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Does more education lead to better health habits? Evidence from the school reforms in Australia

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ABSTRACT

The current study provides new empirical evidence on the causal effect of education on health-related behaviors by exploiting historical changes in the compulsory schooling laws in Australia. Since World War II, Australian states increased the minimum school leaving age from 14 to 15 in different years. Using differences in the laws regarding minimum school leaving age across different cohorts and across different states as a source of exogenous variation in education, we show that more education improves people's diets and their tendency to engage in more regular exercise and drinking moderately, but not necessarily their tendency to avoid smoking and to engage in more preventive health checks. The improvements in health behaviors are also reflected in the estimated positive effect of education on some health outcomes. Our results are robust to alternative measures of education and different estimation methods.

1. Introduction

There is numerous evidence that unhealthy behaviors substantially account for people's poor health outcomes. It is estimated that approximately half of the deaths that occurred in 1990 in the USA were caused by unhealthy behaviors such as smoking, poor diet, insufficient exercise, excessive drinking, and illicit use of drugs (McGinnis and Foege, 1993). Although smoking remains one of the leading causes of mortality, poor diet and physical inactivity may soon overtake tobacco as the leading causes of death (Mokdad et al., 2004). Unhealthy behaviors have also often been cited as the main predictors of chronic diseases such as cancer and heart disease (Doll and Hill, 1956; Wilson, 1994; Boffetta et al., 2006). This raises an important, policy-related question: What can we do to improve people's health-related behaviors to, in turn, improve their health outcomes?

One answer to this question is simply to increase people's education level. Economists have provided a theoretical framework in which education plays an important role in the health production process. According to the demand-for-health model (Grossman, 1972, 1975, 2000), higher levels of schooling have a direct effect on health and health behaviors. Better education leads to more efficient use of a given set of health inputs by improving decision-making abilities (productive efficiency) and improving the "allocative efficiency" among various health inputs by increasing a person's ability to acquire and process health information. All other things being equal, this higher production efficiency generated by education raises future returns (in terms of both future health and lifetime earnings) to health investments, and therefore better-educated individuals are more likely than less educated individuals to choose healthier lifestyles.

Education may also have a positive impact on people's health behaviors indirectly. The most discussed effect of education on health is through its effect in the labor market. For example, studies have shown that better-educated people tend to enjoy better employment outcomes and higher wages (Card, 1999). This may in turn improve health habits by increasing the affordability of other health-improving inputs that are complementary to health habits (e.g., better access to healthier foods and gym membership), or by increasing access to healthcare (via increased income or employment-based health insurance), or by reducing income volatility and income-related stress which are factors that tend to discourage people from engaging in healthier lifestyles (Contoyannis and Jones, 2004). Another theory is that education could lower individuals' discount rates and make them more patient and future-oriented (Becker and Mulligan, 1997), which in turn leads to better health behaviors. Better-educated people may also have healthier peers who encourage better health behaviors

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(Garivia and Raphael, 2001; Duncan et al., 2005). They also tend to be more optimistic, have better coping styles, enjoy higher levels of social support, and have the values and dispositions necessary for achievement, all of which may contribute positively to their health behaviors and health outcomes (Penny and Robinson, 1986; Ross and Wu, 1995; Taylor et al., 2000).

Empirical studies on this subject have found striking correlations between education and health behaviors that are consistent with the theories. For example, researchers have found better-educated people to smoke less, consume less alcohol, exercise more, eat healthier food, and have more frequent health checks (Droomers et al., 1999; Cutler and Glaeser, 2005; Cutler and Lleras-Muney, 2010). However, because their causal implications are difficult to interpret, cross-section patterns are only suggestive. For example, causality may also run in reverse from health behaviors to more schooling; i.e., students with healthier lifestyles may also be more efficient producers of additional human capital, thus implying that estimates of the education effect on health behaviors will be biased upward. There may also be important omitted variables from the estimation model, such as rates of time preferences (Fuch, 1982) and heritable endowments (Behrman and Rosenzweig, 2004), which may influence both education and health behaviors simultaneously. One could imagine that people who are born with predispositions that make them more future-oriented (i.e., those who desire more leisure at an older age) will remain in school for longer, work more at a younger age, and invest more in positive health behaviors during most stages of their lifecycle. Thus, the effect of education will be biased upward if we fail to control for time preferences. Another important bias is the measurement error bias associated with the manner in which education is typically measured in surveys, which may bias the estimated education coefficient towards zero (Blackburn and Neumark, 1995).

