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PARENTAL RETIREMENT AND FERTILITY DECISIONS ACROSS FAMILY POLICY REGIMES

by Edoardo Frattola*

Abstract

This paper investigates whether parental retirement affects the timing of adult couples' fertility decisions and whether the effect is heterogeneous across family policy regimes in Europe. I use SHARE data for the period 2004-2018 and consider 11 countries belonging to three different regimes (Continental, Mediterranean and Nordic). Results from a regression discontinuity design suggest that parental retirement has a significant and positive causal effect only in Mediterranean countries, where it is driven by an increase in the availability of informal childcare. These findings are consistent with the hypothesis that parental support matters more in countries with weaker family policies and stronger family ties, and indicate that increases in the retirement age might have unintended negative consequences on fertility rates in Southern Europe by delaying adult couples' fertility decisions.

JEL Classification: J13, J14, J26.
Keywords: retirement, fertility, grandparents, informal childcare, SHARE.
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Contents

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1 Introduction[1]

Low fertility is one of the main challenges that developed countries have to face in this century. Total fertility rates are currently below the replacement level of 2.1 children per woman in reproductive age in all 27 EU member states. Some countries, like Italy or Spain, seem to be stuck in a "lowest-low fertility" regime, with less than 1.3 children per woman (Kohler, Billari, and Ortega (2002)). Together with recent increases in life expectancy, low fertility is the main source of population ageing and therefore a potential threat to the sustainability of the welfare state as we know it today.

As an attempt to limit the expected growth of public spending for old-age pensions, in the last three decades many European countries have approved reforms that increased the minimum retirement age (Hinrichs (2021); Börsch-Supan and Coile (2021)). However, by extending the length of working life, these reforms may have an unintended negative effect on the offspring generation's fertility rates. If adult children tend to wait until their old parents' retirement before having a child, increasing retirement age would delay fertility decisions as well.[2] In turn, this postponement of births can affect completed fertility, given that, despite recent scientific improvements, women's reproductive life is still limited by biological factors and their fecundability declines with age (Billari, Kohler, Andersson, and Lundström (2007)).[3,4]

In this paper, I study whether parental retirement does affect the timing of adult couples' fertility decisions, and how this effect varies across Europe. A mechanism through which old parents' retirement and adult children's fertility may be connected is the existence of intergenerational downward transfers of time and money within the family.[5] The size of these transfers clearly varies over the life cycle, and parental retirement is likely to be a key event that can change their magnitude. A priori, the effect of parental retirement on the offspring generation's fertility decisions is ambiguous. On the one hand, retired parents have more free time than before and may increase time transfers in favor of their adult children (as shown for instance by Eibich (2015)); in turn, the availability

[1] I would like to thank Andrea Ichino and Thomas Crossley for their extremely valuable advice and suggestions, Marco Bertoni for sharing his data on pension eligibility across countries, and Niccolò Cattadori, Andrea Cintolesi, Antonio Dalla Zuanna, Silvia Del Prete, Peter Eibich, Elena Lazzaro, Fabrizia Mealli, Marco Tonello, three anonymous referees and all participants at the Bank of Italy research seminar and EUI working groups for their very useful comments. The views expressed in the paper are those of the author and do not necessarily reflect those of the Bank of Italy. This paper uses data from SHARE Waves 1, 2, 4, 5, 6, 7 and 8 (DOIs: 10.6103/SHARE.w1.800, 10.6103/SHARE.w2.800, 10.6103/SHARE.w4.800, 10.6103/SHARE.w5.800, 10.6103/SHARE.w6.800, 10.6103/SHARE.w7.800, 10.6103/SHARE.w8.800); see Börsch-Supan, Brandt, Hunkler, Kneip, Korbmacher, Malter, Schaan, Stuck, and Zuber (2013) for methodological details. Contact: edoardo.frattola@bancaditalia.it.

[2] Throughout this paper, I consider a three–generation family composed of "old parents" (first generation), "adult children" (second generation) and "grandchildren" (third generation).

[3] For instance, considering the sample of 11 European countries I will focus on in this paper, the 99^{th} percentile of the distribution of births by age of the mother did not move from age 42 between 2004 and 2020 (source: Eurostat).

[4] Bratti and Tatsiramos (2012) find that a delay in motherhood has a negative effect on the transition to a second birth and so on completed fertility, at least in Southern Europe. Here, the negative effect is due not only to biological but also to socio-cultural factors that may discourage late childbearing.

[5] As shown by Albertini, Kohli, and Vogel (2007), in contemporary Europe downward transfers of time and money are typically much more frequent and much more intense than those in the opposite direction, so that adult children are net beneficiaries of intergenerational support. On average, old parents are net givers of around 2,500 euros and 500 hours per year in favor of their children.

of old parents willing to transfer time for free childcare can increase adult couples' propensity to have a child by reducing expected childcare costs.[6] On the other hand, if pension income is lower than labor income, retired parents may be forced to reduce their downward monetary transfers, thus negatively affecting their adult children's fertility choices.[7] The overall effect on the probability of a childbirth then depends on which of these two effects prevails, either a positive time effect or a negative income effect.

In addition to being a priori ambiguous, the effect of parental retirement is also likely to vary across societies with different family policy regimes, that is with different sets of family norms and public policies supporting families with children (Rutigliano (2020)). Moving from Esping-Andersen (1990)'s seminal work on the typology of welfare regimes, some consensus has then emerged in identifying four main family policy regimes in Europe (as better explained in Section 2; Gauthier (2002)): the Anglo-Saxon, the Continental, the Mediterranean and the Nordic. Given this setting, the supportive role of old parents may be more relevant where family policies are less generous, formal childcare services are less widespread and more expensive and family ties are stronger, as it is the case in Mediterranean countries.[8] Consistently with this hypothesis, Aparicio-Fenoll and Vidal-Fernandez (2015) argue that, regardless of its sign, the causal effect of parental retirement on fertility should be larger in magnitude in areas with lower availability of formal childcare. However, not only the magnitude, but also the sign of the effect might differ across areas, if different societies attach a different value to monetary and time transfers.

This paper addresses the following two research questions: (i) Does parental retirement affect the timing of adult children's fertility decisions? (ii) Does this effect vary across family policy regimes? I use panel data covering the period 2004-2018 from the Survey of Health, Ageing and Retirement in Europe (SHARE) and focus on 11 European countries (Austria, Belgium, Denmark, France, Germany, Greece, Italy, Netherlands, Spain, Sweden and Switzerland). This cross-country sample allows me to check whether and how the effect of retirement on fertility varies across three of the four main regimes introduced above: Continental, Mediterranean and Nordic (no country in my sample belongs to the Anglo-Saxon regime). I integrate this dataset with information on eligibility for old-age pension based on the work by Bertoni, Celidoni, Dal Bianco, and Weber (2021), using it as an instrument for actual retirement status to avoid issues of endogeneity (the most obvious one being reverse causality, with a grandchild birth affecting the old parent's retirement decision; see e.g. Rupert and Zanella (2018)). I construct a balanced panel of European dynasties observed from three years before to three years after the time in which the old parent becomes eligible for old-age

[6] A stream of literature has found indeed a positive correlation between the availability of grandparental childcare and fertility (Rutigliano (2020); Aassve, Meroni, and Pronzato (2012); Kaptijn, Thomese, Van Tilburg, and Liefbroer (2010); Del Boca (2002); Garcia-Moran and Kuehn (2017); Hank and Kreyenfeld (2003)).

[7] Some scholars have provided causal evidence supporting Becker (1960)'s theory that children are "normal" goods and fertility rises with income (see e.g. Black, Kolesnikova, Sanders, and Taylor (2013); Yonzan, Timilsina, and Kelly (2020); Cohen, Dehejia, and Romanov (2013); González (2013); Laroque and Salanié (2014); Milligan (2005)).

[8] Previous research (Herlofson and Hagestad (2012); Hank and Buber (2009); Albertini, Kohli, and Vogel (2007)) has shown indeed that, even if the share of old parents who transfer positive amounts of money and time to their adult children (frequency) tends to be higher in Nordic countries, the average value of these transfers (intensity) is much larger in Mediterranean countries.

pension, and estimate the effect of interest using a fuzzy regression discontinuity design (RDD), comparing observations just above and just below the eligibility threshold.

Intention-to-treat results suggest that dynasties in Mediterranean countries experience a positive and significant jump of around 6 percentage points in the probability of a grandchild birth two years after the eligibility of the old parent, while the estimated effects are not statistically different from zero in the other two regimes (or in other years around eligibility). The magnitude of this coefficient is quite large, given a mean grandchild birth rate of 10% in the Mediterranean regime. Looking at the second-stage estimates, the local average treatment effect for the subpopulation of "compliers" (i.e. those individuals who retire exactly when reaching the eligibility age) is close to 29 percentage points in Mediterranean countries two years after retirement. This positive effect is stronger for dynasties in which the old parent is potentially more available for taking care of new grandchildren (i.e. she is in good health, she lives close to at least one of her adult children and she has at most one grandchild), supporting the hypothesis that the mechanism behind the results is a time effect. Overall, this evidence suggests that parental help matters more in countries with less generous family policies and stronger family ties, as it is the case in the Mediterranean regime, while it does not play a significant role in the rest of Western Europe. From a policy perspective, these findings hint that, as discussed above, increases in retirement age might have unintended consequences on fertility rates in Southern Europe through an effect on the timing of fertility.

