularly for high-skilled workers.[6]

Authors in these papers have taken several approaches to address the potential sample selection problem. Most studies focus on workers with in-state earnings (Andrew, Li, Lovenheim, 2016; Hoekstra, 2010; Altonji and Zimmerman 2018; Andrews and Stange, 2019), dropping workers with no in-state earnings over some time frame, which assumes that dropped workers are similar to non-dropped workers. Other papers retain non-matched workers, setting their earnings to zero, often in conjunction with a bounding exercise (Denning, Marx, Turner, 2018). Many studies test whether treatment is correlated with having matched outcome data, interpreting no effect as evidence of minimal bias. We will see that this test does not necessarily rule out bias. Few researchers have explicitly examined whether treatment is related to the probability of being in-state or directly looked at inter-state migration using other sources. One exception is Andrew, Li, and Lovenheim (2016), who compare the earnings distribution of recent college graduates that are living in Texas vs. out of Texas among those who lived in the same college town five years earlier from the 2000 Census. While suggestive of minimal bias, this test is not conclusive and not possible for many treatments under study.[7]

[4] The Post-Secondary Employment Outcomes experimental data published by the U.S. Census Bureau provides similar estimates, using the national jobs data: https://lehd.ces.census.gov/data/pseo_experimental.html
[5] Differential rates of matching across different administrative databases, such as education and labor market records, due to a lack of unique identifiers or differential inclusion in a third database used to crosswalk records (e.g. Brunner, Daugherty, Ross, 2021) is another potential source of bias we do not explore in this paper.
[6] See Table A1 for out-migration rates by state and emigration rates by country.
[7] In the presence of swapping, this test may be biased toward not finding any differential effect, and is therefore underpowered. Furthermore, the geographic location of many flagship campuses are not separately identified in the public use versions of the Census or ACS.

III. Data Sources

We examine this issue using new data linkages at the U.S. Census Bureau between postsecondary transcript records and a national database of employment and earnings.[8] Our analysis includes enrollment and graduation data for students from all two- and four-year public colleges in Texas, Colorado, New York (CUNY and SUNY), and Ohio, and all four-year institutions in the Pennsylvania State University system.[9] With the exception of Colorado, these states have lower out-migration rates than the average U.S. state, according to ACS migration tables. These data includes degree field, graduation date, degree level, and data on subsequent enrollments. A current limitation of the data is that it contains very few baseline demographic variables. To complement the administrative education records, we also use administrative data from Census to maintain consistent demographic data for our sample.

Student records are matched to the Longitudinal Employer-Household Dynamics (LEHD) data. LEHD data reports quarterly earnings by job (employer-employee match) for all employment covered by unemployment insurance, including those on paid leave. These data do not include the self-employed (independent contractors and unincorporated self-employed), railroad workers covered by the railroad unemployment insurance system, and some smaller categories of workers (some family employees, certain farm workers, etc). Most state and local government employment is included. These data span 2000-2016 for 50 states and the District of Columbia. The LEHD data cover approximately 96% of all private sector employment, though the overall coverage of all employment (including self-employment, all public sector, etc) is lower (Abowd et al., 2009). We supplement the LEHD with information on self-employment (an indicator for any self-employment and total annual income from self-employment) from 1099 filings from the IRS.[10] We ignore the incomplete LEHD coverage and assume that any individuals not matched to the LEHD or self-employment have zero earnings nationally (and in-state). Importantly, this data allow us to measure earnings for graduates that leave the state and work in self-employment,

[8] These data linkages are part of a larger project, which has included the creation of the experimental data product Post-Secondary Employment Outcomes. See https://lehd.ces.census.gov/data/pseo_beta.html for a description of the project as well as the tabulations that have already been released. Technical documentation is available at https://lehd.ces.census.gov/doc/PSEOTechnicalDocumentation.pdf

[9] These data include both in-state resident and out-of-state students.

[10] Self-employment income is available from 2001-2016, but there is a slight change in how it is reported in 2007. Prior to 2007 we are not able to distinguish who is earning the self-employment if multiple people in household earn 1099 wages, though this is not a large share. After 2007 we attribute self-employment to individuals.

which is the main contribution of our paper. For each graduate, we calculate national (including self-employment), national (excluding self-employment) and in-state annual earnings separately for each year since graduation, in order to measure the bias of only measuring in-state earnings. Importantly, our national and in-state earnings measures come from the same source (with in-state a subset of the national), so any difference can be attributed to differences in coverage, not variable definition. All earnings amounts are converted to real 2018 dollars using the CPI-U.

Our four-year sample includes 145 four-year institutions, which collectively span a wide range of institutional size, selectivity, and resources. Our first application compares students graduating from the most selective institutions vs. other public four-year institutions. We use the term "flagship" as shorthand for the most selective institutions.[11] Our four-year analysis sample comprises a 10 percent sample of students who graduated from one of these 145 campuses from 2001 to 2013, though in some analyses we restrict to graduates from 2006 and earlier so that we can have a balanced panel of individuals when looking at earnings outcomes over different time horizons. Each observation is a person-year, beginning with the first full calendar year after graduation and going up to 15 years post-graduation for our earliest cohort. Our full analysis sample includes nearly half a million earnings observations for 52,000 graduates from most selective colleges and 1.4 million observations for 168,000 graduates from other institutions.

Our second sample is a 10% sample of students at public two-year colleges in Colorado, Ohio, New York, and Texas who enrolled from 2001 to 2016 who are age 18-65, with annual earnings from 2000-2016. We only keep graduates from CTE programs and, as a comparison group, enrollees who have 10 or more credits and have listed that they are in a CTE field.[12] We observe students both before and after enrollment.

[11] The institutional characteristics of these groups is reported in Appendix Table A2. Most selective (flagship) schools are defined as those with a mean SAT math score of at least 595, which includes two institutions in Colorado (University of Colorado Boulder, Colorado School of Mines), three in New York (SUNY Geneseo, Binghamton, Stony Brook), two in Ohio (Ohio State University - Columbus, Miami University), one in Pennsylvania (Penn State University Park in State College), and three in Texas (UT Austin, UT Dallas, and Texas A&M). We collectively refer to these institutions as "Flagship" institutions for convenience, though we recognize this label is typically used more narrowly.

[12] We include the CTE fields studied in Stevens Kurleander and Grosz (2019): communication tech (CIP code 10), computer sciences (11), culinary (12), education (13), engineering tech (15), family/consumer sciences (19), homeland security/law enforcement (43), public admin (44), construction trades (46), mechanic tech (47), precision production (48), transportation (49), health (51), and business (52).

Table 1 presents summary statistics for our four-year (Panel A) and two-year (Panel B) samples. Looking at total national (true) earnings and pooling all years since graduation, the earnings advantage of students graduating from a flagship 4-year college is apparent: they earn $7,350 (17%) more in annual earnings than non-flagship graduates. However, erroneously treating migrants as having zero earnings by only looking at in-state earnings, flagship graduates appear to earn $3,550 *less* than non-flagship graduates. These differences arise because rates of non-employment and out-of-state migration differ between flagship and non-flagship graduates, as shown in the final rows. Flagship graduates are about 12 percentage points more likely to have moved and worked out-of-state than non-flagship graduates right after college, with the gap increasing with time since degree. Interestingly, flagship graduates are actually slightly *less* likely to have any positive earnings nationally. Rates of self-employment are slightly higher for flagship grads than non-grads.

Panel B divides our two-year sample into person-year observations that occur before vs. after enrollment (years of enrollment are included in our full sample, but excluded from this tabulation for clarity). Average total quarterly earnings are $7,910 (69%) higher after enrollment than before, but this difference is only $7,320 when measuring earnings only with in-state sources. Rates of migration are just slightly lower post-enrollment than before, though rates of true non-employment are much lower. These summary statistics suggest differences in rates of and causes of attrition between the two- and four-year sectors, which have implications for potential bias.

IV. Is Attrition Ignorable?

To set up our regression analysis, we first establish several facts about the migration of college graduates using graphical evidence. Collectively, these patterns suggest that missing earnings data could affect empirical estimates of several types of postsecondary treatment effects.

First, graduates (from all institutions) leave the state at appreciable rates. **Figure 1** shows the share of graduates that have any national UI earnings, only in-state UI earnings, and self-employment income by type of degree and time since graduation. The gap between true national employment rates and those measured by in-state records is large and increases over time for both Bachelors and Associates graduates. The share of graduates with some self-employment income is low, but is modestly higher for Bachelors graduates, though very few individuals have only self-employment income. This previews our conclusion that self-employment income is not a big source of bias in the applications we explore.

To examine whether this migration is ignorable, we restrict the sample to graduates who have at least three quarters of non-zero earnings and with at least $10,000 of earnings in the calendar year nationally (in-state or out-of-state). These restrictions are intended to capture, in an imperfect way, people that have reasonable attachment to the labor market earning at least the minimum wage.

A second finding is that those that leave the state have measurably different earnings than those that stay. If the stayers have similar outcomes as the movers, then migration is ignorable. In **Figure 2** we plot the share in-state by individuals' location on the national earnings distribution among graduates from our institutions, separately for 1, 5 and 10 years after graduation.[13] For Bachelors graduates, migration has a "U-shape" relationship with earnings: low and particularly high earners are more likely to move out-of-state than middle-earners. Migration is clearly related to earnings, though in a non-linear way. While Figure 1 shows that the share of graduates that stay in-state is steadily falling, Figure 2 shows that the downward shift in the share in-state is not constant across the earnings distribution. Instead, those that leave the state are much more likely to be in the higher earnings percentiles, and that is especially true 10 years after graduation. Graduates at the 50th percentile of the national earnings distribution were about 10 percentage points less likely to be in state after ten years; graduates at the 90th percentile were over 15 percentage points less likely. Out-migration rates are approaching 50% after ten years for the highest earners.[14] Associates degree holders have a lower out-migration rate, though it too is much higher for higher-earning graduates.

Figures 1 and 2 taken together illustrate an important issue in studying long-term earnings outcomes for students when restricted to one state. In-state earnings are a better proxy for total

[13] We only include institutions from Colorado and the University of Texas System in this analysis. We should note that Texas has a relatively low out-migration rate of young workers (6.7% vs. 8.7% for the U.S. overall, as reported in Table 1), suggesting the problem we illustrate may be even more pronounced in other states, while Colorado has relatively high rates of out-migration.

[14] The pattern is similar if one fixes income for individuals by using "lifetime earnings" on the x-axis, constructed as the sum of all national earnings in the first 10 years. This addresses the possibility that high earnings may be associated with mobility by construction (those who leave are high earners due to cost of living differences, for example). This figure shows that mobility is higher for higher earners, but it is particularly higher at the tail of the distribution. This result is available from the authors.

national earnings in years immediately following graduation. However, as Minaya and Scott-Clayton (2016) argue, early earnings years are very noisy and are unlikely to accurately measure the true effect of a postsecondary treatment, such as attending a specific college. If instead researchers focus on later earnings years for graduates who stay in-state, they could capture a biased estimate of the treatment effect because they do not measure the effect for mobile workers.

Figure 3 shows in-state share by sex and residency classification for one and ten years post-graduation. While mobility is similar between male and female students in year 1 for the bottom half of the earnings distribution, males are much more mobile at the top of the distribution. By 10 years after graduation, there is attrition at the top of the distribution for both genders, but it is more pronounced for males. Estimates of degree returns by sex could be affected by these patterns of differential attrition. Many studies focus (implicitly or explicitly) on students who were previously in-state residents. The bottom panels of Figure 3 show that while rates of mobility are lower for in-state resident students than out-of-state students, both groups experience higher rates of mobility among high earners. Additionally, at 10 years after graduation, the mobility is *more* differential at the top of the earnings distribution for in-state students, while out-of-state students move at high rates at every point in the earnings distribution. Several postsecondary treatments (specific institutions and fields of study) display similar patterns of differential mobility across the earnings distribution[15]

To summarize, it is clear that the earnings outcomes of graduates leaving the state are measurably different on a number of dimensions, creating conditions for migration-related bias when estimating postsecondary treatment effects using in-state administrative data. Self-employment income – particularly without some covered earnings – is rarer and thus unlikely to be a major source of bias.

V. Applications

A. College Selectivity

[15] Figure A1 reports in-state retention rates over time and across the earnings distribution for three illustrative institutions and five majors.

The graphical analysis above illustrated three ingredients for biased estimates of the effect of flagship graduation on earnings: (1) substantial out-of-state migration; (2) migration patterns that differ by position on the earnings distribution; and (3) a differential earnings-migration link across institutions. Furthermore, these patterns all differed with time-since-degree, suggesting that the extent of bias could differ over different time horizons. We now evaluate how these factors contribute to bias estimates of the effect of graduating from a flagship university. Our regression analysis uses the complete sample from all five states, summarized in Table 1. We pool across all years since college graduation.

We estimate simple OLS regression models of observed earnings on a dummy for having graduated from a flagship university:

$$Y_{it} = \beta_0 + \beta_1 Flagship_i + \beta_2 X_{it} + \varepsilon_{it} \tag{1}$$

Since we are combining many years and cohorts, we include a full set of calendar year and graduation year dummies to control for any lifecycle and cross-cohort earnings trends that may happen to correlate with flagship enrollment. We also include state fixed effects to account for the fact that earnings and flagship enrollment may differ between states. We have limited demographic controls, but we also include sex and race dummies. Standard errors are clustered at the individual (person) level.

