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The effect of schooling on health: Evidence on several health outcomes and behaviors*

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Abstract

This paper investigates the non-pecuniary benefits of education in terms of several individuals' health outcomes, health-damaging and health-improving behaviors, and preventive care. We exploit a reform which raised compulsory schooling by three years in Italy to identify the causal effect of lower secondary education and, unlike most previous papers in the literature, we analyze a wide range of health indicators. Our analysis shows that the rise in schooling induced by the reform reduced BMI and the incidence of obesity across Italian women, and raised men's likelihood of doing regular physical activity and cholesterol and glycemia checks. No effect is found instead on preventive care and health-improving behavior for women, and on smoking prevalence and intensity for both genders. Some potential reasons for the gender differences in the results are discussed.

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1 Introduction

Noncoummunicable diseases (NCDs) are the primary cause of death in the world, and account for 63% of the 57 million deaths that occurred in 2008 (WHO, 2011b). In most middle- and high-income countries NCDs were responsible for more deaths than all other causes of death combined. Almost all high-income countries report the proportion of NCD deaths to total deaths to be more than 70%. In the same group of countries NCDs accounted for 77% of the total number of years lost, compared to 7% for communicable diseases and 11% for injuries (WHO, 2011a). Italy makes no exception in this respect with an estimated 537,000 deaths due to NCDs, which represent 92% of total deaths (all ages) (WHO, 2011b).

In 2008, the leading causes of NCDs deaths were cardiovascular diseases, cancers, respiratory diseases, including asthma and chronic pulmonary diseases, and diabetes. Important behavioral risk factors of heart diseases and stroke include tobacco use, physical inactivity and unhealthy diet (low fruit and vegetable diets), which are responsible for about 80% of all coronary heart disease and cerebrovascular disease worldwide (WHO, 2011a).

As the incidence of health-risky behaviors tends to be higher among low educated individuals, higher educated individuals tend to live longer and healthier lives (Cutler and Lleras-Muney, 2010). This simple stylized fact has stimulated many researchers to investigate the origins of such a positive association.

Theoretically, three main channels have been identified for the effect of education on individual health and health-related behaviors. Grossman (1972) stressed the *productive efficiency* argument. The underlying idea is that education directly affects the health production function and that, given the same quantity of inputs, more educated individuals produce a higher stock of health than less educated ones. A second channel is *allocative efficiency*: education alters the input mix in the health production function. As Rosenzweig and Schultz (1983) put forward, this argument in its strongest form maintains that education will have no impact on health unless it changes inputs in the health production function, and the coefficient on education in this function would be zero if all inputs were included. Hence, the main mechanism through which education affects the input mix is by increasing health-related knowledge — e.g. on the harmful effects of smoking — or the speed of adoption of health enhancing inputs.[1] Finally, the *third variable hypothesis* suggests instead that education is endogenous with respect to health, because education and health choices are likely to be affected by the same set of unobserved factors (Fuchs, 1982). To clarify this point consider the case of the intertemporal rate of discount. Individuals with a high discount rate are likely to invest less in education and more likely to engage in health-damaging behavior, e.g., smoking, heavy drinking. Hence, from the third variable hypothesis perspective, a negative correlation between education and smoking stems from an unobserved variable, and does not reflect a true causal relationship (see, for instance Farrell and Fuchs, 1982; Sander, 1998).

The last explanation suggests that care should be taken of the potential spurious correlation between education and health owing to individual unobserved heterogeneity. For this reason, whereas early papers only reported simple associations between education and health, researchers have recently adopted careful identification strategies to assess the causal nature of the health-education gradient. In particular, recent work is increasingly using instrumental variable (IVs) strategies and fuzzy regression discontinuity (RD) designs for identification (for a review, see Grossman, 2006). A very popular 'instrument' in this literature is the introduction of compulsory schooling age reforms. This source of identification has been already exploited in a number of countries, including the US (Adams, 2002; Glied and Lleras-Muney, 2003; Lleras-Muney, 2005), the UK (Oreopoulos, 2006; Silles, 2009; Lindeboom et al., 2009; Clark and Royer, 2010), Denmark (Arendt, 2005), France (Albouy and Lequien, 2009), Germany (Kemptner et al., 2011), Italy (Atella and Kopinska, 2011), the Netherlands (van Kippersluis et al., 2011),

[1]However, the evidence supporting this argument is mixed. Studies supporting this explanation of the effect of education include de Walque (2010) while evidence in Kenkel (1991) and Nerín et al. (2004) does not support this argument.

and Sweden (Spasojevic, 2010; Meghir et al., 2012). Brunello et al. (2012) provides a cross-section analysis using compulsory schooling reforms in several European countries. Notwithstanding the high number of existing studies, it is very difficult to draw clear conclusions from this literature. First, almost all studies focus on single or a just a handful of health indicators, most often Body Mass Index, the probability of obesity, and smoking.[2] Second, the results of these studies are 'mixed' and some of the differences may stem from the different identification strategies used by researchers. Whereas some studies implement 'textbook' RD designs (see Lee and Lemieux, 2010), in many other cases they do not control in a flexible way for birth cohort trends, so as the causal effects are not identified in proximity of the discontinuity produced by the educational reforms and the estimates may be contaminated by other factors changing over time across cohorts.

In this paper, we seek to contribute to the previous literature in a number of ways. First, thanks to a very specific health data source available for Italy we provide evidence on the causal effects of an individual's education on a very wide set of health variables. Namely, we consider: (i) general health outcomes, such as chronic diseases and limitations in activities of daily living; (ii) body-weight health outcomes, such as BMI, and the probability of overweightness and obesity; (iii) 'health-damaging' behaviors, namely current smoking status, average number of cigarettes smoked per day, probability of ever smoking; (iv) 'health-improving' behaviors, in particular doing physical activity or following a particular dietary regime; (v) health-preventive behaviors, namely the probability of having ever done a mammogram, a pap smear test or a Computerized Bone Mineralometry (CBM) and cholesterol, glycemia and blood pressure check ups. Considering all these outcomes and behaviors together enables us to provide a more general assessment of the effect of education on 'health'. Moreover, as we report in the next section, the causal effect of education on the last two types of behavior (health-improving and preventive care) has been rarely investigated in the literature.

Second, as we use the same identification strategy and the same data to examine all

[2]The only exceptions are Clark and Royer (2010) for the UK and Kemptner et al. (2011) for Germany.

health outcomes and behaviors, the differences in the causal effect of education among outcomes and behaviors observed in our analysis are likely to reflect true differences rather than being an artefact of adopting different identification strategies, or using different data, like it may be the case when comparing the results of separate studies covering different health measures.

Last but not least, in this paper we provide new causal evidence for Italy, for which little evidence is available, using for identification a compulsory schooling reform implemented in 1963. Among those exploited in the literature, the Italian reform is the one which introduced the largest increase in the length of compulsory schooling (three years, see Section 4.1). The new school obligation spanned the whole length of lower secondary education (grades 6-8), meaning that successful compliance with the reform led individuals to achieve a higher educational qualification with respect to the past.[3]

The structure of the paper is as follows. The following section reports a brief survey of the related literature, with a special focus on papers seeking to estimate causal effects. Section 3 describes the data and section 4 the identification strategy and the main features of the 1963 school reform, which provides exogenous variation in individuals' schooling needed to idenfify causal effects. The main results of our analysis are reported in section 5 and discussed in section 6. Section 7 summarizes our main findings and concludes.

2 Existing evidence

This paper is mainly related to two strands of literature. The first one is the literature on the effects of education on individual health outcomes, while the second strand is the literature about the effects of changes in school laws on individual school attainment. In this section we briefly review the first line of research, while the second one is presented in Section 4.1.

The first group of contributions usually finds a positive correlation between educa-

[3]This is equivalent to junior school in the U.S. context.

tional attainment and health status. In a recent paper Cutler and Lleras-Muney (2012) summarize the available evidence on the relationship between education and health. They conclude that a clear and robust association between education and health emerges both in rich and in poor economies. Education affects a wide range of health indicators such as life expectancy (Cutler and Richards, 2008; Meghir et al., 2012), disease incidence, preventive and risky behaviors (Mokdad, 2004). In addition, education produces positive intergenerational spillovers: children of educated parents are also in better health conditions in both developed and developing countries (Strauss and Thomas, 1995; Schultz, 2007). This positive correlation is extremely robust to the measures of health used, both in terms of general health status and of health related behaviors. Generally speaking, more educated people are healthier, have fewer health conditions and limitations, tend to engage in healthier behaviors and live longer.

From a theoretical point of view, these results can be rationalized in different ways. First, the level of education influences labor market performance in terms of job opportunities and income, which may translate in better health status. Second, more educated individuals are able to choose a better combination of time and consumption which ensures them a better health. Third, education can enhance health productivity, as well as market productivity. Finally, educational attainment can be interpreted as an individual observable characteristic correlated with some individual unobservables, such as psychological and personality traits, ability, self-esteem or self-control, which translates in better health outcomes.

Thus, although there is a well established consensus on the positive correlation between education and health outcomes, it is less obvious that this relation depends on a causal mechanism. Clearly, correlation does not imply causation: individuals having a better health status are less likely to dropout from school and to obtain a higher educational attainment. Furthermore, unobserved individual characteristics may affect both health and education. In all these cases an endogeneity problem is at work and the true causal effect of education on health may differ from the partial correlation between the two variables estimated with OLS.

In the last years a number of studies identified the health-education gradient on a variety of health outcomes, from the childhood to the old age, using as exogenous source of variation changes in school laws[4] or other natural experiments (changes in school tracking, school fees or exemptions from military service). It is worth saying that in this field a variety of outcome measures has been used but the vast majority of studies are focused on the UK and the US, although there is an increasing number of case studies for other countries. In what follows, we summarize the main findings of this literature according to type of health outcome considered.

Some papers focus on individuals' *general health status*. General health status can be measured through self-reported measures or objective biomarkers. While self-reported measures suffer from a variety of biases, biomarkers are unbiased measures since they are medical indicators allowing to characterize a biological processes as normal or pathological or requiring a pharmacologic/therapeutic intervention, but unfortunately are rarely available. For Britain, Oreopoulos (2006) uses a regression discontinuity approach and estimates a positive and statistically significant effect of education on self-reported health. Similarly, Silles (2009) identifies a positive causal effect of education on self-reported health, which is much larger than the OLS estimates. By contrast Clark and Royer (2010) exploit changes in the duration of the compulsory school in Britain and find little evidence of health returns, in terms of improved health outcomes or changed health behaviors. For Sweden, Meghir et al. (2012) do not find any evidence that education positively affects health outcomes as measured by the incidence of hospitalizations and health care costs associated with inpatient episodes. Arendt (2008) uses a Danish school reform to causally estimate the effect of schooling on hospitalization. He finds that education has a large and significant negative effect on the probability of being hospitalized for women, while for men the evidence is mixed. Jürges et al. (2011) find for Germany that education positively affects women's self-reported health. Mazumder (2008) esti-

[4]See Lochner (2011) or Eide and Showalter (2011) for a detailed review.

mates that in the US education reduces health limitations and produces a large effect on general health status, with the exception of diabetes.

