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MWP 2007/22

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Empirical Evidence of the Causal Effect of Education on Fertility in Italy

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EUI Working Paper MWP No. 2007/22

ISSN 1830-7728

Printed in Italy
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Badia Fiesolana
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Italy

http://www.eui.eu/
http://cadmus.eui.eu/

Abstract

This paper assesses the causal effects of education on fertility, allowing for heterogeneity in the effects while controlling for the self-selection of women into education. Identification relies on exogenous variation in schooling induced by a mandatory school reform implemented nationwide in Italy in the early 1960s. The findings suggest that: (i) an increase from primary to junior high school education causes a large proportion of women to postpone early births; (ii) the effect vanishes at older ages and women catch up with the fertility delay before turning 26; (iii) the effect of the increase in education on the number of children a woman has is negligible; (iv) there is large heterogeneity in the fertility behaviour of women.

Keywords
Education, Motherhood, Regression Discontinuity Design.

JEL codes: J1,I2

Just A Matter of Time: Empirical Evidence of the Causal Effect of Education on Fertility In Italy

Margherita Fort *

1 Introduction and Motivation of the Paper

[1] In recent years, economists, demographers and policy makers have devoted increasing attention to the determinants of fertility, due to the greater awareness of the likely consequences of the steady decline in fertility and motherhood postponement experienced by many European countries in the last decades[2]. Concerns

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[1]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. The paper has also benefited from the suggestions of an anonymous referee and the comments of partecipants in several national and international seminars, among others: 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", at the VIth "Villa Mondragone Workshop in Economic Theory and Econometrics", at the Stockholm School of Economics. Financial support from the Italian Ministry of Education and Research (MIUR) to the project *"Evaluating the effects of labour market polices and incentives to firms and welfare policies: methodological issues and case studies"* and from the Max Weber Programme is gratefully acknowledged together with financial support from the Vienna Institute of Demography (VID), the National Bureau of Economic Research (NBER), the 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*). The usual disclaimer applies.

[2]It is important to highlight that: (i) many scholars (Kohler et al. [2002], Billari and Kohler [2004], Gustafsson [2001]) have stressed the role of motherhood postponement in explaining low

about fertility trends have motivated the introduction of specific measures aimed at countering these trends in fertility[3] (Sleebos [2003], Haas [2003], Neyer [2006]) and stimulated a lively and cross-disciplinary discussion on the topic (Boeri et al. [2005], Boushey [2005], Lutz and Skirbekk [2005]). Nevertheless, the design of effective policies depends on an understanding of how the proximate determinants of fertility operate.

Economic models of fertility behaviour (surveyed in Hotz et al. [1997]) originally focused on the description of completed fertility patterns (Leibenstein [1974], Easterlin [1975], Becker [1991]) and relied heavily on the concepts of "children quality", the cost of women's time and the relationship between marriage and fertility. Conversely, dynamic models (Butz and Ward [1980], Hotz and Miller [1988], Cigno and Ermisch [1989], Happel et al. [1984]; see Gustafsson [2001] for a survey) are mainly concerned with the timing and spacing of births and completed fertility results from the sequence of births. These models allow for the possibility that the constraints parents face in terms of prices and budget vary over the life-cycle: earning profiles, investments in human capital and human capital accumulation, time spent out of the labour force, consumption smoothing are usually considered as the main factors determining fertility decisions (Gustafsson [2001]). Recent studies have also emphasized the importance of institutional and cultural constraints (Del Boca [2002], Ichino and de Galdeano [2004]) in shaping fertility decisions.

Following insights from economic theory, public policies aimed at contrasting the recent trends in fertility operate by shaping features of the context in which individuals operate, changing the benefits and/or costs of children, helping to reconcile motherhood and work[4].

levels of (period) fertility rates; (ii) as pointed out by Bettio and Villa [1998] and Gustafsson [2001], there are significant differences in these trends among Nothern, Southern and Eastern European countries.

[3]As discussed by Bjoerklund [2006], Sweden is a rather peculiar case in which policies aimed at increasing female labour force participation where not driven by pronatalistic argument.

[4]Family-friendly policies have usually consisted of one of the following measures or a combination of these actions (Sleebos [2003], Haas [2003], Neyer [2006], Bjoerklund [2006]): cash

At the same time, individual education levels have recently been (and are currently) on the agenda of policy makers (few examples: Commission of the European Communities [2005], UN Millenium Project [2005], Council [2005], Acedo [2002], Leschinsky and Mayer [1990], Eurybase). More specifically, the relevance of the role of education and lifelong learning in increasing employment participation, productivity and economic growth has motivated the introduction of several educational reforms aimed at increasing compulsory schooling in many European countries since the 19th century.

A number of studies report a negative correlation between educational achievement and both *tempo* fertility and *quantum* fertility in most countries (e.g., Blossfeld and Huinink [1991], Nicoletti and Tanturri [2005]). Martin [2000], examining U.S. data, found that the association between educational attainment and fertility is negative in early adulthood at all qualification levels but it becomes positive later in women's fertile lifespan.

This paper focuses on the following issues: (i) do family-friendly policies and policies aimed at increasing average educational achievement pursue compatible goals?; (ii) do these policies affect all women in the same way? The rational is that an increase in education might affect the costs of childbearing and thus affect the fertility decisions of women. If the impact of education on childbearing costs is heterogenous across women, those with different preferences over children and work will react in different way.

In the European scenario described above, addressing the question of how fertility responds to exogenous variation in education might prove extremely useful in planning effective policies aimed at contrasting trends in fertility. It might be particularly relevant for Italy, which is not only characterized by low fertility rates[5]

payments for each child; free or subsidized provision of medical or education services to families with children; taxation incentives related to the presence and number of children; policies promoting gender equality in families and societies; policies encouraging societal support of children and parenting (availability of high quality and affordable childcare facilities, accessibility of jobs characterized by flexible hours, introduction of legal provisions regulating work parental leave); privileged access to public housing.

[5]In the early 1990s Italy was one of the first countries to attain and sustain the lowest-low

but also by low average education levels[6] and low female labour force participation, compared to other European countries. Besides, insights on the variability of the fertility returns of education across women might be relevant for targeting policies to specific subgroups of individuals. The assessment of the causal effect of education on fertility has been hardly achieved in empirical analyses because of the difficulties involved in tackling the unobserved heterogeneity problem: individuals might indeed differ in unobserved characteristics, such as tastes for children and work, which are likely to affect both education and fertility decisions (Blackburn et al. [1993], Mullin and Wang [2002], Ellwood et al. [2004]).

This paper assesses the causal effect of education on the timing of births in Italy exploiting exogenous variation in schooling induced by a school reform rolled out nationwide in the early 1960s[7]. It allows 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 rather than a period one, it can be profitably combined with previous work by Bratti [2003] to widen 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 beneficial future cross-country comparisons.

The remainder of the paper is organized as follows: section 2 presents the identification and the estimation strategy in greater detail, as well as the data used. The main findings are discussed in section 3. Section 4 provides arguments supporting the internal validity of the estimates. Section 5 concludes.

fertility levels, i.e. with the total fertility rate at or below 1.3 (Kohler et al. [2002]).

[6]Still nowadays the Italian schooling system is characterized by high drop-out rates and the latest extension to compulsory schooling was planned in 1999 (Genovesi [2004]).

[7]The use of educational reforms as instruments for educational attainment in economic models, introduced by Angrist and Krueger [1991] is widespread in the literature; among others, Angrist and Acemoglu [2000], Milligan et al. [2004], Patrinos and Sakellariou [2005], Lleras-Muney [2005], McCrary and Royer [2005], Puhani and Weber, Oreopoulos et al. [2006], used it recently.

2 Identification of the Causal Parameters of Interest

In most theoretical models of fertility behaviour, education is generally seen as a "modernization variable" which acts on both the demand for and the supply of children through several channels (Janowitz [1976])[8]. The leading idea is that the education level, by altering initial human capital initial endowment and subsequent further human capital accumulation, affects the marginal market wage of women, thus changing the opportunity cost of having children and inducing modification in the demand for them. 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. Conversely, Walker [1995] reached the conclusion that the steeper the earning profile the less costly are early births, holding wealth constant. He also highlighted that the effect of changes in the sequence of fertility prices depends on the relative magnitude of substitution and wealth effects: in the case of negligible wealth effects, one should expect changes in the sequence of fertility prices to affect only the timing of births.

The empirical evidence available on the causal effect of education on fertility suggests that women with higher education tend to postpone births, but the higher qualification achieved does not affect the number of children they have (Skirbekk et al. [2004] consider the case of Sweden; Bloemen and Kalwij [2001] use data

[8]Education might affect fertility *via* its effect on labour force participation, labour force attachment and time spent out of the labour force (in education) and there is a vaste 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]). Besides, women usally wait to have children until after they have finished their educational careers (Blossfeld and Huinink [1991]) 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 entrance into the labour market, especially in highly developed countries with high returns on human capital; (iv) the desire to establish oneself in a career after completing education and before having a child; (v) social norms that discourage childbearing while women or couples are still in education. Studies have also documented a positive association between the educational attainment of women and their contraceptive knowledge (Rosenzweig and Schultz [1989], Goldin and Katz [2002]): the effective use of contraceptives might contribute to increase ability in "scheduling" births according to the spouses' preferences.

on the Netherlands).

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 on fertility. Let $\mathtt{Y}$ be woman's age at first birth and let $\mathtt{D}$ represent the treatment (namely, "more schooling"). $\mathtt{D_i}$ takes the value 1 if individual $\mathtt{i}$ has a high qualification and the value 0 otherwise. $\mathtt{Y^1}$ and $\mathtt{Y^0}$ denote the *potential* outcomes (Rubin [1974], Holland [1986]): $\mathtt{Y^1}$ is the mother's age at first birth if she is exposed to the treatment, i.e. if she gets a high qualification; $\mathtt{Y^0}$ is the mother's age at first birth if she is not be exposed to the treatment, i.e. if she gets a low qualification. Thus, the parameters of interest are simply $\mathtt{E}[\mathtt{Y^0_i}] - \mathtt{E}[\mathtt{Y^1_i}]$, i.e. the average treatment effect (ATE) or $\mathtt{F^{-1}_{Y^1}(q)} - \mathtt{F^{-1}_{Y^0}(q)}$, i.e. the quantile treatment effect at quantile q (QTE)[9]. Unless $\mathtt{D}$ 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 either ATE or QTEs of education ($\mathtt{D_i}$) on fertility ($\mathtt{Y_i}$). In this application, identification of the causal effect of education on fertility relies on a regression discontinuity design[10] (Trochim [1984], Thistlethwaite and Campbell [1960]), exploiting a mandatory schooling reform rolled out nationwide in Italy in 1963. The **1963 reform** (Act N. 1859 December 31st, 1962) prescribed the unification of the previous junior high schools into 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 1st, 1963), individuals should attend school at least until junior high school (*scuola*

[9]See Koenker [2005]. Dissimilar quantile treatment effects at different quantiles $\mathtt{q}$ suggest heterogeneity of the treatment effect.