Our data permits us to estimate models using true earnings from all sources nationally and identical models using outcomes constructed from the in-state data typically available to researchers. Non-matched records are set to zero and are included or excluded depending on the specification. Our empirical construct of bias is simply the difference between these two estimates.[16] We should note that due to the limited number of control variables and a lack of plausibly exogenous variation in flagship attendance, we do not interpret our estimates as the causal effect of flagship enrollment. However, we use the terminology of "effect" to be consistent with the treatment effect literature. The migration bias problem we describe is not mitigated by having exogenous variation in treatment.[17] However, since different identification

[16] This difference also includes estimation error, which we ignore given our sample size and precision.
[17] In results not reported, the extent of bias does not change when we do or do not include demographic controls, despite their inclusion reducing the estimated flagship premium by several percentage points. We show in Appendix B with Monte Carlo simulations that random assignment of treatment does not mitigate attrition bias.

strategies may estimate treatment effects for different local populations (with different rates of differential migration), the extent of bias could differ across methods.

Table 2 presents our results. Panel A presents the most naïve estimates: earnings differences in levels setting any non-matched records as zeros. As was apparent from the summary statistics, doing so causes large negative bias. Relying on in-state records only would lead a researcher to conclude that flagship graduates earn $1,741 more than non-flagship graduates whereas they actually earn $9,985 more per year. The omission of self-employment income explains a small part of this difference (about 4% of the $8,244 bias). The resulting bias is substantial: any effect of flagship graduation on migration instead appears as a large reduction in earnings (towards zero).

Recognizing the potential for this bias, most scholars have instead focused on individuals with some attachment to the labor market, as indicated by having non-zero earnings.[18] Sometimes this is done implicitly by using log(earnings) as the outcome. Panel B restricts to annual observations with non-zero earnings either in-state (column 1) or nationally (columns 2 and 3).[19] Doing so greatly reduces the bias because any effect of treatment on out-migration rates is no longer recorded as a large reduction in earnings. Nonetheless, this approach does not eliminate the bias. In-state earnings records will understate the true flagship effect by $1,090 per year; the true flagship effect is 9% higher than the in-state earnings records would suggest. This finding is similar if the log of annual earnings is used as the outcome, as is commonly done. In-state earnings would suggest a 0.147 log point premium to flagship graduation, whereas the true effect is 0.021 log points (14%) higher. Andrews, Li, and Lovenheim (2014) find that UT-Austin graduation increases earnings more at the high end of the distribution, concluding that “this university is particularly lucrative for top earners.” The last three rows present quantile regression estimates of the effect of flagship graduation on various moments of the earnings distribution. We too find that the flagship earnings premium increases across the earnings distribution, from 0.10 log points at the 25th percentile up to 0.21 at the 75th, using in-state

[18] Some researchers have restricted it to quarters with non-zero earnings whereas others have restricted it to years with several quarters of non-zero earnings or earnings greater than some minimal threshold.
[19] We should note that imposing this restriction creates the well-known sample selection problem (Heckman, 1974): earnings outcomes for those observed to be working will be different than those choosing not to work. We abstract from this issue, treating the self-selected national earnings outcome as our target for estimation.

earnings. However, the magnitude of the bias differs only slightly across the distribution, from 0.019 log points at the 25th percentile up to 0.021 log points at the 75th.[20]

We also estimated effects separately for each combination of state, graduating cohort, and year since graduation with log earnings as the outcome variable (excluding zeros) and full controls. **Figure 4** reports our estimates of the flagship effect by state and time since graduation, using both in-state earnings and national earnings. The estimated effect is larger in all cases when using national earnings rather than in-state earnings only. Panel A of **Table 3** reports the estimated bias for each state separately by year post-graduation. It shows that the overall bias is driven by Colorado (bias of 0.061 log points), Pennsylvania (0.11) and Ohio (0.036) while Texas and New York have minimal bias. Panel B reports the difference in rate of having no in-state earnings between flagship and other institutions by state and time-since-graduation. Colorado and Pennsylvania have much larger differences in this rate than the other states. Importantly, our analysis seems to confirm that the test by Andrews, Li and Lovenheim (2016) was informative, in that there was no differential migration across the earnings distribution for the flagship institutions in Texas. However, many papers cite their test as confirmation that there is likely no bias due to migration more generally (not just for flagship institutions in Texas), which we have shown is not the case.

B. College Major

Bias may also plague estimates of outcome differences across major fields, given the differential migration across fields (Ransom, 2016). Cross-major differences in bias are important given that several states are now publishing program-level labor market outcomes using in-state earnings data. We illustrate the bias of in-state earnings outcomes by major by estimating major-specific fixed-effects in two sets of regressions:

$$Y_{it}^{in} = \beta_0 + \gamma_f^{in} + \beta_1 X_{it} + \varepsilon_{it} \quad (2)$$

$$Y_{it}^{nat} = \beta_0 + \gamma_f^{nat} + \beta_1 X_{it} + \varepsilon_{it} \quad (3)$$

[20] The proportionate size of the bias is actually decreasing across the earnings distribution, from 19% at the 25th percentile to 10% at the 75th because the base premium is lower at lower percentiles.

Where equation (2) includes all graduates with positive in-state earnings, and equation (3) includes all graduates with positive national earnings. The key coefficients of this regression are γ_f^{in} and γ_f^{nat}, which are the major fixed effects for in-state and national earnings regressions, and measure the field-specific returns. We omit psychology, as it is one of the largest fields, and also relatively generic, so all the coefficients and bias are in reference to the earnings of a psychology degree. We estimate these models separately by state, time since graduation, and graduating cohort.

Figure 5 plots the difference between the national and in-state major fixed effects for each major, combining all years, states, and cohorts. The extent of bias ranges from an underestimate of 0.05 log points for Communication/Journalism and Engineering to essentially zero for Public Administration. There are also large differences across majors in the extent to which graduates have in-state earnings. While there are some exceptions, many majors with positive bias are more likely to be missing in-state earnings records due to movement out-of-state.

Table 4 reports the bias separately for 1, 5, and 10 years after graduation, combining estimates across states and cohorts. Engineering returns are understated when using in-state earnings by 0.05 log points relative to the return for Psychology. Additionally, these biases are larger compared to Psychology in the first year, when few Psychology majors move out of the state. The bias is smaller after 10 years, likely because many Psychology majors leave the state, particularly at the high end of the earnings distribution (Appendix Figure A1). A similar pattern is seen for Business, which sees a reduction in the bias with time since graduation as psychology majors come to match the mobility profile of business students. In contrast, the bias for Physical Science is increasing up to year ten. The implication is that researchers estimating earnings differences across programs will need to confront the likely differential migration between majors which may under- or over-state earnings differences across fields and to an extent that changes with the time horizon.

C. Career and Technical Education at 2-year Institutions

Our final application examines the labor market outcomes associated with participation in career and technical education (CTE) programs at community college. Most prior work on this topic has taken advantage of the fact that many community college students have pre-enrollment work experience and earnings that allow one to control for non-random selection (Bahr, et al 2015; Stevens, Kurlaender, and Grosz, 2019; Carruthers and Sanford 2018).[21] We follow this approach by comparing CTE completers to non-completers after conditioning on individual fixed effects and individual-specific time trends. This approach estimates the return to program completion relative to individuals' own prior earnings pattern and relative to patterns experienced by individuals in the same programs who did not complete. Following Stevens, Kurlaender, and Grosz (2019), we estimate:

$$\mathrm{Y_{it}} = \alpha_i + \delta_i t + \gamma_t + \beta Degree_{it} + \pi Enrolled_{it} + \sum_{j=18}^{65} \theta_j 1(Age = j)_{it} + \varepsilon_{it}$$

We include individual fixed effects (α_i) and individual-specific linear time trends ($\delta_i t$), which control for unobserved factors correlated with completion and earnings that are fixed for individuals and or that change at a constant rate. We also include age (non-linearly), year effects, and an intercept shift for periods of enrollment. The main coefficient of interest is β, which captures the change in earnings or log earnings after degree receipt relative to individuals' predicted earnings. We include non-completers to help identify the counterfactual age-earnings profile as well as the year effects and enrollment effects.

Table 5 presents estimates of β separately for short certificates, long certificates, and Associates degrees. Again we estimate models using three outcome variables: in-state wage earnings, national wage earnings, and national wage earnings plus self-employment income. We restrict the sample to observations where positive earnings are observed (i.e. excluding zero earning observations). Like prior literature, we find large earnings increases associated with long certificate or AA degree completion (0.22 and 0.20 log points, respectively) with short certificates associated with lower earnings gains. However just using in-state earnings records can provide misleading estimates if differential attrition is present among 2-year graduates. Results indicate that bias is less pronounced in the 2-year context than when estimating four-year flagship and college major effects. In-state earnings will understate the earnings return to an Associate degree by about 0.01 log point, or $154. Bias for the effect of long certificates is larger, though still less than we saw for four-year institutions. Self-employment income does not

[21] Much of this work is reviewed in Carruthers & Jepsen (2022).

meaningfully differ with degree completion so its inclusion has minimal bearing on earnings estimates. It is worth noting that our CTE analysis necessarily conditions on having pre-enrollment earnings, which likely isolates a group that is particularly attached to the state in which their institution is located. Migration – and thus bias – may be more problematic for an approach that does not impose this condition.

VI. Testing for and Addressing Attrition Bias

A. Testing for and Predicting Bias

Prior researchers have used several empirical tests for the presence of attrition bias and also brought in supplemental data. Here we describe these approaches and evaluate the ability of these approaches to distinguish settings with minimal from large bias.

1. *Is treatment associated with having non-missing earnings?*

The most common test is whether treatment is associated with the likelihood of having non-zero in-state earnings. While intuitive, this test does not provide a definitive test for bias. Bias can occur if groups possess similar rates of having in-state earnings; the treatment and control groups may simply be experiencing differential (but equally-sized) selection. Zimmerman (2014) tests whether treatment is associated with "In LF sample" and finds a 2 pp decrease in likelihood of positive earnings, interpreted as small. However, this difference grows to an 8 pp reduction in sample inclusion associated with admission when inflated by the first stage of their regression specification. Denning, Marx, and Turner (2019) test whether treatment affects the probability of having either in-state earnings or enrollment, finding a small positive association (< 1 pp for early years, > 1 pp for year seven). These estimates would be larger if appropriately scaled by the first stage. Having an automatic zero EFC and more financial aid (treatment) makes students more likely to be observed in-state. Ost, Pan, and Webber (2018), finding no association between treatment and attrition, nicely sum up a limitation of Lee (2007) bounds to this scenario: "given that there is no evidence of differential attrition to begin with, it is no surprise that our results are robust to [the Lee (2007)] bounding exercise." Finally, Altonji and Zimmerman (2019) reports attrition difference by field of study (relative to education, baseline 12.8% missing) ranging from -1 to +25 percentage points.

We estimated flagship and major earnings premia in settings with quite different out-of-state migration rates, permitting us to correlate the extent of bias with the treatment-control difference in likelihood of having in-state earnings. **Figure 6** plots the bias of our flagship earnings effect against the difference in the likelihood of having in-state earnings between flagship and non-flagship graduates. Each point represents a separate estimate for a cell defined by state, years since graduation, and graduating cohort. Three features are noteworthy. First, the extent of bias is increasing with the difference in rates of in-state earnings. Settings with a greater difference in the likelihood of in-state earnings tend to have more bias, as most analysts have intuited. Second, much of the variation is across states. This suggests that state context is important. Finally, there is substantial variation on both dimensions. Settings with minimal bias may differ considerably in the extent of in-state earnings and even settings with no differences in likelihood of in-state earnings may have bias. For instance, New York includes some cells with no difference in out-migration between flagship and non-flagship graduates, but some bias in the estimated flagship effect.

We quantify this relationship in **Table 6** by regressing cell-level bias on the difference in likelihood of having in-state earnings. Panel A shows that bias in the cell-specific flagship effect is 0.03 log points greater for every 10 percentage point difference in the rate of in-state earnings between flagship and other graduates. Most of this relationship is driven by cross-state correlations in these two variables, as was also apparent from Figure 6. Panel B repeats this using our estimates of bias for major-specific earnings premia (relative to psychology). A 10 percentage point difference in the likelihood of having in-state earnings between majors is associated with a 0.007 to 0.01 log point difference in the bias. Almost all of this relationship is explained by cross-major differences in the likelihood having in-state earnings, which suggests analysts should be worried about identifying high-migration majors generally, not just in their specific context.

