"Workhorses of Opportunity": Regional Universities Increase Local Social Mobility*

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Abstract

Regional public universities educate approximately 70 percent of college students at four-year public universities and an even larger share of students from disadvantaged backgrounds. They aim to provide opportunity for education and social mobility, in part by locating near potential students. In this paper, we use the historical assignment of normal schools and insane asylums (normal schools grew into regional universities while asylums remain small) and data from Opportunity Insights to identify the effects of regional universities on the social mobility of nearby children. Children in counties given a normal school get more education and have better economic and social outcomes, especially lower-income children. For several key outcomes, we show this effect is a causal effect on children, and not only selection on which children live near universities. We use student-level survey data to compare characteristics of college-going students from normal and asylum counties and to study the geographic barriers that keep asylum-county children from attending college.

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Regional public universities have been considered the "colleges of the forgotten Americans" and "workhorses of opportunity" because of their potential to increase social mobility (Dunham, 1969; Wendler, 2018). From their establishment in the mid-20th century, a central part of their mission has been to increase access to higher education, by locating near potential students, being less selective, and having lower tuition. Regional public universities enroll roughly 40 percent of all undergraduate students in the United States, and students at these institutions come disproportionately from lower-parental-income families and are more likely to be racial minorities relative to other four-year public universities.[1,2]

We study whether regional public universities increase nearby children's educational attainment and social mobility, measured using data from Opportunity Insights. These local objectives are an important justification for these regional colleges, making our analysis especially policy-relevant. Our analysis highlights the continued role of geographic frictions in college attendance and economic mobility.

The central identification challenge is that these universities are not located randomly, and they may have been placed in areas expected to have high educational attainment and economic mobility even without the university. We use a strategy developed in Howard, Weinstein and Yang (2022) to identify the impact of regional public universities on the county. Our strategy utilizes the placement of normal schools and insane asylums in the late 19th and early 20th centuries, both part of the period's social reform movements. We show that state governments assigned these institutions to counties using similar criteria, including

[1]Of undergraduates at four-year public universities, almost 70 percent, including 85 percent of Black and 74 percent of Hispanic students, are at regional universities (Fryar, 2015). These statistics are based on Fryar (2015)'s historical definition of comprehensive universities, which includes public, four-year universities that are not the primary research university in the state, not land-grant universities, and not established expressly to serve as a research institution.

[2]On average roughly 15 percent of students at four-year public flagship, public elite, or public highly selective institutions came from the bottom two parental income quintiles. At four-year public non-flagship, non-highly selective institutions that fraction is roughly 28 percent. This is based on the university-level Opportunity Insights, and the 1980 birth cohort. We exclude institutions that report as a system. There are 45 public institutions in the flagship/highly selective group, and 377 in the other (314 of which were classified as selective public colleges and the remainder were nonselective public colleges). We note that the distinctions used for this statistic are not quite the same as Fryar (2015)'s historical definition, but nonetheless separates the very selective and flagships from other public universities. Pell grants are also more common at regional universities (Maxim and Muro, 2020).

political factors, proximity and ease of access to population centers, as well as locations with sufficient property and natural beauty (Humphreys, 1923; Kirkbride, 1854). By the mid-20th century, normal schools had evolved to become regional public universities, and they comprise roughly half of today's regional public universities. In contrast to the universities, most insane asylums were converted into psychiatric health facilities and remain small in size. Our central identification assumption is that the asylum counties are a good counterfactual for what would have happened in the normal counties had the normal schools not converted to regional public universities.

Using data from Chetty et al. (2018), we show that regional public universities increase economic and social mobility in their counties. These universities increase the fraction of children in the county who obtain at least a four-year degree and at least some college, with the largest percentage increases for children of lower-income parents. They also increase the high school graduation rate.

In addition, these universities improve the fraction of children in the county who are employed in their mid-30s as well as their income percentiles, with effects concentrated among children from lower-income families. Finally, we also see that regional public universities have impacts on social outcomes in their county, increasing the fraction of lower-income children in the county who get married, and decreasing the fraction that live in their childhood commuting zone.

Using estimates from Chetty and Hendren (2018), we see evidence that these causal impacts on the county reflect causal impacts on individuals, rather than reflecting sorting of high mobility individuals into counties with regional public universities.

Under our identification assumption, there are two ways in which our specification could have a zero coefficient. First, we would obtain a zero coefficient if universities do not affect social mobility. Second, we would obtain a zero coefficient if these universities affect local outcomes, but outcomes are similar for people growing up in counties that were assigned an asylum. This could be because students travel across counties within a state to attend a regional public college, or because individuals in asylum counties equally access private institutions in their home county to similar effects. So finding a non-zero effect of universities rejects both that universities have no effect on the social outcomes we consider, and it rejects that the geographic sorting of college students or other universities makes the location of regional universities irrelevant.

Finally, we supplement our analysis using rich student-level data from The Freshman Survey, which includes 3.6 million individuals who grew up in normal school or asylum counties. Using these data, we show that the first-year students from normal school counties are more likely to have parents who did not go to college, be more concerned about financing college, have lower academic achievement, and have only applied to one college. This is consistent with regional public universities inducing enrollment of local students that are on the margin of going to college. Furthermore, we use The Freshman Survey to explore the self-reported geographic frictions that may be preventing regional universities from reaching students in asylum counties. We see evidence consistent with proximity to a regional university reducing financial costs of college, reducing information frictions, and drawing in students with home preferences. Specifically, we show that students from normal school counties are more likely to say they would have attended elsewhere if they could afford it, are more likely to live at home, are less reliant on information disseminated by the college in making their choices, and are more likely to have chosen their university because a previous student suggested it.

Our results show proximity to a university still matters for access to higher education and economic mobility. This is relevant for policymakers considering where universities are located and expanding. It also suggests the importance of addressing individuals who are not in close proximity to public universities. Our analysis is further relevant given recent discussions about consolidation and the future of these universities (see McClure and Fryar, 2020; Maxim and Muro, 2020; Seltzer, 2019).[3] Finally, our results contribute to our understanding of where people should live to improve the economic and social mobility of

[3]Recent examples of states that have discussed consolidation of regional public universities include Pennsylvania, Vermont, and Wisconsin (Seltzer, 2020; Quinton, 2020).

their children.

Despite their size and potential to improve economic mobility, the impacts of regional public universities on students and communities have received limited attention in the literature, especially relative to community colleges and elite universities (Schneider and Deane, 2015).[4] While their role as an anchor institution in local communities is often cited, along with their role in enhancing mobility, there is very little work to our knowledge estimating the causal impacts of these public, less research-intensive universities on nearby residents.

The relationship between educational attainment and proximity to universities has been an important topic in the literature.[5] For example, Card (1993) finds that proximity to a college raises education and earnings for men in the 1960s and 1970s, especially for men with the lowest predicted levels of educational attainment. Kling (2001) shows evidence that these effects are smaller for teens in 1979.[6]

Our paper contributes to this literature in several ways. First, we use a novel strategy to identify the causal impact of universities on local educational attainment. Establishing causality in the existing literature is challenging, as differences between areas with and without universities, unrelated to the university, may explain differences in educational attainment. For example, universities may have been established in areas where there is a high return to college education. Second, we focus on regional public universities, a highly

[4]Klor de Alva (2019) uses university-level Opportunity Insights data to highlight that among enrollees at a sample of roughly 300 comprehensive universities whose parents were in the lowest two income quintiles, over half reached the upper two quintiles by their 30s. Crisp, McClure and Orphan (2021) present a volume exploring broadly accessible institutions, institutions which include but are not limited to regional public universities.

[5]More broadly our paper contributes to research studying universities and local economic growth, with many of the papers focusing on innovation. Papers include Aghion et al. (2009); Andersson, Quigley and Wilhelmson (2004); Andrews (2021); Bartik and Erickcek (2008); Cantoni and Yuchtman (2014); Feng and Valero (2020); Hausmann (2020); Kantor and Whalley (2014, 2019); Moretti (2004); Valero and Reenen (2019). Also related to our paper, Garin and Rothbaum (2022) study the long-run effects of counties receiving a large manufacturing plant in World War II, finding impacts on upward economic mobility for children born in these counties before the war.

[6]Other papers studying the relationship between proximity to universities and enrollment or completed education include: Do (2004); Doyle and Skinner (2016); Kane and Rouse (1995); Long (2004); Jepsen and Montgomery (2009); Alm and Winters (2009). Bedard (2001) finds that areas with universities have higher high school drop out rates in the 1960s and early 1970s, consistent with a signaling model, as higher rates of college attendance decrease the value of pooling with high school graduates.

relevant, important, and yet understudied higher education sector. Finally, we utilize the very rich data from the U.S. Census and the IRS made available by Opportunity Insights, allowing us to study the impacts on education for roughly 20 percent of the U.S. population born between 1978 and 1983, as well as study other labor market and social outcomes for nearly the whole population born in these cohorts. Many of the previous papers have used samples from survey data, such as the National Longitudinal Surveys or the High School and Beyond survey.

Russell, Yu and Andrews (2022) and Russell and Andrews (2022), building on the empirical strategy of Andrews (2021), also focus on identifying the causal impact of colleges on educational attainment and economic mobility, respectively, by comparing areas with universities to runners-up locations for universities. Compared to our findings, they find larger effects on college education and smaller effects on income rank.[7] We view our papers as complementary. One of the biggest differences is that we identify the effects of regional public universities, while the sample in Russell, Yu and Andrews (2022) includes primarily research-intensive universities. Given that regional public universities were established to improve local access to higher education and opportunity, ours is an especially relevant sample for understanding the impact of universities on mobility. Second, given the relatively few number of observations inherent to either empirical strategies, bringing more observations to this question is of particularly high return.[8] Finally, the counties in our control group are given a similarly-sized state institution, rather than being only runners-up. Russell, Yu and Andrews (2022) are also interested in the effect of universities relative to counties with a "consolation prize," but have only 27 counties in the sample for this exercise.

Chetty et al. (2014) and Chetty and Hendren (2018) show some evidence of a positive relationship between their local mobility measures and colleges per capita or the graduation

[7]The primary outcomes in Russell and Andrews (2022) are the probability of reaching the top income percentiles, as well as measures of local inequality, two outcomes which we do not investigate given our focus on the primary goals of regional universities.

[8]There are 191 counties in Russell, Yu and Andrews (2022), split between 63 counties with universities and 128 without. There are 320 counties in Howard, Weinstein and Yang (2022), with 204 that received normal schools and 126 that received asylums.

rate at local colleges. As Chetty and Hendren (2018) caution, this does not identify the effect of colleges on local mobility because areas with colleges may be high mobility areas for reasons other than the college.[9] Chetty and Hendren (2018) also show evidence consistent with lower-mobility, not higher-mobility, individuals sorting into areas with more colleges per capita, which is helpful for interpreting our results.

1 History of Normal Schools and Asylums

The social reform movements of the 19th century included support for public institutions aimed at societal improvement.[10] These institutions included normal schools to train teachers and asylums to treat those with mental illnesses (Grob, 2008). In this section, we provide qualitative evidence that locations for these institutions were chosen based on very similar criteria.

The original purpose of normal schools was to train teachers to meet growing demand stemming from the common school movement in the mid 19th century (Labaree, 2008).[11] There were 209 state normal schools opened between 1839 and 1930 (Ogren, 2005). Similarly, as part of the mid-19th century movement to improve care for those with mental illnesses, many states opened insane asylums. The objective of these asylums was to facilitate recovery and to provide compassionate care (Grob, 2008).

The criteria for where to locate normal schools and insane asylums were very similar. Both were political decisions, in which population, geographic accessibility, and natural

[9] Chetty et al. (2014) show a positive correlation between rates of local upward income mobility and two measures of local higher education: colleges per capita and the graduation rate at local colleges (controlling for parental income), though the correlation with colleges per capita disappears when controlling for state fixed effects. There is a negative correlation with mean tuition at local colleges, but it is not statistically significant. Chetty and Hendren (2018) further show a positive correlation between causal effects on upward income mobility and these college variables, though only the relationship with colleges per capita is statistically significant. There is also a negative correlation between the causal effects on upward income mobility and mean tuition at local colleges, but this is not statistically significant.

[10] Howard, Weinstein and Yang (2022) contain a thorough discussion of the history and site selection of normal schools and asylums.

[11] This increased demand for qualified teachers, and as a result many states established normal schools to train teachers according to the "norm" for good teaching (Labaree, 2008).

beauty were important factors. Humphreys (1923) describes in detail the location decisions for normal schools, asserting that political factors were the most important, though other factors included demand for instruction (e.g. local population), geographic accessibility, financial and land donations, location of existing schools, and natural beauty. Kirkbride (1854) developed an architectural plan for asylums, implemented by many states, which emphasized the importance of accessibility to population centers, locations with natural beauty, ample area for recreation, and stately architecture.

During this period local communities desired and took pride in both types of institutions. An article from the *Kankakee Gazette*, written in August 1877 when the city was assigned an asylum, helps illustrate these points, "Our citizens received the news in a spirit of jubilee, and on Friday evening there was a bonfire, band music... and speeches..." The article expresses gratitude for "the great services of Messrs. Bonfeid and Taylor, our representatives in the upper and lower houses of the legislature," highlighting the importance of the political process in determining these locations.[12]

As we show in Howard, Weinstein and Yang (2022), states determined locations for normal schools and insane asylums at roughly the same time.[13] The timing and the similar selection criteria, along with individual state histories, support the idea that whether a community received a normal school or an asylum may have been effectively random.[14] We showed in Howard, Weinstein and Yang (2022) that in the early 20th century, enrollment at normal schools and the population in asylums were similar relative to county population. This provides further supportive evidence that being selected as the location for these two types of institutions may have required similar political influence, as the institutions may have been expected to confer similar advantages.[15]

[12]We provide more evidence from historical newspapers supporting our identification strategy in Howard, Weinstein and Yang (2022).

[13]For reference, we reproduce the figure from Howard, Weinstein and Yang (2022) showing the timeline of institution openings in Figure A1a.

[14]Humphreys (1923) also provides evidence that location decisions for these two types of institutions were relevant for political negotiations. See Howard, Weinstein and Yang (2022) for further details on these political factors.

[15]For reference, we reproduce the figure from Howard, Weinstein and Yang (2022) showing this fact in

We support our identification assumption with several additional observations. First, roughly 17 percent of counties that were assigned asylums also were assigned normal schools (13 percent of normal counties had asylums). This suggests similar selection criteria for the two types of institutions. Second, asylum counties were often runners-up locations for public colleges and universities, as documented in Andrews (2021).[16] In the opposite direction, one example is Bloomington, IL, which was assigned a normal school and was a top contender for an asylum.

1.1 Subsequent Evolution

Demand for higher education increased over the course of the 20th century, and normal schools evolved with these changes. In the early 20th century, many were renamed as teachers colleges, allowing them to confer bachelor's degrees in education. In the mid-20th century there was growing demand for degrees that did not focus on teacher training. Many policy discussions at the time focused on improving access through geographic accessibility (Doyle and Skinner, 2016; Mayhew, 1969; Willingham, 1970; Douglass, 2007).[17]

Many proponents thought normal schools should offer bachelor's degrees in areas other than education. They already existed as higher education institutions, and they were geographically distributed within states. Proponents argued they were uniquely positioned to increase access to a college education for their local areas. For example, in advocating they be permitted to grant liberal arts degrees, college leaders at Eastern Illinois State Teachers College cited the limited number of other colleges in the region, that they were already serving as a regional college, and that many highly qualified high school students were not

Figure A1b.

[16] Of the 62 high-quality public college site selection experiments in Andrews (2021), 17 had runners-up that were asylum counties, although most of these experiments were for land grant institutions. Andrews (2021) discusses consolation prizes, and argues that assignment of one type of institutions versus another was "as good as random".

[17] Mayhew (1969) presents a summary of state master plans for higher education developed during this period of increased demand, stating "all plans seek to provide complete geographical access to higher education."

willing to attend a teachers college but would attend a state college (Coleman, 1950).[18]

The proponents of these changes were successful, and in the mid-20th century many of the teachers colleges were given the authority to grant degrees in areas other than education. As a result, many of the teachers colleges were renamed as state colleges, removing "teachers" from the name.[19] In contrast to state universities, these state colleges focused on undergraduate education, business, teaching, and engineering (as opposed to law, medicine, and scholarship) (Mayhew, 1969). From the 1950s through the 1970s, many obtained university status (Labaree, 2008). Commenting on the frequent name changes, Dunham (1969) humorously noted discounted t-shirts at the college stores with the college's previous name.[20] Figure A1b shows large enrollment increases as normal schools converted to regional public universities.

Institutions that started as normal schools comprise a large fraction of today's regional public universities, or using a similar classification, "comprehensive" universities.[21] Of the 320 public colleges in 1987 that are classified as "comprehensive" based on the 1987 Carnegie classification, roughly 50 percent started as state normal schools.[22] In keeping with their original mission, students at regional public universities are more likely to be from historically

[18]Similarly, proponents of making these changes at Southern Illinois Normal University argued local high school students were demanding a liberal arts degree, and it would be very costly for them to obtain this degree from another college (Lentz, 1955). A 1945 commission report wrote that even though they were only authorized to prepare teachers, the teachers colleges in Illinois had effectively already become regional colleges. These colleges were under pressure from the region to provide broader training, and students were enrolling in the teachers colleges and then not entering the teaching profession. The report noted that over the past seven years approximately 25% of graduates did not enter teaching (Commission to Survey Higher Educational Facilities in Illinois, 1945).

[19]Dunham (1969) observed that while many teachers colleges were renamed state colleges, they still remained focused on teacher training as of 1969. He also noted that for some faculty, "*teachers college* carries with it connotations of mediocrity, especially since Sputnik", and this led some faculty to push for removing "teachers" from the name of their college.

[20]Figure A1a, reproduced from Howard, Weinstein and Yang (2022), shows the years in which normal schools were opened, and converted to state colleges and universities.

[21]See Maxim and Muro (2020) for an overview of various classifications.

