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Education, cognition, health knowledge, and health behavior

Naci Mocan · Duha T. Altindag

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Abstract Using data from NLSY97, we analyze the impact of education on health behavior. Controlling for health knowledge does not influence the impact of education on health behavior, supporting the productive efficiency hypothesis. Accounting for cognitive ability does not significantly alter the relationship between education and health behavior. Similarly, the impact of education on health behavior is the same between those with and without a learning disability, suggesting that cognition is not likely to be a significant factor in explaining the impact of education on health behavior.

Keywords Health inputs · Cognition · Learning · Productive efficiency

JEL Classification I12 · I20

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N. Mocan
Department of Economics, Louisiana State University, 3039 Business Education Complex, Baton Rouge, LA 70803, USA
e-mail: mocan@lsu.edu

N. Mocan
National Bureau of Economic Research (NBER), Cambridge, MA, USA

N. Mocan
Institute for the Study of Labor (IZA), Bonn, Germany

D. T. Altindag (✉)
Department of Economics, Auburn University, 0334 Haley Center, Auburn, AL 36849, USA
e-mail: altindag@auburn.edu

Introduction

Schooling impacts health outcomes. More educated people are healthier than the less educated [13, 14]. This positive relationship between education and health is robust whether one analyzes aggregates (e.g., mortality or morbidity rates) or microunits (e.g., individuals' self-reported health status, or sick days).

If the effect of education on health is causal, then the impact of education on individual well-being is pronounced. For example, it is well established that education raises wages [3]. It is also documented that an improvement in health is associated with increased labor productivity and that an improvement in health outcomes of a given generation produces an improvement in health of their offspring (see Currie [8] and the literature she cites). This means that an increase in education not only has a direct positive impact on the earnings of the individual, but it also has an additional effect on productivity and earnings through an improvement in health. These increases in earnings improve the well-being of the individual in addition to the increase in utility generated by enhanced health. Improved education and health also have an impact on the level of education and health of the individual's children, transmitting the benefit of enhanced education to the second generation [9, 25].

In standard models of health production, schooling has a causal impact on health because schooling increases the efficiency of health production [15, 16]. An alternative hypothesis, which is also consistent with the observed positive relationship between schooling and health, is that of the allocative efficiency. According to this hypothesis, more educated individuals choose input allocations that produce more output (better health) than those who have less education (see [24] and the papers discussed in [14].

Under allocative efficiency, education expands individuals' knowledge base about health, and an increase in health knowledge alters health behaviors (i.e., consumption of health inputs with both positive and negative marginal products, such as medical care and cigarette smoking), which in turn influence health outcomes. Both the productive efficiency and the allocative efficiency hypotheses rely on the assumption that education has a causal impact on health. Empirically, the impact of education on health could emerge as an artifact of omitted variables that could influence both education and health. One example of such a variable is time preference [11].

This article has three aims. The first is to analyze whether the negative effects of schooling on smoking and heavy drinking are causal, using a novel feature of the National Longitudinal Survey of Youth 1997 (NLSY97). The design of the NLSY97 has generated exogenous increases in the amount of schooling for different individuals in the sample between the 2 survey years. As explained in detail in the data section, two identical individuals who were both surveyed in 1997 and 2002 waves could have received significantly differential amounts of schooling (up to 24 months) between these years because of the timing of the 1997 and the 2002 surveys. Thus, individuals are exposed to different amounts of schooling between the two surveys, which is not related in any way to their personal or family background characteristics. Using these plausibly exogenous changes in schooling between the survey years, the article shows that education reduces smoking and heavy drinking.

Second, the article investigates whether education has an impact on input allocation through its impact on health knowledge. Specifically, we employ, for the first time in this literature, a panel data set to analyze the validity of the allocative efficiency hypothesis. Our basic framework is similar to Kenkel [20], where the impact of schooling on health inputs is estimated. If the influence of schooling on health is working through allocative efficiency (i.e., if schooling improves allocative efficiency by increasing the health knowledge of the individual), schooling should have little or no direct effect on health inputs in a regression that controls for health knowledge. Kenkel [20] uses cross-sectional data from the 1985 National Health Interview Survey (NHIS) and focuses on health inputs (behavior) such as smoking, drinking and exercise. His data set also contains information about the knowledge of the subjects regarding the health consequences of smoking, drinking and exercise. He finds that inclusion or exclusion of measures of health knowledge does not alter the magnitude of the education coefficients in regressions that explain health behavior, indicating that allocative efficiency is not the main reason schooling is related to health behavior. The same approach was taken by Cutler and Lleras-Muney [7], who employed cross-sectional data from the NHIS to investigate the impact of knowledge about health risks on the estimated relationship between education and health behaviors. They too found that health knowledge has only a modest impact on how education impacts health behaviors.

Our study differs from Kenkel [20] and Cutler and Lleras-Muney [7] in two important ways. First, we employ panel data of individuals rather than a cross section. Specifically, each person in the NLSY97 was asked questions on health knowledge in both 1997 and 2002 waves. This allows for an investigation of the impact of health knowledge on health behavior by netting out time-invariant individual-specific unobservables that may impact both the intensity of the demand for health knowledge and the demand for health behavior. Second, as mentioned above, the design of the NLSY97 allows us to exploit exogenous variations in schooling received by different individuals.[1] We find that accounting for health knowledge has no impact on the relationship between education and health behaviors. This suggests that schooling does not cause health behavior through health knowledge and calls into question the allocative efficiency hypothesis.

The third goal of the article is to investigate the extent to which cognitive ability is responsible for the impact of education on health behavior. Cutler and Lleras-Muney [7] analyze how the impact of education on health behaviors is influenced by the inclusion of various sets of variables to regression models. Using cross-sectional data sets, they find that the impact of education on health behaviors is diminished (but not eliminated) if income, health insurance and family background are controlled for but that the extent of risk aversion or discounting for the future has no impact on the estimated coefficient of education. They also run regressions of health behavior on education with and without a measure of cognitive ability, and investigate how the estimated coefficient of education is altered. They find evidence suggesting that cognitive ability, measured by the Armed Services Vocational Aptitude Battery (ASVAB) score, accounts for about 20 % of the impact of education on the demand for health inputs.

The use of the NLSY97 allows us to employ the ASVAB score, as well as another alternative measure of conceptual thinking ability and cognition (Peabody

[1] Also, the NLSY97 allows us to employ the number of Months Attended to school by the individual as a measure of education. As explained in more detail in the Data section below, the number of Months Attended to school is measured with a high degree of precision, and it better captures the individual's exposure to schooling. The conventional measure of education (years of completed schooling) contains substantial measurement error, generated by the timing of the survey, in a sample of young adults who are still in school.

Individual Achievement Test-PIAT), to investigate the same question. Entertaining the premise that test scores such as ASVAB are impacted by family background [17, 18], we control for a large set of family background variables and find that cognition, as measured by ASVAB or PIAT, does not have a meaningful influence on the impact of education on health behavior.

