Years of Schooling, Human Capital and the Body Mass Index of Europeans

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PRELIMINARY DRAFT

- The ECHP data used in this paper are available at the Department of Economics, University of Padova, contract n.14/99. We are grateful to the audiences in Florence, Linz and Oxford for comments, to Caroline Lions and Bruno Ventelou for providing the French data used in the paper and to DIW Berlin for access to the German Socio-Economic Panel. Access to the BHPS data was granted by the UK Data Archive, University of Essex. The usual disclaimer applies. The results of the quantile regression analysis are generated using Ox Console version 5.00 (see Doornik, 2007) and the codes developed in OX by Christian Hansen for instrumental variable quantile regression (IVQR) and posted on his research web-page (see http://faculty.chicagobooth.edu/christian.hansen/research/).

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ABSTRACT

We use the compulsory school reforms implemented in European countries after the II World Word to investigate the causal effect of human capital on the Body Mass Index (BMI) and the prevalence of overweight and obesity in Europe. We depart from the current literature in two main directions. First, we complement the standard analysis of the causal impact of years of schooling on BMI with one relying on a broader measure for education, i.e. individual standardized cognitive tests as proxied by gender, cohort and country averages. Second, we ask whether the current focus on conditional mean effects is well placed, or whether it should be replaced by the analysis of heterogenous effects based on IV quantile regressions. We cannot reject the hypothesis that the quantity of education (years of schooling) can be considered as exogenous. Conversely, we find evidence of endogeneity and heterogenous effects when we consider the broader measure of education. Our evidence suggests that education matters for BMI more for females than for males. Depending on the sample and the estimation method, a 10 percent increase in the number of years of schooling is found to reduce the BMI of females by 0.95 to 2.63 percent, and the incidence of overweight females by 2.5 to 8.7 percentage points. We also find that a 10 percent increase in test scores can reduce the average BMI of females by 0.67 to 3.7% and the incidence of overweight by 13.45 percentage points.

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INTRODUCTION

Empirical evidence on the positive association between education and health, the so-called health-education gradient[1], is abundant. Feinstein, Sabates, Anderson, Sorhaindo and Hammond, 2006, after comprehensively reviewing the relevant literature conclude that the causal effects of education are "…particularly robust and substantive for the outcomes of adult depression, adult mortality, child mortality, child anthropometric measures at birth, self-assessed health, physical health, smoking (prevalence and cessation), hospitalizations and use of social health care." (p. 217).

Yet there are still relatively few studies that investigate the causal impact of education on adiposity and obesity, and with rather inconclusive results. The amplified risk associated with high BMI is not negligible for premature death and diseases such as diabetes, high blood pressure, arthritis and postmenopausal breast cancer (see US Department of Health and Human Services, 2001). Obesity is estimated to account for 5% to 7% of total health care costs (Finkelstein, et. al. 2005), and its indirect costs in terms of lost productivity for the 15 member countries of the European Union in 2002 have been estimated at € 33 billion, about 0.5% of Gross Domestic Product (GDP) (Fry and Finley, 2005). For these reasons the recent positive trend in the prevalence of overweight and obesity[2] have generated social and political concern both in the United States (US) and Europe.

The concern about the health implications of increasing adiposity and the state of the empirical evidence so far suggest that additional empirical research on the causal relationship

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[1] Cutler and Lleras-Muney, 2006, estimate that the health returns to education increase the total returns to education by at least 15 percent.

[2] In the US, the percentage of obese individuals in the population has almost doubled between 1990 and 2004 and is now above 30 percent. Europe is also on a rising trend, albeit at a slower pace than the US (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).

between education and the body mass index (BMI) is valuable. In this paper, we investigate this relationship by focusing on Europe. As in previous contributions, we use compulsory school reforms to identify causal effects. While the validity of these reforms as instruments is broadly accepted, there are concerns that they may be weak. This would partly explain the inconclusive results obtained so far (see Kenkel, Lillard and Mathios, 2006; Arendt, 2005). In an effort to address this issue, we depart from the current literature, which typically focuses on a single country, and adopt a multi-country framework. We collect data from different countries in Europe to exploit the variation across countries in the timing of compulsory school reforms and, by so doing, to improve our ability to distinguish school reforms from cohort effects[3].

Virtually all the empirical research in this area is concerned with whether education induces a location shift in BMI. In the presence of heterogeneity, however, the estimated effect of education on the conditional mean of BMI could be rather different from the effect at the lower and higher (conditional) quantiles of the distribution of BMI. Since policy interest concentrates on the overweight and the obese, we depart from current practice and investigate the impact of education on the whole conditional distribution of BMI, with particular attention to its upper quantiles.

In their assessment of the empirical literature, Feinstein, Sabates, Anderson, Sorhaindo and Hammond, 2006, argue that a weakness of the existing evidence is that "… much of the assessment of the effects of education has measured education in terms of years of schooling" (p.175). This approach, probably motivated by lack of data, ignores important dimensions of education, such as school quality, and restricts learning to post-adolescent emerging adulthood, thereby excluding lifelong learning.

[3] Brunello, Fort and Weber, 2009, use a similar approach.

In an effort to capture these additional dimensions, we investigate the relationship between BMI and a broader measure of education, i.e. individual standardized cognitive test scores, as proxied by gender, cohort and country averages. Since these scores are an output measure in the education production fuinction, they reflect both the quantity and the quality of full-time formal education, as well as the impact of subsequent lifelong learning[4].

Compared to the current literature, we find evidence that our instrument – the number of years of compulsory education - is not weak. We also confirm the common finding that instrumental variables estimates of the effect of education on BMI, overweight and obesity tend to be significantly larger than the estimates based on ordinary least squares. In addition, our results point to the fact that the impact of education on BMI, overweight and obesity tends to be larger among females than among males. Very few of our findings for males turn out to be statistically significant. Because of this, we only summarize in the next paragraphs our findings for females.

It turns out that the way we measure education, either with an input variable (years of schooling) or with an output variable (test scores), has important consequences for the empirical estimates of the relationship between education and BMI. In particular, the null hypothesis that education is exogenous is never rejected when we use years of schooling and always rejected when we use test scores. In our view, there are two potential – and tentative - explanations for this result: first, while years of schooling for most adults can be considered as pre-determined, test scores are taken at adult age and reflect not only formal learning when young but also subsequent learning and the depreciation of initial skills. It seems plausible that test scores are more exposed to the risk of reverse causation – running from current BMI to

[4] Hanushek and Wossmann, 2009, use a similar approach in their study of the relationship between education and growth.

current skills – than formal years of schooling. Second, our measure of test scores is more aggregated than individual data, and varies only by gender, country and year of birth. By taking average rather than individual data, we reduce variability in an artificial manner and therefore increase the precision of our estimates.

Depending on the sample – we use a larger dataset which includes at most 13 countries and a smaller dataset with 7 countries – and the estimation method, we find that a 10 percent increase in the years of schooling reduces female BMI by 0.85 to 2.63%, the incidence of overweight females by 2.39 to 8.73 percentage points and the incidence of obese females by 1.21 to 1.43 percentage points. These quantitative effects tend to be smaller than those recently found by Grabner, 2008, for the US[5].

When we measure education with average test scores, we find that a 10 percent increase in these scores – which is close to one standard deviation - reduces the BMI of females by 0.67% to 3.71%, depending on the estimation method. Moreover, the prevalence of overweight females declines by close to 1 percentage point when test scores are treated as exogenous and by a much larger 13.45 percentage points when they are treated as endogenous.

Turning to the quantile estimates, we cannot reject the hypothesis that years of schooling are exogenous. There is also evidence that the estimated reduction in BMI associated to a 10% increase in the quantity of education increases as we move from the bottom to the top quantile and is about four times as large at the 90th than at the 10th percentile. In sharp contrast, we reject the null of no endogeneity when education is measured by test scores, in line with the findings for the conditional means. When an output measure of

[5] Grabner finds that a one – year increase in years of schooling, which is equivalent to an 8% increase in our data – reduces BMi by 1 to 4% and the incidence of obesity by 2 to 4 percentage points.

education is used, we can also reject the hypothesis that the estimated coefficients are equal along the conditional distribution of BMI. It turns out that the estimated elasticity of BMI with respect to test scores is broadly similar below the median, but increases sharply above the median, where it reaches values that are about four times as large as the values below the median. If we compare elasticities, we observe that the size of the impact of a 10 percent increase in years of schooling on BMI at different quantiles is between 60 and 70 percent of the impact of a 10 percent increase in test scores.

Overall, these findings suggest two main conclusions: first, the current focus on locational shifts and conditional mean effects is likely to under-estimate the effect of education at the upper quantiles of the distribution of BMI, where the overweight and the obese concentrate. Second, it is important to consider alternatives to years of schooling in the measurement of educational attainment. Our finding that changes in test scores have often a significant impact on individual BMI is potentially relevant because it suggests that reductions in individual BMI can be attained with a broader set of policies, which affect the quality of learning and the opportunities to improve and maintain cognitive skills at older ages.

The paper is organized as follows: Section 1 reviews the literature. Section 2 presents the empirical model, the methodology and our empirical strategy. The data are introduced in Section 3 and the empirical results in Section 4. Conclusions follow.

1. Review of the Literature

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). More education provides access to better job opportunities in terms of higher monetary and non-monetary rewards. Higher monetary payoffs increase income and improve individual health because of the higher command over resources, including access to healthcare. Non-monetary dimensions of the rewards typically include safer workplaces and tasks and lower exposure to health hazards.

At the same time, better health reduces dropout rates and improves educational attainment and cognitive skills[6]. Therefore, a positive association between education and health can be due to the former causing the latter, to reverse causality, or it may be driven by unobserved third variables which affect both health and education, such as the rate of time preference, the attitude toward risk, mental ability and parental background[7]. The estimate of the causal impact of education on health requires the identification of exogenous sources of variation (instruments) which 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 which examine the causal impact of education on adiposity and 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 one more year of schooling than the former. Her 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 to 59. The endogeneity of education is addressed by using as instruments the Danish school reforms of 1958 and 1975, which affected kids who turned 14 in 1959 and 1976.

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[6] See for instance Ding, Lehrer, Rosenquist and Audrain-McGovern (2006) and Grossman (2004).
[7] See Cutler and Lleras Muney, 2009.

Because of the high standard errors associated to the IV estimates, his results are inconclusive. Clark and Royer, 2008, study the effects of the compulsory school reform of 1947 in the UK and find that the effects of education on BMI and obesity are statistically insignificant. 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 US as an instrument for education and finds that one extra year of schooling lowers individual BMI by 1 to 4% and the probability of being obese by 2 to 4 percentage points. His estimated effects are larger for females than for males.

Webbink, Martin and Visscher, 2008, use a sample of 5967 Australian twins older than 18, who have been 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 sub-sample of males – one additional year of schooling reduces both the likelihood to be overweight and individual BMI. No significant effect is found for females. Lundborg, 2008, also adopts a within-twins estimator, using data on 694 US twins aged 25 to 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, Lillard and Mathios, 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 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 that "having completed high school" does not have a statistically significant effect on the likelihood of being overweight.

Jurges, Reinhold and Salm, 2008, 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 A*bitur* 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 drafting procedures for U.S. 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 measures of adiposity and obesity. These studies adopt different identification strategies to take into account the endogeneity of education. Results are rather inconclusive, with many studies finding no statistically significant effect.

2. OUR APPROACH

We start from a very simple characterization of the relationship between BMI and human capital HC

$$BMI_i = \alpha X_i + \gamma HC_i + v_i \qquad [1]$$

where *X* is a vector of (exogenous) controls, *v* is the error term and *i* is the index for the individual. In this specification, we consider the overall impact of human capital on BMI, which can operate via income, occupational choice, social interaction effects and other mechanisms[8].

