The Causal Effect of Education on Obesity: Evidence from Compulsory Schooling Laws

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Abstract
Obesity is one of the fastest-growing health concerns in developed as well as developing countries. Raising general education levels is one of the primary public interventions suggested to address this issue. Much is known about the positive correlation between education and health outcomes; less about the causality. This paper investigates the relationship between obesity and education in an Instrumental Variables (IV) framework that uses the variation caused by state-specific compulsory schooling laws between 1914 and 1978 as an instrument for education. Examining data from the first two waves of the NHANES I find a strong and statistically significant negative effect of additional schooling on Body Mass Index (BMI) measures, larger than OLS estimates imply. The effect on females is especially pronounced. These results are robust to weak instruments and various other validity checks, and suggest that policies designed to increase years of schooling for at-risk populations might lead to substantial health improvements.

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I. Introduction

Obesity rates have roughly doubled in the United States over the past 25 years, to around 30% of the adult population - over 60 million people. Sixty-five percent of the population is either overweight or obese (CDC, 2006). Increases in weight beyond regular levels are associated with a higher incidence of cardiovascular diseases, diabetes, hypertension, and similar health problems. The direct and indirect costs of this epidemic have been estimated at $117 billion annually (NIH, 2006)[2]. Childhood obesity has grown alongside adult obesity, storing up problems for the future. By 2020 more than 40% of the US population is predicted to be obese, and 70% overweight (Ruhm, 2007).

This raises several questions - what has caused this surge in obesity, and is there scope for public intervention to remedy it? The most frequently proposed policies are education, taxation, and fast-food regulation, as well as a plethora of minor interventions modeled on the campaign against cigarette smoking (Philipson and Posner, 2008). Education comes in different forms: nutrition labeling; advertising on the health consequences of obesity; programs that teach correct nutrition and exercise; and general education in the form of additional formal schooling. Philipson and Posner (2008) argue that most of these policies have failed to work; for instance, the labeling of food products with nutritional information expanded vastly over the past decades, while obesity rates continued to climb. This paper sheds some light on the likely impacts of a policy focused on increasing general education by examining the link between schooling and obesity.

Measures of education and health have a strong positive correlation even after controlling for several measures of socio-economic status, such as income (Cutler and Lleras-Muney, 2006, survey several studies identifying this correlation). But it is not clear whether this relationship is causal. The purpose of this paper is to examine whether education has a causal impact on health; specifically on measures of obesity.

Uncovering causal effects of education on obesity requires an exogenous change in education; i.e., a variation in schooling that is uncorrelated to changes in obesity. In a simple regression of obesity on education the causal impact of education might be confounded by omitted variables, such as personal motivation or time preference, which

[2] Using 1995 data.

can influence both education and obesity. This paper will use compulsory schooling laws (CSLs) as instruments for educational attainment. These schooling laws placed restrictions on the minimum amount of schooling needed before teenagers could apply for a work permit, and changes in these laws forced some people to stay in school longer than they originally planned. If education does have a causal impact on health, we would expect these people to be healthier later in life.

To preview the results, the first stage suggests that compared to a CSL of 6 or fewer years, stricter CSLs caused increases in average educational attainment of 2 months (for a CSL of 7 years) to 9 months (for a CSL of 9 or more years). These results are robust to a careful investigation of the presence of weak instruments. In the second stage, it is estimated that a one-year increase in schooling leads to a reduction in the Body Mass Index (BMI) of about 1-4%, and a decrease in the probability of obesity of 2-4 percentage points. These effects are stronger for females than for males, and about three times larger than the corresponding OLS estimates. Repeating the analysis with a different set of schooling laws leads to similar results. These estimates are consistent in the face of various validity checks. My results suggest that public intervention aimed at increasing general education levels might lead to a substantial reduction in the prevalence of obesity. Such a policy is especially attractive given the various other returns to education (such as higher wages) and the large cost of state-of-the-art medical technology (Clark and Royer, 2008).

This paper is connected to two large and related fields of work: one that is studying the causes of the recent surge in obesity and policies to address it; and another that is examining the relationship between education and health.

The literature on obesity is vast and ranges from descriptive studies and papers studying various potential causes to examinations of public interventions; for concise surveys see Philipson and Posner (2008), Finkelstein et al. (2005), and Rashad and Grossman (2004). Like most other health variables, obesity is strongly correlated with education – more schooling is associated with a lower incidence of obesity (Baum and Ruhm, 2007; Rashad et al., 2006). It is less clear whether this relationship is causal. Moreover, since educational attainment for the general population has, if anything, increased since the early 1980s, it cannot explain the rise in obesity. However, if there is a causal relationship between schooling and obesity, public policy aimed towards increasing general education for at-risk groups might yield significant health benefits.

A related set of literature has long established a strong correlation between education and various health outcomes (Cutler and Lleras-Muney, 2006). There are three possible ways to explain the correlation between schooling and health: (1) more schooling leads to improvements in health; (2) better (or worse) health influences the amount of schooling people receive; and (3) some third variable drives both education and health, such as ability, parental characteristics or “time preference”, as argued by Fuchs (1982). While the evidence on correlation is strong, studies using (quasi-) natural experiments have only recently begun to try to uncover the causal relationships. Most of this work uses various instruments for education to generate an exogenous change in schooling and examine its impact on different health outcomes.

The first wave of papers, such as Berger and Leigh (1989) and Leigh and Dhir (1997), used parents’ schooling, ancestry, or education expenditures in state of residence in childhood as instruments, among others. Blood pressure, smoking rates, and exercise levels are used as dependent variables, and schooling is often, if not always, found to be statistically significant. While these instruments can describe educational attainment well, they are also arguably correlated with children’s health and other variables, such as state health expenditures, that can affect health.

The idea of using compulsory schooling laws as an instrument for education was first suggested by Angrist and Krueger (1991). The instrument in their paper, quarter-of-birth, was criticized as potentially invalid and “weak” (low correlation with education). Recent literature has tried to work around these problems by using the compulsory schooling laws directly as instruments. Acemoglu and Angrist (1999) examine the social returns to education; Goldin and Katz (2003) study the expansion of high-school enrollment in the 1920s; Lochner and Moretti (2004) investigate the relation between education and crime rates; Oreopoulos et al. (2006) study the intergenerational effects of schooling. All these papers successfully use compulsory schooling laws as instruments for education.

To date, only a handful of papers have looked at CSLs and health. Adams (2002) uses data from the first wave of the Health and Retirement Study in 1992. The health measure is "functional ability"[3], and he uses quarter-of-birth dummies as instruments (like Angrist and Krueger, 1991). His results suggest that education does have a causal impact on health, and the effect is somewhat stronger for women. The statistically significant IV results are between 50% and 100% larger than the OLS coefficients.

Black et al. (2004) investigate the impact of compulsory schooling on teenage births in Norway and the US. They find large, negative effects; for instance, a minimum drop-out age of 17 years reduces the probability of a teen birth by 8.8%. They do not include OLS results.

Lleras-Muney (2005) uses U.S. mortality rates as the health variable. She finds that one additional year of education lowers the probability of dying in the next 10 years by about 3.6 percentage points, compared to 1.3 percentage points using OLS. The standard errors are too high, however, to reject the hypothesis that the OLS and IV coefficients are the same. Additionally, her estimates are reduced by a factor of four when state-specific time trends are included in the first-stage equation (Mazumder, 2007).

A few studies using international data reach similar conclusions (see Kenkel et al., 2006, p640; Grossman, 2004). The combined results from these papers suggest a causal impact of education on health (Grossman, 2004, p633).

An apparent exception is Clark and Royer (2008), who study the effects of an increase in the minimum school leaving age from 14 to 15 in the UK in 1947 on mortality and select health outcomes. This policy affected almost 50% of the education distribution and lead to an increase in completed education levels by about half a year. Their reduced-form results suggest a weakly positive and statistically insignificant effect on mortality (a *higher* rate of mortality as a consequence of more education). They find large, negative OLS effects and small, negative IV effects on the proportion of people reporting "fair" or "bad" health. These results are in contrast to what previous research has found for the US, and they speculate that differences in health insurance coverage or more general cultural attributes might be responsible (additionally, their small sample sizes do not allow precise estimates for outcomes other than mortality).

[3] "Functional ability" is a self-reported measurement of how well people are able to complete certain tasks. Tests show that these measures are fairly reliable indicators of health status.

Only a very small set of papers has examined the causal impact of education on obesity. Kenkel et al. (2006) use the NLSY79 to investigate the relation between high school completion and smoking and obesity rates, with a set of self-constructed instruments based on the costs and difficulty of high school graduation and GED certification. Their OLS results suggest that, for men, graduating actually increases the probability of being overweight significantly (the coefficients for women are negative, but not statistically significant). IV coefficients are even larger, but not statistically significant. They also find that parents' schooling has a strong negative effect on being overweight, especially for females.

MacInnis (2008) uses a change in drafting procedures for US males during the 1960s to estimate the effect of college education on health. The pre-lottery Vietnam draft led to increases in college enrollment and completion of 3-4%. He finds that college completion reduces the probability of being obese by 70%, and would save about $44,000 in reduced medical costs per person.

Zhang (2008) analyzes the effects of early school entry on youth obesity in the NLSY97. She finds that delayed school entrance decreases educational attainment by up to a year and increases the probability of being overweight by 10 percentage points (for girls; the effect on boys is small, negative, and statistically insignificant).

Clark and Royer (2008), mentioned above, also examine the impacts of the UK 1947 law change on BMI and obesity rates. While the OLS results are strong and negative, the IV estimates are positive and statistically insignificant (small for BMI, large for obesity).

In this paper I contribute to these literatures in the following ways: I use a well-established instrument for education in a new context – to estimate the effect of schooling on obesity. I use U.S. data with detailed individual schooling information and measured, rather than self-reported, obesity variables. This yields the most credible estimate to date of the causal impact of schooling on obesity. I also stratify my results along various dimensions, including gender and race, with interesting results. Finally, I perform various validity checks, including a detailed discussion of weak instruments.

II. Data and Empirical Approach

The estimation framework for my instrumental variables regression can be written as follows:

(1) $$Educ_{isc} = \gamma CSL_{sc} + \phi_1 X_{isc} + \delta_{1s} + \delta_{1c} + \varepsilon_{isc} ,$$

(2) $$Y_{isc} = \beta Educ_{isc} + \phi_2 X_{isc} + \delta_{2s} + \delta_{2c} + \nu_{isc} .$$

Equation (1) describes the first stage of the two-stage least squares estimation (2SLS). Individual levels of schooling are regressed on the compulsory schooling laws, where *i* denotes individuals, *s* state-of-birth and *c* cohorts (measured by year-of-birth). *X* captures other covariates such as percent female and percent nonwhite. The δ's capture state-of-birth and year-of-birth (cohort) fixed effects, and ε and ν describe the error terms. The construction of the compulsory schooling variable CSL is discussed below.

Equation (2), the second stage, uses the predicted levels of education from the first stage to estimate the effect of schooling on the variable of interest *Y* (in my case, measures of obesity).

My results are based on the first two waves of the National Health and Nutrition Examination Survey (NHANES), conducted from 1971 to 1975 and 1976 to 1980, respectively.[4] NHANES1 interviewed and examined about 32,000 people aged 1 to 74, with a special emphasis on groups thought to be at risk of malnutrition, such as the poor, elderly, and pregnant women. NHANES2 examined about 28,000 people from 6 months to 74 years of age, with a focus on children and the poor. In both waves a subset of persons aged 25-74 received a more detailed medical exam; the data on height and weight that I use for my analysis are from the full "nutrition" survey. The information on basic demographic variables such as age, race, and income was collected through household surveys; the height and weight measurements were taken during general medical

[4] A third wave of examinations took place from 1988 to 1994. Since then, NHANES has been conducted bi-annually. The NHANES3 lacks place-of-birth data and can therefore not be used in my analysis. More information on the data sources is contained in Appendix A.1.

examinations and, as such, should not be subject to biases commonly found in self-reported data (McAdams et al., 2007; Chou et al., 2004).

