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

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ABSTRACT

This paper assesses the causal effects of education on the timing of first births allowing for heterogeneity in the effects across individuals 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 return to schooling of these women is substantially different from the one of the average individual in the population.

Keywords : Causal Effects Identification, Education, Local Average Treatment Effects, Motherhood Decisions, Regression Discontinuity Design.

JEL codes: J10, J13, I2.

NON-TECHNICAL SUMMARY

This paper aims at assessing the causal effects of education on the timing of 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. Besides, the Italian schooling system has undergone lots of changes since 1859, particularly as far as compulsory schooling is concerned; notably, the last increase of compulsory schooling age was planned in 1999.

Thus, addressing the question of how fertility responds to exogenous variation in education 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 a decline in fertility and motherhood postponement: Sleebos (2003) underlined that several OECD governments are considering or have already introduced specific measures aimed at countering these trends in fertility. Besides, also teenage childbearing attracts some politic interest, due to its association with a range of disadvantages, both for the mother and for children: on average, across 13 countries of the European Union, women who give birth as a teenager are twice as likely of living in poverty.

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: 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 and the central role of education in achieving the European Union strategic goal has also been recently stressed during the 2005 summit in Bruxelles.

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

A number of studies report negative association between schooling achievements and completed fertility in most countries. According to the model developed by Mullin and Wang (2002), women with greater ability face larger loss in earnings from having children and thus delay childbearing. Moreover, higher ability women are more sensitive to changes in the utility children provide once born. Ellwood et al. (2004) find some evidence that the lifetime costs of childrearing are particularly high for skilled women and are reduced by delaying childbearing.

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. Indeed, the direct comparison between women with different qualification level does not generally identify the causal effects of education on fertility, since women with preferences for larger number of children are likely to invest less in human capital and have their children earlier. 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.

In this paper, evidence supporting the role of education in determining the timing of first births is provided.

The identification strategy exploits the fact that women born just after year 1949 were affected by the increase in compulsory schooling imposed by a reform rolled out nationwide in Italy in the early 1960s, 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 by the ages 19, 20, 21, whereas they have a higher likelihood to bear their first child by the age 23. No evidence is found of a causal effect of education on the probability of bearing the first child by older ages (24, 25, and 26).

On prior grounds it sounds credible that women born in subsequent cohort are essentially exchangeable, so that these results are essentially as good as comparisons based on randomization. However, 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. The internal validity of the research design is extensively discussed, explicitly considering also these factors: evidence based on the data at hand suggests that the 1963 reform represents a valid instrument, which helps to correctly identify the causal effect of education on the timing of first births for the sub-population of women affected by the reform.

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 and that the fertility return to schooling of 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 to rely on stronger conditions than those who guarantee local identification.
Nonetheless, the sub-population of women affected by the reform might be *per se* an interesting sub-population, if, for example, the women affected by compulsory schooling laws happen to be those at the highest risk of teenage childbearing.

1 Introduction and Motivation of the Paper

This paper aims at assessing 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[1](Kohler and Billari and Ortega [44]). Besides, the Italian schooling system has undergone lots of changes since 1859, particularly as far as compulsory schooling is concerned (see Genovesi [35]); notably, the last increase of compulsory schooling age was planned in 1999. Thus, addressing the question of how fertility responds to exogenous variation in education 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 [36]): Sleebos [56] underlined that several OECD governments are considering or have already introduced specific measures aimed at countering these trends in fertility.

Besides, also teenage childbearing attracts some politic interest, due to its association with a range of disadvantages, both for the mother[2] and for children[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 [59]).

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 [48]).

Do family friendly policies, policies aimed at reducing teenage childbearing and policies aimed at increasing average schooling achievement pursue com-

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[1]Total fertility rate at or below 1.3.

[2]For instance: dropping out school, being unemployed or low paid, live in poor housing conditions, live on welfare.

[3]For instance: being a victim of neglect or abuse, becoming involved in crime, achieving lower qualification, abusing drug or alcohol.

[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 [26])

patible goals? Besides, do these policies affect any woman in the same way? A number of studies report negative association between schooling achievement and completed fertility in most countries: among others, Nicoletti and Tanturri[50], examining data on 10 European countries, found that higher level of education generally lead to both postponement of motherhood and to a reduction of the probability of the first birth event.

According to the model developed by Mullin and Wang[49], women with greater ability face larger loss in earnings from having children and thus delay childbearing. Moreover, higher ability women are more sensitive to changes in the "childrearing preference"[5]. Ellwood et al. [33] find some evidence that the lifetime costs of childrearing are particularly high for skilled women and are reduced by delaying childbearing. Costs of childbearing for high skilled women seem to increase with time. Conversely, low skilled women seem to face a one-time loss.

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. Indeed, the direct comparison between women with different qualification level does not generally identify the causal effects of education on fertility, since women with preferences for larger number of children are likely to invest less in human capital. 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.

Thus, the main focus of this paper is to address the question of how fertility responds to exogenous variations in education 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[17] widening the knowledge on the determinants of the recent trends in fertility in Italy. Moreover, it presents 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 links between female labour force participation and marital fertility (among others, Angrist and Evans [4] and Schultz [54]) 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 discusses

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[5]That is the utility children provide once born.

the predictions of economic models of fertility choices, with particular attention to the potential role of parents educational achievement in determining the timing of births. Besides, section 2 reviews empirical findings of previous studies on the relationship between parent's educational achievement on 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

This section, firstly, discusses how variation in prices, wages and income could affect the optimal age at motherhood according to the various dynamic models developed in the literature[6], highlighting the potential role of parents education in determining the timing of births. Then, it presents empirical evidence found in previous studies on the relationship between parent's educational achievement and *tempo* fertility.

Some of the crucial ideas of the work by Becker[6] are relevant also for the dynamic modelling of fertility: (i) the idea that household members specialize in market or home activities according to their comparative advantages and also allocate investments according to these, (ii) the concept of "children's quality" (future earning ability and life expectancy of the offspring) and the implications of the interaction between number of children and children's quality.

Becker argued that men and women have different comparative advantages in their contribution to childrearing: women, who devote much time in effort-intensive activities like child rearing, would economize the use of energy in the workplace, seeking more convenient and less energy-intensive jobs; as a consequence, women with children might reduce their time in the labour force and their investments in market human capital, leading to a further decline in the opportunity for working. Indeed, most economic models of fertility behaviour consider husband's and wife's contribution to childrearing in an asymmetric way[7], asserting that only wife's time is spent in housing

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[6]Economic models for fertility behaviour can be divided into two main classes: static (one period lifetime) and dynamic (multiple periods lifetime) models of fertility behaviour. Static models focus on the determinants of completed fertility (Becker [6], Easterlin [32], Leibenstein [47]), whereas, dynamic models are mainly concerned in explaining the timing and spacing of births and help in the understanding of completed family size as it results from the sequence of births (Butz and Ward[19], Cigno [24], Cigno and Ermisch [25], Happel et al. [40], Gustafsson [36]).

[7]Willis[pp. 380][60] asserts that even sexual activity is "a matter of choice of the woman".

responsibility and that the demand for children is more sensitive to changes in wife's wage than to household income changes. The last statement follows from the reversal of the quantity-quality ordering for price and income elasticities[8]. The husband's income is usually assumed exogenous.

Butz and Ward[19] considered the options for intertemporal substitution of births and the role of future expectations, adding an additional choice variable (the timing of fertility) to the previous set (quantity and quality of children and allocation of wife's time between work and household responsibilities): a wage increase in the current period will induce a substitution of births toward the future, while a wage increase in the future will lower the relative price of a current birth, which might lead to an increase in current fertility. These effects operate in addition to the usual income and price effects given by the static fertility theory.

Gustafsson [36] highlights the role of three factors in determining the optimal age at motherhood: (i) the value parents attribute to their offspring: parents with positive time preference have an incentive to have their children early in life, in order to enjoy them longer; (ii) how the mother's costs of childbearing evolve over her lifetime; (iii) the structure of capital markets.

In the case of perfectly imperfect capital markets, the optimal age at motherhood results from the comparison between the marginal loss of income due to depreciation of the woman's human capital and the marginal utility of income in terms of consumption: the model presented by Happel et al. [40] suggests that households have an incentive to postpone births until a moment when the cost of child can be offset by man's higher earnings.

In the case of perfect capital markets, the timing of births depends on the opportunity cost of children, i.e. the opportunity cost of mother's time, and husband's income lifetime path plays no role, since it is assumed that man's labour market career is not affected by birth timing. Besides, the cost of mother's time is affected by: (i) the amount of woman's accumulated human capital at the beginning of the planning period;(ii) the rate at which woman's job skills decay with no participation in the labour market; (iii) the slope of woman's age earning curve; (iv) the profile of human capital investments; (v) the length of time spent not participating in the labour market. Most models predict that increases in these factors give an incentive to the postponement of motherhood (Gustafsson [36]). Conversely, according to Cigno [23], if the rate of women human capital depreciation does not vary with the ability level, women with higher education will have their children in the earlier part of their marriage, whereas women with low ability will spread births more evenly over their married life. Nonetheless, Cigno and Ermisch [25] highlighted that this tendency might be offset by the fact that women with greater human capital have also steeper earnings profiles which

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[8]Becker and Lewis[7] conclude that the observed price elasticity of quantity exceed that of quality and, conversely, the observed elasticity of quality exceed that of quantity.

might induce them to delay parenthood.