Recent studies that have attempted to estimate the causal effect of education on health behaviors have mainly dealt with the endogeneity issue by relying on quasi-experiments such as changes in the compulsory schooling laws or an idiosyncrasy of geography or birthdate to generate an exogenous variation in people's schooling levels (Eide and Showalter, 2011), and the findings have been mixed. Many studies have found positive effects of education on various measures of health-related behaviors or variables that are related to behaviors, such as body mass index (BMI). Using the state of residence in childhood as instruments for schooling, Sander (1995) found that a higher level of schooling significantly reduced the probability of smoking among adults in the 1986–1991 US General Social Survey. Arendt (2005) obtained a similar set of findings when he used two compulsory schooling reforms in Denmark to address endogeneity of schooling in BMI and smoking regression equations. He showed that the estimated effects of schooling on BMI and smoking are larger than the coefficients obtained by ordinary least-squares (OLS) when endogeneity was corrected for. Park and Kang (2008) showed, using high-school availability and birth order of Korean men as instruments, that more education induced individuals to exercise more regularly and to get regular check-ups, but had little effect on smoking and drinking. Using changes in compulsory schooling in Sweden as instruments, Spasojevic (2010) reported a positive schooling effect on BMI in the healthy range, although the effect was significant only when one-tailed tests were used. Jürges et al. (2011) used education expansion in western Germany as instruments and found that more education significantly reduced the probability of being a smoker for both men and women.

By contrast, other studies have found education to have virtually zero effect on health-related behaviors. Using changes in compulsory schooling laws in Germany between 1949 and 1969 as instruments, Kemptner et al. (2011) showed that more education did not statistically significantly reduce the probability of smoking for both men and women. Using February birthdates as an instrument for obtaining a high-school qualification in the UK, Braakmann (2011) found a statistically insignificant effect of education on smoking, drinking, and eating certain types of food. In one of the most comprehensive studies on the causal effect of education on health and health-related behaviors, Clark and Royer (2013) exploited two changes in the compulsory schooling laws in the UK to show that the reforms had no significant impact on smoking, eating healthily, and exercise. More examples of nonsignificant effects of education on health-related behaviors can be found in Arendt (2005) and Kenkel et al. (2006) with respect to current smoking behaviors and the probability of having never started smoking in the first place.

One explanation for the mixed findings might be due to the different measures of health-related behaviors, quasi-experiments, statistical methods, sub-samples, and data sets used across these different studies. Thus, in order to advance our understanding of the causal effect of education on health-related behaviors, more evidence is required from contexts that have not as yet been explored in the literature. New research must also follow as closely as possible the statistical methods used by previous studies. It should also examine as many health-related behaviors as possible in a single study. The current study does this by being the first of its kind to exploit one of the most commonly used sources of exogenous variation in education in the literature, i.e. changes in the compulsory schooling laws that occurred in different years and across different states, to study the causal effect of education across ranges of health-related behaviors in Australia.

Our paper, which is the first of its kind for Australia, adds to the existing literature on the causal links between education and health by studying a different institutional environment to those previously examined in the literature; examples include studies that used changes in the compulsory schooling laws in the USA (Adams, 2002; Lleras-Muney, 2005), UK (Oreopoulos, 2007; Clark and Royer, 2013), Germany (Jürges et al., 2011), Denmark (Arendt, 2005), Sweden (Spasojevic, 2010), and South Korea (Park and Kang, 2008). In addition, our rich data set allows us to investigate the causal effect of education on a much wider range of health-related behaviors than has typically been examined in one study.

The current study is organized as followed. Section 2 presents a brief account of the health statistics in Australia and the institutional background of the Australian schooling legislations. Section 3 describes the data and the empirical strategy. Results and discussions are presented in Section 4. Section 5 concludes.

2. Health statistics and institutional background

2.1. Health statistics in Australia

Australia faces the same growing health issues as many OECD countries. Like the USA and the UK, Australia's current leading cause of death is coronary heart disease, with over 20,000 deaths in 2012 (ABS, 2014). This includes angina, blocked arteries, and heart attacks. The rate of obesity is also on the rise in Australia, with 10.8 million adults (63% of the Australian population) either overweight or obese in 2011–2012. Of these, 4.7 million were obese (NHPA Report, 2013). Given the ample evidence in epidemiology and the medical literature that individuals can significantly reduce the risk of developing a heart disease simply by abstaining from smoking, eating healthily, and exercising regularly, it is important that health policymakers in Australia know whether education has a causal effect on later health habits.

2.2. Compulsory schooling in Australia

Responsibility for regulations regarding compulsory schooling in Australia lies with the Australian states (Barcan, 1980). The regulations usually operate by specifying and enforcing both maximum school entry age and minimum school leaving age. Since the start of the twentieth century, there have been three waves of changes in the minimum school leaving age in Australia. The first legislation took place in the early 1900s when all Australian states adopted a minimum school leaving age of 14 years. The second legislation, in which the minimum school leaving age was raised from 14 to 15, took place just after World War II. However, the legislation was only formally proclaimed in New South Wales (and the Australian Capital Territory, whose school system was administered by New South Wales until the mid-1970s) and Tasmania, which was the only state that increased the minimum school leaving age from 14 to 16. Finally, other states eventually increased the minimum school leaving age to 15 years almost simultaneously in the mid-1960s; see Table A1 in the online appendix for a summary of the years the changes in law were introduced in different states. However, as in other countries, not every child complied with the compulsory school leaving laws. For example, New South Wales, the Australian Capital Territory, and Tasmania, which were the first to raise their school leaving ages, had systems of exemptions that allowed many students to leave school prior to the statutory limit.

