Education and the Timing of Births: Evidence from a Natural Experiment in Italy

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Abstract[1]: This paper assesses the causal effects of education on the timing of first order births allowing for heterogeneity in the effects while controlling for self-selection of women into education. Identification relies on exogenous variation in schooling induced by a mandatory school reform rolled out nationwide in Italy in the early 1960s. Findings based on Census data (Italy, 1981) suggest that a large fraction of the women affected by the reform postpones the time of the first birth but catches up with this fertility delay before turning 26. There is some indication that the fertility behaviour of these women is different from the one of the average women in the population.

Keywords: Education, Motherhood, Regression Discontinuity Design.**JEL codes**: J1,I2.

1 Introduction and Motivation of the Paper

This paper assesses the causal effects of education on the timing of first births in Italy by exploiting a school reform rolled out in the early 1960s, which increased the compulsory schooling age by three years (from 11 to 14). Italy was in the early 1990s one of the first countries to attain and sustain the lowest-low fertility levels[2](Kohler et al. [25]). Besides, the Italian

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[2]Total fertility rate at or below 1.3.

schooling system has undergone lots of changes since 1859, particularly as far as compulsory schooling is concerned (see Genovesi [18]); notably, the last increase of compulsory schooling age was planned in 1999. Thus, assessing the causal effect of education on fertility might prove useful for planning effective policies aimed at contrasting the decreasing trends in fertility.

In the last decades, several European countries have experienced both decline in fertility and motherhood postponement (Gustafsson [19]) and several OECD governments are considering or have already introduced specific measures aimed at countering these trends in fertility (Sleebos [33]). Besides, also teenage childbearing attracts some politic interest, due to its association with a range of disadvantages, both for the mother and for children[3]. At the same time, also the education level of individuals has recently been (and is currently) on the agenda of policy makers in most countries[4]: in the period 1950-1970 many European countries carried out major educational reforms aimed at increasing compulsory schooling, at unifying curricula, at delaying or abolishing the selection of more able students into separate schools (Leschinsky and Mayer [27] and Eurybase, the Eurydice database on education systems in Europe).

Do family friendly policies and policies aimed at reducing teenage childbearing, on the one hand, and policies aimed at increasing average schooling achievement, on the other hand, pursue compatible goals? Besides, do these policies affect any woman in the same way?

A number of studies report negative association between schooling achievement and *tempo* fertility in most countries (see, among others, Nicoletti and Tanturri[29]). However, the direct comparison between women with different qualification levels does not generally identify the causal effects of education on fertility: indeed, both education and fertility decisions are affected by women unobserved tastes for children and work (Blackburn et al. [5], Ell-

[3]On average, across 13 countries of the European Union, women who give birth as a teenager are twice as likely of living in poverty (UNICEF [36]).

[4]The Millennium Development goals include “achieve universal primary education” (goal 2) and “eliminate gender disparity in primary and secondary education, preferably by 2005, and in all levels of education no later than 2015” (target 4) (UN Millennium Project 2005 [1]). The central role of education in achieving the European Union strategic goal (“become the most competitive and dynamic knowledge-based economy in the world capable of sustainable economic growth with more and better jobs and greater social cohesion”) has also been recently stressed during the 2005 summit in Bruxelles (European Union [11]).

wood et al. [15], Mullin and Wang[28]) and by the same unobserved background variables. To assess if policies aimed at increasing average schooling achievement and policies aimed at reconciling motherhood and work pursue intrinsically contrasting goals, further knowledge has to be achieved on the *causal effects* of education on fertility. Besides, giving insights on the variability of the fertility returns on education across women might be relevant for targeting policies to specific subgroups of individuals.

This paper assesses the causal effect of education on fertility in Italy, allowing for heterogeneity in the effects across individuals while controlling for self-selection of women into education. Since the analysis is not restricted to marital fertility and it considers a cohort measure of fertility instead than a period one, it can be profitably combined with previous work by Bratti[9] widening the knowledge on the determinants of the recent trends in fertility in Italy. Moreover, this paper exploits an identification strategy that can be easily used for the same purpose in other countries, thus setting the bases for future beneficial cross-country comparisons, which might support the generalizability of the results. The same identification strategy has already been used to investigate the effect of class size on schooling achievement (Angrist et al. [3]), the effect of financial aid offers on college enrolment (van der Klaauw [37]), the value parents attribute to school quality (Black [**?**]) but has not yet been used to deal directly with the links between education and fertility.

The remainder of the paper is organized as follows: section 2 briefly reviews empirical findings of previous studies on the relationship between parent's educational achievement and the timing of births. Section 3 presents in greater detail the identification and the estimation strategy, as well as the data used. Section 4 discusses the main findings and section 5 provides arguments supporting the internal validity of the estimates. Section 6 concludes.

2 Education & *Tempo* Fertility: Theoretical Models and Empirical Evidence

In most economic models of fertility behaviour[5], education is seen as a "modernization variable" which affects both demand and supply for children (Janowitz [24]).
Most dynamic models of fertility behaviour predict the postponement of motherhood as a consequence of women's enhanced schooling achievement. Husband's education is not expected to exert great effects, even if it plays a role shifting family budget constraint and contributing to the allocation of parents' time between market and non-market activities.
There are a number of issues involved in the analysis of the relationship between education and fertility decisions. Firstly, fertility is a multidimensional phenomenon: earlier empirical work on the determinants of fertility focused on completed fertility, whereas recently the determinants of the timing and spacing of births have been investigated[6]. Secondly, measures of fertility have been traditionally referred to women because of the lesser role of men in childrearing. However, recent changes in the appearance of the family in most European countries, might cast doubts on the adequacy of this approach[7]. In addition to this, measures of fertility differ according to the reference calendar time (period or cohort) on which they are built: fertility might be analysed from a period perspective (births in a given time period) or from a cohort perspective (births to a group of women born within a particular time period). Only if the processes determining individual's fertility behaviour are stationary, than period and cohort measures of fertility match exactly. Thirdly, as highlighted by Janowitz [24], the channels through which the effect of education might take place are numerous and, lastly, the effect of education on fertility might be heterogeneous across women with different ability, skill levels (Blackburn et al. [5], Ellwood et al. [15], Mullin and Wang [28]), family background.
Estimating the magnitude of the causal effect of education achievement on

[5]See Fort[17, Sect. 2] for a more detailed discussion on the predictions of economic models of fertility decisions which highlights the potential role of parents educational achievement in determining the timing of births.

[6]In addition, one could also consider *desired* fertility, that is the number of children a woman would have, had she been able to achieve the exact quantity she wanted.

[7]Recently, Willis[38] discusses the economics of fatherhood.

fertility is a non-trivial challenge. Lots of empirical studies have documented positive association between education and fertility postponement (among others see Blossfeld et al. [7] and Nicoletti and Tanturri [29]). However, the identification of the causal effect of education on fertility requires either to be able to control for factors driving women's preferences over children and work or to assign education level randomly to individuals, so that it would not be correlated with personal or social factors.
Bloemen and Kalwij [6] suggest that, in the Netherlands, an increase in the years of schooling of a woman causes her to schedule births later in life but it does not significantly affect her completed fertility.
Bratti[9], in his study on labour force participation and marital fertility in Italy, controls for unobserved heterogeneity including in his model a wide range of background variables and finds that the probability of giving birth for women with primary and lower secondary education decreases monotonically with age, whereas women with upper secondary and tertiary education levels tend to postpone fertility. Bratti uses a period measure of marital fertility.
Skirbekk, Kohler and Prskawetz[32] focus on the effect of "duration of education" on the timing of births and marriage in Sweden. Exploiting differences in birth months, they find that the difference of eleven months in the age at graduation implies a delay of almost 5 months in the age at first birth, event which generally occurs almost 8-10 years after graduation.

