2017
OKSWP1702

Economics Working Paper Series
Department of Economics

OKLAHOMA STATE UNIVERSITY
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In-State College Enrollment and Later Life Location Decisions

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Abstract

State and local policymakers are very interested in how attending college in one's home state affects the likelihood of living in that state after college. This paper uses cohort-level data from the American Community Survey, decennial censuses, and other sources to examine how birth-state college enrollment affects birth-state residence several years later. Ordinary least squares and instrumental variables estimates both suggest a statistically significant positive relationship. The preferred instrumental variable estimates suggest that a one percentage point increase in birth-state enrollment rates increases later life birth-state residence by roughly 0.41 percentage points. Implications for policy are discussed.

JEL Codes: H75, I25, J24, R23
Keywords: higher education policy; in-state college enrollment; migration; college attendance

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The author thanks Jennifer Hunt, two anonymous reviewers, Patricio Aroca, Kathryn Birkeland, John Crooker, Tyler Daun, Luyi Han, Dan Rickman, Dave Sjoquist, Rocio Valdebenito, and conference participants at the Missouri Valley Economic Association Annual Meetings and the North American Regional Science Council Annual Meetings for helpful comments. All opinions, errors, or omissions are solely the responsibility of the author.

I. Introduction

College educated workers are incredibly important for national, state, and local economies. They pay higher taxes and consume fewer public services (Trostel 2010). They also increase wages and employment outcomes for other workers in the same area (Moretti 2004, 2013; Winters 2013, 2014), make the area a more desirable place to live (Shapiro 2006; Winters 2011a), and encourage future employment and population growth (Simon 1998; Simon and Nardinelli 2002; Glaeser and Saiz 2004). In recognition of the significant benefits provided by educated residents, states subsidize higher education in various ways including charging state residents in-state tuition rates at public higher education institutions that are typically well below the full cost of the education received. However, higher education also increases the probability that an individual will move away from his or her birth state after college (Malamud and Wozniak 2012)[1], and many are concerned that out-migration will negate the benefits to the state of college educating more young people. Thus, researchers and policymakers are very interested in how various factors including in-state enrollment affect the decisions of college-educated persons from the state to remain in the state after finishing college. Despite this topic's importance, there is very little empirical research attempting to estimate how where one attends college causally affects where one lives later in life.

The current paper seeks to fill an important gap in the literature by examining the causal effect of attending college in one's birth state on the likelihood of living in one's birth state later in life. Specifically, I use cohort-level data from the American Community Survey (ACS),

[1]Malamud and Wozniak (2012) identify exogenous increases in higher education in the U.S. using Vietnam Era draft induction risk. Böckerman and Haapanen (2013) examine polytechnic reform in Finland and find that converting vocational colleges to universities increased university attendance and as a result increased later-life migration rates. Machin, Salvanes and Pelkonen (2012) examine compulsory schooling laws in Norway and find that they increased secondary education participation and consequently increased later-life migration rates. However, McHenry (2013) is a notable exception this literature; he finds that increases in secondary education attributable to changes in compulsory schooling laws in the U.S. actually reduce out-migration.

decennial censuses, and other sources to examine the effect of birth-state college enrollment on the likelihood of birth-state residence years later. My regression equations include birth-state fixed effects, so identification comes from variation in birth-state enrollment rates across cohorts within states. I also account for age and year effects and a number of cohort-level controls. The regression variables used are cohort-level means, and the individuals included in the ACS sample are generally not the same individuals included in the decennial census samples, but they share common cohort level influences. Ordinary least squares (OLS) estimates could be biased. My preferred estimates are from two stage least squares (2SLS) regressions that instrument for birth-state enrollment rates using in-state tuition at flagship universities in the birth state at age 17. The expectation is that lower in-state tuition increases birth-state enrollment, which then increases birth-state residence years later after college.

The preferred 2SLS results suggest that increasing the percentage of a cohort attending college in their birth state by one percentage point increases the percentage of college attendees living in their birth state later in life by roughly 0.41 percentage points. This is a less than proportional effect, but the magnitude is still considered relatively large and economically meaningful. This finding is consistent with college students developing attachments to people and places during college that alter their post-college location decisions. The results, therefore, suggest that state higher education policies that motivate students to attend college in their home state can considerably affect their post-college location decisions and potentially provide considerable benefits for the state. However, higher education policies are often very expensive and may have limited effects on in-state attendance. In particular, in-state tuition at flagship universities has modest effects on birth-state enrollment. Other policies encouraging young people to attend college in-state may be more cost effective.

II. Conceptual Framework and Previous Literature

A large literature suggests considerable benefits to a state from increasing its stock of college-educated workers (Simon 1998; Simon and Nardinelli 2002; Moretti 2004, 2013; Glaeser and Saiz 2004; Shapiro 2006; Trostel 2010; Winters 2011a, 2013, 2014). States often cite these benefits to justify higher education policies encouraging young people to attend college in-state, with the hope that in-state college enrollment will increase the likelihood of residing and working in that state after finishing college.[2] Many policymakers believe that their state loses too much homegrown talent to other states and want to slow this brain drain. This seems especially prevalent in states that otherwise have a hard time attracting educated workers from other areas. For such states, keeping their homegrown talent might be an especially desirable economic development strategy. However, there is little agreement on the effectiveness of various higher education policies for this purpose. In particular, there is no consensus on whether attending college in-state meaningfully affects the probability of living in one's home state later in life.

This paper examines the relationship between college and post-college location decisions. Each individual will choose the location in each time period that maximizes the net present value (NPV) of expected future utility. Changing locations involves both monetary and emotional costs (Kennan and Walker 2011). The monetary costs involve the expenses of finding a new residence and moving one's possessions. The emotional costs involve the disutility resulting from moving away from one's friends, family, and places of comfort and familiarity. However,

[2] For example, the considerable spread of state merit-based student financial aid programs since 1990 has been partially justified by an imperative to retain more college educated workers in the state (Sjoquist and Winters 2014).

relocating can confer considerable benefits through human capital accumulation, higher future wages, and potentially, a higher quality of life.

A young person just finishing high school must choose whether to attend college and where to attend college, if they decide to attend. They will choose the path that offers the highest NPV of expected future utility. The current study is particularly interested in whether or not a student attends college in-state. Factors that could affect an individual's college location decision include tuition and financial aid policies, the quality of colleges and universities, the strength of various labor markets, the cost of living, the locational amenities that places offer, the location decisions of friends and family, and proximity to prior locations (Rizzo and Ehrenberg 2004; Alm and Winters 2009; Winters 2012; Faggian and Franklin 2014).

Once a person finishes college, they must make another location decision. Some will stay in the area where they attended college, some will move back to a previous location, and some will move on to a new location (Knapp, White, and Wolaver 2013). I again assume that individuals make this decision to maximize the NPV of expected future utility. Factors that could affect an individual's post-college location decision include current and future employment prospects, cost of living, locational amenities, the location decisions of friends and family, and various attachments to prior locations.[3] I argue in this paper that in-state college enrollment likely increases one's attachment to one's home state and therefore increases the probability of residing in that state later in life, as compared to attending college outside of one's home state.

[3] A growing literature has examined the determinants of post-college location decisions in the U.S. (Ferguson et al. 2007; Chen and Rosenthal 2008; Whisler et al. 2008; Partridge et al. 2012; McHenry 2014) and other countries (Corcoran, Faggian, and McCann 2010; Brown and Scott 2012; Miguélez and Moreno 2014; Abreu, Faggian, and McCann 2015).

Many young people leave their home state to attend college out-of-state, and exposure to a new place is likely to weaken their attachment to their previous state. Attending college in a place likely strengthens one's ties to that area by increasing attachments to friends, employers, and location-specific amenities (Winters 2011b). Attending college in one's home state may have an especially important effect on post-college location decisions because of proximity to family and previous familiarity with the area. However, assessing the causal effect of in-state vs. out-of-state enrollment on later life location is challenging because persons who attend college out-of-state likely differ in both observable and unobservable dimensions from those who attend college in-state. In particular, individuals who attend college out-of-state may have greater academic ability or weaker attachment to their home state than those who attend college in-state. These factors could also make them more likely to live outside their birth state later in life. Thus, simply observing the relationship between in-state college enrollment and residence in one's home state later in life could lead to biased results.

Despite this topic's importance, there is relatively little research directly examining the effects of in-state college enrollment on later-life location decisions. Groen (2004) is the only other paper of which I am aware that directly examines this topic for the U.S., and I use a very different identification strategy and address a slightly different question than Groen (2004).[4] He uses individual student data from two sources: the Mellon Foundation's College and Beyond

[4] There is a related international "brain drain" literature that examines the effects of attending college outside one's home country on the probability of residing outside one's home country later in life (e.g., Oosterbeek and Webbink 2011; Parey and Waldinger 2011; Di Pietro 2012). These studies consistently find that foreign college enrollment has a significantly positive effect on living outside of one's home country later in life, but the magnitudes do vary somewhat. However, studying abroad in a foreign country often involves greater cultural, linguistic, financial, and legal challenges than simply going to college out-of-state in the U.S., and it is not clear how comparable the effects should be.

(C&B) survey and the National Longitudinal Study of the High School Class of 1972 (NLS72).[5] His preferred specifications restrict the sample to college attendees who applied to at least one out-of-state college and control for the states to which the individuals applied. His preferred specifications suggest that attending college in-state increases an individual student's likelihood of living in the state 10-15 years later by roughly 10 percentage points, i.e., if a new policy induced 100 additional students to enroll in-state who would have instead enrolled out-of-state, only 10 of those additional students would still be in the state 10-15 years later. He interprets this as a modest effect.

Other researchers have offered more indirect evidence on the effects of in-state college enrollment on later life location decisions. Bound et al. (2004) examine the relationship between the production and stock of college graduates in a state.[6] Bound et al. (2004) use data from the Integrated Postsecondary Education Data System (IPEDS) to measure the production of college graduates *in* a state, but do not differentiate between in-state and out-of-state college enrollment.[7] They find a modest relationship between the production and stock of college graduates. Kennan (2015) also addresses the topic by estimating a dynamic programming model for college enrollment and pre- and post-college migration decisions and illustrates effects of various higher education policy changes for several large states. Kennan (2015) finds considerable moving costs and that state higher education policies may be able to have considerable long run effects on a state's workforce.