This paper mostly contributes to the short stream of literature that studies the effect of parental retirement on adult children's fertility decisions in a causal framework. Scholars have focused so far on single countries and the existing evidence is mixed. Battistin, De Nadai, and Padula (2014) and Aparicio-Fenoll and Vidal-Fernandez (2015) exploit the same pension reforms introduced in Italy in the 1990s, but end up with opposite results: the former show that the number of old parents eligible for retirement (used as a proxy for potential availability of informal childcare) has a positive effect on fertility decisions of the offspring generation, while the latter find that daughters whose mothers are retired are less likely to have children than those whose mothers are still active in the labor market, because of a reduction in intergenerational monetary transfers. Eibich and Siedler (2020) look instead at Germany and their RDD estimates suggest that parental early retirement has a positive effect on the timing of adult children's fertility but no effect on completed fertility in the long run. To the best of my knowledge, my paper is the first to compare the effect of parental retirement on fertility across countries and to consider family policy regimes as a relevant factor in determining its sign and magnitude. In particular, I contribute by finding that parental support positively affects fertility only where family policies are weak and family ties are strong, a result that should be taken into account when thinking about policy implications.

In addition, this study also contributes to three broader streams of economic literature that focus respectively on intergenerational help (see e.g. Aassve, Meroni, and Pronzato (2012)), on the determinants of fertility decisions (see e.g. Doepke, Hannusch, Kindermann, and Tertilt (2022) for a recent and comprehensive review) and on the unintended consequences of pension reforms that delay parental retirement (see e.g. Boeri, Garibaldi, and Moen (2022) for the effect on labor demand;

Bratti, Frattini, and Scervini (2018) on labor supply; Stella (2017) on youth emancipation). My results confirm that health, geographical proximity and family composition are relevant factors for intergenerational help and document that the availability of informal childcare provided by retired family members can positively affect fertility choices, so that pension reforms increasing minimum retirement age may have negative side effects on fertility rates.

The remainder of this paper is organized as follows. Section 2 describes the data and the classification of family policy regimes. Section 3 presents the empirical strategy, while the results and the potential mechanism behind them are discussed in Section 4. Finally, Section 5 concludes.

2 Data

To estimate the effect of parental retirement on adult children's fertility decisions, I use data from the Survey of Health, Ageing and Retirement in Europe (SHARE), a large pan-European panel survey on individuals aged 50 and above. SHARE data contain information on demographics, current socio-economic status, health and social networks of the respondents. In particular, I use the answers from waves 1-2 and 4-8 (wave 3 does not include the regular panel interview) for 11 European countries: Austria, Belgium, Denmark, France, Germany, Greece, Italy, Netherlands, Spain, Sweden and Switzerland.[9] For each respondent, I also recover information on their eligibility age for old-age pension from Bertoni, Celidoni, Dal Bianco, and Weber (2021).

The unit of observation in this study is a dynasty, which I define as a pair composed of an old parent on the one hand, and all their adult children on the other. I restrict the original sample by considering only one old parent per dynasty (the first one in the couple answering the questions in the first wave in which they are interviewed) and dropping those dynasties whose old parent: (i) reports she has never worked in her life (e.g. permanently sick, disabled or homemaker); (ii) reports an implausible retirement age (below 40) or a retirement year before 1961 (since I lack information on eligibility rules); (iii) self-reports as retired but does not state the retirement year; or (iv) does not have any child in reproductive age (20-44) when she becomes eligible for old-age pension. Finally, I focus on a 7-year window around the year in which the old parent becomes eligible and keep only those dynasties that can be observed for each year in this window (so from 3 years before to 3 years after eligibility). After these restrictions, our balanced panel consists of $n = 2{,}040$ dynasties observed for $T = 7$ years, so that the final number of observations is $N = 14{,}280$.[10] This final sample covers the period from 2004 to 2018.

As anticipated in Section 1, I conduct the main analysis by grouping countries in family policy regimes. Applying Gauthier (2002)'s classification to my sample of countries, I can consider three different regimes: the Continental (which includes Austria, Belgium, France, Germany, Netherlands and Switzerland), the Mediterranean (Greece, Italy and Spain) and the Nordic (Denmark and Sweden).[11] This typology of regimes takes into account both the set of public policies supporting

[9] Greece was not covered in waves 4 and 5, while the other 10 countries took part in all waves.
[10] I choose a 7-year window to maximize the final number of observations in the balanced panel.
[11] Switzerland was originally placed in the Anglo-Saxon regime (both in Esping-Andersen (1990) and in Gauthier

families with children, and the social norms about individual roles within the family. The Nordic regime is characterized by universal state support for families with children, large monetary and in-kind benefits for working parents, and a high commitment to gender equality. The Continental regime provides instead a medium level of support for families and shares a more traditional view of the gender division of labor. Finally, in the Mediterranean regime, family policies are highly fragmented and less generous than in the other groups; moreover, these countries have been historically characterized by strong family ties and a high degree of intrafamily solidarity (Reher (1998)).

Table 1: Summary statistics - Characteristics of the old parents

	N	Mean	SD	Min	Max
Age	14,280	64.02	2.81	54	70
Eligible for old-age pension	14,280	0.57	0.49	0	1
Retired	14,280	0.65	0.48	0	1
Female	14,280	0.44	0.50	0	1
Married	14,246	0.73	0.44	0	1
Years of education	11,717	11.79	4.27	0	25
Number of children	14,280	2.38	1.12	1	11
Grandchild birth rate	14,280	0.12	0.33	0	1

Notes: SHARE data, own calculations.

Table A1 presents the distribution of dynasties across countries and family policy regimes, while descriptive statistics on the main characteristics of the old parents are reported in Table 1. By construction of the balanced panel, old parents are eligible in 4 out of 7 years in which they are observed. The share of eligible observations is therefore equal to 57%, while 65% of observations are retired: this difference is mainly due to early-retirement opportunities, not considered in this paper.[12] On average, old parents have 2.4 adult children, while 12% of them report a grandchild birth in the year in which they are observed; as expected, this percentage is higher in the Nordic regime (15%) than in the Continental (11%) and in the Mediterranean (10%). Mean age is 64, with a range that goes from 54 to 70; this corresponds to a range for eligibility age between 57 and 67. Figure A1 shows the cumulative share of eligible old parents by age across family policy regimes. The modal eligibility age in all regimes is by far 65, which is the relevant threshold for 65% of dynasties in Continental countries, 75% in the Mediterranean and up to 97% in the Nordic; 28% of old parents in the Continental regime and 19% in the Mediterranean become eligible at age 60, while all the other ages are much less frequent.

(2002)), but more recent contributions have argued that reforms to the Swiss welfare state approved in the last few decades resulted in a shift towards the Continental regime (see e.g. Obinger, Starke, Moser, Bogedan, Gindulis, and Leibfried (2010)). For this reason, and not to deal with a fourth group including only one country, I consider Switzerland as part of the Continental regime. All results shown in this paper are robust to the exclusion of Switzerland.

[12] I consider retirement as an absorbing state based on the answer to the question "In which year did you retire?". For those respondents who give different answers in different waves, I consider the highest retirement year they state.

3 Empirical Strategy

Following Eibich and Siedler (2020)'s strategy, I estimate the effect of parental retirement on adult children's fertility by means of a fuzzy regression discontinuity design (RDD), comparing dynasties whose old parent is slightly above the eligibility threshold for old-age pension with those whose old parent is slightly below.[13] The empirical model, estimated through a two-stage least squares method, can be written as follows:

First stage:[14]

$$R_{it} \;=\; \alpha \;+\; \beta E_{it} \;+\; \gamma D_{it} \;+\; \delta(E_{it} \times D_{it}) \;+\; \phi_i \;+\; \psi_t \;+\; \epsilon_{it} \tag{1}$$

$$(R_{it} \times D_{it}) \;=\; \tilde{\alpha} \;+\; \tilde{\beta} E_{it} \;+\; \tilde{\gamma} D_{it} \;+\; \tilde{\delta}(E_{it} \times D_{it}) \;+\; \tilde{\phi}_i \;+\; \tilde{\psi}_t \;+\; \tilde{\epsilon}_{it} \tag{2}$$

