

In the Footsteps of Siblings: College Attendance Disparities and the Intragenerational Transmission of Educational Advantage

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Abstract

Studies in social stratification have used siblings as a tool to learn about the intergenerational transmission of advantage but less often have asked how siblings impact one another's life chances. The author draws on social capital theory and hypothesizes that when youths attend college, they increase the probability that their siblings attend college. The author further hypothesizes that this effect is strongest among youths whose parents do not have college degrees. Findings from a U.S. national probability sample support both hypotheses. Although it is possible that confounding factors drive the estimates, the author conducts robustness checks that show that confounding would need to be very atypically strong to invalidate a causal interpretation. The positive main effect suggests that an intragenerational transmission of educational advantage exists alongside the intergenerational transmission that receives more attention. Effect heterogeneity points to the potential redundancy of college-educated siblings' benefits when youths already receive similar benefits from college-educated parents.

Keywords

siblings, college attendance, stratification, family, first-generation college students

Studying intersibling effects is important in order to broaden scholars' conceptions of the directions in which educational advantage flows and the roles siblings play in one another's educational outcomes. Research in social stratification frequently analyzes siblings for the purpose of learning about intergenerational persistence but less frequently analyzes siblings to learn about intragenerational processes whereby siblings directly influence one another. Some research uses correlations between adult siblings' socioeconomic characteristics as proxies for the total impact parents exert on the next generation (for a review, see Torche 2015), usually without considering that intersibling effects might also drive these correlations (cf. Knigge 2015). Other research considers how siblings dilute parent-to-child effects, for example, by studying the effects of birth order and sibship size (Black, Devereux, and Salvanes 2005; Downey 1995; Grätz 2018). In birth order and sibship size studies, the role siblings play in the other siblings' educational outcomes is one of dilution; under this framework, siblings hamper the educational attainment of the other siblings by absorbing parents' time and resources that would otherwise go to a single child. Although this research analyzes siblings, the purpose is not

to study how individuals' outcomes affect their siblings' outcomes; instead, parents remain front and center. Each sibling is considered a passive recipient of parents' benefits, not an active mentor who can provide benefits of their own to siblings (cf. Roksa 2019). Studies of intersibling effects in education exist (e.g., Benin and Johnson 1984; Loury 2004), but they are few.

The comparative rarity of research on intersibling effects is unfortunate because this research can advance knowledge of how family members transmit educational advantage. Most of what the field knows on this topic relates to intergenerational processes, and sociologists have doubled down on this intergenerational focus by studying grandparent and great-grandparent influences (Mare 2011; Pfeffer 2014). The relationship between siblings' educational attainments has substantive importance because a positive effect would

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imply that, above and beyond intergenerational forces, there exists an intragenerational, intersibling transmission of educational advantage. A positive effect in this case would deepen scholars' understanding of the family processes that generate educational attainment. Moreover, a positive effect would be methodologically important because it would inspire caution in estimates of intergenerational persistence that rely on sibling correlations, because these estimates would capture not only the effect of parents but also the effect of siblings on one another.

Intersibling effects also would have practical implications. Because of a variety of disadvantages that students without college-educated parents face (Wilbur and Roscigno 2016), a large socioeconomic gap exists in attendance at institutions of postsecondary education (henceforth, college attendance, collapsing two- and four-year institutions unless otherwise specified). This problem threatens both equity and overall economic prosperity. The U.S. Department of Education regularly produces reports on this issue (Cataldi, Bennett, and Chen 2018; Choy 2001; Redford and Hoyer 2017). The consistent conclusion of these reports is that students without college-educated parents attend college at a far lower rate than students who have college-educated parents and that differences in high school academic achievement cannot fully explain this inequality. Accordingly, when former president Obama described his ambitious goal for the United States to lead the world in college degree production, some argued that youths without college-educated parents needed to be key targets given their comparatively low college participation and large population (Bowen, Chingos, and McPherson 2009; Templin 2011). The United States attempts to increase these youths' college participation with thousands of precollege outreach programs, 71 percent of which specifically target students whose parents are not college educated (Swail and Perna 2002). Among those 71 percent are the federal TRiO programs, a series of federally funded programs with an annual budget of nearly \$1 billion (Falk, Lynch, and Tollestrup 2018). Given the weight the United States accords this problem, intersibling effects may have practical importance: knowing how siblings affect one another can be useful in evaluating the total benefit of interventions that target disadvantaged youth because the interventions might have spillover effects on participants' siblings. For example, if sibling college attendance makes one's own college attendance more likely, then college access programs may promote social mobility among disadvantaged youth who do not participate in the programs but who have siblings who do. The overall benefit of college access programs, then, is greater than typically assessed when studying program participants only.

Undergraduate education is a particularly fruitful stage to examine because this stage is a pivotal step toward upward social mobility (Blau and Duncan 1967; Hout 1988; Torche 2011). A bachelor's degree, in particular, bestows powerful socioeconomic benefits. Hout (1988) found that social

background is unassociated with occupational class among those who have bachelor's degrees. Torche's (2011) more recent analysis showed that social background is associated with occupational class and earnings more weakly among those whose highest degree is a bachelor's degree compared to those without a bachelor's degree (although a strong social background gradient emerges among people who have advanced degrees), and some have found that positive selection of bachelor's degree holders from modest socioeconomic backgrounds cannot explain this result (Karlson 2019; cf. Zhou 2019). Receiving an undergraduate education is a further important outcome because it confers nonpecuniary benefits such as happiness (Andersson 2018) and lifetime health (Cutler and Lleras-Muney 2006).

In this study, I estimate the effect of sibling college attendance on one's own college attendance. I then examine effect heterogeneity by parental education. Informed by Coleman's (1988) concept of social capital, I hypothesize that sibling college attendance positively affects one's own college attendance and further hypothesize that the effect is weaker among individuals with college-educated parents compared with those without. The results support both hypotheses. Because a vast literature in social stratification shows the many shared family and environmental factors that could lead siblings to similar educational outcomes, I then execute robustness checks to determine the extent of bias that would be necessary to invalidate a causal interpretation of my estimates.

