Fertility decisions and pension reforms: Evidence from a natural experiment

Abstract

The emergence of old-age social security has been linked to long-term fertility decline in developed societies. In turn, in recent years pension reforms have emerged as a response to the challenges of population ageing often associated with low fertility. Fertility theories have different predictions on the effects of these pension reforms on fertility. In this paper, we analyze the impact of changes of social security on fertility in a novel way. We exploit a series of pension reforms that were implemented in Italy, one of the first 'lowest-low' fertility societies, during the 1990s, to estimate the effect of expected retirement income on fertility. The design of the reforms, which introduced discontinuities depending on the numbers of years of contributions, allows to treat them as a natural experiment. Analyzing fertility histories reconstructed from a series of repeated Bank of Italy surveys, we show that couples in which the husband or the wife were affected by the reform, therefore facing a lower pension, had subsequently at least 10% higher fertility. We discuss the implications of our findings making reference to the old-age security motive for fertility, the quality-quantity tradeoff, and the consumption theory.

Keywords: fertility, public pension systems, old-age security, quantity-quality trade-off, income effects.

Introduction

The long-term decline of fertility within the demographic transition has been associated also with the rise of modern social security (Entwisle and Winegarden 1984; Friedlander and Silver 1967; Hohm 1975; Hohm et al. 1986). This association rests on the idea that, while before the demographic transition children were essential to insure the old-age security of their parents, after the demographic transition the relevance of what happens during one's old-age as a motive for fertility tends to disappear, with a reversal of intergenerational wealth flows (Caldwell 1982). On top of this long-term trend, the last three decades have been a season of significant social security reforms, induced mostly by current and/or foreseen population aging but also by concerns about public spending. These reforms have essentially aimed at lowering expected income from retirement pensions. It is not surprising that countries characterized by the lowest fertility levels, which also face the potential challenge of faster population aging, have been leading these reforms. This is, for instance, the case of Italy, the setting of our study, where the main pension reforms were kickstarted exactly while the country was experiencing the lowest total fertility rates in the world, during the mid-1990s.

Understanding the effect of social security reforms on fertility is crucial for two reasons. First, from a policy-oriented and future-oriented empirical perspective, it is important to understand the implications of pension reforms on a number of outcomes including fertility, and to inform fertility scenarios for societies affected by changes in social security. Second, pension reforms provide important cases to test economic theories of fertility. A decrease in expected future pension benefits that is not matched by a corresponding reduction in social security contributions induces an expected negative income effect. This negative impact will take place in the future, and thus modifies the age-income profile. Individuals, who expect to become poorer in the future, may choose to adjust their behavior along several dimensions, such as savings, labor supply, within-family *inter vivos* transfers, as well as fertility and investment in children. Interestingly, the pure negative income effect associated with a reduction in future pension benefits leads to different predictions under different fertility theories.

In this paper, we analyze the effect on fertility of a consistent sequence of Italian pension reforms during the 1990s. Italy represents a particularly interesting case for the study of fertility choices. Together with Spain, Italy has been the first country to steadily experience fertility levels below a threshold defined of lowest-low fertility (a total fertility rate of 1.3 children per woman or below) during the 1990s (Billari and Kohler 2004; Kohler, Billari and Ortega 2002). This lowest-low fertility decade coincided with the period of implementation of the two reforms (Caltabiano, Castiglioni and Rosina 2009). The design of the reforms introduced sharp discontinuities in the size of future pension benefits across workers. In the "Amato" reform of 1992, pension benefits of individuals with 15 years of contributions or more at the end of 1992 were not modified, while pension entitlements were largely reduced for all other individuals on a pro-quota basis, which took into account their contributory history. A discontinuity that affected exactly the same cohorts of workers was then introduced by the "Dini" reform in 1995. The joint magnitude of these discontinuities is sizeable. Due to the reforms, a one-year difference in the length of cumulative contributions in 1992 (14 vs. 15 years) for two individuals with otherwise the same characteristics may command a difference in the pension replacement rate (measured as the ratio between the pension benefit and the last wage prior to retirement) of around 15 points -- that is, a replacement rate of 80% for the individual with 15 years in contributions in 1992 versus 65% for a similar individual with 14 years of contributions.[1] We can therefore exploit these discontinuities to estimate the effects of the Amato and Dini pension reforms on fertility.

[1] For more details, see the section on the Italian pension reforms.

We focus on heterosexual married couples as the unity of fertility decision-making. We consider couples in which either the male partner (husband from now onwards), or the female partner (wife from now onwards) – or both – is an employee, whose future pension benefits have (or not) been reduced by the reforms. We expect the fertility effects of husbands affected by the reform to be stronger as compared to wives affected by the reform for two reasons. First, Italy during the nineties was one of the western countries with the strongest prevalence of a male breadwinner model, with husbands as the main family earner (Bettio and Villa 1998). Hence, the magnitude of the impact of the reform on the family income is typically larger when it affects the husband. Second, due to the rigidity of the labor market and to the traditional gender division of labor in Italy, it is safe to assume that the husband's labor supply was not modified by the reform. In fact, labor contracts allow individuals to change their labor supply only on the extensive margin, namely by choosing either not to work at all, or to work part-time (which is extremely rare for males) or full time. Moreover, the division of labor within Italian families during the 1990s was still such that the care of the children was almost entirely provided by the mothers, with a very limited participation by fathers (Sullivan, Billari and Altintas 2014).[2] Female workers contributed, on average, less to the family income, and thus the average effect of the reform on their future pension benefits is of lower magnitude. Since the reform makes also contributions on average less valuable towards future pension benefits, wives may adjust by working less – i.e., either part-time or not at all. Hence, the overall effect on fertility of a change in the wives' future pension benefits may stem both from lower expected future income and from lower labor force participation.

In our analyses, we find a strong *positive* effect of the pension reform on the average number of post-reform children and on the probability of having a(nother) child after the reform. More specifically,

[2] As shown by Fuwa 2004, in a sample of 22 countries, Italy was the second-last in terms of male participation in the division of housework in 1994.

couples in which the husband is affected (about 60% of our sample) are estimated to have 10.7% higher fertility after the reforms with respect to the couples in which the husband is not affected. The estimated effect for treated wives is even stronger, albeit not always statistical significant possibly due to the fact that only about 7% of wives are not affected by the reform. Sensitivity analyses confirm the robustness of our findings.

Our results have multiple implications. First, they constitute clear micro-founded evidence of the link running from public pension systems to fertility decisions: lower pension benefits increase, rather than decrease, fertility in our setting. Along these lines, our results might contribute to explaining, at least in part, recent fertility trends, including the (mild) reversal towards high fertility, which has been observed in Italy after the mid-1990s and until the Great Recession. More generally, our results also suggest that in many developed societies the strong decreasing trend in fertility may be partially due to the large rise in pension spending. Second, our empirical findings hint to the fact that the old-age security motive (Boldrin and Jones 2002; Cain 1981; Caldwell 1978; Leibenstein 1957; Neher 1971) may still be a relevant economic explanation for childbearing decisions in a key contemporary low fertility society. Our findings are thus in line with recent contributions, which have exploited exogenous variations in current income, due for instance to job displacement (Lindo, 2010) or sector specific economic booms (Black et al., 2013), and in housing wealth (Lovenheim and Mumford, 2013) to show that positive current (husband's) income or wealth effects lead to an increase in fertility. In fact, while these results are consistent with children being normal goods, they are also in line with an old age security motive, since higher current income (or wealth) leads to an increase in consumption, but also in saving and fertility.

The remainder of this paper is structured as follows. First, we review and discuss theories and empirical evidence relating changes in social security with fertility decision-making. In particular, we outline the empirical predictions of different theories. Subsequently, we shortly describe the Italian context and the content of the social security reforms of the 1990s. Furthermore, we explain the data and the modeling strategy we use. After illustrating our results, as well as some robustness checks, we present some concluding remarks.