According to the model developed by Mullin and Wang [49], women with greater ability face larger loss in earnings from having children and thus delay childbearing. Moreover, higher ability women are more sensitive to changes in the "childrearing preference"[9]. Ellwood et al. [33] find some evidence that the lifetime costs of childrearing are particularly high for skilled women and are reduced by delaying childbearing. The authors suggest that the period specific costs of childrearing result from the present value of the pay lost and the subsequent reduced-pay spell due to rearing a child in a specific period and the difference between the without-child and with-child wage growth rates. If the earning profile flattens in the period after the woman has a child, early births might be particularly costly in a career when age-earnings profiles are steep. Since the profiles of more educated women are steeper on average, "it would seem plausible that the gains to waiting would be greater for this group" (Ellwood et al. [33, p. 4]).

The dynamic model proposed by Blackburn et al. [9] suggests that individuals who prefer an early child birth are less likely to invest in human capital.

In most models, education is regarded as a "modernization variable" which affects both demand and supply for children: Janowitz [43] distinguished direct effects of education on fertility, consisting in the influence through widening a woman's horizons and increasing contraceptive knowledge, and indirect effects, consisting in the influence through market productivity or labour force participation and age at marriage. Other authors (Blossfeld and De Rose [11], Kohler et al. [55]) highlighted the importance of distinguishing between the level of educational investments and the enrolment status itself.

The leading idea is that education level might affect marginal market wage of the woman and her earning profile, thus changing the opportunity cost of children and inducing modification in the demand for children.

The supply of children could be affected by changes in education achievement as well: enhancement of average education might alter fertility behaviour accruing knowledge and more efficient use of contraceptive methods[10].

Greater schooling achievement has also consequences for marriage and divorce, making the division of labour between wife and husband less straightforward and making therefore less efficient to marry. Gustafsson and Worku [37] find that "higher education of one of the spouses, the duration in education and unfavourable labour market conditions delay couple formation (and first birth)" in Britain and Sweden.

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[9]That is the utility children provide once born.

[10]Schultz and Rosenzweig [51] found evidence suggesting that couples with higher education level have a wider knowledge of contraceptive methods and use them more efficiently.

Women usually wait with children until after they have finished educational careers because of: (i) the incompatibility of education and childbearing; (ii) the increased risk of not completing education due to a birth and the high opportunity cost of failing to complete education; (iii) the high life cycle costs of delaying completed education and delaying the entrance into the labour market, especially in high developed countries with high returns to human capital; (iv) the desire to establish oneself in career after completing education and before having a child; (v) social norms that discourage child-bearing while human or couples are still in education (Blossfeld et al. [11], [12], [13]).

Changes in the husband's schooling achievement are not expected to strongly affect completed fertility and the timing and spacing of children. Nonetheless, the educational attainment of the wife is not an exogenous variable with respect to her husband's wage rate, education, or tastes for children: mate selection and allocation of both spouse's time between the market and non-market activities are decisions that are intimately related to price and income variables as well as underlying tastes, which might be driven partly also by education.

In short, most dynamic models of fertility behaviour predict the postponement of motherhood as a consequence of 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[11]. Secondly, measures of fertility have been traditionally referred to women because of the lesser role of men in child rearing. However, recent changes in the appearance of the family in most European countries, might cast doubts on the adequacy of this approach[12]. 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). If the processes determining individual's fertility behaviour are stationary, than period and cohort measures of fertility match exactly. The two sets of measures differ when changes in fertility behaviour

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[11]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.

[12]Recently, Willis[60] discusses the economics of fatherhood.

occur (for instance due to effects of a war or because of changes in laws or as a result of a recession, and so on). Observed changes in *quantum* fertility based on period measure are misleading, since they do not take into account that younger cohorts might later catch up. Thirdly, as previously highlighted in this section, the channels through which the effect of education might take place are numerous (Janowitz [43]) and, lastly, the effect of education on fertility might be heterogeneous across women with different ability, skill levels (Blackburn et al. [9], Ellwood et al. [33], Mullin and Wang [49]), family background .

Estimating the magnitude of the causal effect of education achievement on fertility is a non-trivial challenge. The major issue is the typical problem of econometric identification. Generally variation in income and prices recorded in the data may not correspond to an exogenous variation because of unobserved heterogeneity: woman with preferences for larger number of children are likely to spend more time not engaged in the labour force and the less time spent working lowers the returns on human capital accumulation and thus her investments (in human capital); women with preferences for earlier births are less likely to invest in education (Blackburn et al. [9]). As a consequence, the direct comparison of the fertility behaviour of women with different education level is likely to lead to biased estimates of the impact of education on fertility.

Lots of empirical studies have documented positive association between education and fertility postponement.
Blossfeld and Huinink [12], Blossfeld and Jaenichen [13] and Blossfeld and De Rose [11] distinguish two distinct roles of education in determining women's fertility decisions: on the one hand, the role of human capital accumulation (i.e., the specific level of qualification acquired) and, on the other hand, the role of educational enrolment itself. Using longitudinal data and event-history analysis methods, the authors document a delaying effect of education on the timing of first marriage and entry into motherhood common both in Germany (Blossfeld and Jaenichen [13]) and in Italy (Blossfeld and De Rose [11]). However, Blossfeld and Huinink [12] and Blossfeld and Jaenichen [13] include in their model a number of controls for the unobserved heterogeneity among individuals (mainly social background variables, such as father's social class, number of siblings and type of residence at age 15) whereas Blossfeld and de Rose [11] do not take endogeneity of schooling decisions into account.
Nicoletti and Tanturri [50] consider the determinants of the motherhood postponement in 10 European countries[13] and find that higher levels of ed-

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[13]Belgium, Denmark, France, Germany, Greece, Ireland, Italy, Portugal, Spain, United Kingdom.

ucation generally lead to both the postponement of parenthood and the reduction of the probability of the first birth event. According to their findings, an early completion of education and an early entry into the labour market are associated with early entry into motherhood in all European countries.

However, as pointed out in the previous section, the identification and estimation 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 (and thus human capital investments and accumulation) or to assign education level randomly to individuals, so that it would not be correlated with personal or social factors.

Bloemen and Kalwij [10], in their analysis of the timing of births and labour market transitions of women in the Netherlands, show that unobserved heterogeneity is empirically important. Their findings suggest that women with higher preference for work over children have significantly higher employment rates at all ages, delay births and have a significant lower level of completed fertility. Moreover, their results show that 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[17], 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, such as father's and mother's education, job qualification and branch of activity. Using survey data (1993 Survey on Households Income and Wealth, Bank of Italy), he 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. It should be highlighted that Bratti's measure of marital fertility is a period measure of marital fertility, i.e. it measures marital fertility of the hypothetical cohort with age-specific marital fertility rates observed in a given year.

Skirbekk, Kohler and Prskawetz[55] use the exogenous variation in school graduation resulting from differences in birth month to estimate the effect of "duration of education" or "age at graduation" on the timing of births and marriage in Sweden. Using data from the Swedish registration system, 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[14].

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[14]In addition to this, the authors note that, at relatively young childbearing ages, those who were born in the first half of the year (that is, those who were the oldest in their class) have a lower risk of having a first child than those who were born in the second half of the year. However, this pattern reverses at older ages. The same results hold also as regards the timing of second-order births and the timing of marriage. The authors suggest that this pattern might result from the fact that women tend to synchronize the timing of births and marriage with women in their school cohorts, rather than with women of a

The results by Bratti [17] apply to women with "mean taste for children and work". This characterization is sensible when the effect is constant across different levels of (unobserved) tastes for children and work (and human capital accumulation) or , equivalently, when an increase in schooling has similar effects on the fertility behaviour of observationally identical women. In this case, education and tastes do not interact in shaping the women's behaviour: both factors have an independent contribution. Nevertheless, education might interact with tastes in a non-trivial way, inducing a more intricate change in the timing and in the distribution of births.

This paper focuses on the *total* effect of education on fertility and no attempt is made to disentangle direct and indirect effects. 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 data on completed fertility.

3   Empirical Analysis

As the discussion in the previous section highlighted, the identification and estimation of the **causal effects** of education on fertility requires either to be able to control for unobserved heterogeneity in the individuals decisions as regards education and fertility 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"[15] guarantees the identification of causal effects for a **sub-population** , the so called *compliers* (Angrist, Imbens and Rubin [5], Imbens and Rubin [42], Abadie, Angrist and Imbens [2]). The *compliers* represent the sub-population of individuals whose treatment status can be influenced by the instrument. This identification strategy is grounded on mild non parametric restrictions and does not fully spell out the underlying theoretical relationships among outcome and the "cause".

This section firstly introduces the framework for causal inference and presents the causal parameters of interest, highlighting the crucial assumptions for identification characterizing the research design exploited. Then,

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similar age.

[15]See Rosenzweig and Wolpin [52] for a critical review of recent studies in different areas of enquiry which used this approach.

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 described as a general function of inputs, some of which are choice variables of the mother (X) and some of which are concomitants (W), i.e. factors affecting fertility decisions which are not determined by the mother: 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. The choice variables X can be affected by the schooling level (E) and concomitants (W), whereas the concomitants may not and the schooling level might itself be included in X. This can be formalized as: $Y = f(X, W)$ and $X = g(E, W)$ and this formalization leads to the following gradient of fertility in schooling: $\dfrac{\partial Y}{\partial E} = \dfrac{\partial f}{\partial X}' \dfrac{\partial g}{\partial E}$.

The effect of education on fertility is a reduced form parameter summarizing the impact of schooling on behaviour $\frac{\partial g}{\partial E}$ and the impact of behaviour on fertility $\frac{\partial f}{\partial X}$.