2. *Balance Tests for Full vs. Restricted Samples*

Some authors demonstrated the balance of covariates between treatment and control groups for the selected sample with non-zero earnings. Ost, Pan, and Webber (2018) and Zimmerman (2014) perform such a test, finding that covariates are still balanced in their RD setting. Andrews, Li, and Lovenhiem (2016) present means of covariates for treatment and control groups separately for those included and excluded in the analysis due to lack of earnings observations. While they do not present formal tests, there does not appear to be any differential attrition between treatment and control groups based on these covariates. However, neither of these rules out the possibility of differential attrition due to unobserved factors, most importantly latent earnings offers.[22]

3. *Supplemental data: How much migration is there? Is it associated with treatment or outcomes?*

While researchers rarely have access to migration data specifically pertaining to their sample, supplemental data such as the American Community Survey (ACS), the Current Population Survey (CPS), or other migration sources (e.g. Conzelmann et al, 2022) can be informative. Using the ACS, Ost, Pan, and Webber (2018) estimate that non-earnings among individuals with at least some college is half attributable to leaving the state in the last year (56%), a third due to true non-employment (32%), and the remainder due to self-employment (8%) and federal government employment (4%). From the IPUMS-CPS, Denning, Marx, and Turner (2018) estimate an annual interstate migration rate of 3.2 percent for young adults with some college from Texas between 2010 and 2016. Compounding these annual rates over a decade could result in substantial migration from the state, though this data is not able to determine differential rates between treatment and control groups nor how this correlates with earnings potential. In our application, the state with the greatest difference in out-migration between its flagship and other institutions, Pennsylvania at 28 percentage points, has the greatest amount of bias while New York (6 percentage point difference), has the least.[23] This suggests that the amount of bias could, to some extent, be predicted by differential migration, measured by the American Community Survey (ACS), by treatment status.

Andrews, Li, and Lovenheim (2016) provide the best illustration of this approach. In the 2000 Census, they identify recent college graduates who lived in Texas five years earlier (when they

[22] We lack rich pre-college characteristics, but present such a test using demographic characteristics in Appendix Table A3. We examine whether the extent of covariate balance between flagship and non-flagship graduates differs between the full and in-state earnings samples in Texas and Colorado (other states were not available for this analysis). Though treatment is not balanced on covariates (as expected, given our lack of quasi-experimental variation), the extent of balance does not appear to differ between the full and restricted samples. This suggests that passing this test does not rule out economically meaningful bias, possibly because such tests are low power.
[23] See Appendix Table A2.

were aged 17-21). They use living in the Austin or College Station metropolitan statistical areas (MSAs) (vs. rest of Texas) five years earlier as proxies for having graduated from the flagships UT-Austin and Texas A&M, respectively, which correspond to their treatments of interest. They then document the log earnings distribution for these workers separately by Texas MSA and whether the workers are in or out of Texas. For all three MSA groups (Austin, College Station, rest of Texas), the in- and out-of-state earnings distributions are similar, suggesting that higher earners are not more likely to move out-of-state, whether from a flagship or not. While quite encouraging, any error in the measurement of treatment status will tend to attenuate differences. Furthermore, this approach is simply not available for other treatments. For example, Boulder, CO is not separately identifiable in the Census.

B. Is Bounding Appropriate? How Does It Perform?

Lee (2009) proposed a bounding approach to estimate treatment effects in the presence of sample attrition. He developed the approach to estimate treatment effects on wage rates (rather than total earnings) in the presence of non-random employment: wages are only available for people who work, so conditioning on working introduces sample selection bias. The idea is to exclude individuals from the group that experiences less attrition so that treatment and control groups are comparable on the remaining distribution. Subsequent work has applied the approach to more general settings where sample attrition is correlated with treatment. This would seem a natural approach to dealing with attrition in our setting, as treatment (college selectivity, major, degree completion) is correlated with the likelihood of observing non-zero earnings, because treatment affects both employment and migration. The key assumption to this approach is monotonicity: treatment must only affect attrition in one direction. In Lee's case, the treatment effect on the employment probability is assumed to have the same sign for all individuals. This assumption rules out that treatment may increase employment for some individuals, while reducing it for others. In our case, there are good reasons to think that the monotonicity assumption would be violated since individuals can attrit on two margins: non-employment and out-of-state migration. Monotonicity would be violated if treatment increased employment for some individuals and increased out-of-state migration for others, which seems likely.

Nonetheless, **Table 7** implements this bounding approach for our flagship estimates. Note that the resulting coefficients are not directly comparable to our results in Table 2 as they do not include any controls, including for state, graduating cohort, or year, though they are qualitatively similar. Given the large difference in match rates between flagship and non-flagships (combining both migration and non-employment), this procedure trims a large share of the sample. As a consequence, the trimming produces uninformative wide bounds, ranging from -0.38 to + 0.37 log points combining all time periods. The range still includes zero ten years out.

Grogger (2013) proposes an alternative bounding approach which uses runs of zeros at the end of the sample period to construct bounds. Chyn (2018) applies this approach in his evaluation of housing demolition in Chicago and finds no evidence that his treatment is related to attrition from administrative data. This approach holds promise, but again is likely to result in very wide bounds given the high rates of migration out-of-state observed among college graduates.

VII. Monte Carlo Evidence

To examine the extent of bias under a more general set of conditions than our empirical example, we develop and simulate a simple model of earnings, work, and migration in the presence of a treatment. The full model is described in Appendix B. Here we summarize the four main lessons from this simulation. First, the ratio of the treatment effect on out-of-state to in-state earnings is a key determinant of bias. Bias is zero when treatment has a similar effect on in-state and out-of-state earnings, as this is what induces differential migration by earnings in the treatment and control groups. Second, there can be bias even if migration is completely exogenous. Again, differential treatment effects for in-state and out-of-state earnings means that the observed earnings distribution of the treatment group will be truncated more than the control group even if migration is exogenous. Third, bias is reduced when the sample is conditioned on having positive observed earnings. This drops both movers and in-state non-participants, so it does change the target of estimation to a parameter that does not fully capture the consequences of the treatment under study. This conclusion is borne out in our empirical applications. Finally, a test for the presence of bias is whether the relationship between migration and earnings differs between the treatment and control groups. While this test can be difficult to implement directly in practice, it does speak to the value of supplement data, as was done in Andrews, Li, and Lovenheim (2016). Interestingly, having migration unrelated to treatment is not sufficient to rule out bias. Nor is a relationship between moving and actual earnings necessarily evidence of bias.

VIII. Conclusion and Lessons for Researchers

Many analysts seek to estimate the effect of some policy or treatment on short- and long-term outcomes using administrative data, which can offer large samples at lower cost and with less measurement error or attrition than survey sources. Accurate estimates of the magnitudes of such treatment effects are critical inputs in cost-benefit and welfare analysis (e.g. Hendren & Sprung-Keyser, 2020). However, most of those papers use administrative earnings records from a single jurisdiction, whether a U.S. state or a country, and are thus restricted to measure earnings outcomes using individuals that remain and work in-jurisdiction. While most authors have recognized the potential for bias, prior work has not had access to data that would permit them to directly test the extent of bias. This study takes advantage of a unique match between postsecondary records from five states with administrative records nationally, permitting us to quantify the extent of bias due to out-of-state migration among U.S. college graduates.

In three different applications that commonly use such data, we find bias from the exclusive use of in-state earnings records. We conclude that the flagship effect is actually 10% higher than suggested by in-state administrative earnings records, at least in the five states we study. Migration bias also confounds estimates of earnings differences across majors and the extent that earning an Associate's degree or Long Certificate increases earnings. Importantly, the extent of bias differs across contexts, due to the large differences in out-migration across states and majors.

Simulations show that this bias can arise *even if migration itself is random*, as long as the distribution of earnings is different for treated and non-treated individuals. We also evaluate the performance of various strategies (e.g. Lee, 2008) commonly used to deal with attrition bias, though such bounding exercises are uninformative in this setting. As the use of administrative data continues to proliferate, a better understanding of the bias resulting from inter-jurisdiction migration and how to address it will be valuable.

We offer a few lessons for researchers who do not have access to national administrative data. First, conditioning an analysis sample on having positive earnings greatly reduces the extent of bias. This is something that most researchers have done instinctively, but our analysis demonstrates the value of doing so. Second, researchers should test for differential rates of matching with earnings records between treatment and control groups. While this test is not definitive, our estimates imply that settings with larger differences in the presence of earnings are more likely to suffer from attrition bias. Researchers examining flagship or major effects specifically could use our point estimates to approximate the extent of bias. Supplemental data that specifically examines migration differences by treatment status and some measure of earnings potential could be particularly informative. Third, bounding exercises are likely to be uninformative given the substantial level of attrition and may be inappropriate if treatment effects both migration and likelihood of any employment. Fourth, it appears unlikely that the exclusion of self-employment earnings will be a substantial source of bias. Analysts should worry primarily about migration.

Our analysis also has two implications for states. First, states disproportionately lose many of their highest-paid university graduates, which is a goal of many state merit-aid programs. An inability to follow students that leave the state is a big barrier to evaluating the ability of such programs to retain talent. Second, earnings estimates published by state higher education boards and made available to students will understate earnings differences between programs (institutions and fields) due to systematic differences in rates of out-migration. Published earnings records for smaller states with high rates of out-migration will be particularly misleading.

References

Abowd, J. M., Stephens, B. E., Vilhuber, L., Andersson, F., McKinney, K. L., Roemer, M., & Woodcock, S. (2009). The LEHD infrastructure files and the creation of the Quarterly Workforce Indicators. In *Producer dynamics: New evidence from micro data* (pp. 149-230). University of Chicago Press.

Altonji, J. and S. Zimmerman 2019. The Costs of and Net Returns to College Major. In C. Hoxby and K. Stange eds., *Productivity in Higher Education*, Chicago, Illinois: University of Chicago Press.

Anelli, M, 2018. The Labor Market Determinants of the Payoffs to University Field of Study. Unpublished working paper. February 15, 2018.

Andrews, R, J. Li, and M. Lovenheim, 2016. “Quantile Treatment Effects of College Quality on Earnings. *Journal of Human Resources*, 51(1): 201-238.

Andrews, Rodney J., and Kevin M. Stange. 2019. "Price Regulation, Price Discrimination, and Equality of Opportunity in Higher Education: Evidence from Texas." *American Economic Journal: Economic Policy*, 11 (4): 31-65.

Artmann, Elisabeth, Nadine Ketel, Hessel Oosterbeek, Bas van der Klaauw, 2018. Field of Study and Family Outcomes. IZA DP No. 11658

Bahr, P. R., Dynarski, S., Jacob, B., Kreisman, D., Sosa, A. Wiederspan, M, 2015. Labor market returns to community college awards: Evidence from Michigan. CAPSEE working paper

Backes, B. , Holzer, H. J. , & Velez, E. D. 2015. Is it worth it? Postsecondary education and labor market outcomes for the disadvantaged. *IZA Journal of Labor Policy*, 4 (1).

Barnow, Burt and David Greenberg, 2015. "Do Estimated Impacts on Earnings Depend on the Source of the Data Used to Measure Them? Evidence From Previous Social Experiments." *Evaluation Review*. Vol. 39(2) 179-228.

Behaghel, Luc, Bruno Crépon, Marc Gurgand, and Thomas Le Barbanchon, 2015. Please Call Again: Correcting Non-Response Bias in Treatment Effect Models. *The Review of Economics and Statistics*, December 2015, 97(5): 1070–1080

Behaghel, L., B. Crépon, M. Gurgand, and T. Le Barbanchon, 2012. "Please Call Again: Correcting Non-Response Bias in Treatment Effect Models," IZA discussion paper 6751.

Belfield, C, J. Britton, F. Buscha, L. Dearden, M. Dickson, L. Van der Erve, L. Sibieta, A. Vignoles, I. Walker, and Y. Zhu, 2018. "The relative labour market returns to different degrees." Institute for Fiscal Studies Research Report, June 2018.

Bollinger, C., B. Hirsch, C. Hokayem, J. Ziliak, 2019. Trouble in the Tails? What We Know about Earnings Nonresponse Thirty Years after Lillard, Smith, and Welch. *Journal of Political Economy*. 127 (5): October 2019.

Böckerman, Petri, Mika Haapanen, Christopher Jepsen, 2019. "Back to school: Labor-market returns to higher vocational schooling. *Labour Economics*. 61(2019).

Britton, Jack, N. Shephard, and A. Vignoles, 2018. "A comparison of sample survey measures of English graduates with administrative data." *Journal of the Royal Statistical Society*, A (2019) 182 (Part 3): 1-30.

Brunner, Eric, Shaun Dougherty, and Stephen Ross, 2021. "The Effects of Career and Technical Education: Evidence from the Connecticut Technical High School System." *NBER WP No. 28790*.

Canaan, Serena, and Pierre Mouganie. 2018. Returns to Education Quality for Low-Skilled Students: Evidence from a Discontinuity. *Journal of Labor Economics*. 36(2).

Card, D., R. Chetty, M. Feldstein, and E. Saez, 2010. Expanding Access to Administrative Data for Research in the United States. *National Science Foundation White Paper* 10-069 Sept 2010.

Carlson, D., A. Schmidt, S. Souders, B. Wolfe. 2020. The Effects of Need-Based Financial Aid on Employment and Earnings: Experimental Evidence from the Fund for Wisconsin Scholars. NBER WP No. 27125. May 2020

Carruthers, Celeste K., and Christopher Jepsen. (2022). "Vocational Education: An International Perspective," in *The Routledge Handbook of the Economics of Education,* edited by Brian McCall, London, UK: Routledge. CESifo Working Paper 2020-8718.