The choice of data set was determined by the availability of some key variables: state-of-birth and year-of-birth (needed for the application of compulsory schooling laws), and height and weight (to derive measures of obesity). Since most of the variation in the schooling laws occurred from 1910 to 1940, the sample would ideally include a large number of people born between 1900 and 1930. The Census could provide these numbers, but unfortunately it lacks variables related to obesity. Other datasets, such as the NHIS, PSID, BRFSS or NLSY either lack state-of-birth, or do not go back in time far enough.

I use the BMI (Body Mass Index) as a measure of health, defined as weight in kilograms divided by height in meters squared. Overweight is defined as having a $BMI \geq 25$, and obesity as having a $BMI \geq 30$ (NIH, 2006; notice that overweight includes obese in these definitions).[5]

The data on the compulsory schooling laws were taken from Oreopoulos et al. (2006), which are the same as used in Acemoglu and Angrist (1999)[6], as well as from Oreopoulos (2008). The laws covered a variety of age, schooling, and work restrictions, but only a few of those had a real impact on individual educational attainment. Following the literature, I combine these different measures into a single variable that captures the minimum amount of schooling necessary for a work permit (the main alternative to being in school was to be working).

This variable is defined as

CSL = maximum {required years of schooling before receiving work permit,
(minimum age for work permit – enrollment age)}.

This definition captures the cases where a separate law regarding the required years of schooling for a work permit was in place. While these laws were seen as ineffective

[5] Since BMI does not account for variations in muscle mass or the distribution of body fat it is considered less accurate than other measures of obesity such as waist circumference (see Cawley and Burkhauser, 2006). None of these are available in the NHANES.

[6] These data are themselves similar, but not identical, to the ones used by Lleras-Muney (2005).

until 1915, they were widely enforced afterwards, but declined again in importance after the 1940s (Lleras-Muney, 2005; Schmidt, 1995). Law data are available from 1914 to 1978.

CSL is a categorical variable with values between 0 and 12. Instead of including it in raw form, the literature has taken the approach of splitting it into 4 dummy variables as follows:

CSL6 for CSL $\leq$ 6 years,
CSL7 for CSL = 7 years,
CSL8 for CSL = 8 years,
CSL9 for CSL $\geq$ 9 years.

About 90% of the sample is in the 6-9 years range (see Table 1), so this captures most of the variation. It also permits a nonlinear relationship between the laws and educational attainment. The variables used for instrumenting are thus CSL7, CSL8, and CSL9, with CSL6 being omitted.

Each person in the sample is matched to the CSLs that were in place in their state-of-birth when they were 14 years old. Again, this has become the norm in the literature, based on the observation that 14 years is the lowest common drop-out age across states. This approach assumes that individuals went to school in their state-of-birth, which might result in some mismatching. As Lleras-Muney (2005) notes, however, mobility across states was low during this period, and probably also uncorrelated to the laws.

To get an impression about the fraction of my sample that was actually affected by compulsory schooling laws, it is useful to look at a cross-tabulation of educational attainment and the minimum number of years of schooling required as stated in the laws. This is done in Tables 2a/b/c. Values along the diagonal indicate the total number of people that had education levels exactly at the constraint imposed by the schooling laws. The values in brackets describe this number as a fraction of all people that had the same compulsory schooling constraint (the column sums). Summing along the diagonal of Table 2b we can infer that about 1319/11874 $\approx$ 11% of adults in the NHANES1 were directly affected by the compulsory schooling laws in their state.[7] The corresponding number for the NHANES2 is about 7.8%, and 9.5% for the combined dataset (see Tables 2c and 2a). This fraction is quite low, but very similar to rates found in other studies using US data and different schooling instruments (Oreopoulos, 2006b, p153).

I also use a different set of schooling laws taken from Oreopoulos (2008) that focuses on the minimum school leaving age (*drop_age*). These laws are available from 1914 to 2005 and would thus allow me to extend my observation period; however, I am still restricted by the availability of the NHANES data (I only observe adults - aged 18 or older - between 1914 and 1976). The minimum school leaving age varies between 12 and 18 years over that time period; it is equal to 16 years for 75% of the sample, however.

To what extent are compulsory schooling laws a valid instrument for education? There are several requirements. First, they must satisfy the exclusion restriction, i.e. we need to be sure that they do not influence obesity directly. This seems reasonable. The laws just placed restrictions on the amount of schooling required; they did not include provisions for, say, school lunches, or other programs that would affect health. The exclusion restriction would also be violated in the case of reverse causality, i.e. if changes in the compulsory schooling laws were implemented as a result of changes in the average level of education within states. This is difficult to rule out. I address this problem by presenting some graphical evidence suggesting that educational attainment did indeed follow changes in the CSLs, and not vice versa (see section A.2 in the appendix).

Second, the instruments should be "strong", i.e. they should be highly correlated with the variable they instrument for, in this case educational attainment. This is commonly judged by examining an F-test on the instruments in a regression of the endogenous variable on the instruments (the first stage), as well as the partial R^2. Bound et al. (1995, 1996) suggested that this F-statistic should be large and statistically significant; Staiger and Stock (1997) provided a rule-of-thumb stating that an F-statistic of less than 10 could signal weak instruments. Results for my regressions are presented below.

[7] Out of those with more than nine years of schooling, who make up the majority of my sample, 81.8% ended up with a high school degree or more (49.7% with just a high school degree).

Figure 1 shows the number of states with schooling laws mandating 7 or fewer, 8 or fewer, and 9 or fewer years of schooling (based on Table 1 of Oreopoulos et al., 2006).[8] It shows a clear downward trend, as states raised their minimum schooling requirements. About two-thirds of the variation in the laws is due to changes within states over time (Oreopoulos et al., 2006, p739). Since my regressions will include state-of-birth dummies, I rely on this within-state variation for identification.

The NHANES1 surveyed and examined about 32,000 people. However, height and weight information is only available for 23,808 individuals. 3,059 observations have to be discarded due to missing sample weights, and another 3,216 due to incomplete information on educational attainment. As mentioned above, I have data on compulsory schooling laws from 1914 to 1978, which requires individuals with birth years between 1900 and 1964. Law data are also not available for Alaska and Hawaii, as well as for foreign-borns. Dropping observations with missing CSL data results in a sample size of 15,315. Since the BMI is only defined for adults, the estimation sample is restricted to people aged 18 or older, which leaves a final sample size of 11,874. Similar steps reduce the effective sample size of the NHANES2 from 20,322 to 11,214.

III. Results

Sample statistics

Tables 3A and 3B contain sample statistics for the estimation sample (aged 18 or older), using the weights provided in the NHANES. Approximately 50% of males and 39% of females are classified as overweight in the two waves of the NHANES, proportions that have risen by about 10 percentage points each since then (compared to estimates from the 1998 NHIS).[9] Basic demographic variables correspond well to the values reported in the 1970 Census, which are listed as comparison in the last columns of Table 3A. About 70% are married, and about 30% have less than a high school degree.

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[8] The reduction in the number of states with 9 or fewer after 1945 is due to many states requiring more than 9 years of schooling.
[9] The incidence of obesity is roughly the same across the first two waves of the NHANES. The NHANES2 (1976-1980) was conducted immediately following the NHANES1 (1971-1975).

The fraction of non-whites is small in all samples, around 11%. The comparison of income variables is problematic since the NHANES only provides income brackets. Nonetheless, the imputed income variables for the NHANES are quite similar to the Census numbers.[10]

Table 3B splits the samples by gender and race and reveals that obesity and overweight rates are substantially higher for nonwhite females than for white females. Almost a third of the female nonwhite population has a BMI exceeding 30. This difference is highly statistically significant (t-statistic = 12.5).

First Stage

In the first stage of the Two-stage Least Squares Estimation (2SLS) I regress educational attainment on the compulsory schooling laws to determine the power of the instruments. Table 4 contains the results. The regression includes fixed effects for state-of-birth and year-of-birth, as well as dummies for race, sex, marital status, and SMSA. Standard errors are clustered by state-of-birth and year-of-birth, as well as by state-of-birth only.

The coefficients on the CSL variable are easily interpreted: Consider, for instance, CSL9 in the NHANES1 column of Table 4. Someone born in a state whose compulsory schooling laws required 9 or more years of schooling would acquire almost a full additional year of completed education (87.5% of a year), compared to someone born in a state with a requirement of 6 or fewer years (CSL6 is the omitted dummy variable).

The coefficients are monotonically increasing, as expected: an increase in years of required schooling should lead to a rise in educational attainment. In addition, CSL7 is never statistically significant, CSL8 sometimes, and CSL9 always. The F-statistic on the instruments is between 9 and 14, strongly statistically significant but slightly lower than in other CSL papers, and close to the rule-of-thumb threshold of 10 mentioned earlier. If the standard errors are clustered by state-of-birth alone the F-statistic drops to between 3 and 5. This is suggestive of weak instruments, an issue that I examine in more detail in

[10] Since the NHANES only provides brackets for total family income, I assign midpoints (the weighted mean of which is reported as “Family Income”) and divide by household size to get an imputed personal income variable (“Per capita Income”).

the robustness section. In that section I show that all results are robust to techniques that allow for weak instruments.

The additional controls have the expected signs; e.g., educational attainment is lower for non-whites and higher for urban residents. They also compare well with the results from a similar first stage using Census data from 1960 to 1980.[11]

Columns 2, 4 and 6 (Table 4) report the results from a linear probability model, where the dependent variable is an indicator for educational attainment exceeding 12^{th} grade. We do not expect the CSLs to have an impact on receiving more than a high school education, and this presumption is confirmed by the results – the coefficients on the laws are very small and statistically insignificant.

OLS and IV

Table 5 contains the OLS and IV results, with BMI as the dependent variable.[12] The OLS coefficient is highly statistically significant and suggests a modest decrease in BMI for an additional year of schooling (0.13 units or 0.4%). Assuming a height of 1.8m, an extra year of schooling would reduce weight by about 0.4 kgs.[13] This is consistent with results from the epidemiological literature (Leigh et al., 1992; Wardle et al., 2002).

BMI values are on average lower for females; presumably because they tend to have a lower weight than men, given the same height (Halls, 2008). Nonwhites have a substantially larger BMI than whites, while urban residence is associated with a slightly lower BMI. Standard errors clustered by state-of-birth only are very similar to those clustered by state and year.

Turning to the IV results, we notice that the coefficient on education is almost six times as large as in the OLS case in the NHANES1, and about three times as large for the combined NHANES. This is somewhat surprising, as we would have expected the IV result to be lower (in absolute value) than the OLS estimate, and will be discussed in more detail below. A one-year increase in schooling is now associated with a decrease in BMI of 0.41 units or 1.4%, which equals a loss of 1.3 kgs for a person of height equal to

[11] Results available upon request.
[12] Reduced form results are presented in section A.3 of the appendix.
[13] Since BMI = (weight in kg)/(height in meters)^2 and assuming an average height of 1.8meters, a one-year increase in education would lower a person's weight by 0.128 * 3.24 = 0.4 kgs (using the result for the combined NHANES).

1.8m. The median weight for a person of this size is 76kg, so this represents a 1.7% reduction in weight (using the Combined NHANES results).