In this application, the outcome of interest Y represents woman's age at her first child's birth (measure of *tempo* fertility). D_i is a dummy variable representing the treatment (namely, "more schooling"): it takes the value 1 if individual i has a high qualification and the value 0 otherwise. D_i

Potential outcomes (Rubin [53], Holland [41]) are defined, for all the individuals in the population regardless their actual treatment status, as follows:

$\mathbf{Y_i^1}$ is the mother's age at first birth i if she would be exposed to the treatment, i.e. if she would get a high qualification;

$\mathbf{Y_i^0}$ is the mother's age at first birth i if she would not be exposed to the treatment, i.e. if she would get a low qualification.

For each individual i, one observes $Y_i = Y_i^1 D_i + Y_i^0 \left(1 - D_i\right)$ and D_i: since, for each individual i, D_i can either take the value 0 or 1 but not both, one observes Y_i^1 on individuals with high education level and Y_i^0 for individuals with low education level. The observed outcome is *factual* and the not-observed outcome is referred as *counterfactual* (Rubin [53], Holland [41]). The individual specific causal effect is defined as $Y_i^1 - Y_i^0 \equiv \beta_i$ and is intrinsically not observable. The fact that usually one is not interested in the specific sample units but, conversely, in making inference on the behaviour of units under the influence of the treatment generally sustains the shift from individual causal effects to average effects. Indeed, one is usually interested in the following causal parameters:

- the **average treatment effect** $ATE = E[Y_i^1 - Y_i^0] = E[Y_i^1] - E[Y_i^0]$

- the **average treatment on the treated effect**
  $ATTE = E[Y_i^1 - Y_i^0 | D_i = 1]$

- the **effect of treatment on quantile q** (QTE)

$$QTE_q = F_{Y1}^{-1}(q) - F_{Y0}^{-1}(q), \forall q \in [0, 1]$$

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 [46] and definitions by Doksum [31].
The quantile treatment effect represents the change in the response function required to stay on the qth conditional quantile function (horizontal distance between the distribution functions F_{Y1} and F_{Y0}).

If the treatment effect is homogeneous, the average treatment effect represents the treatment effect for a randomly chosen individual in the population. Otherwise, it represents the average of the different effects over the whole population. Since quantile treatment effects might differ at different values of q, ideally, one can test the hypothesis of heterogeneity of the impact comparing quantile treatment effects at different quantiles: dissimilar quantile treatment effects at different quantiles q suggest heterogeneity of the treatment effect.
The average treatment effect ATE is the average of all possible quantile treatment effects. Thus, when the treatment affects only the location of the distribution, QTE and ATE correspond exactly; conversely, the two differ when the potential outcomes distribution differ by scale or by location and scale.

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 $F_{Y1}^{-1}(q)$ $F_{Y0}^{-1}(q)$ in the observed data, unless D was randomly assigned to individuals, eventually conditioning on a set of covariates. Indeed, in observational studies, D is generally not randomly assigned to individuals and individuals with different values of D_i are likely to be systematically different as regards both their socie-economic status and their fertility Y_i: variation in D might actually reflect endowments such as parental resources and time preferences which are likely to affect women decisions but are not observed by the analysts.

In this application, identification of the causal effect of education on fertility relies on a regression discontinuity design (Trochim [58], Thistlethwaite

and Campbell [57]), exploiting a mandatory schooling reform rolled out nationwide in Italy in 1963[16]. The **1963 reform** (*N*.1859 Act December 31, 1962) prescribed the unification of the previous junior high school (*scuola media* and *scuola di avviamento professionale*) in a single compulsory junior high school (*scuola media*). Until 1963, individuals basically completed primary school (5 years); from 1963 onwards, it was effectively compulsory to attend at least 8 years of schooling, which were common for all individuals, regardless their preferences for high education courses or vocational training. 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 14th birthday were allowed to drop out. Basically, due to the new law, individuals born after 1949 were compelled to attend 3 more years schooling. Since assignment to the treatment ("more schooling") was fully determined by the individuals' date of birth (S), it can be argued that it was random. 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. On the other hand, 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 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). Moreover, 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 [39]).
Compliance with the reform was not perfect (Brandolini and Cipollone[16]): some individuals born after 1949, i.e. assigned to the treatment, did not reach high qualification level (compulsory schooling) and some individuals born before 1949, i.e. not assigned to the treatment, attended 8 years of schooling even if not compelled to. Due to the imperfect compliance with the assignment to the treatment, this identification strategy does not lead to the identification of the average treatment on the treated effect unless the causal effect of education on fertility is homogeneous in the population. In the case of heterogeneous effect, instead, it identifies the average causal effect of education on fertility for those individuals persuaded to obtain additional education by virtue of the reform (*compliers*), that is the local

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[16]For an overview on recent changes in the Italian schooling system, see Genovesi [35].

average treatment effect, $LATE$ (Angrist, Imbens and Rubin[5]). 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[17] 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 close to the threshold $\bar{s}$ are similar as regards potential outcomes; (iii) there are no individuals who do exactly the opposite of their assignment.

Note that the result on identification does not rely on a parametric specification of the relationship between Y_i and D_i, neither it relies on the assumption of homogeneity of the treatment effect across individuals. To allow for heterogeneous effects over the distribution of Y_i, the attention will be devoted to quantile treatment effects. Quantile treatment effect can be easily obtained from the potential outcomes' marginal distributions. Indeed, Imbens and Rubin [42] showed that, under the $LATE$ identifying assumptions[18], the *compliers'* potential outcomes' distributions $F_{.1}^{C}(y) \equiv Prob[Y_i^1 \leq y|\text{C}]$ and $F_{.0}^{C}(y) \equiv Prob[Y_i^0 \leq y|\text{C}]$ can be written as a weighted average of observed distribution by treatment status and assignment to the treatment. Similarly, in the regression discontinuity design framework, it can be easily shown that the following holds:

$$F_{.1}^{C}(y, \bar{s}) = \frac{(\phi_a + \phi_c)}{\phi_c} F_{11}(y, \bar{s}) - \frac{\phi_a}{\phi_c} F_{01}(y, \bar{s}) \qquad (1)$$

$$F_{.0}^{C}(y, \bar{s}) = \frac{(\phi_n + \phi_c)}{\phi_c} F_{10}(y, \bar{s}) - \frac{\phi_n}{\phi_c} F_{00}(y, \bar{s}) \qquad (2)$$

where $F.1^C$ and $F.0^C$ 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 $Z = z$; Z_i is a dummy variable which describes the assignment to the treatment: it takes the value 1 if the individual i is assigned to the treatment, i.e. she is born after the first year since the 1963 reform

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[17]See Hahn, Todd and Van der Klaauw[39] for a formal discussion on identification and estimation of treatment effects in a regression-discontinuity design.

[18]Namely, stable unit treatment value assumption, the exclusion restriction, the strict monotonicity and the random assignment assumption. See Angrist, Imbens and Rubin [5] for an extensive discussion.

started to be effective, and 0 otherwise[19]; ϕ_a, ϕ_n, ϕ_c represent the population proportions of *always takers*, *never takers* and *compliers*, respectively.

The four distribution $F_{zd}(y, \bar{s})$, $d \in \{0, 1\}$, $z \in \{0, 1\}$, can be identified from the observed data (Y, D, S, Z), as well as the proportions ϕ_a, ϕ_n, ϕ_c since:

$$1 - E[D_i | S_i = \bar{s}, Z = 1] = \phi_n \tag{3}$$

$$E[D_i | S_i = \bar{s}, Z = 0] = \phi_a \tag{4}$$

$$1 = \phi_n + \phi_c + \phi_a \tag{5}$$

As a consequence, also the potential outcomes' distributions $F_{.1}^C(y, \bar{s})$ and $F_{.0}^C(y, \bar{s})$ for *compliers* (and thus quantile treatment effects for *compliers*) can be identified.

It can be shown (Imbens and Rubin [42]) that $F_{01} \equiv F_{.1}^{AT}$ and $F_{10} \equiv F.0^{NT}$, where $F_{.1}^{AT}$ denotes the distribution of Y^1 among *always takers* and $F.0^{NT}$ denotes the distribution of Y^0 among *never takers*.

Moreover, note that the proportion of *compliers* corresponds exactly with the (first stage) effect of the reform on education[20].

Finally, it is easy to show that:

$$F_{.1}^C(y, \bar{s}) - F_{.0}^C(y, \bar{s}) = \frac{F_{1.}(y, \bar{s}) - F_{0.}(y, \bar{s})}{\phi_c(\bar{s})} \tag{6}$$

where:
- $F_{1.}(y, \bar{s}) \equiv Prob[Y_i \leq y | S_i = \bar{s}, Z_i = 1]$
- $F_{0.}(y, \bar{s}) \equiv Prob[Y_i \leq y | S_i = \bar{s}, Z_i = 0]$
- $\phi_c(\bar{s})$ is the proportion of *compliers* at $\bar{s}$.

Equation (6) represents the causal effect of education at s on $F(y)$ for *compliers*. $F_{1.}(y, s) - F_{0.}(y, s)$ 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.

The identification strategy employed enables to control for the unobserved heterogeneity in decision to entry into motherhood and educational choices on the basis of random assignment but it is valid only for the subpopulation of *compliers* at $\bar{s}$. Notably, it does not require additional assumptions to retrieve quantile treatment effects.

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, one seeks to estimate: (i) $E[D_i | S_i, Z_i]$ in order

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[19]Thus, $Z_i = 1$ if $S_i \geq \bar{s}$ and $Z_i = 0$ if $S_i < \bar{s}$, where S_i represents the year of birth of individual i and $\bar{s}$ is the threshold year since the 1963 reform started to be effective.

[20]That is $\phi_c = E[D_i | S_i = \bar{s}, Z = 1] - E[D_i | S_i = \bar{s}, Z = 0]$.

to ascertain the size of the discontinuity in woman's schooling achievement resulting from the compliance with the 1963 reform on mandatory schooling in Italy; (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. To estimate these conditional expectations one can either use parametric or non parametric techniques.