Carruthers, C. and T. Sanford, 2018. Way station or launching pad? Unpacking the returns to adult technical education. *Journal of Public Economics*. 165: 146-159

Chetty, Raj. 2012. "Time Trends in the Use of Administrative Data for Empirical Research" presentation at NBER Summer Institute, July 2012.

Chyn, Eric. 2018. "Moved to Opportunity: The Long-Run Effects of Public Housing Demolition on Children." *American Economic Review* 2018, 108(10): 3028–3056

Conzelmann, Johnathan, Hemelt, Steven, Hershbein, Brad, Martin, Shawn, Simon, Andrew, and Stange, Kevin. 2022a. Grads on the Go: Measuring College-Specific Labor Markets for Graduates. NBER Working Paper 30088.

Conzelmann, Johnathan, Hemelt, Steven, Hershbein, Brad, Martin, Shawn, Simon, Andrew, and Stange, Kevin. 2022b) Grads on the Go: Measuring College-Specific Labor Markets for Graduates [US, 2010-2018]. Ann Arbor, MI: Inter-university Consortium for Political and Social Research [distributor], 2022-05-18. https://doi.org/10.3886/E170381V3

Council of Economic Advisors, Executive Office of the President of the United States, 2017. "Using Federal Data to Measure and Improve the Performance of U.S. Institutions of Higher Education." Updated January 2017.

Cunha, J. M. and T. Miller, 2014. Measuring value-added in higher education: possibilities and limitations in the use of administrative data. *Economics of Education Review* 42, 64–77.

Denning, Jeffrey T., Benjamin M. Marx, and Lesley J. Turner. 2019. "ProPelled: The Effects of Grants on Graduation, Earnings, and Welfare." *American Economic Journal: Applied Economics*, 11 (3): 193-224.

Doyle, Joseph. 2013. "Causal effects of foster care: An instrumental-variables approach" *Children and Youth Services Review* 35 (2013) 1143–1151

Dyke, A. , Heinrich, C. J. , Mueser, P. R. , Troske, K. R. , & Jeon, K.-S. 2006. The effects of welfare-to-work program activities on labor market outcomes. *Journal of Labor Economics,* 24 (3), 567–607

Engbom N. and C. Moser, 2017. Returns to Education Through Access to Higher-Paying Firms: Evidence from US Matched Employer-Employee Data. Unpublished paper April 8, 2017.

Figlio, David, Krzysztof Karbownik, and K. Salvenes, 2017. "The Promise of Administrative Data in Education Research" *Education Finance and Policy*, Presidential Essay. 2017.

Fitzpatrick, Maria D. and A. Damon Jones. 2016. "Higher Education, Merit-Based Scholarships and PostBaccalaureate Migration. *Economics of Education Review*. 54: 155-172.

Franklin, Rachel S. 2013. "Domestic Migration Across Regions, Divisions, and States: 1995 to 2000" Census 2000 Special Reports.

Grogger, Jeffrey. 2013 "Bounding the Effects of Social Experiments: Accounting for Attrition in Administrative Data." *Evaluation Review* 36(6) 449-474.

Hastings, J.S., C.A. Neilson, and S.D. Zimmerman, 2013. "Are Some Degrees Worth More Than Others? Evidence from College Admissions Cutoffs in Chile," NBER WP 19241

Hendren, Nathaniel & Ben Sprung-Keyser, 2020. "A Unified Welfare Analysis of Government Policies*," *The Quarterly Journal of Economics*, vol 135(3), pages 1209-1318.1.

Hoekstra, Mark. 2009. "The Effect of Attending the State Flagship University on Earnings: A Discontinuity- Based Approach." *Review of Economics and Statistics* 91(4):717–24.

Jepsen, Christopher, Kenneth Troske, and Paul Coomes. 2014. "The Labor- Market Returns to Community College Degrees, Diplomas, and Certificates." *Journal of Labor Economics* 32(1):95–121.

Kirkebøen, Lars, Edwin Leuven, and Magne Mogstad, 2016. "Field of Study, Earnings, and Self-Selection," *The Quarterly Journal of Economics*, 2016, p. qjw019.

Lachowska, Marta, Alexandre Mas, Stephen A. Woodbury, 2018. "Sources of Displaced Workers' Long-Term Earnings Losses" NBER WP No. 24217 Issued January 2018.

Lee, D., 2009. "Training, Wages, and Sample Selection: Estimating Sharp Bounds on Treatment Effects," *Review of Economic Studies* 76: 1071–1102.

Lillard, Lee, James P. Smith, and Finis Welch. 1986. "What Do We Really Know about Wages? The Importance of Nonreporting and Census Imputation," *Journal of Political Economy* 94 (June 1986): 489-506.

Liu, V. Y. T., Belfield, C. R. , & Trimble, M. J. 2015. The medium-term labor market returns to community college awards: Evidence from North Carolina. *Economics of Education Review*, 44, 42–55 .

Malamud, O. and A. Wozniak, 2012. The Impact of College on Migration Evidence from the Vietnam Generation. *Journal of Human Resources.* 47(4): 913-950.

Minaya, V. and Judith Scott-Clayton, 2018. Labor Market Outcomes and Postsecondary Accountability: Are Imperfect Metrics Better than None? In C. Hoxby and K. Stange eds., *Productivity in Higher Education*, forthcoming, Chicago, Illinois: University of Chicago Press.

Mueller-Smith, Michael, 2018. "The Criminal and Labor Market Impacts of Incarceration." *Working paper. University of Michigan.*

Organization for Economic Cooperation and Development (OECD), 2015. *Connecting with Emigrants - A Global Profile of Diasporas 2015*. Table 4.2 total emigration rates and emigration rates of the highly skilled, by country of origin, 2010/2011.

Ost, Ben, Weixiang Pan, and Douglas Webber. 2018. The Returns to College Persistence for Marginal Students: Regression Discontinuity Evidence from University Dismissal Policies. *Journal of Labor Economics.* 36 (3): 779-805.

Penner, A. M. & K.A. Dodge, 2019. "Using Administrative Data for Social Science and Policy" *RSF: The Russell Sage Foundation Journal of the Social Sciences* 5(3): 103–27. DOI: 10.7758/RSF.2019.5.3.06.

Ransom, T., 2016. "Selective Migration, Occupational Choice, and the Wage Returns to College Majors." Unpublished working paper.

Scott-Clayton, J. and Q. Wen, 2017. "Estimating Returns to College Attainment: Comparing Survey and State Administrative Data Based Estimates." CAPSEE WP. January 2017.

Stevens, A., M. Kurleander, and M. Groz, 2019. Career Technical Education and Labor Market Outcomes: Evidence from California Community Colleges. *Journal of Human Resources*. 54(4): 986-1036.

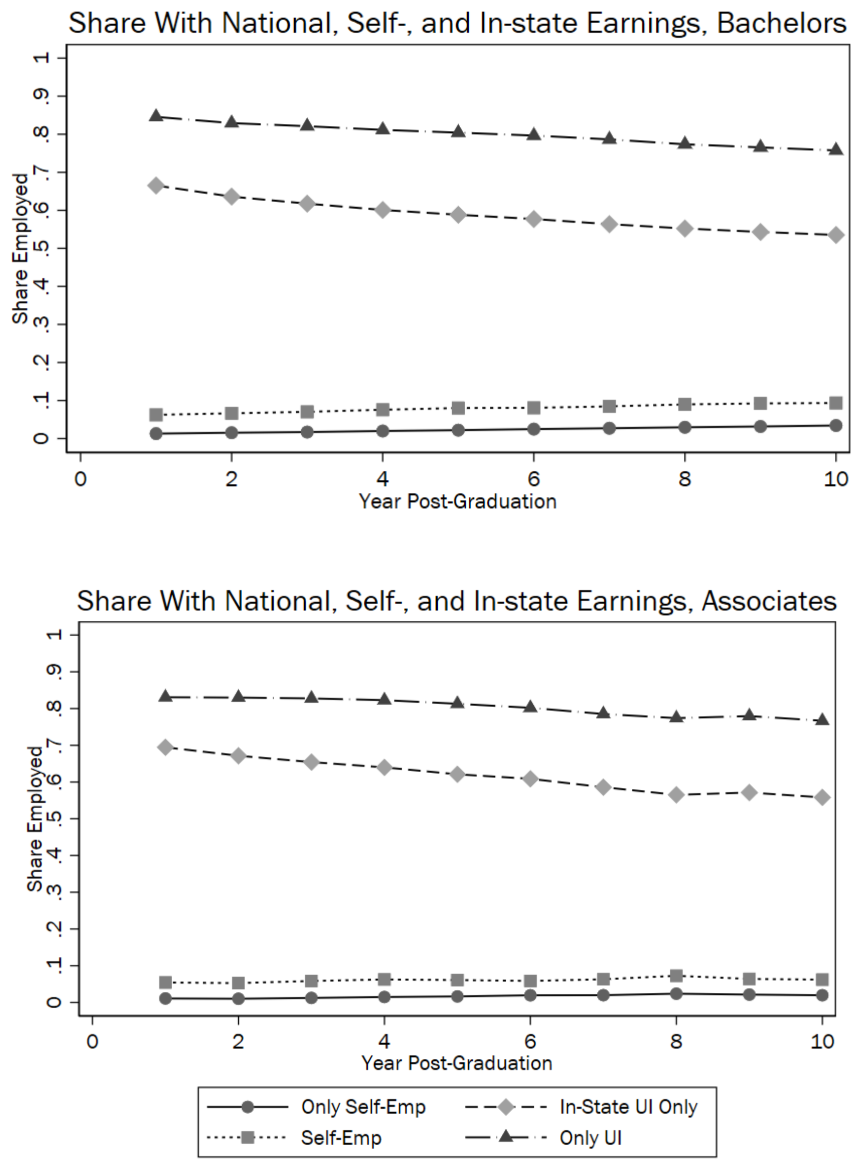
Turner, L. 2015. The returns to higher education for marginal students: Evidence from Colorado Welfare recipients. *Economics of Education Review* 51 (2016) 169–184

Walker, I. and Zhu, Y. 2011. Differences by degree: evidence of the net financial rates of return to undergraduate study for England and Wales. *Economics of Education Review*, 30, 1177–1186.

Walker, I. and Y. Zhu, 2017. University selectivity and the graduate wage premium: evidence from the UK. IZA Discussion Paper 10536.

Zimmerman, Seth. 2014. "The Returns to College Admissions for Academically Marginal Students." *Journal of Labor Economics* 32(4):711–54.

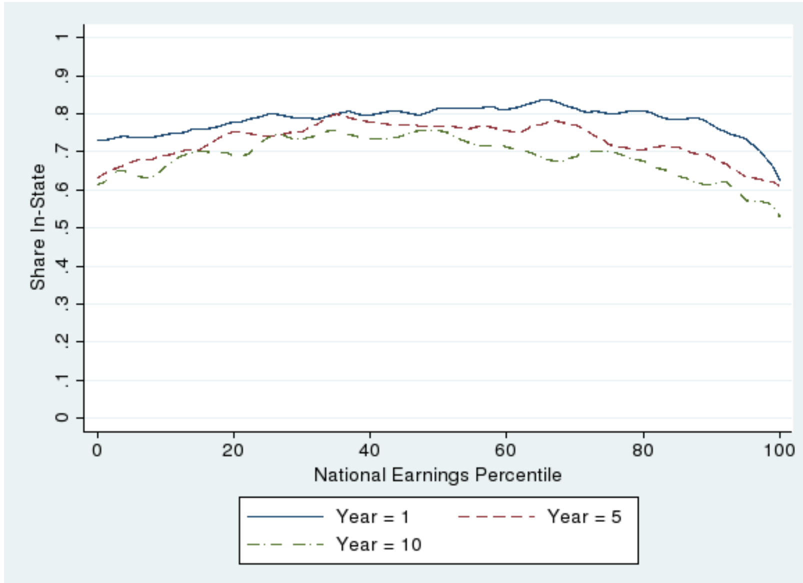
Figure 1: Share of Graduates with National, Self-Employment, and In-State Earnings



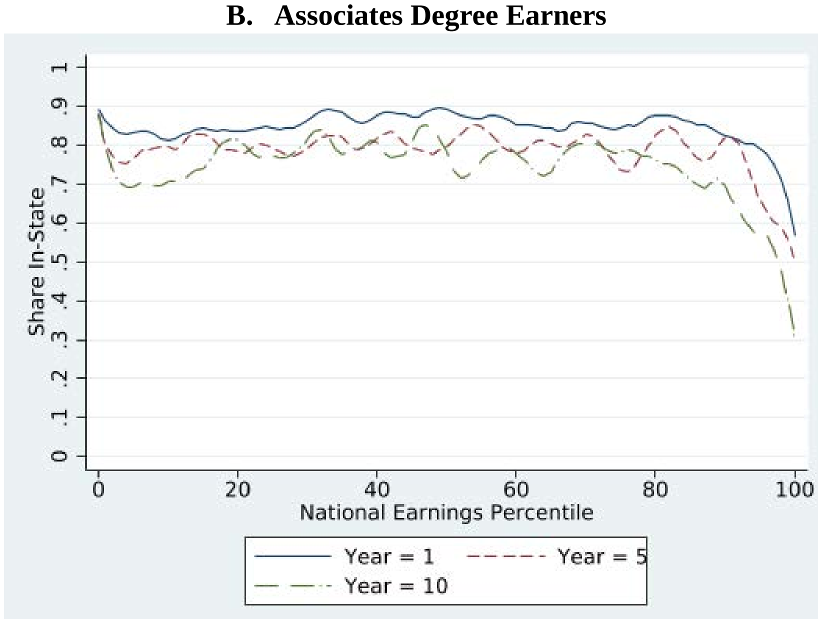
Note: Author's calculations using merged data from five state college systems, earnings data from the LEHD and self-employment income from the Internal Revenue Service. "Only UI" is graduates that have national earnings records that are covered in the state UI system and included in the LEHD. "In-State UI Only" is the subset of "Only UI" graduates that have earnings records only in the state from which they graduated college. "Self-Emp" is the share of graduates that have any self-employment and "Only Self-Emp" is the subset of those whose income comes exclusively from self-employment.

