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COLLEGE ACCESS AND ADULT HEALTH

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ABSTRACT

We investigate the relationship between college openings, college attainment, and health behaviors and outcomes later in life. Though a large prior literature attempts to isolate the causal effect of education on health via instrumental variables (IV), most studies use instruments that affect schooling behavior in childhood or adolescence, i.e. before the college enrollment decision. Our paper examines whether an increase in 2- and 4-year institutions per capita ("college accessibility") in a state contributes to higher college attainment and better health later in life. Using 1980-2015 Census and American Community Survey data, we find consistent evidence that accessibility of public 2-year institutions positively affects schooling attainment and subsequent employment and earnings levels, particularly among whites and Hispanics. With restricted-use 1984-2000 National Health Interview Survey data, we again find that public 2-year accessibility increases schooling and benefits a host of health behaviors and outcomes in adulthood: it deters smoking, raises exercise levels, and improves self-reported health. However, most long-term health conditions are unaffected, which may be partially due to the age of our sample.

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Introduction

A large literature examines whether years of formal schooling have a causal effect on health (see Grossman, 2015 and Galama, Lleras-Muney, and van Kippersluis, 2018, for recent reviews). The fundamental issue is that though education and health measures are positively correlated in the data, this need not reflect a causal relationship from education to health. Recent papers on this topic try to overcome this problem by exploiting variation in schooling that is otherwise unrelated to health; a majority of these use variation in schooling stemming from policies that affect individuals when they are in childhood or adolescence (e.g. compulsory schooling laws or primary/secondary school-building programs), with mixed results.

Less is known about how policies that affect higher education decisions contribute to subsequent health outcomes. This is particularly poignant given that the positive education-health gradient is strongest for post-secondary years of schooling in the case of many health behaviors (Cutler and Lleras-Muney, 2010). Recently, some scholars have called attention to the need for evidence of how schooling factors other than compulsory schooling reforms affect later health outcomes, since these reforms affect a particular type of student at one margin of schooling (Galama, Lleras-Muney, and van Kippersluis, 2018). This paper seeks to fill this gap by examining how college openings when individuals are young enough to take advantage of them affect health behaviors and outcomes later in adulthood.

The question of how college access affects college attainment and subsequent outcomes (including health) is timely from a policy perspective. In the United States, the college wage gap is as high as it has been in 100 years (Goldin and Katz, 2008). At the same time, the percentage of individuals obtaining a college degree has flattened, and the gap in college attainment by family income has grown over time (Page and Scott-Clayton, 2016). These trends, plus the rapidly rising pecuniary cost of college over the past few decades, has renewed policymakers' interest in dramatically raising subsidies for higher education, particularly at the 2-year (community) college level and for disadvantaged groups.[1] While recent evidence suggests 2-year tuition subsidies and other programs designed to increase degree completion among 2-year students can raise attainment (Carrell and Sacerdote, 2017; Carruthers and Fox, 2016; Denning, 2017; Evans et al., 2018), there is still a lack of evidence on the private and social benefits of doing so. In particular, how does the additional college attainment induced by increased college "access" affect not only individuals' labor market but also health outcomes later in life?

We tackle this question by exploiting the differential build-up of 2 and 4-year institutions per capita across states over the period of 1960-1996, with the lion's share of 2-year college building occurring in the 1960's and early 1970's. Figure 1 shows that there is substantial variation in the number of public 2-year colleges per 18-22 year-old across states and over time, both during and after the build-up period. This is consistent with Kane and Rouse (1999), who

[1] New York, Tennessee, and Oregon have recently made public 2-year college tuition-free for many families. See http://money.cnn.com/2017/05/11/pf/college/tennessee-free-community-college/index.html (last accessed February 11, 2019).

show that public 2-year college attendance increased dramatically over this same time period from roughly 20% of first-time college attenders to nearly 50%.

Our identification strategy is closely related to that of Currie and Moretti (2003; hereafter, “CM”), who exploit differences in the number of colleges at age 17 between cohorts of women observed in the same county and year to examine how schooling affects birth outcomes. By contrast, we examine outcomes for all adults after typical college-going years including individual and family earnings, employment, and health outcomes. A difficulty in both CM and our paper is that we do not observe individuals’ locations at the time they make college decisions (or even at age 17). Rather, CM only observe county of current residence and assume that was where individuals lived at the time they made their college choices. In this paper, we examine differences in the number of colleges by state (rather than county) and assign individuals to their state of birth rather than state of residence. Though state of birth also measures “state at age of college decision(s)” with error, it is not subject to bias related to endogenous migration on the part of the respondent.

Using data from the 1980, 1990, and 2000 Census and the 2001-15 American Community Survey (1% PUMS), we find that an increase in public 2-year colleges per 1,000 18-22 year-olds in the state of 0.02 (one standard deviation of this variable in the data) when an individual is 17 years old raises years of formal schooling in adulthood by a little more than 0.05 years. This change occurs through a reduction in individuals finishing their schooling as high-school dropouts but especially high-school diploma earners only and an increase in some college attendance as well as 4-year college graduation. On the other hand, public and private 4-year institutions per capita have little effect on completed schooling. The effects of 2-year college access are concentrated among whites and Hispanics (as opposed to blacks and members of other races). For these groups, we estimate a financial return to a year of schooling of roughly 14%, which is similar to recent findings in the returns-to-schooling literature (Oreopoulos and Petronijevic, 2013).

Coupling these results with restricted-use 1984-2000 National Health Interview Survey data, we again find that public 2-year accessibility positively affects schooling and a host of health behaviors and outcomes in adulthood: it deters smoking, raises exercise levels, and improves self-reported health. Some other behaviors and outcomes, including binge drinking, obesity, flu shot receipt, and major health activity limitations, are unaffected. Some of these null effects are consistent with other findings in the literature (e.g., obesity, see Galama, Lleras-Muney, and van Kippersluis, 2018) and others are not likely to manifest themselves in a non-elderly sample (e.g., major activity limitations), and some we would not expect to be influenced by schooling at all (e.g., asthma and allergies). We discuss these results in the context of theories regarding education and health, the related literature, and multiple hypothesis testing later in the paper.

Background

Theoretical considerations

College attainment may affect the trajectory of employment and earnings, health-related behaviors, and health outcomes later in life. Although the primary aim of this paper is to study the effects of college attainment on health, we also examine how college availability impacts earnings and employment, with special attention to their life-cycle trajectories. Human capital theory suggests and previous studies confirm that the earnings return to education is most evident after the completion of college and through prime-working ages, after a worker gains experience and before they transition towards retirement (Bhuller, Mogstad, and Salvanes, 2017).

Greater investments in “knowledge” or “skill” capital may affect decisions regarding health capital. Grossman (1972, 2000) hypothesized that these two forms of human capital are complementary. For example, knowledge or skill capital may increase individuals’ ability to process information, raising their efficiency in producing health. At the same time, if one expects to have more consumption and leisure enjoyment in life, investing in health and longevity may be more attractive (Becker, 2007; Cowan, 2011). There are also likely to be direct income and substitution effects associated with higher earnings that contribute to differences in the purchase of health-related products and services (such as drugs and alcohol, healthy foods, gym memberships, and health care) as well as time spent in health-enhancing activities (such as exercise). Various patterns of such purchases by income level can have ambiguous total effects on health behaviors and outcomes.

College attendance also has the potential to change individuals’ attitudes, preferences, and choices that affect health. For example, because attending college typically changes the composition of a student’s peer group, there are likely to be important peer effects impacting health-related behaviors, either by accelerating the adoption of health-promoting behaviors, or by encouraging unhealthy behaviors through social norms or historical cultural practices (Kremer and Levy, 2008; Carrell, Hoekstra, and West, 2011). The net impact of such peer effects on health-related behaviors depends in part on the differences between a student’s childhood and college environment, which may differ based on a student’s gender, race, and ethnicity.

The theories of health and human capital share the feature that capital accumulates over an extended period through investment and return. For this reason, even if health behaviors (such as diet, exercise, or substance use) are affected by college attendance at younger ages, these changes in health-related behaviors may not translate into changes in health outcomes until later in life. We discuss the implications of this vis a vis the age of our sample later in the paper.

Of course, the positive correlation between (college) education and health need not reflect causal effects from more schooling to better health. Other research has found evidence of an effect of early-life health on educational attainment (e.g., Case, Fertig, and Paxson, 2005). Unobserved factors, including cognitive and non-cognitive abilities, risk and time preferences, and individual values can influence both health and education decisions (Fuchs, 1982; Heckman, Stixrud, and Urzua, 2006). Our study aims to isolate the causal effect of college schooling on health, though we largely leave the identification of specific mechanisms behind any such effect to other research.

