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 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 Italian Census (1981, 1991) 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

∗This paper is based on my PhD thesis at the University of Padova. I am grateful to my supervisor E. Rettore, E. Battistin, M. Bratti, J. Ermisch, M. Francesconi, E. Del Bono, A. Datar, J. Angrist, F. Mealli, F. Billari for useful discussions. This paper has also benefited from comments of participants in seminars at the University of Padova, LABORatorio Revelli, at the Institute of Social and Economic Research (ISER), at the International Conference on “Postponement of Childbearing in Europe”, at the IVth “Brucchi Luchino” Labour Economics Workshop, at the 2006 Population Association of America Annual conference, at the NBER Education and Children’s Program Joint Meeting, at the workshop “The Evaluation of Labour Market, Welfare and Firm Incentives Programmes” and at the V/Ith “Villa Mondragone Workshop in Economic Theory and Econometrics”. Financial support from MIUR to the project “Evaluating the effects of labour market polices and incentives to firms and welfare policies: methodological issues and case studies” is gratefully acknowledged. I also gratefully acknowledge financial support from the Vienna Institute of Demography (VID), the National Bureau of Economic Research (NBER) and University of Rome - Tor Vergata to attend workshops in Vienna, Boston and Rome, respectively. An earlier version of the paper appeared in the ISER working paper series (WP 20) and in the research group working paper series (WP 69). Hospitality from the Dept. of Statistics, University of Padova and ISER (Colchester) is also gratefully acknowledged. The usual disclaimer applies.

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before turning 26. Conversely, causal effects of education on the timing of marriage are generally negligible. There is some indication that the fertility behaviour of these women is different from the one of the average women in the population.

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

## 1 Introduction and Motivation of the Paper

This paper assesses the causal effects of education on the timing of first births in Italy by exploiting a mandatory school reform rolled out nationwide in 1963. Italy was in the early 1990s one of the first countries to attain and sustain the lowest-low fertility levels\(^1\) (Kohler et al. [2002]). Similarly, in the last decades, other European countries have experienced both decline in fertility and motherhood postponement (Gustafsson [2003]). In the late XX century, most of these countries carried out major educational reforms aimed at increasing compulsory schooling (Leschinsky and Mayer [1990], Eurybase). To contrast recent trends in fertility, several OECD governments are considering or have already introduced specific measures aimed at countering trends in fertility (Sleebos [2003], Haas [2003]) and there is a lively discussion on the topic (Boeri et al. [2005], Boushey [2005], Lutz and Skirbekk [2005]). Do family friendly policies 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 reports

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\(^1\)Total fertility rate at or below 1.3.
negative association between schooling achievement and *tempo* fertility in most countries (see, among others, Nicoletti and Tanturri [2005]). However, to assess if policies aimed at increasing average schooling achievement and policies aimed at “boosting” fertility pursue intrinsically contrasting goals, further knowledge has to be achieved on the *causal effects* of education on fertility. Besides, insights on the heterogeneity of the effects across individuals might be relevant for targeting policies.

This paper assesses the causal effect of education on fertility in Italy, allowing for heterogeneity in the effects across individuals while controlling for self-selection of women into education. Since the analysis is not restricted to marital fertility and it considers a cohort measure of fertility instead than a period one, it can be profitably combined with previous work by Bratti [2003] widening the knowledge of the determinants of the recent trends in fertility in Italy. Moreover, the identification strategy exploited here can be easily used for the same purpose in other countries, setting the bases for future beneficial cross-country comparisons. This approach has already been used to investigate causal effects in a variety of other settings and it has recently been exploited to assess the (average) effects of education on fertility and infant health in Texas and California (McCrary and Royer [2005]).

The remainder of the paper is organized as follows: section 2 briefly reviews the economics of *tempo* fertility and previous findings on the relationship between
education and the timing of births. Section 3 presents in greater detail the identification and the estimation strategy, as well as the data used. The main findings are discussed in section 4 and 5. Section 6 provides arguments supporting the internal validity of the estimates. Section 7 concludes.

2 Education & Tempo Fertility: Theoretical Models and Empirical Evidence

In both static (Leibenstein [1974], Easterlin [1975], Becker [1991]) and dynamic (Butz and Ward [1980], Cigno and Ermisch [1989], Happel et al. [1984]) models of fertility behaviour, education is generally seen as a “modernization variable” which affects both demand and supply for children. The leading idea is that education level, affecting human capital initial endowment and subsequent further human capital accumulation, affects the marginal market wage of women, thus changing the opportunity cost of children and inducing modification in the demand for children. Cigno and Ermisch [1989] pointed out that women with greater human capital, who generally have steeper earnings profiles, might have an incentive to delay parenthood. Besides, women usually wait with children until after they have finished educational careers (Blossfeld and Huinink [1991]). Studies have documented positive association between schooling attainment of women and their contraceptive knowledge (see Rosenzweig and Schultz [1989], Goldin and Katz [2002]): the effective use of contraceptives might contribute to
“scheduling” births according to the spouses’ preferences.

Most economic theories\(^2\) predict the postponement of motherhood as a consequence of enhanced schooling achievement, particularly in households where the wife works. As highlighted by Janowitz [1976], the channels through which the effect of education might take place are numerous\(^3\) and, the effect of education on fertility might be heterogeneous across women with different ability, skill levels (Blackburn et al. [1993], Ellwood et al. [2004]), family background.

Estimating the magnitude of the causal effect of education achievement on fertility requires either to be able to control for factors driving women’s preferences over children and work or to assign education level randomly to individuals, so that it would not be correlated with personal or social factors. Bloemen and Kalwij [2001], controlling for unobserved heterogeneity in an event-history setting, suggest that, in the Netherlands, an increase in the years of schooling causes a woman to schedule births later in life but it does not significantly affect her completed fertility. Bratti [2003], in his study on labour force participation and marital fertility in Italy, controls for unobserved heterogeneity including in his model a wide range of factors.

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\(^2\) 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. For a more detailed discussion of different theoretical models on fertility behaviour, see Gustafsson [2003], Hotz et al. [1997]. Gustafsson [2003] focuses mainly on (dynamic) models predictions as regards the timing of births, whereas Hotz et al. [1997] consider in greater detail also static models predictions, mainly concerned with the determinants of completed fertility.

\(^3\) In addition to those already mentioned, education might affect fertility via its effect on labour force participation, labour force attachment and time spent out of the labour force (in education). There is a vast literature discussing the links between labour force participation and fertility (among others, Willis [1973], Heckman and Macurdy [1980], Heckman et al. [1985], Heckman and Walker [1990], Moffit [1984], Francesconi [2002], Cigno and Rosati [1996]).
of background variables and finds that the probability of giving birth for women with primary and lower secondary education decreases monotonically with age, whereas women with upper secondary and tertiary education levels tend to postpone fertility. Bratti uses a period measure of marital fertility\(^4\). Skirbekk et al. [2004] focus on the effect of duration of education on the timing of births and marriage in Sweden. Exploiting differences in birth months, they find that the difference of 11 months in the age at graduation implies a delay of almost 5 months in the age at first birth, event which generally occurs almost 8-10 years after graduation.