This paper focuses on the *causal* effect of education on fertility. Sticking to the traditional approach, fertility is defined referring only to women status and leaving men contributions to fertility decisions aside. The identification strategy employed allows both to control for endogeneity in the selection of individuals into education and to allow for heterogeneity in the effects across individuals. Finally, effects on one dimension of the phenomenon (*tempo*) are considered, due to limitations of the availability of adequate data on completed fertility.

3 Empirical Analysis

The identification of the **causal effects** of education on fertility requires either to be able to control for heterogeneity in the individuals's education

and fertility choices or to assign education level randomly to individuals, so that it would not be correlated with personal or social factors. Holding some regularity conditions, the "natural experiment approach"[8] guarantees the identification of causal effects for a **sub-population** , the so called *compliers* (Angrist, Imbens and Rubin [2]).
This section presents the causal parameters of interest and highlights the crucial assumptions for identification within the research design exploited. Then, it gives a description of the data used.

3.1 Identification of the Causal Parameters of Interest

Economic models of fertility behaviour suggest that *tempo* fertility (Y) can be described as a general function of choice variables of the mother (X) and concomitants (W), i.e. factors affecting fertility decisions which are not determined by the mother[9]. The choice variables X can be affected by the schooling level (E) and concomitants (W) and the schooling level might itself be included in X. The effect of education on fertility is a reduced form parameter summarizing the impact of schooling on behaviour (X) and the impact of behaviour on fertility (Y).
In this application, Y represents woman's age at her first child's birth (measure of *tempo* fertility) and D represents the treatment (namely, "more schooling"). D_i takes the value 1 if individual i has a high qualification and the value 0 otherwise. Y^1 and Y^0 denote the *potential* outcomes (Rubin [31], Holland [22]) defined as follows: $\boldsymbol{Y^1}$ is the mother's age at first birth if she would be exposed to the treatment, i.e. if she would get a high qualification; $\boldsymbol{Y^0}$ is the mother's age at first birth if she would not be exposed to the treatment, i.e. if she would get a low qualification. Although potential outcomes are logically well defined for all the individuals in the population, one only observes Y^1 on individuals with high education level and Y^0 for individuals with low education level. The individual specific causal effect is defined as $Y_i^1 - Y_i^0 \equiv \beta_i$ and is intrinsically not observable. Thus, the attention is shifted from individual causal effects to average effects: for in-

[8]See Rosenzweig and Wolpin [30] for a critical review of recent studies in different areas of enquiry which used this approach.

[9]X might include whether a mother is enrolled in school, whether she works, the extent to which she seeks parental care, whether she lives with a man and the characteristics of the man she lives with; W might include mother's genetic ability to conceive and give birth to a child, the woman's parents characteristics.

stance, average treatment effect (ATE), average treatment on the treated effect, ($ATTE$) and the effect of treatment on quantile q (QTE)[10]. When the treatment affects only the location of the distribution, QTE and ATE correspond exactly; the two differ when the potential outcomes distribution differ either by scale, or by location and scale or by shape. Dissimilar quantile treatment effects at different quantiles q suggest heterogeneity of the treatment effect.
Average causal effects and quantile treatment effects of education (D_i) on fertility (Y_i) cannot be directly identified from the comparison of $E[Y_i^0]$ and $E[Y_i^1]$ or $F_{Y^1}^{-1}(q)$ $F_{Y^0}^{-1}(q)$ in the observed data, unless D were randomly assigned to individuals, eventually conditioning on a set of covariates.
In this application, identification of the causal effect of education on fertility relies on a regression discontinuity design (Trochim [35], Thistlethwaite and Campbell [34]), exploiting a mandatory schooling reform rolled out nationwide in Italy in 1963. The **1963 reform** (N.1859 Act December 31, 1962) prescribed the unification of the previous junior high schools in a single compulsory junior high school (*scuola media*). Until 1963, individuals basically completed primary school (5 years); from 1963 onwards, it was compulsory to attend at least 8 years of schooling. According to the new law in force, individuals should attend school at least until junior high school (*scuola media*) graduation. Individuals who had been in school for at least 8 years at the time of their 14^{th} birthday were allowed to drop out. Basically, due to the new law, individuals born after 1949 were compelled to attend 3 more years of schooling. Assignment to the treatment ("more schooling") was fully determined by the individuals' date of birth (S). The individuals' date of birth is observed by the analyst. Let $\bar{s}$ be the *threshold* date of birth from which the increase in compulsory schooling started to be effective: a discontinuity in the conditional distribution of D given S around $\bar{s}$ is expected, due to the effect of the 1963 reform. The conditional distribution of any predetermined characteristic W given S is expected to be smooth around $\bar{s}$ and it is assumed that the 1963 reform did not exert any *direct* effect on

[10] $QTE_q = F_{Y^1}^{-1}(q) - F_{Y^0}^{-1}(q), \forall q \in [0,1]$, where $F_X^{-1}(q) = \min\{x \in \mathcal{X} : F_X(x) \geq q\}$, $\mathcal{X}$ is the set of values of the random variable X and F_X is its cumulative distribution function. This definition of QTE is consistent with the general model of treatment response proposed by Lehmann [26] and definitions by Doksum [14]. The quantile treatment effect represents the change in the response function required to stay on the q^{th} conditional quantile function (horizontal distance between the distribution functions F_{Y^1} and F_{Y^0}).

women's fertility decisions. If this is so, a discontinuity in the conditional distribution of D given S would map directly into a discontinuity in the conditional distribution of Y given S, provided schooling achievement (the treatment D) causally affects fertility decisions (Y)[11].

Due to the imperfect compliance with the assignment to the treatment (Brandolini and Cipollone[p. 9][8], Checchi [10]), this identification strategy identifies the average causal effect of education on fertility for those individuals persuaded to obtain additional education by virtue of the reform (*compliers*), i.e. the strategy allow to identify the local average treatment effect, ($LATE$, see Angrist, Imbens and Rubin[2]). Indeed, the reform does not affect the educational attainment of individuals who would achieve a high qualification whether compelled or not (*always takers*) and individuals who would not achieve high qualification whether compelled or not (*never takers*) and it is assumed that there are no individuals who would not attain high qualification if compelled but would attain high qualification if not compelled (*defiers*).

To sum up, the research design guarantees the identification of the causal effect[12] of education (the treatment D, namely "more schooling") on the fertility index Y around the threshold $\bar{s}$ for the subpopulation of *compliers* provided that: (i) the average effect of the 1963 reform on schooling achievement is not null *around the threshold*; (ii) individuals around the threshold $\bar{s}$ are similar as regards potential outcomes; (iii) there are no individuals who do exactly the opposite of their assignment; (iv) there are no spill-over effects (stable unit treatment value assumption, see Angrist, Imbens and Rubin[2]). In what follows, attention will be devoted only to quantile treatment effects. Quantile treatment effect can be easily obtained from the potential outcomes' marginal distributions. Indeed, Imbens and Rubin [23] showed that, under the $LATE$ identifying assumptions[13], the *compliers*' potential outcome distributions can be written as a weighted

[11]The discontinuity in the distribution of Y will be proportional to the average causal effect of education on fertility in the same way the reduced form effect in an instrumental variable setting is proportional to the structural parameter (Hahn, Todd and Van der Klaauw [21]).