[5] Groen and White (2004) report a similar analysis using the C&B survey, but their primary focus is to assess whether public universities set lower admission standards for in-state applicants than out-of-state applicants. They find evidence that public universities do set lower admissions for in-state students.

[6] Winters (2017a) uses the American Community Survey (ACS) to measure the production of college graduates among persons *from* a state regardless of where they completed the degree. He finds that increasing the production of college graduates from a state by one percentage increases the stock of college graduates residing in the state by 0.5 percentage points.

[7] Abel and Deitz (2012) use IPEDS data to examine the production-stock relationship for metropolitan areas and also find a modest relationship.

Other researchers have indirectly addressed this topic by examining the reduced form effects of state merit-based financial aid programs on post-college location decisions (Hickman 2009, Hawley and Rork 2013; Sjoquist and Winters 2014; Fitzpatrick and Jones 2016; Bettinger et al. 2016; Harrington et al. 2016). They find positive effects of merit aid on the probability of living in one's birth state years later and suggest that this is likely at least partially due to the positive effects of merit aid on in-state college enrollment found by other researchers (e.g. Cornwell et al. 2006; Toutkoushian and Hillman 2012; Winters 2012). However, merit aid may also affect post-college location decisions through other mechanisms related to student effort, renewal requirements, peer quality, and other factors. Sjoquist and Winters (2014) estimate state-specific effects of merit aid on in-state enrollment and adult birth-state residence and compare the two. They find the correlation is only 0.187, suggesting that the effect of merit aid on adult location decisions depends on more than just in-state enrollment effects. I will later compare my preferred 2SLS approach in this paper to a 2SLS approach where state merit aid programs are used as the instrument. There are notable differences.

More generally, states may be able to influence in-state enrollment rates through other higher education policies such as increased subsidies to lower gross tuition rates, increased need-based aid, increased access in underserved geographic areas, and increased resources devoted to academic quality, campus amenities, marketing and recruiting. Some of these policies have the potential to affect numerous outcomes including educational attainment, educational quality, educational inequality, and others. The current paper uses a 2SLS approach with the preferred instrument log in-state tuition at public flagship universities. I suggest that this instrument will affect college location decisions but not affect other factors related to post-college locations decisions. This approach is intended to provide causal estimates of the effects of birth-state college enrollment on later life birth-state residence. This relationship will have a considerable influence on the extent to which various higher education policies actually benefit a state.

Of course, even if state human capital retention policies benefit an individual state, it does not imply that such policies benefit the nation as a whole. Policies that distort location decisions may even have a negative effect on national well-being. Highly mobile college-educated workers generally move toward productive locations where they can earn high incomes and advance their careers (Chen and Rosenthal 2008). Also, agglomeration economies from concentrating skilled workers in a few areas may lead to human capital spillovers and increased innovation that increases national economic productivity (Moretti 2013). State policies that reduce mobility may reduce match quality between workers and firms and lead to fewer human capital externalities and less innovation at the national level, lowering national productivity and well-being.

State higher education policies may also have some undesirable unintended consequences. For example, state merit aid programs have been shown to shift students away from STEM majors and toward lower paying majors, perhaps due to student efforts to increase their grade point averages to maintain scholarship eligibility (Sjoquist and Winters 2015b). Similarly, some students who are induced by financial aid policies to attend an in-state institution may be shifting from a higher quality institution to a lower quality one (Sjoquist and Winters 2016). These unintended effects are likely harmful at the national level and reduce the net benefits to the states adopting them. Other higher education policies could have unintended consequences as well. The current study does not address the efficacy of any particular policy. Still, the relationship between birth-state college enrollment and later life residence is an important one to study and better understand, even if the policy implications that result are more general than specific.

III. Data and Methods

The main data in this paper are constructed using the 2006-2014 ACS and the 1980-2000 decennial census microdata accessed from IPUMS (Ruggles et al. 2015). Each year of the ACS includes a one percent random sample of the U.S. population and each decennial census file includes a five percent sample of the population. The ACS and census microdata both report the state in which an individual currently resides and the state in which they were born, and these variables are used to construct the main variables in this study. These samples include persons residing in traditional households and group quarters such as college dorms, military barracks, and correctional institutions. Survey weights are provided that can be used to account for sampling design and individual non-response to make the samples representative of the U.S. population. I use survey weights for all variables constructed from the decennial censuses and ACS.

The main dependent variable in this study is the percentage of all college attendees (including graduates and non-graduates) from a given birth state and birth year cohort who live in their birth state in the 2006-2014 ACS. I first use ordinary least squares (OLS) to estimate variants of:

$$Y_{scat} = \alpha + \beta PctInState_{sc} + \gamma StateControls_{scat} + \delta_s + \varphi_{sc} + \pi_a + \theta_t + \varepsilon_{scat}$$

, where the outcome (Y_{scat}) is measured for persons born in state s born in cohort year c observed at age a during survey year t. The regression controls for a number of explanatory variables including fixed effects for state of birth (δ_s), region×year-of-birth (φ_{sc}), survey year

(θ_t), and age (π_a) at the time of the ACS. Including birth state fixed effects means that identification comes from variation across cohorts within states. Region×year-of-birth fixed effects account for aggregate trends at the regional level. Survey year and age effects capture aggregate effects due to survey year and age. For all regressions, standard errors are clustered by birth state.

The main explanatory variable of interest is the percentage of college enrollees from birth state s and birth-year cohort c who enrolled in their birth state ($PctInState_{sc}$), computed using the 1980, 1990, and 2000 decennial censuses for persons ages 18-22 at the time of one of these censuses.[8] Census survey instructions explicitly inform household respondents that they are not to include college students who live somewhere else while attending college. College students are enumerated at the location where they reside while attending college.[9] The 18-22 age range was selected to reflect the peak college-going ages for traditional students.[10] This gives 750 (=3×5×50) state and year-of-birth cohorts.[11] Each cohort is observed once in each of the nine ACS years giving 6750 total observations. The expectation is that a higher percentage of college enrollees enrolled in their birth state will increase the percentage of college attendees (both graduates and non-graduates) who live in their birth state later in life.[12]

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[8] More specifically, for each birth state and birth year cohort, $PctInState_{sc}$ is computed as the number of college students enrolled in their birth state divided by the number of college students in the cohort enrolled anywhere in the U.S.

[9] Instructions can be found at https://usa.ipums.org/usa/voliii/tEnumForm.shtml.

[10] The percentage of all college students who are ages 18-22 varies over time due to demographic and educational trends. It was a little more than 50 percent in all three census years from 1980-2000.

[11] The sample includes three census years, five year-of-birth cohorts per census year, and 50 states.

[12] The dependent variable is measured based on educational attainment at adulthood. Some people first attend college after age 22; such people will affect the dependent variable but not the main explanatory variable of interest. This introduces some degree of measurement error in the latter to the extent that it is intended to measure the college location decisions of all college attendees in the cohort. The instrumental variables approach discussed below is intended to provide consistent local average treatment effect (LATE) estimates for traditional students.

Notice that the dependent variable is observed in the 2006-2014 ACS at different lengths of time post-college for the cohorts included in the study. The earliest cohorts have their college enrollment location observed at ages 18-22 in the 1980 census, while the most recent cohorts have their college enrollment location observed at ages 18-22 in the 2000 census. In a given ACS year, the oldest cohorts are about 20 years older than the youngest cohorts. The control variables including region×year-of-birth fixed effects and age dummies generally account for broad differences in birth-state location due to the amount of time since college attendance. Recall that birth-state fixed effects account for time-invariant differences across states, i.e., their inclusion subtracts out the time-invariant factors for each state, so that identifying variation comes from cohort differences relative to their birth-state's mean over time. Also including region×year-of-birth fixed effects means that identifying variation is further driven by comparing cohorts (net of birth-state fixed effects) relative to cohorts born in the same year and census region. Any aggregate differences at the region×year-of-birth level are netted out. Age dummies account for the fact that a given cohort is observed multiple times across the ACS years and will account for differences as cohorts age. I will later consider some additional model specifications to check the sensitivity of the main results.

The ACS and decennial census microdata are independently drawn samples, so persons in the census samples are generally not the same as those in the ACS samples.[13] Thus, the analysis is based on a synthetic panel of cohort-level means from repeated cross-sections (Deaton 1985). This approach assumes that relative cohort size and composition are stable over time. Using the native population and defining cohorts by birth-state should minimize any effects of migration on cohort size and composition. The oldest cohorts in my sample reach a

[13] Independently drawn samples imply that roughly five percent of the ACS sample is also included in the census sample, but we cannot link persons across surveys to identify which observations appear in both the ACS and census microdata.

maximum age of 56 during the ACS years examined, so differential mortality is assumed to be a minimal concern.[14] Differences in individual endowments and preferences within cohorts are largely removed by cohort level averages. However, some differences in unobservables could still exist across cohorts within states. Therefore, in addition to birth state, birth year, survey year and age fixed effects, I also include several time-varying state control variables intended to account for differences in labor market conditions and home-state attachment across cohorts within states that might be correlated with both the dependent variable and the main explanatory variable.

From the 2006-2014 ACS, I compute the percentages of college attendees in the cohort that are female, Black, Asian, Hispanic, and other non-white and include these as regression control variables. The baseline specification also includes control variables from other sources. To account for state economic conditions around the time of initial college choices, I control for the log real personal income per capita and the log Housing Price Index the year a cohort is age 18; personal income data come from the Bureau of Economic Analysis and the Housing Price Index is from the Federal Housing Finance Agency.[15] I also include the log of the cohort size at age 18 obtained from Census Bureau annual population estimates. I control for possible effects of state merit-based financial aid programs by including a merit aid dummy variable based on Sjoquist and Winters (2014). This merit dummy variable equals one for cohorts born in states that implemented a merit scholarship program before or during the year the cohort was age 18.[16]

[14] In results not shown but available from the author by request, I tested the relationship between log cohort size in the ACS and the decennial censuses. An IV estimator accounting for measurement error indicates a proportional relationship. I also included the flagship tuition instrument discussed below and found no significant relationship with ACS cohort size conditional on census cohort size.