This paper focuses on the causal effect of education on fertility. Sticking to the traditional approach, fertility is defined referring only to women status and leaving men’s 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 and over the distribution of mother’s age at first birth. Finally, effects on one dimension of the phenomenon (tempo) are considered, due to limitations of the availability of adequate data on completed fertility.

\(^4\)Measures of fertility differ according to the reference calendar time (period or cohort) on which they are built: fertility might be analysed from a period perspective (births in a given time period) or from a cohort perspective (births to a group of women born within a particular time period). Only if the processes determining individual’s fertility behaviour are stationary, than period and cohort measures of fertility match exactly.
3 Identification of the Causal Parameters of Interest

The parameter of interest in this application is a reduced form parameter that summarizes the impact of schooling on behaviour and the impact of behaviour of fertility. Let $Y$ be woman’s age at first birth and let $D$ represent the treatment (namely, “more schooling”). $D_i$ takes the value 1 if individual $i$ has a high qualification and the value 0 otherwise. $Y^1$ and $Y^0$ denote the potential outcomes (Rubin [1974], Holland [1986]): $Y^1$ is the mother’s age at first birth if she would be exposed to the treatment, i.e. if she would get a high qualification; $Y^0$ is the mother’s age at first birth if she would not be exposed to the treatment, i.e. if she would get a low qualification. Thus, the parameters of interest are simply $E[Y^0_i] - E[Y^1_i]$, i.e. the average treatment effect ($ATE$) or $F_{Y^1}^{-1}(q) - F_{Y^0}^{-1}(q)$, i.e. the quantile treatment effect at quantile $q$ ($QTE$).\footnote{$QTE_q = F_{Y^1}^{-1}(q) - F_{Y^0}^{-1}(q), \forall q \in [0, 1]$, where $F_X^{-1}(q) = \min\{x \in X : F_X(x) \geq q\}$, $X$ is the set of values of the random variable $X$ and $F_X$ is its cumulative distribution function. The quantile treatment effect represents the change in the response function required to stay on the $q^{th}$ conditional quantile function (horizontal distance between the distribution functions $F_{Y^1}$ and $F_{Y^0}$). Dissimilar quantile treatment effects at different quantiles $q$ suggest heterogeneity of the treatment effect.} Unless $D$ were randomly assigned to individuals, eventually conditioning on a set of covariates, the direct comparison of conditional means or conditional distributions in the observed data does not identify neither $ATE$ or $QTE$s of education ($D_i$) on fertility ($Y_i$). In this application, identification of the causal effect of education on fertility relies on a regression discontinuity design (Trochim [1984], Thistlethwaite and Camp-
bell [1960]), exploiting a mandatory schooling reform rolled out nationwide in Italy in 1963. The 1963 reform (N.1859 Act December 31, 1962) prescribed the unification of the previous junior high schools in a single compulsory junior high school (scuola media) and increased compulsory schooling from 5 to 8 years. According to the new law (in force since October 1, 1963), individuals should attend school at least until junior high school (scuola media) graduation but individuals who had been in school for at least 8 years at the time of their 14th birthday were allowed to drop out. Due to the new law, individuals born after 19496 were compelled to attend 3 more years of schooling7. Compliance with the reform was far from perfect: only in 1976, the proportion of compulsory school age children accomplishing with their obligation approached 100% ([Brandolini and Cipollone, 2002, p. 9], Checchi [1997]).

Assignment to the treatment (“more schooling”) was fully determined by the individuals’ date of birth (S), which is observed by the analyst. Let \( \bar{s} \) be the threshold date of birth from which the increase in compulsory schooling started to be effective: a discontinuity in the conditional distribution of \( D \) given \( S \) around \( \bar{s} \) is expected, due to the effect of the 1963 reform. The conditional distribution of any predetermined characteristic \( W \) given \( S \) is expected to be smooth around \( \bar{s} \) and it is assumed that the 1963 reform did not exert any direct effect on women’s

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6See [Brandolini and Cipollone, 2002, p. 11], Flabbi [1999].
7The law prescribed also that, in case of need, new schools should be build (by 1966) in municipalities with more than 3,000 inhabitants.
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 ($Y$)\textsuperscript{8}. Due to the imperfect compliance with the assignment to the treatment, this strategy identifies the average causal effect of education on fertility for those individuals persuaded to obtain additional education by virtue of the reform (compliers), i.e. the strategy allow to identify the local average treatment effect ($LATE$, see Angrist et al. [1996])\textsuperscript{9}. Under particular conditions, $LATE$ is informative on $ATE$ (Angrist [2004]).

To sum up, the research design guarantees the identification of the causal effect\textsuperscript{10} of education on mother’s age at first birth 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 $\bar{s}$; (ii) individuals around the threshold $\bar{s}$ are similar as regards potential outcomes; (iii) there are no individuals who do exactly the opposite of their assignment; (iv) there are no spill-over effects (stable unit treatment value assumption, see Angrist et al. [1996]). Indeed,

\textsuperscript{8}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 is proportional to the structural parameter in an instrumental variable setting (Hahn et al. [2001]).

\textsuperscript{9}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).

\textsuperscript{10}See Hahn et al. [2001] for a formal discussion on identification and estimation of treatment effects in a regression-discontinuity design.
Imbens and Rubin [1997] show that, under the LATE identifying assumptions\(^{11}\), the compliers’ potential outcomes’ distributions can be written as a weighted average of observed distribution by treatment status and assignment to the treatment.

The same holds also in the regression discontinuity design framework\(^{12}\). Equation (3) represents the causal effect of education at \(\bar{s}\) on \(F(y)\) for compliers.

\[
F_{C_1}^C(y) - F_{C_0}^C(y) = \frac{F_1(y|\bar{s}) - F_0(y|\bar{s})}{\phi_c(\bar{s})}
\]

where: (i) \(F_d^C(y) \equiv \text{Prob}[Y_i^d \leq y|C_i],\) \(d \in \{0, 1\}\), are the compliers’ potential outcome distributions; (ii) \(F_z(y|\bar{s}) \equiv \text{Prob}[Y_i \leq y|S_i = \bar{s}, Z_i = z],\) \(z \in \{0, 1\}\); (ii) \(Z_i\) is a dummy variable which describes the assignment to the treatment: it takes the value 1 if individual \(i\) is assigned to the treatment and 0 otherwise (i.e., \(Z \equiv I(S_i \geq \bar{s})\)); and \(\phi_c(\bar{s})\) is the proportion of compliers at \(\bar{s}\). \(F_1(y, s) - F_0(y, s)\) is the intention-to-treat effect, i.e. the difference in the outcome \(F(y)\) by the instrument \(Z\), regardless actual treatment status, that is regardless the observed

\(^{11}\)Namely, stable unit treatment value assumption, the exclusion restriction, the strict monotonicity and the random assignment assumption. See Angrist et al. [1996] for an extensive discussion.