Empirical Literature

The most recent studies using U.S. data that have examined the effects of college education specifically on adult health have typically used variation in schooling induced by differences in draft risk surrounding the Vietnam War.[2] They find that the increase in college education due to draft avoidance led to a reduction in mortality (Buckles et al., 2013), smoking (Buckles et al., 2013; De Walque, 2007; Grimard and Parent, 2007), and obesity (Buckles et al., 2013; MacInnis, 2006).[3] Collectively, these papers provide considerable evidence that college education has a positive, causal effect on health in this context, but they are limited in terms of external validity because their sample is confined to men who were at risk of military induction in the 1960's.[4] By focusing on colleges per capita, we exploit a policy-relevant variable that affects a large swath of young people over several decades and thus may have different effects than does Vietnam War draft avoidance.

The paper most closely related to ours is Currie and Moretti (2003) in that we use the same data on U.S. college openings (provided by the authors) and a similar identification strategy. Papers using the related variables of individual proximity to colleges or primary/secondary school openings as instruments for education are abundant in the financial returns-to-schooling literature.[5] However, we do not know of any studies that use U.S. college openings to examine the effect of education on adult health, nor do we know of any paper that has tried to replicate CM's results using their data on college access in a different context.

CM examine how 2- and 4-year colleges per capita within counties affect the education levels and other characteristics of new white mothers (as measured in Vital Statistics Natality files for 1970-1999) as well as birth outcomes of their newborns. The authors face the same issue with their natality data as we have in our BRFSS data: only current location (county in their case, state in ours) is measured rather than location at a younger (pre-college) age. They use alternative datasets (Census and NLSY) in which it is possible to obtain location of residence at age 17 for some individuals to verify that their first-stage results are not driven by higher-educated mothers sorting into states with more institutions per capita.

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[2] In a recent study, Kamhofer, Schmitz, and Westphal (2018) use college expansions in Germany to identify the effects of college education on later health outcomes. They find positive average physical health effects but zero mental health effects, along with positive average effects on cognitive ability and wages, of college schooling.
[3] Buckles et al. (2013) also find that exercise increases and heavy drinking decreases (in some specifications) when years in college increases.
[4] Recent work has also argued that draft risk has a direct effect on health through stress, which challenges the validity of the instrument (see Cawley, de Walque, and Grossman, 2017).
[5] Some examples of papers using college proximity as an instrument include Card (1995), Cameron and Taber (2004), and Heckman, Humphries, and Veramendi (2016). Duflo (2001) uses primary school openings in Indonesia as an instrument to estimate the financial returns to schooling (see also Akresh et al., 2018).

In our paper, we use the same data on colleges per capita for 1960-95 as CM, but we aggregate the information to the state level. This is because we have access to state of birth, but not county of birth, in the public Census and restricted NHIS data.[6] Assigning college accessibility measures based on state of birth means that we do not have to be concerned that our results are driven by individuals who selectively move into states with greater accessibility prior to college enrollment, or that individuals who already have more education relocate to states with greater accessibility (a concern if such accessibility is correlated with better labor-market prospects for college graduates, for example).

CM focus on a very particular segment of the population—new white mothers (though all white mothers are considered in robustness checks). We have no *a priori* reason to believe that colleges per capita would not affect educational and subsequent health outcomes across gender, race, and age. Thus, our primary study sample includes all adults, and we also examine how our results vary by demographics.

Data

Figures 1 and 2 show how the number of public 2- and 4-year colleges per capita, respectively, changed in each state over our sample period. 2-year colleges per capita experienced more (in some cases sudden) growth in most states than did 4-year colleges, especially during the 1960's and early 70's (though the baseline level of 4-year schools per capita is higher overall). Figures 3 and 4 show, respectively, how overall increases in public 2- and 4-year colleges per capita from 1960 to 1995 vary by state. As is noticeable from the figures, the correlation between growth in public 2-year access and 4-year access at the state level is small.

Figure 5 shows the result of regressing state-level first differences in years of schooling attained between the 1960 and 1996 cohorts on the same differences in 2-year colleges per capita. State observations are weighted by their population of 18-22 year-olds in 1960, and larger circles represent more heavily weighted states in the figure. There is a clear positive correlation between these two variables in the figure. It should of course be noted that overall growth in these measures is not what we use to identify effects on schooling and other outcomes in our econometric analysis; rather, it is cohort-by-cohort variation across states and over time that identifies our effects. This is described in the next section.

We first employ the 1980, 1990, and 2000 Census surveys and the 2001-15 American Community Survey (1% PUMS). We include all individuals who are age 22 and above and were 17 years old between 1960 and 1996 (since these are the years for which we have data on their college accessibility variables). This leaves us with over 18 million observations. For

[6] Data on location at age 17 in the Census data is available for those who are 22 at the time of the survey, since there is a question about a respondent's residence 5 years prior to the interview. However, this is not a suitable sample for our analysis, since we want to measure health effects of education throughout adulthood.

computational ease, and because we are focused on state-level policies, we aggregate our data into state by cohort by year of observation cells.

Summary statistics are contained in Table 1. Schooling responses in the Census are assigned to highly disaggregated categories; we create several dummy variables indicating different schooling levels as well as a measure of years of schooling (which we treat as continuous in the analysis). As seen in the table, there is roughly 0.04 (0.03) public 2-year (4-year) institutions per 1,000 18-22 year-olds in a state, on average (compared to 0.06 private 4-year institutions per capita).[7] The methods for counting the number of institutions and estimating the population of 18-22 year-olds are discussed in detail in the Data Appendix of Currie and Moretti (2003).

Our National Health Interview Survey sample is from 1984-2000 and contains the restricted-access variable state of birth (for which assignment to colleges per capita at age 17 is necessary, as described earlier). We restrict the age and cohorts of the sample in the same we do for the Census/ACS data. We examine a host of health-related behaviors, symptoms, and conditions as dependent variables in our analysis. Summary statistics on all variables are included in Table 2. We note that sample sizes differ for each variable, sometimes greatly, because the years in which a question was asked varies.

Methodology

We follow Currie and Moretti (2003) in implementing our estimating equations. In particular, our first-stage equation relating educational outcomes to our measures of college access takes the following form:

$$(1)\ Ed_{sct} = \beta_0 + \beta_1 pub2_{sc} + \beta_2 pub4_{sc} + \beta_3 pri4_{sc} + \alpha_s + \gamma_c + \theta_t + X_{sct}\delta + \varepsilon_{sct}.$$

In this equation, Ed_{sct} represents the average education level of cohort *c* born in state *s* observed in year *t*. $pub4_{sc}$ ($pub2_{sc}$) is the number of public 4-year (2-year) institutions per capita that each cohort would have faced at age 17 in their state of birth. $pri4_{sc}$ is defined similarly for 4-year private institutions. We call these our “college access” measures. α_s represents state of birth dummies, γ_c cohort dummies, and θ_t year dummies. X_{sct} contains additional control variables, including dummies for gender, race, and ethnicity categories as well as age 17 controls for the state per-capita income, k-12 per pupil spending, and unemployment rate.[8]

[7] Currie and Moretti (2003) combine public and private 2-year institutions in their analysis. This may be because private 2-year institutions are relatively scarce over our sample window (the per-capita average is 5-10 times smaller than any other category). We do not include private 2-year institutions in our analysis. However, if we combine them with 2-year public institutions, our results change little. If we allow 2-year private institutions per capita to enter as a separate independent variable in our analysis, coefficients on 2-year public institutions are practically unchanged, but the coefficients on 2-year private institutions are generally negatively associated with schooling outcomes. Because these institutions are scarce, however, we are cautious about the interpretation of these effects. These results are available upon request.

[8] Sources for these data are as follows:

When we use NHIS data rather than Census data, Equation (1) (and Equations (2) and (3) to follow) is modified to be at the individual level rather than the state-cohort-year level (due to its significantly smaller sample size). Because we examine current health measures as dependent variables in NHIS, we also include state-specific control variables in the year of observation including the unemployment rate, the cigarette tax amount, and the beer tax amount in that analysis.

Our models are identified by differences in education between different cohorts from the same state observed in the same year. In particular, any difference in outcomes when college access is the same across cohorts is proxy for the difference across cohorts who face different college access *had their access been the same*. This is the standard difference-in-differences assumption, though we note that in our context it is more accurate to say that since every new cohort is "treated" to some extent (due to fluctuations in the number of institutions, 18-22 year-olds, or both), we identify exposure-response relationships across cohorts.

We also examine the reduced-form impact of our college access measures on several additional outcomes in both Census and NHIS:

$$(2)\ Y_{sct} = \beta_0 + \beta_1 pub4_{sc} + \beta_2 pub2_{sc} + \beta_3 pri4_{sc} + \alpha_s + \gamma_c + \theta_t + X_{sct}\delta + \varepsilon_{sct}.$$

This allows us to examine the effect of our access measures on health and labor-market outcomes directly. Finally, we use our access measures as instruments for education to estimate the effect of education on our outcome variables of interest:

$$(3)\ Y_{sct} = \beta_0 + \beta_1 Ed_{sct} + \alpha_s + \gamma_c + \theta_t + X_{sct}\delta + \varepsilon_{sct}.$$

Equations (1) and (2) are estimated via OLS, and Equation (3) is estimated via 2-stage least squares. In all equations, standard errors are clustered at the state level to account for intra-state correlation in errors both in a cross section and over time (Bertrand et al., 2004).