[15] Lovenheim and Reynolds (2013) indicate that family housing wealth shifts some students, especially from lower income families, toward public flagship university enrollment and away from community college enrollment.

[16] The merit dummy equals zero for cohorts in states with no merit aid program and for cohorts in states that adopted a merit aid program after they had passed age 18. This reflects typical eligibility rules for state merit aid programs. Eligibility was restricted to students completing high school after the program was adopted. Students graduating

I also include three state-level variables measured at age 22 to control for labor market conditions around the time the cohort was finishing college; these include the state unemployment rate from the Bureau of Labor Statistics (BLS), the log of median household income computed from the Current Population Survey (CPS), and the return to a bachelor's degree computed from the CPS as the regression adjusted log wage differential between adults (ages 25-54) with a college degree and those with only a high school diploma.[17]

I also add some additional explanatory variables from the ACS and censuses to estimate denser specifications. This first is the percentage of a cohort's non-college attendees (persons who never attended any college) who live in their birth state during the 2006-2014 ACS to account for changing birth-state attachment across cohorts within states. I next include ACS controls for the percentage of the cohort with at least some college, the percentage of the cohort with at least a bachelor's degree, and the percentage of the cohort with a graduate degree. Finally, I also use the 1980-2000 censuses to compute the percentage of the entire cohort enrolled in college during ages 18-22. These additional birth-state explanatory variables could all be viewed as outcomes or mechanisms to some extent, so I estimate results with and without them, but my preferred results include them.

Summary statistics for non-dummy variables are reported in Table 1. All statistics and regression results reported in this study utilize cohort weights, computed as the sum of ACS survey weights for individual observations in the cohort. This makes estimates nationally representative for sampled cohorts and increases precision by giving more weight to larger

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before program adoption were not eligible. Eligibility was also restricted to students graduating from an in-state high school, attending an in-state college or university, and meeting some academic merit requirement based on high school grade point average or standardized test scores. See Sjoquist and Winters (2014) for more details.

[17] One could also consider controlling for these variables at age 18, but publicly available CPS data do not identify all individual states until 1977, so age 18 CPS variables are not available for some of the earliest cohorts included in this study.

cohorts. The dependent variable's mean is 0.59 indicating that roughly 59 percent of college attendees live in their birth state averaged across cohorts in the analytical sample. The mean for the main explanatory variable of interest is 0.68, meaning that about 68 percent of college enrollees in the sampled cohorts attended college in their birth-state.

Of course, many college enrollees leave their birth state even before college. To provide additional background, I separately computed the percentage of young people ages 14-17 in the 1980-2000 decennial censuses residing in their birth state. Averaged over 1980-2000, 78.6 percent of people ages 14-17 reside in their birth state.[18] These are not the same cohorts as in the main analysis, but this gives a rough sense of the percentage of young people that attend high school in their birth state. Some people leave their birth state before college, some leave during college, and some leave after college. Of course, some people move back at various points, but the cumulative outflow appears to increase as a cohort ages.

Appendix Table A further illustrates the main data by reporting the analytical sample means by birth-state for the birth-state college enrollment rates in 1980, 1990, and 2000 and the means for the birth-state residence rate for adult college attendees in 2006-2014. As expected, these variables vary significantly across states, e.g., more populous states tend to have higher birth-state retention; the empirical analysis accounts for persistent differences via state fixed effects. There are also meaningful differences in birth-state enrollment rates across cohorts within states that provide the needed variation for the analysis below.[19] The variation likely depends in part on higher education policies such as tuition levels and financial aid programs.

[18] Examining this separately by age and year reveals a slight decrease with age and a slight increase across census years. The percentages also differ by state, following a similar pattern as Appendix Table A below.

[19] Regression results utilize variation by birth-state and birth-year and therefore utilize more variation, e.g., across cohorts ages 18-22 within a census year, than that illustrated in Appendix Table A.

Even after the large set of controls that are included, OLS estimates could still be biased and inconsistent. For one, there could still be unobserved differences in home-state attachment across cohorts within states even after including the detailed set of controls. Unobservable attachments could increase the share of the cohort staying in the birth-state for college and after college, inducing a positive bias in OLS estimates. Another important concern is that the main explanatory variable of interest is subject to measurement error due to sampling since it is computed based on a five percent sample of the U.S. population. Appendix Table B reports summary statistics for college enrollee observation counts per year-of-birth cohort for each birth-state for the relevant cohorts in the 1980-2000 censuses. Appendix Table B shows that some low population states like Alaska, Wyoming, Vermont, Nevada, and Delaware have on average fewer than 200 college enrollees per birth-year cohort. Sample means for small cohorts will be especially prone to measurement error. Classical measurement error will attenuate OLS coefficients toward zero, and the large set of control variables, including many fixed effects, is expected to exacerbate measurement error bias from small sub-samples (Devereux 2007a, b).[20] Measuring the explanatory variable of interest as a sample mean also biases OLS standard errors.[21] To account for these concerns, my preferred specifications utilize instrumental variables (IV) to estimate two stage least squares (2SLS) regressions.[22]

[20] Devereux (2007a) indicates that sizable bias can exist for OLS estimates of synthetic cohort models even with 200 observations per group when cohort and year fixed effects are included in his analysis. The exact number of observations per group needed to eliminate meaningful bias depends on the application, but Devereux (2007a) indicates that thousands of observations per group may be necessary in his setting. My analysis includes an even larger set of fixed effects including state-of-birth and region×year-of-birth.

[21] Murphy and Topel (1985) suggest a correction approach for standard errors in two-step econometric models, in which an explanatory variable in the second step is predicted in a first step. A simple application of their approach does not account for bias in OLS coefficient estimates. 2SLS standard errors estimated by Stata (and most other econometric software) account for the fact that the second-stage explanatory variable of interest is constructed from parameters estimated in the first stage. Hardin (2002) finds that the robust sandwich estimator of variance is a reasonable alternative to the Murphy-Topel estimator in many settings. All standard errors in the current study are clustered by state of birth estimated via a robust sandwich estimator.

[22] If measurement error were the only source of bias, an errors-in-variables regression model might be a reasonable alternative (Deaton 1985; Devereux 2007b). However, errors-in-variables estimation does not account for potential

I instrument for the percentage of a cohort's college attendees enrolled in their birth state using in-state tuition rates at public flagship universities in their birth state. I follow Rizzo and Ehrenberg (2004) and Winters (2012) and define flagship universities to include the 85 public institutions classified by the Carnegie Foundation in 1994 as Research Universities (I or II) and the top public institution in each state without a Carnegie designated Research University. The tuition data were obtained from the National Science Foundation's WebCASPAR database and based on data collected by the National Center for Education Statistics (NCES) Higher Education General Information Survey (HEGIS) and Integrated Postsecondary Education Data System (IPEDS). Specifically, I compute the log of mean in-state undergraduate tuition and fees (in real $1000s) at flagship universities for each state. I match this to cohort birth states and use in-state tuition at age 17 as my preferred instrument. I call this the tuition instrument.

The expectation is that higher flagship in-state tuition will reduce in-state enrollment, consistent with the law of demand. My instrument focuses on flagship institutions because students making marginal decisions to enroll in-state or out-of-state are likely choosing among higher quality options and tuition rates at less prestigious options are likely less important. However, results are qualitatively robust to using log mean in-state tuition at all bachelor's degree granting colleges and universities in one's birth state for the instrument. Age 17 is chosen for the tuition instrument because it is likely the most salient and exogenous information for college attendees choosing to attend in-state or not. Tuition at ages 18 or older is often not known when making initial college decisions, especially for institutions that don't finalize tuition

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positive bias due to unobservable differences in birth-state attachment across cohorts. Errors-in-variables estimation is also complicated by my setting that uses data for a large number of groups across several surveys. Devereux (2007b) shows that the traditional error-in-variables estimator has considerable finite sample bias that is exacerbated with large numbers of cohort and year fixed effects even with relatively large samples. Devereux (2007b) proposes an errors-in-variables estimator that is approximately unbiased and applies his estimator to a labor supply model with contemporaneous data on wages and hours worked.

rates until just before the start of the fall semester. Tuition at older ages could also be influenced by a cohort's demand for in-state education, inducing possible endogeneity in the instrument. Results below are qualitatively robust to using in-state tuition at age 18 or age 16 as the instrument.

A valid instrument should be both relevant and exogenous. The first condition requires that the instrument be strongly correlated with the potentially endogenous explanatory variable. I offer evidence on this below. The exogeneity condition requires that the instrument be uncorrelated with the error term in the second-stage equation, i.e., it should not affect later life birth-state residence except through its effect on in-state college enrollment. I cannot empirically test this with only one valid instrument, but I argue that the exogeneity condition is likely satisfied after conditioning on the full set of control variables. Tuition rate changes within states over time are likely driven by as good as random variation in public policies, the political process, and other idiosyncratic factors that are otherwise uncorrelated with future location decisions.

More specifically, in-state tuition at flagship universities depends on numerous factors including state and university resources, instructional costs, student demand, the tuition policy environment, political preferences, past tuition levels, and interactions among these factors (McLendon, Hearn, and Mokher 2009; McLendon, Mokher, and Doyle 2009; Calhoun and Kamerschen 2010; Bell et al. 2011; Deming and Walters 2017; Li 2017). Some of these factors are largely fixed over time; state fixed effects will account for persistent differences across states and require identifying variation to come across cohorts within states. Some factors increase tuition over time for all institutions at a roughly similar rate and will be accounted for by region×year-of-birth dummies. Still, some factors related to tuition vary across cohorts within a state in differential ways across states in a region. Changes in resources for higher education largely depend on state business cycles and long run income growth; economic downturns are one of the most important causes of tuition increases (Deming and Walters 2017; Li 2017). Changes in instructional costs over time within states largely reflect increased compensation and living costs for workers. The demand for flagship attendance depends on macroeconomic conditions, institutional quality, and local amenities. The tuition policy environment varies across states, from very centralized to very decentralized, but these differences are largely fixed over time.[23] Political preferences for higher education evolve over time in complicated ways including party affiliation, gubernatorial power, unified government, lobbying influence, and where an elected official attended school (McLendon, Hearn, and Mokher 2009; McLendon, Mokher, and Doyle 2009; Li 2017). Tuition levels depend on past tuition rates both because nominal decreases are rare and because states occasionally limit nominal tuition increases (Deming and Walters 2017).