Social security and fertility decision-making

The emergence and scope of social security have long been linked to fertility decision-making (Entwisle and Winegarden 1984; Friedlander and Silver 1967; Hohm 1975; Hohm et al. 1986). Sinn (2004) presents the existence of public pension systems as devices to provide insurance against the risk of not having children, or of having 'ungrateful' children, who are unwilling (or unable) to care for their old parents. Yet, a drawback of public pension systems is that, even in households with grateful children, they tend to reduce transfers from children to parents in later stages of the parental life course. Potential parents, while pondering fertility decisions, may therefore have an incentive to 'free ride' on the social security contributions paid by other people's children (England and Folbre 1999). As a result, with the spread of old-age social security, fertility is bound to fall. The free-riding argument based on the presence of social security has also used by Demeny (1987:130) in a pronatalist fashion, stating that "institutional innovation that would reestablish the material link between fertility behavior and old-age security" would contribute to increasing fertility. Macro-level empirical studies on the negative correlation between fertility and various measures of the size or the generosity of the public pension system have long been present in the literature (Cigno and Rosati 1992; Entwisle and Winegarden 1984; Galasso, Gatti and Profeta 2009; Hohm 1975; Hohm et al. 1986). In a study that is particularly relevant for what we do, Gábos, Gál and Kézdi (2009) use aggregate time-series data from post-war Hungary, comparing the effect of pensions and of child-related benefits on fertility, and estimate that a

1-per-cent increase in pensions decreases fertility by 0.2 per cent, while a 1-percent increase in child-related benefits increases fertility by the same amount.

Theoretical and empirical analyses of the link between social security and fertility are related to different theories. The old-age security theory of fertility focuses on intergenerational flows within the family, and considers children as an investment (or ‘production’) good (Boldrin and Jones 2002; Cain 1981; Caldwell 1978; Leibenstein 1957; Neher 1971). According to this theory, parents may decide to have children because they expect to receive back a (monetary or in-kind) transfer when old. Although rare empirical contributions have provided evidence in favor of the old-age security motive for childbearing in contemporary societies, this motive is seen as particularly relevant in societies where family ties are more binding and/or no reliable saving instruments are available (Cunningham et al. 2013). Kağitçibaşi (1982) argues that old-age security was not a reason for fertility in societies such as Germany and the U.S. during the 1970s, despite this motive having been cited as somewhat important or very important by 32 percent of married German women and 27 percent of married U.S. women during interviews. Boldrin, De Nardi and Jones (2015) quantify the effect of the rise in pension spending on fertility trends. According to their model, around 50% of the long-term drop in fertility in the U.S. is accounted for by the pension system. Rendall and Bahchieva (1998) point out the potentially high relevance of old-age security motives in contemporary developed societies. They provide an extensive documentation of the relevance of children for providing support to their elderly parents in contemporary U.S.: 11 percent of all unmarried elderly in the U.S. live above poverty only because they co-reside with adult children, and observed poverty rates would double in absence of such co-residence. Consistently, McGarry and Schoeni (2000) find that the increase of social security benefits in the U.S. has contributed to rising residential independence of elderly widows. Co-residence is therefore a crucial way to transfer income from adult children to their elderly parents also in the U.S., a country that has almost the same strength of family ties as Italy, according to the measure of Alesina and Giuliano (2010). Recent analyses of comparative data on support for parents show that, in countries with strong family ties, help to parents is more widespread (Kalmijn and Saraceno 2008). In a 1998 Eurobarometer survey, 76 percent of adult Italians state that, in the future, working adults may have to look after their parents more than they do now, 52 percent that a needy elderly parent should co-reside with a child, and 23 percent that children should have the main economic responsibility when elderly parents are in need (Kalmijn and Saraceno 2008). According to the old-age security theory of fertility, therefore, reforms that, by decreasing expected pension benefits, create a negative income effect in old age, should induce individuals to adjust their current behavior and *increase* fertility (as well as savings).

The family economics approach, pioneered by Becker (1960), suggests that individuals obtain direct utility from having and raising children, and from their children's well-being. Kids, and possibly their quality level, thus resemble a consumption good in the utility function of their parents. Evidence that genetic endowments influence the propensity to have children is consistent with this view (Kohler, Behrman and Skytthe 2005; Mills and Tropf 2015). The initial formulation of Becker's theory predicts a positive correlation between fertility and income, which is also consistent with Easterlin's 'relative income' hypothesis (Easterlin 1978) and with evidence on pro-cyclical fertility in advanced societies (Andersson 2000; Ben-Porath 1973; Luci-Greulich and Thévenon 2014; Myrskylä, Kohler and Billari 2009; Sobotka, Skirbekk and Philipov 2011). Indeed, the idea that income can 'buy' children is a standard one behind discussions on pronatalistic policies or welfare incentives based on implicit or explicity income transfers (Gauthier and Hatzius 1997; Whittington, Alm and Peters 1990). According to this 'consumption' theory of fertility, reforms that decrease expected pension benefits should *decrease* fertility.

The positive correlation between fertility and income posited by Becker (1960) has however been challenged empirically, so that Freedman and Thornton (1982) defined the income-fertility relationship "the elusive relationship". Subsequent contributions in this literature have emphasized two aspects as central: the quantity-quality trade-off, and the role of the cost of parental time (Guinnane 2011; Jones, Schoonbroodt and Tertilt 2011). Along the former lines, Becker and Lewis (1973) showed that an increase in income may lead to fewer children, but of higher quality. This is because rich parents value kids' quality, but higher quality increases the cost of having (and raising) kids, and may thus lead to lower fertility. According to this theory of fertility, reforms that decrease expected future benefits, but leave current contributions unaffected, should thus *increase* fertility levels, and *decrease* parental investments in each child. The latter theory emphasizes the 'price' of parental time. Since raising kids requires parental time, fertility is more costly for high-income parents, who thus choose to have fewer kids. This mechanism is related to the current income of the parents, and is particularly relevant for women's potential income. In particular, a reform that reduces also the positive effect of the current contributions on the future pension benefits effectively decreases the overall value of working today. Women, who are – at least in Italy – less attached to the labor market, may thus choose to reduce their working hours, and to have more children.

The setting: Italian pension reforms

Before moving to the analyses, we describe our setting. During the same period in which total fertility rates were reaching the lowest levels in the world, a sequence of major pension reforms took place in Italy in the 1990s. Before these reforms, pension spending had almost reached 15% of GDP, thereby becoming one of the largest in the world. The pension system featured a large deficit, since yearly contributions were not sufficient to finance yearly benefits, and large transfers from the central government were needed to balance the budget. Faced with the expectations of further aging and with a financial crisis, the Italian system was hence widely re-designed, mainly through the “Amato” reform in 1992 and the “Dini” reform in 1995 (see Table 1 for details).

The Amato reform largely decreased pension spending by introducing a tightening of the eligibility requirements, and by reducing the generosity of the benefits. Retirement age was gradually increased to 60 years for women and to 65 years for men, and the minimum contribution period for pension eligibility was extended to 20 years – to be achieved respectively by the years 2000 and 2001. Pension benefit indexation was scaled down from nominal wages to inflation only, and social security contributions were increased from 24.5% to 27%. Crucially for our analysis, other important reform measures affected (pro-quota) only those workers, who did not already have 15 years of contributions at the time of the reform. In particular, the minimum contribution period to be eligible to an early retirement pension was extended to 35 years for all (private and public) workers. More importantly, the reference wage used to calculate the pension benefit, which for private employees was equal to the average wage over the last five years prior to retirement, was extended to the average wage during the entire working carrier, with past earnings capitalized at the cost of living index plus 1% per year. Since the labor earning profile is typically increasing in age, and thus wages in later years are higher than those at the beginning of the working career, this change caused on average a reduction in the reference wage (Galasso 2006). Hence, pension benefits, which were calculated as the product between the number of years of contributions, this reference wage, and a rate of return of two per cent per year, also dropped. This last retrenchment measure introduced a clear and sizeable negative income effect, but only for those affected by the reform, i.e., those who had less than 15 years of contributions at the end of 1992. The design of these reforms gave raise to large differences in the reduction of social security wealth across workers with different seniorities. Additional differences emerged between public and private employees, who faced different initial treatments. In particular, the reduction was on average larger for public employees, who initially enjoyed a more generous benefit calculation, since their reference wage was equal to the very last wage prior to retirement.