3. Data and empirical strategy

3.1. Data

The data comes from the Household, Income and Labour Dynamics in Australia (HILDA) longitudinal survey, which has been tracking members of a nationally representative sample of Australian households since 2001. A total of 7682 households participated in wave 1, providing an initial sample of 19,914 persons (Wooden et al., 2002). The members of these participating households form the basis of the panel pursued in subsequent annual survey waves. Interviews are conducted with all adults aged 15 years or older who are members of the original sample, as well as any other adults who, in later waves, are residing with an original sample member. Annual re-interview rates are reasonably high, rising from 87% in wave 2 to over 96% by wave 9 (Watson and Wooden, 2012).

Alongside socio-economic questions typically asked in standard household surveys, the HILDA survey collects data on a number of health behavior variables in waves 7 and 9. From this information, we constructed five dichotomized outcome variables of risky health behaviors (three on smoking and two on drinking), six measures of dietary behaviors (two on regular consumption of fruits, two on regular consumption of vegetables, one on avoidance of fatty food, and one on the use of low-fat milk), four BMI-related indicators of health endowments, and other healthy lifestyles (exercising frequently, eating breakfast regularly, and undergone preventive health checks in the last year).

In addition, we constructed two different aggregated measures of health behaviors based on a selection of some of these individual health habit measures, following a previous study of health-related behaviors in Australia (Cobb-Clark et al., 2014). The first aggregate variable of health-related behavior is the Healthy Eating Index (HEI), which is constructed based on the following indicators: (1) eating fruits seven days a week; (2) eating vegetables seven days a week; (3) avoiding (i.e., eating less than once per month) fatty, high-cholesterol foods such as potatoes, French fries, hot chips, or wedges; and (4) avoiding milk fat by drinking skimmed or low-fat milk. These four variables were then added together to form an index that ranges from 0 “unhealthiest diet” to 4 “healthiest diet.” The second aggregate variable of health-related behavior is a short version of the Alameda-7 (ALMDA) index, which was originally designed to capture a combination of healthy lifestyle choices (see, e.g., Schoenborn, 1986). ALMDA was constructed based on the following behaviors: (1) eating breakfast seven times a week; (2) drinking moderately (avoiding binge-drinking), i.e., fewer than 7 units for men and 5 for women on any given sitting; (3) exercising at least three times per week at moderate or intensive physical exertion; and (4) currently not smoking. Similar to the HEI, the ALMDA variable ranges from 0 “worst combination of health habits” to 4 “best combination of health habits.” We standardized all of our continuous health-related behavioral variables – as well as other continuous health outcome variables – to have a mean of 0 and a standard deviation of 1. Descriptive statistics of the (unstandardized) variables can be found in Table A2 and the detailed descriptions of each variable can be found in the online appendix in Table A3.

Following Leigh and Ryan (2008), estimates of years of education, which is our main educational variable, are derived from respondents' highest educational attainment. As is conventional, we are not measuring actual years spent in education (which would vary with the time within which qualifications are completed, the number of qualifications obtained, and the time spent studying that did not lead to a qualification) but instead the time typically taken to obtain the highest qualification reported. Thus, a respondent reporting having completed secondary school (Year 12) is assumed to have completed 12 years of education, a person completing an ordinary university degree is assumed to have completed 15 years of education, and so on. Nevertheless, as described in Section 4, we will also extend our analysis on different measures of education, including an indicator variable representing whether a person left school at age 14, a continuous variable representing age of leaving school, and an indicator variable representing whether a person had at least completed a university degree.

We focus our attention on all adults born between 1939 and 1972 who were interviewed in waves 7 (year = 2007) and 9 (year = 2009) where questions about health-related behaviors were asked. The response rates for waves 7 and 9 were 95% and 96%, respectively. We focus on adults born between 1939 and 1972 because they consist of cohorts who left school some years before the policy changes in the 1960s' and cohorts who left school some years after these policy changes. This leaves us with 14,728 observations. We further restrict the sample to those who responded to the health behaviors questionnaires and completed secondary school in Australia. Because we need to accurately assign the applicable minimum school leaving age laws to each individual, we also restrict the sample to those who responded to the question in wave 12 about the Australian state in which the highest year of schooling was completed. This further reduces our sample size. The final pooled sample consists of almost 10,000 observations, with the number of observations varying between 4408 in the “preventive health-check” equation (for those aged 40+) and 9894 in the “eating fruit seven days a week” equation.

3.2. Compulsory schooling laws as instruments

To instrument for education, we adopt the identification strategy outlined in Leigh and Ryan (2008) and use within-state differences in the minimum school leaving age laws as a source of exogenous variations in people's education levels. The identification assumption is that these education reforms would have caused some youths – irrespective of their abilities and socio-economic backgrounds – who would have left earlier in the absence of the more restrictive laws to remain in school, thus raising their education level in the process (Harmon and Walker, 1995; Oreopoulos and Salvanes, 2007). However, it is further assumed that these education reforms would not have had a direct effect on health-related behaviors measured at the later stages of the lifecycle beyond their direct effect at raising the education level of those who were exposed to the laws.