Second stage:

$$Y_{it+j} \;=\; \xi_j \;+\; \lambda_j \widehat{R_{it}} \;+\; \mu_j D_{it} \;+\; \pi_j (\widehat{R_{it} \times D}_{it}) \;+\; \omega_i \;+\; \tau_{t+j} \;+\; \eta_{it+j} \tag{3}$$

where the outcome Y_{it+j} is a dummy equal to 1 if a grandchild is born in dynasty i in year $t+j$; the treatment R_{it} is a dummy equal to 1 if the old parent of dynasty i is retired in year t; the running variable D_{it} is the distance in years from/to eligibility for old-age pension for the old parent of dynasty i in year t (or equivalently, her age centered around the dynasty-specific eligibility cutoff); and the instrument $E_{it} = \mathbb{1}(D_{it} \geq 0)$ is a dummy equal to 1 if the old parent of dynasty i is eligible for old-age pension in year t. All equations also include dynasty and year fixed effects, a linear polynomial of the running variable, and an interaction term to allow for different trends on the two sides of the cutoff. $\widehat{R_{it}}$ and $(\widehat{R_{it} \times D}_{it})$ are fitted values coming from the first stage equations (1) and (2), respectively. Also notice that the parameters in equation (3) are indexed by j to indicate that we are measuring the outcome variable j years later with respect to the running and treatment variables. In particular, we can expect that an effect on births might emerge one or two years after the eligibility, because adult children need time to implement their fertility decisions in response to parental retirement. For this reason, in what follows I consider $j = 1, 2$.[15]

The main parameters of interest in this setting are β and λ_j: β is the first-stage effect, i.e. the effect of crossing the eligibility cutoff on the probability that the old parent is retired; λ_j is the local average treatment effect (LATE) at the cutoff, i.e. the effect of parental retirement on the

[13] By definition, this RDD can only estimate sharp jumps in the probability of a grandchild birth around the cutoff point. Of course, one might also be interested in detecting smoother changes, for instance by estimating the parameters of a structural model of fertility decisions around parental retirement. However, this goes beyond the scope of this paper.

[14] Because of the interaction terms, in this setting I have two endogenous variables (R_{it} and $(R_{it} \times D_{it})$) and two instruments (E_{it} and $(E_{it} \times D_{it})$), and therefore two first stage equations.

[15] In principle, as long as eligibility age is known in advance, we could also have some anticipation effects (either positive or negative). However, RDD estimates of the effect with $j \in \{-3, -2, -1, 0\}$ are not statistically different from zero in any of the three regimes, meaning that adult children do not seem to respond to parental retirement before it takes place. These results are available upon request.

probability of a grandchild birth (j years later) for the subpopulation of compliers, that is those individuals who retire exactly when they become eligible for old-age pension. In addition, I also estimate the following intention-to-treat (ITT) equation:

$$Y_{it+j} = \zeta_j + \theta_j E_{it} + \rho_j D_{it} + \sigma_j (D_{it} \times E_{it}) + \chi_i + \kappa_{t+j} + \nu_{it+j} \quad (4)$$

where the parameter θ_j is the ITT effect of crossing the eligibility cutoff on the probability of a grandchild birth (j years later).

The identifying assumption for θ_j is the continuity of potential outcomes at the cutoff (Imbens and Lemieux (2008)), i.e. we need that $\mathbb{E}[Y_{it+j}(1) \mid D_{it} = d]$ and $\mathbb{E}[Y_{it+j}(0) \mid D_{it} = d]$ are both continuous functions in d, in particular when $d = 0$.[16] This assumption is not directly testable since it involves potential outcomes, but the standard practice is to look at the continuity of the main pre-treatment covariates at the cutoff. In our balanced panel RDD, dynasties to the left and to the right of the cutoff are exactly the same, with the only difference being that they are observed at a distance of one year. In such a setting, as highlighted by Lee and Lemieux (2010), all characteristics determined before reaching the age threshold (e.g. gender of the old parent) are by construction identical on both sides of the cutoff; similarly, those that mechanically increase over time (e.g. age of the adult children) are clearly continuous at the cutoff.[17]

To identify λ_j, some additional assumptions must hold. Given the similarity between a fuzzy RDD and an instrumental variable (IV) setting, we can apply the Angrist-Imbens-Rubin IV framework (Angrist, Imbens, and Rubin (1996)) and consider the following three assumptions. First, the instrument must be relevant: this requires a non-zero first stage effect, i.e. that the probability of being retired jumps at the eligibility cutoff; as we will see below, this is indeed the case in our data. Second, the instrument must affect the outcome only through the treatment, i.e. being slightly eligible for old-age pension should not affect the probability of a grandchild birth in the near future, except that by increasing the probability of being retired. This assumption would be violated, for instance, if other policies were turned on exactly at the same age as our instrument and such a change could influence adult children's propensity to have a child. However, to the best of my knowledge there are no such relevant policies in the European countries that I am considering. Finally, we must rule out the existence of "defiers", that is individuals who would decide to retire if they were not eligible for old-age pension and to remain instead in the labor market if they were eligible. Even if we can not test this assumption, it is difficult to think of a reason why it should not hold in this setting.

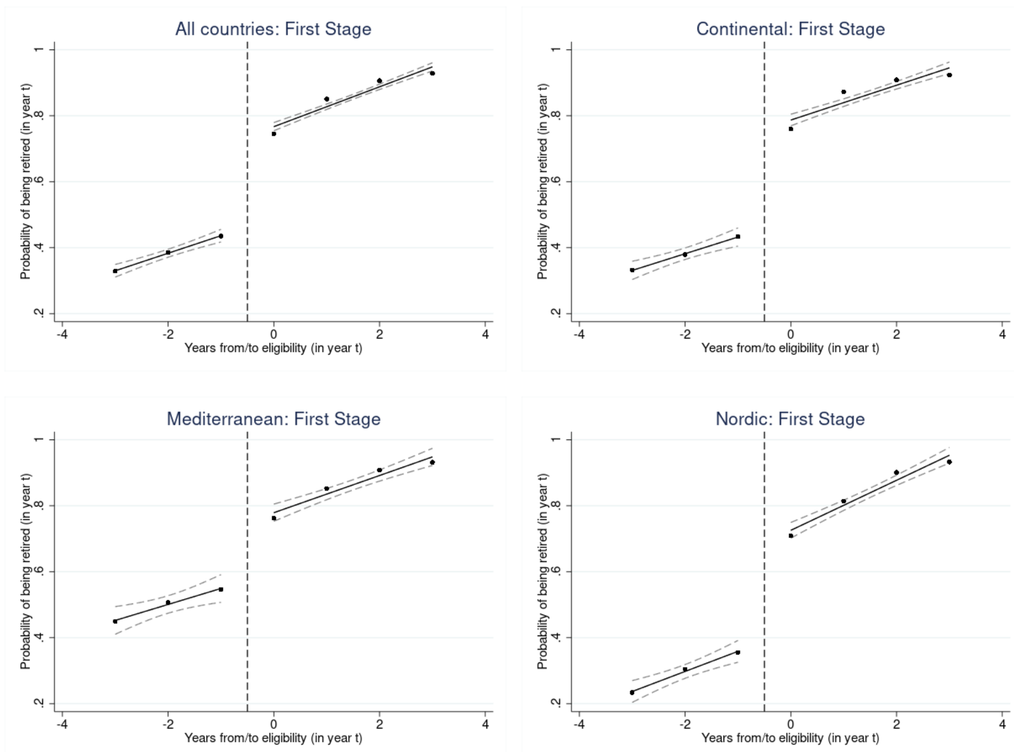
[16] $Y_{it+j}(1)$ is the potential outcome in the case in which the old parent is eligible in year t ($E_{it} = 1$), while $Y_{it+j}(0)$ is the potential outcome in the case in which she is not ($E_{it} = 0$).

[17] Another typical concern in standard RDDs is that of manipulation of the running variable. The common practice is then to plot the density of the running variable and test the null hypothesis of continuity of the density at the cutoff point. In our specific case, this test would be uninformative, because the density of the running variable is uniform by construction (with 2,040 observations at each point). However, Lee and Lemieux (2010) notice that, when following a balanced panel over time and if the assignment to treatment is inevitable, there is no risk of manipulation of the running variable.

4 Results

Figure 1 shows the graphical evidence for the first stage. As expected, in all groups of countries there is a large and positive jump in the probability of being retired when crossing the eligibility threshold, which suggests that the assumption of relevance of the instrument is satisfied. On average, around 40% of old parents are already retired before becoming eligible for old-age pension, probably because of early-retirement opportunities that are not exploited in this paper; however, this percentage almost doubles immediately to the right of the cutoff, in particular in Continental and Nordic countries.