The changes in the coefficients of Black and SMSA are also of interest. Both are now smaller for the IV than for the OLS regressions and no longer statistically significant. To the extent that they are proxies for education (Black signaling lower education on average, urban residence higher education), their contributions to BMI measures are now subsumed in the education variable, thereby explaining their insignificance in the IV regression.

Lastly, Table 5 presents two tests commonly used in the IV context. The underidentification test is a test of instrument relevance, i.e. whether the instruments are correlated with the endogenous regressor. Under the null hypothesis the regression is underidentified. This is clearly rejected in my case. Hansen's J Statistic represents a test of overidentifying restrictions, where the null hypothesis corresponds to valid overidentifying instruments (uncorrelated with the error term) and correct exclusion restrictions. A rejection casts doubt on the validity of the instruments. In my case the null hypothesis is not rejected.

IV. Discussion

Why are the IV results so much bigger than the OLS estimates? As a recent survey by Grossman (2004) pointed out, this is actually a feature of virtually all papers that use CSLs and similar instruments in regressions of health on education. He lists three possible reasons:

1. If we assume that the health returns to education vary between individuals, the IV estimate will reflect the rate of return of the group that is most affected by the policy change used as an instrument. In other words, the IV estimate reflects the changes in health experienced by those who were primarily affected by

compulsory schooling laws, those with low levels of education[14]. For policy purposes this impact might even be of more interest than the one for the whole population.

Oreopoulos (2006b) studies this discrepancy between Average Treatment Effects (ATE; the average effect for the whole population) and Local Average Treatment Effects (LATE; the average effect on those influenced by the instrument) using data from the UK. His analysis suggests that these two parameters are closer than commonly thought, and that the difference between OLS and IV estimates of the rate of return to schooling is probably not due to differences in the population group affected by the instrument. This has not yet been examined in the US context, however.

2. If there is random measurement error in the education variable, the OLS estimates will be biased downwards. If the compulsory schooling laws are not correlated with this error, using IV will remove this bias.
3. There might also be spillover effects, implying that a person's health might not only depend on his or her own education, but also on the schooling levels of individuals living in his or her area. Currie and Moretti (2003) find that OLS analysis tends to underestimate this effect.

Stratification

Table 6 reports OLS and IV results for the Combined NHANES stratified by various demographic variables. The most striking result is that the effect of education on BMI is much stronger for females than for males. Using the IV result, for a female of average height (1.62m) a one-year increase in schooling would reduce weight by 2.7 kgs, or about 4% (at a median weight of 66kg). For males, the coefficient is both much smaller and statistically insignificant; in fact, even the OLS estimates are negligible and statistically insignificant for males.[15] This is consistent with previous literature on differences across

[14] These people, at the same time, should benefit the most from additional schooling, in terms of both health and labor market outcomes. (IV estimates of income returns to education based on CSLs usually find larger rates of return than the OLS models. See Appendix A.6 and fn26. See also Card, 2001.)

[15] Regarding these gender differences, OLS estimates are very consistent across the two different waves. The IV results are much weaker in the NHANES2, however; this might be because a smaller share of respondents was affected by the CSLs than in the NHANES1 (see Tables 2b/c and the discussion on

genders in the correlation between socio-economic status (such as education) and obesity (Sobal and Stunkard, 1989; Baum and Ruhm, 2007).

Probabilities of being overweight and obese

Instead of the continuous BMI variable one can also examine the probabilities of being classified as overweight (BMI ≥ 25) and obese (BMI ≥ 30). Table 7 contains the results from a linear probability model that uses indicator variables for obesity and overweight as dependent variables. For the full sample, an additional year of schooling will decrease the probability of being overweight or obese by about one percentage point in the OLS model. This masks a considerable difference between genders; the impact on males is negligible, while the at-risk probabilities for females are reduced by around two percentage points. The IV results again indicate that the strongest impact is on females; an additional year of schooling is associated with a 6.5 percentage point reduction in overweight incidence and a 4.4 percentage point reduction in obesity incidence, which is two to three times as high as the OLS results. Female Blacks respond even stronger than female Whites, although the standard errors are also quite high due to the small sample size.

Alternative schooling laws

Besides schooling laws that focus on the ability of getting a work permit I also use the minimum school leaving age in each state as an instrument, taken from Oreopoulos (2008). This works as a validity check and also has the potential of increasing the statistical power of my estimates.

Table 8A contains the First-stage results. Regressions in columns marked [1] use the "standard" set of compulsory schooling laws; those marked [2] use the "new" set of laws, and those marked [3] combine both sets. Two things are noticeable: the "new" set of laws works better than the old one in the more recent cohorts represented in the NHANES2; and the combination of old and new laws in the same regression reduces their individual impact but slightly improves standard errors and the F-statistic on the instruments.

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pages 9-10). Still, the addition of the NHANES2 allows a more precise estimation of the IV effects; standard errors are reduced by about 25% in the combined dataset as compared to the NHANES1 (results available upon request).

Table 8B shows the IV results. The estimates are remarkably similar across the three different setups. Standard errors in the combined dataset improve by between 10% and 30% when both sets of laws are included simultaneously, since the "new" laws work better for the cohorts in the NHANES2.

Validity check: Height

Standard OLS estimates indicate a very small, but statistically significant positive relationship between a person's education and his or her height.[16] This is presumably the result of omitted variables, such as early childhood conditions, since an exogenous increase in education should have no impact on a person's height. This omitted variables bias in the OLS estimates should not be present in the IV regression if the instruments satisfy the exclusion restriction. In other words, instrumenting for education should yield a statistically insignificant relationship between schooling and height, and this "falsification test" can be used as another way of gaining confidence in my instrumental variables. Using the original schooling laws and the combined NHANES dataset, there is indeed no significant relationship (both statistically and in magnitude) between height and years of schooling in the IV setup (see Table 9). This strengthens my belief in the validity of the instruments.

Validity check: State-specific time trends

It is possible that the instruments in my analysis are correlated with other state-level changes that affect educational outcomes. One way to account for this is to include linear state-specific time trends. As documented in Table 10a, doing so leaves the IV estimates for the total effect of education on BMI unchanged (although the standard errors are almost three times as large), but generates a reversed sign for the female subpopulation together with enormous standard errors. Moreover, including state-specific time trends greatly diminishes the first-stage power of the instruments, since in most states both the compulsory schooling laws and general educational attainment trended upwards (see Table 10b, and Oreopoulos et al., 2006, p750).

[16] See Table 9. An additional year of schooling is associated with an increase in height of about 3.4mm, or 0.13 inches.

Following Oreopoulos et al.'s (2006) suggestion, I replicate my analysis using a subsample of states where the minimum required years of schooling *decreased*. I construct four different subsamples consisting of all state/year cells that are within range of five, ten, fifteen and twenty years before and after a decline in the minimum schooling requirements. While these states were subject to the same nationwide increase in educational attainment, their compulsory schooling laws changed in the opposite direction, so the IV estimates should be less influenced by an underlying trend. Table 10c contains the results of this exercise. The coefficients derived from the various subsamples are very consistent with the baseline result using the full sample of law changes. Indeed, the results for females are stronger in the subsamples, and get weaker as the time window expands and upward law changes are included.

Weak instruments

If the correlation between the instruments and the endogenous regressors is low, the instruments are considered to be "weak". To identify weak instruments, Staiger and Stock (1997) proposed to examine the F-statistic on the instruments in the first-stage regression. An F-statistic of less than 10 is indicative of weak instruments. Tables 4 and 8A display F-statistics between 5 and 9, and therefore suggest that weak instruments might be a problem.

Assuming that the instruments are valid (satisfying the exclusion restriction), the IV estimator is still consistent. However, if the instruments are weak, the IV estimator will be biased in small samples, and its distribution will not follow standard asymptotic theory, thereby invalidating standard inference.

Several estimators have been suggested to address the issue of finite sample bias (see Hahn et al., 2004, and Andrews and Stock, 2005, for recent surveys), such as the Limited Information Maximum Likelihood estimator (LIML), Nagar, Jackknife IV, and Fuller[17] estimator (which is among those that perform the best). Table 11a contains the results.[18] It is noticeable that the reported coefficients, as well as the standard errors, are very

[17] The Fuller estimator (Fuller, 1977) is a variant of the LIML estimator, designed to have finite sample moments. When using the Fuller estimator the researcher has to choose a parameter $a > 0$. Following the literature, I chose $a = 1$ (which yields a higher-order mean bias of zero) and $a = 4$ (yielding a nonzero higher mean bias but a smaller MSE).

[18] All weak instrument regressions were performed with the STATA addon *ivreg2* (Baum et al., 2007).

similar for all the five different estimators, and across the two samples (full and females only). This suggests that finite sample bias due to weak instruments is not a problem.

The table also reports the test statistics and critical values for a more recent test for the presence of weak instruments, due to Stock and Yogo (2002). The "KP F-stat" is the Kleibergen-Paap *rk* statistic[19] – an F-statistic adjusted for non-i.i.d. errors – on the instruments in the first stage. The various critical values correspond to different definitions of "poor performance" of IV estimation in the presence of weak instruments. For instance, the instruments are considered weak if the bias of the IV estimator relative to the bias of OLS exceeds a certain threshold, say 10%, i.e. when the first-stage F-stat is less than the critical value of 9.08. These tests mostly fail to reject the null of weak instruments (although for the full sample I can rule out more than 20% relative bias).[20]

Weak instruments also lead to incorrect inference. Even the standard errors generated by more robust estimators, such as the Fuller estimator, are not correct in the presence of weak instruments. Several ways have been suggested to construct tests and confidence intervals that are robust to the presence of weak instruments; see Andrews et al. (2007), Andrews and Stock (2005) and Chernozhukov and Hansen (2005).

Table 11b reports coverage-corrected confidence intervals and p-values for 2SLS and LIML regressions, based on the conditional likelihood (CLR) approach developed by Moreira (2003), as well as the Anderson and Rubin (AR; Anderson and Rubin, 1949) and Lagrange Multiplier (score) tests (LM; Kleibergen, 2002; Moreira, 2001).[21] The CLR test dominates both the AR and the LM test (Andrews et al., 2006) and is therefore the most preferred test. The intervals were constructed without sample weights, and assume i.i.d. errors. Table 11b therefore also reports the basic regression results without weights and clustering, to make valid comparisons. The confidence intervals for the full sample suggest that the negative effect of education on obesity is not statistically significant once we take the weak instruments into account; this might change, however, if the interval could be constructed using weighted data. For the female subsample, the strong, negative

[19] See Kleibergen and Paap (2006) and Kleibergen and Schaffer (2007).
[20] It should be noted that the Stock and Yogo (2002) critical values assume i.i.d. errors, which is not the assumption that I use for my regressions.
[21] These confidence intervals were constructed using the STATA addon *condivreg* (Mikusheva and Poi, 2006).

coefficient discovered earlier is confirmed as statistically significant, even in the presence of weak instruments.

A different approach to correct inference in the presence of weak instruments relies on the reduced form. Under the null hypothesis that the endogenous regressor is equal to zero, the exclusion restriction implies that the coefficients on the instruments in the reduced form for Y – where Y is being regressed on the instruments directly – should also equal zero. As Chernozhukov and Hansen (2005) describe, this test is robust to weak instruments, has the correct size and good power, works with weights and under heteroskedasticity, and is very simple to compute. It follows a Chi-squared distribution with degrees of freedom equal to the number of excluded instruments.[22] This test, reported in the last line of Table 11b, agrees with the confidence interval results – there is a strong and statistically significant negative relationship between education and BMI for the female subsample.

In conclusion, while my analysis is subject to the presence of weak instruments, the results are robust.

