Higher Education and Health Investments: Does More Schooling Affect Preventive Health Care Use?

Jason M. Fletcher
Yale University

David E. Frisvold
Emory University

In this paper, we use regression analysis, sibling fixed effects, and matching estimators to examine the impact of education on preventive care. Using a large cohort of Wisconsin high school graduates that has been followed for nearly 50 years, we find that attending college is associated with an approximately 5–15 percent increase in the likelihood of using several types of preventive care. We also find that greater education may influence preventive care, partly through occupational channels and access to care. These findings suggest that increases in education have the potential to spill over onto long-term health choices.

Introduction

It is well known that individuals with higher levels of education are healthier than individuals with lower levels of education. For example, Cutler and Lleras-Muney (2008) report sizable correlations between education and mortality, heart disease, diabetes, lost days of work, smoking, alcohol consumption, and self-reported poor health. The effects on these outcomes of increasing education by 4 years are comparable in

The authors thank Anna Aizer, Susan Busch, Isaac Ehrlich, Michael Grossman, Robert Hauser, Ted Joyce, Patricia Keenan, Steven Lehrer, Bo MacInnis, Robert Michael, John Mullahy, Carol Roan, Jody Sindelar, Rob Warren, Barbara Wolfe, and participants of the 2007 and 2008 Wisconsin Longitudinal Study (WLS) pilot grant workshops and the 2008 National Bureau of Economic Research Summer Institute Health Economics Workshop for helpful comments. This research was supported by the 2007 WLS pilot grant program (Fletcher and Frisvold), the National Institute on Aging (R01 AG027045, Fletcher), the Robert Wood Johnson Foundation (Frisvold), and the Emory Global Health Institute (Frisvold). The authors thank John Zimmerman and Yu-Pu Chen for excellent research assistance. This research uses data from the WLS of the University of Wisconsin–Madison. Since 1991, the WLS has been supported principally by the National Institute on Aging (AG-9775 and AG-21079), with additional support from the Vilas Estate Trust, the National Science Foundation, the Spencer Foundation, and the Graduate School of the University of Wisconsin–Madison. A public use file of data from the WLS is available from the Wisconsin Longitudinal Study, University of Wisconsin–Madison, 1180 Observatory Drive, Madison, WI 53706, and at http://www.ssc.wisc.edu/wlsresearch/data/. The opinions expressed herein are those of the authors.

[*Journal of Human Capital,* 2009, vol. 3, no. 2]

magnitude to the gender gap or the black-white gap in these outcomes. Education is also correlated with the use of preventive care services; Cutler and Lleras-Muney (2007, 2008) document that individuals with higher levels of education obtain more flu shots, vaccines, mammograms, Pap smears, and colonoscopies.

There are three general explanations for the observed correlation between education and health: (1) reverse causality, that is, poor health leads to low levels of schooling; (2) education causally improves health; and (3) additional factors lead to a spurious correlation between education and health. It is important to establish whether the relationship between education and health choices is causal or merely represents a correlation, in order to assess the total social benefit of education and to determine the potential impact of education policies on health.

To date, there have been relatively few credible attempts to estimate the causal effects of education on health. Research that has used experimental or quasi-experimental designs has typically focused on a small set of health outcomes such as mortality and smoking (e.g., Lleras-Muney 2005; de Walque 2007; Grimard and Parent 2007; Clark and Royer 2009).[1] However, it might be the case that education affects health in additional domains that are important for policy priorities. For instance, because of the relatively low costs and high long-term benefits, increasing a broad set of prevention practices is one of the nine current priorities for the U.S. Department of Health and Human Services in increasing the health of the nation.[2] Additionally, the use of various preventive care services, as opposed to smoking or curative treatment that also has consumption value, is a behavior that is closer to a pure investment in health capital (Kenkel 1994).[3] Thus, determining the nature of the relationship between education and preventive care use is an important part of understanding health investment decisions.

We add to the literature that assesses the effect of education on health by examining whether individuals who attended college in the late 1950s and early 1960s were more likely to receive physical examinations, dental examinations, flu shots, and cholesterol tests in the early 2000s. In addition to extending the literature by examining a broad set of preventive health care choices, this paper also implements several estimators to assess the robustness of the findings and examines competing hypotheses for the links between education and health. Specifically, we use data in the WLS, which contains rich information that can be used to examine competing hypotheses such as reverse causality and omitted

[1] Related work by Chou et al. (forthcoming) uses a natural experiment in Taiwan to estimate the causal effects of parental education on children's health. Currie and Moretti (2003) examine the impact of mothers' education on smoking during pregnancy and low birth weight.

[2] See http://www.hhs.gov; accessed May 2007.

[3] While some measures of prevention, such as exercising, may have both consumption and investment qualities, the preventive care measures we use in this paper are arguably purely investments.

variables. In particular, this data set contains extensive information on usually unobserved variables, such as IQ measured at age 18 and basic proxy measures of time preference. To combat issues of reverse causality, we are also able to control for measures of childhood health status. Additionally, since the data set follows siblings across 50 years, we control for important unobserved family background factors by using sibling fixed-effects methods. Finally, as an additional robustness check, we use matching to estimate the impact of college attendance on the demand for preventive services.

Our results suggest that the effect of college attendance on health decisions in old age is large in magnitude and consistent across preventive care choices. We find increases in each of our four measures of preventive care choices (receiving a physical examination, dental examination, flu shot, and cholesterol test) of between 5 and 8 percentage points, or approximately 10 percent. Further, we investigate the potential mechanisms linking education with later preventive health care choices and find evidence that the effects operate primarily through occupational pathways; specifically, we find suggestive evidence that a fraction of the returns to college attendance flow through general measures of occupational prestige. There is also some evidence that individuals who attended college are more likely to receive preventive care because of greater access to health care. In contrast, we find limited evidence that marital status, health insurance, income, or wealth are primary mechanisms. These results suggest that increases in college attendance during the past 40 years could have had large positive impacts on health investments and public health.

Background

In Grossman's (1972) model of the demand for health, individuals invest in health capital because health is an important determinant of the amount of time available for market and nonmarket activities and health is valued as a consumption commodity. Cropper (1977) extends this model to incorporate uncertainty; individuals demand preventive care to decrease their probability of illness.[4] Preventive care may also decrease the severity of illness without influencing the probability of illness (Kenkel 1994). Many forms of health investments have consumption as well as investment aspects; however, many types of preventive care use are an input in the production of health capital that is almost entirely driven by investment motives (Kenkel 1994).

In this analysis, we focus on four specific measures of preventive care use: receiving a flu shot, cholesterol screening, physical examination,

[4] For other extensions to the Grossman model that incorporate uncertainty, see Grossman (2000). For additional literature that distinguishes preventive from curative care, see Kenkel (2000). See also the seminal work by Ehrlich and Becker (1972), who termed a broad class of preventive care and actions as self-protection.

and dental examination. Flu shots and cholesterol screening are among the top 10 priorities for effective clinical preventive services based on preventable burden and cost effectiveness (Maciosek et al. 2006).[5] Each year in the United States, approximately 36,000 people die from the flu, and more than 200,000 people are hospitalized because of complications from the flu.[6] The Centers for Disease Control and Prevention's Advisory Committee on Immunization Practices recommends that adults age 50 and older receive a flu shot (or influenza vaccination) annually to reduce the probability of contracting the flu.[7] The U.S. Preventive Services Task Force strongly recommends routine cholesterol screening for all adults age 45 and older to identify increased risk of coronary heart disease (Berg 2001). Routine physical examinations enable physicians to counsel patients about healthy lifestyles and determine whether additional preventive services are appropriate.[8] Routine dental examinations are important for the early detection of oral cancers and periodontal disease, and professional cleaning may reduce the probability of periodontal disease.[9]

According to the model of the demand for health (Grossman 1972), an increase in education is expected to increase investment in health. A consistent conclusion in the public health and economics literatures is that education is correlated with the use of preventive health care for adults (e.g., Kenkel 2000). Cutler and Lleras-Muney (2008) estimate that each year of schooling is associated with an increase of 1.7 percentage points in the likelihood of receiving a flu shot. Mullahy (1999) and Xakelis (2005) reach similar conclusions for adults age 65 and older. Centers for Disease Control and Prevention (1990) and Sambamoorthi and McAlpine (2003) find that individuals with more schooling are more likely to receive a cholesterol screening. Manski (1998) finds that higher levels of schooling are associated with visiting a dentist. However, these studies focus on establishing correlations between schooling and preventive care, rather than causality.[10]

There are a variety of explanations for the correlation between education and preventive care use, which represent both causal and non-

[5] Other preventive services included childhood immunization, daily aspirin use, and screens and counseling for tobacco use.

[6] See http://www.cdc.gov/flu/about/disease/index.htm; accessed March 26, 2008.

[7] See http://www.cdc.gov/flu/protect/keyfacts.htm; accessed March 25, 2008.

[8] We do not analyze whether individuals received a blood pressure screening because nearly all sample respondents in the WLS report receiving a blood pressure test in the past 12 months.

[9] See http://www.ahrq.gov and http://www.cdc.gov; accessed March 26, 2008.

[10] For example, Mullahy (1999) focuses on the impact of labor market behavior and perceived risks of infection on receiving a flu shot and does not attempt to determine whether the estimated correlation between schooling and receiving a flu shot represents a causal relationship.

causal mechanisms.[11] Education and preventive care use may be correlated because poor childhood health reduces educational attainment and leads individuals to reduce their adult health investments. For example, Case, Fertig, and Paxson (2005) show that children who are sick or malnourished are more likely to miss additional days of school, have lower school performance, and complete fewer years of schooling. As suggested by Ehrlich and Chuma (1990), an individual's initial stock of health, or childhood health, influences later health investments. Thus, individuals who were in poor health in childhood may also be less likely to use preventive health care.

Alternatively, there are potentially omitted factors that jointly affect educational outcomes and preventive care use. In a seminal paper by Fuchs (1982), variation in individuals' discount rates was hypothesized to account for part of the correlations found between education and health outcomes. Unmeasured ability differences across individuals could also determine both years of completed schooling and health outcomes. Commonly unobserved family or environmental factors, including family resources and neighborhood disadvantage, could also jointly affect health and education outcomes.[12] Importantly, each of these competing hypotheses can be tested in our data, which allows increased confidence that our results represent causal effects of education on preventive health care choices in old age and directs our attention to examining potential mechanisms for the relationships we uncover.