[22]This is based on the evolution of name changes of state normal schools in Ogren (2005). In 1987, there are a total of 188 colleges that originated as state normal schools, based on Ogren (2005). Of these, 156 are classified as "comprehensive" in the 1987 Carnegie classifications, and 187 are "Research II," "Doctorate-Granting," "Comprehensive," or "Liberal Arts". Using an alternative classification, of the 439 public, non-Research I colleges in 1987 that are classified as "Research II," "Doctorate-Granting," "Comprehensive," or "Liberal Arts," roughly 43 percent started as state normal schools.

underrepresented or nontraditional groups in higher education (Fryar, 2015).

While some of the asylum buildings are no longer in use, states continue to own many of the asylum properties, and many are still used as psychiatric health facilities. Some properties are used as correctional facilities, while other have been acquired by universities (Hoopes, 2015). During the deinstitutionalization movement in the mid-20th century, institutionalized population per capita in asylum counties fell, though only modestly, and was twice the level in normal counties in 2010.[23]

1.2 Data on Normal Schools and Asylums

As we describe in Howard, Weinstein and Yang (2022), we obtain data on normal schools' locations, opening years, and years corresponding to name changes, from Ogren (2005).[24] There were 209 normal schools across 204 counties, opened between 1839 and 1930, with median opening year of 1891 (Figure A1a).

We digitize data on asylums' geographic locations and opening years from the 1923 special census of "institutions of mental disease" (Furbush et al., 1926). As in Howard, Weinstein and Yang (2022), we focus on institutions established around the same time, so we exclude five asylums that were established before 1830.[25]

Counties that had both normal schools and asylums are defined as normal counties (there are 25 of these counties).[26] Our sample includes 204 normal counties and 126 asylum counties. Figure A1 shows the geographic distribution of normal and asylum counties in our sample.

[23]We show this in Figure A1b, reproduced from Howard, Weinstein and Yang (2022).

[24]Using the city and state of the normal school, we identified the county using StatsAmerica (Indiana Business Research Center, 2020).

[25]The opening years and locations were extracted from Table 64 and Table 104 of Furbush et al. (1926). Seventeen of these asylums did not have opening years in the 1923 Census, and we obtain them from government websites or other open sources.

[26]In Howard, Weinstein and Yang (2022), we showed that excluding these counties had no effect on the outcomes we considered in that paper.

1.3 Historical Measures of Mobility

Our identification assumption is that asylum counties are a good counterfactual for what would have happened in normal school counties, had the normal schools not converted to regional public universities. Howard, Weinstein and Yang (2022) showed balance between normal and asylum counties in 1840, before most of the normal schools and asylums were established. Here we show balance on economic mobility in 1850 because, as we will discuss, in that year we are able to construct a more meaningful measure of mobility. It is possible that normal schools had an effect on local educational mobility before they were converted to universities, and that this explains part of the effect we see today.[27] To address this we test for differences in upward education mobility between normal and asylum counties in 1940, before most of the normal schools converted to regional universities.

For our first test, before most normal schools were established, we use the 1850 full count of the U.S. Census. Following Card, Domnisoru and Taylor (2022) and Derenoncourt (2022), our historical measure of mobility is based on education–the likelihood that children of parents with lower incomes or education levels have high levels of educational attainment.[28]

We measure upward educational mobility as the school attendance rate of 14-17 year olds whose father's reported value of real estate owned is less than or equal to the median. This is constructed using teens living with their fathers, allowing us to match children to their fathers, and also avoids capturing teens who have traveled from other locations for the purposes of enrolling in school.[29] For robustness, we also construct this measure for 7-13

[27]Dunham (1969) states that at institutions which train people to be teachers, the students are from lower-middle-income families and often first-generation college students. Ogren (2003) also discusses the normal schools enrolling students from lower-income backgrounds. However, as we show in Figure A1b, enrollment in normal schools before 1940 was only 2.5% of county population. While they may have had a direct effect on enrollees, this was a small number of people. By the 1970s, enrollment was over 10% of the county population.

[28]Derenoncourt (2022) uses the occupational score of the fathers to identify socioeconomic status, but this is based on 1950 incomes and this score could be quite different in 1850. Specifically, among seven to seventeen year-old children in 1850 who were living with their fathers, 60 percent had fathers who were farmers, and 85 percent had fathers whose occupation was in one of five codes (farmer, manager, carpenter, laborer, operative). Card, Domnisoru and Taylor (2022) uses the educational attainment of the parents, but this is not available in the 1850 census.

[29]We calculate the median value of father's real estate, among seven to seventeen year olds living with

year-olds. Given differences across counties in the fraction of non-slave black individuals, we separate these measures for black and white individuals. Figure A2 shows the school attendance rate for teens is upward sloping in their father's real estate value. This suggests that we capture upward educational mobility by using the school attendance rate for teens with father's real estate value below the median.[30]

For the second test, after the establishment of normal schools but before they convert to universities, we use data from Card, Domnisoru and Taylor (2022). These county-level data show the fraction of children attaining eighth grade, living with parents with grade six maximal educational attainment.

Table 1 shows there are no significant differences in upward educational mobility in 1850 or in 1940.[31] Normal counties are smaller in population in 1850 (though the differences are not significant in 1920 when we have data on all states, or in 1840), and there is some evidence they are less urban and have lower real estate values per capita in 1850 (Appendix Table A2). We further show balance on other variables in 1840 and in 1920 in Howard, Weinstein and Yang (2022).

1.4 Effect on Higher Education and the Economy

Before showing the effects of historical normal school assignment on economic and social mobility, we document that most normal schools indeed became regional public universities and changed the landscape of higher education. These results are in Appendix Table A1, and are reproduced from Howard, Weinstein and Yang (2022)In 1980, around the time the children in our sample were born, 91 percent of counties that were historically assigned a normal school have a regional public college or university that had been a normal school,

their father.

[30]Figure A3 also shows an upward-sloping, although flatter, pattern for non-slave black teens.

[31]Using fraction of 7-13 year olds attending school, among those whose father's reported value of real estate is less than or equal to the median, shows this fraction is actually slightly lower in normal counties. This suggests lower upward mobility before normal schools were established.

Table 1: Historical Measures of Upward Educational Mobility

	Normal	Asylum	Within-State Difference
Upward educational mobility, 1850			
White	0.4	0.44	-0.01
	(.23)	(.19)	(0.02)
Black	0.22	0.25	0.04
	(.26)	(.27)	(0.04)
Upward educational mobility, 1940			
White	0.7	0.75	0.00
	(.18)	(.15)	(0.01)
Black	0.58	0.67	0.02
	(.29)	(.26)	(0.03)

Notes: Columns 1 and 2 show mean and standard deviation of county characteristics for normal and asylum counties. Column 3 shows the coefficient on normal county, when the dependent variable is the county characteristic, and we include state fixed effects. Educational mobility measures in 1850 are the fraction of 14-17 year olds attending school, among those whose father's real estate value is less than or equal to the median. Educational mobility measures in 1940 are from Card, Domnisoru and Taylor (2022), and denote the fraction of children attaining eighth grade, living with parents with grade six maximal educational attainment. We show standard errors clustered at the state level in parentheses in column 3. For the 1850 educational mobility of white individuals there are 161 normal and 102 asylum counties, and for black individuals there are 100 normal and 61 asylum counties. We restrict the 1850 samples to counties covered in the 1850 complete census from IPUMS USA. For the 1850 measures, we use the Eckert et al. (2020) crosswalk to 1990 counties. For the 1940 measure of educational mobility of white individuals, there are 203 normal counties and 122 asylum counties. For the 1940 measure of educational mobility of black individuals, there are 137 normal counties and 78 asylum counties.
$^{+}$ $p < 0.1$, * $p < .05$, ** $p < .01$

while this percentage is mechanically zero in asylum counties.[32] Some asylum counties have public four-year colleges, and the within-state differences imply normal counties have 0.7 additional public four-year colleges than asylum counties. The facts that not all normal counties have a regional public college, and that some asylum counties do have a public four-year college, both imply that our reduced-form empirical strategy will underestimate the impact of regional public universities.

On average, asylum counties have more private four-year colleges and two-year colleges. The results imply the total number of colleges is not statistically different in the two types of counties (adding the coefficients for total public four-year, private four-year, and two-year colleges). However, the universities in the normal counties are much larger. Enrollment as a percent of population is an additional 8.4 percentage points higher in normal counties, with enrollment equal to 4.5% of population in asylum counties. Finally, the fraction of the population with a college degree is 2 percentage points higher in normal counties, which is large relative to the level, though small relative to the number of degrees awarded per year as a percent of population. This suggests many students leave after graduating.

We also wish to emphasize economic comparisons between normal and asylum counties. As stressed in Howard, Weinstein and Yang (2022), the 1980 economies in normal and asylum counties look similar in both levels and growth, with the biggest difference—aside from those mechanically associated with having a large number of students—being that normal counties have a slightly larger retail sector and a slightly smaller manufacturing sector.[33]

Given the focus of our paper on social mobility, we also wish to highlight the income distribution of normal and asylum counties. Figure 1 shows that the fraction of parents in each national income decile is similar in normal and asylum counties within the same state.[34]

[32]For an additional two counties, the normal school closed and the site of the normal school became a different university. This was true of UCLA and Maine Maritime Academy.

[33]We reproduce the effect on the manufacturing sector using data from Chetty et al. (2018) in Appendix Table A9.

[34]The specification for the regression shown in Figure 1 is the same as our main specification which we discuss in detail in the next section. It is a regression of the outcome variable of interest—in this case, fraction of parents in an income decile—on an indicator variable for a normal county, with state fixed effects. The sample is only normal and asylum counties.

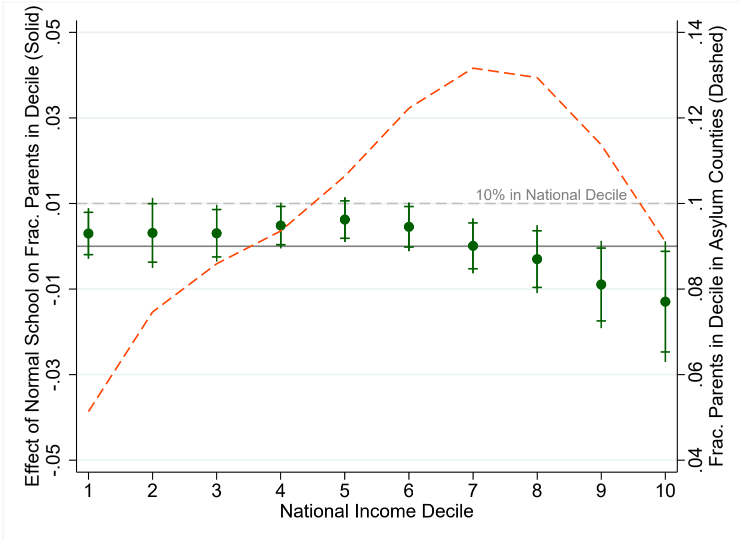


Figure 1: **Fraction of Parents by National Income Decile.** Green dots with spikes represent the estimated effect of a normal school on the outcome variable, conditional on state fixed effects. Outcome variables are the fraction of parents in the county in each national income decile. We estimate a separate regression for each decile, with effects across the x-axis. The green spikes span the 95 percent confidence intervals, with crossbars at the 90 percent confidence intervals. The estimates and confidence intervals correspond to the y-axis on the left-hand side of the figure. The orange dashed line shows the mean of the outcome variable in asylum counties, and corresponds to the y-axis on the right-hand side of the figure. Fraction of parents in national income decile, from Chetty and Hendren (2018), is based on parents of children in the 1980-1986 birth cohorts and average family income over 1996-2000.

However, normal counties have slightly higher percentages in the fourth and fifth decile, and lower percentages in the ninth and tenth. Figure 1 also makes clear that asylum and normal counties are different than the country as a whole, with substantial underrepresentation of people with very low incomes, as well as underrepresentation of people with the highest incomes.[35]

2 Data on Economic and Social Mobility

For our primary outcomes, we obtain data from Chetty et al. (2018). Using IRS and Census data, this includes county-level outcomes of children born between 1978 and 1983 who grew up in the county, by their parents' income. The sample includes 96 percent of all children

[35]If counties were representative of the country as a whole, then 10 percent of the population would be in each decile.

born between 1978 and 1983, who were born in the U.S. or are authorized immigrants who arrived in the U.S. as children and whose parents were U.S. citizens or authorized immigrants. Parents are defined as the first person who claims the child as a dependent between 1994 and 2015. Individuals are attributed to a county in Chetty et al. (2018), weighted by the fraction of years that they spend in the county before age 23.

We test for effects on educational attainment, income and employment, and other social outcomes. For education, we analyze the fraction obtaining at least a four-year degree, the fraction with some college, and the fraction with at least a high school degree or a GED. These education outcomes are observed only in the ACS, and thus are only available for the subsample that is observed in the ACS between 2005 and 2015. The number of children in this subsample is roughly four million, relative to the full sample of 20.5 million. The fraction obtaining at least a four-year degree, and the fraction with some college, are measured only for people at least 25.[36] The fraction with at least a high school degree or GED is measured only for those at least 19.

The income and employment outcomes we analyze include the fraction with positive W-2 earnings in 2015, family income percentile in 2014-2015, and individual income percentile in 2014-2015. Children's income as an adult is measured as the average of their adjusted gross incomes in 2014 and 2015, when they are 31-37 years old. The other social outcomes we analyze include the fraction married in 2015, teen birth (for women only), fraction incarcerated on April 1, 2010, fraction staying with their parents in 2015, and fraction staying in their childhood commuting zone based on their most recent address.[37] These income, employment, and social outcomes are observed for the full sample.

[36] Median age at graduation was 23 for public four-year institutions that were not very high research activity based on the 2005 Carnegie ratings (U.S. Department of Education, National Center for Education Statistics, 2021). Thus, this sample restriction will likely not capture too many people who are still enrolled and have yet to obtain a degree.

[37] The measure of teen motherhood is constructed based on whether a woman ever claims a dependent who was born while she was 13 to 19 years old. As Chetty et al. (2018) discuss, this is an imperfect measure since it relies on the woman claiming the child as a dependent at some point, but they document that this is aligned with estimates from the ACS. Staying with parents is defined as having an address that matches their parents' in 2015. Staying in childhood commuting zone is defined as the most recent commuting zone matching any commuting zone they lived in before 23.

Chetty et al. (2018) provide predicted children's outcomes in each county at five different percentiles of the parental income distribution.[38] Parental income is measured as the mean of parents' household adjusted gross income in 1994, 1995, and 1998-2000, when children are 11-22 years old. Given the children's age when parents' income is measured, we are less concerned that lower-income children in normal school counties are the children of graduate students, who may be experiencing only temporarily reduced income levels.

For suggestive evidence on whether our primary outcomes reflect causal effects on individuals, in addition to causal effects on counties, we use data from Chetty and Hendren (2018). This dataset contains causal estimates of counties on economic and social mobility of children born from 1980-1986 who grew up in the county, using IRS tax records. The causal effects are identified based on families who move across counties, whose children are of different ages at the time of the move. The causal effect is the effect of one additional year in the county during childhood.

3 Effects on Local Social Mobility

3.1 Empirical Strategy

Based on the history of normal schools and asylums, the main specification in our paper is

$$y_i = \beta \text{Normal}_i + \alpha_s + \epsilon_i \tag{1}$$

where y is our outcome of interest from Chetty et al. (2018), i is a county, and α_s is a state fixed effect. The sample consists of counties that had an insane asylum or normal school, and Normal_i is equal to 1 if the county had a normal school. β can be interpreted as an average effect of having been assigned a normal school on the outcome y. We cluster standard errors

[38] These predictions are based on regressing children's outcomes on parents' income percentiles, and allowing the coefficient to vary by county. Chetty et al. (2018) parameterize the relationship between child and parent income using a lowess regression of children's outcomes on parent's income percentile at the national level.

at the state level.

The identification assumption is that asylum counties in the same state are a good counterfactual for the social mobility of normal counties, had the normal school not converted to a university.

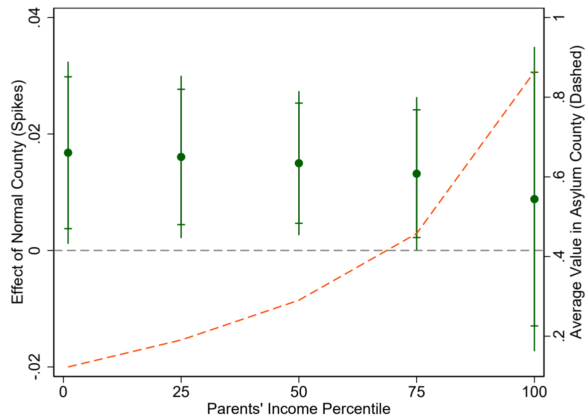
3.2 Effects on education

We first study the educational attainment of children who grow up in the county.

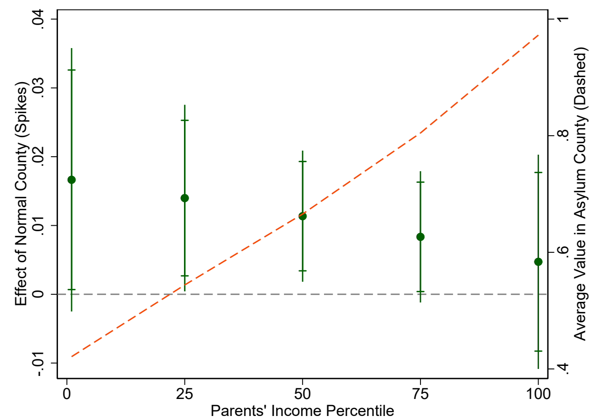
The regression results are shown in Figure 2. The green dots are the estimated coefficients from regression (1). The spikes are the 95 percent confidence intervals, and the cross-bars are 90 percent confidence intervals. The x-axis is the parents' income percentile, so the estimates to the right are for children of high-income parents, and the estimates to the left are for children of low-income parents. The estimates correspond to the y-axis on the left. For example, in panel (a), the effect of having been assigned a normal school is about a 2 percentage point increase in the probability of getting a four-year college degree, for a child who grows up in that county with a parent at the 1st percentile of the national income distribution. In the dotted orange line, the mean value of the outcome in asylum counties is plotted against the parents' income percentile, and the corresponding y-axis is on the right. For example, at the 1st percentile, less than 20 percent of the children in asylum counties get a four-year college degree. The orange line is not a causal estimate, but provides important context for interpreting the magnitudes of the effect. Note that the scales on each axis are different and vary from figure to figure.