[10]An anonymous referee made the point that the identification strategy exploited here is closer to an interrupted time series design (Campbell and Stanley [1963], McDowall et al. [1985]) than to a regression discontinuity design since the treatment determining variable is not reported on the continuous scale. However, following the discussion in Lee and Card [2006] it seems legitimate to cast the econometric model as regression discontinuity design also when the treatment covariate is reported on a discrete case, althought this poses additional issues related to the misspecification of the functional form of the smoothing polynomial. Robustness of the inferencial conclusion reported in this paper is extensively documented in the supplementary material associated with it (see Fort [2007]).

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 1949[11] were compelled to attend 3 more years of schooling[12]. Compliance with the reform was far from perfect: only in 1976, did the proportion of compulsory school age children meeting their obligation approach 100% ([Brandolini and Cipollone, 2002, p. 9], Checchi [1997]).

Assignment to the treatment ("more schooling") was fully determined by the individuals' date of birth ($\mathtt{S}$), which can be observed by the analyst. Let $\bar{\mathtt{s}}$ be the *threshold* date of birth from which the increase in compulsory schooling started to be effective: a discontinuity in the conditional distribution of $\mathtt{D}$ given $\mathtt{S}$ around $\bar{\mathtt{s}}$ is expected, due to the effect of the 1963 reform. The conditional distribution of any predetermined characteristic $\mathtt{W}$ given $\mathtt{S}$ is expected to be smooth around $\bar{\mathtt{s}}$ 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 $\mathtt{D}$ given $\mathtt{S}$ would map directly into a discontinuity in the conditional distribution of $\mathtt{Y}$ given $\mathtt{S}$, provided educational achievement (the treatment $\mathtt{D}$) causally affects fertility decisions ($\mathtt{Y}$)[13]. Due to the imperfect compliance with the assignment to the treatment, this strategy identifies the average causal effect of education on the fertility for those individuals persuaded to obtain additional education by virtue of the reform (*compliers*), i.e. the strategy allows the identification of the local average treatment effect (LATE, see Angrist et al. [1996])[14]. Under particular conditions, LATE is informative of ATE (Angrist [2004]).

[11] See [Brandolini and Cipollone, 2002, p. 11], Flabbi [1999].

[12] The law prescribed also that, in case of need, new schools should be built (by 1966) in municipalities with more than 3.000 inhabitans.

[13] The discontinuity in the distribution of $\mathtt{Y}$ will be proportional to the average causal effect of education on fertility in the same way that the reduced form effect is proportional to the structural parameter in an instrumental variable setting (Hahn et al. [2001]).

[14] 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[15] 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 educational 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, Imbens and Rubin [1997] show that, under the LATE identifying assumptions[16], 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[17]. Equation (3) represents the causal effect of education at $\bar{s}$ on $F(y)$ for *compliers*.

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

where: (i) $F_{.d}^{C_{\bar{s}}}(y) \equiv \mathtt{Prob}[Y_i^d \leq y | C_{\bar{s}}]$, $d \in \{0, 1\}$, are the *compliers*' potential outcome distributions; (ii) $F_{z.}(y|\bar{s}) \equiv \mathtt{Prob}[Y_i \leq y | S_i = \bar{s}, Z_i = z]$, $z \in \{0, 1\}$; (iii) Z_i is a dummy variable which describes the assignment to the treatment: it takes

[15] See Hahn et al. [2001] for a formal discussion on identification and estimation of treatment effects in a regression-discontinuity design.

[16] 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.

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

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

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

where $F_{.1}^{C_{\bar{s}}}(\cdot)$ and $F_{.0}^{C_{\bar{s}}}(\cdot)$ denote the *potential outcomes*' distributions among *compliers* at $\bar{s}$; $F_{zd}(y|\bar{s})$ denotes 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., $Z \equiv I(S_i \geq \bar{s})$); $\phi_a(\bar{s})$, $\phi_n(\bar{s})$, $\phi_c(\bar{s})$ represent the population proportions of *always takers*, *never takers* and *compliers* - at $\bar{s}$ -, respectively. Under the identifying assumptions, the following also holds: $\phi_a(\bar{s}) = E[D|Z = 0, S = \bar{s}]$; $\phi_n(\bar{s}) = 1 - E[D|Z = 1, S = \bar{s}]$; $\phi_c(\bar{s}) = 1 - \phi_a(\bar{s}) - \phi_n(\bar{s}) = E[D|Z = 1, S = \bar{s}] - E[D|Z = 0, S = \bar{s}]$. The proportion of *compliers* ($\phi_c(\bar{s})$) is exactly the effect of Z on D at the threshold $\bar{s}$.

the value 1 if individual $\mathtt{i}$ is assigned to the treatment and 0 otherwise (i.e., $\mathtt{Z} \equiv \mathtt{I}(\mathtt{S_i} \geq \bar{\mathtt{s}})$); and $\phi_c(\bar{s})$ is the proportion of *compliers* at $\bar{\mathtt{s}}$. $\mathtt{F_{1.}}(\mathtt{y}, \mathtt{s}) - \mathtt{F_{0.}}(\mathtt{y}, \mathtt{s})$ is the intention-to-treat effect, i.e. the difference in the outcome $\mathtt{F}(\mathtt{y})$ by the instrument $\mathtt{Z}$, regardless of actual treatment status, that is regardless of the observed value of $\mathtt{D}$.

Note that one can characterize the sub-population of *compliers* by their average characteristic $\mathtt{X}$ ([Angrist, 2004, p. C69])[18].

2.1 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 educational achievement and the size of the discontinuities over the distribution of mothers' ages at first birth $\mathtt{Y_i}$. Although estimators proposed in the literature (Hahn et al. [2001]) emphasize the use of local polynomial techniques, these tecniques are no longer appropriate when the covariate that determines the treatment is discrete (or is reported in coarse intervals) (Lee and Card [2006]). 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 $\mathtt{S}$ and $\mathtt{Z} \equiv \mathtt{1}(\mathtt{S} \geq \bar{\mathtt{s}})$[19] and this will be used to construct counterfactuals at the threshold.

The analysis is based on the Census (2% sample, 1981; 1% sample, 1991) data (see ISTAT [1983], ISTAT [1991])[20] and tables of cohort measures of fertil-

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

[19] Sensitivity of the parametric results to different smoothing techniques, specifically to the choice of the degree of smoothness, is checked and documented extensively in the supplementary material associated with this paper (see Fort [2007]).

[20] 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 of analysis of the causal relationship between education and fertility in settings such as the one considered in this

ity published by the National Institute of Statistics (ISTAT [1997])[21]. The analysis on the timing of childbearing is based on the 1981 Census data; the analysis on the timing of marriage is based on the 1991 Census data[22]; and, the analysis on completed fertility is based on tables produced by the National Institute of Statistics. Only census samples provide information on the individual's education.

The mother's age at the birth of the oldest child (still at home) is referred to as "mother's age at first birth". As a consequence: (i) it is only possible to calculate a 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 the parental home, regardless of their marital status. The first point raised is definitely unlikely to affect the identification of the causal effect of education on the timing of births. Indeed, this can be ascertained 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[23]. Since the mean age at first birth of women of these cohorts is nearly 25 ([ISTAT, 1997, Table 2, p. 82]) and Italian adolescents tend to leave the parental home late[24], this is most likely to hold in practice. Mothers and children might be mismatched when the natural mother of each child is not the household head or the wife of the household head[25]. However, the identification

application.

[21]The volume collects information on completed fertility and fertility by order of birth of women born between 1920 and 1963, building on different data sources, namely data from registry offices (period 1969-1993), official data from the Annals of Demographic Statistics and the archives of the National Institute of Statistics (ISTAT) (1952-1968), data from the survey on fecundity held by ISTAT at the time of the 1961 Census and data from a survey on household structures and fertility behaviour held by ISTAT in 1993.

[22]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 individual's year of marriage.

[23]Note that children of women born between 1948 and 1952, who gave birth between the ages of 18 and 26, were between 2 and 15 years old in 1981.

[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 it is 26.7, whereas for females of the same cohorts it is 23.6 ([Billari, 2000, Tab. 3.8, p. 96]).

[25]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.

strategy is unaffected provided this mismatch takes place the same way on both sides of the threshold. The analysis will be limited to the timing of births occurring before these women turn 27, since the mother's age at first birth is right-censored.

The 1991 Census collected data on the year of the *last* marriage, which does not necessarly correspond to the *first* marriage. However, in recent 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 practices (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 Census data are not adequate to examine completed fertility[26] and it was necessary to rely on data published by ISTAT [1997].

3 Main Findings

This section firstly describes the effect of the 1963 school reform on education and secondly it discusses the estimates of the causal effect of education on fertility.

3.1 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[27]. The

[26]Most of the women of the cohorts 1938-1956 had not yet completed their fertile lifespan by 1981 and 1991.

[27]The undelying 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. Descriptive statistics are reported in Table 1.

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 it under the new law. $\mathtt{D_i}$, the binary variable describing treatment status, takes the value 1 if individual $\mathtt{i}$ has attained *exactly* junior high school qualification and 0 if he/she has a lower qualification[28]. The effect of the reform is estimated at $\mathtt{s} = 1949$, 1950, 1951, 1952 (see Table 3). The motivation to consider this particular set of values of $\mathtt{s}$ is twofold: firstly, it is interesting to explore whether the 1963 reform had different effects depending on the time elapsed since primary school completion[29]; secondly, extrapolation becomes less plausible as one moves further from the threshold year $\bar{\mathtt{s}} = \mathtt{1949}$. Table 3 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 $\mathtt{s}$ closer to 1952: women who were 14 at the time of the 1963 reform, did not go back to school to fulfill their obligations, whereas some women, for whom the time elapsed between the completion of primary school and the year in which the reform came into force was smaller, did, so that the reform exerted a greater influence on this 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 a high school qualification: no effect is detectable (see Table 4 and Figure 1). This result is robust to the choice of the smoothing technique.

[28]The analysis is limited to women with at most junior high school qualification. The analysis has also been carried out using data from the whole sample of women and defining treated individuals those women with *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 3 and lead to consistent inferential conclusions. These estimates, not reported here for brevity, are reported in the supplementary material associated with this paper (see Fort [2007]).