\(^{12}\)It can be easily shown that the following holds:

\[
F_{11}^C(y) = \frac{(\phi_o(\bar{s}) + \phi_c(\bar{s}))}{\phi_c(\bar{s})} F_{11}(y|\bar{s}) - \frac{\phi_o(\bar{s})}{\phi_c(\bar{s})} F_{01}(y|\bar{s})
\]

\[
F_{10}^C(y) = \frac{(\phi_o(\bar{s}) + \phi_c(\bar{s}))}{\phi_c(\bar{s})} F_{10}(y|\bar{s}) - \frac{\phi_o(\bar{s})}{\phi_c(\bar{s})} F_{00}(y|\bar{s})
\]

where \(F_1^C(\cdot)\) and \(F_0^C(\cdot)\) denote the potential outcomes’ distributions among compliers at \(\bar{s}\); \(F_{1d}(y|\bar{s})\) denote the distribution of \(Y\) conditional on \(S = \bar{s}, D = d\) and \(Z = z\); \(Z\) is a dummy variable describing the assignment to the treatment (i.e., \(Z \equiv I(S_i \geq \bar{s})\)); \(\phi_o(\bar{s}), \phi_n(\bar{s}), \phi_c(\bar{s})\) represent the population proportions of always takers, never takers and compliers - at \(\bar{s}\), respectively. Under the identifying assumptions, the following also holds: \(\phi_o(\bar{s}) = E[D|Z = 0, S = \bar{s}]; \phi_n(\bar{s}) = 1 - E[D|Z = 1, S = \bar{s}]; \phi_c(\bar{s}) = 1 - \phi_o(\bar{s}) - \phi_n(\bar{s}) = E[D|Z = 1, S = \bar{s}] - E[D|Z = 0, S = \bar{s}].\) The proportion of compliers (\(\phi_c(\bar{s})\)) is exactly the effect of \(Z\) on \(D\) at the threshold \(\bar{s}\).
value of $D$.

Note that one can characterize the sub-population of compliers via their average characteristic $X$ ([Angrist, 2004, p. C69])\textsuperscript{13}.

### 3.1 Data and Related Issues

To implement the identification strategy outlined in the previous section one has to ascertain the size of the discontinuity in women’s schooling achievement and the size of the discontinuities over the distribution of mother’s age at first birth $Y_i$. Although estimators proposed in the literature (Hahn et al. [2001]) emphasize the use of local polynomial techniques, these techniques are no longer appropriate when the covariate that determines the treatment is discrete (or is reported in coarse intervals). In these cases, “inference in a regression discontinuity design involves extrapolation from observation below the threshold to construct a counterfactual for observation above the threshold” (Lee and Card [2006]). Thus, each conditional expectation will be smoothed by means of a parsimonious global polynomial in $S$ and $Z \equiv 1(S \geq \bar{s})$\textsuperscript{14} and this will be used to construct counterfactuals at the threshold.

The analysis is based on the 1981 Census (2% sample) and the 1991 Census (1% sample).

\textsuperscript{13}[Angrist, 2004, p. C69] noted that $Prob[X_i|C] \equiv \frac{Prob[C|X_i = x]Prob[X_i = x]}{\phi_c} = \frac{\phi_{c,x}Prob[X_i = x]}{\phi_c}$, where $\phi_c$ denotes the proportion of compliers and $\phi_{c,x}$ denotes the proportion of compliers in the sub-population of individuals with $X = x$.

\textsuperscript{14}Sensitivity of the parametric results to different smoothing techniques, specifically to the choice of the degree of smoothness, is checked and documented extensively in earlier versions of the paper, available from the author upon request.
sample) data (see ISTAT [1983], ISTAT [1991])\textsuperscript{15}. The analysis on the timing of childbearing is based on the 1981 Census data, whereas the analysis on the timing of marriage is based on the 1991 Census data\textsuperscript{16}. Both samples provide information on individual’s education.

Mother’s age at birth of the oldest child (still at home) is referred as mother’s age at first birth. As a consequence: (i) one is only able to calculate mother’s age at birth of children still living in the parental home at the time of the interview; (ii) one is only able to assign children to women who have already left parental home, regardless their marital status. The first point risen is definitely not likely to affect the identification of the causal effect of education on the timing of births. Indeed, this 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 ([ISTA T, 1997, Table 2, p. 82]) and Italian adolescents tend to leave parental home late\textsuperscript{17}, this is most likely to hold in practice. Mothers and children might be mismatched when the natural mother of each child is not the household head or the wife of the

\textsuperscript{15}The Census data can be used to create sizeable samples whereas 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.

\textsuperscript{16}Details on the procedure used to link individuals in the same household using the 1981 Census data are available from the author upon request. The same procedure could not be applied to the 1991 Census data, which cannot thus be used to analyse fertility. Conversely, the 1981 Census does not provide information on the individuals’ year of marriage.

\textsuperscript{17}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, 2000, Tab. 3.8, p. 96]).
household head\textsuperscript{18}. However, the identification strategy is unaffected provided this mismatch takes place the same way on both sides of the threshold. The analysis will be limited on the timing of births occurred before these women turn 27, since mother’s age at first birth is right-censored.

1991 Census collect data on the year of the last marriage, which does not necessarily correspond to the first marriage. However, in the last decades, Italy has not experienced the massive increase in the number of divorces that is apparent in some other European countries: the family structure has not yet substantially changed and still nowadays the most common model of living together is marriage; divorce and cohabitation are not widespread practice (see Castiglioni and Dalla Zuanna [1994], Castiglioni [1999]). Moreover, until 1970 it was not possible to re-marry after a legal separation. These considerations suggest that the year of marriage reported is actually, for almost all individuals, the year of first marriage.

The available data are not adequate to examine completed fertility\textsuperscript{19}.

\textsuperscript{18}I.e., when a woman rears her child in the parents’ home or when she has divorced, re-married and she lives with the children of the “new” husband.

\textsuperscript{19}Most of the women of the cohorts 1938-1956 had not yet completed their fertile lifespan by 1981 and 1991.
4 The Effect of the 1963 School Reform on Education

This section assesses the size of the effect of the 1963 reform on education (first-stage effect), relying on the pooled sample of the 1981 and the 1991 Census\textsuperscript{20}. The individuals affected by the 1963 reform are those who would not have completed junior high school in the absence of the reform and would complete junior high school under the new law. $D_i$, the binary variable describing treatment status, takes the value 1 if individual $i$ has attained exactly junior high school qualification and 0 if he/she has a lower qualification\textsuperscript{21}. The effect of the reform is estimated at $s = 1949, 1950, 1951, 1952$ (see Table 1). The motivation to consider this particular set of values of $s$ is twofold: firstly, it is interesting to explore whether the 1963 reform had different effects depending on the time elapsed since primary school completion\textsuperscript{22}; secondly, extrapolation becomes less plausible as one

\textsuperscript{20}The underlying assumption here is that both the 1981 Census and the 1991 Census surveyed the qualification level of the same population of women, namely those born between 1938 and 1956. These women were aged between 25 and 43 at the time of the 1981 Census interview and between 35 and 53 at the time of the 1991 Census interview. One can thus safely assume that, in both cases, they have already completed their secondary school education.

\textsuperscript{21}The analysis is limited to women with at most junior high school qualification. The analysis has also been carried out using data of the whole sample of women and defining treated individuals those women with \textit{at least} junior high school qualification at the time of the Census interview. The first stage effect estimates obtained on this wider sample have the same magnitude of those presented in Table 1 and lead to consistent inferential conclusions. These estimates, not reported here for brevity, are available from the author upon request.

