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The Causal Effect of Education on Body Mass: Evidence from Europe

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We adopt a multi-country setup to show that years of schooling have a causal protective effect on the body mass index of females living in nine European countries. No such effect is found for males. The protective effect for European females is not negligible but is smaller than one recently found for the United States and stronger among overweight females. We discuss possible mechanisms justifying both the protective role of education and the gender difference in this role. We argue that the effects of additional schooling on income, the probability of employment, and the frequency of vigorous physical activities, both on and off the job, may help explain our results.

I. Introduction

The health consequences and the economic costs of rising obesity have generated social and political concern both in the United States and Eu-

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rope.[1] The principal public interventions proposed and implemented so far to tackle the problem are information policies, including information campaigns, advertising regulations, labeling rules, and regulations on nutritional claims. The use of regulatory tools such as standards, taxes, and incentives is still in its infancy.[2] Recently Philipson and Posner (2008) and Mazzocchi, Traill, and Shogren (2009) have doubted the effectiveness of information policies in changing knowledge and attitudes: on the one hand, there is little evidence that deficiencies in information affect behavior; on the other hand, incentives to invest in health may be related to education since "longevity and utility of living of uneducated persons are below average" (Philipson and Posner 2008, 979).

General education policies that increase the years of schooling attained by vulnerable individuals are more promising than information policies in combating the upsurge of obesity because they are likely to affect individuals' longevity and utility from living. For these policies to work, education must have a causal protective effect on obesity and overweight. The empirical evidence that this is the case is far from settled: in spite of the growing concern about these risky behaviors as sources of health problems, there are relatively few studies to date that investigate the causal impact of education on overweight and obesity, and these have rather inconclusive results (e.g., Arendt 2005; Kenkel, Lillard, and Mathios 2006; and Clark and Royer 2010; see Lochner [2011] for a review).

In this article, we use European data to study the effects of education on body mass index (BMI) and the propensity to be overweight or obese.

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able at the Department of Economics, University of Padova, contract no. 14/99. We also thank DIW Berlin for access to the German Socioeconomic Panel. Access to the British Household Panel Survey and the English Longitudinal Study of Ageing data was granted by the UK Data Archive, University of Essex. The Survey of Health, Ageing, and Retirement in Europe data collection has been primarily funded by the European Commission through the fifth, sixth, and seventh framework programs, as well as by the US National Institute on Aging and other national funds. Macro data on gross domestic product and labor force participation used in this article come from OECD.stat, International Labour Organization labor force statistics, and historical data on Austria, Italy, and Germany. The life expectancy data are from the Human Mortality and Human Life table databases. Financial support from Fondazione Cariparo is gratefully acknowledged. The usual disclaimer applies.
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[1] In the United States, the percentage of obese individuals in the population almost doubled between 1990 and 2004 and was now above 30%. Europe was also on a rising trend, albeit at a slower pace than the United States (Brunello, Michaud, and Sanz-de-Galdeano 2009). This increase has happened much too quickly to be explicable exclusively by genetic factors (Philipson and Posner 2008).

[2] In October 2011, the Danish government introduced a tax on foods containing more that 2.3% of saturated fat, including butter, milk, cheese, pizza, meat, and processed food. In the same period, the UK government published a document on obesity in England that considered the possibility of introducing a "fat tax."

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As in previous papers, causal effects are identified by the exogenous variation induced by compulsory school reforms. We depart from the existing literature by adopting a multi-country framework, rather than the single-country setup typical of previous contributions, in an effort to avoid the problems associated with instrument weakness, a potential source of the inconclusive results obtained so far. To this purpose, we collect a data set containing information on individuals' education and BMI that covers 13 European countries. We select from this original sample a subsample of countries (nine for females and seven for males) with the following features: (*a*) both the effect of compulsory years of schooling on attained education (first-stage estimate) and on BMI (intention-to-treat estimate) and the effect of education on BMI (instrumental variable estimate) are not statistically different across countries; (*b*) compulsory years of schooling exert a positive and statistically significant effect on education, as measured by attained years of schooling. While the first condition guarantees that pooling the selected countries with respect to the effects of education on BMI is appropriate, the second condition ensures that there is a significant first-stage effect, a precondition for having plausible instrumental variable (IV) estimates.

We show that our instrument—the number of years of compulsory education—is not weak and that years of schooling have a protective effect on the BMI of females living in nine European countries (Austria, Denmark, Germany, Greece, Italy, Portugal, Spain, Sweden, and the United Kingdom). We also find evidence that the marginal effect of education on the BMI of females is larger in absolute value for overweight females. Conversely, we find that the effect of education on the BMI of males living in Austria, Denmark, Germany, Greece, Italy, Sweden, and the United Kingdom is very close to zero and imprecisely estimated.[3] The estimates for our sample of European females suggest that a 10% increase in the years of schooling—which corresponds in our sample to slightly more than 1 additional year at school—reduces average BMI by 1.84% and the prevalence of overweight and obesity by 11.37% and 14.83%, respectively (corresponding to 4.72 percentage points and 1.85 percentage points).[4] The magnitude of these effects, based on IV estimates, is not negligible but is smaller than that recently found by Grabner (2008) for the United States.[5] In order to gain some perspective on their size, we notice that, in the European countries for which we have data, the prevalence of overweight and obesity among females has in-

[3] Portugal and Spain are not in the estimation sample for males because their inclusion is rejected by the pooling tests.

[4] For males, the estimated effects are 0, −0.63, and −2.21, respectively.

[5] Grabner finds that a 1 year increase in years of schooling, which is equivalent to an 8% increase in our data, reduces the BMI of US females by 4% and the prevalence of overweight and obesity by 6.5 and 4.4 percentage points.

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creased between 1991 and 2006 from 24.3% to 27.9% and from 9.1% to 14.8%, respectively.[6] Assuming a similar trend for the European countries in our sample, our results suggest that the effect of adding 1 year of compulsory schooling is almost equivalent to rolling back the percentage of overweight females to its value in the early 1990s but that it is moderate when compared to the substantial increase in the prevalence of obesity in Europe during the past 15 years.

We discuss possible mechanisms justifying both the protective role of education and the gender difference in this role and argue that the effects of additional schooling on income, the probability of employment, and the frequency of vigorous physical activities, both on and off the job, may help explain our results. We also show that gender differences in life expectancy play a negligible role.

This article is organized as follows. Section II briefly reviews the literature, and Section III presents our empirical strategy. The data are introduced in Section IV, and the empirical findings are reported in Section V. Section VI discusses our results, and the conclusions follow in Section VII.

II. Education, Overweight, and Obesity: A Review of the Literature

There are a number of reasons why the effect of education on health, or education gradient, is positive. On the one hand, educated individuals have a better understanding of what a healthy life is and are better endowed in making improved choices that affect health (Kenkel 1991). On the other hand, more education provides access to better job opportunities in terms of higher monetary and nonmonetary rewards. Higher monetary payoffs increase income and improve individual health because of the higher command over resources, including better access to health care.