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 [38] and Hahn, Todd and Van der Klaauw [39]). However, for relatively well-behaved conditional expectations, estimates based on local polynomials differ little from 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, conditional expectations of the fertility index Y and the treatment variable D by parsimonious global polynomial methods: more precisely, each conditional expectation will be smoothed by means of a polynomial in S and $Z \equiv 1(S \geq \bar{s})$ of appropriate degree of smoothness. Sensitivity of the parametric results to different smoothing techniques, specifically to the choice of the degree of smoothness, is checked and documented.

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: according to Brandolini and Cipollone [16, pp. 12], people potentially affected by the 1963 reform on mandatory schooling are those who in 1963 were less than 15 years old and without middle school degree, those who were between 6 and 14 years old in 1963, that is those born between 1949-1956; instead, Flabbi [34, pp. 13] claimed that the reform starts "to be effective on people born after the 1950". 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[21]. However, a disadvantage of using Census data compared to survey data is that Census do not usually collect information on a wide set of variables and are not readily available. Indeed, information from the Census data is not appropriate to examine the completed fertility of the cohorts of interests, namely 1938-1956: women of those cohorts were actually still too young in 1981 to infer their completed fertility

[21]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.

from the number of children they already have had at the time of the interview. Therefore, the analysis is limited on the causal effects of education on *tempo* fertility.

3.2.1 12th Census: the 2% Sample

The data used come from the 2% random sample drawn from 12th Census data for preliminary analysis (see ISTAT[29]). On the whole, data provide information on 1,118,570 individuals, 372,102 households.

The data contain single records for each individual in each household with information on his/her characteristics[22]. These informations allowed to link individuals in the same household and to match own-children to mothers within the household.

Data have been distributed by the National Institute of Statistic without the household identifying code, therefore an algorithm has been implemented to link individuals in the same household. The algorithm exploited the fact that data are ordered. To assess the success of the algorithm in locating individuals in households, characteristics of the resulting household sample and characteristics of the household sample constructed by ISTAT on the same data (see ISTAT[29]) have been compared. Results of the linkage are satisfactory (see Table 1).

The sample of analysis is restricted to individuals with Italian citizenship[23] born between 1938 and 1956, that is to 285,129 individuals (142,051 males and 143,078 females). The sample has been then further restricted to consider only individuals with Italian citizenship living in households with all Italian members.

Data on *potential* and *factual* mothers of cohorts 1938-1956 are extracted considering all the women of these cohorts within each household 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). Once children are matched to mothers, the calculation of mothers' age at birth of each children living in the household is straightforward, since information on the date of birth of each household member is available. Then, mother's age at birth of the oldest child (remaining at home) is considered, and 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

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[22]Date of birth, age, gender, education (highest level achieved by the time of the 1981 Census interview), labour market status, marital status, region of residence, citizenship, the individual's relationship to the householder.

[23]1,114,503 individuals, 99.6% of the original sample.

Table 1: Households Characteristics. Italy. 2% Sample of the 12th Census Data.

	HH's by Number of Members			HH's by Age of the Reference Person	
N	ISTAT[29] tables	Linked Sample	Age	ISTAT[29] tables	Linked Sample
1	66,421	66,421	20-24	2,618	2,618
2	88,440	88,442	25-29	13,684	13,684
3	82,035	82,033	30-34	27,236	27,236
4	79,781	79,780	35-39	28,711	28,711
5	35,471	35,471	40-44	32,658	32,658
6	12,483	12,484	45-49	30,911	30,911
> 7	7,471	7,471	50-54	28,985	28,985
			55-59	23,255	23,255
	HH's by Marital Status of the Reference Person			HH's by Number of Children Aged less than 6	
Mar. Status	ISTAT[29] tables	Linked Sample	N	ISTAT[29] tables	Linked Sample
Single	33,517	33,517	None	305,665	305,665
Married	273,219	273,219	1	52,206	52,206
Widowed	56,916	56,916	2	13,028	13,028
Leg. Sep.	6,113	6,113	>=3	1,203	1,203
Divorced	2,307	2,308			

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 might be upward biased, the bias being more serious 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 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[30, Table 2, pp. 82]) and Italian adolescents tend to leave parental home late[24], this is likely to hold in practice.

Mothers and children might be mismatched when the natural mother of

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[24]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[8, Tab. 3.8, pp. 96]).

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. Lastly, children and mothers might be mismatched if the children living in the household have been adopted. The number of households in which there is a woman aged between 20 and 50 who is neither the wife of the household head nor the household head and there are individuals in the household aged less than 18 who are not children of the household head is roughly 4,351, nearly 1% of all the households. So, 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%.

Mother's age at first birth is right-censored, since one can only observe births occurred up to the date of the interview. 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 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%.

4   Main Findings

The results are presented in two steps. Firstly, the impact of the 1963 reform on education is presented. The reform exerted an effect on the qualification level of women who in 1963 had just completed primary school, namely those of the cohorts 1949-1952, 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. Thus, for some fraction of individuals, being born just after the reform on mandatory schooling was introduced leads them to obtain more schooling than they otherwise would have. Estimates are robust to the choice of smoothing technique and to the choice of the degree of smoothness.
Secondly, the causal effects on maternal education on fertility decisions are considered. Findings based on Census data suggest that, a large fraction of the women affected by the reform tend to postpone the time of first birth but catch up the fertility delay before turning 26. There is some indication that the fertility returns on education among these women might be substantially different from the one of the average individual in the population: compared with women who do not comply with the reform, the *compliers* tend to have their first child earlier in the absence of the treatment and later in the

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presence of the treatment.

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

In this section, firstly, the measure of education exploited in this application is discussed and, secondly, the size of the effect of the 1963 mandatory school reform on education is assessed.

The Census data provide information only on the highest educational level achieved by the time of the Census interview. In principle, the 1963 reform could have affected the whole distribution of women's educational level. However, individuals affected by the 1963 reform are the peculiar subpopulation of those individuals who would not have completed junior high school if not compelled. This suggests that 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. Thus, the binary variable describing treatment status, D_i, is defined as follows: it takes the value 1 if individual i has *exactly* junior high school degree and 0 otherwise. The analysis is limited to women with at most junior high school degree[25].

A descriptive analysis, not reported here for brevity, 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.
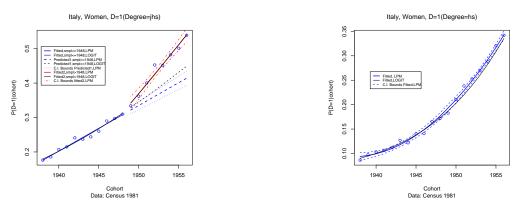
The empirical strategy followed to get estimates of $E[D_i|S_i = \bar{s}]$ helps to address this quandary: firstly, the evolution of the series $E[D_i|S_i]$ over time is smoothed using a polynomial of appropriate degree of smoothness; secondly, the information on the qualification level of individuals born up to the year 1948[26] 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.

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[25]The analysis has also been carried out using data of the whole sample of potential and factual mothers 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 2 and lead to consistent inferential conclusions. These estimates, not reported here for brevity, are available from the author upon request.

[26]No one born before the year 1948 could have been affected by the 1963 reform.

Figure 1: First Stage Effect of the 1963 Reform on Women's Schooling Achievement. 2% Sample of the 12th Census Data. *Potential* and *factual* mothers (i.e. women with Italian citizenship living in households with all Italian members whose age at first birth was either censored or greater than 15 years).

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

The motivation to consider this particular set of values of s is twofold: firstly, it is interesting to explore whether the 1963 reform had had different effects depending on the time elapsed since primary school completion: 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. Secondly, extrapolation becomes less plausible once one moves further from the threshold year $\bar{s} = 1949$.

Table 2 reports estimates of the proportion of *compliers*[27] $\phi_c(s)$ computed at different values of s for different degrees of the smoothness. The preferred specification[28] is written using bold characters.

The hypothesis that the effect is null ($H_0 : \phi_c(s) = 0 \ vs \ H_1 : \phi_c(s) \neq 0$) is tested using standard results on testing linear hypothesis in linear models and p-values corresponding to the test are reported in Table 2.

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

[28]Different specifications have been tested using standard test statistics for linear models and the more parsimonious model which adequately describes the data has been selected.

Table 2: First Stage Effect of the 1963 Reform on the proportion of Women who achieved *exactly* Junior High School Degree ($P(just)$) by the time of the 12th Census Interview. 2% Sample 12th Census Data. Sample of *potential* and *factual* mothers (i.e. 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

Smoothing Technique: Linear Probability Model

	$s = 1949$			$s = 1950$		
	mod1	**mod2**	**mod3**	**mod1**	**mod2**	**mod3**
$\widehat{\phi_c(s)}$	**0.02**	0.02	0.01	**0.03**	0.03	0.03
test	**5.08**	1.48	0.42	**16.11**	16.58	3.54
p-value	**0.04**	0.24	0.53	**0.00**	0.00	0.08

	$s = 1951$			$s = 1952$		
	mod1	**mod2**	**mod3**	**mod1**	**mod2**	**mod3**
$\widehat{\phi_c(s)}$	**0.05**	0.05	0.05	**0.06**	0.07	0.06
test	**31.36**	37.36	5.56	**47.15**	53.56	6.02
p-value	**0.00**	0.00	0.04	**0.00**	0.00	0.03

Smoothing Technique: Logit Model

	$s = 1949$			$s = 1950$		
	mod1	**mod2**	**mod3**	**mod1**	**mod2**	**mod3**
$\widehat{\phi_c(s)}$	**0.01**	0.01	0.01	**0.02**	0.02	0.03
test[*]	**5.83**	0.65	1.26	**17.26**	15.68	8.21
p-value	**0.02**	0.42	0.26	**0.00**	0.00	0.01

	$s = 1951$			$s = 1952$		
	mod1	**mod2**	**mod3**	**mod1**	**mod2**	**mod3**
$\widehat{\phi_c(s)}$	**0.04**	0.04	0.05	**0.05**	0.05	0.07
test[*]	**32.45**	38.00	12.29	**47.73**	54.40	13.14
p-value	**0.00**	0.00	0.00	**0.00**	0.00	0.00

mod1 linear trend on 1938-1948 cohorts, linear trend on 1948-1956 cohorts; **mod2** linear trend on 1938-1948 cohorts, quadratic trend on 1948-1956 cohorts; **mod3** quadratic trend on 1938-1948, quadratic trend on 1948-1956 cohorts. Estimates under the preferred specification are reported using bold characters. The hypothesis tested by test[*] is a necessary and sufficient condition for the null hypothesis $H_0 : \phi_c(s) = 0$ *vs* $H_1 : \phi_c(s) \neq 0$.