[29]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.

3.2 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 women's ages at marriage and on the total number of children a woman has.

The 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 $18, 20, 22, 24$ [30]. Point estimates of the discontinuity (intention-to-treat effects), are reported in Table 5 and Table 6. In short, the evidence points toward the conclusion that the 1963 reform led to: (i) a reduced likelihood (nearly 3 percentage points) of giving birth by younger ages (19, 20, 21); (ii) positive effects on the likelihood of giving birth by the age $\mathtt{y} = 23$; (iii) negligible effects on the likelihood of giving birth by older ages (25, 26); (iv) negligible effects on the timing of marriage at any $\mathtt{y} \in [18, 26]$.

To provide insights into the causal effect of education on 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 outcome' distribution functions increase monotonically, an alternative *naive* estimator[31] has also been used. The sampling distribution of the revised estimates is obtained by resampling from the empirical distribution of $(\mathtt{Y}, \mathtt{D}|\mathtt{s})$. Estimates and standard errors (computed *via* delta method for the Wald estimates) are reported in Table 7, Table 8 and in Table 9 (see also Figure 4). The figures suggest that education causes a postponement in the transition to motherhood only in women who, in the absence of the treatment (i.e., "more schooling"), would have had their first child

[30] Note that the same pattern apparent in these graphs is observed considering the sample of all women. These graphs, not reported for brevity, are included in the supplementary material associated with the paper (see Fort [2007]).

[31] See equations 1 and 2 in footnote 16. Imbens and Rubin [1997] considered alternative estimators and found that a *naive* estimator of the density functions, obtained simply by imposing non negativity, performs essentially as an estimator based on the likelihood. Here, their approach is followed by operating directly on the cumulative distribution functions, i.e. $\widehat{\mathtt{F}^{\mathtt{C}_{\bar{\mathtt{s}}}}_{.\mathtt{k}}(\mathtt{y})}^{*} = \min\Big(\max\Big(0, \widehat{\mathtt{F}^{\mathtt{C}_{\bar{\mathtt{s}}}}_{.\mathtt{k}}(\mathtt{y})}, \widehat{\mathtt{F}^{\mathtt{C}_{\bar{\mathtt{s}}}}_{.\mathtt{k}}(\mathtt{y}-1)}^{*}\Big), 1\Big), \mathtt{k} = 0, 1.$

by young ages (see also Figure 4). On the whole, this does not seem to be driven mainly by the effect of education on the timing of marriage: indeed, although the pattern of the effect on the distribution of women's ages at marriage is consistent with that observed on the distribution of mothers0 ages at first birth, the magnitude of the effect is generally negligible[32] and there is no evidence of any effect on the number of children a woman has.

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 to be those who, in the absence of the treatment, face a lower opportunity cost of having children. Therefore, these women are less likely to participate in the labour market. A rise in the level of education achieved, by increasing their current market wage[33], increases the probability that they participate in the labour market and increases the opportunity cost of children[34]. Thus, women end up delaying early childbearing. The increase in qualification (from primary to junior high school qualfication) leads to an increase in the opportunity cost of children but the earning profiles of these women remain rather flat, so that the incentives to postpone births only operate at younger ages (19-22). Testing this hypothesis requires data on work histories[35]. These further analyses are already on my research agenda.

[32]Note that the precision of these estimates is much lower.

[33]Brandolini and Cipollone [2002] exploited the 1963 reform as an 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% increases in wage due to the 3-year increase in education induced by the 1963 reform.

[34]The study by Pencavel [1998], based on CPS data, suggests that the increase in women's wages 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 workplace relative to the household.

[35]I plan to rely on data from the Work Histories Italian Panel (WHIP), released by *Laboratorio Revelli*, Center for Employment Studies (see `http://www.laboratoriorevelli.it/whip`).

Now we turn to the issue of the heterogeneity in the impact across individuals. The graphs[36] in Figure 5 depict the cumulative distribution functions of $\mathtt{Y}^1$ and $\mathtt{Y}^0$ for the sub-population of *compliers* and for the *non-compliers* (*always takers* and *never takers*). There is some indication of heterogeneity in 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*[37]. Conversely, there is no clear-cut evidence of heterogeneity in the impact of education on the timing of marriage. This consideration suggests that the fertility behaviour of the women affected by the reform is likely to be substantially different from that of the average woman in the population.

Compliers are mostly resident in the northern regions of Italy (see Table 10, the characterization follows from Angrist [2004]): this might be due to the well-known economic differences between Centre-North and South of Italy: southern regions have traditionally been characterized by lower levels of 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 degree of compliance[38].

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 three reasons:

[36] Additional graphs for the case $\mathtt{s} = 1950$ and $\mathtt{s} = 1951$ show broadly the same pattern observed at $s = 1952$ and are not reported for brevity. They are included in the supplementary material associated with this paper (see Fort [2007]).

[37] Differences between the fertility behaviour of *compliers* and *non-compliers* become negligible at older ages, i.e. $\mathtt{y} = 25$, $\mathtt{y} = 26$.

[38] It is also well know that fertility behaviour in northern and central regions and that in southern regions differs: in southern regions, the traditional family structure is more common, i.e. the wife primarily tends to the 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 for these analyses to be conducted at the macro-region level: sample sizes become too small for meaningfull elaborations.

firstly, Bratti considers a period measure of fertility, conversely here the analysis is based on cohort measures of fertility; secondly, Brattu considers the effects of education on the probability of *a birth event*[39], whereas here the analysis is focused on the timing of first births.

4 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 during the 1970s women's position in the society, in Italy, went through major changes, partly driven 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 affected women born before and after 1949 differently, 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 $\mathtt{F_s(y)}$[40] (as a function of $\mathtt{s}$) is fully attributable to the effect of the 1963 reform and it is not driven by the above-mentioned innovations. To test the validity of this assumption, the fertility behaviour (i.e., the mother's age at first birth) of women who achieved high school qualification is be considered. Since these women were not affected by the 1963 reform (see Table 4 and Figure 1), one would expect their fertility behaviour to change smoothly over cohorts. On the whole, as the figures in Table 11 suggest, the changes are indeed negligible[41]. In principle, women with a high school qualification might have different fertility behaviour from those with a junior high school qualification. However, the first group of women would probably be more affected by, for

[39]"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].

[40]$\mathtt{F_s(y)}$ denotes the proportion of women in the cohort $\mathtt{s}$ who bear their first child by age $\mathtt{y}$.

[41]The small number of events occurring ("events" are births to women of the 1938- 1956 cohorts with a high school qualification) does not allow for precise estimates or for ages younger that $y = 20$ to be considered. Notwithstanding this caution, the precision of the estimates remains quite low.

instance, the introduction of the pill (see Goldin and Katz [2002]).

Migrations might also be relevant confounders: between 1950 and 1970, both internal and international migration were very pronounced in Italy (Pugliese [2002]). Compared to international migration, internal migration involved a larger number 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 as a consequence of restrictive immigration policies adopted by the host country. Emigrants and (internal) migrants at that time were mostly prime-age poorly educated men; conversely, during the 1980s and 1990s, emigrants were mostly highly educated young men ([Pugliese, 2002, p.64]). The intensity of internal migration increased in the 1950s and exihibited a peak in 1961-1963, which is partly attributable to the effects of post-census adjustments and the repeal of the fascist law on urbanization[42] (see Bonifazi and Heins [2000], Treves [1976]). The intensity of internal migration then started to decrease during the 1970s. Bonifazi and Heins [2000] noted that these trends are mainly driven by flows within the same province. This does not affect the analyses of this paper, since they 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[43]. As a consequence of migration flows, individuals who were not in Italy at the time the reform was introduced but returned during the 1970s or the 1980s (*return migration*) might have been included in the analysis and individuals 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 led to *attenuating* the effect of the reform on *compliers*.

[42]This law (Law $No.358$, April 9, 1931) placed severe limitations on changes of residence; the act was repealled by the Law $No.5$ in 1961.

[43]Data on place of birth would have mirrored more truthfully the place were individuals attended junior high school. Unfortunately, these data were not available; I relied instead on data on the region of residence at the time of the Census survey.

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 had been in place one year earlier.

Lastly, the small number of *compliers* supports the stable unit treatment value assumption: the behaviour of less than 6% of the whole population would have been unlikely to induce spill-over effects.

In short, 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*.

5 Concluding Remarks

This paper provides evidence of the role of education in determining the timing of first birth exploiting exogenous variation in schooling induced by a school reform introduced in Italy in 1963. The findings suggest that a large proportion of the women affected by the reform postponed early first births but caught up with this fertility delay before turning 26. Consistently with this, the effect of education on the number of children a woman has seems to be negligible at the country level. Moreover, the results on the timing of births do not seem to be driven by a causal effect of education on the timing of marriage.

The internal validity of the research design has been extensively discussed: evidence based on the data at hand suggests that the 1963 reform represents a valid instrument, helping 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 and generalizing this effect to a wider set of individuals typically requires relying on stronger

conditions than those which guarantee local identification.

In addition, the findings suggest that heterogeneity in the fertility behaviour across women is substantial.

Since new mandatory schooling laws have been introduced in many countries in recent decades, the identification strategy employed in this study can be easily replicated in other countries. Indeed, the subpopulation of *compliers* 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. The role of education in reducing rates of teenage pregnancy has long been emphasized: the results presented here give further evidence for this for Italy.

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

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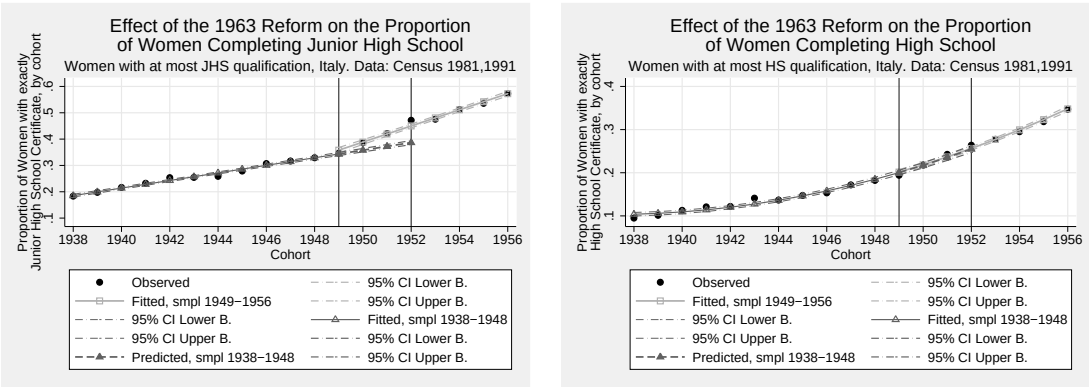
Figure 1: Effect of the 1963 Reform on Women's Schooling Achievement.