In panel (a), we see a significant increase in college degree attainment for children growing up in normal counties, by almost two percentage points for children of parents at the 1st, 25th, 50th, and 75th percentiles. For the 100th percentile, the point estimate is a bit smaller and the confidence interval is quite wide.

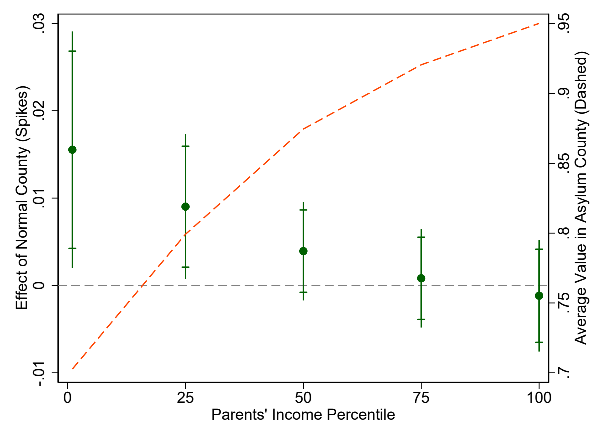
While there are not significantly significant differences in the effects across the income distribution, the effects for lower income percentiles are much larger relative to the baseline.



(a) At least 4-year College Degree, Age 25 and over



(b) At Least Some College, Age 25 and over



(c) At least HS Graduate or GED, Age 19 and over

Figure 2: **Effect of a normal school on education.** Green dots with spikes represent the estimated effect of a normal school on the outcome variable, conditional on state fixed effects. Outcome variables are measured at different percentiles of the parents' income distribution, which is the x-axis. The green spikes span the 95 percent confidence intervals, with the cross-bars at the 90 percent confidence intervals. The estimates and confidence intervals correspond to the y-axis on the left-hand side of the figure. The orange dashed line shows the mean of the outcome variable in asylum counties, and correspond to the y-axis on the right-hand side of the figure.

For a child with parents at the 75th percentile, the increase is less than 5 percent of the baseline, while it is about a 10 percent increase for a child at the 1st percentile, and 8 percent for a child at the 25th percentile.

In Panel (b), we look at the effects on some college attendance. The point estimates are generally comparable, which is noteworthy for several reasons. First, if normal schools only affected substitution between two- and four-year colleges, or if they only affected completion among those who enroll, then the effects on some college would be zero. In fact, for students with parents at the 75th percentile, the effects on some college are smaller than the four-year effects. This suggests the effect on four-year degrees for more affluent students is driven to a greater extent by changing the type of college they attend (2 versus 4 year) or increasing likelihood of completion. Second, if normal schools increased enrollment in four-year colleges, but these marginal students were unlikely to complete a degree, the effects on some college would be larger than the four-year effects. However, that is not what we find.

In Panel (c), we find significant increases in the high-school degree or GED attainment for children with parents at the low-end of the income distribution. The point estimate at the 1st percentile is comparable to the point estimate of the effect on some college or the effect on four-year college degree attainment. If normal schools' only effect was incentivizing high school graduates to enroll in college, there would be no effect on high school completion.

We note our results are in contrast to Bedard (2001), who finds that proximity to a college increases the high school dropout rate among teenagers in the 1960s. A central difference in our analyses is the identification of the control group to areas with universities.

The results from our causal identification strategy confirm the results of Card (1993) and the subsequent literature, that has used proximity to a college as a predictor of college attendance. For comparison, Kling (2001) shows that for the lowest-quartile of family background, having a college in the county increases highest grade completed by roughly one year in 1976 for individuals who were 14-19 in 1966. In 1989, this had fallen to 0.5 years for individuals who were 14-19 in 1979. While not directly comparable to our outcome variables, our empirical strategy appears to yield substantially smaller effects.[39] One reason may be that colleges are located in areas that have higher attainment for reasons other than the college, and our empirical strategy accounts for those. In our strategy, colleges may affect attainment through the direct effect on students and also through indirect effects (e.g., on the economy), but we eliminate the non-causal relationship between colleges and local educational attainment.

Russell, Yu and Andrews (2022) finds a substantially larger effect on college attainment. Their baseline estimate, using data from the American Community Survey, is that the presence of a university increases the share of the population with a college degree by 14 percentage points. When using data from Chetty et al. (2018), they estimate the fraction with at least a bachelor's degree is 8 percentage points higher for people who grew up in counties with universities, which is still substantially larger than our estimate. This likely reflects different effects of top-tier flagships and private universities on their local economies, compared to the effects of regional public universities which are our focus. For example, the universities in Russell, Yu and Andrews (2022) have larger effects on the local industry composition than the universities in our sample (see Howard, Weinstein and Yang, 2022). That could mean they attract parents more likely to send their children to college. It could also be that the universities in Russell, Yu and Andrews (2022) provide a higher return to a college degree.

In Appendix F, we look at the effects on education by race and sex. The sample of counties is different across races due to data availability, making comparisons across race difficult.[40] The results are also noisier, making it hard to say anything conclusive. However,

[39] The reason this comparison is challenging is that we do not observe years of education, the main outcome variable in those studies. However, if both point estimates are correct, it would need to be the case that almost all of the increase in years of schooling is due to students who do not obtain an additional degree. For example, if we take our coefficients, and assume that every additional college graduate or high school graduate gets another four years of schooling, that would contribute only 0.1 additional years of schooling ($1.6 \times 4 + 0.9 \times 4$), which means that 0.4 years would have to come from students who get more schooling but not additional degrees.

[40] For example, there are 325 counties in the regressions comparing college attainment of white individuals in normal versus asylum counties, but only 172 counties in the regressions for black individuals.

there are several interesting observations within race. For Hispanics, the effects on high school attainment are very large for those from lower-income families. And for some of the results regarding college degrees and some college, the effects for black and Hispanic children are the strongest at the top of the income distribution. For college degrees, the effects are stronger for women, while for high school degrees, the effects are slightly stronger for men at the bottom of the income distribution.[41]

3.2.1 Comparison to causal effects on people

Our estimates in Figure 2 identify the causal effects of having a university on the fraction of children in the county who attain high school degrees, or who enroll in or complete college. This is important for understanding how regional public universities affect their local communities. While these estimates identify the causal impact on the place, they do not identify the causal impact on the child's education, because the university may also affect the composition of children who grow up in the county. We use Chetty and Hendren (2018)'s estimates of causal effects of an additional year of exposure to a county, which accounts for this selective location choice, to see if the college does indeed have an effect on the educational outcomes of a child.[42]

In Table 2, we use our same empirical strategy but use the causal estimates on individuals from Chetty and Hendren (2018) as the dependent variables. We focus on students with parental income at the 25th percentile, but show the 75th percentile in the appendix. We use the outcome that is most comparable between the two datasets: some college from Chetty et al. (2018) and having attended college from Chetty and Hendren (2018). Column

[41] For black individuals from higher-income families the effects on at least some college are larger in magnitude than the effects on four-year degree attainment (and they are statistically significant). If the increase in those with exactly some college (e.g., at least some college minus at least a four-year degree) were statistically significant, this could imply that regional public universities are inducing additional enrollment but completion rates are low. However, this increase is not statistically significant.

[42] Chetty and Hendren (2018) estimates these causal effects using children that move into a county at different ages. One potential concern would be if the differences between families that move into normal counties at different children's ages and the differences between families that move into asylum counties at different children's ages are themselves different. We look for evidence of this in Appendix G and do not find any.

Table 2: Causal Effects on College Attendance, 25th percentile parental income

	(1) Some College, Age 25+	(2) Attended College, Age 18-23
Normal	1.398^{*}	0.139^{+}
	(0.672)	(0.0749)
Observations	325	306
Birth Cohorts	1978-1983	1980-1986
Weights	Unweighted	Precision Weights
Scale	Per Childhood	Per Year
Interpretation	Effect on Place	Effect on Person

Standard errors clustered by state. $^{+}$ $p < 0.1$, * $p < .05$, ** $p < .01$. Outcome data in column 1 are from Chetty et al. (2018), and outcome data in column 2 are from Chetty and Hendren (2018).

(1) shows the same results as from Figure 2b, for the 25th percentile. Column (2) shows the comparable result using the causal effects on children as outcomes.

There are a few differences to note when comparing these columns. First, following Chetty and Hendren (2018), to maximize precision, when using the causal impacts on people, we weight the observations using the inverse of the variance of the estimate. These weights are correlated with county population, so if the effect size is correlated to the size of the county, then the coefficients may reflect different average effects. Second, the causal estimates in column (2) are to be interpreted as the effect of having one additional year in that county, whereas the scale in column (1) is based on a childhood. The suggested comparison would be to scale the coefficient in column (2) by about 15 or 20 (see Derenoncourt (2022) for a discussion). Third, the variables are slightly different, with the variable in column (1) from ACS respondents and in column (2) from 1098-T forms that universities file with the IRS.[43] Finally, the results are based on different birth cohorts.[44]

At the 10 percent level, there is evidence that having a normal school has a causal effect

[43]Specifically, this variable is based on whether the individual had any 1098-T forms filed by colleges on their behalf from the ages of 18-23. This is required for all Title-IV institutions.

[44]Appendix Table A3 shows the causal estimates without weights and the observational estimates with the same weighting scheme as the causal estimates. Neither is significant, but both feature much larger standard errors than those in Table 2. In addition Table A3 shows estimates of the effects of a normal school on non-movers in the same cohort and using the same outcome as the causal estimates (column 3), which is slightly larger and statistically significant than our estimate of the causal effect of the place in Figure 2.

on the child's outcome. If we took the point-estimates seriously, it would seem that the causal effects are a bit bigger, but between the large standard errors, the different samples, and the different weightings, our main takeaway is that the magnitudes are roughly similar.

3.3 Effects on Income

In Figure 3, we show effects on measures of income from Chetty et al. (2018). Panel (a) shows the effect on having any positive wage income in 2015, when the sample is 32 to 37 years old. At the first percentile of the parental income distribution, regional public universities increase the probability of positive W-2 earnings by 1.4 percentage points, significant at the 1 percent level, which is an increase of 2.2 percent relative to the baseline. At the 25th percentile of parental income, there is a 0.6 percentage point increase, significant at the 5 percent level, which is an increase of 0.8 percent. Recall that we see a 1.4 percentage point increase in four-year degree attainment, at the 25th percentile of parental income. If the 0.6 percentage point increase in employment is driven by the 1.4 percentage point increase in education, this implies large positive employment effects on the additional degree recipients.

When we look at the family income percentile or the individual income percentile, there is an increase that is more pronounced at the low-end of the distribution and that is borderline significant at conventional levels. We find that regional public universities raise household income percentile rank of children at the 25th percentile by roughly 0.7 percentile ranks (p-value ≤ 0.1), when measuring their incomes in 2014-2015 at age 31-37. [45]

To put it in comparison to the baseline, the slope of the social mobility curve in asylum

[45] For comparison, Chetty and Hendren (2018) show that growing up in a commuting zone with one standard deviation lower racial segregation is associated with higher household income rank of children at the 25th percentile by 1.6 percentile ranks. One standard deviation lower income segregation is associated with higher rank by 1.1 percentile ranks. That is a purely correlational result, while the 0.7 percentile rank increase we identify is the causal effect of regional public universities on their local community.

Using a different set of birth cohorts, and measuring income at a different age than in our sample, Chetty and Hendren (2018) show that for the 1980-1986 birth cohorts, an increase of one percentile rank in household income at age 26 translates to an additional 818 dollars, for children whose parents were at the 25th income percentile, which is an increase in income of roughly 3.14 percent. If the relationship between percentile rank and percent increase in income holds for the slightly older individuals in our sample, our results would imply regional universities increase income by roughly 2.2 percent for children who grew up in the county with parents at the 25th income percentile.

counties (the orange dotted line) is about 0.4. Taking the point-estimates at face value, the causal effect of being assigned a normal school would be to reduce that by about 0.01, or about 2.5 percent. Both the slope and the impact are somewhat muted when focusing on individual income, with normal schools reducing the slope by about 1.5 percent.

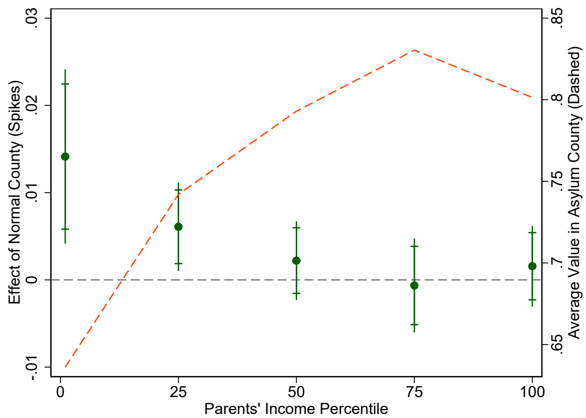
While the confidence intervals are large, it is of note that the effects on college attainment were roughly constant across parental income, but the effects on employment and income are much more pronounced for children from lower-income families. This is consistent with the additional enrollees experiencing stronger labor market benefits of college if they were from lower-income families.

Russell and Andrews (2022) looks at the effects of universities on income rank, although they estimate the effect of primarily research-intensive universities. For children born to parents at the 1st or 25th percentile, they find an increase in the mean income rank in 2014-15 of 0.003, although the effect is insignificant. This is somewhat smaller than our estimated effect of about 0.01. Interestingly, given our smaller effect on education attainment, these results could suggest a higher income return to regional universities, although there are certainly other possible mechanisms, and the estimated effects are not particularly precise for either type of university.

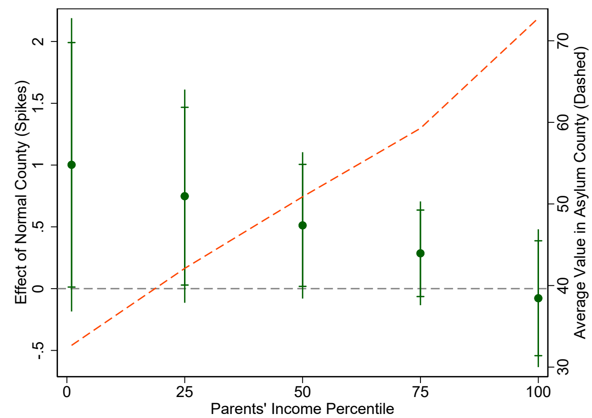
3.3.1 Comparison to causal effects on people

Subject to the same caveats as when we examined college attendance, we also compare the results in Figure 3b to the most comparable datapoint in the Chetty and Hendren (2018) dataset with causal effects on individuals, at the 25th percentile.[46] In this case, we compare our estimates to the measure of the family income percentile at age 26. The results are comparable, in terms of statistical significance, and once we multiply by 15 or 20, the point estimates are the same order of magnitude. Again, given the different cohort, measure, and weighting, as well as the large standard errors, we are hesitant to draw conclusions about

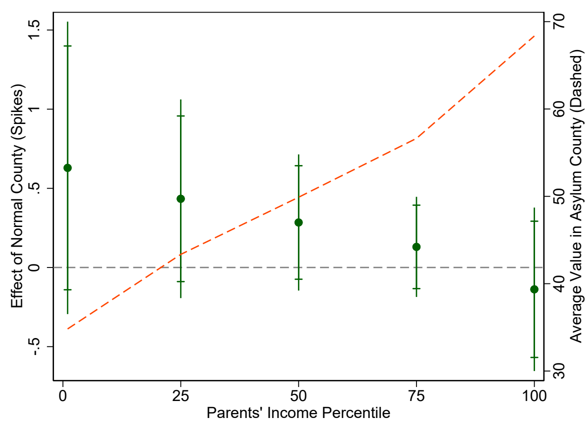
[46] The results comparing effects at the 75th percentile can be found in Appendix C.



(a) Positive W-2 Earnings, 2015



(b) Family Income Percentile, 2014-2015



(c) Individual Income Percentile, 2014-2015

Figure 3: **Effect of a normal school on income.** Green dots with spikes represent the estimated effect of a normal school on the outcome variable, conditional on state fixed effects. Outcome variables are measured at different percentiles of the parents' income distribution, which is the x-axis. The green spikes span the 95 percent confidence intervals, with the cross-bars at the 90 percent confidence intervals. The estimates and confidence intervals correspond to the y-axis on the left-hand side of the figure. The orange dashed line shows the mean of the outcome variable in asylum counties, and correspond to the y-axis on the right-hand side of the figure.

Table 3: Causal Effects on Income, 25th percentile parental income

	(1) Family Income Percentile, 2014-15	(2) Family Income Percentile, Age 26
Normal	0.748^{+}	0.0794^{+}
	(0.428)	(0.0428)
Observations	325	306
Birth Cohorts	1978-1983	1980-1986
Weights	Unweighted	Precision Weights
Scale	Per Childhood	Per Year
Interpretation	Effect on Place	Effect on Person

Standard errors clustered by state. $^{+}$ $p < 0.1$, * $p < .05$, ** $p < .01$. Outcome data in column 1 are from Chetty et al. (2018), and outcome data in column 2 are from Chetty and Hendren (2018).

the fact that the causal point estimate seems to be larger.[47]

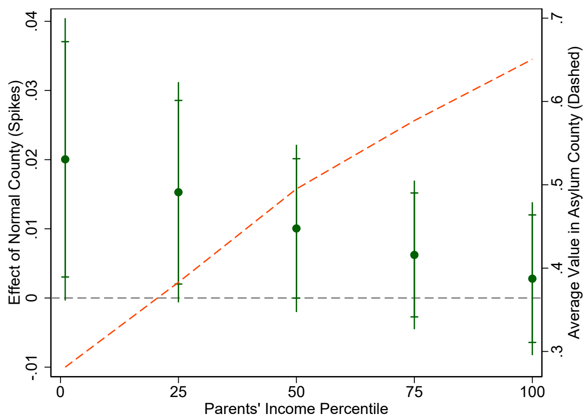
3.4 Effects on other social outcomes

We also examine the effects of being assigned a normal school on marriage, teen childbirth, incarceration, and migration. These results are presented in Figure 4. In Panel (a), we look at the effects of normal school assignment on marriage rates across the parental income distribution. Consistent with the larger effects we found on family income relative to individual income, we find positive effects on marriage in 2015 when the sample is age 32 to 37, with children born to parents in the 1st percentile being 2 percentage points more likely to get married, approximately a 7 percent increase.For the 25th percentile, the increase is 1.5 percentage points, roughly a 4 percent increase.