In addition to state fixed effects and region×year-of-birth effects, I also include control variables for time-varying economic conditions the year a cohort is age 18 including the log real personal income per capita, the log Housing Price Index, and the log cohort size. These variables will largely capture overall time-varying effects of business cycles, long run income growth, instructional costs, and student demand on tuition levels. I intentionally do not control for political variables because they are very numerous and believed to be exogenous to later life location decisions after conditioning on the control variables. The flagship tuition instrument is powered by these time-varying political factors, their interactions with state economic conditions, and semi-arbitrary decisions that result. One could alternatively include a large

[23] In particular, the final decision-making authority for tuition decisions at flagship universities varies by states. Some states allow universities considerable autonomy, while in others the state legislature or a statewide governing board has final say (Lenth 1993; Calhoun and Kamerschen 2010; Bell et al. 2011)

number of political variables as excluded instruments, but they are likely to be individually weak instruments, and using a large number of weak instruments is undesirable (Angrist and Pischke 2009). My preferred approach is to use a single strong instrument, log in-state tuition at public flagship universities.[24]

One possible concern is that the tuition instrument could directly affect education attainment levels, and thus alter the sample of those with any college or alter migration propensities through effects of education levels. I account for this by controlling for education levels in the full specification. However, I also present reduced form results below suggesting that in-state flagship tuition does not affect adult educational attainment levels.[25] The reduced form effects on education levels also serve somewhat as a falsification test. If in-state flagship tuition was correlated with the supply or demand for education across cohorts in a state, we would expect to see it correlated with educational attainment levels, but we do not, which lends support to the exogeneity of in-state flagship tuition as an instrument for birth-state college enrollment.[26]

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[24] Another related possibility might be to use some measure of state appropriations to higher education institutions to instrument for birth-state enrollment. However, appropriations in many states depend on enrollment levels, so these are potentially endogenous to student enrollment decisions.

[25] Another possible concern is that changes in flagship tuition might be related to academic quality. Unfortunately, time-varying student quality is hard to measure, especially for the early years in this study. Carnegie classifications and various ranking guides are useful for cross-sectional comparisons, but changes in these over time may depend more on changes in ranking formulas and measurement error than actual changes in quality. IPEDS did not begin collecting institution-level data on student test scores until 2001, and I know of no other publicly available dataset providing such information. With that in mind, I examined the relationship between 2001-2015 changes in flagship university test scores and in-state tuition and found no significant relationship. Thus, student quality changes appear unrelated to the flagship tuition instrument.

[26] I also considered using alternative instruments such as supply-side factors found to be linked to college attainment by Bound and Turner (2007) and Bound, Lovenheim, and Turner (2012). However, factors that primarily affect college attainment are likely not ideal instruments for my primary question of interest, even with controls for adult educational attainment levels, because those induced to increase attainment may have differing geographic attachments and mobility propensities from those already likely to attend college. The in-state flagship tuition instrument that I use is particularly well-suited to my setting because it is expected to primarily affect in-state enrollment.

IV coefficient estimates are interpreted as local average treatment effects (LATE), which may differ from the average treatment effect (ATE) with heterogeneous effects. College students have heterogeneous abilities and attachments to their home states, and the treatment effect from birth-state college enrollment may vary across students. Students induced to enroll in their birth-state by in-state flagship tuition may be affected by birth-state enrollment differently than students not affected by the instrument. For example, some students who are very wealthy or very high ability may be especially likely to attend out-of-state elite private colleges and universities regardless of in-state tuition levels; they may also be especially likely to reside outside the state after college regardless of where they attend. Similarly, flagship universities are typically the most selective public higher education institutions in their states (Rizzo and Ehrenberg 2004).[27] Many students lack the pre-college skills to gain admission to their states' flagship universities and are, therefore, not influenced by flagship in-state tuition. The IV approach estimates a local effect based on those potentially affected by the instrument, i.e., students qualified for flagship attendance, considering it as an option, and at the margin of attending college in-state or out-of-state. This is certainly an important group to study. Students enrolled at public flagship universities have been shown to earn above average incomes after college, which may represent some combination of selection and treatment effects (Hoekstra 2009). Retaining high earning potential students is especially important to states because they later pay higher taxes and may have labor market spillover effects that increase job opportunities, incomes, and quality of life for other workers (Moretti 2004, 2013; Shapiro 2006; Winters 2011a, 2013, 2014).

[27] Flagship universities on average also have higher in-state tuition, out-of-state tuition, and shares of out-of-state students than non-flagships (Winters 2012).

IV. Empirical Results

A. OLS Estimates

OLS regression results for the main explanatory variable of interest are reported in Table 2. The first column reports results that include birth state, region×year-of-birth, survey year and age fixed effects, along with ACS demographic variables and other baseline explanatory variables. The second column adds the ACS percentage of non-college attendees who reside in their birth state, and the third column adds the rest of the additional birth state explanatory variables (Census college enrollment rates and ACS educational attainment rates).

The OLS results in all three columns suggest that the percentage of a cohort's college enrollees enrolled in their birth state significantly increases the proportion of college attendees living in their birth state later in life. Adding the additional controls alters the coefficient only slightly. Results with the full set of control variables in column 3 suggest that a one percentage point increase in birth state college enrollment increases later life birth-state residence by 0.168 percentage points.

B. Main 2SLS Estimates

The main 2SLS results are presented in Table 3. The three columns again add progressively more controls as in Table 2. As expected, the instrument has a statistically significant negative effect in the first stage for each of the columns. To assess the strength of the instrument, I first report the excluded instrument F-statistics from the first stage. Stock, Yogo and Wright (2002) and Angrist and Pischke (2009) suggest that a first-stage F-statistic greater than 10 indicates that weak instrument bias is likely a minimal concern. The first-stage F-statistics for the 2SLS regressions in Table 3 all exceed 10, so I can confidently rule out concerns that the instrument is weak. I also report the underidentification and weak identification statistics suggested by Kleibergen and Paap (2006). The underidentification p-values are all less than 0.01, and the weak identification statistics exceed the critical value of 16.38 for 10% maximum IV size, which again indicates that weak instrument issues are not concerns.

The 2SLS results in Table 3 indicate that the percentage of a cohort's college enrollees enrolled in their birth state has a statistically significant positive effect on the percentage of college attendees living in their birth state later in life. Like the OLS estimates, adding the additional control variables in columns 2 and 3 only minimally affects the 2SLS coefficient estimates. The 2SLS coefficient estimates are meaningfully larger than their OLS counterparts, possibly due to measurement error bias in the latter exacerbated by the large set of control variables including fixed effects. The differences between the 2SLS coefficient estimates in Table 3 and corresponding estimates in Table 2 are statistically significant at the ten percent level.

The results with the full set of controls in column 3 suggest that a one percentage point increase in the birth-state enrollment rate among college attendees increases later-life home state residence for college attendees by 0.41 percentage points.[28] This suggests that if changes in higher education policies induced 100 additional students to enroll in-state who would have previously enrolled out-of-state, roughly 41 of these additional students would still be in the state later in life. This is a relatively large effect that is four times as large as the effect estimated by Groen (2004) using different data and methods. However, it should also be noted that the first-stage results suggest modest effects of flagship in-state tuition on birth-state enrollment. Lowering in-state tuition at public flagship universities by 10 percent would only increase birth-

[28] Appendix Table C provides results for the birth-state-year cohort control variables from the full specifications in column 3 of Tables 2 and 3.

state enrollment by about four tenths of one percentage point. Thus, keeping young people in-state for college does affect their post-college location decisions, but specific policies to do so may not be cost-effective.

C. Alternative Instruments

The 2SLS coefficient estimates in Table 3 are a good bit larger than OLS estimates in Table 2. I suggest that this is likely due to attenuation bias due to measurement error from sampling in the OLS estimates, but some readers may be surprised that attenuation bias is so large. To address this, I next estimate 2SLS regressions using alternative instruments and the full set of control variables with results in Table 4.[29]

The 1980-2000 decennial census data used above come from the 5% state samples, but for these years the U.S. Census Bureau also released separate 1% metro samples. The census 5% and 1% samples have the same underlying sampling structure, but they provide slightly different information useful for different purposes; see Ruggles et al. (2015) for more details. One major difference is identifying small geographic areas. The 5% samples always identify state of residence but often do not identify metropolitan area for small areas in order to preserve confidentiality. The 1% metro samples in 1980 and 1990 suppress state of residence for some observations in order to identify some small metropolitan areas and still preserve confidentiality. The 5% samples are clearly preferable for my main analysis because they include state of residence and have much larger samples.

I use the 1980-2000 1% decennial census samples to compute for each birth-state and birth-year cohort the percentage of college enrollees who are enrolled in their birth state and use

[29] Results with partial sets of control variables as in Tables 2 and 3 are similar and available by request.

this as an alternative instrument.[30] I.e., I use birth-state college enrollment rates in the 1% samples as an instrument for birth-state college enrollment rates in the 5% samples. Because the 5% and 1% samples are independently drawn random samples of the population, sampling error should be independent across the two, and this IV strategy should account for measurement error bias. However, this IV does not account for potential omitted variables that affect the changing desirability of or attachment to a birth state. 2SLS results using this instrument with the full set of control variables included are in the first column of Table 4. The first-stage is very strong as expected, and the second-stage gives a coefficient estimate of 0.444 that is statistically significant at the one percent level. This is larger than the OLS estimates in Table 2 and moderately larger than the preferred 2SLS estimate in Table 3, but the difference relative to Table 3 is not statistically significant at the ten percent level.