Since better health reduces dropout rates and improves educational attainment and cognitive skills (see Grossman 2004; Ding et al. 2006), a positive association between education and health can be due to the former causing the latter or to reverse causality, or it may be driven by unobserved third variables that affect both health and education, such as the rate of time preference, the attitude toward risk, mental ability, and parental background (see Cutler and Lleras-Muney 2010). Therefore, estimating the causal impact of education on health requires exogenous sources of variation (instruments) that are correlated with observed education but orthogonal to the selected measure of health.

In spite of a large literature investigating the relationship between education and health, there are only a few contributions that examine the causal

[6] The OECD health data in 1991 and 2006 cover Austria, Finland, France, the United Kingdom, and the Netherlands.

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impact of education on obesity. Spasojevic (2003) uses the 1950 Swedish comprehensive school reform to instrument education in a regression of BMI on education and additional controls. Because of the reform, the cohorts of individuals born between 1945 and 1955 went through two different systems, with the latter requiring at least 1 more year of schooling than the former. Spasojevic's results show that an additional year of schooling improves the likelihood of having BMI in the healthy range—between 18.5 and 25—by 12 percentage points, from 60% to nearly 72%.

Arendt (2005) estimates the effects of education on BMI using a sample of Danish workers aged 18–59. The endogeneity of education is addressed by using as instruments the Danish school reforms of 1958 and 1975, which affected children who turned 14 in 1959 and 1976. Because of the high standard errors associated with the IV estimates, his results are inconclusive. Clark and Royer (2010) study the effects of the UK compulsory school reforms in 1947 and 1972 and find that the effects of education on BMI and obesity are not statistically significant. On a more positive note is the study by Grabner (2008), who uses the variation caused by state-specific compulsory schooling laws between 1914 and 1978 in the United States as an instrument for education and finds that 1 extra year of schooling lowers individual BMI by 1%–4% and the probability of being obese by 2–4 percentage points. His estimated effects are larger for females than for males.

Webbink, Martin, and Visscher (2010) use a sample of 5,967 Australian twins older than age 18, who were interviewed twice, in 1980 and 1988. They adopt a within-twins estimator to eliminate the influence of unobservable common genetic and environment effects and find evidence that, in the subsample of males, 1 additional year of schooling reduces both the likelihood to be overweight and individual BMI. No significant effect is found for females. A notable feature of this study is that results are found to be robust to the way information about BMI is collected, that is, self-reported or based on stadiometric measures. Lundborg (forthcoming) also adopts a within-twins estimator, using data on 694 US twins aged 25–74 drawn from the National Survey of Midlife Development in the United States (MIDUS). He finds no evidence of a statistically significant relationship between education and BMI.

Kenkel et al. (2006) use data from the 1979 wave of the US National Longitudinal Survey of Youth to estimate the impact of high school completion on obesity and overweight. They cope with the endogeneity of education by using as instruments education policies that vary with the state of residence at the time of school attendance and the cohort. These policies include high school graduation requirements, the ease of General Educational Development (GED) certification, and per capita expenditure in education. Since their empirical specification includes state fixed effects, they rely on the within-state variation in their instruments. Their results show

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that "having completed high school" does not have a statistically significant effect on the likelihood of being overweight.

Jürges, Reinhold, and Salm (2011) use a similar approach on German data drawn from three waves of the German Microcensus. They investigate whether having attained the highest level of secondary education in Germany (the so-called Abitur) affects the likelihood to be overweight, using as instrument for endogenous education the proportion of individuals obtaining an Abitur in the relevant cohort and state (Länder) of residence. They find evidence that additional education reduces the likelihood to be overweight more for males than for females. Finally, McInnis (2008) uses a change in the Vietnam drafting procedures for US males during the 1960s and finds that college completion reduces the probability of being obese by 70%.

In summary, there are still relatively few empirical studies investigating the causal effect of education on BMI and obesity. These studies adopt different identification strategies to take into account the endogeneity of education. Results are rather inconclusive, with several studies finding no statistically significant effect.

III. Empirical Strategy

A. Identification Strategy

To identify the causal effect of education on BMI we rely upon the exogenous variation generated by the changes in years of compulsory education that took place in several European countries during the 1960s and the 1970s.[7] We estimate the following equations:

$$\mathrm{BMI}_{ics} = \beta_c f_c + \beta_s f_s + \beta_{cs} f_{cs} + \theta S_{ics} + \varepsilon_{ics}, \tag{1}$$

$$S_{ics} = \alpha_c g_c + \alpha_s g_s + \alpha_{cs} g_{cs} + \phi \mathrm{YCOMP}_{cs} + v_{ics}, \tag{2}$$

where S is years of schooling, f_c and g_c are country dummies, f_s and g_s are year-of-birth dummies, f_{cs} and g_{cs} are country-specific quadratic trends in age, YCOMP is the number of years of compulsory education, the subscript i is for the individual, c for the country, and s for the cohort.[8] Pischke and von Wachter (2008), Clark and Royer (2010), and Lochner (2011) adopt similar specifications. Country-specific quadratic trends in age are included

[7] Minor exceptions are the German cities of Hamburg and Bremen and the Länder of Schleswig Holstein, where the selected compulsory school reform raised the minimum school-leaving age from 14 to 15 in the late 1940s or in the 1950s.

[8] For Germany and the United Kingdom, we use state (the German Länders and England, Scotland, and Wales) rather than country dummies. Our specification also includes survey dummies because we use data from different surveys.

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because failure to account for trends in BMI "may incorrectly attribute these changes to school reforms, biasing estimates toward finding health benefits of schooling" (Lochner 2011, 41). Finally, ε and ν are error terms, which are likely to be correlated either because they include common factors, such as genetic and parental background effects, or because omitted BMI at the age of schooling is correlated both with current BMI and with education. The coefficient of interest in equation (1), θ, includes both the direct effects of S on BMI and the indirect effects, for example, those affecting BMI via income.

In a multi-country setup, equations (1) and (2) impose the restriction that, conditional on country and year-of-birth effects and on country-specific trends, the impact of changes both in the years of compulsory education on years of schooling and in the years of schooling on BMI are the same across countries. Because of cross-country differences in school systems, governments, and public attitude, this condition may not hold in our entire original sample including all the 13 European countries we start with. Therefore, we look for subsamples of countries for which this restriction holds. We proceed as follows. First, we estimate an augmented version of (1) and (2), adding to the regressors also the interactions of the instrument, YCOMP, with country dummies. Second, we select the subsample of countries for which the following two conditions hold: (*a*) the null hypothesis that the coefficients of the interactions are equal to zero in equations (1) and (2) as well as in the reduced form estimate of BMI on YCOMP is not rejected at the 5% level of confidence; (*b*) the effect of YCOMP on years of schooling in the first-stage regression is positive and statistically significant in the pooled subsample.

In practice, we start by estimating equation (2) for each country in the sample and selecting a small group of countries with similar values of ϕ.[9] For instance, in the case of females, we select Austria, Italy, Greece, and Portugal. Next, we estimate in turn the augmented versions of equations (1) and (2)—which include interactions of schooling and compulsory schooling with country dummies—on the pooled sample of these four countries, and we test for pooling. If we cannot reject the null hypothesis that the countries can be pooled with respect to coefficients ϕ and θ, we augment the pooled sample with another country and repeat our tests. We keep adding one country at a time until we find evidence against additional pooling.[10] Our final sample contains nine countries in the case of females (Austria, Denmark, Germany, Greece, Italy, Portugal, Spain, Sweden, and the United

[9] The sample comprises the following countries: Austria, Belgium, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Portugal, Spain, Sweden, and the United Kingdom. See the appendix for further details.