What is the relevance of using these reforms as our instruments? In our sample, the individuals who completed their highest year of schooling in the states of Victoria, Queensland, Western Australia, Southern Australia, and the Northern Territory studied in regions where there had been a change in the minimum schooling leaving age laws from 14 to 15 years in the mid-1960s. As an aid to thinking about the validity of these school reforms, we set out to illustrate for these cohorts the discontinuity in the level of education around the time the laws were changed. To do this, we divided individuals into two groups – those who were exposed to the old law, and those who were exposed to the new law *regardless* of where they completed their highest year of schooling (i.e., an individual who was born in 1950 and completed his or her highest year of schooling in Victoria would be given the same 'timing to treatment' value of 0 as a person who was born in 1951 and completed his or her highest year of schooling in Queensland) – and used a regression discontinuity (RD) design to estimate the effect of the raising of the minimum school leaving age law from 14 to 15 on the proportion of individuals leaving school at age 14 and the proportion of individuals leaving school at age 15. The idea of an RD design is to allow us to conduct a test of whether there is a noticeable increase in the average education level "just" before and after the introduction of the education reforms in Australia; for a more comprehensive description of the RD design, see, e.g., Imbens and Lemieux (2008) and Lee and Lemieux (2009).

Fig. 1A, in which the data are aggregated into cell means by "year to and from the introduction of the law" to create yearly averages, illustrates that there is a sharp drop in the average proportion of individuals leaving school at age 14, from around 14% one year before the introduction of the law to around 7% in the first year when the law was introduced; the local linear Wald estimate for the drop is −0.095 and statistically significantly different from zero. However, Fig. 1B shows that the change in the law did not induce a significant drop in the average proportion of individuals leaving school at age 15. Instead, the law change ended up raising the probability of people leaving school at age 15 by a small fraction; the Wald estimate here is positive, at 0.031, and statistically significantly different from zero. This small jump might be representing individuals who would have left at age 14 under the old law but now could only leave at age 15 under the new law.

Although visually effective, the statistical significance of these RD estimates is sensitive to choices of bandwidth and functional form. This is not entirely surprising, given that our estimation sample here is probably too small to draw firm conclusions, and care must therefore be taken when interpreting the RD estimates. In addition, our RD approach does not allow for the within-state and birth year differences in terms of people's reactions to the laws. For this reason, it is important to incorporate individuals from other states who had not experienced a change in the law and estimate a pooled instrumental variables (IV) regression that includes both "state where highest year of schooling was completed" and "birth year" fixed effects as follows:

$$S_{ibs} = \alpha + \beta SLA_s + X_i'\gamma + SE_s + BY_b + \varepsilon_i, \tag{1}$$

$$H_{ibs} = \theta + \pi \widehat{S}_{ibs} + X_i'\rho + SE_s + BY_b + u_i, \tag{2}$$

where S_{ibs} denotes years of education for individual i, who was born in year b and completed the highest year of schooling in state s; H_{ibs}

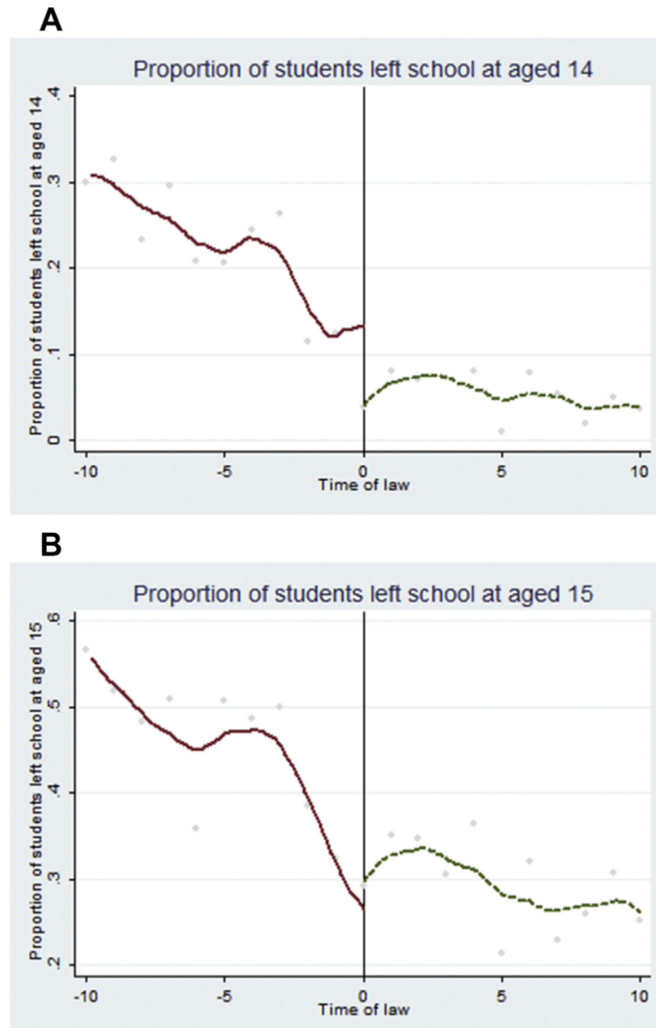

Fig. 1. The impact of the within-state compulsory schooling changes on educational attainment. A: Left school at 14. B: Left school at 15. **Note**: Samples are based on individuals born between 1939 and 1972 in the States of Victoria, Queensland, Western Australia, Southern Australia, and Northern Territory. Points represent means among people in each year-to-and-from-law cell.