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State cigarette taxes: Orzechowski and Walker, The Tax Burden on Tobacco, 2014.
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Results

1. Schooling outcomes

Table 3 contains first-stage results related to the effects of 2 and 4-year institutions per capita on educational attainment using our full Census sample. The first column indicates that the number of 2-year colleges per capita at age 17 reduces the probability that an individual has less than a high-school diploma. The effect of an increase of one 2-year institution per 1,000 18-22 year-olds in the state is -0.197. This means that an increase of 2-year institutions per 1,000 18-22 year-olds equal to one standard deviation (0.02) lowers the chances of being a high-school dropout by about 0.4 percentage points. For a similar change in 2-year access, the reduction in the probability of only having a high-school diploma is about twice as large. This is accompanied by an increase in the probability of obtaining some college (0.4 percentage points) and graduating from college (0.8 percentage points). Overall, years of schooling increase by about 0.05 years.

Table 3 also shows that neither 4-year public nor 4-year private institutions per capita have a significant effect on completed school years. We note that other researchers have found smaller enrollment elasticities with respect to 4-year policy variables (e.g. tuition) than with respect to 2-year ones (examples include Kane, 1995; Cameron and Heckman, 2001). This may be due to the fact that because public 2-year institutions tend to be the cheapest and least exclusive form of U.S. higher education, they attract many “marginal” college goers who are especially sensitive with respect to factors that affect the cost of college. On the other hand, Currie and Moretti (2003) find that both 4 and 2-year colleges per capita in one’s county of residence affect maternal schooling outcomes.

2. Identification checks

Perhaps the greatest concern regarding the validity of our identification strategy is whether an increase in colleges per capita in a particular state should be viewed as a college supply shock (rather than the product of rising demand). In other words, it may be that unobserved factors correlated with both cohort schooling levels and cohort college availability are in fact driving our results. One way to scrutinize our approach is to examine the effect of college access at different ages simultaneously: if the number of institutions is causing college attainment and not vice versa, then effects should be concentrated at typical college-going ages. Unfortunately, a high degree of serial correlation in number of colleges per capita over time makes it difficult to separately identify effects of college access at different ages.

To deal with this issue, we run our models using cohort differences in our dependent and independent variables rather than levels. To do this, we aggregate the data to the state-cohort level and take 3-cohort differences in years of schooling (the dependent variable) as well as each of our independent variables. The difference in schooling years is then regressed on each of the independent variable differences along with state and cohort fixed effects (in case such effects are not totally time-invariant). In addition to the 3-cohort difference in public 2-year colleges at age 17, we include 3-cohort differences at ages 7, 12, 22, and 27 (5 and 10 years before and after age 17).[9]

Results of this exercise are shown in Figure 6, which includes point estimates and 95% confidence intervals for each 3-cohort difference in public 2-year access at each age. The only coefficient that is positive and statistically different from zero at the 5% level is the one at age 17. On the other hand, it is again difficult to statistically distinguish effects at age 17 from effects at other ages (note the especially large confidence intervals for later ages). We view the results from this exercise as providing modest support for our identification strategy.

Another way to indirectly assess the exogeneity of public 2-year colleges per capita is to examine its correlation with other state-level factors that potentially affect the educational attainment of cohorts. If 2-year college access were associated with observable factors affecting educational attainment, it could indicate a correlation with unobservable drivers as well (Altonji, Elder, and Taber, 2005; Oster, 2019). This is shown in Table 4. Each column represents results from regressing a different dependent variable on colleges per capita and other controls. As seen in the table, public college access is uncorrelated with age 17 measures of state k-12 spending, income per capita, the unemployment rate, and both public and private 4-year colleges per capita. Rather, the only factor significantly associated (at the 10% level) with 2-year colleges per capita is the number of 18-22 year-olds in one's birth state, and this effect is small economically.[10] This analysis again does not raise concerns with treating 2-year public colleges per capita as exogenous (see Pei, Pischke, and Schwandt, 2019).

3. Labor-market outcomes

We see from the final three columns of Table 3 that although 2-year college access is positively related to individual income, family income, and the probability of current employment, none of these effects are significant at conventional levels. However, this masks significant heterogeneity by race and ethnicity. Table 5 shows results from regressions run on three separate groups: non-Hispanic whites (hereafter referred to simply as "whites"), non-Hispanic non-whites (who are predominantly black), and Hispanics.[11] Both whites and Hispanics experience substantially larger effects of 2-year colleges per capita on schooling and adult income measures than do non-Hispanic blacks and members of other races. An increase of 0.02 2-year institutions per capita raises white individual income by about 1% and Hispanic income by about 3.5%.[12]

We are unsure of the reason(s) why 2-year college access fails to affect the educational outcomes of black individuals, though this finding is consistent with Currie and Moretti (2003).

[9] In this analysis, we only include public 2-year institutions in the regressions, though results with the other institutions types in the regressions are similar (available upon request).
[10] The coefficient indicates that a one standard deviation increase in public 2-year colleges per 1,000 18-22 year-olds is associated with a decrease of about 22 individuals.
[11] Roughly two-thirds of Hispanics report their race as white.
[12] We also tried estimating our regression model for males and females separately, but we found very similar effects across gender. These results are available upon request.

We believe this is an important question to examine in the future, especially because recent interventions to promote college attainment suggest that treatment effects are no different for white and non-white students or perhaps even larger for the latter group (Carrell and Sacerdote, 2017; Denning, 2017; Evans et al., 2018).

Table 6 examines how 2-year college access affects the schooling and labor-market outcomes of whites and Hispanics across the age distribution. For this exercise, we split the sample into the following categories (age 30 and under, 31-40, 41-50, 51-60, and 61 and above).[13] As seen in the table, the effect of 2-year college access on schooling is roughly similar across age. However, income and employment effects vary largely over the age distribution in a way that we would expect: effects are hump-shaped over the age range. In particular, we see strong effects on individual and family income as well as employment for adults in their prime working years: 31-50 and to a lesser extent 51-60. Effects for younger individuals (who are new to their careers and may not yet have overtaken peers with less education but more experience) and older individuals (who are likely to retire) are smaller and generally insignificant.

Table 7 complements Table 6 by using Equation (3) to estimate the effect of schooling on log individual income using our measure of public 2-year college availability as an instrument for attainment (since public and private 4-year availability are not consistently correlated with attainment, they are not used as instruments). Again, this analysis is performed on our white and Hispanic subsample. We see that the IV result indicate an additional year of schooling leads to an increase in annual earnings of 13.7%. To put this results in context, our financial returns-to-schooling estimate is in the range of IV estimates found in the literature (Card, 2001; Oreopoulos and Petronijevic, 2013).

4. Health outcomes

In dividing our NHIS sample into the three racial/ethnic groups used in our ACS analysis, we find that 2-year college access is again predictive of later educational attainment for non-Hispanic whites (Table 8, column 1). 2-year colleges per capita does not increase the educational attainment of non-Hispanic non-whites (see Appendix Table 1), which is again consistent with our ACS results. In the case of Hispanics, however, we now find no evidence of a positive effect of 2-year access on years of schooling, which contradicts our Census results (see Appendix Table 2). This finding is puzzling, but may be explained in part by the difference in sample time frames: in fact, when we run our Census analysis on Hispanics using only years up to and including 2000, the positive effect of 2-year access on years of schooling diminishes and becomes insignificant at conventional levels.[14] As a results of these first-stage findings, we focus our main analysis on the health outcomes of non-Hispanic whites (Appendix Tables 1 and 2 show that for black and Hispanics, 2-year institutions per capita has no statistically significant effects on health outcomes that are found to be statistically associated with 2-year access for whites).

[13] The oldest cohort in our data was 72 as of the last survey used (2015).
[14] This appears to be a result of the effects being larger for younger Hispanic cohorts.

The other columns in Table 8 show how college access affects three behaviors that are strongly associated with health outcomes: whether an individual engaged in binge drinking at least once in the past month, whether she is a current smoker, and the number of times she performed moderate or strenuous exercise in the past week. Binge drinking is found to be unrelated to 2-year college access, but the latter two behaviors are. The coefficients imply that a one standard deviation increase in 2-year colleges per capita lowers the probability of current smoking by 1.4 percentage points and raises exercise frequency by 0.22 occasions (a 5% increase relative to the mean).

Table 9 is similar to Table 8 but shows results from models in which health conditions or outcomes are the dependent variables. The first of these is self-reported health (a binary variable indicating excellent or very good health, the top two of five possibilities), and we again find an effect of 2-year college access on this variable for whites. The coefficient indicates that a one standard deviation increase raises the probability of excellent/very good health by about 0.7 percentage points. There are negative but imprecisely estimated effects of 2-year access on obesity, the occurrence of 7 or more bed days in the past year, having at least one hospital stay, and the presence of activity limitation. However, it is associated with the occurrence of 4 or more visits to a healthcare professional in the previous year. Given that doctor visits and hospital stays are determined in part by out-of-pocket costs in addition to health problems, these effects are perhaps more surprising given that more educated individuals are generally more likely to be insured and have more generous coverage over this time period (Assaf et al., 2009).