We also use information on whether the individual suffers from a learning disability. For any given amount of schooling, individuals with learning disability are expected to learn less in school in comparison to their peers who have no such disability. If learning in school is a determinant of the influence of education on health inputs, then a particular increase in schooling would have a smaller impact on health behavior for those with learning disability. However, our results show that learning disability does not influence the impact of schooling on health behaviors. In examining the sensitivity of the results, we investigate and find no evidence for the hypothesis that more attentive parents are more likely to report that their child has a learning disability. Nevertheless, this last set of results should be taken with caution because even though we control for a host of family background characteristics, it is possible that awareness and diagnosis of learning disability might be correlated with some other family attributes. Also, a child's learning disability may prompt the parents to involve a special education teacher or invest in other resources to counteract the disability. If this is the case, students with learning disability would have no significant learning disadvantage in comparison to students with no disability.

Because not each variable is reported for each person, the samples change between specifications. To make sure the results are not artifacts of changing sample compositions, we estimated all models using all samples and report them in Appendix 4 in Online Resource 1. In "Empirical specification" section we describe the empirical implementation. "Data and measurement of variables" section presents the data. "Results" section incudes the discussion of the results, and "Summary and conclusion" section is the conclusion.

Empirical specification

Consider Eq. (1) below

$$H_i = \beta_0 + \beta_1 \text{Education}_i + X_i\beta_2 + \varepsilon_i \tag{1}$$

where H stands for the demand for various health inputs that are deleterious to health (such as the demand for cigarettes) for person (i). *Education* represents the level of schooling of the person, X is a vector of control variables, and ε is a standard error term.

Equation (2) is similar to Eq. (1), but it includes an additional variable, *Health Knowledge*, which measures the extent of the knowledge of person (i) regarding the health input H. For example, if H stands for consumption of cigarettes, *Health Knowledge* measures the extent of the person's knowledge about the health risks associated with smoking.

$$H_i = \delta_0 + \delta_1 \text{Education}_i + \delta_2 \text{Health Knowledge}_i + X_i\delta_3 + \omega_i. \tag{2}$$

Kenkel [20] and Cutler and Lleras-Muney [7] estimate versions of Eqs. (1) and (2) and investigate the difference between the estimated β_1 and δ_1, i.e., the extent to which health knowledge alters the impact of education on health inputs. Both articles employ cross-sectional data sets, and they find that health knowledge has a modest (Cutler and Lleras-Muney) or negligible (Kenkel) impact on health behavior; that is, β_1 is not appreciably different from δ_1.[2]

In this article we employ panel data, which allow us to measure the demand for health inputs, the amount of schooling and the extent of input-specific health knowledge of individuals in two time periods. Specifically, the respondents of the NLSY97 were asked questions about their health behaviors. Furthermore, information is obtained from survey participants regarding their health knowledge in the 1997 and 2002 waves of the survey along with information on schooling. Time variation in the data allows us to entertain a specification as depicted by Eq. (3) where the demand for health inputs for person (i) at time (t) depends on the same set of variables as in Eq. (2) and on an individual-specific, time-invariant heterogeneity component μ_i.

$$H_{it} = \delta_0 + \delta_1 \text{Education}_{it} + \delta_2 \text{Health Knowledge}_{it} + X_{it}\Psi_1 + \mu_i + \omega_{it} \tag{3}$$

Because the health knowledge questions were administered only in 1997 and 2002 waves, we will employ data from these years. A valuable feature of the data is that among individuals who took the survey in 1997 and again in 2002, there is substantial variation in the distance between the timing of the survey. For example, while some individuals were surveyed as little as 4.5 years apart, the difference between the two surveys was more than 6 years for some others. (The mean difference

[2] Kenkel also runs instrumental variable regressions where health knowledge questions are instrumented with whether the individual received advice from a physician on lifestyle-related topics and for smoking, years of schooling completed after 1964 (the year of surgeon general's report on smoking), as well as indicator variables for occupation and industry and whether the person is employed in a health field. He obtains results similar to OLS (with larger standard errors) and concludes that the OLS results are not biased because of endogeneity.

between the two surveys is 68 months.) This exogenous variation in the distance between the two interviews translates into variation in schooling received by individuals between the two surveys.

Time-differencing Eq. (3) allows us to eliminate individual-specific unobservables (μ_i) that may be correlated with health behaviors as well as education and health knowledge. In Eq. (4), Δ^p stands for p-month difference, where p represents the number of months between the surveys, which is different for different people.

$$\Delta^p H_{it} = \delta_1 \Delta^p \text{Education}_{it} + \delta_2 \Delta^p \text{Health Knowledge}_{it} + \Delta^p X_{it} \Psi_2 + \Delta^p \omega_{it} \quad (4)$$

The vector X contains time-varying attributes of the individual. Because a higher value of (Δ^p Education) embodies the effect of increased schooling as well as aging, we also control for the difference in age between the 2 survey years. As mentioned earlier, Cutler and Lleras-Muney [7] attribute some of the observed relationship between education and health to cognitive ability. They argue that schooling improves cognition and enhanced cognitive skills alter health behaviors and improve health outcomes. Along the same lines, Auld and Sidhu [2] find that controlling for test scores has an impact on the estimated impact of education on self-reported health using adjusted AFQT scores as a measure of ability and find that schooling has an effect on health only for those with low schooling, in particular those with low ability.

To test this hypothesis, we estimate regressions very similar to Cutler and Lleras-Muney [7]. Specifically, we run cross-sectional models depicted by Eq. 5 below.

$$H_i = \gamma_0 + \gamma_1 \text{Education}_i + \gamma_2 \text{Cognitive Ability}_i + \gamma_3 \text{Health Knowledge}_i + X_i \Psi_3 + \upsilon_i \quad (5)$$

where, following Cutler and Lleras-Muney [7], *Cognitive Ability* is measured by the ASVAB score. Equation (5) allows us to investigate the sensitivity of the impact of education on health behavior (γ_1) to the inclusion/exclusion of Cognitive Ability. Note that as was the case in Cutler and Lleras-Muney [7], the ASVAB score of each individual is constant over time. Thus, although each individual contributes two observations (one from 1997 wave, the other from 2002 wave), Eq. (5) is a pooled cross section. In addition to ASVAB, we also employ the Peabody Individual Achievement Test (PIAT) as an alternative correlate of cognition.