As for the papers using more *objective health measures*, Powdthavee (2010) uses hypertension as a biomarker for health and finds that in the UK an additional year of school from the 1947 law change leads to a reduction in the probability of hypertension by approximately 7-10 percentage points, but that this effect disappears when considering the 1973 law change. By contrast, Jürges et al. (2011) do not find any causal effect of education on objective biomarkers. The same conclusions are reached by Clark and Royer (2010) when using as objective health measures blood pressure, BMI or levels of inflammatory blood markers.

In this area of research, many works consider as a natural outcome for health status *mortality*. van Kippersluis et al. (2011) use a Dutch compulsory schooling law to estimate the causal effect of education on mortality. They find that education has a significant and robust negative effect on mortality. Using as an alternative source of variation for years of schooling unexpected exemption from compulsory military service after a major quake in Southern Italy, Cipollone and Rosolia (2011) find that the cohorts exempted have higher graduation rates from tertiary education and a significantly lower mortality rate.

Almost all previous studies consider a single country. One of the few exceptions is represented by Brunello et al. (2012) that in a cross-country analysis, identify a causal effect of years of schooling on female BMI only, although the effect is much smaller than the one found for the US by Grabner (2009) using variations in compulsory schooling and child labor laws across states.

A second group of studies focuses on the effect of education on *health-damaging behaviors*. For example, Jürges et al. (2011) use cross-state variation in the number of academic tracks schools in Germany to identify the effect of schooling on two main health related outcomes: smoking and obesity. Education is negatively related with smoking both for women and men, while no causal effect is found on reducing overweight and obesity. Kenkel et al. (2006) find similar results for the US when considering high school education. A negative effect of education on smoking rates is also found by de Walque (2007) and Grimard and Parent (2007) considering increases in tertiary education in the US. Using a sample of US twins, Lundborg (2008) does no find any statistically significant effect of education on smoking and obesity. Studies using compulsory schooling reforms generally do not find instead any effect of education on smoking (Clark and Royer, 2010; Kemptner et al., 2011) while do find a protecting effect for obesity (Kemptner et al., 2011; Brunello et al., 2012). All in all, the evidence is therefore quite 'mixed'. Studies which do find positive effects of education on smoking, for instance, generally do it for educational levels equal to high school or higher.

A third group of contributions consider the relationship between education and *health-improving behaviors* or *health-preventive behaviors*. On average, education induces individuals to have healthy lifestyles. For example, using a sample of Korean men the results of Park and Kang (2008) suggest that high school education is associated with a higher probability of regular physical exercise and regular health checkups. According to Cutler and Lleras-Muney (2010) better educated people are less likely to be heavy drinkers, more likely to drive safely, live in a safe house and use preventive care.[5] Using data from a sample of Italian adults, Atella and Kopinska (2011) conclude that education affects individual BMI both directly and indirectly by increasing productive and allocative efficiency of lifestyles, health inputs and energy balance components. Education enhances physical activity and sport practice, but its effect is heterogeneous along the BMI distribution. There is a very limited number of studies investigating the effect on these behaviors of compulsory schooling laws, and our paper partly contributes to fill this gap. Clark and Royer (2010) show no evidence that education improves health behaviors in terms of dietary regime and regular physical activity in the UK while O' Sullivan (2012) uses Irish data and finds that exogenous changes in the schooling of men had a positive effect on physical exercise later in life.

[5] Currie and Moretti (2003) use exogenous variation across US counties in the number of two and four-year colleges and find that education translates in better prenatal care.

3 Data

In this paper we use micro data from the survey 'Health conditions and use of health services' (*Condizioni di salute e ricorso ai servizi sanitari*) administered by the Italian National Statistical Institute (ISTAT). For the sake of brevity we will refer to this survey as the Italian Survey of Health (ISH, hereafter). At present, there are three waves of data, for the 1994, 1999 and 2004 years, respectively. This survey is a repeated cross section on a yearly nationally representative sample of the Italian population. It collects information on different aspects of everyday life at individual and household level and it has a specific focus on health. Together with basic demographic characteristics, for individuals aged at least 18, also information about objective and subjective health status, self-reported anthropometric measures, smoking, dietary regimes, physical activity, health checkups, and use of health services are collected. Anthropometric measures allow us to compute individual BMI that is defined as the weight in kilograms divided by the height in meters squared. According to this index, the international standards classify individuals as severely underweight (BMI below 16.5), normal weight (BMI over 16.5 up to 24.9), overweight (BMI from 25 up to 30) or obese (BMI over 30).

In our empirical analysis, we consider several health outcomes and behaviors defined as follows:

- Health Outcomes

  Chronic diseases. The survey asks for the presence of specific chronic diseases. Although the questions changed a bit across waves, all three waves ask for the presence of the following health conditions: asthma, allergy, diabetes, cataract, hypertension, heart attack, angina pectoris, other heart disease, ictus, bronchitis, arthrosis, osteoporosis, ulcer, malignant tumor, migraine, stones (liver or kidney), cirrhosis. For each disease, two measures are provided, one for the diseases being self-reported and the other for the diseases being diagnosed by a doctor (also in this case the source is survey and not administrative hospital data). We defined accordingly two dummy variables, one for the presence of at least one of the above self-reported diseases, and one for these diseases being diagnosed.

Limitations in activities of daily living. The survey gathers information on the number of days during the last four weeks individuals were limited from doing activities of daily living or were constrained in bed.

Body Mass Index (BMI). Individuals self-reported their height (in meters) and their body weight (in kilograms), from which it is possible to build a measure of BMI.

Overweightness. This is defined as BMI being greater than or equal to 25.

Obesity. This is defined as BMI being greater than 30.

- 'Health-damaging' behavior

  Current smoking status. Individuals were asked their current smoking status. We define a dummy variable which equals one if an individual is a current smoker and zero otherwise.

  Average number of cigarettes smoked per day. Current smokers were asked the average number of cigarettes smoked per day. The number of cigarettes is set to zero for current non-smokers.

  Ever smoked. This is a dummy variable which equals one if an individual is either a current smoker or smoked in the past and zero otherwise.

- 'Health-improving' behavior

  Intense physical activity. Individuals were asked if, in their spare time, they did intense physical activity (such as agonistic sports, fast running or biking) at least once a week and for how long. Although the data have been collected in all the waves, they are publicly available for research only for the last two waves.

  Regular physical activity. Individuals were asked if, in their free time, they did regular physical activity (such as going to the gym, running, biking up to a level where they sweat a bit) at least once a week and for how long. Although the data have been collected in all the waves, they are publicly available for research only for the last two waves.

Any physical activity. From the previous two questions and combining an additional item about doing soft physical activity (such as walking at least one kilometer or doing soft exercise) we generate a dummy variable taking value one for those individuals doing at least once a week any type of physical activity.

Dietary regime. All the waves collected information on the specific dietary regime (if any) followed by the respondents. In particular, we have information on the type of diet usually followed (i.e. low salt, low fat, low sugar, vegetarian, macrobiotic) and the reasons why it has been chosen. From these questions, we defined one dummy variable for having a 'special' dietary regime.

- Preventive-care behavior

Mammogram. Women were asked if they ever had a mammogram.

Pap smear test. Women were asked if they ever had a pap smear test.

CBM. Women were asked if they ever had a CBM.

Glycemia check, cholesterol check, blood pressure check. Individuals were asked if they had regular glycemia, cholesterol and blood pressure checks, when was the last one and why they did this check. The questions are slightly different from one wave to the other, but we were able to construct a dummy variable taking value one if the respondent had a check in the last year.

Diagnostic medical examinations. Individuals were asked if they had medical examinations in the last 4 weeks, their typology and the reason why they had these visits. From these questions we construct a variable with the number of diagnostic medical examinations made in absence of any particular symptom. The number of preventive exams is set to zero for individuals who had no exams.

Paid examinations. This variable indicates the number of paid preventive examinations.

*Flu immunization.*This variable takes value one for those individuals who had a flu shot in the last year.

Tables A1 and A2 in the Appendix report sample summary statistics by gender for the outcome variables that we will use in our analysis.

4 Identification strategy

In October 1963, Law n. 1859 December 31st, 1962 comes into effect, increasing the length of compulsory schooling from 5 (primary education) to 8 years (5 years of primary education and 3 years of lower secondary education), and raising school-leaving age to 15 in Italy. The law prescribed individuals to attend school at least until graduation from lower secondary education. However, 15 years old children with at least 8 years of schooling were exempted from the obligation. Enforcement of the law was far from being perfect initially, and improved gradually, although it is necessary to wait until the mid '70s to observe full compliance with the new school obligation (Checchi, 1997; Brandolini and Cipollone, 2002).

The individuals potentially affected by the reform were aged less than 15 in 1963 and without a lower secondary education diploma. For the initial three cohorts affected by the reform, we expect the effect to be stronger for younger cohorts. Indeed, those born in 1949 were required to stay only one additional year in education, those born in 1950 two years, and those born in 1951 or later three years.

The 1963 reform allows us to divide the Italian population between individuals who were subject to the reform (the cohorts born from 1949 onwards) and those who were not. This is the main source of identification that we exploit. In particular, we use the eligibility for the 1963 reform as an instrumental variable in a 'fuzzy' regression discontinuity (RD) design (Lee and Lemieux, 2010). As we will see, the reform created a discontinuity in lower secondary schooling achievement. The design is 'fuzzy' since, like we said, compliance with the reform was not perfect. The typical 'compliers' are those individuals who in the absence of the reform would have quit education at the minimum legal age (11 years). Using the reform for identification enables us to estimate the causal effect of rising compulsory education on this specific subpopulation. The results will be hardly generalizable to the general population, but the subpopulation of compliers is nonetheless an informative one as it comprises individuals who are more likely to experience worse health outcomes (e.g., because they have higher intertemporal discount rates), and for whom policy interventions may matter most.

In what follows, we describe in more detail our 'fuzzy' RD design. Our identification strategy can be described by a two-equation system, one equation for the outcome variable (Y) and one for the endogenous variable (D):

$$Y = a_0 + \tau D + a_{l1} f(B-c) + (a_{r1} - a_{l1}) \quad f(B-c)T + a_2 X + \epsilon \tag{1}$$

$$D = b_0 + \gamma T + b_{l1} g(B-c) + (b_{r1} - b_{l1}) \quad g(B-c)T + b_2 X + v \tag{2}$$

where the individual subscript has been omitted, B is the individual birth cohort, c is the *pivotal cohort* or *cut-off point*, i.e. the first birth cohort affected by the reform, $g(.)$ and $f(.)$ are two polynomials in birth cohort (with the pivotal cohort normalized to zero). D is an endogenous treatment dummy, corresponding to the fact of holding a lower secondary education diploma, $T \equiv I(B \geq c)$ is a dummy for the reform eligibility (i.e. $B \geq c$), and X a vector of covariates. Among the covariates we included a quadratic term in age, and year and region fixed effects.[6] ϵ and v are two classical error terms and the as, the bs, τ and γ the parameters to be estimated.[7]

There are two ways of implementing parametrically the RD, one is the 'local linear

[6] It is not possible to include a linear term in age as it is perfectly collinear with birth cohort trends, treatment status and year dummies. At the same time including the quadratic term in age is likely to capture age-specific effects on health and health-related behaviors. The same is done, for instance, in Kemptner et al. (2011).