The Salience of Siblings

The sociology of the family is a core subfield of sociology, yet family research investigating intersibling influences is sparse, especially in relation to education (Davies 2018). Most family studies explore romantic relationships or parent-child relationships, paying little attention to how siblings influence one another. An early article (Irish 1964) made three claims: (1) few sociologists of the family have researched sibling relationships; (2) most sibling studies investigate how siblings differ in their relationships with other people, such as parents; and (3) more studies on sibling relationships would enrich scholarship in the sociology of the family. Progress has been slow, though: McHale, Updegraff, and Whiteman (2012) offered an updated review on sibling relationships research, concluding that "although siblings are building blocks of family structure and key players in family dynamics, their role has been relatively neglected by family scholars" (p. 913).

The relatively small literature on sibling relationships suggests that siblings, especially older siblings, are formative in one's development. Older siblings who rapidly progress in their identity development facilitate rapid progress on the part of their younger siblings (Wong et al. 2010). Youths' narratives of themselves frequently center around the ways they are similar to and different from their siblings (Davies

2015). As individuals go through adolescence, they often adopt the attitudes and tastes of their older siblings (McHale et al. 2001). Younger siblings use their older siblings as models for behavior, often mirroring their older siblings in age of sexual debut (Widmer 1997), body weight fluctuations (Christakis and Fowler 2007), alcohol and drug use (Altonji, Cattani, and Ware 2017), cigarette use (Massey and Krohn 1986), and career paths (Bingley, Lundborg, and Lyk-Jensen forthcoming; Schultheiss et al. 2002). Massey and Krohn (1986), in fact, showed that adolescents mimic their older siblings' smoking behavior more than their parents' smoking behavior. Even in adulthood, many individuals maintain extremely strong ties to their siblings: national data show that 60 percent of adults consider at least one sibling to be among their closest friends, and 30 percent would call a sibling first in case of an emergency (White and Riedmann 1992). The intimacy of sibling relationships is perhaps not surprising given that children spend more time with their siblings than they do with friends, with any other family members, or alone (McHale and Crouter 1996). Moreover, 82 percent of children live with at least one sibling, a percentage greater than the percentage living with a father figure (McHale et al. 2012).

College Outcomes and Sibling College Attendance

Why would sibling college attendance raise the probability of one's own college attendance? One possible reason is that college-educated individuals may promote their siblings' college attendance by providing social capital. As conceived by Coleman (1988), social capital is an individual's stock of between-person relations that facilitate desired social outcomes. Social capital resembles other forms of capital in cultivating desired outcomes and having some degree of fungibility. This concept has been an important theory explaining how parents transmit their educational advantages to their children (Kim and Schneider 2005) but has seldom been applied to intersibling effects. Social capital takes three forms that capture, respectively, the information channels, social norms, and obligations people share with one another. I propose that information channels are particularly relevant to the present study, and thus, I describe below how this form of social capital could explain intersibling effects in college attendance. Because the data do not allow me to test this mechanism, my description is speculative only.

College-educated individuals may facilitate their siblings' college attendance by providing information that improves access to college. Coleman argued that information that inheres in social relations can help an individual achieve a desired end, such as attending college. Thus, if youths tend to lack information about the steps needed to attend college, and if college-educated individuals transmit such information to their siblings, then relations with college-educated

siblings constitute a form of social capital that promotes college attendance.

The weight of the evidence suggests that high school students have large holes in their knowledge of applying to and attending college. Avery and Kane (2004) showed that high school seniors overestimate college tuition by a factor greater than two. The parents of high school students, too, overestimate college tuition, and parents with less education often refuse even to hazard a guess of the price of college (Grotsky and Jones 2007). An experiment involving high school students who primarily do not have college-educated parents shows that telling them the tuition at nearby colleges substantially increases expectations of attending college and reduces concerns about costs (Oreopoulos and Dunn 2013). Nonpecuniary information barriers also impede college attendance. High school seniors' behavior suggests confusion about the requisite steps for attending college: among 12th grade students in one low-income school who (1) express in the fall that they want to immediately attend a four-year college and (2) have the academic credentials to do so, more than 20 percent do not take the SAT, and 35 percent do not end up enrolling in a four-year college right away (Avery and Kane 2004). Additionally, filing the application for federal student aid is complex (Dynarski and Scott-Clayton 2006), and experimental evidence suggests that this complexity discourages college attendance. In particular, low-income, dependent students are 8 percentage points more likely to attend college when their parents receive an intervention including personalized help filing the federal student aid application, a streamlined process for filing, personalized estimates of financial aid, and tuition estimates (Bettinger et al. 2012). An important insight from this experiment is that merely listing information in a brochure does not affect college attendance, but hands-on help applying this information, which college-educated siblings may offer, makes a substantial impact.

Given scarce knowledge about postsecondary education, especially among disadvantaged youths and their parents, individuals who are in college plausibly provide siblings firsthand information and assistance that helps the siblings attend college. McDonough's (1997) interviews with adolescents and their families support this theory. College-educated older siblings are key information sources for younger siblings as they consider whether and where to attend college. In some cases in which older siblings have ample knowledge about college attendance but parents have little, older siblings are the younger siblings' primary sources of input, support, and expertise. Stanton-Salazar and Spina's (2003) qualitative evidence similarly suggests that older siblings transmit college-related information to their siblings.