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[8] One mechanism is differential mortality by education. Lleras-Muney (2005) finds a large (positive) total effect of education on mortality.

We do not distinguish between direct and indirect effects, and avoid to elucidate which pathways are followed by cognitive skills to causally affect adiposity and obesity - see Cutler and Lleras-Muney, 2007, for a detailed discussion of these issues.

Following Hanushek and Wossmann, 2009, we posit that cognitive skills are affected by several factors, which include family inputs *F*, the quantity and quality of education received at schools, *S* and *Q* respectively, individual ability *A* and other factors *R* including labour market experience and learning on the job

$$HC_i = \lambda_1 F_i + \lambda_2 Q_i S_i + \lambda_3 A_i + \lambda_4 R_i + \mu_i \qquad [2]$$

As it happens in the empirical growth literature, the vast majority of empirical studies that investigates the impact of education on health uses the quantity of schooling *S* as a direct measure of *HC*, and therefore estimates

$$BMI_i = \alpha X_i + \theta S_i + \xi_i \qquad [3]$$

Let *YCOMP* be the number of years of compulsory education, which we use to instrument endogenous schooling *S*. The identification of the causal effect of schooling on BMI requires that the selected instrument affects the dependent variable only by influencing the quantity of schooling, with no direct influence on omitted measures of human capital, which include school quality *Q*.

As an alternative to [3] we could measure *HC* not as the exposure to educational inputs but as an outcome of the formal and lifelong education process. We consider here the average test score, *TS*, attained in standardized cognitive tests administered in adulthood and focussing on the quantitative and reading skills of individuals. Under this alternative, we estimate

$$BMI_i = \alpha X_i + \theta TS_i + \xi_i \qquad [4]$$

Compared to years of schooling *S*, the test score *TS* reflects both the influence of *S* and the impact of school quality *Q* and other factors such as learning from experience.

In this paper, we experiment with both specifications. A clear advantage of specification (4) is that the identifying assumption, the instrument *YCOMP* affecting BMI only via its effects on test scores *TS*, is weaker than the one required when human capital is measured with years of schooling, provided that the variation in the quality of education an individual is exposed to is accounted for in test scores. Moreover, while specification (3) informs us of the effects on BMI induced by adding one extra year of schooling, specification (4) tells us how much BMI is affected by a change in test scores, that can be produced either by additional years of schooling, or by increased school quality or finally by the accumulation of cognitive skills in the labour market.

2.1 Conditional Mean Effects

The baseline empirical model is described by the following pair of equations:

$$BMI_{ics} = \beta_c f_c + \beta_s f_{cs} + \beta_X X_{ics} + \delta W_{cs} + \theta E_{ics} + \varepsilon_{ics} \qquad [5]$$

$$E_{ics} = \alpha_c g_c + \alpha_s g_{cs} + \alpha_X X_{ics} + \gamma W_{cs} + \phi YCOMP_{cs} + v_{ics} \qquad [6]$$

where *E* (education) is either years of schooling *S* or the test score *TS*, depending on the selected specification, f_c and g_c are country dummies, f_{cs} and g_{cs} are country specific trends[9], W a vector of variables which vary by country and cohort, *YCOMP* is the instrument, and the subscript i is for the individual, c for the cohort and s for the country[10]. Finally, ε and v are the error terms, which are likely to be correlated either because they include common factors, such as genetic and environment 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 (5), ϑ, includes both the direct effects of E on BMI, and the indirect effects, for instance those affecting health via income.

The linear specification in equation (5) assumes that the effect of education on individual BMI does not vary with the level of BMI, and that only the location of the BMI conditional distribution is affected by changes in E. Given this, the average causal effect summarizes all the relevant information. For identification we use educational reforms which change the number of years of compulsory education, and rely on the theoretical results by Angrist and Imbens, 1994.[11] The assumptions that guarantee identification in our application are the following: (1) compulsory school reforms have had a non negligible impact on education E, and affect individual BMI only through their effect on E; (2) individuals who went to school under the new legislation attained at least as much schooling as they would have attained under the old schooling system; (3) individuals who went to school under the old system attained at most as much schooling as they would have attained under the new legislation; (4) there are no spill-over effects (Stable Unit Treatment Value Assumption). In this set up, the average causal effect can be identified only for the subpopulation of *compliers*, i.e.

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[9] We use linear and quadratic trends, where the trends are defined as the distance between each cohort and the first cohort affected by compulsory school reforms.

[10] We have experimented with two alternative specifications: first, we have added to (5) and (6) year of birth dummies, but we cannot reject the hypothesis that these dummies are jointly equal to zero. Second, we have used both year of birth dummies and country specific linear trends in the age of birth, as in Pischke and von Watcher, 2008, with results that are very similar to the ones discussed in the text.

[11] See Angrist, Imbens and Rubin, 1996, for a detailed discussion of several aspects of the approach.

for those individuals who have changed their educational attainment because of the mandatory schooling reforms.

Recent social and political concern focuses on the prevalence of overweight and obese individuals in the population rather than on BMI per se. In order to explore how education affects the probabilities of being overweight or obese, we integrate model (5) and (6) with the additional equation

$$D = 1[BMI_{ics} > \omega] \qquad [7]$$

where *D* is a dummy equal to 1 if individual BMI is above the threshold *ω* and to zero otherwise. It is useful to write equation (5) more compactly as $BMI_{ics} = Y_{ics}\pi + \theta E_{ics} + \varepsilon_{ics}$, where the vector *Y* includes the variables in vectors *X, W*, the country specific trends and the country dummies, 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 $\Phi(\frac{Y_{ics}\pi + \theta E_{ics} + \rho v_{ics} - \omega}{\sigma})$, where Φ is the standard normal distribution. The model [6]-[7] can be estimated using a control variate approach (see Wooldridge, 2002): first, we fit the endogenous variable on the set of exogenous regressors plus the instrument and take the residuals. Secondly, we estimate the probit model after adding the residuals to the set of regressors. Since the residuals are generated regressors, we bootstrap the standard errors using 500 replications. In each replication, we re-estimate both the first and the second step.[12]

[12] As discussed by Wooldridge, 2002, this procedure gives consistent estimates and allows to test in a simple way whether education is exogenous in the probit regressions.

The assumption that the effects of education on BMI is constant over the BMI distribution is overly restrictive, and rules out a priori the possibility that the impact of exogenous changes in schooling vary with individual BMI before the treatment. When the effects of education on adiposity are heterogeneous, we need to go beyond mean treatment effects, and investigate the effect of education on the conditional quantiles of the distribution of BMI.

2.2 QUANTILE TREATMENT EFFECTS

Let $Q_{BMI|Y,E}(\tau \mid Y, E)$ be the τ-th quantile of the conditional distribution of BMI. We consider the following specification

$$Q_{BMI|Y,E}(\tau \mid Y, E) = \pi(\tau) Y_{ics} + \theta(\tau) E_{ics} \qquad [8]$$

where the parameter of interest is $\theta(\tau)$. One appealing feature of this approach is that equation [8] nests the location shift model of equation [5] (see Koenker, 2005), and allows at the same time to study how education affects the upper quantiles of the distribution of BMI, where overweight and obese individuals are concentrated. Notice that inconclusive evidence about the effects of education on the conditional mean does not preclude that these effects are relevant at some conditional quantiles. By estimating quantile treatment effects, we can also tell whether education affects the conditional distribution of BMI, in particular if it plays any role in reducing inequalities in health behaviors, as proxied by BMI.

Abadie, Angrist and Imbens et al, 2002, generalize the approach by Angrist, Imbens, 1994, to the estimation of the effect of a binary (potentially endogenous) treatment on the quantiles of the distribution of a scalar continuously distributed outcome. Their approach requires the availability of a binary instrumental variable and identifies the quantile treatment effects (QTE) for the sub-population of treated individuals whose behavior is affected by the instrument (the so-called *compliers*). Since this approach has not yet been extended to the case where the endogenous variable is either discrete or continuously distributed, it is not well-suited to the application at hand, where education is measured in years of schooling, a discrete variable.

Chesher, 2003, considers the non-parametric identification of a structural model with a recursive structure. His approach relies on four main ingredients: (i) the system of equations must have a triangular structure, both in the observable variables and in the latent variables (error terms / stochastic components); (ii) the monotonicity of the (unknown) non-additive structural functions in the latent variables; (iii) local invariance conditions on conditional quantiles; (iv) no excess variation: the number of latent variables admitted in the model is not greater than the number of endogenous regressors. Chesher, 2001, points out that the continuity of the endogenous variable is needed for the unambiguous definition of quantiles[13], and guarantees the point identification of the quantiles of interest. When the continuity assumption fails, his approach can be extended but does not generally lead to point identification of the function describing the impact without further assumptions. Estimation and inference in the Chesher's setup have been developed for the parametric case by Ma and Koenker, 2006. They assume that the conditional quantile functions are known up to a finite number of parameters and add some technical regularity conditions. In their framework, the conditional quantile functions need not be linear in the parameters and the asymptotic theory is developed for nonlinear quantile regression estimation[14].

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[13] In Chesher, 2002, there is no requirement on the scale of the regressors and of the instruments but a completeness condition has to be met.

[14] Arias, Hallock and Sosa-Escudero, 2001, use data on US twins to address the issue of the endogeneity of education in quantile wage regressions. Their methodology, however, has been recently questioned

The requirement of a triangular structure in the un-observables is rather problematic in the current context. The error terms ε and v in the pair of equations (5) and (6) are likely to contain common factors, such as genetic and non-genetic environment effects. A triangular structure would require that there exists an additional latent factor which at the same time does not affect education but influences current BMI and is orthogonal to ε. One potential candidate in our setting could be the stream of income shocks affecting individuals after they completed their education. Assume that equation (5) above is augmented with current income, and that income depends on observables – including education and BMI – and un-observables, such as for instance income shocks. By replacing the definition of income into the augmented BMI equation we would obtain a triangular structure, with two orthogonal latent factors affecting BMI and only one latent factor affecting education.

This construction would entirely rely, however, on the assumption that current income affects BMI. To date, the empirical evidence on the presence of a causal relationship is far from convincing. Quintana-Domeque and Villar, 2008, look at the association between income and BMI in a sample of European countries and conclude that there is evidence of a negative correlation for females. Their estimates, however, fail to consider the endogeneity of income. In contrast, Cawley and Spiess, 2008, exploit a natural experiment to investigate the causal effect of income on the BMI of elderly Americans and find no evidence in support. This result suggests that Chesher's approach is probably not suitable for the application at hand.

We therefore turn to the last approach reviewed in this section. Chernozhukov and Hansen, 2005, propose a method that applies to the whole (treated) population, not only to

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by Ma and Koenker, who use Montecarlo simulations to show that the two-stage quantile regression which replaces education with predicted education from the ordinary least square (mean) regression of schooling on the set of instruments performs rather badly in terms of simulated bias when compared with Chesher's Weighted Average Derivative (WAD) estimator and with the control variate approach they suggest.

the sub-population of compliers, and is based on imposing some structure to the evolution of ranks across treatment states. Their approach is applicable when the outcome variable is continuous, while both the endogenous variable and the instrumental variable can be either continuous or discrete, as in our setup.

Recall our definition of the conditional τ-quantile of BMI as $Q_{BMI|Y,E}(\tau \mid Y,E) = \pi(\tau)Y_{ics} + \theta(\tau)E_{ics}$, and let $U \approx U(0,1)$ be the latent factor responsible for the heterogeneity of outcomes for individuals with the same *Y* and *E*. The random variable *U* is the rank variable. The critical assumption in this method is that, conditional on the instrument *Z*, the distribution of the rank variable does not vary with the treatment *E* (rank similarity). In our setup, this is equivalent to say that the treatment does not alter the ordering induced by genetics and early life conditions ("nature and nurture")[15].