[7] After centering the birth cohorts to c, i.e. the pivotal cohort, γ becomes the effect of the reform on educational attainment.

approach' and the other the 'global polynomial approach'.[8] In the first approach, the researcher estimates a linear regression ($f(B-c)$ and $g(B-c)$ are both linear) only using observations very close to the discontinuity. This reduces the risk of bias at the cost of reducing efficiency. Clearly, this approach can only be implemented with very large datasets (e.g., with Census data) so as even focusing on a few cohorts around the threshold one is likely to have a sufficient number of observations. The second consists in using all the data available, but specifying a flexible polynomial to capture cohort trends. We use here a 'mixed' approach, namely a 'local polynomial approach'. Indeed, our survey data are not suitable to the local linear approach, as we only have a limited number of observations by birth cohort (around 1,999 for women and 1,963 for men). At the same time, our view is that the assumption that a polynomial can properly control for all long-term unobserved factors changing between cohorts is tenable only if we consider cohorts which are not too distant in time from the pivotal cohort. Indeed, as the instrument exhibits only time variation, the 'reform dummy' could capture also other historical trends which might have affected both education and health behavior or outcomes. For this reason, we limit our analysis to individuals born between 1940 and 1960 considering a time window of 10 years around the cut off cohort.[9] Thus, the estimation sample includes individuals whose ages in the three waves are comprised between 34 and 65. We think that for such age range differential mortality rates by educational level (lower secondary schooling vs. lower education) are unlikely to be an important source of bias for our

[8]In the education and health literature the local linear approach has been used in Brunello et al. (2012); Kemptner et al. (2011); Lindeboom et al. (2009); Clark and Royer (2010) and the global polynomial approach by Kemptner et al. (2011). The validity of the global polynomial approach critically hinges on the assumption that the polynomial included is sufficient to control for secular trends in education and health, while the local linear approach produces 'more local' estimates (Lee and Lemieux, 2010; Imbens and Lemieux, 2008).

[9]This is often done in this literature. Indeed, country-wide long-term changes which may be correlated with the reform dummy and affect individual health, such as a contemporaneous increases in the supply of medical and educational infrastructures or the improvement of the welfare system, are likely to affect all cohorts in our relatively short time window, but unlike compulsory schooling not to experience a sudden discontinuity just the year of the reform. Clark and Royer (2010) use the same approach examining health measures and behaviors in the Health Survey of England data, in particular they adopt a 5-year bandwidth. We chose a 10-year bandwidth in order to consider cohorts sufficiently close to the reform, but at the same time to avoid having too few clusters, as in our analysis standard errors are clustered by birth year.

estimates.[10] ISTAT mortality tables show that the likelihood to die between age 34 and 65 was 10 percent in 2004 (similar figures are observed in 1999/2000 and 1994, the years covered by our analysis). This may not be a substantial figure for differential mortality by education to show up (see the discussion in van Kippersluis et al., 2011). Unfortunately, we do not have data to test this claim formally, but have to rely on the findings of other researchers. Albouy and Lequien (2009) find for France that survival rates at 50 and 80 years old do not seem to be affected by years of schooling between 13 and 16. Clark and Royer (2010), whose sample includes individuals under 70, find no evidence of a significant negative effect of compulsory education on mortality. By contrast, Cipollone and Rosolia (2011) investigate the effect of a military service mass exemption —introduced following an earthquake— which increased high school completion in Southern Italy, and find that increasing by 1 percentage point the proportion of high school graduates (corresponding to upper secondary schooling) reduces subsequent 10-year mortality rates (between the age 25 and 35) at the municipality level by 0.1-0.2 percentage points. However, they do not study the effect of lower secondary schooling. Last but not least, van Kippersluis et al. (2011) do find a negative effect of compulsory schooling on mortality, but considering much older individuals than we do: for men surviving to age 81, an extra year of schooling is estimated to reduce the probability of dying before the age of 89 by almost three percentage points relative to a baseline of 50 percent.

A peculiarity of our 'fuzzy' RD design is that the discontinuity is defined on a discrete variable (birth cohort). We follow Lee and Card (2008) and assume that the specification error is the same at each side of the discontinuity (see Lee and Card, 2008, Appendix B). In this case, standard errors must be clustered by birth cohort. As stressed by Lee and Card (2008), in the discrete case, identification can only be achieved parametrically by

[10] We expect that differential mortality by education should introduce a downward bias in our estimates. Indeed, at a given age relatively healthier low-educated individuals will be over-represented in each birth cohort, that is they will be drawn from the top of the health stock distribution. This may happen, for instance, as low-educated individuals have access to low quality care and are less likely to survive. Thus, low-educated surviving individuals may be relatively less likely to be sick compared to highly-educated individuals, which will be drawn from the whole distribution of the unobserved health stock. This may lead to an underestimate of the differences in health outcomes and behaviors by education.

assuming a specific form for the regression function, and critically hinges on the correct specification of the polynomials $g(.)$ and $f(.)$. In our case, the order of the polynomial $g(.)$ has been determined by a test of the unrestricted model including birth cohort dummies and the restricted model only considering polynomials of different orders in birth cohort, as suggested by Lee and Card. As stressed by Lee and Lemieux (2010) this can be interpreted as a test for the polynomial specification *vs.* a more flexible nonparametric alternative.[11] The lowest polynomial order for which the restriction was not rejected was one, and $g(.)$ is accordingly specified as a linear trend in birth cohort. The same specification was also used for $f(.)$.[12]

In equations (1) and (2) we have used the 'fully interacted specification', by interacting the polynomials with the treatment variable. Lee and Lemieux (2010) stress the importance of letting the regression function differ on both sides of the cut-off point, as constraining the slope to be identical would amount to using data on the right hand side of the discontinuity to estimate a_{l1} (b_{l1}) in equation (1) ((2)) and observations to the left to estimate $a_{r1} - a_{l1}$ ($b_{r1} - b_{l1}$), which 'would thus be inconsistent with the spirit of the RD design' (Lee and Lemieux, 2010, p. 319).[13]

4.1 Italy's 1963 school reform in comparative perspective

For the sake of comparison of our results with those reported in previous papers, it is important to contrast the 1963 Italian reform with the compulsory schooling reforms

[11] The test can also be implemented in the version suggested in Lee and Lemieux (2010), which consists of including in the regression both the polynomial in birth cohorts and the cohort dummies. This is also a test for the existence of more than one discontinuity in the data.

[12] We follow Lee and Lemieux (2010) (p. 329) and use the same order of polynomial for the treatment and the outcome equations.

[13] This is also a main difference of our analysis from many specifications estimating the health returns to education existing in the literature (Brunello et al., 2012; Kemptner et al., 2011; Lindeboom et al., 2009; Atella and Kopinska, 2011). Another difference with respect to Atella and Kopinska (2011) which focus on the Italian case to investigate the effect of education on body weight — using the same reform but different data — is that they use a local linear approach but do not include birth cohort trends (p. 9). These estimates are therefore subject to the criticism reported by Oreopoulos (2006) as they cannot be given a RD design interpretation, in particular 'do not allow for systematic inter-cohort changes in educational attainment, and do not identify effects from cohorts who attended school just before and just after the law changes' (p. 155).

exploited for other countries.[14] Most evidence is related to anglosaxon countries (the US and the UK), although recent studies also cover a number of European countries.

Lleras-Muney (2005) exploits differences in compulsory schooling laws across US states, and their changes over-time, to estimate the effect of education on mortality among cohorts born between 1915 and 1940. During this period 57 changes in legislation were introduced in 36 states, 16 decreases and 41 increases. The author considers positive changes in the length of compulsory schooling to implement an IVs strategy. Unlike more recent papers she does not use a 'fuzzy' RD design by comparing cohorts around the policy change but a difference-in-difference approach.[15] The laws appear to be significant predictors of individual educational attainment, but the effect turns out to be quite small. Indeed, Cutler and Lleras-Muney (2012) observe how 'the laws that are typically studied increased educational attainment by 0.05 of a year, that is only 1 in 20 individuals obtained one more year of schooling' (p. 20).

For the UK Oreopoulos (2006) and Clark and Royer (2010) use the 1947 and/or the 1972 reforms. The 1947 reform increased the compulsory school-leaving age from fourteen to fifteen. This reform also implied de-tracking. As stressed in Oreopoulos (2006) 'The 1944 Education Act removed secondary school fees and made the first year of secondary school compulsory' (p. 159). Before the reform those who were planning to go on in education after compulsory schooling enrolled in secondary schools already at age 12 while those planning to quit at the minimum leaving age remained in elementary school. The second reform, introduced in 1972, increased the school-leaving age by one additional year. Oreopoulos (2006) reports a very high percentage of compliers for the 1947 reform, about 50% of the affected cohorts, and Clark and Royer (2010) report 25% compliance for the second reform. In both reforms, then, although the increase in the age of compulsory schooling was relatively small compared to reforms in other countries, compliance was very high. Both reforms appear to have significantly increased individual schooling and

[14] A summary of the main characteristics of compulsory schooling reforms in various countries can be found in Brunello et al. (2009).

[15] The same source of identification and strategy is used in Adams (2002) and Glied and Lleras-Muney (2003).

constitute valid instruments in a fuzzy RD design. Clark and Royer (2010) report, for instance, an increase in years of schooling of around 0.4 for men and 0.5 for women for the 1947 reform, and of 0.3 (for both genders) for the 1972 reform.

Arendt (2005) uses for Denmark a reform introduced in 1975 which raised the minimum school-leaving-age increasing the compulsory years of education from 7 to 9 years. However, they posit that the effect of such reform on educational attainment is dubious as most children already obtained 9 years of schooling at the time it was introduced, although it might have had additional effects on higher educational levels, as it was introduced together with de-tracking during the 8th and 10th forms. In fact, from their first stages the 1975 reform dummy does not appear to be a significant predictor of individual schooling.

Albouy and Lequien (2009) use for France two reforms, the Zay's (1936) and the Berthoin's (1967) reforms, which increased compulsory schooling by one (from 13 to 14) and two (from 14 to 16) years, respectively. Also in this case the authors show significant positive effects on the schooling levels of the cohorts affected. For the first reform the increase in an individual's probability to be enrolled in school at age 14 increased by 1.3 percent points (p.p.), while the second reform raised this probability by 5.6 p.p.