Why would intersibling effects be stronger for youths without college-educated parents? Effect heterogeneity would be in harmony with an information channels explanation: youths with college-educated parents already receive college-related information from their parents, and thus most

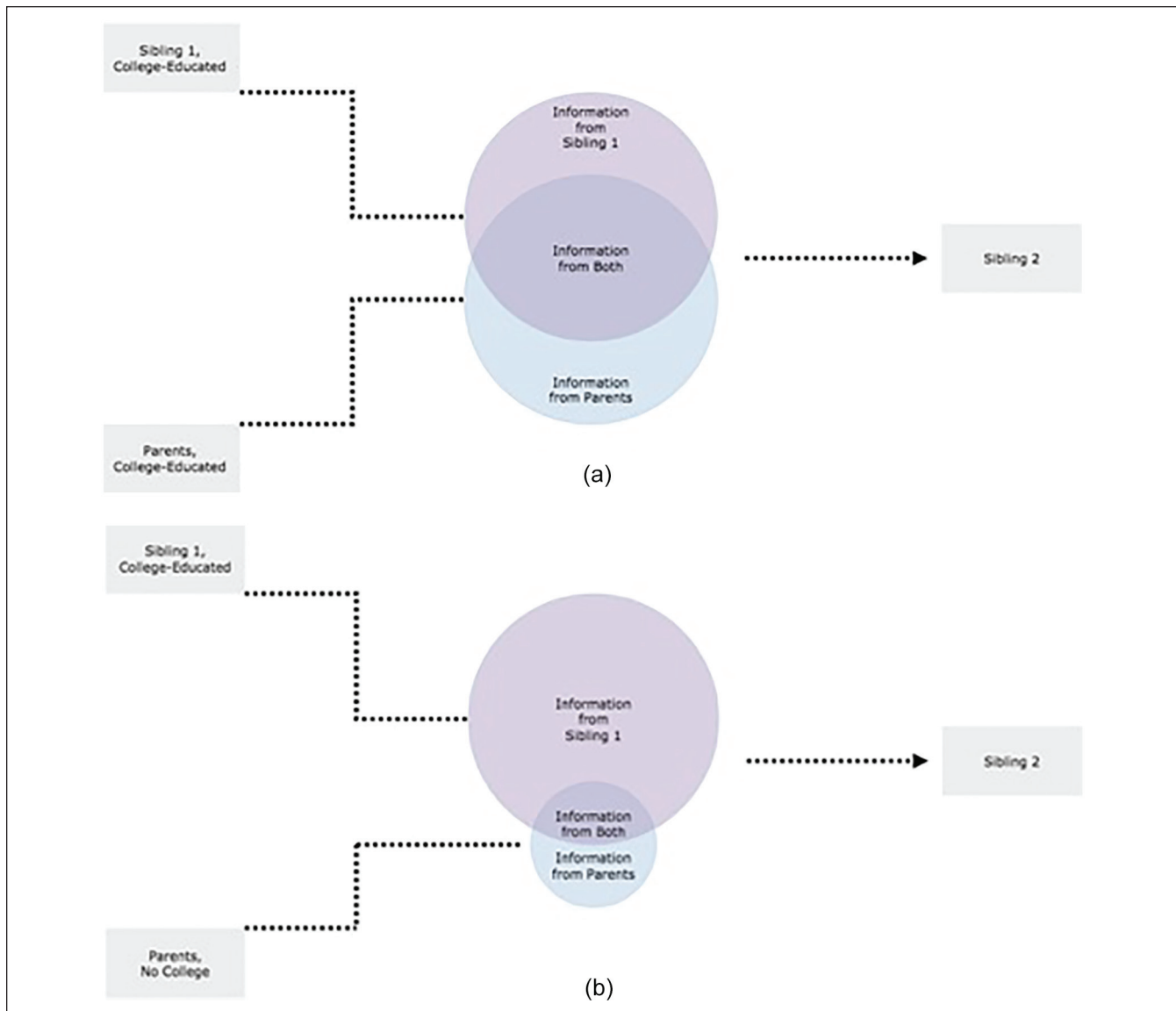


Figure 1. (A) College-educated parents and siblings both provide information that promotes college attendance, but the information largely overlaps. (B) Parents without college educations can provide little information about college, and therefore college-educated siblings provide information of novel value to their siblings.

information from siblings is likely redundant, whereas information from college-educated siblings is likely more novel for youths without college-educated parents. Figure 1 illustrates this idea. Parents and siblings both may provide information about college, and this information is much more extensive if they attended college. However, if both parties have attended college, the information each provides should largely overlap with the information the other provides (Figure 1A). Thus, although a college-educated sibling probably provides some additional insights because he or she typically has attended college more recently than the parents, much of the information this sibling offers likely is redundant with information from college-educated parents. In contrast, if no parents have attended college, most of the

information the sibling provides should be novel information that the parents cannot offer (Figure 1B). Consequently, the impact of a college-educated sibling should be stronger for those without college-educated parents.

Prior Empirical Studies

Just a handful of studies have asked how siblings influence one another's educational attainment. Analyzing data from Nebraska, Benin and Johnson (1984) found a conditional association between siblings' educational attainments. They claimed that this association reflects causal intersibling effects because the association is strongest among brother pairs and weakest among older sister–younger brother pairs,

a form of heterogeneity that captures how sibling role modeling is most influential among same-sex siblings. Hauser and Wong (1989) also found particularly weak associations between older sisters' and younger brothers' educational attainments in Michigan and Nebraska. Loury (2004) studied a sample of African Americans in the baby boomer generation and found that sibling college attendance increases the odds of individuals' own college attendance. More recent work applying regression adjustment to data on SAT takers revealed a positive effect of sibling college attendance (Goodman et al. 2015). However, that study suffered from the limitation that SAT takers are a positively selected sample, especially of the population without college-educated parents, and therefore the results may have missed especially strong intersibling effects among those expected to be less inclined to attend college in the first place. All of these studies are in harmony with recent literature in economics that documents sibling spillovers in academic test scores (Karbownik and Özek 2019; Qureshi 2018), though this literature does not consider educational attainment.

Contributions of the Present Study

In this study I test two hypotheses: (1) sibling college attendance increases the probability of one's own college attendance, and (2) the effect of sibling college attendance on one's own college attendance is greater among those whose parents do not have college degrees compared with those whose parents do. I test these hypotheses using data from the High School Longitudinal Study of 2009 (HSLs), which provides a large national probability sample. Prior literature germane to either hypothesis is sparse, but hypothesis 2 is especially understudied. Furthermore, to my knowledge, no published study has estimated intersibling effects on college attendance, or parental education-based effect heterogeneity therein, using data representative of the full youth population in the United States. Within the small set of prior studies investigating intersibling effects on educational attainment, each has used samples heavily restricted with respect to geographic region, race, or propensity to attend college, with most studies using much older data. In sum, this study contributes to prior literature by assessing how intersibling effects vary with respect to parental education and doing so using data that are unique in being both nationally representative and recent.