In practice, the estimation requires two steps (see Chernozukhov and Hansen, 2004): first, we estimate quantile regressions of $BMI - Q_{BMI|Y,E}(\tau \mid Y,E)$ on *Y* and the instrument *YCOMP* for tentative estimates of $Q_{BMI|Y,E}(\tau \mid Y,E)$; second, we choose as the estimate of the coefficient $\theta(\tau)$ the one that minimizes the absolute value of the coefficient of *YCOMP* in the first step. This procedure requires an estimate of $Q_{BMI|Y,E}(\tau \mid Y,E)$ in the first step. Chernozhukov and Hansen consider linear quantile regression models and suggest to use a grid for $\theta(\tau)$, centered around the two stage quantile regression estimates, that is the estimates of α in the quantile regression of BMI on *E** and *Y*, where *E** is the expectation of *E* conditional on *Y* and *Z*. We implement the method proposed by Chernozhukov and Hansen and estimate

[15] The rank similarity assumption is the (untestable) identifying assumption in the model proposed by Chernozhukov and Hansen, 2005. This assumption is not required under the approach proposed by Abadie Angrist and Imbens, 2002.

quantile treatment effects when education is treated as endogenous by adapting to our application the OX algorithm provided by Hansen in his web-page.

2.3 THE SETUP

We identify the causal effect of education on the conditional quantiles of BMI by using the exogenous variation of schooling induced by compulsory school reforms implemented at different times and with different intensity in 11 European countries after the Second World War. We pool data from several countries to increase the number of points on the support of the instrumental variable *YCOMP* and to exploit variation in the timing of compulsory school reforms across countries. We select for each country a school reform affecting compulsory education and define $T \equiv \left(C - \overline{c}_k\right)$ as the distance between birth cohort *C* and the cohort $\overline{c}_k$, defined as the first cohort potentially affected by the change in mandatory school leaving age in country *k*. Since each selected reform occurs at a different point in time, our instrument varies both across countries and over cohorts.

For each country, we construct a pre-treatment and a post-treatment sample composed of those individuals born within the baseline range defined by 7 years before and 7 years after the year of birth of cohort $\overline{c}_k$. The breadth of the window is designed to exclude the occurrence of other compulsory school reforms, which would blur the difference between pre- and post-treatment 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. We also experiment with a much shorter window, defined by 3 years before and 3 years after the critical year, which allows us to add two countries to our sample.[16]

Table 1 presents for each country in our sample the selected reform, the year of birth of the first cohort potentially affected by the reform, the change in the minimum school leaving age and in the years of compulsory education induced by the reform, and the expected change in school attainment, expressed in terms of the ISCED classification.[17] The selected reforms increased the minimum school leaving age by one year in Austria, Germany, Ireland, Britain and Sweden; by two years in Denmark, France, Portugal and Spain; by three years in Finland, Greece, Italy and by four years in Belgium. In some of these countries, namely Germany, Finland and Sweden, the introduction of the reform varied by region. Since we do not have access to data at the municipality level, for Finland and Sweden we define the year of the reform in each area as the year when the largest share of municipalities in that area changed the schooling legislation.

4. THE DATA

Our empirical estimates use two datasets: a larger set which contains information on individual BMI and years of schooling, and a smaller set which also includes information on test scores. In the rest of this section, we describe each dataset in turn.

4.1 DATASET 1

This dataset is obtained by pooling together data drawn from the 1998 wave of the European Community Household Panel (ECHP), the second release of the first wave of the

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[16] Brunello, Fort and Weber, 2009, use a similar strategy in their study of the impact of education on the distribution of earnings.
[17] See Brunello, Fort and Weber, 2009, for details on the sources of these data.

Survey on Household Health, Ageing and Retirement in Europe (SHARE) for the year 2004, the 2002 wave of the German Socio Economic Panel (SOEP), the 2003 wave of the British Household Panel Survey and the 2003 wave of the French *Enquete sur la Sante*[18]. The countries included in this dataset are: Austria, Belgium, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Portugal, Spain, Sweden and the United Kingdom. In the case of a few countries, we use data from two different surveys. Since Portugal experienced a second school reform in 1968, four years after the 1964 reform, the window of observation for this country is only the shortest one (three years before and after the critical cohort). Swedish data are from SHARE, and have a small number of observations at the upper tail of the longer window (seven years before and after). Because of these reasons, in the empirical analysis we use data from Portugal and Sweden only when the shortest time window is considered.

Our dependent variable is the body-mass index (BMI), defined as weight in kilograms divided by the square of height in meters (kg/m2). The BMI, albeit somewhat crude, has been found to be highly correlated with more precise (and more costly to collect) measures of adiposity.[19] 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.

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[18] 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 France, Germany and the UK. For these countries we select national surveys, using waves that include information on BMI.

[19] Other anthropometric methods for measuring individuals body fat include the waist-hip ratio, sagittal abdominal diameter, skin folds thickness. More accurate measures are based on bioelectrical impedance analysis, infrared interactance, dual energy X-ray absorptiometry. All these methods imply some instrumental measurement that is usually far from being viable in social surveys.

In this dataset, we measure educational attainment with the number of years of schooling. With the exception of French data, where we have detailed information on the highest attained degree, for the rest of the countries in the sample the number of years of education is based on responses to questions asking 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.

4.2 DATASET 2

Our measure of cognitive skills is drawn from the International Adult Literacy Survey (IALS), which tests three components of individual cognitive skills in the population aged 15 to 65 and in different countries according to a common, standardized format. This is not the sole source of international test scores[20], but the best suited to our purposes, both because it covers the adult population and because it was carried out in the second part of the 1990s, when most of our dataset 1 was collected.

The approach followed by IALS is to measure cognitive skills in three domains – quantitative literacy, prose literacy, and document literacy. The former is defined as the ability to apply "arithmetic operations, either alone or sequentially, to numbers embedded in printed materials". Prose literacy is defined as the ability to understand and to use "information in texts". Document literacy is defined as the ability to "locate and use information in various formats" (see Cascio, Clark and Gordon, 2008). Cognitive skills as measured by IALS test scores reflect both the formal education process – its quantity and quality - and the learning activities

[20] By and large, international surveys of cognitive skills focus on the population at schooling age. For instance, the Trends in International Mathematics and Science Study (TIMSS) covers mainly 13 year old students, and the OECD Programme for International Student Assessment (PISA) focuses on 15 year old pupils,

taking place after education is completed. Therefore, they are a good proxy of the stock of cognitive human capital accumulated by each individual until the time of the interview.

This information is available only for a subset of the countries included in dataset 1 (Belgium, Denmark, Finland, Germany, Ireland, Italy, Sweden and the United Kingdom). Notice that IALS and our dataset 1 are based on different samples. Therefore, it is impossible to retrieve from IALS individual test scores. What we can do, however, is to assign to the individuals in dataset 1 the average test score of individuals belonging to the same country, gender and year of birth. The imputation of average test scores to individuals implies that we cannot estimate equation (4). We can estimate instead

$$BMI_i = \alpha X_i + \theta \overline{TS} + \xi_i \qquad [9]$$

where the upper bar refers to average values. Given that our instrument *YCOMP* is at the same level of aggregation as the imputed test score, the key orthogonality condition for instrument validity is not violated by replacing TS_i with $\overline{TS}$. We take into account the fact that the dependent variable and the test score in [9] are at different levels of aggregation by clustering standard errors at the level of aggregation of $\overline{TS}$.

Individual test scores in IALS are allowed to vary between 0 and a maximum of 500. The average score attained by females in our data ranges from 221.15 in Italy to 302.53 in Finland. In the case of males it ranges from 245.63 in Italy to 298.76 in Denmark.

4.3 Additional controls

Since we intend to identify from the data the causal relationship between the education and BMI, we need to control as accurately as possible for additional factors affecting the dependent variable. We include in the empirical specification both country and survey dummies. Furthermore, trend-like changes in both education and BMI relative to the time of the school reform are controlled with a second order polynomial in $K=T+7$ – where T is the distance between each cohort and the first cohort potentially affected by the reform - and its interactions with country dummies[21].

Recent empirical research has documented that adult BMI is correlated to weight at birth, and that the latter is correlated to the season of birth and the climatic conditions prevailing at the time of birth (see for instance Phillips and Young, 2000; Murray et al, 2000; van Hamswijck et al, 2002). We use individual information on the month and year of birth to construct two variables: a dummy equal to 1 if the individual is born in autumn or winter, and 0 otherwise; the average temperature registered in the country during a window spanning three months before and after birth. Data on historical temperature for each country come from the Global Historical Climatology Network monthly data base, made available by the National Climatic Data Center at the US Department of Commerce.

Changes in educational attainment after a compulsory school reform could be due to the reform itself or to confounding factors, which may alter the incentives to invest in education at the time of the reform but independently of it. To illustrate, take a reform that increases the minimum school leaving age from 14 to 15 in a certain year. If individuals at age 14 - or their parents - find it more attractive to invest in education because of a reduction in the opportunity costs generated by a contemporaneous increase in the unemployment rate, they might invest more independently of the reform. To control for this, we use the

[21]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. The country specific trends may help capture the effects of unmeasured school quality on BMI. Following Lee and Barro, 1997, we have tried to improve our ability to control for school quality by computing measures of the pupil – teacher ratio in secondary schools in the neighborhood of the time when school reforms took place. Unfortunately, the available data do not cover in a satisfactory way the full set of countries available in our dataset.

unemployment rate (by country and gender) and the country specific real GDP per head, and match these variables to individuals around the age when school reforms have taken place.

Both GDP per capita and the unemployment rate near school reforms are also likely to affect BMI because they influence health conditions at the time when critical schooling decisions are taken. We try to further control for factors affecting initial health conditions by adding to the regressions life expectancy at birth and average calorie intake at the age when school reforms have occurred.

Figure 1 presents the cumulative distribution function of years of education both for the cohorts affected (broken line) and for the cohorts not affected by the reforms (continuous line). For both males and females, the empirical distribution 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 2 for the cohorts born before and after the first cohort potentially affected by the reforms. The upward jump at the time of the reforms is clearly visible and corresponds to about 0.4 years in the case of females and to about 0.2 years in the case of males for each additional year of mandatory schooling prescribed by law.[22]

Figure 3 compares the empirical cumulative distribution function of BMI for the cohorts who are potentially affected by the reforms and for the older cohorts. The distribution shifts to the left, especially in the case of females, suggesting that BMI is lower among the

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[22] Figure 1 is based on the sample of individuals born 7 years before and 7 years after the critical cohort and excludes data from Sweden and Portugal. This jump is slightly larger than the one found by Brunello, Fort and Weber, 2009, who consider however only employed individuals. Here we include in our sample all individuals independently of their labour market status.

younger cohorts. Clearly, the observed shift could be both an effect of compulsory school reforms and the result of confounding factors, including cohort and composition effects.

Figure 4 plots average test scores versus years of schooling in our sample of females. It is clear that, for each given year of education, test scores vary substantially, reflecting a host of relevant factors, such as country specific schooling institutions, school quality effects and learning after schooling. Finally, Figure 5 reproduces the exercise carried out in Figure 2 for the case of test scores, again only for females. Again, we detect a clear jump at the time of the reforms, which corresponds to about one sixth of the standard deviation of test scores in our sample for each year of compulsory education.