Table 10 continues this analysis by showing the effects of colleges per capita on nine different categories of chronic conditions. In at least the first three cases (asthma/allergies, learning disability, or birth condition), we would not expect to find any effect of education via college access on these outcomes. This is because they are either present at birth or typically develop in childhood. Indeed, we find no effect of 2-year college access on these kinds of conditions. The next two outcomes, the past occurrence of a cardiac event (heart attack, stroke, etc.) and the past occurrence of cancer are in part determined by lifestyle decisions that could be affected by schooling. However, they are still relatively rare in such a young sample (mean age of 36 across the entire dataset with the oldest cohort being 57 in 2000). We again find no effect of college access on these outcomes.

The last four conditions, in addition to affecting one’s present health, are also pre-cursors to more serious health conditions and mortality: the presence of a cardiovascular condition (e.g., diabetes, hypertension, or heart disease), respiration condition (e.g., emphysema, chronic bronchitis), “other” condition (e.g., arthritis), and “major” depression. Only one of these conditions is found to be affected by public 2-year access (cardiovascular condition). This could be due to schooling’s effect on smoking and exercise shown in Table 7. We do not find any effect on “major” depression, but a lack of data on other measures of mental health (save self-reported overall health) preclude us from being able to look at schooling and mental health more generally.

Of the 13 behaviors and conditions we might reasonably expect to be correlated with schooling via 2-year college access, 5 are found to be statistically significant at the 5% level or better. If we adjust the p-values associated with each null hypothesis of zero effect for multiple hypothesis testing using the Holm-Sidak method, only one (4 or more healthcare visits) survives at the 5% level. However, we note two important caveats: first, an effect of schooling on some of these conditions, including activity limitation, the occurrence of a cardiac event, or bed days, might only show up at later ages. Given the age distribution of individuals in our sample, we must leave the exploration of this possibility to future research. Second, the finding that schooling affects smoking (when it causes individuals to change schooling "tracks") but not obesity is consistent with the literature (see Galama, Lleras-Muney, and van Kippersluis, 2018).

We also examine the effects of 2-year college access on health outcomes by gender (Table 11) and age (Table 12). In each case, we examine only those behaviors and conditions that are found to be significantly affected by 2-year access in the whole sample (there are only two cases total in which the coefficient is statistically insignificant for the entire sample but is significant for one of the separate groups in Tables 11 and 12). As seen in Table 11, the effect of 2-year colleges per capita on years of schooling is roughly twice as large for men as it is for women. Nevertheless, 2-year access has a larger effect on the health outcomes of women in three out of five cases (doctor visits, smoking, and cardiovascular condition). In every case, however, the signs of the coefficients are the same across gender.

Turning attention to the results by age in Table 12, where we divide the sample into individuals 35 and younger and those over age 35, we see a substantially stronger effect of 2-year access on schooling for the younger group. Though the effect for those over 35 remains positive, it is not significant at the 10% level. This is in contrast to our ACS results, where we find similar effects by age category (our ACS sample, however, contains 2001-2015, which our NHIS sample does not). Given the much larger effect on education, it is natural to expect the health effects of 2-year college access to also be concentrated among younger individuals. This, however, is countered by the possibility that there is more "scope" for affecting the health of older individuals, since health conditions are sometimes only manifest at later ages. In actuality, we find larger effects (in terms of point estimates) on the younger group in all cases but one: the presence of a cardiovascular health condition. This is not surprising given the relatively lower risk of such conditions among young people (Kannel and McGee, 1979).

Conclusion

Making use of changes in the accessibility of 2 and 4-year colleges over time and across states with Census and ACS data, we find that public 2-year colleges per capita at the time an individual was 17 years old has a significant effect on their schooling and, for some groups, adult earnings and employment. Our estimates imply substantial market returns to higher education, which is consistent with many papers in the returns-to-schooling literature. Furthermore, using NHIS data, we find that public 2-year access improves the health behaviors and outcomes later in adulthood—it reduces the likelihood of smoking, increases exercise frequency, and is associated with better self-reported health.

Our paper has implications for the current debate surrounding how to improve higher education enrollment and completion in the U.S. In particular, we find that public 2-year college accessibility is a key driver that induces individuals who would not have otherwise attended college to enroll. In our data, educational and post-educational outcomes improve overall when more 2-year colleges are available despite some individuals possibly substituting 2-year schooling for 4-year schooling, which some studies suggest is detrimental to final schooling levels and other outcomes (Reynolds, 2012; Zimmerman, 2014; Goodman, Hurwitz, and Smith, 2015).[15]

Given that there are many more 2-year colleges now than at the beginning of our sample period (1960), a key question for future research is to determine whether increasing access to 2-year institutions in other ways has similar effects to what we measure in this paper. One possibility is to make community colleges tuition-free, as has been proposed by former president Barack Obama and enacted in New York, Tennessee, and Oregon for many families.[16] Recent work has suggested that 2-year tuition reductions can have significant impacts on college attainment (Denning, 2017). Other possibilities include efforts to make the financial aid process easier for prospective college goers, which has been shown to have large net benefits (Bettinger et al., 2012; Marx and Turner, 2017). Whether individuals who are affected by such policies experience different labor-market or health outcomes later in life is a question for future research.

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[15] This is consistent with Jepsen, Troske, and Coomes (2014) and Belfield and Bailey (2017), who find substantial monetary returns to 2-year schooling specifically for those who otherwise would not have attended.

[16] http://money.cnn.com/2017/05/11/pf/college/tennessee-free-community-college/index.html (last accessed January 25, 2018).

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Figure 1:

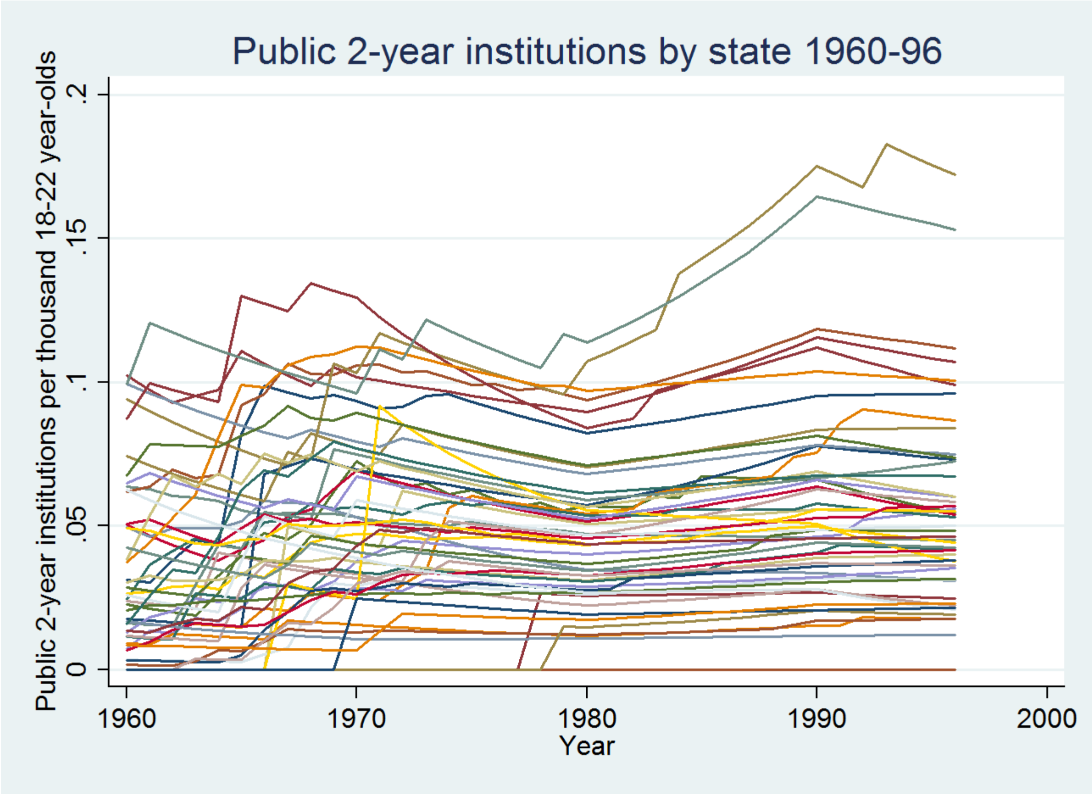

Figure 2:

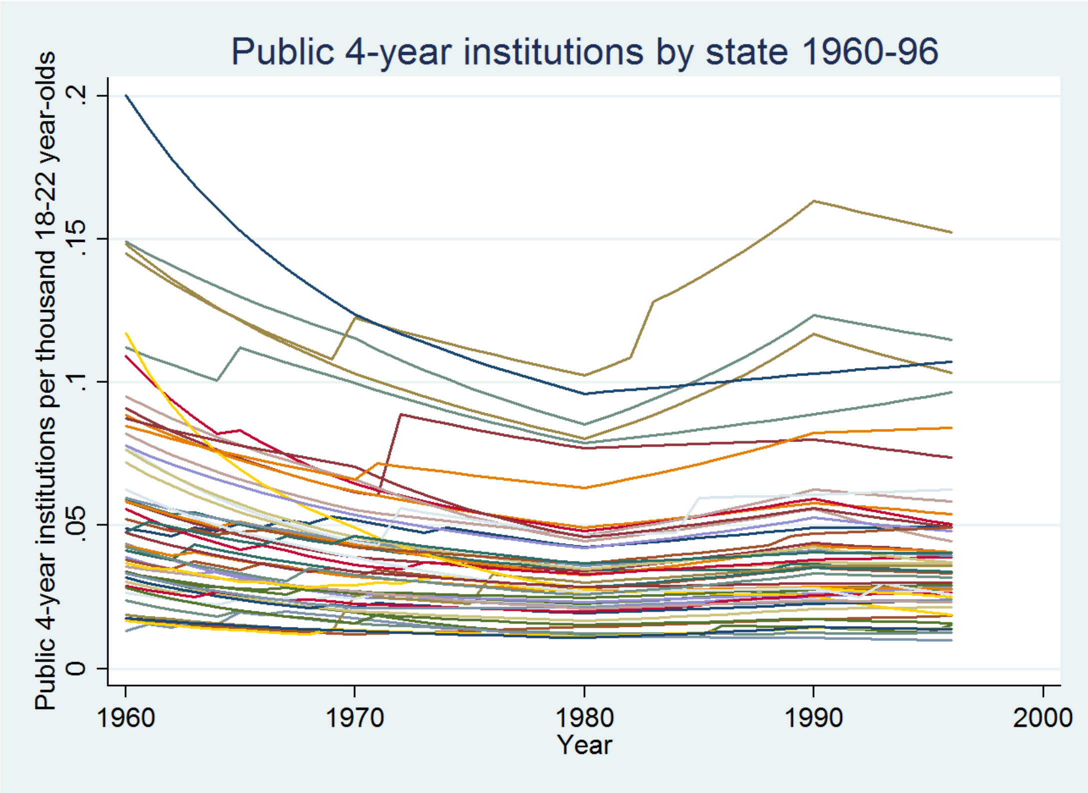

Figure 3:

Growth in public two-year colleges per capita, 1960-96

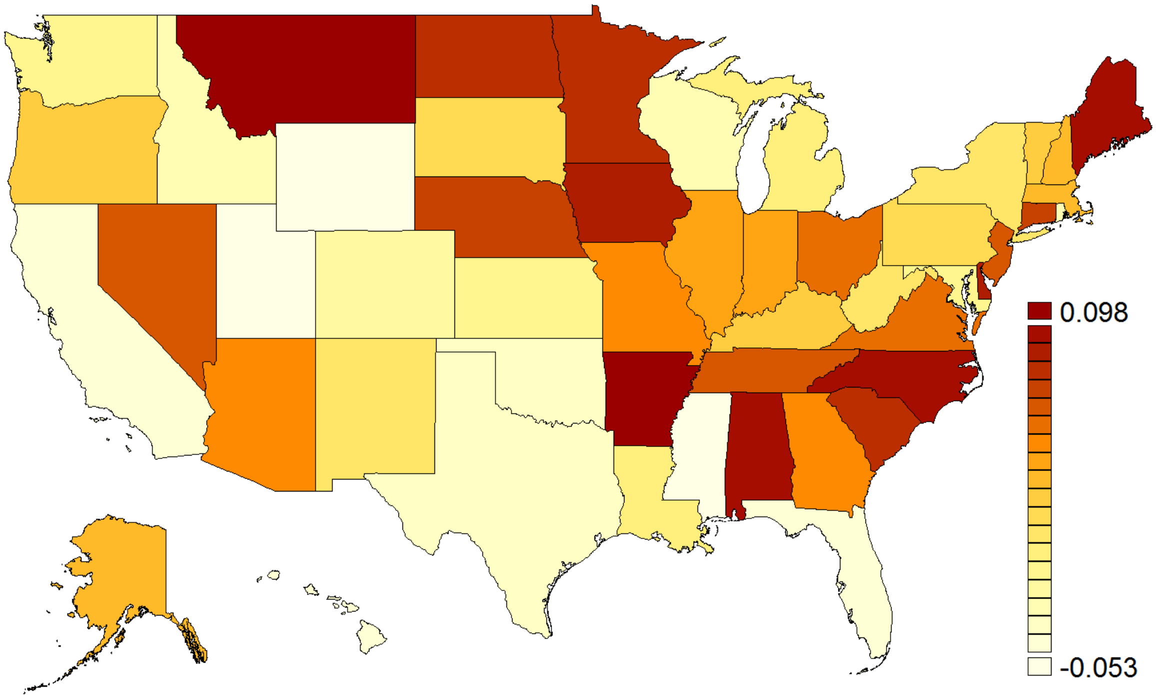

Figure 4:

Growth in public four-year colleges per capita, 1960-96

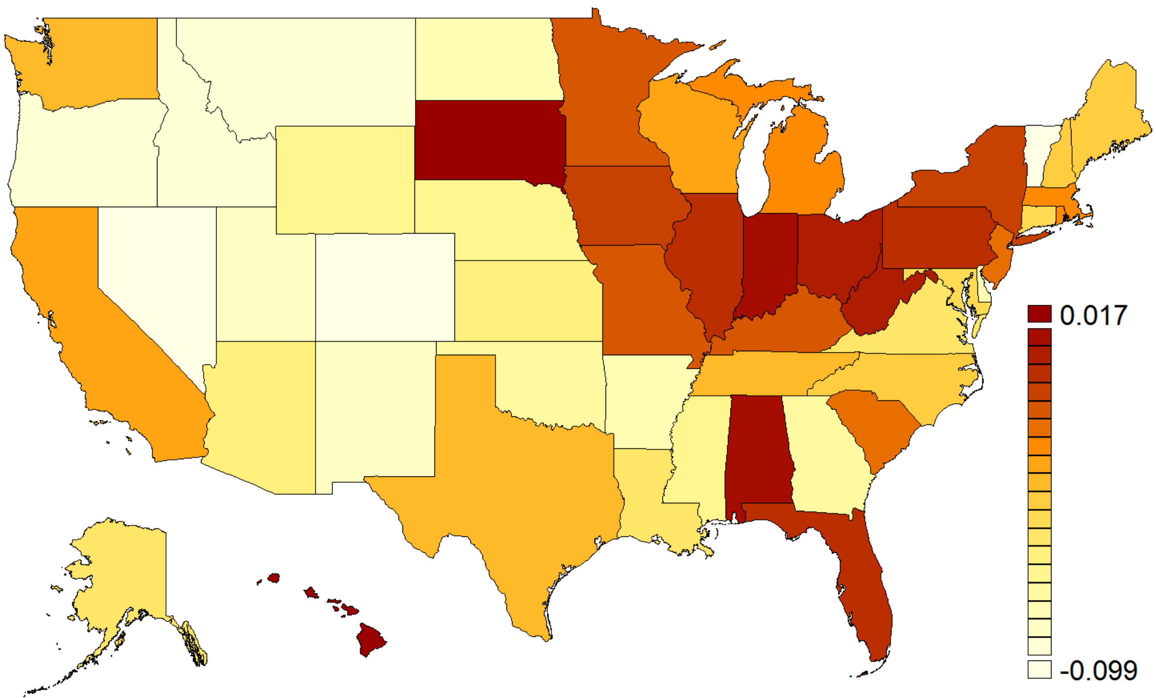

Figure 5:

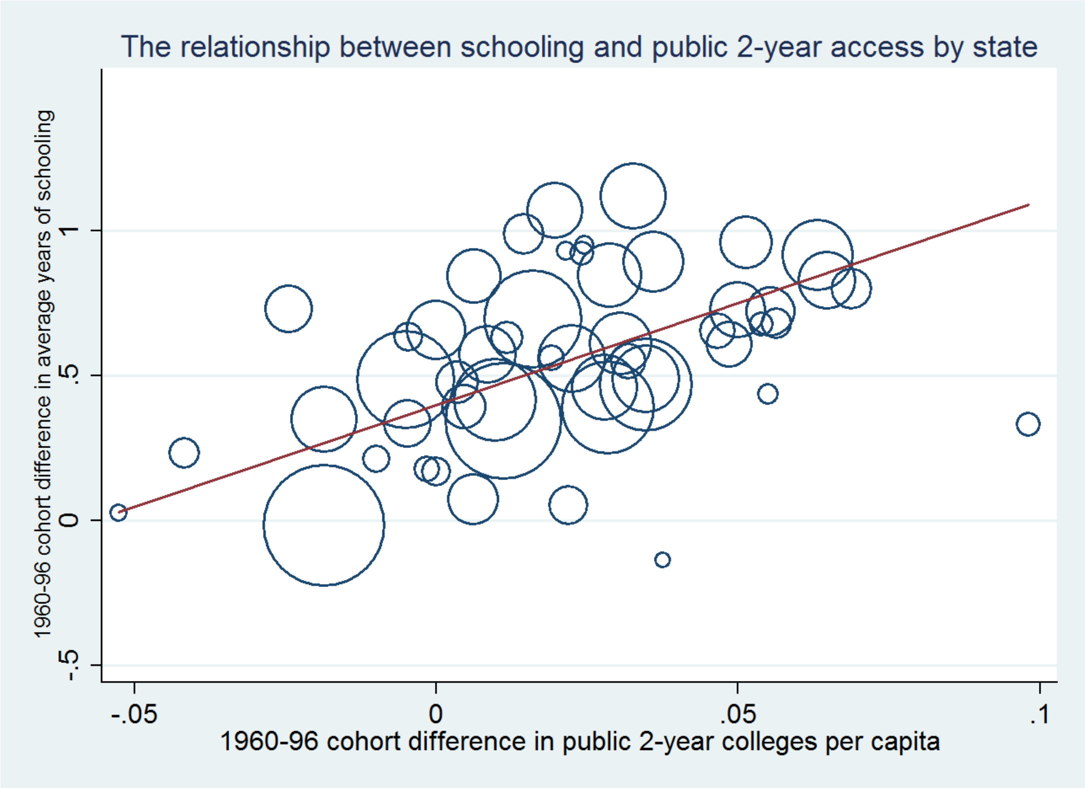

Figure 6:

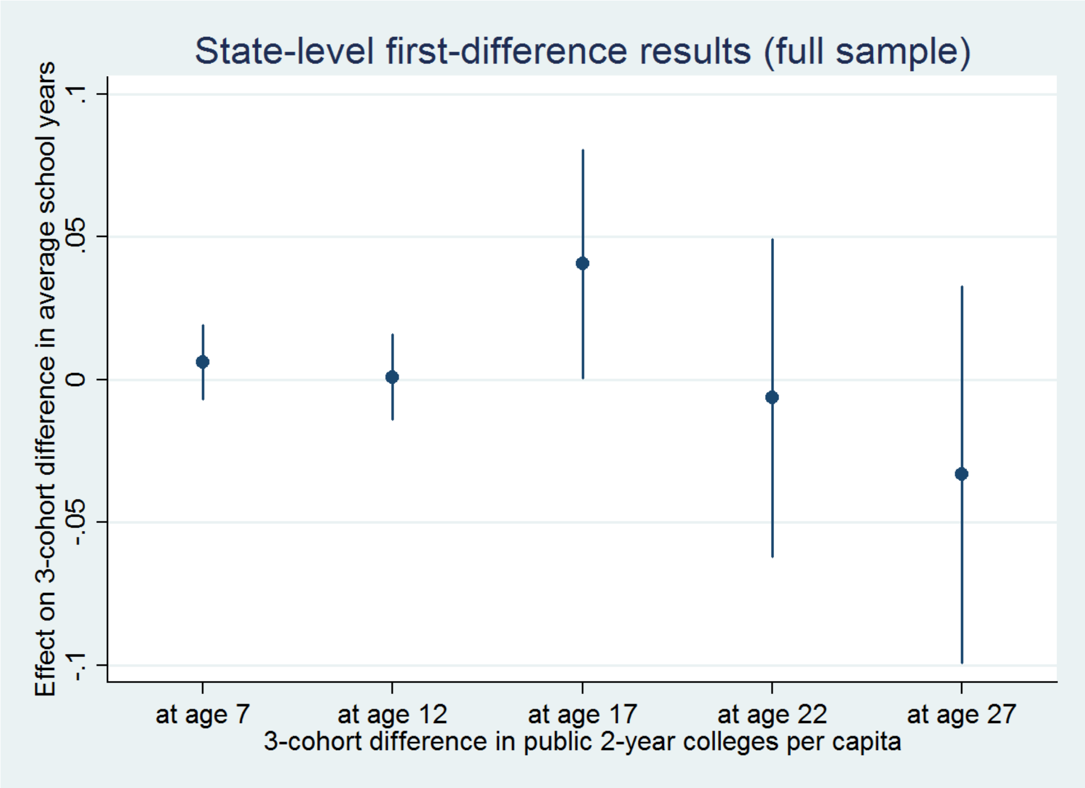

Table 1:

Summary statistics: 1980, 1990, and 2000 Census and 2001-15 ACS		
	mean	sd
Age	44.46	11.37
Years of schooling	13.42	0.38
Less than high school	0.08	
High school diploma	0.38	
Some college but no degree	0.25	
College graduate	0.29	
Log of personal income ($2012)	10.03	0.20
Log of family income ($2012)	10.70	0.18
Employed currently	0.72	
Female	0.51	
Non-white race	0.18	
Hispanic ethnicity	0.07	
State public 4-year institutions per capita, age 17	0.03	0.02
State public 2-year institutions per capita, age 17	0.05	0.02
State private 4-year institutions per capita, age 17	0.06	0.03
State per-capita income, age 17 ($1000's, 2012)	28.34	6.30
State per-pupil spending on K-12, age 17 ($1000's, 2012)	6.38	2.31
State unemployment rate, age 17	6.22	2.09

Sample is made up of state of birth-cohort-year averages for individuals age 22 and above who were 17 between 1960 and 1996 (N=31,959). Estimates are weighted by person sample weights within cells and state of birth populations of 18-22 year-olds in the year a cohort turns 17 across cells.

Table 2:

Summary statistics: 1984-2000 NHIS			
	N	mean	sd
State public 4-year institutions per capita, age 17	511,220	0.03	0.02
State private 4-year institutions per capita, age 17	511,220	0.07	0.03
State public 2-year institutions per capita, age 17	511,220	0.04	0.03
State per-capita income, age 17 ($1000's, 2012)	511,220	26.17	5.82
State per-pupil spending on K-12, age 17 ($1000's, 2012)	511,220	5.70	2.10
State unemployment rate, age 17	508,760	6.06	2.17
State unemployment rate, current	511,220	5.58	1.61
Current cigarette tax per pack ($2012)	511,220	0.46	0.26
Current beer tax per gallon ($2012)	511,220	0.32	0.23
Female	511,220	0.51	
Age	511,220	35.92	8.58
Number of dependent children	511,220	1.13	1.22
Married	508,561	0.67	
White race	511,220	0.83	
Hispanic ethnicity	509,075	0.07	
Years of schooling	506,178	13.39	2.60
Currently employed	510,431	0.81	
No health insurance	403,607	0.16	
Excellent or very good health	509,512	0.71	
Obese (BMI>30)	418,063	0.15	
More than 7 days in bed due to illness or injury, previous year	428,382	0.09	
Binge drank (5 or more drinks on one occasion) in previous month	63,945	0.15	
Current smoker	154,363	0.31	
Number of occasions of moderate or vigorous exercise in last week	59,587	4.29	5.14
Activity limitation due to one or more chronic conditions	510,563	0.11	
No visits to healthcare professional in previous year	360,068	0.29	
1-3 visits to healthcare professional in previous year	360,068	0.47	
4 or more visits to healthcare professional in previous year	359,064	0.25	
Spent at least one night in hospital in previous year	511,176	0.08	
Has major depression	102,835	0.02	
Has had cancer	107,251	0.03	
Has a learning disability	75,803	0.01	
Has had a cardiac event (stroke, heart attack, etc.)	70,406	0.02	
Has a cardiovascular condition (diabetes, hypertension, heart disease, etc.)	70,353	0.18	
Has a respiratory condition (emphysema, chronic bronchitis, etc.)	70,400	0.05	
Has other condition	70,325	0.13	
Has birth condition (cystic fibrosis, etc.)	75,768	<0.01	
Has asthma, seasonal allergies, or sinusitis	70,323	0.29	

Sample is made up of individuals age 22 and above who were 17 between 1960 and 1996. Estimates are weighted by person sample weights. Sample sizes differ because some questions are asked in some years but not others.

Table 3:

Effects of college access measures on educational outcomes, Census and ACS								
	Less than HS	HS grad	Some college	College graduate	Years of schooling	Ln(personal income)	Ln(family income)	Employed
Public 4-year schools per	0.123	-0.648***	0.325**	0.199	0.818	-0.104	-0.346	-0.140
1,000 18-22 year-olds, age 17	(0.201)	(0.224)	(0.133)	(0.214)	(1.403)	(0.333)	(0.344)	(0.121)
Public 2-year schools per	-0.197*	-0.414***	0.209***	0.402***	2.539***	0.252	0.101	0.057
1,000 18-22 year-olds, age 17	(0.101)	(0.144)	(0.070)	(0.117)	(0.758)	(0.155)	(0.159)	(0.082)
Private 4-year schools per	0.083	-0.146	-0.054	0.117	0.248	-0.358*	-0.531**	-0.010
1,000 18-22 year-olds, age 17	(0.132)	(0.133)	(0.079)	(0.158)	(1.011)	(0.194)	(0.245)	(0.111)
R-square	0.788	0.657	0.650	0.778	0.841	0.593	0.664	0.706

*** p<0.01, ** p<0.05, * p<0.1. N=31,959. All models include state of birth, cohort, and year dummies; sex, race, and ethnicity dummies, state-level k-12 spending per pupil at age 17 (and its square), per capita income at age 17 (and its square), and unemployment rate at age 17 (and its square). Estimates are weighted by person sample weights within cells and state of birth populations of 18-22 year-olds in the year a cohort turns 17 across cells. Standard errors are clustered at the state level.