Methods

Data

I analyze data from all waves of the HSLs, a study with three interview waves (2009, 2012, and 2016) plus a high school transcript collection in 2013. The 23,000 HSLs participants make up a probability sample of U.S. youth who were in ninth grade in the fall of 2009. There exist several large-scale,

longitudinal studies with national probability samples of youth, but HSLs has important advantages: it is among those with the largest sample sizes, and it is the most recent. These properties of the data aid in generalizing to the secondary student population of today.

Measures. The outcome of interest is whether the respondent ever attended college. I code this outcome as a binary outcome equal to 1 if the respondent had ever enrolled in an institution of postsecondary education by the last wave of data and equal to 0 otherwise. This measure comes from the last wave, during which respondents indicated the last month and year that they were enrolled in an institution of postsecondary education. If the respondent listed a date, I code the outcome as 1, and if the respondent indicated that he or she had never enrolled in an institution of postsecondary education, I code the outcome as 0.

The chief independent variable, or treatment, is whether the respondent has a sibling who attended college. I code treatment status according to respondents' self-reports of siblings' college attendance. In particular, respondents who had not attended college answered the question "Do you have any brothers or sisters who had started college or trade school by the end of February 2016?" and respondents who had attended college answered the question "Do you have any brothers or sisters who started college or trade school before you did?" I code the treatment as 1 if the respondent answered yes and 0 if the respondent answered no.¹ Thus, if a respondent has a sibling who attended college but this sibling started college after the respondent, I consider the respondent to be in the nontreated group. This coding accords with principles of causal ordering: if the respondent started college first, then the sibling's subsequent college attendance could not possibly have caused the respondent to attend college.

The moderator variable is whether the respondent has a parent with a postsecondary degree or certificate. If the respondent reports that the highest level of education completed by both the mother or female guardian and the father or male guardian is a high school diploma or less, I classify the respondent as a would-be first-generation college student. For brevity, I say that these students are first-generation. If the respondent reports that either parent or guardian has attained a postsecondary credential, I classify the respondent as a would-be continuing-generation college student. I say that these respondents are continuing generation. Postsecondary credentials include bachelor's degrees, associate's degrees,

¹The available measure of sibling college attendance does not allow differentiation between siblings who attended a four-year college and siblings who attended a two-year college, nor does the measure allow differentiation between part-time and full-time enrollment. I choose to measure the outcome variable, detailed above, in a similarly broad fashion in order for the outcome to optimally mirror the independent variable.

certificates or diplomas from schools providing occupational training, and advanced degrees.²

Respondents whose siblings attend college are likely different from other respondents in a host of ways that predict whether the respondents themselves attend college. Observed differences between the college attendance rates of individuals with and without college-educated siblings, then, may reflect the influence of confounding factors rather than a true causal relationship. To ameliorate this problem, I control for several factors that may have influenced the siblings' college attendance. For each respondent, I control for sex, race, father's and mother's years of schooling,³ whether the father is unemployed, whether the mother is unemployed, family income, number of siblings, number of household members, parental marital status, parent age, hours spent with family on a typical school day, parent's educational expectations of the respondent, whether the respondent is a member of a religious group, standardized math test score, cumulative high school grade point average (GPA), the highest level of high school math completed by the respondent, number of Advanced Placement (AP) or International Baccalaureate (IB) courses taken by the respondent, how often the respondent is late to class, how often the respondent fails to finish assigned homework, how often the respondent is absent from school, hours spent on homework on a typical school day, the respondent's effort in math class, the respondent's effort in science class, the respondent's region of the country, and the respondent's locale type. Table A.1 in Online Appendix A details the coding procedures for all measures analyzed in this study. Some of the listed control variables, such as effort in math class, present an ambiguous case in which it is unclear whether the variables are mediators or confounders, a pervasive problem in social science (King 2010). On the one hand, if the sibling's college

²The data do not allow me to distinguish between parents who never attended college and those who started but did not attain a credential. On the basis of my own calculations using Current Population Survey data (available at <https://www.census.gov/cps/data/cpstablecreator.html>), in 2009, about 17 percent of U.S. residents 35 to 54 years of age (a plausible age range for the parents of HSLs respondents) had attended college without receiving a postsecondary degree. Therefore, especially if information channels from people who have applied to and attended college drive youths' college attendance, the available measure of parental education likely understates effect heterogeneity between youths who have parents with postsecondary degrees and youths whose parents have never attended college, as parents who attended but did not complete college can provide some of the same information that parents with college degrees provide.

³This covariate adjusts for the remaining variation in parental education after stratifying the sample into the first-generation and continuing-generation subsamples. For example, the control variables help distinguish a continuing-generation student whose father has a bachelor's degree and whose mother has a high school diploma from a continuing-generation student for whom both parents have advanced degrees.

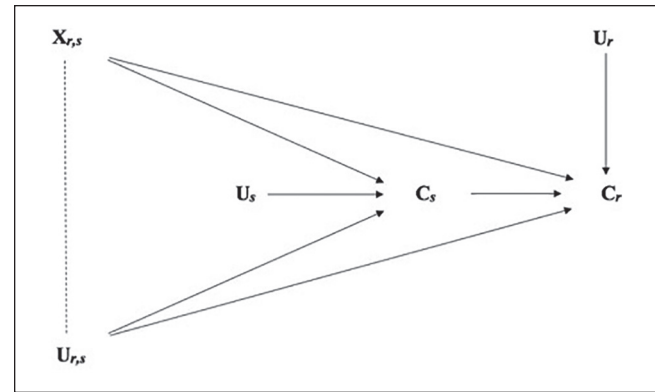


Figure 2. Causal model describing the relationship between sibling college attendance C_s and respondent college attendance C_r . Note. $X_{r,s}$ captures all measured confounders that influence both siblings' college attendance, and $U_{r,s}$ captures unmeasured confounders. U_s represents the factors that influence the sibling's college attendance but are not directly related to that of the respondent. U_r represents the factors that influence the respondent's college attendance but are not directly related to that of the sibling. Arrows denote causal relationships and the dashed line denotes a correlation.