There are a variety of mechanisms through which education may affect the use of preventive health care. One primary pathway is through occupational choice. Individuals with higher levels of education are more likely to be able to choose occupations that confer higher social prestige and monetary rewards (Card 1999). Over time, these advantages in prestige and income may accumulate, and individuals in higher-paying occupations may be more likely to use preventive care because of higher costs associated with illness (Mullahy 1999). In support of this potential mechanism, income, wealth, and employment characteristics such as job control have been shown to be related to later health choices (Smith 1999). These occupations are also more likely to offer health insurance coverage and easier access to a regular source of medical care, which increases preventive care through reductions in costs to the consumer (Powell-Griner, Bolen, and Bland 1999; DeVoe et al. 2003). Alternatively, higher wages may lead individuals to underconsume some health inputs, such as preventive care, because of

[11] For a more general discussion about the mechanisms between education and health, broadly construed, see Grossman and Kaestner (1997) and Cutler and Lleras-Muney (2008).

[12] For example, Case et al. (2005) provide evidence that maternal education is associated with an individual's self-reported health status at age 42. Additionally, Wolfe and Behrman (1987) find no education effect on a health outcome after controlling for family factors.

their higher time costs of investing (Ehrlich and Yin 2006). Importantly, our data include measures of income, wealth, health insurance coverage, access to care, firm size, job tenure, and an overall measure of occupational characteristics (prestige) that allow us to begin to examine these potential channels.

Another potential mechanism is that education increases allocative efficiency and improves the choices of health inputs (Kenkel 1991, 1994). This could occur if more educated individuals are better informed or are more able to process available information about preventive medical care. For example, Scott (2002) finds that individuals with low levels of health literacy are less likely to receive a flu shot or use other forms of preventive care. We examine this potential mechanism by using data on cognition tests in later life, which allows us to examine whether educated individuals are more able to process available information but likely does not capture whether individuals are better informed.[13]

An additional mechanism is that education may influence health status broadly (Cutler and Lleras-Muney 2008). Individuals in worse health may be more likely to seek a flu shot because the consequences of influenza are more severe for less healthy individuals (Mullahy 1999). Finally, education could improve the efficiency of an individual's health production function (Grossman 1972). This change in efficiency would lower the costs of health capital (relative to consumption). If the price elasticity of demand for health capital were greater than one, the individual would demand more health capital and health inputs (such as preventive care; Kenkel 1994).[14]

Data

In this study, we use data from the WLS to examine the impact of college attendance on preventive health care use. The WLS is a longitudinal study of a one-third random sample of the graduating high school class of 1957 in Wisconsin.[15] Survey information from 10,317 of the graduates was collected in 1957, 1964, 1975, 1992–93, and 2003–4. The WLS sample also includes information from a randomly selected sibling that was collected in 1977, 1993–94, and 2005–7. The WLS includes extensive information about schooling, social background, labor market experi-

[13] We also examine whether marital status is a potential mechanism linking education with preventive care choices. While this mechanism could suggest one channel through which information could be provided, we find little evidence for our measures of marital status, including spouse's education level. Additional specifications that control for previous self-reported health status, measured in the early 1990s, do not influence our results for any of the preventive care measures (see table 6).

[14] We are not able to directly test this potential mechanism.

[15] Cameron and Heckman (1998) present evidence that a focus on high school graduates will likely bias downward the effects of early life conditions. The reader should view the results with this caveat in mind.

ences, and health.[16] The sample is roughly representative of white, non-Hispanic American men and women who have completed at least a high school education.[17] Along several measures, Wisconsin is an "average" or "typical" state. For example, the proportion of individuals age 60–70 who reported receiving a physical exam in the previous 12 months in the 2006 Behavioral Risk Factor Surveillance System (BRFSS) data was 80 percent, which was not statistically different for individuals living in Wisconsin versus other states. Likewise, we find no statistical difference in the likelihood of receiving a flu shot (53 percent) for residents of Wisconsin compared to residents of other states. We also find that the rate of college attendance in Wisconsin for these cohorts is only slightly lower than the national average using the BRFSS data (50 vs. 54 percent, $p < .05$).[18]

An important feature of the WLS for this study is the longitudinal nature of the data set that combines information about the preventive health care choices of elderly individuals with information about family economic characteristics before college attendance. Whether individuals have received a flu shot, cholesterol test, physical exam, and dental exam is available in the most recent survey wave for the original WLS respondents and their siblings.[19]

While the WLS contains information on 10,317 original respondents and their siblings, we must constrain our analysis sample in order to take advantage of the use of sibling fixed effects.[20] In 2004, 7,732 original respondents completed the survey for a response rate of 75 percent;

[16] Full information can be found online at http://www.ssc.wisc.edu/wlsresearch/.

[17] As noted in the *Wisconsin Longitudinal Study Handbook* (Wollmering 2007, 14), "among Americans aged 50 to 54 in 1990 and 1991, approximately 66 percent were non-Hispanic white persons who completed at least 12 years of schooling. Some strata of American society are not well represented. The WLS sample is mainly of German, English, Irish, Scandinavian, Polish, or Czech ancestry. It is estimated that about 75 percent of Wisconsin youth graduated from high school in the late 1950s—everyone in the primary WLS sample graduated from high school; about seven percent of their siblings did not graduate from high school. Minorities are not well-represented: there are only a handful of African American, Hispanic, or Asian persons in the sample. . . . About 19 percent of the WLS sample is of farm origin, and that is consistent with national estimates of persons of farm origin in cohorts born in the late 1930s. . . . In 1964, 1975, and again in 1992, about two-thirds of the sample lived in Wisconsin, and about one-third lived elsewhere in the U.S. or abroad."

[18] Source: authors' calculations using BRFSS data.

[19] The specific wordings for the preventive care questions are "In the last 12 months, have you had a complete health exam or physical?" "In the last 12 months, have you had a routine dental check-up?" "In the last 12 months, have you had a flu shot?" and "In the last 12 months, have you had a cholesterol test?" Similar questions are asked in the National Health Interview Survey (NHIS) and BRFSS, and the NHIS includes a physical examination as routine or preventive care. These variables may be subject to measurement error because of the 1-year recall period; however, this would increase the standard errors without affecting the consistency of the estimates if recall bias is random.

[20] Importantly, we include results in the appendix that show that our baseline results are similar for the full sample and the analysis sample of sibling pairs. We define the full sample as the 10,037 individuals with information about college attendance and at least one measure of preventive care use.

however, only 6,845 respondents, or 66 percent of the original respondents, completed the mail questionnaire that included the preventive care questions.[21] Of the 7,928 siblings of the original respondents, 4,004, or 51 percent, completed the mail questionnaire in 2005.[22] We drop individuals without any health outcome information or schooling information, leaving a sample of 10,037 observations that includes 6,419 original respondents, but this sample size varies with the health outcome.[23] We then limit our sample to respondents with a sibling in the sample. Our analysis sample consists of 2,789 original respondents and their 2,789 siblings, for a total of 5,578 observations. In order to maximize our sample size, we impute missing values of the family background variables and include missing-value indicator variables in our specifications.[24]

We present means and standard deviations of the full sample of 10,037 individuals with nonmissing preventive care and education data and our analysis sample of 5,578 individuals in table 1. This table provides evidence that our analysis sample is quite similar to the dropped sample

[21] Among the 3,472 respondents who did not complete the mail questionnaire in 2004, 1,288 were deceased, 785 were not able to be contacted, and 1,399 refused to complete the questionnaire (WLS 2009).

[22] Among the 3,924 respondents who did not complete the mail questionnaire in 2005, 1,226 were deceased, 1,365 were not able to be contacted, and 1,333 refused to complete the questionnaire (WLS 2009). For comparison, the response rate of survey respondents who lived in the original 1968 households in the 1988 wave of the Panel Study of Income Dynamics is 56.1 percent (PSID User Guide; available at http://psidonline.isr.umich.edu/Guide/ug/chap5.html).

[23] College attendance and years of schooling completed are gathered from the 1992–93 and 1993–94 survey years. After removing observations with missing data for preventive health care use, 601 observations are removed because of missing schooling data. Comparing the individual and family characteristics of the original WLS respondents to those individuals remaining in our sample after this restriction suggests that the WLS respondents in our sample are similar to those excluded from the analysis sample in many ways but that there are a few exceptions. In particular, the respondents in our sample are 6.9 percentage points more likely to be female, are 1.8 percentage points more likely to have lived with both parents in high school, and have an IQ 3.6 points higher, on average, than the original WLS respondents excluded from our sample. Regressions of whether the survey respondent is dropped from the sample on precollege individual and family characteristics suggest that gender and IQ are related to whether the original WLS respondent remains in our sample. In our estimates, IQ is not predictive of preventive care use. Further, although gender is predictive of preventive care use, interaction terms between gender and college attendance are never statistically significant in ordinary least squares (OLS) and fixed-effects specifications.

[24] We impute missing values using linear regression based on the full sample of 10,037 observations that includes individuals without siblings in the sample. We impute mother's education for 78 observations, father's education for 228 observations, family income for 452 observations, the number of siblings for 13 observations, age for 623 observations, birth order for 15 observations, living with both parents for 399 observations, poor childhood health for 32 observations, missed school for 1 month for 137 observations, and IQ for 581 observations. We also assign siblings with missing values the value of the nonmissing sibling for living with both parents and assign sibling pairs with missing values the imputed value of the graduate for mother's education, father's education, and family income; these missing values will difference out in the fixed-effects model. Our results are robust to using mean imputation and adding dummy variables to denote that the value is missing, as opposed to regression-based imputation.