[12]See Hahn, Todd and Van der Klaauw[21] for a formal discussion on identification and estimation of treatment effects in a regression-discontinuity design.

[13]Namely, stable unit treatment value assumption, the exclusion restriction, the strict monotonicity and the random assignment assumption. See Angrist, Imbens and Rubin [2] for an extensive discussion.

average of observed distribution by treatment status and assignment to the treatment. The same holds also in the regression discontinuity design framework[14]. Equation (3) represents the causal effect of education at s on $F(y)$ for *compliers*.

$$F^C_{.1}(y|\bar{s}) - F^C_{.0}(y|\bar{s}) = \frac{F_{1.}(y,|\bar{s}) - F_{0.}(y|\bar{s})}{\phi_c(\bar{s})} \tag{3}$$

where: (i) Z_i is a dummy variable which describes the assignment to the treatment (i.e., it takes the value 1 if individual i is assigned to the treatment and 0 otherwise; $Z \equiv I(S_i \geq \bar{s})$);(ii) $F^C_{.1}(y) \equiv Prob[Y^1_i \leq y|\mathrm{C}]$ and $F^C_{.0}(y) \equiv Prob[Y^0_i \leq y|\mathrm{C}]$ are the *compliers*'potential outcome distributions; (ii) $F_{1.}(y,\bar{s}) \equiv Prob[Y_i \leq y|S_i = \bar{s}, Z_i = 1]$; (iii) $F_{0.}(y,\bar{s}) \equiv Prob[Y_i \leq y|S_i = \bar{s}, Z_i = 0]$ and $\phi_c(\bar{s})$ is the proportion of *compliers* at $\bar{s}$. $F_{1.}(y,s) - F_{0.}(y,s)$ is the intention-to-treat effect, i.e. the difference in the outcome $F(y)$ by the instrument Z, regardless actual treatment status, that is regardless the observed value of D.

3.2 Data and Related Issues

Implementation of the identification strategy outlined in the previous section hinges on the estimation of a set of conditional expectations and conditional distributions, in particular: (i) $E[D_i|S_i, Z_i]$ in order to ascertain the size of the discontinuity in woman's schooling achievement resulting from the compliance with the 1963 reform and (ii) $Prob[Y_i < y|S_i, Z_i]$ in order to identify treatment effects of education on the distribution of mother's age at first birth Y_i up to a scale. The econometric literature has emphasized the use of local polynomial techniques to estimate conditional expectations in the regression discontinuity design (see Hahn and Van der Klaauw [20] and Hahn, Todd and Van der Klaauw [21]). However, for relatively well-behaved conditional expectations, estimates based on local polynomials differ little from

[14] It can be easily shown that the following holds:

$$F^C_{.1}(y|\bar{s}) = \frac{(\phi_a + \phi_c)}{\phi_c} F_{11}(y|\bar{s}) - \frac{\phi_a}{\phi_c} F_{01}(y|\bar{s}) \tag{1}$$

$$F^C_{.0}(y|\bar{s}) = \frac{(\phi_n + \phi_c)}{\phi_c} F_{10}(y|\bar{s}) - \frac{\phi_n}{\phi_c} F_{00}(y|\bar{s}) \tag{2}$$

where $F^C_{.1}$ and $F^C_{.0}$ denote the *potential outcomes* distributions among *compliers*; $F_{zd}(y|\bar{s})$ denote the distribution of Y conditional on $S = \bar{s}$, $D = d$ and $Z = z$; ϕ_a, ϕ_n, ϕ_c represent the population proportions of *always takers*, *never takers* and *compliers*, respectively.

those based on global polynomials. Moreover, local polynomial techniques are not necessarily the most appropriate when extrapolation is concerned, as it is the case in this application. Therefore, each conditional expectation will be smoothed by means of a parsimonious global polynomial in S and $Z \equiv 1(S \geq \bar{s})$ of appropriate degree of smoothness[15].
An additional issue one has to face with the empirical analysis, is fixing $\bar{s}$, i.e. the threshold from which the 1963 reform started to be effective[16]. The empirical strategy followed to get the first stage effect estimates (see section 4.1) addresses this quandary in a very simple way.
The main drawback of the using the 1963 reform as instrument is that it affects the schooling attainment of a relatively small subpopulation, namely those born in 1949-1952. Containing records on millions of individuals, the Census data can be used to create sizeable cohort's samples, even for relatively small target population groups such as women born in a specific year and with specific educational levels[17]. However, information from Census data is not readily available: indeed, unfortunately, the data used are not appropriate neither to examine completed fertility of women of the cohorts 1949-1952. Besides Census do not collect information on the women parental background, labour force partecipation, wage, etc. thus it is not possible to characterize the subpopulation of *compliers*.

3.2.1 12^{th} Census: the 2% Sample

Data come from the 2% random sample drawn from 12^{th} Census data for preliminary analysis (see ISTAT[12]). Data[18] on women of cohorts 1938-1956 are extracted considering all the women of these cohorts within households with all Italian members, which leaves with a sample of 142,386 women. Children are considered own-children of the woman who is either the household head or the wife of the household head of the household in which children live at the time of the 1981 Census Interview (October 25, 1981).

[15] Sensitivity of the parametric results to different smoothing techniques, specifically to the choice of the degree of smoothness, is checked and documented.

[16] Brandolini and Cipollone [8, pp. 12] and Flabbi [16, pp. 13], who exploited the reform to assess the (average) return to schooling for women in Italy, give different suggestions.

[17] Survey data provide relatively few individuals in each cohort and, therefore, offer less powerful means to the analysis of the causal relationship between education and fertility in settings such the one considered in this application.

[18] See Fort[17, p. 16 and Table 1] for details on the procedure used to link individuals in the same household.

Mother's age at birth of the oldest child (still at home) is referred as mother's age at first birth. Only records of women for whom age at first birth resulted greater than 15 years where considered, which leaves with a final sample of 141,311 women.

The empirical procedure used to matched children and mothers has two drawbacks. Firstly, one is only able to calculate mother's age at birth of children still leaving in the parental home at the time of the interview. This entails that the age at first birth assigned to mothers is likely to be upward biased, in particular for women of the older cohorts. Secondly, one is only able to assign children to women who have already left parental home, i.e. women who are either living on their own, regardless marital status, or are living with their husband at the time of the Census interview.

The first point risen brings to question the adequacy of the data to describe the timing of fertility of the older cohorts. However, it is definitely not likely to affect the identification of the causal effect of education on the timing of births. Indeed, the causal effect of education on fertility is correctly identified provided that children born to women of the cohorts 1948-1952 are still living in the parental home at the time of the Census interview. Since mean age at first birth of women of these cohorts is nearly 25 (ISTAT[13, Table 2, pp. 82]) and Italian adolescents tend to leave parental home late[19], this is most likely to hold in practice.

Mothers and children might be mismatched when the natural mother of each child is not the household head or the wife of the household head: this might happen when a woman rears her child in her parents' home or when the woman has divorced and re-married and lives with the children of the "new" husband. In the worst case, the proportion of households in which the empirical strategy exploited to match mothers and children might have lead to wrongly assign children to mothers would not exceed 1% (see Fort[17, p. 18]). Besides, the identification strategy is unaffected provided this mismatch takes place the same way on both sides of the threshold.