[10] If more than one country passes the pooling test, we add first the country that yields the highest p-values in the tests. An alternative procedure that yields the same results is to start with the full sample and to eliminate sequentially the countries with statistically significant interactions.

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Kingdom) and seven countries in the case of males (Austria, Denmark, Germany, Greece, Italy, Sweden, and the United Kingdom). In the rest of this article, we shall restrict our attention to these two subsamples.

The linear specification in equation (1) summarizes the behavior of BMI at fixed levels of the covariates using a measure of central tendency (the conditional mean) and assumes that the marginal effect of schooling on BMI is constant. Provided that the impact of education is constant at different levels of BMI, focusing on the conditional mean does not produce any loss of relevant information and the average causal effect is the only parameter of interest. We identify this effect by relying on the theoretical results by Angrist and Imbens (1994) and by using the variation in the number of years of compulsory schooling induced by educational reforms.[11]

The social and political attention drawn by overweight and obesity suggests that we augment model (1) and (2) with the additional equation:

$$D = 1[\mathrm{BMI}_{ics} \geq \omega], \tag{3}$$

where D is a dummy equal to one if individual BMI is above the threshold ω and zero otherwise.[12] It is useful to write equation (1) more compactly as $\mathrm{BMI}_{ics} = Y_{ics}\pi + \theta S_{ics} + \varepsilon_{ics}$,, where the vector Y includes the dummies and age trends, and to assume (*a*) $\varepsilon_{ics} = \rho v_{ics} - e_{ics}$, where e_{ics} is independent of YCOMP and normally distributed with zero mean and variance σ^2; (*b*) the error term ε_{ics} has unit variance. Under these assumptions, the probability of being overweight or obese is

$$\Phi\left(\frac{Y_{ics}\pi + \theta S_{ics} + \rho v_{ics} - \omega}{\sigma}\right),$$

where ϕ is the standard normal distribution.[13]

B. Empirical Setup

Our empirical setup can be defined as a "pooled regression discontinuity design." For each country, we construct a pretreatment and a posttreatment sample as follows. First, we select one school reform affecting compulsory schooling and identify a pivotal birth cohort $\bar{c}_k$, defined as the first cohort potentially affected by the change in mandatory school–leaving age. Second,

[11] In this setup, the average causal effect can be identified only for the subpopulation of compliers, i.e., for those individuals who have changed their educational attainment because of the mandatory schooling reforms.

[12] We consider two threshold values, namely, $\omega = 25$ and $\omega = 30$, and study the conditional prevalence of overweight and obese individuals in the population, respectively. An extension of this analysis to the entire distribution of BMI using quantile regressions is presented in an earlier version of this article (Brunello, Fabbri, and Fort 2009).

[13] The parameters of the model can be estimated using two different approaches: (*a*) a maximum likelihood estimator (MLE); (*b*) a two-step estimator (TW). Since the

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we define $C - \bar{c}_k$ as the distance between the year-of-birth of cohort C and that of the pivotal cohort, and we include in the pretreatment and posttreatment samples the individuals born at most 7 years before or 7 years after the pivotal cohort.[14] The width of the window is designed to exclude the occurrence of other compulsory school reforms, which would blur the difference between pretreatment and posttreatment in our data. Our choice also trades off the increase in sample size with the need to reduce the risk that unaccounted confounders affect our results.

By construction, the number of years of compulsory education, YCOMP, "jumps" in correspondence of the pivotal cohort and is higher in the posttreatment sample. The timing and intensity of these jumps varies across countries, and we use this and the within-country exogenous variation in YCOMP to identify the causal effects of schooling on BMI. Table 1 shows for each country in our sample the selected reform, the year-of-birth of the pivotal cohort, the change in minimum school-leaving age and in years of compulsory education induced by the reform, the minimum expected school attainment, expressed in terms of the ISCED classification, and the school-entry age.

For each country in our sample, we select a reform that occurred mainly during the 1960s and the 1970s and affected individuals at roughly the same level of education. In our choice, we are careful to guarantee that the youngest individual in each window around the pivotal cohort is at least 28 years old, an age when full-time education has typically been completed. Additional details on each selected reform are provided in the appendix. These reforms increased the minimum school-leaving age by 1 year in Austria, Germany, Sweden, and Britain; by 2 years in Denmark, Portugal, and Spain; and by 3 years in Greece and Italy. In Germany, the introduction of the reform varied by state (Länder). In the United Kingdom, we distinguish between England and Wales, on the one hand, and Scotland, on the other hand, because of the different timing of the reform that raised minimum school-leaving age from 15 to 16 in the 1970s.[15]

IV. The Data

We pool together data drawn from the 1998 wave of the European Community Household Panel (ECHP), the second release of the second wave of the Survey of Health, Ageing, and Retirement in Europe (SHARE) for the years 2006 and 2007, the English Longitudinal Survey of Ageing (ELSA) for the years 2008 and 2009, the 2002 wave of the German Socioeconomic

[14] As explained later in the text, we use narrower windows for Portugal and Sweden.

[15] A good description of this and previous reforms in the United Kingdom is in Clark and Royer (2010). We exclude Northern Ireland from our sample because of the very few observations.

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Table 1
School Reforms in Nine European Countries

Country	Year of Reform	First Affected Cohort	Change in Minimum School-Leaving Age	Change in Years of Compulsory Schooling	Minimum Expected Qualification	School-Entry Age
Austria	1962	1947	14 ⇒ 15	8 ⇒ 9	ISCED2	6
Denmark	1971	1957	14 ⇒ 16	7 ⇒ 9	ISCED3	7
Germany:						
Schleswig-Holstein	1956	1941	14 ⇒ 15	8 ⇒ 9	ISCED3	6
Hamburg	1949	1934	14 ⇒ 15	8 ⇒ 9	ISCED3	6
Niedersachsen	1962	1947	14 ⇒ 15	8 ⇒ 9	ISCED3	6
Bremen	1958	1943	14 ⇒ 15	8 ⇒ 9	ISCED3	6
Nordrhein-Westfalen	1967	1953	14 ⇒ 15	8 ⇒ 9	ISCED3	6
Hessen	1967	1953	14 ⇒ 15	8 ⇒ 9	ISCED3	6
Rheinland-Pfalz	1967	1953	14 ⇒ 15	8 ⇒ 9	ISCED3	6
Baden-Wurttemberg	1967	1953	14 ⇒ 15	8 ⇒ 9	ISCED3	6
Bayern	1969	1955	14 ⇒ 15	8 ⇒ 9	ISCED3	6
Saarland	1964	1949		8 ⇒ 9	ISCED3	6
Greece	1975	1963	12 ⇒ 15	6 ⇒ 9	ISCED2	6
Italy	1963	1949	11 ⇒ 14	5 ⇒ 8	ISCED2	6
United Kingdom:						
Scotland	1976	1961	15 ⇒ 16	10 ⇒ 11	ISCED3	5
England, Wales, Northern Ireland	1972	1957	15 ⇒ 16	10 ⇒ 11	ISCED3	5
Spain	1970	1957	12 ⇒ 14	6 ⇒ 8	ISCED2	6
Portugal	1964	1956	12 14	4 ⇒ 6	ISCED2	8
Sweden	1962	1950	14/15 ⇒ 15/16	8 ⇒ 9	ISCED3	6/7

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Panel (SOEP), and the 2003 wave of the British Household Panel Survey.[16] Since Portugal experienced a second school reform in 1968, 4 years after the 1964 reform, the window of observation for this country is shorter (3 years before and after the critical cohort). This window is shorter than standard also for Sweden (4 years before and after the critical cohort), because of the small number of observations in the upper tail of the standard window.