Figure 1: RDD plot - First Stage

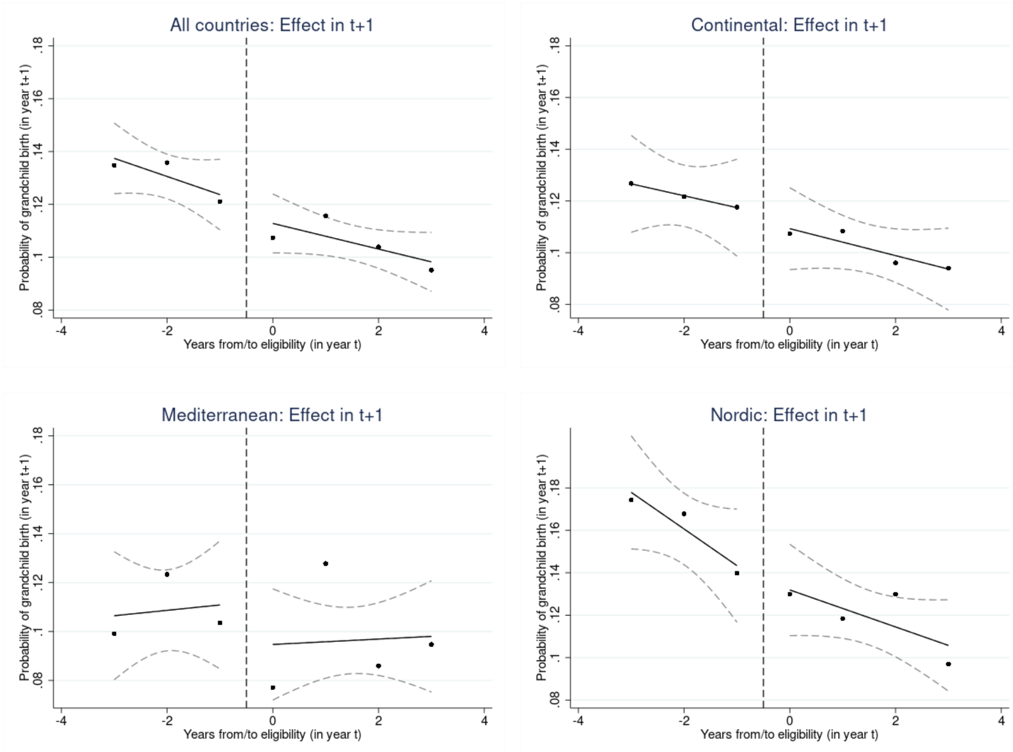


Notes: The dots show the share of dynasties whose old parent is retired. Local polynomial fits (solid black lines) are of degree 1, while confidence intervals (dashed grey lines) are at the 95% level.

Figures 2 and 3 show instead the results for the ITT looking at grandchild births in year $t+1$ and $t+2$, respectively (where t is the year in which the running variable is measured). The effect in $t+1$ seems to be negative (at least in the Continental and Mediterranean regimes) but very small in absolute value. On the other hand, Figure 3 reports a positive jump in the probability of a grandchild birth in $t+2$ in all groups of countries, with the discontinuity being larger in the Mediterranean regime.

Tables 2 and 3 provide the estimates for the main parameters of interest of equations (1), (3) and (4). In this 2SLS estimation, I use a 3-year bandwidth around the cutoff and a uniform kernel and I cluster standard errors at the dynasty level. In Table 2, I consider the case with $j = 1$, i.e. I estimate whether there is an effect of parental retirement on grandchild births occurring the following year.

Figure 2: RDD plot - ITT in $t + 1$

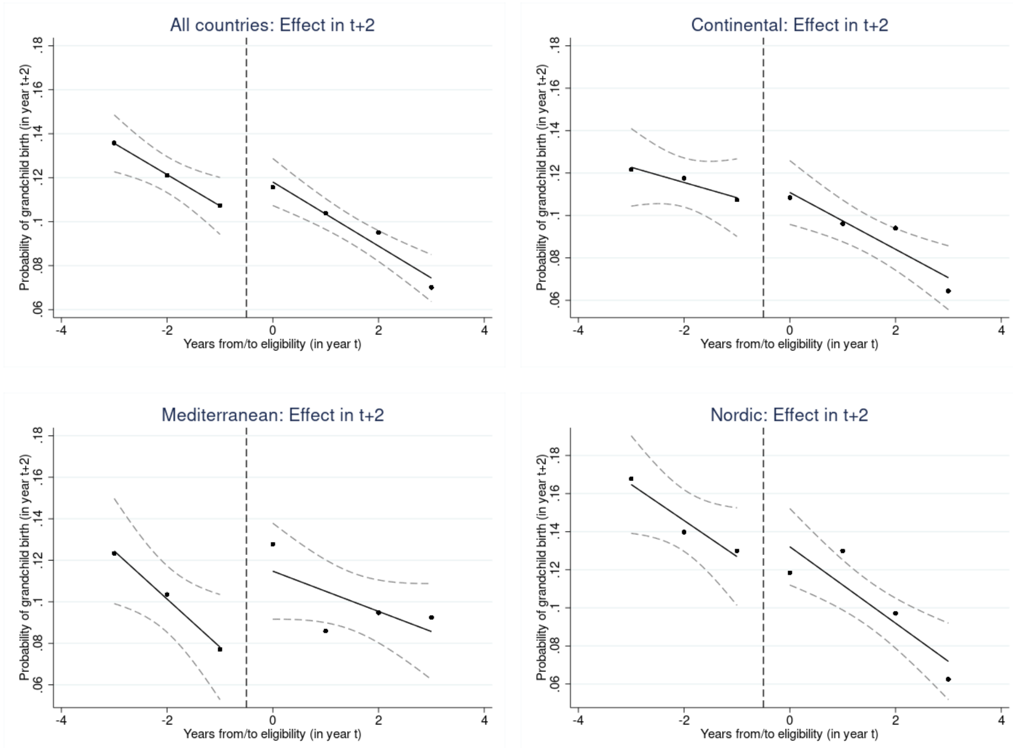


Notes: The dots show the share of dynasties who report a grandchild birth one year later. Local polynomial fits (solid black lines) are of degree 1, while confidence intervals (dashed grey lines) are at the 95% level.

The first row shows the estimates for β, the first-stage effect, which is positive, large and highly significant in all groups of countries: old parents to the right of the cutoff tend to be around 30 percentage points more likely to be retired than those to the left (19 pp in Mediterranean countries). The second row shows the estimates for the LATE at the cutoff, λ_1. This effect is not significant in any of the three regimes and very small in magnitude in Continental and Nordic countries; only in Mediterranean countries it is not negligible in size (-12 pp), but the corresponding p-value is above 0.3. The lack of any effect in $t + 1$ is clearly confirmed in the third row, where we can see that the estimates of the ITT effect, θ_1, are highly non-significant.

In Table 3, I consider instead $j = 2$, i.e. I estimate whether there is an effect of parental retirement on grandchild births occurring two years later. Both second-stage and ITT estimates are now positive in all regimes and significant at the 5% level when considering all countries together and Mediterranean countries alone. This suggests that the result for the entire sample is driven by the Mediterranean regime, since point estimates in the other two groups are much smaller in absolute value and not significant. In particular, I estimate a LATE of 28.8 percentage points for Mediterranean dynasties, meaning that old parents who retire exactly when reaching the eligibility threshold for old-age pension tend to be 28.8 pp more likely to have a grandchild two years later. Looking at the ITT, I find that crossing the eligibility threshold has a 5.6 percentage points effect

Figure 3: RDD plot - ITT in $t+2$



Notes: The dots show the share of dynasties who report a grandchild birth two years later. Local polynomial fits (solid black lines) are of degree 1, while confidence intervals (dashed grey lines) are at the 95% level.

on the probability of a grandchild birth two years later. The magnitude of this effect is quite large if compared with a mean grandchild birth rate of 10% in Mediterranean countries.

Table A2 shows the ITT estimates for each of the 11 countries separately. Despite a loss in power due to smaller sample sizes, we can see that the result for the Mediterranean regime is driven by Spain and Italy, while none of the other countries reports a statistically significant coefficient (with the marginal exception of Austria in $t+2$).

Summing up these findings, evidence from fuzzy RDD regressions suggests that parental retirement significantly increases the probability of a grandchild birth two years later only in Mediterranean countries, while this effect does not seem relevant in the other two regimes. Before testing whether the above results are robust to different specifications, it is worth emphasizing that what we are estimating here is not the effect of parental retirement on the *quantum* of fertility, but only on its *tempo* (Bongaarts and Feeney (1998)). In other words, I can not exclude that part of the estimated effect in Mediterranean countries in $t+2$ is simply a postponement of births from the previous year, for which I estimate indeed a negative (though not statistically significant) coefficient, as shown in Table 2. A naive comparison of the estimated magnitudes in $t+1$ and $t+2$ would suggest that postponement does not explain the whole positive effect in the Mediterranean regime in $t+2$, but even if that was the case, we could still conclude that parental retirement affects the timing of adult children's fertility in those countries, with all the potential implications discussed in Section 1.