My second alternative IV approach is to use the one-cohort-year lag of the birth-state college enrollment rate (from the 5% sample). Results are in column 2 of Table 4. If the sampling error is independent across cohorts, using the one-cohort-year lag as an instrument will account for bias due to measurement error. However, it may not account for omitted variable bias from factors correlated with both birth-state college enrollment and later life birth-state residence. Additionally, the birth-state enrollment rate is only measured for cohorts ages 18-22 in the 1980-2000 decennial censuses, so the lag is not measured for cohorts age 22 during the census year, meaning that the lagged IV approach loses one fifth of the total observations and reduces the number from 6750 to 5400.[31]

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[30] In constructing this instrument, I exclude observations with suppressed state of residence; this eliminates less than three percent of observations in 1980, less than two percent in 1990, and no observations in 2000.

[31] Cohort years are measured based on year of birth, so the one-cohort-year lag instrument is measured using the cohort that was born in the same state the year before a given cohort but observed during the same census year. Cohorts age 22 in a census year are excluded because the cohorts one year older than them are age 23 during the same census year, an age at which many people historically have finished attending college and moved on to post-college endeavors. The few still in college are unlikely to be representative of the broader cohort.

The first-stage results in column 2 of Table 4 show that the one-cohort-year lag is indeed a strong predictor of the birth-state college enrollment rate for the following cohort. The second-stage coefficient is 0.450 and statistically significant at the one percent level. This exceeds coefficient estimates in both Table 2 and 3, but the difference relative to Table 3 is not statistically significant at the ten percent level. The estimates in columns 1 and 2 of Table 4 are very similar. Both are potentially subject to omitted variable bias, so they are not the preferred estimates. However, their value is that they support the contention that OLS estimates in Table 2 are likely attenuated due to measurement error bias.[32] Thus, finding 2SLS estimates with the preferred tuition IV in Table 3 that exceed OLS estimates is plausible and not unexpected.

I also explored using the merit aid variable as an instrument with results in column 3 of Table 4. Previous literature has found reduced form effects indicating that being exposed to a state merit aid program increases the probability of later life residence in one's birth state and suggested that this may be partially driven by the effect of merit aid on birth-state college enrollment (Hickman 2009, Hawley and Rork 2013; Sjoquist and Winters 2014; Fitzpatrick and Jones 2016). However, merit aid may have a wide variety of other effects, some of which may influence future geographic mobility. The analytical sample for the current study is limited to cohorts at least age 18 by year 2000 yielding fewer merit states and fewer merit-exposed cohorts than these previous studies, so results are not directly comparable but still of interest.

The first-stage F-Statistic in column 3 is slightly less than 10, and the Kleibergen-Paap test statistics suggest that weak instrument concerns are not substantial.[33] The second-stage coefficient estimate of 0.674 in column 3 is significant at the one percent level and larger than

[32] Of course, column 2 of Table 4 also includes fewer cohorts than the OLS estimates. Estimating the full OLS specification for the same cohorts as in Table 4 gives a coefficient of 0.215, so the difference is not primarily due to the differing cohorts included.

[33] The Kleibergen-Paap weak identification statistic does not reject 10% maximal IV size, but it does reject 15% max IV size.

any corresponding estimate in Tables 2-4, though not statistically different at the ten percent level than the preferred estimate of 0.408 from Table 3. One plausible interpretation is that state merit aid programs may affect post-college location decisions through mechanisms other than their effect on in-state enrollment. For example, Sjoquist and Winters (2015b) find that merit aid programs reduce the percentage of college graduates majoring in science, technology, engineering, and mathematics (STEM) fields, which may result from students switching to majors for which it is easier to maintain the required GPA to renew the scholarship. The college major chosen likely affects future labor market opportunities and migration propensities (Winters 2017b). Other effects of merit aid could matter as well. Additionally, merit aid program characteristics differ across states in numerous ways, and merit program effects on in-state enrollment and later life residence likely differ across states also (Sjoquist and Winters 2014). Thus, while using merit aid as an instrument yields directionally consistent estimates as before, there is some reason to doubt this IV approach. The preferred results are those that use the flagship tuition instrument.

D. Other Related Outcomes

I next consider some additional outcomes related to the main dependent variable. Table 5 presents results for birth-state enrollment effects on later life birth-state residence separately for college graduates and non-graduating college attendees in panels A and B, respectively. The results thus far use a dependent variable that pools all college attendees, but graduates and non-graduating attendees might behave somewhat differently. Table 5 dependent variables in panels A and B are the share of college graduates (A) and non-graduating attendees (B) who live in their birth state in the 2006-2014 ACS. Results are reported for OLS and for 2SLS using the tuition instrument in columns 1 and 2, respectively. For brevity, the 2SLS results only report the F-statistics for the first-stage; other first-stage statistics are very similar to Table 3. Both OLS and 2SLS results weakly suggest that the response to birth-state enrollment is stronger for non-graduating attendees, but the differences are not statistically significant at the ten percent level. The 2SLS coefficient for graduates (0.286) is not statistically significant at the five percent level, but it is significant at the ten percent level and the magnitude is larger than the OLS estimate for graduates (0.134), which is statistically significant at the one percent level. The imprecision in 2SLS estimates in Table 5 prevents strong conclusions, but it appears that the effect of birth-state enrollment is likely still meaningfully large for both graduates and non-graduates, with the effect perhaps smaller for college graduates. However, a major limitation to Table 5 is that the birth-state enrollment rate is measured for the full cohort of college enrollees and we cannot necessarily assume that those who eventually graduate and those who do not graduate have the same pattern of birth-state enrollment. Thus, Table 5 results are suggestive but far from conclusive.

I next return to an issue mentioned at the end of Section III that the tuition instrument might affect education levels and somehow distort the main results. Table 6 reports results from reduced form OLS regressions that use three cohort level education outcomes as the dependent variable and examines effects of the in-state flagship tuition variable on these. The educational outcome dependent variables are the shares of the cohort with some college or higher, a bachelor’s degree or higher, and a master’s degree or higher. The controls are the same as in column 1 of Tables 2-3, except that the demographic shares are measured for persons of all education levels and not conditional on college attendance; however, using the same demographic controls as Tables 2-3 does not alter results. The coefficient estimates in Table 6 are small and not statistically significant at conventional levels. Furthermore, the associated confidence intervals are relatively narrow, so the estimates can be interpreted as somewhat precisely estimated zeroes. In other words, I can reject nulls of relatively modest magnitude effects. Thus, results in Table 6 suggest that in-state flagship tuition does not affect these educational attainment outcomes. This, of course, does not mean that education levels are completely unrelated to all tuition levels, but they do appear unrelated to in-state flagship tuition levels. Students whose educational attainment is especially sensitive to the financial costs of higher education are likely more sensitive to tuition levels at less prestigious and lower cost public institutions. However, in-state flagship tuition likely plays an especially important role for students deciding whether to study in-state or out-of-state and is thus a useful instrument for birth-state college enrollment.

Results in Table 6 are useful for multiple reasons. First, finding no meaningful effect of the in-state flagship tuition variable on adult education levels reduces concern that the instrument might affect factors affecting adult location decisions other than just birth-state college enrollment. Second, the finding that the instrument is uncorrelated with adult education levels also reduces concerns that flagship tuition rates might be directly affected by supply or demand for higher education across cohorts within states. As noted above, this serves somewhat as a falsification test. In-state flagship tuition significantly affects what we expect it to (birth-state enrollment) and does not affect something else (educational attainment levels) that it might if it were driven by endogenous factors. Thus, my use of the flagship tuition instrument appears to isolate exogenous variation in birth-state enrollment rates that can be used to estimate causal effects of birth-state enrollment on later life birth-state residence.

Table 7 reports a robustness check in which the dependent variable is the share of the year-state-age cohort that currently resides in their birth state AND has attended at least some college. Thus, instead of measuring birth-state location conditional on college attendance as done for the main dependent variable in this study, the dependent variable in Table 7 measures the joint outcome of birth-state location as an adult and having attended at least some college. The explanatory variable of interest for Table 7 is again the share of college enrollees enrolled in their birth state, measured from the 1980-2000 decennial censuses. We have already seen from Table 6 that the in-state flagship tuition instrument has no meaningful effect on educational attainment. However, there might possibly be a complex relationship between educational attainment and the other main variables in this study that complicates the main analysis and interpretations. Thus, it is worth checking if the main results are qualitatively robust to this alternative dependent variable.

Column 1 of Table 7 presents OLS results similar to column 1 of Table 2. Columns 2 and 3 of Table 7 present 2SLS results using the flagship tuition instrument similar to columns 1 and 2 of Table 3. I do not include the ACS or Census education control variables because these are jointly determined with the educational portion of the dependent variable in Table 7. First-stage results for Table 7 are very similar to Table 3 and are omitted to conserve space. It should be noted that the alternative dependent variable in Table 7 has a mean of 0.37, roughly equal to the product of means for the primary dependent variable in this study (0.59) and the cohort share with some college or higher (0.64), subject to some rounding error. Consequently, I expect the coefficients in Table 7 to be somewhat smaller than corresponding coefficients in Table 2 and 3. The coefficient estimates are positive and statistically significant at the one percent level in all three columns of Table 7. The OLS coefficient in Table 7 is 0.101, which is about 60 percent as large as the corresponding estimate in Table 2. The Table 7 columns 2 and 3 coefficients are 0.308 and 0.309, respectively, which are about 80 percent as large as corresponding estimates in Table 3. However, interpreting the results for the joint outcome variable in Table 7 is a bit complicated and not the intention of the exercise. The goal here is to confirm that the positive effect for the preferred dependent variable in Table 3 is not wiped away by using this joint outcome. The relative insensitivity here strengthens confidence that conditioning on college attendance in the main dependent variable is not distorting the analysis and interpretations.

I also looked at additional outcomes related to marriage. Staying in one's birth-state for college is expected to increase the likelihood of marrying someone from the same birth state, since many relationships leading to marriage start in college and staying in-state for college will expose someone to more potential spouses from the same birth-state. Staying in one's birth-state after college might also increase the likelihood of marrying someone from the same birth state. Of course, causality could also go in the other direction. Meeting a future marriage partner before or during college could affect college and post-college location decisions. Table 8 takes a brief look at the effects of in-state college enrollment on marriage outcomes via 2SLS regressions with the flagship tuition instrument and the full set of controls. The three dependent variables in Table 8 are constructed using the 2006-2014 ACS as cohort percentages for college attendees by birth state, birth year, and ACS year similar to the main dependent variable. The specifications in Table 8 are identical to Table 3 column 3 except for the dependent variables; first-stage results are the same as in Table 3 column 3.