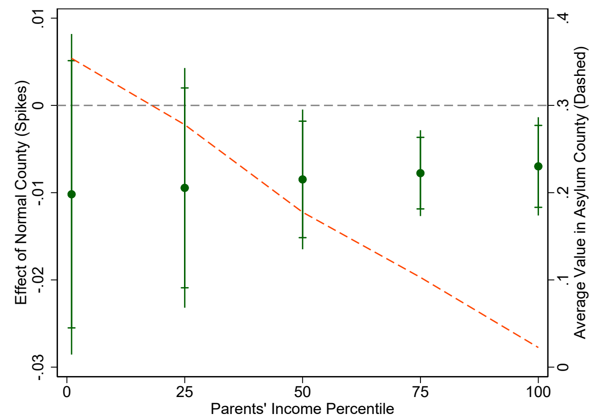
In Panel (b) we also find negative effects on teen childbirth. The point estimates are larger for children of lower-income parents, but the standard errors are also larger, so the only statistically significant results are at the top end of the distribution. However, these are large: about 1 percentage point across the distribution, off of a baseline ranging from close to 0 at the top end to about 33 percent for children of the lowest-income parents.

Panel (c) shows negative effects on incarceration. As with teen birth the point estimates

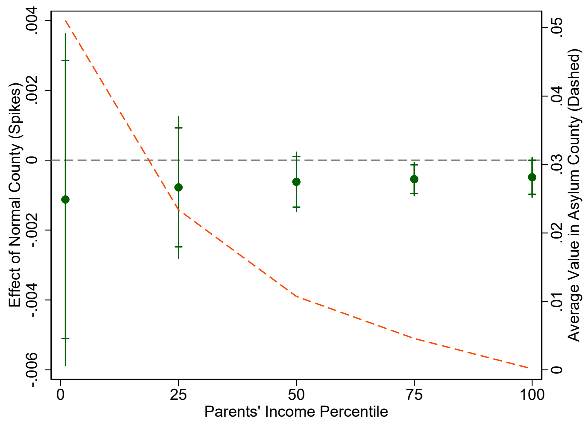
[47]See Appendix Table A4 for a breakdown of how the weighting and different datasets affect the results.



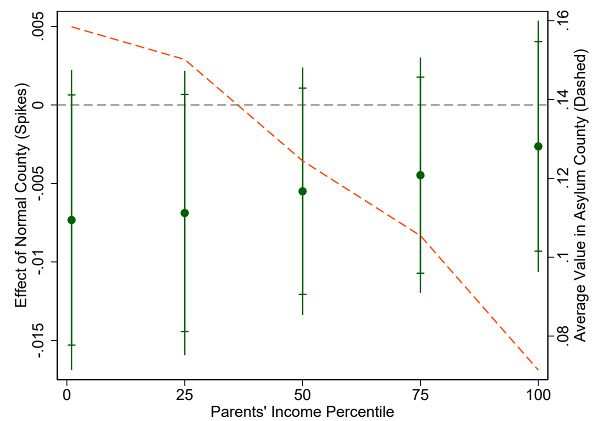
(a) Marriage, 2015



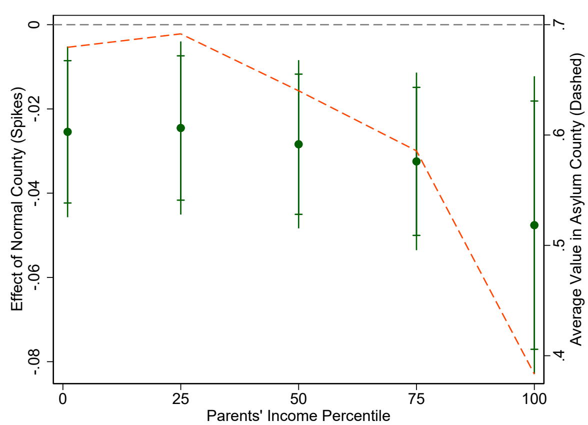
(b) Teen Birth (women-only)



(c) Incarcerated, April 1, 2010



(d) Staying with Parents, 2015



(e) Live in Childhood Commuting Zone

Figure 4: **Effect of a normal school on social outcomes.** Green dots with spikes represent the estimated effect of a normal school on the outcome variable, conditional on state fixed effects. Outcome variables are measured at different percentiles of the parents' income distribution, which is the x-axis. The green spikes span the 95 percent confidence intervals, with the cross-bars at the 90 percent confidence intervals. The estimates and confidence intervals correspond to the left y-axis. The orange dashed line shows the mean of the outcome variable in asylum counties, and correspond to the right y-axis.

are larger for children of lower-income parents, but the results are more precise for higher-income parents. For example, for children whose parents were at the 75th percentile, regional public universities reduce the fraction that were in jail on April 1, 2010 by 0.05 percentage points, from a baseline rate of 0.4 percent in asylum counties.

Panel (d) shows that children are less likely to live with their parents in 2015 if they grew up in a county that had been assigned a normal school. However, these results are not statistically significant.

Panel (e) shows that in normal school counties, children are less likely to remain in the commuting zone in which they grew up. The effect is more negative for high-income parents, despite already having a much lower baseline. These effects are large, with children in normal counties being about 3 percentage points more likely to move out of the commuting zone, and about 5 percentage points for the 100th percentile of parental income. This effect is roughly a 12 percent increase in the probability of leaving the commuting zone.[48]

3.4.1 Comparison to causal effects on people

For marriage, we can compare the results from the Chetty et al. (2018) dataset to the causal estimates on individuals in Chetty and Hendren (2018). We do this in Table 4. At the 25th percentile, there is also a causal effect on being married at age 26.[49] The result is of similar significance to the primary measure, and once we multiply by 15 or 20 to adjust for the different scales, the effects are of similar magnitudes.[50]

Unfortunately, causal estimates on individuals for the other social outcomes are not included in the Chetty and Hendren (2018) dataset.

[48] We see larger effects for children from the highest income families, even though there were not effects on educational attainment for this group. This may reflect that children of faculty and higher-level university administrators are more geographically mobile, given the likely greater geographic mobility of their parents. In asylum counties, it is less likely that the higher-income families are faculty or university administrators. As we show in Table A9, children in normal counties spent less of their childhood in the commuting zone than children in asylum counties. This is consistent with children in normal counties growing up in more geographically mobile families.

[49] The results comparing to Chetty and Hendren (2018) at the 75th percentile, rather than the 25th, can be found in Appendix C.

[50] See Appendix Table A5 for a breakdown of how the weighting and different datasets affect the results.

Table 4: Causal Effects on Marriage, 25th percentile parental income

	(1) Married, 2015	(2) Married, Age 26
Normal	1.529^{+}	0.0880^{+}
	(0.790)	(0.0468)
Observations	325	301
Birth Cohorts	1978-1983	1980-1986
Weights	Unweighted	Precision Weights
Scale	Per Childhood	Per Year
Interpretation	Effect on Place	Effect on Person

Standard errors clustered by state. $^{+}$ $p < 0.1$, * $p < .05$, ** $p < .01$. Outcome data in column 1 are from Chetty et al. (2018), and outcome data in column 2 are from Chetty and Hendren (2018).

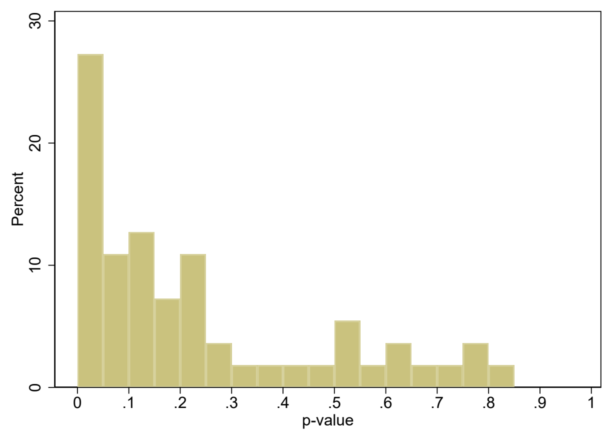


Figure 5: p-values from the 55 regressions. Each bin has a width of 0.05, so if the p-values were distributed uniformly, 5 percent would be in each bin.

3.5 Discussion of multiple hypothesis testing

To this point, we have used our empirical strategy to investigate the effect of universities on 11 different outcomes at 5 different points of the parents' income distribution. A reader may reasonably wonder which takeaways are robust to considering multiple hypothesis testing. To give a general idea of the overall significance of our results, Figure 5 shows a distribution of the p-values for the 55 outcomes. More than a quarter of the unadjusted p-values are less than 5 percent, and another tenth are less than 10 percent. Of course, this is not a formal test, but is suggestive that universities have some causal effect.

To formally show that universities matter, we implement Young (2020), a randomization-based omnibus test to see if we can reject the null hypothesis that the normal schools have no effect on any of the 55 outcomes. The p-value associated with this test is 0.023 or 0.031, depending on whether you take the randomization-c or the randomization-t value, coming from two different randomization-based test statistics outlined in Young (2020).Either way, the null hypothesis of no effect of the normal schools is rejected at conventional levels.

Given that there is *some* effect, we turn our focus to what the effect is. Before doing any econometrics, we must ask what makes this study interesting. The answer is not that regional universities affect any one particular outcome that we tested above. In our opinion, the main point of this paper is that universities affect "social mobility," i.e. they affect the common part of all these various outcomes, and that they do so particularly at the lower end of the income distribution.

Based on wanting to test "social mobility," we create a measure that is the principal component of the 11 outcomes we have previously considered: having a college degree, attending college, having a high school degree, working, the percentile of family income, the percentile of individual income, marriage, teen birth, incarceration, living at home, and living outside of their childhood commuting zone.[51] We calculate this principal component treating each county in our sample by each percentile we consider as one observation. We then see if there is an effect on this principal component at each of the five percentiles. We adjust for the fact that this is five different tests by applying the Romano and Wolf (2005) correction for adjusting p-values.

The results of this procedure are in Figure 6.[52] The confidence intervals in the Figure are not adjusted for multiple hypothesis testing. However, the p-values associated with each

[51]Our use of a principal component is distinct from Anderson (2008), who emphasized creating an index that overweights outcomes that are less correlated to the others. We are not interested in maximizing the power of our test, but think there is economic significance in the underlying factor that can explain the most variation across these eleven outcomes.

[52]The principal component has similar scoring coefficient magnitudes, between 0.22 and 0.34 for all 11 outcomes. Teen birth, jail, staying within the commuting zone, and staying at the parent's home have negative coefficients.

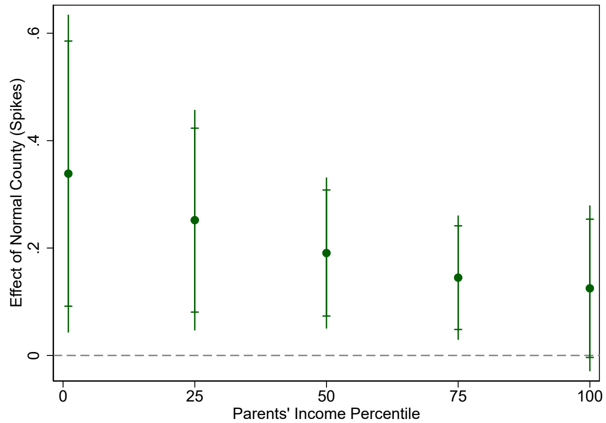


Figure 6: Principal Components

percentile, from the Romano and Wolf (2005) procedure, are for the 1st percentile, 0.043; for the 25th percentile, 0.037; for the 50th percentile, 0.026; for the 75th percentile, 0.037; and for the 100th percentile, 0.120. So even adjusting for multiple hypothesis testing, there is statistically significant evidence that universities have an effect on the principal component of these outcomes for all but the very top of the parents' income distribution.

The point estimates are bigger for children of lower-income parents, but we do not view the differences as a key aspect of our study. Whether or not universities help the outcomes of high-parental-income children, the fact that they help the outcomes of low-parental-income children implies that they improve social mobility for children that grow up near them, relative to the *national* distribution. As we discussed in the introduction, this is more of the policy purpose of the regional university, rather than whether they move up in the *local* distribution. Further, we note that college completion rates are still only 50 percent for children whose parents were at the 75th percentile of the income distribution (roughly $95,000 in 2015 dollars), and who grew up in asylum counties. Thus, even among relatively high income families, there is room for large increases in college degree attainment and economic and social mobility, and regional public universities are having an impact.

4 Mechanisms: Evidence from The Freshman Survey

Our results show that regional public universities raise the fraction of children in the local area who attend and graduate from college. In this section, we use The Freshman Survey to understand the mechanism for this result.[53]

The Freshman Survey (TFS) has been conducted since 1966 through the Higher Education Research Institute at the University of California, Los Angeles. The survey is administered by colleges to their freshman classes during orientation, and has been administered by over 1900 institutions surveying 15 million students since it began (Higher Education Research Institute, 2023).[54] The survey contains questions about why the student decided to attend college, why the student chose their university, and many individual background characteristics. Importantly, we also have the student's home zip code, which we use to identify students who grew up in normal school and asylum counties.[55] We also have the IPEDS ID for the university they attend, allowing us to identify students attending previous normal schools.[56]

We have three main objectives with TFS data. First, our education results in Figure 2 show students growing up in normal school counties are more likely to attain a college degree. With TFS data, we will show whether this is driven by enrollment at the previous normal schools in their home county. Second, we will show whether students growing up in normal

[53] We also do a more exploratory analysis of county-level covariates in Appendix E.

[54] Because universities administer these surveys during orientation, the response rate within a university is quite high. Merging TFS data with IPEDS enrollment data, median response rates are 83% (and 78% when weighting by enrollment) between 1982 and 2010.

[55] The student zip code data become available starting with the 1982 survey. Through 2000, students were asked for their address at the top of the survey, including their zip code. Starting in 2001, students were asked for their "permanent/home address", including their zip code. We will discuss, and show robustness to, the possibility that students are reporting their address at the university rather than their home address. When we refer to students who grew up in normal school or asylum counties based on TFS data, this is based on the home zip code they report on the survey. We acknowledge we do not have information on how long they may have lived at that address. We merge zip codes to counties using the CDC County Cross Reference File (Centers for Disease Control and Prevention, 1988).

[56] Our restricted-access data contain only a subset of the variables in the public-access data, including importantly the IPEDS ID and TFS ID of the student's university. Thus, we use the public datasets with the full set of variables, after merging them to the IPEDS ID of the university using our restricted-access data (by merging on TFS university ID and year). See Appendix H for details.

school counties have characteristics consistent with them being more marginal college-goers, consistent with the greater college enrollment among people growing up in normal school counties in Figure 2. Finally, we will provide insight into whether regional universities have greater effects on nearby students because the financial costs are lower for nearby students, the information frictions are reduced, or because marginal college-goers have preferences for living near home.

We first verify that the universities in normal school counties are not differentially more likely to participate in TFS relative to universities in asylum counties. This would bias our analysis of whether students growing up in normal school counties are more likely to attend college near home. Details are in Appendix H.1.

Having found no evidence of differential university selection into the survey, we then test whether individuals from normal school counties are more likely to attend university close to where they grew up, relative to individuals from asylum counties in the same state and year. Specifically, we estimate the following regression, clustering standard errors at the level of the student's home county:

$$y_{it} = \beta \text{Normal}_{it} + \alpha_{st} + \epsilon_{it} \tag{2}$$

In our baseline regression, y_{it} is an indicator for whether individual i reported that their university is within 10 miles of where they grew up. This is based on a survey question from TFS in which there are five bins of home-university distance. Here, the variable Normal_{it} is an indicator for whether individual i, responding to the survey in t, reports a home zip code that is in a normal school county. The regression includes only individuals responding to TFS who report home zip codes located in normal school or asylum counties. As in previous regressions, we include home state-year fixed effects, implying we compare individuals growing up in the same state, and responding to TFS in the same year.

In addition to testing whether a student is attending a university within ten miles of home, we also test whether they are attending any university that is in the same county as their home. We show both measures of proximity because students are asked directly their

Table 5: Differential Likelihood of Attending University Close to Home

	(1) Attend Univ. within 10mi	(2) Attend Former-Normal within 10mi	(3) Attend Univ in county	(4) Attend Former-Normal in county
Grew up in normal school county	0.0499***	0.0490***	0.121***	0.120***
	(0.0150)	(0.00600)	(0.0230)	(0.0127)
Observations	3568915	3554527	3644600	3633963

Standard errors clustered by county. * p<0.05, ** p<0.01, *** p<0.001

Observations are at the individual level. All regressions include state-year fixed effects.

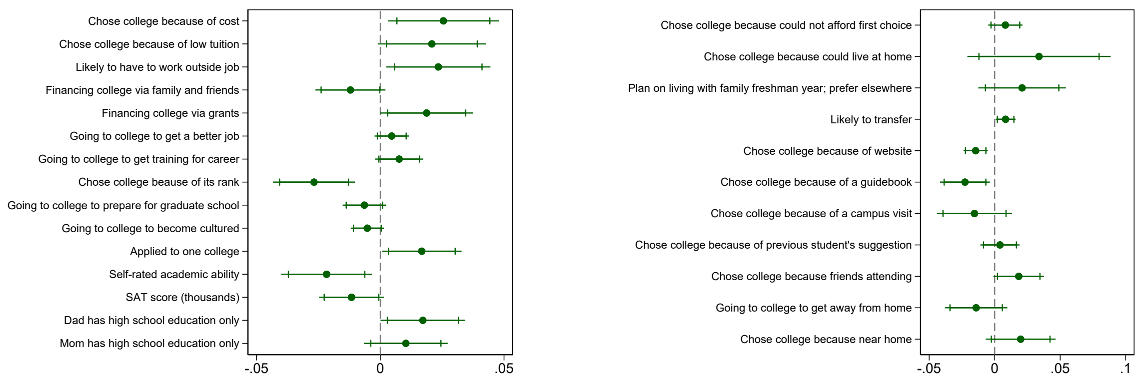
home-university distance, but we have to infer their county based on the zip code they list on the survey. While the county results are more closely related to our previous findings, the within-10-miles results are more robust to concerns about zip code misreporting. We show the results where y_{it} is dependent on attending a previous normal school in order to show how much the presence of those schools drive the results.