Table 3: Effect of the 1963 reform on the proportion of Women who achieved *exactly* High School Degree ($P(hs)$) by the time of the 12th Census Interview. 2% Sample of the 12th Census data. Sample of *potential* and *factual* mothers (i.e. women living in households with all Italian members whose age at first birth was either censored or greater than 15 years) with at most High School Degree.

Overall Sample Size: 165,018. Average Cohort Sample Size: 7,175

Smoothing Technique: Linear Probability Model

	$s = 1949$			$s = 1950$		
	mod.a	**mod.b**	**mod.c**	**mod.a**	**mod.b**	**mod.c**
$\widehat{\phi_c(s)}$	0.01	0.00	**0.00**	0.03	0.01	**0.00**
test[*]	4.47	0.20	**0.13**	23.00	1.57	**0.13**
p-value	0.05	0.66	**0.90**	0.00	0.23	**0.90**

	$s = 1951$			$s = 1952$		
	mod.a	**mod.b**	**mod.c**	**mod.a**	**mod.b**	**mod.c**
$\widehat{\phi_c(s)}$	0.04	0.02	**0.00**	0.05	0.03	**0.00**
test[*]	46.04	2.53	**0.13**	66.95	2.86	**0.13**
p-value	0.00	0.14	**0.90**	0.00	0.11	**0.90**

Smoothing Technique: Logit Model

	$s = 1949$			$s = 1950$		
	mod.a	**mod.b**	**mod.c**	**mod.a**	**mod.b**	**mod.c**
$\widehat{\phi_c(s)}$	0.01	0.00	**0.01**	0.02	0.01	**0.01**
test[*]	2.65	0.11	**1.50**	0.85	14.06	**1.50**
p-value	0.1	0.74	**0.13**	0.36	0.00	**0.13**

	$s = 1951$			$s = 1952$		
	mod.a	**mod.b**	**mod.c**	**mod.a**	**mod.b**	**mod.c**
$\widehat{\phi_c(s)}$	0.03	0.01	**0.01**	0.04	0.02	**0.01**
test[*]	26.86	1.14	**1.50**	36.09	1.03	**1.50**
p-value	0.00	0.29	**0.13**	0.00	0.31	**0.13**

mod.a linear trend on 1938-1948 cohorts, quadratic trend on 1948-1956 cohorts; **mod.b** quadratic trend on 1938-1948 cohorts, quadratic trend on 1948-1956 cohorts; **mod.c** common quadratic trend on 1938-1948 and on 1948-1956 cohorts. Estimates under the preferred specification are reported using bold characters. The hypothesis tested by test[*] is a necessary and sufficient condition for the null hypothesis $H_0 : \phi_c(s) = 0$ *vs* $H_1 : \phi_c(s) \neq 0$.

Figures reported in Table 2 suggest that the proportion of *compliers* $\phi_c(s)$ increases as one moves s closer to 1952, regardless the specific smoothing technique employed and the degree of smoothness used. The observed pattern in the magnitude of the proportion of *compliers* is consistent with the following story: women who were 14 at the time of the 1963 reform, i.e. 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. The inspection of figures in Table 3 and the graph in the right-hand panel of Figure 1 suggest that there is no effect of the 1963 reform on the proportion of women who achieve high school degree.

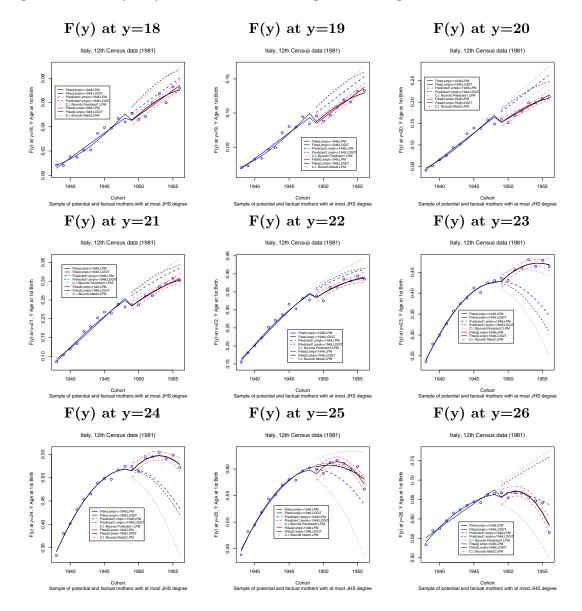## 4.2   The Effect of Education on the Timing of Births

The analysis carried out in the previous section suggests that the 1963 reform lead to nearly 6% increase in the proportion of individuals who achieve junior high school qualification. 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 $y \in [18, 26]$ for the sample of *potential* and *factual* mothers with at most junior high school degree. Each graph shows a marked increasing trend[29]. This counter-intuitive tendency is due to the fact that graphs actually represent the probability that a woman 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

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[29]The same pattern is observed considering the sample of all *potential* and *factual* mothers. Graphs, not reported for brevity, are available from the author upon request.

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 12th Census data. Sample of *potential* and *factual* mothers (i.e. 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.

F(y) at y=18 **F(y) at y=19** **F(y) at y=20**

F(y) at y=21 **F(y) at y=22** **F(y) at y=23**

F(y) at y=24 **F(y) at y=25** **F(y) at y=26**

descriptive evidence supporting this prediction. Point estimates[30] of the discontinuity (intention-to-treat effects) based on the sample of *potential* and *factual* mothers with at most junior high school degree, not reported here for brevity, suggest that women born between 1950 and 1952 have a reduced likelihood of nearly 3% of bearing their first child by the age $y =$19, 20, 21. There is some evidence of a positive effect of the reform on the probability of bearing the first child by the age $y =$23 and 24, whereas estimates of the effects at older ages, namely ages 25 and 26, are not statistically distinct from zero. The results are broadly robust with respect to the choice of the parametric smoothing technique[31].

Intention-to-treat effects seem rather stable over the different cohorts of women affected by the 1963 reform, i.e. across different values of s.

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 by younger ages (19, 20, 21), (iii) negligible effects of giving births at older ages (25, 26). There is some indication that women delay early first births, then anticipate first births around age 23, 24 and then catch up with the fertility delay before turning 26.

Nonetheless, a reduced form analysis does not provide insights on the magnitude of the causal effects of education on fertility. To address this, the Wald estimator (see equation (6)) is computed: these estimates, reported in Table 4, are simply the ratio of the reduced-form estimates (intention-to-treat effect estimates to first-stage effect estimate). Standard errors are calculated using the delta method[32].

Estimates suggest that a large fraction of the women affected by the reform postpone the transition to motherhood due to the higher qualification achieved.

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 by young ages, namely by the age $y =$19, 20, 21. There is some evidence that these women then (by the age $y = $23, 24) anticipate first

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[30]Estimates were computed following the same empirical strategy exploited to get the first stage effect estimates. Sensitivity of the estimates to the degree of smoothness has been explored: inferential conclusions are generally robust to choice of the degree of the smoothing polynomial. Estimates have been computed only at the values of s at which the first stage effect of the 1963 reform was significantly different from zero according to tests performed at 1%significance level.

[31]The same analysis has been carried out using the whole sample of *potential* and *factual* mothers and defining treated individuals those women with *at least* junior high school degree. Estimates of the intention-to-treat effects, not reported here for brevity but available from the author upon request, do not differ from those based on the sample of potential and factual mothers with at most junior high school degree.

[32]Results based on data of the whole sample of potential and factual mothers, not reported for brevity but available from the author upon request, are broadly consistent with those presented in Table 4.

births and catch up with the fertility delay later in their fertile lifespan (there seems to be no effect of education on the timing of the transition to motherhood at older ages, i.e. by the age $y = 25, 26$).
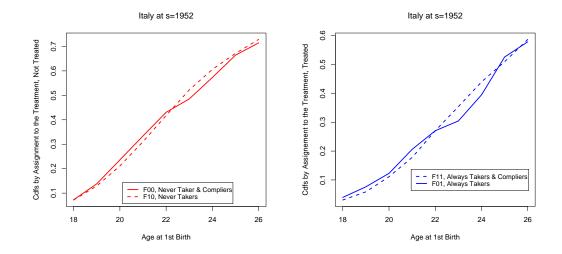
Table 4: Causal Effect of Education on the Timing of First Births for *compliers* at s ($LATE$). 2% Sample of the 12th Census data. Sample of *potential* and *factual* mothers (i.e. women living in households with all Italian members whose age at first birth was either censored or greater than 15 years) with at most Junior High School Degree.

