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Fertility and Parental Retirement

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Fertility and Parental Retirement

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Abstract

I study how retirement delays in one generation affect fertility in the subsequent generation. I use administrative Dutch data and exploit the 2006 Dutch pension reform. The reform induced individuals born from January 1, 1950 onward to delay retirement while exempting those born earlier. I find that this reduced fertility among women with reform-affected mothers. The reduction is likely permanent and economically significant. I supplement my analysis with survey evidence and argue that the fertility reduction is driven by reduced grandparental child care supply. My results suggest that delaying retirement may undermine the goal of balancing pension systems through a resulting fertility reduction.

1 Introduction

Child care access is central to female labor force participation and fertility. The existing literature has paid considerable attention to formal child care access (Gelbach, 2002; Berlinski & Galiani, 2007; Lefebvre & Merrigan, 2008; Lundin et al., 2008; Havnes & Mogstad, 2011; Fitzpatrick, 2012; Mörk et al., 2013). However, many households rely on grandparental child care. 44% and 39% of grandparents in the EU and the US provide child care respectively (Glaser et al., 2013; Livingston & Parker, 2010). Families may consider the availability of grandparental child care when making fertility and work decisions. This availability likely depends on grandparents' labor market status. I study if retirement delays in one generation decrease fertility and female labor force participation in the subsequent generation.

I exploit the 2006 Dutch pension reform which created variation in people's retirement opportunities.[1] The reform abolished the preferential tax treatment of sectoral pension contributions for individuals born from January 1, 1950 onward. It preserved the favorable tax regime for those born earlier. As a result, individuals born from January 1, 1950 onward had to retire much later to reach the same pension benefit level as those born earlier.

I use Dutch administrative data to construct women's life-cycle fertility and employment profiles. My main sample consists of women who were cohabiting with male partners before the reform and who had at least one parent(-in-law) born around January 1, 1950. I apply a discontinuity design in women's parents' birth dates. I estimate the effect of adverse retirement conditions in one generation on fertility and labor market outcomes in the subsequent generation. In addition, I use the Generations and Gender Survey (GGS) data on grandparental child care and income transfers to get descriptive insights into the mechanisms behind my results.

I first show that the reform delayed retirement among directly affected individuals. Then, I show that it reduced fertility among women with reform-affected mothers. The reduction is likely permanent because it is driven by older women who are at the end of their fertile period. I argue that the effect likely runs through reduced grandparental child care access. I find support for this mechanism in the GGS. I find no effects on women's labor market outcomes. I argue that potential labor market effects are mitigated by formal child care subsidies. These subsidies do not eliminate the effect on fertility because they only cover the hours that the parents spend working.

My findings are important because they help understand the determinants of stagnating female labor force participation and falling fertility. In addition, my results suggest that retirement delays may be a less effective tool for balancing pension systems than was thought previously. Delaying retirement may undermine pension system sustainability in

[1] De Grip et al. (2012) study how this reform affected individuals' mental health.

the long run via a decrease in the size of the labor force.

My work is linked to the few papers that connect parental retirement to fertility and labor force participation in the subsequent generation.[2] These papers reach conflicting results. Eibich & Siedler (2020) exploit a discontinuity in the probability of retirement generated by an early retirement age threshold in Germany. They compare women whose parents are just about to become eligible for retirement to women whose parents just became eligible. They find that women are less likely to have children before their parents reach the threshold and are more likely to have children once their parents cross the threshold. Battistin et al. (2014), Aparicio-Fenoll & Vidal-Fernandez (2015), and Bratti et al. (2018) all exploit a series of Italian pension reforms set forth in years 1992 through 1997. These reforms gradually increased the retirement age for each subsequent cohort. To identify the effect of parental retirement opportunities the authors compare fertility patterns among women whose parents belong to earlier cohorts to those among women whose parents belong to later cohorts. Battistin et al. (2014) find that delaying parents' eligibility reduced their daughters' fertility. Bratti et al. (2018) find that delaying eligibility reduced daughters' labor force participation. Meanwhile, Aparicio-Fenoll & Vidal-Fernandez (2015) find that delaying parents' eligibility increased fertility and reduced labor force participation among their daughters. The conflicting findings on fertility are surprising because Battistin et al. (2014) and Aparicio-Fenoll & Vidal-Fernandez (2015) both use the Family and Social Subjects Survey data and study a similar timeframe.

My contribution to the existing literature on intergenerational effects of parental retirement (delays) is three-fold. First, I separately identify anticipation effects, timing effects, and, most notably, permanent effects of parental retirement delays. I am able to do this because the reform I study provides an unaffected comparison group. Having an exempted comparison group also helps ensure that my findings are not biased by an anticipation response.[3] In addition, my identification strategy relies on comparing individuals who were born just a few days apart but faced drastically different retirement opportunities. This helps ensure that my findings are driven solely by the differences in their retirement opportunities.

Second, I contribute with different data. The previous studies (Battistin et al., 2014;

[2] Frimmel et al. (2020) focus on the effect running from the fertility of the younger generation to the retirement decision of the older generation. They instrument the number of grandchildren with twin births. They show that the arrival of grandchildren may accelerate retirement. This indicates that the supply of grandparental child care might be driven by women's decision to have children. The result highlights reverse causality concerns in studies lacking exogenous variation in grandparental child care access.

[3] The existing literature compares individuals before and after their parents become retirement-eligible. Such estimates might be biased if individuals react in anticipation of parental retirement. For example, if women increase fertility before their parents become eligible, then these estimates might falsely suggest that parental retirement eligibility does not affect fertility. Alternatively, if women delay fertility the year before their parents become eligible, then these estimates might falsely suggest a permanent positive effect of parental retirement eligibility on fertility.

Aparicio-Fenoll & Vidal-Fernandez, 2015; Bratti et al., 2018; Eibich & Siedler, 2020) use survey data. I use administrative data. This makes my results less susceptible to sample selection, which might be a concern in the context of fertility and labor force participation.

And third, I contribute with evidence from a different institutional and cultural setting. All previous papers on the effects of parental retirement delays (Battistin et al., 2014; Aparicio-Fenoll & Vidal-Fernandez, 2015; Bratti et al., 2018) study Italy. I study the Netherlands. The two countries differ in many important aspects. Arguably most relevant differences include higher formal child care costs, lower female labor force participation, and generally lower retirement ages (in the periods studied) in Italy. The two countries also differ significantly in family structure. For example, Italian adults co-reside with their parents more often than their Dutch counterparts (De Jong Gierveld & Van Tilburg, 1999).

My work is also linked to studies on the effects of grandparental child care access. This includes studies that apply structural approaches (Cardia & Ng, 2003; Garcia-Moran & Kuehn, 2017; Zamarro, 2020), exploit grandparents' geographic proximity (Compton & Pollak, 2014; Raymo et al., 2010), or deaths (Aassve et al., 2012; Posadas & Vidal-Fernandez, 2013; Arpino et al., 2014). I contribute to this literature with estimates that are arguably more robust to confounding factors. I also contribute to the broader literature on the determinants of female labor force participation and fertility. I provide evidence on the importance of grandparents and their labor market status.

The remainder of the paper unfolds as follows. Section 2 describes the Dutch institutional pension context, including the 2006 pension reform. Sections 3 and 4 discuss the data and the empirical strategy respectively. Section 5 presents the results. Section 6 discusses mechanisms and alternative interpretations of my results. Section 7 concludes.

2 Institutional Background

2.1 Dutch Pension System

The Dutch pension system consists of three pillars. The first pillar consists of the old-age state pension. It provides income to all individuals who have resided in the Netherlands at some point in their lives. The benefits are financed through a pay-as-you-go scheme. They are linked to the minimum wage. The eligibility age has been 65 until 2013. It is gradually rising and will reach 67 and 3 months by 2024.

The second pillar encompasses occupational pensions. These pensions are managed by non-profit pension funds. The funds may serve all businesses in an industry, one specific company, or a group of people working in a specific (usually medical) profession. Approximately three-quarters of those participating in the second pillar do so through industry-wide pension funds. Participation in these funds is usually mandatory. A typical pension agree-

ment provides some flexibility over the exact retirement age. However, the benefits usually increase non-linearly in retirement age. This makes retirement after a certain agreement-specific threshold much more attractive than before. The second pillar is relatively large in the Netherlands. Around 90% of employers participate in it.[4] It is the main doorway to retirement for most individuals.

The third pillar consists of individual insurance schemes. It plays a relatively small role in the Netherlands. In 2006 the first and the second pillars made up 50% and 45% of total pension entitlements respectively (Van de Grift, 2009).

2.2 2006 Reform

The 2006 pension reform abolished the preferential tax treatment of sectoral pension contributions for individuals born from January 1, 1950 onward. At the time of the reform, sectoral pensions operated through the Early Retirement and Prepension scheme (*Vervroegde Uittreding en Prepensioen*). The reform eliminated the tax deductibility of employee contributions and the tax-exempt status of employer contributions made through this scheme.

After the reform was announced, labor unions and employer representatives renegotiated sectoral pension agreements. Adjustments were necessary to ensure that sectoral pensions are feasible under the new tax regime. While scheme details differ per fund, under the new agreements individuals born from January 1, 1950 onward typically had to work substantially longer than those born earlier to reach any given benefit level.

The following example illustrates the impact of the reform. Public sector employees born on December 31, 1949 were not affected by the reform. They could retire at the age of 62 and three months with a replacement rate of 70%. In contrast, public sector employees born on January 1, 1950 could retire at the same age with a replacement rate of 64%. They would have to work additional 13 months to achieve the 70% replacement rate (De Grip et al., 2012). I present cross-sector differences in resulting retirement patterns in Appendix A.

A pension reform could have been anticipated. However, the sharp differential treatment of those born from January 1, 1950 onward was unexpected to the public. It was announced on July 5, 2005. This unexpected differential treatment provides the basis for identifying the causal effect of reduced retirement opportunities.