Mother's age at first birth is right-censored, since one can only observe births occurred up to the date of the interview[20]: thus the analysis will be limited

[19] The median age at which individuals born between 1966-1975 leave the parental home is 26.2 years for females, 24.9 years for males; for males born between 1956-1965 is 26.7, whereas for females of the same cohorts is 23.6 (Billari[4, Tab. 3.8, pp. 96]).

[20] Actually, in 1981, women of the cohorts 1938-1956 are aged between 25 and 43 and they have not yet completed their fertile lifespan. The extent of censoring of distribution

on the timing of births occurred before these women turn 27.

4 Main Findings

This section, firstly, presents the impact of the 1963 reform on education and, secondly, the causal effects of maternal education on fertility decisions. The reform exerted an effect on the qualification level of women who in 1963 had just completed primary school increasing the proportion of women of those cohorts who achieved junior high school degree. The influence was larger for women who were younger at the time the reform was introduced: the effect ranges between 0.01 and 0.06. Findings suggest that, a large fraction of the women affected by the reform tends to postpone the time of first birth but catches up the fertility delay before turning 26. There is some indication that the fertility behaviour of these women is different from the one of the average women in the population. Results are generally robust to the choice of smoothing technique and to the choice of the degree of smoothness.

4.1 The (First Stage) Effect of the 1963 School Reform on Education

In this section, firstly, the measure of education exploited in the application is discussed and, secondly, the size of the effect of the 1963 reform on education is assessed. Note that, individuals affected by the 1963 reform are the peculiar subpopulation of those who would not have completed junior high school in the absence of the reform and complete junior high school under the new law. The 1963 reform has eventually increased only the proportion of women achieving *exactly* junior high school degree, correspondingly reducing the proportion of individuals with primary school degree, but leaving the rest of the distribution unchanged[21]. D_i, the binary variable describing treatment status, takes the value 1 if individual i has attained *exactly* junior high school degree and 0 if he/she has a lower qualification. The analysis is

of age at first birth varies by cohort with the older cohorts being less affected, ranging from nearly 16% for the cohorts 1938-1945 to a maximum of 54% for the cohort of women born in 1956. The extent of censoring for those born between 1946 and 1952 ranges between 19% and 35%.

[21]Evidence supporting this statement is provided in the following.

Table 1: First Stage Effect of the 1963 Reform on the proportion of Women who achieved *exactly* Junior High School Degree by the time of the 12^{th} Census Interview. 2% Sample 12^{th} Census Data. Sample of women with Italian citizenship living in households with all Italian members whose age at first birth was either censored or greater than 15 years with at most Junior High School Degree.

Overall Sample Size: 128,086. Average Cohort Sample Size: 5,569				
	$\boldsymbol{s = 1949}$	$\boldsymbol{s = 1950}$	$\boldsymbol{s = 1951}$	$\boldsymbol{s = 1952}$
$\widehat{\boldsymbol{\phi_c(s)}}$	0.02	0.03	0.05	0.06
test (p-value)	5.1 (0.1)	16.11 (0.0)	31.36 (0.0)	47.15 (0.0)

Results are robust to the choice of degrees of smoothness and to the choice of the smoothing technique as documented in Fort[17, Table 2, p. 21].

limited to women with at most junior high school degree[22].

Previous analysis (see Flabbi [16] and Brandolini et al. [8]) suggests to fix $\bar{s} = 1952$, as the threshold year from which the 1963 reform started to be effective. However, the 1963 reform was effective also for individuals born a few years before, namely in 1949, 1950 and 1951. Therefore, contrasting directly the proportion of individuals with high qualification level in the cohorts around $\bar{s}$ might give biased estimates of the effect of the 1963 reform. The empirical strategy followed to get estimates of $E[D_i|S_i = \bar{s}]$ helps to address this quandary: firstly, the evolution of the series $E[D_i|S_i]$ over time is smoothed using a polynomial of appropriate degree; secondly, the information on the qualification level of individuals born up to the year 1948[23] is exploited to get estimates of $E[D_i|S_i = s, Z = 0]$, s = 1949, 1950, 1951, 1952 and, similarly, the information on the qualification level of individuals born after the year 1948 is exploited to get estimates of $E[D_i|S_i = s, Z = 1]$, s =1949, 1950, 1951, 1952. The motivation to consider this particular set of values of s is twofold: firstly, it is interesting to explore whether the 1963 reform had different effects depending on the time elapsed since primary

[22] The analysis has also been carried out using data of the whole sample of women and defining treated individuals those women with *at least* junior high school qualification at the time of the 1981 Census interview. The first stage effect estimates obtained on this wider sample have the same magnitude of those presented in Table 1 and lead to consistent inferential conclusions. These estimates, not reported here for brevity, are available from the author upon request.

[23] No one born before the year 1948 could have been affected by the 1963 reform (see Brandolini and Cipollone [8, pp. 11], Flabbi [16]).

school completion[24]; secondly, extrapolation becomes less plausible as one moves further from the threshold year $\bar{s} = 1949$.

Table 1 reports the estimates of the proportion of *compliers*[25] $\phi_c(s)$ computed at different values of s together with results of tests for the hypothesis that the effect is null ($H_0 : \phi_c(s) = 0$ *vs* $H_1 : \phi_c(s) \neq 0$): figures suggest that the proportion of *compliers* $\phi_c(s)$ increases as one moves s closer to 1952, i.e. women who were 14 at the time of the 1963 reform (most of those born in year 1949), did not go back to school to accomplish their obligations, whereas some women, for who the time elapsed between the completion of primary school and the year 1963 (in which the reform has been in force) was smaller, did, so that the reform exerted a larger influence on these second group of women.

A similar exercise has been performed to check if there has been any effect of the reform on the proportion of women who achieved high school qualification: as the graph in the right-hand panel of Figure 1 shows there is no effect of the 1963 reform on the proportion of women who achieve high school degree (see Fort[17, Table 3, p.23]). This result is robust to the choice of the smoothing technique and the choice of the degrees of smoothness.

4.2 The Effect of Education on the Timing of Births

This section examines the effects of the 1963 reform on the timing of births and provides insights on the magnitude of the causal effects of education on fertility.

Graphs in Figure 2 depict the cohort pattern in $F(y)$ at the ages $18, 20, 22, 24$ for the sample of women with at most junior high school degree[26]. Each graph shows a marked increasing trend[27]. This counter-intuitive tendency is due to the fact that graphs actually represent the probability that a woman

[24] Individuals born in 1952 were exactly 11 years old in 1963, that is they just completed primary school at the time the 1963 reform started to be effective, whereas individuals of younger cohorts were still attending primary school at the time the reform has been introduced and individuals of older cohorts (those born between 1949 and 1951) (should have) completed primary education years before.