Table 2: RDD regressions - Effect in $t+1$

	All countries	Continental	Mediterranean	Nordic
First stage:				
Retired in t (β)	0.277***	0.298***	0.191***	0.307***
	(0.012)	(0.017)	(0.023)	(0.023)
Second stage:				
Grandchild birth in $t+1$ (λ_1)	-0.020	-0.017	-0.120	0.031
	(0.043)	(0.057)	(0.128)	(0.077)
Intention-to-treat:				
Grandchild birth in $t+1$ (θ_1)	-0.005	-0.005	-0.024	0.012
	(0.013)	(0.018)	(0.025)	(0.025)
N. observations	14,280	6,846	3,178	4,256
N. dynasties	2,040	978	454	608

Notes: Standard errors are clustered at the dynasty level and shown in parentheses. All regressions include dynasty and year fixed effects, a linear polynomial of the running variable and an interaction between this polynomial and the dummy for being eligible/retired, and they use a uniform kernel and a 3-year bandwidth on both sides of the cutoff. *** $p<0.01$ ** $p<0.05$ * $p<0.1$

Table 3: RDD regressions - Effect in $t+2$

	All countries	Continental	Mediterranean	Nordic
First stage:				
Retired in t (β)	0.277***	0.298***	0.191***	0.307***
	(0.012)	(0.017)	(0.023)	(0.023)
Second stage:				
Grandchild birth in $t+2$ (λ_2)	0.087**	0.039	0.288**	0.087
	(0.042)	(0.056)	(0.124)	(0.073)
Intention-to-treat:				
Grandchild birth in $t+2$ (θ_2)	0.025**	0.011	0.056**	0.027
	(0.012)	(0.018)	(0.023)	(0.023)
N. observations	14,280	6,846	3,178	4,256
N. dynasties	2,040	978	454	608

Notes: Standard errors are clustered at the dynasty level and shown in parentheses. All regressions include dynasty and year fixed effects, a linear polynomial of the running variable and an interaction between this polynomial and the dummy for being eligible/retired, and they use a uniform kernel and a 3-year bandwidth on both sides of the cutoff. *** $p<0.01$ ** $p<0.05$ * $p<0.1$

4.1 Robustness Checks

As a first test of the robustness of these results, Table A3 shows some checks for the effect in $t+2$ in Mediterranean countries.[18] In particular, I check whether the estimated coefficients for β, λ_2 and θ_2 change when: (i) using a bandwidth of 2 years (instead of 3) around the cutoff; (ii) removing the interaction between the linear polynomial of the running variable and the instrument; (iii) using a quadratic polynomial for the running variable; (iv) using a quadratic polynomial without interaction with the instrument; or (v) removing dynasty and year FE from the regressions. The magnitude of the estimates remain pretty similar across specifications: between 17 and 20 percentage points for the first stage, between 26 and 31 pp for the LATE and between 4 and 7 pp for the ITT. Moreover, results remain significant in all specifications, except for the LATE and ITT when using a quadratic polynomial with the interaction term.

A second test consists in estimating a cross-sectional fuzzy RDD on a different sample. A potential drawback of using a balanced panel of dynasties is that we may restrict the original sample in a non-random way: if dynasties who are observed without gaps for each of the 7 years around the time of eligibility are different from those who are not, then the final sample used so far may not be representative of the population of interest anymore. To check whether the results found above depend on my sample restriction, I consider the original *unbalanced* sample of SHARE respondents, regardless of whether a dynasty is observed in all years around eligibility or not. I use this larger sample as a cross-sectional sample, meaning that I consider each dynasty-year pair on its own without taking into account the longitudinal dimension of the dataset. I then estimate the following first-stage, second-stage and ITT regressions:

First stage:

$$R_i \;=\; \alpha \;+\; \beta E_i \;+\; \gamma D_i \;+\; \delta(E_i \times D_i) \;+\; \epsilon_i \tag{5}$$

$$(R_i \times D_i) \;=\; \tilde{\alpha} \;+\; \tilde{\beta} E_i \;+\; \tilde{\gamma} D_i \;+\; \tilde{\delta}(E_i \times D_i) \;+\; \tilde{\epsilon}_i \tag{6}$$

Second stage:

$$Y_{ij} \;=\; \xi_j \;+\; \lambda_j \widehat{R_i} \;+\; \mu_j D_i \;+\; \pi_j (\widehat{R_i \times D_i}) \;+\; \eta_{ij} \tag{7}$$

Intention-to-treat:

$$Y_{ij} \;=\; \zeta_j \;+\; \theta_j E_i \;+\; \rho_j D_i \;+\; \sigma_j (E_i \times D_i) \;+\; \nu_{ij} \tag{8}$$

where each variable has the same meaning as in equations (1), (2), (3) and (4), but without any time index. To make results comparable to those presented before, I still use a 3-year bandwidth, i.e. $D_i \in [-3, 3]$. The outcome, Y_{ij}, is a dummy equal to 1 if a grandchild is born in dynasty i, j

[18] The same robustness checks for the effect in $t+1$ in all three regimes and for the effect in $t+2$ in Continental and Nordic countries confirm the results shown in Tables 2 and 3: the effect of parental retirement is never statistically significant at conventional levels. These results are available upon request.

years later (with $j = 1, 2$).[19] Results for this cross-sectional RDD are shown in Table A4 (for the effect in $t+1$) and Table A5 (for the effect in $t+2$). First-stage estimates are lower than before in magnitude, but still large and highly significant. Both LATE and ITT in $t+1$ are very similar to their corresponding coefficients in Table 2, very small in absolute value and not significantly different from zero in all groups of countries. Also the pattern found above for $t+2$ is confirmed: the effect is positive and significant only in the Mediterranean regime, even if the magnitude is lower than in Table 3 (16.2 versus 28.8 pp for the LATE, 2.9 versus 5.6 pp for the ITT); the coefficients in the other two regimes are instead once again very small in size and not statistically significant.

A third piece of evidence supporting the robustness of the previous results is discussed in Appendix B. As a methodological contribution, I first prove that, with a balanced panel of observations, parameters from a RDD with age as running variable and from an event study regression identify the same estimand. Then, in Table B1 I demonstrate that event study estimates for our parameters of interest are indeed consistent with those presented in Tables 2 and 3.

Finally, as a falsification exercise, Table A6 shows that parental eligibility for retirement does not affect other outcome variables that we do not expect to vary at the cutoff, such as the number of adult children in the dynasty or the old parent's marital status, education and dominant hand. By the same token, we should not find any effect on fertility decisions when considering two subsamples previously excluded from the analysis, namely those dynasties in which there are no adult children or the old parent has never worked in her life: this is confirmed in Table A7.

4.2 Potential Mechanism: Availability of Informal Childcare

As discussed in Section 1, a potential mechanism behind the positive causal effect of parental retirement found in Mediterranean countries can be an increase in the availability of free informal childcare within the family. When their old parents retire, some adult children know they would sustain lower childcare costs if they now decided to have a child, thanks to the increase in the amount of time transfers they could receive from their parents. But this is not true for everyone. In order for this channel to be relevant, a few "necessary conditions" must hold at the time of retirement. First, the old parent must be in sufficiently good health to be able to take care of grandchildren. Second, the old parent and the adult child must not live too far away from each other. Third, the number of grandchildren already born in the dynasty must not be so high as to limit the time that the old parent could devote to take care of a new grandchild. For these reasons, if the availability of informal childcare is the main mechanism through which parental retirement affects fertility decisions in the Mediterranean regime, then we should find that the positive effect shown above is stronger for dynasties that satisfy at least some of the conditions we have just seen.

This is what I check in Table 4, where I look at the heterogeneity of the ITT effect in Mediterranean countries two years after eligibility according to three individual characteristics of the dynasty: (i) the health of the old parent, as proxied by grip strength, which is an objective measure

[19] Even if the regressions are now free of time indices, in what follows I will still refer to t as the year in which a dynasty is observed, and so I will call "effect in $t+1$" the effect on grandchild births occurring one year after the observation (and similarly for the "effect in $t+2$").

considered a good biomarker of current and future medical status;[20] (ii) the presence of at least one adult child living closer than 1 km to the old parent; and (iii) the number of grandchildren already born. Results provide evidence in favor of a positive time effect that passes through an increase in the availability of informal childcare after the old parent's eligibility for retirement. As for health status, I find that the ITT effect is significant only for dynasties in which the old parent has a grip strength (and therefore health conditions) above the gender-specific median, and the associated magnitude is stronger than the baseline effect (7.6 against 5.6 percentage points). Similarly, only those dynasties in which at least one adult child lives close to her old parent report a highly significant and large (7.3 pp) positive effect, while the coefficient is not statistically different from zero for the rest of the sample. In terms of number of grandchildren already born at the time of eligibility, the estimates point to a positive effect only at the extensive or first intensive margin (7.0 pp); if instead there are already two or more grandchildren in the dynasty, we can argue that the increase in time availability for the old parent is not perceived as a sufficient reduction in expected childcare costs by adult couples considering the hypothesis of having a child.[21]

In addition to a higher value attached to time transfers, other factors might in principle explain why we find a positive effect only in Southern Europe. One can argue that Gauthier (2002)'s classification of countries into family policy regimes at least partially overlaps with alternative classifications that in turn might matter for the heterogeneity of fertility responses to parental retirement. For instance, countries belonging to our three regimes also differ in terms of female labor force participation, generosity of the pension system, or mean age at childbearing. However, based on the available data, these characteristics are unlikely to play a decisive role in driving our main finding. First, the female labor force participation rate is way lower in the Mediterranean regime (59%) than in the Continental (70%) and the Nordic (77%),[22] where adult women have therefore relatively less time to devote to childcare: if anything, this difference implies that finding a positive effect of time transfers related to parental retirement should be easier in Central and Northern Europe, which is not what we see in this paper. Second, the average replacement rate between pension income and labor income is higher in Mediterranean countries (58% versus 51% in the other two regimes),[23] so that the positive effect found only in Southern Europe might depend on a lower reduction in intergenerational monetary transfers in this area. While I can not directly test this hypothesis due to data limitations, I can follow Stella (2017) and compare dynasties living in Italy, where there is a large bonus payment for employees at the time of retirement, with those living in Spain, where instead such a large severance payment does not exist. Point estimates shown in Table A2 are remarkably

[20]Based on a review of existing medical evidence, Bohannon (2019) suggests to use grip strength as a proxy measurement for identifying older adults at risk of poor health status. Grip strength is indeed "largely consistent as an explanator of concurrent overall strength, upper limb function, bone mineral density, fractures, falls, malnutrition, cognitive impairment, depression, sleep problems, diabetes, multimorbidity, and quality of life", and it has good predictive power for "all-cause and disease-specific mortality, future function, bone mineral density, fractures, cognition and depression, and problems associated with hospitalization".