[4] https://ec.europa.eu/social/main.jsp?catId=1122&langId=en&intPageId=4993 (accessed March 2, 2022)

3 Data

I use administrative data from Statistics Netherlands. The data covers all Dutch citizens living in the Netherlands. For my main sample, I select all females who were childless and cohabiting with male partners before the reform.[5] I restrict my sample to women with Dutch-born parents(-in-law). Data on fertility outcomes concern legal children. Data on labor market outcomes is constructed from tax records. It covers yearly labor earnings, contract and overtime hours, sectors of employment, and distance to work.[6] Birth dates are known up to the month of birth. I also make use of several covariates measured before the reform. They include an indicator for having completed higher education and a measure of distance between couples' and parents' households in a straight line.

I separate my analysis by woman-parent link. In my main analysis, I focus on women whose selected parent(-in-law) was born within 12 months of January 1, 1950. My sample contains 35,449 women whose mothers were born in this period. My sample contains 35,338, 34,670, and 31,621 women whose fathers, mothers-in-law, and fathers-in-law were born in this period respectively.

To gain additional insights into mechanisms I use GGS data. The data covers a representative sample of Dutch households. I use responses from the round conducted in the Netherlands in 2006 and 2007. I select households consisting of a male partner, a female partner, and at least one child under 15. I use data on child care help and material transfers that these households received from either of the partner's parents. I also use data on the weekly working hours of the female partners. My GGS sample contains approximately 1,600 households.

4 Methodology

To estimate the effects of the reform I apply a sharp regression discontinuity design in women's parents' birth dates. I separate my analysis by woman-parent link.[7] I regress women's fertility and labor market outcomes on a dummy that takes value 1 if their selected parent was born from January 1, 1950 onward and 0 otherwise. I control for a smooth function of the distance from the birth date of the parent to January 1950. In my main specification, I use a flexible linear function for the running variable and a 12-month bandwidth around the cutoff. My estimator for the effect of the reform is the coefficient for the dummy that reflects if the selected parent was born from January 1, 1950 onward. It

[5] I include all couples independent of their legal partnership status. While some couples are not married, I refer to the male partners' parents as the women's parents-in-law.

[6] Distance to work is only available starting 2014.

[7] Separating the analysis by woman-parent link allows me to restrict my sample to parents born just around the cutoff. This helps ensure that my identifying assumptions are satisfied.

measures the average effect of the reform on women whose selected parent was born right after December 31, 1949.

The estimator is unbiased under the assumption that women's expected fertility and labor force participation outcomes would be continuous in the birth dates of their parents if all parents were either affected by the reform or exempted from it. To the best of my knowledge, there are no factors that might violate this assumption. To provide further support for this assumption I check for discontinuities in the means of women's observable pre-reform characteristics. I apply the discontinuity design from my main specification but replace the dependent variable with different pre-reform characteristics.

Table 1 presents the results with respect to women's mothers' birth dates.[8] It shows that observable characteristics are balanced between women whose mothers were born before the January 1, 1950 cutoff and women whose mothers were born after the cutoff. The table also contains the p-values used to test for a discontinuity in the mean of a given characteristic around the cutoff. Notably, I find no discontinuities in women's average age and the average number of years they had spent cohabiting with their partners before the reform. I also find no discontinuities in women's probability of working and their average labor income before the reform. These results support my identification strategy.

In specifications with controls, I include several arguably exogenous covariates. Namely, a quadratic function of woman's age, a quadratic function of their labor income in 2004, and a dummy for having non-zero labor income in 2004. I also include equivalent controls for women's partners. In addition, I include dummies for the number of years the couple had spent cohabiting before the reform and the number of woman's parents(-in-law) who were exempted from the reform (excluding the one I study in the respective specification). My results are insensitive to the inclusion of different controls. This provides further evidence that the treatment assignment is as good as random conditional on a smooth function of the running variable.

I believe that my estimates capture the persistent effect of reduced retirement opportunities for future cohorts. The 2006 reform was announced well before most affected individuals reached typical early retirement ages.[9] Moreover, most people were likely aware of the reform. The reform was extensively discussed in the media and pension funds and unions undertook campaigns to inform individuals about it. These campaigns highlighted

[8] I present additional balancing checks in Appendix B. Tables B1 through B3 present results for balance in observables with respect to the birth dates of other parents(-in-law). Tables B4 through B7 present results for balance in observables expanding the sample to include women with children born before the reform. Figures B1 through B8 present scatter plots for the probability of having children and the average number of children in the years leading up to the reform (expanding the sample to include women with children born before the reform). I find no large imbalances in any of the subsamples. Figure B9 presents the empirical distributions of birth dates for women's parents and parents-in-law. I find no evidence of manipulation of birth dates around the cutoff.

[9] Individuals directly affected by the reform were no older than 55 when the reform was announced. They were away from the common early retirement age of 62.

Table 1: Balance table for women in relation to their mothers' birth dates

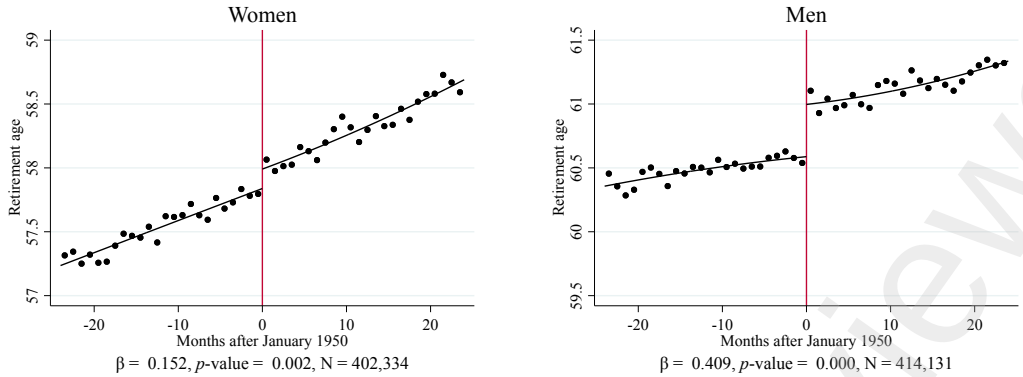
	Exempted mean	Affected mean	Cond. p-val.
Background			
Age 2004	27.67	27.07	0.355
Partner's age 2004	30.12	29.63	0.871
Relationship duration	3.26	3.01	0.843
Other unaffected parents	2.219	2.059	0.630
Oldest parent	0.052	0.040	0.975
Distance to parent (km)	25.39	24.53	0.206
Higher education	0.400	0.384	0.646
Labor market outcomes in 2004			
Working	0.944	0.951	0.677
Income (1,000s of euros)	22.53	21.75	0.355
Couple's income (1,000s of euros)	51.02	49.55	0.476
Parent's income (1,000s of euros)	8.27	8.93	0.424
Parent working	0.494	0.528	0.865
Parent working in public sector	0.254	0.271	0.750

Comparison of females with a selected parent(-in-law) born up to 12 months before 1950 (exempted group) and females with a selected parent(-in-law) born up to 12 months after 1949 (affected group). p-values for regression discontinuity point estimates from a from a flexible linear specification using a 12-month bandwidth. Relationship duration is the number of year a couple had spent living together in 2004 since 1996. Other unaffected parents is the number of parents(-in-law) born before 1950 and alive in 2004 excluding the one in question. Oldest parent takes value 1 if the selected parent is the oldest parent(-in-law). Distance to parent (km) is the distance in a straight line between the household of the couple and the household of the parent in question in 2004. Higher education takes value 1 if the female has higher education. Income measures capture total employment and self-employment income in 2004. Work measures capture if the individual has non-zero employment income N = 35,449, cluster = 32,215 (N = 35,193, cluster = 31,978 for distance).

the differential reform implications for individuals born before January 1, 1950 and those born later.[10] Therefore, affected parents had ample time to adjust retirement plans and inform their children of these plans. Thus, I believe that my estimates are not driven by a cohort-specific shock resulting from the inability to prepare for retirement delays. Instead, I believe that my estimates capture a more general effect of worse retirement opportunities.

[10] De Grip et al. (2012) provide survey evidence that the majority of public sector employees were aware of the reform and its differential implications.

Figure 1: Retirement age



Note: Scatter plots for the birth month averages of retirement age censoring at 53 and 67. Point estimates refer to regression discontinuity effect estimates from a flexible quadratic specification. Sample covers the full population of Dutch born individuals living in the Netherlands and born within the selected bandwidth.

5 Results

5.1 Parents' Retirement

I hypothesize that women in the subsequent generation might be affected by the reform through the retirement status of their parents. I first show that the reform had a sizable impact on the retirement timing of the directly affected individuals. I use a sample that contains all individuals who were born in the Netherlands around January 1, 1950 and alive in 2004. Figure 1 presents their average censored retirement age[11] by birth month.[12] The figure also presents the corresponding discontinuity estimates. The point estimates show that the reform increased the average retirement age by approximately 2 months for women and 5 months for men.

The relatively lower effect on women can be explained by two factors. First, relatively more women had already left the labor force before the reform.[13] Second, among those in the labor force at the time of the reform women retired earlier than men on average.[14] Thus, most women likely had to delay retirement less than men to reach their planned retirement income. Figure 2 presents the result on retirement age for individuals who were

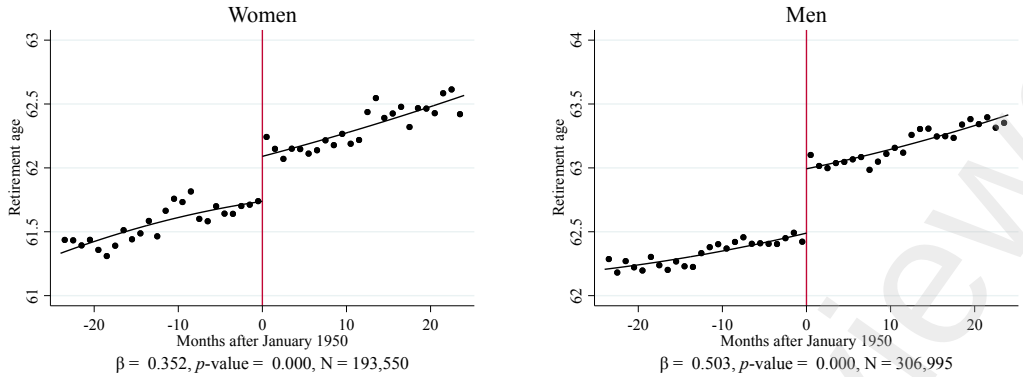
[11] I compute age in terms of years and months at which individuals were last employed. The variable takes value 53 for those who were never employed between the ages of 53 and 67. It takes value 67 for those who were employed at the age of 67. Setting the lower bound to 53 ensures that early retirees are represented equally in the control and treatment groups because all individuals in the sample turned 53 before the reform was announced.