5. The Findings

We start the presentation of our results by reporting the first stage estimates of years of schooling on the vector of exogenous variables plus the instrument *YCOMP*. We do this for dataset 1 and for two time windows, our baseline window [+7,-7] (columns 1 and 2 in the table) and the shorter window [+3,-3] (columns 3 and 4). In these estimates, robust standard errors are clustered by country and year of birth. We find that our instrument is significantly correlated with the endogenous variable. As anticipated by Figure 2, the impact of years of compulsory education on the years of schooling attained by females is about twice as big as the impact on the years of schooling attained by males. One possible explanation is that females at the time of the school reforms had lower educational attainment than males and were in larger number affected by the reforms.

Moreover, we test for the presence of weak instruments 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-test should be at least 10 for weak identification not to be considered a problem. In all specifications, we can reject the hypothesis of weak instruments, albeit only marginally in the case of females and the shorter window.

Table 4 presents the ordinary least squares (OLS) and instrumental variables (IV) estimates in the two selected windows, separately for males and females. With OLS, the estimated association between BMI and years of schooling is negative and more than twice as big for females as for males (-0.200 versus -0.085 in the larger window). The size of the effect for females is similar to that estimated by Cutler and Lleras-Muney, 2007 for US whites aged over 25 (-0.190) but smaller than the estimate for US females (-0.302) reported by Grabner, 2008.

The IV estimates of the impact of years of schooling on BMI are always larger in size than the OLS estimate – a standard result in this literature – but statistically significant at the 10 percent level of confidence or higher only in the case of females. In the case of males, the estimated IV effect is small, positive and imprecisely estimated. Our results imply that a 10 percent increase in years of schooling reduces the BMI of females by 0.95 to 1.48 percent in the larger window and by 0.93 to 2.27 percent in the narrower window, a moderate effect when compared to the 4 percent decline estimated by Grabner for the US, using compulsory school reforms to instrument years of schooling, as we do[23].

Turning to the other regressors in Table 4, we find no evidence that either the average temperature at birth or the average calorie intake at the age first affected by school reforms influence individual BMI: their estimated coefficients are never statistically significant at the 5 or 1 percent level of confidence. There is instead evidence that individual BMI is lower among individuals born in autumn and winter, and that a lower average life expectancy at birth, a

[23] When interpreting the IV estimates in Table 4, it is important to notice that the Hausman test never rejects the null of no endogeneity of years of schooling.

higher GDP per capita, and a higher unemployment rate at the age first affected by the reforms reduce individual BMI.

Next, we consider the effects of education on the probability of being overweight or obese. We estimate probit models when education is treated as exogenous and report the results in the first and third column of Table 5. We then augment these models with the residuals from the first stage regression of years of schooling on the vector *Y* of exogenous controls and the instrument. These estimates are reported in the second and fourth column of the table.

In line with the findings of Table 4, we cannot reject the null hypothesis that education can be treated as exogenous. Therefore, we focus on the odd columns of the table, which show that years of schooling significantly affect the incidence of overweight and obesity among males and females. The size of the effect, however, is rather small: on the one hand, a 10 percent increase in the years of schooling reduces the incidence of overweight females by 2.53 percentage points (average incidence: 38%), close to twice as much as the effect estimated for males (1.21 percentage point out of an average incidence of 60.4%). On the other hand, the same increase in years of schooling reduces the estimated percentage of obese individuals by 1.43 percentage points for females (average incidence: 11.2%) and by 1 percent for males (average incidence: 13.47%). The estimated effects for females are between one third and one fourth of the effects found by Grabner, 2008, for the US, using a similar specification.

So far, we have followed the relevant literature and measured individual human capital with years of schooling. We next present the results from our smaller dataset, which includes two alternative measures of human capital, years of schooling and average test scores. Confirming the results of Table 4, we don't find any evidence that the causal relationship between human capital and BMI is statistically significant among males. We therefore focus in Table 6 only on females and the broader window (+7,-7). The table is organized in four columns: in the former two human capital is measured with years of schooling and in the remaining two it is measured with average test scores (by country, gender and year of birth).

Consider first the estimates of the impact of years of schooling on BMI. As in Table 4, which refer to a larger sample of countries, we find that the Hausman test fails to reject the null hypothesis of exogeneity for education. Moreover, we estimate that the effect on BMI of a 10 percent increase in the years of schooling ranges from -0.85 to -2.63 percent, depending on the estimation method. Turning to the estimates that measure human capital with average (country by year of birth) test scores, we find that the exogeneity of education is rejected, that the instrument YCOMP is not weak and that the IV estimates are much larger than OLS estimates. Focusing on the IV estimates, our results suggest that a 10 percent increase in test scores would reduce individual BMI by 3.71 percent, a larger effect that that estimated using OLS and years of schooling.

Clearly, one might argue that one source of increase in test scores is additional years of schooling. This is an important but by no means the unique source of variation in test scores, however. To illustrate using our data, the average test score and its standard deviation when educational attainment is equal to 11 years are 274 and 19.88 respectively. If we were to increase test scores by a standard deviation while keeping years of schooling constant (at 11 years) – for instance by raising learning opportunities on the job - our estimates would suggest a reduction of individual BMI by 2.69 percent.

Table 7 shows the estimates of the effects of years of schooling and test scores on the probability of being overweight or obese, using the smaller dataset and considering only females. In the case of overweight females, the statistical significance of the generated residuals points clearly to the endogeneity of education when this is measured with test scores. The evidence in support of endogeneity when education is measured with years of schooling is much weaker, because we can reject the hypothesis that the estimated residuals are equal to zero only at the 10 percent level of confidence. Following Wooldridge, 2002, when education is endogenous, we need to replace marginal effects with average partial effects (APE), which are calculated as follows: first, we compute the marginal effects $\frac{\partial \Phi}{\partial E}\left(\frac{Y_{ics}\pi + \theta E_{ics} + \rho v_{ics} - \omega}{\sigma}\right)$ and evaluate them at the sample mean of each explanatory variable; second, we average across the sample distribution of *v*[24].

It turns out that the marginal reduction in the probability of being overweight induced by a 10 percent increase in years of schooling ranges from 2.39 percentage points when years of schooling are treated as exogenous to 8.73 percentage points when they are treated as endogenous. The upper range effect is comparable to the one found by Grabner for the US (6.5 percentage points). The gap associated to the estimation method is even larger in the case of test scores, with the marginal decline associated to a 10% increase in the score ranging from 0.95 to 13.45 percentage points.

The findings from the smaller dataset have two implications: first, education policies that address health lifestyles need not restrict themselves to changes in the quantity of education. Measures that affect test scores for a given number of years of education, for instance because they improve school quality or foster adult learning and skill maintenance after school completion, may affect individual BMI. Second, the estimated impact of years of schooling is much larger when data from Austria, France, Spain and Greece are excluded from

[24] Figure 6 illustrates how the APE vary with years of schooling and the average test score.

the dataset, which points to the presence of heterogeneous effects across countries in Europe[25].

Finally, we turn to quantile regressions and to the question whether the effects of education vary with the quantiles of the distribution of BMI. We start by showing in Table 8 the Kolmogorov – Smirnov tests for the exogeneity of education in quantile regressions. In line with the results above, we can only reject the null of no endogeneity when education is measured with test scores. Therefore, we present the estimates of the impact of years of schooling on the conditional distribution of BMI only when education is treated as exogenous, i.e. when we use standard linear quantile regression (QR) methods. Next, we address the endogeneity of test scores by estimating quantile treatment effects by instrumental variables, using the approach proposed by Chernozhukov and Hansen (2006). In this exercise, we only consider the broader window of observation [+7,-7] and restrict our attention to females.

Figures 7 and Table 9 show how the estimated (marginal) effect of years of schooling on BMI changes as we move from the lowest to the highest quantiles on the distribution of BMI, using both dataset 1 ("FULL") and dataset 2 ("IALS"). We find that the estimated effect is negative over the whole distribution, and ranges between -0.08 and -0.31 with dataset 1 and between -0.06 and -0.25 with dataset 2. The uncovered negative association points to the possibility that increasing education could be particularly beneficial to overweight and obese individuals.

When human capital is measured with test scores, the difference between quantile estimates when education is treated as exogenous or endogenous tends to be higher in the

[25] We have investigated this issue further by splitting the larger dataset in sub-samples of countries to take into account: i) the location of the country in the European map (North versus South); ii) the period when the relevant school reforms took place (before and after 1970). In either case, however, we find no statistical evidence that estimated coefficients are different.

upper quantiles of the distribution of BMI, as shown by Figure 8 and Table 9[26]. When the endogeneity of education is ignored, the point estimates fluctuate around zero. When instead we instrument education with years of compulsory schooling, we find that the point estimates are below zero over the whole distribution and are larger in absolute terms at higher quantiles, i.e. for higher levels of BMI.

The heterogeneity of the effect is confirmed by the tests carried out in Table 8: when education is measured with test scores the null hypothesis of constant treatment effect, defined by taking the effect at the median of the BMI distribution as the benchmark, as in Chernozhukov and Hansen (2006), is clearly rejected. This suggests that one needs to look beyond conditional mean effects when considering the impact of education on BMI.

CONCLUSIONS

In this paper, we have departed from the empirical literature on the effects of education on adiposity and obesity in three main directions. First, we have argued that measuring human capital and education exclusively with the number of years of schooling runs the risk of omitting important dimensions of education – such as school quality – which might affect the empirical results in a significant way, in line with what happens in the empirical growth literature. Second, we have questioned the usual approach in this literature of focusing on conditional mean effects. In the presence of heterogeneity, the estimated effect of education on the conditional mean of BMI could be rather different from the effect at the higher (conditional) quantiles of the distribution of BMI, which are the ones that have

[26] In the figure, the estimated effects are multiplied by 10.

attracted policy interest. Third, we have tried to overcome the "weak instrument" problem that is typical in this literature by collecting international data on school reforms, that add to the variation over time in compulsory education the variation across countries.

Our results suggest that these departures can be fruitful directions of further research in this area. By exploiting cross country variation and well as variation across cohorts, we never find evidence that our selected instrument, the number of years of compulsory education, is weak. The distinction between years of schooling and test scores as alternative measures of education and human capital is not only meaningful from a policy viewpoint, but leads to rather distinct results. In particular, we find evidence that education is endogenous with respect to current BMI only when we measure it with test scores. Furthermore, there is evidence that a 10 percent increase in test scores have larger effects both on BMI and on the probability of being overweight than a 10 percent increase in years of schooling. From a policy point of view, these results suggest that one can try to reduce the problem of excessive overweight by a broader span of education policies, that might include lifelong learning as well as the improvement in the quality of schooling.

Finally, there is evidence that focusing on locational shifts in the (conditional) distribution of BMI is likely to miss the fact that changes in education, and especially test scores, are more effective on individual BMI when they affect individuals in the upper part of the (initial) distribution of BMI.

While these findings require a number of qualifications, we conclude with only one. We have compared younger individuals who are affected by school reforms with older individuals who are not affected. Since mortality increases with age and decreases with education, our control group consists mostly of survivors, and cannot be considered as fully representative of the entire population of individuals not affected by school reforms. These survivors are typically in better health and have lower BMI and higher education than those who could not survive. An implication of this unavoidable feature of our data is that our empirical estimates should be considered as a lower bound of the true effects of education on adiposity and obesity.

TECHNICAL APPENDIX

THE EDUCATIONAL REFORMS USED IN THIS STUDY

In this section we provide a brief description of the educational reforms considered in the study. Furthermore, we motivate the choice of the first cohort potentially affected. We devote a paragraph to each country considered. Further details on country specific educational systems and reforms are in Fort (2006).

AUSTRIA

The 1962 School Amendment Act increased compulsory schooling by one year, from 8 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 afterwards. 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.

BELGIUM

In 1983 (Law of 28 June 1983), the length of compulsory schooling was increased to 18 years (from 8 to 12 compulsory years of education), which could be completed with part time schooling during the final three years. Student potentially affected by the reform were those aged 14 or younger in 1983, i.e. those born after 1969.