V. Conclusion

The surge in obesity rates over the past few decades presents a major challenge for public health policy.[23] Several ways have been suggested to address this problem, one of them being an expansion in obesity education and education in general. At the same time, our understanding of the positive correlation between education and health has been augmented by research that uses quasi-natural experiments to establish causality. This research suggests that there is a causal effect of schooling on a variety of health measures.

This paper adds to both strands of the literature by studying the causal effect of education on obesity, using a fairly novel instrument to account for omitted variables bias. Compulsory schooling laws from the first half of the 20th century are shown to have

[22] *ivreg2* reports the same statistic when invoked with the "first" option.
[23] This of course assumes that obesity generates negative externalities that warrant government intervention in the first place, which is a completely separate issue (see Philipson and Posner, 2008, Section 3).

increased average educational attainment by two to nine months.[24] Estimates based on this exogenous increase in schooling suggest that an extra year of education lowers a person's BMI by 1-4%, and the probability of being obese by 2-4 percentage points. This effect is stronger for women than men, and about three times larger than conventional OLS estimates would imply. The estimates are consistent in the face of several robustness checks, such as state-specific time trends and the presence of weak instruments.

These results strengthen the idea that there is a causal pathway from schooling to better health, and suggest that policies aimed at increasing general educational attainment can be an effective tool to lower the prevalence of obesity. Moreover, these health benefits accrue in addition to the positive effects on income and other non-health outcomes usually associated with higher education. It should be noted that the instruments used in this paper have their biggest impact at low levels of education, and it is unclear what the results of such an intervention would be at higher levels of schooling. However, the population subgroups with lower levels of education are also most at risk from obesity and related diseases. Finally, this paper does not address the channels through which schooling affects health (Increases in future lifetime utility? Better cognitive skills? Preferences? Rank in society? See Cutler and Lleras-Muney, 2006). This is an important question left for future research.

[24] More precisely, compared to a CSL of 6 or fewer years, stricter CSLs caused increases in average educational attainment of 2 months (for a CSL of 7 years) to 9 months (for a CSL of 9 or more years).

Appendix

A.1 Data sources

NHANES1
Downloaded from http://www.cdc.gov/nchs/about/major/nhanes/nhanesi.htm, specifically the data file named "Anthropometry, Goniometry, Skeletal Age, Bone Density, and Cortical Thickness, Ages 1-74 yeas, 4111."
Last accessed 6/24/2008.

Race definition: "Mexicans were included with 'White' unless definitely known to be American Indian or of other nonwhite race." (NCHS, 1981, p57).

NHANES2
Downloaded from http://www.cdc.gov/nchs/about/major/nhanes/nhanesii.htm, specifically the data file named "Anthropometry, 5301."
Last accessed 6/24/2008.

See also NCHS (1984).

Compulsory Schooling Laws
Taken from Oreopoulos et al. (2006) and Oreopoulos (2008).

A.2 CSLs and educational attainment

Did the compulsory schooling laws cause an increase in educational attainment, or did the change in legislation follow a general rise in educational outcomes, or economic prosperity? This is an argument about instrument validity that cannot be formally settled (the Hansen J test cited above can only fail to reject validity), and relies more on persuasive arguments and results derived in previous empirical studies.

Oreopoulos et al. (2006) construct a graph that compares parents' educational attainment before and after a law change (p742), and convincingly suggests that increases in educational outcomes were indeed caused by a change in the mandatory schooling laws. I have obtained various versions of the code used to generate this graph, and constructed my own code, but unfortunately I am unable to exactly replicate it.
I nevertheless applied the same code to the NHANES. There are various ways to cope with states that experienced more than one increase in the compulsory schooling laws, or that had declines as well as increases. I chose to only include states that had a single

increase in the minimum school leaving age (*drop_age*) over the entire sample period, which reduced the number of states to 28. The sample is restricted to adults who completed less than 12 years of education. The resulting Figure 2 shows the coefficients on ten yearly dummy variables both before and after the increase in the CSL. Educational attainment is more or less flat before the change, and increases by more than half a year each in the three following periods afterwards, before settling down again. Due to the small number of observations the estimated coefficients are not individually statistically significant.

A.3 Reduced Form

Table A.3 contains the reduced form results, where BMI is directly regressed onto the instruments, stratified by gender and dataset. As expected, the relationships are a lot stronger for females than for males. For males there is often a *positive* impact of an additional year of compulsory schooling on BMI, especially in the NHANES2.

A.4 Subset regressions

Classic linear regressions such as OLS estimate conditional mean functions; they measure the impact on the mean of the outcome variable. It is however easy to imagine that the effects differ in magnitude and significance for different parts of the response variable's distribution. As a crude approximation to this "quantile regression" approach one can generate ordered subsets of the dependent variable based on its unconditional distribution and then run OLS regressions on each subset (this is somewhat troublesome due to endogeneity issues). IV results can be constructed similarly.

The results, presented in Table A.4, are quite interesting. First, there is a positive correlation between education and BMI for the lowest quintile, which becomes negative and increasingly stronger for higher quintiles. This is true for both males and females, and also shows up when the analysis is repeated with terciles. Second, the IV results are mostly negative and larger in absolute terms than the OLS estimates, although the standard errors are too high for any inference. Increasing the size of the subsamples by using terciles further increases the magnitude of the estimates for the high-BMI group, but leaves the general pattern unchanged. Why do we not find strongly statistically significant results? One reason could be that individuals affected by the compulsory schooling laws not only display a lower BMI but are, in fact, pushed into a lower BMI bracket, which weakens the intra-group relationship.

A.5 Survey estimation

The NHANES1 dataset was generated by means of a survey; indeed, it is a "multistage, stratified, probability sample of loose clusters of persons in land-based segments." (NCHS, 1981, p66). Data derived from surveys differ from regular cross-section data in several ways:

- <u>Sampling weights:</u> Different observations can have different probabilities of selection. In fact, the NHANES1 was designed to oversample certain population groups who were thought to be at a higher risk of malnutrition (ibid, p1). Sample weights have to be included to arrive at unbiased estimates of parameters in the full population; they also affect standard errors.
- <u>Clustering:</u> In most survey designs the observations are not sampled independently; rather, they are sampled as a group or "cluster" (e.g. by states, counties, or households). There might be further subsampling within the clusters; the units at the first level of sampling are called "primary sampling units" (PSU). Since observations within clusters are *not* independent, using estimators that assume independence will not produce the correct standard errors (typically, they will be too low).
- <u>Stratification:</u> Strata refer to different groups of clusters that are sampled separately (e.g. the counties in a state may be divided into "urban" and "rural" counties, forming two strata). These strata divisions are fixed in advance, and since sampling is done independently across strata, they are statistically independent. Taking this into account often leads to smaller estimated standard errors.
- <u>Finite Population Correction (FPC):</u> If the survey design used without-replacement sampling, and the number of people sampled is large relative to the total number of people in that stratum, there can be a substantial increase in the

precision of the estimates. The variance estimator should then use an FPC term to reflect that increase in precision.

In short, sample weights can crucially affect parameter estimates, and together with clustering and stratification they are required to produce unbiased and efficient estimates of standard errors. Some of these issues can be addressed using regular, non-survey estimation techniques. Sample weights can be included in normal regressions, and these also allow clustered standard errors. Stratification, however, can not be accounted for outside the survey setup, to the best of my knowledge. As the STATA manual puts it, "[p]ersons with bona fide survey data who care about getting all the details right should use *svy* commands." (StataCorp., 2003, p4). To the extent that stratification and FPCs tend to *reduce* standard errors, however, a "conservative" regular regression using sample weights and clustering would appear to be a good reference point.

How would a proper survey estimation setup affect my results? Since I did include sample weights in all my regressions, the coefficient estimates should be unchanged. I also allowed for clustering at the state and year level. The survey setup uses the PSUs as clusters. The impact of these different definitions of clusters on standard errors is not known a priori. The same is true of stratification, which I have not modeled in my own regressions.

Tables A.5a and A.5b contain the results. The unmodified data set contained 35 strata, 1263 PSUs, and 23,808 observations; my estimation sample contains 35 strata, 1063 PSUs, and 11,874 observations. Standard errors are calculated using Taylor linearization.

Table A.5a presents the results for the first stage. The parameter estimates coincide exactly with the ones obtained earlier (as expected; cf. Table 4). Compared to clustering by state-of-birth and year-of-birth, some standard errors have slightly increased in size (CL8, CL9, Black, SMSA), while others declined (CL7, Female, Married); the F-statistic equals 5.55 compared to 9.44 above. Compared to clustering at the state level only, however, standard errors are lower using the survey setup.[25]

[25] Clustering by state and year yields 2150 clusters; by PSU, 1063; and by state alone, 49.

Turning to the OLS and IV results in Table A.5b, we again see that parameter estimates are identical to those derived using regular regressions (cf. Table 5). Standard errors have increased a little, but overall the results are very similar to the regular regression estimates.

A.6 Income regressions

One way to check the validity of the instruments is to repeat the regressions with income as the dependent variable. IV estimates of the return to education are usually larger than the corresponding OLS estimates (Card, 1999).

The main income variable available in the NHANES is “total family income group”, which consists of 12 income brackets and lists the number of people in each bracket.[26] It measures gross family income over the past 12 months and contains all sources such as wages, salaries, social security benefits and property income. To transform this into a continuous variable I assigned midpoint values to each bracket, divided by household size and took the logarithm. Figure 3 displays a histogram of this income variable; the distribution appears fairly normal.

The regression results are in Table A.6a. For the NHANES1, OLS suggests a rate of return of about 9.6%, highly statistically significant and very similar in size to other estimates of this parameter. Instrumenting with the CSLs, however, gives a small, negative, statistically insignificant coefficient, which is a little puzzling. I stratified the regressions by gender, race, urban/rural, and education, but I cannot see anything that would help explain this result. The results are similar for the NHANES2.

Table A.6b documents two other attempts at explaining the income results. The first two columns use the total family income groups recorded in the NHANES1 instead of my imputed income. While the coefficients are not clearly interpretable, the picture is unfortunately the same – the IV result is negative and statistically insignificant. Columns 3 and 4 display results from the 1970 Census (using cell means with cells based on birthplace, birth year, gender, and race). OLS estimates yield a rate of return of 17.7%, very tightly estimated; the IV rate is 15.6%, with a much larger standard error but still

[26] For households with a total family income per year of less than $7,000 additional income subcategories are available.

very precisely estimated. This is the result we would expect from an income IV regression – a roughly similar parameter estimate (smaller or larger, depending on the relative sizes of the biases that IV corrects for), and higher standard errors.

The only other paper that uses US data and CSLs as instruments (and also lists income regressions) reports IV estimates with a higher rate of return and much larger standard errors than OLS (Acemoglu & Angrist, 1999, Tables 2 and 5).[27] Pischke and von Wachter (2005) look at the introduction of a compulsory 9th grade in Germany between 1945 and 1970. Interestingly, their estimate of the return to education based on this change in schooling is a precise zero (compared to 6-8% using OLS). They speculate that this might be due to the fact that basic academic skills relevant for the labor market are acquired earlier (i.e. before the 9th grade) in Germany than elsewhere. Three other IV papers with German data but using different instruments find rates of return of about 10%, larger than OLS estimates.

One common feature of Pischke and von Wachter's and my results is that in both cases income data are only available in brackets. Increases in income caused by more education might not push a specific person into a higher bracket, which would explain the statistically insignificant, zero coefficient in the IV regression. I tested this idea by constructing artificial income brackets in the 1970 Census; the results are in Table A.6c. The first two entries reproduce the results in the NHANES1; the IV estimates are negative and statistically insignificant for the actual income groups as well as for the continuous income measure derived thereof. The last two entries contain results for a 1% sample of the 1970 Census.[28] The "Imputed income groups" were constructed by dividing "Family total income" into 12 groups whose ranges (in absolute value) correspond to those reported in the NHANES1. To derive "Imputed continuous income", I assigned midpoints to each income group, divided by family size and took the logarithm.