Kemptner et al. (2011) exploit for Germany the prolonging of compulsory basic track schooling from 8 to 9 years,[16] which was implemented by state governments at different points in time. The first state introduced the new obligation in 1949 and the last one in 1969. The authors do find a significant positive effect of the reform on years of schooling, for which the average increase was in the range of 0.6-0.7 years.

van Kippersluis et al. (2011) focus on a law which raised compulsory schooling from six to seven years in the Netherlands, and which was introduced in 1928. Their estimates show that the law increased average schooling years of the affected cohorts by between 0.6 and 1 year, depending on the specification of the smoothing polynomial (see section 4).

[16]This is the vocational track of lower secondary education.

For Sweden, Spasojevic (2010), and Meghir et al. (2012) use the comprehensive school reform which extended the number of years of compulsory schooling from 7 or 8 years, depending on the municipality, to 9 years, and which was implemented between 1949 and 1962 at different times across municipalities. Also in this case the reform implied a postponement of the age at students' tracking. Both papers find non negligible effects of the reform on educational attainment in Sweden. Meghir et al. (2012) estimate, for instance, an average effect of 0.18 years of schooling for men and 0.11 for women.

How does the Italian reform compare with respect to those already exploited for identification in the literature? A first peculiarity of the 1963 Italian reform is that it increased age of compulsory schooling by three years (grades 6-8), the largest amount among the reforms cited above. Secondly, compliance with the new obligation would have meant for successful students (those who were not subject to grade retention) leaving compulsory schooling with a higher educational qualification compared to the past, as the additional three years corresponded (and still do) to the length of lower secondary schooling in Italy. Considering reforms raising school-leaving ages by different amounts may be important if the effects of education on health and health behaviors are nonlinear or there are sheepskin effects, which correspond to achievement of certain educational titles. Moreover, the 1963 reform not only increased age of compulsory education, but also the age at tracking. Before the reform lower secondary schooling was divided into two tracks: the vocational track and the academic track (*scuola media*, middle school). The vocational track was chosen by individuals with lower socio-economic status who planned to quit education after completing lower secondary schooling since it did not allow access to upper secondary education, while the *scuola media* was chosen by by those individuals who planned to go on in education. After the reform, the two tracks were unified into a 'common' middle school (*scuola media unica*, comprehensive middle school).

Detracking of the school system is likely to have also changed the peer group of compliers, that is for those individuals who in the absence of the reform would have quit education at the minimum school leaving age, in the direction of increasing the average quality of the peer group measured in terms of both academic ability and socio-economic status. As we have seen above, detracking was not a distinctive feature of the Italian reform, but it is common to many reforms increasing the length of compulsory schooling, such as those implemented in the UK, Denmark and Sweden.

4.2 First-stage results: The effect of the 1963 reform on educational attainment

The 'fuzzy' RD design can be implemented through two-stage least squares (2SLS). In this section we report the results from the first-stage. Before going through the regression results, it is useful to visually inspect the data and look for the evidence of a discontinuity in schooling in correspondence with the 1963 reform.

The reform was targeted to 'marginal students', those who would not have continued in education in the absence of the increase of mandatory schooling age. These are also the individuals who are less likely to continue in post-compulsory schooling after having met the school obligation. For this reason, we limit our analysis only to individuals who completed at most lower secondary schooling. As emphasized by Lindeboom et al. (2009) focusing on the whole population, including in the estimation sample also highly educated individuals which were not targeted by the reform, is likely to reduce the estimated effect of the reform and generate a weak instrument problem.

Figure 1 reports the ratio of women completing lower secondary schooling by birth cohort. Since, as we said, compliance with the reform was gradual, and the first affected cohort (the one born in 1949) was required to attend compulsory education only for one additional year, we report two graphs one with the pivotal cohort set at 1949, and the other set at 1950. For women, it appears a clear discontinuity for the 1950 cohort. The 1958 cohort seems to be an outlier, and therefore it is important to test for more than one discontinuity in the data as suggested in section 4. Figure 2 reports the same graph for men. For men the reform appears to be effective since the very beginning as the discontinuity clearly appears already for the 1949 cohort. The time series does not show any additional discontinuity.

Table 1 reports the first-stage of the 'fuzzy' RD design. As the estimation samples change according to the dependent variable (health outcome or behavior) considered, we report here for exemplificative purposes only the first stage for the analysis of BMI. Some statistics on the first stage of all 2SLS estimates are reported in the following section. For each gender we have presented two estimates, one setting the pivotal cohort at 1949 and the other at 1950. The table shows the effect of the reform dummy, the cohort trends, the F-test on the excluded instrument and the test suggested by Lee and Lemieux (2010) to select the order of the polynomial. The parametric specifications mainly confirm the main results of the visual inspection of the data. For women, the discontinuity is stronger for the 1950 cohort and for men for the 1949 cohort. Thus, since in the 'fuzzy' RD design the reform dummy represents our excluded instrument, to avoid a weak instrument problem we set the pivotal cohorts to 1950 for women and 1949 for men.[17] The first-stage does not show any sign of a weak instrument problem, although the instrument is stronger on the female sample. The $t-$statistics are 6.2 for women and 4 for men. The linear specification for the polynomial seems to be appropriate for both genders, indeed in a linear regression including both a linear polynomial in birth cohort and birth cohort dummies, the latter are jointly insignificant.

Overall, the reform seems to have produced a significant positive effect on the probability of completing lower secondary education, which amounts to 5.8 p.p. for women and 4.1 p.p. for men. The reform was therefore more effective for women. These are non-negligible effects, given that the last pre-reform birth cohorts in the estimation sample (1949 for men and 1950 for women) exhibit percentages of lower secondary school educated individuals of 34.1% and 44.8%, for women and men respectively.[18] The mag-

[17]As it is stressed in van Kippersluis et al. (2011) also in our case "It is not therefore possible to determine the reform threshold with 100 percent accuracy for every observation. But this is not vital in a Fuzzy Regression Discontinuity Design (RDD) since the only requirement is that the probability of receiving treatment jumps discontinuously at the threshold, which, as we will show later, is obviously the case in our application" (p. 698).

[18]Hence, the percentage of lower secondary educated women and men rose by 17% and 9%, respectively.

nitude of the effect is comparable to the reforms used by Lleras-Muney (2005) for the US, but much lower than for the UK reforms used by Clark and Royer (2010). This is not necessarily a weakness of our study. Indeed, although we will able to estimate 'more local' treatment effects, i.e. effects on a smaller subpopulation of compliers, in our case the Stable Unit Treatment Value Assumption (SUTVA) —that is the absence of general equilibrium effects— is more likely to hold. Like in Lleras-Muney (2005) but contrary to Clark and Royer (2010), we may expect a substantial change in the peer group for the compliers with the 1963 reform, and potentially strong peer effects.

In Table 2 we report the results of a 'placebo experiment', in which we check for the presence of a discontinuity also in the likelihood of graduation from upper secondary education in correspondence with the 1963 reform. The idea is that if there were some concomitant unobserved factors (e.g., other concomitant reforms) which acted selectively on the pivotal cohort, and increased its demand for both education and health, this should have shown up also at higher levels of education. The dependent variable for this analysis is a dummy which equals one if the individual completed upper secondary schooling and zero if she achieved a lower level of education. The estimation sample is restricted to individuals with upper secondary schooling or lower. As the table shows, a similar discontinuity is not present for upper secondary education, neither for women nor for men. This also suggests, however, that (i) the reform did not have any 'spillover' effect on higher levels of education, and that (ii) it only affected 'marginal' students, i.e. those who would have quit before lower secondary education in the absence of the reform. Thus, including in the estimation sample also highly educated individuals would have greatly reduced the power of the instrument and the precision of the 2SLS estimates.

5 Empirical results

Before commenting on the results of the RD design, we report as a benchmark the OLS estimates for women and men in tables A3 and A4 in the Appendix, respectively. For

BMI, we find a negative association with education only for women (-1.071). By contrast education is negatively associated with overweightness and obesity for both genders. When we consider self-reported health measures (such as chronic illness, days with limitations or in bed), they exhibit a negative correlation especially with women's education, while a negative association is found for men only for days spent in bed. Education is significantly and positively associated with women's smoking behavior, and with men's ever smoking. As for physical activity, the OLS estimates show a positive coefficient on education for both genders. Considering special dietary regimes, education is positively associated with men's and women's following a diet mainly for non-health reasons. Switching to preventive behavior, for both women and men education is positively associated with the probability of doing a number of health check ups. Since as widely stressed in previous work, the OLS estimates are likely to suffer from the the bias produced by the endogeneity of education, in the remainder of this section we report and comment on the RD estimates only.

5.1 Effect of education on health outcomes

Table 3 presents the causal effect of education on *weight related outcomes*, in terms of BMI, overweightness and obesity, and on *general health outcomes*, in terms of incidence and potential consequences of chronic diseases.

The estimates in Table 3 show that lower secondary schooling contributes to reducing women's BMI by 3.254, corresponding to -12.52 per cent (as the average female BMI for women without lower secondary education in the estimation sample is 25.99). As lower secondary schooling consists in Italy of three years of education, this roughly corresponds to a -4.2 per cent reduction in BMI by one additional ('effective') year of education, a magnitude which in line with the figure (4 per cent) reported by Grabner (2009) for US women. The effect is precisely estimated and significant at the 1% statistical level. An equally precise and sizeable negative effect is found also for women's likelihood of being obese, which falls by 24 per cent points (p.p.), but not for overweightness.

The same effects are not observed for men. Indeed, the point estimates are much lower than for women, and never statistically significant.

Our result of a negative effect of lower secondary schooling on BMI and obesity only for women is not new to the literature, and it is in line with findings in Brunello et al. (2012), which report a protecting causal effect of years on education only for females using data from several European countries. Grabner (2009) finds for the US an effect which is stronger for women than for men.

In the following regressions we analyze whether lower secondary education decreases the incidence of (self-reported and diagnosed) chronic diseases and limitations in activities of daily living. For both men and women, the effects of education on the incidence of chronic diseases go in the expected direction, while those on the number of days in which individuals were limited in their daily activities or they spent in bed because of illness have a counter-intuitive positive sign.[19] In any case, the estimated effects are never statistically significant at conventional levels. Our results are in contrast with those reported in Kemptner et al. (2011) which find a negative effect of education both on long-term illness and work disability for men (but nor for women), but consistent with Clark and Royer (2010) who do not find any effect on long-term illness and reduced activity for both men and women.

As the instrument tends to be stronger for women than for men, in the table we also report the Anderson-Rubin Wald tests (Anderson and Rubin, 1949), for the null that the causal effect of education on the different outcomes is zero, which is robust in the presence of a weak instrument problem (Chernozhukov and Hansen, 2008). The tests confirm that lower secondary schooling appears to only affect health outcomes related to women's body weight.

[19] A possible explanation could be that individuals who work are more subject to illness, and schooling increases the likelihood of employment.

5.2 Effect of education on health-damaging behavior

In Table 4 we consider the effect of lower secondary schooling on health-damaging behaviors, namely individuals' smoking behavior.