[21]Table A8 in the Appendix looks at the same sources of heterogeneity for dynasties living in Continental and Nordic countries. For these regimes, the availability of informal childcare does not seem to play a relevant role in affecting fertility decisions.

[22]Source: Eurostat, average female labor force participation rate for the period 2004-2020.

[23]Source: Eurostat, average aggregate replacement ratio (excluding other social benefits) for the period 2004-2020.

similar in Italy (8.9 pp) and Spain (9.2 pp): this is not what we would expect if the underlying mechanism had to do with monetary transfers. Finally, given that the mean age at childbirth is lower in the Continental and Nordic regimes, it is possible that the lack of significant effects in those countries is simply due to the fact that adult children are too old when their parents become eligible for retirement, while children in Mediterranean countries are still in the peak of their fertile age. This can be a threat in my sample, where the mean age of adult children at the time of parental eligibility is 33.7 in the Mediterranean regime, 34.7 in the Continental and 36.8 in the Nordic. In order to net out the heterogeneity due to this difference, in Table A9 I show the ITT estimates in the three regimes when restricting the sample to dynasties with children of similar age.[24] Once again, Mediterranean countries report larger magnitudes and levels of significance, highlighting that our main finding is not driven by differences in the age structure of the dynasties.

Summing up, the heterogeneity according to individual characteristics of Mediterranean dynasties suggests that the positive causal effect found in this regime is likely to depend on the increase in availability of informal childcare within the family and on the subsequent reduction in expected childcare costs, while other mechanisms do not find consistent support in the data.

Table 4: Heterogeneity of the ITT effect in $t+2$ in Mediterranean countries

	Baseline	Grip strength		One child closer 1km		N. of grandch.	
		$\geq$ median	$<$ median	Yes	No	0/1	2+
Intention-to-treat:							
Grandchild birth in $t+2$ (θ_2)	0.056**	0.076***	0.016	0.073***	-0.013	0.070***	0.025
	(0.023)	(0.028)	(0.039)	(0.026)	(0.050)	(0.027)	(0.044)
N. observations	3,178	1,673	1,470	2,506	672	2,163	1,015
N. dynasties	454	239	210	358	96	309	145

Notes: Standard errors are clustered at the dynasty level and shown in parentheses. All regressions include dynasty and year fixed effects, a linear polynomial of the running variable and an interaction between this polynomial and the dummy for being eligible, and they use a uniform kernel and a 3-year bandwidth on both sides of the cutoff. Grip strength, the presence of an adult child living closer than 1 km and the number of grandchildren already born are all measured in the year in which the old parent becomes eligible.*** $p<0.01$ ** $p<0.05$ * $p<0.1$

5 Conclusion

In many European countries, retirement age has been raised in the last three decades to limit the pressure of population ageing on public spending. However, as argued in the introduction, these pension reforms may have unintended consequences on fertility rates, thus reinforcing population

[24] In particular, with mean age between 32 and 35 in Panel A and between 36 and 39 in Panel B. These two age groups correspond to the second and third quartile of the distribution of adult children's mean age. In the group 32-35, mean age is 33.2 in the Mediterranean regime, 33.3 in the Continental and 33.6 in the Nordic; in the group 36-39, mean age is 37.4, 37.7 and 37.6 respectively.

ageing itself. In particular, this is the case if the timing of retirement influences the timing of the offspring generation's fertility, so that an increase in retirement age may lead to a postponement of births with negative effects on completed fertility.

In this paper, I consider a balanced panel of European dynasties and find that parental retirement does affect the timing of adult couples' fertility decisions, but only in Mediterranean countries. Here, RDD estimates suggest that the probability of a grandchild birth significantly jumps two years after the time in which the old parent becomes eligible for old-age pension. The magnitude of the ITT effect is quite large, around 6 percentage points with respect to a mean grandchild birth rate of 10% in this group of countries. For the other two family policy regimes that I consider, the Continental and the Nordic, all effects are instead much smaller in size and not statistically different from zero. In addition, when looking at the heterogeneity of the effect by individual characteristics within the Mediterranean regime, I find that an increase in the availability of informal childcare is the most likely mechanism underlying the result: the positive effect is indeed driven by those dynasties in which the old parent is potentially more available for taking care of new grandchildren (i.e. she is in good health, she lives close to at least one of her adult children and she has at most one grandchild), meaning that the intensity of downward time transfers is potentially higher.

With respect to the existing literature, this evidence is in line with both Battistin, De Nadai, and Padula (2014) and Eibich and Siedler (2020), who find a positive causal effect of parental retirement on adult children's fertility choices.[25] At least in the Mediterranean regime, I can argue that, when old parents retire, the associated positive time effect seems to prevail over the negative income effect, as opposed to the conclusions of Aparicio-Fenoll and Vidal-Fernandez (2015). Moreover, the heterogeneity analysis in Mediterranean countries confirms that health, geographical proximity and family composition seem to be relevant factors for intergenerational help, as discussed e.g. in Aassve, Meroni, and Pronzato (2012).

In conclusion, this paper shows that the effect of parental retirement on fertility decisions varies across family policy regimes in Europe. This result is consistent with the hypothesis that parental support matters more in countries with less generous family policies and stronger family ties, as it is the case in the Mediterranean regime, and hints that increases in retirement age might reduce fertility rates in Southern Europe through an effect on the timing of fertility. However, in order to derive the correct policy implications, it would be important to disentangle between family policies and family norms. If what determines the positive effect in Mediterranean countries was the weakness of public policies supporting families, then a good countermeasure to limit the side effects of pension reforms on fertility would be, for instance, to increase the supply of formal childcare. But if instead what matters is the strength of family ties, then formal childcare would not be perceived as an adequate substitute for informal childcare, and such a countermeasure would be much less effective in sustaining fertility. Future research is needed to better address this issue.

[25] Notice that, while Eibich and Siedler (2020) find a positive and significant effect for Germany, I do not. This difference may depend on the choice of eligibility age: while they consider early retirement, I only look at eligibility for old-age pension.

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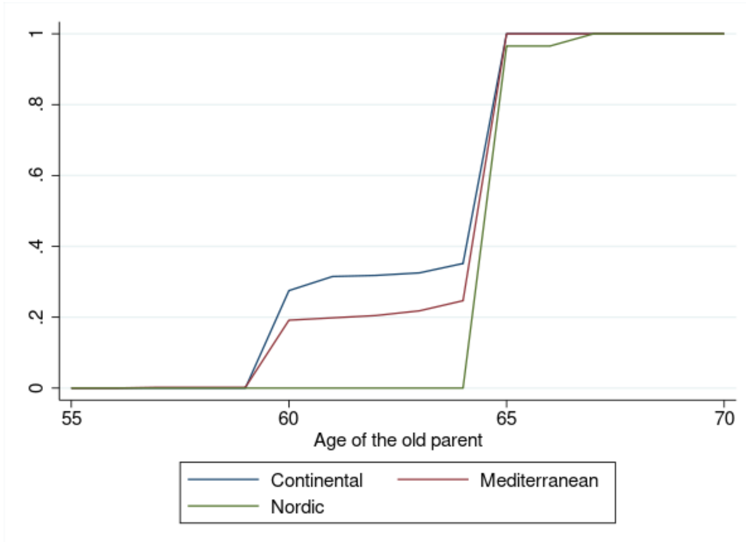
A Additional Figures and Tables

Figure A1: Cumulative share of eligible old parents by age



Notes: SHARE data, own calculations.

Table A1: Distribution of dynasties across countries

	n		n
Austria	142	Greece	162
Belgium	175	Italy	123
France	236	Spain	169
Germany	134	*Mediterranean regime*	*454*
Netherlands	191	Denmark	264
Switzerland	100	Sweden	344
Continental regime	*978*	*Nordic regime*	*608*

Notes: SHARE data, own calculations.