The Census estimates for these imputed income measures based on brackets are quite similar to the ones for the NHANES1. In particular, the IV estimates for the Census are

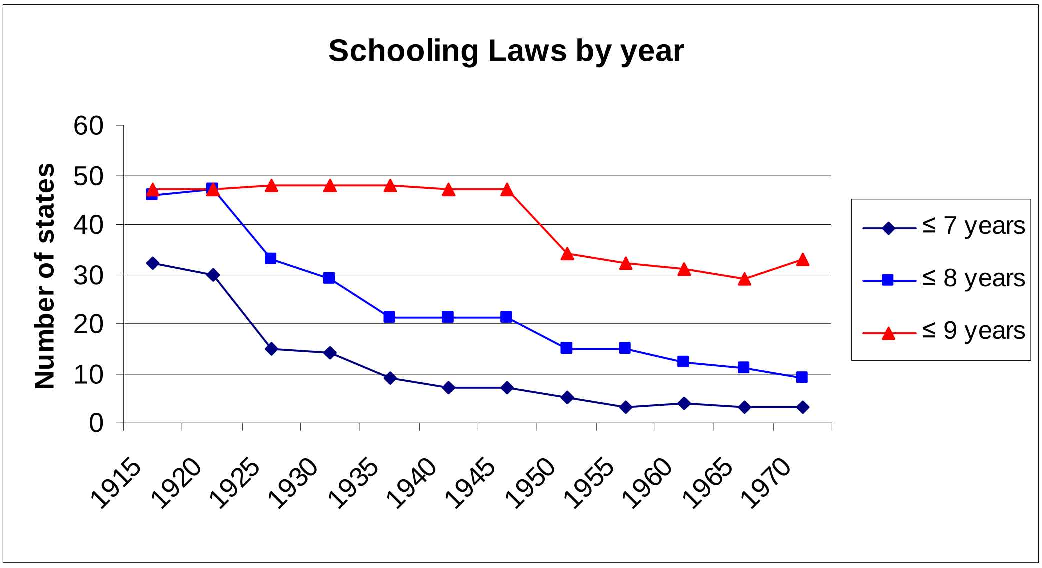
[27] Studies of the UK and Canada have found similar annual gains of 10-15% from compulsory schooling (Harmon and Walker, 1995; Oreopoulos, 2003, 2006a,b).

[28] Collapsing the Census data by cell means removes the income bracket problem; therefore I use an uncollapsed, randomly generated 1% sample. The 1% sample also generates a sample size comparable to the NHANES1.

now statistically insignificant, corresponding to what was observed in the NHANES1. This lends some weight to the idea that it is the presence of income brackets that impedes the usefulness of the chosen instruments to reveal anything about the education-earnings relationship.

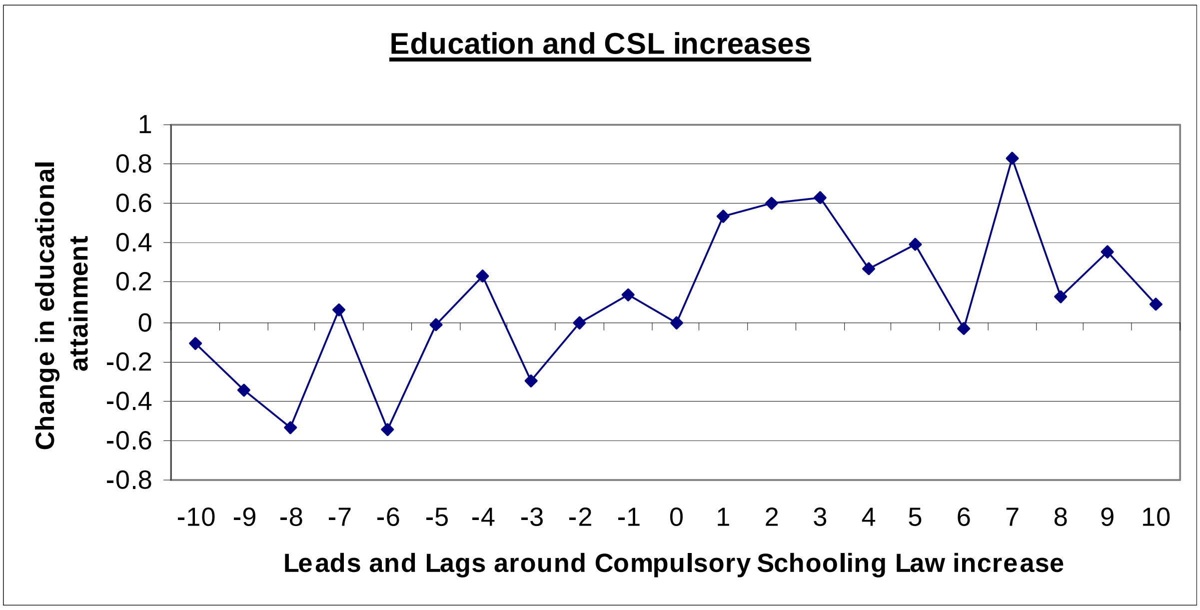
Tables and Figures

Figure 1



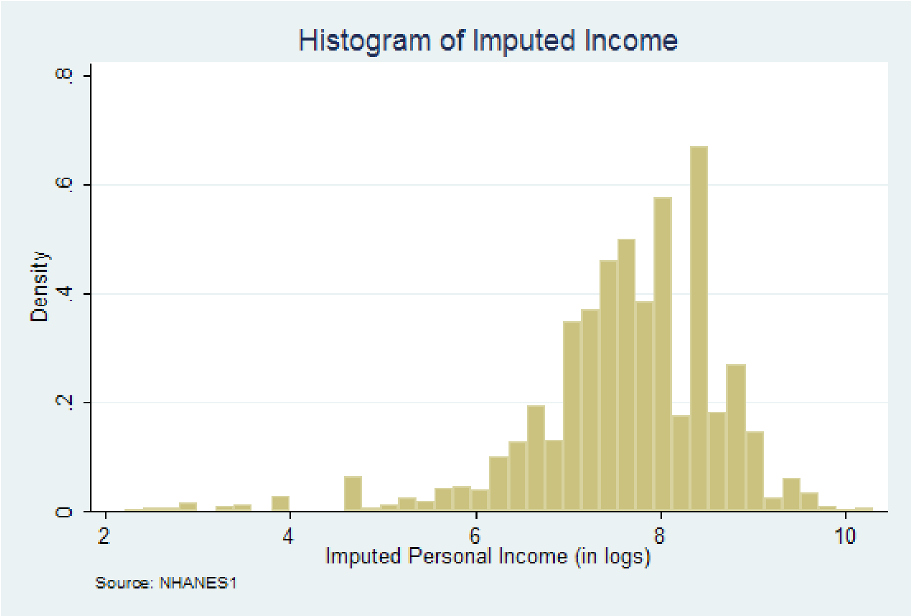
Notes: Based on Table 1 in Oreopoulos et al. (2006).

Figure 2



Notes: Coefficients derived from a regression of highest grade attained on 10 leads and 10 lags around the year of the schooling law increase, based on a sample of 28 states which experienced a single increase over the entire time period. Weighted; aged 18 or older. The regression also included state and year dummies. None of the estimated coefficients is statistically significant individually; the p-value of an F-stat on all 20 dummies is 0.0564.

Figure 3



<u>Notes:</u> “Imputed Personal Income” is derived by assigning midpoint values to each income bracket and dividing by household size.

Table 1: Distribution across *Compulsory Schooling Laws* categories

	NHANES1 (71-75)		NHANES2 (76-80)		1960/70/80 Census	
CSL	#	%	#	%	#	%
0	343	2.24	172	1.34	89,676	1.99
4	78	0.51	41	0.32	24,714	0.55
5	118	0.77	93	0.73	48,976	1.08
6	1,560	10.19	937	7.31	401,112	8.88
7	3,128	20.43	2,816	21.99	997,901	22.09
8	5,757	37.59	4,764	37.20	1,582,577	35.03
9	3,046	19.89	2,827	22.08	992,039	21.96
10	1,125	7.34	951	7.43	333,085	7.37
11	38	0.25	23	0.18	6,658	0.15
12	121	0.79	182	1.42	40,596	0.9
Total	15,315	100	12,807	100	4,517,334	100

Notes: Weighted. The CSL variable measures the minimum amount of schooling in years necessary for a work permit.

Table 2a: Educational attainment at the CSL constraint, Combined NHANES

NHANES Combined	**Minimum number of years of schooling required before work permit at age 14**				
Number of people aged 18 or older with…	≤ 6	7	8	≥ 9	Sum
Schooling ≤ 6	**1124** (24.3)	695 (11.6)	193 (2.6)	132 (2.6)	2144
Schooling = 7	298 (6.5)	**302** (5.1)	138 (1.9)	92 (1.8)	830
Schooling = 8	708 (15.3)	803 (13.4)	**505** (6.9)	250 (4.8)	2266
Schooling = 9	288 (6.2)	370 (6.2)	313 (4.3)	**269** (5.2)	1240
Schooling > 9	2200 (47.6)	3803 (63.7)	6185 (84.3)	4433 (85.6)	16621
Sum	4618	5973	7334	5176	23101

Table 2b: Educational attainment at the CSL constraint, NHANES1

NHANES1	Minimum number of years of schooling required before work permit at age 14				
Number of people aged 18 or older with…	≤ 6	7	8	≥ 9	Sum
Schooling ≤ 6	**755** (26.5)	316 (11.1)	91 (2.5)	69 (2.7)	1231
Schooling = 7	182 (6.4)	**143** (5.0)	55 (1.5)	52 (2.0)	432
Schooling = 8	451 (15.9)	407 (14.3)	**258** (7.1)	147 (5.8)	1263
Schooling = 9	180 (6.3)	182 (6.4)	157 (4.3)	**163** (6.4)	682
Schooling > 9	1276 (44.9)	1799 (63.2)	3068 (84.5)	2123 (83.1)	8266
Sum	2844	2847	3629	2554	11874

Table 2c: Educational attainment at the CSL constraint, NHANES2

NHANES2	Minimum number of years of schooling required before work permit at age 14				
Number of people aged 18 or older with…	≤ 6	7	8	≥ 9	Sum
Schooling ≤ 6	**369** (20.8)	379 (12.1)	102 (2.8)	63 (2.4)	913
Schooling = 7	116 (6.5)	**159** (5.1)	83 (2.2)	40 (1.5)	398
Schooling = 8	257 (14.5)	396 (12.7)	**247** (6.7)	103 (3.9)	1003
Schooling = 9	108 (6.1)	188 (6)	156 (4.2)	**106** (4)	558
Schooling > 9	924 (52.1)	2004 (64.1)	3117 (84.1)	2310 (88.1)	8355
Sum	1774	3126	3705	2622	11227

Table 3A: Sample Statistics

	NHANES1 (71-75)		NHANES2 (76-80)		1970 Census	
	Mean	Std Dev	Mean	Std Dev	Mean	Std Dev
Age	40.58	15.37	40.70	15.98	40.34	14.91
BMI	25.12	4.99	25.16	4.92	na	na
HiGrade	11.57	3.18	11.95	3.18	14.24	1.34
HHSize	3.51	1.91	3.19	1.76	3.48	0.87
Income:						
per capita	3,769.39	3,063.18	5,664.23	4,101.20	4,912.41	3,523.18
family	10,947.56	6,841.53	15,093.70	8,759.64	11,046.03	2,551.46
Female	52.6%		52.3%		51.9%	
Married	71.7%		66.3%		70.0%	
Black	10.5%		10.7%		10.6%	
Poor	11.4%		11.7%		na	
SMSA	63.3%		60.5%		na	
Working	59.3%		61.5%		62.4%	
No HS	34.3%		29.7%		39.7%	
N	11,874		11,214		1,068,444	

<u>Notes:</u> Overweight = BMI ≥ 25; Obese = BMI ≥ 30. In the 1970 Census, 12th grade is coded as "15". No HS = No high school degree. "Working" is defined as "has worked in the past 3 months" in the NHANES and as "has been employed over a given reference period" in the Census. All data are weighted (by cell size in the case of the Census). Aged 18 or older. Since the NHANES only provides brackets for total family income, I assign midpoints (the weighted mean of which is reported as "Family Income") and divide by household size to get an imputed personal income variable ("Personal Income"). For the Census, dividing family income by family size yields a mean personal income of 3,319.13 with a std. dev. of 929. The table reports the total personal income variable included in the Census.