We do not find any significant effect of schooling on smoking behavior, neither for women nor for men. The coefficients are however negative for the smoking intensive margin (average daily cigarettes consumption) and positive on the likelihood of being a current smoker or having ever smoked. Using IVs and compulsory schooling laws for identification, both Clark and Royer (2010) and Kemptner et al. (2011) find no evidence of a causal effect of schooling on smoking behavior. Then, on the basis of the existing evidence, there does not seem to be substantial health returns to increasing compulsory schooling in terms of reduced smoking participation and tobacco consumption.[20]

5.3 Effect of education on health-improving behavior

In Table 4 we also consider the effect of lower secondary schooling on health-improving behaviors, namely physical exercise and specific dietary regimes. Estimates indicate that education does not have any causal effect on the health-improving behaviors of women. By the contrary, on the male sub-sample, education translates into a higher probability of doing regular exercise at least once a week, a lower probability of making very intense exercise and a lower probability of following a particular dietary regime. If, on average, education induces individuals to have healthy lifestyles, it may also reduce their need to follow a particular dietary regime. Our results partially confirm those in Park and Kang (2008) who find a positive effect of education on exercise for men in Korea, but they are not fully comparable since their dependent variable takes value one for doing exercise or following a diet.

We make an attempt to go more in depth in the understanding of health-improving behavior related to body weight by considering alternative dietary regimes prescribed for

[20]We also tried to estimate regressions for quitting smoking but the samples are too small and the instrument is not significant in the first stage.

health related problems. IHS survey contains information on individual nutrition habits. In particular, individuals were asked whether they followed a diet with low fat and sugar or low salt for health reasons or if they were used to having a specific dietary regime for cultural reasons or for choice. Presumably, since more educated people are aware of the benefits of healthy eating, they should adopt such behavior regularly and they should have a lower need to follow a specific diet prescribing a low total intake of fats, sugar, salt and sodium. The results are reported in Table 5. No evidence of the influence of education also on these types of diet is found for women. By way of contrast, the likelihood to follow an hypocaloric diet, a diet lower in salt or a specific diet prescribed by a doctor is statistically lower among more educated men. This could partly explain why more educated men also tend to do more regular physical activity than women: they might have worse dietary habits.[21] Men are more likely than women to choose alternative dietary regimes and to adopt a diet but not for health reasons.

5.4 Effect of education on preventive-care behavior

One strength of the IHS data is that it gathers very detailed information on the use of health services, for which we can estimate the effect on health-preventive behavior.

The effect of lower secondary schooling on health-preventing behaviors is presented in Table 6. In particular, we consider preventive medical examinations in absence of symptoms, regular health checkups and immunizations. For the checkups specific for women (mammogram, pap smear and CBM tests) we find a marginally statistically significant negative effect of education. For these outcomes, only descriptive evidence is available in the literature (Cutler and Lleras-Muney, 2010, 2012; Wübker, 2012) pointing to a positive association between education and the probability of check ups (such as mammograms, pap smears and colonoscopy) or flu shots. At this stage further analysis is required to better understand the underlying mechanism for such behavior. Considering the other preventive behaviors, we do not find any significant effect of schooling for women. For

[21] Alternatively, they may be less in need of following these dietary regimes as the exercise regularly.

men, instead, compulsory lower secondary schooling increases the probability of having regular checkups of glycemia and cholesterol levels. The effects are precisely estimated and significant at the 1% statistical level. No effect is found for checks of blood pressure. Also when considering the total number of preventive medical examinations in absence of symptoms, the number of paid examinations or flu immunization no significant effect is found, neither for men nor for women.

6 Discussion of the possible causal pathways

In the previous section we have shown significant negative effects of increasing compulsory schooling on women's BMI and obesity. By way of contrast, a similar effect is not found for men. We have also found evidence that education only affects some health-improving and preventing behaviors of men but not of women. We explore in this section some potential reasons for the gender differences observed in our analysis.

Let us start with the effect of education on BMI and obesity. We explore the following potential causal pathways which may explain why education may have a protective effect only for women:[22]

- income and the labor market returns to physical appearance;
- the 'double burden', that is educated women working simultaneously at home and in the labor market;
- anxiety and depression;
- marital status and fertility.

A first explanation for the protective effect of education on BMI being observed only for women may be related to the *mediating effect of income*. Sanz-de Galdeano (2005) finds, for instance, that obesity declines more with increasing household income for women

[22]See also the related discussion in Brunello et al. (2012).

than for men. A possible reason for the BMI-income gradient is that low income individuals tend to eat cheap food, which is often high in fat and sugar, and therefore calories. It is not clear though why this explanation should be relevant only for women and not for men. Quintana Domeque and Villar (2009) stress how the income-BMI relationship in nine European countries is negative for women and nonexistent for men. Moreover, they find that the different relationship for men and women appears to be driven by the negative association between BMI and 'own labor earnings' for women. This is consistent with women suffering a wage penalty associated with high BMI, and the association not being mainly driven by eating 'junk food'.[23] Similar findings are reported in Gregory and Ruhm (2011) showing that earnings peak for women at levels far below the clinical threshold of 'obesity' or even 'overweightness', which suggests that it is not obesity but rather some other factor —such as physical attractiveness— that produces the observed positive gradient between BMI and wages. As a consequence women may care more about their body weight than men. The high-BMI wage penalty may also vary by level of education. The underlying idea is that while physical appearance may matter less if a woman is low educated and employed in a blue-collar job, it becomes important especially for highly educated women in white-collar jobs.

To investigate this hypothesis further we estimated a LPM model for an individual's labor force participation (LFP) status. The fuzzy RD estimates are reported in Table 7: for both men and women education contributes to increasing labor force participation. The estimated effect for men is however implausibly high, probably due to the weakness of the instrument.The Kleibergen-Paap rk Wald $F-$statistic is 10.692, well below the Stock-Yogo weak identification test's critical values, which is 16.38 for a 10% maximal IV size. We estimated with OLS a simple descriptive model of the link between BMI and an individual's LFP. LFP status seems to be more negatively associated with females' than with males' BMI. This is consistent with a greater mediating effect of labor income —whatever the direction of causality is— for women than for men, but the size of the effect

[23]In which case a similar negative relationship between non-labor earnings and BMI should equally emerge.

is not large enough to explain the whole impact of education on BMI. Indeed, women participating in the labor force have on average just a 0.3 lower BMI than women out of the labor force. Probably, part of the effect is likely to be mediated by differences in labor earnings conditional on labor force participation more than by LFP status. Unfortunately, the ISH does no provide income or earnings data and we cannot explore this hypothesis. In any case, as we have seen, the potential higher investments in physical appearance for women do no show up in our analysis under the form of voluntary physical exercise (regular or intense) or the probability of following specific dietary regimes. The explanation that highly educated women may keep themselves in good shape by consuming higher quality food, compared to men, seems instead to be more in line with our empirical evidence, being consistent with both lower BMI and a lower need for physical activity. Unfortunately, the ISH does not contain data to further test this speculation.

A second possible explanation of gender differences in the education-BMI link is related to the so-called '*double burden*' of educated women. Indeed, in Italy working women must also take care of housework, and this may imply a higher consumption of calories during the day. The ISH provides information of the degree of physical intensity of housework and external work, which is expressed on a 3-point Likert scale: 1 for low, 2 for medium, and 3 for high. For each individual we summed the scores in the two variables to obtain a joint indicator of the intensity of physical work at home and in the job. Then we applied the usual fuzzy RD scheme using this dependent variable.[24] The results are reported in Table 7 and show an effect which is significant at the 10% level for women, but much larger in magnitude and more precisely estimated for men. Thus, these results do not help to explain the gender gap. Moreover, from a simple OLS regression of BMI on this indicator, reported in Table 8, the association appears to be very low and statistically insignificant. We also built a dichotomous indicator for doing at least one of the following: heavy work, heavy housework, heavy sport. This is very similar to the indicator used in Brunello et al. (2012), and for which the authors found a higher effect

[24]Although the variable is not strictly cardinal, we use a simple linear model.

of education for women than for men (+10.8% and +4.5% increase in the probability of heavy activities, for a one-year increase in education, for women and men respectively). However, in this case we do not observe any significant effect of low secondary education for both genders (see Table 7). Thus the 'double burden' hypothesis does not seem to be the main explanation for the gender differences observed in the BMI-education gradient.

Another explanation may be related to *anxiety* and *depression*, which are more frequent among women. Education may positively contribute to an individual's mental health. This explanation is potentially relevant in our sample as simple OLS show a strong positive association between having mental problems and female BMI (see Table 8). In particular, the ISH asked the following question: 'in the last four weeks, did you perform worse than you desired in the job or in house activities because of your emotional state (e.g., depression or anxiety)?'. We adopted the fuzzy RD to estimate the effect of education on emotional health. In spite of emotional health being an important correlate of BMI, Table 7 does not show any significant effect of education on experiencing emotional problems.[25]

Other two candidate explanations for the effect of education on BMI and the gender gap may be (i) *marital status*; (ii) *fertility*. As for the first, married individuals could have a higher BMI, for instance because they are out of the marriage market. At the same time education may have a negative effect on the likelihood of getting married (i.e. highly educated individuals may be 'choosy'). The positive association between marital status and BMI is confirmed in Table 8, showing that married men and women have higher BMI than singles, and that the 'marriage gap' in BMI is higher for women than for men (0.642 vs. 0.376). Table 7 reports the causal effect of education on the likelihood of being (or having been) married, estimated using the fuzzy RD design. The estimated coefficients are negative but never statistically significant, and very similar across genders. As for the second explanation, fertility, low educated women may have a higher fertility and

[25]We also tried with another mental health indicator provided in the survey, corresponding to the question 'In the last two weeks, did you experience a fall in concentration in the job or in daily activities because of your emotional state?', and the results were very similar.

gain weights during the pregnancies, with some of the weight gain being permanent. The ISH does not collect information on total fertility but only on cohabiting children. For this reason, and given the relatively high average age of the individuals included in our estimation sample (many children may have left the parental home), it is not possible to estimate the causal effect of education on fertility with our data.[26] However, just to provide some descriptive evidence, we checked with our data if low educated mothers gain more weight during pregnancy, and if weight gains are permanent. The ISH provides in each wave data on the weight gained during pregnancy but only for mothers of children aged less than five. We defined a dichotomous indicator for having gained more than 15 Kg. and explored the statistical association between education and this indicator, and the one between the latter and current BMI (reported in Table 8), which both turned out to be statistically insignificant. Thus weight gain during past pregnancies does not seem to be permanent, and be positively associated with women's current BMI.

Then, of the various explanations put forward at the beginning of this section, only the one related to the gender differences in the labor market returns to physical appearance seems to have some potential for explaining the men-women gap in the protective effect played by education on BMI and obesity.