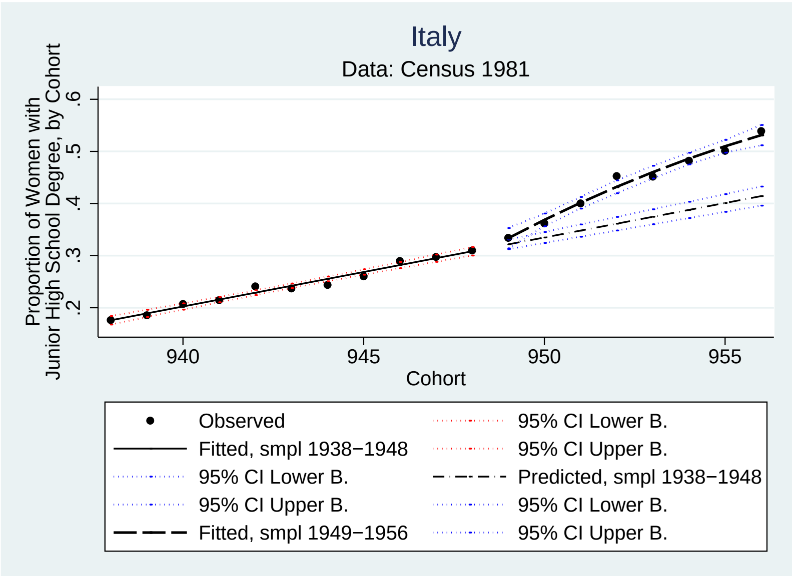
[25] Recall that the estimates of the effect of the 1963 reform on education (D) correspond exactly to estimates of the proportion of *compliers*.

[26] Additional graphs, not reported for brevity, are available from the author upon request.

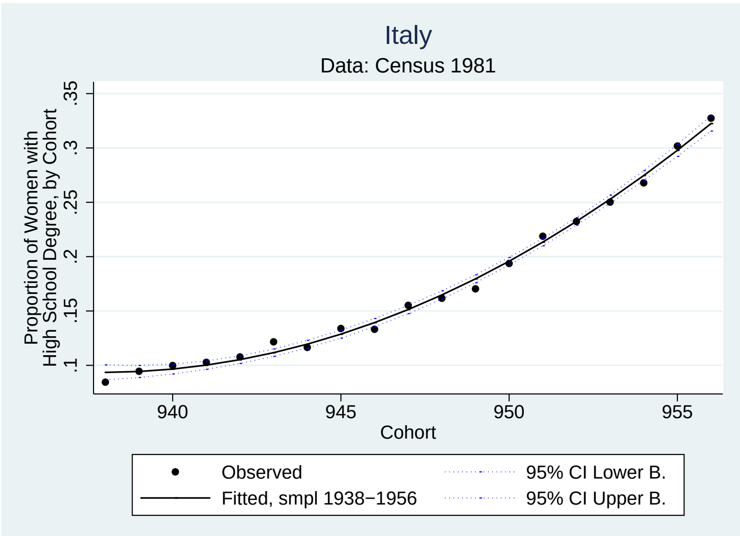
[27] The same pattern is observed considering the sample of all women. Graphs, not reported for brevity, are available from the author upon request.

Figure 1: Effect of the 1963 Reform on Women's Schooling Achievement.

Effect on the Proportion of Women with exactly Junior High School Degree



Effect on the Proportion of Women with exactly High School Degree



2% Sample of the 12^{th} Census Data. Sample of women with Italian citizenship living in households with all Italian members whose age at first birth was either censored or greater than 15 years.

Figure 2: Effect of the 1963 reform on $F(y) = Prob[Y_i \leq y]$ at distinct values of y, Y Woman's Age at First Birth. 2% Sample of the 12^{th} Census data. Sample of women living in households with all Italian members whose age at first birth was either censored or greater than 15 years with at most Junior High School Degree.

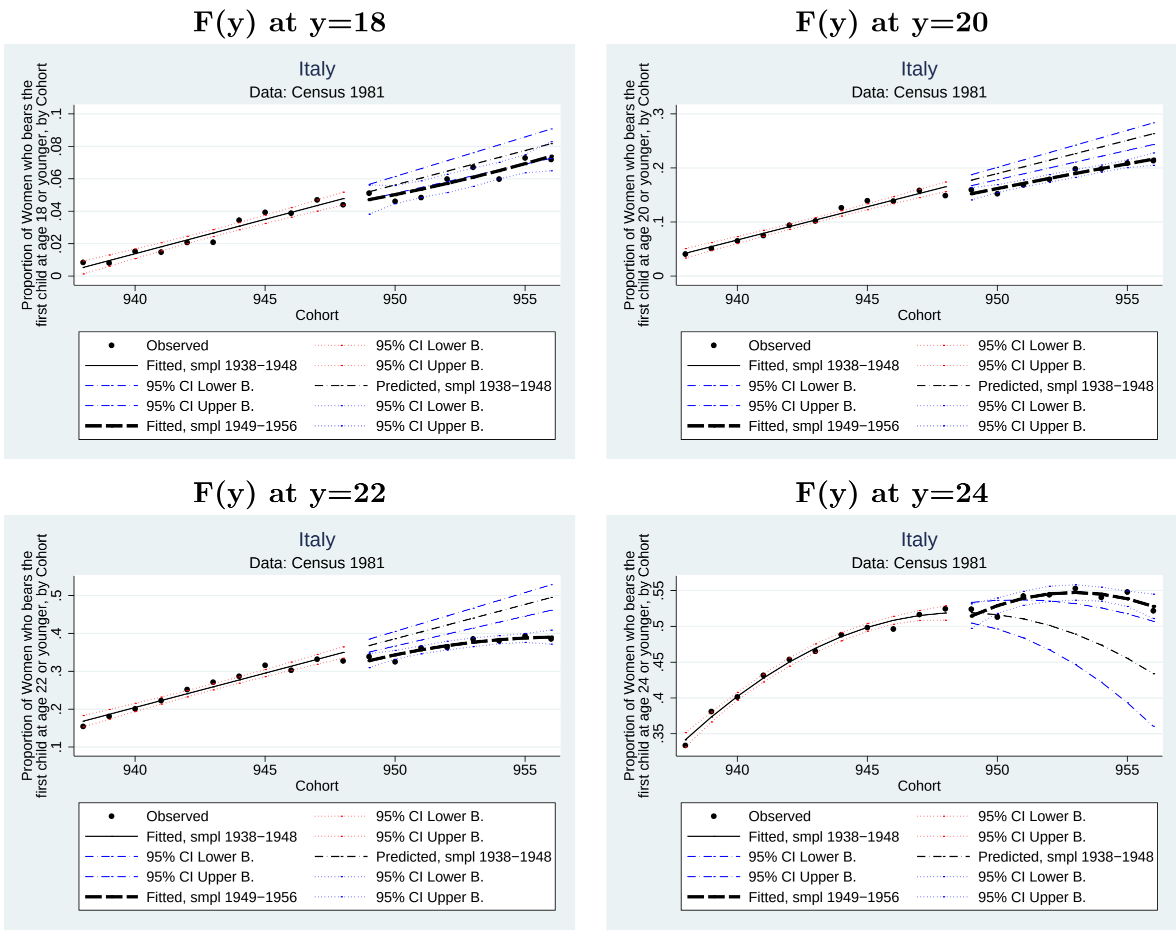


Table 2: Effect of the 1963 reform on $F(y) = Prob[Y_i \leq y]$ at distinct values of y, Y Woman's Age at First Birth (Intention-to-Treat Effect). 2% Sample of the 12^{th} Census data. Sample of women living in households with all italian members whose age at first birth was either censored or greater than 15 years with at most Junior High School Degree.