[12] Here I apply a 24 months bandwidth to demonstrate that the discontinuity does not result from comparing individuals born in different years.

[13] Around half of the women were not employed on their 53rd birthday. Meanwhile, three-quarters of men were still working at that age. This can be seen by comparing observation numbers in Figures 1 and 2.

[14] Conditional on working on their 53rd birthday, unaffected women retired earlier than unaffected men. This can be seen by comparing retirement ages for men and women in Figure 2.

Figure 2: Retirement age for individuals employed on 53rd birthday



Note: Scatter plots for the means of retirement age censoring at 53 and 67. Point estimates refer to regression discontinuity effect estimates from a flexible quadratic specification. Sample covers the full population of Dutch born individuals living in the Netherlands, born within the selected bandwidth, and employed on their 53rd birthday.

employed on their 53rd birthday. It suggests that the reform led to retirement delays of approximately 4 and 6 months among women and men who were working before the reform respectively.

Figure 3 presents the retirement delay estimates for the directly affected generation by year. Every data point represents a discontinuity effect estimate for the probability of being employed in the respective year. The results suggest that the reform did not affect individuals' labor market status before 2010. The reform resulted in retirement delays in the period from 2010 to 2015. The share of affected individuals rose over time. By 2016 the difference in retirement status suddenly disappeared as individuals reached the state pension age.

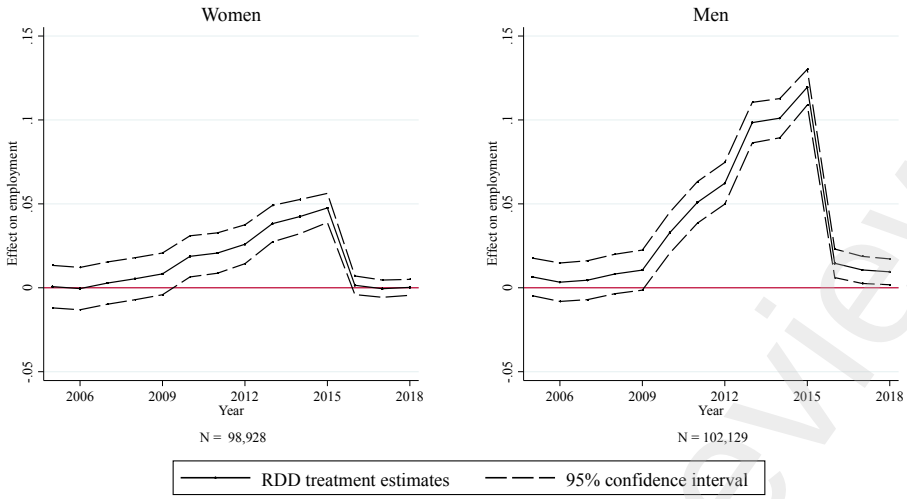
The documented retirement delays capture any amplifying or diminishing effects that arise from changes to retirement expectations. For example, suppose that the children of the affected parents reduce their fertility in response to the reform. Then, the affected parents might delay their retirement even more than if they were to have grandchildren. Therefore, all of my results should be interpreted as the effects of a reform resulting in the estimated delays in retirement, rather than the effects of the estimated delays.[15]

5.2 Fertility

Next, I investigate how the reform affected fertility among women with directly impacted parents(-in-law). I find that reform reduced fertility among women with affected mothers. Figure 4 presents the average number of children per woman in a given year by woman's

[15]For this reason I focus on reduced form estimates as opposed to estimating the effect of parental retirement using an instrumental variable approach.

Figure 3: Retirement delays by year



Note: Regression discontinuity effect estimates by year from a flexible linear specification. 6 month bandwidth. Sample covers the full population of Dutch born individuals living in the Netherlands born within the selected bandwidth.

mother's birth month. The figure also presents the corresponding discontinuity estimates. A discontinuity of -0.03 emerges in 2010. By 2013 the difference grows to -0.05 and remains stable until at least 2018.

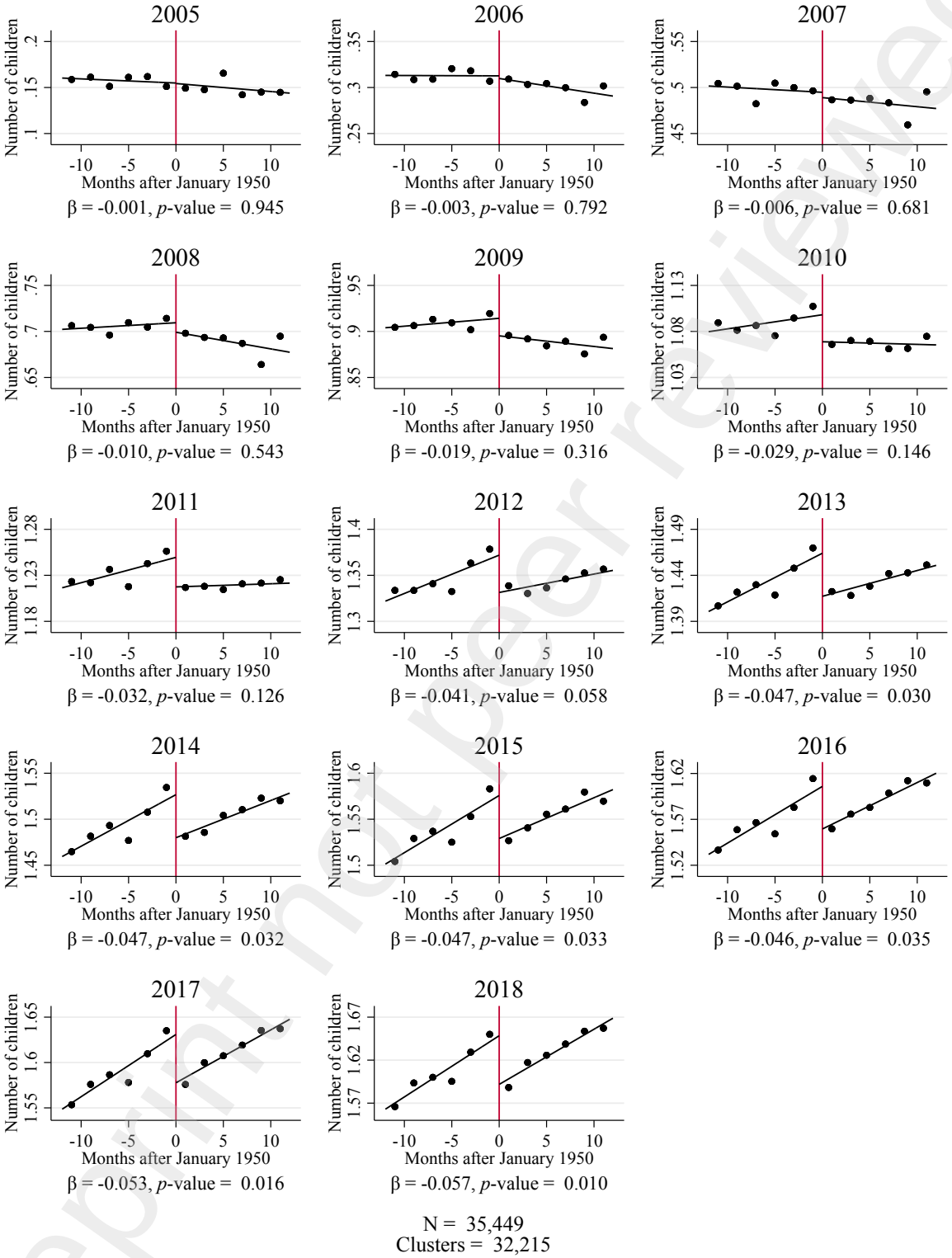
My results suggest that women time fertility with their mothers' retirement, rather than having children in anticipation of their mothers' retirement. Taken together, Figures 3 and 4 indicate that the effects on women's fertility and their mothers' retirement status both emerge around 2010. The effects continue to grow in parallel.

The fertility reduction is likely permanent because it is concentrated among women who are at the end of their fertile period. I separate women born before the median birth date (March 1977) and those born later. Women in the older sample half were at least 28 years old at the time of the reform. They were at least 41 years old at the end of my sample period. Figure 5 presents the discontinuity point estimates by year. The effect is concentrated among women in the older half of the sample. Considering the age of this group, the fertility gap I estimate in 2018 is unlikely to close.

The reform also reduced the probability of having any children. Table 2 presents the results for the extensive fertility margin. As before, every point estimate refers to the discontinuity estimate for the given year. The estimated effect follows a similar pattern as that on the number of children.