\textsuperscript{22}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. Individuals of older cohorts, namely those born between 1949 and 1951, (should have) completed primary education years before.
moves further from the threshold year $\bar{s} = 1949$. Table 1 reports the estimates of the proportion of compliers: since inferential conclusions under smoothing via linear probability or logit models are broadly consistent, estimates under the first specification are considered for ease of interpretation and use. The proportion of compliers $\phi_c(s)$ increases as one moves $s$ closer to 1952: women who were 14 at the time of the 1963 reform, 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 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 is any effect of the reform on the proportion of women achieving high school qualification: there is no effect of the 1963 reform on the proportion of women who achieve high school qualification (see Table 2 and Figure 1). This result is robust to the choice of the smoothing technique.

5 The Effect of Education on the Timing of Births

This section assesses the magnitude of the causal effects of education on fertility and, for complementary reasons, the causal effect of education on the distribution of woman’s age at marriage.

Graphs in Figure 2 (Figure 3) depict the cohort pattern in the proportion of women with at most junior high school qualification bearing their first child (marrying) by
the ages 18, 20, 22, 24. Point estimates of the discontinuity (intention-to-treat effects), are reported in Table 3 and Table 4. In short, the evidence points toward the conclusion that the 1963 reform lead to: (i) reduced likelihood (nearly 3 percentage points) of giving births by younger ages (19, 20, 21); (ii) positive effects on the likelihood of giving births by the age $y = 23$; (iii) negligible effects on the likelihood of giving births by older ages (25, 26); (iv) negligible effects on the timing of marriage at any $y \in [18, 26]$.

To provide insights on the causal effect of education of fertility, the reduced-form estimates are combined using the Wald estimator described by equation (3). Since the Wald estimator does not guarantee that the compliers potential outcomes’ distribution functions are monotonically increasing, an alternative naive estimator has also been used. The sampling distribution of the revised estimates is obtained resampling from the empirical distribution of $(Y, D|s)$. Estimates and standard errors (computed via delta method for the Wald estimates) are reported in Table 5 and in Table 6 (see also Figure 4). Figures suggest 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 women. These graphs, not reported for brevity, are available from the author upon request.

Note that the same pattern apparent in these graphs is observed considering the sample of all women. These graphs, not reported for brevity, are available from the author upon request.

See equations 1 and 2 in footnote 9. Imbens and Rubin [1997] 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 likelihood. Here, their approach is followed operating directly on the cumulative distribution functions, i.e. $\hat{F}_{C_k}(y)^* = \min\left(\max\left(0, \hat{F}_{C_k}(y), \hat{F}_{C_k}(y - 1)^*\right), 1\right)$, $k = 0, 1$. 

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ages (see also Figure 4). This seems not driven mainly by the effect of education on the timing of marriage: indeed, although the pattern of the effect over the distribution of woman’s age at marriage is consistent with the one observed on the distribution of mother’s age at first birth, the magnitude of the effects is generally negligible\textsuperscript{25}. The magnitude of the effect of education on the timing of births is quite large, reaching \(-40\) percentage points. Note, however, that the comparison by observed treatment status delivers a difference of nearly 20 percentage points, on average, and it is fairly constant over the distribution. Women who postpone early motherhood under the effect of the treatment are likely those who, in the absence of the treatment, face a lower opportunity cost of children. Therefore, these women are less likely to participate in the labour market. A rise in the achieved education, by increasing their current market wage\textsuperscript{26}, increases the probability that they participate in the labour market and rises the opportunity cost of children\textsuperscript{27}. Thus, women end up delaying early childrearing. The increase in qualification (from primary to junior high school qualification) leads to an increase in the opportunity cost of children but the earning profiles of these women remain

\textsuperscript{25}Note that the precision of these estimates is much lower.

\textsuperscript{26}Brandolini and Cipollone [2002] exploited the 1963 reform as instrument to assess the return on education in Italy. Their estimate of the average return on education for women over the years 1992 and 1997 ranges from 7\% to 10\% per year: these estimates lead to approximately between 21\% and 30\% increase in wage due to the 3-year increase in education induced by the 1963 reform.

\textsuperscript{27}The study by Pencavel [1998], based on CPS data, suggests that the increase in women’s wage accounts for between 25\% and 50\% of the increase in women’s labour supply, depending on the cohort of women considered. The main factor accounting for the residual change is the increased attractiveness of the market place relative to the household.
rather flat, so that the incentives to postpone births operate only at younger ages (19-22). Testing this hypothesis requires data on work histories\textsuperscript{28}. These further analysis are already on my research agenda.

Now we turn to the issue of heterogeneity of the impact across individuals. Graphs\textsuperscript{29} in Figure 5 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). There is some indication of heterogeneity of the impact of education on the timing of births 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\textsuperscript{30}. Conversely, there is no clear-cut evidence of heterogeneity of the impact of education on the timing of marriage: on the whole, differences between compliers and non-compliers are broadly negligible. The discussion suggests that the fertility behaviour of the women affected by the reform is likely to be substantially different from the one of the average woman in the population.

Compliers are mostly resident in northern regions of Italy (see Table 7, charac-

\textsuperscript{28}I plan to rely on data of the Work Histories Italian Panel (WHIP), released by Laboratorio Revelli, Center for Employment Studies (see http://www.laboratoriorevelli.it/whip).

\textsuperscript{29}Additional graphs for the case $s = 1950$ and $s = 1951$ show broadly the same pattern observed at $s = 1952$ and are not reported for brevity. They are available upon request from the author.

\textsuperscript{30}Differences between the fertility behaviour of compliers and non-compliers become negligible at older ages, i.e. $y = 25$, $y = 26$. 

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terization follows from Angrist [2004]): this might be due to the well known economic differences between North-Center and South of Italy: southern regions have traditionally been characterized by a lower socio-economic development. In particular, at the time the 1963 reform was introduced, in southern regions school buildings were unfit and most of the teachers untenured. This might have contributed to a lower compliance\textsuperscript{31}.

The results of this empirical analysis are consistent with previous findings by Bloemen and Kalwij [2001] for the Netherlands, whereas they are not fully consistent with previous results by Bratti [2003]. This might be for mainly three reasons: firstly, Bratti considers a period measure of fertility, conversely here the analysis is based on cohort measures of fertility; secondly, Bratti [2003] considers the effects of education on the probability of a birth event\textsuperscript{32}, whereas here the analysis is focused on the timing of first births.

6 The Internal Validity of the Design: A Discussion

In this section evidence is provided to ensure that the results have a causal interpretation. Indeed, one can claim that over the 1970s women position in the society, 

\textsuperscript{31}It is also well known that the fertility behaviour in northern and central regions and the one in southern regions differs: in southern regions, the traditional family structure is more common, i.e. the wife primarily tends to housework and raises children and the husband works and keeps the family on his salary, and families are started earlier, with a formal union (marriage). Unfortunately, available data do not allow to conduct these analyses at the macro-region level: sample sizes became too small for meaningful elaborations.