To test this conjecture in a different framework, we hypothesize that if cognition matters, the impact of education on health behaviors should be different between those who suffer from a learning disability and those who do not. That is, if education improves cognition, which in turn impacts health behavior, an additional amount of education should have *a smaller* impact on health behavior among those who have *a learning disability*. More specifically, the coefficient γ_2 should be negative in Eq. (6) below, mitigating the impact of education on health behavior, where *Learning Disability* is an indicator that takes the value of 1 if the person suffers from a learning disability.

$$\Delta^p H_{it} = \gamma_1 \Delta^p \text{Education}_{it} + \gamma_2 (\Delta^p \text{Education}_{it} \times \text{Learning Disability}_i) + \gamma_3 \Delta^p \text{Health Knowledge}_{it} + \Delta^p X_{it} \Psi_4 + \upsilon_{it} \quad (6)$$

We have information, obtained from parents, on whether the individuals in the sample have a learning disability such as dyslexia or attention deficit disorder. This is an indicator of the extent of cognitive difficulty of the individual, which is largely independent of family socioeconomic circumstances. The National Center for Learning Disabilities defines learning disability as “a neurological disorder that affects the brain’s ability to receive, process, store and respond to information.” These disorders can be categorized according to the types of cognitive function that is impaired. The most common learning disability is dyslexia, which can negatively affect reading, writing, spelling and speaking. Other types of learning disabilities are dyscalculia (disorders involving math), dysgraphia (disorders involving visual information processing skills) and executive functioning disorder (involves disorders of executive functions such as planning, organizing and remembering details). In addition to these learning disabilities, attention disorders such as AD/HD (attention-deficit/hyperactivity disorder) can also impede learning. Medical studies point to defects in information processing parts of the brain and environmental factors for causes of learning disabilities [5, 6, 12]. Certain genes are found to have an influence on learning disorders [23, 26], and individuals with learning disabilities are likely to have family members with similar disorders. In addition, learning disabilities can arise from traumas affecting brain cells of an individual. For example, serious illnesses during the development period of the brain or head injuries may give rise to learning disabilities [5, 12]. Problems during pregnancy and birth such as illness or injury, low birth weight and use of drugs and alcohol during pregnancy are also listed among causes. The National Joint Committee on Learning Disabilities [21] and Cruz and Brier [6] argue that learning disabilities are not caused by economic disadvantage or cultural differences.

As explained below, we investigate and find no evidence for the hypothesis that more attentive parents are more likely to report disability of their children. Note that the main effect of learning disability on health behavior cannot be identified in this specification because the indicator of learning disability is time-invariant.

The difference in education between the 2 survey years is measured by counting the number of months of school attendance between the survey years. The creation of this variable is described in the data section below. The difference in school attendance between two individuals between the 2 survey years could be in part due to attachment to school. For example, if more motivated people are more attached to school than the less motivated, then the number of months of school attendance might be greater for more motivated students in comparison to the less motivated, even if they are surveyed on the same date in both 1997 and 2002 interviews. To account for such potential confounding, we divide the sample into two groups based on whether they were in school with no interruption between 1997 and 2002. Individuals who went to school without interruption are those who were in school each year between 1997 and 2002. Such individuals constitute those who received "uninterrupted schooling." Those who stopped going to school, temporarily or permanently, between 1997 and 2002 constitute the second group. In this latter group are those who graduated from high school but did not pursue further education as well as those who dropped out of high school. Individuals who stopped going to school but later continued are also in this group of "interrupted schooling." We estimate our models separately for each group.

Data and measurement of variables

The data are obtained from the NLSY97, which contains a nationally representative sample of 8,984 youths who were aged 12–16 as of 31 December 1996. The respondents have been followed annually since the survey was initiated. The cohort born in 1983 was asked health knowledge questions in the 1997 and 2002 waves of the survey. Therefore, the bulk of our analysis uses data from these two waves.

The 1997 wave of the NLSY97 was administered between January 1997 and May 1998, and the 2002 survey was administered between November 2002 and July 2003. This means, for example, that a 9th grader in the 1997 wave could have been interviewed 54 months later in the 2002 wave of the survey, while another 9th grader could have been interviewed 78 months after the first survey. As the timing of the surveys is random and therefore not correlated with student or parent attributes, this design implies that the second student could have been exposed to 20 additional months of schooling in comparison to the first student [1]. (The difference in exposure to schooling in this example is 20 months rather than 24 because there is no schooling in summer months.)

Appendix 1 in Online Resource 1 shows the number of individuals who are interviewed in the 1997 and 2002 waves and the months of these interviews. This table does not pertain to all individuals surveyed; it is only for those who are in our sample. For example, as column 1 and row 2 of the table in Appendix 1 show, there are 59 people who were interviewed in February 1997 in the 1997 wave and in November 2002 during the 2002 wave. Using these dates, we calculated the time between the interviews for all individuals. Figure 1 displays the distribution of the distance in months between the 1997 and 2002 interviews. The average distance is 68 months, and the standard deviation is 3. The correlation between the interview distance and observable household attributes is essentially zero. For example, the correlation between household size in 1997 and time between interviews is 0.02, and the correlation between household income in 1997 and distance between interviews is −0.01.

Tables 1 and 2 provide the summary statistics of the variables employed in the analysis for the two sub-samples. Table 1 pertains to individuals who had uninterrupted schooling between 1997 and 2002, and Table 2 pertains to all others as explained at the end of "Empirical specification" section above. Health behavior variables are cigarette smoking and alcohol consumption. *Cigarettes per Day* stands for the average number of cigarettes smoked by the individual during the last 30 days. *Smoker* is an indicator for smoking participation (smoked at least one cigarette per day). *Cigarettes per Day among Smokers* gives the number of cigarettes smoked among smokers. *One Pack per Day* is a measure of heavy smoking. It takes the value of 1 if an individual has smoked at least 20 cigarettes per day in the 30 days prior to the interview, and 0 otherwise. *Heavy Drinker* is an indicator that takes the value of 1 if an individual has consumed more than 60 alcoholic drinks in the last 30 days. (This cutoff of 60 drinks is not arbitrary. According to Dawson et al. [10], individuals who consume more than two drinks every day are considered heavy

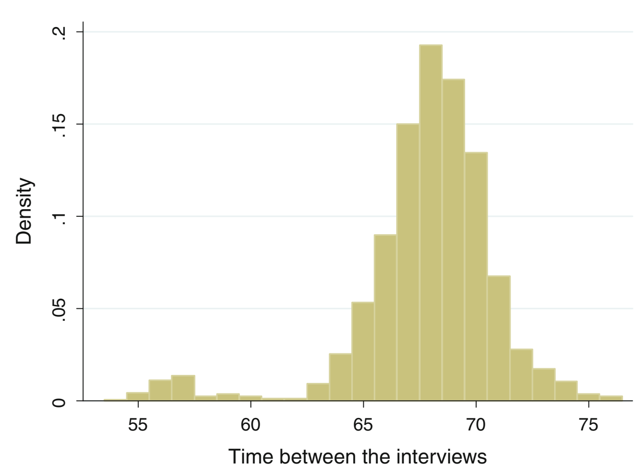

Fig. 1 Distribution of time between the interviews in months

Table 1 Summary statistics for those with uninterrupted schooling between 1997–2002