attendance motivates the respondent to put effort into math class, then controlling for this effort may induce overcontrol bias as well as endogenous selection bias (Elwert and Winship 2014). On the other hand, if effort in math class measured in the second wave captures respondent characteristics not attributable to sibling college attendance as measured during the final wave, then controlling for effort reduces omitted variable bias. The data do not indicate when the sibling first attended college, so it is unclear whether effort in math class precedes sibling college attendance. Given the real concern for upward bias due to unobserved confounding, I choose to err on the side of more control variables. However, for reference, I also report the estimates from models that omit covariates that are potentially posttreatment. These models yield estimates that are qualitatively the same but quantitatively less conservative compared with those I will present in my main documentation.

Analytic Strategy

Estimating the effect of sibling college attendance on one's own college attendance is challenging because siblings usually, though not always, share environmental and genetic features that predispose them to attend or not attend college. According to the causal model in Figure 2, sibling college attendance, C_s , increases the probability of respondent college attendance, C_r . Sibling college attendance and respondent college attendance also are both functions of observed confounders $X_{r,s}$ that jointly influence both siblings' college attendance. Elements of $X_{r,s}$ are the control variables listed in the previous section, such as family income and hours spent

with family. In addition to observed confounders, unobserved confounders $U_{r,s}$ also jointly influence siblings' college attendance. Elements of $U_{r,s}$ may include grandparental wealth, educational backgrounds of adults in the neighborhood, shared genes that predict educational attainment, and family processes such as bedtime reading that vary above and beyond factors in $X_{r,s}$. Finally, each sibling has a set of factors that influences his or her college attendance but is not directly related to the other sibling's college attendance (U_r for the respondent and U_s for the sibling). Examples may include teachers (Chetty, Friedman, and Rockoff 2014) and genes (Domingue et al. 2015) that the siblings do not share. This set of idiosyncratic factors is the engine of intersibling effects because it provides the variation in the sibling's college attendance that is not due to shared influences; in turn, this variation allows sibling college attendance independently to cause changes in respondent college attendance. Take the concrete example of genes. Some genotypic features can help predict educational attainment, and within full biological sibling pairs, these features are allocated randomly at conception (Fletcher and Lehrer 2011). Thus, the exogenous component of the sibling's genes can cause changes in the respondent's college attendance that operate only by way of the sibling's college attendance.

To obtain plausible causal estimates, I aim to minimize the unobserved confounders $U_{r,s}$ so that I compare the outcomes of respondents who are similar in as many ways as possible, except that one of the respondents has a sibling who attended college and the other has a sibling who did not. To this end, I control for observed factors $X_{r,s}$ using the inverse probability-weighted regression adjustment (IPWRA) estimator. IPWRA estimates a treatment model that includes each individual's observed characteristics. The treatment model yields a propensity score for each individual, representing the probability that someone with his or her characteristics receives the treatment. Next, IPWRA estimates an outcome model that includes observed characteristics while also weighting cases by their inverse probability weights; for individuals with a college-educated sibling, the inverse probability weight is the inverse of the propensity score, and for individuals without a college-educated sibling, the inverse probability weight is the inverse of 1 minus the propensity score. This model generates predicted probabilities of college attendance for all respondents, and the difference in average predicted probabilities between those with and without a college-educated sibling provides the point estimate. I apply this estimator to first-generation and continuing-generation sibling sets separately. For each subgroup, I use a logistic regression model for both the treatment and outcome models and include all control variables listed above.⁴ Even though I use logistic regression models for both the treatment

⁴Results (available upon request) are virtually identical when using a linear probability model instead of a logistic regression model to generate predicted probabilities of respondent college attendance.

and outcome, IPWRA point estimates are always expressed in probability units because they come from subtracting average predicted probabilities. I use base wave–final wave panel weights and robust standard errors to account for the complex design of HSLs. Because I include panel weights, I weight each case not merely by its inverse probability weight but rather by the product of its inverse probability weight and panel weight. In Online Appendix A, I check two of IPWRA's undergirding assumptions—covariate balance and positivity—and find support for both in my case.

IPWRA is gaining favor in the social sciences because it is consistent if either the treatment model or the outcome model is correctly specified (Wooldridge 2007). This doubly robust property offers the advantages of other propensity score methods (such as matching) as well as traditional regression adjustment techniques. As with propensity score matching, proper assumptions about the functional form between covariates and respondent college attendance are not required for consistent estimation, because inverse probability weights have the potential to balance the treated and nontreated samples without these parametric assumptions (Thoemmes and Ong 2016). If the relationship between covariates and respondent college attendance is properly specified, though, IPWRA retains the ordinary least squares regression advantage of being consistent without needing to properly model the relationship between covariates and sibling college attendance. The latter advantage is attractive for this study because HSLs measures no sibling characteristics besides college attendance, and therefore the outcome model is more likely to be correctly specified than the propensity score model, which estimates sibling college attendance using only characteristics of respondents and their parents.

Missing Data

The target population contains U.S. ninth graders from the fall of 2009 who, in 2016, had siblings who were of traditional college age or older. The initial HSLs sample contains 23,503 individuals, but I drop cases from the sample in two steps. First, I drop 13,899 cases that appear outside the target population on account of being singletons, being the oldest of their siblings,⁵ or having provided no data on whether they have siblings. Second, I drop 2,087 cases that are in the target population but missing in the outcome, either because of unit

⁵I drop eldest children because most such respondents are unlikely to have had younger siblings who had attended college before the respondents, with enough time to influence the respondents' own decisions to attend college. Respondents were about 21 years old during the final survey wave, thus they were at most about 20 years old when their younger siblings might have started college in time to influence the eldest child's decision to start college the following year. Because I do not know the ages of the younger siblings, I drop eldest child cases in order not to assume that the respondent had a college-aged younger sibling the year before the final wave.

nonresponse in the final wave or item nonresponse on the item about the last postsecondary enrollment.⁶ This leaves an analytic sample size of 7,517 individuals, about 39 percent of whom are first generation. To address missingness in other measures, I perform multiple imputation by chained equations with five imputations.