TABLE 1
Summary Statistics of Full and Sibling Samples

	Full Sample		Sibling Sample	
	Mean	SD	Mean	SD
Flu shot in 2004	.581	.493	.569	.495
Physical exam in last 12 months in 2004	.763	.425	.768	.422
Dental exam in last 12 months in 2004	.786	.410	.795	.403
Cholesterol test in last 12 months in 2004	.781	.414	.784	.411
Years of schooling completed	13.705	2.384	13.822	2.442
Ever attended college	.498	.500	.525	.499
Mother's years of schooling in 1957	10.532	2.795	10.601	2.750
Father's years of schooling in 1957	9.802	3.413	9.881	3.450
Family income during high school ($10,000s)	3.979	2.140	4.023	2.172
Number of siblings	3.280	2.488	3.352	2.393
Female	.542	.498	.536	.499
Age in 2004	64.260	4.074	64.197	4.802
Birth order	2.544	1.830	2.541	1.732
Lived with both parents in high school	.916	.278	.931	.254
Graduate from class of 1957	.640	.480	.500	.500
Self-reported health is poor or fair in childhood	.037	.189	.038	.190
Missed school for ≥1 month because of health as child	.082	.272	.084	.276
IQ score during high school	102.602	14.532	103.432	14.457
Planned to attend college at age 16	.435	.496	.447	.497
Employer prestige for last or current job in 2004	.000	1.000	.037	1.023
Covered by any type of health insurance in 1992–94	.972	.166	.977	.150
Employer-provided health insurance in 1992–94	.887	.317	.888	.315
Privately purchased health insurance in 1992–94	.067	.251	.072	.259
Other health insurance, including Medicaid in 1992–94	.017	.130	.016	.127
Covered by any type of health insurance in 2004	.973	.162	.976	.153
Employer-provided health insurance in 2004	.467	.499	.486	.500
Medicare in 2004	.397	.489	.390	.488
Privately purchased health insurance in 2004	.061	.240	.060	.238
Other health insurance, including Medicaid in 2004	.016	.126	.014	.119
Difficulty obtaining health care in 2004	.089	.285	.090	.286
Access to health care satisfaction in 2004	3.650	.671	3.655	.669
Usual source of care in 2004	.957	.195	.959	.191
Total household income in 2004 ($10,000s)	6.520	7.683	6.752	7.791
Total household assets in 2004 ($100,000s)	5.920	10.732	6.161	10.828
Cognition score in 2004	6.658	2.223	6.733	2.241
Sample size	10,037		5,578	

Source.—Wisconsin Longitudinal Study.
Note.—Sample sizes are individuals with at least one nonmissing measure of preventive health care. Full sample size for flu shot is 10,001, for physical exam is 10,002, for dental exam is 10,006, and for cholesterol test is 9,994. Sibling sample size for flu shot is 5,578, for physical exam is 5,556, for dental exam is 5,564, and for cholesterol test is 5,556. All dollar values are converted to 2004 dollars, using the consumer price index for all urban consumers.

of individuals. Fifty-three percent of individuals in the analysis sample attended college. Fifty-seven percent of these individuals received a flu shot, and between 77 and 80 percent received a physical exam, dental exam, and cholesterol test in the last 12 months.

Methods

In this paper, we use a variety of empirical strategies to extend the scope of previous examinations of the causal impact of education on health choices, focusing on whether education increases individuals' use of preventive health care. First, we examine the determinants of the use of preventive care with regression analysis. We estimate the use of preventive care, P_i, as a function of an individual's educational level, E_i; individual and family characteristics, X_i; and an idiosyncratic shock, ε_i:

$$P_i = \alpha_0 + \alpha_1 E_i + \alpha_2 X_i + \varepsilon_i. \tag{1}$$

Individual and family characteristics included in X are mother's education, father's education, family income during high school, number of siblings, sex, age, birth order, whether the individual lived with both parents during high school, and a dummy variable indicating whether the individual is in the graduate sample (vs. the sibling sample).[25] Including a wide array of family background characteristics measured before college attendance is important because of the influence of family background on education and health outcomes (Wolfe and Behrman 1987; Case et al. 2005).

Next, to explore the possibility that the observed correlation that exists between education and the use of preventive care services is the result of reverse causality from health to education, we include measures of childhood health (whether at least 1 month of school was missed because of illness and an indicator variable for poor or fair self-reported health in childhood).[26] We thus augment the above equation to include past health, $H_{i,t-1}$:

$$P_{i,t} = \beta_0 + \beta_1 H_{i,t-1} + \beta_2 E_i + \beta_3 X_i + \varepsilon_i. \tag{2}$$

To explore the possibility that third factors lead to a spurious correlation between education and preventive care use, we augment the previous demand equation with additional characteristics that may jointly influence both education and preventive care use. We include the Henmon-Nelson measure of IQ as a proxy for innate ability.[27] We also include measures of whether the individuals planned to attend college when they were 16 years old and whether the students discussed

[25] Family income is collected from the public records of the Wisconsin Tax Department data (Wollmering 2007). Family income during 1957–60 is averaged by the WLS to reduce measurement error and create a variable closer to the permanent income of the family during the high school years. Family income is converted to 2004 dollars, using the consumer price index for all urban consumers. The other family and individual variables are derived from the survey responses in 1957.

[26] These measures of childhood health are derived from questions asked of respondents in the latest survey wave, which introduces the possibility of substantial recall error. However, significant events, such as illness for at least 1 month, are less likely to be subject to recall error.

[27] This variable is based on tests of mental ability conducted by the Wisconsin State Testing Service for all ninth- and eleventh-grade high school students (Hauser 2005). We use the recommended measure constructed by the WLS that consists of the eleventh-grade score and a transformation of the ninth-grade score if the eleventh-grade score is missing.

future plans with teachers, counselors, and parents as measures of the discount rate before college attendance.

Next, we use a family fixed-effects estimator that compares the preventive care use of siblings with differing levels of education. To implement this strategy, we compare the use of preventive services of WLS graduates in 2003–4 to their siblings' use of preventive services, using the 2005–6 data, by adding a family fixed effect, μ_f, to the previous equation:

$$P_{i,f,t} = \delta_0 + \delta_1 H_{i,f,t-1} + \delta_2 E_{i,f} + \delta_3 X_{i,f} + \mu_f + \varepsilon_{i,f}. \quad (3)$$

By controlling for a wide array of exogenous individual characteristics that influence educational attainment and might differ between siblings, in addition to any fixed family characteristics that determine educational attainment, we hope to identify the nature of the relationship between education and the use of preventive services.[28]

This estimate is derived by comparing sibling pairs that consist of one sibling who attended college and one sibling who did not. The identifying assumption is that the underlying reasons why one sibling attended college and the other did not are (conditionally) uncorrelated with later preventive care use. In table 2, we provide descriptive statistics for the siblings who are discordant, as well as comparisons with concordant sibling pairs. This table shows that sibling pairs in which neither sibling attended college are less advantaged along many characteristics than are discordant siblings and sibling pairs who both attended college. Within the discordant siblings, the sibling who attended college is more likely to be male and have a higher IQ but is also slightly more likely to be in poor health as a child. What is essential for our identification strategy is that we capture important differences between siblings in our measurable characteristics. Our results below suggest that there would need to be a critical measure of individual heterogeneity between siblings that is correlated with both college attendance and later health

[28] A limitation of the fixed-effects strategy is the exaggeration of the influence of measurement error, which could bias the estimate of δ_2 toward zero. Additionally, while the fixed-effects strategy is implemented to reduce endogenous variation in college attendance, exogenous variation may also be reduced (Bound and Solon 1999). Further, moving from eq. (2) to eq. (3) limits the source of variation that identifies the coefficient for college attendance to the 1,870 siblings who are discordant in their college attendance choice—sibling pairs in which both attended college or neither attended college no longer help identify the coefficient. We do find evidence that limiting the sample to include only siblings who were discordant in their college attendance slightly increases the point estimates. Results from additional specifications that limit the sample to include only discordant sibling pairs and do not include fixed effects suggest that the increase from the fixed-effects specification shown in table 4 is driven by the change in the composition of the sample; however, these additional estimates are never statistically different from the OLS results in cols. 2 and 5 of tables 3 and 4 at the 5 percent level of significance and are only statistically significant at the 10 percent level for receiving a physical exam and a dental exam. Additionally, these results from the discordant pairs' sample show that the use of fixed effects, per se, only leads to a small change in our estimates. This finding further demonstrates the robustness of the OLS results.

TABLE 2

Descriptive Statistics Stratified by Sibling Pair Type

	Both No College (N = 1,716)		No College/ Discordant (N = 935)		Discordant (N = 1,870)		College/ Discordant (N = 935)		Both College (N = 1,992)	
	Mean (1)	SD (2)	Mean (3)	SD (4)	Mean (5)	SD (6)	Mean (7)	SD (8)	Mean (9)	SD (10)
Maternal education	9.50	2.38			10.43	2.64			11.71	2.73
Paternal education	8.43	2.62			9.41	2.98			11.58	3.76
Family income	3.05	1.72			3.60	1.96			4.85	2.67
Number siblings	4.00	2.70			3.36	2.33			2.79	2.00
Female	.59	.49	.60	.49	.53	.50	.47	.50	.49	.50
Age	64.79	4.85	64.53	4.70	64.06	4.93	63.59	5.10	63.81	4.59
Birth order	2.85	1.97	2.41	1.58	2.56	1.72	2.70	1.83	2.26	1.46
Both parents	.92	.27	.93	.25	.93	.26	.93	.26	.94	.24
Poor child health	.04	.19	.03	.18	.04	.20	.05	.21	.04	.19
Miss school	.08	.27	.08	.27	.08	.27	.09	.28	.09	.28
IQ	96.49	12.98	99.58	13.30	102.69	13.76	105.81	13.51	109.97	13.36
Plan college	.16	.37	.23	.42	.39	.49	.54	.50	.75	.43

Note.—Descriptive statistics are based on the following samples: cols. 1–2, neither sibling attended college; 3–4, sibling who did not attend college is in a discordant pair; 5–6, discordant siblings combined; 7–8, sibling who did attend college is in a discordant pair; and 9–10, both siblings attended college.

and that explains more of the variance in preventive care use than our measured characteristics, such as IQ, time preferences, and childhood health, to eliminate the estimated impact of college attendance on preventive care use.

In addition to unmeasured between-sibling heterogeneity, another concern about our strategy is that unobserved parental investments that differ among siblings influence both college attendance and preventive care use. To explore this possibility, we examine additional specifications that include the interaction of two proxy variables for parental investments—birth order and living with both parents during high school—and college attendance. We focus on birth order and whether these individuals lived with both parents because these variables are related to the amount of time that parents spend with their children, which is an important measure of parental investment (Price 2008). Our results are robust to these additional specifications for all measures of preventive care use, and no interaction term is ever significant, which minimizes the potential that unobserved parental investments are influencing the estimates of the impact of college attendance.

Finally, as an additional robustness check, we use matching methods to estimate the impact of college attendance on the use of preventive services, which relaxes the parametric assumption of linearity embedded in the previous specifications and relaxes the need for control variables to be exogenous as long as they are balanced between "treatment groups" (e.g., Black and Smith 2004). Specifically, we use the bias-corrected nearest-neighbor matching estimator described in Abadie and Imbens (2002) as well as several alternative propensity-score matching estimators, such as stratification, nearest-neighbor, and kernel matching.[29] These techniques compare the preventive care use of individuals with similar observable characteristics but whose college attendance choices differed. If individuals choose whether to attend college based on the extensive list of observable characteristics that we can match on in the WLS, then matching estimates the causal effect of college attendance on preventive care use.