Table 3B: Sample statistics, continued.

NHANES1

	Overweight		Obese	
Total	44.6%		14.0%	
Male	51.6%		11.9%	
Female	38.3%		15.8%	
	Male	Female	Male	Female
White	52.0%	36.1%	11.5%	14.3%
Black	47.1%	55.1%	15.4%	27.6%

NHANES2

	Overweight		Obese	
Total	44.1%		14.2%	
Male	49.4%		11.9%	
Female	39.3%		16.3%	
	Male	Female	Male	Female
White	49.9%	37.1%	11.7%	14.7%
Black	45.5%	56.1%	14.1%	28.8%

Notes Weighted; Aged 18 or older.

Table 4: First Stage

	NHANES1		NHANES2		Combined NHANES	
	(1)	(2)	(3)	(4)	(5)	(6)
Dep. Var. =	Education	P(HS)	Education	P(HS)	Education	P(HS)
CSL7	0.160	0.012	0.258	-0.013	0.214	-0.000
	(0.219)	(0.020)	(0.217)	(0.027)	(0.196)	(0.017)
	(0.168)	*(0.020)*	*(0.160)*	*(0.021)*	*(0.117)**	*(0.014)*
CSL8	0.337	0.005	0.470	-0.001	0.416	0.001
	(0.256)	(0.022)	(0.257)*	(0.024)	(0.251)	(0.016)
	*(0.172)**	*(0.022)*	*(0.154)****	*(0.022)*	*(0.127)****	*(0.016)*
CSL9	0.875	0.017	0.718	-0.002	0.800	0.004
	(0.307)***	(0.029)	(0.248)***	(0.033)	(0.245)***	(0.020)
	*(0.194)****	*(0.026)*	*(0.183)****	*(0.026)*	*(0.137)****	*(0.018)*
Black	-1.630	-0.147	-1.316	-0.148	-1.443	-0.144
	(0.326)***	(0.024)***	(0.159)***	(0.019)***	(0.206)***	(0.018)***
	*(0.137)****	*(0.017)****	*(0.116)****	*(0.018)****	*(0.093)****	*(0.013)****
Female	-0.140	-0.077	-0.208	-0.058	-0.171	-0.067
	(0.088)	(0.011)***	(0.077)***	(0.011)***	(0.065)**	(0.008)***
	*(0.075)**	*(0.011)****	*(0.064)****	*(0.011)****	*(0.050)****	*(0.008)****
Married	0.249	-0.053	0.115	-0.050	0.236	-0.042
	(0.116)**	(0.015)***	(0.099)	(0.016)***	(0.078)***	(0.010)***
	*(0.098)***	*(0.014)****	*(0.077)*	*(0.013)****	*(0.061)****	*(0.009)****
SMSA	0.973	0.109	0.909	0.105	0.924	0.102
	(0.139)***	(0.016)***	(0.128)***	(0.015)***	(0.090)***	(0.010)***
	*(0.083)****	*(0.013)****	*(0.078)****	*(0.013)****	*(0.054)****	*(0.008)****
F-stat	4.72	0.14	3.16	0.14	4.95	0.02
	9.44	*0.22*	*6.08*	*0.23*	*14.31*	*0.04*
p-value	0.01	0.93	0.03	0.94	0.01	0.99
	0.00	*0.89*	*0.00*	*0.88*	*0.00*	*0.99*
R-squared	0.22	0.1	0.22	0.11	0.21	0.10
N	11869	11869	11214	11214	23083	23083

Notes: Weighted; aged 18 or older.
S.E. in parentheses (* significant at 10%; ** significant at 5%; *** significant at 1%).
Regular S.E clustered by state. *S.E. in italics = Clustered by birthplace and birth year.*
All regressions contain dummies for state-of-birth and year-of-birth.
P(HS) represents a linear probability model regression of an indicator variable for more than a high school degree on the CSLs and other covariates.
The coefficients on the CSL variable can be interpreted as follows: Looking, for instance, at CSL9 in the NHANES1 column, a person born in a state whose compulsory schooling laws required 9 or more years of schooling would end up with almost a full additional year of completed education (87.5% of a year), compared to someone who was born in a state with a requirement of 6 or fewer years (CSL6 is the omitted dummy variable).

Table 5: OLS and IV

Dependent variable = *BMI*.

	NHANES1		NHANES2		Combined NHANES	
	OLS	IV	OLS	IV	OLS	IV
Education	-0.133	-0.763	-0.134	0.156	-0.128	-0.410
	(0.032)***	(0.305)**	(0.023)***	(0.360)	(0.022)***	(0.243)*
	*(0.026)****	*(0.336)***	*(0.019)****	*(0.371)*	*(0.016)****	*(0.246)**
Black	1.746	0.716	1.684	2.064	1.740	1.333
	(0.260)***	(0.572)	(0.201)***	(0.521)***	(0.179)***	(0.363)***
	*(0.272)****	*(0.576)*	*(0.214)****	*(0.542)****	*(0.172)****	*(0.379)****
Female	-0.683	-0.769	-0.416	-0.356	-0.552	-0.599
	(0.188)***	(0.243)***	(0.096)***	(0.130)***	(0.115)***	(0.144)***
	*(0.127)****	*(0.144)****	*(0.104)****	*(0.133)****	*(0.084)****	*(0.097)****
Married	0.149	0.309	0.282	0.247	0.277	0.345
	(0.142)	(0.165)*	(0.121)**	(0.151)	(0.091)***	(0.104)***
	(0.161)	*(0.195)*	*(0.131)***	*(0.143)**	*(0.102)****	*(0.122)****
SMSA	-0.324	0.284	-0.396	-0.659	-0.413	-0.153
	(0.136)**	(0.253)	(0.116)***	(0.355)*	(0.094)***	(0.234)
	*(0.129)***	*(0.348)*	*(0.124)****	*(0.351)**	*(0.085)****	*(0.237)*
R-squared	0.09	na	0.10	0.07	0.08	0.06
N	11869	11869	11214	11214	23083	23083

	X^2	p-value
Underidentification test	16.226	0.0010
Hansen J statistic	1.063	0.5878

Notes: Weighted; aged 18 or older.
S.E. in parentheses (* significant at 10%; ** significant at 5%; *** significant at 1%).
Regular S.E clustered by state. *S.E. in italics = Clustered by birthplace and birth year.*
All regressions contain dummies for state-of-birth and year-of-birth.
The "Underidentification test" is an LM version of the Kleibergen-Paap *rk* statistic, robust to heteroskedasticity.

Table 6: Stratified results for the Combined NHANES

Dependent variable = *BMI* and *log(BMI)*.
Cells report the coefficient on *Education*.

	OLS	IV	log-OLS	log-IV	N
Full sample	-0.128	-0.410	-0.004	-0.014	23083
	(0.022)***	(0.243)*	(0.001)***	(0.009)	
Males	-0.010	0.151	0.000	0.009	9785
	(0.020)	(0.277)	(0.001)	(0.009)	
Females	-0.302	-1.038	-0.011	-0.039	13298
	(0.027)***	(0.342)***	(0.001)***	(0.013)***	
Whites	-0.137	-0.289	-0.005	-0.011	19566
	(0.022)***	(0.310)	(0.001)***	(0.012)	
Blacks	-0.072	-0.887	-0.002	-0.027	3517
	(0.042)*	(0.965)	(0.002)	(0.031)	
Female whites	-0.294	-0.990	-0.011	-0.038	11121
	(0.031)***	(0.454)**	(0.001)***	(0.018)**	
Female blacks	-0.268	-1.264	-0.008	-0.044	2177
	(0.058)***	(1.033)	(0.002)***	(0.034)	
Urban	-0.142	-0.722	-0.005	-0.025	12821
	(0.028)***	(0.384)*	(0.001)***	(0.014)*	
Rural	-0.108	0.126	-0.003	0.005	10262
	(0.023)***	(0.321)	(0.001)***	(0.012)	
Married	-0.110	-0.360	-0.004	-0.011	15514
	(0.026)***	(0.264)	(0.001)***	(0.010)	
Unmarried	-0.165	-0.531	-0.005	-0.016	7569
	(0.024)***	(0.396)	(0.001)***	(0.013)	
Less than HS	-0.124	-0.444	-0.004	-0.015	9309
	(0.048)**	(0.292)	(0.002)**	(0.011)	
HS or more	-0.128	1.119	-0.005	0.043	13774
	(0.025)***	(1.875)	(0.001)***	(0.072)	

Notes: Weighted; aged 18 or older. HS = high school degree.
S.E. in parentheses (* significant at 10%; ** significant at 5%; *** significant at 1%).
All S.E. are clustered at the state level.
All regressions contain dummies for state-of-birth and year-of-birth.

Table 7: Overweight and obese probabilities, Combined NHANES

Cells report the coefficient on *Education*.

	P(Overweight)		P(Obese)		N
	OLS	IV	OLS	IV	
Full sample	-0.008	0.001	-0.010	-0.010	23083
	(0.002)***	(0.025)	(0.001)***	(0.015)	
Males	0.000	0.062	-0.005	0.019	9785
	(0.002)	(0.031)**	(0.002)***	(0.019)	
Females	-0.022	-0.065	-0.017	-0.044	13298
	(0.003)***	(0.034)*	(0.001)***	(0.019)**	
Whites	-0.010	0.013	-0.011	-0.006	19566
	(0.002)***	(0.033)	(0.001)***	(0.020)	
Blacks	0.002	-0.056	-0.006	-0.027	3517
	(0.004)	(0.064)	(0.003)*	(0.038)	
Female whites	-0.023	-0.063	-0.016	-0.044	11121
	(0.003)***	(0.045)	(0.002)***	(0.028)	
Female blacks	-0.009	-0.100	-0.017	-0.090	2177
	(0.004)**	(0.068)	(0.005)***	(0.051)*	
Urban	-0.011	-0.028	-0.010	-0.025	12821
	(0.002)***	(0.040)	(0.001)***	(0.023)	
Rural	-0.003	0.050	-0.011	0.025	10262
	(0.002)	(0.035)	(0.002)***	(0.026)	
Married	-0.006	0.016	-0.009	-0.020	15514
	(0.002)***	(0.032)	(0.002)***	(0.017)	
Unmarried	-0.012	-0.043	-0.012	-0.030	7569
	(0.003)***	(0.035)	(0.001)***	(0.024)	
Less than HS	-0.002	-0.013	-0.009	-0.027	9309
	(0.003)	(0.029)	(0.003)***	(0.025)	
HS or more	-0.009	-0.132	-0.011	-0.114	13774
	(0.003)***	(0.263)	(0.002)***	(0.171)	

Notes: Weighted; aged 18 or older. HS = high school degree.
S.E. in parentheses (* significant at 10%; ** significant at 5%; *** significant at 1%).
All S.E. are clustered at the state level. All regressions contain dummies for state-of-birth and year-of-birth.
Coefficients are taken from a linear probability model of an indicator variable for overweight and obesity on the covariates.