is either a standardized measure of health-related behavior with mean 0 and a standard deviation of 1 whenever the health-relate behavior variable is continuous, or an indicator variable with values 0 and 1 whenever the health-related behavior variable is a binary variable; SLA_s is an indicator variable representing the compulsory school leaving age; X_i' is a vector of control variables, including gender, age, age-squared, and a wave dummy (wave 9); SE_s is set of dummy variables representing the state where highest year of schooling was completed; BY_b is fixed effects of birth year; ε_i and u_i are the error terms; and $\widehat{S}_{ibs}$ is the predicted level of schooling obtained from Equation (1). Our hypothesis is that the average increase in the education level would be larger for individuals from states and birth cohorts that had undergone a compulsory schooling law change than for individuals from states and birth cohorts that had not undergone a compulsory schooling law change.

How much of an impact did raising compulsory schooling laws have on education attainment? Panel A of Table 1 shows the results from regressing total years of education on the minimum school leaving age in a given state and year, in a specification that includes gender, fixed effects of birth year, and fixed effects of state where

Table 1
Healthy eating index regressions, OLS and different IV estimators.

Panel A Dependent variable: years of education	(1)	(2)	(3)	(4)	(5)
Compulsory school-leaving age	0.411*** [0.113]				
Female	−0.195*** [0.066]				
Observations	9088				
Panel B **Dependent variable: HEI**	**OLS-MC**	**IV leaving age** **IV-2SLS**	**IV leaving age × birth year** **IV-2SLS**	**IV leaving age × birth year** **IV-GMM2S**	**IV leaving age × birth year** **IV-LIML**
Years of education	0.102*** [0.004]	−0.022 [0.109]	0.172*** [0.043]	0.183*** [0.019]	0.245*** [0.095]
Female	0.465*** [0.020]	0.441*** [0.034]	0.479*** [0.026]	0.475*** [0.023]	0.494*** [0.032]
F-test on the excluded instruments:					
F-statistics		13.14	3.9E+07	3.9E+07	3.9E+07
Hansen J statistics (over-identification test)			45.893	45.893	45.670
p-value			[0.3532]	[0.3532]	[0.3617]
Observations	9088	9088	9088	9088	9088

Note: ***<1%; **<5%. Robust standard errors are in parentheses. The dependent variable is the standardized healthy eating index with a mean of 0 and a standard deviation of 1. All regressions are clustered at two different dimensions, first by panel (personal identification) and second by state× birth year. OLS-MC = Ordinary Least Squares with multi-way clustering; 2SLS = Two-step least squares; GMM2S = Two-step generalized method of moments; LIML = Limited-information maximum likelihood. All regressions controlled for state fixed effects (state where completed highest education), birth year fixed effects and a wave dummy.

highest year of schooling was completed. Similar to the estimate obtained by Leigh and Ryan (2008), we find that a one-year increase in the leaving age raises educational attainment by about 0.41 years, which is also statistically significantly different from zero at the 1% level.

However, this approach, which directly uses the compulsory school leaving age as the single IV, constrains the effect of the instrument to operate equally for the younger cohorts (e.g., those born in 1972) and the older cohorts (e.g., those born in 1939), despite possible changes in enforcement and a possible reduction in the fraction of students dropping out of school at the earliest opportunity. We thus follow Leigh and Ryan (2008) and interact each of the school leaving age law indicator variables with the respondent's birth year dummies and use this set of interaction terms — which totals 44 excluded variables in the first-stage regression — as instruments. This changes our first-stage equation to

$$S_{ibs} = \alpha + \beta(SLA_s \times BY_b) + X_i'\gamma + SE_s + BY_b + \varepsilon_i, \tag{3}$$

while keeping the second-stage equation the same. The above regression equations are estimated using a variety of IV estimators including two-step least-squares (2SLS), two-step general method of moment (GMM2S), and limited information maximum likelihood (LIML). We follow Leigh and Ryan (2008) and account for clustering at the state birth year level and clustering at the individual level by using the non-nested two-way clustering approach (Cameron et al., 2006). It should also be noted that we are able to improve upon Leigh and Ryan and use the new variable ("state or territory where highest year of schooling was completed"), which had only been recently introduced in wave 12, to correctly assign individuals to their corresponding state-specific compulsory schooling laws, thus reducing some of the measurement error biases associated with the instruments.