Figure 2: Share In-State, by Percentile in National Earnings Distribution and Year Post-Grad

A. Bachelors Degree Earners



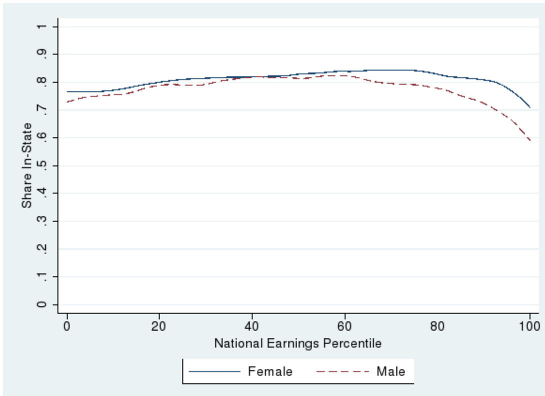
B. Associates Degree Earners



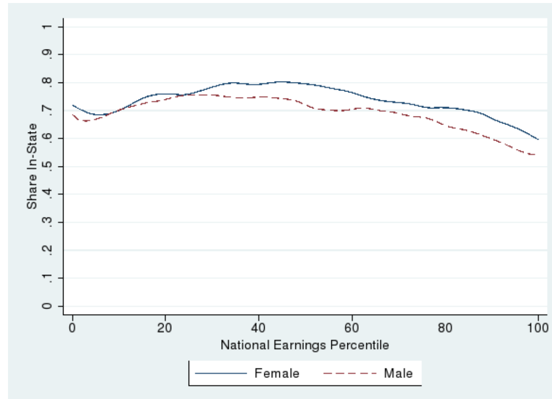
Note: Author's calculations using merged data from education records and earnings data from the LEHD. Sample is 10% of graduates from the University of Texas institutions and public four-year universities in Colorado who have at least three quarters of non-zero earnings and with at least $10,000 of earnings in the calendar year nationally (in-state or out-of-state). National earnings percentile is defined relative to other graduates in the sample observed in years 1, 5, and 10, respectively.

Figure 3: Share In-State by Percentile in the Earnings Distribution, by Sex and Residency

A. Sex

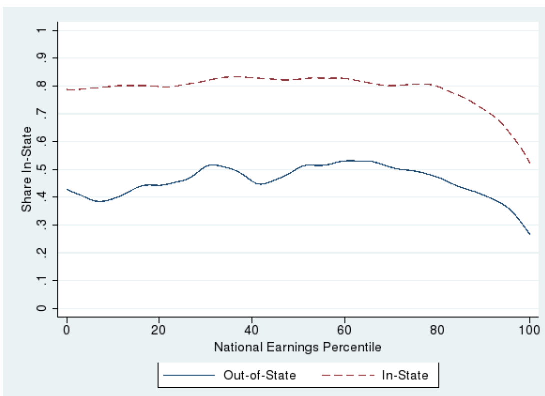


(a) 1 year post-graduation

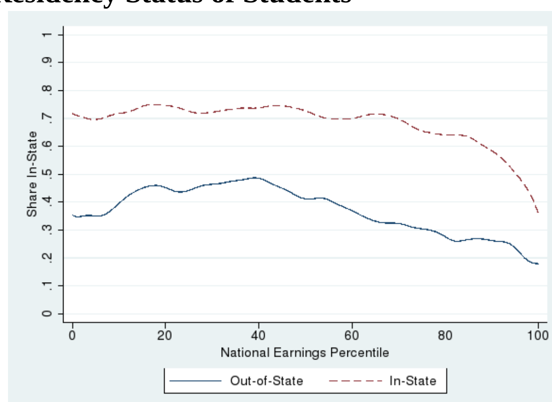


(b) 10 years post-graduation

B. In-state vs. Out-of-state Residency Status of Students



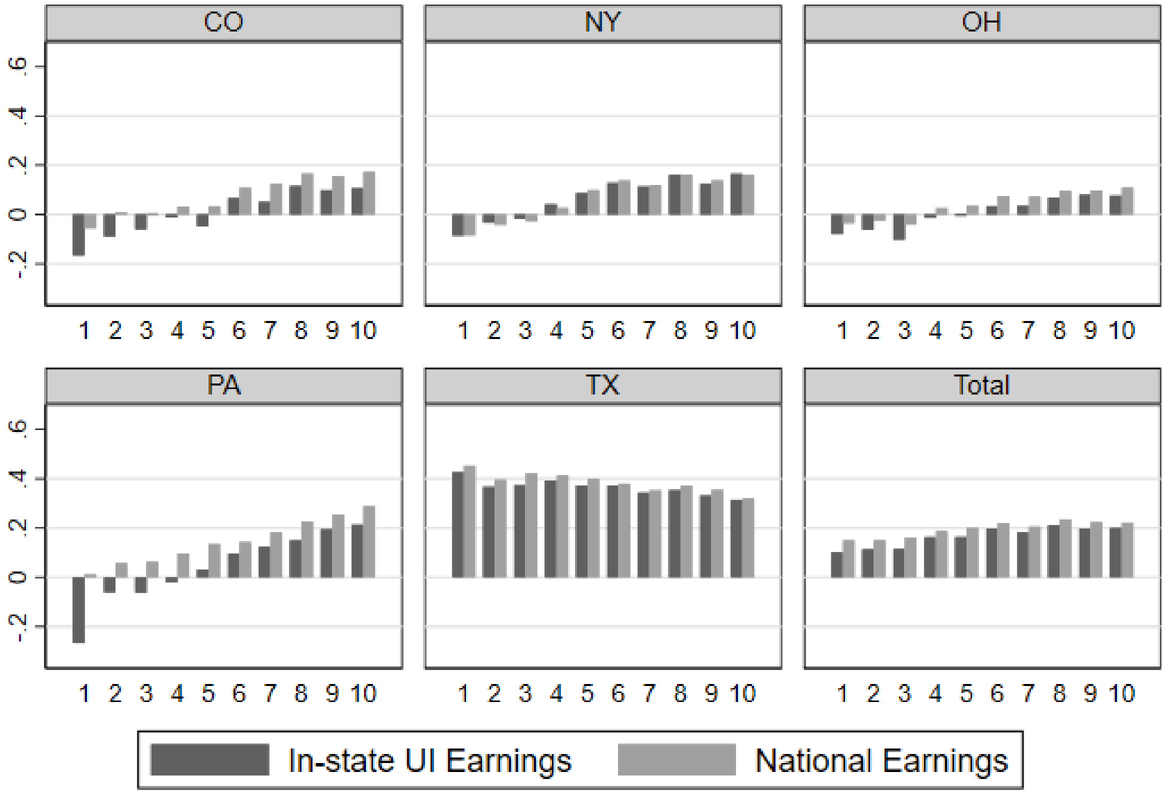
(c) 1 year post-graduation



(d) 10 years post-graduation

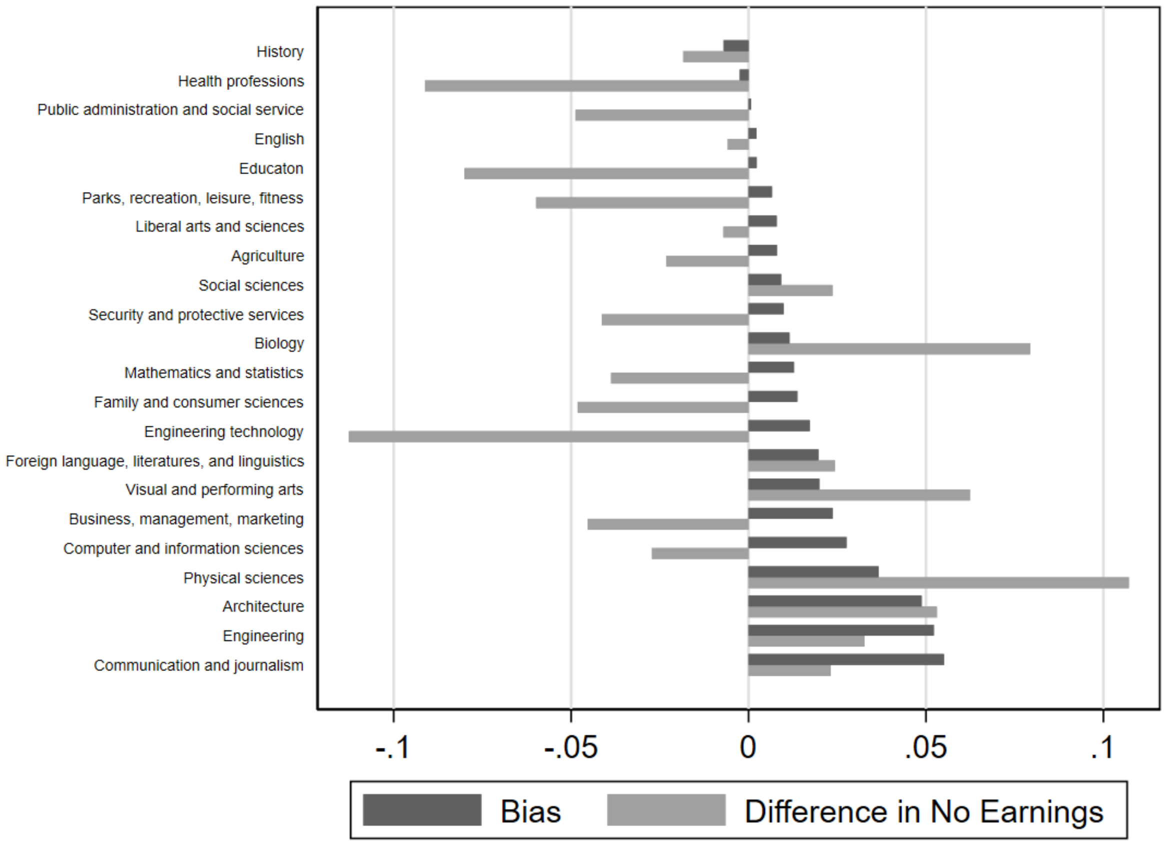
Note: Author's calculations using merged data from education records and earnings data from the LEHD. Sample is 10% of graduates from the University of Texas institutions and public four-year universities in Colorado who have at least three quarters of non-zero earnings and with at least $10,000 of earnings in the calendar year nationally (in-state or out-of-state). National earnings percentile is defined relative to other graduates in the sample with the same gender or residence classification.