Our dependent variable is the body mass index (BMI), defined as weight in kilograms divided by the square of height in meters (kg/m^2). The BMI, albeit somewhat crude, has been found to be highly correlated with more precise (and more costly to collect) measures of adiposity.[17] In all our data sources individual height and weight are self-reported. As such our measure of BMI may be affected by measurement error, with heavier persons more likely to underreport their weight (see Burkhauser and Cawley 2008). Notice, however, that Sanz-de-Galdeano (2007) finds that the rank correlation between country-level self-reported and objective measures of weight is very high. Following Hamermesh (2009), we only consider individuals with a BMI in the range 15–55.

The standard measure of education used in this article and in most of the relevant literature is the number of years of schooling (S), which we compute using the available information on the age when full-time education was stopped and the highest level of education was attained. Table 2 reports average BMI, years of schooling, years of compulsory education, age, and the number of observations in the sample by country and gender. Due to the different timing of the selected reforms, our sample contains oldest individuals from Austria, Germany, Sweden, and Italy, and youngest ones from Greece. Portugal has the lowest level of education, and Denmark has the highest. Average BMI is highest in the United Kingdom and lowest in Denmark. Average BMI is equal to 25 for females and 26.5 for males, close to the 60th and the 55th percentile, respectively. Since median BMI is 24.21 for females and 26.06 for males, the unconditional distribution is not symmetric.

To identify the causal relationship between education and BMI we need to control as accurately as possible for additional factors affecting the dependent variable. We include in the empirical specification country, year-of-birth, and survey dummies. Furthermore, we control for smooth changes

[16] The ECHP is a panel of European households. We choose the 1998 wave so as to maximize the number of observations in the sample. These data do not contain information on BMI for key countries such as Germany and the United Kingdom. For these countries, we select national surveys, using waves that include information on BMI. The original data set includes also the 2003 wave of the French *Enquête sur la Santé.*

[17] Other anthropometric methods for measuring an individual's body fat include the waist-hip ratio, sagittal abdominal diameter, and skin folds thickness. These and other more accurate measures all require some instrumental measurement that is usually not viable in social surveys.

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Table 2
Summary Statistics

	BMI		Years of Education		Age		No. Observations	
Country	Females	Males	Females	Males	Females	Males	Females	Males
Austria	25.7	26.8	11.0	12.1	51.3	51.1	734	682
Denmark	24.1	25.6	13.6	13.0	43.3	43.3	749	741
Germany	25.1	26.6	12.2	12.9	49.3	49.5	2,610	2,531
Greece	24.2	26.0	11.6	12.1	36.6	35.2	1,269	1,065
Italy	24.8	26.3	9.5	10.6	50.9	50.6	2,197	2,089
Portugal	25.4		6.8		42.3		474	
Spain	24.2		9.7		42.2		1,626	
Sweden	25.2	26.5	11.7	11.3	54.2	54.6	447	378
United Kingdom	26.4	27.3	12.7	13.0	47.0	46.7	1,766	1,527
All	25.0	26.5	11.1	12.2	46.6	47.4	11,872	9,013

NOTE.—Data for Portugal and Sweden refer to the cohorts born between 3 years before and 3 years after and between 4 years before and 4 years after the year-of-birth of the first affected cohort. Data for all other countries refer to the cohorts born between 7 years before and 7 years after the year-of-birth of the first affected cohort. See the text for more details. In the table we exclude records with missing values for the variables used in the estimates.

over time in both education and BMI with a second-order polynomial in age and its interactions with country dummies.[18]

The empirical distribution of years of schooling typically shifts to the right after the reforms, suggesting that the proportion of individuals attaining relatively low education declines among the younger cohorts. To check whether this shift is partially induced by compulsory school reforms, we purge years of schooling from the influence of exogenous controls and cohort effects and plot the residuals in figure 1 for those cohorts born before and after the pivotal. The upward jump at the time of the reforms is remarkable, and it corresponds to about 0.6 years for females and between 0.4 and 0.5 for males for each additional year of mandatory schooling prescribed by law.

V. The Effects of Schooling on BMI

We organize the presentation of results as follows. First, we discuss the estimates of the first-stage equation (eq. [2]). Second, we present the OLS and IV estimates of equation (1). Finally, we turn to the estimates of the probabilities of being overweight and obese.

[18] The relatively low order of the polynomial follows the suggestions by Lee and Card (2008). Compared to higher order polynomials, the second-order specification is the most parsimonious and provides adequate fit of the data. In the baseline empirical specification, we use common age trends for Germany and the United Kingdom. Our sensitivity analysis shows that results are unaffected if we use a less parsimonious specification with state specific trends for these two countries.

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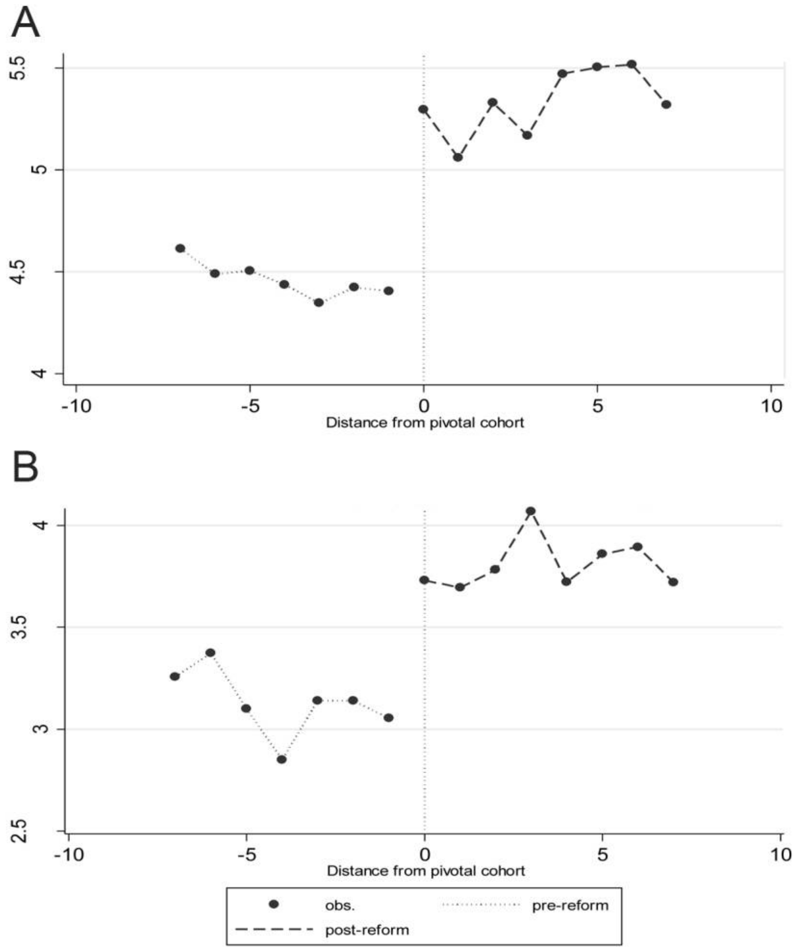

FIG. 1.—Effect of the years of compulsory education (YCOMP) on average years of schooling, net of exogenous controls, by gender: *A*, Females; *B*, Males.