4. Results

We present in Panel B of Table 1 the results of the OLS estimation and the two abovementioned IV approaches. Using the compulsory school leaving age as an instrument in the 2SLS estimator, we find that years of education have a negative, albeit statistically insignificant, effect on our first outcome variable of interest, i.e., the HEI. The *F*-test on the excluded instrument is statistically significant; the *F*-statistic is 13.14, which is slightly above the rule of thumb value of 10 for an exactly identified equation. When leaving age is interacted with birth year, the *F*-test on the excluded instruments produces *F*-statistic that is sufficiently large for us to comfortably reject the null hypothesis of weak IVs. We also find that, when leaving age is interacted with birth year, one more year of education raised the HEI by approximately 17.2% of the standard deviation, which is noticeably larger than the 10.2% obtained by OLS. This is a sizeable effect; it is almost half of the gender effect. Switching the estimator from 2SLS to GMM2S and LIML increases the estimated effect slightly to 18.3% and 24.5%, respectively, although the qualitative interpretation of the results remains the same. Because the causal effect is more precisely identified using the second approach, which uses the interaction term as the IVs, we employ this approach as our preferred model and focus on the results generated from the GMM2S estimator for all of the other health habit outcomes.

Table 2 shows that the estimated effect of educational attainment on the aggregated measure of health habits (ALMDA) is positive and statistically significant. The estimated effect is slightly smaller than the one obtained by OLS, thus indicating that the OLS may have overestimated the impact of educational attainment on this particular aggregated measure of healthy lifestyle choices.

Tables 1 and 2 show a significant education effect on the aggregated index of healthy eating but a much weaker effect on the index of healthy lifestyle. Yet it remains unclear which aspects of either measure are driving these results. To examine this, we estimate the education effect (Tables 3–5) for disaggregated lifestyle decisions and health endowments, which includes disaggregated measures of risky health behaviors, dietary behaviors, health endowments, and other healthy lifestyles.

Focusing on the IV results in Column 1 of Table 3, we find that one more year of education reduces the probability of being a current smoker by about 0.6%, although this is not statistically

Table 2
Alameda index regressions.

Dependent variable: ALMDA	OLS	IV-GMM2S
Years of education	0.089*** [0.005]	0.056** [0.025]
Female	0.302*** [0.022]	0.298*** [0.024]
F-test on the excluded instruments:		
F-statistic		1591.22
Hansen J statistics		42.790
p-value		[0.4803]
Observations	7822	7822

Note: See Table 1. ***<1%.

Table 3
Risky health behaviors regressions.

	(1) Currently smoking *versus* currently non-smoking	(2) Quitted smoking *versus* still smoking	(3) Smoking initiation: Never smoked *versus* ever smoked	(4) Moderate drinker	(5) # of standard drinks
a) OLS					
Years of education	−0.033*** [0.000]	0.038*** [0.001]	0.034*** [0.001]	0.023*** [0.001]	−0.053*** [0.003]
b) IV-GMM2S					
Years of education	−0.006 [0.009]	−0.013 [0.015]	−0.020 [0.018]	0.032** [0.013]	−0.032 [0.026]
F-test on the excluded instruments:					
F-statistic	71,601.86	348.57	71,601.86	9.5E+05	49,547.38
Hansen J statistics (over-identification test)	40.459	37.780	39.299	38.709	42.195
p-value	0.5821	0.6145	0.6325	0.6578	0.5061
Observations	9099	4925	9099	7868	7953

Note: ***<1%; *<10%. See notes for Table 1.

significantly different from zero. Since the findings from recent studies suggest education has different effects on quitting *versus* initiating smoking (e.g. Maralani, 2013), we also estimate the effect of education on these two outcomes separately. Our OLS estimates in Columns 2 and 3 suggest that one more year of education reduced the probability of smoking initiation (i.e. smoked at any point) and increased the probability of quitting after initiated smoking, and educational gap in quitting smoking is slightly larger than in smoking initiation. However, the IV estimates are again not statistically well-determined. Columns 4 and 5 show that one more year of education increases the probability of having never exceeded the gender-based drinking threshold in the past year by 3%, but does not significantly influence the typical number of units per session.

Education appears to have a more significant effect on an individual's diet. Looking across Table 4's columns, the IV estimates suggest that one more year of education significantly increases the total number of days per week eating fruits by 6% of the standard deviation, the probability of eating vegetables seven days a week by 10%, and the total number of days per week eating vegetables by 14% of the standard deviation. It also increases the probability of choosing to consume low-fat milk instead of full-fat milk by 5%. Nevertheless, education does not seem to have a significant impact on the probability of avoiding fatty food including fried potatoes, French fries, hot chips, or wedges.

Table 5's IV results show that education has a positive and statistically significant impact on BMI. However, its estimated effect on the probability of being underweight and overweight is found to be statistically insignificant. There is a small positive effect of education on the probability of being obese, although this is only marginally significant at the 10% level. Education has a positive impact on individuals reporting to exercise at least three times per week. Yet it does not seem to have a significant impact on individuals reporting to undergo more preventive health checks if they are aged 40 or over. Finally, education significantly increases the probability of individuals reporting to have breakfast seven days a week by 4% and the total number of days per week having breakfast by 8% of the standard deviation.

As a robustness check, we reconducted the analysis from Table 1 by using as alternative measures of education: (i) leaving school at age 14; (ii) age left school; and (iii) completing at least a university degree (Table 6). We find that our set of IVs works just as well at predicting these other measures of education and that these alternative measures of education still affect HEI in the right directions (except for the "Left at age 14", which is statistically insignificant), thus lending further support for our earlier findings.