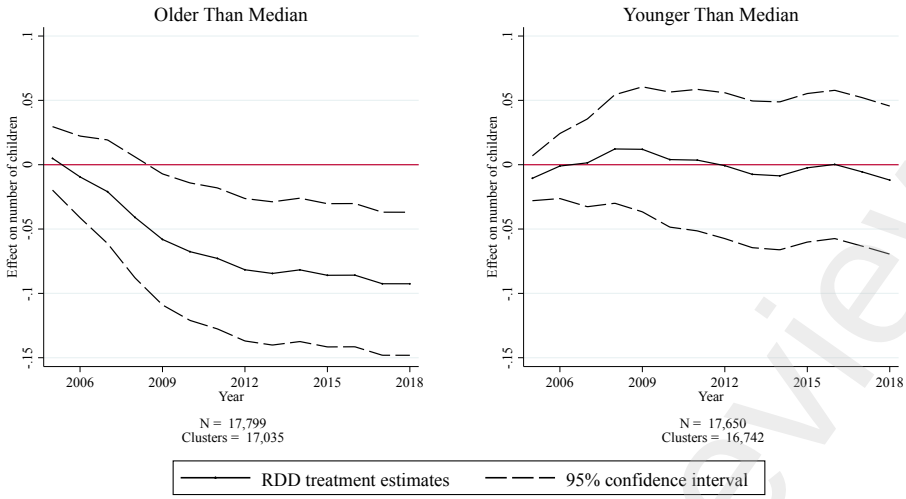
I do not find any effects running through women's fathers and parents-in-law. Table 2 presents the discontinuity estimates. These findings are in line with survey results on grandparental child care, which I will present in Section 6. Specifically, that women's mothers play a significantly larger role in grandparental child care than any other parent(-

Figure 4: Fertility in years 2005-2018 in relation to women's mothers' birth dates



Note: Scatter plots for average number of children per woman by their mother's birth month. Two-month bins. Point estimates refer to regression discontinuity effect estimates from a flexible linear specification.

Figure 5: Effect on fertility through women's mothers, heterogeneity by women's age



Note: Regression discontinuity effect estimates from a flexible linear specification. 12 month bandwidth. Sample split into women born before the median birth date (March 1977) and women born later. Controls include quadratic functions of woman's and their partner's ages, quadratic functions of woman's and their partner's labor incomes in 2004, dummies for having non-zero labor income in 2004 for the woman and their partner, dummies for the number of years the couple had spent cohabiting before the reform and the number of woman's parents(-in-law) exempted from the reform (excluding the one we study in this specification). Standard errors clustered at the parent level.

in-law).

Overall, my findings suggest that the hidden cost of the reform (reduced fertility in the subsequent generation) is substantial relative to the benefit (achieved retirement delay). The reform reduced fertility by 1 child per 20 couples in my sample. My sample covers a quarter of all women in the generation with reform-affected parents. Therefore, the reform reduced fertility by at least 1 child per 80 women in the generation.[16] Meanwhile, it induced directly affected individuals to delay retirement by less than half a year on average.

5.3 Labor Market

Next, I investigate how the reform affected labor market outcomes among women with directly impacted parents(-in-law). The theoretical effect of reduced access to grandparental child care on female labor force participation is ambiguous. On the one hand, decreased child care access may make working more costly for young mothers. On the other hand, reduced fertility stemming from reduced child care access may boost female labor force participation.

[16] I also estimate the effect using a sample that includes all couples (including those who already had children before the reform) and a sample that includes couples formed before 2010 (before the reform had material effects on parents' retirement). The resulting estimates presented in subsection 5.4 are close to those I extrapolate here.

Table 2: Effect on fertility through women's parents and parents-in-law

	2006	2008	2010	2012	2014	2016	2018
Via mother							
Children	-0.006	-0.014	-0.032	-0.041**	-0.045**	-0.043**	-0.053**
	(0.010)	(0.016)	(0.019)	(0.020)	(0.021)	(0.021)	(0.021)
Children>0	-0.007	-0.013	-0.019*	-0.018**	-0.019**	-0.015*	-0.019**
	(0.010)	(0.010)	(0.010)	(0.009)	(0.009)	(0.008)	(0.008)
Via father							
Children	-0.003	0.009	0.017	0.003	-0.001	0.008	0.007
	(0.010)	(0.016)	(0.019)	(0.020)	(0.021)	(0.021)	(0.021)
Children>0	0.001	0.008	0.006	0.002	0.006	0.011	0.011
	(0.009)	(0.010)	(0.010)	(0.009)	(0.008)	(0.008)	(0.008)
Via partner's mother							
Children	-0.002	0.008	0.019	0.031	0.023	0.022	0.028
	(0.010)	(0.016)	(0.019)	(0.020)	(0.020)	(0.021)	(0.021)
Children>0	-0.001	0.002	0.008	0.009	-0.002	-0.003	-0.002
	(0.009)	(0.010)	(0.010)	(0.009)	(0.008)	(0.008)	(0.008)
Via partner's father							
Children	0.011	-0.010	-0.002	0.003	-0.008	-0.003	-0.001
	(0.011)	(0.017)	(0.020)	(0.022)	(0.022)	(0.022)	(0.022)
Children>0	0.010	-0.012	-0.007	-0.004	-0.004	-0.000	-0.001
	(0.010)	(0.011)	(0.011)	(0.010)	(0.009)	(0.008)	(0.008)

Note: Regression discontinuity effect estimates from a flexible linear specification. 12-month bandwidth. I estimate every coefficient in a separate regression for the given year and the given parent(-in-law). Children is the number of children. Children>0 takes value 1 if the woman has any children and 0 otherwise. Controls include quadratic functions of woman's and their partner's ages, quadratic functions of woman's and their partner's labor incomes in 2004, dummies for having non-zero labor income in 2004 for the woman and their partner, dummies for the number of years the couple had spent cohabiting before the reform and the number of woman's parents(-in-law) exempted from the reform (excluding the one we study in the respective specification). Via mother: N = 35,449, cluster = 32,215. Via father: N = 35,338, cluster = 32,091. Via partner's mother: N = 34,670, cluster = 31,621. Via partner's father: N = 29,927, cluster = 27,460. *significant at 10%, **significant at 5%, ***significant at 1%.

Table 3: Effect on labor market outcomes through women's parents

	2006	2008	2010	2012	2014	2016	2018
Via mother							
Work	0.001	0.004	0.005	0.008	0.008	0.007	0.011
	(0.004)	(0.006)	(0.006)	(0.007)	(0.008)	(0.008)	(0.008)
Hours	3.759	16.645	13.379	13.861	7.494	10.313	8.969
	(9.417)	(10.708)	(11.087)	(11.244)	(11.584)	(11.673)	(11.758)
Income	0.024	0.150	0.251	0.104	0.127	0.087	0.021
	(0.200)	(0.271)	(0.325)	(0.377)	(0.392)	(0.464)	(0.498)
Private	0.006	0.004	0.003	0.004	0.006	0.003	0.005
	(0.010)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
Distance					-0.130	1.205*	0.369
					(0.692)	(0.694)	(0.660)
Via father							
Work	-0.002	-0.002	-0.006	-0.014**	-0.011	-0.013	-0.012
	(0.004)	(0.005)	(0.006)	(0.007)	(0.008)	(0.008)	(0.008)
Hours	-9.875	-15.496	-11.307	-16.859	-12.354	-16.524	-19.972*
	(9.563)	(10.824)	(11.188)	(11.340)	(11.674)	(11.661)	(11.591)
Income	-0.141	-0.406	-0.233	-0.392	-0.494	-0.657	-0.808*
	(0.184)	(0.264)	(0.316)	(0.355)	(0.373)	(0.442)	(0.480)
Private	0.003	0.006	0.002	0.003	0.009	0.010	0.007
	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
Distance					0.009	0.035	-0.487
					(0.687)	(0.666)	(0.662)

Note: Regression discontinuity effect estimates from a flexible linear specification. 12-month bandwidth. I estimate every coefficient in a separate regression for the given year and the given parent(-in-law). Work takes value 1 if the woman has non-zero labor income and 0 otherwise. Hours consist of contract hours and overtime hours. Income is measured in thousands of euros and consists of employment and self-employment income. Private takes value 1 if the woman is working in the private sector and 0 otherwise. Distance is the distance to work measured in kilometers. Controls include quadratic functions of woman's and their partner's ages, quadratic functions of woman's and their partner's labor incomes in 2004, dummies for having non-zero labor income in 2004 for the woman and their partner, dummies for the number of years the couple had spent cohabiting before the reform and the number of woman's parents(-in-law) exempted from the reform (excluding the one we study in the respective specification). Via mother: N = 35,449, cluster = 32,215, (N = 27,952, cluster = 25,809 for distance to work). Via father: N = 35,338, cluster = 32,091, (N = 27,997, cluster = 25,830 for distance to work). *significant at 10%, **significant at 5%, ***significant at 1%.

Table 4: Effect on labor market outcomes through women's parents-in-law

	2006	2008	2010	2012	2014	2016	2018
Via partner's mother							
Work	-0.007	-0.007	0.004	-0.001	0.009	0.003	0.005
	(0.004)	(0.006)	(0.006)	(0.007)	(0.008)	(0.008)	(0.008)
Hours	-8.515	-0.384	4.515	3.809	2.512	-3.418	9.820
	(9.719)	(10.942)	(11.352)	(11.434)	(11.713)	(11.658)	(11.665)
Income	-0.183	-0.285	0.039	0.032	-0.033	-0.364	-0.007
	(0.188)	(0.392)	(0.312)	(0.354)	(0.373)	(0.437)	(0.475)
Private	0.004	-0.001	0.001	-0.001	0.006	0.011	0.006
	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
Distance					1.583**	0.302	-0.035
					(0.683)	(0.670)	(0.658)
Via partner's father							
Work	-0.015***	-0.011*	-0.003	-0.011	-0.008	-0.006	-0.005
	(0.005)	(0.006)	(0.007)	(0.007)	(0.008)	(0.008)	(0.009)
Hours	-24.140**	-21.497*	-12.130	-15.681	-12.101	-3.644	-9.346
	(10.381)	(11.724)	(12.148)	(12.180)	(12.501)	(12.511)	(12.455)
Income	-0.512***	-0.497*	-0.487	-0.448	-0.473	-0.180	0.109
	(0.191)	(0.271)	(0.324)	(0.371)	(0.393)	(0.461)	(0.668)
Private	-0.020*	-0.008	0.000	-0.008	0.002	-0.007	-0.012
	(0.011)	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)
Distance					-0.366	-1.611**	-1.816**
					(0.756)	(0.713)	(0.706)

Note: Regression discontinuity effect estimates from a flexible linear specification. 12-month bandwidth. I estimate every coefficient in a separate regression for the given year and the given parent(-in-law). Work takes value 1 if the woman has non-zero labor income and 0 otherwise. Hours consist of contract hours and overtime hours. Income is measured in thousands of euros and consists of employment and self-employment income. Private takes value 1 if the woman is working in the private sector and 0 otherwise. Distance is the distance to work measured in kilometers. Controls include quadratic functions of woman's and their partner's ages, quadratic functions of woman's and their partner's labor incomes in 2004, dummies for having non-zero labor income in 2004 for the woman and their partner, dummies for the number of years the couple had spent cohabiting before the reform and the number of woman's parents(-in-law) exempted from the reform (excluding the one we study in the respective specification). Via partner's mother: N = 34,670, cluster = 31,621, (N = 27,346, cluster = 25,399 for distance to work). Via partner's father: N = 29,927, cluster = 27,460, (N = 23,800, cluster = 22,207 for distance to work). *significant at 10%, **significant at 5%, ***significant at 1%.