As to *preventive behaviors*, we found significant positive effects of rising compulsory schooling for glycemia and cholesterol checks for men. Here the men-women difference is easier to explain as many health-risk factors are gender specific. There is a well established evidence in medical sciences that the incidence of high blood pressure and hypertension is higher among men than women and increases with age. In particular, the risk is higher for men aged 45 or more, although according to the WHO in the last decades the incidence is increasing also among younger men and women because of the significant changes in population lifestyle. No similar evidence is available for diabetes, since Type 1 diabetes has almost the same incidence among men and women, while Type 2 diabetes is more common in older and overweight people with no gender difference. This evidence

[26] A recent study by Fort (2012) uses for identification the same reform used in this paper and does not find significant effects of increasing compulsory schooling on completed fertility of Italian women.

is confirmed also by the last available national statistics for Italy. According to the Italian National Statistical Institute (ISTAT), among men cardiovascular diseases were the first cause of death in 2009 and also in the previous years, although the incidence is higher for women. No difference emerges among genders for diabetes nor as a cause of death nor as illness. Yet what is more difficult to rationalize is why a significant effect of education shows up only for some checks, especially for men, and not for others, for instance for mammograms, paper smear tests and CBM for women. It is difficult to give a definitive answer to this question with our data. We limit ourselves to saying here that it may due to the consciousness-raising campaigns which were widespread for specific NCDs diseases such as breast cancer, and which reached most women irrespective of their levels of education and socio-economic status (Whynes et al., 2007). Women may be more responsive than men to these campaigns or to health information in general, and education may accordingly play a less important role for women than for men. Alternatively, there might be gender differences in the effect of education on health risk perception. Our data are not suitable to give an answer to these questions, and we leave a deeper investigation of these important issues for future work.

7 Concluding remarks

In this paper we have used three waves of a very rich health survey for Italy gathering a wealth of information on individuals' health, health-related behaviors and use of health services. This survey, combined with a 'quasi-natural' experiment represented by an increase in compulsory schooling age introduced in 1963, enables us to analyze the causal effects of lower secondary schooling on a very wide range of health-related variables.

Our analysis suggests that lower secondary schooling reduced BMI and the incidence of obesity among Italian women, while no effect is found for men, and is in line with the recent literature. Furthermore, we find no evidence that education reduced health-damaging behaviors, such as smoking, neither for women nor for men.

As for health-improving behavior (i.e. healthy lifestyles) we only find a positive causal effect of education on men, for whom it increased the likelihood of doing regular physical exercise. Education also improved some health-preventive behaviors of men, by raising the probability of cholesterol and glycemia checkups in the last year.

We provide some tentative explanations related to the gender differences in the relationship between education and BMI, and preventive behavior. As for the former, our analysis suggests that these differences may be related to the higher labor market returns to physical attractiveness existing for women, while do not seem to be mediated by the fact that highly educated women have a 'double burden' in terms of job and housework (consuming more calories), by mental health, marital status or fertility behavior. As for the differences in preventive behaviors, they could be explained by the gender specificity of many health risk factors, and the wide diffusion of awareness-rising campaigns for female-specific NCDs such as breast cancer, which might have provided valuable information to most individuals irrespective of their educational levels. However, further research is needed to fully understand the origin of these gender differences in the education-health gradient.

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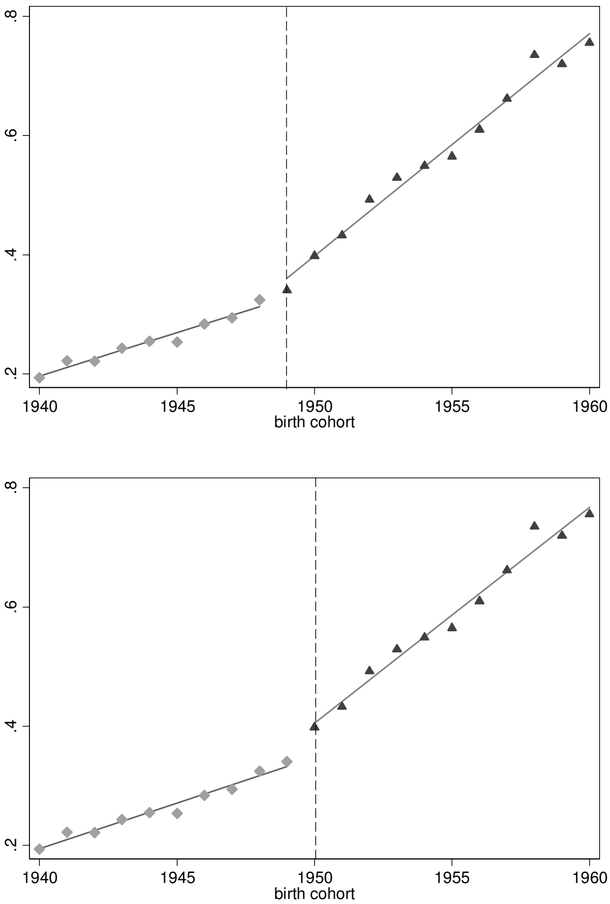
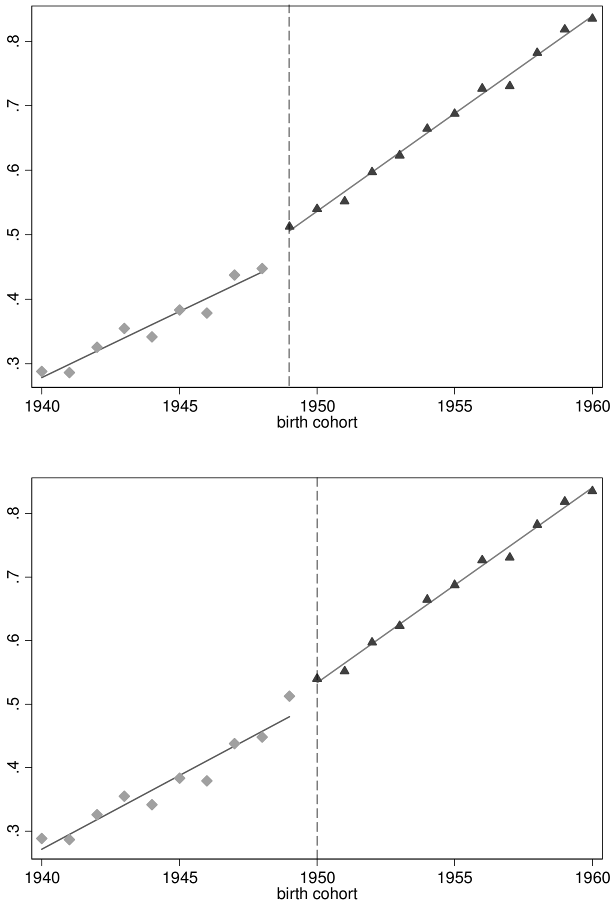
Figure 1: Effect of the 1963 reform on women's likelihood to complete lower secondary schooling



Note. In the top graph the pivotal cohort (i.e. the first cohort affected by the reform) is set to 1949 and in the bottom graph to 1950. The data plotted refer to the estimation sample in Table 1 and includes women in the IHS born between 1940 and 1960 with at most lower secondary schooling (and non-missing covariates). Estimated linear trends are super-imposed to the scatter plot.

Figure 2: Effect of the 1963 reform on men's likelihood to complete lower secondary schooling



Note. In the top graph the pivotal cohort (i.e. the first cohort affected by the reform) is set to 1949 and in the bottom graph to 1950. The data plotted refer to the estimation sample in Table 1 and includes men in the IHS born between 1940 and 1960 with at most lower secondary schooling (and non-missing covariates). Estimated linear trends are super-imposed to the scatter plot.

Table 1: First-stage results: Probability of attaining *lower* secondary schooling

	women $c = 1949$	women $c = 1950$	men $c = 1949$	men $c = 1950$
$T \equiv I(B \geq c)$	0.033***	0.058***	0.041***	0.030*
	(0.011)	(0.009)	(0.010)	(0.015)
$g(B - c)$	-0.011	-0.009	0.017*	0.020**
	(0.007)	(0.007)	(0.009)	(0.009)
$g(B - c) \times T$	0.026***	0.024***	0.010***	0.008**
	(0.002)	(0.002)	(0.002)	(0.003)
F-test linear cohort trend vs. birth cohort dummies$^{(a)}$	1.32 [0.17]	1.30 [0.18]	0.87 [0.61]	1.36 [0.14]
F-test treated $T = 0$	9.60 [0.01]	37.19 [0.00]	16.01 [0.00]	3.91 [0.06]
R^2	0.178	0.178	0.162	0.162
N. obs.	28896	28896	24965	24965

*** significant at 1%; ** significant at 5%; * significant at 10%.

$^{(a)}$ Test that birth cohort dummies are jointly equal to zero in a specification including both a linear polynomial in birth cohort and cohort dummies (Lee and Lemieux, 2010).
Note. c stands for the 'pivotal cohort' (cutoff) and T is the dummy defining eligibility to the 1963 reform. $g(B-c)$ and $g(B-c) \times T$ are the pre- and post-reform trends. P-values in brackets. Standard errors clustered by birth cohort in parentheses. The table presents results setting the cutoffs at different cohorts. All first-stage estimates also include region and year fixed effects and a quadratic term in age and are estimated on individuals for the birth cohorts 1940-1960 who attained no more than lower secondary schooling.

Table 2: First-stage placebo: Probability of attaining *upper* secondary schooling

	women $c = 1949$	men $c = 1950$
$T \equiv I(B \geq c)$	0.006	-0.003
	(0.006)	(0.007)
$g(B - c)$	0.117***	0.093***
	(0.005)	(0.005)
$g(B - c) \times T$	-0.016***	-0.018***
	(0.002)	(0.001)
R^2	0.192	0.214
N. obs.	33711	30213

*** significant at 1%; ** significant at 5%; * significant at 10%.

Note. P-values in brackets. Standard errors clustered by birth cohort in parentheses. All first-stage estimates also include region and year fixed effects and a quadratic term in age. The regressions are estimated on individuals for the birth cohorts 1940-1960 who attained no more than upper secondary schooling.