A. The First-Stage Effect of Compulsory Education on Years of Schooling

Table 3 shows the estimates of equation (2) separately by gender. Since the dependent variable *S* varies across individuals while the instrument YCOMP varies only by country and cohort of birth, we take the different levels of aggregation into account by clustering standard errors by country and cohort (see Moulton 1990).[19] Pischke and von Wachter (2008) use a similar clustering strategy in their study of the effects of compulsory school reforms in Germany. The last row in the table reports the *p*-value of the

[19] There are 236 clusters for females and 212 clusters for males.

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Table 3
First-Stage Effects

	Females	Males
Years of compulsory education (YCOMP)	.570**	.422**
	(.065)	(.059)
Observations	11,872	9,013
F-test for instrument weakness	76.73	50.34
Males-test for pooling (*p*-value)	.105	.204

NOTE.—Dependent variable is years of schooling (*S*). Robust standard errors, clustered by country (state for Germany and the United Kingdom) and year-of-birth are in parentheses. All regressions include survey year, country (state for Germany and the United Kingdom), year-of-birth dummies, and a country-specific second-order polynomial in age. The sample for males includes Austria, Denmark, Germany, Greece, Italy, Sweden, and the United Kingdom. The sample for females also includes Portugal and Spain.
** Statistically significant at the 1% level.

pooling test, which verifies, for the selected sample, the null hypothesis that the interactions of country dummies with years of compulsory education are jointly equal to zero. This value is equal to 0.105 for females and 0.204 for males. Therefore, we reject the null at the 5% level of confidence for both genders.

The first row in the table reports the marginal effect of 1 additional year of compulsory education on actual years of schooling. As anticipated by figure 1, this effect is 0.570 years for females and 0.422 years for males. We test whether our selected instrument is weak by comparing the *F*-statistic for the exclusion of YCOMP from the first-stage regressions with the rule of thumb indicated by Staiger and Stock (1997), suggesting that the *F*-statistic should be greater than 10 for weak identification not to be considered a problem. For both genders, the estimated value of the *F*-statistic is above 50, telling us that the instrument can be assumed to be relevant.

The higher marginal effect found for females as compared to males can be ascribed to the fact that in the 1960s and early 1970s European females typically had a lower level of education than males.[20] Therefore, they were more likely to be affected by changes in compulsory school reforms and to have a higher percentage of compliers. Our estimates are higher than those found in previous studies that use compulsory schooling reforms to investigate the returns on education. Since the less educated are more likely to be affected by changes in minimum school-leaving age and less likely to be employed, the higher marginal effect found in this study can be explained at least in part by the fact that previous studies normally considered only employees rather than all individuals, as we do here.[21]

[20] According to the Barro and Lee databank (available at www.nber.org), the average years of schooling attained by males in the nine countries under study in 1965 were about half a year higher than the average years attained by females.

[21] See the Technical Appendix in Brunello, Fort, and Weber (2009) for a survey of this literature.

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To check the sensitivity of the results in table 3 to alterations in the baseline specification, we (*a*) drop the country-specific quadratic age trends, (*b*) drop the year-of-birth dummies, (*c*) use state-specific trends in age for Germany and the United Kingdom, (*d*) add covariates that capture potential confounding factors, which may alter the incentive to invest in education at the time of the reform but independently of it and affect at the same time the outcome BMI by influencing body weight when at school,[22] (*e*) exclude from the estimation sample one country at a time, and (*f*) present estimates for the original sample consisting of 13 countries. As shown in table A1 in the appendix, the estimated marginal effect of compulsory schooling on years of education increases when we drop the trends in age and declines when we consider the original sample of 13 countries for both genders. The exclusion of countries from the sample produces significant variations in the estimated coefficient only when we drop Italy.[23] In this case, the estimated marginal effect drops by close to 12% and 25% for females and males, respectively.

Our identification strategy relies upon the comparison of younger individuals who are affected by school reforms with older individuals who are not. Provided that mortality increases with age and decreases with education, our control group may contain a larger share of survivors, thus being not fully representative of the population of individuals not affected by school reforms. Since survivors are typically healthier and more educated and have lower BMI than the deceased, we may underestimate the causal effect of education on BMI. We investigate this implication of our setup by using two alternative strategies: first, we add a measure of gender and country-specific life expectancy to our set of regressors; second, we run weighted regressions and give lower weights to individuals aged above their life expectancy.[24] These estimates are reported in the last two rows of tables A1 and A2 in the appendix and should be compared to our baseline estimates without Greece because the relevant life expectancy data are not available for this country. We find that the estimates are very similar to the baseline, and we conclude that selective mortality does not affect our results.

[22] These covariates are the unemployment rate and the log real GDP per capita at time $b + a$, where b is year of birth and a the age affected by minimum school-leaving age reforms.

[23] As shown in table 2, Italy accounts for a relatively high share of the total number of observations in our sample, second only to Germany.

[24] Following Fort, Schneeweis, and Winter-Ebmer (2011), we define weights as weight = 1/(age − life-expectancy) if age is higher than life expectancy, and as one otherwise. The individuals who receive smaller weights in our sample are from Austria, Germany, and Italy. The life expectancy data are from the Human Mortality and Human Life table databases, provided by the Max Planck Institute for Demographic Research (www.demogr.mpg.de) and consist of period measures of life expectancy at birth.

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B. The Causal Impact of Years of Schooling on Adult BMI

Table 4 presents the ordinary least squares (OLS) and instrumental variables (IV) estimates of equation (1) separately by gender. Considering first the OLS estimates, we find that the estimated association between BMI and years of schooling is negative and statistically significant. Moreover, the size of the association is about three times as large in absolute value for females as for males, and it is similar for females to that estimated by Cutler and Lleras-Muney (2010) for US whites aged over 25 (−0.190). Our estimated effects are also smaller and larger in absolute value than those reported by Grabner (2008) for US females and males.

In the case of females, the IV estimates of the impact of years of schooling on BMI are larger in absolute value than the OLS estimates (−0.414 vs. −0.218), a result not new in this literature (see Grossman [2008] for a discussion),[25] and one that is statistically significant at the 1% level of confidence. Our findings imply that a 10% increase in years of schooling, which corresponds roughly to 1 additional year, reduces the BMI of females by 1.84%, a moderate effect when compared to the 4% decline estimated by Grabner for US females using compulsory school reforms to instrument years of schooling, as we do. One possible explanation is that the distribution of BMI in the United States has a longer and fatter tail compared to that of Europe. Therefore, the IV estimates are not estimated at the same point in Europe and the United States and are larger in the United States if education has a stronger protective effect in the upper part of the distribution of the BMI.