TABLE 1 ABOUT HERE

To better understand the discontinuity introduced by the 15 years of contributions threshold, consider two male workers in the private sector with a high-school degree, who entered the labor market at the same age (20 years), and featured the same labor earning profile by age. Suppose however that they were born one year apart and had different years of contributions at the end of 1992. While Mr. Old is one year older -- he was born in 1957 -- and already had 15 years of contributions, Mr. Young (born in 1958) only had 14. Suppose that they will both retire at age 60 upon reaching forty years of contributions. Mr. Old will then retire in 2017 and his pension benefits will entirely be calculated according to the pre-reform rules. His replacement rate -- that is, the share of his labor income at age 59 replaced by the pension benefit -- would be around 80%. If he earned an average gross monthly income[3] of €1,400, the gross monthly pension benefit would be equal to €1,120. Mr. Young will instead retire a year later, in 2018. His pension benefits will be calculated for almost two thirds (26/40) according to the new rules, and only for the remaining part (14/40) according to the pre-Amato reform scheme. For Mr. Young, the replacement rate would only be around 70%, and his gross monthly pension benefits of €980. This amounts to a large discontinuity: the pension treatment of individuals who at the end of 1992 differed in one year of contribution only was set to be noticeably large. Our

[3] In 1995, the average annual labor income for private employees was almost 22 million liras, corresponding in current prices to an annual labor income of around €16,800, or €1,400 monthly.

stylized example is in line with estimates by Attanasio and Brugiavini (2003), who measure the drop in social security wealth in the private sector due to the Amato reform to be equal to 27.6% for workers born after 1957, and to 17% for those born between 1945 and 1957. This reduction was even larger among public employees: respectively 32.1% for the younger workers and 27.1% for the 1945-1957 generation.

In 1995, the Dini reform completely redesigned the architecture of the Italian social security system, shifting from a defined benefit to a notional defined contribution (NDC) system. In this NDC system, returns on social security contributions are not fixed (as in the previous system, when they were equal to 2%), but depend on the growth rate of the economy. Social security contributions were increased to 33%, although this large raise was due to regrouping existing contributions under the social security contribution rate.[4] Eligibility criteria were also largely revised. Under the new NDC system, seniority pensions, whose eligibility was exclusively based on reaching a minimum contribution period, were abolished. For the private employees' scheme, the minimum number of years of contribution to be eligible for a pension was reduced to 5 years only; however, only individuals aged between 57 and 65 years are entitled to a pension. These measures were however introduced with a long transition path, so that only workers entering the labor market in 1996 were entirely accommodated into the new system. Workers with at least 18 years of contributions at the end of 1995 were unaffected, and less senior workers were affected pro-quota. Retirement age was also increased, but only marginally, and this measure applied equally to senior and less senior workers. Overall, these retrenchment measures thus induced to a negative and sizable income effect for all workers with less than 18 years of contributions at the time of the reform

[4] This increase in contributions was essentially compensated by a corresponding reduction in the contributions for the family benefits (assegno per il nucleo familiare).

Interestingly, the same workers who managed to escape the retrenchment of the Dini reform had already slipped through the Amato reform. Returning to our example, while Mr. Old maintained his (expected) replacement rate of 80%, and his pension benefit to €1,120, Mr. Young had only 17 years of contributions in 1995 and thus had to face a further reduction in his (expected) pension benefits. Leaving his retirement age unchanged at 60 years, his replacement rate would in fact drop to around 65%, and his pension benefit to €910. The overall effect of the two reforms is thus sizable. In our example, the difference in the pension benefit for affected individual (Mr. Young), with respect to the unaffected individual (Mr. Old), is 23%. Moreover, since pension benefits constitute the (almost) exclusive source of old age income (around 95% of the average income), this large gap is likely to induce individual responses to the reforms.

The literature has already exploited this peculiar discontinuity and the related natural experiment created by the Italian (Amato and Dini) pension reforms. These contributions include Attanasio and Brugiavini (2003), who estimate the effect of the reduction in pension benefits on savings, Bottazzi, Jappelli and Padula (2006), who analyze the impact on retirement decisions, Manacorda and Moretti (2006), who concentrate on the decision of young children to leave the parental home, Battistin et al. (2009), who investigate the size of the consumption drop at retirement, and Battistin, De Nadai and Padula (2014), who analyze the effect of the reforms on the potential childcare supply of grandparents—and therefore indirectly on fertility. Bottazzi, Jappelli and Padula (2006) assess the differential impact of the Amato and Dini reforms on three classes of workers: those with a seniority of 18 years in 1995 (and 15 in 1992), those with a lower seniority and those who entered the labor market after 1995. The differences in the reduction of their replacement rate -- as measured by the ratio of pension benefit to the average wage in the last five years prior to retirement -- are quite large. Among the private employees retiring at age 60, the replacement rate is reduced by 1 point (from 67.3% to 66.3%) for the senior workers, by 9.1 points (from 67.3% to 58.2%) for the less senior and by 12.4 points for the young. The impact is larger among public sector employees, with a drop of 5.1 points among the senior, of 20.6 among the less senior and of 26.7 among the young.

These major changes did not come unperceived, nor was the differential impact of the reform across generation of workers underestimated. Quite the opposite. Massive strikes broke out in 1992 and 1994, and a large debate took place in the press. Moreover, estimates by Bottazzi et al. (2006) suggest that private employees were well aware of the magnitude of the reform and of the fact that its differential impact depended on the years of contributions. In particular, less senior private employees expecting to retire at age 60 quite accurately forecasted their replacement rate to be reduced by 8.4 points. The relevance of these reforms and their differential effect is also evident in the workers' intention to postpone retirement after the reform. Faced with a large negative income effect, as shown in Bottazzi et al. (2006), middle aged individuals (born after 1957) reported a higher expected retirement age than more senior workers.

Data and modelling strategy

Data

In order to assess the effect of pension reforms in Italy on fertility, we compare individuals who are affected by these reforms with individuals who are not affected. We therefore exploit the discontinuity induced by the two reforms. We analyze two specific datasets that we built using data from the Bank of Italy's Survey of Italian Households' Income and Wealth (SHIW from now onwards). The SHIW is a biannual survey, which mostly collects data on income and wealth of Italian households, and we pool data from the 1998, 2000, 2002, 2004 and 2006 surveys. Crucial for our identification strategy is the fact that the SHIW contains data, provided by respondents, on the total number of years each household member has contributed to the pension system (at December 31st of the reference year of the survey). We assume that the number of years of contributions at December 31st 1992 (or 1995), i.e., the reform reference date, can be derived from the number of years of contributions at December 31st 1992+ζ , where ζ=6,8,10,12,14, depending on the most recent SHIW wave for which a given respondent was interviewed. For example, a person who has 29 years of contributions in 2006 (December 31st) is assumed to have had 15 years of years of contributions in 1992 (December 31st) -- and therefore to be unaffected by the reforms. On the contrary, a person who has 28 years of contributions in 2006 is assumed to have 14 years of contributions in 1992 -- and thus to be affected by the reforms.