DENMARK

Two major reforms of compulsory schooling took place in Denmark in 1958 and 1971. In 1958 compulsory schooling years were increased by 3 years (from 4 to 7) and in 1971 they were further increased by additional two years (from 7 to 9). Pupils who were 14 years (or younger) in 1971 were potentially affected by the 1971 reform. We only consider this reform in this study.

Finland

The relevant reform considered in this study took place during the 1970s. The reform introduced a new curriculum and changed the structure of the educational system, increasing compulsory education from 6 to 9 years. The reform was adopted gradually by Finnish municipalities. Since we do not have access to data at the municipality level, we define the year of the reform in each area as the year when the largest share of municipalities in the area experienced the change in the schooling legislation, as reported in Table A.1 below. Following Pekkarinen (2005), we consider the cohort aged 11 when the reform was implemented as the first cohort potentially affected.

France

During the XX century, compulsory schooling age in France was extended twice: from 13 to 14 in 1936 and from 14 to 16 in 1959 (Bethoin Reform). The 1936 reform affected mainly pupils born after 1923, whereas the 1959 reform - which was implemented from 1967 after a long transition period - affected individuals who were born from 1953 onwards (see Grenet (2004)).

Germany

The peculiar political situation of the country produced the separate evolution of two distinct education systems between 1949 and 1990. We refer to reader to Pischke et al (2005), and Pischke (2003), for a description of the compulsory school reforms and for the selection of the first cohort potentially affected in each German Lander.

Greece

In 1975 the Greek Parliament increased compulsory education by three years (from 6 to 9). Individuals potentially affected by this change are those who were 12 in 1975. In particular, those born 1963 and later were compelled to attend 3 additional years of schooling, whereas those born in 1962 were not.

Ireland

Compulsory schooling was modified in 1972, when the school leaving age was raised to the age 15. A further raise in compulsory schooling age (to 16 years) announced in 1998, came into effect when the

Education (Welfare) Act (2000) became law. Individuals potentially affected by the 1972 reform are the individuals who were 14 in 1972. These individuals where compelled to attend an additional year of schooling, whereas individuals who were 14 in 1971 were not. Therefore we choose 1958 as the first cohort potentially affected.

Italy

Junior high school became effectively compulsory in Italy only since 1963. Compliance with the 1963 reform was not instantaneous: only in 1976 the proportion of children attending junior high school approached 100%. According to Brandolini and Cipollone (2002), the individuals potentially affected by the reform were those born after 1949.

The United Kingdom

See Silles, 2009, for details.

Spain

The compulsory school reform considered in this study was carried out under the 1970 General Act on Education and Financing of Educational Reform (LGE), and increased compulsory years of education from 6 to 8. Individuals potentially affected by the reform were those born in 1957 and after (see Pons and Gonzalo (2003), p.753 and Table A1 p.767).

Sweden

According to Meghir and Palme (2005), compulsory school reform in Sweden was gradually implemented between 1949 and 1962. The take-up of the experiment varied over the period 1949-1962 across municipalities with the largest number of municipalities involved in the years 1961/1962 (39.4%; 18,665 classes; 436,595 students). It was fully implemented only in 1962. Unfortunately, we do not have access to data at the municipality level but only at the county level. For the purposes of this paper, and based on personal communication with Marten Palme, we considered as potentially affected by the reform all the individuals born after 1950.

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Tables

Table 1. School reforms in Europe

Country	Year of Reform	First affected cohort	Change in minimum school-leaving age		Change in years of compulsory schooling		Min. expected qualification	School entry age
Austria	1962	1947	14 =>	15	8 =>	9	ISCED2	6
Belgium	1983	1969	14 =>	18	8 =>	12	ISCED3	6
Denmark	1971	1957	14 =>	16	7 =>	9	ISCED3	7
Finland (Uusima)	1977	1966	13 =>	16	6 =>	9	ISCED3	7
Finland (Etela-Suomi)	1976	1965	13 =>	16	6 =>	9	ISCED3	7
Finland (Ita-Suomi)	1974	1963	13 =>	16	6 =>	9	ISCED3	7
Finland (Vali-Suomi)	1973	1962	13 =>	16	6 =>	9	ISCED3	7
Finland (Pohjois-Suomi)	1972	1961	13 =>	16	6 =>	9	ISCED3	7
France	1959	1953	14 =>	16	8 =>	10	ISCED3	6
Germany (Sch. Hols.)	1956	1941	14 =>	15	8 =>	9	ISCED3	6
Germany (Hamburg)	1949	1934	14 =>	15	8 =>	9	ISCED3	6
Germany (Nieders.)	1962	1947	14 =>	15	8 =>	9	ISCED3	6
Germany (Bremen)	1958	1943	14 =>	15	8 =>	9	ISCED3	6
Germany (Nord.Wes.)	1967	1953	14 =>	15	8 =>	9	ISCED3	6
Germany (Hessen)	1967	1953	14 =>	15	8 =>	9	ISCED3	6
Germany (Rhein.Pf.)	1967	1953	14 =>	15	8 =>	9	ISCED3	6
Germany (Baden-W.)	1967	1953	14 =>	15	8 =>	9	ISCED3	6
Germany (Bayern)	1969	1955	14 =>	15	8 =>	9	ISCED3	6
Germany (Saarland)	1964	1949	14 =>	15	8 =>	9	ISCED3	6
Greece	1975	1963	12 =>	15	6 =>	9	ISCED2	6
Ireland	1972	1958	14 =>	15	8 =>	9	ISCED3	6
Italy	1963	1949	11 =>	14	5 =>	8	ISCED2	6
UK	1973	1959	15 =>	16	9 =>	10	ISCED2	6
Spain	1970	1957	12 =>	14	6 =>	8	ISCED2	6
Sweden	1962	1950	14/15 =>	15/16	8 =>	9	ISCED3	6/7
Portugal	1964	1956	12	14	4 =>	6	ISCED2	8

Table 2. Summary statistics

Females

country	Bmi	Years of education	Years of compulsory education	Age	N° observations
Austria	26.1	11.1	8.5	53.8	1,202
Belgium	22.7	15.1	9.8	29.8	638
Denmark	24.1	14.0	7.9	43.3	767
Finland	23.9	15.3	7.4	34.8	843
France	24.2	12.3	9.0	49.6	3,593
Germany	25.1	12.2	8.6	50.3	2,722
Greece	24.2	11.7	7.3	36.6	1,272
Ireland	24.4	11.0	8.5	39.9	765
Italy	24.8	9.6	6.4	50.9	2,216
Portugal	25.4	6.8	4.9	42.3	475
Spain	24.2	9.6	7.0	42.2	1,630
Sweden	25.2	11.8	8.5	54.3	357
UK	25.8	13.2	10.6	43.4	1,471

Males

country	Bmi	Years of education	Years of compulsory education	Age	N° observations
Austria	27.0	12.3	8.5	53.4	1,070
Belgium	24.2	15.3	9.9	29.8	514
Denmark	25.5	13.5	7.9	43.2	756
Finland	25.5	14.0	7.4	34.7	880
France	26.0	12.7	9.0	49.6	3,321
Germany	26.5	12.9	8.5	50.6	2,665
Greece	26.0	12.2	7.5	35.2	1,070
Ireland	26.1	10.9	8.5	39.8	758
Italy	26.3	10.7	6.4	50.5	2,114
Portugal	26.1	6.4	5	42.2	487
Spain	26.5	9.8	7.0	41.5	1,517
Sweden	26.5	11.6	8.6	54.1	295
UK	26.9	13.4	10.6	43.7	1,360

Note: data for Portugal and Sweden refer to the cohorts born between 3 years before and 3 years after the year of birth of the first affected cohort (see Table 1). 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 Section 4 in the text for more details. In the table we exclude records with missing values for the variables used in the estimates.

Table 3: First stage effects. Dependent variable: years of schooling

	Window +7,-7		*Window +3,-3*	
	Females	Males	Females	Males
Years of compulsory education	0.342***	0.236***	0.377***	0.283***
	[0.078]	[0.060]	[0.115]	[0.061]
Log GDP per-capita at pivotal age	1.345**	0.438	0.787	0.791
	[0.647]	[0.837]	[1.066]	[1.252]
Unemployment rate at pivotal age	-0.059	-0.043	-0.100	-0.051
	[0.041]	[0.038]	[0.065]	[0.053]
Life expectancy at pivotal age	-0.112	0.143	0.091	0.168
	[0.138]	[0.144]	[0.166]	[0.191]
Calories intake at pivotal age	-0.202	-1.227	-1.058	-1.260
	[0.565]	[0.689]	[1.036]	[0.945]
Born in autumn or winter	0.004	0.056	-0.114	0.051
	[0.061]	[0.070]	[0.084]	[0.093]
Avg. temperature at month of birth	-0.008	0.010	-0.008	0.019
	[0.010]	[0.009]	[0.014]	[0.013]
Observations	17119	15992	8872	8257
F test	18.98	15.16	10.69	21.17

Note: Robust standard errors clustered by year of birth and country within brackets. All regressions include year and country dummies and a country specific second order polynomial in K=T+7. The benchmark country is France. Pivotal age is the age first affected by the school reform. The estimates in the shorter window include Portugal and Sweden. These countries are excluded in the broader window. One, two and three stars for coefficients statistically significant at the 10, 5 and 1 percent level of confidence.

Table 4. Ordinary least squares and instrumental variable estimates. Dependent variable: BMI

Panel a: Females

	Window +7,-7		*Window +3,-3*	
	OLS	IV	OLS	IV
Years of education (ys)	-0.200***	-0.309*	-0.199***	-0.483**
	[0.009]	[0.169]	[0.012]	[0.237]
Log GDP per-capita at pivotal age	-0.538	-0.397	-3.063**	-2.877*
	[0.822]	[0.855]	[1.421]	[1.471]
Unemployment rate at pivotal age	-0.144***	-0.150***	-0.303***	-0.338***
	[0.046]	[0.048]	[0.093]	[0.102]
Life expectancy at pivotal age	0.122	0.109	0.438**	0.467**
	[0.126]	[0.129]	[0.204]	[0.212]
Calories intake at pivotal age	-1.318*	-1.337*	0.968	0.652
	[0.767]	[0.774]	[1.409]	[1.500]
Born in autumn or winter	-0.183**	-0.183**	0.047	0.014
	[0.075]	[0.075]	[0.102]	[0.108]
Avg. temperature at month of birth	-0.012	-0.013	0.010	0.007
	[0.011]	[0.011]	[0.015]	[0.016]
% change in BMI for a 10% increase in years of education	-0.95	-1.48	-0.93	-2.27
Hausman test for endogeneity (p-value)		0.527		0.316
Observations	17119	17119	8872	8872

Panel b: Males

	Window +7,-7		*Window +3,-3*	
	OLS	IV	OLS	IV
Years of education (ys)	-0.085***	0.087	-0.094***	0.106
	[0.008]	[0.220]	[0.010]	[0.276]
Log GDP per-capita at pivotal age	0.339	0.266	-0.154	-0.304
	[0.759]	[0.772]	[1.290]	[1.329]
Unemployment rate at pivotal age	-0.012	-0.007	-0.025	-0.016
	[0.038]	[0.039]	[0.071]	[0.073]
Life expectancy at pivotal age	0.106	0.0843	0.237	0.204
	[0.114]	[0.1204]	[0.184]	[0.193]
Calories intake at pivotal age	-0.003	0.192	1.416	1.653
	[0.664]	[0.718]	[1.249]	[1.315]
Born in autumn or winter	-0.127*	-0.136**	-0.174* *	-0.183**
	[0.065]	[0.067]	[0.092]	[0.095]
Avg. temperature at month of birth	-0.007	-0.009	-0.010	-0.014
	[0.010]	[0.010]	[0.014]	[0.015]
% change in BMI for a 10% increase in years of education	-0.36	0.36	-0.42	0.48
Hausman test for endogeneity (p-value)		0.519		0.664
Observations	15992	15992	8257	8257

Notes: see Table 3.