Figure 4: Flagship Effect by State and Year Since Graduation



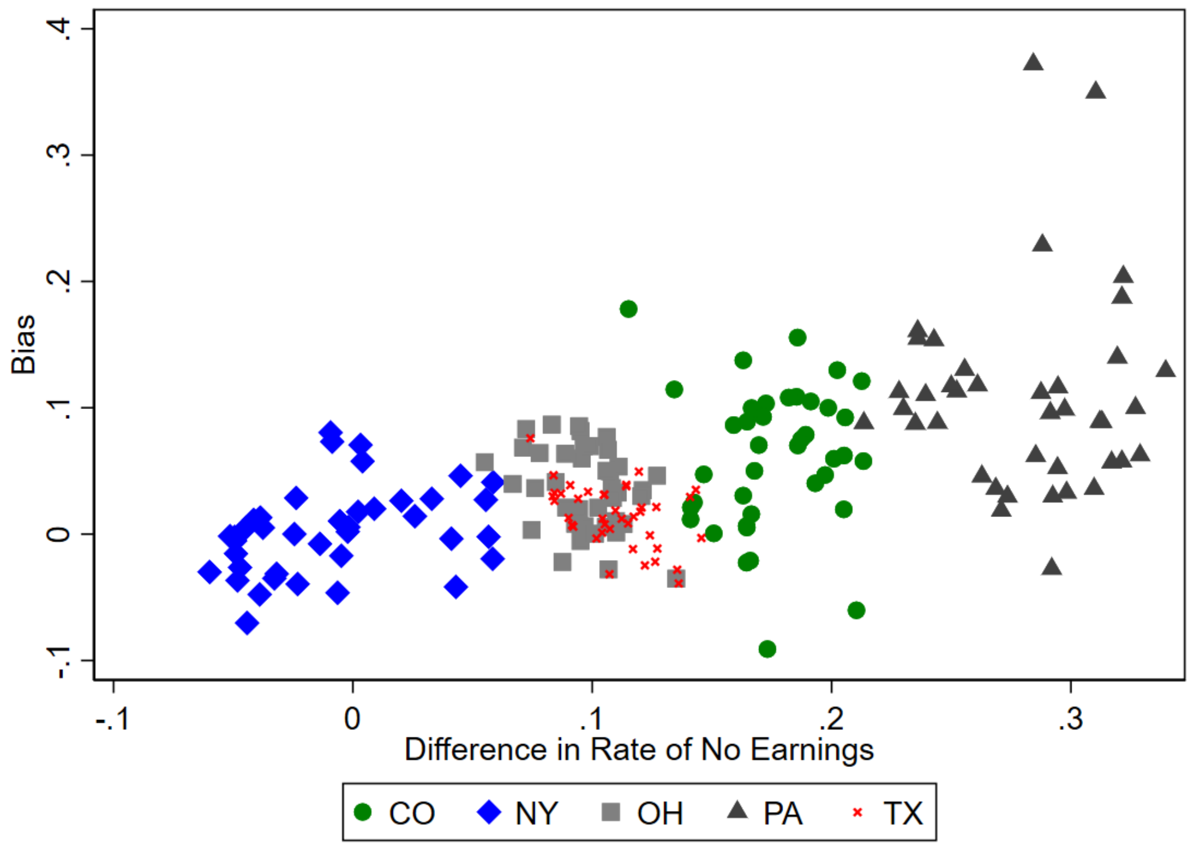
Notes: Flagship effect is estimated by regressing log annual earnings on a Flagship dummy, fixed effects for each calendar year, and dummies for male and race (4 categories), separately by state, graduating cohort, and year since graduation. This is done for records with non-zero earnings in the state and then nationally. Cell-specific estimates are averaged using number of students with national earnings as the weights. The difference between the flagship coefficients with these two samples is the bias reported in Panel A of Table 3.

Figure 5: Bias in Returns to Major



Note: Major-specific fixed effects (relative to psychology) were estimated separately by the three-way combination of state, time since graduation, and graduating cohort, including race and gender dummies and log earnings as the outcome. This table reports cell-specific estimates averaged using number of students with national earnings as the weights. Bias is the differences in estimated fixed effect when the sample includes all graduates with positive national earnings and when the sample includes all graduates with positive in-state earnings, as described in Section V.B.

Figure 6: Cell-level Flagship Bias by Difference in Rate of In-State Earnings



Notes: Flagship effect is estimated separately for cells defined by state, year since graduation, and graduation cohort by regressing log annual earnings on a Flagship dummy, fixed effects for each calendar year, and dummies for male and race (4 categories). This is done for records with non-zero earnings in the state and then nationally. The difference between the flagship coefficients with these two samples is the bias reported on the vertical axis. The horizontal axis is constructed similarly, but the outcome is an indicator for whether non-zero in-state earnings is observed.

Table 1. Summary Statistics for Full Analysis Sample

	Panel A. 4-year Graduates			Panel B. 2-year Enrollees		
		Institutional Selectivity			Time relative to Enrollment	
	All	Flagship	All Others	All	Before	After
National total earnings (UI + Self-employment)	45,790	51,370	44,020	13,740	11,470	19,380
	(41,540)	(49,280)	(38,600)	(23,660)	(26,110)	(25,050)
National UI earnings (annual, include zeros)	44,710	50,060	43,010	13,430	11,220	18,920
	(40,300)	(47,310)	(37,650)	(23,540)	(26,000)	(24,890)
In-state UI earnings (annual, include zeros)	34,690	31,990	35,540	11,990	9,590	16,910
	(39,020)	(44,080)	(37,230)	(22,240)	(25,190)	(22,210)
Log national total earnings (annual, zeros dropped)	10.500	10.600	10.470	9.139	8.949	9.548
	(1.052)	(1.107)	(1.032)	(1.362)	(1.444)	(1.213)
Log national UI earnings (annual, zeros dropped)	10.510	10.600	10.480	9.118	8.929	9.528
	(1.064)	(1.119)	(1.044)	(1.385)	(1.462)	(1.244)
Log in-state UI earnings (annual, zeros dropped)	10.450	10.500	10.440	9.080	8.888	9.488
	(1.105)	(1.191)	(1.083)	(1.394)	(1.473)	(1.265)
Male	0.436	0.479	0.422	0.440	0.440	0.452
	(0.496)	(0.494)	(0.494)	(0.496)	(0.496)	(0.498)
White	0.820	0.840	0.813	0.755	0.737	0.768
	(0.384)	(0.366)	(0.390)	(0.430)	(0.441)	(0.422)
Black	0.092	0.046	0.107	0.176	0.194	0.168
	(0.289)	(0.210)	(0.309)	(0.381)	(0.396)	(0.374)
Share with no national UI earnings (true zeros)				0.234	0.291	0.172
1 year post-grad	0.119	0.138	0.113			
5 year post-grad	0.137	0.149	0.134			
10 year post-grad	0.128	0.138	0.124			
Share with out-of-state UI earnings but no in-state UI earnings (migrants)				0.061	0.086	0.065
1 year post-grad	0.112	0.203	0.083			
5 year post-grad	0.178	0.291	0.142			
10 year post-grad	0.192	0.301	0.159			
Share with self-employment earnings	0.070	0.074	0.069	0.042	0.033	0.056
Observation (person-year) Count	1,901,000	458,000	1,443,000	3,685,000	1,633,000	983,700
Person Count	220,000	52,000	168,000	401,100	401,100	401,100

Notes: Table reports means (and standard deviations) of variables for combined person-year observations. Panel A includes a 10% random sample of 2001 to 2013 graduates from 145 four-year institutions in Colorado, New York, Ohio, Pennsylvania, and Texas observed each year in the labor market from first full calendar year after graduation through 2016. Panel B includes a 10% random sample of students enrolled in CTE programs (completers and non-completers with at least 10 credits) at public two-year colleges in Colorado, Ohio, New York, and Texas between 2001 and 2016 with annual earnings from 2000 to 2016. Observations for years of enrollment are included in "All" column of Panel B and included in analysis, but not broken out separately.

Table 2. Estimates of Effect of Flagship Graduation on Earnings

	In-state earnings (1)	National earnings (2)	National + Self Empl earnings (3)	Bias (4)	Bias (5)
Panel A. Full sample					
Earnings	1,741	9,634	9,985	7,893	8,244
	(206)	(208)	(211)		
Observations	1,901,000	1,901,000	1,901,000		
Panel B. Earnings > 0					
Earnings	11,880	13,130	12,970	1,250	1,090
	(220)	(198)	(203)		
Log Earnings	0.1467	0.1723	0.1679	0.0256	0.0212
	(0.0051)	(0.0044)	(0.0044)		
Log Earn P25	0.09921	0.1259	0.1179	0.0267	0.0187
	(0.0033)	(0.0027)	(0.0027)		
Log Earn P50	0.1656	0.1917	0.1878	0.0261	0.0222
	(0.0017)	(0.0015)	(0.0015)		
Log Earn P75	0.2109	0.2326	0.2314	0.0217	0.0205
	(0.0017)	(0.0015)	(0.0014)		
Observations	1,308,000	1,624,000	1,664,000		

Notes: Dependent variable is annual earnings. All models include fixed effects for each calendar year and graduation year (but not the interaction), dummy for each state, and dummies for male and race (4 categories). Standard errors clustered by individual. Panel A includes all observations while panel B only includes annual observations for which earnings are non-zero in the state (column 1), nationally (2), or nationally including self-employment (3).

Table 3. Estimates of Flagship Bias, Separately by State and Time Since Graduation

Years since graduation	state: CO	NY	OH	PA	TX	5 States
Panel A. Bias (Flagship Coeff National minus Flagship Coeff In-State)						
1	0.097	0.005	0.055	0.278	0.025	0.051
2	0.098	0.000	0.041	0.126	0.024	0.036
3	0.062	-0.007	0.056	0.129	0.038	0.040
4	0.044	-0.010	0.030	0.119	0.021	0.024
5	0.076	0.018	0.037	0.097	0.021	0.035
6	0.035	0.004	0.034	0.050	-0.004	0.014
7	0.064	0.013	0.032	0.060	0.000	0.021
8	0.026	0.002	0.023	0.084	0.009	0.017
9	0.038	0.014	0.013	0.067	0.012	0.020
10	0.061	-0.001	0.033	0.081	0.003	0.020
Total	0.061	0.004	0.036	0.110	0.015	0.028
Panel B. Difference in Rate of No Earnings (Flagship minus Non-Flagship School)						
1	0.153	-0.010	0.091	0.282	0.099	0.089
2	0.181	0.002	0.105	0.311	0.105	0.102
3	0.189	-0.005	0.104	0.300	0.089	0.094
4	0.199	-0.017	0.103	0.283	0.100	0.095
5	0.178	-0.010	0.095	0.276	0.106	0.095
6	0.185	-0.011	0.094	0.284	0.110	0.097
7	0.172	-0.010	0.091	0.279	0.114	0.097
8	0.169	-0.003	0.101	0.280	0.123	0.103
9	0.166	-0.006	0.097	0.273	0.122	0.101
10	0.162	-0.004	0.097	0.256	0.128	0.102
Total	0.176	-0.007	0.098	0.282	0.110	0.097

Notes: Effect of flagship on log earnings was estimated separately by the three-way combination of state, year since graduation, and graduating cohort, including race and gender dummies. This table reports cell-specific estimates averaged using number of students with national earnings as the weights. Sample includes graduating cohorts 2003 to 2006 to ensure comparable time frames for all states since the 2001 and 2002 cohorts include an incomplete sample for NY and PA.

Table 4. Estimates of Major Bias, Separately by Time Since Graduation
(relative to psychology major)

			Bias (Major FE National minus Major FE In-State)			
				Years since Graduation		
	CIP code	Share of grads	All years	1	5	10
Agriculture	1	1.5%	0.008	0.023	0.009	-0.006
Architecture	4	1.0%	0.049	0.065	0.037	0.044
Area, ethnic, cultural, gender studie	5	0.2%	-0.045	-0.081	-0.008	-0.019
Communication and journalism	9	6.6%	0.055	0.073	0.052	0.019
Computer and information sciences	11	3.2%	0.028	0.034	0.005	0.000
Educaton	13	5.4%	0.002	0.058	-0.024	-0.018
Engineering	14	5.8%	0.052	0.108	0.027	0.004
Engineering technology	15	1.3%	0.017	0.047	0.011	-0.002
Foreign language, literatures, and lir	16	1.5%	0.020	0.004	0.009	0.017
Family and consumer sciences	19	2.3%	0.014	0.048	-0.001	-0.018
English	23	4.9%	0.002	0.013	-0.008	-0.001
Liberal arts and sciences	24	1.7%	0.008	0.003	-0.012	-0.010
Biology	26	4.7%	0.012	0.017	-0.026	0.018
Mathematics and statistics	27	1.3%	0.013	0.026	0.019	0.002
Parks, recreation, leisure, fitness	31	3.2%	0.007	0.043	-0.007	-0.037
Philosophy and religious studies	38	0.3%	0.018	-0.063	0.066	0.029
Physical sciences	40	1.3%	0.037	0.021	-0.008	0.111
Security and protective services	43	2.7%	0.010	-0.004	0.001	-0.001
Public administration and social serv	44	1.5%	0.001	-0.018	0.009	-0.003
Social sciences	45	9.8%	0.009	0.010	0.021	-0.014
Visual and performing arts	50	5.1%	0.020	0.053	0.020	-0.014
Health professions	51	7.9%	-0.002	-0.014	-0.019	-0.002
Business, management, marketing	52	24.6%	0.024	0.036	0.012	0.007
History	54	1.8%	-0.007	-0.010	-0.017	-0.028
	Total	100.0%	0.019	0.032	0.008	0.001

Notes: Major-specific fixed effects (relative to psychology) were estimated separately by the three-way combination of state, time since graduation, and graduating cohort, including race and gender dummies and log earnings as the outcome. This table reports cell-specific estimates averaged using number of students with national earnings as the weights. Sample includes all graduates from 2001 to 2006.

Table 5. Estimates of Effect of CTE Credential

	Estimated Effect of CTE Completion				
	In-state earnings (1)	National earnings (2)	National + Self Empl earnings (3)	Bias (National minus In-State) (4)	Bias (Total minus In-state) (4)
Panel A. Short Certificates (n =727,000; 804,000; 820,000)					
Earnings (>0)	803	811	750	7.8	-53.1
	(296)	(294)	(297)		
Log Earnings (>0)	0.110	0.115	0.115	0.005	0.005
	(0.020)	(0.019)	(0.018)		
Panel B. Long Certificates (n=685,000;736,000; 750,000)					
Earnings (>0)	3165	3384	3448	219	283
	(140)	(144)	(143)		
Log Earnings (>0)	0.200	0.214	0.220	0.014	0.020
	(0.011)	(0.010)	(0.010)		
Panel C. Associates Degree (n = 799,000; 875,000; 892,000)					
Earnings (>0)	2610	2776	2764	166	154
	(119)	(118)	(118)		
Log Earnings (>0)	0.183	0.193	0.196	0.009	0.013
	(0.009)	(0.009)	(0.009)		

Notes: Dependent variable is annual earnings (level or log). All models include individual-specific intercepts and slopes, combining all earnings records before, during, and after enrollment. Models also include age (non-linearly), calendar year fixed effects, and a dummy for years of any enrollment. Standard errors clustered by individual. Sample includes students enrolled in CTE programs (completers and non-completers with at least 10 credits) at public two-year colleges in Colorado, Ohio, New York, and Texas between 2001 and 2016 with annual earnings measured from 2000 to 2016.