In the case of males, the effects of schooling on BMI are equal to zero and imprecisely estimated.[26] In Section VI below, we discuss mechanisms that could help explain why education may have a protective role for females but not for males. At the bottom of table 4, we present the results of two tests. The former test verifies the hypothesis that the interactions of country dummies with years of schooling are jointly equal to zero, and the latter

[25] When individuals face different health returns to education, IV estimates reflect the marginal return for the group affected by school reforms (compliers). Reasons why the OLS estimates are lower (in absolute value) than IV estimates include (*a*) years of schooling are measured with error and (*b*) BMI is negatively correlated with unobserved ability, which is positively correlated with schooling.

[26] Grabner also finds no statistically significant protective effect of education on the BMI of US males. Apparently in contrast with our results, Clark and Royer (2010), when using the 1972 school reform in Britain, find no statistically significant protective effects in their pooled sample of males and females. To compare their results with ours, we run our estimates for Britain only on the pooled sample of males and females and confirm the absence of a statistically significant relationship. However, when we estimate equation (1) separately for males and females, we find that education has a statistically significant protective effect for the latter but not for the former.

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Table 4
Ordinary Least Squares and Instrumental Variable Estimates

	Females		Males	
	OLS	IV	OLS	IV
Years of schooling (S)	−.218**	−.414**	−.073**	.0003
	(.012)	(.149)	(.010)	(.178)
Elasticity of BMI to years of schooling at sample mean	−.097**	−.184**	−.033**	.0001
	(.005)	(.066)	(.005)	(.082)
Mean of the dependent variable	25.02		26.49	
F-test for pooling 2SLS estimates (*p*-value)	.546		.981	
F-test for pooling reduced form estimates (*p*-value)	.301		.109	
Observations	11,872		9,013	

NOTE.—Dependent variable is BMI. Robust standard errors, clustered by country (state for Germany and the United Kingdom) and year-of-birth are in parentheses. All regressions include survey year, country (state for Germany and the United Kingdom), year-of-birth dummies, and a country-specific second-order polynomial in age. The sample for males includes Austria, Denmark, Germany, Greece, Italy, Sweden, and the United Kingdom. The sample for females also includes Portugal and Spain.
** Statistically significant at the 1% level.

test verifies a similar null hypothesis against the alternative of heterogeneous effects in the reduced form regression of BMI on the instrument and other controls. The results suggest that in both cases we cannot reject the null, that is, we find no evidence of cross-country heterogeneity in the IV estimates and intention-to-treat effects, both for males and for females.

We check the sensitivity of these results by replicating in table A2 the robustness exercises shown in table A1 for the first-stage estimates. In the case of females, the protective effect of education is robust to all specification changes, including those dealing with selective mortality, although the size of the effect is lower in absolute value when we drop year-of-birth dummies or Italy, or when we consider the original sample of 13 countries. For males, the effect of education on BMI is always imprecisely estimated and is sensitive to the elimination of year-of-birth dummies or of countries such as Italy or the United Kingdom. Therefore, we conclude that results for males are rather inconclusive.

C. Probit Estimates

We estimate probit models by treating schooling either as exogenous (the odd columns in table 5) or as endogenous (the even columns in the table), using years of compulsory schooling as the instrument. In the case of overweight, the evidence suggests that a 10% increase in years of schooling reduces the probability of being overweight by 1.80 for males and 6.68% for females when schooling is treated as exogenous, and by 0.63% and 11.37%, respectively, when it is treated as endogenous. In the case of obesity, these elasticities tend to be higher in absolute value but imprecisely estimated.

The comparison of the conditional mean effects in table 4 with the estimates in table 5 is informative of the presence of heterogeneous effects of

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Table 5
Probit Models for Probability of Being Overweight (BMI ≥ 25) and Obese (BMI ≥ 30)

	Females				Males			
	Overweight		Obese		Overweight		Obese	
	Probit	IV Probit	Probit	IV Probit	Probit	IV Probit	Probit	IV Probit
Years of schooling	−.063**	−.106**	−.065**	−.077	−.025**	−.009	−.029**	−.011
	(.004)	(.040)	(.006)	(.053)	(.004)	(.068)	(.005)	(.081)
Marginal effects of years of schooling at sample mean	−.025**	−.042**	−.012**	−.014	−.009**	−.003	−.006	−.002
	(.002)	(.017)	(.001)	(.010)	(.001)	(.026)	(.001)	(.018)
Elasticity of BMI to years of schooling at sample mean	−.668**	−1.137**	−1.264**	−1.483	−.180**	−.063	−.560	−.221
	(.044)	(.455)	(.114)	(1.031)	(.027)	(.492)	(.105)	(1.592)
Mean of the dependent variable	41.57		12.50		63.39		14.33	
Observations	11,872		11,872		9,013		9,013	

Note.—Robust standard errors, clustered by country (state for Germany and the United Kingdom) and year-of-birth are in parentheses. All regressions include survey year, country (state for Germany and the United Kingdom), year-of-birth dummies, and a country-specific second-order polynomial in age. The sample for males includes Austria, Denmark, Germany, Greece, Italy, Sweden, and the United Kingdom. The sample for females also includes Portugal and Spain.

** Statistically significant at the 1% level.

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schooling on BMI at different points of the distribution. To illustrate why, consider overweight females, for whom the estimates are sufficiently precise. If the effects of 1 additional year of schooling were the same across the distribution of BMI (i.e., −0.41; see table 4), only the individuals who had, before the increase of schooling, a BMI between 25 and 25.41 would cease to be overweight because of the policy. Since this group corresponds to 3.8 percentage points of the relevant population, the percentage of overweight females would decline by the same amount, but below the estimated decline (−4.2 percentage points according to the estimated marginal effect reported in table 5). This comparison suggests that the protective effect of education on BMI is slightly larger in the upper part of the distribution of BMI.[27]

VI. Discussion

We have shown that education exerts a causal protective role on the BMI of European females but no (statistically significant) effect on the BMI of males. Although results are precise for females and inconclusive for males, it still makes sense to discuss potential mechanisms and sources of gender differences in the protective effect of education. We focus, in this final section of the article, on (*a*) income and employment, (*b*) physical activity, (*c*) depression, and (*d*) fertility.

Using European data, Sanz-de-Galdeano (2005) finds that obesity declines with household income for women but not for men. Garcia and Quintana-Domeque (2009) decompose household income into own labor earnings and other household earnings and find that gender differences in the relationship between obesity and income are driven by the negative relationship between BMI and own labor earnings for females. They argue that lower-income individuals tend to be overweight because they often meet nutritional needs with high-calorie food, which tends to be cheaper than low-calorie food.