Table A2: RDD regressions by country

ITT effect in...	$t+1$	$t+2$
Austria	0.030	0.073*
$[N = 994]$	(0.038)	(0.043)
Belgium	-0.048	-0.017
$[N = 1,225]$	(0.040)	(0.040)
France	0.051	-0.019
$[N = 1,652]$	(0.041)	(0.045)
Germany	-0.055	0.045
$[N = 938]$	(0.048)	(0.038)
Netherlands	-0.036	-0.031
$[N = 1,337]$	(0.043)	(0.041)
Switzerland	0.045	0.026
$[N = 700]$	(0.050)	(0.057)
Greece	-0.010	-0.006
$[N = 1,134]$	(0.026)	(0.031)
Italy	-0.031	0.089*
$[N = 861]$	(0.048)	(0.050)
Spain	-0.026	0.092**
$[N = 1,183]$	(0.053)	(0.041)
Denmark	0.047	0.036
$[N = 1,848]$	(0.036)	(0.035)
Sweden	-0.018	0.022
$[N = 2,408]$	(0.034)	(0.031)

Notes: Standard errors are clustered at the dynasty level and shown in parentheses. All regressions include dynasty and year fixed effects, a linear polynomial of the running variable and an interaction between this polynomial and the dummy for being eligible, and they use a uniform kernel and a 3-year bandwidth on both sides of the cutoff. *** p<0.01 ** p<0.05 * p<0.1

Table A3: RDD robustness checks - Effect in $t+2$ in Mediterranean countries

	2-year bandwidth	Linear polynomial, no inter.	Quadratic polynomial	Quadratic polynomial, no inter.	No FE
First stage:					
Retired in t (β)	0.189***	0.165***	0.196***	0.177***	0.181***
	(0.023)	(0.022)	(0.030)	(0.022)	(0.022)
Second stage:					
Grandchild birth in $t+2$ (λ_2)	0.295*	0.261**	0.290	0.303**	0.315**
	(0.175)	(0.130)	(0.292)	(0.128)	(0.123)
Intention-to-treat:					
Grandchild birth in $t+2$ (θ_2)	0.055*	0.043**	0.071	0.053**	0.060***
	(0.032)	(0.021)	(0.060)	(0.022)	(0.022)
Dynasty and year FE	yes	yes	yes	yes	no
N. observations	2,270	3,178	3,178	3,178	3,178
N. dynasties	454	454	454	454	454

Notes: Standard errors are clustered at the dynasty level and shown in parentheses. All regressions use a uniform kernel. *** p<0.01 ** p<0.05 * p<0.1

Table A4: Cross-sectional RDD regressions - Effect in $t+1$

	All countries	Continental	Mediterranean	Nordic
First stage:				
Retired in t (β)	0.227***	0.227***	0.153***	0.287***
	(0.008)	(0.011)	(0.016)	(0.015)
Second stage:				
Grandchild birth in $t+1$ (λ_1)	-0.014	-0.035	-0.112	0.059
	(0.028)	(0.037)	(0.091)	(0.048)
Intention-to-treat:				
Grandchild birth in $t+1$ (θ_1)	-0.003	-0.008	-0.017	0.018
	(0.007)	(0.010)	(0.015)	(0.015)
N. observations	35,969	19,226	7,080	9,663

Notes: Standard errors are clustered at the dynasty level and shown in parentheses. All regressions include a linear polynomial of the running variable and an interaction between this polynomial and the dummy for being eligible/retired, and they use a uniform kernel and a 3-year bandwidth on both sides of the cutoff. *** p<0.01 ** p<0.05 * p<0.1

Table A5: Cross-sectional RDD regressions - Effect in $t+2$

	All countries	Continental	Mediterranean	Nordic
First stage:				
Retired in t (β)	0.227***	0.227***	0.153***	0.287***
	(0.008)	(0.011)	(0.016)	(0.015)
Second stage:				
Grandchild birth in $t+1$ (λ_2)	0.054**	0.042	0.162*	0.029
	(0.027)	(0.036)	(0.084)	(0.046)
Intention-to-treat:				
Grandchild birth in $t+1$ (θ_2)	0.014**	0.011	0.029**	0.007
	(0.007)	(0.009)	(0.013)	(0.014)
N. observations	35,969	19,226	7,080	9,663

Notes: Standard errors are clustered at the dynasty level and shown in parentheses. All regressions include a linear polynomial of the running variable and an interaction between this polynomial and the dummy for being eligible/retired, and they use a uniform kernel and a 3-year bandwidth on both sides of the cutoff. *** $p<0.01$ ** $p<0.05$ * $p<0.1$

Table A6: ITT effect on placebo outcomes

	All countries	Continental	Mediterranean	Nordic
N. of adult children	0.009	0.015	-0.001	0.008
	(0.008)	(0.011)	(0.007)	(0.018)
	[14,280]	[6,846]	[3,178]	[4,256]
Married	0.001	-0.000	-0.001	0.004
	(0.002)	(0.003)	(0.002)	(0.005)
	[14,280]	[6,846]	[3,178]	[4,256]
Years of education	0.003	0.006	-0.001	0.000
	(0.002)	(0.004)	(0.001)	(0.000)
	[11,717]	[5,712]	[2,429]	[3,576]
Right-handed	0.000	-0.001	-0.009	0.008
	(0.004)	(0.006)	(0.008)	(0.005)
	[14,280]	[6,846]	[3,178]	[4,256]

Notes: Standard errors are clustered at the dynasty level and shown in parentheses. The number of observations is shown in brackets. All regressions include dynasty and year fixed effects, a linear polynomial of the running variable and an interaction between this polynomial and the dummy for being eligible, and they use a uniform kernel and a 3-year bandwidth on both sides of the cutoff. *** p<0.01 ** p<0.05 * p<0.1

Table A7: ITT effect in $t+2$ for placebo subsamples

	All countries	Continental	Mediterranean	Nordic
Zero adult children	-0.000	0.004	-0.001	-0.012
	(0.009)	(0.016)	(0.008)	(0.012)
	[2,000]	[1,073]	[541]	[386]
Never worked	0.020	0.030	0.016	n.a.
	(0.050)	(0.091)	(0.059)	
	[711]	[228]	[476]	

Notes: Standard errors are clustered at the dynasty level and shown in parentheses. The number of observations is shown in brackets. All regressions include dynasty and year fixed effects, a linear polynomial of the running variable and an interaction between this polynomial and the dummy for being eligible, and they use a uniform kernel and a 3-year bandwidth on both sides of the cutoff. In the Nordic regime, only 1 old parent has never worked in her life. *** p<0.01 ** p<0.05 * p<0.1

Table A8: Heterogeneity of the ITT effect in $t + 2$ in Continental and Nordic countries

A. CONTINENTAL	Baseline	Grip strength		One child closer 1km		N. of grandch.	
		$\geq$ median	$<$ median	Yes	No	0/1	2+
Intention-to-treat:							
Grandchild birth in $t + 2$ (θ_2)	0.011	0.015	0.009	0.018	0.007	0.039	-0.013
	(0.018)	(0.026)	(0.024)	(0.029)	(0.022)	(0.025)	(0.024)
N. observations	6,846	3,640	3,136	2,800	4,046	3,346	3,500
N. dynasties	978	520	448	400	578	478	500
B. NORDIC	Baseline	Grip strength		One child closer 1km		N. of grandch.	
		$\geq$ median	$<$ median	Yes	No	0/1	2+
Intention-to-treat:							
Grandchild birth in $t + 2$ (θ_2)	0.027	0.059*	-0.014	0.045	0.022	0.032	0.027
	(0.023)	(0.032)	(0.035)	(0.050)	(0.026)	(0.042)	(0.028)
N. observations	4,256	2,261	1,953	889	3,367	1,372	2,884
N. dynasties	608	323	279	127	481	196	412

Notes: Standard errors are clustered at the dynasty level and shown in parentheses. All regressions include dynasty and year fixed effects, a linear polynomial of the running variable and an interaction between this polynomial and the dummy for being eligible, and they use a uniform kernel and a 3-year bandwidth on both sides of the cutoff. Grip strength, the presence of an adult child living closer than 1 km and the number of grandchildren already born are all measured in the year in which the old parent becomes eligible. *** $p<0.01$ ** $p<0.05$ * $p<0.1$

Table A9: ITT effect in $t+2$ by age groups

	All countries	Continental	Mediterranean	Nordic
A. Mean age 32-35				
Intention-to-treat:				
Grandchild birth in $t+2$ (θ_2)	0.018	0.004	0.050	0.040
	(0.027)	(0.039)	(0.043)	(0.064)
N. observations	3,738	1,701	1,169	868
N. dynasties	534	243	167	124
B. Mean age 36-39				
Intention-to-treat:				
Grandchild birth in $t+2$ (θ_2)	0.030	-0.006	0.106**	0.034
	(0.022)	(0.029)	(0.049)	(0.041)
N. observations	4,382	2,044	826	1,512
N. dynasties	626	292	118	216

Notes: Standard errors are clustered at the dynasty level and shown in parentheses. All regressions include dynasty and year fixed effects, a linear polynomial of the running variable and an interaction between this polynomial and the dummy for being eligible/retired, and they use a uniform kernel and a 3-year bandwidth on both sides of the cutoff. The sample includes only dynasties in which the mean age of adult children at the time of parental eligibility is between 32 and 35 in Panel A and between 36 and 39 in Panel B. *** $p<0.01$ ** $p<0.05$ * $p<0.1$

B Methodological Appendix

The purpose of this section is to show that, with a balanced panel of observations, parameters from a regression discontinuity design (RDD) with age as running variable and from an event study (ES) identify the same estimand.