Table 6. Predicting Bias with Difference in Rate of No In-State Earnings

	A. Flagship bias (State-cohort-time cells)				B. Field bias (CIP-state-cohort-time cells)				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Difference in Likelihood of no	0.315***	0.330***	0.335***	0.124	0.0913***	0.108***	0.103***	0.0680***	0.0173
In-state Earnings	(0.0338)	(0.0353)	(0.0353)	(0.127)	(0.0220)	(0.0223)	(0.0222)	(0.0220)	(0.0324)
Grad year FE		Yes	Yes	Yes		Yes	Yes	Yes	Yes
Time since FE			Yes	Yes			Yes	Yes	Yes
State FE				Yes				Yes	Yes
CIP FE									Yes
Observations	280	280	280	280	5,587	5,587	5,587	5,587	5,587
R-squared	0.248	0.286	0.335	0.401	0.005	0.029	0.043	0.058	0.085

Table reports regression of amount of bias on difference in likelihood of not having positive in-state earnings at the cell-level. Panel A includes cells constructed by three-way combination of state, graduating cohort, and years since graduation, as reported in Table 3. Panel B includes cells constructed by four-way combination of major, state, graduating cohort, and years since graduation, as reported in Table 4. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 7. Estimates of Effect of Flagship Graduation on Earnings, Lee Bounds

	All Years		1st Year Post Grad		5th Year Post Grad		10th Year Post Grad	
	Coefficient	Lee Bounds	Coefficient	Lee Bounds	Coefficien	Lee Bounds	Coefficient	Lee Bounds
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Earn In-State	7577	[-3384; 22450]	3329	[-2499; 11640]	9410	[-1539; 23,000]	11370	[-3943; 30,540]
	(145.0)		(120.0)		(235.6)		(399.0)	
Log Earn In-State	0.0654	[-0.3772; 0.3654]	-0.0246	[-0.4609; 0.2471]	0.1051	[-0.313; 0.3917]	0.1513	[-0.2662; 0.4552]
	(0.008)		(0.012)		(0.014)		(0.0159)	

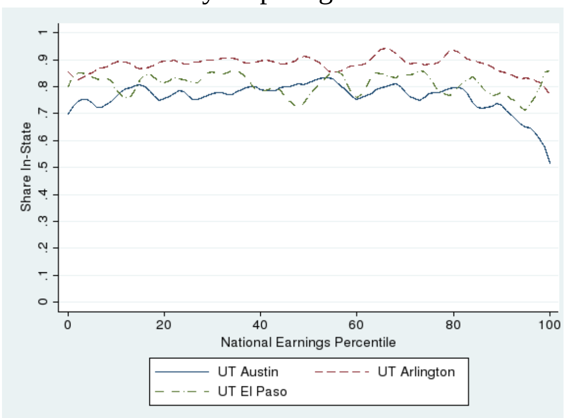
Notes: Sample includes all annual observations from graduating classes of 2001-2006 for which earnings are non-zero in the state. Models do not include any covariates or controls. N = 1308000; 70,500; 61,500; 56,000

Online Appendix A. Additional Figures and Tables

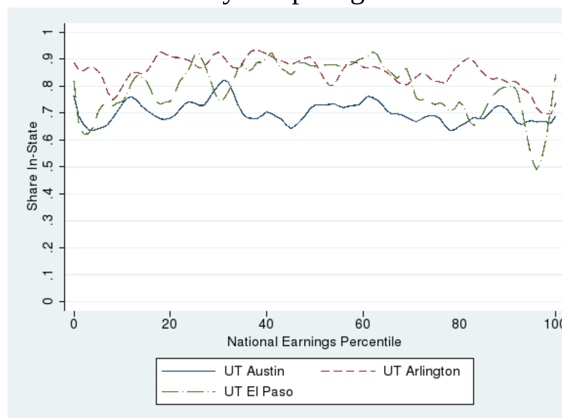
Figure A1: Share In-State by Percentile in the Earnings Distribution, by Institution and Major

A. By Institution

1 year post-graduation

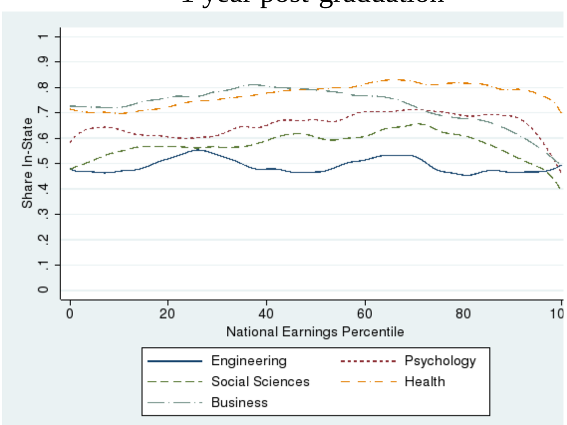


10 years post-graduation

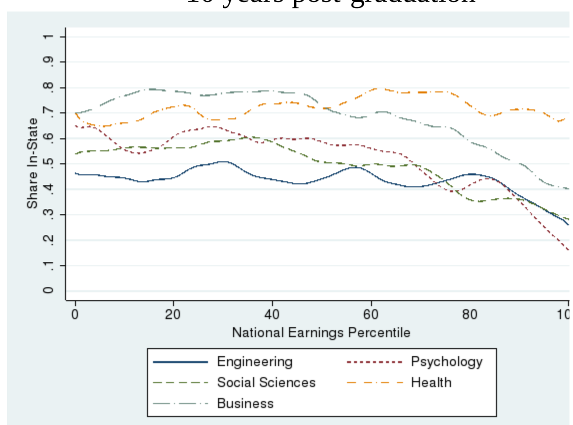


B. By Major

1 year post-graduation



10 years post-graduation



Note: Author's calculations using merged data from education records and earnings data from the LEHD. Sample is 10% of graduates from the University of Texas or public universities in Colorado who have at least three quarters of non-zero earnings and with at least $10,000 of earnings in the calendar year nationally (in-state or out-of-state). National earnings percentile is defined relative to other graduates in the sample in the same major.

Table A1. Select Recent Articles using Administrative Earnings Records in Postsecondary Education

					Emigration rate	
State	Study	5-year out migration rate	Country	Study	Total	High skilled
All US states		8.7%	Canada	Several	5.5%	6.4%
CA	Stevens, Kurleander, Groz (2019)	7.2%	Chile	Hastings, Neilson, Zimmerman (2013)	3.7%	4.0%
CO	Turner (2015)	13.0%	Finland	Bockerman, Haapanen, Jepsen (2019	6.0%	6.6%
FL	Hoekstra (2010); Altonji and Zimmerman, (2018); Zimmerman (2014); Bakkes Holzer Valez (2015)	9.1%				
			France	Canaan Mouganie 2018	3.1%	5.5%
KY	Jepsen, Troske, Coomes (2012)	7.7%	Italy	Anelli (2018)	5.1%	7.5%
MI	Bahr, Dynarski, Jacob, Kreisman, Sosa, Wiederspan, 2015	6.1%	Netherlands	Artmann, Hessel, Oosterbeek, van der Klaauw (2018)	6.1%	8.6%
MO	Dyke Heinrich Mueser Troske Jeon (2006)	8.4%	Norway	Kirkeboen, Leuven, Mogstad (2016)	4.4%	5.5%
NC	Liu Belfield, and Trimble (2015)	8.3%				
OH	Minaya and Scott-Clayton (2018); Engbom and Moser (2017); Ost, Pan, Webber (2018)	6.7%	UK	Belfield, Britton, Buscha, Dearden, Dickson, an der Erve, Sibieta, Vignoles, Walker, Zhu (2018)	8.1%	11.5%
TN	Carruthers Sanford (2018)	8.3%	US	Reference	0.8%	1.1%
TX	Andrews, Li, Lovenheim (2016); Andrews and Stange (2016); Cunha and Miller (2014); Denning Marx and Turner (2018)	6.7%				

Notes: US out-migration rates pertain to residents of all ages, from 1995 to 2000. Emigration rate is the fraction of citizens at least 15 years old living outside the country in 2010. High skilled refers to those with college degree.

Sources: Franklin, Rachel S. (2013). "Domestic Migration Across Regions, Divisions, and States: 1995 to 2000" Census 2000 Special Reports. Table 1. August 2003. OECD (2015). *Connecting with Emigrants - A Global Profile of Diasopras 2015.* Table 4.2 total emigration rates and emigration rates of the highly skilled, by country of origin, 2010/2011.

Table A2. Institutional Characteristics by Selectivity, 4-year Institutions

		count	Total FTE enrollment (2006)	Undergraduate FTE enrollment (2006)	SAT Math 75-25 Midpoint	Graduation rate (2006)	Instructional spending per fall FTE (2006)	Percent admitted (2006)	% Instate (PSEO)
Colorado	All others	10	13,762	11,794	524	0.43	7,402	0.79	0.705
	Flagship	2	25,004	22,516	603	0.67	10,695	0.87	0.600
New York	All others	39	10,299	8,752	535	0.45	9,270	0.53	0.821
	Flagship	3	15,683	12,435	634	0.69	11,796	0.45	0.752
Ohio	All others	25	15,639	12,701	533	0.44	8,378	0.83	0.714
	Flagship	2	42,220	33,338	615	0.74	14,332	0.69	0.603
Pennsylvania	All others	19	2,325	2,241	505	0.53	.	0.80	0.787
	Flagship	1	43,736	38,234	620	0.84	.	0.62	0.506
Texas	All others	41	15,987	13,825	523	0.38	7,576	0.73	0.845
	Flagship	3	41,006	32,531	622	0.76	11,922	0.61	0.751
All states	All others	134	13,534	11,493	527	0.42	8,242	0.68	0.797
	Flagship	11	36,216	29,608	619	0.75	12,412	0.64	0.657

Notes: Most selective (flagship) schools are defined as those with a mean SAT math score of at least 595, which includes University of Colorado Boulder, Colorado School of Mines, SUNY Genesco, SUNY Binghampton, SUNY Stony Brook, Ohio State University - Columbus, Miami University, Penn State University Park in State College, UT Austin, UT Dallas, and Texas A&M. All data comes from the IPEDS data center, with the exception of the % in-state, which comes from Conzelmann et al (2022), available at https://doi.org/10.3886/E170381V3

Table A3. Test of Difference in Covariates by Treatment Status and Sample Inclusion

	Male		White		Black		Asian		Hispanic	
	Full sample	Positive In-state Earnings	Full Sample	Positive In-state Earnings	Full Sample	Positive In-state Earnings	Full Sample	Positive In-state Earnings	Full Sample	Positive In-state Earnings
1 Year after	0.0645	0.0683	-0.0212	-0.0330	-0.0256	-0.0243	-0.0033	-0.0017	-0.1740	-0.1895
	(0.0073)	(0.0104)	(0.0052)	(0.0076)	(0.0027)	(0.0040)	(0.0124)	(0.0016)	(0.0051)	(0.0077)
5 years after	0.0645	0.0775	-0.0212	-0.0369	-0.0256	-0.0233	-0.0033	-0.0027	-0.1740	-0.2111
	(0.0073)	(0.0105)	(0.0052)	(0.0076)	(0.0027)	(0.0038)	(0.0124)	(0.0017)	(0.0051)	(0.0079)
10 years after	0.0645	0.0773	-0.0212	-0.0356	-0.0256	-0.0225	-0.0033	-0.0025	-0.1740	-0.2040
	(0.0073)	(0.0106)	(0.0052)	(0.0077)	(0.0027)	(0.0040)	(0.0124)	(0.0017)	(0.0051)	(0.0079)

Notes: Dependent variable is covariate listed and each cell reports the coefficient on a flagship dummy. All models include fixed effects for each calendar year and dummy for being a Texas institution. One observation per person is included. For each outcome, the first column includes all observations from graduating classes of 2001-2013 while the second column only includes observations for which earnings are non-zero in at least three quarters in the state in that year. Sample is 10% of graduates from the University of Texas or public universities in Colorado.

Online Appendix B. Simulation Details and Results

To examine the extent of bias under a more general set of conditions than our empirical example, we develop and simulate a simple model of earnings, work, and migration in the presence of a treatment. This Appendix describes the details of the model and simulation results.