I find no effects on women's labor market outcomes. Table 3 present the discontinuity point estimates for labor market effects running through women's parents. Table 4 presents estimates for effects running through women's parents-in-law. I find no effects on the extensive employment margin, work hours, labor income, the probability of working in the private sector, and distance to work. My point estimates are precise and close to zero.[17].

5.4 Sensitivity Analysis

To test the robustness of my estimates I implement standard sensitivity tests. Table 5 presents the sensitivity analysis results for the effect on fertility that runs through women's mothers. I test if my results are sensitive to bandwidth choice and choice of running variable function following Imbens & Lemieux (2008). I reduce (increase) the bandwidth to 6 (24) months and use second and third degree polynomial functions of the running variable. Rows (1) through (7) present the results. My estimates are fairly robust to these changes.

I also implement a formal bandwidth selection procedure and conduct bias-corrected inference following Calonico et al. (2014). Row (8) presents the results. The optimal bandwidth fluctuates from 10 to 14 months over different years. Using bias-corrected confidence intervals does not affect the statistical significance of my estimates in a meaningful way (the relevant p-values change by no more than 1 percentage point).

I test if my estimates are robust to alternative sample selection protocols. I expand my sample to include couples who had children before the reform. Row (9) presents the results. Relative to my main specification, my point estimates decrease but their precision improves. The result is in line with limited effects on fertility among women who already had children before the reform.[18] I also expand my sample to include couples that formed before the reform affected their parents' retirement status (2010).[19] Row (10) presents the results. They are similar to my main specification.

Finally, I test for seasonality-related effects by moving the discontinuity threshold to January 1, 1949. Row (11) presents the results. The estimated effects are relatively precise and close to zero. This suggests that my findings are not driven by omitted variables that might be captured by parents' birth months.

[17] Results from my main specification for effects running through partners' parents suggest a small negative effect on employment and income at the year of the reform. These results appear to be driven by outliers and are not robust. I present more details in Appendix C

[18] Estimates based on a sample of women who had children before the reform are precise and indicate no effect on their fertility (not reported).

[19] I also test for impact on couple formation and relationship stability but find no effects (not reported).

Table 5: Sensitivity analysis for the effect on fertility through women's mothers

Spec.	2006	2008	2010	2012	2014	2016	2018	N
(1)	0.002	-0.015	-0.048*	-0.055*	-0.070**	-0.069**	-0.077***	17,693
	(0.015)	(0.023)	(0.028)	(0.029)	(0.030)	(0.030)	(0.029)	
(2)	-0.006	-0.014	-0.032	-0.041**	-0.045**	-0.043**	-0.053**	35,449
	(0.010)	(0.016)	(0.019)	(0.020)	(0.021)	(0.021)	(0.021)	
(3)	0.004	-0.011	-0.042	-0.059*	-0.069**	-0.070**	-0.076**	35,449
	(0.016)	(0.025)	(0.029)	(0.031)	(0.031)	(0.031)	(0.031)	
(4)	0.012	-0.031	-0.059	-0.055	-0.067	-0.080*	-0.085**	35,449
	(0.021)	(0.034)	(0.040)	(0.043)	(0.043)	(0.043)	(0.043)	
(5)	-0.000	-0.005	-0.026*	-0.026*	-0.025*	-0.023	-0.028*	70,434
	(0.007)	(0.011)	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)	
(6)	-0.003	-0.010	-0.028	-0.040*	-0.049**	-0.051**	-0.058***	70,434
	(0.011)	(0.017)	(0.020)	(0.022)	(0.022)	(0.022)	(0.022)	
(7)	-0.006	-0.022	-0.050*	-0.061**	-0.071**	-0.074**	-0.085***	70,434
	(0.015)	(0.023)	(0.028)	(0.029)	(0.029)	(0.029)	(0.029)	
(8)	-0.001	-0.010	-0.033	-0.046**	-0.055**	-0.054**	-0.067**	105,107
	(0.011)	(0.017)	(0.021)	(0.023)	(0.025)	(0.024)	(0.026)	
(9)	-0.005	-0.008	-0.014	-0.021*	-0.023**	-0.022*	-0.026**	76,175
	(0.006)	(0.009)	(0.011)	(0.012)	(0.012)	(0.012)	(0.012)	
(10)	-0.010	-0.018	-0.022	-0.028*	-0.035**	-0.032*	-0.036**	51,756
	(0.007)	(0.012)	(0.015)	(0.016)	(0.017)	(0.017)	(0.017)	
(11)	-0.009	-0.004	-0.005	-0.000	0.002	0.009	0.008	31,386
	(0.012)	(0.018)	(0.020)	(0.021)	(0.021)	(0.021)	(0.021)	

Note: Regression discontinuity effect estimates. I estimate every coefficient in a separate regression for the given year. The outcome is the number of children in the given year. The running variable is the woman's mother's birth date. Each row presents a different robustness check. All specifications include the same controls as the main specification unless stated otherwise. (1) linear specification, 6-month bandwidth, (2) linear specification, 12-month bandwidth, (3) quadratic specification, 12-month bandwidth, (4) cubic specification, 12-month bandwidth, (5) linear specification, 24-month bandwidth, (6) quadratic specification, 24-month bandwidth, (7) cubic specification, 24-month bandwidth, (8) specification and bandwidth selected following Calonico et al. (2014) (no controls). p-values based on bias-corrected confidence intervals. 36-month bandwidth used for model selection. Optimal bandwidth fluctuates from 10 months to 14 months, (9) as the main specification but the sample also includes women who had children before the reform (controlling for the number of pre-reform children), (10) as the main specification but the sample includes all women in a relationship in 2010, (11) placebo test, as the main specification but cutoff moved to January 1, 1949. Standard errors clustered at the parent level. *significant at 10%, **significant at 5%, ***significant at 1%

6 Mechanisms and Heterogeneity

I found that the 2006 Dutch pension reform reduced fertility among women with reform-affected mothers. Here I investigate which mechanism can better explain the fertility reduction: a reduction in access to grandparental child care or reduced material transfers from mothers to their adult daughters. I also found no effects on labor market outcomes among women with reform-affected parents. In the subsequent subsection, I investigate joint explanations for a sizable effect on fertility together with a negligible effect on women's labor market outcomes. Finally, I investigate how the effect on fertility varies among different socioeconomic groups.

6.1 Mechanisms Behind Fertility Results

Pension reforms may reduce fertility in the subsequent generation through two primary channels. First, pension reforms often pressure individuals to work longer. Working longer may reduce how much time individuals can spend on grandparental child care. The subsequent generation may view the reduction in grandparental child care supply as an increase in the cost of having children. In turn, women in the subsequent generation might reduce their fertility. Second, pension reforms may reduce individuals' income. Lower income can translate to reduced material transfers from parents to adult children. If children are a normal good (from the perspective of the adult children), then reduced material transfers may lead women to reduce fertility.

I aim to infer the relative importance of grandparental child care and intergenerational income transfers for explaining my results. I use three approaches. First, I investigate the patterns of the two transfer types in the GGS. Table 6 presents the results. Almost 50% of households with children under 15 receive child care help from the female partner's mother. The share rises to 66% among households with children younger than 3.[20] In contrast, 32% of these households receive child care help from maternal grandfathers. Even fewer households receive such help from paternal grandparents. Thus, maternal grandmothers are the largest providers of grandparental child care in the Netherlands. An effect running through them is consistent with the grandparental child care channel.

In contrast, material transfers from parents to adult children are much less prevalent. There is little room for a reform to reduce them further. Moreover, fewer households with children under 3 report material transfers from maternal grandmothers than from maternal grandfathers (13% and 18% respectively). If maternal grandfathers provide more material transfers, then I might expect an income reduction effect running through women's

[20] This large role that maternal grandmothers play when the grandchildren are young also helps explain why women time their fertility with their mother's retirement.