Table 4:

Effects of college access measures on state-level characteristics						
	k-12 spending per pupil	Income per capita	Unemployment rate	Public 4-year schools	Private 4-year schools	Number of 18-22 year olds
Public 4-year schools per	13.168*	-14.137	16.240	---	0.404**	2,476.426**
1,000 18-22 year-olds, age 17	(7.692)	(18.201)	(10.328)	---	(0.163)	(938.492)
Public 2-year schools per	3.204	-3.352	0.944	0.051	0.033	-1,109.233*
1,000 18-22 year-olds, age 17	(3.970)	(8.326)	(5.338)	(0.037)	(0.068)	(582.473)
Private 4-year schools per	-3.552	-19.111	-15.675	0.206***	---	-1,174.699*
1,000 18-22 year-olds, age 17	(5.693)	(15.090)	(10.625)	(0.072)	---	(591.974)
R-squared	0.967	0.981	0.780	0.962	0.973	0.970

*** p<0.01, ** p<0.05, * p<0.1. N=31,959. All models include state of birth, cohort, and year dummies; sex, race, and ethnicity dummies, state-level k-12 spending per pupil at age 17 and its square (except column 1), per capita income at age 17 and its square (except column 2), and unemployment rate at age 17 and its square (except column 3). Estimates are weighted by person sample weights within cells and state of birth populations of 18-22 year-olds in the year a cohort turns 17 across cells. Standard errors are clustered at the state level.

Table 5:

Effects of college access measures on educational outcomes by race/ethnicity, Census and ACS												
	Non-Hispanic Whites				Non-Hispanic Non-whites				Hispanics			
	Years of schooling	Ln(personal income)	Ln(family income)	Employed	Years of schooling	Ln(personal income)	Ln(family income)	Employed	Years of schooling	Ln(personal income)	Ln(family income)	Employed
Public 4-year schools per	1.210	0.129	-0.120	-0.015	-1.359	-0.076	-0.097	0.022	-0.394	0.166	-0.845	-0.579
1,000 18-22 year-olds, age 17	(1.484)	(0.302)	(0.325)	(0.114)	(2.735)	(1.130)	(0.951)	(0.348)	(3.042)	(0.978)	(1.305)	(0.395)
Public 2-year schools per	4.008***	0.522***	0.335*	0.126	1.672*	-0.122	-0.374	-0.094	5.391***	1.796***	1.235**	0.289
1,000 18-22 year-olds, age 17	(0.906)	(0.183)	(0.178)	(0.077)	(0.934)	(0.258)	(0.287)	(0.142)	(1.545)	(0.509)	(0.544)	(0.178)
Private 4-year schools per	0.433	-0.037	-0.163	0.051	-1.925	-1.398**	-1.394**	-0.136	2.335	-1.092	-0.972	0.073
1,000 18-22 year-olds, age 17	(1.144)	(0.225)	(0.230)	(0.123)	(1.576)	(0.542)	(0.577)	(0.204)	(1.946)	(0.745)	(0.661)	(0.249)
Observations	31,958	31,958	31,958	31,958	31,476	31,423	31,446	31,476	27,726	27,292	27,557	27,726
R-square	0.804	0.540	0.591	0.704	0.362	0.242	0.269	0.434	0.134	0.102	0.085	0.198

*** p<0.01, ** p<0.05, * p<0.1. All models include state of birth, cohort, and year dummies; sex dummies, state-level k-12 spending per pupil at age 17 (and its square), per capita income at age 17 (and its square), and unemployment rate at age 17 (and its square). Estimates are weighted by person sample weights within cells and state of birth populations of 18-22 year-olds in the year a cohort turns 17 across cells. Standard errors are clustered at the state level.

Table 6:

Effects of 2-year college access on educational outcomes by age, Census and ACS				
	Years of schooling	Ln(personal income)	Ln(family income)	Employed
Age 30 and below (N=3,663)	1.928**	0.312	0.177	0.131
	(0.790)	(0.411)	(0.562)	(0.125)
Age 31-40 (N=8,244)	3.117***	0.629**	0.195	0.277**
	(1.026)	(0.262)	(0.308)	(0.112)
Age 41-50 (N=8,463)	2.484***	0.769**	0.507	0.157*
	(0.757)	(0.323)	(0.324)	(0.081)
Age 51-60 N=7,712)	1.979***	0.371*	0.247	0.010
	(0.637)	(0.191)	(0.214)	(0.086)
Age 60 and above (N=3,876)	1.801***	0.013	0.093	0.169
	(0.581)	(0.208)	(0.203)	(0.133)

*** p<0.01, ** p<0.05, * p<0.1. Sample composed of non-Hispanic whites and Hispanics. All models include state of birth, cohort, and year dummies; sex, race, and ethnicity dummies, state-level k-12 spending per pupil at age 17 (and its square), per capita income at age 17 (and its square), and unemployment rate at age 17 (and its square). Estimates are weighted by person sample weights within cells and state of birth populations of 18-22 year-olds in the year a cohort turns 17 across cells. Standard errors are clustered at the state level.

Table 7:

Instrumental variable estimate of the return to schooling, Census and ACS	
	Ln(income)
Years of schooling	0.137***
	(0.042)
Observations	31,958
F-stat on excluded instrument	16.40

*** p<0.01, ** p<0.05, * p<0.1. Sample composed of non-Hispanic whites and Hispanics. All models include state of birth, cohort, and year dummies; sex, race, and ethnicity dummies, state-level k-12 spending per pupil at age 17 (and its square), per capita income at age 17 (and its square), and unemployment rate at age 17 (and its square). Years of schooling instrumented with state-level public 2-year colleges per capita. Estimates are weighted by person sample weights within cells and state of birth populations of 18-22 year-olds in the year a cohort turns 17 across cells. Standard errors are clustered at the state level.

Table 8:

Effects of college access measures on schooling and health behaviors, NHIS				
	Years of schooling	Binge drank in past month	Current smoker	Exercise frequency in last week
Public 2-year schools per 1,000	2.861***	0.115	-0.405**	7.220**
18-22 year-olds, age 17	(0.872)	(0.257)	(0.159)	(3.167)
Public 4-year schools per 1,000	0.042	0.058	0.584	-5.531
18-22 year-olds, age 17	(2.421)	(0.363)	(0.385)	(7.831)
Private 4-year schools per 1,000	1.557	-0.030	-0.400	2.634
18-22 year-olds, age 17	(1.577)	(0.293)	(0.289)	(4.042)
Observations	369,314	49,791	113,346	43,555
R-square	0.015	0.075	0.007	0.032

*** p<0.01, ** p<0.05, * p<0.1. Sample is composed of non-Hispanic whites. All models include state of birth, cohort, year, and sex dummies; quadratics of the following variables based on state of birth: k-12 spending per pupil at age 17, per capita income at age 17, unemployment rate at age 17; and quadratics of the following variables based on current state: current unemployment rate, cigarette tax, and beer tax. Estimates are weighted by person sample weights. Standard errors are clustered at the state of birth level.

Table 9:

Effects of college access measures on health conditions, NHIS	Self-reported health excellent or very good	Obese (BMI>=30)	More than 7 bed days in past year	4 or more doctor visits in previous year	At least 1 night in hospital in previous year	Activity limitation
Public 2-year schools per 1,000	0.232**	-0.100	-0.019	-0.285***	-0.081	-0.079
18-22 year-olds, age 17	(0.111)	(0.098)	(0.051)	(0.095)	(0.055)	(0.071)
Public 4-year schools per 1,000	-0.050	-0.382	-0.048	0.048	0.044	0.104
18-22 year-olds, age 17	(0.275)	(0.246)	(0.138)	(0.219)	(0.126)	(0.153)
Private 4-year schools per 1,000	-0.031	0.145	-0.098	-0.141	-0.137*	-0.130
18-22 year-olds, age 17	(0.162)	(0.162)	(0.130)	(0.168)	(0.076)	(0.145)
Observations	370,653	304,367	311,382	260,167	371,596	371,218
R-square	0.017	0.017	0.009	0.037	0.014	0.014

*** p<0.01, ** p<0.05, * p<0.1. Sample is composed of non-Hispanic whites. All models include state of birth, cohort, year, and sex dummies; quadratics of the following variables based on state of birth: k-12 spending per pupil at age 17, per capita income at age 17, unemployment rate at age 17; and quadratics of the following variables based on current state: current unemployment rate, cigarette tax, and beer tax. Estimates are weighted by person sample weights. Standard errors are clustered at the state of birth level.