Overall Sample Size: 128,086. Average Cohort Sample Size: 5,569									
Smoothing Technique: Linear Probability Model									
y	**18**	**19**	**20**	**21**	**22**	**23**	**24**	**25**	**26**
					1950				
effect	-0.01	-0.02	-0.03	-0.03	-0.02	0.02	0.01	0.00	0.00
test	-1.63	-2.78	17.41	-3.38	-2.17	1.75	0.88	0.26	0.00
p-value	0.12	0.01	0.00	0.01	0.05	0.21	0.37	0.62	0.99
					1951				
effect	-0.01	-0.02	-0.03	-0.03	-0.02	0.03	0.03	0.02	0.01
test	-1.63	-2.78	20.54	-3.38	-2.17	4.29	2.78	1.21	0.06
p-value	0.12	0.01	0.00	0.01	0.05	0.06	0.12	0.29	0.81
					1952				
effect	-0.01	-0.02	-0.03	-0.03	-0.02	0.05	0.04	0.03	0.01
test	-1.63	-2.78	20.54	-3.38	-2.17	6.02	3.99	1.67	0.03
p-value	0.12	0.01	22.15	0.01	0.05	0.03	0.07	0.22	0.86

Estimates and standard errors under the preferred specification of the general tendency in the series $F_s(y) = Prob[Y_i \leq y|S_i = s]$. The test statistics tests the hypothesis that effect is null. Results are robust to the choice of the smoothing polynomial and smoothing technique (i.e., either linear probability or logit models).

of a specific cohort bore by the age y the oldest child who is still living with her at the time of the Census interview, who is not necessarily the first child ever born to that woman. This "mismatch" leads to assign to older cohorts' women a value of age at first birth which is higher than the true one. As previously highlighted (section 3.2), the arising bias does not affect the result on *local* identification of the causal effect of education on the timing of births for *compliers* at s, s =1949, 1950, 1951, 1952, provided children born to women of the cohorts close to s, that is 1948-1952, are still living in the parental home at the time of the interview.

If additional schooling reduces the incidence of first births by the age y, one would expect a decrease in the likelihood of experiencing first birth by age y for women born in the cohorts 1949-1952: the graphs in Figure 2 provide descriptive evidence supporting this prediction. Point estimates of the discontinuity (intention-to-treat effects), computed following the same empirical strategy exploited to get the first-stage effect estimates, are reported in Table 2.

In short, the evidence points toward the conclusion that the 1963 reform lead to: (i) increased education (nearly 6% increase in the proportion of individuals who achieve junior high school qualification), (ii) reduced likelihood (nearly 3%) of giving births at younger ages (19, 20, 21), (iii) negligible effects of giving births at older ages (25, 26). Nonetheless, a reduced form analysis does not provide insights on the magnitude of the causal effects of education on fertility. To address this, causal effects estimates are computed using the Wald estimator described by equation (3), i.e. the ratio of the reduced-form estimates (intention-to-treat effect estimates to first-stage effect estimate). Estimates and standard errors (computed using the delta method), reported in Table 3, suggest that a large fraction of the women affected by the reform postpones the transition to motherhood due to the higher qualification achieved[28]. However, education causes a delay in the transition to motherhood only for those women who, in the absence of the treatment, would have had their first child at young ages, namely at y =19, 20, 21. There is some evidence that these women catch up with the fertility delay later in their fertile lifespan: indeed, there seems to be no effect of education on the timing of the transition to motherhood at older ages, i.e. at y = 25,26[29].

To guarantee the key properties of the *compliers* potential outcomes' distribution functions (namely, monotonicity), estimates have been computed using an alternative *naive* estimator for the compliers' cumulative distribution functions[30]. "Revised" estimates are presented in Figure 3 and in Table 3. There is some evidence of heterogeneity of the impact of education over the distribution of births.

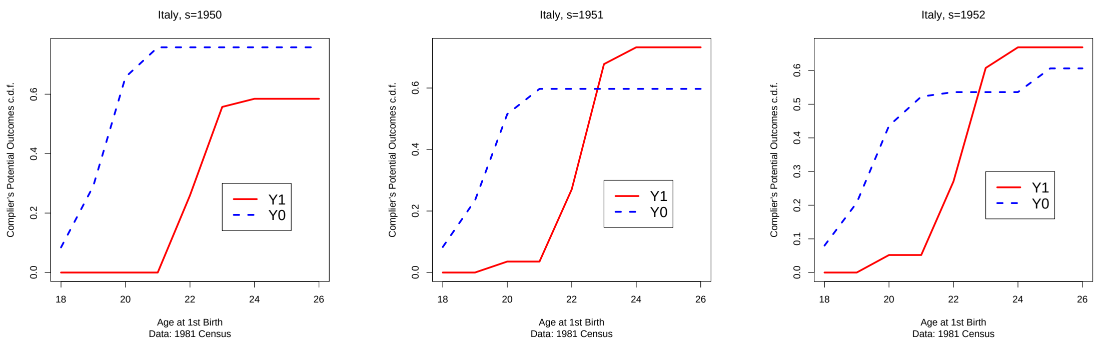
[28]Only cohorts for which the first stage effect of the 1963 reform resulted significantly different from zero are considered in the analysis, namely $s = 1950$, $s = 1951$, $s = 1952$. See Table 1.

[29]Results based on data of the whole sample of women, not reported for brevity but available from the author upon request, are broadly consistent with those presented in Table 3.

[30]Imbens and Rubin[23] consideredalternative estimators and found that a *naive* estimator of the density functions, obtained simply imposing non negativity, performs essentially as estimators based on the maximum likelihood. Here, their approach is followed operating directly on the cumulative distribution functions, i.e. $\widehat{F^C_{.k}(y,s)}^* = \max\left(0, \widehat{F^C_{.k}(y,s)}, F^C_{.k}\widehat{(y-1,s)^*}\right)$, $k = 0, 1$.

Figure 3: *Compliers*' Potential Outcomes' Cumulative Distributions functions.

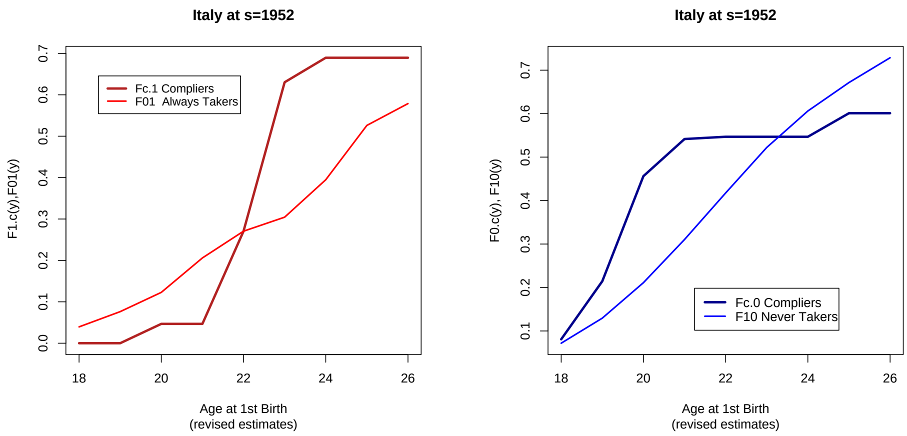
s=1950 s=1951 s=1952



Data: 2% Sample of the 12^{th} Census; sample of women living in households with all Italian members whose age at first birth was either censored or greater than 15 years with at most Junior High School Degree. Y^1 (solid lines), Y^0 (dashed lines).