$reform^M$ and $reform^F$ are therefore a simple dichotomous variables representing the treatment effect, respectively for males and females, in this natural-experiment setting. Recall bias, lack of precision in reporting years of contributions and gaps in working careers may induce measurement error in the identification of the treatment and control group. However, the bias of such measurement errors implies an underestimation of the effect of the reform, therefore the subsequent results are likely to be conservative, and therefore constitute lower bound estimates. This measurement error is certainly smaller for working-age Italian men, who show a strong attachment to the labor market, than for women, who experience more discontinuous working careers (e.g., Bernardi 2001). We use this strategy to identify 'affected' and 'unaffected' individuals. The lack of more precise and reliable data on contributions (e.g. months, or weeks) prevents us from adopting a regression discontinuity design in subsequent analyses, which would allow to estimate treatment effects around the exact discontinuity (Lee and Lemieux 2010; Thistlethwaite and Campbell 1960).

Fertility is considered as a household decision. We focus on households with individuals who were married at the time of the surveys. The focus on married couples should not bias our results, given the particularly low extra-marital birth rates and divorce rates during the period covered (see, e.g., Castiglioni and Dalla-Zuanna 2009). We choose to evaluate the effect of the reform primarily focusing on husbands as the affected or unaffected individuals, because of the larger relevance of their income on the family budget, but we also consider the effect for wives, who are directly affected or unaffected. Yet, given the low labor force attachment of Italian women during this period – and thus their few years of contributions –only few women in the age range of interest to us were not directly affected by the reform during their childbearing ages.

We exploit additional data provided by the SHIW, such as the date of births of all household members, including co-resident children. Moreover, we use data on the number (although not the date of birth, which is not available) of non-resident children. In our analyses, we also include other control variables, such as educational level of both partners and the area of birth of the husband. We can reconstruct the couple's fertility history after 1992 by using the date of birth of co-resident children, i.e. using the 'own-children' method for the reconstruction of fertility histories (Cho, Retherford and Choe 1986). The own-children method has recently been reappraised favorably by Avery et al. (2013) in general, and has been applied to the case of Italy with success (Bordone, Billari and Dalla Zuanna 2008; Cicali and De Santis 2002). We assume that all non co-resident children are born before the time of the first reform, i.e. up to 1992 (this assumption does not affect subsequent results as we focus on childbirths from 1993 onwards). The assumptions behind the own-children method are relatively mild in the case of Italy where children tend to co-reside with parents for a long time (well into their mid 20s) and infant mortality is extremely low. We only use data on household with wives born in 1955 or after (who were therefore not older than 41 in 1996).

It is important to note that reforms affected couples in what in Italy are central childbearing ages. In our sample, men who were affected by the reform were slightly older than 35 (the average age of affected husbands in 1992 was 35.45 years). Their wives were on average 3.8 years younger (the average age of the wife in 1992 was 31.67 years). According to data from the Italian Statistical Office (ISTAT), Italy's average age at motherhood was 29.3 in 1992, 30.8 in 2004, and 31.8 in 2016. Therefore, the age interval we are considering is particularly relevant for fertility choices, and Italy has become a leading industrialized country in late childbearing (Billari et al. 2007). This is a clear consequence of the postponement of childbearing, a phenomenon that accompanies (and partly causes) the emergence of lowest-low fertility, and that is linked to a -- at least partial -- recuperation of the postponed births at later ages (Balbo, Billari and Mills 2013).

Modelling Strategy

We first perform a series of simple t-tests in order to compare the mean fertility of individuals who are just before (up to a year) and just after (up to a year) the threshold (15 years of contributions at the end of 1992, 18 years of contributions at the end of 1995) using information available or reconstructed at the time of the surveys. Table 2 contains the results of these tests on individuals who are as close to the discontinuity as we could get, performed on a sample of 200 unaffected individuals and 198 affected individuals. Indeed, while the number of children prior to 1993 is not significantly different between the two groups, fertility after the reforms is significantly higher for the treated. More specifically, up to 1993 unaffected individuals have on average 1.39 children, while affected individuals have on average 1.38 children. After 1993, affected individuals have on average 0.49 children, while unaffected individuals have on average 0.31 children during the same period, which includes the effect of both reforms. The average number of children up to 2006 for affected individuals (1.87) is 10.6% higher than the total number of children for unaffected individuals (1.69). Our first evidence is therefore in favor of a significant, and sizable, negative effect of pensions on fertility.

TABLE 2 ABOUT HERE

FIGURE 1 ABOUT HERE

A graphical representation on this effect is shown in Figure 1. In the upper panel, average pre-reform fertility by years of contribution at the end of 1992 is shown. There is a clear increasing trend, which heavily depends on the link between age and years of contributions--an issue that we will address in our multivariate models later. In the lower panel there is an equally clear decreasing trend in the average number of post-reform children. At first glance, when looking at simple nonparametric (moving average) trend estimates, there is a discontinuity of about 0.15 children corresponding to the reform.

Whether a person is affected or not by the reforms depends on the assumption of continuous labor market attachment and on the good measurement of the variable of our interest. Moreover, other covariates may influence the estimation of the reform effect. The results of the simple comparisons using t-tests displayed in Table 2 and of the discontinuity plots in Figure 1 are hence subject to limitations. In particular, given the link between age at entry in the labor market and exposure to the reform, we can expect that unaffected individuals are, on average, older than affected individuals. If we take, as in Table 2, a one-year window around the reforms' threshold, we find that affected husbands have an average age of 35.45 years, against an average age of 36.62 years for unaffected husbands. An average difference of one year in age translates almost equally into an average difference of one year in contributions. The same is true for the wives, as the average age of the wives of affected individuals is

31.67 years, against 32.91 years for the unaffected. Figures 2 and 3 show, however, that, despite the average one-year age difference between affected and unaffected individuals, there is a substantial amount of variability in age, with some common support and an important overlap in the age distributions of affected and unaffected individuals, which allows us to identify the effect of reform, while controlling for the age of individuals (both husbands and wives). Therefore, in what follows we choose to develop a series of regression models that control for the different age distribution, as well as for other potentially influential factors. As we shall see, these models confirm the findings of the previous approach.

FIGURES 2 AND 3 ABOUT HERE

We therefore extend our analyses to a multivariate setting, with the introduction of a series of control variables, which are likely to affect both the inclusion into the treatment or control group and fertility outcomes. In particular, we control for age (and education) of the husband, age (and education) of the wife, geographical area (using the area of birth of the husband). In order to have a more robust sample size, we also extend our sample to include individuals who are more distant from the discontinuity induced by the reform. In the following section, we also carry some additional sensitivity analyses for the robustness of our results.

Most analyses are conducted using a dataset where we compare individuals who are up to 7 years below the threshold number of years of contributions (and thus affected by the reform) with individuals who are up to 7 above the threshold (unaffected). The sample size for these analyses is 2,675, with 59.65 percent of husbands and 92.89 percent of wives being affected by the reform. Table 3 presents descriptive statistics on this sample (all variables, with the exception of fertility and reform refer to the time of the survey). First, we estimate simple OLS models of the type

$$f_i = \beta_0^0 + \beta_1^0 \cdot reform_i^M + \beta_2^0 \cdot x_i + \varepsilon_i \qquad (1)$$

In (1), f_i is post-reform fertility for the i-th individual in our sample, $reform_i^M$ is a dichotomous indicator (=1 if the i-th husband is affected, =0 otherwise), x_i is a vector of control variables, ε_i is white noise. The coefficient β_1^0 therefore allows to estimate the average effect of the reform on treated individuals on the number of post-reform children.

TABLE 3 ABOUT HERE

We then estimate parallel models with the effect of reforms on both husbands and wives:

$$f_i = \beta_0^0 + \beta_1^{0M} \cdot reform_i^M + \beta_1^{0F} \cdot reform_i^F + \beta_2^0 \cdot x_i + \varepsilon_i \qquad (2)$$

In (2), the coefficient β_1^{0M} allows to estimate the average effect of the reform on treated husbands net of the effect on wives, and β_1^{0F} allows to estimate the average effect of the reform on treated wives net of the effect on husbands.