	$s = 1950$		$s = 1951$		$s = 1952$	
	$LATE$	st.err.	$LATE$	st.err.	$LATE$	st.err.
$F(y)at$ $y = 18$	-0.17	0.12	-0.12	0.08	-0.09	0.06
$F(y)at$ $y = 19$	-0.46	0.20	-0.32	0.13	-0.25	0.10
$F(y)at$ $y = 20$	-0.79	0.27	-0.62	0.18	-0.52	0.13
$F(y)at$ $y = 21$	-0.78	0.30	-0.55	0.19	-0.42	0.14
$F(y)at$ $y = 22$	-0.60	0.31	-0.42	0.21	-0.32	0.16
$F(y)at$ $y = 23$	0.48	0.38	0.68	0.35	0.79	0.34
$F(y)at$ $y = 24$	0.36	0.39	0.58	0.36	0.68	0.35
$F(y)at$ $y = 25$	-0.10	0.27	-0.07	(0.19)	-0.06	0.14
$F(y)at$ $y = 26$	0.01	0.53	0.12	0.48	0.08	0.47

One could also consider the distribution of *compliers*' potential outcomes. The four distributions used to estimate the compliers' potential outcomes' distributions in equations (1) and (2) are illustrated (at $s = 1952$) in Figure 3: the small differences between the directly estimable cumulative distribution functions reflect the fact that the instrument weakly affects D, the treatment of interest. The same holds at $s = 1950$ and $s = 1951$. Actually, the proportion of *compliers* is nearly 0.03 at $s = 1950$, around 0.05 at $s = 1951$, almost 0.06 at $s = 1952$.

The estimates of the *compliers* potential outcomes' distribution resulting from equations (1) and (2) are not everywhere monotonically increasing,

Figure 3: Distribution of Woman's Age at First Birth, by Instrument and Treatment Status. 2% Sample of the 12th Census data. Sample of all *potential* and *factual* mothers (i.e. 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.
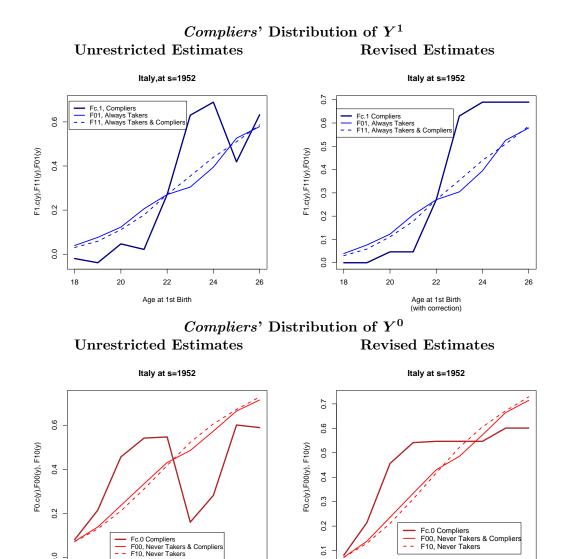

neither nonnegative (see graphs[33] on the left in Figure 4). Imbens and Rubin[42] firstly highlighted this drawback: the unrestricted estimates of the compliers' potential outcomes density functions they considered in their application were not everywhere nonnegative. The authors underlined that the non-negativity of density functions might result from sampling variation as well as from violations of the assumptions. In this application, as it will become clearer after the discussion on the internal validity of the research design (section 5), the main reason driving the non-monotonicity of the unrestricted estimates of the *compliers'* cumulative distribution functions is sample variation.

Imbens and Rubin[42] considered alternative 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. They also noted that , even *naive* correction, can change inference considerably.

The alternative estimator considered for the compliers' cumulative distribu-

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[33]For the sake of conciseness, only graphs referring to the case $s = 1952$ are reported. Graphs referring to the case $s = 1950$ and $s = 1951$ show a similar pattern. These additional graphs are available upon request from the author.

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)). 2% Sample of the 12th Census data. Sample of all *potential* and *factual* mothers (i.e. 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.

Compliers' Distribution of Y^1

Unrestricted Estimates   **Revised Estimates**

Compliers' Distribution of Y^0

Unrestricted Estimates   **Revised Estimates**

tion functions is the the following *naive* estimator:

$$\widehat{F_{.1}^{C}(y,s)}^{*} = \max\left(0, \widehat{F_{.1}^{C}(y,s)}, F_{.1}^{C}\widehat{(y-1},s)^{*}\right),$$

$$\widehat{F_{.0}^{C}(y,s)}^{*} = \max\left(0, \widehat{F_{.0}^{C}(y,s)}, F_{.0}^{C}\widehat{(y-1},s)^{*}\right)$$

Unrestricted and "revised" estimates are presented on the left-hand panel in Figure 5.

As it is clear from the inspection of the graphs in Figure 5, "revised" estimates of the cumulative distribution functions of *compliers* are not able to reproduce the reversal of the local average treatment effect observed at the age 23 at $s = 1950$, that is when the proportion of *compliers* is lowest ($\widehat{\phi}_c = 0.03$). Instead, the reversal is correctly reproduced as the proportion of *compliers* increases, i.e. at $s = 1951$ ($\widehat{\phi}_c = 0.05$), $s = 1952$ ($\widehat{\phi}_c = 0.06$).

Pointwise estimates of the vertical distance between the "revised" estimates of *compliers'* potential outcome distribution functions, i.e. $\widehat{F_{.1}^{C}(y,s)}^{*} - \widehat{F_{.0}^{C}(y,s)}^{*}$, are presented in Table 5, together with the corresponding unrestricted estimates[34], i.e. $\widehat{F_{.1}^{C}(y,s)} - \widehat{F_{.0}^{C}(y,s)}$, and estimates of the proportions of *always takers* $(\widehat{\phi}_a)$ and *never takers* $(\widehat{\phi}_n)$.

Revised and unrestricted estimates of the causal effect of education on the proportion of women who raise their first child by the ages $18 - 22$ are broadly consistent. Both revised and unrestricted estimates show a reversal in the effect at $y = 23$. However, revised estimates suggest that the effect at older ages is stable and much smaller than the one the corresponding unrestricted estimate depict.

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[34]These estimates might slightly differ from estimates already reported in Table 4 because of sample variation: unrestricted estimates in Table 4 are computed contrasting pointwise estimates of the two cumulative distribution function by assignment to the treatment Z and re-weighting these difference by the proportion of *compliers* (see equation (6)), whereas calculations of the unrestricted estimates reported in Table 5 are made moving from the weighted average of the pointwise estimates of four cumulative distribution functions, that is the cumulative distribution functions of the outcome by treatment and assignment to the treatment (see equations (1) and (2)). Unrestricted estimates of Table 4 and 5 are broadly consistent.

Table 5: Causal Effect of Education on the Timing of First Births for *compliers* at s ($LATE$). "Revised" and Unrestricted Estimates. Summary. 2% Sample of the 12th Census data. Sample of all *potential* and *factual* mothers (i.e. 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.

	$s = 1950$		$s = 1951$		$s = 1952$	
	$\widehat{\phi_a}$	$\widehat{\phi_n}$	$\widehat{\phi_a}$	$\widehat{\phi_n}$	$\widehat{\phi_a}$	$\widehat{\phi_n}$
	0.335	0.630	0.348	0.602	0.361	0.573
$F(y)at$	unr.	rev.	unr.	rev.	unr.	rev.
$y = 18$	-0.14	-0.08	-0.12	-0.08	-0.10	-0.08
$y = 19$	-0.40	-0.29	-0.31	-0.24	-0.25	-0.21
$y = 20$	-0.69	-0.64	-0.51	-0.51	-0.41	-0.41
$y = 21$	-0.85	-0.74	-0.64	-0.59	-0.52	-0.49
$y = 22$	-0.39	-0.48	-0.32	-0.35	-0.28	-0.28
$y = 23$	0.44	-0.19	0.66	0.08	0.47	0.08
$y = 24$	0.33	-0.16	0.58	0.14	0.41	0.14
$y = 25$	-0.19	-0.16	-0.18	0.14	-0.18	0.09
$y = 26$	-0.02	-0.16	-0.10	0.14	0.04	0.09

Revised estimates (REV.) are computed as $\widehat{F_{.1}^C(y,s)}^* - \widehat{F_{.0}^C(y,s)}^*$, where $\widehat{F_{.1}^C(\cdot,s)}^*$, $\widehat{F_{.0}^C(\cdot,s)}^*$ represent the "revised" estimates of the *compliers'* potential outcome distribution functions. Unrestricted estimates are obtained as $\widehat{F_{.1}^C(y,s)} - \widehat{F_{.0}^C(y,s)}$, $\widehat{F_{.1}^C(\cdot,s)}$, $\widehat{F_{.0}^C(\cdot,s)}$ represent the unrestricted estimates of the *compliers'* potential outcome distribution functions. These estimates might differ a little from those presented in Table 4 because of sample variation.

Graphs 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, i.e. the mother's age at first birth if she would achieve high qualification, 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 $y = 24$ 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 gap between the *compliers'* and *always takers'* cumulative distribution functions of Y^1 over the interval $y \in [18, 21]$ reduces as the proportion of *compliers* increases whereas the gap between the two distribution function over the interval $[22, 24]$ rises slightly.

The differences between the cumulative distribution function of Y^0, i.e. the mother's age at first birth if she would achieve low qualification 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*. The gap between the *compliers'* and *never takers'* cumulative distribution functions of Y^0 over the support $[18, 26]$ slightly reduces as the proportion of *compliers* increases, reversing its sign at higher values of y, namely $y = 25$, $y = 26$, at $s = 1951$, $s = 1952$.

On the whole, it seems that, at all points[35] 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 $y = 25$, $y = 26$.