Variable	1997 Wave			2002 Wave		
	Obs	Mean	SD	Obs	Mean	SD
Smoker 0/1	1,241	0.07	0.26	1,119	0.28	0.45
Cigarettes per day	1,241	0.19	1.25	1,119	2.02	5.05
Cigarettes per day among smokers	88	2.68	3.95	312	7.23	7.33
One pack per day 0/1	1,241	0.00	0.04	1,119	0.03	0.17
Heavy drinker 0/1	1,240	0.00	0.07	1,116	0.08	0.28
Months Attended	1,241	0.98	0.15	1,119	56.35	6.96
Months Attended Ever	1,241	66.48	7.09	1,119	121.84	9.49
Smoking knowledge	1,241	0.90	0.21	1,119	0.94	0.17
Drinking knowledge	1,240	0.83	0.17	1,119	0.83	0.18
Learning disability	1,102	0.09	0.28	998	0.09	0.28
ASVAB	1,004	50.20	29.17	923	50.70	29.16
PIAT	1,172	53.40	34.32	1,056	54.23	34.44
Age	1,241	13.35	0.50	1,119	19.02	0.30
Household income	1,241	38.91	44.09	1,119	51.44	58.44
Married	1,241	0.00	0.00	1,119	0.02	0.14
Cumulative hours worked (1,000 s)	1,241	0.00	0.06	1,119	2.31	1.66
Household size	1,241	4.55	1.43	1,119	4.01	1.68
Male	1,241	0.50	0.50	1,119	0.49	0.50
Black	1,241	0.25	0.43	1,119	0.25	0.43
Hispanic	1,241	0.20	0.40	1,119	0.20	0.40
Mother high school graduate	1,241	0.78	0.41	1,119	0.79	0.41

Table 2 Summary statistics for those with interrupted schooling between 1997–2002

Variable	1997 Wave			2002 Wave		
	Obs	Mean	SD	Obs	Mean	SD
Smoker 0/1	550	0.20	0.40	510	0.48	0.50
Cigarettes per day	550	0.87	2.80	510	5.32	8.75
Cigarettes per day among smokers	112	4.27	4.91	244	11.11	9.79
One pack per day 0/1	550	0.01	0.09	510	0.11	0.31
Heavy drinker 0/1	549	0.01	0.10	509	0.11	0.31
Months Attended	550	0.91	0.28	510	37.87	9.80
Months Attended Ever	550	66.59	8.54	510	103.52	14.29
Smoking knowledge	550	0.90	0.20	510	0.91	0.19
Drinking knowledge	550	0.82	0.17	510	0.80	0.20
Learning disability	485	0.09	0.29	457	0.09	0.29
ASVAB	416	33.84	25.87	392	33.77	25.93
PIAT	517	39.78	32.89	479	39.71	32.81
Age	550	13.42	0.51	510	19.05	0.34
Household income	550	26.18	29.26	510	31.34	44.45
Married	550	0.00	0.00	510	0.08	0.27
Cumulative hours worked (1,000 s)	550	0.00	0.03	510	3.02	2.23
Household size	550	4.66	1.70	510	3.98	1.97
Male	550	0.55	0.50	510	0.55	0.50
Black	550	0.28	0.45	510	0.28	0.45
Hispanic	550	0.21	0.41	510	0.22	0.41
Mother high school graduate	550	0.62	0.48	510	0.62	0.48

drinkers.) As Tables 1 and 2 show, the demographic characteristics are similar between those with and without interrupted schooling. On the other hand, smoking and drinking propensity is higher in the group of students with interrupted schooling, and their household income is lower. Summary statistics in Tables 1 and 2 suggest an increase between the two waves in smoking participation, number of cigarettes smoked per day and heavy alcohol consumption for the individuals in our sample. Note also that average age has increased from 13 to 19 between the two survey waves.

We measure schooling by the number of *Months Attended*, which is the cumulative number of months the individual has attended any type of school (kindergarten to college) since the first interview in the 1997 wave. (However, none of the individuals in our sample were in kindergarten or in primary school at the time of the first interview.) This variable is created using monthly schooling status information available in the schooling event history of each wave of the NSLY97 between 1997 and 2002 waves. Note that the event history variables are not created by asking the individual about his/her enrollment status for each month. Instead, they are generated based on a series of questions to the respondents. First, the individuals are asked whether or not they have been enrolled in school since the last interview. They are then surveyed about the gaps in their enrollment (such as vacation, dropping out and so on) or whether they dropped out of school, and if so when they dropped out. As an example, consider an individual who is interviewed in April 2001 and then again in May 2002. Suppose he reported that he was enrolled in school in both the April 2001 and May 2002 interviews. In the May 2002 interview, the interviewer probes about the gaps in his school attendance between the interviews. For example, assume that the individual reveals that he completed the 10th grade in May 2001, went on vacation in Summer 2001 (June, July and August) and started the 11th grade in September 2001, which was completed in May 2002. This information is then reflected in the monthly event history variables such that the individual is coded as enrolled in all months' educational attainment variables between April 2001 and May 2002 except for June–August 2001.

Note that *Months Attended,* which measures the number of months in school *since the 1997 interview*, can only take the value of 0 (for those who were interviewed at a time when school semester was over) or 1 (for those who were interviewed in a month when school was in session). As shown in the table in Appendix 1 (Online Resource 1), most of the respondents were interviewed in school months during the 1997 survey. Consequently, the average of *Months Attended* variable in 1997 is close to 1. In Table 1, the average value of *Months Attended* is 56 in the 2002 survey for those who went to school without interruption between 1997 and 2002. It is about 38 in Table 2 in the sample of people whose schooling is interrupted between 1997 and 2002.

In some specifications, we run cross-sectional regressions. For such regressions, the *Months Attended* variable is not usable because it does not measure all of the schooling the individual has completed. Rather, it measures the change in schooling between 1997 and 2002 surveys. Thus, instead of *Months Attended*, we use *Months Attended-Ever* in cross-sectional regressions. This variable measures *all* of the attained schooling of the person until that particular year. More specifically, *Months Attended-Ever* measures the amount of schooling, in months, the individual has completed since they started their education. To construct this variable, we added the number of months of school attendance of the individual to their *Months Attended* variable. The number of months of school attendance prior to the first wave of interviews in the NSLY97 has not been recorded. Consequently, to proxy for previous schooling, we used information about the individual's highest grade completed at the 1997 interview date. Assuming that each school year consists of nine months of schooling, to obtain *Months Attended-Ever*, we added nine times the individual's highest grade completed as of the 1997 wave to *Months Attended*. For example, if an individual who was interviewed in September 1997 reported that their highest completed grade was 10, we added 90 months to the *Months Attended* variable to obtain *Months Attended-Ever*. The resulting variable is a measure of the stock of individual's schooling in 1997 and therefore can be used in cross-sectional regressions.

The variables *Smoking Knowledge* and *Drinking Knowledge* indicate the proportion of correctly answered questions about health risks of smoking and drinking, respectively. For *Smoking Knowledge*, the questions gauge whether the individual has correct information about the connection between smoking and heart disease, and smoking and AIDS. For *Drinking Knowledge*, the questions are based on the connection between drinking and liver disease, heart disease, arthritis, addiction to alcohol and harm to an unborn child. The list of the questions and the correct answers are listed in Appendix 3 in Online Resource 1. Summary statistics in Table 1 indicate that most of the individuals have high levels of health knowledge about both smoking and drinking. The proportion of correct answers increased between 1997 and 2002 in case of smoking.