Results

We present results for several preventive care choices that are suggested for near-elderly males and females, including physical examinations, dental examinations, flu shots, and cholesterol tests. All estimates are

[29] We use these matching estimators for the full sample of observations, including individuals without a sibling in the sample, in the common support and the trimmed sample of observations with propensity scores in the range of [0.1, 0.9], which is the optimal subsample for estimating the average treatment effect under a wide range of distributions (Crump et al. 2006). The common support is the intersection of the range of the distribution of the propensity score that contains positive values for individuals who attended college and for individuals who did not.

from linear probability models, and the standard errors are clustered to allow for arbitrary correlation within families and for heteroskedasticity.[30]

While we focus on the effects of attending college on these preventive care choices, results for years of schooling are qualitatively similar and available from the authors. Briefly, our sibling fixed-effects results for years of schooling are robust, and our OLS results for years of schooling are qualitatively similar but less precisely estimated. For example, our sibling fixed-effects results suggest that an additional year of schooling increases the probability of receiving a physical exam by 1.1 percentage points, a dental exam by 1.3 percentage points, a flu shot by 1.7 percentage points, and a cholesterol test by 0.8 percentage points.[31] One potential explanation for the lower estimates in examining years of schooling versus college attendance is that individuals who attended college, who differ in both education and accumulated wealth, have a greater and nonlinear incentive to pursue self-protective measures than do individuals with less than college attainments (see Ehrlich [2000] and Ehrlich and Yin [2006] for further discussion and evidence). However, the time costs associated with seeking preventive care may favor the less educated (Ehrlich and Yin 2006).

Results that examine the associations between college attendance and receiving a physical examination are shown in table 3. Column 1 presents results from estimating equation (1), which controls for predetermined individual and family characteristics. The results suggest that attending college is associated with a 3.1 percentage point increase in receiving a physical examination.[32] Females are found to be more likely to receive a physical exam (by nearly 5 percentage points), as are older individuals. No family background characteristics are statistically significant. In column 2, we add measures of usually unobserved heterogeneity that alternative hypotheses predict will lead to both higher educational attainment and better health care decisions. IQ measured at age 18 is not predictive of receiving a physical examination in old age. Individuals with poor self-rated childhood health are 7 percentage points less likely to receive a physical examination. However, the association between college attendance and physical examination receipt is not influenced by the addition of these control variables. Including variables that measure individuals' plans for the future also does not influence the relationship between college education and receiving a physical

[30] Similar estimates are obtained from a logit model and Chamberlain's conditional logit model.

[31] Our results for years of schooling are similar to the results reported by Mullahy (1999) and Cutler and Lleras-Muney (2007, 2008) for receiving a flu shot.

[32] These results are based on the sample of individuals with a sibling; the *p*-value from a Wald test for the null hypothesis that the college attendance estimate from the analysis sample is equivalent to the estimate from the full sample is .76.

TABLE 3

Estimates of the Impact of College Attendance on Receiving a Physical and a Dental Exam (Pairs Sample)

	Dependent Variable					
	Physical Exam			Dental Exam		
	No Fixed Effects (1)	No Fixed Effects (2)	Fixed Effects (3)	No Fixed Effects (4)	No Fixed Effects (5)	Fixed Effects (6)
Attended college	.031**	.033**	.061***	.107***	.099***	.063***
	(.012)	(.013)	(.020)	(.012)	(.012)	(.018)
Mother's education	−.001	−.001		.001	.001	
	(.003)	(.003)		(.002)	(.002)	
Father's education	.002	.002		.003	.002	
	(.002)	(.002)		(.002)	(.002)	
Family income	−.002	−.002		.005***	.005***	
	(.002)	(.002)		(.002)	(.002)	
Number of siblings	.000	.001		−.003	−.003	
	(.003)	(.003)		(.003)	(.003)	
Female	.047***	.047***	.040**	.044***	.043***	.048***
	(.012)	(.012)	(.016)	(.011)	(.011)	(.015)
Age	.008***	.008***	.004	−.003**	−.003*	−.005*
	(.001)	(.001)	(.003)	(.001)	(.001)	(.003)
Birth order	−.002	−.002	−.015	.000	.001	−.001
	(.005)	(.005)	(.009)	(.005)	(.005)	(.010)
Live with both parents	.005	.005	.054	.019	.017	.037
	(.023)	(.023)	(.044)	(.023)	(.023)	(.043)
Wisconsin Longitudinal Study graduate	.021*	.017	.017	.025**	.021*	.023*
	(.011)	(.012)	(.013)	(.011)	(.011)	(.012)
Poor childhood health		−.073**	−.115***		−.005	.017
		(.032)	(.041)		(.028)	(.040)
Missed school as child		.006	.017		.005	−.013
		(.021)	(.028)		(.020)	(.026)
IQ		−.000	−.000		.001**	.001*
		(.000)	(.001)		(.000)	(.001)
Constant	.208**	.250**	.470**	.809***	.721***	.881***
	(.102)	(.114)	(.233)	(.098)	(.109)	(.227)
Observations	5,556	5,556	5,556	5,564	5,564	5,564
R^2	.02	.02	.02	.04	.04	.02
p-value difference with full sample[a]	.761			.934		
p-value difference with IQ[b]		.603			.028	

Note.—Heteroskedasticity-robust standard errors that allow for clustering within families are in parentheses. Estimates are calculated using a linear probability model.

[a] Calculated for the null hypothesis that the coefficient estimate for college attendance reported in cols. 1 and 4 is equal to the coefficient estimate from a similar specification for the full sample.

[b] Calculated for the null hypothesis that the coefficient estimate for college attendance reported in col. 1 is equivalent to the estimate reported in col. 2 (and likewise for cols. 4 and 5).

* $p < .1$.

** $p < .05$.

*** $p < .01$.

examination.[33] Column 3 estimates a family fixed-effects specification and finds evidence that college attendance increases physical examination receipt (controlling for all common family factors) by 6.1 percentage points.

Results examining the link between college attendance and dental examination receipt are presented in columns 4–6 of table 3 and largely follow the results for physical examinations. Like previous results, females are nearly 5 percentage points more likely to receive a dental examination than males. Individuals with higher IQs at age 18 are also more likely to receive a dental examination in old age. The results show that individuals who attended college are between 6.3 and 10.7 percentage points more likely to receive a dental examination.

In table 4, we examine the association between receipt of a flu shot in old age and college attendance. Our results are structured much like the results for physical examinations. In columns 1–3, our results show that college attendance is robustly linked to receiving a flu shot in old age; the association for the sibling sample remains between 5.9 and 7.5 percentage points across the specifications. Controlling for several usually omitted variables, such as IQ, early child health status, and later health status, seems to slightly strengthen the association we find. Finally, columns 4–6 present our results examining the relationship between college attendance and receiving a cholesterol examination in old age. We find that individuals who attend college are approximately 2–5 percentage points more likely to receive a cholesterol examination.

The results in table 5 are based on various matching estimators. The top half of the table includes all individuals in the full sample, including individuals without siblings, in the common support. The bottom half is the subsample of individuals with propensity scores in the range of [0.1, 0.9]. In general, the results in table 5 are consistent across the different matching estimators and are similar to the results shown in the previous tables. Attending college is estimated to increase the likelihood of receiving a physical exam by approximately 5 percentage points, a dental exam by approximately 8 percentage points, a flu shot by approximately 5 percentage points, and a cholesterol test by approximately 3 percentage points.

[33] These results are shown in table A2. There are two variables that we include to proxy for an individual's discount rate before college attendance. The first variable measures whether the individual planned to attend college at age 16. The survey question was asked of graduates in 1975 and of siblings in 1977. The second variable is the sum of the individuals' responses about the extent to which they discussed future plans with teachers, counselors, and parents. This survey question was asked in 1957 of graduates only. Individuals who discussed future plans with teachers, counselors, and parents were more likely to receive a physical examination; however, including this measure of an individual's discount rate does not influence the estimated relationship between college attendance and receiving a physical exam. The results are similar for the three other measures of preventive care use, as shown in tables A3–A5.

TABLE 4
ESTIMATES OF THE IMPACT OF COLLEGE ATTENDANCE ON RECEIVING A FLU SHOT AND CHOLESTEROL TEST (Pairs Sample)

	Dependent Variable					
	Flu Shot			Cholesterol Test		
	No Fixed Effects (1)	No Fixed Effects (2)	Fixed Effects (3)	No Fixed Effects (4)	No Fixed Effects (5)	Fixed Effects (6)
Attended college	.059***	.062***	.075***	.024**	.026**	.049***
	(.014)	(.015)	(.022)	(.012)	(.013)	(.019)
Mother's education	−.002	−.002		−.003	−.003	
	(.003)	(.003)		(.002)	(.002)	
Father's education	.003	.003		−.001	−.001	
	(.002)	(.002)		(.002)	(.002)	
Family income	.000	.000		−.002	−.001	
	(.002)	(.002)		(.002)	(.002)	
Number of siblings	−.009**	−.009**		−.005	−.005	
	(.004)	(.004)		(.003)	(.003)	
Female	.040***	.040***	.033*	−.023**	−.023**	−.023
	(.013)	(.013)	(.018)	(.011)	(.011)	(.016)
Age	.025***	.025***	.020***	.009***	.008***	.003
	(.001)	(.001)	(.003)	(.001)	(.001)	(.003)
Birth order	.007	.007	−.013	.004	.003	−.011
	(.005)	(.005)	(.010)	(.005)	(.005)	(.009)
Live with both parents	−.022	−.022	−.063	.029	.027	.041
	(.027)	(.027)	(.050)	(.024)	(.024)	(.045)
Wisconsin Longitudinal Study graduate	.101***	.098***	.111***	.009	.003	−.003
	(.013)	(.013)	(.014)	(.011)	(.011)	(.012)
Poor childhood health		−.003	−.028		−.043	−.057
		(.035)	(.045)		(.032)	(.043)
Missed school as child		.008	.023		−.007	.001
		(.024)	(.032)		(.021)	(.029)
IQ		−.000	.000		−.000	−.001
		(.000)	(.001)		(.000)	(.001)
Constant	−1.137***	−1.085***	−.735***	.256**	.311***	.644***
	(.112)	(.124)	(.251)	(.105)	(.115)	(.231)
Observations	5,578	5,578	5,578	5,556	5,556	5,556
R^2	.07	.07	.08	.01	.01	.01
p-value difference with full sample[a]	.149			.983		
p-value difference with IQ[b]		.459			.579	

Note.—Heteroskedasticity-robust standard errors that allow for clustering within families are in parentheses. Estimates are calculated using a linear probability model.
[a] Calculated for the null hypothesis that the coefficient estimate for college attendance reported in cols. 1 and 4 is equal to the coefficient estimate from a similar specification for the full sample.
[b] Calculated for the null hypothesis that the coefficient estimate for college attendance reported in col. 1 is equivalent to the estimate reported in col. 2 (and likewise for cols. 4 and 5).
* $p < .1$.
** $p < .05$.
*** $p < .01$.