Table 5. Probit models. Window (-7,+7). By gender.

Panel a: probability of being overweight (BMI>=25)

	Females		*Males*	
Years of education (ys)	-0.057***	-0.125**	-0.026***	0.013
	[0.003]	[0.059]	[0.003]	[0.095]
Residuals		0.068		-0.039
		[0.058]		[0.095]
Log GDP per-capita at pivotal age	0.091	0.179	0.002	-0.015
	[0.261]	[0.287]	[0.353]	[0.291]
Unemployment rate at pivotal age	-0.033**	-0.036**	-0.018	-0.017
	[0.014]	[0.015]	[0.013]	[0.014]
Life expectancy at pivotal age	0.017	0.009	0.089**	0.084*
	[0.040]	[0.045]	[0.042]	[0.047]
Calories intake at pivotal age	-0.662**	-0.674**	-0.244	-0.199
	[0.247]	[0.259]	[0.249]	[0.279]
Born in 2nd semester	-0.012	-0.011	-0.020	-0.022
	[0.023]	[0.025]	[0.024]	[0.004]
Avg. temperature at birth	0.001	0.001	0.003	0.003
	[0.003]	[0.004]	[0.004]	[0.004]
Marginal effect of adding 10% more schooling (percentage points)	-2.53		-1.21	
Percentage overweight	38.04		60.44	
Observations	17119	17119	15992	15992

Panel b: probability of being obese (BMI>=30)

	Females		*Males*	
Years of education (ys)	-0.064***	-0.059	-0.036***	-0.026
	[0.005]	[0.080]	[0.004]	[0.120]
Residuals		-0.005		-0.010
		[0.074]		[0.112]
Log GDP per-capita at pivotal age	-0.454	-0.461	0.502	0.497
	[0.339]	[0.365]	[0.353]	[0.383]
Unemployment rate at pivotal age	-0.039**	-0.038*	0.001	0.001
	[0.019]	[0.020]	[0.017]	[0.018]
Life expectancy at pivotal age	0.103*	0.104*	-0.015	-0.016
	[0.053]	[0.058]	[0.053]	[0.056]
Calories intake at pivotal age	-0.306	-0.306	-0.212	-0.201
	[0.330]	[0.339]	[0.328]	[0.368]
Born in 2nd semester	-0.082***	-0.082**	-0.053*	-0.054*
	[0.030]	[0.032]	[0.030]	[0.030]
Avg. temperature at birth	-0.003	-0.004	-0.013***	-0.013***
	[0.005]	[0.005]	[0.004]	[0.005]
Marginal effect of adding 10% more schooling (percentage points)	-1.43		-1.00	
Percentage obese	11.19		13.47	
Observations	17119	17119	15992	15992

Note. See Table 3. The bootstrapped standard errors in columns 2 and 4 are estimated using 500 replications. See details in the text.

Table 6. Ordinary least squares and instrumental variable estimates for the sub-sample of countries with data on test scores. Dependent variable: BMI. Window +7,-7. Females only

	OLS	*IV*	*OLS*	*IV*
Years of education (ys)	-0.172***	-0.530**		
	[0.011]	[0.208]		
Test score (ts)			-0.006	-0.034**
			[0.004]	[0.016]
Log GDP per-capita at pivotal age	0.174	0.739	-0.133	-0.288
	[1.065]	[1.169]	[1.159]	[1.210]
Unemployment rate at pivotal age	-0.183***	-0.192***	-0.187***	-0.227***
	[.056]	[.058]	[.060]	[.072]
Life expectancy at pivotal age	0.085	0.041	0.150	0.345*
	[.172]	[.185]	[.184]	[.193]
Calories intake at pivotal age	-2.717***	-2.905**	-2.773**	-3.434**
	[1.085]	[1.157]	[1.236]	[1.402]
Born in 2nd semester	-0.084	-0.103	-0.076	-0.077
	[.012]	[.107]	[.110]	[.110]
Avg. temperature at birth	-0.002	-0.004	-0.001	-0.003
	[.014]	[.015]	[.013]	[.013]
% change in BMI for a 10% increase in yeas of schooling	-0.85	-2.63		
% change in BMI for a 10% increase in the test score			-0.67	-3.71
Hausman test for endogeneity (p-value)		0.109		0.017
F-test first stage		11.09		10.71
Observations	9422	9422	9422	9422

Note: Robust standard errors clustered by year of birth and country within brackets. All regressions include year and country dummies and a country specific second order polynomial in K=T+7. The benchmark country is the UK. Pivotal age is the age first affected by the school reform. The estimates in the shorter window include Portugal and Sweden. These countries are excluded in the broader window. One, two and three stars for coefficients statistically significant at the 10, 5 and 1 percent level of confidence.

Table 7. Probit models for the sub-sample of countries with data on test scores. Window (-7,+7). Females only

	Overweight		*Obesity*	
Years of education (ys)	-0.050***	-0.200**	-0.055***	-0.123
	[0.004]	[0.079]	[0.006]	[0.093]
Residuals		0.150*		0.068
		[0.080]		[0.074]
Marginal effect of adding 10% more schooling (percentage points)	-2.39	-8.73	-1.21	
Test score (ts)	-0.000	-0.013***	-0.003**	-0.007
	[0.001]	[0.004]	[0.001]	[0.005]
Residuals		0.013***		0.005
		[0.004]		[0.005]
APE of a 10% increase in the test score (percentage points)		-13.45	-1.35	
Observations	9422	9422	9422	9422

Note. See Table 6. The bootstrapped standard errors in columns 2 and 4 are estimated using 500 replications. See details in the text. Not reported in the table, each regression includes also the log of GDP per capita, the unemployment rate, life expectancy, calorie intake at pivotal age. Average partial effects rather than marginal effects in column (2).

Table 8. Quantile regressions: Kolmogorov Smirnov tests. Window [-7,7]. By dataset.

	Estimated value	90% critical value	95% critical value	99% critical value
Null hypothesis: equality of coefficients				
Females, dataset 2, E=TS	4.326***	2.617	3.040	3.494
Females, dataset 2, E=YS	2.560	2.678	2.993	3.629
Females, dataset 1, E=YS	2.799*	2.738	2.975	3.290
Null hypothesis : location shift based on median				
Females, dataset 2, E=TS	4.650***	2.990	3.414	3.641
Females, dataset 2, E=YS	1.611	2.922	3.295	3.985
Females, dataset 1, E=YS	1.086	2.748	3.066	3.557
Null hypothesis: exogeneity of E				
Females, dataset 2, E=TS	4.208***	2.730	3.105	3.766
Females, dataset 2, E=YS	1.974	2.707	2.993	3.342
Females, dataset 1, E=YS	1.547	2.713	2.994	3.425

Note: one, two and three stars when the statistic rejects the null at the 10, 5 and 1 percent level of confidence

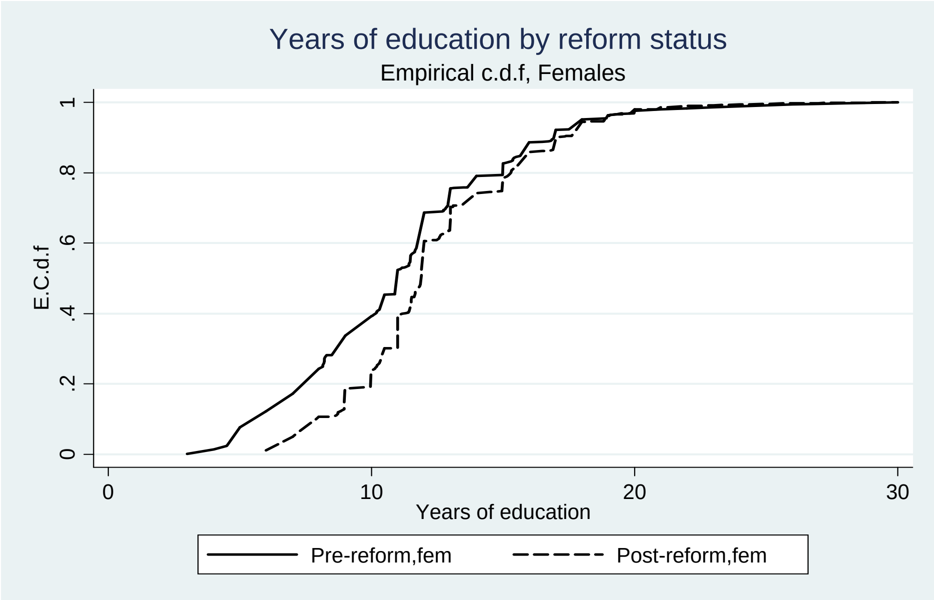
Table 9. Quantile estimates of the effect of years of education and test scores on BMI. Window (-7,+7), datasets 1 and 2, females only.

	Exogenous years of schooling – dataset 1	*Exogenous years of schooling – dataset 2*	*Exogenous test scores – dataset 2*	*Endogenous test scores – dataset 2*
Quantile 0.1	-0.081***	-0.063***	-0.003	-0.009
	[0.010]	[0.013]	[0.004]	[0.013]
Quantile 0.2	-0.106***	-0.086***	0.000	-0.003
	[0.009]	[0.012]	[0.004]	[0.014]
Quantile 0.3	-0.129***	-0.113***	0.002	-0.011
	[0.009]	[0.012]	[0.004]	[0.012]
Quantile 0.4	-0.155***	-0.140***	0.002	-0.009
	[0.009]	[0.012]	[0.005]	[0.013]
Quantile 0.5	-0.174***	-0.160***	0.000	-0.010
	[0.009]	[0.012]	[0.005]	[0.015]
Quantile 0.6	-0.206***	-0.181***	-0.007	-0.032**
	[0.009]	[0.013]	[0.006]	[0.015]
Quantile 0.7	-0.229***	-0.197***	-0.007	-0.045**
	[0.011]	[0.015]	[0.006]	[0.019]
Quantile 0.8	-0.258***	-0.208***	-0.001	-0.047**
	[0.014]	[0.017]	[0.008]	[0.022]
Quantile 0.9	-0.313***	-0.247***	-0.008	-0.063
	[0.018]	[0.021]	[0.011]	[0.038]

Note. All quantile regressions include country dummies and a country specific second order polynomial in K=T-7. The benchmark country is France in the larger dataset and the UK in the smaller dataset. Pivotal age is the age first affected by the school reform. Portugal and Sweden are not included. Standard errors within brackets. One, two and three stars for coefficients statistically significant at the 10, 5 and 1 percent level of confidence. Not reported in the table, each regression includes also the log of GDP per capita, the unemployment rate, life expectancy, calorie intake at pivotal age, average temperature at birth and a dummy for birth in the second part of the year. The estimates are obtained using the software Ox and the algorithm provided by Hansen in his research web-page.