It is important to acknowledge that the estimated education effects vary significantly across males and females and across individuals of different socio-economic backgrounds. For example, education significantly reduces the probability of being obese for women, but not for men. Given the limited space available, we are not able to discuss all of the results here and report our IV estimates by subsamples in the online appendix (Table A4).

Our findings indicate that not every dimension of health-related behaviors is significantly affected by education. Although more education has a strong and positive effect on people's dietary choices and a moderate effect on their tendency to engage in more regular exercise and drink moderately, its effect is virtually zero on their smoking behavior, BMI-related outcomes and the tendency to engage in more preventive health checks.

The heterogeneous effect of education on various health behaviors is consistent with the findings that the correlation between various health-related behaviors is almost zero (Cutler and Glaeser, 2005). This may indicate that different causal mechanisms are at

Table 4
Dietary behaviors regressions.

	(1) Eating fruits 7 day/week	(2) # of days/week eating fruits	(3) Eating vegetables 7 day/week	(4) # of days/week eating vegetables	(5) Avoid fatty food	(6) Use of low fat milk
a) OLS						
Years of education	0.034*** [0.001]	0.093*** [0.005]	0.047*** [0.001]	0.097*** [0.004]	0.013*** [0.001]	0.023*** [0.001]
b) IV-GMM2S						
Years of education	0.025 [0.016]	0.059** [0.027]	0.100*** [0.005]	0.137*** [0.018]	0.000 [0.012]	0.054*** [0.011]
F-test on the excluded instruments:						
F-statistic	7.3E+07	7.3E+07	7.3E+07	7.3E+07	3.9E+07	7.3E+07
Hansen J statistics	43.286	45.063	39.407	37.980	52.573	51.035
p-value	0.4591	0.3857	0.6279	0.6884	0.1503	0.1871
Observations	9894	9894	9893	9893	9089	9894

Note: ***<1%; **<5%. See notes for Table 1.

Table 5
Health endowments and other healthy life-styles.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	BMI	Underweight	Overweight	Obese	Exercise ≥ 3times/week	Preventive health checks (40 years +)	Having breakfast 7days/week	# of days/week having breakfast
a) OLS								
Years of education	−0.044*** [0.006]	−0.001*** [0.000]	−0.016*** [0.001]	−0.020*** [0.001]	0.012*** [0.001]	0.005*** [0.000]	0.030*** [0.000]	0.077*** [0.004]
b) IV-GMM2S								
Years of education	0.045** [0.018]	−0.002 [0.003]	−0.004 [0.013]	0.025* [0.014]	0.030** [0.015]	−0.003 [0.011]	0.039*** [0.010]	0.082*** [0.020]
F-test on the excluded instruments:								
F-statistic	1.5E+07	1.5E+07	1.5E+07	1.5E+07	2.9E+08	491.25	7.2E+07	7.2E+07
Hansen J statistics	38.940	23.537	47.381	45.048	41.728	42.725	41.787	44.113
p-value	[0.6878]	[0.9951]	[0.3364]	[0.4279]	[0.5265]	[0.3969]	[0.5239]	[0.4243]
Observations	8873	8873	8873	8873	9132	4408	9893	9893

Note: ***<1%. See notes for Table 1.

play simultaneously thus in combination will generate complex patterns. This can provide explanations as for why we observe differential education effect on different health behaviors. For example, higher education increases income and affordability for healthier food and gym membership, but it also increases the opportunity cost of time; therefore in our results we observe more education leads to better diet and more regular exercise but maybe less time to undergo preventive health-checks.

The insignificant education effect on smoking behaviors might again reflect the combined effects through different mechanisms. The first mechanism that is particularly important for smoking behaviors is through peer effects. Since our study exploits the policy change that increased at most one more year of schooling, the changes in education did not affect the teenage peers of the affected cohorts thus could not induce substantial changes in smoking behaviors. Second, higher education might actually lead to jobs that increase responsibility and stress therefore induce individuals to compensate by smoking and/or drinking (Oreopoulos and Salvanes, 2011). Third, as remarked by Cutler and Lleras-Muney (2012), the effect of education on smoking depends on the level of development defined both in terms of income and knowledge and will therefore vary over time and space. Since our study cohorts include a large proportion of older cohorts whose smoking behaviors were shaped as early as in the 1960s, higher income for the affected cohorts also increased the affordability of cigarettes thus could have increased smoking initiation. At that time income effect outweighed information as it was much before the era of widespread information about the harmful effects of smoking in Australia, marked by the success of large-scale, mass-media anti-smoking campaigns in the early 1980s (Pierce et al., 1987). These campaigns later caused substantial drop in smoking for both more and less educated groups (Macaskill et al., 1992).

It is worth nothing that another potential explanation for the insignificant effect on smoking is the mortality selection of the less educated. One might argue that people with lower education who smoke more might die disproportionately often, or move into hospital, and this would give the mere appearance that education does not matter much to smoking behaviors due to the composition of those who remain in the sample. This is particularly difficult to rule out for the very old cohorts in our sample.