By raising the probability of employment, education can affect overweight and obesity because it increases income and also provides access to healthier neighborhoods, peers, and friends (see Lochner 2011). To investigate the causal effect of education on the probability of employment, we estimate linear probability models by gender and adopt the same specification of tables 4 and 5, using years of compulsory education as instrument for years of education. As reported in the first two columns of table 6, we find that while education significantly increases the probability of being em-

[27] The same argument applied to obese females yields similar results. One can look at heterogeneous effects in more detail by estimating instrumental variables quantile treatment effects. Brunello, Fabbri, and Fort (2009), e.g., use the approach by Chernozhukov and Hansen (2005) and find that the response of BMI to education is largest in absolute value among mildly overweight females, who are located between the 55th and the 70th percentile of the distribution of BMI.

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Table 6
Effects of Education on the Probability of Being Employed, the Probability of Doing Vigorous Activities at Least Once a Week, and the Probability of Being Depressed: IV Estimates of Linear Probability Models by Gender

	Employment		Vigorous Activities		Depression	
	Females (1)	Males (2)	Females (3)	Males (4)	Females (5)	Males (6)
Years of schooling	.080**	.052*	.063*	−.029	−.042+	−.032
	(.018)	(.021)	(.024)	(.044)	(.023)	(.036)
Sample mean of dependent variable	.59	.83	.58	.64	.32	.19
Semi-elasticity of the outcome to years of schooling at sample mean	.133**	.061*	.108**	−.045	−.132+	−.170
	(.03)	(.024)	(.042)	(.070)	(.072)	(.188)
Observations	11,745	8,974	3,651	2,513	3,642	2,506

Note.—All regressions include survey year, country, year-of-birth dummies, and a country-specific second-order polynomial in age. Columns 1 and 2 are based on a sample that includes nine countries for females and seven countries for males and uses data from different surveys (see note to table 3 for the list of countries and the text for more details). Columns 3–6 refer to the smaller sample of countries in the SHARE survey, which includes Austria, Denmark, Germany, Greece, Italy, and Sweden for males and also Spain for females. Robust standard errors clustered by country (state for Germany and the United Kingdom) are in parentheses.
+ Statistically significant at the 10% level.
* Statistically significant at the 5% level.
** Statistically significant at the 1% level.

ployed for both genders, the percent increase in this probability due to an additional year of schooling is much larger for females than for males (13.3% vs. 6.1%). There is also evidence that education has a stronger causal effect on the earnings of females: using data for 12 European countries and an instrumental variable approach similar to the one adopted here, Brunello, Fort, and Weber (2009) estimate returns to education in the range of 4.3 to 5.9 for males and 6.4 to 7.9 for females. Overall, this evidence suggests that a candidate source of the protective effect of education on the BMI of females—and of the absence of such role for males—is its stronger impact on women's income and employment.

The relationship between education, job strenuousness, and BMI has been already investigated in the literature (see, e.g., Lakdawalla and Philipson 2007). Since strenuous jobs are typically filled by men in agriculture and industry, an increase in education that shifts individuals to more sedentary occupations in the service sector is likely to reduce strenuousness to a higher extent for males than for females, with stronger negative effects on the BMI of the former. In support of this view, empirical results by Böckerman et al. (2008) for Finland suggest that, while moving from a physically demanding occupation to a sedentary one increases male BMI, other things being equal, the contribution of changes in the occupational structure is definitely smaller for females.

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One may object that the reduction in job-related exercise induced by higher education can be compensated for by exercise off the job. To investigate the effect of education on physical activities, both on and off the job, we use a question in the SHARE portion of our data set that asks interviewed individuals whether they have engaged at least once a week in vigorous physical activities, such as sports, heavy housework, or a job that involves intense physical efforts. We regress the probability of having engaged in any of these activities on education and our standard set of covariates, using a linear probability model and years of compulsory education to instrument education. As shown in table 6 (cols. 3 and 4), we find that an additional year of education causes a 10.8% increase in the share of females and a 4.5% reduction in the share of males engaged in vigorous activities.[28] Since vigorous activities increase calorie expenditure, these findings point to an additional reason why additional education is protective for females but not for males.

Turning to the role played by depression, it is well established in the literature that women are about twice as likely as men to suffer from it (Noel-Hoeksema 1990; Borooah 2010) and that depression might contribute to overweight and obesity (see, e.g., Bline 2008; Markovitz, Friedman, and Arent 2008). We examine whether education has a causal impact on depression by using the SHARE portion of our data set, which includes a question on whether the interviewed individual has suffered since the last interview of symptoms of depression that lasted at least 2 weeks. We construct a dummy variable equal to one for positive answers and zero otherwise and regress this variable on our set of covariates and education, using again a linear probability model and years of compulsory education to instrument years of schooling. As reported in the columns 5 and 6 of table 6, we find that education reduces depression both for males and for females, with rather similar quantitative effects. These estimates, however, are imprecise.

Finally, a further reason for education being protective for females but not for males may be that less educated women have more children and are more likely than better educated females to retain their pregnancy weight gains. The empirical evidence on the causal effects of education on fertility does not clearly indicate, however, that better educated females have lower fertility (see, e.g., McCrary and Royer [2011] for the United States, and Monstad, Propper, and Salvanes [2008] and Fort et al. [2011] for Europe).

Overall, the evidence presented in this section supports the effects of education on income, the probability of employment, and the frequency of vigorous activities, both on and off the job, as candidate mechanisms explaining the protective role of education on the BMI of European females and the lack of such role for males.

[28] The effect of education on the vigorous activities of males is not statistically significant.

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VII. Conclusions

In this article, we have investigated the causal effects of education on the BMI of Europeans by adopting a multi-country framework rather than the single-country setup typical of previous contributions, in an effort to avoid the problems associated with instrument weakness. Our empirical findings can be summarized as follows. Education has a protective effect on the BMI of females living in nine European countries and reduces their probability of being obese or overweight. No statistically significant protective effect is found for males in a smaller sample of countries that excludes Spain and Portugal. The size of the estimated effect for females is not negligible but is smaller than the one found in recent comparable estimates for the United States. There is evidence that this effect is larger in absolute value in the upper part of the distribution of female BMI.

We have argued that candidate mechanisms that could explain gender differences in the protective effect of education include the effects of education on income, the probability of employment, and the frequency of vigorous activity on and off the job. Less promising mechanisms are differences in life expectancy and the relationship between education and depression and between education and fertility.

Our results suggest that general education campaigns that affect individuals with low education—who are particularly at risk of having unhealthy lifestyles—can play a role in reducing the importance of overweight and obesity among European females. Based on our results, we are less optimistic on the effects on European males. Notice, however, that the recent surge in the phenomenon, both in the United States and to a lesser extent in Europe, indicates that these campaigns alone are unlikely to turn the tide. Other policies, such as the establishment of standards and the introduction of appropriate taxes and subsidies, seem required if we intend to drastically reduce the prevalence of severe obesity.

Since our findings apply to a subset of European countries, a natural question to ask is whether they hold more generally for Europe. Our evidence suggests a positive answer to this question. On the one hand, our sample includes a rather broad group of European countries, some from central or northern Europe (Germany, Austria, Denmark, Sweden, and the United Kingdom) and some from southern Europe (Spain, Italy, Greece, Portugal, and Spain). On the other hand, we have shown in table A2 that education retains a statistically significant protective effect on the BMI of females (at a 10% level of confidence) but has no statistically significant effect on the BMI of males even when we consider a broader sample of 13 European countries.