Figure 4: *Compliers*' Distribution of Y^1 (Age at First Birth if women achieve High Qualification (Junior High School Degree)) and Y^0 (Age at First Birth if women achieve Low Qualification (less than Junior High School Degree)). Revised estimates.

$\boldsymbol{Y^1}$ $\boldsymbol{Y^0}$



Data: 2% Sample of the 12^{th} Census data; sample of women living in households with all Italian members whose age at first birth was either censored or greater than 15 years with at most Junior High School Degree.

Table 3: Causal Effect of Education on the Timing of Births for *compliers* at s ($LATE$). 2% Sample of the 12^{th} Census data. Sample of women living in households with all Italian members whose age at first birth was either censored or greater than 15 years with at most Junior High School Degree. First column ($LATE$): estimates moving from the Wald estimator (see equation 3); second column "revised" estimates moving from estimates of the compliers's potential outcomes' cumulative distribution functions.

	$s = 1950$		**$s = 1951$**		**$s = 1952$**	
	$\widehat{\phi_a}$ - $\widehat{\phi_n}$	$\widehat{\phi_c}$	$\widehat{\phi_a}$ - $\widehat{\phi_n}$	$\widehat{\phi_c}$	$\widehat{\phi_a}$ - $\widehat{\phi_n}$	$\widehat{\phi_c}$
	0.34 - 0.63	0.03	0.35 - 0.60	0.05	0.36 - 0.58	0.06
$F(y)at$	**$LATE$ (st.err.)**	**rev.**	**$LATE$ (st.err.)**	**rev.**	**$LATE$ (st.err.)**	**rev.**
$y = 18$	-0.2 (0.1)	-0.1	-0.1 (0.1)	-0.1	-0.1 (0.1)	-0.1
$y = 19$	-0.5 (0.2)	-0.3	-0.3 (0.1)	-0.2	-0.3 (0.1)	-0.2
$y = 20$	-0.8 (0.3)	-0.6	-0.6 (0.2)	-0.5	-0.5 (0.1)	-0.4
$y = 21$	-0.8 (0.3)	-0.7	-0.6 (0.2)	-0.6	-0.4 (0.1)	-0.4
$y = 22$	-0.6 (0.3)	-0.5	-0.4 (0.2)	-0.3	-0.3 (0.2)	-0.3
$y = 23$	0.5 (0.4)	-0.2	0.7 (0.4)	0.1	0.8 (0.3)	0.1
$y = 24$	0.4 (0.4)	-0.2	0.6 (0.4)	0.1	0.7 (0.4)	0.1
$y = 25$	-0.1 (0.3)	-0.2	-0.1 (0.2)	0.1	-0.1 (0.1)	0.1
$y = 26$	0.01 (0.5)	-0.1	0.1 (0.5)	0.1	0.1 (0.5)	0.1

Revised estimates (REV.) are computed as $\widehat{F^C_{.1}(y,s)}^* - \widehat{F^C_{.0}(y,s)}^*$, where $\widehat{F^C_{.1}(\cdot,s)}^*$, $\widehat{F^C_{.0}(\cdot,s)}^*$ represent the "revised" estimates of the *compliers*' potential outcome distribution functions.

$\widehat{\phi_n}$ and $\widehat{\phi_a}$ represent the estimates of proportion of *never takers* and of the proportion of *always takers*, respectively.

Lastly, heterogeneity of the impact across individuals is considered. Graphs[31] in Figure 4 depict the cumulative distribution functions of Y^1 and Y^0 for the sub-population of *compliers* and for the *non-compliers* (*always takers* and *never takers*).

The cumulative distribution function of Y^1 for *compliers* and *always takers* (see for instance graphs in Figure 4) exhibits striking differences: in the presence of the treatment, the proportion of *compliers* who bear their first child by young ages (18-21) is smaller than the proportion of *always takers* who bears their first child by the same age. However, between $y = 22$ and

[31] Additional graphs for the case $s = 1950$ and $s = 1951$ show the same pattern observed at $s = 1952$ and are not reported for brevity. They are available upon request from the author.

$y = 24$ the relationship reverses. This is consistent with the fact that, in the presence of the treatment *compliers* postpone the first birth event with respect to *always takers*.

The differences between the cumulative distribution function of Y^0 for *compliers* and *never takers* (see for instance graphs in Figure 4) are noticeable, with the *compliers* distribution being relatively steeper over almost the whole support $y \in [18, 26]$; thus, in the absence of the treatment, *compliers* seems to have their first child by younger ages than *never takers*.

On the whole, it seems that, at all points[32] s ($s = 1950$, $s = 1951$, $s = 1952$), in the presence of the treatment, the potential outcome distribution of *compliers* gets closer to the potential outcome distribution of *always takers*, at $y = 25$, $y = 26$. Similarly, in the absence of the treatment, the potential outcome distribution of *compliers* gets closer to the potential outcome distribution of *never takers*, at $y = 25$, $y = 26$.
The empirical evidence provided suggests that education causes a postponement in the transition to motherhood only to women who, in the absence of the treatment (i.e., "more schooling"), would have had their first child by young ages. These women are likely those who, in the absence of the treatment, face a lower opportunity cost of children and are therefore less likely to participate in the labour market. A rise in the achieved education, by increasing their current market wage[33], increases the probability that they participate in the labour market and rises the opportunity cost of children. Thus, women end up delaying early childrearing. As a consequence, the number of women who would experience motherhood for the first time between 19 and 22 years decreases. However, there seems to be no effect of the education on the proportion of women who experience motherhood at older ages, namely at the age 25, 26. This result might be driven by the following: the increase in qualification (from primary to junior high school degree) leads to an increase in the opportunity cost of children but the earning profile of these women remains rather flat, so that the incentives

[32]Additional graphs show the same pattern observed at s=1952 and are not reported for brevity. They are available upon request from the author.

[33]Brandolini and Cipollone [8] exploited the 1963 reform as instrument to assess the return on education for women in Italy. Their estimate of the average return on education for women over the years 1992 and 1997 ranges from 7% to 10%.

to postpone births operate only at younger ages (19-22).
Results, however, hold only for *compliers* and findings suggest heterogeneity of the effects across individuals: compared to *always takers*, under the effect of the treatment, *compliers* tend to have their first child later in their fertile lifespan, whereas, in the absence of the treatment, *compliers* tend to have their first child earlier compared to *never takers*. This discussion suggests that the fertility behaviour of the women affected by the reform is likely to be substantially different from the one of the average woman in the population. The identification strategy exploited in this application is capturing only the average marginal effect for women affected by the 1963 reform.

Lastly, findings are consistent with previous results by Bloemen and Kalwij [6] for the Netherlands, whereas they are not fully consistent with previous findings by Bratti [9]. This might be for mainly two reasons: firstly, Bratti considers a period measure of fertility, conversely here the analysis is based on cohort measures of fertility; secondly, Bratti [9] considers the effects of education on the probability of *a birth event*[34], whereas here the analysis is focused on the timing of first birth.

5 The Internal Validity of the Design: A Discussion

In this section evidence is provided to ensure that the results have a causal interpretation.
Nonetheless, most of the crucial assumptions for identification (local continuity at $\bar{s}$, exclusion restriction, stable unit treatment value assumption and local monotonicity) are intrinsically not testable.