Column 1 examines the percentage of the cohort who are married during the ACS survey as the dependent variable. Column 2 has a dependent variable defined as the percentage of the cohort that is married and to someone from the same birth state. The column 3 dependent variable is the cohort percentage that is married and to a college attendee from the same birth state. Thus, the second and third column dependent variables are joint outcomes (and not conditional on being married at all). The second and third outcomes differ in that marrying a non-college attendee is counted in column 2 but not column 3.

The coefficient estimate in column 1 is positive but not statistically significant at conventional levels. The coefficient estimates of 0.449 and 0.462 in columns 2 and 3, respectively, are both positive and significant at the one percent level. Thus, there is no clear effect of birth-state enrollment on the overall probability of marriage, but there is a strong effect of birth-state enrollment on marrying someone from the same birth-state, especially a college attendee from the same birth state. This is an interesting finding in itself in helping understand how marriages form. It also is consistent with the network formation story as a possible mechanism for why birth-state college enrollment affects later-life birth-state residence. Specifically, birth-state college enrollment likely affects one's social network and strengthens ties to other people from the same state including potential marriage partners. This likely increases the desirability of remaining in the birth-state after college. Of course, this does not rule out other possible mechanisms. We also cannot be too precise about the timing for the possible mechanism. Some people might choose to stay in their birth-state after college before beginning a relationship with their future spouse. Thus, the results in Table 8 are consistent with the network formation mechanism, but we cannot definitively assess the mechanism's importance.

E. Additional Sensitivity Analysis

I also conducted additional analysis to examine the sensitivity of my main results to alternative specifications. Results for these are not reported but are available from the author by request. I first considered the possible effect of the main variables from the censuses and ACS being especially noisily estimated for small population states due to sampling error. I conducted a simple robustness check by excluding the 12 states with year 1980 population less than one million people. This had no meaningful effect on the coefficient of interest. The 2SLS specification with the flagship tuition instrument and the full set of controls yield a coefficient of 0.416 that is statistically significant at the one percent level. Further excluding the additional five states with 1980 population less than two million yielded a significant 2SLS coefficient of 0.421. These estimates are very similar to the corresponding coefficient estimate of 0.408 in column 3 of Table 3.

I next consider the possibility that my empirical approach might be adversely affected by age patterns of birth-state out-migration rates that differ across states. Recall that the dependent variable for all cohorts is observed during 2006-2014, but the earliest cohorts are about 20 years older than the youngest cohorts, so that the amount of time passed since college varies across cohorts. Descriptive evidence indicates that cumulative birth-state out-migration increases as cohorts age. Control variable dummies for region×year-of-birth and age will adequately account for aggregate effects due to cohort aging. However, there might be heterogeneous age patterns in out-migration. In particular, some states have consistently low percentages of their natives residing in the state during and after college. Birth-state-year cohorts that start off with initially low birth-state residence percentages may experience especially modest decreases as the cohort ages because the starting level was so low.

To address these concerns, I first re-estimated the main 2SLS results excluding states with especially low levels of birth-state college enrollment. Specifically, I excluded the seven states in Appendix Table A in which less than half of 18-22 year old college students from the state in 1980 were enrolled in their birth-state.[34] The 2SLS specification with the flagship instrument and the full set of controls produced a coefficient estimate of 0.450 that is statistically different from zero at the one percent level, but quite similar to the preferred estimate of 0.408 in Table 3.

I next tested a log-log specification as another alternative. In my main specification, both the dependent variable and explanatory variable of interest are measured as cohort-level population shares and a linear relationship is assumed. A reasonable alternative to consider is to measure both the dependent variable and explanatory variable of interest as logs of cohort-level population shares and model that relationship linearly with 2SLS using the flagship tuition instrument and the same control variables as column 3 of Table 3.[35] Doing this yields a coefficient of 0.505 that is significant at the one percent level. Thus, the coefficient is a little larger, but the log-log specification has a slightly different interpretation that is somewhat less intuitive. I prefer the primary specification a priori. Still, it is useful to know that the main qualitative result is not significantly affected by using a log-log specification.

F. Reconciling Results with Previous Literature

My analysis differs substantially from previous literature in terms of research question, data and identification strategy. Groen (2004) is the study most similar in research question, and

[34] Less populous states tend to have lower birth-state enrollment rates, so this is somewhat similar to excluding the smallest states discussed above.

[35] Some of the control variables are already measured in logs. I experimented with converting the other control variables to logs but some have non-trivial numbers of zeros, e.g., the population shares of minorities are zero for many cohorts, so I keep the control variables the same as in Table 3 column 3.

both studies find statistically significant positive effects, but my preferred 2SLS estimates are a good bit larger than his preferred estimates. However, the difference in results should not be especially surprising given differences in data and methods. Perhaps the most important difference is that Groen (2004) uses individual-level variation in in-state enrollment while my variables are cohort-level means. One possible implication is that there may be cohort-level network effects in both college and post-college location decisions. For example, if a higher percentage of one's high school friends stay in-state for college, it may make it easier to maintain these friendships during college and make one's home state a more attractive place post-college. Network effects may be especially important through the effect on spouses and significant others. An increase in in-state enrollment among one's cohort peers may make one more likely to find a spouse or significant other from the same home state, and this may further increase the likelihood of residing in that home state post-college. Thus, my results are correctly interpreted as cohort level effects and this may differ from the individual-level estimates of Groen (2004),

A second difference with Groen (2004) is that my measure captures enrollment in an individual's birth state, while his measure captures enrollment in the individual's state of residence during high school. These states are the same for most young people, but they do differ for some. Young people who have lived in the same state since birth may be especially attached to that state and be very likely to stay in the state after college. However, students who leave their birth state before high school may not be very strongly attached to their state of residence of during high school.

A third difference is that his preferred sample is restricted to persons applying to college in multiple states, while my main results include all college attendees but exploit variation in birth-state enrollment due to in-state flagship tuition rates. Groen's (2004) summary statistics indicate that his preferred sample has above average SAT scores and above average likelihoods of attending a private higher education institution. Students making marginal decisions about birth-state flagship enrollment may also have above average ability and mobility, but they may still differ somewhat from the marginal students in Groen (2004) and yield different LATE estimates.

A final important difference with Groen is that my data are more recent and the larger effects found may be partially attributable to declining internal migration rates in the U.S. documented in previous literature (Molloy et al. 2011; Partridge et al. 2012). As people become less mobile, where they attend college may play an even more important role in where they locate later in life. The larger 2SLS estimates using the merit aid instrument could also be partially explained by declining internal migration as nearly all of the state merit aid programs were adopted since 1991. This also suggests that the future relationship between birth-state enrollment and later life residence could grow even stronger than the preferred estimate in this study.

My analysis and results are also distinct from Sjoquist and Winters (2014), despite the use of some similar data. Sjoquist and Winters (2014) estimate significantly positive reduced form effects of state merit aid programs on birth-state residence of adults, but they are unable to strongly connect their results to in-state enrollment as a driving mechanism. The results both here and there suggest that other factors play important roles in the relationship between merit aid and post-college location decisions. The flagship in-state tuition IV strategy in the current paper is argued to provide causal estimates of the effect of birth-state college enrollment on later life birth-state residence.

V. Conclusion

This paper uses data from the 1980-2000 decennial censuses and 2006-2014 ACS to examine the effects of birth-state college enrollment on the likelihood of living in one's birth state later in life. My variables are cohort-level averages, and I include state and year fixed effects and a number of time-varying controls including cohort education levels, out-migration rates of non-college attendees, and various labor market characteristics. My preferred results use flagship in-state tuition rates to instrument for birth-state college enrollment. The IV approach is intended to provide consistent estimates of the local average treatment effect for students making marginal college location decisions affected by the instrument.

Both OLS and 2SLS results suggest that attending college in one's birth state statistically significantly increases the likelihood of residing in that state later in life. These findings are consistent with suggestions that in-state college enrollment increases one's attachment to friends, family, and objects in one's home state and increases the probability of residing in that state later in life. Results also support a broader literature suggesting that opportunity costs of moving can be substantial barriers to migration, even for college attendees. Relative to students attending college outside their birth-state, those who stay in-state are likely to develop more location-specific social capital in their home state that makes moving more costly later on.

Finding a non-zero effect of birth-state enrollment on later life birth-state residence is important for understanding college attendee location decisions, but the specific magnitude of the effect is likely even more important for policy implications. The preferred 2SLS estimates suggest that a one percentage point increase in the share of a cohort's college enrollees who are enrolled in their birth-state increases later life residence in that state by 0.408 percentage points. Equivalently, the preferred estimate suggests that state policies and practices that induce 100 additional students to attend college in their home state who would have instead attended college out-of-state would cause 41 additional college attendees from the state to reside in the state later in life. This is an economically meaningful effect, even though less than proportional.

The relatively large effect of birth-state college enrollment on later life birth-state residence found in this study suggests that policymakers may be able to help build the stock of college-educated workers in their state by creating policies that encourage young persons from the state to attend college in-state. However, it is not clear if such policies are actually cost effective for the given state. For example, tuition and financial aid policies likely affect in-state enrollment and as a result post-college residence, but they are also potentially very expensive policy levers (Groen 2011). The first-stage results for the flagship tuition instrument in this study suggest only a modest effect on birth-state enrollment. Lowering in-state tuition at public flagship universities by 10 percent would only increase birth-state enrollment by about four tenths of one percentage point. Furthermore, reducing flagship tuition does not significantly affect educational attainment levels. Similarly, while state merit aid programs do keep some students in-state for college, the proportion is small relative to the considerable costs and merit aid programs are not effective at increasing overall college participation and completion (Sjoquist and Winters 2015a; Fitzpatrick and Jones 2016). Higher education policies may also have unintended consequences that reduce their net benefits to the individual state (Sjoquist and Winters 2015b, 2016). Furthermore, policies that reduce geographic mobility may reduce national productivity and well-being, even if they have positive net benefits for an individual state.