We use the ASVAB score, as is the case in Cutler and Lleras-Muney [7]. About 80 % of the respondents in the NLSY97 sample took the computer-adaptive form of the Armed Services Vocational Aptitude Battery (ASVAB) test. The ASVAB test consists of 12 subtests that measure vocational aptitude in areas such as arithmetic reasoning, assembling objects, auto information and so on. The variable used in our analysis is constructed based on age-adjusted test scores of individuals in four subtests: mathematical knowledge, arithmetic reasoning, word knowledge and paragraph comprehension as obtained from the NLSY97 data set [these four subtests are used by the Department of Defense to calculate AFQT scores (Armed Forces Qualification Test scores)]. The final variable is the percentile in which the individual's test scores fall in comparison to other ASVAB takers.

As an alternative test score, we utilize individual's PIAT (Peabody Individual Achievement Test) math assessment scores. Specifically, we use the individual's percentile score for the PIAT. The version of PIAT administered for the NLSY97 respondents involved answering several mathematics questions. The difficulty of the questions is age-adjusted. Ninety-four percent of the individuals in our sample took the PIAT test during the 1997 wave.

Learning Disability is an indicator for whether the individual has a learning disability. This variable is constructed based on reports of parents, who were asked the following question. "*Does your child now have or has [he/she] ever had a learning or emotional problem that limits or has limited the kind of schoolwork or other daily activities [he/she] can perform, the amount of time [he/she] can spend on these activities or [his/her] performance in these activities?*" If the parent answered in the affirmative, a second question was asked as follows. "What (is/are) the condition(s)? (Select all that apply.) Learning disability (i.e., dyslexia) or attention disorder; Emotional/mental problem or behavior problem; Eating disorder like anorexia or bulimia; Mental retardation; Other (Specify)." We coded our *Learning Disability* variable to take the value of 1 if the parent declared the existence of a learning disability (i.e., dyslexia) or attention disorder. In our sample, about 9 % of the individuals have learning disabilities. This is consistent with the findings of a CDC report by Pastor and Reuben [22] who find that about 8–9 % of all children aged between 6 and 11 have learning disorders.

Table 3 Health knowledge, school attendance and health behaviors: first differences individuals with uninterrupted schooling between 1997 and 2002

	Smoker		Cigarettes/day		Cigarettes/day among smokers		One pack/day		Heavy drinker	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Months Attended	−0.004*	−0.004*	−0.085***	−0.085***	−0.033	−0.034	−0.002**	−0.002**	−0.000	−0.000
	(0.002)	(0.002)	(0.024)	(0.024)	(0.052)	(0.052)	(0.001)	(0.001)	(0.001)	(0.001)
Health knowledge		−0.028		−0.304		0.784		−0.012		−0.064
		(0.056)		(0.543)		(1.779)		(0.020)		(0.039)
Observations	1,108	1,108	1,108	1,108	339	339	1,108	1,108	1,105	1,105

Months Attended is the cumulative number of Months Attended to any school. The outcome variables are listed at the top of columns. Odd (even) numbered columns exclude (include) Health Knowledge (Smoking or Drinking). Health Knowledge is measured as the share of the correct responses individual provided to the questions related to potential health risks of smoking or of heavy alcohol consumption. OLS is employed on the first differenced data. Robust standard errors are in parentheses. *, **, and ***Significance at 10, 5 and 1 % levels, respectively. Only the coefficients of the variables of interest are reported. For the coefficients of the full set of control variables, refer to Online Resource 2

[a] The sample included individuals who were smokers in the 1997 wave or in the 2002 wave

[b] Indicator for whether individual drinks more than two alcoholic drinks every day for a month as defined by Dawson et al. [10]

Table 4 Health knowledge, school attendance and health behaviors: first differences individuals with interrupted schooling between 1997 and 2002

	Smoker		Cigarettes/day		Cigarettes/day among smokers		One pack/day		Heavy drinker	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Months Attended	−0.001	−0.001	−0.120***	−0.122***	−0.111*	−0.115*	−0.004**	−0.004**	−0.003**	−0.003**
	(0.002)	(0.003)	(0.043)	(0.043)	(0.063)	(0.062)	(0.001)	(0.001)	(0.002)	
Health knowledge		−0.009		−3.023**		−4.419*		−0.069		−0.067
		(0.098)		(1.534)		(2.573)		(0.046)		(0.052)
Observations	505	505	505	505	272	272	505	505	505	505

See notes to Table 3

It could be that the parent's report of their children's disability is non-random and instead it depends on the extent to which the parent is involved with their children. For example, more attentive parents could be more likely to report the diagnosis of their children's learning disability. We investigated this possibility by analyzing the correlation between proxies of parent's attentiveness and whether they reported a learning disability for their child. Specifically, we constructed four proxies that are indicators for whether the parent knows most things or everything about *their children's close friends*, *children's close friend's parents*, *who their children are with when they are not at home* and *who their teachers are and what they are doing in school*. Regressing the *Learning Disability* indicator on the parental attentiveness indicators separately resulted in insignificant coefficients presented in Appendix 2 in Online Resource 1.

Time-dependent variables shown in Table 1 are included as control variables in the empirical analyses. All individuals in the sample are born in 1983. (This is because of the design of the survey. Only individuals in the cohort born in 1983 are asked health knowledge questions. These individuals make up our estimation sample.) However, due to the differences in the interview date, there is variation in *Age*. On average, respondents age by about 6 years between the two survey waves. *Household Income* is deflated by 1,000. Unsurprisingly, none of the individuals in the 1997 wave were married, and very few were married as of the 2002 wave. *Cumulative Hours Worked* measures the total number of hours an individual has worked in the labor market. *Household Size* gives the number of individuals in the respondent's household.

The remaining variables in Table 1 are time-invariant individual characteristics. They are included in cross-sectional sections as control variables. About half of the sample consists of males. Individuals who identify themselves as *Hispanic* and non-Hispanic *Black* make up 20 and 26 % of the whole sample, respectively.

Results

The impact of education and the influence of health knowledge

The results obtained from estimating Eq. (4) are presented in Tables 3 and 4. In these tables (and in other tables), we provide only the coefficients of the variables of interest because of space limitations. Table 3 presents the results obtained from the sample of those who had uninterrupted education between 1997 and 2002, and Table 4 displays the results obtained from the sample that consists of individuals whose education was interrupted or completed before 2002. The estimates with the whole set of control variables are displayed in the tables in Online Resource 2.

In specification (4), which is the basis for Tables 3 and 4, all variables are in first-differences. Thus, *Months Attended* in the tables stands for the change in the number of months the individual attended school between the 2 survey years. For each health behavior, two columns of results are presented. The odd-numbered (even-numbered) columns exclude (include) individual's health knowledge about the health behavior. For example, columns (1) and (2) report the regression results where the dependent variable is whether the person is a smoker. Both columns are based on the same specification except that column (2) controls for smoking knowledge, and column (1) omits it.

In Table 3 education has a negative impact on smoking, at both the extensive and intensive margins. An increase in *Months Attended* by 1 school year (9 months) decreases the propensity to smoke by 3.6 % points (0.4×9), which translates into a 16 % decline. A 1-year increase in schooling (9 months) reduces the daily number of cigarettes smoked by about 0.9 cigarettes for everyone. In Table 3, education has no impact on heavy drinking. Note again that we analyze the propensity for heavy drinking because questions on drinking knowledge are based on heavy drinking. An increase in knowledge about smoking has a negative impact on smoking, and an increase in drinking knowledge has a negative impact on heavy drinking, although these impacts are not significantly different from zero in any regression in Table 3. The inclusion of the knowledge variables does not change the estimated coefficients of education.