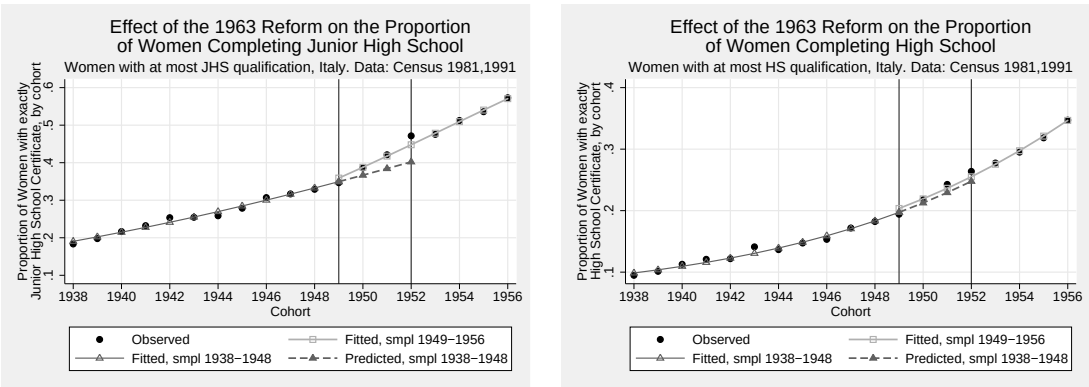
Effect on the Proportion of Women with exactly

Junior High School Qualification **High School Qualification**

Smoothing *via* linear probability model

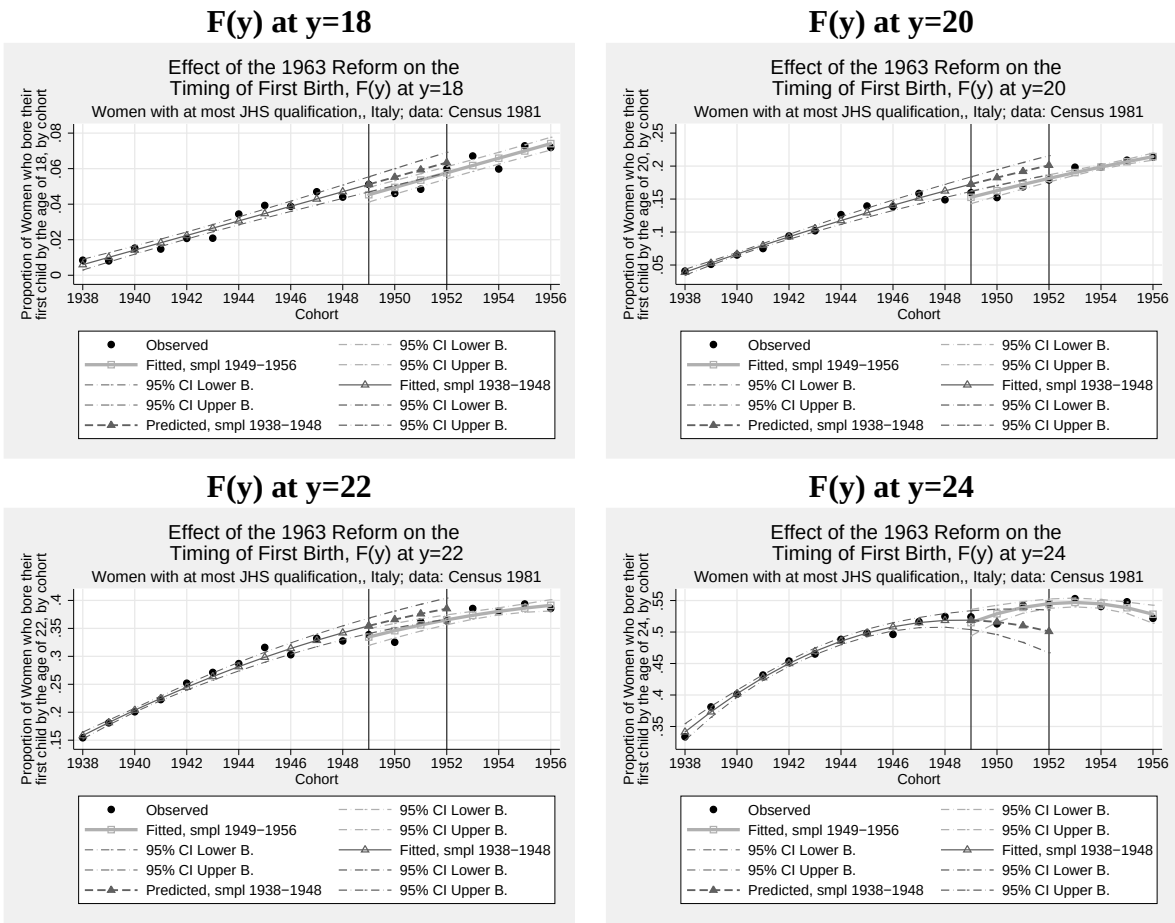


Smoothing *via* logit model



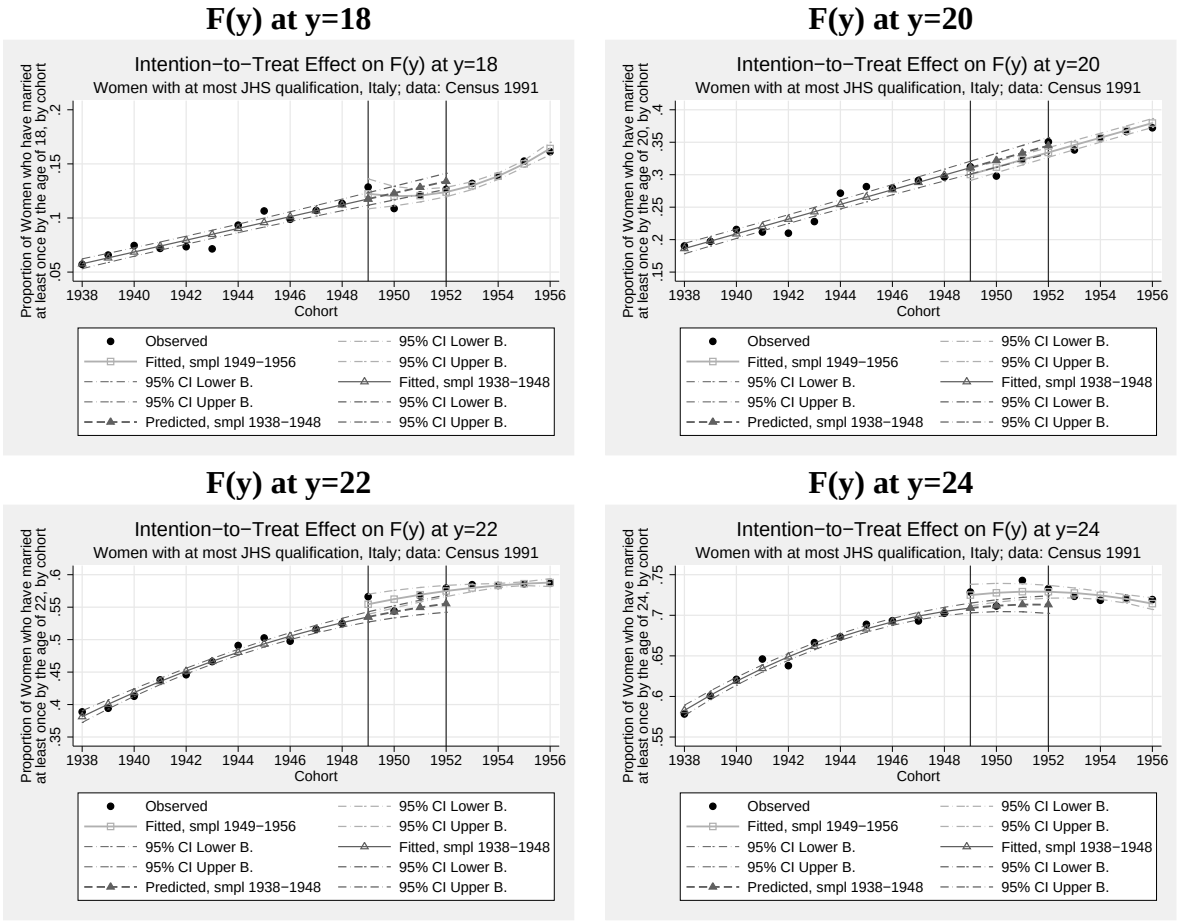
1981 and 1991 Census data, 2% and 1% sample, respectively. Italian women born between 1938 and 1956. **Right-hand panels**: see Table 3. **Left-hand panels**: see Table 4. Estimates are obtained using weighted linear regression correcting for heteroskedasticity due to data grouping (by cohort and census, 19*2=38 clusters). Graphs report averages by birth cohort of the observed, fitted and predicted values.

Figure 2: Effect of the 1963 reform on the distribution of women's ages at first birth, i.e. on $\mathtt{F(y)} = \mathtt{Prob}[\mathtt{Y_i} \leq \mathtt{y}]$ at distinct values of $\mathtt{y}$, where $\mathtt{Y}$ denotes women's ages at first birth.



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. **Top right-hand panel:** smoothing polynomial over cohorts 1938-1946: linear in cohort. **Top left-hand panel:** smoothing polynomial over cohorts 1938-1956: quadratic in cohort. **Bottom right-hand panel:** smoothing polynomial over cohorts 1938-1956: quadratic in cohort. **Bottom left-hand panel:** smoothing polynomial over cohorts 1938-1948: quadratic in cohort; smoothing polynomial over cohorts 1949-1956: quadratic in cohort. **Types of polynomials fitted**: linear probability models. Estimates are obtained using weighted linear regression correcting for heteroskedasticity due to data grouping (by cohort, 19 clusters). Using logit models, one gets broadly the same results.