Results

Descriptive Statistics by Subgroup

Table 1 shows means and standard deviations of each measure, by treatment status and parental education group. The raw association between sibling college attendance and the respondent's college attendance is strong: 91 percent of those with siblings who attended college themselves attend college, compared with 54 percent among those who have at least one older sibling but no siblings that attended college. Table 1 also demonstrates how those with siblings who attended college are advantaged with respect to socioeconomic, academic, and geographic factors that predict college attendance, such as family income, parental employment, math test scores, high school GPA, AP or IB participation, location in the Northeast, and suburbanicity. In sum, descriptive statistics show a relationship between siblings' college attendance but also demonstrate the need to control for a host of factors on which those with and without college-educated siblings differ if one wishes to test a causal relationship.

Effect of Sibling College Attendance

The findings support the theory that, among first-generation sibling sets, sibling college attendance raises the probability that an individual attends college. Figure 3 shows sibling college attendance effect estimates on respondent college attendance. Sibling college attendance raises the probability of first-generation individuals' college attendance by an estimated 28 percentage points. The estimate is quite precise with a narrow 95 percent confidence interval (0.23–0.33) that is far from including zero. As reference for the practical significance of this estimate, note that the effect size exceeds the impact of Head Start participation on college attendance by a factor of about 5 (Ludwig and Miller 2005). Similarly,

⁶Comparing the retained 7,517 cases with the dropped 2,087 cases with respect to observed covariates, it seems likely that retained cases have a higher college attendance rate than the dropped cases; retained cases have greater academic achievement as measured by factors such as high school GPA, as well as higher family incomes and a greater likelihood of being female (full table of standardized differences available upon request). Therefore, to the extent that individuals with greater initial propensities to attend college are less sensitive to their siblings' college attendance, omitting cases missing in the outcome probably leads to downward bias when estimating the effect of sibling college attendance on respondent college attendance.

the unconditional female advantage in college attendance, which rose meteorically over the second half of the twentieth century (Buchmann and DiPrete 2006), is less than a third of the magnitude of the estimated sibling college attendance effect, on the basis of the present data source (8 percentage point advantage in college attendance rates for women compared with men).

Findings among the continuing-generation sample also support a causal link between siblings' college attendance. Sibling college attendance raises the probability of continuing-generation individuals' college attendance by an estimated 14 percentage points.⁷ The narrow 95 percent confidence interval (0.10–0.18) does not include zero, so sampling error is not a likely explanation for the positive estimate.

The estimated effect of sibling college attendance among the first-generation sample is nearly twice the magnitude of the estimated effect among the continuing-generation sample, suggesting that sibling college attendance may well have a greater impact on first-generation people than on continuing-generation people.⁸ Moreover, because the 95 percent confidence intervals of the two estimates do not overlap, it is unlikely that the difference in magnitude is due to sampling error (Figure 3).⁹ Previous research not using national probability samples has shown a positive main effect of sibling

⁷I obtain alternative estimates by omitting all covariates that I consider potentially posttreatment: math score, GPA, highest math course, number of AP or IB courses, class tardies, school absences, frequency of failing to finish homework, time on homework, effort in math, and effort in science. For the first-generation sample, the estimate is 0.35 (greater by a factor of 1.25), and for the continuing-generation sample, the estimate is 0.19 (greater by a factor of 1.36). Qualitative conclusions remain the same, but unsurprisingly, the estimates are quantitatively greater than when controlling for the potentially posttreatment measures.

⁸It is possible that the apparent effect heterogeneity reflects differential sensitivity to unobserved confounders. Although I cannot directly test this proposition, I use each parental education group's sensitivity to observed covariates as a proxy for their sensitivity to unobserved confounders. I first run a model predicting college attendance, separately for each parental education group. I then divide the coefficient for each predictor from the first-generation sample model by the corresponding continuing-generation coefficient. Next, I compute the average of the absolute values of the ratios, weighted by the *t* statistics to give more weight to covariates with stronger impacts. I find that observed covariates are, on average, 1.4 times stronger in determining the first-generation sample's college attendance compared with that of the continuing-generation sample. If the twice-as-great estimate for the first-generation sample were due only to differential sensitivity, one would expect observed covariates to be twice as strong for the first-generation sample. Thus, these results are not consistent with the proposition that differential sensitivity is the sole reason the estimated effect of sibling college attendance is greater among first-generation youths.

⁹Because the unconditional college attendance rate for the continuing-generation sample (0.89) is so much closer to 1 than is the

Table 1. Means (Standard Deviations) of Each Measure Used in the Study, Separated by Treatment Status.