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Appendix

The Educational Reforms Used in This Study

In this appendix we provide a brief description of the educational reforms considered in the baseline specification of this study and motivate the choice of the first cohort potentially affected by each reform. For the description of the reforms in Belgium, Finland, France, and Ireland, see Fort (2006), Brunello, Fabbri, and Fort (2009), and Brunello, Fort, and Weber (2009).

Austria

The 1962 School Amendment Act increased compulsory schooling by 1 year, from 8 years to 9 years. Pupils who were 14 years old or younger at the time the reform was introduced were compelled to attend an additional year of schooling. This suggests that the individuals potentially affected by the reform are those born in 1948 and afterward. However, individuals born in 1947 who might have already left school when the reform was introduced were required to go back to school and complete the additional year. Therefore, we select the cohort born in 1947 as the first cohort potentially affected by the reform.

Denmark

In 1971 compulsory schooling in Denmark was increased from 7 years to 9 years. Pupils born before 1957 completed compulsory schooling in 7 years, whereas individuals born from 1957 onward had to spend 9 years in full-time education.

Germany

In Germany, the year when minimum school-leaving age was increased from 8 years to 9 years, varied across the different Länder, as explained by Pischke and von Wachter (2005), and ranged between 1949 for Hamburg and 1969 for Bayern. Consequently, the first cohort potentially affected also varied. We adopt in this article the classification used by Pischke and von Wachter in their table 1.

Greece

In 1975 the Greek Parliament increased compulsory education by 3 years (from 6 years to 9 years). Individuals born in 1963 or later were compelled to attend 3 additional years of schooling, whereas those born in 1962 were not. See Gouvias (1998) for further details.

Italy

Junior high school became effectively compulsory in Italy only since 1963. Starting from that year, the minimum school-leaving age rose from

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5 years to 8 years. According to Brandolini and Cipollone (2002), the individuals potentially affected by the reform were those born after 1949.

Spain

The compulsory school reform considered in this study was carried out under the 1970 General Act on Education and Financing of Educational Reform and increased compulsory years of education from 6 years to 8 years. Individuals potentially affected by the reform were those born in 1957 and after (see Pons and Gonzalo [2002], 753 and table A1, 767).

Portugal

The 1964 the law established 6 years of compulsory schooling (up to age 14), thus increasing the previous limit by 2 years. The individuals potentially affected are those born after 1956. See Vieria (1999) for details.

Sweden

Compulsory school reform in Sweden was gradually implemented between 1949 and 1962. The take-up of the experiment varied over the period 1949–62 across municipalities, with the largest number of municipalities involved in the years 1961 and 1962 (39.4%; 18,665 classes; 436,595 students). Unfortunately, we do not have access to data at the municipality level, only at the county level. For the purposes of this article, and based on personal communication with Marten Palme, we consider as potentially affected by the reform all the individuals born after 1950.

The United Kingdom

The 1972 reform in the United Kingdom (Statutory Instrument No. 444) increased the minimum school-leaving age to 16 on September 1, 1972 (1976 in Scotland). According to Clark and Royer 2010, the first affected cohort was born in 1957.

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Table A1
First-Stage Effects: Sensitivity Analysis by Gender

	Female	Males
Baseline estimate in table 3	.570**	.422**
	(.065)	(.059)
With macroeconomic controls	.565**	.411**
	(.058)	(.057)
With state trends for Germany and United Kingdom	.559**	.446**
	(.066)	(.067)
No trends	.780**	.606**
	(.049)	(.056)
No year-of-birth dummies	.619**	.461**
	(.089)	(.062)
13 countries	.369**	.251**
	(.065)	(.044)
Greece excluded from baseline	.594**	.418**
	(.076)	(.068)
Italy excluded from baseline	.495**	.319**
	(.071)	(.094)
Denmark excluded from baseline	.568**	.458**
	(.067)	(.062)
Portugal excluded from baseline	.569**	
	(.068)	
Spain excluded from baseline	.547**	
	(.073)	
Germany excluded from baseline	.586**	.353**
	(.070)	(.088)
United Kingdom excluded from baseline	.567**	.392**
	(.066)	(.062)
Austria excluded from baseline	.588**	.421**
	(.067)	(.063)
Sweden excluded from baseline	.578**	.441**
	(.074)	(.059)
With controls for life expectancy (Greece excluded)	.596**	.436**
	(.076)	(.070)
Weighted by excess life expectancy (Greece excluded)	.585**	.403**
	(.076)	(.069)

NOTE.—Robust standard errors, clustered by country (state for Germany and the United Kingdom) and year-of-birth are in parentheses. The baseline estimates (first row) include survey year, country (state for Germany and the United Kingdom), year-of-birth dummies, and a country-specific second-order polynomial in age. In the baseline estimates (first row), the sample for males includes Austria, Denmark, Germany, Greece, Italy, Sweden, and the United Kingdom. The sample for females also includes Portugal and Spain. The specification differs across rows according to what is indicated in the first column.

** Statistically significant at the 1% level.

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Table A2
IV Estimates: Sensitivity Analysis by Gender

	Females	Males
Baseline estimate in table 4	−.414**	.0003
	(.149)	(.178)
With macroeconomic controls	−.395**	−.015
	(.150)	(.186)
With state trends for Germany and United Kingdom	−.418**	.081
	(.154)	(.179)
No trends	−.271**	−.003
	(.079)	(.094)
No year-of-birth dummies	−.252*	−.102
	(.109)	(.124)
13 countries	−.265+	.054
	(.152)	(.156)
Greece excluded from baseline	−.538**	.065
	(.167)	(.207)
Italy excluded from baseline	−.314	−.606
	(.219)	(.370)
Denmark excluded from baseline	−.375*	.019
	(.152)	(.175)
Portugal excluded from baseline	−.413**	
	(.152)	
Spain excluded from baseline	−.449**	
	(.169)	
Germany excluded from baseline	−.443**	−.095
	(.168)	(.266)
United Kingdom excluded from baseline	−.458**	.108
	(.147)	(.197)
Austria excluded from baseline	−.376*	−.001
	(.148)	(.184)
Sweden excluded from baseline	−.394**	.03
	(.148)	(.173)
With controls for life expectancy (Greece excluded)	−.537**	.017
	(.166)	(.191)
Weighted by excess life expectancy (Greece excluded)	−.542**	.067
	(.168)	(.224)

NOTE.—Robust standard errors, clustered by country (state for Germany and the United Kingdom) and year-of-birth are in parentheses. The baseline estimates (first row) include survery year, country (state for Germany and the United Kingdom), year-of-birth dummies, and a country-specific second-order polynomial in age. In the baseline estimates (first row), the sample for males includes Austria, Denmark, Germany, Greece, Italy, Sweden, and the United Kingdom. The sample for females also includes Portugal and Spain The specification differs across rows according to what is indicated in the first column.
+ Statistically significant at the 10% level.
* Statistically significant at the 5% level.
** Statistically significant at the 1% level.

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