With our sample restrictions, the dataset consists of over 2.1 million individuals growing up in all 204 normal school counties, attending 1466 universities from 1982-2010, and nearly 1.5 million individuals growing up in all 126 asylum counties attending 1431 universities.

Among students who grew up in normal school counties, the likelihood of attending a university within 10 miles of where they grew up is higher by 5 percentage points, relative to individuals growing up in asylum counties in the same state and responding to TFS in the same year (Table 5, column 1). All of this effect is driven by students that are attending the former-normal school within 10 miles (column 2). When we consider the alternative measure of whether students are attending a university in their home county, the fraction among students growing up in normal school counties is 12 percentage points higher (column 3). Again, this is entirely driven by students attending the former-normal school (column 4).[57]

The Freshman Survey is especially rich in its survey questions on student demographics, why students chose to go to college, and why they chose their college. We use these questions to better understand the channels through which regional universities raise local college-

[57] Appendix Table A12 shows differential likelihood of home-university distance falling within each of the distance bins listed on the survey.



(a) Characteristics of Students

(b) Geographic Frictions of College Attendance

Figure 7: Differential answers to The Freshman Survey by students who grew up in normal counties relative to same-state same-year students who grew up in asylum counties. For questions that are not naturally binary, we create a dummy variable if the student answered that the reason was "Very important/good" or "Somewhat important/good." Spikes are 95 percent confidence intervals and cross-hatches are 90 percent confidence intervals. Standard errors clustered by county.

going.

We estimate equation (2), using as the dependent variables the TFS questions of interest. We emphasize that the coefficients β reflect differences in who chooses to attend college (and which college) in normal school versus asylum counties, along with any effects of the regional university on characteristics of children growing up nearby. For many of the regressions, the most natural interpretation is that the regional university is changing the composition of students that attend college, but it is not the only possible interpretation.

Our results are presented in Figure 7. Many questions ask students to rank something as important on a three point scale. In these figures, we create an indicator variable for either "Very important" or "Somewhat important" and use that as the outcome variable.[58] In each figure, we plot the point-estimate of the coefficient on growing up in a normal school county, along with 95 percent confidence intervals. Given the many outcomes we look at and that these results are primarily interesting because they help elucidate the mechanisms in the previous sections, we do not emphasize the statistical significance of our findings.[59]

[58]In Appendix H.3, we show the results for a dummy variable just using "Very important."

[59]In fact, in this section, we include only a subset of results that we looked at because we felt they were

The standard errors are not corrected for multiple hypothesis testing.

The evidence in Panel (a) is consistent with the regional universities attracting more local students who are at the margin of going to college, whether for financial, social, or academic reasons. This is consistent with our results in Figure 2 that more students in normal school counties go to college. Students are more likely to rate the cost—and in particular the low tuition—as the reason they chose their college. They are more likely to think there is a good chance they will have to work an outside job, and they are less likely to be paying for college with money from friends and family, but more likely to be paying with grants. The reasons that they are going to college are more likely to be to get a better job or to get training for a career. However, they are less likely to choose a college based on its academic rank, and they are less likely to say that they are going to college because they want to go to graduate school or to become cultured. They are more likely to have applied to one college, and they rate themselves lower in terms of academic ability, which is consistent with their lower SAT scores. Both their mom and their dad are more likely to only have a high school education.

These estimates are additional evidence that the education effects in Section 3.2 are not driven by higher-academic-ability students, or more economically-mobile families, moving into normal school counties prior to college. If that were the case, we might expect students growing up in normal school counties to look like they are less on the margin of going to college.

In Panel (b), we use the survey to potentially disentangle why regional universities are better at reaching students in their own counties than in the same-state asylum counties. We can think of three main hypotheses for why this geographic friction exists. First, having a university nearby may relax the financial burden of attending college, especially if the student can live at home.[60] Second, having a university nearby may decrease information frictions

most elucidating of our mechanisms. All the questions that we looked at can be found in Appendix H.3. We did not include them all in this section because many questions are at least partially redundant and the sheer number of results would make it harder to interpret them. We think the qualitative conclusions that we suggest based on the results in the main text are generally consistent with all the results in the appendix.

[60]Living close to home during college may also ease financial burden, even if not living at home, if this makes it easier for parents to continue providing for students' needs (perhaps most notably food and access

about colleges. Third, people may simply prefer to live near home. We note that if the channel is information or financial costs, this raises the potential for policies or interventions to target students who do not live near universities. However, if the channel is through preferences for living at home, these policies will be less effective.

We see that freshmen from normal school counties are more likely to say that they could not afford their first choice, consistent with the hypothesis that the nearby university eases financial constraints. Furthermore, we see that students from normal school counties are more likely to live at home, consistent with proximity lowering costs. However, it is also consistent with marginal college students preferring to live at home. We also see that they rate it as more likely that they will end up transferring. This could be because the nearby regional university is a more affordable way to get the first few years of college education, especially if students live at home, while still preserving the option of a degree from somewhere else.

We additionally see some evidence that growing up next to a regional university reduces information frictions about college. Freshmen from normal school counties are less likely to say they chose their college because of a website, a guidebook, or a campus visit. These students may not rely on these sources because they already have sufficient information about their local college. In addition, they are more likely to say they made their college choice because a previous student suggested it.[61] They are also more likely to say that they chose it because they have friends attending, which could be due to information, though it may also reflect home preferences. Also consistent with marginal college students preferring to live near home, students growing up in normal school counties say that the college being near home was important to their choice, and that getting away from home was less of a reason for them to go to college.

to a car).

[61] This is more significant when looking at students who answered that a previous student's suggestion was "very important" (see Appendix H.3).

5 Conclusion

Regional public universities were established to improve access to higher education in their local communities, thereby improving economic and social mobility. Using a novel strategy and rich data from Opportunity Insights, we show that regional public universities do have these impacts on their counties, with effects on high school graduation and college attainment, employment, household income, marriage, and geographic mobility. These effects are large for children from lower-income families. We also show suggestive evidence that these causal effects on the counties are driven by causal effects on people, rather than operating only through sorting.

While there are many costs and benefits to consider when allocating university funding, we provide insights on a key set of benefits of regional public universities that are central to their mission. These results also provide evidence on the types of places that generate positive outcomes for children from lower-income families.

Our results present important questions for policymakers and future research. The local impact of these universities raises the question of whether they are located optimally, if their objective is to help low-income individuals. We showed that these universities are located in communities with underrepresentation of the lowest-income families, and over-representation of middle-income families.[62] Expanding to lower-income communities will likely have general equilibrium effects, but this seems like an important area for future consideration.

Second, how should policymakers address individuals who do not benefit from proximity to one of these institutions? We show that individuals in our control set of counties have access to a greater number of four-year private institutions. Are they less likely to attain a four-year degree because these private institutions are smaller, more expensive, have less outreach to lower-income families, or do not have the types majors or training they desire?[63]

[62] Hillman (2016) studies the location of colleges relative to racial, demographic, and economic characteristics of the local area.

[63] When policymakers were considering how to address the growing demand for higher education in the mid-20th century, one area of discussion was having the government contribute to private universities' ability to increase access, especially in areas where public universities would not reach (Mayhew, 1969).

Are these individuals less likely to enroll in the farther regional public universities because of migration frictions, or information frictions about their offerings or costs? While we cannot rule out the importance of preferences for living near home, our analysis suggests proximity to a regional university reduces financial costs and information frictions about college, both of which suggest the potential for policy to target assistance to students in underserved areas.[64]

[64] A number of studies have analyzed information interventions to increase college attendance among low-income high-achieving students. For example, Dynarski et al. (2021) finds positive impacts of personalized e-mails to students from the University of Michigan that clarified the costs of attendance. Andrews, Imberman and Lovenheim (2020) finds positive impacts of UT-Austin's recruiting program at high schools in low-income areas, but no enrollment impacts of Texas A&M's high school recruiting program.

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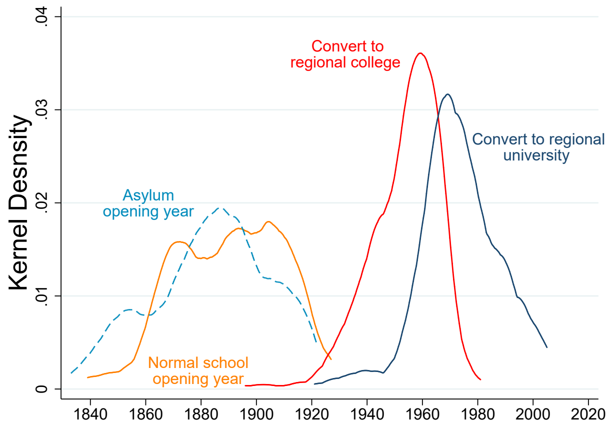
A Details on Normal School and Asylum History

Figure A1 and Table A1 are reproduced from Howard, Weinstein and Yang (2022).

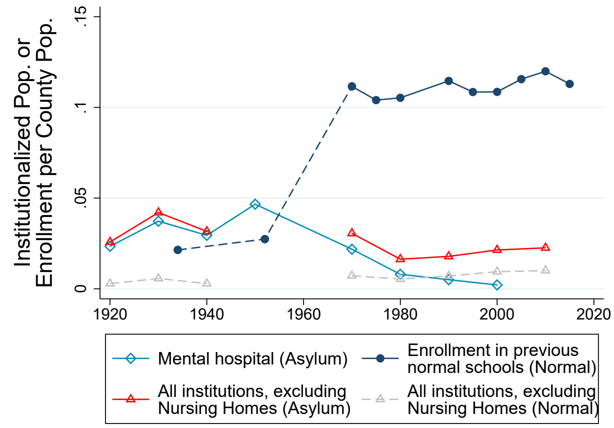
In Figure A1, we show the timeline of the opening and conversion of normal schools, compared to asylum counties (Panel a), as well as the statistics on the size of these institutions over time (Panel b). We also include a map of the institutions to show that both normal schools and asylums were common across the entire country (Panel c).

Table A1 shows the effects that normal schools had on the size of the higher-education sector in the counties, showing that normal school counties have more public four-year colleges, and the colleges have higher enrollment and more degrees awarded per population. The normal counties also have a higher share of the population with a bachelor's degree. While there are some insignificant negative effects on other types of universities, these universities are small, so the net effect is still a much larger university presence, when measured by enrollment or degrees, even if that is not the case when measured by the total number of colleges.

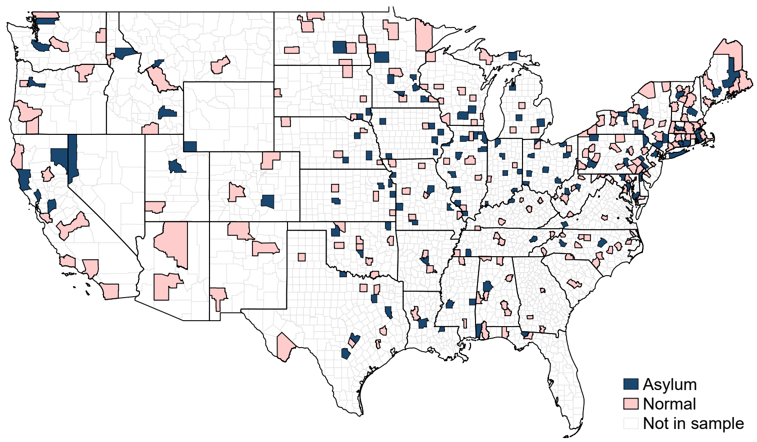
Table A2 shows balance on characteristics in 1850. Normal counties are smaller in population in 1850, and there is some evidence they are less urban and have lower real estate values per capita in 1850. We note that while there is not a statistically significant difference in 1850 population levels, using log population yields a coefficient of -.31, statistically significant at the 5% level. As we show in Howard, Weinstein and Yang (2022), there is not a statistically significant difference between normal and asylum counties in log population in 1920, when we have data on all states. There is also not a significant difference in log population in 1840. Finally, we note that there is an extreme outlier in terms of 1850 population: New York County. Omitting this county yields a statistically insignificant coefficient of -785 in row 1, and a mean of 22,674 in column 2 row 1.



(a) Asylum and Normal School Opening Years



(b) Asylum and Normal School Size Over Time



(c) Locations of Normal Schools and Asylums

Figure A1: **History of Normal Schools and Insane Asylums.** *Notes*: Figure (a) shows opening years for normal schools and asylums. We use an Epanechnikov kernel with a five-year bandwidth for density estimation. The year in which previous normal schools convert to state colleges and state universities is defined to be the year that the school's name changes to college and university respectively. Figure (b) shows average enrollment in normal schools (or in colleges that had been normal schools) per county population in normal counties. We also show average institutionalized population per county population for both normal and asylum counties. Depending on the year, institutionalized population includes population in mental institutions, correctional institutions, institutions for the elderly, handicapped, and poor, juvenile facilities, and nursing/skilled nursing facilities. College enrollment in Maine and Vermont is missing in 1952; however, using a balanced sample yields a similar figure. Figure (c) shows a map of the locations of the normal and asylum counties in our sample. See the Appendix in Howard, Weinstein and Yang (2022) for data sources.

Table A1: County-level Higher Education Sector, 1980

	(1)	(2)	(3)
	Variable Means		Difference in Means With State FE
	Normal	Asylum	(1) - (2)
Has regional college formerly normal school	0.91	0.00	0.93**
	(0.29)	(0.00)	(0.02)
Total public four-year colleges	1.11	0.44	0.69**
	(0.67)	(0.88)	(0.12)
Total private four-year colleges	1.39	1.94	-0.45
	(3.27)	(4.62)	(0.53)
Total two-year colleges	0.97	1.16	-0.22
	(2.17)	(2.17)	(0.31)
Enrollment as % of population	11.72	4.56	8.41**
	(9.23)	(5.51)	(1.59)
Full-time enrollment as % of population	8.52	2.97	6.48**
	(7.4)	(4.34)	(1.26)
Total degrees awarded as % of population	3.04	0.93	2.47**
	(2.77)	(1.41)	(0.5)
Bachelor's degrees awarded as % of population	1.43	0.39	1.23**
	(1.38)	(0.69)	(0.25)
% Population over 25 with Bachelor's degree	16.57	15.02	2.04*
	(4.79)	(6.1)	(0.86)
% Population over 25 with 1-3 years college	15.40	15.01	0.57
	(3.89)	(3.97)	(0.35)

Notes: Source: Howard, Weinstein and Yang (2022). Columns (1) and (2) show means and standard deviations in parentheses. For panel A, column (1) includes 204 normal counties, and column (2) includes 126 asylum counties. Panel A data are constructed using IPEDS, except the bachelor's share and some-college share which are from the Census, obtained from NHGIS. Column (3) displays coefficients from regressing each variable on the normal county indicator with state fixed effects, clustering standard errors at the state level. $^{+}$ $p < 0.1$, * $p < .05$, ** $p < .01$.

Table A2: County Characteristics in 1850

	Normal	Asylum	Within-State Difference
Population, 1850	23,187	27,460	-5,746
	(32,218)	(55,570)	(5,671)
Proportion of population, 1850:			
Urban, 2500 and over	0.09	0.13	-0.04^{+}
	(.19)	(.22)	(0.02)
In cities, 25,000 and over	0.03	0.04	-0.01
	(.13)	(.18)	(0.02)
Non-white, free	0.01	0.02	-0.001
	(.03)	(.03)	(0.002)
Non-white, slave	0.12	0.07	0.01
	(.19)	(.15)	(0.01)
Farmer	0.43	0.44	0.02
	(.24)	(.24)	(0.03)
Real estate value per capita	224.67	245.35	-25.69^{+}
	(159.34)	(220.47)	(12.77)

Notes: Columns 1 and 2 show mean and standard deviation of county characteristics for normal and asylum counties. Column 3 shows the coefficient on normal county, when the dependent variable is the county characteristic, and we include state fixed effects. We show standard errors clustered at the state level in parentheses in column 3. There are 162 normal counties and 102 asylum counties. We restrict the 1850 samples to counties covered in the 1850 complete census from IPUMS USA. We use the Eckert et al. (2020) crosswalk to 1990 counties. When using log population in 1850 as the dependent variable in column 3, the coefficient on normal county is -.31, statistically significant at the 5% level. Fraction of the population that is a farmer is the fraction of the males who are at least 15, and not living in group quarters. Real estate value per capita is the sum of all real estate value owned by individuals in the county (not living in group quarters), divided by the total non-group-quarters population. See Howard, Weinstein and Yang (2022) for balance on other variables in 1840 and in 1920.
$^{+}$ $p < 0.1$, * $p < .05$, ** $p < .01$

B Detail on Comparing Chetty et al. (2018) and Chetty and Hendren (2018) Results

In Table A3, we show alternative specifications for Table 2. In the main text, we used separate weighting schemes for the baseline results using the Chetty et al. (2018) data, in which the regressions were unweighted, and the causal effects on people results using the Chetty and Hendren (2018) data, in which the results were weighted with precision weights. These are reproduced in columns (1) and columns (5) in Table A3.