Inspecting the Mechanisms

In this section, we examine several potential mechanisms that could link educational attainment during young adulthood with preventive health care choices in old age. We focus on several alterative hypotheses for which we have information in the data, including (1) links between education, occupation, and preventive care; (2) long-term links between

TABLE 5
MATCHING ESTIMATES OF THE IMPACT OF COLLEGE ATTENDANCE ON PREVENTIVE CARE USE

	Dependent Variable			
	Physical Exam	Dental Exam	Flu Shot	Cholesterol Test
Full, common support sample:				
Nearest-neighbor matching[a]	.052***	.075***	.050***	.054***
	(.013)	(.012)	(.014)	(.013)
Stratification matching	.054***	.080***	.052**	.071***
	(.020)	(.011)	(.022)	(.018)
Kernel-based matching	.047***	.092***	.056***	.047***
	(.015)	(.011)	(.013)	(.013)
Nearest-neighbor matching	.051***	.083***	.073***	.069***
	(.020)	(.012)	(.025)	(.020)
Sample size	9,984	9,989	9,985	9,979
Trimmed sample:				
Nearest-neighbor matching[a]	.050***	.082***	.047***	.038***
	(.012)	(.011)	(.013)	(.011)
Stratification matching	.044***	.105***	.057***	.034***
	(.012)	(.012)	(.014)	(.011)
Kernel-based matching	.042***	.102***	.052***	.032***
	(.013)	(.010)	(.012)	(.012)
Nearest-neighbor matching	.042***	.109***	.059***	.031**
	(.016)	(.015)	(.020)	(.015)
Sample size	9,374	9,380	9,369	9,366

Note.—Heteroskedasticity-robust standard errors are in parentheses. Trimmed sample is the subset of individuals from the full sample in the common support with propensity scores in the range [0.1, 0.9].
[a] Estimates based on the bias-adjusted nearest-neighbor matching with replacement estimator developed by Abadie and Imbens (2002). These estimates are based on a minimum of three matches per observation; similar results are obtained using a minimum of two and four matches per observation.
** $p < .05$.
*** $p < .01$.

education, cognition, and preventive care; (3) links between education, adult health status, and preventive care; and (4) links between education, marital status, and preventive care. For our hypothesis that occupation may link education and preventive care, we are also able to broadly distinguish between several potential mechanisms tied with occupation, including prestige, income, assets, health insurance, and access to health care. While our data contain information related to several potential mechanisms linking education to preventive care, we are not able to capture fully all potential pathways.[34] A few pathways of note that should be the subject of future research include social relations/networks, proximity to family, and retirement/employment dynamics.

[34] The WLS contains information about cognitive ability but not knowledge specifically related to health. Kenkel (1991) finds that differences in health knowledge can explain some, but not most, of the relationship between education and smoking, drinking, and exercise. Additionally, Cutler and Lleras-Muney (2008) find that health knowledge explains about 10 percent of the relationship between years of schooling and health behaviors.

Our results must be viewed within the limitations of the measures available in our data.

Our basic strategy for inspecting potential mechanisms is to examine the change in our previous results in our preferred sibling fixed-effects empirical models after controlling for our outlined mechanisms.[35] We generally add our mechanism controls in chronological order.[36] Table 6 presents the estimate of the influence of college attendance on the use of each preventive care measure and documents the change in the estimate as additional variables are added.[37] The first row of table 6 replicates the fixed-effects results shown in tables 2 and 3. The second row displays the results from fixed-effects specifications that also include the Nakao-Treas occupational prestige rating from the individual's current or last occupation.[38] The third row adds a set of indicator variables denoting the type, if any, of health insurance in 1992–94 to the above specification. The fourth row adds three measures of access to health care: whether the individual has had any difficulty obtaining access to care, whether the individual has a usual source of health care, and a scale of access to health care satisfaction.[39] The fifth row adds marital status. The sixth row adds total household income. The seventh row adds total assets. The eighth row adds a cognition score.[40] The ninth row adds an individual's self-reported health status from the prior survey wave. Because the order in which these additional variables are entered in the equation could influence the estimates, we present results that highlight each potential mechanism individually. The tenth row includes variables from the baseline fixed-effects specification and health insurance type in 1992–94. Similarly, the eleventh through sixteenth rows include variables from the baseline specification and separately add the

[35] Our results are quite similar if we do not include sibling fixed effects and are available upon request.

[36] Cutler and Lleras-Muney (2007, 2008) use a similar strategy to examine the relationship between years of schooling and health behaviors.

[37] The full results for each specification are available upon request from the authors.

[38] The Nakao-Treas prestige rating is the percentage of respondents in the 1989 General Social Survey who ranked an occupation in the top half of a 9-point scale of the prestige of the occupation. Thus, this measure is a proxy for respondents' views of the social standing of occupations. We standardized this rating to have a mean of zero and a standard deviation of one to ease the interpretation of the coefficient. The reader should view the results with the caveat that this proxy is imperfect and potentially measured with error.

[39] The access to health care satisfaction is a scale running from 1 (poor) to 5 (excellent) that is the average response to the following questions in the WLS survey: "Thinking about your own health care, how would you rate the convenience of location of the doctor's office? The hours when the doctor's office is open? Your access to specialty care if you need it? Your access to hospital care if you needed it? Your access to medical care in an emergency? Arrangements for making appointments for medical care by phone? The length of time spent waiting at the office to see the doctor? The length of time you wait between making an appointment for routine care and the day of your visit? The availability of medical information or advice by phone? Your access to medical care whenever you need it? The services available for getting prescriptions filled?"

[40] The measure of cognition is derived from six items from the Weschler Adult Intelligence Scale.

TABLE 6
Examining the Mechanisms through which College Attendance Influences Preventive Care Use

	Dependent Variable			
	Physical Exam	Dental Exam	Flu Shot	Cholesterol Test
Baseline	.061***	.063***	.075***	.049***
	(.020)	(.018)	(.022)	(.019)
Plus occupational prestige	.047**	.052***	.060***	.044**
	(.021)	(.019)	(.022)	(.019)
Plus health insurance type	.046**	.050***	.061***	.042**
	(.021)	(.019)	(.022)	(.019)
Plus access to care	.042**	.050***	.058***	.040**
	(.020)	(.019)	(.022)	(.019)
Plus marital status	.043**	.050***	.059***	.042**
	(.020)	(.019)	(.022)	(.019)
Plus income	.043**	.048**	.059***	.041**
	(.020)	(.019)	(.022)	(.019)
Plus assets	.043**	.047**	.058***	.040**
	(.020)	(.019)	(.022)	(.019)
Plus cognition	.045**	.046**	.058**	.049**
	(.021)	(.019)	(.023)	(.019)
Plus self-reported health	.047**	.045**	.060***	.050**
	(.021)	(.019)	(.023)	(.019)
Baseline with health insurance type	.058***	.059***	.074***	.047***
	(.020)	(.018)	(.022)	(.019)
Baseline with access to care	.056***	.061***	.071***	.046**
	(.020)	(.018)	(.022)	(.018)
Baseline with income	.060***	.059***	.073***	.048**
	(.020)	(.018)	(.022)	(.019)
Baseline with marital status	.062***	.064***	.076***	.052***
	(.020)	(.018)	(.022)	(.019)
Baseline with assets	.060***	.060***	.074***	.048**
	(.020)	(.018)	(.022)	(.019)
Baseline with cognition	.062***	.061***	.073***	.058***
	(.020)	(.019)	(.022)	(.019)
Baseline with self-reported health	.063***	.062***	.078***	.051***
	(.020)	(.018)	(.022)	(.019)
Observations	5,556	5,564	5,578	5,556

Note.—Heteroskedasticity-robust standard errors that allow for clustering within families are in parentheses. Estimates are calculated using a linear probability model. Each cell is the estimated coefficient for college attendance from separate regressions that also control for sex, age, birth order, whether the individual lived with both parents during high school, a dummy variable indicating whether the individual is in the graduate sample (vs. the sibling sample), an indicator variable for poor childhood self-reported health status, an indicator variable for missing at least 1 month of school as a child because of health problems, IQ, and family fixed effects. Baseline results are the fixed-effects results shown in tables 2 and 3. Rows beginning with "plus" report estimates that add the denoted variable to the specification estimated in the row directly above. The categories of health insurance type are employer provided, privately purchased, other insurance, and no insurance. The access-to-care variables are the three measures: difficulty obtaining health care, access to health care satisfaction, and usual source of care.

** $p < .05$.

*** $p < .01$.

access-to-care variables, total household income, marital status, total assets, cognition, and self-reported health status from the prior survey wave.

Broadly, we interpret the results to suggest that the most important mechanism linking education with preventive care choices in old age is occupational prestige, and we find modest links with access to care and very small changes after controlling for health insurance, income, assets, or cognition at midlife. Our measure of occupational prestige generally reduces the total effect of education on the health choices by 10–20 percent. Controlling for access to care reduces the impact of college attendance by 5–15 percent.[41]

There are a variety of reasons why a broad measure of occupational prestige might be a mechanism through which college attendance influences preventive care use. More prestigious occupations may provide employees with health insurance benefits or more generous health insurance plans. We do not find that controlling for health insurance influences the impact of education, but we are not able to measure the benefits of the employer-provided health insurance options. We use firm size as a crude proxy for the generosity of health insurance options and find that firm size has little impact on the relationship between education and preventive care choices. More prestigious occupations may provide employees with higher income and assets; however, we find little influence of income and assets on the impact of college attendance. Additionally, more prestigious occupations may have lower turnover, and high employee turnover may lead to underinvestment in health by companies that are unlikely to receive as large of a return from their investment if the employee leaves the firm (Fang and Gavazza 2007). To explore this possibility, we control for job tenure but find little impact on the relationship between education and preventive care use; however, we do not have information about industry or occupation-specific measures of turnover. Other reasons why occupational prestige might be an influential mechanism are that more prestigious occupations may have more job control, which would allow individuals greater flexibility in their work schedule to be able to seek preventive care, and that more prestigious occupations may have nonmonetary rewards, which leads to greater invest to reduce the nonmonetary costs of illness. Unfortunately, data limitations prohibit definitive conclusions about exactly why occupational prestige is a mechanism through which college attendance influences preventive care use.