In a second series of models we focus on parity progression, i.e. probability that at least one post-reform child is born, with a probit specification:

$$\Phi^{-1}\left(\Pr(f_i > 0)\right) = \beta_0^1 + \beta_1^1 \cdot reform_i^M + \beta_2^1 \cdot x_i \qquad (3)$$

In (3), $\Phi^{-1}(.)$ is the inverse standard-normal distribution, and the estimated coefficient β_1^1 allows to estimate the average effect (via inverse Mill's ratio) of the reform on the probability of having at least one post-reform child for treated individuals. Analogously to equation (2), we also estimate probit models, which include the potential effect of the reform on wives.

The data, however, contain more information than just the number of post-reform children. More specifically, we also exploit information on the timing of births and build a discrete-time event-history analysis model (see, e.g., Allison 1982) adopting a hazard rate approach to the timing of births. We can exploit the fact that some of the factors we focus on vary over the observation time (this is the case of husband's and wife's ages, or calendar year). To this purpose, we build a second dataset that contains observations in terms of persons-years, i.e., an entry for each individual *i* in each given year of observation *t*, from 1993 onwards. In this second dataset, the age of husbands and wives is updated every year. The appropriate method to analyze persons-years datasets is discrete-time event history analysis. Each household contributes to the sample as long as they are observed, and they leave the sample either when they are interviewed (in this case information is right-censored) or when they have another child. As the number of post-reform children is on average low, we only consider the progression to the first birth after the reform. Therefore, with the second dataset, we use a discrete-time probit specification, where the left-hand-side variable is the hazard rate, i.e., the annual probability of having an additional birth for the individual *i* during the year *t*, given that the same individual has not yet had an additional birth in earlier years of observation:

$$\Phi^{-1}\left(\Pr(B_i = t \mid B_i \geq t)\right) = \beta_0^2 + \beta_1^2 \cdot reform_i^M + \beta_2^2 \cdot x_i + \beta_3^2 \cdot v_{ij} \tag{4}$$

In (4), B_i denotes the time of first post-reform birth, *t* is the year of observation, potentially between

1993 (t=1) and 2006 (t=14), (t=1,…,J_i, where J_i=14 is the last year of observation for the i-th individual), x_i is a vector of time-constant control variables, v_{ij} is a vector of variables that vary across years. β_1^2 is related the estimated average effect (via inverse Mill's ratio) of the reform on the yearly hazard of a post-reform birth for treated individuals. Also in this case, we estimate models including wives' reform effect as well.

RESULTS

We now examine the results of our analyses (complete results of regressions and scripts are available upon request from the authors), starting from our first dataset. Table 4 displays the results on the effect of the reform on the: a) number of children born starting from the year after each of the reforms and until the date of the survey (panel a); b) probability of having an additional child during the same period (panel b). The estimated effects of the reforms on husbands are displayed in the Reform line (in terms of marginal effects for Probit regressions). The estimated effects of the reforms on wives are displayed in the "Wife's reform" line. In these regressions, we control for several elements that may affect the number of years of contributions individuals had up to the end of 1992 and their fertility behavior. In particular, we control for level of education of husbands and of wives, geographical area, and the number of kids that they already had prior to the reforms. As might be expected, some of these controls have a significant effect. For instance, more educated women -- who presumably decided to postpone fertility -- are more likely to have kids after the reform. Individuals in the South are more likely to have children, whereas individuals who had more kids prior to the reform are less likely to have additional children afterwards. We also present two distinct specifications of the model, one with age fixed-effects for the age of husbands and wives (columns (1) and (3)), and one controlling for a linear effect of the years of contributions (centered around reform eligibility), which is allowed to vary for treated and non-treated individuals, in line with the regression-discontinuity approach (columns (2) and (4)).

TABLE 4 ABOUT HERE

According to these estimates, after controlling for all these covariates, the average number of children for couples with a treated husband is 0.0558 higher (significant at the 10% level) with respect to couples with a non-treated husband (column (1) in panel a). This effect only slightly diminishes when adding the effect of the reform on wives, which is not statistically significant but large. Interestingly, the effect on wives is positive, in accordance with theory, albeit not statistically significant, possibly due to the fact that only a small share of wives are unaffected by the reform. The magnitude of the first effect (on husbands) should be compared to an average of 0.5089 post-reform children (i.e., it amounts to 11% higher fertility for affected individuals). The effects tend to be similar, albeit with different levels of statistical significance, for models that control for age fixed effects and for models that control for years of contribution. Results of regression models on the larger sample of individuals therefore confirm the findings obtained with the smaller time window around the reform. Using estimates by Attanasio and Brugiavini (2003) on the effect of pension wealth of the reforms, and our own estimates on the replacement rate discussed earlier, our results suggest that a 1 per cent decrease in pension wealth leads to an increase in fertility between 0.26 and 0.46 per cent.[5] This result is therefore slightly higher than the effect obtained by Gábos et al. (2009) using a time-series approach, who estimate that a 1 per cent decrease in pensions increases fertility by 0.2 per cent.

Results using model (3) on the probability of having an additional post-reform child also point towards

[5] Our estimates suggest a drop in the replacement rate, and thus in the pension wealth, induced by the reforms of 23%, while Attanasio and Brugiavini (2003) estimate the largest overall drop in pension wealth – occurred for the public employees, to be around 41.8%.

the same direction: individuals who are affected by the reform have a 7.1% higher probability of having post-1992 reform children (with consistent results across different model specifications). Also the effect of the reform due to the change in the wives' future pension benefits is positive and large (8.3%, controlling for husband's treatment), as predicted by all fertility theories, and statistically significant. Therefore, the findings from this set of regression models confirm that the joint effects of the two reforms (post-1992) is statistically significant, and strong.

From now onwards, our results refer to the second dataset, i.e., the one with discrete-time data on persons years, and to estimates based on equation (4). We specify a model in which we estimate the joint effect of the reforms, i.e., the post-1992 effect. Table 5 displays the results of a first probit hazard model, in which both husbands' and wives' ages are time-varying covariates (using fixed-effects), and in which we control for fixed period effects using dummy variables for each year (columns (1) and (3)). The marginal effect of the reform on the annual probability of having an additional birth is 0.67%. This effect can be compared to the observed (average) annual probability, which is above 5%. The reform is estimated to raise the annual probability of having a(nother) child by 12.9% in relative terms. The effect is statistically significant at the 5% level. The effect on wives, albeit strong, is not statistically significant. This analysis, which makes use of additional information contained in the data and controls for time-varying effects, thus confirms the results obtained with the first dataset. Results are consistent when we control for the running variable rather than for age fixed effects (columns (2) and (4)).

TABLE 5 ABOUT HERE

We also run two types of robustness checks for the reform effect using the second dataset (similar robustness checks have been run on the first dataset giving analogous results). A first robustness check regards the size of the time window around the reform that we use. Our standard models use a +/-7 year-wide window. The fact that age (controlled via fixed effects) is not behind the estimated effect is reassuring, but we conduct a robustness check by using shorter time windows around the reform. Table 6 contains the output of such checks, compared to the reform effect displayed in Table 5. The effect is stable with a +/- 3 year window. It is much higher, still significantly positive, but estimated with lower precision, as the window becomes the smallest one (+/- 1 year). The stability of the estimates with the variation of the time window is a sign of robustness of the positive effect of the reform on fertility, while the fact that the effect becomes higher with the shorter window is consistent with the effect of reform being captured in a cleaner way with the shorter window.

TABLE 6 ABOUT HERE

A second robustness check of our identification strategy is a "placebo" test, which is often used in studies on natural experiments, like ours, which exploits discontinuities. More specifically, we estimate the effect of two discontinuities that we expect not to matter, as they are in fact not related to the reform. A first discontinuity (Younger placebo) is placed around 10 years of contributions in 1992, with a window of +/- 1 years around the discontinuity. A second discontinuity (Older placebo) is placed around 20 years of contributions in 1992, with a window of +/- 1 years around the discontinuity. The estimates of placebo effects are compared with the estimates of the reform effect with a +/-1 year time window in Table 7. Indeed, placebo effects are not statistically significant, which is what we expect if our identification strategy through a discontinuity in years of contributions picks the reform effect: only the discontinuity around the actual reform matters.