Table 8A: Alternative Schooling Laws, First stage

Dependent variable = *Education.*

	NHANES1			NHANES2			Combined NHANES		
	[1]	[2]	[3]	[1]	[2]	[3]	[1]	[2]	[3]
Drop_Age		0.216	0.132		0.298	0.244		0.268	0.200
		(0.129)	(0.107)		(0.095)***	(0.094)**		(0.093)***	(0.077)**
CSL7	0.160		0.136	0.258		0.251	0.214		0.192
	(0.219)		(0.218)	(0.217)		(0.221)	(0.196)		(0.196)
CSL8	0.337		0.276	0.470		0.361	0.416		0.325
	(0.256)		(0.246)	(0.257)*		(0.231)	(0.251)		(0.228)
CSL9	0.875		0.785	0.718		0.593	0.800		0.681
	(0.307)***		(0.286)***	(0.248)***		(0.244)**	(0.245)***		(0.228)***
Black	-1.630	-1.636	-1.630	-1.316	-1.308	-1.317	-1.443	-1.441	-1.444
	(0.326)***	(0.329)***	(0.327)***	(0.159)***	(0.159)***	(0.159)***	(0.206)***	(0.206)***	(0.207)***
Female	-0.140	-0.139	-0.142	-0.208	-0.209	-0.209	-0.171	-0.172	-0.173
	(0.088)	(0.088)	(0.087)	(0.077)***	(0.077)***	(0.076)***	(0.065)**	(0.066)**	(0.065)**
Married	0.249	0.252	0.249	0.115	0.103	0.103	0.236	0.230	0.229
	(0.116)**	(0.116)**	(0.116)**	(0.099)	(0.098)	(0.098)	(0.078)***	(0.077)***	(0.078)***
SMSA	0.973	0.969	0.975	0.909	0.904	0.909	0.924	0.919	0.925
	(0.139)***	(0.137)***	(0.138)***	(0.128)***	(0.129)***	(0.129)***	(0.090)***	(0.090)***	(0.090)***
F-Stat	4.72	2.78	4.33	3.16	9.9	5.71	4.95	8.36	6.72
R-squared	0.22	0.22	0.22	0.22	0.22	0.22	0.21	0.21	0.21
N	11869	11869	11869	11214	11214	11214	23083	23083	23083

Notes: Weighted; aged 18 or older.
S.E. in parentheses (* significant at 10%; ** significant at 5%; *** significant at 1%).
All S.E. are clustered at the state level.
All regressions contain dummies for state-of-birth and year-of-birth.
Columns marked [1] use the set of schooling laws based on work permits; those marked [2] use the minimum school leaving age; and those marked [3] combine both sets.

Table 8B: Alternative Schooling Laws, IV

Dependent variable = *BMI*.
Cells report the coefficient on *Education*.

NHANES1

	[1]	[2]	[3]	N
Total	-0.763	-0.721	-0.745	11869
	(0.310)**	(0.615)	(0.313)**	
Male	-0.246	0.838	-0.207	4487
	(0.348)	(1.255)	(0.337)	
Female	-1.365	-1.231	-1.301	7382
	(0.569)**	(0.544)**	(0.484)***	

NHANES2

	[1]	[2]	[3]	N
Total	0.156	0.169	0.114	11214
	(0.365)	(0.270)	(0.233)	
Male	0.745	1.090	0.860	5298
	(0.337)**	(0.718)	(0.349)**	
Female	-0.207	-0.431	-0.417	5916
	(0.561)	(0.378)	(0.339)	

Combined NHANES

	[1]	[2]	[3]	N
Total	-0.410	-0.114	-0.311	23083
	(0.246)	(0.268)	(0.192)	
Male	0.151	1.034	0.307	9785
	(0.281)	(0.676)	(0.254)	
Female	-1.038	-0.761	-0.914	13298
	(0.347)***	(0.275)***	(0.239)***	

Notes: Weighted; aged 18 or older.
S.E. in parentheses (* significant at 10%; ** significant at 5%; *** significant at 1%).
All S.E. are clustered at the state level.
All regressions contain dummies for state-of-birth and year-of-birth.
Columns marked [1] use the set of schooling laws based on work permits; those marked [2] use the minimum school leaving age; and those marked [3] combine both sets.

Table 9: Validity check - Height

Dependent variable = *Height*.
Cells report the coefficient on *Education*.

Combined NHANES

	OLS	IV	N
Total	3.440	0.721	23083
	(0.320)***	(4.319)	
Male	3.310	0.301	9785
	(0.307)***	(5.437)	
Female	3.564	0.208	13298
	(0.440)***	(4.575)	

Notes: Weighted; aged 18 or older.
S.E. in parentheses (* significant at 10%; ** significant at 5%; *** significant at 1%).
All S.E. are clustered at the state level.
All regressions contain dummies for state-of-birth and year-of-birth.
"Height" is measured in millimeters.

Table 10a: Validity check - State-specific time trends (IV)

Dependent variable = *BMI*.
Cells report the coefficient on *Education*.

Combined NHANES

	Regular	With state-specific time trends	N
Total	-0.410	-0.415	23083
	(0.246)	(0.657)	
Male	0.151	0.124	9785
	(0.281)	(0.561)	
Female	-1.038	0.901	13298
	(0.347)***	(4.313)	

Notes: Weighted; aged 18 or older.
S.E. in parentheses (* significant at 10%; ** significant at 5%; *** significant at 1%).
All S.E. are clustered at the state level.
All regressions contain dummies for state-of-birth and year-of-birth.

Table 10b: Validity check - State-specific time trends (First stage)

Dependent variable = *Education.*

Combined NHANES

	Regular			With state-specific time trends		
	Total	Male	Female	Total	Male	Female
CSL7	0.214	0.249	0.211	0.094	0.186	0.060
	(0.196)	(0.201)	(0.240)	(0.205)	(0.220)	(0.258)
CSL8	0.416	0.493	0.385	0.226	0.379	0.147
	(0.251)	(0.259)*	(0.296)	(0.285)	(0.326)	(0.377)
CSL9	0.800	0.941	0.729	0.271	0.557	0.081
	(0.245)***	(0.253)***	(0.284)**	(0.214)	(0.247)**	(0.248)
F-stat	4.95	6.94	3	0.97	2.6	0.07
N	23083	9785	13298	23083	9785	13298

Notes: Weighted; aged 18 or older. HS = high school degree.
S.E. in parentheses (* significant at 10%; ** significant at 5%; *** significant at 1%).
All S.E. are clustered at the state level.
All regressions contain dummies for state-of-birth and year-of-birth.

Table 10c: Validity check - State-specific time trends (Subsample results)

Dependent variable = *BMI.*
Cells report the coefficient on *Education.*

Combined NHANES

	Males		Females	
	OLS	IV	OLS	IV
5 years	-0.066	0.567	-0.468	-0.535
	(0.058)	(0.993)	(0.097)***	(1.953)
	N=751		N=1067	
10 years	-0.046	0.358	-0.475	-3.601
	(0.031)	(0.851)	(0.078)***	(2.059)*
	N=1206		N=1739	
15 years	-0.035	0.579	-0.414	-2.515
	(0.027)	(1.083)	(0.071)***	(0.994)**
	N=1556		N=2210	
20 years	-0.020	0.067	-0.377	-0.867
	(0.029)	(0.798)	(0.065)***	(1.390)
	N=1773		N=2470	
Baseline	-0.010	0.151	-0.302	-1.038
	(0.020)	(0.277)	(0.027)***	(0.342)***
	N=9785		N=13298	

Notes: Weighted; aged 18 or older.
S.E. in parentheses (* significant at 10%; ** significant at 5%; *** significant at 1%).
All S.E. are clustered at the state level.
All regressions contain dummies for state-of-birth and year-of-birth.
“5 years” denotes a subsample of state/year combinations up to 5 years prior to and 5 years after a decline in the state’s compulsory schooling law. The other subsamples are constructed similarly. “Baseline” contains the entire sample of states and years.

Table 11a: Validity check – Estimation methods robust to weak instruments

Dependent variable = *BMI*.
Cells report the coefficient on *Education*.

Combined NHANES

Full sample	OLS	2SLS	LIML	Nagar	Fuller (1)	Fuller (4)	N
Education	-0.128 (0.022)***	-0.410 (0.243)*	-0.418 (0.250)*	-0.418 (0.250)*	-0.414 (0.247)*	-0.403 (0.237)*	23083
KP F-stat		7.153	7.153	7.153	7.153	7.153	
Females	OLS	2SLS	LIML	Nagar	Fuller (1)	Fuller (4)	N
Education	-0.302 (0.027)***	-1.038 (0.342)***	-1.146 (0.409)***	-1.059 (0.354)***	-1.124 (0.394)***	-1.063 (0.357)***	13298
KP F-stat		4.672	4.672	4.672	4.672	4.672	
5% max IV rel. bias		13.91		-			
10% max IV rel. bias		9.08		-			
20% max IV rel. bias		6.46		-			
30% max IV rel. bias		5.39		-			
10% max IV size		22.30		-			
15% max IV size		12.83		-			
20% max IV size		9.54		-			
25% max IV size		7.80		-			
10% max LIML size			6.46	-			
15% max LIML size			4.36	-			
20% max LIML size			3.69	-			
25% max LIML size			3.32	-			
5% max Fuller rel. bias				-	9.61	9.61	
10% max Fuller rel. bias				-	7.9	7.9	
20% max Fuller rel. bias				-	6.61	6.61	
30% max Fuller rel. bias				-	5.6	5.6	
5% Fuller max bias				-	8.66	8.66	
10% Fuller max bias				-	7.18	7.18	
20% Fuller max bias				-	5.87	5.87	
30% Fuller max bias				-	5.11	5.11	

Notes: Weighted; aged 18 or older.
S.E. in parentheses (* significant at 10%; ** significant at 5%; *** significant at 1%).
All S.E. are clustered at the state level.
All regressions contain dummies for state-of-birth and year-of-birth.
The Nagar estimator is a *k*-class estimator with $k = 1 + (L - K)/N$, where L – K = # of overidentifying restrictions and N = sample size. In my case $k = 1.00008664$. Critical values for the Stock and Yogo (2002) weak instrument tests are not available for the Nagar estimator.
In the full sample regressions, the LIML and Nagar results are not identical, but very similar.

Table 11b: Validity check – Inference robust to weak instruments

Dependent variable = *BMI*.
Cells report the coefficient on *Education.*

Combined NHANES

	Full sample			Females	
	Weighted	Unweighted		Weighted	Unweighted
Clustered	-0.410	-0.260	Clustered	-1.038	-0.804
	(0.243)*	(0.194)		(0.342)***	(0.304)***
Not clustered	-0.410	**-0.260**	Not clustered	-1.038	**-0.804**
	(0.246)*	**(0.162)**		(0.393)***	**(0.259)*** **
Test	Confidence Interval	p-value		Confidence Interval	p-value
2SLS	[-.577, .058]	0.109		[-1.314, -.293]	0.002
Conditional LR	[-.593, .062]	0.112		[-1.389, -.306]	0.002
AR	[-.735, .193]	0.423		[-1.644, -.126]	0.014
Score (LM)	[-.589, .059]	0.109		[-1.381, -.312]	0.002
Reduced Form					
Joint Wald stat	Chi-sq(3)=3.62	0.306		Chi-sq(3)=12.10	0.0071

Notes: Aged 18 or older.
S.E. in parentheses (* significant at 10%; ** significant at 5%; *** significant at 1%).
All regressions contain dummies for state-of-birth and year-of-birth.
In the reduced form, the test statistic is a joint Wald statistic on the excluded instruments.