Table 4 presents the same analysis using the sample of individuals who had interrupted education experience. There are observable differences between this group and those with uninterrupted schooling as revealed by descriptive statistics in Tables 1 and 2. This group of individuals could also be different from those used in Table 3 regressions in unobservable ways such as motivation and time preference. The results in Table 4, however, are similar to those displayed in Table 3. Here, education has no statistically significant impact on the propensity to smoke, but it impacts the frequency of smoking as well as the propensity to drink heavily. Health knowledge has a negative impact on the number of cigarettes smoked, but controlling for health knowledge does not influence the magnitude of the coefficient of education.

As Tables 3 and 4 in Online Resource 2 show, the point estimates for *Married* are negative but insignificant for most outcomes. The number of hours worked in the labor market is positively associated with smoking and also with heavy drinking.

These results suggest that education has a causal impact on health behavior and that accounting for health knowledge does not eliminate or reduce the impact of education on health behavior. Thus, they indicate that allocative efficiency is not likely a primary mechanism through which education impacts health inputs.

An alternative schooling measure is *Highest Grade Completed*. We prefer *Months Attended* to *Highest Grade Completed* because the latter does not measure schooling with precision. For example, consider the case where some respondents are interviewed right after the end of the school year and others are interviewed right before the end of the school year. Those who are interviewed when the school was in session (but close to the end of the school year) will report a value for the number of years of completed schooling that is 1 year fewer in comparison to those who are interviewed right after the end of the school year. However, the actual difference in terms of schooling received is much smaller than 1 full year of schooling. Similarly, years of completed schooling will not reflect the true difference in schooling for two students who are interviewed in different months of the same academic year. In this case, the lack of precision in *Highest Grade Completed* translates into the inability to reflect the true difference in education between individuals conditional on observables (such as age of the individual). In fact, when we estimate the models displayed in Tables 3 and 4 using the Highest Grade Completed as the measure of education, we find that very few of the estimated coefficients of education are different from zero. These results are reported in Appendix Table 3B and 4B in Online Resource 2.

Cognition

In this section, we present the results of the analyses that investigate whether variation in cognitive ability is the reason behind the impact of education on health behaviors. Table 5 presents the results obtained from estimating versions of Eq. (5) using the sample of individuals who had

Table 5 The impact of highest grade completed on health behaviors, models with and without ASVAB: cross section of individuals with uninterrupted schooling between 1997 and 2002

	Smoker		Cigarettes/day		Cigarettes/day among smokers		One pack/day		Heavy drinker	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Months Attended-Ever	−0.004***	−0.003**	−0.036***	−0.032**	−0.065	−0.077	−0.001	−0.001**	−0.000	0.000
	(0.001)	(0.001)	(0.013)	(0.013)	(0.050)	(0.052)	(0.000)	(0.000)	(0.001)	(0.001)
Health knowledge	0.040	0.051	0.186	0.229	1.142	1.005	−0.012	−0.014	−0.034	−0.027
	(0.037)	(0.037)	(0.372)	(0.373)	(2.286)	(2.281)	(0.015)	(0.015)	(0.029)	(0.030)
ASVAB		−0.001***		−0.004		0.012		0.000*		−0.000
		(0.000)		(0.004)		(0.015)		(0.000)		(0.000)
Observations	1,927	1,927	1,927	1,927	326	326	1,927	1,927	1,925	1,925

Months Attended-Ever stands for total months of schooling obtained as of the survey date. Asvab is the individual's percentile score in the math and verbal sections of the ASVAB test. See notes to Table 3

Table 6 The impact of highest grade completed on health behaviors, models with and without ASVAB: cross section of individuals with interrupted schooling between 1997 and 2002

	Smoker		Cigarettes/day		Cigarettes/day among smokers		One pack/day		Heavy drinker	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Months Attended-Ever	−0.005**	−0.004**	−0.117***	−0.111**	−0.160***	−0.165***	−0.003**	−0.003**	−0.001	−0.001
	(0.002)	(0.002)	(0.041)	(0.044)	(0.055)	(0.062)	(0.001)	(0.001)	(0.001)	(0.001)
Health knowledge	−0.009	0.012	−3.214**	−3.081**	−8.814**	−8.889**	−0.049	−0.047	−0.044	−0.042
	(0.079)	(0.080)	(1.549)	(1.537)	(4.003)	(3.969)	(0.038)	(0.039)	(0.044)	(0.045)
ASVAB		−0.002*		−0.011		0.008		−0.000		−0.000
		(0.001)		(0.013)		(0.023)		(0.000)		(0.001)
Observations	808	808	808	808	270	270	808	808	808	808

Months Attended-Ever stands for total months of schooling obtained as of the survey date. Asvab is the individual's percentile score in the math and verbal sections of the ASVAB test. See notes to Table 3

uninterrupted schooling. ASVAB is a measure of cognitive ability; it stands for the percentile ranking of the individual's ASVAB score, ranging from 0 to 100 where higher scores represent higher ability [7]. In Tables 5 and 6 as well as in Tables 7 and 8, we replicate Cutler and Lleras-Muney specifications by running cross-sectional regressions using data from 1997 (the first wave) as well as from 2002. These are the 2 years in which health knowledge questions were administered. We measure education by *Months Attended-Ever*. This variable measures the number of months the individual has attended any school *since* the individual started school, and it incorporates schooling both before and after the 1997 wave.

Models reported in Tables 5 and 6 include a host of family background variables, in addition to personal characteristics of the individuals such as family income, household size and mother's education. As Table 5 demonstrates, education has a negative impact on smoking, and controlling for the ASVAB score reduces the magnitude of the coefficient of education only very slightly. For example, an additional month of schooling reduces the propensity to smoke by 0.4 % points in column (1) when the model does not include ASVAB, but the marginal effect of an extra month of schooling is 0.3 % points when the model contains the ASVAB score (column 2). The same is true for cigarettes smoked per day. These results are consistent with those reported by Cutler and Lleras-Muney [7]. Similar results are displayed in Table 6. In the sample of individuals with interrupted education displayed in this table, the coefficient of education does not change appreciably either when the model includes the ASVAB score, and it even goes up slightly in absolute value (see columns 5 and 6 of Table 6).[3]

[3] It is plausible that the ASVAB score is not a reliable indicator of cognitive ability. For example, Heckman et al. [18] and Hansen et al. [17] stress that a person's schooling and family background at the time tests are taken affect test scores. Although we control for some family background characteristics in the regressions reported in Table 5, it is likely that important family attributes are omitted.