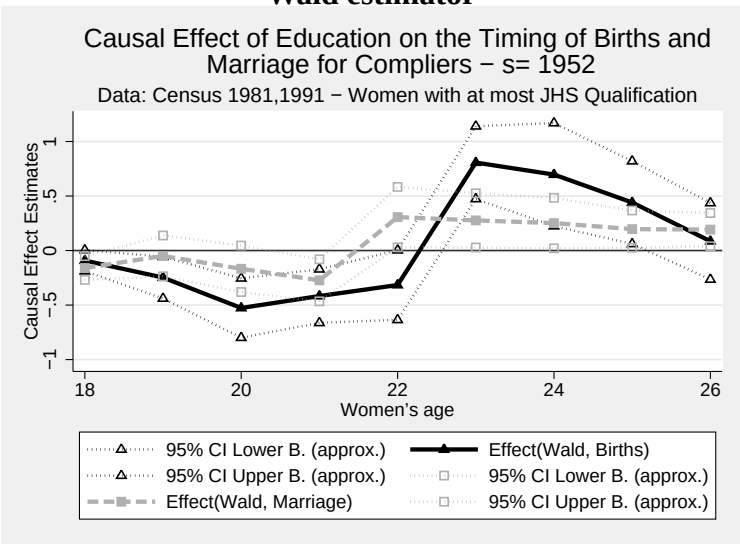
Figure 3: Effect of the 1963 reform on the distribution of women's ages at marriage, i.e. on $\mathtt{F(y)} = \mathtt{Prob}[\mathtt{Y_i} \leq \mathtt{y}]$ at distinct values of $\mathtt{y}$, where $\mathtt{Y}$ denotes women's ages at marriage.



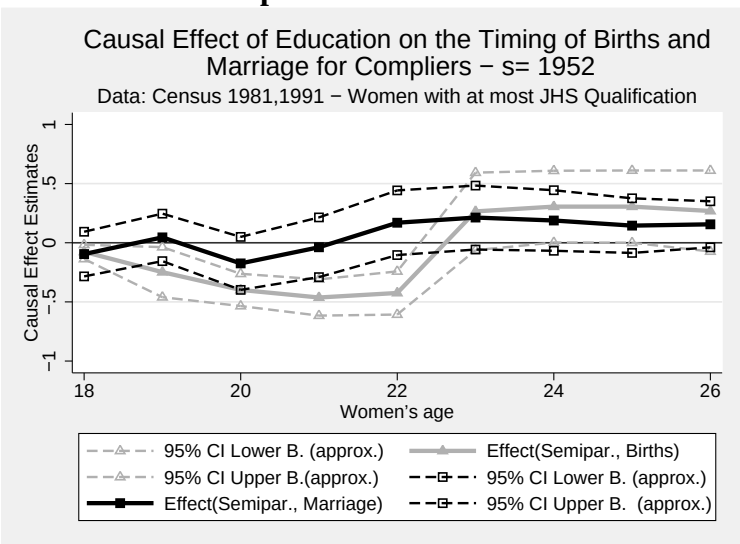
1% Sample of the 13th Census data. Sample of women with at most junior high school qualification. **Top right-hand panel:** smoothing polynomial over cohorts 1938-1948: linear in cohort; smoothing polynomial over cohorts 1949-1956: quadratic in cohort. **Top left-hand panel:** smoothing polynomial over 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% samples respectively. Italian women with at most junior high school qualification; women born between 1938 and 1956.

Wald estimator



Semiparametric estimator

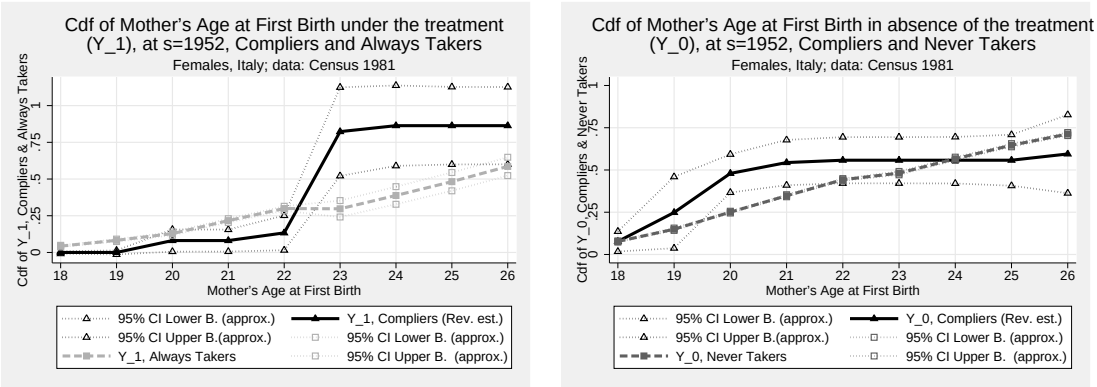


Estimates of the causal effects of education on the timing of births and on the timing of marriage are based on the sample of women with at most junior high school qualification (cohorts 1938-1956) from the 1981 Census data and the 1991 Census data, respectively. Standard errors are based on the delta method (Wald estimator) and on a semiparametric bootstraap procedure (semiparametric estimator), with 10,000 replications. 95% confidence intervals for the Wald estimates for the semiparametric estimates are computed relying on normal approximation.

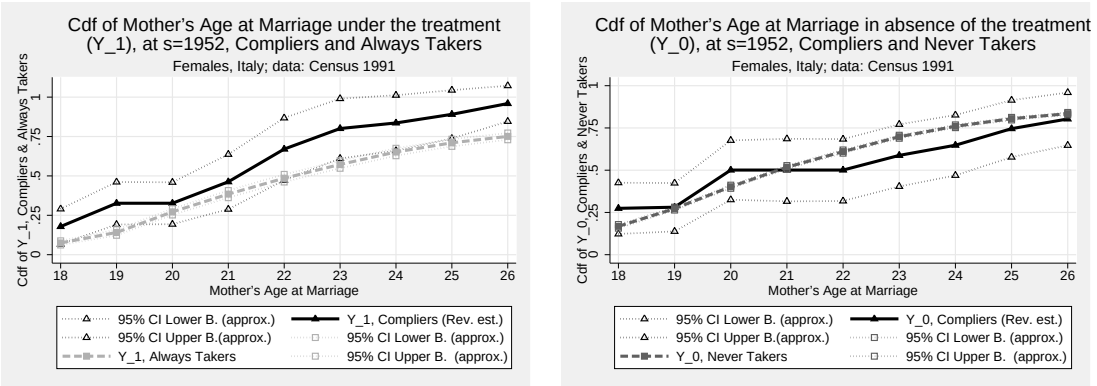
Figure 5: *Compliers*' distribution of Y^1 (outcome under the effect of the treatment, i.e. if the woman achieves junior high school qualification) and Y^0 (outcome in the absence of the treatment, i.e. if the woman achieves less than junior high school qualification). Revised estimates.

Y^1 Y^0

Outcome: Woman's Age at First Birth



Outcome: Woman's Age at Marriage



Analyses of 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 bootstraap procedure, with 10,000 replications. 95% confidence intervals are computed relying on normal approximation.

Table 1: Descriptive Statistics. Birth Cohorts 1932-1956. Observed Distribuition of Qualification Levels by Reform Status. Census data.

12th Census (1981)

Highest Qual.	North 1932-48	North 1949-52	North 1953-56	Centre 1932-48	Centre 1949-52	Centre 1953-56	South 1932-48	South 1949-52	South 1953-56
Females									
No Qual.	3.91	1.62	1.43	7.49	2.41	1.65	19.08	8.90	7.11
Prim. Sc.	56.75	37.74	25.06	52.74	37.88	25.54	50.95	44.48	36.99
Jun. H. Sc.	24.36	33.43	38.41	20.19	26.02	31.72	14.07	20.78	24.99
High Sc.	11.37	20.41	29.52	14.29	24.25	34.11	11.48	17.91	24.91
College	3.61	6.80	5.58	5.30	9.43	6.98	4.42	7.93	6.00
	100.00	100.00	100.00	100.00	100.00	100.00	100.00	100.00	100.00
N. of ind.	39,429	13,935	13,541	16,363	5,760	5,700	26,422	10,503	11,425
Males									
No Qual.	2.42	1.24	0.98	3.20	1.26	1.49	10.09	5.09	4.69
Prim. Sc.	45.88	28.77	19.77	42.39	25.33	18.08	44.59	34.67	27.02
Jun. H. Sc.	29.28	37.44	41.42	28.03	34.89	36.20	24.68	31.62	35.63
High Sc.	16.39	24.18	32.36	18.04	27.67	37.51	14.41	21.00	27.72
College	6.03	8.57	5.47	8.34	10.85	6.72	6.23	7.61	4.94
	100.00	100.00	100.00	100.00	100.00	100.00	100.00	100.00	100.00
N. of ind.	39,892	13,900	13,776	15,974	5,483	5,503	25,796	10,603	11,124

13th Census (1991)

Highest Qual.	North 1932-48	North 1949-52	North 1953-56	Centre 1932-48	Centre 1949-52	Centre 1953-56	South 1932-48	South 1949-52	South 1953-56
Females									
No Qual.	3.48	1.32	1.30	6.17	2.54	1.72	16.73	7.17	6.1
Prim. Sc.	53.76	34.45	20.80	49.11	34.06	21.37	49.47	41.53	31.73
Jun. H. Sc.	25.32	34.93	39.40	23.07	28.24	33.60	16.92	23.62	29.52
High Sc.	14.01	22.00	30.39	16.11	25.65	33.13	12.41	19.54	24.29
College	3.43	7.30	8.12	5.55	9.50	10.18	4.46	8.15	8.37
	100.00	100.00	100.00	100.00	100.00	100.00	100.00	100.00	100.00
N. of ind.	19,246	6,783	6,871	7,964	2,904	2,976	12,948	5,302	5,475
Males									
No Qual.	1.81	0.97	1.08	2.99	1.40	1.69	8.59	4.93	4.68
Prim. Sc.	42.04	24.97	16.30	39.34	23.78	14.91	40.98	29.28	20.78
Jun. H. Sc.	31.15	36.94	41.71	29.01	32.89	38.64	27.61	34.37	40.72
High Sc.	18.69	27.77	31.56	20.45	31.21	33.54	16.13	23.13	24.47
College	6.31	9.35	9.35	8.21	10.72	11.22	6.70	8.29	9.35
	100.00	100.00	100.00	100.00	100.00	100.00	100.00	100.00	100.00
N. of ind.	19,095	7,098	6,951	7,956	2,919	2,844	12,385	5,028	5,423

Table 2: Descriptive Statistics: Sample Size and amount of censoring assessing Mother's Age at First Birth. Cohorts 1940-1956. 12th Census data*