One can claim that over the 1970s women position in the society, in Italy, went trough major changes, driven also by the newly introduced law on divorce (1970), the decrease in the threshold age at which a person becomes of age (1975), the law on abortion (1978) and the availability of oral contraceptives. Had these changes differently affected women born before and after 1949, the validity of the identification strategy exploited in this study

[34] "We consider a birth event to be the presence in the family of a child aged more than one and less then two years old." Bratti [9, pp.537]

could be questioned. Note that, for the result on identification to be valid, it is crucial that the discontinuity in the series $F_s(y)$[35] (as a function of s) is fully attributable to the effect of the 1963 reform and it is not driven by the mentioned innovations[36]. In other words, it is essential that **the same cohorts** of women affected by the 1963 reform have not been affected by any other “treatment” or “event” except the one under consideration (i.e., additional schooling).

To test the validity of this assumption, fertility of women (cohorts 1938-1956) who achieved high school qualification will be considered. Since these women have not been affected by the 1963 reform (see the right-hand panel of Figure 1), one would expect that the fertility behaviour of these women changes smoothly over cohorts. This prediction has been checked considering the proportion of women with high school qualification who had their first child[37] by the age y, by cohort, $F(y|s)$, where $y \in [20, 26]$ denotes the woman’s age at first birth and $s \in [1938, 1956]$ denotes the cohort to which women belong. The small number of events occurred[38] does not allow to get precise estimates and forced to consider ages not younger than $y = 20$. Notwithstanding this caution, the precision of the estimates remains quite low. On the whole, even slightly different parametric specification of the smoothing polynomial lead to conclude that the differences at s =1949, 1950, 1951, 1952 are negligible, as one can see from the figures in Table 4. Although, this result might not be conclusive due to the low precision of the estimates, nonetheless it provides some evidence supporting the validity of the local independence assumption.

In this application, the assumption of *no defiers* seems rather plausible since it basically requires that: (i) each woman born in years 1949-1952 got at least as much schooling as she would have in the absence of the 1963 reform and (ii) each woman born in 1948 got at most as much schooling as she would have if the 1963 reform has been in place one year before.

[35] $F_s(y)$ denotes proportion of women of the cohort s who bear their first child by the age y.

[36] Fort[17, p. 38-41] presents the main features of these innovations and discusses arguments suggesting that the discontinuity observed around $s = 1949$ has not been due to their impact.

[37] The discussion of the previous section applies: the age considered is the age at the oldest child still living in the parental home at the time of the interview.

[38] Events are births occurred to women with high school qualification belonging any cohort between 1938-1956.

The small amount of *compliers* supports the stable unit treatment value assumption: hardly the behaviour of less than 6% of the whole population might have induced spill-over effects.

Table 4: Effect of the Assignment to the Treatment (Z) on the Proportion of Women with High School Degree who bear their First Child by the Age y, $F(y,s) = Prob[Y_i \leq y|S = s]$, Y Woman's Age at First Birth (test p-values for the null hypothesis of no effect in paretheses). Italy ($Prob[Y_i \leq y]-$). 2% Sample of the 12^{th} Census Data. Sample of women living in households with all Italian members whose age at first birth was either censored or greater than 15 years with High School Degree.

	Overall Sample Size: 36,932. Average Cohort Sample Size: 1,605						
y	**20**	**21**	**22**	**23**	**24**	**25**	**26**
Smoot. Tech.							
				1949			
lpm	-0.01 (0.1)	-0.01 (0.5)	0.01 (0.2)	0.02 (0.3)	0.02 (0.1)	0.02 (0.2)	0.01 (0.8)
logit	-0.01 (0.0)	-0.02 (0.0)	0.00 (0.6)	-0.01 (0.5)	0.01 (0.7)	0.00 (0.8)	0.00 (0.9)
				1950			
lpm	-0.01 (0.1)	-0.01 (0.5)	0.01 (0.2)	0.02 (0.3)	0.01 (0.5)	0.00 (0.9)	0.01 (0.7)
logit	-0.01 (0.1)	-0.01 (0.1)	0.00 (0.8)	0.00 (0.7)	0.01 (0.6)	0.00 (0.9)	0.00 (0.8)
				1951			
lpm	-0.01 (0.1)	-0.01 (0.5)	0.01 (0.2)	0.02 (0.3)	0.00 (0.9)	-0.02 (0.4)	0.00 (1)
logit	-0.01 (0.34)	-0.01 (0.27)	0.01 (0.58)	0.00 (0.71)	0.01 (0.92)	-0.01 (0.44)	0.00 (0.88)
				1952			
lpm	-0.01 (0.1)	-0.01 (0.5)	0.01 (0.2)	0.02 (0.3)	-0.02 (0.3)	-0.04 (0.1)	-0.02 (0.4)
logit	0.00 (0.65)	-0.01 (0.41)	0.01 (0.62)	-0.01 (0.53)	-0.01 (0.59)	-0.03 (0.09)	-0.02 (0.22)

Estimates under the preferred specification of the general tendency in the series $F_s(y) = Prob[Y_i \leq y|S_i = s]$. *lpm* stands for linear probability model.

To sum up, the arguments provided suggests that the 1963 reform represent a valid instrument, which helps to correctly identify the causal effect of education on the timing of (first order) births for *compliers*.

6 Concluding Remarks

This paper provides evidence on the role of education in determining the timing of first birth exploiting exogenous variation in schooling induced by a school reform rolled out in Italy in the early 1960s. The identification strategy exploits the fact that women born just after year 1949 were

affected by the increase in compulsory schooling introduced by a reform, whereas women born just before year 1949 were not. Compared to women born before 1949, women of the cohorts 1950-1952 have substantially lower likelihood to experience childbearing for the first time at the ages y=19, 20, 21, whereas no evidence is found of a causal effect of education on the probability of bearing the first child at older ages (y = 24, 25, 26).
These results are essentially as good as comparisons based on randomization provided that confounders show a smooth cohort pattern. On prior grounds it sounds credible that women born in subsequent cohort are essentially exchangeable. The internal validity of the research design is extensively discussed: evidence based on the data at hand suggests that the 1963 reform represent a valid instrument, which helps to correctly identify the causal effect of education on the timing of first birth for *compliers*.

The estimates provided apply only to women who were affected by the 1963 reform on compulsory schooling, i.e. to 3%-6% of the population. Besides, findings suggest heterogeneity of the effects across individuals: under the effect of the treatment, *compliers* tend to have their first child later in their fertile lifespan compared to *always takers*, whereas, in the absence of the treatment, *compliers* tend to have their first child earlier compared to *never takers*. This discussion suggests that the fertility behaviour of the women affected by the reform is likely to be substantially different from the one of the average woman in the population.
Generalizing this effect to a wider set of individuals requires typically relying on stronger conditions than those who guarantee local identification.
Since new mandatory schooling laws have been introduced in many countries in the last decades, the identification strategy employed in this study can be easily replicated in other countries. This would represent an intriguing way to generalize results to other countries.
Indeed, the subpopulation of *compliers* might be *per se* and interesting subpopulation, if, for example, the women affected by compulsory schooling laws happen to be those at the highest risk of teenage childbearing. It has long been emphasized the role of education in reducing rates of teenage pregnancy: the results presented here further support this evidence for Italy.
Besides, further research is needed to assessing whether changes in education produce similar effects regardless of the level at which additional education

is obtained: it would be interesting to explore the effects of increase in education at higher educational level using the same approach. In principle, this would be possible for Italy, where a reform of the higher education system (university) has been implemented in year 2000 (*Decreto Ministeriale* N. 509/99).

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