A. Simulation model setup

Each person is characterized by six random variables:

- Treatment status (T) is randomly assigned, allowing us to abstract from bias arising from non-random selection into treatment.
- Draw from an ability distribution $A \sim N(0, \sigma_{ability})$
- Draw from in-state earnings offer distribution: $\tilde{y}_{in} = \beta_0 + A + \beta_{in}T + \varepsilon_{in}$ where $\varepsilon_{in} \sim N(0, \sigma_{in})$
- Draw from out-of-state earnings offer distribution $\tilde{y}_{out} = \beta_0 + A + \beta_{out}T + \varepsilon_{out}$ where $\varepsilon_{out} \sim N(0, \sigma_{out})$
- Reservation wage $r \sim U(0, r_{max})$ which is the same for in-state and out-of-state jobs
- Moving cost: $c \sim U(0, c_{max})$, which is uncorrelated with reservation wages and job offers

Note that in-state and out-of-state earnings offers are correlated both through the inclusion of ability A in both distributions and because treatment influences the means of both distributions (by β_{in} and β_{out}, respectively). This model assumes treatment effect homogeneity on earnings offers, though there will be heterogeneity in actual earnings effects depending on an individual's reservation wage and moving cost. Individuals with a high reservation wage will have a lower treatment effect because they will be less likely to move from non-employment.

Labor force participation decisions are made (separately for in-state and out-of-state earnings offers) by comparing offered earnings to the reservation level. Thus accepted earnings in each labor market are truncated:

$$y_{in} = \tilde{y}_{in} * 1\{\tilde{y}_{in} > r\} \text{ and } y_{out} = \tilde{y}_{out} * 1\{\tilde{y}_{out} > r\}$$

Mobility decisions are made by comparing the difference in accepted offers between labor markets to moving costs:

$$Move = 1\{y_{out} - y_{in} > c\}$$

Finally, actual earnings is given by

$$y_{actual} = y_{in}(1 - Move) + y_{out}Move$$

The problem arises in that y_{actual} is not observed by the researcher, but rather earnings are observed as zero if the worker moves out of state:

$$y_{observed} = y_{in}(1 - Move) + 0 * Move$$

To illustrate the bias that arises in such a model, we simulate 100,000 draws with the following parameters: β_0=\$8000, $\sigma_{ability}$=\$2000, σ_{in}=\$1000, σ_{out}=\$1000, r_{max}=\$8000, c_{max}=\$3000. We set β_{in}=\$2000, which corresponds to a 25% treatment effect on the mean of the in-state earnings distribution. We'll see that results are particularly sensitive to the relative treatment effects on out-of-state and in-state earnings, so we present results where β_{out} equals different multiples of β_{in}. We also examine simulations in which migration is exogenous.

B. Simulation results

In Table B1 we report results of regressions using this simulated data. Panel A depicts our base model, where treatment differentially increases out-of-state earnings ($\beta_{out} > \beta_{in}$) and migration is endogenous in the sense of responding to earnings differentials across areas. The most naïve comparison – simply comparing observed earnings between treatment and control group – is very negatively biased. In fact, the point estimate is close to zero when in fact the true effect of treatment is \$2874.[1] This is because out-of-state workers have higher earnings as a consequence of the treatment, but are coded as having no earnings. Many researchers restrict the analysis sample to workers with non-zero in-state earnings. Doing so lowers, but does not eliminate the migration bias (second row). Furthermore, it should be noted that this restriction changes the estimand to the effect of treatment on earnings conditional on (non-random) participation.

[1] Note that the true treatment effect is a weighted average of the treatment effects on the in- and out-of-state earnings offer distributions (2000 and 3000, respectively, in Panel A) combined with any effects on migration and labor supply.

Ignoring the extensive earnings margin will understate the total earnings (and welfare) effect of an intervention. With this caveat, we continue to focus on this estimand. Estimates of effects on log earnings (restricting to individuals with positive earnings) will also be biased downwards, particularly at the high end of the distribution. The bottom of the table describe migration patterns for the sample. Moving is highly correlated with both treatment and earnings: treatment is associated with a 20 percentage point increase in likelihood of moving and $1000 more in actual earnings is associated with a 4 percentage point increase in likelihood of moving. Individuals that move have earnings that are $1878 higher than those that don't. This suggests two conditions for the presence of bias: migration is related to earnings and treatment is related to migration.

Panel B shows results from a simulation where treatment does not differentially affect in- and out-of-state earnings offers ($\beta_{out} = \beta_{in}$). The naïve model is still biased downwards slightly because out-of-state workers have higher earnings (moving costs must be overcome) and these higher earnings are erroneously set to zero. However, there is no bias in any other specifications. While migration is still endogenous and related to actual earnings (higher earning individuals are more likely to move), treatment is now unrelated to moving. Non-random migration will still affect estimates of the overall earnings distribution, but treatment effect estimates will not be subject to bias.

Finally, Panel C shows results from a simulation where treatment differentially affects in- and out-of-state earnings offers ($\beta_{out} > \beta_{in}$), but migration is exogenous (set at 25%). That is, individuals are randomly assigned to move regardless of their earnings offers, moving costs, or treatment status. While this feature reduces the bias relative to the base case, it does not eliminate it. Indeed even with exogenous migration there is still an association between migration and actual earnings because treatment effects out-of-state earnings. Thus any effect of treatment on earnings that only occurs on out-of-state earnings is lost when you only use in-state students.

C. Bounding Approaches

We examine the performance of some alternative bounding approaches with our Monto Carlo simulations and report results in Table B2. In the first, we use the full sample of individuals, including those with no matched earnings (due to either non-employment or migration). We substitute zero earnings for the actual earnings of individuals in the top or bottom D% of the non-zero earnings distribution of the control group. D is the difference in match rates between the two groups as a proportion of the control group match rate. In our simulation D equals 17%, with the control group more likely to match. This generates an upper (lower) bound of the true treatment effect under the extreme assumption that all of difference in match probabilities comes from untreated individuals with the highest (lowest) earnings who would have otherwise left the state if they were treated, assuming no effect of treatment on employment.[2] Panel B reports these results. While an improvement over the naïve regression, the bounds [$796, $1568] nonetheless do not contain the actual treatment effect ($2874). The upper bound fails to capture any earnings improvement operating via increased employment.

In Panel C, we specifically implement Lee's (2009) approach by restricting our analysis to records with non-zero (positive) observed earnings. We then omit the top (bottom) 17% of the control group observed earnings distribution when calculating the treatment-control outcome difference. We do this for mean earnings levels, log earnings, and for moments of the log earnings distribution. The constructed bounds do contain the true parameter (as well as the biased estimated parameter) in all cases, though the truth is typically closer to the upper than lower bound and the bounds are wide.

In Panels D and E we implement sharper Lee (2009) bounds by introducing a baseline (pre-treatment) covariate. The process essentially involves computing bounds separately for twenty groups defined by individual's latent ability (Panel D) or moving cost (Panel E), then computing a weighted average of these group-specific estimates. Latent ability, which is much more highly correlated with earnings than moving costs, tightens the bounds considerably. However, the upper bound [$2,390] nearly omits the true effect [$2,315]. Finally, in Panel F we implement bounds that are robust to a failure of the monotonicity assumption, as suggested by Zhang and Rubin (2003): we trim both the treatment and control groups by their rates of missing in-state employment. Given the high rates of non-employment and out-migration, this approach yields bounds that are completely uninformative.

[2] This is the approach taken by Denning, Marx, and Turner (2019), though the rate of differential attrition in their setting is much lower than our simulations.

D. Lessons from the simulations

We take four lessons from this simulation analysis. First, the ratio $\frac{\beta_{out}}{\beta_{in}}$ is a key determinant of bias. Bias is zero when treatment has a similar effect on in-state and out-of-state earnings ($\beta_{out} = \beta_{in}$), as this is what induces differential migration by earnings in the treatment and control groups. Second, there can be bias even if migration is completely exogenous. Again, differential treatment effects for in-state and out-of-state earnings will truncate the observed earnings distribution of the treatment group more than the control group even if migration is exogenous. Third, bias is reduced when the sample is conditioned on having positive observed earnings. This drops both movers and in-state non-participants, so it does change the target of estimation to a parameter that does not fully capture the consequences of the treatment under study. Finally, a test of the presence of bias is whether the relationship between migration and earnings differs between the treatment and control groups. Interestingly, having migration unrelated to treatment (the exogenous mobility case in Panel C) is not sufficient to rule out bias. Nor is a relationship between moving and actual earnings necessarily evidence of bias (the endogenous mobility with $\beta_{out} = \beta_{in}$ in Panel B).

Table B1. Simulation Results

Sample	Moment		Panel A. Base Simulation β_out = 1.5Xβ_in endogenous migration			Panel B. β_out = β_in endogenous migration			Panel C. β_out = 1.5Xβ_in exogenous migration			Panel D. β_out = 1.5Xβ_in, sd_out_treat = 1.5Xsd_out endogenous migration			Panel E. β_out = β_in, sd_out_treat = 1.5Xsd_out endogenous migration	
		Actual	Observed	Bias	Actual	Observed	Bias	Actual	Observed	Bias	Actual	Observed	Bias	Actual	Observed	Bias
Coefficient on treatment indicator with earnings outcomes																
Full	mean level	2,896	82	-2,814	2,332	1,961	-372	2,727	1,821	-906	3,079	-138	-3,217	2,502	1,530	-973
Earn > 0	mean level	2,315	1,972	-343	1,823	1,803	-20	2,021	1,776	-245	2,510	1,932	-578	1,989	1,806	-184
Earn > 0	mean log	0.248	0.218	-0.030	0.201	0.201	0.000	0.226	0.202	-0.024	0.264	0.214	-0.050	0.216	0.201	-0.015
Earn > 0	p10 log	0.296	0.274	-0.022	0.250	0.254	0.004	0.283	0.260	-0.023	0.300	0.268	-0.032	0.260	0.253	-0.006
Earn > 0	p50 log	0.242	0.209	-0.033	0.195	0.193	-0.002	0.216	0.192	-0.024	0.258	0.205	-0.053	0.209	0.193	-0.016
Earn > 0	p90 log	0.203	0.175	-0.028	0.161	0.162	0.001	0.186	0.162	-0.024	0.232	0.173	-0.059	0.181	0.163	-0.018
Migration and work patterns of sample																
% Move			28.9%			19.0%			24.9%			29.8%			21.1%	
% Don't work			4.4%			4.7%			6.9%			4.4%			4.7%	
Outcome = move																
coefficient on treatment			0.200			0.000			0.002			0.218			0.043	
coefficient on actual earnings (x 100(			0.040			0.017			0.010			0.046			0.025	
Outcome = actual earnings																
coefficient on move			1872			983			569			2271			1441	

Notes: Top half of table reports coefficient estimate from OLS regression of earnings outcome on indicator for treatment. Actual uses true earnings outcome and observed uses outcome where earnings is set to zero for individuals that move out of state. Sample restriction of positive earnings is imposed on either actual or observed earnings depending on the specification. Simulations for 100,000 observations use the following parameter values: β_0=\$8000, σ_ability=\$2000, σ_in=\$1000, σ_out=\$1000, r_max=\$8000, c_max=\$3000, β_in=\$2000.

Table B2. Monte Carlo Bounding Results

Sample	Moment	Panel A. Point estimates of earnings effects		Panel B. Replace the top and bottom X% of positive earnings distribution from control group with zero		Panel C. Lee Bounds: exclude the top and bottom X% of positive earnings distribution from control group		Panel D. "Tight" Lee Bounds: Compute bounds separately by ventile of ability distribution and re-weight		Panel E. "Tight" Lee Bounds: Compute bounds separately by ventile of moving cost distribution and re-weight		Panel F. Monotonicity failure: trim X% of control group and Y% of treatment group	
		Actual	Observed	Lower	Upper	Lower	Upper	Lower	Upper	Lower	Upper	Lower	Upper
<u>Coefficient on treatment indicator with earnings outcomes</u>													
Full	mean level	2,896	82	804	1581								
Earn > 0	mean level	2,315	1,972			1,336	2,626	1,874	2,390	1,478	2,512	-1,322	5,249
Earn > 0	mean log	0.248	0.218			0.129	0.290	0.216	0.267	0.153	0.279	-0.143	0.587
			Fraction trimmed from T									45%	
			Fraction trimmed from C			18%		18%		18%		54%	

Notes: Panel A reports coefficient estimate from OLS regression of earnings outcome on indicator for treatment. Actual uses true earnings outcome and observed uses outcome where earnings is set to zero for individuals that move out of state. Sample restriction of positive earnings is imposed on either actual or observed earnings depending on the specification. Panel B replaces bottom or top 17% of observed earnings distribution of control group with zero eearnings to construct lower and upper bound for true estimate, respectively. Panel C excludes the bottom or top 17% of observed earnings distribution of control group from regression. Panel D calculates bound for 20 groups defined by ability and then calculates weighted average of these bounds. Simulations for 100,000 observations use the following parameter values: $\beta\_0=\$8000$, $\sigma\_ability=\$2000$, $\sigma\_in=\$1000$, $\sigma\_out=\$1000$, $r\_max=\$8000$, $c\_max=\$3000$, $\beta\_in=\$2000$, $\beta\_out = 1.5X\beta\_in$. Migration is endogenous.