		Northern Italy			
Cohort	Sample Size	Censored obs. (1st Birth)	**Cohort**	Sample Size	Censored obs. (1st Birth)
1940	3,426	313 (9.1%)	**1949**	3,193	383 (12.0%)
1941	3,140	259 (8.3%)	**1950**	3,096	445 (14.4%)
1942	3,137	269 (8.6%)	**1951**	2,817	485 (17.2%)
1943	3,101	296 (9.6%)	**1952**	2,814	604 (21.5%)
1944	2,970	285 (9.6%)	**1953**	2,665	596 (22.4%)
1945	2,579	200 (7.8%)	**1954**	2,635	717 (27.2%)
1946	3,664	361 (9.9%)	**1955**	2,496	788 (31.6%)
1947	3,403	336 (9.9%)	**1956**	2,241	807 (36.0%)
1948	3,365	350 (10.4%)			
		Central Italy			
Cohort	Sample Size	Censored obs. (1st Birth)	**Cohort**	Sample Size	Censored obs. (1st Birth)
1940	1,406	112 (8.0%)	**1949**	1,279	132 (10.3%)
1941	1,202	88 (7.3%)	**1950**	1,244	136 (10.9%)
1942	1,273	99 (7.8%)	**1951**	1,128	154 (13.7%)
1943	1,248	80 (6.4%)	**1952**	1,159	196 (16.9%)
1944	1,184	101 (8.5%)	**1953**	1,112	226 (20.3%)
1945	1,148	91 (8.0%)	**1954**	1,065	237 (27.2%)
1946	1,451	109 (7.5%)	**1955**	950	252 (26.5%)
1947	1,405	123 (8.8%)	**1956**	891	269 (30.2%)
1948	1,338	134 (10.0%)			
		Southern Italy			
Cohort	Sample Size	Censored obs. (1st Birth)	**Cohort**	Sample Size	Censored obs. (1st Birth)
1940	2,244	152 (6.8%)	**1949**	2,281	179 (7.9%)
1941	1,877	134 (7.1%)	**1950**	2,148	202 (9.4%)
1942	1,960	131 (6.7%)	**1951**	2,128	230 (10.8%)
1943	1,954	129 (6.6%)	**1952**	2,049	217 (10.6%)
1944	1,836	110 (6.0%)	**1953**	2,073	275 (13.3%)
1945	1,973	133 (6.7%)	**1954**	2,078	297 (14.3%)
1946	2,198	159 (7.2%)	**1955**	2,027	357 (17.6%)
1947	2,483	182 (7.3%)	**1956**	1,804	335 (19.6%)
1948	2,400	178 (7.4%)			

* Subsample of 331,475 Households with all Italian members, where a woman is present and age at 1st birth is greater or equal to 15 years.

Table 3: 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.

Overall sample size: 162,289. Average cohort sample size: nearly 4,271.								
Smoothing Tecnique	**Linear Probability Model**				**Logit Model**			
s	**1949**	**1950**	**1951**	**1952**	**1949**	**1950**	**1951**	**1952**
$\widehat{\phi_c(s)}$	0.01	0.03	0.05	0.06	0.01	0.02	0.02	0.03
t-test	2.64	6.15	10.03	13.84	1.41*	3.10*	4.64*	5.88*
p-value	0.01	0.00	0.00	0.00	0.17	0.00	0.00	0.00

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 $\mathtt{E[D|S]} = \beta_0 + \beta_1 \mathtt{1(cens = 1991)} + \beta_2(\mathtt{S} - \mathtt{1937}) + \beta_3(\mathtt{S} - \mathtt{1937})\mathtt{1(cens = 1991)} + \beta_4 \mathtt{Z} + \beta_5 \mathtt{Z}(\mathtt{S} - \mathtt{1937}) + \varepsilon$, $\mathtt{S}$ is the individual birth cohort,$\mathtt{1(A)}$ takes the value one if the event $\mathtt{A}$ is true and $\mathtt{Z} = \mathtt{1(S \geq 1949)}$, $\mathtt{D} = \mathtt{1}$(h as exactly junior high school qualification). The effect of the reform on education at $\mathtt{S} = \mathtt{s}$ (i.e., $\mathtt{E[D|S = s, Z = 1]} - \mathtt{E[D|S = s, Z = 0]}$), $\mathtt{s} \in \{\mathtt{1949, 1950, 1951, 1952}\}$ is given by $\beta_4 + \beta_5(\mathtt{s} - \mathtt{1937})$. 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 $\mathtt{log}(\frac{\mathtt{p_s}}{\mathtt{1-p_s}}) = \gamma_0 + \gamma_1 \mathtt{1(cens = 1991)} + \gamma_2(\mathtt{S} - \mathtt{1937}) + \gamma_3 \mathtt{Z} + \gamma_4 \mathtt{Z}(\mathtt{S} - \mathtt{1937}) + \zeta$, $\mathtt{p_s} \equiv \mathtt{E[D|S]} \equiv \mathtt{Prob(D = 1|S)}$. * For each $\mathtt{s} \in \{\mathtt{1949, 1950, 1951, 1952}\}$, the hypothesis tested under the logit specification is $\mathtt{H_0} : \gamma_3 + \gamma_4(\mathtt{s} - \mathtt{1937}) = \mathtt{0}$ against $\bar{\mathtt{H}}_0$, i.e. a necessary and sufficient condition for the null hypothesis $\mathtt{H_0} : \phi_\mathtt{c}(\mathtt{s}) = \mathtt{0}$ vs $\mathtt{H_1} : \phi_\mathtt{c}(\mathtt{s}) \neq \mathtt{0}$. The effect of the 1963 reform on education achievement at $\mathtt{s}$ under this specification is given by $\mathtt{E[D|S = s, Z = 1]} - \mathtt{E[D|s, Z = 0]} = \frac{\mathtt{exp}[\gamma_0 + \gamma_1 \mathtt{1(cens=1991)} + \gamma_2(\mathtt{s-1937})]\mathtt{exp}[\gamma_3 + \gamma_4(\mathtt{s-1937})]}{\mathtt{1+exp}[\gamma_0 + \gamma_1 \mathtt{1(cens=1991)} + \gamma_2(\mathtt{s-1937})]\mathtt{exp}[\gamma_3 + \gamma_4(\mathtt{s-1937})]} - \frac{\mathtt{exp}[\gamma_0 + \gamma_1 \mathtt{1(cens=1991)} + \gamma_2(\mathtt{s-1937})]}{\mathtt{1+exp}[\gamma_0 + \gamma_1 \mathtt{1(cens=1991)} + \gamma_2(\mathtt{s-1937})]}$. The corresponding estimates for the 1981 Census and 1991 Census are broadly equal: differences are of the order $\mathtt{10^{-3}}$.

Table 4: 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 Tecnique	**Linear Probability Model**				**Logit Model**			
s	**1949**	**1950**	**1951**	**1952**	**1949**	**1950**	**1951**	**1952**
$\widehat{\phi_c(s)}$	0.00				0.01			
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 $\mathtt{E}[\mathtt{D}|\mathtt{S}] = \beta_0 + \beta_1 1(\mathtt{cens} = 1991) + \beta_2(\mathtt{S} - 1937)^2 + \beta_4 \mathtt{Z} + \varepsilon$, $\mathtt{S}$ is the individuals' birth cohort, $1(\mathtt{A})$ takes the value one if the event $\mathtt{A}$ is true and $\mathtt{Z} = 1(\mathtt{S} \geq 1949)$, $\mathtt{D} = 1$(has exactly high school qualification). The effect of the reform on education at $\mathtt{S} = \mathtt{s}$ (i.e., $\mathtt{E}[\mathtt{D}|\mathtt{S} = \mathtt{s}, \mathtt{Z} = 1] - \mathtt{E}[\mathtt{D}|\mathtt{S} = \mathtt{s}, \mathtt{Z} = 0]$), $\mathtt{s} \in \{1949, 1950, 1951, 1952\}$ is given by β_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). $\mathtt{R}^2$ of the regression is around 0.99. The logit model fitted, using weighted regression, is $\mathtt{log}(\frac{\mathtt{p_s}}{1-\mathtt{p_s}}) = \gamma_0 + \gamma_1 1(\mathtt{cens} = 1991) + \gamma_2(\mathtt{S} - 1937)^2 + \gamma_3(\mathtt{S} - 1937)^2 1(\mathtt{cens} = 1991)$ $+\gamma_4 \mathtt{Z} + \zeta$, $\mathtt{p_s} \equiv \mathtt{E}[\mathtt{D}|\mathtt{S}] \equiv \mathtt{Prob}(\mathtt{D} = 1|\mathtt{S})$.

* For each $\mathtt{s} \in \{1949, 1950, 1951, 1952\}$, the hypothesis tested under the logit specification is $\gamma_4 = 0$ against $\bar{\mathtt{H}_0}$, i.e. a necessary and sufficient condition for the null hypothesis $\mathtt{H}_0 : \phi_\mathtt{c}(\mathtt{s}) = 0$ vs $\mathtt{H}_1 : \phi_{\bar{\mathtt{c}}}(\mathtt{s}) \neq 0$. The effect of the 1963 reform on education achievement at s under this specification is given by
$\mathtt{E}[\mathtt{D}|\mathtt{S} = \mathtt{s}, \mathtt{Z} = 1] - \mathtt{E}[\mathtt{D}|\mathtt{s}, \mathtt{Z} = 0] =$

$$\frac{\mathtt{exp}[\gamma_0+\gamma_1 1(\mathtt{cens}=1991)+\gamma_2(\mathtt{s}-1937)^2+\gamma_3(\mathtt{s}-1937)^2 1(\mathtt{cens}=91)]\mathtt{exp}[\gamma_4]}{1+\mathtt{exp}[\gamma_0+\gamma_1 1(\mathtt{cens}=1991)+\gamma_2(\mathtt{s}-1937)^2+\gamma_3(\mathtt{s}-1937)^2 1(\mathtt{cens}=91)]\mathtt{exp}[\gamma_4]}$$

$$-\frac{\mathtt{exp}[\gamma_0+\gamma_1 1(\mathtt{cens}=1991)+\gamma_2(\mathtt{s}-1937)^2+\gamma_3(\mathtt{s}-1937)^2 1(\mathtt{cens}=91)]}{1+\mathtt{exp}[\gamma_0+\gamma_1 1(\mathtt{cens}=1991)+\gamma_2(\mathtt{s}-1937)^2+\gamma_3(\mathtt{s}-1937)^2 1(\mathtt{cens}=91)]}.$$

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 5: Effect of the 1963 reform on $\mathtt{F}(\mathtt{y}) = \mathtt{Prob}[\mathtt{Y_i} \leq \mathtt{y}]$ at distinct values of $\mathtt{y}$, $\mathtt{Y}$ women's age at first birth (intention-to-treat effect).