Table 6: GGS: grandparental child care and income transfers

	Fraction	*p*-value against subsequent row	Fraction	*p*-value against subsequent row
Child care help	*With children under 15*		*With children under 3*	
Female's mother	0.486	0.000	0.659	0.000
Female's father	0.316	0.498	0.463	0.737
Male's mother	0.306	0.000	0.452	0.014
Male's father	0.219		0.373	
Income transfers	*With children under 15*		*With children under 3*	
Female's father	0.114	0.201	0.184	0.007
Female's mother	0.101	0.031	0.126	0.828
Male's father	0.080	0.120	0.122	0.005
Male's mother	0.067		0.073	

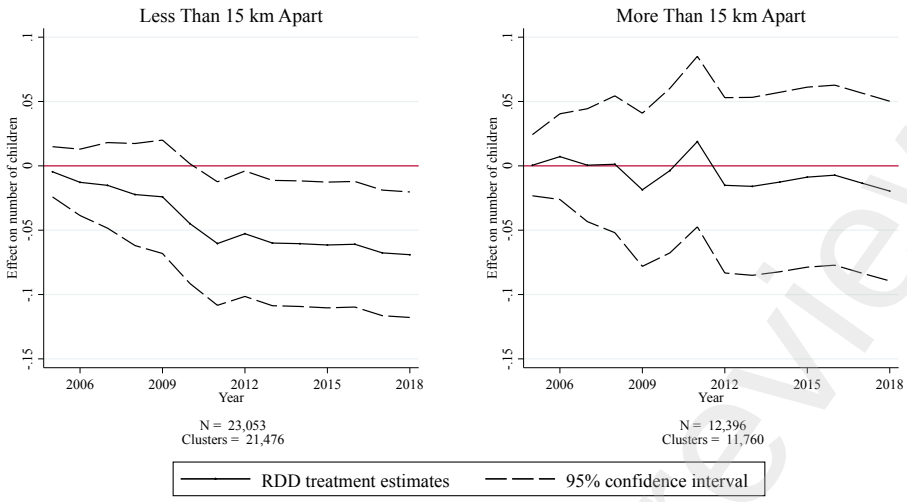
1. *Child care help*: Representatives of households with children under 15 were asked if their household regularly receives help with child care from informal sources. Those answering positively were asked to list up to five informal child care providers. The table presents the fractions of households receiving help by parental connection. With children under 15: N = 1,643 , With children under 3: N = 451.
2. *Income transfers*: Household representatives were asked if their household received money, goods, or assets from people outside the household within the last 12 months. Those answering positively were asked to list up to six sources of these transfers. The table presents the fractions of households receiving transfers by parental connection. With children under 15: N = 1,638, With children under 3: N = 451
3. *p*-values from a paired *t*-test for equal means

fathers.[21] However, I only observe an effect running through women's mothers. Combined with already low rates of material transfers, the results do not support the income transfers channel.

Next, I exploit the fact that the cost of providing grandparental child care varies by geographic distance to the grandchildren while the cost of income transfers does not. If the fertility reduction is driven by reduced grandparental child care access, then I would expect the effect to be concentrated among women living close to their mothers. Alternatively, if the fertility reduction is driven by reduced income transfers from women's mothers, then I would also expect an effect on women living further away. I split the sample by distance from the couple's home to the woman's mother's home before the reform. I separate women who were living within 15 kilometers of their mothers and those who were living further away. In the Netherlands, this roughly translates to living in the same city. Figure 6 presents the discontinuity point estimates. The effect is only present in the subsample of women who lived within 15 kilometers of their mothers. This provides further support for

[21] This argument assumes that the reform reduced men's income as least as much as women's. Later I will show that the reform had similar effects on men's and women's incomes (figure 9).

Figure 6: Effect on fertility through women's mothers, heterogeneity by distance between households



Note: Regression discontinuity effect estimates from a flexible linear specification. 12-month bandwidth. Sample split into women who were living within 15 kilometers of their mothers in 2004 and women who were living further away. Controls include quadratic functions of woman's and their partner's ages, quadratic functions of woman's and their partner's labor incomes in 2004, dummies for having non-zero labor income in 2004 for the woman and their partner, dummies for the number of years the couple had spent cohabiting before the reform and the number of woman's parents(-in-law) exempted from the reform (excluding the one we study in this specification). Standard errors clustered at the parent level.

the grandparental child care channel.

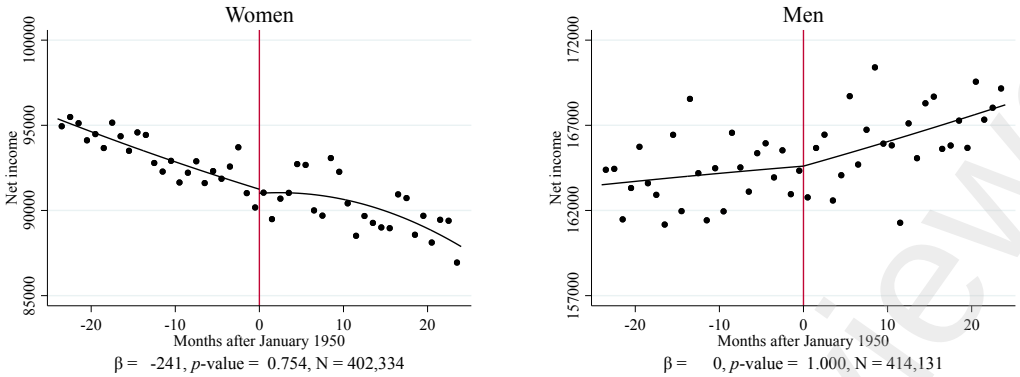
Lastly, I investigate how the reform affected the income of the older generation. If the fertility reduction is driven by reduced income transfers from mothers to their adult daughters, then I would expect that the reform reduced income among directly impacted individuals. I construct a comprehensive income measure aimed at capturing any income parents could transfer to their adult children. It includes labor earnings, various benefits, and saving scheme withdrawals. I subtract taxes, insurance premiums, and saving scheme contributions.[22] Figure 7 presents the average net income per individual from 2011 to 2018 by their birth month. The results suggest that the reform had an economically small effect on individuals' income during this period. Thus, I see little reason to expect a meaningful impact on material transfers between generations. Therefore, I do not find support for the income reduction channel.

Summarizing the above, my results indicate that the effect on fertility is better explained by a reduction in access to grandparental child care rather than a reduction in income transfers from parents to their adult children.

I believe that a reduction in the supply of grandparental child care is the most plausible mechanism behind my results on fertility. However, I acknowledge that there are other

[22] I describe the measure and the motivation behind it in detail in Appendix D.

Figure 7: Effect on income of parent generation



Note: Scatter plots for the birth month averages of net income during the 2011-2018 period. Point estimates refer to regression discontinuity effect estimates from a flexible quadratic specification. Sample covers the full population of Dutch born individuals living in the Netherlands born within the selected bandwidth.

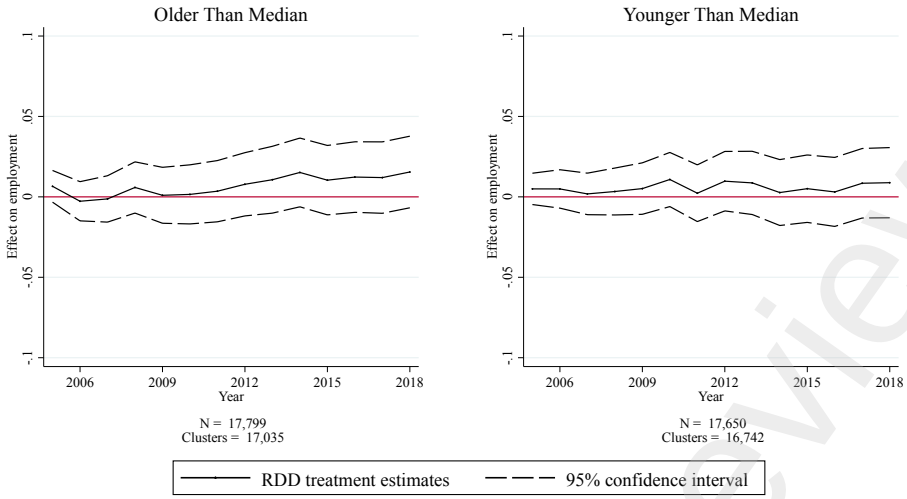
potential mechanisms that I cannot distinguish. One notable alternative mechanism is based on the demand for grandchildren, rather than the supply of grandparental child care. That is, upon retirement mothers may pressure their daughters to have children. Distinguishing this mechanism may be important from a policy perspective. Policymakers may hope to prevent the fertility reduction resulting from retirement delays by expanding formal childcare access. However, expanding formal child care access might be ineffective if the fertility reduction is driven by the lack of social pressure from women's mothers.

6.2 Mechanisms Behind Labor Market Results

I found a sizable fertility reduction among women with reform-affected mothers. However, I found no effects on their labor market outcomes. In this subsection, I investigate joint explanations for the two results. First, I investigate if the direct negative effect of reduced grandparental child care access on female labor market outcomes is offset by a positive effect running through reduced fertility (Lundborg et al., 2017; Kleven et al., 2019). Then, I discuss how grandparental child care access may affect fertility but leave female labor market outcomes unaffected.

I aim to test if the direct negative impact of reduced grandparental child care access on female labor market outcomes is concealed by an offsetting positive effect running through reduced fertility. If the direct effect is comparable among different groups of women, then I can expect to identify it by focusing on women who did not reduce their fertility in response to the reform. In the light of my results on fertility, the most appropriate subsample for

Figure 8: Effect on employment through women's mothers, heterogeneity by women's age



Note: Regression discontinuity effect estimates from a flexible linear specification. 12-month bandwidth. Sample split into women born before the median birth date (March 1977) and women born later. Controls include quadratic functions of woman's and their partner's ages, quadratic functions of woman's and their partner's labor incomes in 2004, dummies for having non-zero labor income in 2004 for the woman and their partner, dummies for the number of years the couple had spent cohabiting before the reform and the number of woman's parents(-in-law) exempted from the reform (excluding the one we study in this specification). Standard errors clustered at the parent level.

this consists of women with below-median age.[23] I found that this group of women did not reduce their fertility much in response to the reform. However, access to grandparental child care may nonetheless affect their labor market outcomes after having children.

I separate women born before the median birth date (March 1977) and those born later. Figure 8 presents the discontinuity point estimates for the effect on women's employment. I find no effect on younger women who did not adjust their fertility in response to the reform. Thus, I find no support for the explanation that the direct negative effect on labor force participation is concealed by an offsetting positive effect running through reduced fertility.[24]

Next, I discuss how grandparental child care access can affect fertility while having a limited effect on female labor force participation. The first mechanism concerns the nature of grandparental child care. Grandparental child care supply may be incidental and hard to predict. Access to such irregular child care may be of limited use for enabling female

[23] Another candidate subsample consists of women who completed their fertility before the reform. However, by the time that the reform had material effects on parental retirement most of the children of these women were in their teens, and therefore, less dependent on child care. As such, the lack of effects on the labor market outcomes of these women would not be informative (this is also what I find, not reported).