| | All | Treated ^a | Nontreated ^b |
|---|--------------|----------------------|-------------------------|
| Attended college | .77 | .91 | .54 |
| Sibling attended college | .65 | .1 | .0 |
| First generation | .37 | .25 | .54 |
| Female | .5 | .52 | .52 |
| Non-Hispanic white | .57 | .62 | .52 |
| Non-Hispanic black | .11 | .09 | .13 |
| Hispanic | .16 | .13 | .2 |
| Non-Hispanic Asian | .06 | .08 | .03 |
| Other/multiple race, non-Hispanic | .1 | .09 | .11 |
| Father's years of schooling | 13.92 (2.79) | 14.65 (2.85) | 12.88 (2.36) |
| Mother's years of schooling | 13.74 (2.48) | 14.42 (2.48) | 12.89 (2.16) |
| Father unemployed | .15 | .1 | .2 |
| Mother unemployed | .25 | .22 | .27 |
| Family Income (\$1,000s) | 78.4 (62.3) | 94.6 (65.5) | 55.9 (46.9) |
| Number of siblings | 2.62 (1.79) | 2.4 (1.59) | 2.8 (1.95) |
| Number of household members | 4.26 (1.5) | 4.27 (1.45) | 4.22 (1.56) |
| Parent married | .75 | .82 | .65 |
| Parent divorced | .14 | .11 | .18 |
| Parent separated | .04 | .02 | .04 |
| Parent never married | .06 | .03 | .10 |
| Parent widowed | .02 | .02 | .03 |
| Parent's age | 44.99 (6.60) | 46.02 (5.76) | 43.82 (7.37) |
| Family time | 3.06 (1.95) | 2.96 (1.89) | 3.16 (2.01) |
| Parent's educational expectations for child | 16.78 (2.47) | 17.30 (2.11) | 16.21 (2.73) |
| Religious group participant | .55 | .6 | .49 |
| Math test score | .09 (.96) | .38 (.9) | -.18 (.93) |
| High school GPA | 2.78 (.84) | 3.07 (.71) | 2.52 (.84) |
| No or low math | .08 | .03 | .11 |
| Mid-academic 1 math | .28 | .2 | .37 |
| Mid-academic 2 math | .25 | .31 | .19 |
| Advanced academic math | .34 | .42 | .28 |
| Number of AP/IB courses | 1.25 (2.3) | 1.79 (2.63) | .74 (1.8) |
| Never late to class | .45 | .48 | .43 |
| Rarely late to class | .38 | .39 | .38 |
| Sometimes late to class | .11 | .08 | .12 |
| Often late to class | .02 | .01 | .03 |
| Never fails to finish homework | .19 | .23 | .17 |
| Rarely fails to finish homework | .42 | .46 | .4 |
| Sometimes fails to finish homework | .25 | .2 | .28 |
| Often fails to finish homework | .1 | .09 | .12 |
| Number of absences | 3.49 (3.31) | 3.1 (3.07) | 3.95 (3.57) |
| Time on homework | 1.06 (.84) | 1.07 (.83) | 1.05 (.87) |
| Effort in math | .07 (.96) | .16 (.88) | -.02 (1.04) |
| Effort in science | .07 (.94) | .13 (.88) | .01 (1) |
| Northeast | .16 | .17 | .13 |
| Midwest | .28 | .29 | .26 |
| South | .39 | .38 | .41 |
| West | .18 | .16 | .2 |
| City | .28 | .31 | .25 |
| Suburb | .36 | .37 | .35 |
| Town | .12 | .11 | .12 |
| Rural | .24 | .22 | .28 |

^aSibling attended college.^bNo sibling attended college.

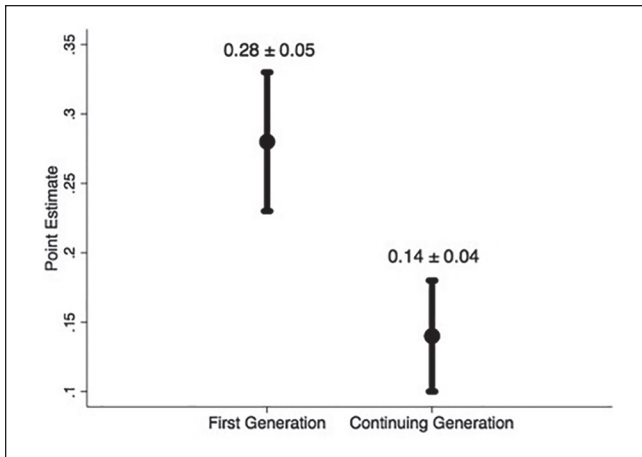


Figure 3. Estimates of the effect of sibling college attendance on the probability of respondent college attendance, by parental education group.

Note. Dots and bars represent the point estimates and 95 percent confidence intervals in the numerical expressions. Estimates come from inverse probability-weighted regression adjustment and are in probability units.

college attendance on individuals' own college attendance (Goodman et al. 2015; Loury 2004). My findings suggest that first-generation siblings see the strongest effects at the national level. Thus, efforts to increase the college attendance rate may be most efficient if they target first-generation individuals because they are likely to induce the greatest spillover effects onto their siblings.

The results are consistent with the theory depicted in Figure 1, even though the results cannot definitively prove it. The theory predicts a weaker effect among continuing-generation youths because of the partially redundant nature of college-educated siblings' social capital when the respondent already receives the same benefits from college-educated parents. One can easily see why there might be diminishing returns to significant people explaining the process of applying to college and applying for financial aid: conceivably, it is

rate for the first-generation sample (0.65), effect heterogeneity as measured by the IPWRA estimator captures both differences in the effect of sibling college attendance on respondents' latent propensities to attend college as well as differences in how proximate each sample is to the ceiling of college attendance probability. In analyses available upon request, I estimate a logistic regression model of college attendance with each control variable listed above and with an interaction term between sibling college attendance and first-generation status. The results reaffirm a positive treatment effect in both groups and a greater effect among the first-generation sample, although the interaction term is not statistically significant. Thus, even though, in an absolute sense (most relevant to the present study), sibling college attendance has a much greater effect on the probability of respondent college attendance in the first-generation sample than in the continuing-generation sample, the cross-group difference in how sibling college attendance affects the latent propensity to attend college is a bit less marked.

enough to have one trusted point person, and for continuing-generation youths this point person is likely to be the parent, even in the presence of college-educated siblings. College-educated siblings may provide some extra, more up-to-date information, but much of the information they offer is likely to overlap with information that college-educated parents already provide.

Robustness to Unobserved Confounders

The estimated effect of sibling college attendance may reflect the influence of unobserved confounders, such as grandparental wealth, and/or may reflect a genuine causal relationship. Even if sibling college attendance has a positive causal effect on youths' own college attendance, estimates that are not (quasi)-experiment almost certainly overstate the impact because of unobserved factors that lead both siblings to attend college. Whether bias due to unobserved confounding is present is not a very helpful question, because these estimates cannot realistically adjust for every confounding factor. Frank et al. (2013) proposed the more useful question of how much bias must exist to invalidate a researcher's claim that one variable causes another. I use the robustness check developed by Frank et al. to assess the extent of bias that would need to be present to invalidate my causal claims.¹⁰

For the first-generation sample, I find that in order to drive the estimate down to statistical insignificance at the 0.05 level, 83 percent of cases would need to be replaced with cases for which there is an effect of zero. This is quite a high percentage given that unobserved confounders can only bias my estimates via variation that is orthogonal to observed covariates (e.g., the variation in grandparental wealth that is unrelated to parental education, family income, test scores). Nevertheless, the nature of observational estimates and the infeasibility of random assignment mean that I cannot rule out the possibility that siblings share unobserved influences that are this strong. For the continuing-generation sample, the percentage is slightly lower, but still high, at 75 percent.