Table 3: Effect of education on health outcomes

Type of health outcome	Outcome	Gender	First-stage coefficient (δ)	First-stage F-test[(a)]	First-stage Anderson-Rubin test[(b)]	Second-stage coefficient (τ)	N. observations
Weight related outcomes							
	BMI	Women	0.058***	38.42	11.92	-3.254***	28896
			(0.009)	[0.00]	[0.00]	(1.053)	
		Men	0.041***	16.01	0.05	-0.263	24965
			(0.010)	[0.00]	[0.83]	(1.172)	
	Overweightness	Women	0.058***	38.42	2.20	-0.285	28896
			(0.009)	[0.00]	[0.15]	(0.195)	
		Men	0.041***	16.01	0.02	-0.041	24965
			(0.010)	[0.00]	[0.88]	(0.253)	
	Obesity	Women	0.058***	38.42	11.89	-0.243***	28896
			(0.009)	[0.00]	[0.00]	(0.063)	
		Men	0.041***	16.01	0.82	-0.144	24965
			(0.010)	[0.00]	[0.38]	(0.147)	
General health outcomes							
	Chronic illness (self-reported)	Women	0.058***	37.19	0.49	-0.135	28922
			(0.009)	[0.00]	[0.48]	(0.186)	
		Men	0.041***	16.03	1.55	-0.457	24985
			(0.010)	[0.00]	[0.23]	(0.344)	
	Chronic illness (diagnosed)	Women	0.058***	37.19	2.35	-0.264*	28922
			(0.009)	[0.00]	[0.14]	(0.160)	
		Men	0.041***	16.03	1.25	-0.444	24985
			(0.010)	[0.00]	[0.28]	(0.362)	
	Days with limitations (last 4 weeks)	Women	0.058***	37.19	0.18	0 .690	28922
			(0.009)	[0.00]	[0.68]	(1.651)	
		Men	0.041***	16.03	0.12	0.816	24985
			(0.010)	[0.00]	[0.73]	(2.272)	
	Days in bed (last 4 weeks)	Women	0.058***	37.19	0.75	0.966	28922
			(0.009)	[0.00]	[0.40]	(1.117)	
		Men	0.041***	16.03	0.07	0.440	24985
			(0.010)	[0.00]	[0.80]	(1.683)	

*** significant at 1%; ** significant at 5%; * significant at 10%.

[(a)] F-test for the exclusion of the reform dummy T in the first stage.

[(b)] Anderson-Rubin Wald test for robust inference in the presence of weak instruments (the null hypothesis is $\tau = 0$).

Note. The column 'first-stage' reports the coefficient of the instrument (reform eligibility) and the column 'second-stage the coefficient of lower secondary schooling. P-values in brackets. Standard errors clustered by birth cohort in parentheses. Both the first-stage and the second-stage also include region and year fixed effects and a quadratic term in age and separate polynomial trends in birth cohorts for the pre- and post-reform cohorts. The regressions are estimated on individuals for the birth cohorts 1940-1960 who attained no more than lower secondary schooling.

Table 4: Effect of education on health-damaging and health-improving behaviors

Type of health behavior	Behavior	Gender	First-stage coefficient (δ)	F-test[(a)]	Anderson-Rubin test[(b)]	Second-stage coefficient (τ)	N. observations
			coefficient	F-test	Anderson-Rubin test*	coefficient	observations
Health-damaging behavior							
	Smoking	Women	0.056***	24.53	1.72	0.194	24992
			(0.011)	[0.000]	[0.204]	(0.141)	
		Men	0.047***	27.38	0.32	0.119	22072
			(0.009)	[0.00]	[0.58]	[0.213]	
	Cigarettes per day	Women	0.056***	24.53	0.27	-1.080	24992
			(0.011)	[0.000]	[0.610]	(2.052)	
		Men	0.047***	27.38	0.18	-2.516	22072
			(0.009)	[0.00]	[0.67]	(5.642)	
	Ever smoker	Women	0.056***	24.53	0.37	0.101	24992
			(0.011)	[0.000]	[0.552]	(0.158)	
		Men	0.047***	27.38	0.00	0.012	22072
			(0.009)	[0.00]	[0.95]	(0.206)	
Health-improving behavior	Intense physical activity	Women	0.056***	24.53	2.09	0.077	24,992
			(0.011)	[0.000]	[0.16]	(0.061)	
		Men	0.049***	25.86	2.83	-0.122**	22,072
			[0.009]	[0.000]	[0.11]	(0.061)	
	Regular physical activity	Women	0.056***	24.53	1.06	-0.176	24,992
			(0.011)	[0.000]	[0.32]	(0.165)	
		Men	0.049***	25.86	6.60	0.469**	22,072
			(0.009)	[0.000]	[0.02]	(0.200)	
	Any continuous physical activity	Women	0.056***	24.53	0.27	-0.089	24,992
			(0.011)	[0.000]	[0.61]	(0.172)	
		Men	0.049***	25.86	0.02	-0.025	22,072
			(0.009)	[0.000]	[0.90]	(0.196)	
	Dietary regime	Women	0.057***	36.7	0.36	0.082	28,890
			[0.009]	[0.000]	[0.55]	(0.135)	
		Men	0.048**	18.59	4.78	-0.198**	24963
			(0.011)	[0.000]	[0.04]	(0.086)	

*** significant at 1%; ** significant at 5%; * significant at 10%.

[(a)] F-test for the exclusion of the reform dummy T in the first stage.

[(b)] Anderson-Rubin Wald test for robust inference in the presence of weak instruments (the null hypothesis is $\tau = 0$).

Note. The column 'first-stage' reports the coefficient of the instrument (reform eligibility) and the column 'second-stage the coefficient of lower secondary schooling. P-values in brackets. Standard errors clustered by birth cohort in parentheses. Both the first-stage and the second-stage also include region and year fixed effects and a quadratic term in age and separate polynomial trends in birth cohorts for the pre- and post-reform cohorts. The regressions are estimated on individuals for the birth cohorts 1940-1960 who attained no more than lower secondary schooling.

Table 5: Effect of education on type of diet

Type of health behavior	Behavior	Gender	First-stage coefficient (δ)	F-test[(a)]	Anderson-Rubin test[(b)]	Second-stage coefficient (τ)	N. observations
			coefficient	F-test	Anderson-Rubin test*	coefficient	observations
Type of diet	Hypocaloric diet	Women	0.057***	36.7	1.14	0.153	28890
			(0.009)	[0.000]	[0.29]	-0.142	
		Men	0.048**	18.59	5.04	-0.259***	24963
			(0.011)	[0.000]	[0.036]	-0.094	
	Salt free diet	Women	0.057***	36.7	1.36	-0.067	28890
			(0.009)	[0.000]	[0.26]	-0.056	
		Men	0.048**	18.59	4.76	-0.341***	24963
			(0.011)	[0.000]	[0.0413]	-0.111	
	Diet for health reasons	Women	0.057***	36.7	0.02	-0.017	28890
			(0.009)	[0.000]	[0.89]	-0.125	
		Men	0.048**	18.59	4.99	-0.260**	24963
			(0.011)	[0.000]	[0.037]	-0.105	
	Diet not for health reasons	Women	0.057***	36.7	1.68	0.099	28890
			(0.009)	[0.000]	[0.210]	-0.071	
		Men	0.048**	18.59	2.24	0.062*	24963
			(0.011)	[0.000]	[0.150]	-0.036	

*** significant at 1%; ** significant at 5%; * significant at 10%.

[(a)] F-test for the exclusion of the reform dummy T in the first stage.

[(b)] Anderson-Rubin Wald test for robust inference in the presence of weak instruments (the null hypothesis is $\tau = 0$).

Note. The column 'first-stage' reports the coefficient of the instrument (reform eligibility) and the column 'second-stage the coefficient of lower secondary schooling. P-values in brackets. Standard errors clustered by birth cohort in parentheses. Both the first-stage and the second-stage also include region and year fixed effects and a quadratic term in age and separate polynomial trends in birth cohorts for the pre- and post-reform cohorts. The regressions are estimated on individuals for the birth cohorts 1940-1960 who attained no more than lower secondary schooling.

Table 6: Effect of education on preventive-care behaviors

Type of health behavior	Behavior	Gender	First-stage			Second-stage	N.
			coefficient (δ)	F-test[(a)]	Anderson-Rubin test[(b)]	coefficient (τ)	observations
Preventive-care behavior	Mammogram	Women	0.057***	37.45	2.88	-0.342*	28,883
			(0.009)	[0.000]	[0.11]	(0.189)	
	Pap smear test	Women	0.057***	36.37	0.28	-0.109	28,877
			(0.009)	[0.000]	[0.60]	(0.195)	
	CBM	Women	0.040**	7.12	4.74	-0.850*	11,812
			(0.015)	[0.015]	[0.04]	(0.440)	
	Glycemia check last year	Women	0.056***	24.53	1.61	-0.438	24,992
			(0.011)	[0.000]	[0.22]	(0.368)	
		Men	0.049***	25.86	11.15	0.410***	22,072
			(0.009)	[0.000]	[0.00]	(0.124)	
	Cholesterol check last year	Women	0.056***	24.53	2.82	-0.572	24,992
			(0.011)	[0.000]	[0.11]	(0.380)	
		Men	0.049***	25.86	23.17	0.446***	22,072
			(0.009)	[0.000]	[0.00]	(0.111)	
	Blood pressure check last year	Women	0.056***	24.53	1.99	-0.398	24,992
			(0.011)	[0.000]	[0.17]	(0.315)	
		Men	0.049***	25.86	2.20	0.227	22,072
			(0.009)	[0.000]	[0.15]	(0.140)	
	N. medical examinations without symptoms	Women	0.056***	24.53	3.09	-0.195	24,992
			(0.011)	[0.000]	[0.09]	(0.121)	
		Men	0.049***	25.86	0.02	0.018	22,072
			(0.009)	[0.000]	[0.89]	(0.128)	
	N. paid examinations	Women	0.056***	24.53	0.31	-0.113	24,992
			(0.011)	[0.000]	[0.59]	(0.189)	
		Men	0.049***	25.86	1.95	-0.273	22,072
			(0.009)	[0.000]	[0.18]	(0.166)	
	Flu shot	Women	0.056***	24.53	0.45	-0.070	24,992
			[0.011]	[0.000]	[0.51]	(0.100)	
		Men	0.049***	25.86	0.34	0.096	22,072
			(0.009)	[0.000]	[0.57]	(0.164)	

*** significant at 1%; ** significant at 5%; * significant at 10%.
[(a)] F-test for the exclusion of the reform dummy T in the first stage.
[(b)] Anderson-Rubin Wald test for robust inference in the presence of weak instruments (the null hypothesis is $\tau = 0$).
Note. The column 'first-stage' reports the coefficient of the instrument (reform eligibility) and the column 'second-stage the coefficient of lower secondary schooling. P-values in brackets. Standard errors clustered by birth cohort in parentheses. Both the first-stage and the second-stage also include region and year fixed effects and a quadratic term in age and separate polynomial trends in birth cohorts for the pre- and post-reform cohorts. The regressions are estimated on individuals for the birth cohorts 1940-1960 who attained no more than lower secondary schooling.