Empirical quantiles of the *compliers'* potential outcomes' distributions have been computed, moving from the "revised" estimates: due to censoring in the observed distribution by treatment and assignment to the treatment, higher quantiles of the *compliers'* potential outcomes' distribution cannot be identified. Some of the identifiable quantiles, namely the first six deciles, and corresponding quantile treatment effects for the subpopulation of *compliers* are reported in Table 6: figures suggest heterogeneity of the impact

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[35]Additional graphs show the same pattern observed at s=1952 and are not reported for brevity. However, they are available upon request from the author.
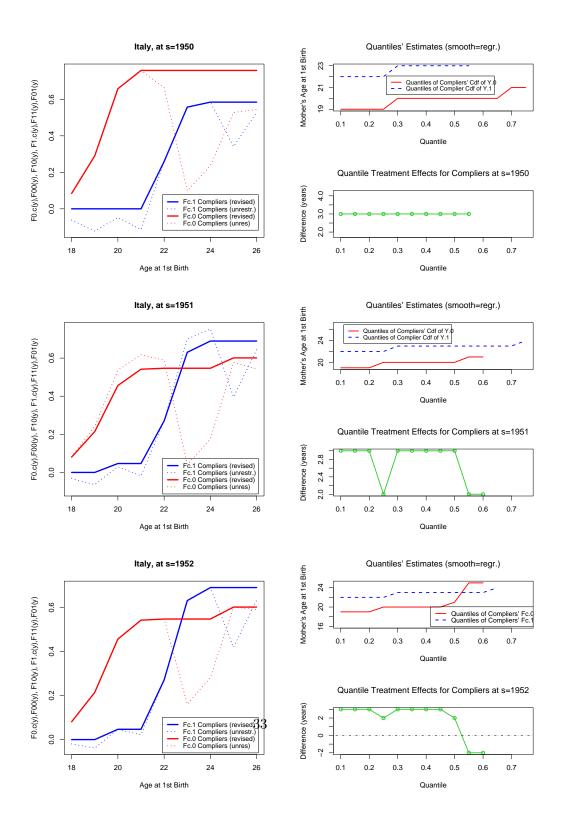
of education over the distribution of births.

Table 6: Quantile Treatment Effect of Education on the Timing of First Births for *compliers* at s ($IV - QTE$), selected quantiles q. 2% Sample of the 12th Census data. Sample of all *potential* and *factual* mothers (i.e. 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.

quantile q	$q.1$ (Y^1)	$q.0$ (Y^0)	$q.1 - q.0$	$q.1$ (Y^1)	$q.0$ (Y^0)	$q.1 - q.0$	$q.1$ (Y^1)	$q.0$ (Y^0)	$q.1 - q.0$
		$s = 1950$			$s = 1951$			$s = 1952$	
$q = 0.1$	22	19	3	22	19	3	22	19	3
$q = 0.2$	22	19	3	22	19	3	22	19	3
$q = 0.3$	23	20	3	23	20	3	23	20	3
$q = 0.4$	23	20	3	23	20	3	23	20	3
$q = 0.5$	23	20	3	23	20	3	23	21	2
$q = 0.6$	-	20	-	23	21	2	23	25	-2

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[36], 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 bear their first child by the age 19, 20, 21 and 22 years decreases. However, they later anticipate first births and catch up the fertility delay before turning 26: indeed, the number of women who experience motherhood by the age 23 or 24 increases as schooling achievement increases and there seems to be no effect of the education on the proportion of women who experience motherhood by older ages, namely by the age 25, 26.

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[36]Brandolini and Cipollone [16] exploited the 1963 reform as instrument to assess the returns 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%.

Figure 5: *Compliers*' Potential Outcomes' Cumulative Distributions functions, quantile and quantile treatment effects. 2% Sample of the 12th Census data. Sample of all *potential* and *factual* mothers (i.e. 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.

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 return to schooling of 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. Generalizing this effect to a wider set of individuals requires typically relying on stronger conditions than those who guarantee local identification. Angrist [3] proposed a set of assumptions which allow to link average treatment effect and local average treatment effect. In further research, attempts will be made to link these results more closely to a theoretical framework so that the generalization of the effect from *compliers* to a wider sets of individuals can be easily pursued.

An important issue remains unresolved: the internal validity of the research design, which is a precondition for the interpretation of the results. This issue is extensively discussed in the next section. Evidence is provided supporting the identifying assumption that, local to the cohorts of women born in the years when the reform started to be effective, the precise timing of motherhood is effectively random, giving credence to the causal effects estimates discussed so far.

5 The Internal Validity of the Research Design: A Discussion

In this section evidence is provided to ensure that the research design exploited manipulates only maternal education, so that results have a causal interpretation.
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.
This comment applies in particular to the local monotonicity condition.
Violation of the monotonicity assumption might lead to severe consequences in this application, since the instrument (the 1963 reform) weakly affects the treatment status (Angrist, Imbens and Rubin[5]). Nonetheless, 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 on compulsory schooling 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.

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

Finally, a case can be made on the violation of the local continuity assumption: if **the same cohorts** of women affected by the 1963 reform have also been affected by other treatments, the identification strategy proposed to identify the causal effect of education on fertility is no longer valid, even for the small sub-population of *compliers*. The assumption of local continuity requires that any variable determined prior to the assignment to the treatment is independent of treatment status. Thus, the local continuity assumption can be tested examining if any pre-treatment variable W has a smooth conditional distribution (given S) around $\bar{s}$ (Lee[45]). Similarly, one would expect that the fertility behaviour of women not affected by the 1963 reform changes smoothly over cohorts. To test the validity of the local continuity assumption, fertility of women of the 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 and Table 3), 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 7 and the graphs in Figure 6. As already mentioned, since estimates are far from being precise, this result might not be conclusive, nonetheless it provides some evidence supporting the validity of the local independence assumption.

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[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.

Figure 6: $F(y) = Prob[Y_i \leq y]$, Y Woman's Age at First Birth at distinct values of y. Italy. 2% Sample of the 12th Census Data. Sample of Women with High School Degree.

F(y) at y=20

F(y) at y=21

F(y) at y=22

F(y) at y=23

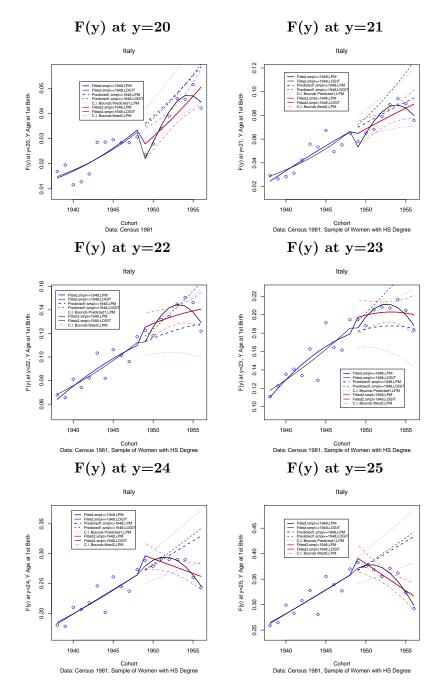**F(y) at y=24**

F(y) at y=25

36

Table 7: 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. Italy ($Prob[Y_i \leq y]-$). 2% Sample of the 12th Census Data. Sample of *potential* and *factual* mothers (i.e. 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

Smoothing Technique: Linear Probability Model

y	20	21	22	23	24	25	26
				1949			
effect	-0.01	-0.01	0.01	0.02	0.02	0.02	0.01
test	1.75	-0.66	1.32	1.10	2.76	1.87	0.07
p-value	0.10	0.52	0.21	0.29	0.12	0.19	0.80
				1950			
effect	-0.01	-0.01	0.01	0.02	0.01	0.00	0.01
test	1.75	-0.66	1.32	1.10	0.59	0.04	0.16
p-value	0.10	0.52	0.21	0.29	0.45	0.89	0.70
				1951			
effect	-0.01	-0.01	0.01	0.02	0.00	-0.02	0.00
test	1.75	-0.66	1.32	1.10	0.02	0.89	0.00
p-value	0.10	0.52	0.21	0.29	0.89	0.36	0.96
				1952			
effect	-0.01	-0.01	0.01	0.02	-0.02	-0.04	-0.02
test	1.75	-0.66	1.32	1.10	0.96	3.88	0.83
p-value	0.10	0.52	0.21	0.29	0.34	0.07	0.38

Smoothing Technique: Logit Model

y	20	21	22	23	24	25	26
				1949			
effect	-0.01	-0.02	0.00	-0.01	0.01	0.00	0.00
test*	6.62	6.24	0.26	0.43	0.16	0.05	0.03
p-value	0.01	0.01	0.61	0.51	0.69	0.83	0.86
				1950			
effect	-0.01	-0.01	0.00	0.00	0.01	0.00	0.00
test*	3.24	3.10	0.04	0.14	0.24	0.01	0.06
p-value	0.07	0.08	0.84	0.71	0.62	0.94	0.80
				1951			
effect	-0.01	-0.01	0.01	0.00	0.01	-0.01	0.00
test*	-0.93	-1.19	0.30	0.13	0.01	0.61	0.02
p-value	0.34	0.27	0.58	0.71	0.92	0.44	0.88
				1952			
effect	0.00	-0.01	0.01	-0.01	-0.01	-0.03	-0.02
test*	0.21	-0.67	0.25	0.47	0.29	2.95	1.52
p-value	0.65	0.41	0.62	0.53	0.59	0.09	0.22

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 hypothesis tested by test* is a necessary and sufficient condition for the effect to be null.

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 could be questioned. Note that for the result on identification to be valid, one does not need that none of these changes have affected the women behaviour, nor to maintain that they have not occurred, but just that they affected women around 1949 in a similar way. In particular, it is crucial that the discontinuity in the series $F_s(y)$[39] (as a function of s) is fully attributable to the effect of the 1963 reform and it is not driven by the mentioned innovations.