\textsuperscript{32}“We consider a birth event to be the presence in the family of a child aged more than one and less then two years old.” [Bratti, 2003, p. 537].
in Italy, went through 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, it is crucial that the discontinuity in the series $F_s(y)$ (as a function of $s$) is fully attributable to the effect of the 1963 reform and it is not driven by the mentioned innovations. To test the validity of this assumption, the fertility behaviour (i.e., mother’s age at first birth) of women who achieved high school qualification is be considered. Since these women have not been affected by the 1963 reform (see Table 2 and Figure 1), one would expect their fertility behaviour to change smoothly over cohorts. On the whole, as figures in Table 8 suggest, changes are indeed negligible. In principle, women with high school qualification might have a different fertility behaviour from those with junior high school qualification. However, the first group of women would possibly be more affected by, for instance, the introduction of the pill (see Goldin and Katz [2002]).

Also migrations might be relevant confounders: between 1950 and 1970, both

---

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

33The small number of events occurred ("events" are births occurred to women of the 1938-1956 cohorts with high school qualification) 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.

20
internal and international migrations were very pronounced in Italy (Pugliese [2002]). Compared to international migrations, internal migrations involved a larger share of individuals and were to a lesser extent associated with the phenomenon of return migration. Emigration was generally characterized by intermittent stays for varying intervals and emigrants generally returned to their country of origin, often also as a consequence of restrictive immigration policies adopted by the host country. Emigrants and (internal) migrants at that time were mostly prime-age low educated men; conversely, during the 1980s and 1990s, emigrants were mostly highly educated young men ([Pugliese, 2002, p.64]). Internal migrations’ intensity increased in the 1950s and exhibit a peak in 1961-1963, which is partly attributed to the effects of post-census adjustments and the repeal of the fascist law on urbanization\textsuperscript{35} (see Bonifazi and Heins [2000], Treves [1976]). Then, the intensity of internal migrations started to decrease during the 1970s. Bonifazi and Heins [2000] noted that these trends are mainly driven by flows within the same province. However, the analyses of this paper are conducted at the country level. Moreover, they involve individuals who were Italian citizens and were resident in an Italian region at the time of the Census interview\textsuperscript{36}. As a consequence of migration flows, individuals who were not in Italy at the time the reform was

\textsuperscript{35}This law (Law N.358, April 9, 1931) placed severe limitations on changes of residence; the act was repealed by the Law N.5 in 1961.

\textsuperscript{36}Data on place of birth would have mirrored more truthfully the place were individual attended junior high school. Unfortunately, those data were not available; I relied on data on the region of residence at the time of the Census survey, instead.
introduced but returned during the 1970s or the 1980s (return migration) might have been included in the analysis and individual who emigrated before the Census interview but after the reform introduction might have been excluded. Given the characterization of the emigrants by age and education and the direction of the migration flows, these factors would have possibly lead to attenuating the effect of the reform on compliers.

The assumption of no defiers seems rather plausible since it basically requires that: (i) each woman born from 1949 onwards got at least as much schooling as she would have in the absence of the 1963 reform and (ii) each woman born up to 1948 got at most as much schooling as she would have if the 1963 reform has been in place one year before. Besides, the small amount of compliers supports the stable unit treatment value assumption: hardly the behaviour of less than 6% of the whole population might have induced spill-over effects.

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

7 Concluding Remarks

This paper provides evidence on the role of education in determining the timing of first birth exploiting exogenous variation in schooling induced by a school reform rolled out in Italy in 1963. Findings suggest that a large fraction of the women
affected by the reform postpones early first births but catches up with this fertility delay before turning 26. These results seems not be driven by causal effect of education on the timing of marriage, which are generally negligible.

The internal validity of the research design is extensively discussed: evidence based on the data at hand suggests that the 1963 reform represent a valid instrument, which helps to correctly identify the causal effect of education on the timing of first birth for compliers. However, the estimates 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. This suggests that the fertility behaviour of the women affected by the reform is likely to be substantially different from the one of the average woman in the population. Generalizing this effect to a wider set of individuals requires typically relying on stronger conditions than those who guarantee local identification.

Since new mandatory schooling laws have been introduced in many countries in the last decades, the identification strategy employed in this study can be easily replicated in other countries. Indeed, 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.

Further steps of this research entail examining the causal effect of education on
labour force participation, over the distribution of wages and on earning profiles of these women. Besides, further research is needed to assessing whether changes in education produce similar effects regardless of the level at which additional education is obtained.
References


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

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

Smoothing via linear probability model

<table>
<thead>
<tr>
<th>Year</th>
<th>Observed</th>
<th>Fitted, smpl 1949–1956</th>
<th>95% CI Lower B.</th>
<th>Fitted, smpl 1938–1948</th>
<th>95% CI Lower B.</th>
<th>Predicted, smpl 1938–1948</th>
<th>95% CI Upper B.</th>
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<td>1.0</td>
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<td></td>
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<tr>
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<td>1.0</td>
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<td>1.0</td>
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<td>1956</td>
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<td>1.0</td>
<td></td>
<td>1.0</td>
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</tr>
</tbody>
</table>

1981 and 1991 Census data, 2% and 1% sample, respectively. Italian women born between 1938 and 1956. **Right hand panels**: see Table 1. **Left hand panels**: see Table 2. Estimates are obtained using weighted linear regression correcting for heteroskedasticity due to data grouping (by cohort and census, 19*2=38 clusters). Graphs reports averages by birth cohort of the observed, fitted and predicted values.
Figure 2: Effect of the 1963 reform on the distribution of woman’s age at first birth, i.e. on $F(y) = \text{Prob}[Y_i \leq y]$ at distinct values of $y$, where $Y$ denotes woman’s age at first birth.

$F(y)$ at $y=18$

$F(y)$ at $y=20$

$F(y)$ at $y=22$

$F(y)$ at $y=24$

Figure 3: Effect of the 1963 reform on the distribution of woman’s age at marriage, i.e. on $F(y) = \text{Prob}[Y_i \leq y]$ at distinct values of $y$, where $Y$ denotes woman’s age at marriage.

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

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

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

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

1% Sample of the 13th Census data. Sample of women with at most junior high school qualification. **Top right hand panel:** smoothing polynomial over cohorts 1938-1948: linear in cohort; smoothing polynomial over cohorts 1949-1956: quadratic in cohort. **Top left hand panel:** smoothing polynomial over cohorts 1938-1956: linear in cohort. **Bottom right-hand panel:** smoothing polynomial over cohorts 1938-1956: quadratic in cohort. **Bottom left-hand panel:** smoothing polynomial over cohorts 1938-1956: quadratic in cohort. **Types of polynomials fitted:** linear probability models; all models are estimated using weighted least squares estimators, allowing for heteroskedasticity.
Figure 4: Causal effects of education on the timing of births and marriage for compliers at $s = 1952$. Data: 1981 and 1991 Census, 2% and 1% sample respectively. Italian women with at most junior high school qualification; women born between 1938 and 1956.

**Wald estimator**

Causal Effect of Education on the Timing of Births and Marriage for Compliers – $s = 1952$

Data: Census 1981, 1991 – Women with at most JHS Qualification

**Semiparametric estimator**

Causal Effect of Education on the Timing of Births and Marriage for Compliers – $s = 1952$

Data: Census 1981, 1991 – Women with at most JHS Qualification

Estimates of the causal effects of education on the timing of births and on the timing of marriage are based on the sample of women with at most junior high school qualification (cohorts 1938-1956) from the 1981 Census data and the 1991 Census data, respectively. Standard errors are based on the delta method (Wald estimator) and on a semiparametric bootstrap procedure (semiparametric estimator), with 10,000 replications. 95% confidence intervals for the Wald estimates for the semiparametric estimates are computed relying on normal approximation.
Figure 5: Compliers’ distribution of $Y^1$ (outcome under the effect of the treatment, i.e. in case the woman achieves junior high school qualification) and $Y^0$ (outcome in the absence of the treatment, i.e. in case the woman achieves less than junior high school qualification). Revised estimates.