Table 10:

Effects of college access measures on health conditions, NHIS	Has asthma or allergies	Has learning disability	Has birth condition	Has had cardiac event	Has had cancer	Has cardiovascular condition	Has respiratory condition	Has other condition	Has major depression
Public 2-year schools per 1,000	0.410	-0.016	0.015	0.037	0.124	-0.479**	0.133	0.062	0.039
18-22 year-olds, age 17	(0.343)	(0.067)	(0.021)	(0.106)	(0.144)	(0.224)	(0.168)	(0.199)	(0.092)
Public 4-year schools per 1,000	-0.578	-0.229	0.020	0.094	-0.237	-1.200***	-0.105	-0.451	-0.195
18-22 year-olds, age 17	(0.656)	(0.174)	(0.048)	(0.259)	(0.254)	(0.441)	(0.409)	(0.398)	(0.147)
Private 4-year schools per 1,000	0.348	0.187*	-0.033	0.230	0.012	0.506	0.218	0.480	-0.031
18-22 year-olds, age 17	(0.420)	(0.094)	(0.036)	(0.164)	(0.175)	(0.310)	(0.211)	(0.308)	(0.101)
Observations	51,506	55,312	55,295	51,574	78,656	51,533	51,572	51,521	75,294
R-square	0.026	0.005	0.002	0.020	0.019	0.066	0.013	0.016	0.005

*** p<0.01, ** p<0.05, * p<0.1. Sample is composed of non-Hispanic whites. All models include state of birth, cohort, year, and sex dummies; quadratics of the following variables based on state of birth: k-12 spending per pupil at age 17, per capita income at age 17, unemployment rate at age 17; and quadratics of the following variables based on current state: current unemployment rate, cigarette tax, and beer tax. Estimates are weighted by person sample weights. Standard errors are clustered at the state of birth level.

Table 11:

Effects of college access measures on schooling and health behaviors by gender, NHIS												
	Years of schooling		Self-reported health excellent or very good		4 or more doctor visits in previous year		Current smoker		Exercise frequency in last week		Has a cardiovascular condition	
	men	women	men	women	men	women	men	women	men	women	men	women
Public 2-year schools per 1,000	3.900***	1.864*	0.294**	0.169	-0.132	-0.430**	-0.130	-0.614***	10.249**	5.100	-0.291	-0.633*
18-22 year-olds, age 17	(0.999)	(1.012)	(0.128)	(0.132)	(0.119)	(0.161)	(0.288)	(0.184)	(4.464)	(3.359)	(0.261)	(0.352)
Public 4-year schools per 1,000	-0.059	-0.083	0.262	-0.351	-0.220	0.271	0.712	0.409	-23.581**	10.537	-2.128***	-0.382
18-22 year-olds, age 17	(2.374)	(2.741)	(0.332)	(0.364)	(0.319)	(0.273)	(0.711)	(0.538)	(11.735)	(9.067)	(0.677)	(0.629)
Private 4-year schools per 1,000	2.698	0.420	0.087	-0.158	-0.080	-0.174	-0.591	-0.232	-0.774	5.870	0.522	0.487
18-22 year-olds, age 17	(1.685)	(1.914)	(0.248)	(0.252)	(0.265)	(0.196)	(0.404)	(0.406)	(7.083)	(5.136)	(0.434)	(0.436)
Observations	179,413	189,901	180,170	190,483	126,622	133,545	52,002	61,344	20,134	23,421	23,832	27,701
R-square	0.019	0.017	0.018	0.014	0.008	0.005	0.007	0.008	0.037	0.029	0.075	0.061

*** p<0.01, ** p<0.05, * p<0.1. Sample is composed of non-Hispanic whites. All models include state of birth, cohort, year, and sex dummies; quadratics of the following variables based on state of birth: k-12 spending per pupil at age 17, per capita income at age 17, unemployment rate at age 17; and quadratics of the following variables based on current state: current unemployment rate, cigarette tax, and beer tax. Estimates are weighted by person sample weights. Standard errors are clustered at the state of birth level.

Table 12:

Effects of college access measures on schooling and health behaviors by age, NHIS												
	Years of schooling		Self-reported health excellent or very good		4 or more doctor visits in previous year		Current smoker		Exercise frequency in last week		Has a cardiovascular condition	
	age<=35	age>35	age<=35	age>35	age<=35	age>35	age<=35	age>35	age<=35	age>35	age<=35	age>35
Public 2-year schools per 1,000	6.105***	1.298	0.455**	0.249*	-0.964***	-0.323***	-0.773	-0.482*	8.199	4.278	0.718	-0.534*
18-22 year-olds, age 17	(1.786)	(0.930)	(0.224)	(0.126)	(0.226)	(0.112)	(0.503)	(0.264)	(16.817)	(3.587)	(0.828)	(0.297)
Public 4-year schools per 1,000	-1.126	0.129	-0.038	0.052	-0.140	-0.223	1.400	0.619	16.071	-10.879	-1.368	-1.254**
18-22 year-olds, age 17	(4.782)	(2.510)	(0.619)	(0.359)	(0.486)	(0.382)	(1.348)	(0.536)	(24.898)	(9.444)	(1.007)	(0.479)
Private 4-year schools per 1,000	8.488**	0.223	0.062	0.090	0.451	-0.079	-0.522	-0.671	3.499	0.373	-0.107	0.257
18-22 year-olds, age 17	(3.498)	(1.312)	(0.458)	(0.217)	(0.383)	(0.237)	(0.641)	(0.476)	(16.561)	(4.690)	(0.634)	(0.447)
Observations	164,355	204,959	164,691	205,962	127,811	132,356	49,244	64,102	14,876	28,679	17,867	33,666
R-square	0.017	0.016	0.006	0.015	0.063	0.019	0.006	0.008	0.038	0.028	0.011	0.049

*** p<0.01, ** p<0.05, * p<0.1. Sample is composed of non-Hispanic whites. All models include state of birth, cohort, year, and sex dummies; quadratics of the following variables based on state of birth: k-12 spending per pupil at age 17, per capita income at age 17, unemployment rate at age 17; and quadratics of the following variables based on current state: current unemployment rate, cigarette tax, and beer tax. Estimates are weighted by person sample weights. Standard errors are clustered at the state of birth level.

Appendix Table 1:

Effects of college access measures on schooling and health behaviors, NHIS						
	Years of schooling	Current smoker	Exercise frequency in last week	Self-reported health excellent or very good	4 or more doctor visits in previous year	Has a cardiovascular condition
Public 2-year schools per	-1.459	0.157	-2.538	0.001	0.106	-0.314
1,000 18-22 year-olds, age 17	(1.560)	(0.386)	(6.112)	(0.229)	(0.261)	(0.462)
Public 4-year schools per	-0.023	0.020	-15.563	-0.081	-0.302	0.041
1,000 18-22 year-olds, age 17	(3.275)	(0.931)	(15.605)	(0.592)	(0.583)	(1.101)
Private 4-year schools per	-2.670	1.533**	-9.359	-0.338	-0.447	-0.227
1,000 18-22 year-olds, age 17	(2.446)	(0.660)	(10.458)	(0.317)	(0.296)	(0.753)
Observations	85,224	26,431	10,391	86,097	61,471	12,226
R-square	0.014	0.029	0.049	0.032	0.036	0.116

*** p<0.01, ** p<0.05, * p<0.1. Sample is composed of non-Hispanic non-whites. All models include state of birth, cohort, year, and sex dummies; quadratics of the following variables based on state of birth: k-12 spending per pupil at age 17, per capita income at age 17, unemployment rate at age 17; and quadratics of the following variables based on current state: current unemployment rate, cigarette tax, and beer tax. Estimates are weighted by person sample weights. Standard errors are clustered at the state of birth level.

Appendix Table 2:

Effects of college access measures on schooling and health behaviors, NHIS						
	Years of schooling	Current smoker	Exercise frequency in last week	Self-reported health excellent or very good	4 or more doctor visits in previous year	Has a cardiovascular condition
Public 2-year schools per	-2.554	0.289	7.184	-0.074	-0.190	1.455
1,000 18-22 year-olds, age 17	(3.423)	(0.672)	(12.573)	(0.354)	(0.388)	(1.288)
Public 4-year schools per	10.480**	0.825	-14.513	0.654	1.291*	0.553
1,000 18-22 year-olds, age 17	(4.460)	(1.293)	(15.197)	(0.742)	(0.742)	(1.781)
Private 4-year schools per	-4.619	-0.689	5.010	0.659	0.002	-2.299
1,000 18-22 year-olds, age 17	(3.696)	(0.902)	(23.309)	(0.580)	(0.447)	(1.438)
Observations	47,920	13,564	5,343	48,294	33,573	6,253
R-square	0.063	0.028	0.048	0.028	0.048	0.089

*** p<0.01, ** p<0.05, * p<0.1. Sample is composed of Hispanics. All models include state of birth, cohort, year, and sex dummies; quadratics of the following variables based on state of birth: k-12 spending per pupil at age 17, per capita income at age 17, unemployment rate at age 17; and quadratics of the following variables based on current state: current unemployment rate, cigarette tax, and beer tax. Standard errors clustered at the state of birth level. Estimates are weighted by person sample weights. Standard errors are clustered at the state of birth level.