What may account for the differences between our results and those of Clark and Royer (2013)? One possible explanation lies in the different approaches used in the two studies. For example, we were unable to follow Clark and Royer's approach of using month-of-birth to set up our IVs, which could have led to estimates that are relatively less precise than if they were estimated using treatment status that was assigned at the precise month that each law was first introduced in each state. Another potential explanation is that our set of IVs strongly predicts the probability of individuals completing at least a university degree (see *F*-test in Table 6), whereas changes in the school leaving age laws in the UK have been shown by Clark and Royer not to predict any significant increase in educational attainment far beyond the secondary school level, which may partly explain why we find any effects at all for Australia. Note that school leaving age on its own (i.e., without interacting it with birth years) raises the probability of completing at least a university degree by approximately 4.2%, and this is statistically significant at the 5% level.

If more education causes people to adopt better health habits for certain types of health-related behavior, can we also see its benefits being picked up in people's health outcomes? Table 7 shows that education has a positive impact on people's overall evaluation of health (e.g., self-assessed health, SF-6D, and physical functioning). It however does not reduce the probability of individuals reporting to have any long-term health conditions and serious illnesses, nor has a positive effect on people's mental health. Although not all of these results can be attributed to the improvements in health-related behaviors brought about by education, they are consistent with the overall findings that there are some significant health benefits to obtaining more education, at least in Australia.

Table 6
Different measures of education.

Dependent variable: HEI	OLS		
	(1)	(2)	(3)
Left at aged 14	−0.297*** [0.047]		
Age left school		0.130*** [0.010]	
Completed at least a university degree			0.454*** [0.026]
Dependent variable: HEI	**IV-GMM2S**		
	(4)	(5)	(6)
Left at age 14	−0.252 [0.178]		
Age left school		0.251*** [0.018]	
Completed at least a university degree			0.623*** [0.130]
F-test on the excluded instruments:			
F-statistic	2.1E+09	1.2E+09	4.30E+09
Hansen J statistics	43.202	40.055	44.534
p-value	[0.4627]	[0.5998]	[0.4070]
Observations	9013	9013	9091

Note: ***<1%. See notes for Table 1.

5. Conclusion

This paper provides some of the first empirical evidence of the causal links between education and health habits in Australia. Using an exogenous variation in people's education generated by differences in the compulsory schooling laws across Australian

Table 7
Health outcomes regressions.

	(1) Self-assessed health: excellent health	(2) Any long-term conditions	(3) Any serious illness	(4) SF-6D: quality-adjusted life year	(5) SF-36: mental health	(6) SF-36: physical functioning
a) OLS						
Years of education	0.015*** [0.000]	−0.021*** [0.000]	−0.020*** [0.001]	0.048*** [0.005]	0.038*** [0.006]	0.073*** [0.004]
b) IV-GMM2S						
Years of education	0.017*** [0.005]	−0.013 [0.017]	−0.010 [0.015]	0.113*** [0.038]	−0.019 [0.023]	0.116*** [0.020]
F-test on the excluded instruments:						
F-statistic	4.1E+07	7.1E+07	542.39	4.7E+06	2.9E+08	3.1E+07
Hansen J statistics	40.927	40.311	45.515	48.512	42.047	39.819
p-value	[0.5616]	[0.5886]	[0.3678]	[0.2960]	[0.5125]	[0.6100]
Observations	9048	9892	5035	8760	9120	9031

Note: ***<1%. Health outcomes in columns (1) (2) and (3) are dichotomized variables; the other health outcomes are standardized with mean zero and a standard deviation of one. See also notes for Table 1.

states and birth cohorts, we show that more education improves people's diet and their tendency to have more regular exercise but not necessarily their tendency to avoid risky health behaviors (e.g., smoking and drinking) or the tendency to undergo more preventive health checks. This is somewhat different from the evidence generated by studies that indicate no effect at all (Braakmann, 2011; Kemptner et al., 2011; Clark and Royer, 2013), but is in line with the few studies in other countries such as Sweden and South Korea that indicate a substantial positive education effect (Park and Kang, 2008; Spasojevic, 2010). This may reflect differences in the education and healthcare systems or an interaction between these two systems across different countries. Further cross-country comparisons are thus warranted to provide a better understanding of the environment in which education may exert a sizable effect on health-related behaviors and health outcomes.

The current study is not without shortcomings. One particular limitation stems from the lack of a much larger sample of relevant birth cohorts in the HILDA surveys required for an analysis based on much weaker identification assumptions, e.g. the RD approach carried out by Clark and Royer (2013). Another limitation concerns the self-reported nature of the health-related behavioral variables, which may impart upward bias into our estimates if people with more education tend to overestimate their healthy habits. There is probably no way to reject such concerns definitely, but at least latest research in this area finds no evidence of systematic reporting bias in self-assessed health for people with different levels of education (Lindeboom and van Doorslaer, 2004). Nevertheless, future research may need to incorporate more objective measures of health behaviors – e.g., assessments of food intakes by nutritionists – into the analysis of the education effect.

Appendix A. Supplementary data

Supplementary data related to this article can be found at http://dx.doi.org/10.1016/j.socscimed.2014.07.021.

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