$Y^1$

**Outcome: Woman’s Age at First Birth**

<table>
<thead>
<tr>
<th>Mother’s Age at First Birth</th>
<th>Cdf of Y_1, Compliers &amp; Always Takers</th>
<th>Cdf of Y_0, Compliers &amp; Never Takers</th>
</tr>
</thead>
<tbody>
<tr>
<td>Females, Italy; data: Census 1981</td>
<td></td>
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</table>

Analysis on the causal effect of education on the timing of births are based on the 2% Sample of the 12th Census data; sample of women living in households with all Italian members whose age at first birth was either censored or greater than 15 years with at most junior high school qualification. Analysis on the causal effect of education on the timing of marriage are based on the 1% Sample of the 13th Census data; sample of women with at most junior high school qualification. Cohorts 1938-1956. Characteristics of the estimators’ sampling distributions are retrieved relying on a semiparametric bootstrap procedure, with 10,000 replications. 95% confidence intervals are computed relying on normal approximation.
Table 1: First stage effect of the 1963 reform on the proportion of women who achieved exactly junior high school qualification by the time of the Census interview.

<table>
<thead>
<tr>
<th></th>
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</thead>
<tbody>
<tr>
<td><strong>Smoothing Technique</strong></td>
</tr>
<tr>
<td>$\phi_c(s)$</td>
</tr>
<tr>
<td>t-test</td>
</tr>
<tr>
<td>p-value</td>
</tr>
</tbody>
</table>

Census data (1991 Census 1% Sample and 1981 Census 2% Sample). Sample of Italian women with at most junior high school qualification; analysis limited to women born between 1938-1956. The linear probability model fitted is $E[D|S] = \beta_0 + \beta_1(\text{cens} = 1991) + \beta_2(S - 1937) + \beta_3(\text{cens} = 1991) + \beta_4Z + \beta_5Z(S - 1937) + \varepsilon$, $S$ is the individual birth cohort,$1(A)$ takes the value one if the event $A$ is true and $Z = 1(S \geq 1949)$, $D = 1$(has exactly junior high school qualification). The effect of the reform on education at $S = s$ (i.e., $E[D|S = s, Z = 1] - E[D|S = s, Z = 0]$), $s \in \{1949, 1950, 1951, 1952\}$ is given by $\beta_4 + \beta_5(s - 1937)$. Estimates are obtained using weighted linear regression with robust standard errors, correcting for heteroskedasticity due to data grouping (by cohort and census, 19*2=38 clusters on the pooled sample). $R^2$ of the regression is around 0.99. The logit model fitted, using weighted regression, is $\log\left(\frac{p_s}{1-p_s}\right) = \gamma_0 + \gamma_1(\text{cens} = 1991) + \gamma_2(S - 1937) + \gamma_3Z + \gamma_4Z(S - 1937) + \zeta$, $p_s \equiv E[D|S] \equiv \Pr(D = 1|S)$. * For each $s \in \{1949, 1950, 1951, 1952\}$, the hypothesis tested under the logit specification is $H_0 : \gamma_3 + \gamma_4(s - 1937) = 0$ against $H_1$ , i.e. a necessary and sufficient condition for the null hypothesis $H_0 : \phi_c(s) = 0$ vs $H_1 : \phi_c(s) \neq 0$. The effect of the 1963 reform on education achievement at $s$ under this specification is given by $E[D|S = s, Z = 1] - E[D|S = s, Z = 0] = \frac{\exp[\gamma_0 + \gamma_1(\text{cens} = 1991) + \gamma_2(s - 1937)] \exp[\gamma_3(s - 1937)]}{1 + \exp[\gamma_0 + \gamma_1(\text{cens} = 1991) + \gamma_2(s - 1937)]} - \frac{\exp[\gamma_0 + \gamma_1(\text{cens} = 1991) + \gamma_2(s - 1937)]}{1 + \exp[\gamma_0 + \gamma_1(\text{cens} = 1991) + \gamma_2(s - 1937)]}$. The corresponding estimates for the 1981 Census and 1991 Census are broadly equal: differences are of the order $10^{-3}$.
Table 2: Effect of the 1963 reform on the proportion of women who achieved exactly high school qualification by the time of the Census interview.

| Overall sample size: 200,039. Average cohort sample size: nearly 5,890. |
|--------------------|-------------------|--------------------|-------------------|-------------------|
| Smoothing Technique | Linear Probability Model | Logit Model |
| \( \hat{\varphi}_c(s) \) |        |        |        |        |        |        |        |
| \( t \)-test       | 0.28   | 1.44*  |        |        |        |        |        |
| p-value            | 0.78   | 0.16   |        |        |        |        |        |

Census data (1991 Census 1% Sample and 1981 Census 2% Sample). Sample of Italian women with at most high school qualification; analysis limited to women born between 1938-1956. The linear probability model fitted is \( E[D|S] = \beta_0 + \beta_1 1(cens = 1991) + \beta_2 (S - 1937)^2 + \beta_3 Z + \varepsilon \). \( S \) is the individuals’ birth cohort, \( 1(A) \) takes the value one if the event \( A \) is true and \( Z = 1(S \geq 1949) \), \( D = 1 \) (has exactly high school qualification). The effect of the reform on education at \( S = s \) (i.e., \( E[D|S = s, Z = 1] - E[D|S = s, Z = 0] \), \( s \in \{1949, 1950, 1951, 1952\} \) is given by \( \beta_4 \). Estimates are obtained using weighted linear regression with robust standard errors, correcting for heteroskedasticity due to data grouping (by cohort and census, 19*2=38 clusters on the pooled sample). \( R^2 \) of the regression is around 0.99. The logit model fitted, using weighted regression, is \( \log(\frac{1-p_s}{p_s}) = \gamma_0 + \gamma_1 1(cens = 1991) + \gamma_2 (S - 1937)^2 + \gamma_3 (S - 1937)^21(cens = 1991) + \gamma_4 Z + \zeta, p_s \equiv E[D|S] \equiv \text{Prob}(D = 1|S) \).

For each \( s \in \{1949, 1950, 1951, 1952\} \), the hypothesis tested under the logit specification is \( \gamma_4 = 0 \) against \( H_0 : \phi_c(s) = 0 \) vs \( H_1 : \phi_c(s) \neq 0 \). The effect of the 1963 reform on education achievement at \( s \) under this specification is given by \( E[D|S = s, Z = 1] - E[D|s, Z = 0] = \frac{\exp[\gamma_0 + \gamma_1 1(cens = 1991) + \gamma_2 (s-1937)^2 + \gamma_3 (s-1937)^21(cens = 1991)] - \exp[\gamma_0 + \gamma_1 1(cens = 1991) + \gamma_2 (s-1937)^2 + \gamma_3 (s-1937)^21(cens = 1991)]}{1 + \exp[\gamma_0 + \gamma_1 1(cens = 1991) + \gamma_2 (s-1937)^2 + \gamma_3 (s-1937)^21(cens = 1991)]} \). The corresponding estimates for the 1981 Census and 1991 Census and over the different values \( s \) are broadly equal: differences are of the order \( 10^{-3} \).
Table 3: Effect of the 1963 reform on $F(y) = \text{Prob}[Y_i \leq y]$ at distinct values of $y$, $Y$ woman’s age at first birth (intention-to-treat effect).