TABLE 7 ABOUT HERE

SUMMARY AND CONCLUDING REMARKS

In a contemporary low fertility society, characterized by strong family ties -- Italy, we have exploited the discontinuities induced by two parallel pension reforms held in 1992 and 1995 to test the effect of a change in expected pension income on fertility. These reforms have in fact generated a natural experiment that has exogenously reduced the pension income prospects of individuals with years of contribution below specific thresholds, while leaving others unaffected. Our results show that individuals who have lower pension income prospects have had significantly higher fertility with respect to their counterparts, who were not affected by the reforms. The relative increase of the realized fertility or of the probability of having a child is above 10%. An increase in fertility is caused by the expected negative income effect on the husbands' future pension benefits. Moreover, an increase in fertility is caused also by the reduction in future pension benefits for the wives. In the latter case, the reform – by reducing the impact of the current contributions on future pension benefits – might also have induced wives to reduce their working days.

We believe that our results are of general relevance for the study of fertility motives in developed societies, as they contribute to identify a clear negative impact of the retrenchment of pension systems on fertility decisions. This is of particular relevance to the study of very-low and lowest-low fertility. If part of the fertility decline can be attributed to the diffusion of pension systems, the introduction of pension reforms that decrease the income prospects after retirement might contribute to a rise in fertility. Indeed, fertility in Italy had its minimum in 1996 and since then it has been slowly rising until the Great Recession.

Our findings are supportive of the old-age security theory of fertility or of the economic theory of fertility that emphasizes a quality-quantity trade-off. Old-age security motives would also be consistent with the existence of strong family ties in the Italian (Dalla Zuanna 2001; Reher 1998), as well as in several other contemporary developed societies. According to a measure of the strength of family ties constructed by Alesina and Giuliano (2010), Italy ranks third among the OECD countries, after Mexico and Poland and followed closely by the US and Spain, while Germany and the Scandinavian countries have the weakest family ties. In this environment of family culture, parents may reasonably expect their kids to give them old-age support, for instance as in-kind, monetary transfers or co-residence. To further test the quality-quantity tradeoff idea, one would need to have access to data on parental investments on each child of affected vs. unaffected individuals, which unfortunately we do not have access to. However, there is evidence for 'dilution effects', i.e. lower educational outcomes for children raising in larger families also in the case of Italy (Ferrari and Dalla Zuanna 2010).

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TABLES AND FIGURES

Table 1. Pension reforms of the 1990s in Italy

	Pre-1993 regime	1992 reform	1995 reform
Normal retirement age	60 (men) 55(women)	65 (men) 60(women)	Any age after 56 (for both men and women)
Transitional period		Until about 2032	Until about 2035
Pensionable earnings	Average of last 5 years real earnings (converted to real values through price index)	Career average earnings (converted to real values through price index + 1%)	Career contributions (capitalized using a 5-year moving average of GDP growth rate)
Pension benefit	2%*(pensionable earnings)*(t), where t is years of tax payments (at most 40)	2%*(pensionable earnings)*(t), where t is years of tax payments (at most 40)	Proportional to capitalized value of career contributions, the proportionality factor increasing with age at retirement (from .04720 at age 57 to .06136 at age 65)
Pension indexation	Cost of living plus real earnings growth	Cost of living	Cost of living
Pension to survivor	60% to spouse 20% to each child 40% to each child (if no spouse)	Same	Same
Years of contributions for eligibility	15	20	5
Early retirement provision	Any age if contributed to SS for 35 years or more, no actuarial adjustment	Any age if contributed to SS for 35 years or more, no actuarial adjustment	No early retirement provision
Total Payroll tax	24.5% of gross earnings	27.17% of gross earnings	32.7% of gross earnings

Table 2. Differences between husbands who are affected and unaffected by the reforms. +/- 1 year-window around the reforms' thresholds.

	Unaffected (up to - 1 year)	Affected (up to +1 year)
Number of children (up to 1993)	1.3850	1.3788
	(0.0746)	(0.0802)
Number of children (after 1993)	0.3050	0.4899***
	(0.0414)	(0.0468)
Total number of children (up to 2006)	1.6900	1.8687*
	(0.0670)	(0.0772)
N	*200*	*198*

Standard errors in parentheses.
*Significance levels on the 2-tail t-test on the hypothesis of difference between the affected and the unaffected: * significant at 10%; ** significant at 5%; *** significant at 1%*
Source: own analyses on Bank of Italy's Survey on Household Income and Wealth (joint dataset waves 1998, 2000, 2002, 2004, 2006).

Figure 1. Mean number of children before 1993 (upper panel) and 1993 onwards (lower panel) by years of contribution at the end of 1992. Husbands affected by the reform to the left (up to 14 years), individuals unaffected by the reform to the right (15 years and over). Marks are empirical means, lines represent nonparametric smoothed values (3-values moving average up to the discontinuity point.