Table 7 The impact of highest grade completed on health behaviors, models with and without PIAT: cross section of individuals with uninterrupted schooling between 1997 and 2002

	Smoker		Cigarettes/day		Cigarettes/day among smokers		One pack/day		Heavy drinker	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Months Attended-Ever	−0.005***	−0.005***	−0.056***	−0.054***	−0.095**	−0.102**	−0.001**	−0.001**	−0.000	−0.000
	(0.001)	(0.001)	(0.013)	(0.014)	(0.042)	(0.043)	(0.000)	(0.000)	(0.001)	(0.001)
Health knowledge	0.063*	0.072**	0.473	0.502	2.360	2.242	0.001	0.000	−0.028	−0.028
	(0.034)	(0.035)	(0.306)	(0.308)	(2.004)	(1.996)	(0.012)	(0.012)	(0.027)	(0.027)
PIAT		−0.001**		−0.002		0.008		0.000		0.000
		(0.000)		(0.002)		(0.009)		(0.000)		(0.000)
Observations	2,228	2,228	2,228	2,228	380	380	2,228	2,228	2,225	2,225

Months Attended-Ever stands for total months of schooling obtained as of the survey date. PIAT is the individual's percentile score in the math section of the PIAT test. See notes to Table 3

Table 8 The impact of highest grade completed on health behaviors, models with and without PIAT: cross section of individuals with interrupted schooling between 1997 and 2002

	Smoker		Cigarettes/day		Cigarettes/day among smokers		One pack/day		Heavy drinker	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Months Attended-Ever	−0.003**	−0.003*	−0.077***	−0.072**	−0.139***	−0.135***	−0.002**	−0.002**	−0.001	−0.001
	(0.002)	(0.002)	(0.030)	(0.029)	(0.050)	(0.051)	(0.001)	(0.001)	(0.001)	(0.001)
Health knowledge	0.036	0.051	−1.925	−1.755	−6.274*	−6.158*	−0.016	−0.013	−0.031	−0.027
	(0.070)	(0.069)	(1.263)	(1.255)	(3.389)	(3.393)	(0.032)	(0.032)	(0.040)	(0.040)
PIAT		−0.001**		−0.014**		−0.007		−0.000		−0.000
		(0.001)		(0.006)		(0.014)		(0.000)		(0.000)
Observations	996	996	996	996	337	337	996	996	995	995

Months Attended-Ever stands for total months of schooling obtained as of the survey date. Piat is the individual's percentile score in the math section of the PIAT test. See notes to Table 3

We repeat the same exercise using the PIAT (the percentile score of the individual's Peabody Individual Achievement Test) score instead of ASVAB (PIAT and ASVAB are highly correlated with a correlation coefficient of 0.72). The results are displayed in Tables 7 and 8. Again, inclusion of the PIAT score does not reduce the magnitude of the estimated education coefficient appreciably. For example, in column 1 of Table 7 we observe that an additional month of education reduces the propensity to smoke by 0.5 % points. The regression result reported in column 2 controls for the PIAT score; in this specification the impact of an additional month of education on smoking propensity is the same as the one reported in column (1). Similar results are obtained for most outcomes reported in Tables 7 and 8. In other words, controlling for cognition, as measured by the PIAT score, does not significantly alter the relationship between education and health behaviors in models that control for a host of personal and family attributes.[4]

These results should be read with one reservation in mind: The test scores we employ in our article are likely to be measured with error. This is because perfect measurement of individual's cognitive ability is not possible. For example, an individual's test score may not reflect their true ability depending on whether they are having a bad day or a lucky day with a lot of correct guesses in the tests. Also, the way the test is written could influence the accuracy of the measurement. A comprehensive measurement of all cognitive skills with one test is impossible. Such error in measures of cognitive ability will lead to estimates that are biased towards zero (in case of classical

[4] We also run specifications that entertain nonlinear effect of cognition [19]. In almost all specifications the quadratic term of cognition was insignificant, and these specifications provided the same results as those with linear cognition.

Table 9 The impact of highest grade completed on health behaviors, models with and without learning disability: cross section of individuals with uninterrupted schooling between 1997 and 2002

	Smoker		Cigarettes/day		Cigarettes/day among smokers		One pack/day		Heavy drinker	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Months Attended-Ever	−0.005***	−0.005***	−0.055***	−0.056***	−0.088*	−0.090*	−0.001**	−0.001**	−0.000	−0.000
	(0.001)	(0.001)	(0.014)	(0.014)	(0.046)	(0.046)	(0.000)	(0.001)	(0.000)	(0.001)
Health knowledge	0.092***	0.094***	0.473	0.451	2.066	1.877	−0.005	−0.007	−0.041	−0.042
	(0.035)	(0.035)	(0.338)	(0.337)	(2.448)	(2.432)	(0.014)	(0.014)	(0.027)	(0.027)
Learning disability		0.015		−0.167		−0.836		−0.015		−0.010
		(0.035)		(0.308)		(1.080)		(0.010)		(0.017)
Observations	2,100	2,100	2,100	2,100	362	362	2,100	2,100	2,097	2,097

Months Attended-Ever stands for total months of schooling obtained as of the survey date. Learning Disability is an indicator that takes the value of 1 if the parent of the individual reported that the individual has a learning disability such as dyslexia or attention deficit disorder. See notes to Table 3

Table 10 The impact of highest grade completed on health behaviors, models with and without learning disability: cross section of individuals with interrupted schooling between 1997 and 2002

	Smoker		Cigarettes/day		Cigarettes/day among smokers		One pack/day		Heavy drinker	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Months Attended-Ever	−0.005**	−0.004**	−0.094***	−0.092***	−0.139***	−0.138***	−0.003***	−0.003***	−0.001	−0.001
	(0.002)	(0.002)	(0.032)	(0.032)	(0.050)	(0.050)	(0.001)	(0.001)	(0.001)	(0.001)
Health knowledge	0.039	0.052	−2.099	−1.927	−7.131**	−6.803*	−0.033	−0.029	−0.061	−0.063
	(0.072)	(0.072)	(1.346)	(1.347)	(3.579)	(3.667)	(0.035)	(0.035)	(0.042)	(0.043)
Learning disability		0.148**		1.908**		1.595		0.040		−0.011
		(0.060)		(0.839)		(1.315)		(0.030)		(0.025)
Observations	942	942	942	942	324	324	942	942	943	943

Months Attended-Ever stands for total months of schooling obtained as of the survey date. Learning Disability is an indicator that takes the value of 1 if the parent of the individual reported that the individual has a learning disability such as dyslexia or attention deficit disorder. See notes to Table 3

measurement error). In addition, if cognitive ability is correlated with other control variables, those will be biased as well. These issues are discussed in several previous articles (for example, Hansen et al. [17] and Conti and Heckman [4]).

The results in this section show that the estimate of education is not sensitive to inclusion of cognitive ability measures into the regressions. Our measure of education is arguably exogenous (at least in the sample of individuals with uninterrupted schooling) because of the random timing of the surveys in NLSY97. Therefore, we can obtain consistent estimates of education on health inputs. Any change in the magnitude of the education coefficient when regressions include cognitive ability reflects education's effect on health due to its correlation with cognitive ability. This is not the case here. Also, measurement error in measures of cognition is less likely to be correlated with our arguably exogenous measure of education. On the other hand, because our measures of cognitive ability are not fully reliable, we cannot speculate on the causality from cognitive ability to health.