<table>
<thead>
<tr>
<th>Year</th>
<th>Smoothing Technique: Linear Probability Model</th>
</tr>
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<tr>
<td></td>
<td>$y$</td>
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<tr>
<td>1950</td>
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</tr>
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<td>p-value</td>
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</tbody>
</table>

2% Sample of the 12th Census data. Sample of women living in households with all Italian members whose age at first birth was either censored or greater than 15 years; analysis limited to women with at most junior high school qualification, born between 1938 and 1956. Estimates and standard errors under the preferred specification of the general tendency in the series $F_s(y) = \text{Prob}[Y_i \leq y | S_i = s]$: (i) for values $y \in [18, 19]$ common linear trend in cohort over cohorts 1938-1956; (ii) for values $y \in [20, 22]$ common quadratic trend in cohort over cohorts 1938-1956; (iii) for values $y \in [23, 26]$ distinct polynomials quadratic in cohort over the pre- and post-reform cohorts. Estimates are obtained using weighted linear regression correcting for heteroskedasticity due to data grouping (by cohort, 19 clusters). The test statistics test the hypothesis that effect is null. Results are broadly robust to the choice of the smoothing polynomial and smoothing technique (i.e., either linear probability or logit models).
Table 4: Effect of the 1963 reform on $F(y) = \text{Prob}[Y_i \leq y]$ at distinct values of $y$, $Y$ woman’s age at marriage (intention-to-treat effect).

<table>
<thead>
<tr>
<th>$y$ (woman’s age at marriage)</th>
<th>18</th>
<th>19</th>
<th>20</th>
<th>21</th>
<th>22</th>
<th>23</th>
<th>24</th>
<th>25</th>
<th>26</th>
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<td></td>
<td></td>
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<td></td>
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</tr>
<tr>
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<td>1.89</td>
<td>2.08</td>
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<td>1.89</td>
<td>2.08</td>
</tr>
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<td>0.22</td>
<td>0.03</td>
<td>0.03</td>
<td>0.09</td>
<td>0.09</td>
<td>0.08</td>
<td>0.06</td>
</tr>
</tbody>
</table>

1% Sample of the 13th Census data. Sample of women with at most junior high school qualification. Estimates and standard errors under the preferred specification of the general tendency in the series $F_s(y) = \text{Prob}[Y_i \leq y|S_i = s]$; (i) for $y = 18$, linear trend in cohort over cohorts 1938-1948, quadratic trend over cohorts 1949-1956; (ii) for values $y \in [19, 20]$, common linear trend over cohorts 1938-1956; (iii) for $y = 21$, distinct polynomials linear in cohort over the pre- and post-reform cohorts; (iv) for values $y \in [22, 26]$, common quadratic trend in cohort over the cohorts 1938-1956. Estimates are obtained using weighted linear regression correcting for heteroskedasticity due to data grouping (by cohort, 19 clusters). The test statistics test the hypothesis that effect is null.
Table 5: Causal effect of education on the timing of births for compliers at \( s \) (LATE).

<table>
<thead>
<tr>
<th>( s )</th>
<th>( \hat{\phi_n}(s) - \hat{\phi_a}(s) )</th>
<th>( \hat{\phi_n}(s) )</th>
<th>( \hat{\phi_a}(s) - \hat{\phi_n}(s) )</th>
<th>( \hat{\phi_a}(s) )</th>
<th>( \hat{\phi_c}(s) )</th>
<th>( \hat{\phi_c}(s) )</th>
</tr>
</thead>
<tbody>
<tr>
<td>1950</td>
<td>0.36 - 0.61</td>
<td>0.03</td>
<td>0.37 - 0.58</td>
<td>0.05</td>
<td>0.39 - 0.55</td>
<td>0.06</td>
</tr>
<tr>
<td>1951</td>
<td>0.37 - 0.58</td>
<td>0.05</td>
<td>0.39 - 0.55</td>
<td>0.06</td>
<td>0.39 - 0.55</td>
<td>0.06</td>
</tr>
<tr>
<td>1952</td>
<td>0.39 - 0.55</td>
<td>0.06</td>
<td>0.39 - 0.55</td>
<td>0.06</td>
<td>0.39 - 0.55</td>
<td>0.06</td>
</tr>
</tbody>
</table>

Data: 1981 Census (2% sample); sample of women living in households with all Italian members whose age at first birth was either censored or greater than 15 years with at most junior high school qualification. Cohorts 1938-1956. First column (LATE): estimates moving from the Wald estimator (see equation 3) and standard errors computed using the delta-method; second column: “revised” estimates moving from estimates of the compliers’ potential outcomes’ cumulative distribution functions. Revised estimates (rev.) are computed as \( \hat{F}_1^C(s) \ast \hat{F}_0^C(y) \), where \( \hat{F}_1^C(y) \), \( \hat{F}_0^C(y) \) represent the “revised” estimates of the compliers’ potential outcome distribution functions. \( \hat{\phi_n}(s), \hat{\phi_a}(s), \hat{\phi_c}(s) \) represent the estimates of proportion of never takers, of the proportion of always takers and of the proportion of compliers, respectively. \( \ast \) Standard errors are computed relying on a non-parametric bootstrap procedure (10,000 replications). Details on the bootstrap algorithm implemented, not reported from brevity, are available from the author upon request. Points \( s \) considered in the analysis are those for which the first stage age effect resulted significantly different from zero, namely \( s = 1950, s = 1951, s = 1952 \).
Table 6: Causal effect of education on the timing of marriage for compliers at \( s \) (LATE).

<table>
<thead>
<tr>
<th></th>
<th>( s = 1950 )</th>
<th>( s = 1951 )</th>
<th>( s = 1952 )</th>
</tr>
</thead>
<tbody>
<tr>
<td>( \hat{\phi}_a(s) - \hat{\phi}_n(s) )</td>
<td>0.36 - 0.61</td>
<td>0.37 - 0.58</td>
<td>0.39 - 0.55</td>
</tr>
<tr>
<td>( \hat{\phi}_c(s) )</td>
<td>0.03</td>
<td>0.05</td>
<td>0.06</td>
</tr>
</tbody>
</table>