In this section, we additionally show three alternative specifications. Column (2) uses the same data as Column (1) but the weights from Column (5). The point estimate is slightly lower, but the standard error increases by a factor of 2. The very large increase in standard error is why we do not prefer this regression for our main specification. The point estimates are also not very different. Column (4) uses the same data as Column (5), but is unweighted as in Column (1). While columns (1) and (4) are both unweighted, a few observations have an outcome in the Chetty et al. (2018) data (column 1) but not the Chetty and Hendren (2018) data (column 4), so implicitly those counties get zero weight in column (4). Here, the point estimate also falls slightly, but the standard errors also increase. Chetty and Hendren (2018) suggests that the weights are necessary to account for the fact that some of the coefficients are quite noisy, so we prefer Column (5) as our main specification. Finally, as another check on the comparability of the two datasets, we also look at college attendance as measured in Chetty and Hendren (2018), but using the sample of permanent residents. Using this sample the effect can be interpreted as an effect on the place, and is measured per childhood, not per year. The differences between Columns (1) and (3) are how college attendance is measured, and also that Column (1) included people that lived in the county for part of their childhood, weighted to reflect how many years they spent there. Column (3) is a bit noisier, but the point estimate is actually larger, and still statistically significant. Overall, this exercise justifies why we prefer Columns (1) and (5): because they maximize power, but also shows that the positive point-estimates seem to be robust to alternative specifications.

Table A3: Effect on College Attendance, 25th percentile parental income, Robustness

	(1) Some College, Age 25+	(2) Some College, Age 25+	(3) Attended College, Age 18-23	(4) Attended College, Age 18-23	(5) Attended College, Age 18-23
Normal	1.398^{*}	0.829	1.866^{*}	0.0751	0.139^{+}
	(0.672)	(1.218)	(0.843)	(0.0892)	(0.0749)
Observations	325	306	325	306	306
Weights	Unweighted	Precision Weights	Unweighted	Unweighted	Precision Weights
Scale	Per Childhood	Per Childhood	Per Childhood	Per Year	Per Year
Interpretation	Effect on Place	Effect on Place	Effect on Place	Effect on Person	Effect on Person

Standard errors clustered by state. $^{+}$ $p < 0.1$, * $p < .05$, ** $p < .01$. Outcome data in columns 1 and 2 are from Chetty et al. (2018), and outcome data in columns 3-5 are from Chetty and Hendren (2018).

Table A4 shows a similar analysis for income, and is an expanded version of Table 3. Again, the alternative weighting schemes which make the regressions in Columns (1) and (5) more comparable are also less powerful, and while the point-estimates are still positive, they are not statistically significant. In this case, the point estimate on permanent residents in Column (3) does differ from our main results, but we cannot rule out a positive effect.

Table A4: Effect on Income Percentile, 25th percentile parental income, Robustness

	(1) Family Income Percentile, 2014-15	(2) Family Income Percentile, 2014-15	(3) Family Income Percentile, Age 26	(4) Family Income Percentile, Age 26	(5) Family Income Percentile, Age 26
Normal	0.748^{+}	0.459	-0.0943	0.00317	0.0794^{+}
	(0.428)	(0.811)	(0.453)	(0.0973)	(0.0428)
Observations	325	306	325	306	306
Weights	Unweighted	Precision Weights	Unweighted	Unweighted	Precision Weights
Scale	Per Childhood	Per Childhood	Per Childhood	Per Year	Per Year
Interpretation	Effect on Place	Effect on Place	Effect on Place	Effect on Person	Effect on Person

Standard errors clustered by state. $^{+}$ $p < 0.1$, * $p < .05$, ** $p < .01$. Outcome data in columns 1 and 2 are from Chetty et al. (2018), and outcome data in columns 3-5 are from Chetty and Hendren (2018).

Table A5 shows a similar analysis for marriage, and is an expanded version of Table 4. As with the other two outcomes in Tables A3 and A4, the alternative weighting schemes which make the regressions in Columns (1) and (5) more comparable are also less powerful, and while the point-estimates are still positive, they are not statistically significant. In this case, the point estimate on permanent residents in Column (3) is also positive, but not statistically significant.

Table A5: Effect on Marriage, 25th percentile parental income, Robustness

	(1) Married, 2015	(2) Married, 2015	(3) Married, Age 26	(4) Married, Age 26	(5) Married, Age 26
Normal	1.529^{+}	0.959	0.316	0.152	0.0880^{+}
	(0.790)	(1.694)	(0.778)	(0.201)	(0.0468)
Observations	325	301	325	306	301
Weights	Unweighted	Precision Weights	Unweighted	Unweighted	Precision Weights
Scale	Per Childhood	Per Childhood	Per Childhood	Per Year	Per Year
Interpretation	Effect on Place	Effect on Place	Effect on Place	Effect on Person	Effect on Person

Standard errors clustered by state. $^{+}$ $p < 0.1$, * $p < .05$, ** $p < .01$. Outcome data in columns 1 and 2 are from Chetty et al. (2018), and outcome data in columns 3-5 are from Chetty and Hendren (2018).

C Causal Results at the 75th Percentile of Parental Income

In this appendix, we show the same tables as Tables 2, 3, and 4, but for children born to parents at the 75th percentile rather than the 25th percentile (Tables A6, A7, and A8, respectively). For every outcome, the effect using the Chetty and Hendren (2018) data are insignificant. These outcomes were also insignificant using the Chetty et al. (2018) measures, except for the some college measure, which was marginally significant. Once applying the appropriate rescaling (multiplying the "effect on person" results by between 15 and 20), the confidence interval in column (2) would be so large that it includes the point estimate in Column (1).

Table A6: Causal Effects on College Attendance, 75th percentile parental income

	(1) Some College, Age 25+	(2) Attended College, Age 18-23
Normal	0.835^{+}	0.0115
	(0.473)	(0.0468)
Observations	325	306
Weights	Unweighted	Precision Weights
Scale	Per Childhood	Per Year
Interpretation	Effect on Place	Effect on Person

Standard errors clustered by state. $^{+}$ $p < 0.1$, * $p < .05$, ** $p < .01$. Outcome data in column 1 are from Chetty et al. (2018), and outcome data in column 2 are from Chetty and Hendren (2018).

Table A7: Causal Effects on Income, 75th percentile parental income

	(1) Family Income Percentile, 2014-15	(2) Family Income Percentile, Age 26
Normal	0.286	0.0160
	(0.208)	(0.0426)
Observations	325	306
Weights	Unweighted	Precision Weights
Scale	Per Childhood	Per Year
Interpretation	Effect on Place	Effect on Person

Standard errors clustered by state. $^{+}$ $p < 0.1$, * $p < .05$, ** $p < .01$. Outcome data in column 1 are from Chetty et al. (2018), and outcome data in column 2 are from Chetty and Hendren (2018).

Table A8: Causal Effects on Marriage, 75th percentile parental income

	(1) Married, 2015	(2) Married, Age 26
Normal	0.623	0.0153
	(0.533)	(0.0645)
Observations	325	301
Weights	Unweighted	Precision Weights
Scale	Per Childhood	Per Year
Interpretation	Effect on Place	Effect on Person

Standard errors clustered by state. $^{+}$ $p < 0.1$, * $p < .05$, ** $p < .01$. Outcome data in column 1 are from Chetty et al. (2018), and outcome data in column 2 are from Chetty and Hendren (2018).

D Historical measures of educational mobility

In this section we show that the likelihood of school attendance in 1850 increases with father's real estate value. This suggests that the fraction of children in the county attending school, among those with fathers whose real estate value is below the median, is reflective of the extent of upward mobility in the county.

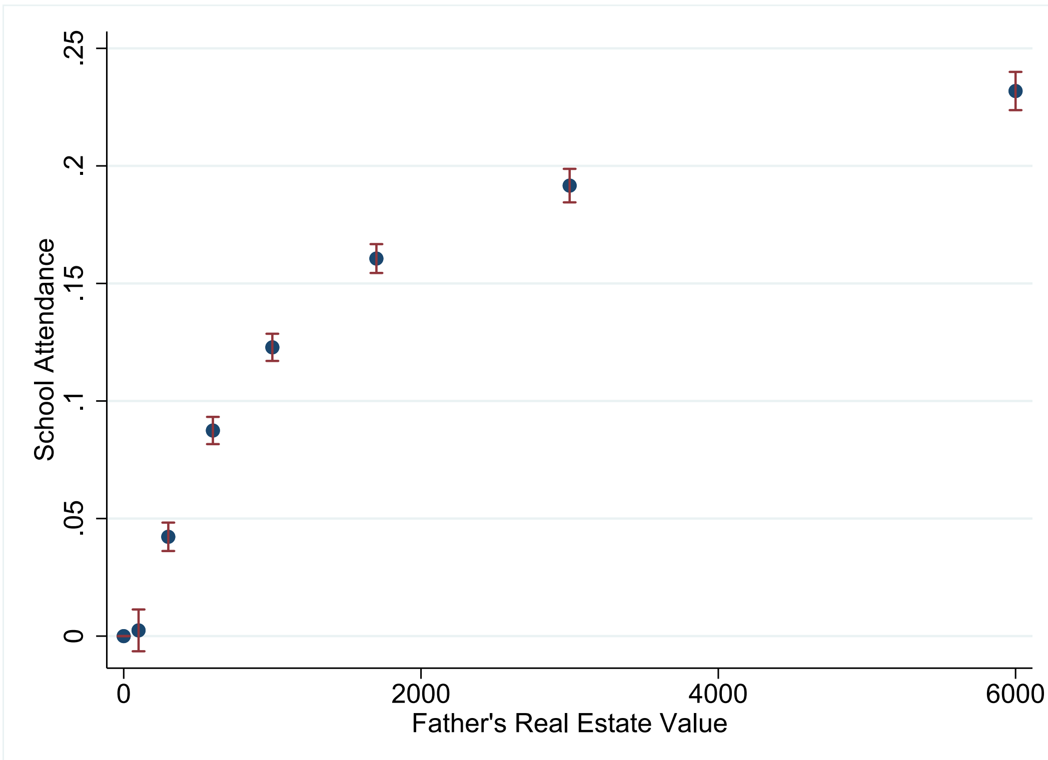


Figure A2: **School Attendance of White 14-17 Year Olds in 1850 by Father's Real Estate Value, relative to those whose fathers have real estate value of zero, with county fixed effects**. Estimates are from a regression of an indicator for school attendance on indicators for deciles of father's real estate value, and including county fixed effects. Sample includes white teens aged 14 to 17 who were living with their father.

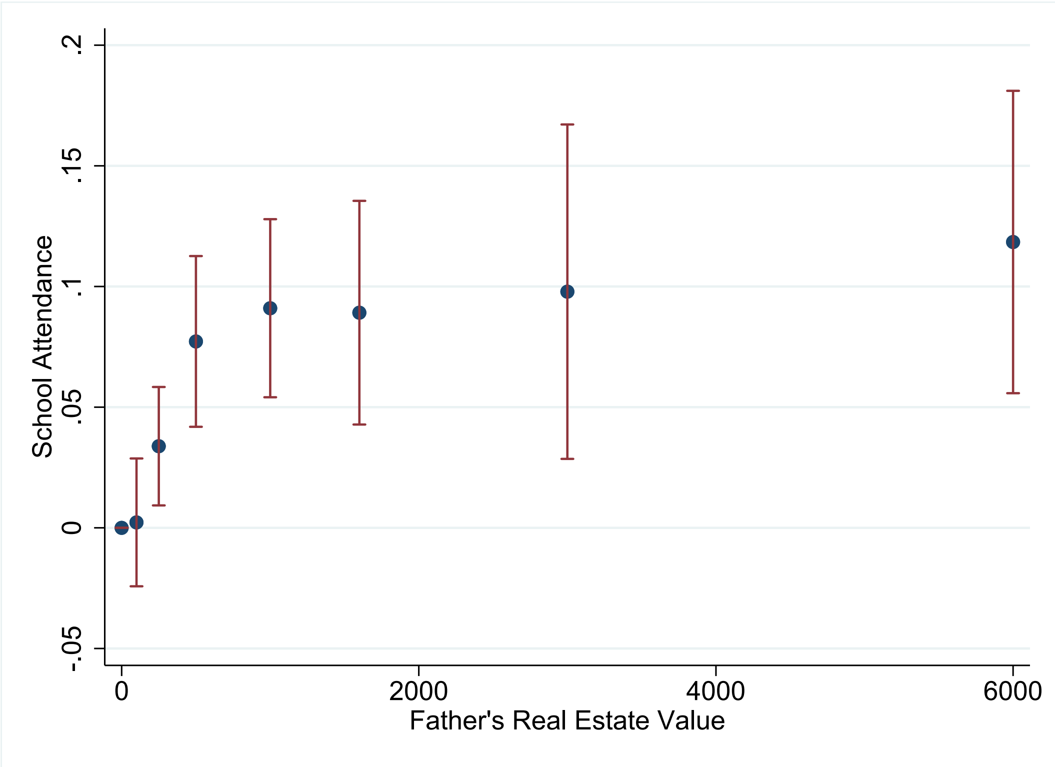


Figure A3: **School Attendance of Non-Slave Black 14-17 Year Olds in 1850 by Father's Real Estate Value, relative to those whose fathers have real estate value of zero, with county fixed effects**. Estimates are from a regression of an indicator for school attendance on indicators for deciles of father's real estate value, and including county fixed effects. Sample includes non-slave black teens aged 14 to 17 who were living with their father.

E Covariates of normal schools using data from Chetty et al. (2018) and Chetty and Hendren (2018)

We find that regional public universities affect education and mobility in their local communities. The most natural explanation is that these universities reduce geographic frictions in college attendance, and this affects college attainment as well as income mobility and social outcomes. However, regional public universities may impact these outcomes through other channels as well. For example, regional universities may impact local economic outcomes, such as industrial composition, in ways that increase the return to education in the local community, and this may increase high school and college attainment. Regional universities may also affect other characteristics of the local community, such as the income distribution or family composition, which may affect mobility directly or indirectly, for example through affecting primary and secondary school quality. For suggestive evidence on the importance of these potential channels, we test for differences between normal and asylum counties on a number of characteristics related to these mechanisms, using data from Chetty et al. (2018) and Chetty and Hendren (2018).[65]

Consistent with Howard, Weinstein and Yang (2022) we find very little difference in economic characteristics between normal and asylum counties within states.[66] The manufacturing share is slightly lower in normal counties, and in Howard, Weinstein and Yang (2022) we show that in normal counties the employment share in accommodations and food services is about 1 percentage point higher (significant at the 1 percent level), the share in retail trade is higher by about 0.6 percentage points, and the share in wholesale trade and finance and insurance are both lower by about 0.4 percentage points. These differences are small, and none of them suggest jobs with a higher return to college degrees in normal counties. There is no difference in wage growth for high school graduates, or overall job

[65]We focus on variables that do not come from the Census, given that students are included in the Census in the location where they live as students, and this will affect per capita estimates.

[66]We also find insignificant differences in racial and income segregation indices from Chetty and Hendren (2018).

Table A9: Chetty et al. (2018) and Chetty and Hendren (2018) Covariates

	Normal	Asylum	Within-State Difference
Economic Characteristics			
Manufacturing employment share, 2000	0.13	0.15	-0.01^{+}
	(0.06)	(0.07)	(0.01)
Average annualized job growth, 2004-2013	0	0	-0.00
	(0.01)	(0.01)	(0.001)
HS grad. wage growth, 2005-2009 - 2010-2014	0.06	0.05	0.01
	(0.11)	(0.07)	(0.01)
Bachelor's degree share, age $\geq$ 25, 2000	0.24	0.22	0.02*
	(0.07)	(0.09)	(0.01)
Population, 2000	269,614	304,082	-27,213
	(765,738)	(591,179)	(91,084)
Children < 18, 2000	67,974	76,844	-7,406
	(209,691)	(152,362)	(23,965)
K-12 Public Schools and Colleges			
K-12 expenditures per stud., 1996-1997	6.38	6.39	0.01
	(1.43)	(1.43)	(0.07)
K-12 student teacher ratio, 1996-1997	16.88	17.47	-0.42*
	(2.18)	(2.16)	(0.17)
Mean 3rd grade math test scores, 2013	3.28	3.29	0.02
	(0.63)	(0.71)	(0.07)
College tuition, local colleges, IPEDS 2000	4149.01	6836.79	-2,508.13**
	(3,836.2)	(4,652.87)	(597.97)
Family characteristics, children in Chetty et al. (2018)			
Children claimed by two people			
parent income at p25	0.51	0.49	0.02*
	(0.12)	(0.12)	(0.01)
parent income at p75	0.94	0.93	0.00
	(0.04)	(0.06)	(0.01)
Fraction of childhood spent in the county	0.74	0.76	-0.01^{+}
	(0.07)	(0.06)	(0.01)

Notes: Columns 1 and 2 show mean and standard deviation of county characteristics for normal and asylum counties. Column 3 shows the coefficient on normal county, when the dependent variable is the county characteristic, and we include state fixed effects. We show standard errors clustered at the state level in parentheses in column 3. All economic variables except county population are from Chetty et al. (2018). Variables related to K-12 public schools and colleges are from Chetty and Hendren (2018), except 3rd grade math scores which are from Chetty et al. (2018). Fraction of children claimed by two people as a dependent is from Chetty et al. (2018), and is based on parents of children in the 1978-1983 birth cohorts, and parents' average household adjusted gross income in 1994, 1995, and 1998-2000. Fraction of childhood spent in the county is from Chetty et al. (2018). $^{+}$ $p < 0.1$, * $p < .05$, ** $p < .01$.

growth. As we also show in Howard, Weinstein and Yang (2022) we see higher bachelor's degree share by about 2 percentage points in normal counties. Higher bachelor's share may affect education levels of lower-income children in several ways, one of which is the quality of the local public elementary and secondary schools.