Let us start considering the ITT setting for the RDD shown in Section 3, with the following variables defined for a balanced panel of dynasties:

1. Running variable: $D_{it} \rightarrow$ Distance (in years) from/to eligibility for old-age pension for the old parent of dynasty i in year t (or equivalently, the age of the old parent centered at the cutoff); a positive value means that the old parent is older than the eligibility age threshold.

2. Treatment variable: $E_{it} \rightarrow$ Dummy equal to 1 if the old parent of dynasty i is eligible for old-age pension in year t (i.e. if $D_{it} \geq 0$).

3. Outcome variable: $Y_{it+j} \rightarrow$ Dummy equal to 1 if a grandchild is born in dynasty i in year $t+j$.

Let us define as $Y_{it+j}(1)$ the potential outcome in case of treatment (i.e. if the old parent is eligible in year t) and as $Y_{it+j}(0)$ the potential outcome in case of no treatment (i.e. if the old parent is not eligible). Then the usual RDD estimand is the average treatment effect at the cutoff, that is:

$$\mathbf{E}\left[Y_{it+j}(1) - Y_{it+j}(0) \mid D_{it} = 0\right]$$

Let us consider the following functional form for the observed outcome (the same as in equation (4)):

$$Y_{it+j} \;=\; \zeta_j \;+\; \theta_j E_{it} \;+\; \rho_j D_{it} \;+\; \sigma_j (E_{it} \times D_{it}) \;+\; \chi_i \;+\; \kappa_{t+j} \;+\; \nu_{it+j}$$

where the parameters are indexed by j to indicate that we are interested in measuring the outcome variable j years after the time in which the running and the treatment variables are measured. Then we can show that, as usual:

$$\begin{aligned}
&\mathbf{E}\left[Y_{it+j}(1) - Y_{it+j}(0) \mid D_{it} = 0\right] \\
&\qquad = \; \mathbf{E}\left[Y_{it+j}(1) \mid D_{it} = 0\right] \;-\; \mathbf{E}\left[Y_{it+j}(0) \mid D_{it} = 0\right] \\
&\qquad = \; \lim_{d \to 0^+} \; \mathbf{E}\left[Y_{it+j}(1) \mid D_{it} = d\right] \;-\; \lim_{d \to 0^-} \; \mathbf{E}\left[Y_{it+j}(0) \mid D_{it} = d\right] \\
&\qquad = \; \lim_{d \to 0^+} \; \mathbf{E}\left[Y_{it+j} \mid D_{it} = d\right] \;-\; \lim_{d \to 0^-} \; \mathbf{E}\left[Y_{it+j} \mid D_{it} = d\right] \\
&\qquad = \; \zeta_j \;+\; \theta_j \;+\; \chi_i \;+\; \kappa_{t+j} \;-\; \zeta_j \;-\; \chi_i \;-\; \kappa_{t+j} \\
&\qquad = \; \theta_j
\end{aligned}$$

where the second equality comes from the assumption of continuity of potential outcomes. This derivation gives us the first result that we need:

$$\theta_j \;=\; \lim_{d \to 0^+} \; \mathbf{E}\left[Y_{it+j} \mid D_{it} = d\right] \;-\; \lim_{d \to 0^-} \; \mathbf{E}\left[Y_{it+j} \mid D_{it} = d\right]$$

We can also notice that the following two results hold in our setting. First, given that in a balanced panel $D_{it} = d \iff D_{it+j} = d + j$, it is also true that:

$$\begin{aligned} & \lim_{d \to 0^+} \; \mathbf{E}\left[Y_{it+j} \mid D_{it} = d\right] \;-\; \lim_{d \to 0^-} \; \mathbf{E}\left[Y_{it+j} \mid D_{it} = d\right] \\ = & \lim_{d \to 0^+} \; \mathbf{E}\left[Y_{it+j} \mid D_{it+j} = d + j\right] \;-\; \lim_{d \to 0^-} \; \mathbf{E}\left[Y_{it+j} \mid D_{it+j} = d + j\right] \end{aligned}$$

Second, given the functional form we specified for the observed outcome, it holds that:

$$\begin{aligned} & \lim_{d \to 0^+} \; \mathbf{E}\left[Y_{it+j} \mid D_{it} = d\right] \;-\; \lim_{d \to 0^-} \; \mathbf{E}\left[Y_{it+j} \mid D_{it} = d\right] \\ = & \lim_{d \to 0^+} \; \mathbf{E}\left[Y_{it} \mid D_{it-j} = d\right] \;-\; \lim_{d \to 0^-} \; \mathbf{E}\left[Y_{it} \mid D_{it-j} = d\right] \end{aligned}$$

Therefore, combining the three results together we get that:

$$\begin{aligned} \theta_j \;&=\; \lim_{d \to 0^+} \; \mathbf{E}\left[Y_{it+j} \mid D_{it} = d\right] \;-\; \lim_{d \to 0^-} \; \mathbf{E}\left[Y_{it+j} \mid D_{it} = d\right] \\ &=\; \lim_{d \to 0^+} \; \mathbf{E}\left[Y_{it+j} \mid D_{it+j} = d + j\right] \;-\; \lim_{d \to 0^-} \; \mathbf{E}\left[Y_{it+j} \mid D_{it+j} = d + j\right] \\ &=\; \lim_{d \to 0^+} \; \mathbf{E}\left[Y_{it} \mid D_{it} = d + j\right] \;-\; \lim_{d \to 0^-} \; \mathbf{E}\left[Y_{it} \mid D_{it} = d + j\right] \end{aligned}$$

which in discrete time (e.g. when the running variable is measured in years, as it is the case here) approximates to:

$$\theta_j \;=\; \mathbf{E}\left[Y_{it} \mid D_{it} = j\right] \;-\; \mathbf{E}\left[Y_{it} \mid D_{it} = j - 1\right] \tag{9}$$

Now consider an ES dynamic two-way fixed-effect specification like the following:

$$Y_{it} \;=\; \alpha \;+\; \sum_{d=\underline{d}}^{\bar{d}} \beta_d \; \mathbb{1}(D_{it} = d) \;+\; \gamma_i \;+\; \delta_t \;+\; \epsilon_{it} \tag{10}$$

where Y_{it} is a dummy equal to 1 if a grandchild is born in dynasty i in year t and $\mathbb{1}(D_{it} = d)$ is a dummy equal to 1 if the old parent of dynasty i is d years above (if $d > 0$) or below (if $d < 0$) the eligibility age threshold in year t; $\underline{d}$ and $\bar{d}$ are the number of leads and lags, respectively, included in the regression, while γ_i and δ_t are dynasty and year fixed effects. It is then immediate to show that:

$$\mathbf{E}\left[Y_{it} \mid D_{it} = j\right] \;-\; \mathbf{E}\left[Y_{it} \mid D_{it} = j - 1\right] \;=\; \beta_j - \beta_{j-1} \tag{11}$$

and by combining equations (9) and (11) we can conclude that:

$$\theta_j \; = \; \beta_j - \beta_{j-1} \tag{12}$$

When considering a balanced panel of observations, the difference between two "consecutive" coefficients of an ES regression ($\beta_j - \beta_{j-1}$) then identifies the same estimand as the main coefficient of a RDD regression (θ_j) with age centered at the cutoff as running variable. Both parameters capture whether there is any jump in the outcome variable when crossing the age threshold (i.e. after experiencing the event).

As an empirical test for the validity of this result, in Table B1 I show the estimates of $\beta_1 - \beta_0$ and $\beta_2 - \beta_1$ from equation (10) when using the same balanced sample as in the main analysis of this paper. As we can see, results are remarkably similar to the ones discussed in Section 4. From the first row, there is no jump in the probability of a grandchild birth the year after the event: $\beta_1 - \beta_0$ is negative, very small in absolute value (between 0 and 2 pp) and not statistically different from zero in any of the three regimes, as it was the case for θ_1. Point estimates for $\beta_2 - \beta_1$ in the second row are again small and not statistically significant when looking at all countries together and at Continental or Nordic regimes; only if we consider the Mediterranean regime alone, we find a positive and significant jump in the probability of a grandchild birth two years after the old parent's eligibility. The magnitude of this ITT effect is around 4.5 percentage points, which is consistent with the estimate of θ_2 shown in Table 3 (5.6 pp).

Table B1: Event Study regressions - Effect 1 and 2 years after the event

Family Policy Regime:	All countries	Continental	Mediterranean	Nordic
Effect 1 year after event ($\beta_1 - \beta_0$)	-0.011	-0.007	-0.023	-0.004
	(0.010)	(0.015)	(0.022)	(0.020)
Effect 2 years after event ($\beta_2 - \beta_1$)	0.010	0.005	0.045*	-0.006
	(0.011)	(0.015)	(0.024)	(0.021)
Dynasty and year FE	yes	yes	yes	yes
N. observations	14,280	6,846	3,178	4,256
N. dynasties	2,040	978	454	608

Notes: Standard errors are clustered at the dynasty level and shown in parentheses.
*** p<0.01 ** p<0.05 * p<0.1

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