[24] I also check for heterogeneity in labor market effects along other dimensions. Namely, dimensions for which I found heterogeneity in the fertility response. Figures C2 and C3 in Appendix C repeat the analysis for the effect on working by distance to mothers' households and women's income respectively. I find no heterogeneity in the effect on women's labor force participation in either dimension.

Table 7: Grandparental child care and work in GGS

	Weekly working hours			
	With children under 15		With children under 3	
Receiving child care help	0.196	0.345	0.340	0.328
	(0.656)	(0.707)	(1.292)	(1.291)
Constant	17.620***	17.512***	17.714***	17.722***
	(0.456)	(0.474)	(1.047)	(1.046)
Child age fixed effects	No	Yes	No	Yes
N	1,622	1,615	448	448

1. Gender and Generations Survey results. Representatives of households with children under 15 were asked if their household regularly receives help with child care from informal sources. Those answering positively were asked to list up to five informal child care providers.
2. The table presents point estimates from a regression of female partner's weekly working hours on a dummy taking value one if the household reported receiving regular help with child care from the female's mother. The coefficient for the constant in specifications with fixed effects refers to the average fixed effect. *significant at 10%, **significant at 5%, ***significant at 1%.

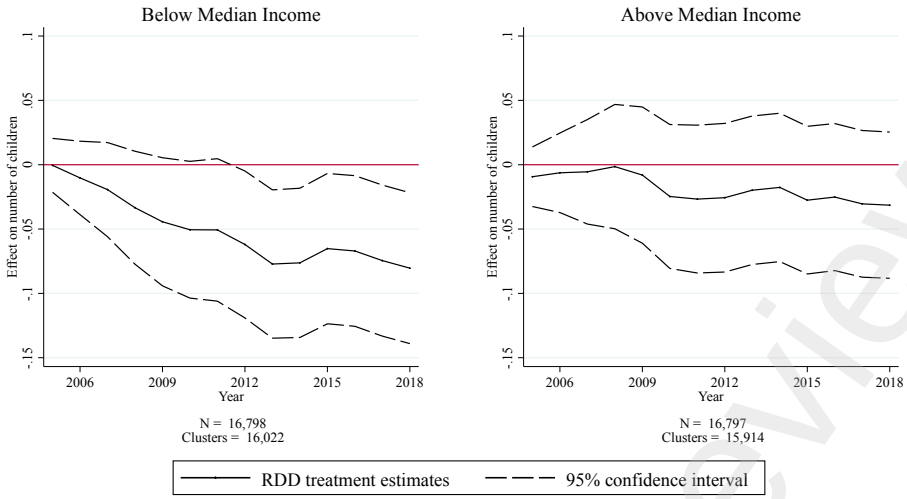
labor force participation. However, this type of child care may provide opportunities for informal work or leisure. This could make having children more attractive. In addition, access to grandparental child care may be desirable for reasons unrelated to labor market opportunities. For example, grandparental child care may come with psychological support or advice about raising children. Losing these resources may make women less likely to have children but not affect their labor supply decision after having children.

The second mechanism concerns the formal Dutch child care system. The effective cost of child care in the Netherlands depends on how much the parents work. Five days of kindergarten per week from 7 am to 6 pm can often cost as much as 1,500 euros per month. This makes child care expensive. However, to stimulate female labor force participation, the government provides child care subsidies. These subsidies can be as high as 80% of the formal child care cost. The number of subsidized hours depends on the number of hours that both parents spend working.

Subsidized child care during parents' work hours limits children's effect on women's labor market outcomes. This likely diminishes the importance of grandparental child care access for women's labor market opportunities. However, such subsidies preserve the role of grandparents in providing child care outside parents' work hours. In the resulting setting, grandparental child care access may make having children more attractive by providing women with more time for leisure or informal work. However, it may have limited effects on women's labor market opportunities.

I provide support for this explanation using GGS data. If grandparental child care access is not important for women's labor market opportunities, then I would expect a

Figure 9: Effect on fertility through women's mothers, heterogeneity by women's income



Note: Regression discontinuity effect estimates from a flexible linear specification. 12-month bandwidth. Sample split into working women who were earning less than the median in 2004 and women who were earning more. Controls include quadratic functions of woman's and their partner's ages, quadratic functions of woman's and their partner's labor incomes in 2004, dummies for having non-zero labor income in 2004 for the woman and their partner, dummies for the number of years the couple had spent cohabiting before the reform and the number of woman's parents(-in-law) exempted from the reform (excluding the one we study in this specification). Standard errors clustered at the parent level.

similar take-up of grandparental child care help among women with varying labor market profiles. I regress weekly female working hours on a dummy that takes values 1 if the household receives regular child care help from the maternal grandmother and 0 otherwise. Table 7 presents the results. I do not find any clear relationship between women's labor force participation and their mothers' engagement in child care.

The result has two implications. First, it provides further support for the hypothesis that grandparental child care is not pivotal for young mothers' labor force participation in the Netherlands. Second, given the prevalence of grandparental child care (Table 6), it suggests that grandparental child care is just as popular among women with weaker labor market engagement. This supports the hypothesis that grandparental child care may be important to young mothers for reasons other than enabling labor force participation. Jointly, these results help explain my findings on reduced fertility and unchanged labor market outcomes among women with reform-affected mothers.

6.3 Socioeconomic Differences

Finally, I investigate if the effect on fertility differs across socioeconomic categories. I select women who were working before the reform. Then, I separate women who were earning less

than the median and those who were earning more.[25] Figure 9 presents the discontinuity point estimates. The effect on fertility is around three times larger in the lower earnings group. The finding is consistent with previous research which suggests that grandparental child care is more popular among lower income women (Fergusson et al., 2008). The result also indicates that a disproportionate share of the reform's burden might be born by lower socioeconomic status families.

7 Conclusion

I have shown that a reform designed to delay retirement reduced fertility in the subsequent generation. The reform induced directly impacted individuals to delay retirement by less than half a year on average. Meanwhile, it reduced fertility in the subsequent generation by approximately 1 child per 80 women. The reduction is likely permanent. It operates on both extensive and intensive margins. It is concentrated among lower-income women living close to their mothers. My results suggest that the effect runs through a grandparental child care reduction. I found no effects running through women's fathers and parents-in-law. In addition, I found no effects on female labor market outcomes. I believe that potential labor market effects are limited by the incidental nature of grandparental child care and generous formal child care subsidies linked to hours spent at work.

Above all, my results are important from a policy perspective. Reforms aiming to delay retirement are a popular tool for balancing pension systems in aging societies. However, my findings suggest that such reforms might undermine pension system sustainability in the long run. Specifically, they may decrease the size of the labor force through a fertility reduction in the subsequent generation. The potential negative effects on fertility should be accounted for when considering the effectiveness of retirement delays in the long run. My results also suggest that policymakers might be able to boost fertility by allowing women to retire earlier or by encouraging men to provide more grandparental child care.

[25] The results are similar when I include all women independent of their employment status or when I split the sample by couple's income. I cannot estimate an informative separate effect for unemployed women because less than 2,000 women in my sample were not working before the reform.

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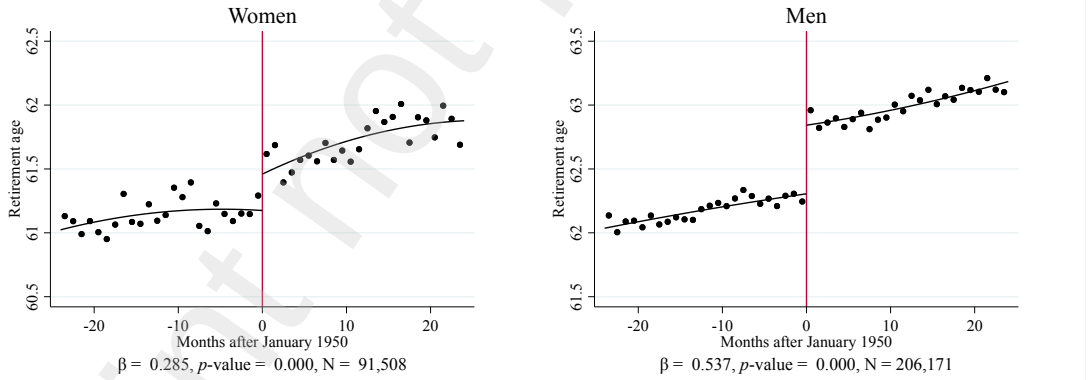
Appendices

Appendix A

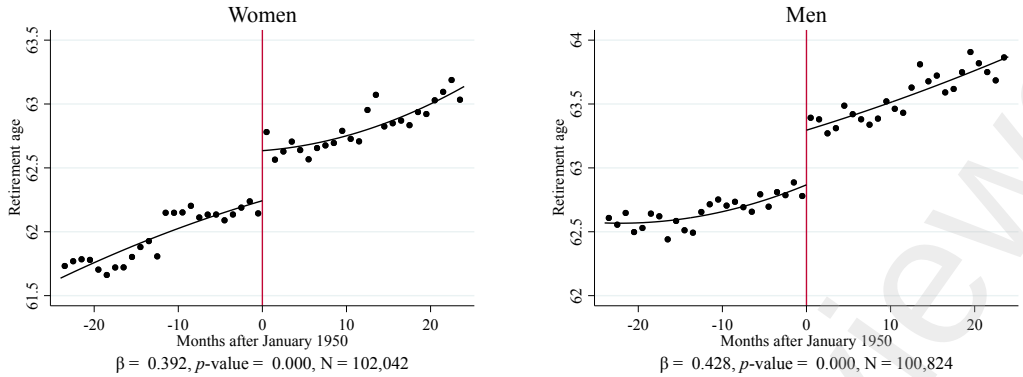
I present how the effect of the reform differs between those who were employed in the private sector and those who were employed in the public sector. Separating these sectors is useful for two reasons. First, they are comparable in size. Second, all public sector pensions are managed by one fund (*Stichting Pensioenfonds ABP*). I define being employed in the public sector as having at least one employment contract in the public sector on the 53rd birthday. I define employment in the private sector as having only private sector employment on the 53rd birthday. These definitions ensure that estimates for the private sector employees are not affected by the sectoral pension agreement renegotiations in the public sector. Despite sector-specific differences, the reform had comparable effects on the retirement age in both sectors. Figures A1 and A2 present the results for private and public sector employees respectively. The resulting delay is around 0.31 and 0.4 years for women employed in private and public sectors respectively. The delay is around 0.54 and 0.46 years for men employed in private and public sectors respectively. While public sector employees faced slightly larger retirement delays (potentially because of lower retirement ages in the private sector), the effects on the two groups are comparable.