If the introduction of any of these innovations affected fertility, one would observe a discontinuity in the series $F_s(y)$ (as a function of s) at the point s corresponding to the cohort who has firstly been affected. Note that the position of the discontinuity is expected to differ at different values of s.

The main features of these innovations are presented below and arguments suggesting that the discontinuity observed around $s = 1949$ has not been due to their impact are discussed.

Law N. 898, December 1, 1970 on divorce: the newly introduced law allowed to men and women who experience a legal separation to remarry. Before 1970, in Italy, marriage was considered an indissoluble bond. The 1970 law might have affected the decision to marry and to have children[40]. However, , in the last decades, Italy has not experienced the massive increase in the number of divorces which is apparent in some other European countries and the family structure has not yet substantially changed: still nowadays the most common model of living together is marriage and divorce and cohabitation are not widespread practice (see Castiglioni[20], De Sandre et al.[28],[27]). It seems that the link between country-specific cultural factors and the family structure is strong and slowly evolving over time.

The fertility behaviour (between the age 18 and 26) of women born between 1944 and 1952[41] might, in principle, have been affected by the law $N. 898$. Women of the cohort 1944 were 26 years old in 1970; thus if the law exerted any effect on their fertility behaviour, one would observe a discontinuity examining the graph of $F_s(y)$ at $y = 26$ in Figure 2. However, the series seems to evolve smoothly over cohorts around

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

[40]In Italy, the rate of out of wedlock births is extremely low. Thus, one can claim that the decision to marry might also have consequences on fertility decisions.

[41]Younger cohorts were less than 18 at the time the law $N. 898$ has been introduced.

the points $s = 1944, s = 1945$. Similarly, there seems to be no evidence supporting the presence of a discontinuity at $s = 1945$ as one examines the graph of $F_s(y)$ at $y = 25$, or the presence of a discontinuity at $s = 1946$ in the graph of $F_s(y)$ at $y = 24$. As one examines the graph of $F_s(y)$ at $y = 23$ the observed series seems to evolve smoothly between at $s = 1947, s = 1948$; similarly, as one considers $F_s(y)$ at $y = 20$, the series seems to evenly increase between $s = 1950$ and $s = 1951$. At younger ages, the pattern of the series is more "noisy", therefore no attempt is made to include also those graphs in the analysis. On the whole, it seems that basically the law $N.$ 898 did not exert effects on the individuals fertility behaviour of such magnitude that it could be meaningful in explaining the discontinuity which has been attributed to the effect of the 1963 reform on fertility.

Law N. 39, March 8,1975 : due to the law $N.$ 39 , individuals become of age at the age 18, whereas until 1975 the threshold age was 21. Thus, from 1975 onwards, individuals could vote and become legally responsible for themselves 3 years before what stated the older law. Cazzola[22, pp. 313] suggests that the age at which individuals become of age is strongly linked to the age at which individuals experience their first sexual intercourse, since individuals generally feel it is appropriate to experience sexual intercourse (an "adult matter") since that age in particular.

Individuals who might have been affected by the decrease in the threshold age, and thus might have anticipate their first sexual intercourse, are mainly individuals who turned 18 around 1975, that is individuals born around 1957. However, women belonging to the cohorts 1948-1956 were already of age at the time this change occurred, therefore it is claimed the decrease in the threshold age did not affect the behaviour of any of them.

Law N. 194, May 22, 1978 & Availability of the Pill : the newly introduced law $N.$ 194 legalized abortion in Italy[42]. By virtue of the law any woman can request an abortion within the first 90 days of pregnancy[43]. The request for abortion might be connected to mothers' health (both physical and mental) or children' health (including the possibility of malformation), to the specific circumstances in which conception was brought about and/or it might be grounded on the woman specific economic or social conditions (lack of means). After the introduction of the Law $N.$ 194, Italy firstly experienced an increase in the number of induced abortions (1980-1984), caused

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[42]In 1981, a referendum to abrogate the law was held and almost 70% of the population voted against the abrogation.

[43]After this period, in cases when the continuation of the pregnancy would seriously endanger the woman's health, an abortion is still possible.

both by the progressive improvement of the statistics (Buratta and Boccuzzo[18]) and the replacement of some illegal abortion by legal operation; then, the number of abortions steady and consistently declined until 1995, remaining rather stable around 138,000-140,000 in the following years. Boccuzzo and Loghi[14] and Boccuzzo and Buratta[18] underlined similarities between Italy and the other European countries where the abortion has been legalized: almost all countries experienced a shift from a regime where abortion was mainly requested by married women (just after the legalization) to a regime where abortion is mainly requested by women who are not married. It is claimed that the legalization of abortion in Italy has not significantly changed the opinion of individuals towards abortion as a practical mean of fertility control, but it has eventually[44] succeeded in reducing the number of illegal abortions: women still continue to consider carefully this opportunity and request abortion mainly when the pregnancy is expected to endanger the woman's health[45]. Indeed, the issue on abortion is connected to a number of ethical and cultural issues and debate on the topic is still not concluded after almost thirty years the law has been in force.

Cazzola[21, pp. 442] pointed out that favourable opinions on abortion seems to be associated with higher socio-economic conditions and higher qualifications and with favourable opinion on consensual unions as well.

The contraceptive pill was developed in 1956 by the American Dr. Gregory Goodwin Pincus and it was introduced in the U.S. in 1960 and in 1961 in Europe. Oral contraceptives have been firstly available in Italy in 1965. However, both the use of contraceptives and the circulating of information on contraceptives were forbidden in Italy (art. 553 *Codice Penale*) until 1978[46]. Between 1965 and 1978, the availability of the pill was highly restricted: it was considered a drug, not an effective contraceptive method. Then, the Law *N.* 194 officially repealed the norm together with those which used to forbid abortion. Although the proportion of women who uses oral contraceptives has been increasing, still, in 2000 less than 20% of the Italian women between 15 and 44 years old uses the pill as current contraceptive method, placing Italy

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[44]The assessment of the impact of the Law deserve a specific analysis which is not attempted here.

[45]Cazzola [21], using survey data on fertility in Italy, shows that, in 1995, women basically agree to abortion in cases when the pregnancy would seriously endanger the woman's heatlh but generally do not have a favourable opinion on abortion when it is used to avoid unexpected pregnancies.

[46]Actually, already in 1971 the Constitutional Court stated that the norm was not constitutionally lawful (Act March 24, 1971 n. 49).

among the European countries with the lowest levels of oral contraceptives' use (France, Spain, United Kingdom).

Contrasting data of two distinct survey on fertility behaviour held in Italy in 1979 and 1995, Bonarini[15] presented evidence of a marked change in the characteristics of the contraceptive methods used, consistent with a shift from the less to the more effective means of contraception: the rate of use of *coitus interruptus* decreased from 51% in 1979 to 17% in 1995, whereas the rate of use of the pill increased from 14% to 21% between the same years and the rate of use of IUD (Intra Uterine Device) almost doubled (3% in 1979, 7% in 1995). Nonetheless, the overall rate of contraceptive use remained quite stable between the two surveys and in 1995 the use of natural methods of contraception widespread among young women (33%). Bonarini[15, pp. 404] also highlighted that the non-use of contraception, as well as the rate of use of less effective means of contraception, decreases as education level increases.

The fertility behaviour of women of the cohorts 1952-1956 might in principle have been affected by the law N. 194 and the availability of oral contraceptives. A discontinuity in the series $F_s(y)$ at $y = 25$ at $s = 1953$ or in the series $F_s(y)$ at $y = 24$ at $s = 1954$ or in the series $F_s(y)$ at $y = 23$ at $s = 1955$ might be related to the effect of the law N. 194. The data at hand, however, do not provide evidence suggesting that such a discontinuity exists or that it might have been the main factor driving the discontinuity observed in the series $F_s(y)$ around $s = 1948, s = 1949$.

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 births for *compliers*.

6 Concluding Remarks

In this paper, evidence supporting the role of education in determining the timing of first births has been provided. Evidence is based on the comparison of the likelihood that a woman bears her first child by a given age between women born around 1949. 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 rolled out nationwide in Italy in the early 1960s, 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 by the ages y=19, 20, 21, whereas they have a higher likelihood to bear

their first child by the age $y = 23$. No evidence is found of a causal effect of education on the probability of bearing the first child at older ages (by the age $y = 24, 25, 26$).

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. However, 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. The internal validity of the research design is extensively discussed, explicitly considering also these factors: 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 returns to schooling of the women affected by the reform are 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 to rely on stronger conditions than those who guarantee local identification. In further research, attempts will be made to link these results more closely to a theoretical framework so that the generalization of the effect from *compliers* to a wider sets of individuals can be easily pursued.

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 a wider population.

Nonetheless, the subpopulation of *compliers* might be *per se* and interesting sub-population, 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.

Another caution in interpreting these results should be mentioned: whether changes in education produce similar effects regardless of the level at which additional education is obtained has not yet been assessed and, similarly, neither which level of female schooling has the greatest or the smallest effect on fertility. Thus, 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*).

Finally, findings are consistent with previous results by Bloemen and Kalwij [10] for the Netherlands, whereas they are not fully consistent with previous findings by Bratti [17]. This might be for mainly three reasons: firstly, Bratti focuses on marital fertility, whereas this application does not restrict the analysis to married women; secondly Bratti considers a period measure of fertility, conversely here the analysis is based on cohort measures of fertility; lastly, Bratti [17] considers the effects of education on the probability of *a birth event*[47], whereas here the analysis is focused on the timing of first birth.

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