Figures

Figure 1: Empirical cumulative distributions of years of education by reform status and gender



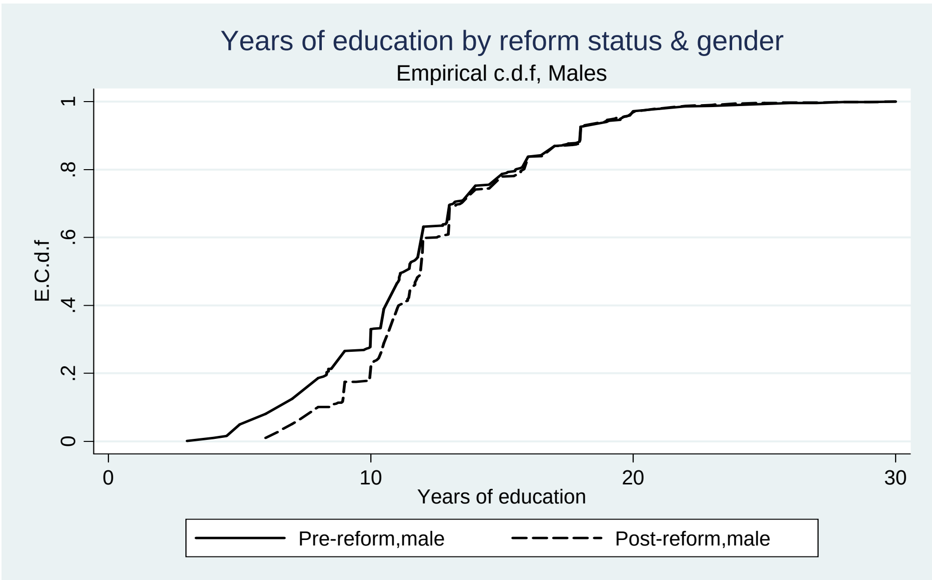
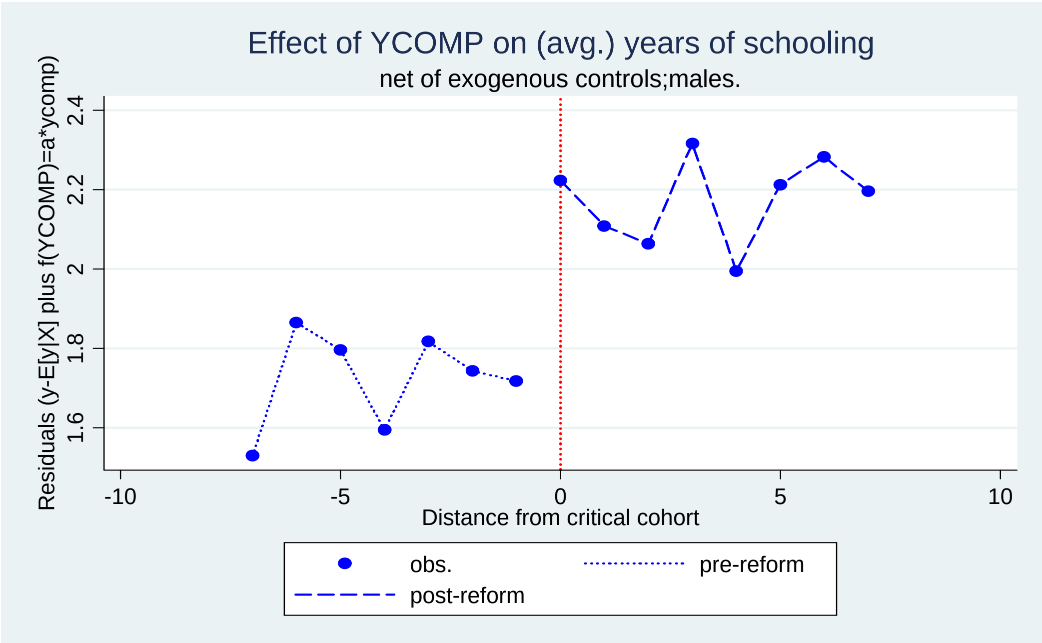


Figure 2: Effect of the instrument YCOMP on (average) years of schooling, net of exogeneous controls. By gender



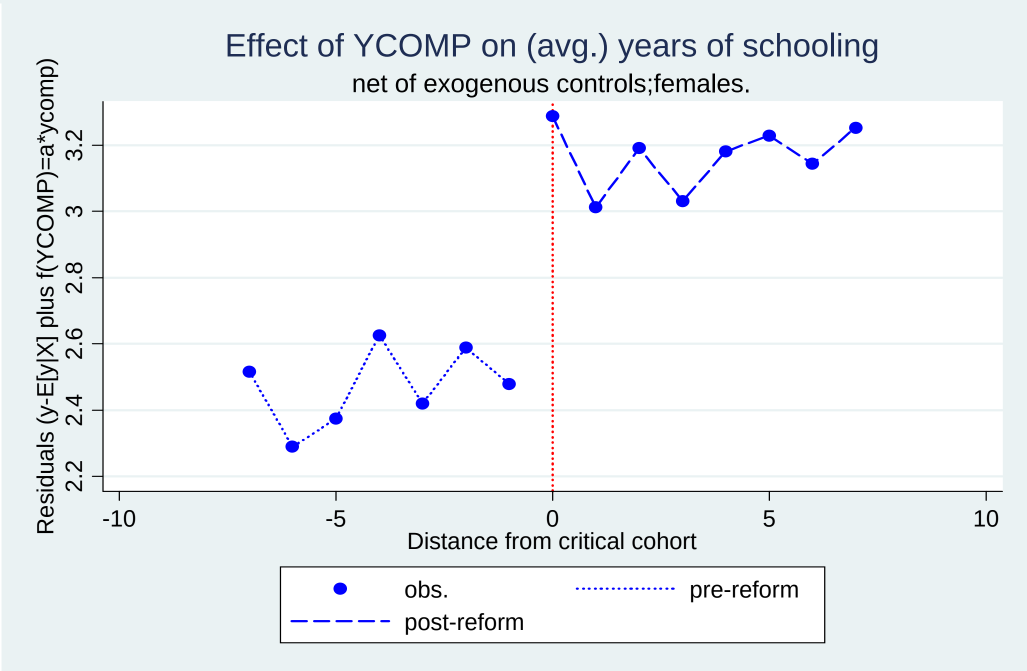


Figure 3: Empirical cumulative distribution function of BMI, by reform status and gender.

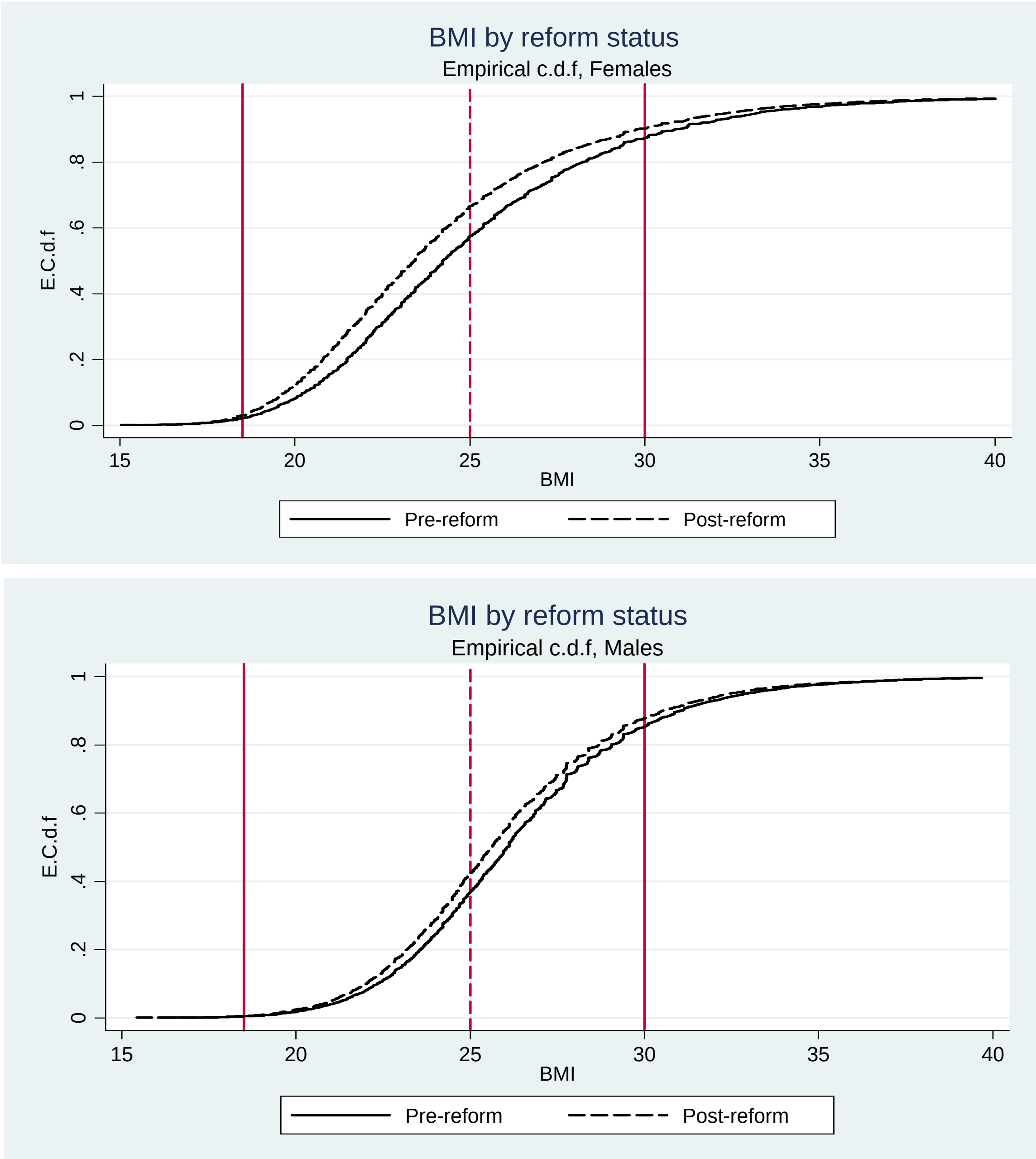


Figure 4: Average test scores and years of schooling. Dataset 2. Females only

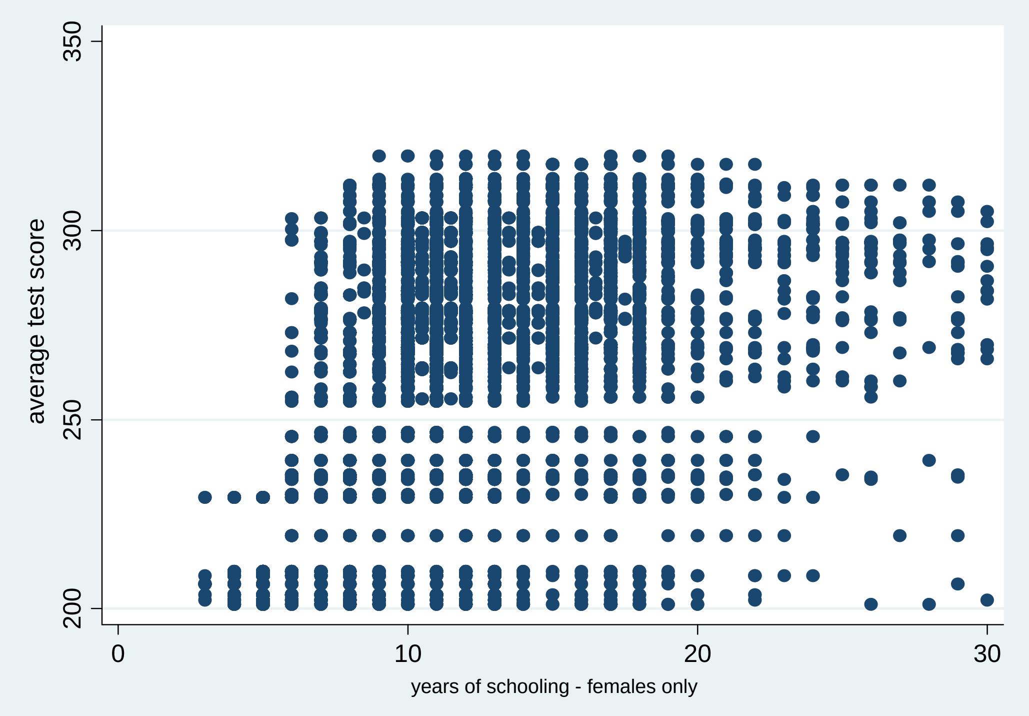


Figure 5: Effect of the instrument YCOMP on (average) test scores, net of exogenous controls. By gender