Given the limitations of using tuition and merit aid programs, policymakers interested in building the stock of college-educated labor in their state may want to consider other potential policies as well. Facilitating stronger social networks among college students and alumni residing in the state may be an especially cost effective way to increase local attachments and encourage young people to stay in the state after college. Other possibilities include service-forgivable student loans for graduates who work in the state for a certain number of years after college, enhanced academic quality, increased resources devoted to marketing and recruiting, more robust campus amenities, internship programs with in-state employers, state need-based financial aid programs, increased access by creating new higher educational institutions in geographically remote areas and enhancing distance learning programs, and improved K-12 education to help students succeed in college. There is still a great deal that we do not know about how various policies would affect college attendance, college completion, in-state enrollment, post-college residential location decisions and other important outcomes. The current study does not answer every question or assess the efficacy of any particular higher education policy, but it does provide strong evidence that increasing the share of young people who attend college in their home state increases the share living in their home state later in life.

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Table 1: Summary Statistics

	Mean	Std. Dev.
Dependent Variable (from 2006-2014 ACS)		
% with Any College Living in Birth State	0.590	0.086
Main Explanatory Variable (from 1980-2000 Census)		
% of College Enrollees Enrolled in Birth State	0.681	0.083
Instrumental Variables		
Log Mean In-State Tuition ($1000s) of Flagship Universities at Age 17	1.287	0.411
% of College Enrollees Enrolled in Birth State for 1% PUMS	0.686	0.092
One Cohort Year Lag of Main Explanatory Variable	0.678	0.083
2006-2014 ACS Demographic Variables		
% Female for All College Attendees	0.538	0.032
% Black for All College Attendees	0.112	0.084
% Asian for All College Attendees	0.015	0.034
% Hispanic for All College Attendees	0.066	0.083
% Other Non-white for All College Attendees	0.022	0.025
Other Baseline Explanatory Variables		
Log of Personal Income Per Capita at Age 18	10.434	0.180
Log of Housing Price Index at Age 18	4.938	0.453
Log of Cohort Size at Age 18	11.683	0.870
Merit Dummy at Age 18	0.094	0.293
Unemployment Rate at Age 22	6.772	2.124
Return to Bachelor's Degree at Age 22	0.259	0.106
Log Median Household Income at Age 22	10.844	0.156
Additional Birth State Explanatory Variables		
% in 2006-2014 ACS with No College Living in Birth State	0.705	0.082
% in 1980-2000 Census Enrolled in College	0.364	0.103
% in 2006-2014 ACS with Some College or Higher	0.638	0.066
% in 2006-2014 ACS with a Bachelor's Degree or Higher	0.309	0.061
% in 2006-2014 ACS with a Graduate Degree	0.102	0.033

Notes: the full analytical sample includes 6,750 year-state-age cohort observations. All variables from the American Community Survey and decennial censuses are constructed using survey weights. All summary statistics are weighted using cohort weights computed as the sum of ACS survey weights for all college attendee individual observations in the cohort.

Table 2: OLS Estimates of Birth-State College Enrollment and Later Life Residence in Birth-State

	(1)	(2)	(3)
% of College Enrollees Enrolled in Birth State	0.174	0.165	0.168
	(0.032)***	(0.031)***	(0.033)***
Birth-State Dummies	Yes	Yes	Yes
Region*Year-of-Birth Dummies	Yes	Yes	Yes
Age Dummies	Yes	Yes	Yes
Survey Year Dummies	Yes	Yes	Yes
ACS Demographic Variables	Yes	Yes	Yes
Other Baseline Explanatory Variables	Yes	Yes	Yes
ACS % with No College Living in Birth State	No	Yes	Yes
ACS and Census Education Control Variables	No	No	Yes
Number of Observations (Year-State-Age Cohorts)	6750	6750	6750

Notes: Standard errors in parentheses are clustered by birth state. The dependent variable is the year-state-age cohort percent with any college who live in their birth state during the 2006-2014 ACS. All regressions are weighted using cohort weights computed as the sum of ACS survey weights for all college attendee individual observations in the cohort.

***Statistically significantly different from zero at the 1% level of significance.

Table 3: 2SLS Estimates for Birth-State Enrollment and Later Residence Using In-State Tuition IV

	(1)	(2)	(3)
A. First-Stage			
Log In-State Tuition Flagship Universities at Age 17	-0.042	-0.042	-0.038
	(0.010)***	(0.010)***	(0.009)***
F-Statistic	18.13	19.49	18.27
Kleibergen-Paap Underidentification Statistic	8.01	8.23	8.46
Kleibergen-Paap Underidentification P-value	0.005	0.004	0.004
Kleibergen-Paap Weak Identification Statistic	22.20	23.72	22.25
B. Second Stage			
% of College Enrollees Enrolled in Birth State	0.392	0.393	0.408
	(0.111)***	(0.105)***	(0.124)***
Birth-State Dummies	Yes	Yes	Yes
Region*Year-of-Birth Dummies	Yes	Yes	Yes
Age Dummies	Yes	Yes	Yes
Survey Year Dummies	Yes	Yes	Yes
ACS Demographic Variables	Yes	Yes	Yes
Other Baseline Explanatory Variables	Yes	Yes	Yes
ACS % with No College Living in Birth State	No	Yes	Yes
ACS and Census Education Control Variables	No	No	Yes
Number of Observations (Year-State-Age Cohorts)	6750	6750	6750

Notes: Standard errors in parentheses are clustered by birth state. The dependent variable is the year-state-age cohort percent with any college who live in their birth state during the 2006-2014 ACS. All regressions are weighted using cohort weights computed as the sum of ACS survey weights for all college attendee individual observations in the cohort.

***Statistically significantly different from zero at the 1% level of significance.

Table 4: 2SLS Estimates for Birth-State Enrollment and Later Residence Using Alternative Instruments

	(1)	(2)	(3)
A. First-Stage			
% of College Enrollees Enrolled in Birth State for 1% PUMS	0.176		
	(0.027)***		
% of College Enrollees Enrolled in Birth State for cohort c-1		0.382	
		(0.055)***	
Merit Aid Dummy			0.021
			(0.007)***
F-Statistic	41.35	48.57	9.43
Kleibergen-Paap Underidentification Statistic	28.77	20.26	7.658
Kleibergen-Paap Underidentification P-value	0.000	0.000	0.006
Kleibergen-Paap Weak Identification Statistic	50.53	59.44	11.51
B. Second Stage			
% of College Enrollees Enrolled in Birth State	0.444	0.450	0.674
	(0.068)***	(0.079)***	(0.215)***
Birth-State Dummies	Yes	Yes	Yes
Region*Year-of-Birth Dummies	Yes	Yes	Yes
Age Dummies	Yes	Yes	Yes
Survey Year Dummies	Yes	Yes	Yes
ACS Demographic Variables	Yes	Yes	Yes
Other Baseline Explanatory Variables	Yes	Yes	Yes
ACS % with No College Living in Birth State	Yes	Yes	Yes
ACS and Census Education Control Variables	Yes	Yes	Yes
Number of Observations (Year-State-Age Cohorts)	6750	5400	6750

Notes: Standard errors are clustered by birth state. The dependent variable is the year-state-age cohort percent with any college who live in their birth state during the 2006-2014 ACS. All regressions are weighted using cohort weights computed as the sum of ACS survey weights for all college attendee individual observations in the cohort.

***Statistically significantly different from zero at the 1% level of significance.

Table 5: OLS and 2SLS Estimates for Birth-State Enrollment Effects for Alternative Education Groups

	(1) OLS	(2) 2SLS Tuition IV
A. College Graduates (Bachelor's or Higher)		
% of College Enrollees Enrolled in Birth State	0.134	0.286
	(0.037)***	(0.164)*
First-stage F-Statistic	N/A	16.85
B. Non-Graduating College Attendees	0.196	0.541
% of College Enrollees Enrolled in Birth State	(0.041)***	(0.157)***
First-stage F-Statistic	N/A	19.47
Birth-State Dummies	Yes	Yes
Region*Year-of-Birth Dummies	Yes	Yes
Age Dummies	Yes	Yes
Survey Year Dummies	Yes	Yes
ACS Demographic Variables	Yes	Yes
Other Baseline Explanatory Variables	Yes	Yes
ACS % with No College Living in Birth State	Yes	Yes
ACS and Census Education Control Variables	Yes	Yes
Number of Observations (Year-State-Age Cohorts)	6750	6750

Notes: Standard errors in parentheses are clustered by birth state. The dependent variables are year-state-age cohort percents who live in their birth state during the 2006-2014 ACS for college graduates (bachelor's or higher) in Panel A and non-graduating college attendees in Panel B. Other first-stage statistics for the 2SLS tuition IV model are very similar to Tables 3 and are suppressed for brevity. All regressions are weighted using cohort weights computed as the sum of ACS survey weights for individual observations in the cohort and corresponding education group.
*Statistically significantly different from zero at the 10% level of significance; ***Significant at the 1% level.

Table 6: Reduced Form OLS Estimates of In-State Flagship Tuition Effects on Educational Attainment

	(1)	(2)	(3)
	% with Some College or Higher	% with Bachelor's or Higher	% with Master's or Higher
Log In-State Tuition of Flagship Universities at Age 17	-0.006	0.003	-0.0003
	(0.005)	(0.004)	(0.002)
Birth-State Dummies	Yes	Yes	Yes
Region*Year-of-Birth Dummies	Yes	Yes	Yes
Age Dummies	Yes	Yes	Yes
Survey Year Dummies	Yes	Yes	Yes
ACS Demographic Variables	Yes	Yes	Yes
Other Baseline Explanatory Variables	Yes	Yes	Yes
ACS % with No College Living in Birth State	No	No	No
ACS and Census Education Control Variables	No	No	No
Number of Observations (Year-State-Age Cohorts)	6750	6750	6750

Notes: Standard errors in parentheses are clustered by birth state. The dependent variable is the year-state-age cohort percent in the 2006-2014 ACS with specified education level. All regressions are weighted using total cohort weights computed as the sum of ACS survey weights for all individual observations in the cohort.