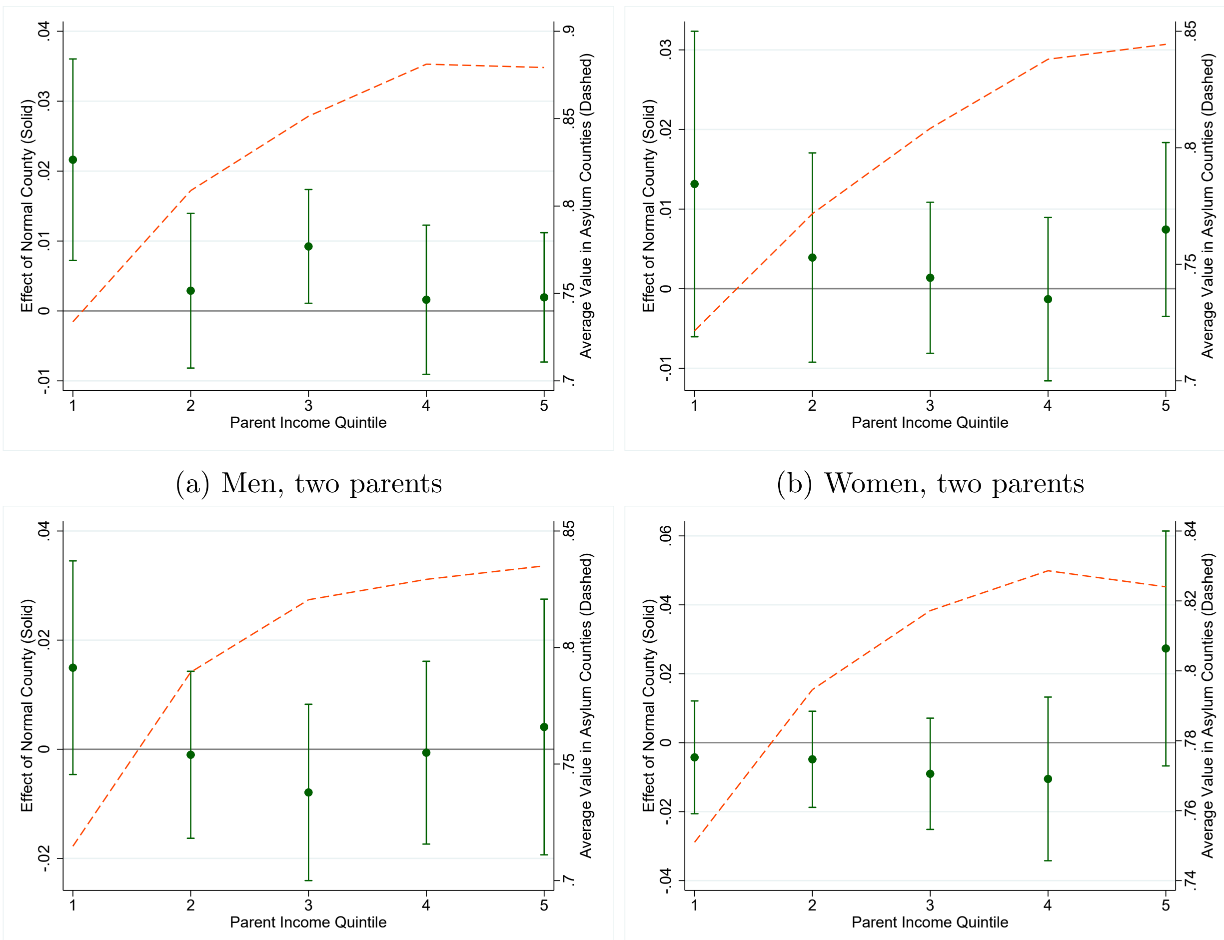
While there is no difference in expenditures per student, or in 3rd grade math scores, the student-teacher ratio is modestly lower in normal counties by about 0.4, which is approximately 2 percent lower. This may suggest other differences in local schools that affect high school graduation and college enrollment rates in normal relative to asylum counties. Consistent with regional public universities affecting outcomes by making a local college education more affordable, the tuition at colleges in the county is lower by about $2500 in normal counties, which is roughly 37 percent lower.

Children living in low-income households in normal counties are more likely to have two parents whose income together is the same as single parents' income in asylum counties. The fraction of children claimed by two people as a dependent, among those whose parents are at the 25th income percentile, is higher by two percentage points in normal counties, which is roughly 4 percent higher based on the average in asylum counties.[67] There is no difference for children whose parents are at the 75th percentile. As regional public universities raise education levels and marriage of children from lower-income families, they may also have done so for their parents. In this case, some of the effect of regional public universities on children may come through the effect they had on the previous generation.

Using data from Chetty et al. (2016), we provide suggestive evidence that the mobility effects are not driven by differences in likelihood of having two parents. These data are similar to the other outcome data we use, but further disaggregate outcomes by whether children have one or two parents who claim them on their taxes. The only outcomes available are regarding the likelihood of employment, and only disaggregated by gender. Among those

[67] This does not say that children of low-income parents are more likely to live with both parents in normal counties than asylum counties because this statement is dependent on the total income of their parents being at the 25th percentile, which is endogenous to how many parents the child has.

who have two parents claim them on their taxes, we show normal school assignment increases employment by two percentage points for men whose parents are in the first income quintile (Appendix Figure A4). The magnitude is similar for men with single parents, and one percentage point for women with two parents, though neither are statistically significant. These results suggest our main effects are not driven by differences between normal and asylum counties in likelihood of having two parents during childhood.



(a) Men, two parents

(b) Women, two parents

(c) Men, single parent

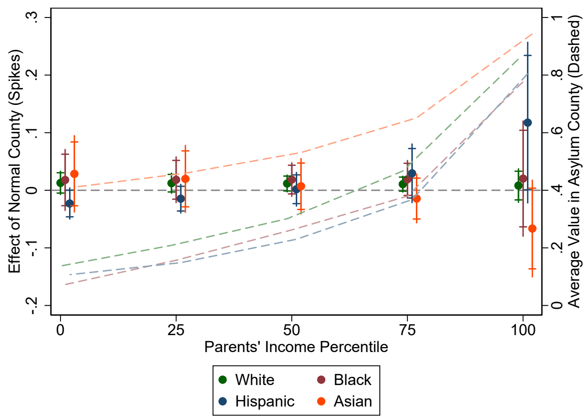
(d) Women, single parent

Figure A4: **Effect of a normal school on employment, by sex and parental structure.** Dots with spikes represent the estimated effect of a normal school on the outcome variable, conditional on state fixed effects. Outcome variables are measured at different percentiles of the parents' income distribution, which is the x-axis. The spikes span the 95 percent confidence intervals. The estimates and confidence intervals correspond to the y-axis on the left-hand side of the figure. The dashed lines show the mean of the outcome variable in asylum counties, and correspond to the y-axis on the right-hand side of the figure.

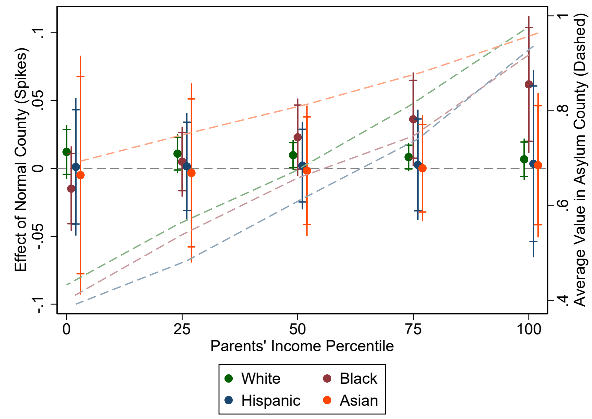
F Education Results by Race and Sex

In this appendix, we present the effect of normal schools on educational attainment, by race and gender. In Figure A5, we show the results from Figure 2, by race. The effects on high school attainment are very large for Hispanics, especially at the lower end of the income distribution. For at least some college, there is a large effect for Black children whose parents are at the top of the income distribution. And for college degrees, there is a large effect for Hispanic children with parents at the top of the income distribution. The results at the top of the distribution contrast with the results averaging across races being the least significant at the top of the distribution.

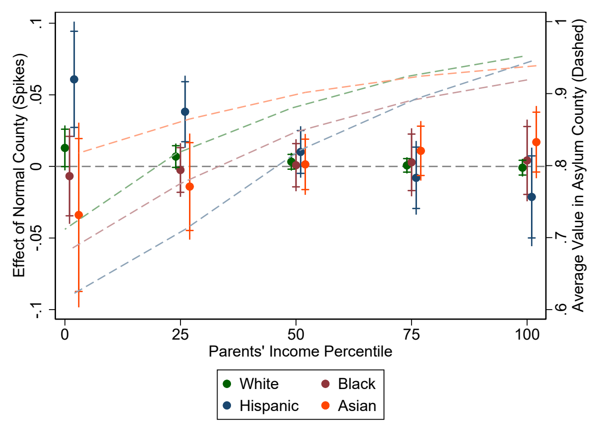
For sex, presented in Figure A6, the most interesting result is that across the income distribution, the effect on 4-year college degrees is stronger for women. For high school degrees, the result is slightly stronger for men, at least at the bottom of the income distribution.



(a) At least 4-year College Degree, Age 25 and over

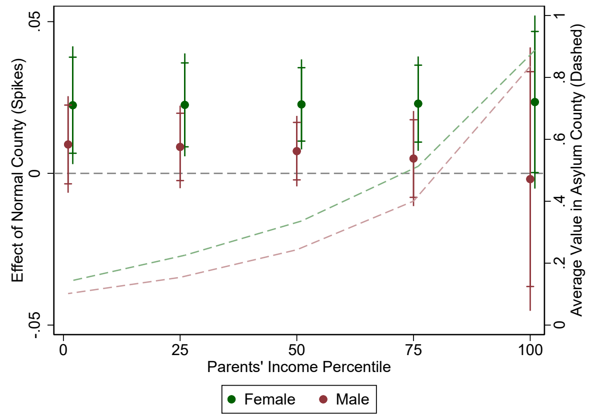


(b) At Least Some College, Age 25 and over

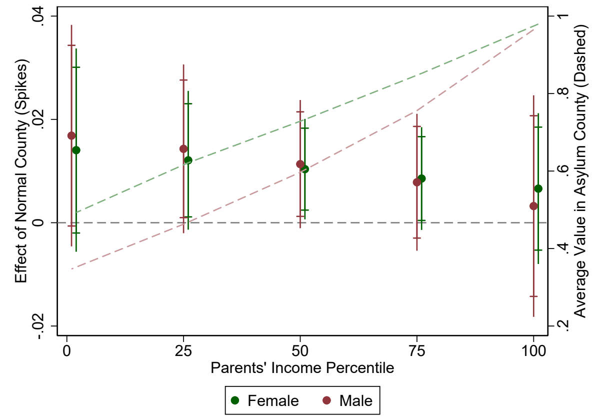


(c) At least HS Graduate or GED, Age 19 and over

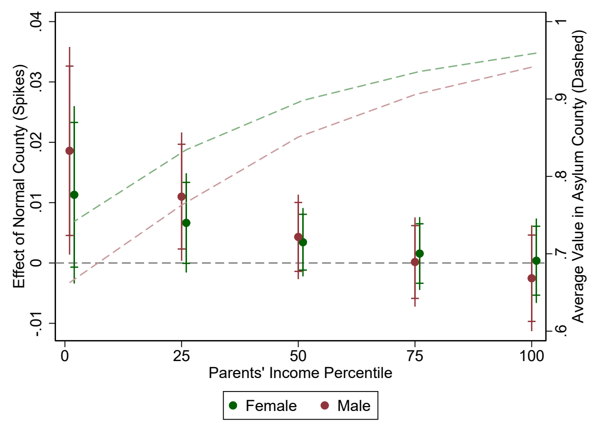
Figure A5: **Effect of a normal school on education, by race.** Dots with spikes represent the estimated effect of a normal school on the outcome variable, conditional on state fixed effects. Outcome variables are measured at different percentiles of the parents' income distribution, which is the x-axis. The spikes span the 95 percent confidence intervals, with the cross-bars at the 90 percent confidence intervals. The estimates and confidence intervals correspond to the y-axis on the left-hand side of the figure. The dashed lines show the mean of the outcome variable in asylum counties, and correspond to the y-axis on the right-hand side of the figure.



(a) At least 4-year College Degree, Age 25 and over



(b) At Least Some College, Age 25 and over



(c) At least HS Graduate or GED, Age 19 and over

Figure A6: **Effect of a normal school on education, by sex.** Dots with spikes represent the estimated effect of a normal school on the outcome variable, conditional on state fixed effects. Outcome variables are measured at different percentiles of the parents' income distribution, which is the x-axis. The spikes span the 95 percent confidence intervals, with the cross-bars at the 90 percent confidence intervals. The estimates and confidence intervals correspond to the y-axis on the left-hand side of the figure. The dashed lines show the mean of the outcome variable in asylum counties, and correspond to the y-axis on the right-hand side of the figure.

G Is there evidence that children moving into normal school regions at later ages are different than they are in asylum regions?

One threat to our analysis based on the causal estimates on individuals, from Chetty and Hendren (2018), is that it could be that different children move into counties with normal schools at different ages. In this appendix, we look for evidence of this, and do not find any.

To do this analysis, we use data from the 2005-2009 ACS to identify children of differing ages that moved into PUMAs with either a normal county or asylum county in the last year.[68]

We then look at characteristics of their parents to see if they differ on several important observable characteristics that would predict outcomes for the children. In particular, we investigate whether parents of older children have more education, more income, or more working adults, differentially in normal or asylum counties.

It is important to note that we are not claiming that there are no differences in family characteristics based on the child's age when they migrate. Of course, migrant parents with older children are going to look different than migrant parents with younger children. We also are not concerned if there are overall differences in the migrants that move into normal counties or asylum counties; this is the endogeneity that the Chetty and Hendren (2018) is supposed to overcome. What would be a concern is if migrant parents with older children look different that migrant parents with younger children in different ways, depending on whether they live in a normal or asylum county.

One issue with our approach is that the public ACS data does not identify the county reliably for our sample. So we we expand our geographic definition to be based on the public use microdata area (PUMA), which we can observe in the public data. We then

[68] 2005-2009 is a bit later than the children that are tracked in the Chetty and Hendren (2018) study. However, using the 1990 or 2000 Census would require us to base these estimates on 5-year migration, which means we cannot precisely estimate the age at which the child moves.

assign households to normal counties based on geographic crosswalks. For many counties, the PUMA exactly coincides with the county, and for many others, the PUMA is larger but has only one normal or asylum county in it. For a few, the PUMA encompasses both normal and asylum counties. Unfortunately, this biases our results to not finding anything, but we do not think there is a better approach with publicly available data.

For each county in our sample, we calculate the average log household income for all adults in the household (so as not to pick up any effects from jobs the children might have which could be affected by their future college decision), the average number of adults working in the household, and the average maximum number of years of schooling of all adults in the household. For all these variables, we consider an adult anyone over the age of 18 living in the same household.

We then run the regression

$$y_{ia} = \beta \text{Normal}_i \times a + \gamma \text{Normal}_i + \delta_{sa} + \epsilon_{ia}$$

where y_{ia} is the average value for households with children of age a in county i, a is the age of the child, and δ_{sa} is a state-age fixed effect. The unit of observation is at the county-age level.

The results are in Table A10. None of the coefficients on Normal $\times$ a are significant, which reassures us that we are not finding evidence of selection that would bias our results based on the Chetty and Hendren (2018) data.

We also separately look a specification where we look at each age individually, in case of a non-linearity that is not captured by our previous specification. We run the regression

$$y_{ia} = \beta_a \times \text{Normal}_i + \delta_{sa} + \epsilon_{ia} \tag{A1}$$

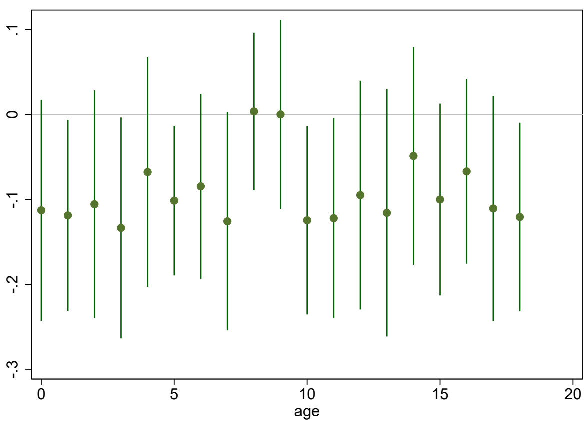
A plot of the β_a are in Figure A7. There is no clear trend or visual evidence of differential effects by age.

Table A10: Comparison of households of migrant children in normal vs. asylum counties, interacted with age

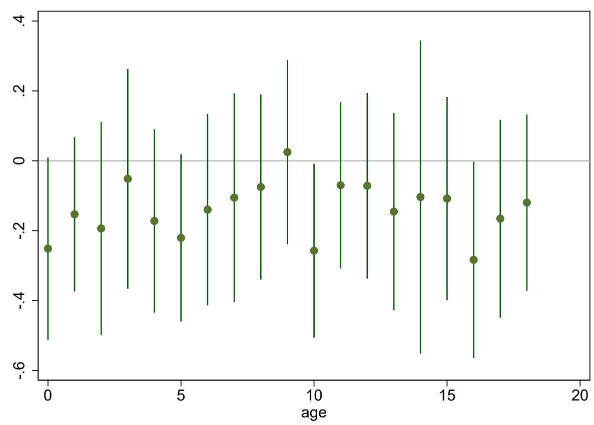
	(1) Highest Education	(2) Num. Adults Employed	(3) Log HH Income
Normal	-0.206*	-0.126*	-0.106
	(0.101)	(0.0557)	(0.0525)
Age X Normal	0.00717	0.00331	0.00159
	(0.00623)	(0.00261)	(0.00266)
Observations	6089	6089	6088

Standard errors clustered by state

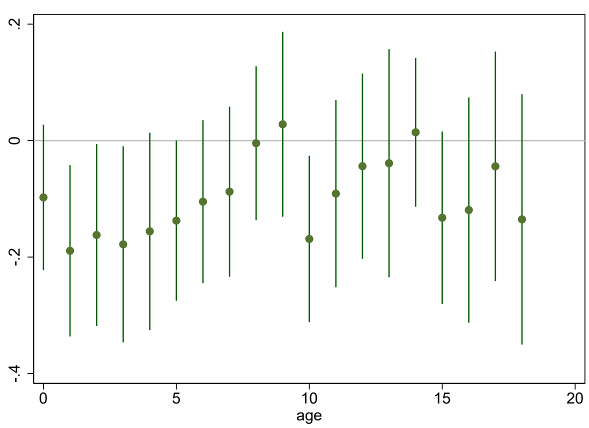
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$



(a) Log Household Income (Adults only)



(b) Years of education (max of adults in household)



(c) Number of Working Adults in Household

Figure A7: Comparison of parents of migrant children in normal vs. asylum counties, by age of child. Each spike plots the coefficient and 95 percent confidence interval from the regression in equation (A1).