To put the robustness of these estimates further in context, I make two comparisons. First, I compare the robustness of my estimates with the robustness of estimates in the 10 observational studies that Frank et al. (2013) examined, which made up all of the studies then available online first for publication in *Educational Evaluation and Policy Analysis*. The estimates in these studies required that between 2 percent and 60 percent of the corresponding estimates be

¹⁰Because the ultimate point estimates are linear (in probability units), I execute Frank et al.'s robustness check as one would for a linear regression coefficient. In particular, I use the `pconfound` command in Stata and input each relevant point estimate and standard error derived from IPWRA, along with the number of covariates (43, the number of covariates in both the outcome and treatment models).

due to bias. The present percentages of 83 percent and 75 percent substantially exceed the maximum from these studies. Second, I compare the bias necessary to invalidate my inferences to the bias accounted for by the strongest observed covariate (high school GPA¹¹). Supplementary analysis of the first-generation sample shows that controlling for GPA reduces the estimate by 3.4 percent. Because 83 percent is 24 times greater than 3.4, a statistically insignificant effect would require that there be 24 times more bias than is accounted for by high school GPA. Supplementary analysis of the continuing-generation sample shows that controlling for GPA reduces the estimate by 7.7 percent, and thus a statistically insignificant effect would require that there be 9.7 times more bias than is accounted for by high school GPA.

Discussion

I find that sibling college attendance increases the probability of an individual's own college attendance. My study is the first to garner such evidence using data representative of the full youth population in the United States. Furthermore, my study is the first to uncover a more pronounced effect of sibling college attendance among first-generation sibling sets compared with continuing-generation sibling sets. This effect heterogeneity points to the potential redundancy of college-educated siblings' benefits when youths already receive similar benefits from college-educated parents. The findings come from the HSLs and causal inference methods designed to be robust against confounding factors.

The findings complement stratification research by suggesting that educational (dis)advantage flows intragenerationally in addition to the intergenerational flow that receives comparatively more attention. Research on socioeconomic persistence largely centers parents (Mare 2011), with a recent uptick in studies about grandparents and great-grandparents (Pfeffer 2014). This study suggests that there are yet other family members who causally affect individuals' educational attainment and, thus, is a timely companion to the new extensions of intergenerational research. Simultaneously, this study advances the sociology of the family by addressing early (Irish 1964) and recent (McHale et al. 2012) calls to give intersibling effects more attention.

Efforts to curb social reproduction benefit from knowledge of intersibling effects. Harvill et al. (2012) conducted a meta-analysis of college access program evaluations and found that, according to experimental studies, college access programs cause a 4 percentage point increase in participants' probability of attending college. However, these experiments

compare the outcomes of only the treatment and control groups, without also comparing the outcomes of each group's siblings. If college access programs indirectly increase sibling college attendance by increasing participant college attendance, then one underestimates the total benefit of such programs when not considering spillover effects on siblings. These spillover effects are perhaps most relevant in cost-benefit analyses of programs, for which precise estimates of program benefits are crucial (Loury 2004). High school administrators, counselors, and nonprofit officers stand to benefit from this knowledge because it helps them evaluate the programs they are funding and implementing. More particularly, the results of this study suggest that the programs deserve more credit than these stakeholders tend to give. On a broader scale, given that intersibling effects appear more prevalent among first-generation than continuing-generation sibling sets, and given that first-generation individuals have more siblings to begin with, interventions that cause secular increases in college attendance in one birth cohort can narrow inequality in the long term by causing especially great increases in college attendance among that birth cohort's first-generation siblings. This process may be one of many reasons that overall educational expansion promotes social mobility (Breen 2010).

The results of this study suggest that intersibling effects are sources of adult siblings' socioeconomic correlations and, therefore, that intersibling effects can pollute estimates of intergenerational persistence when these estimates rely on sibling correlations. In her review of the intergenerational mobility literature, Torche (2015) explained that intergenerational persistence estimates from sibling-to-sibling socioeconomic correlations tend to be greater than estimates from parent-to-child correlations. Reflecting the traditional view, she argued that this is because the former also capture shared community influences and all unmeasured parent influences, but she did not attribute the differences to intersibling effects. My findings suggest that intersibling effects also swell sibling-to-sibling socioeconomic correlations. Thus, although sibling-to-sibling socioeconomic correlations can capture the combined impact of siblings' shared influences, including their influences on one another, researchers should be cautious interpreting these correlations as anything but upper bounds on how much socioeconomic advantage flows directly from one generation to the next.

At least a few directions for future research arise from this study. To explore mechanisms, future research might build on both the present, quantitative evidence of intersibling effects and the exclusively qualitative evidence suggesting that individuals give their siblings college-related information (McDonough 1997; Stanton-Salazar and Spina 2003). Survey measures of how much youths receive this information from their siblings would allow mediation analyses that could adjudicate between an information channels explanation and other explanations for intersibling effects in college attendance. In addition, future experimental studies of

¹¹High school GPA is the most important covariate not only in my study but in a wealth of studies related to college attendance. Research consistently finds that high school GPA is the strongest predictor of college attendance (Bui and Rush 2016; Patrick, Schulenberg, and O'Malley 2016), surpassing important factors such as standardized test scores and parental education.

college access programs can make two related, simultaneous advances in our understanding of intersibling effects. First, provided the programs are effective, these studies can obtain more plausibly exogenous estimates of intersibling college attendance effects by using program assignment as an instrument for sibling college attendance. Second, these studies can provide a thorough picture of how much college access programs affect college attendance by estimating spillover effects on program participants' siblings.

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