Table 7: OLS and 2SLS Joint Effect of Attending At Least Some College and Residing in Birth State

	(1) OLS	(2) 2SLS	(3) 2SLS
% of College Enrollees Enrolled in Birth State	0.101	0.308	0.309
	(0.020)***	(0.113)***	(0.112)***
Birth-State Dummies	Yes	Yes	Yes
Region*Year-of-Birth Dummies	Yes	Yes	Yes
Age Dummies	Yes	Yes	Yes
Survey Year Dummies	Yes	Yes	Yes
ACS Demographic Variables	Yes	Yes	Yes
Other Baseline Explanatory Variables	Yes	Yes	Yes
ACS % with No College Living in Birth State	No	No	Yes
ACS and Census Education Control Variables	No	No	No
Number of Observations (Year-State-Age Cohorts)	6750	6750	6750

Notes: Standard errors are clustered by birth state. The dependent variable is the year-state-age cohort percentage of the population that has attended at least some college AND currently resides in their birth state during the 2006-2014 ACS; this variables has a mean of 0.37. The instrument for columns 2 and 3 is the log in-state tuition of flagship universities at age 17, the same as used in Table 3. First-stage statistics for columns 2 and 3 are very similar to Table 3 and are omitted to conserve space. All regressions are weighted using total cohort weights computed as the sum of ACS survey weights for all individual observations in the cohort.

***Statistically significantly different from zero at the 1% level of significance.

Table 8: 2SLS Results for Marriage Outcomes of College Attendees Using In-State Tuition IV

	(1)	(2)	(3)
Second-Stage Dependent Variable:	% Married	% Married and Spouse Same Birth State	% Married and College-Educated Spouse Same Birth State
% of College Enrollees Enrolled in Birth State	0.219	0.449	0.462
	(0.228)	(0.123)***	(0.118)***
Birth-State Dummies	Yes	Yes	Yes
Region*Year-of-Birth Dummies	Yes	Yes	Yes
Age Dummies	Yes	Yes	Yes
Survey Year Dummies	Yes	Yes	Yes
ACS Demographic Variables	Yes	Yes	Yes
Other Baseline Explanatory Variables	Yes	Yes	Yes
ACS % with No College Living in Birth State	Yes	Yes	Yes
ACS and Census Education Control Variables	Yes	Yes	Yes
Number of Observations (Year-State-Age Cohorts)	6750	6750	6750

Notes: Standard errors are clustered by birth state. The specifications are identical to Table 3 column 3 except for the dependent variables. All regressions are weighted using cohort weights computed as the sum of ACS survey weights for all college attendee individual observations in the cohort.

***Statistically significantly different from zero at the 1% level of significance.

Appendix Table A: Analytical Sample Mean Birth-State Enrollment and Adult Residence Rates by State

	% of College Enrollees Enrolled in Birth State, 1980	% of College Enrollees Enrolled in Birth State, 1990	% of College Enrollees Enrolled in Birth State, 2000	% with Any College Living in Birth State, 2006-2014
Alabama	0.711	0.745	0.733	0.618
Alaska	0.176	0.287	0.242	0.258
Arizona	0.598	0.629	0.647	0.584
Arkansas	0.630	0.682	0.730	0.566
California	0.792	0.766	0.779	0.665
Colorado	0.550	0.545	0.563	0.528
Connecticut	0.542	0.541	0.513	0.513
Delaware	0.471	0.486	0.518	0.433
Florida	0.613	0.630	0.696	0.584
Georgia	0.667	0.673	0.695	0.656
Hawaii	0.536	0.487	0.482	0.473
Idaho	0.428	0.500	0.540	0.463
Illinois	0.650	0.641	0.657	0.573
Indiana	0.678	0.696	0.718	0.577
Iowa	0.631	0.642	0.664	0.511
Kansas	0.588	0.619	0.660	0.496
Kentucky	0.696	0.713	0.683	0.615
Louisiana	0.749	0.733	0.733	0.561
Maine	0.576	0.598	0.555	0.540
Maryland	0.590	0.551	0.584	0.536
Massachusetts	0.677	0.664	0.623	0.563
Michigan	0.771	0.760	0.781	0.628
Minnesota	0.712	0.681	0.653	0.663
Mississippi	0.701	0.725	0.746	0.547
Missouri	0.625	0.638	0.662	0.581
Montana	0.552	0.521	0.522	0.436
Nebraska	0.622	0.621	0.618	0.512
Nevada	0.327	0.438	0.462	0.440
New Hampshire	0.533	0.477	0.428	0.467
New Jersey	0.538	0.547	0.554	0.506
New Mexico	0.487	0.536	0.574	0.469
New York	0.660	0.654	0.662	0.524
North Carolina	0.754	0.770	0.777	0.686
North Dakota	0.586	0.542	0.592	0.417
Ohio	0.689	0.700	0.723	0.602
Oklahoma	0.646	0.689	0.688	0.562
Oregon	0.658	0.630	0.600	0.557
Pennsylvania	0.687	0.717	0.721	0.587
Rhode Island	0.612	0.546	0.557	0.478
South Carolina	0.722	0.739	0.715	0.608
South Dakota	0.525	0.523	0.514	0.430
Tennessee	0.698	0.695	0.681	0.622
Texas	0.772	0.790	0.771	0.739
Utah	0.721	0.734	0.729	0.627
Vermont	0.470	0.508	0.490	0.448
Virginia	0.603	0.610	0.637	0.540
Washington	0.639	0.651	0.665	0.620
West Virginia	0.608	0.632	0.630	0.455
Wisconsin	0.748	0.730	0.727	0.634
Wyoming	0.346	0.480	0.379	0.337

Notes: means are weighted averages over cohorts within states.

Appendix Table B: College Enrollee Sample Observations Counts per Cohort by Birth-State

	Mean	Std. Dev.	Min	Max
Alabama	1,118	442	672	2,279
Alaska	119	52	56	248
Arizona	615	258	302	1,216
Arkansas	530	224	297	1,139
California	6,838	2,017	4,094	10,879
Colorado	800	314	429	1,506
Connecticut	1,011	290	658	1,539
Delaware	182	58	114	329
Florida	1,841	754	908	3,624
Georgia	1,482	655	770	3,198
Hawaii	332	84	209	517
Idaho	251	86	150	477
Illinois	3,830	1,152	2,375	6,426
Indiana	1,584	624	760	3,162
Iowa	956	343	486	1,624
Kansas	738	250	414	1,284
Kentucky	906	343	523	1,767
Louisiana	1,260	446	669	2,400
Maine	292	124	156	636
Maryland	1,090	330	666	1,938
Massachusetts	2,001	631	1,103	3,157
Michigan	2,970	993	1,645	5,355
Minnesota	1,406	494	758	2,558
Mississippi	832	364	420	1,760
Missouri	1,414	508	744	2,529
Montana	235	83	141	457
Nebraska	538	198	297	985
Nevada	163	64	81	313
New Hampshire	222	92	89	442
New Jersey	2,334	708	1,394	3,810
New Mexico	384	155	219	801
New York	6,404	1,958	3,841	10,264
North Carolina	1,610	642	950	3,301
North Dakota	247	88	118	439
Ohio	3,354	1,250	1,704	6,503
Oklahoma	749	278	365	1,421
Oregon	601	215	338	1,031
Pennsylvania	3,561	1,171	2,085	6,229
Rhode Island	310	88	191	520
South Carolina	867	339	466	1,707
South Dakota	226	90	107	435
Tennessee	1,157	455	678	2,369
Texas	4,140	1,760	1,970	8,357
Utah	491	206	272	938
Vermont	130	46	67	237
Virginia	1,428	487	871	2,611
Washington	1,080	352	653	1,892
West Virginia	472	179	283	947
Wisconsin	1,422	483	818	2,522
Wyoming	120	49	56	222

Notes: Cohorts are defined by state-of-birth and year-of-birth and observed at ages 18-22 in the 1980-2000 decennial census 5% PUMS. The observation counts are based on individuals enrolled in college at the time of the census survey, roughly on April 1 of the census year.

Appendix Table C: Results for Control Variables in Full Specification of Tables 2 & 3

	(1) OLS	(2) IV w/ Tuition
% Female for All College Attendees	0.036	0.036
	(0.012)***	(0.011)***
% Black for All College Attendees	0.031	0.032
	(0.035)	(0.033)
% Asian for All College Attendees	0.162	0.171
	(0.094)*	(0.083)**
% Hispanic for All College Attendees	0.109	0.122
	(0.040)***	(0.037)***
% Other Non-white for All College Attendees	-0.018	-0.015
	(0.046)	(0.045)
Log Personal Income Per Capita at Age 18	-0.082	-0.077
	(0.048)*	(0.045)*
Log Housing Price Index at Age 18	-0.016	-0.004
	(0.019)	(0.019)
Log of Cohort Size at Age 18	-0.057	-0.075
	(0.026)**	(0.026)***
Merit Dummy at Age 18	0.011	0.006
	(0.005)**	(0.005)
Unemployment Rate at Age 22	-0.002	-0.001
	(0.002)	(0.002)
Return to Bachelor's Degree at Age 22	-0.001	-0.003
	(0.007)	(0.007)
Log Median Household Income at Age 22	0.006	0.012
	(0.018)	(0.019)
% in 2006-2014 ACS w/ No College Living in Birth State	0.047	0.036
	(0.013)***	(0.014)**
% in 1980-2000 Census Enrolled in College	-0.049	-0.126
	(0.031)	(0.050)**
% in 2006-2014 ACS with Some College or More	0.098	0.092
	(0.020)***	(0.019)***
% in 2006-2014 ACS with a Bachelor's Degree or More	-0.158	-0.147
	(0.024)***	(0.024)***
% in 2006-2014 ACS with a Graduate Degree	-0.088	-0.082
	(0.040)**	(0.039)**
Full Specification in Column 3 of Tables 2 & 3	Yes	Yes
Number of Observations (Year-State-Age Cohorts)	6750	6750

Notes: Standard errors are clustered by birth state. The dependent variable is the year-state-age cohort percent with any college who live in their birth state during the 2006-2014 ACS. *Statistically significantly different from zero at the 10% level of significance; **Significant at the 5% level; ***Significant at the 1% level.