H Details on The Freshman Survey Analysis

We merge student zip codes to counties using the CDC County Cross Reference File (Centers for Disease Control and Prevention, 1988). If the zip code is in a normal school and an asylum county, we assign the zip code to the normal county. Roughly 3% of the observations that we classify as being from a normal school or asylum county reported a zip code that merged to both a normal school and an asylum county. If the zip code is in multiple normal, or in multiple asylum, counties we assign it to just one of the counties. If one of the counties matches the county of the university we choose that county. Roughly 3% of the students who grew up in normal school or asylum counties reported zip codes that merged to more than one normal or more than one asylum county.

We merge the public-access TFS data to the restricted-access TFS data in order to obtain the IPEDS ID of the university the student is attending, by merging on TFS university code and year. A very small number of observations in the public-access data have TFS code - year pairs that are not in the restricted-access TFS data (roughly 3000 or .08%, coming from 62 TFS code - year pairs). Since we do not have the IPEDS ID for these students' universities, the variables denoting whether they attend a previous normal school, and whether they attend a university in their county will be missing. However, our main regressions focus on differences in reasons for choosing universities based on the student's home county. Thus, as long as the variable denoting their home county is not missing they will be included in these regressions.

There are roughly 33,000 observations from the public-access data for whom we do not obtain the county FIPS or other IPEDS variables for their university (roughly .9% of the sample of individuals who grew up in normal school or asylum counties). As discussed above, for roughly 3000 observations this is because the TFS university - year pair for the observation in the public data is not in the restricted data, so we cannot obtain an IPEDS ID for the university. For roughly 13,000, the TFS code - year pair is in the restricted-access data, but there is not an IPEDS ID associated with that pair in the restricted-access data.

For the roughly 17,000 remaining observations, there is no IPEDS ID because the individual is attending a university that is not Title-IV eligible, or is in a state or geographic area without any normal schools or asylums in our data (Alaska, Hawaii, Puerto Rico, and the Virgin Islands).[69] For roughly 97% of these 17,000 observations, they are attending U.S. service academies (specifically the U.S. Military Academy, U.S. Coast Guard Academy, and U.S. Naval Academy). As discussed above, as long as these 33,000 observations have non-missing home county FIPS codes, they will be included in the main analysis which does not rely on the university the student attended. However, their universities will not be included in the regression to test differential participation of universities in normal school versus asylum counties, because these regressions are weighted by total bachelor's degrees awarded by the university that year. Because these universities are missing all IPEDS variables, they will be excluded from this regression. This TFS participation regression addresses the concern that we may be missing many individuals from some counties who stay close to home for college if their college does not participate in TFS. However, this is less of a concern for the U.S. service academies given that applicants to these academies must be nominated to apply, and there are geographic restrictions on the number of students from each congressional district in the U.S.[70]

For the roughly 30,000 individuals whose TFS code - year pair is in the restricted access data, but missing data from IPEDS, we obtain their institution's county using the institution's zip code in the restricted data. We merge zip codes to counties using a similar procedure as that described for student zip codes above. For these observations, if the institution zip code merges to a normal school and asylum county we keep the observation from the normal school county. If any of the counties that merge to an institution's zip code are the same as any of the counties that merge to the student's zip code, we use that

[69] We use a roster of Title IV eligible institutions in IPEDS.

[70] Individuals attending the U.S. Air Force Academy did get merged to the university's county FIPS code even though it is not Title-IV eligible. However, this institution is not in a normal school or asylum county, and so also will not be included in the regression testing differential participation of universities in normal school versus asylum counties.

institution and student county. By using the institution zip code from TFS restricted data, rather than IPEDS, we are able to include some additional observations when looking at the outcome of whether a student attended a university in their home county. There are roughly 600 individuals who are in the restricted and public TFS data, but missing data from IPEDs and missing the institution zip code from TFS. These individuals attend five different universities, and we obtain the university's county using the institution's city and state in TFS. We look up the county name using the City-to-County Finder (StatsAmerica, 2023), and then obtain the county FIPS code using U.S. Census Bureau (2002).[71] Further, for the roughly 17,000 observations attending institutions that are not in our IPEDS roster, we are able to impute that they are not attending a previous normal school, because all of the universities that started as previous normal schools are in the IPEDS roster.

When merging TFS data with IPEDS enrollment data, a small fraction of universities have response rates greater than one. This may be due to differences between the enrollment measure in IPEDS (first-time undergraduate degree/certificate-seeking students) and the set of students to whom the university administers the survey.

H.1 Differences in TFS Participation Between Universities in Normal School and Asylum Counties

In this section we provide more details on our test for whether universities in normal school counties are more likely to respond to TFS than universities in asylum counties. Using IPEDS, we construct a dataset of all four-year, Title-IV-eligible universities in normal school and asylum counties in each year (using the university's county FIPS code). We construct a separate dataset of all the universities that respond to TFS in each year. To do this, we first merge the full public data (not limited to students who grew up in normal school or asylum counties) to the restricted data with IPEDS ID, merging on TFS ID and year. We then

[71]For one city, O'Fallon, MO, the tool StatsAmerica (2023) does not yield the county name, and so we obtain it from National Association of Counties (2023).

collapse at the university-year level to obtain a dataset with the universities responding to TFS in each year. We then merge this to the roster of all universities in normal school and asylum counties in each year, and keep the universities located in normal school and asylum counties.[72] The university-year observations in the full IPEDS roster that merge to TFS data are those that respond to TFS that year. The remaining university-year observations do not respond to TFS.

We test for within-state-year differences in TFS participation between universities in normal school counties and universities in asylum counties.[73] Specifically, we estimate:

$$\text{TFS participation}_{jt} = \beta \text{Normal}_j + \alpha_{st} + \epsilon_{jt}$$

We weight observations by the total number of bachelor's degrees awarded by the university in each year. This weighting incorporates that we should be less concerned if small universities in asylum counties are not participating in TFS, as this is less likely to bias our individual-level results. We cluster standard errors at the county level. We find there is no statistically significant difference in TFS participation in a given year between universities in normal school counties and same-state universities in asylum counties (Table A11).[74]

[72] As we discuss above, this will drop universities in the public TFS that were not merged to an IPEDS ID. However, as noted above, these numbers were small.

[73] A reader may also be interested in the raw averages by type of county. For universities in normal school counties, the mean likelihood of TFS participation, over the years from 1982-2010, is 26%, while in asylum counties it is 21%. Weighted by total bachelor's degrees awarded in each year, these means are 36% in both normal and asylum counties. There are 379 universities across 168 normal school counties, and 198 universities in 66 asylum counties, that respond to TFS from 1982-2010.

[74] The point estimate is -.048 with a standard error of .043. In the regression sample, the weighted mean of the dependent variable in asylum counties is .358. We focus on showing no differential participation between universities in normal school and asylum counties. When we look at students' differential likelihood of attending college close to home (equation (2)), we include state-year fixed effects. Thus, participation of farther universities, for example in other states, should matter more similarly for students growing up in normal and asylum counties.

Table A11: University Participation in TFS: Universities in Normal School vs. Asylum Counties

Y = Respond to TFS	(1)	(2)
Univ. in Normal School County	-0.048	-0.048
	(0.043)	(0.043)
Sample	All	Exclude distance educ.
Observations	22,959	22,306
R-squared	0.169	0.172

Notes: * $p < 0.1$, $^{*}*$ $p < .05$, *** $p < .01$. Observations are at the university-year level. All regressions include state-year fixed effects. Column 2 excludes universities in any year for which at least 50% of the university's enrollment in 2018 was enrolled in distance education. The distance education variable is not available in earlier years. Roughly 13% of the universities in column 2 did not merge to the 2018 roster, and for the purposes of this specification we assume they were not "distance enrollment" universities. Observations are weighted by the total bachelor's degrees awarded by the university in that year. Standard errors are clustered at the county level. See text for details.

H.2 Student's Reported Zip Code

We identify students from normal school and asylum counties using the zip code they report on the survey. Through 2000, students were asked for their address at the top of the survey, including their zip code. Starting in 2001, students were asked for their "permanent/home address", including their zip code. In this section we address the possibility that students report their address at the university rather than their family's address, which we are using as a measure for where the student grew up.

If students are reporting zip codes for their residence in college, we would expect many of the reported zip codes would match the university's zip code. This does not seem to be the case. Of the people we classify as growing up in a normal school or asylum county, based on their reported zip code, only 4% are reporting a zip code that is the same as their university. This suggests students are not filling out their address using their residence in college.

In addition to asking students for their address, the survey separately asks students for the distance between the college and their "permanent home". As a second test, we compare the zip codes reported to the separate question on home-university distance. If the people who report the same zip code as their university (or a zip code in the same county as their university) are actually reporting their college residence, we would not expect them to report home-university distances that are closer relative to the full sample of students we have classified as being from normal school or asylum counties. We do this comparison for people whose reported home zip code is in the same county as their university, as well as for people whose reported zip code is the same as their university.

Among all the students who we have classified as being from normal school or asylum counties, roughly 14% report the university is less than or equal to 10 miles from their permanent home and 25% within 11-50 miles. Among students whose reported zip code is in the same county as their university, and who have a non-missing response to the home-university distance question (roughly 27% of the sample), those percentages are 43 and 44%.[75] This suggests that for most students, they are reporting the zip code where they grew up rather than where the university is located. Some may report their residence in college, but this fraction appears small. Only roughly 141,000 students report a zip code that is the same as their university's zip code, and have a non-missing response to the home-university distance question. Of those, 43% report their permanent home is within 10 miles of the university, and an additional 8.5% report it is within 11-50 miles. While roughly 50% report farther homes despite reporting a zip code that is the same as their university's, and this is potentially consistent with reporting college residence, the overall number of these individuals is low (roughly 4% of the overall sample of people we have classified as having grown up in normal school or asylum counties, and who have non-missing responses to the home-university distance question).

Finally, we test whether there is a discontinuous change in the results in 2001 consistent with the survey asking for "permanent/home address" instead of simply for the "address". The results are in Figure A8, and there is no obvious jump in 2001.

[75] As we note above, a very small fraction of zip codes are in multiple counties. For this exercise, for each individual we merge to all the counties associated with their home zip code. We then determine whether any of these counties matches the university's county, and obtain the distribution of home-university distance for those individuals whose reported zip code is in their university's county (using just one observation per individual).

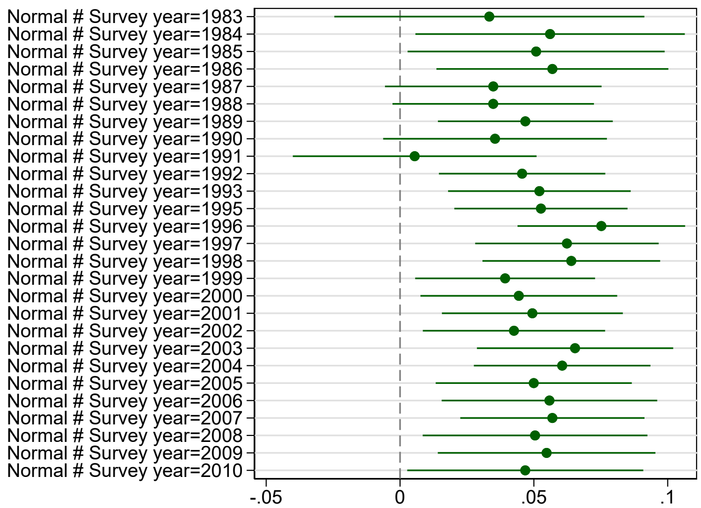


Figure A8: Differential probability of going to university within 10 miles of students from normal and asylum counties, conditional on state, by year

H.3 More Questions from the Freshman Survey

In this section, we show the differential answers from many more questions in The Freshman Survey. As in Section 4, we regress the answer to the question on a dummy for having a home zip code in a normal county, with state-year fixed effects (regression specification (2)).

In Table A12, we show the likelihood of going to a college in various distance bins. We previously showed the first column in Table 5, but we show all the other bins of distance asked in the survey here for completeness. We see that students from normal counties are less likely to be attending universities over 500 miles away. Other distances are not statistically significant.

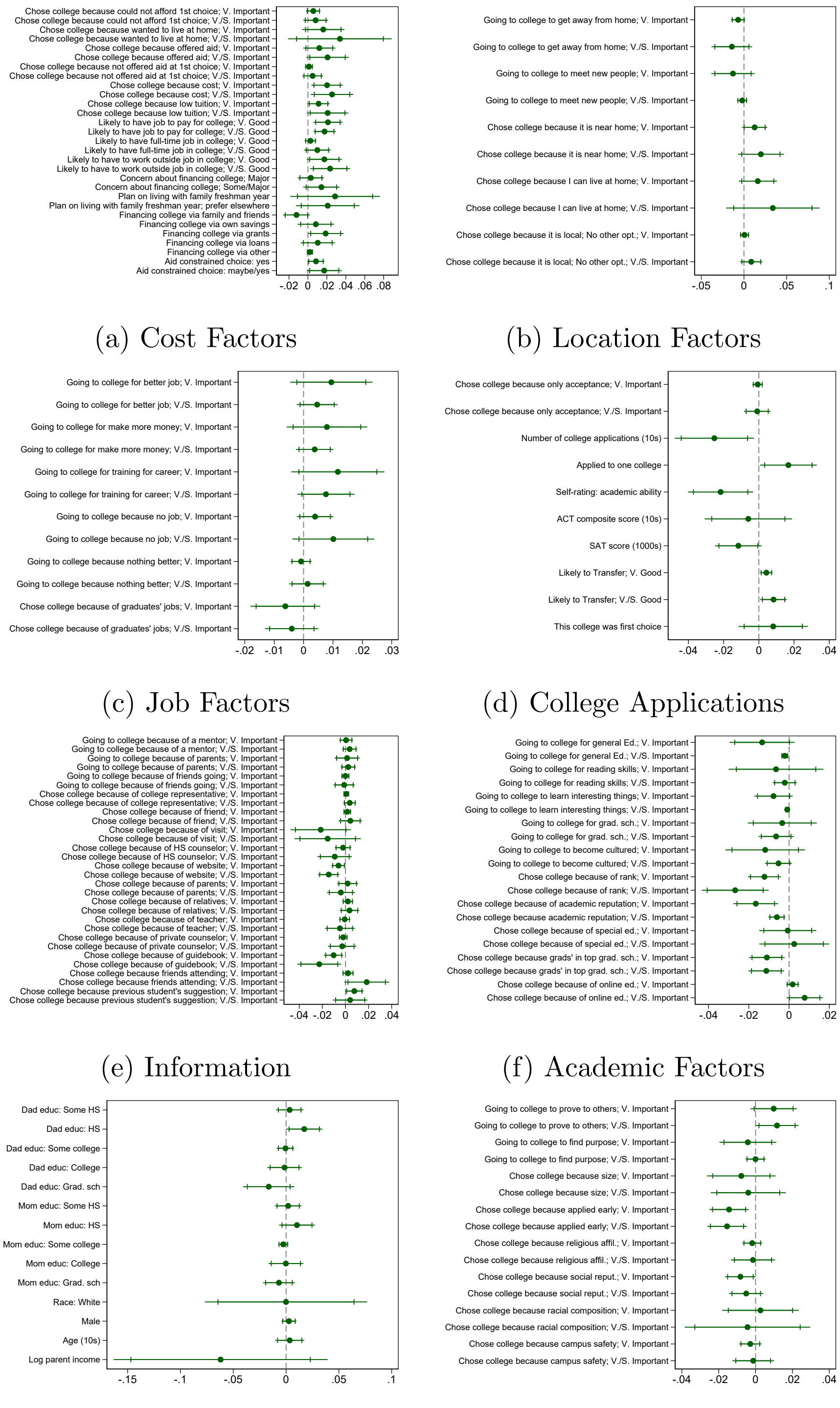
In Figure A9, we show the results from many more questions that were asked in The Freshman Survey. As in Figure 7, the point is the regression coefficient from (2), and the spikes show 90 and 95 percent confidence intervals. We manually chose the questions from the survey that we thought would reflect differences in the composition of freshman from normal and asylum counties, as well as questions about their academic preparedness and application behavior. We split the questions into eight categories: cost factors, location factors, job factors, college applications, information, academic factors, demographic characteristics, and

Table A12: Differential Likelihood of Attending a College that is Various Distances from Home, for Children Growing Up in Normal or Asylum Counties

	Home-University Distance (miles)				
	(1)	(2)	(3)	(4)	(5)
	$\leq$10	11-50	51-100	101-500	>500
Grew up in normal school county	0.0499***	-0.00935	0.00350	-0.0225	-0.0216*
	(0.0150)	(0.0155)	(0.0101)	(0.0208)	(0.00883)
Observations	3568915	3568915	3568915	3568915	3568915
R^2	0.040	0.026	0.023	0.036	0.037

Standard errors clustered by county. * p<0.05, ** p<0.01, *** p<0.001
Observations are at the individual level. All regressions include state-year fixed effects.

other. In general, many of these questions are similar to ones in the main text, so these are shown to show that the results are robust to these other measures. In particular, for questions that ask students to rank the importance of various factors, we now include a dummy variable for just "very" important as an outcome variable. In the main text, we looked only at a dummy variable for "very" or "somewhat" important.



(a) Cost Factors

(b) Location Factors

(c) Job Factors

(d) College Applications

(e) Information

(f) Academic Factors

(g) Characteristics

(h) Other

Figure A9: The difference in Freshman Survey answers between college freshman who grew up in normal school counties versus asylum counties. For some questions that were on a three point scale, we report the difference in students reporting the top choice (”Very”) or the top two choices (”Very” and ”Somewhat”). These are abbreviated as ”V.” and ”V./S.” in the above figure. Spikes represent 95 percent confidence intervals and cross-hatches are at the 90 percent confidence intervals. Standard errors clustered by county.