Overall Sample Size: 109,285. Average Cohort Sample Size: 5,752. Smoothing Technique: Linear Probability Model									
y	**18**	**19**	**20**	**21**	**22**	**23**	**24**	**25**	**26**
					1950				
effect	-0.01	-0.02	-0.03	-0.03	-0.02	0.02	0.01	0.01	0.00
t-test	-1.56	-2.14	-2.93	-2.83	-1.63	1.92	1.04	0.72	0.08
p-value	0.14	0.05	0.01	0.01	0.12	0.08	0.32	0.49	0.94
					1951				
effect	-0.01	-0.02	-0.03	-0.03	-0.02	0.03	0.03	0.02	0.01
t-test	-1.56	-2.14	-3.12	-2.38	-1.63	3.44	2.02	1.64	0.53
p-value	0.14	0.05	0.01	0.01	0.12	0.00	0.06	0.13	0.60
					1952				
effect	-0.01	-0.02	-0.03	-0.03	-0.02	0.05	0.04	0.03	0.01
t-test	-1.56	-2.14	-3.24	-2.38	-1.63	4.26	2.48	1.94	0.40
p-value	0.14	0.05	0.01	0.01	0.12	0.00	0.03	0.07	0.69

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 $\mathtt{F_s}(\mathtt{y}) = \mathtt{Prob}[\mathtt{Y_i} \leq \mathtt{y} | \mathtt{S_i} = \mathtt{s}]$: (i) for values $\mathtt{y} \in [\mathtt{18}, \mathtt{19}]$ common linear trend in cohort over cohorts 1938-1956; (ii) for values $\mathtt{y} \in [\mathtt{20}, \mathtt{22}]$ common quadratic trend in cohort over cohorts 1938-1956; (iii) for values $\mathtt{y} \in [\mathtt{23}, \mathtt{26}]$ 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 the 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 6: Effect of the 1963 reform on $\mathtt{F}(\mathtt{y}) = \mathtt{Prob}[\mathtt{Y_i} \leq \mathtt{y}]$ at distinct values of $\mathtt{y}$, $\mathtt{Y}$ women's age at marriage (intention-to-treat effect).

Overall Sample Size: 53,004. Average Cohort Sample Size: 2,827									
Smoothing Technique: Linear Probability Model									
y	**18**	**19**	**20**	**21**	**22**	**23**	**24**	**25**	**26**
					1950				
effect	-0.00	-0.00	-0.01	-0.01	0.02	0.02	0.02	0.01	0.01
t-test	-0.56	-0.43	-1.27	-0.65	0.01	1.83	1.79	1.89	2.08
p-value	0.58	0.67	0.22	0.52	0.03	0.09	0.09	0.08	0.06
					1951				
effect	-0.01	-0.00	-0.01	-0.01	0.02	0.02	0.02	0.01	0.01
t-test	-1.80	-0.43	-1.27	-1.49	0.01	1.83	1.79	1.89	2.08
p-value	0.09	0.67	0.22	0.16	0.03	0.09	0.09	0.08	0.06
					1952				
effect	-0.01	-0.00	-0.01	-0.02	0.02	0.02	0.02	0.01	0.01
t-test	-2.43	-0.43	-1.27	-2.34	0.01	1.83	1.79	1.89	2.08
p-value	0.03	0.67	0.22	0.03	0.03	0.09	0.09	0.08	0.06

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 $\mathtt{F_s}(\mathtt{y}) = \mathtt{Prob}[\mathtt{Y_i} \leq \mathtt{y}|\mathtt{S_i} = \mathtt{s}]$: (i) for $\mathtt{y} = 18$, linear trend in cohort over cohorts 1938-1948, quadratic trend over cohorts 1949-1956; (ii) for values $\mathtt{y} \in [19, 20]$, common linear trend over cohorts 1938-1956; (iii) for $\mathtt{y} = 21$, distinct polynomials linear in cohort over the pre- and post- reform cohorts; (iv) for values $\mathtt{y} \in [22, 26]$, 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 the effect is null.

Table 7: Causal effect of education on the timing of births for *compliers* at s ($LATE$).

	$s = 1950$		$s = 1951$		$s = 1952$	
	$\widehat{\phi_a(s)}$ - $\widehat{\phi_n(s)}$	$\widehat{\phi_c(s)}$	$\widehat{\phi_a(s)}$ - $\widehat{\phi_n(s)}$	$\widehat{\phi_c(s)}$	$\widehat{\phi_a(s)}$ - $\widehat{\phi_n(s)}$	$\widehat{\phi_c(s)}$
	0.36 - 0.61	0.03	0.37 - 0.58	0.05	0.39 - 0.55	0.06
$F(y)at$	$LATE$	**rev.**	$LATE$	**rev.**	$LATE$	**rev.**
	(st.err.)	**(st.err.$^+$)**	**(st.err.)**	**(st.err.$^+$)**	**(st.err.)**	**(st.err.$^+$)**
$y = 18$	-0.19 (0.12)	-0.07 (0.06)	-0.12 (0.08)	-0.07 (0.04)	-0.09 (0.06)	-0.08 (0.03)
$y = 19$	-0.51 (0.25)	-0.33 (0.16)	-0.33 (0.16)	-0.27 (0.12)	-0.25 (0.12)	-0.25 (0.11)
$y = 20$	-0.88 (0.33)	-0.68 (0.13)	-0.64 (0.21)	-0.51 (0.09)	-0.53 (0.17)	-0.40 (0.07)
$y = 21$	-0.86 (0.33)	-0.77 (0.14)	-0.56 (0.20)	-0.58 (0.10)	-0.42 (0.15)	-0.46 (0.08)
$y = 22$	-0.65 (0.41)	-0.77 (0.14)	-0.43 (0.26)	-0.53 (0.12)	-0.32 (0.19)	-0.42 (0.09)
$y = 23$	0.56 (0.31)	-0.18 (0.24)	0.72 (0.23)	0.13 (0.19)	0.81 (0.20)	0.27 (0.17)
$y = 24$	0.42 (0.41)	-0.18 (0.23)	0.62 (0.31)	0.16 (0.18)	0.70 (0.29)	0.31 (0.15)
$y = 25$	0.23 (0.32)	-0.16 (0.23)	0.40 (0.25)	0.14 (0.18)	0.44 (0.23)	0.27 (0.15)
$y = 26$	0.03 (0.34)	-0.16 (0.23)	0.13 (0.24)	0.12 (0.19)	0.09 (0.21)	0.20 (0.18)

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's potential outcomes' cumulative distribution functions. Revised estimates (rev.) are computed as $\widehat{\mathtt{F}^{\mathtt{C_s}}_{.1}(\mathtt{y})}^{*} - \widehat{\mathtt{F}^{\mathtt{C_s}}_{.0}(\mathtt{y})}^{*}$, where $\widehat{\mathtt{F}^{\mathtt{C_s}}_{.1}(\cdot)}^{*}$, $\widehat{\mathtt{F}^{\mathtt{C_s}}_{.0}(\cdot)}^{*}$ represent the "revised" estimates of the *compliers*' potential outcome distribution functions. $\widehat{\phi_{\mathtt{n}}(\mathtt{s})}$, $\widehat{\phi_a(s)}$,$\widehat{\phi_c(s)}$ 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 of the bootstrap algorithm implemented, not reported for brevity, are available from the author upon request. Points s considered in the analysis are those for which the first stage age effect was significantly different from zero, namely $\mathtt{s} = 1950$, $\mathtt{s} = 1951$, $\mathtt{s} = 1952$.

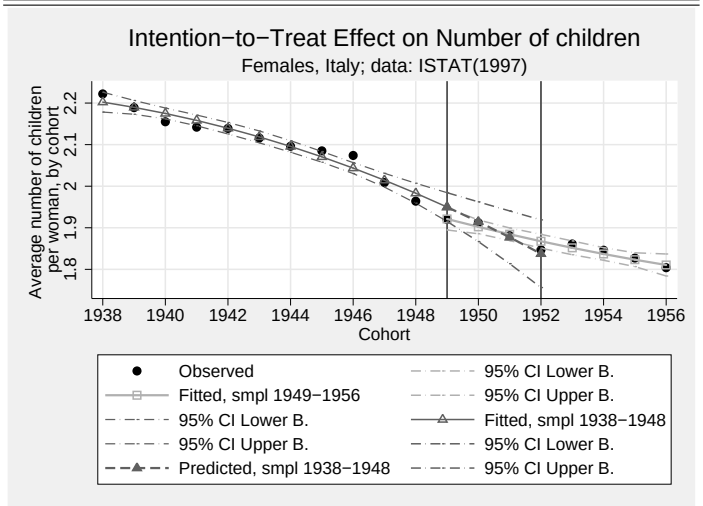
Table 8: Causal effect of education on the timing of marriage for *compliers* at s ($LATE$).

	$s = 1950$		$s = 1951$		$s = 1952$	
	$\widehat{\phi_a(s)}$ - $\widehat{\phi_n(s)}$	$\widehat{\phi_c(s)}$	$\widehat{\phi_a(s)}$ - $\widehat{\phi_n(s)}$	$\widehat{\phi_c(s)}$	$\widehat{\phi_a(s)}$ - $\widehat{\phi_n(s)}$	$\widehat{\phi_c(s)}$
	0.36 - 0.61	0.03	0.37 - 0.58	0.05	0.39 - 0.55	0.06
$F(y)at$	$LATE$ (st.err.)	rev. (st.err.$^+$)	$LATE$ (st.err.)	rev. (st.err.$^+$)	$LATE$ (st.err.)	rev. (st.err.$^+$)
$y = 18$	-0.10 (0.18)	-0.05 (0.17)	-0.17 (0.09)	-0.11 (0.12)	-0.16 (0.07)	-0.10 (0.10)
$y = 19$	-0.10 (0.23)	-0.04 (0.19)	-0.07 (0.15)	-0.01 (0.13)	-0.05 (0.11)	0.05 (0.10)
$y = 20$	-0.34 (0.27)	-0.33 (0.23)	-0.22 (0.17)	-0.24 (0.15)	-0.17 (0.13)	-0.17 (0.11)
$y = 21$	-0.17 (0.26)	-0.10 (0.26)	-0.24 (0.16)	-0.06 (0.17)	-0.28 (0.12)	-0.04 (0.13)
$y = 22$	0.63 (0.37)	0.19 (0.25)	0.41 (0.22)	0.18 (0.18)	0.31 (0.17)	0.17 (0.14)
$y = 23$	0.57 (0.34)	0.36 (0.22)	0.37 (0.20)	0.31 (0.17)	0.28 (0.15)	0.21 (0.14)
$y = 24$	0.52 (0.31)	0.37 (0.20)	0.34 (0.19)	0.28 (0.16)	0.25 (0.14)	0.19 (0.13)
$y = 25$	0.41 (0.24)	0.33 (0.18)	0.26 (0.14)	0.21 (0.14)	0.20 (0.10)	0.14 (0.12)
$y = 26$	0.40 (0.21)	0.23 (0.15)	0.26 (0.12)	0.21 (0.12)	0.19 (0.09)	0.16 (0.10)

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's potential outcomes' cumulative distribution functions. Revised estimates (rev.) are computed as $\widehat{\mathtt{F}^{\mathtt{C_s}}_{.1}(\mathtt{y})}^{*} - \widehat{\mathtt{F}^{\mathtt{C_s}}_{.0}(\mathtt{y})}^{*}$, where $\widehat{\mathtt{F}^{\mathtt{C_s}}_{.1}(\cdot)}^{*}$, $\widehat{\mathtt{F}^{\mathtt{C_s}}_{.0}(\cdot)}^{*}$ represent the "revised" estimates of the *compliers*' potential outcome distribution functions. $\widehat{\phi_n(s)}$, $\widehat{\phi_a(s)}$,$\widehat{\phi_c(s)}$ 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 of the bootstrap algorithm implemented, not reported for brevity, are available from the author upon request. Points s considered in the analysis are those for which the first stage age effect was significantly different from zero, namely $\mathtt{s} = 1950$, $\mathtt{s} = 1951$, $\mathtt{s} = 1952$.