Although we find that health insurance is predictive of preventive health care choices, health insurance does not influence the relationship between college attendance and preventive care use. Measuring

[41] Ayyagari and Sloan (2009) link education with measures of self-rated health for diabetics in the Health and Retirement Study data and attempt to examine potential pathways. Using a different set of potential mechanisms (self-control, social support, etc.), they also are unable to fully explain the pathways.

health insurance with an insurance summary variable denoting whether the individual had any type of insurance in 1992–94 or in 2003–7 does not influence the college attendance results. However, an important caveat to these results is that health insurance coverage is nearly universal for this sample. Thus, we also examine categories of health insurance: employer provided, privately purchased, other insurance, and no insurance. Controlling for the types of health insurance, as shown in table 6, has little impact on the influence of college attendance.

We use health insurance information from 1992–94 as a measure of health insurance status before Medicare because the original respondents of the WLS are between the ages of 63 and 67 when preventive care use is measured. As noted by Card, Dobkin, and Maestas (2004), Medicare eligibility could change individuals' incentives to use preventive health care, when individuals without health insurance before age 65 or individuals with less generous insurance than Medicare postpone preventive health care use until they become Medicare eligible. Because health insurance is correlated with education, the incentives associated with Medicare eligibility may differ for individuals who attended college and individuals who did not. Although Card et al. (2004, 2008) conclude that individuals do not delay medical procedures or preventive care in anticipation of Medicare eligibility, we further investigate the potential influence of Medicare eligibility on the influence of college attendance on preventive care use in table A6.[42] In particular, we find that controlling for health insurance information in 2004, Medicare eligibility, or employment/retirement status has little influence on the impact of college attendance.[43]

Conclusion

In this paper, we use a rich data set of individuals and their siblings who have been followed over 50 years to assess the potential effects of college attendance on preventive care choices in old age. We focus on college attendance because there have been multiple policies over the last several decades that have attempted to improve rates of college

[42] For example, Card et al. (2004) note that a potential threat to the validity of their regression discontinuity design, which is based on the age 65 eligibility threshold, is that individuals may "delay certain medical procedures until after age 65 in anticipation of Medicare coverage," but they find "no evidence of anticipatory behavior" (6).

[43] Employment/retirement status may influence the impact of college attendance on preventive care use because employment is correlated with health insurance status. Additionally, retired individuals have more time available to seek preventive care but also have lower costs of forgone wages associated with becoming ill from not receiving preventive care (Mullahy 1999). Further, if less educated individuals with less desirable occupational characteristics retire at an earlier age, then these individuals may delay preventive care until becoming eligible for Medicare. However, we do not find that employment/retirement status at the time of preventive care use has an influence on the estimated impact of college attendance. We also do not find that high school graduates are less likely than college attendees to be employed in 2004.

attendance, and it continues to be an important policy area. Previous policies have been justified by appealing to both the need to enhance the stock of the nation's human capital as well as the many potential nonmarket benefits of increasing schooling (Haveman and Wolfe 1984), including health benefits. Specifically, Cutler and Lleras-Muney (2008) suggest that there may be substantial health returns to education policies that promote college attendance because increasing levels of education may lead to different thinking and decision-making patterns in health-related choices. They also suggest that the monetary value of the rate of return to education in terms of health may be as high as half the return to education on earnings. In this paper, we examine the conjecture that there are important spillover effects of increasing education in the context of increasing one domain of health—preventive health care choices.

Broadly, our results suggest that increases in college attendance in the late 1950s/early 1960s led to large increases in preventive health care use, such as the receipt of flu shots, physical examinations, dental examinations, and cholesterol tests. We provide evidence against standard alternative hypotheses such as reverse causality and several types of omitted variables, including differences in ability, differences in time preferences, or unobserved family-level factors. Other factors, such as determinants of preventive care by gender as well as an examination of the links between education and health care choices for gender-specific preventive care, are left for future research.

We also extend the literature by examining the potential mechanisms linking college attendance with preventive care choices at old age, particularly focusing on occupation-related factors and cognition. Our results suggest that measures of occupational prestige and access to care may link college attendance to preventive care choices and that health insurance, income, assets, marital status, several job characteristics, and cognition likely play a limited role. Some limitations of our data include the low proportions of nonwhite individuals and high school dropouts, attrition from the sample, measurement error in the data, and the focus on a cohort from one state. Our methods are also unable to control for all potential sources of individual heterogeneity. However, the results are quite robust across a number of specifications and the inclusion of a variety of controls and, overall, suggest important health benefits to college attendance for this cohort of individuals who have been followed for over a half a century.

Data Appendix

TABLE A1

Estimates of the Impact of College Attendance on the Use of Preventive Care (Full Sample, No Fixed Effects)

	Dependent Variable							
	Physical Exam		Dental Exam		Flu Shot		Cholesterol Test	
Attended college	.033***	.038***	.107***	.097***	.046***	.048***	.024***	.027***
	(.009)	(.010)	(.009)	(.009)	(.010)	(.011)	(.009)	(.010)
Mother's education	.001	.001	.000	−.000	−.001	−.001	−.003	−.003
	(.002)	(.002)	(.002)	(.002)	(.002)	(.002)	(.002)	(.002)
Father's education	−.000	.000	.003**	.003*	.004**	.004**	−.001	−.001
	(.002)	(.002)	(.001)	(.001)	(.002)	(.002)	(.001)	(.001)
Family income	−.001	−.001	.005***	.005***	.000	.000	.001	.001
	(.002)	(.002)	(.001)	(.001)	(.002)	(.002)	(.001)	(.001)
Number of siblings	.001	.001	−.006***	−.006**	−.007**	−.007**	−.003	−.003
	(.002)	(.002)	(.002)	(.002)	(.003)	(.003)	(.002)	(.002)
Female	.049***	.050***	.050***	.048***	.050***	.050***	−.021**	−.021**
	(.009)	(.009)	(.008)	(.008)	(.010)	(.010)	(.008)	(.008)
Age	.008***	.008***	−.003***	−.003***	.025***	.025***	.009***	.009***
	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)
Birth order	.001	.001	.004	.004	.007*	.007*	.005	.005
	(.003)	(.003)	(.003)	(.003)	(.004)	(.004)	(.003)	(.003)

TABLE A1
(*Continued*)

	Dependent Variable							
	Physical Exam		Dental Exam		Flu Shot		Cholesterol Test	
Live with both parents	−.006	−.006	.002	.001	−.039**	−.038**	.012	.011
	(.016)	(.016)	(.016)	(.016)	(.019)	(.019)	(.016)	(.016)
Wisconsin Longitudinal Study graduate	.016*	.011	.023**	.019**	.102***	.102***	.017*	.011
	(.009)	(.010)	(.009)	(.009)	(.010)	(.011)	(.009)	(.009)
Poor childhood health		−.048**		.010		−.004		−.041*
		(.024)		(.022)		(.026)		(.024)
Missed school as child		−.002		−.026*		.016		.000
		(.016)		(.016)		(.018)		(.016)
IQ		−.001*		.001***		−.000		−.000
		(.000)		(.000)		(.000)		(.000)
Constant	.170**	.247***	.851***	.746***	−1.122***	−1.096***	.227***	.286***
	(.084)	(.092)	(.080)	(.088)	(.091)	(.099)	(.085)	(.093)
Observations	10,002	10,002	10,006	10,006	10,001	10,001	9,994	9,994
R^2	.01	.01	.04	.04	.05	.05	.01	.01

Note.—Heteroskedasticity-robust standard errors that allow for clustering within families are in parentheses. Estimates are calculated using a linear probability model.
* $p < .1$.
** $p < .05$.
*** $p < .01$.

TABLE A2

Including Plans for the Future in Estimates of the Impact of College Attendance on Receiving a Physical Exam

	Sample					
	Graduates		Pairs			
	No Fixed Effects (1)	No Fixed Effects (2)	No Fixed Effects (3)	No Fixed Effects (4)	Fixed Effects (5)	Fixed Effects (6)
Attended college	.044***	.036**	.033**	.036**	.061**	.061**
	(.012)	(.014)	(.013)	(.014)	(.028)	(.029)
Mother's education	.002	.003	−.001	−.001		
	(.002)	(.002)	(.003)	(.003)		
Father's education	−.000	−.001	.002	.002		
	(.002)	(.002)	(.002)	(.002)		
Family income	−.002	−.002	−.002	−.002		
	(.002)	(.002)	(.002)	(.002)		
Number of siblings	−.003	−.002	.001	.000		
	(.003)	(.003)	(.003)	(.003)		
Female	.048***	.042***	.047***	.047***	.040*	.040*
	(.011)	(.011)	(.012)	(.012)	(.023)	(.023)
Age	.010	.010	.008***	.008***	.004	.004
	(.007)	(.007)	(.001)	(.001)	(.004)	(.004)
Birth order	.005	.005	−.002	−.001	−.015	−.015
	(.004)	(.004)	(.005)	(.005)	(.013)	(.013)
Live with both parents	−.001	−.001	.005	.005	.054	.054
	(.020)	(.020)	(.023)	(.023)	(.062)	(.062)
Wisconsin Longitudinal Study graduate	.000	.000	.017	.024	.017	.021
	(.000)	(.000)	(.012)	(.017)	(.018)	(.029)
Poor childhood health	−.046	−.044	−.073**	−.073**	−.115**	−.115*
	(.031)	(.031)	(.032)	(.032)	(.059)	(.059)
Missed school as child	.008	.008	.006	.006	.017	.017
	(.020)	(.020)	(.021)	(.021)	(.040)	(.040)
IQ	−.001**	−.001***	−.000	−.000	−.000	−.000
	(.000)	(.000)	(.000)	(.000)	(.001)	(.001)
Planned to attend college at age 16		.006		−.009		−.000
		(.014)		(.015)		(.028)
Discussed plans for the future		.019***				
		(.007)				
Constant	.190	.084	.250**	.237**	.470	.465
	(.466)	(.466)	(.114)	(.115)	(.330)	(.334)
Observations	6,402	6,402	5,556	5,556	5,556	5,556
R^2	.007	.010	.017	.017	.020	.020
p-value difference vs. pairs basic[a]				.549		
p-value difference vs. graduates basic[b]		.218	.380			

Note.—Heteroskedasticity-robust standard errors that allow for clustering within families are in parentheses. Estimates are calculated using a linear probability model.