Table 7: Potential pathways for the effect of education on health

Dependent variable	Gender	First-stage coefficient (δ)	First-stage F-test[a]	First-stage Anderson-Rubin test[b]	Second-stage coefficient (τ)	N. observations
Labour force participation	Women	0.058***	37.19	5.35	0.520**	28922
		(0.009)	[0.00]	[0.03]	(0.210)	
	Men	0.041***	16.03	15.83	1.951***	24985
		(0.010)	[0.00]	[0.00]	(0.636)	
Physical intensity of housework and job	Women	0.056***	24.53	2.23	0.911*	24992
		(0.011)	[0.00]	[0.15]	(0.552)	
	Men	0.047***	27.38	11.94	4.660***	22072
		(0.009)	[0.00]	[0.00]	(1.709)	
Any high intensity physical activity (housework, job, physical exercise)	Women	0.056***	24.53	0.53	0.096	24992
		(0.011)	[0.00]	[0.47]	(0.127)	
	Men	0.047***	27.38	2.30	0.496	22072
		(0.009)	[0.00]	[0.14]	(0.362)	
Emotional problems	Women	0.056***	24.53	0.70	0.149	24992
		(0.011)	[0.00]	[0.41]	(0.177)	
	Men	0.047***	27.38	1.13	0.181	22072
		(0.009)	[0.00]	[0.30]	(0.171)	
Marital status	Women	0.058***	37.19	1.28	-0.074	28922
		(0.009)	[0.00]	[0.27]	(0.068)	
	Men	0.041***	16.03	1.59	-0.112	24985
		(0.010)	[0.00]	[0.22]	(0.075)	

*** significant at 1%; ** significant at 5%; * significant at 10%.
[a] F-test for the exclusion of the reform dummy T in the first stage.
[b] Anderson-Rubin Wald test for robust inference in the presence of weak instruments (the null hypothesis is $\tau = 0$).
Note. Each row represents the result of a separate regression using the dependent variables listed in the first column, and estimated using a fuzzy RD. The column 'first-stage' reports the coefficient of the instrument (reform eligibility) and the column 'second-stage the coefficient of lower secondary schooling. P-values in brackets. Standard errors clustered by birth cohort in parentheses. Both the first-stage and the second-stage also include region and year fixed effects and a quadratic term in age and separate polynomial trends in birth cohorts for the pre- and post-reform cohorts and are estimated on individuals for the birth cohorts 1940-1960 who attained no more than lower secondary schooling.

Table 8: Statistical associations between selected variables

Main independent variable	Gender	coefficient	R^2	N. observations
Dependent variable is BMI				
Labour force participation	Women	-0.295***	0.050	28896
		(0.057)		
	Men	-0.084	0.018	24965
		(0.065)		
Physical intensity of housework and job	Women	-0.044	0.048	24992
		(0.028)		
	Men	-0.035	0.016	22072
		(0.017)		
Married	Women	0.642***	0.051	28896
		(0.132)		
	Men	0.376***	0.019	24965
		(0.067)		
Emotional problems	Women	0.592***	0.051	24992
		(0.061)		
	Men	-0.102	0.016	22072
		(0.075)		
Pregnancy weight gain more than 15 Kg.	Women	0.105	0.040	4753
		(0.177)		
Dependent variable is pregnancy weight gain more than 15 Kg.				
Lower secondary schooling	Women	0.009	0.02	4753
		(0.016)		

*** significant at 1%; ** significant at 5%; * significant at 10%.

Note. Each row represents the result of a separate regression using the dependent variables listed in the first column, and estimated using OLS. 'Coefficients' refer to the coefficients of lower secondary schooling in each regression. Standard errors clustered by birth cohort in parentheses. All specifications also include region and year fixed effects and a quadratic term in age and separate polynomial trends in birth cohorts for the pre- and post-reform cohorts and are estimated on individuals for the birth cohorts 1940-1960 who attained no more than lower secondary schooling.

Table A1. Descriptive statistics for women

Variable	Obs	Mean	Std. Dev.	Min	Max
Body Mass Index (BMI)	28896	25.435	4.235	12.05	52.07
Overweightness	28896	0.475	0.499	0	1
Obesity	28896	0.141	0.348	0	1
Chronic illness (self-reported)	28922	0.488	0.500	0	1
Chronic illness (diagnosed)	28922	0.443	0.497	0	1
Days with limitations (last 4 weeks)	28922	1.648	5.267	0	28
Days in bed (last 4 weeks)	28922	0.501	2.371	0	28
Smoking	24992	0.189	0.391	0	1
Ever smoker	24992	0.327	0.469	0	1
Intense physical activity	24992	0.028	0.165	0	1
Regular physical activity	24992	0.156	0.363	0	1
Any continuous physical activity	24992	0.514	0.500	0	1
Dietary regime	28890	0.146	0.353	0	1
Hypocaloric diet	28890	0.099	0.299	0	1
Salt free diet	28890	0.058	0.234	0	1
Diet for health reasons	28890	0.114	0.318	0	1
Diet for non health reasons	28890	0.031	0.174	0	1
Mammogram	28883	0.558	0.497	0	1
Pap smear test	28877	0.697	0.460	0	1
CBM	11812	0.336	0.472	0	1
Glycemia check last year	24992	0.546	0.498	0	1
Cholesterol check last year	24992	0.541	0.498	0	1
Blood pressure check last year	24992	0.640	0.480	0	1
N. medical examinations without symptom	24992	0.088	0.283	0	1
N. paid examinations	24992	0.173	0.616	0	14
Flu shot	24992	0.134	0.341	0	1

Table A2. Descriptive statistics for men

Variable	Obs	Mean	Std. Dev.	Min	Max
Body Mass Index (BMI)	24965	26.631	3.455	12.79	55.56
Overweightness	24965	0.661	0.474	0	1
Obesity	24965	0.155	0.362	0	1
Chronic illness (self-reported)	24985	0.409	0.492	0	1
Chronic illness (diagnosed)	24985	0.369	0.483	0	1
Days with limitations (last 4 weeks)	24985	1.306	4.731	0	28
Days in bed (last 4 weeks)	24985	0.431	2.219	0	28
Smoking	22072	0.339	0.473	0	1
Ever smoker	22072	0.683	0.465	0	1
Intense physical activity	22072	0.055	0.229	0	1
Regular physical activity	22072	0.212	0.409	0	1
Dietary regime	24963	0.107	0.310	0	1
Hypocaloric diet	24963	0.071	0.257	0	1
Salt free diet	24963	0.046	0.209	0	1
Diet for health reasons	24963	0.091	0.287	0	1
Diet for non health reasons	24963	0.017	0.128	0	1
Glycemia check last year	22072	0.520	0.500	0	1
Cholesterol check last year	22072	0.516	0.500	0	1
Blood pressure check last year	22072	0.604	0.489	0	1
Any continuous physical activity	22072	0.557	0.497	0	1
N. medical examinations without symptom	22072	0.062	0.241	0	1
N. paid examinations	22072	0.122	0.538	0	13
Flu shot	22072	0.121	0.326	0	1

Table A3. OLS estimates for women

Dependent variable	Coeff.	St.err.	Observations	R-squared
BMI	-1.071***	(0.068)	28,896	0.062
Overweightness	-0.113***	(0.007)	28,896	0.052
Obesity	-0.060***	(0.005)	28,896	0.019
Chronic illness (self-reported)	-0.035***	(0.006)	28,922	0.124
Chronic illness (diagnosed)	-0.039***	(0.007)	28,922	0.172
Days with limitations (last 4 weeks)	-0.156**	(0.069)	28,922	0.008
Days in bed (last 4 weeks)	-0.039	(0.027)	28,922	0.005
Smoking	0.044***	(0.006)	24,992	0.038
Ever smoker	0.088***	(0.007)	24,992	0.063
Intense physical activity	0.018***	(0.002)	24,992	0.015
Regular physical activity	0.035***	(0.004)	24,992	0.066
Any continuous physical activity	0.071***	(0.006)	24,992	0.122
Dietary regime	-0.001	(0.005)	28,890	0.020
Hypocaloric diet	-0.002	(0.005)	28,890	0.047
Salt free diet	-0.005	(0.003)	28,890	0.051
Diet for health reasons	-0.009	(0.005)	28,890	0.033
Diet for non health reasons	0.008**	(0.003)	28,890	0.005
Mammogram	0.049***	(0.006)	28,883	0.134
Pap smear test	0.059***	(0.005)	28,877	0.110
CBM	0.029***	(0.010)	11,812	0.097
Glycemia check last year	0.016**	(0.007)	24,992	0.052
Cholesterol check last year	0.015**	(0.007)	24,992	0.055
Blood pressure check last year	0.012*	(0.006)	24,992	0.077
N. medical examinations without symptom	0.006	(0.004)	24,992	0.003
N. paid examinations	0.025**	(0.010)	24,992	0.002
Flu shot	-0.002	(0.006)	24,992	0.073

*** significant at 1%; ** significant at 5%; * significant at 10%.

Note. Each row represents the result of a separate regression using the dependent variables listed in the first column, and estimated using OLS. 'Coefficients' refer to the coefficients of lower secondary schooling in each regression. Standard errors clustered by birth cohort in parentheses. All specifications also include region and year fixed effects and a quadratic term in age and separate polynomial trends in birth cohorts for the pre- and post-reform cohorts and are estimated on individuals for the birth cohorts 1940-1960 who attained no more than lower secondary schooling.

Table A4. OLS estimates for men

Dependent variable	Coeff.	St.err.	Observations	R-squared
BMI	-0.211	(0.040)	24,965	0.019
Overweightness	-0.011**	(0.005)	24,965	0.016
Obesity	-0.030***	(0.003)	24,965	0.010
Chronic illness (self-reported)	-0.011	(0.007)	24,985	0.077
Chronic illness (diagnosed)	-0.010	(0.007)	24,985	0.116
Days with limitations (last 4 weeks)	-0.207	(0.086)	24,985	0.004
Days in bed (last 4 weeks)	-0.121***	(0.039)	24,985	0.004
Smoking	-0.011	(0.008)	22,072	0.028
Ever smoker	0.012**	(0.005)	22,072	0.005
Intense physical activity	-0.002	(0.005)	22,072	0.022
Regular physical activity	0.036***	(0.004)	22,072	0.021
Any continuous physical activity	0.041***	(0.007)	22,072	0.061
Dietary regime	0.078***	(0.008)	24,963	0.095
Hypocaloric diet	-0.000	(0.004)	24,963	0.042
Salt free diet	-0.003	(0.003)	24,963	0.037
Diet for health reasons	-0.005	(0.005)	24,963	0.030
Diet for non health reasons	0.003**	(0.001)	24,963	0.005
Glycemia check last year	0.034***	(0.006)	22,072	0.041
Cholesterol check last year	0.034***	(0.006)	22,072	0.041
Blood pressure check last year	0.035***	(0.007)	22,072	0.060
N. medical examinations without symptom	0.006**	(0.002)	22,072	0.007
N. paid examinations	0.034***	(0.008)	22,072	0.004
Flu shot	-0.003	(0.004)	22,072	0.056

*** significant at 1%; ** significant at 5%; * significant at 10%.

Note. Each row represents the result of a separate regression using the dependent variables listed in the first column, and estimated using OLS. 'Coefficients' refer to the coefficients of lower secondary schooling in each regression. Standard errors clustered by birth cohort in parentheses. All specifications also include region and year fixed effects and a quadratic term in age and separate polynomial trends in birth cohorts for the pre- and post-reform cohorts and are estimated on individuals for the birth cohorts 1940-1960 who attained no more than lower secondary schooling.