Data: 1991 Census (1% sample); sample of women with at most junior high school qualification, born between 1938 and 1956. First column (LATE): estimates moving from the Wald estimator (see equation 3) and standard errors computed using the delta-method; second column: “revised” estimates moving from estimates of the compliers’ potential outcomes’ cumulative distribution functions. Revised estimates (rev.) are computed as \( \hat{F}_C(s) \cdot (\cdot) - \hat{F}_C(s) \cdot (\cdot) \), where \( \hat{F}_C(s) \cdot (\cdot) \) represent the “revised” estimates of the compliers’ potential outcome distribution functions. \( \hat{\phi}_n(s) \), \( \hat{\phi}_a(s), \hat{\phi}_c(s) \) represent the estimates of proportion of never takers, of the proportion of always takers and of the proportion of compliers, respectively. Standard errors are computed relying on a non-parametric bootstrap procedure (10,000 replications). Details on the bootstrap algorithm implemented, not reported from brevity, are available from the author upon request. Points \( s \) considered in the analysis are those for which the first stage age effect resulted significantly different from zero, namely \( s = 1950 \), \( s = 1951 \), \( s = 1952 \).
Estimates based on 1981 Census and 1991 Census data, 2% and 1% sample respectively; women born between 1938 and 1956 with at most junior high school qualification. **Legend:** **North:** Piemonte, Val D’Aosta, Friuli-Venezia Giulia, Emilia Romagna, Liguria, Lombardia, Trentino Alto Adige, Veneto; **Center:** Lazio, Marche, Toscana, Umbria; **South:** Abruzzo, Molise, Basilicata, Calabria, Campania, Puglia, Sardegna, Sicilia. Figures in the table follow from \( E[X|C_s] = \text{Prob}[X = 1|C_s] \frac{\text{Prob}[C_s|X = 1]}{\phi_c(s)} = \frac{\phi_{c,x}(s)}{\phi_c(s)} \text{Prob}[X = 1] \), where \( \phi_c(s) \) denotes the proportion of compliers at \( s \) in the population and \( \phi_{c,x}(s) \) denotes the proportion of compliers in the sub-population of individuals with \( X = x \) and \( S = s \). Estimates of \( \phi_{c,x}(s) \) are obtained following the same empirical strategy exploited to get the estimates of \( \phi_c \), i.e. running the following regressions: **North:** \( E[D|S, North] = \alpha_0 + \alpha_1(cens = 91) + \alpha_2(S - 1937) + \alpha_3 Z + \alpha_4(S - 1937)^2 Z + \varepsilon; \) **Center:** \( E[D|S, Center] = \beta_0 + \beta_1(1(cens = 91) + \beta_2(S - 1937) + \beta_3 Z + \beta_4(S - 1937) Z + \delta; \) **South:** \( E[D|S, South] = \gamma_0 + \gamma_1(S - 1937) + \gamma_2(S - 1937)^2 + \gamma_3 Z + \gamma_4(S - 1937) Z + \zeta. \) \( S \) is the individuals’ birth cohort, \( 1(A) \) takes the value one if the event \( A \) is true and \( Z = 1(S \geq 1949), D = 1 \) has exactly high school qualification. The effect of the reform on education at \( S = s \) (i.e., \( E[D|S = s, Z = 1] - E[D|S = s, Z = 0] \)), \( s \in \{1949, 1950, 1951, 1952\} \) is given by: **North:** \( \alpha_3 + \alpha_4(S - 1937) + \alpha_5(S - 1937)^2; \) **Center:** \( \beta_3 + \beta_4(S - 1937); \) **South:** \( \gamma_3 + \gamma_4(S - 1937). \) Estimates are obtained using weighted linear regression with robust standard errors (clusters by cohort, census and area). Adjusted \( R^2 \) of the regressions is around 0.99 (North) and 0.98 (Center, South).

<table>
<thead>
<tr>
<th>Area</th>
<th>( s = 1949 )</th>
<th>( s = 1950 )</th>
<th>( s = 1951 )</th>
<th>( s = 1952 )</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>( \phi_{c,x}(s) ) (s.e) \quad \phi_c(s) (s.e) \quad E[X = 1] \quad E[X</td>
<td>C_s]</td>
<td>( \phi_{c,x}(s) ) (s.e) \quad \phi_c(s) (s.e) \quad E[X = 1] \quad E[X</td>
<td>C_s]</td>
</tr>
<tr>
<td>( X = 1(North) )</td>
<td>0.010 (0.008) 0.014 (0.005) 0.474 0.343</td>
<td>0.031 (0.006) 0.031 (0.005) 0.474 0.472</td>
<td>0.059 (0.007) 0.047 (0.005) 0.474 0.595</td>
<td>0.079 (0.008) 0.063 (0.005) 0.474 0.343</td>
</tr>
<tr>
<td>( X = 1(Center) )</td>
<td>0.005 (0.012) 0.014 (0.005) 0.184 0.064</td>
<td>0.029 (0.011) 0.031 (0.005) 0.184 0.171</td>
<td>0.043 (0.009) 0.047 (0.005) 0.184 0.205</td>
<td>0.058 (0.009) 0.063 (0.005) 0.184 0.320</td>
</tr>
<tr>
<td>( X = 1(South) )</td>
<td>0.014 (0.009) 0.014 (0.005) 0.342 0.334</td>
<td>0.029 (0.009) 0.031 (0.005) 0.342 0.320</td>
<td>0.058 (0.009) 0.063 (0.005) 0.342 0.313</td>
<td>0.076 (0.009) 0.063 (0.005) 0.342 0.313</td>
</tr>
</tbody>
</table>
Table 8: Effect of the assignment to the treatment (Z) on the proportion of women with high school qualification who bear their first child by the age \( y \), \( F(y, s) = \text{Prob}[Y_i \leq y | S = s] \), \( Y \) woman’s age at first birth. Italy.

<table>
<thead>
<tr>
<th>y</th>
<th>Overall Sample Size: 36,932. Average Cohort Sample Size: 1,605</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>20</td>
</tr>
<tr>
<td>Smoot. Tech.</td>
<td>1949</td>
</tr>
<tr>
<td>lpm</td>
<td>-0.01 (0.1)</td>
</tr>
<tr>
<td>logit</td>
<td>-0.01 (0.0)</td>
</tr>
<tr>
<td></td>
<td>1950</td>
</tr>
<tr>
<td>lpm</td>
<td>-0.01 (0.1)</td>
</tr>
<tr>
<td>logit</td>
<td>-0.01 (0.1)</td>
</tr>
<tr>
<td></td>
<td>1951</td>
</tr>
<tr>
<td>lpm</td>
<td>-0.01 (0.1)</td>
</tr>
<tr>
<td>logit</td>
<td>-0.01 (0.3)</td>
</tr>
<tr>
<td></td>
<td>1952</td>
</tr>
<tr>
<td>lpm</td>
<td>-0.01 (0.1)</td>
</tr>
<tr>
<td>logit</td>
<td>0.00 (0.7)</td>
</tr>
</tbody>
</table>

2% Sample of the 12th Census data. Sample of women living in households with all Italian members whose age at first birth was either censored or greater than 15 years with high school qualification. Cohorts 1938-1956. Estimates under the preferred specification of the general tendency in the series \( F_s(y) = \text{Prob}[Y_i \leq y | S_i = s] \). Two different smoothing methods have been used: \( lpm \) stands for linear probability model (for values \( y \in [20, 23] \) polynomials with common quadratic trend in cohorts; for values \( y \in [24, 26] \) distinct polynomials linear in cohort over the pre- and post-reform cohorts); \( logit \) stands for logit model. P-values corresponding to tests for the null hypothesis of no effect (against the alternative that the effect is not null) are reported in parentheses.