[a] Calculated for the null hypothesis that the coefficient estimate for college attendance reported in col. 4 is equivalent to the estimate reported in col. 1.

[b] Calculated for the null hypothesis that the coefficient estimate for college attendance reported in cols. 2 and 3 is equivalent to the estimate reported in col. 1.

* $p < .1$.

** $p < .05$.

*** $p < .01$.

TABLE A3

Including Plans for the Future in Estimates of the Impact of College Attendance on Receiving a Dental Exam

	Sample					
	Graduates		Pairs			
	No Fixed Effects (1)	No Fixed Effects (2)	No Fixed Effects (3)	No Fixed Effects (4)	Fixed Effects (5)	Fixed Effects (6)
Attended college	.090***	.088***	.099***	.099***	.063**	.066**
	(.012)	(.013)	(.012)	(.013)	(.026)	(.027)
Mother's education	−.002	−.002	.001	.001		
	(.002)	(.002)	(.002)	(.002)		
Father's education	.003	.003	.002	.002		
	(.002)	(.002)	(.002)	(.002)		
Family income	.004**	.004**	.005***	.005***		
	(.002)	(.002)	(.002)	(.002)		
Number of siblings	−.008***	−.008***	−.003	−.003		
	(.003)	(.003)	(.003)	(.003)		
Female	.047***	.043***	.043***	.043***	.048**	.047**
	(.010)	(.011)	(.011)	(.011)	(.021)	(.021)
Age	.003	.003	−.003*	−.003*	−.005	−.005
	(.007)	(.007)	(.001)	(.001)	(.004)	(.004)
Birth order	.004	.005	.001	.001	−.001	−.000
	(.004)	(.004)	(.005)	(.005)	(.013)	(.013)
Live with both parents	.017	.018	.017	.017	.037	.037
	(.020)	(.020)	(.023)	(.023)	(.062)	(.062)
Wisconsin Longitudinal	.000	.000	.021*	.025	.023	.052*
Study graduate	(.000)	(.000)	(.011)	(.016)	(.018)	(.028)
Poor childhood health	.007	.010	−.005	−.005	.017	.020
	(.028)	(.028)	(.028)	(.028)	(.056)	(.056)
Missed school as child	−.042**	−.042**	.005	.005	−.013	−.014
	(.020)	(.020)	(.020)	(.020)	(.037)	(.037)
IQ	.001***	.001***	.001**	.001**	.001	.001
	(.000)	(.000)	(.000)	(.000)	(.001)	(.001)
Planned to attend college at age 16		−.002		.001		−.016
		(.013)		(.014)		(.027)
Discussed plans for the future		.011*				
		(.006)				
Constant	.385	.313	.721***	.717***	.881***	.844***
	(.474)	(.474)	(.109)	(.110)	(.322)	(.325)
Observations	6,403	6,403	5,564	5,564	5,564	5,564
R^2	.036	.038	.040	.040	.019	.021
p-value difference vs. pairs basic[a]				.912		
p-value difference vs. graduates basic[b]		.784	.443			

Note.—Heteroskedasticity-robust standard errors that allow for clustering within families are in parentheses. Estimates are calculated using a linear probability model.

[a] Calculated for the null hypothesis that the coefficient estimate for college attendance reported in col. 4 is equivalent to the estimate reported in col. 1.

[b] Calculated for the null hypothesis that the coefficient estimate for college attendance reported in cols. 2 and 3 is equivalent to the estimate reported in col. 1.

* $p < .1$.
** $p < .05$.
*** $p < .01$.

TABLE A4
Including Plans for the Future in Estimates of the Impact of College Attendance on Receiving a Flu Shot

	Sample					
	Graduates		Pairs			
	No Fixed Effects (1)	No Fixed Effects (2)	No Fixed Effects (3)	No Fixed Effects (4)	Fixed Effects (5)	Fixed Effects (6)
Attended college	.041***	.041***	.062***	.069***	.075**	.086***
	(.014)	(.016)	(.015)	(.016)	(.031)	(.032)
Mother's education	−.001	−.001	−.002	−.002		
	(.003)	(.003)	(.003)	(.003)		
Father's education	.006***	.006***	.003	.004		
	(.002)	(.002)	(.002)	(.002)		
Family income	.000	.000	.000	.001		
	(.002)	(.002)	(.002)	(.002)		
Number of siblings	−.009***	−.009***	−.009**	−.009**		
	(.003)	(.003)	(.004)	(.004)		
Female	.046***	.039***	.040***	.040***	.033	.031
	(.012)	(.013)	(.013)	(.013)	(.025)	(.026)
Age	.020**	.020**	.025***	.025***	.020***	.020***
	(.008)	(.008)	(.001)	(.001)	(.005)	(.005)
Birth order	.009**	.009**	.007	.008	−.013	−.012
	(.004)	(.004)	(.005)	(.005)	(.015)	(.015)
Live with both parents	−.036	−.036	−.022	−.021	−.063	−.062
	(.022)	(.022)	(.027)	(.027)	(.071)	(.071)
Wisconsin Longitudinal Study graduate	.000	.000	.098***	.130***	.111***	.148***
	(.000)	(.000)	(.013)	(.019)	(.020)	(.032)
Poor childhood health	.005	.005	−.003	−.002	−.028	−.024
	(.033)	(.033)	(.035)	(.035)	(.064)	(.064)
Missed school as child	.029	.030	.008	.007	.023	.020
	(.023)	(.023)	(.024)	(.024)	(.046)	(.046)
IQ	−.000	−.001	−.000	−.000	.000	.000
	(.000)	(.000)	(.000)	(.000)	(.001)	(.001)
Planned to attend college at age 16		−.011		−.022		−.043
		(.016)		(.016)		(.031)
Discussed plans for the future		.017**				
		(.008)				
Constant	−.675	−.741	−1.085***	−1.135***	−.735**	−.769**
	(.549)	(.550)	(.124)	(.125)	(.356)	(.358)
Observations	6,397	6,397	5,578	5,578	5,578	5,578
R^2	.014	.015	.071	.072	.079	.082
p-value difference vs. pairs basic[a]				.241		
p-value difference vs. graduates basic[b]		.974	.149			

Note.—Heteroskedasticity-robust standard errors that allow for clustering within families are in parentheses. Estimates are calculated using a linear probability model.

[a] Calculated for the null hypothesis that the coefficient estimate for college attendance reported in col. 4 is equivalent to the estimate reported in col. 1.

[b] Calculated for the null hypothesis that the coefficient estimate for college attendance reported in cols. 2 and 3 is equivalent to the estimate reported in col. 1.

** $p < .05$.

*** $p < .01$.

TABLE A5
INCLUDING PLANS FOR THE FUTURE IN ESTIMATES OF THE IMPACT OF COLLEGE ATTENDANCE ON RECEIVING A CHOLESTEROL TEST

	Sample					
	Graduates		Pairs			
	No Fixed Effects (1)	No Fixed Effects (2)	No Fixed Effects (3)	No Fixed Effects (4)	Fixed Effects (5)	Fixed Effects (6)
Attended college	.032***	.027**	.026**	.031**	.049*	.051*
	(.012)	(.014)	(.013)	(.014)	(.026)	(.027)
Mother's education	−.003	−.003	−.003	−.003		
	(.002)	(.002)	(.002)	(.002)		
Father's education	−.002	−.002	−.001	−.000		
	(.002)	(.002)	(.002)	(.002)		
Family income	.001	.001	−.001	−.001		
	(.002)	(.002)	(.002)	(.002)		
Number of siblings	−.004	−.003	−.005	−.005		
	(.003)	(.003)	(.003)	(.003)		
Female	−.017	−.024**	−.023**	−.023**	−.023	−.023
	(.010)	(.011)	(.011)	(.011)	(.022)	(.022)
Age	.015**	.015**	.008***	.008***	.003	.004
	(.007)	(.007)	(.001)	(.001)	(.004)	(.004)
Birth order	.007**	.008**	.003	.004	−.011	−.011
	(.004)	(.004)	(.005)	(.005)	(.013)	(.013)
Live with both parents	.020	.020	.027	.027	.041	.042
	(.020)	(.020)	(.024)	(.024)	(.064)	(.064)
Wisconsin Longitudinal Study graduate	.000	.000	.003	.013	−.003	.027
	(.000)	(.000)	(.011)	(.016)	(.017)	(.027)
Poor childhood health	−.050*	−.049	−.043	−.042	−.057	−.055
	(.030)	(.030)	(.032)	(.032)	(.062)	(.062)
Missed school as child	.009	.009	−.007	−.007	.001	−.000
	(.019)	(.020)	(.021)	(.021)	(.041)	(.041)
IQ	−.000	−.000	−.000	−.000	−.001	−.001
	(.000)	(.000)	(.000)	(.000)	(.001)	(.001)
Planned to attend college at age 16		−.005		−.014		−.010
		(.013)		(.014)		(.027)
Discussed plans for the future		.023***				
		(.006)				
Constant	−.142	−.237	.311***	.291**	.644*	.601*
	(.456)	(.457)	(.115)	(.116)	(.328)	(.332)
Observations	6,391	6,391	5,556	5,556	5,556	5,556
R^2	.005	.007	.014	.014	.013	.014
p-value difference vs. pairs basic[a]				.354		
p-value difference vs. graduates basic[b]		.511	.680			

Note.—Heteroskedasticity-robust standard errors that allow for clustering within families are in parentheses. Estimates are calculated using a linear probability model.

[a] Calculated for the null hypothesis that the coefficient estimate for college attendance reported in col. 4 is equivalent to the estimate reported in col. 1.

[b] Calculated for the null hypothesis that the coefficient estimate for college attendance reported in cols. 2 and 3 is equivalent to the estimate reported in col. 1.

* $p < .1$.