Table A.3: Reduced Form Dependent variable = *BMI*.

TOTAL	NHANES1			NHANES2			Combined NHANES		
	[1]	[2]	[3]	[1]	[2]	[3]	[1]	[2]	[3]
Drop_Age		-0.155	-0.079		0.051	0.012		-0.031	-0.006
		(0.130)	(0.123)		(0.078)	(0.090)		(0.071)	(0.074)
CSL7	-0.203		-0.189	-0.174		-0.175	-0.147		-0.146
	(0.203)		(0.198)	(0.197)		(0.197)	(0.162)		(0.158)
CSL8	-0.457		-0.421	0.270		0.264	-0.071		-0.069
	(0.228)**		(0.206)**	(0.135)**		(0.146)*	(0.148)		(0.136)
CSL9	-0.731		-0.678	-0.076		-0.082	-0.369		-0.366
	(0.313)**		(0.284)**	(0.251)		(0.264)	(0.210)*		(0.204)*
p-value on F	0.05	0.23	0.07	0.01	0.52	0.02	0.31	0.67	0.46
Observations	11869	11869	11869	11214	11214	11214	23083	23083	23083

MALES	NHANES1			NHANES2			Combined NHANES		
	[1]	[2]	[3]	[1]	[2]	[3]	[1]	[2]	[3]
Drop_Age		0.131	0.181		0.277	0.245		0.217	0.228
		(0.140)	(0.162)		(0.105)***	(0.113)**		(0.085)**	(0.090)**
CSL7	0.287		0.265	0.426		0.433	0.383		0.372
	(0.245)		(0.239)	(0.277)		(0.277)	(0.181)**		(0.169)**
CSL8	-0.089		-0.164	0.536		0.438	0.259		0.164
	(0.294)		(0.316)	(0.380)		(0.368)	(0.237)		(0.207)
CSL9	-0.056		-0.170	0.708		0.596	0.334		0.211
	(0.374)		(0.396)	(0.299)**		(0.307)*	(0.268)		(0.255)
p-value on F	0.24	0.35	0.27	0.13	0.01	0.01	0.15	0.01	0.04
Observations	4487	4487	4487	5298	5298	5298	9785	9785	9785

FEMALES	NHANES1			NHANES2			Combined NHANES		
	[1]	[2]	[3]	[1]	[2]	[3]	[1]	[2]	[3]
Drop_Age		-0.361	-0.258		-0.143	-0.185		-0.246	-0.199
		(0.192)*	(0.173)		(0.114)	(0.140)		(0.097)**	(0.103)*
CSL7	-0.337		-0.278	-0.711		-0.697	-0.500		-0.469
	(0.324)		(0.311)	(0.368)*		(0.374)*	(0.292)*		(0.288)
CSL8	-0.543		-0.416	0.052		0.140	-0.259		-0.164
	(0.373)		(0.333)	(0.383)		(0.358)	(0.317)		(0.283)
CSL9	-1.142		-0.956	-0.642		-0.539	-0.914		-0.787
	(0.487)**		(0.414)**	(0.385)*		(0.387)	(0.331)***		(0.299)***
p-value on F	0.09	0.06	0.13	0.01	0.21	0.01	0.01	0.01	0.01
Observations	7382	7382	7382	5916	5916	5916	13298	13298	13298

Notes: Weighted; aged 18 or older. S.E. in parentheses (* significant at 10%; ** significant at 5%; *** significant at 1%). All S.E. are clustered at the state level. All regressions contain dummies for state-of-birth and year-of-birth. Columns marked [1] use the set of schooling laws based on work permits; those marked [2] use the minimum school leaving age; and those marked [3] combine both sets.

Table A.4: Subset regressions, Combined NHANES

Dependent variable = *BMI*.
Cells report the coefficient on *Education*.

Females, weighted quintiles					Males, weighted quintiles				
	OLS	IV	N	BMI		OLS	IV	N	BMI
Total	-0.302	-1.038	13298		Total	-0.011	0.151	9785	
	(0.027)***	(0.342)***				(0.017)	(0.277)		
1st	0.036	-0.193	2464	12.4 to 20.5	1st	0.055	-0.001	2017	12.9 to 22
	(0.010)***	(0.167)				(0.012)***	(0.173)		
2nd	-0.011	-0.105	2468	20.5 to 22.5	2nd	0.004	-0.043	1933	22 to 24.1
	(0.006)*	(0.152)				(0.005)	(0.080)		
3rd	-0.007	-0.040	2538	22.5 to 24.8	3rd	0.008	-0.009	1933	24.1 to 26.1
	(0.007)	(0.088)				(0.006)	(0.117)		
4th	-0.020	0.124	2857	24.8 to 28.8	4th	-0.001	-0.007	1909	26.1 to 28.4
	(0.010)**	(0.125)				(0.006)	(0.085)		
5th	-0.145	-0.013	2971	28.8 to 72.3	5th	-0.049	-0.483	1993	28.4 to 56.6
	(0.038)***	(0.631)				(0.032)	(0.330)		

Females, weighted terciles					Males, weighted terciles				
	OLS	IV	N	BMI		OLS	IV	N	BMI
Total	-0.302	-1.038	13298		Total	-0.010	0.151	9785	
	(0.027)***	(0.342)***				(0.020)	(0.277)		
1st	0.031	-0.264	4101	12.4 to 21.9	1st	0.068	-0.098	3289	12.9 to 23.5
	(0.013)**	(0.244)				(0.013)***	(0.137)		
2nd	-0.020	-0.039	4291	21.9 to 25.8	2nd	0.003	0.223	3199	23.5 to 26.7
	(0.007)***	(0.200)				(0.005)	(0.147)		
3rd	-0.199	-0.463	4906	25.8 to 72.3	3rd	-0.073	-0.720	3297	26.7 to 56.6
	(0.030)***	(0.372)				(0.020)***	(0.395)*		

Notes: Weighted; aged 18 or older.
S.E. in parentheses (* significant at 10%; ** significant at 5%; *** significant at 1%).
All S.E. are clustered at the state level.
All regressions contain dummies for state-of-birth and year-of-birth.

Table A.5a: NHANES1 estimation using the STATA *survey* setup, First stage

Dependent variable = Education	
CSL7	0.160
	(0.140)
CSL8	0.337
	(0.219)
CSL9	0.875
	(0.242)***
Black	-1.630
	(0.170)***
Female	-0.140
	(0.065)**
Married	0.249
	(0.095)***
SMSA	0.973
	(0.120)***
F-Stat on Instruments	5.55
R-squared	0.22
N	11869

<u>Notes for both tables:</u>
Standard errors in parentheses (* significant at 5%; ** significant at 1%).
Estimations were performed in STATA 9.2 using the *survey* set of commands. The information on PSUs, strata and sampling weights were taken from the NHANES1 tape. The sampling weights correspond to "all persons sampled – exam locations 1-65."
Variance estimation uses Taylor linearization.

Table A.5b: NHANES1 estimation using the STATA *survey* setup, OLS and IV

Dependent variable = BMI

	OLS	IV, all	IV, females	IV, males	IV, whites	IV, blacks
Education	-0.133	-0.763	-1.365	-0.246	-0.813	-0.099
	(0.023)***	(0.372)**	(0.536)**	(0.375)	(0.495)	(0.797)
Black	1.746	0.716	1.462	0.080		
	(0.252)***	(0.665)	(0.727)**	(0.849)		
Female	-0.683	-0.769			-1.148	1.959
	(0.117)***	(0.154)***			(0.230)***	(0.883)**
Married	0.149	0.309	0.252	0.793	0.241	0.307
	(0.131)	(0.167)*	(0.225)	(0.252)***	(0.133)*	(0.883)
SMSA	-0.324	0.284	0.510	0.056	0.281	-0.480
	(0.117)***	(0.389)	(0.542)	(0.402)	(0.490)	(0.944)
N	11869	11869	7382	4487	9623	2246
R-squared	0.09	na	na	0.06	na	0.18

Table A.6a: Income as dependent variable

Dependent variable = *log(Income)*.
Cells report the coefficient on *Education*.

NHANES1

	OLS	IV	N
Full sample	0.096	-0.035	11464
	(0.004)**	(0.055)	
Males	0.089	-0.058	4331
	(0.005)**	(0.065)	
Females	0.107	0.012	7133
	(0.005)**	(0.080)	
White	0.095	-0.061	9306
	(0.004)**	(0.075)	
Nonwhite	0.106	-0.001	2158
	(0.010)**	(0.072)	
Urban	0.099	0.004	6631
	(0.004)**	(0.109)	
Rural	0.089	-0.060	4833
	(0.006)**	(0.060)	
Less than HS	0.0933	-0.092	4929
	(0.0076)**	(0.103)	
HS or more	0.0828	-0.1706	6535
	(0.0069)**	(0.614)	

NHANES2

	OLS	IV	N
Full sample	0.083	-0.023	10830
	(0.003)**	(0.073)	

Notes: Weighted; aged 18 or older. HS = high school degree.
S.E. in parentheses (* significant at 5%; ** significant at 1%). All S.E. are clustered at the state-by-year level. All regressions contain dummies for state-of-birth and year-of-birth.

Table A.6b: Income as dependent variable, tests

	NHANES1, Using original income groups		1970 Census	
	OLS	IV	OLS	IV
Education	0.251	-0.252	0.177	0.156
	(0.010)**	(0.166)	(0.004)**	(0.021)**
Nonwhite	-0.989	-1.781	0.120	0.090
	(0.093)**	(0.289)**	(0.014)**	(0.032)**
Female	-0.283	-0.363	-1.108	-1.109
	(0.055)**	(0.073)**	(0.013)**	(0.013)**
Married	1.576	1.705	0.637	0.632
	(0.087)**	(0.110)**	(0.064)**	(0.065)**
SMSA	0.595	1.075	na	na
	(0.068)**	(0.179)**	na	na
R-squared	0.35	0.05	0.91	0.91
N	11464	11464	1068323	1068323

Notes: Weighted; aged 18 or older.
S.E. in parentheses (* significant at 5%; ** significant at 1%). All S.E. are clustered at the state-by-year level. All regressions contain dummies for state-of-birth and year-of-birth.
Census data are collapsed by birthplace, birth year, gender, and race. Regression weighted by cell size. The income variable in the Census regression is "total personal income" as reported in the Census.

Table A.6c: Income as dependent variable, Census replication

	OLS	IV	N
NHANES1	0.251	-0.252	11464
(Actual income groups)	(0.010)**	(0.166)	
NHANES1	0.096	-0.035	11464
(Imputed continuous income)	(0.004)**	(0.055)	
1970 Census, 1% sample	0.201	0.336	10668
(Imputed income groups)	(0.009)**	(0.265)	
1970 Census, 1% sample	0.055	0.071	10668
(Imputed continuous income)	(0.003)**	(0.076)	

Notes: NHANES1 results are weighted; Census results are unweighted. Aged 18 or older.
S.E. in parentheses (* significant at 5%; ** significant at 1%). All S.E. are clustered at the state-by-year level. All regressions contain dummies for state-of-birth and year-of-birth. The Census "Imputed income groups" were constructed by dividing "Family total income" into 12 groups whose ranges (in absolute value) correspond to those reported in the NHANES1. To derive "Imputed continuous income", I assigned midpoints to each income group, divided by family size and took the logarithm.

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