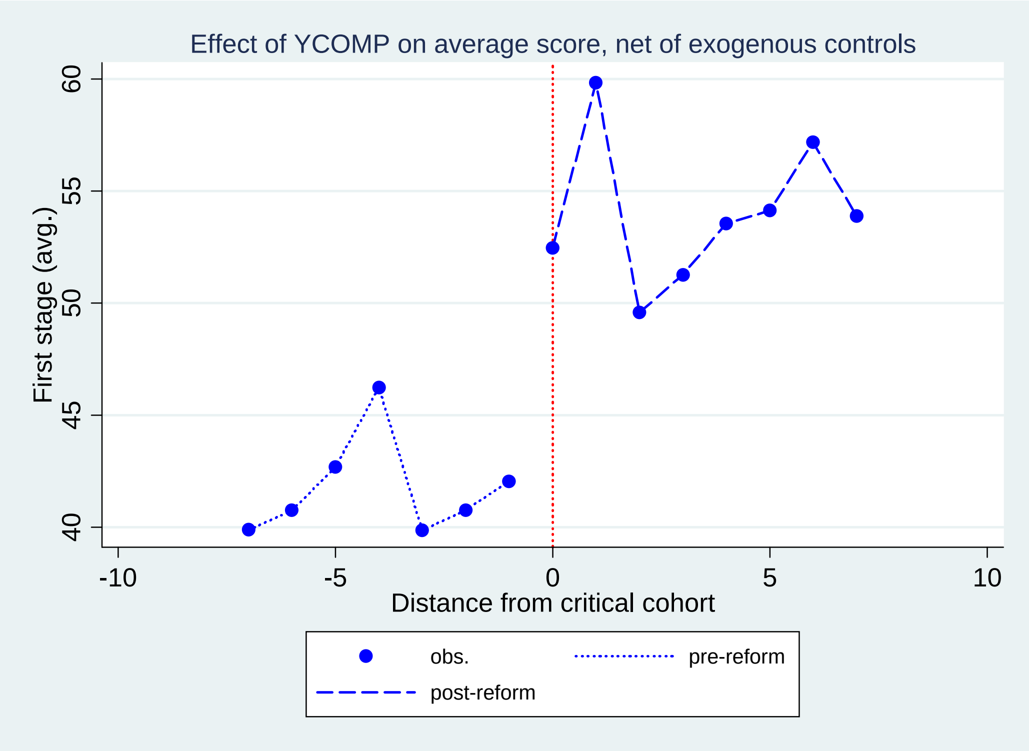
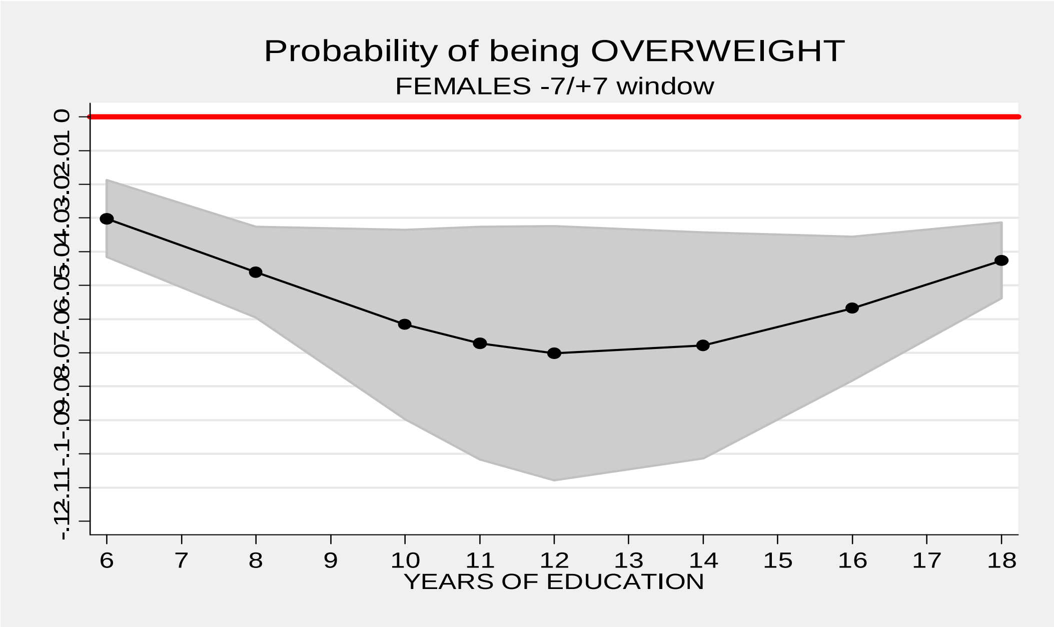
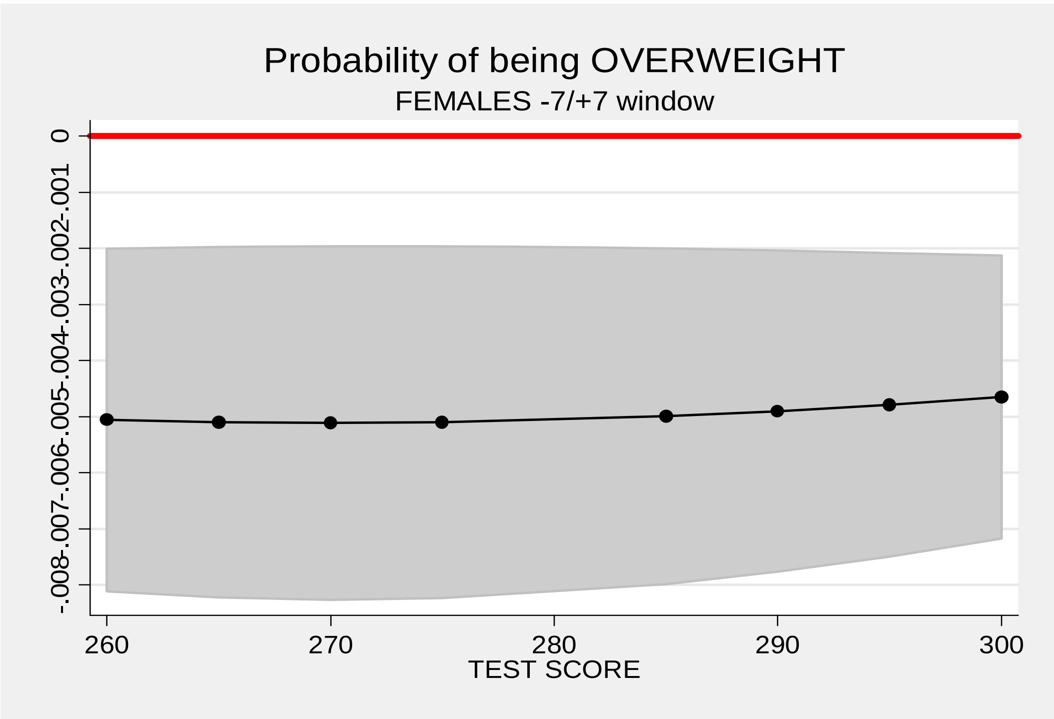


Figure 6. Estimated average partial effects of education on the probability of being overweight as functions of years of schooling and test score.





Notes. We compute the average partial effects (APEs) following Wooldridge (2002, section 15.7). Standard errors are based on 500 boostrap replications. The partial effects are calculated at several levels of education considering France as reference country and the first post-reform cohort as reference cohort. We set the value of the regressors at their average value in the sample of French women and men respectively born in 1954.

Figure 7. Treatment effect when the treatment (years of schooling) is assumed to be exogenous. Females only.

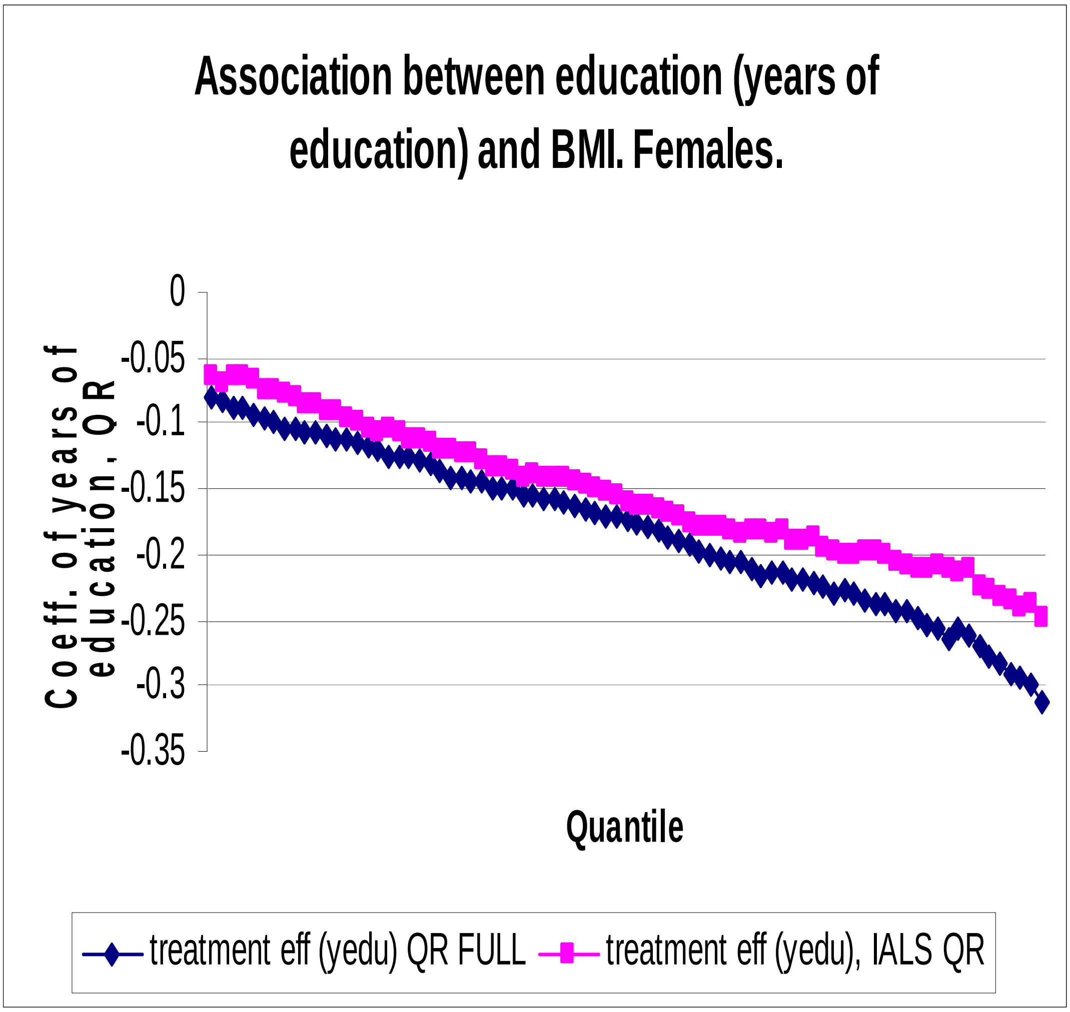


Figure 8. Quantile Treatment Effect when the treatment is instrumented with the years of compulsory schooling, following the approach by Chernozhukov et al (2006). Females only.