To analyze whether the results are altered if we employ a different measure of cognition, we estimate models where an indicator for learning disability of the individual is employed. To make the results comparable to those obtained from the regressions with ASVAB and PIAT, we first use cross-sectional data from 1997 and 2002 and employ *Month Attended-Ever* as the measure of schooling. The results, which are presented in Tables 9 and 10, show that controlling for learning disability does not alter the estimated coefficient of education.

Table 11 presents the models that perform the same analysis, but here the panel nature of the data is exploited. These models are based on Eq. (6). We use the *Months Attended* in these regressions. Once again, exposure to additional months of schooling between the 2 survey years reduces the propensity to smoke and the number of cigarettes smoked. In the sample of individuals with interrupted

Table 11 The impact of school attendance on health behaviors, models with and without learning disability: first differences individuals with uninterrupted schooling between 1997 and 2002

	Smoker		Cigarettes/day		Cigarettes/day among smokers		One pack/day		Heavy drinker	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Months Attended	−0.005*	−0.005*	−0.092***	−0.092***	−0.023	−0.024	−0.002**	−0.002**	−0.000	−0.000
	(0.002)	(0.002)	(0.027)	(0.027)	(0.056)	(0.056)	(0.001)	(0.001)	(0.001)	(0.001)
Months Attended * disability	−0.001	−0.001	−0.007	−0.007	−0.028	−0.029	−0.000	−0.000	−0.000	−0.000
	(0.001)	(0.001)	(0.010)	(0.010)	(0.027)	(0.027)	(0.000)	(0.000)	(0.001)	(0.001)
Health knowledge		−0.023		−0.323		1.112		−0.013		−0.075*
		(0.058)		(0.600)		(2.172)		(0.022)		(0.041)
Observations	989	989	989	989	308	308	989	989	986	986

Months Attended is the cumulative number of months the individual has attended any school. Learning Disability is an indicator that takes the value of 1 if the parent of the individual reported that the individual has a learning disability such as dyslexia or attention deficit disorder. See notes to Table 3

Table 12 The impact of school attendance on health behaviors, models with and without learning disability: first differences in individuals with interrupted schooling between 1997 and 2002

	Smoker		Cigarettes/day		Cigarettes/day among smokers		One pack/day		Heavy drinker	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Months Attended	−0.002	−0.002	−0.128***	−0.132***	−0.130*	−0.135**	−0.004***	−0.004***	−0.003**	−0.003**
	(0.003)	(0.003)	(0.046)	(0.046)	(0.068)	(0.067)	(0.001)	(0.001)	(0.002)	(0.002)
Months Attended * disability	0.001	0.001	0.061	0.057	0.054	0.041	0.004**	0.004**	0.001	0.001
	(0.002)	(0.002)	(0.040)	(0.041)	(0.054)	(0.059)	(0.002)	(0.002)	(0.001)	(0.001)
Health knowledge		−0.052		−2.946*		−5.181*		−0.074		−0.081
		(0.105)		(1.712)		(2.967)		(0.049)		(0.056)
Observations	453	453	453	453	246	246	453	453	454	454

Months Attended is the cumulative number of months the individual has attended any school. Learning Disability is an indicator that takes the value of 1 if the parent of the individual reported that the individual has a learning disability such as dyslexia or attention deficit disorder. See notes to Table 3

education, it also reduces the propensity to be a heavy drinker. However, the impact of schooling is not different between students with and without learning disability. Controlling for learning disability does not alter the relationship between education and health behavior. Furthermore, the marginal effect of education on health behaviors is the same between those who have a learning disability and those who do not. These results suggest that cognition does not impact the education gradient in health behaviors. More specifically, the results do not lend support to the hypothesis that education increases cognition, and enhanced cognition and intelligence enable people to make better health decisions (Tables 11 and 12).

Because variables such as education, health knowledge, disability, ASVAB and PIAT scores are not available for each observation, the sample composition is not identical behind each table. To make sure the variation in results is not due to the change in samples, we re-estimated all models in all samples. The results are reported in tables in Appendix 4 in Online Resource 1. For example, the regressions reported in Tables 3 and 4 use about 1,100 and 505 observations for the sample of individuals with uninterrupted and interrupted schooling, respectively. We re-estimated these models using the sample of individuals who have non-missing values for education, health knowledge and ASVAB. This is termed the ASVAB sample in Tables 3 and 4 in Appendix 4 and includes about 910 and 390 observations for the sample of individuals with uninterrupted and interrupted schooling, respectively. As Appendix 4 shows, the results are insensitive to the sample employed.

Summary and conclusion

Using a panel data set of young individuals from the NLSY97, we pose three questions. The first question is whether the negative effect of schooling on smoking and heavy drinking is causal. We exploit the design of the NLSY97 that has generated an exogenous increase in schooling between the survey years of 1997 and 2002. More specifically, observationally identical individuals who were surveyed in 1997 and then in 2002 could have received differential amounts of schooling up to 24 months because of the timing of the surveys. Using this arguably exogenous increase in educational attainment between the survey years, we find that an increase in schooling has an impact on health behavior.

The second question is whether schooling increases the efficiency of health production. The productive efficiency hypothesis suggests that education has a direct impact on health, much like the impact of technology on production. More educated people are more efficient producers of health, perhaps because the marginal product of health inputs differs by education. An alternative hypothesis is that of allocative efficiency, where more educated people make different choices about health inputs, i.e., they allocate inputs differently, which in turn produces more health output. Under allocative efficiency, education has no direct influence on health as the impact of education is only working through the pathway of health inputs. For example, education provides knowledge about the benefits or harmful effects of health inputs (such as nutrition or smoking), and this knowledge alters health behaviors and health outcomes.

To investigate the relative validity of these hypotheses, we estimate models of health behavior where the change in various measures of smoking and heavy drinking between the 2 survey years are regressed on increases in educational attainment between the same years and on the change in the relevant health knowledge. We find that accounting for health knowledge does not eliminate or reduce the impact of education on health behavior. This finding supports the productive efficiency hypothesis.

We also investigate whether cognitive ability is responsible for the impact of education on health behavior. Using the ASVAB and PIAT scores as alternative measures of cognitive ability, we find that accounting for ability does not significantly alter the relationship between education and health behaviors in models that control for a host of personal and family attributes.

Finally, we perform another test to investigate how cognitive ability impacts the relationship between education and health behaviors. The test involves a comparison of health input demands of two individuals who are observationally identical except for one dimension: One of them has a learning disability such as dyslexia or attention disorder. The individual with the learning disability is expected to learn less in school compared to the individual without the disorder for a given level of schooling. If what is learned in school is a determinant of the influence of education on health inputs, then a particular increase in schooling would have a smaller impact on health behavior for those with learning disability. Our results, however, show that learning disability does not influence the impact of schooling on health behaviors. An increase in schooling has the same impact on health behaviors for those who have a learning disability as for those who don't have a learning disability. These findings, taken together, suggest that cognition is unlikely to be a primary factor in explaining the relationship between education and the demand for health inputs.

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