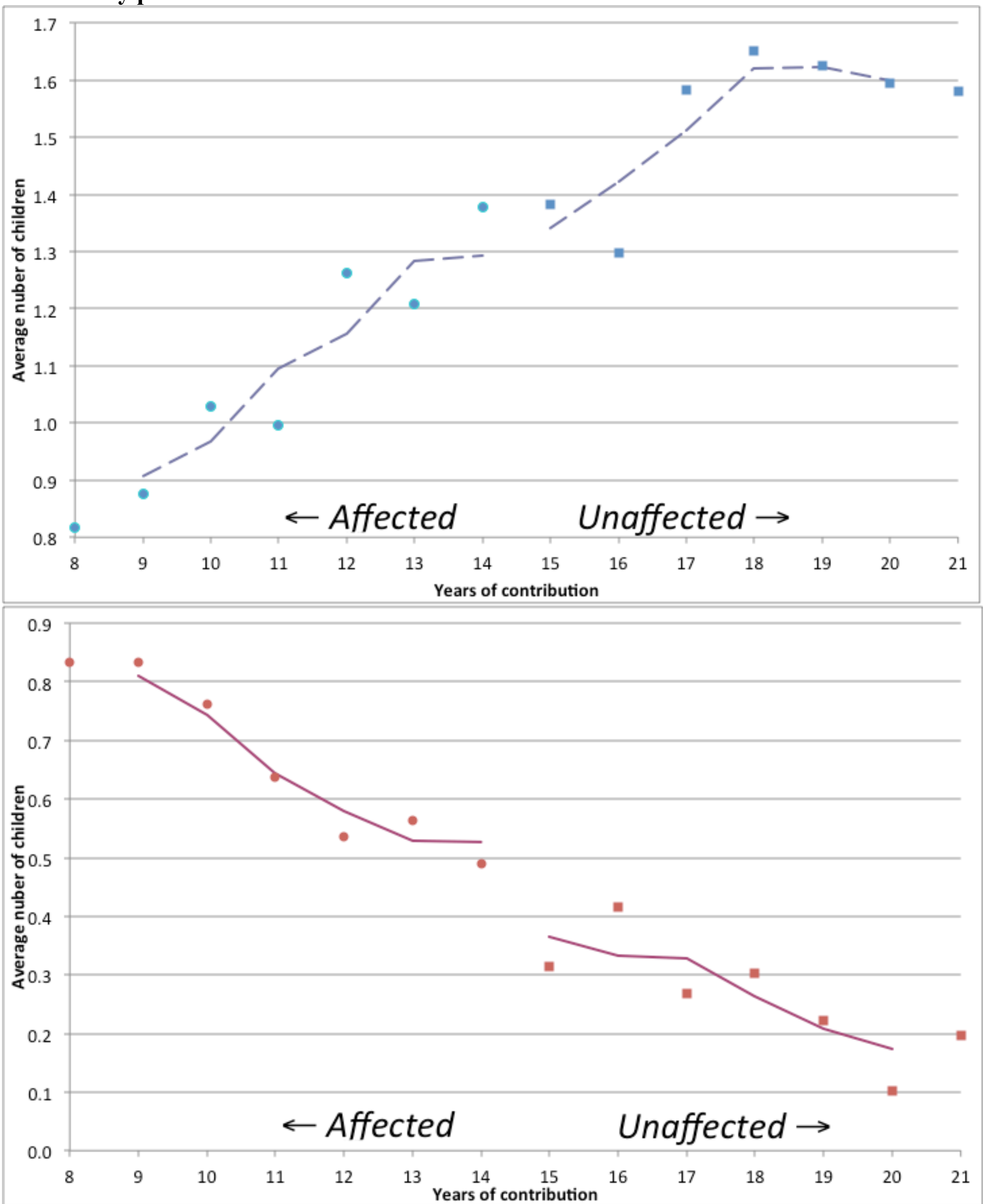
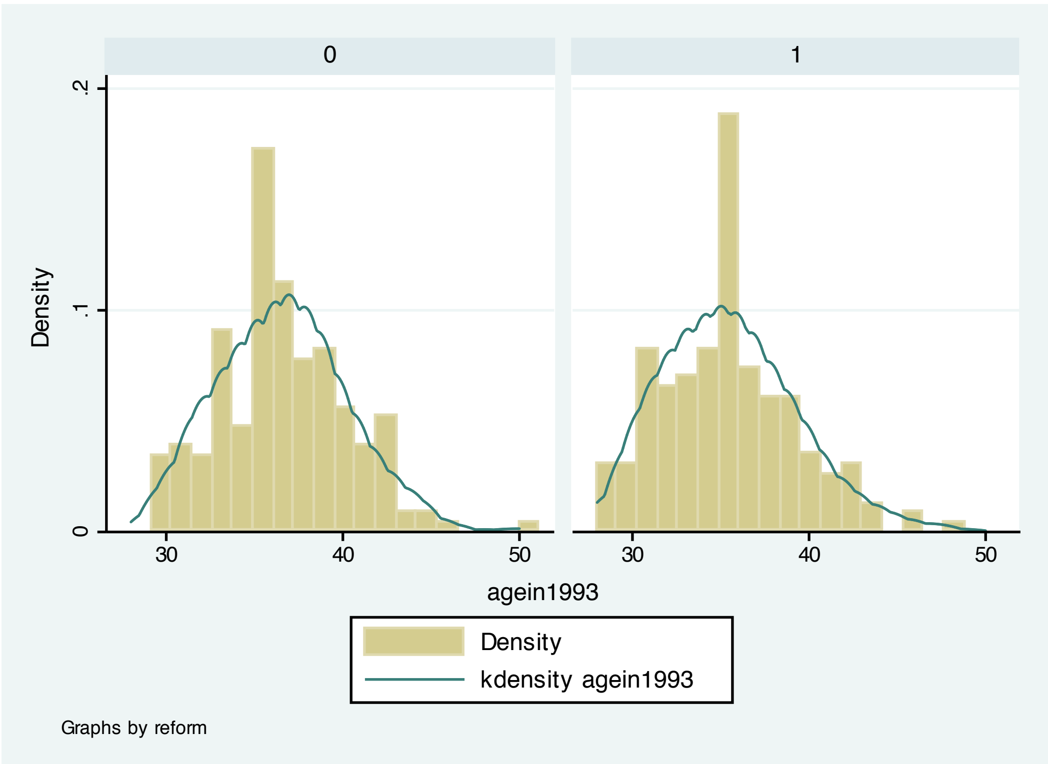
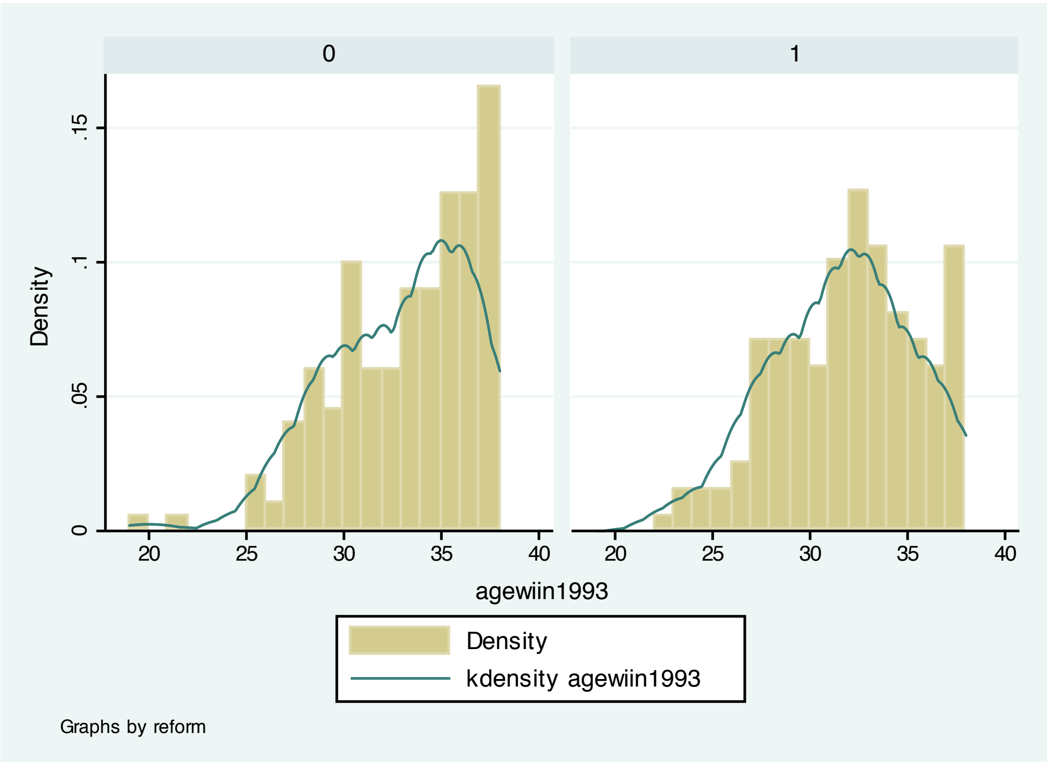


Figure 2. Age distribution in 1993 for husbands unaffected and affected by the reforms. Window: +/- 1 year of contributions around the reforms' thresholds. Wives born 1955 or after.



Mean age in 1993 for N=200 unaffected husbands is 36.62 years, for N=198 affected husbands is 35.45 years. The difference is statistically significant at the 1% level (t-test).

Figure 3. Age distribution in 1993 for wives whose husbands have been unaffected and affected by the reforms. Window: +/- 1 year of contributions around the reforms' thresholds for wives. Wives born 1955 or after.



Mean wife's age in 1993 for N=200 with unaffected husbands is 32.91 years, for N=198 with affected husbands is 31.67 years. The difference is statistically significant at the 1% level (t-test).

Table 3. Descriptive statistics for variables used in subsequent analyses. Window: +/- 7 years of contributions around the reforms' thresholds for husbands. Wives born 1955 or after.

	Mean	s.d.
Reform (dummy)	0.5965	
Wife's reform (dummy)	0.9289	
Number of children (up to 1993)	1.2522	1.0260
Number of children (after 1993)	0.5089	0.7338
Education (husband, years)	10.4411	3.4473
Education (wife, years)	10.6971	3.4377
Age at interview (husband)	45.6997	5.4437
Age at interview (wife)	41.6651	5.1768
Center as area of birth (dummy)	.1803	
South as area of birth (dummy)	.3882	
Survey year 1998 (dummy)	.0384	
Survey year 2000 (dummy)	.2086	
Survey year 2002 (dummy)	.2198	
Survey year 2004 (dummy)	.1907	
N	2,675	

Source: own analyses on Bank of Italy's Survey on Household Income and Wealth (joint dataset waves 1998, 2000, 2002, 2004, 2006).