Table 9: Effect of the 1963 reform on the average number of children (intention-to-treat effect) and causal effect of education on completed fertility.

	Intention-to-treat effects			**Causal effects**		
	1950	**1951**	**1952**	**1950**	**1951**	**1952**
effect	-0.01	0.01	0.03	-0.39	0.16	0.47
st. err.	0.001	0.001	0.001	0.63	0.54	0.50
t-test	-0.47	0.22	0.71	-0.63	0.29	0.94
p-value	0.64	0.829	0.49	1	0.77	0.35



Data sources: ISTAT [1997] (intention-to-treat effects); Census (1981,1991) (first stage effect). Smoothing: linear regression, polynomial which allows for distinct quadratic trends over cohorts 1938-1948 and over cohorts 1949-1956. The test statistics test the hypothesis that the effect is null. Causal effects estimates computed according to the Wald estimator, i.e. $\mathtt{E}[\mathtt{Y}_{\mathtt{i}}^{0}|\mathtt{C}_{\bar{\mathtt{s}}}] - \mathtt{E}[\mathtt{Y}_{\mathtt{i}}^{1}|\mathtt{C}_{\bar{\mathtt{s}}}] = \frac{\mathtt{E}[\mathtt{Y}|\mathtt{Z}=1,\mathtt{S}=\bar{\mathtt{s}}] - \mathtt{E}[\mathtt{Y}|\mathtt{Z}=0,\mathtt{S}=\bar{\mathtt{s}}]}{\phi_c(\bar{s})}$, standard errors computed using the delta-method. Inference relies on asymptotic normality.

Table 10: Distribution of the region of residence among *compliers*.

Area	$\phi_{c,x}(s)$ (s.e)	$\phi_c(s)$ (s.e)	$\mathtt{E}[\mathtt{X}=1]$	$\mathtt{E}[\mathtt{X}\vert\mathtt{C_s}]$
		$\mathtt{s}=1949$		
$\mathtt{X}=1(\mathtt{North})$	0.010 (0.008)	0.014 (0.005)	0.474	0.343
$\mathtt{X}=1(\mathtt{Centre})$	0.005 (0.012)	0.014 (0.005)	0.184	0.064
$\mathtt{X}=1(\mathtt{South})$	0.014 (0.009)	0.014 (0.005)	0.342	0.334
		$\mathtt{s}=1950$		
$\mathtt{X}=1(\mathtt{North})$	0.031 (0.006)	0.031 (0.005)	0.474	0.472
$\mathtt{X}=1(\mathtt{Centre})$	0.029 (0.011)	0.031 (0.005)	0.184	0.171
$\mathtt{X}=1(\mathtt{South})$	0.029 (0.009)	0.031 (0.005)	0.342	0.320
		$\mathtt{s}=1951$		
$\mathtt{X}=1(\mathtt{North})$	0.059 (0.007)	0.047 (0.005)	0.474	0.595
$\mathtt{X}=1(\mathtt{Centre})$	0.052 (0.012)	0.047 (0.005)	0.184	0.205
$\mathtt{X}=1(\mathtt{South})$	0.043 (0.009)	0.047 (0.005)	0.342	0.318
		$\mathtt{s}=1952$		
$\mathtt{X}=1(\mathtt{North})$	0.079 (0.008)	0.063 (0.005)	0.474	0.593
$\mathtt{X}=1(\mathtt{Centre})$	0.076 (0.012)	0.063 (0.005)	0.184	0.221
$\mathtt{X}=1(\mathtt{South})$	0.058 (0.009)	0.063 (0.005)	0.342	0.313

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 ; **Centre**: Lazio, Marche, Toscana, Umbria; **South**: Abruzzo, Molise, Basilicata, Calabria, Campania, Puglia, Sardegna, Sicilia. Figures in the table follow from $\mathtt{E}[\mathtt{X}|\mathtt{C_s}] \equiv \mathtt{Prob}[\mathtt{X}=1|\mathtt{C_s}]\frac{\mathtt{Prob}[\mathtt{C_s}|\mathtt{X}=1]}{\phi_{\mathtt{c}}(\mathtt{s})} = \frac{\phi_{\mathtt{c,x}}(\mathtt{s})}{\phi_{\mathtt{c}}(\mathtt{s})}\mathtt{Prob}[\mathtt{X}=1]$, 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 $\mathtt{X}=\mathtt{x}$ and $\mathtt{S}=\mathtt{s}$. Estimates of $\phi_{c,x}(s)$ are obtained following the same empirical strategy exploited to get the estimates of ϕ_c, i.e. running the following regressions: **North**: $\mathtt{E}[\mathtt{D}|\mathtt{S},\mathtt{North}] = \alpha_0 + \alpha_1 1(\mathtt{cens}=91) + \alpha_2(\mathtt{S}-1937) + \alpha_3\mathtt{Z} + \alpha_4(\mathtt{S}-1937)\mathtt{Z} + \alpha_5(\mathtt{S}-1937)^2\mathtt{Z} + \varepsilon$; **Centre**: $\mathtt{E}[\mathtt{D}|\mathtt{S},\mathtt{Centre}] = \beta_0 + \beta_1 1(\mathtt{cens}=91) + \beta_2(\mathtt{S}-1937) + \beta_3\mathtt{Z} + \beta_4(\mathtt{S}-1937)\mathtt{Z} + \vartheta$; **South**: $\mathtt{E}[\mathtt{D}|\mathtt{S},\mathtt{South}] = \gamma_0 + \gamma_1(\mathtt{S}-1937) + \gamma_2(\mathtt{S}-1937)1(\mathtt{cens}=91) + \gamma_3\mathtt{Z} + \gamma_4(\mathtt{S}-1937)\mathtt{Z} + \zeta$. $\mathtt{S}$ is the individuals' birth cohort,$1(\mathtt{A})$ takes the value one if the event $\mathtt{A}$ is true and $\mathtt{Z}=1(\mathtt{S}\geq 1949)$, $\mathtt{D}=1$(has exactly high school qualification). The effect of the reform on education at $\mathtt{S}=\mathtt{s}$ (i.e., $\mathtt{E}[\mathtt{D}|\mathtt{S}=\mathtt{s},\mathtt{Z}=1]-\mathtt{E}[\mathtt{D}|\mathtt{S}=\mathtt{s},\mathtt{Z}=0]$), $\mathtt{s}\in\{1949,1950,1951,1952\}$ is given by: **North**: $\alpha_3+\alpha_4(\mathtt{S}-1937)+\alpha_5(\mathtt{S}-1937)^2$; **Centre**: $\beta_3+\beta_4(\mathtt{S}-1937)$; **South**: $\gamma_3+\gamma_4(\mathtt{S}-1937)$. 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 (Centre, South).

Table 11: Effect of assignment to the treatment ($\mathtt{Z}$) on the proportion of women with high school qualification who bear their first child by age y, $\mathtt{F(y,s)} = \mathtt{Prob}[\mathtt{Y_i} \leq \mathtt{y}|\mathtt{S} = \mathtt{s}]$, $\mathtt{Y}$ women's age at first birth. Italy.

	Overall Sample Size: 36,932. Average Cohort Sample Size: 1,605						
y	**20**	**21**	**22**	**23**	**24**	**25**	**26**
Smoot. Tech.							
				1949			
lpm	-0.01 (0.1)	-0.01 (0.5)	0.01 (0.2)	0.02 (0.3)	0.02 (0.1)	0.02 (0.2)	0.01 (0.8)
logit	-0.01 (0.0)	-0.02 (0.0)	0.00 (0.6)	-0.01 (0.5)	0.01 (0.7)	0.00 (0.8)	0.00 (0.9)
				1950			
lpm	-0.01 (0.1)	-0.01 (0.5)	0.01 (0.2)	0.02 (0.3)	0.01 (0.5)	0.00 (0.9)	0.01 (0.7)
logit	-0.01 (0.1)	-0.01 (0.1)	0.00 (0.8)	0.00 (0.7)	0.01 (0.6)	0.00 (0.9)	0.00 (0.8)
				1951			
lpm	-0.01 (0.1)	-0.01 (0.5)	0.01 (0.2)	0.02 (0.3)	0.00 (0.9)	-0.02 (0.4)	0.00 (1)
logit	-0.01 (0.3)	-0.01 (0.3)	0.01 (0.6)	0.00 (0.7)	0.01 (0.9)	-0.01 (0.4)	0.00 (0.9)
				1952			
lpm	-0.01 (0.1)	-0.01 (0.5)	0.01 (0.2)	0.02 (0.3)	-0.02 (0.3)	-0.04 (0.1)	-0.02 (0.4)
logit	0.00 (0.7)	-0.01 (0.4)	0.01 (0.6)	-0.01 (0.5)	-0.01 (0.6)	-0.03 (0.1)	-0.02 (0.2)

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 $\mathtt{F_s(y)} = \mathtt{Prob}[\mathtt{Y_i} \leq \mathtt{y}|\mathtt{S_i} = \mathtt{s}]$. Two different smoothing methods have been used: *lpm* stands for linear probability model (for values $\mathtt{y} \in [20, 23]$ polynomials with common quadratic trend in cohorts; for values $\mathtt{y} \in [24, 26]$ 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 paretheses.