** $p < .05$.

*** $p < .01$.

TABLE A6

Alternative Specifications that Examine the Influence of Medicare Eligibility

	(1)	(2)	(3)	(4)	(5)
Physical exam:					
Attended college	.061***	.061***	.060**	.057***	.062***
	(.020)	(.020)	(.023)	(.020)	(.020)
Eligible for Medicare		.024	.022		
		(.021)	(.026)		
Attended college × eligible for Medicare			.004		
			(.031)		
Currently employed					−.026
					(.017)
Dental exam:					
Attended college	.063***	.063***	.039*	.062***	.063***
	(.018)	(.018)	(.021)	(.018)	(.018)
Eligible for Medicare		−.001	−.034		
		(.020)	(.027)		
Attended college × eligible for Medicare			.066**		
			(.030)		
Currently employed					.013
					(.016)
Flu shot:					
Attended college	.075***	.076***	.062**	.075***	.076***
	(.022)	(.022)	(.025)	(.022)	(.022)
Eligible for Medicare		.035	.015		
		(.023)	(.029)		
Attended college × eligible for Medicare			.039		
			(.036)		
Currently employed					−.026
					(.020)
Cholesterol test:					
Attended college	.049***	.049***	.053**	.047**	.050***
	(.019)	(.019)	(.023)	(.019)	(.019)
Eligible for Medicare		.000	.005		
		(.020)	(.025)		
Attended college × eligible for Medicare			−.011		
			(.030)		
Currently employed					−.016
					(.017)

Note.—Heteroskedasticity-robust standard errors that allow for clustering within families are in parentheses. Estimates are calculated using a linear probability model. Column 1 is the fixed-effects results shown in tables 3 and 4; col. 2 adds whether the individual is eligible for Medicare, which is measured as age 65 years and older; col. 3 adds an interaction term of college attendance and Medicare eligibility; col. 4 adds the type of health insurance at the time of preventive care use (Medicare, employer based, privately purchased, and other) to col. 1; and col. 5 adds employment status to col. 1.

* $p < .1$.

** $p < .05$.

*** $p < .01$.

References

Abadie, Alberto, and Guido Imbens. 2002. "Simple and Bias-Corrected Matching Estimators for Average Treatment Effects." Technical Working Paper no. 283, NBER, Cambridge, MA.

Ayyagari, Padmaja, and Frank Sloan. 2009. "Education and Health: A Peek Inside the Black Box." Manuscript, Dept. Econ., Duke Univ.

Berg, Alfred O. 2001. "Screening Adults for Lipid Disorders: Recommendations and Rationale." *American J. Preventive Medicine* 20 (suppl.; 3): 73–76.

Black, Dan A., and Jeffrey A. Smith. 2004. "How Robust Is the Evidence on the Effects of College Quality? Evidence from Matching." *J. Econometrics* 121 (1–2): 99–124.

Bound, John, and Gary Solon. 1999. "Double Trouble: On the Value of Twins-Based Estimation of the Return to Schooling." *Econ. Educ. Rev.* 18:169–82.

Cameron, Stephen V., and James J. Heckman. 1998. "Life Cycle Schooling and Dynamic Selection Bias: Models and Evidence for Five Cohorts." Working Paper no. 6385, NBER, Cambridge, MA.

Card, David. 1999. "The Causal Effect of Education on Earnings." In *Handbook of Labor Economics*, edited by Orley C. Ashenfelter and David Card, 1802–63. Amsterdam: Elsevier.

Card, David, Carlos Dobkin, and Nicole Maestas. 2004. "The Impact of Nearly Universal Insurance Coverage on Health Care Utilization and Health: Evidence from Medicare." Working Paper no. 10365, NBER, Cambridge, MA.

———. 2008. "The Impact of Nearly Universal Insurance Coverage on Health Care Utilization: Evidence from Medicare." *A.E.R.* 98 (5): 2242–58.

Case, Anne, Angela Fertig, and Christina Paxson. 2005. "The Lasting Impact of Childhood Health and Circumstance." *J. Health Econ.* 24:365–89.

Centers for Disease Control and Prevention. 1990. "Progress in Chronic Disease Prevention Factors Related to Cholesterol Screening and Cholesterol Level Awareness—United States, 1989." *Morbidity and Mortality Weekly Report* 39 (37): 633–37.

Chou, Shin-Yi, Jin-Tan Liu, Michael Grossman, and Theodore Joyce. Forthcoming. "Parental Education and Child Health: Evidence from a Natural Experiment in Taiwan." *American Econ. J.: Appl. Econ.*

Clark, Damon, and Heather Royer. 2009. "The Impact of Education on Mortality and Health: Evidence from UK Compulsory Schooling Laws." Manuscript, Dept. Econ., Univ. California, Santa Barbara.

Cropper, M. L. 1977. "Health, Investment in Health, and Occupational Choice." *J.P.E.* 85:1273–94.

Crump, Richard K., V. Joseph Hotz, Guido W. Imbens, and Oscar A. Mitnik. 2006. "Moving the Goalposts: Addressing Limited Overlap in the Estimation of Average Treatment Effects by Changing the Estimand." Technical Working Paper no. 330, NBER, Cambridge, MA.

Currie, Janet, and Enrico Moretti. 2003. "Mother's Education and the Intergenerational Transmission of Human Capital: Evidence from College Openings." *Q.J.E.* 118 (4): 1495–1532.

Cutler, David, and Adriana Lleras-Muney. 2007. "Understanding Differences in Health Behaviors by Education." Manuscript, Dept. Econ., Univ. California, Los Angeles.

———. 2008. "Education and Health: Evaluating Theories and Evidence." In *Making Americans Healthier: Social and Economic Policy as Health Policy*, edited by J. House, R. Schoeni, G. Kaplan, and H. Pollack. New York: Sage.

DeVoe, J. E., G. E. Fryer, R. Phillips, and L. Green. 2003. "Receipt of Preventive Care among Adults: Insurance Status and Usual Source of Care." *American J. Public Heath* 93:786–91.

de Walque, Damien. 2007. "Does Education Affect Smoking Behaviors? Evidence Using the Vietnam Draft as an Instrument for College Education." *J. Health Econ.* 26:877–95.

Ehrlich, Isaac. 2000. "Uncertain Lifetime, Life Protection, and the Value of Life Saving." *J. Health Econ.* 19:341–67.

Ehrlich, Isaac, and Gary S. Becker. 1972. "Market Insurance, Self-Insurance, and Self-Protection." *J.P.E.* 80 (4): 623–48.

Ehrlich, Isaac, and Hiroyuki Chuma. 1990. "A Model of the Demand for Longevity and the Value of Life Extension." *J.P.E.* 98 (4): 761–82.

Ehrlich, Isaac, and Yong Yin. 2006. "Explaining Diversities in Age-Specific Life Expectancies and Values of Life Saving: A Numerical Analysis." *J. Risk and Uncertainty* 31 (2): 129–62.

Fang, Hanming, and Alessandro Gavazza. 2007. "Dynamic Inefficiencies in Employment-Based Health Insurance System: Theory and Evidence." Working Paper no. 13371, NBER, Cambridge, MA.

Fuchs, Victor R. 1982. "Time Preference and Health: An Exploratory Study." In *Economic Aspects of Health*, edited by V. Fuchs. Chicago: Univ. Chicago Press.

Grimard, Franque, and Daniel Parent. 2007. "Education and Smoking: Were Vietnam War Draft Avoiders Also More Likely to Avoid Smoking?" *J. Health Econ.* 26:896–926.

Grossman, M. 1972. "On the Concept of Health Capital and the Demand for Health." *J.P.E.* 80 (2): 223–55.

———. 2000. "The Human Capital Model." In *Handbook of Health Economics*, edited by A. Culyer and J. Newhouse. Amsterdam: Elsevier.

Grossman, M., and R. Kaestner. 1997. "Effects of Education on Health." In *The Social Benefits of Education*, edited by Jere R. Behrman and Nevzer Stacey. Ann Arbor: Univ. Michigan Press.

Hauser, Robert M. 2005. "Survey Response in the Long Run: The Wisconsin Longitudinal Study." *Field Methods* 17 (1): 3–29.

Haveman, Robert, and Barbara Wolfe. 1984. "Schooling and Economic Well-Being: The Role of Nonmarket Effects." *J. Human Res.* 19 (3): 377–407.

Kenkel, D. S. 1991. "Health Behavior, Health Knowledge, and Schooling." *J.P.E.* 99 (2): 287–305.

———. 1994. "The Demand for Preventive Medical Care." *Appl. Econ.* 26:313–25.

———. 2000. "Prevention." In *Handbook of Health Economics*, edited by A. Culyer and J. Newhouse. Amsterdam: Elsevier.

Lleras-Muney, A. 2005. "The Relationship between Education and Adult Mortality in the United States." *Rev. Econ. Studies* 72:189–221.

Maciosek, Michael, Ashley Coffield, Nichol Edwards, Thomas Flottenmesch, Michael Goodman, and Leif Solberg. 2006. "Priorities among Effective Clinical Preventive Services: Results of a Systematic Review and Analysis." *American J. Preventive Medicine* 31 (1): 52–61.

Manski, Richard J. 1998. "Demographic and Socioeconomic Predictors of Dental Care Utilization." *J. American Dietetic Assoc.* 129:195.

Mullahy, J. 1999. "It'll Only Hurt a Second? Microeconomic Determinants of Who Gets Flu Shots." *Health Econ.* 8 (1): 9–24.

Powell-Griner, Eve, Julie Bolen, and Shayne Bland. 1999. "Health Care Coverage and Use of Preventive Services among the Near Elderly in the United States." *American J. Public Health* 89 (6): 882–86.

Price, Joseph. 2008. "Parent-Child Quality Time: Does Birth Order Matter?" *J. Human Res.* 43 (1): 240–65.

Sambamoorthi, Usha, and Donna D. McAlpine. 2003. "Racial, Ethnic, Socioeconomic, and Access Disparities in the Use of Preventive Services among Women." *Preventive Medicine* 37:475–84.

Scott, Tracy L. 2002. "Health Literacy and Preventive Health Care Use among Medicare Enrollees in a Managed Care Organization." *Medical Care* 40 (5): 395–404.

Smith, James. 1999. "Healthy Bodies and Thick Wallets: The Dual Relation between Health and Economic Status." *J. Econ. Perspectives* 13 (2): 145–66.

Wisconsin Longitudinal Study. 2009. “Retention and Response Rates of Wisconsin Longitudinal Study, 1957–2005.” University of Wisconsin–Madison. http://www.ssc.wisc.edu/wlsresearch/documentation/retention/cor1004_retention.pdf.

Wolfe, Barbara, and Jere Behrman. 1987. “Women’s Schooling and Children’s Health: Are the Effects Robust with Adult Sibling Control for the Women’s Childhood Background?” *J. Health Econ.* 6 (3): 239–54.

Wollmering Erica, ed. 2007. *Wisconsin Longitudinal Study Handbook*. Madison: Wisconsin Longitudinal Study. http://www.ssc.wisc.edu/wlsresearch/documentation/handbook/WLS_Handbook.pdf.

Xakelis, George. 2005. “Predictors of Influenza Immunization in Persons over Age 65.” *J. American Board of Family Practice* 18 (5): 426–33.