Table 4. Effect of pension reforms on post-reform fertility (panel a: total number of children; panel b: probability of having at least an additional child). Window: +/- 7 years of contributions around the reforms' thresholds for husbands. Wives born 1955 or after.

a)

	(1)	(2)	(3)	(4)
	OLS Model 1	OLS Model 1 (w/ Linear Running Var)	OLS Model 2	OLS Model 2 (w/ Linear Running Var)
Reform	0.0558*	0.0612	0.0507*	0.0479
	(0.0286)	(0.0489)	(0.0288)	(0.0488)
Wife's reform			0.0671	0.261***
			(0.0491)	(0.0555)
Education (wife)	0.0176***	0.0142***	0.0173***	0.0132***
	(0.00426)	(0.00448)	(0.00426)	(0.00447)
Center	-0.00247	-0.0404	-0.00504	-0.0470
	(0.0336)	(0.0352)	(0.0337)	(0.0352)
South	0.108***	0.0573*	0.104***	0.0457
	(0.0284)	(0.0294)	(0.0286)	(0.0295)
Year 1998	-0.730***	-0.158**	-0.723***	-0.163**
	(0.0724)	(0.0679)	(0.0725)	(0.0677)
Year 2000	-0.455***	-0.0607*	-0.449***	-0.0600*
	(0.0409)	(0.0349)	(0.0411)	(0.0348)
Year 2002	-0.323***	-0.0748**	-0.320***	-0.0756**
	(0.0365)	(0.0342)	(0.0366)	(0.0341)
Year 2004	-0.153***	-0.0158	-0.151***	-0.0175
	(0.0356)	(0.0360)	(0.0357)	(0.0359)
Number of children (up to 1993)	-0.175***	-0.267***	-0.176***	-0.266***
	(0.0140)	(0.0134)	(0.0140)	(0.0134)
Age fixed effects (husband)	YES	NO	YES	NO
Age fixed effects (wife)	YES	NO	YES	NO
N	2,675	2,675	2,675	2,675
R-squared	0.302	0.225	0.303	0.231

b)

	(1)	(2)	(3)	(4)
	Probit Model 1	Probit Model 1 (w/ Linear Running Var)	Probit Model 2	Probit Model (2 w/ Linear Running Var)
Reform	0.0713***	0.0867**	0.0655***	0.0771**
	(0.0244)	(0.0378)	(0.0246)	(0.0380)
Wife's reform			0.0829*	0.201***
			(0.0443)	(0.0359)
Education (husband)	0.00500	-0.00823**	0.00520	-0.00708**
	(0.00382)	(0.00353)	(0.00382)	(0.00354)
Education (wife)	0.0160***	0.0117***	0.0154***	0.0107***
	(0.00370)	(0.00357)	(0.00370)	(0.00357)
Center	0.0278	0.00651	0.0256	0.00198
	(0.0293)	(0.0280)	(0.0293)	(0.0280)
South	0.0831***	0.0403*	0.0783***	0.0312
	(0.0248)	(0.0235)	(0.0249)	(0.0236)
Year 1998	-0.334***	-0.116**	-0.332***	-0.119**
	(0.0175)	(0.0493)	(0.0179)	(0.0488)
Year 2000	-0.290***	-0.0401	-0.285***	-0.0404
	(0.0245)	(0.0271)	(0.0249)	(0.0272)
Year 2002	-0.216***	-0.0371	-0.212***	-0.0379
	(0.0254)	(0.0265)	(0.0255)	(0.0265)
Year 2004	-0.104***	0.00486	-0.102***	0.00325
	(0.0278)	(0.0283)	(0.0278)	(0.0283)
Number of children (up to 1993)	-0.128***	-0.192***	-0.130***	-0.191***
	(0.0124)	(0.0114)	(0.0124)	(0.0114)
Age fixed effects (husband)	YES	NO	YES	NO
Age fixed effects (wife)	YES	NO	YES	NO
N	2,675	2,675	2,675	2,675

*Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. In columns(2) and (4), the running variable is a linear polynomial in distance from the threshold of years of contributions needed to be targeted by the reform, and specifications include the linear polynomial and its interaction with the Reform and Wife's reform dummies.*

Table 5. Marginal effect of pension reforms on the annual probability of having an additional child (discrete-time probit event-history model on persons-years). Window: +/- 7 years of contributions around the reforms' thresholds for husbands. Wives born 1955 or after.

	(1)	(2)	(3)	(4)
	PP Probit Model 1	PP Probit Model 1 (w/ Linear Running Var)	PP Probit Model 2	PP Probit Model 2 (w/ Linear Running Var)
Reform	0.00672**	0.00957**	0.00618**	0.00824**
	(0.00286)	(0.00402)	(0.00288)	(0.00399)
Wife's reform			0.00790	0.0185***
			(0.00518)	(0.00321)
Education (husband)	0.000513	-0.000774**	0.000526	-0.000661*
	(0.000428)	(0.000366)	(0.000427)	(0.000363)
Education (wife)	0.00155***	0.00105***	0.00151***	0.000942***
	(0.000402)	(0.000364)	(0.000402)	(0.000360)
South	0.00980***	0.00439*	0.00941***	0.00349
	(0.00285)	(0.00248)	(0.00285)	(0.00245)
Center	0.00264	0.000637	0.00244	0.000102
	(0.00326)	(0.00289)	(0.00325)	(0.00283)
Number of children (up to 1993)	-0.0144***	-0.0197***	-0.0145***	-0.0193***
	(0.00147)	(0.00124)	(0.00147)	(0.00123)
Age fixed effects (husband, time-varying)	YES	NO	YES	NO
Age fixed effect (wife, time-varying)	YES	NO	YES	NO
N	19,708	19,708	19,708	19,708

*Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. In columns(2) and (4), the running variable is a linear polynomial in distance from the threshold of years of contributions needed to be targeted by the reform, and specifications include the linear polynomial and its interaction with the Reform and Wife's reform dummies.*

Table 6. Robustness checks. Marginal effect of pension reforms on the annual probability of having an additional child (discrete-time probit event-history model on persons-years) (varying window around the reforms' thresholds for husbands, wives born 1955 or after).

	(1) window: +/- 7 years	(2) window: +/- 3 years	(3) window: +/- 1 year
Reform	0.00672**	0.00662*	0.0170**
	(0.00286)	(0.00341)	(0.00726)
N (persons-years)	19708	9150	2447
Observed P	0.0521	0.0483	0.0527

*Standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%*
Source: own analyses on Bank of Italy's Survey on Household Income and Wealth (joint dataset waves 1998, 2000, 2002, 2004, 2006, persons-years reconstruction).
Same control variables as for the models in Table 4.

Table 7. Marginal effect of pension reforms on the annual probability of having an additional child (discrete-time probit event-history model on persons-years). Placebo test (one-year window around different contribution thresholds for husbands, wives born 1955 or after).

	(1) "Younger" placebo (window: +/- 1 year)	(2) Real reform (window: +/- 1 year)	(3) "Older" placebo (window: +/- 1 year)
Reform	-0.0096 (0.0118)	0.0170** (0.00726)	0.0043 (0.0049)
N (persons-years)	2197	2447	1137
Observed P	0.0992	0.0527	0.0237

Standard errors in parentheses
** significant at 10%; ** significant at 5%; *** significant at 1%*
Source: own analyses on Bank of Italy's Survey on Household Income and Wealth (joint dataset waves 1998, 2000, 2002, 2004, persons-years reconstruction).
Same control variables as for the models in Table 4. The "Younger" placebo model estimates the effect of a discontinuity around 10 years of contributions in 1992, with a window of +/- 1 year around the discontinuity. The "Older" placebo model estimates the effect of a discontinuity around 20 years of contributions in 1992, with a window of +/- 1 year around the discontinuity.