Figure A1: Retirement age for individuals employed in the private sector on 53rd birthday



Note: Scatter plots for the means of retirement age censoring at 53 and 67. Point estimates refer to regression discontinuity effect estimates from a flexible quadratic specification. Sample covers the full population of Dutch born individuals living in the Netherlands, born within the selected bandwidth, and employed in the private sector on their 53rd birthday (excluding those employed in the public sector on their 53rd birthday).

Figure A2: Retirement age for individuals employed in the public sector on 53rd birthday



Note: Scatter plots for the means of retirement age censoring at 53 and 67. Point estimates refer to regression discontinuity effect estimates from a flexible quadratic specification. Sample covers the full population of Dutch born individuals living in the Netherlands, born within the selected bandwidth, and employed in the public sector on their 53rd birthday.

Appendix B

Table B1: Balance table for women in relation to their fathers' birth dates

	Exempted mean	Affected mean	Cond. p-val.
Background			
Age 2004	26.47	25.90	0.384
Partner's age 2004	29.12	28.65	0.903
Relationship duration	2.76	2.55	0.742
Other unaffected parents	1.532	1.342	0.708
Oldest parent	0.231	0.200	0.758
Distance to parent (km)	24.00	22.86	0.562
Higher education	0.362	0.338	0.464
Labor market outcomes in 2004			
Working	0.954	0.952	0.145
Income (1,000s of euros)	21.23	20.36	0.678
Couple's income (1,000s of euros)	48.25	46.78	0.946
Parent's income (1,000s of euros)	33.64	33.82	0.578
Parent working	0.799	0.807	0.401
Parent working in public sector	0.252	0.249	0.481

Comparison of females with a selected parent(-in-law) born up to 12 months before 1950 (exempted group) and females with a selected parent(-in-law) born up to 12 months after 1949 (affected group). p-values for regression discontinuity point estimates from a from a flexible linear specification using a 12-month bandwidth. Relationship duration is the number of year a couple had spent living together in 2004 since 1996. Other unaffected parents is the number of parents(-in-law) born before 1950 and alive in 2004 excluding the one in question. Oldest parent takes value 1 if the selected parent is the oldest parent(-in-law). Distance to parent (km) is the distance in a straight line between the household of the couple and the household of the parent in question in 2004. Higher education takes value 1 if the female has higher education. Income measures capture total employment and self-employment income in 2004. Work measures capture if the individual has non-zero employment income N = 35,338, cluster = 32,091 (N = 34,951, cluster = 31,740 for distance).

Table B2: Balance table for women in relation to their partners mothers' birth dates

	Exempted mean	Affected mean	Cond. p-val.
Background			
Age 2004	26.84	26.34	0.022
Partner's age 2004	28.83	28.25	0.785
Relationship duration	2.82	2.61	0.669
Other unaffected parents	1.861	1.687	0.744
Oldest parent	0.077	0.066	0.375
Distance to parent (km)	21.75	20.30	0.305
Higher education	0.359	0.343	0.426
Labor market outcomes in 2004			
Working	0.953	0.954	0.568
Income (1,000s of euros)	21.31	20.79	0.395
Couple's income (1,000s of euros)	48.75	47.41	0.172
Parent's income (1,000s of euros)	7.95	8.47	0.454
Parent working	0.478	0.511	0.316
Parent working in public sector	0.236	0.253	0.030

Comparison of females with a selected parent(-in-law) born up to 12 months before 1950 (exempted group) and females with a selected parent(-in-law) born up to 12 months after 1949 (affected group). p-values for regression discontinuity point estimates from a from a flexible linear specification using a 12-month bandwidth. Relationship duration is the number of year a couple had spent living together in 2004 since 1996. Other unaffected parents is the number of parents(-in-law) born before 1950 and alive in 2004 excluding the one in question. Oldest parent takes value 1 if the selected parent is the oldest parent(-in-law). Distance to parent (km) is the distance in a straight line between the household of the couple and the household of the parent in question in 2004. Higher education takes value 1 if the female has higher education. Income measures capture total employment and self-employment income in 2004. Work measures capture if the individual has non-zero employment income N = 34,670, cluster = 31,621 (N = 34,424, cluster = 31,393 for distance).

Table B3: Balance table for women in relation to their partners fathers' birth dates

	Exempted mean	Affected mean	Cond. p-val.
Background			
Age 2004	25.92	25.46	0.691
Partner's age 2004	27.66	27.06	0.356
Relationship duration	2.41	2.19	0.865
Other unaffected parents	1.149	0.946	0.037
Oldest parent	0.393	0.358	0.170
Distance to parent (km)	21.32	20.78	0.099
Higher education	0.332	0.308	0.336
Labor market outcomes in 2004			
Working	0.957	0.957	0.461
Income (1,000s of euros)	20.35	19.49	0.754
Couple's income (1,000s of euros)	46.33	44.55	0.421
Parent's income (1,000s of euros)	33.28	33.61	0.326
Parent working	0.794	0.801	0.179
Parent working in public sector	0.245	0.247	0.515

Comparison of females with a selected parent(-in-law) born up to 12 months before 1950 (exempted group) and females with a selected parent(-in-law) born up to 12 months after 1949 (affected group). p-values for regression discontinuity point estimates from a from a flexible linear specification using a 12-month bandwidth. Relationship duration is the number of year a couple had spent living together in 2004 since 1996. Other unaffected parents is the number of parents(-in-law) born before 1950 and alive in 2004 excluding the one in question. Oldest parent takes value 1 if the selected parent is the oldest parent(-in-law). Distance to parent (km) is the distance in a straight line between the household of the couple and the household of the parent in question in 2004. Higher education takes value 1 if the female has higher education. Income measures capture total employment and self-employment income in 2004. Work measures capture if the individual has non-zero employment income N = 29,927, cluster = 27,460 (N = 29,610, cluster = 27,160 for distance).

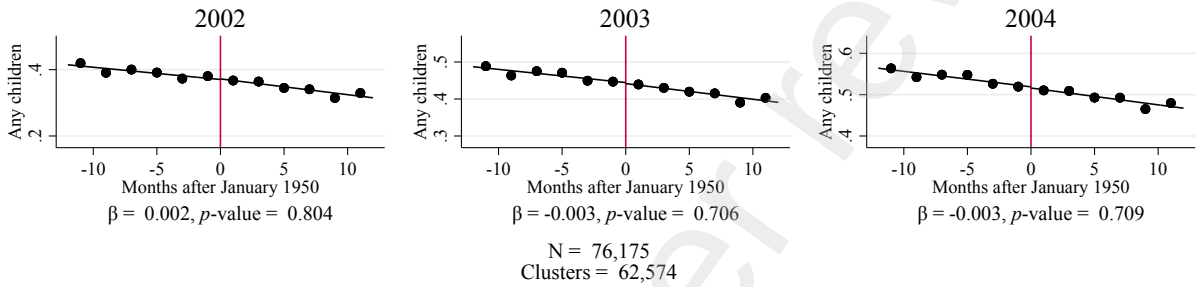
In my main analysis, I focus on couples who did not have children before 2005. First, because I am interested in both intensive and extensive fertility margins. Second, because I expect that couples with older children were not affected (much) by the reform. The majority of women with children born before 2005 had completed their fertility a while before the reform had material effects on retirement. Thus, there was little room for the reform to affect the fertility of these women. Moreover, since very few of these women had young children by the time that the reform affected retirement, it is unlikely that grandparental help was pivotal for the labor force participation decision of these women. My results are consistent with this explanation: I find no effects on fertility and labor market outcomes of women who had already had children before the reform (not reported).

To ensure that my results are not affected by the selection of childless women, I check for differences in the pre-reform fertility trends between couples with affected and unaf-

fected parents. Figure B1 plots the probability of having any children by female's mother's birth month for years 2002 through 2005 (the selection margin). The discontinuity point estimates are close to zero. Figures B2 through B4 plot the results for women's fathers and parents-in-law. Figures B5 through B8 repeat this for the intensive fertility margin. I find no differences that might affect my results.

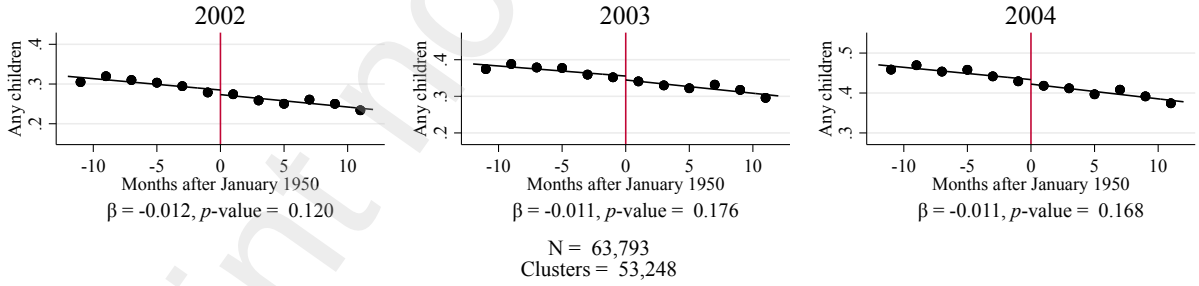
I also test for discontinuities in the means of observable pre-reform characteristics in a sample that includes women who had children before the reform. Tables B4 through B7 present the results.

Figure B1: Any children in years 2002-2004 in relation to women's mothers' birth dates, incl. those with children before the reform



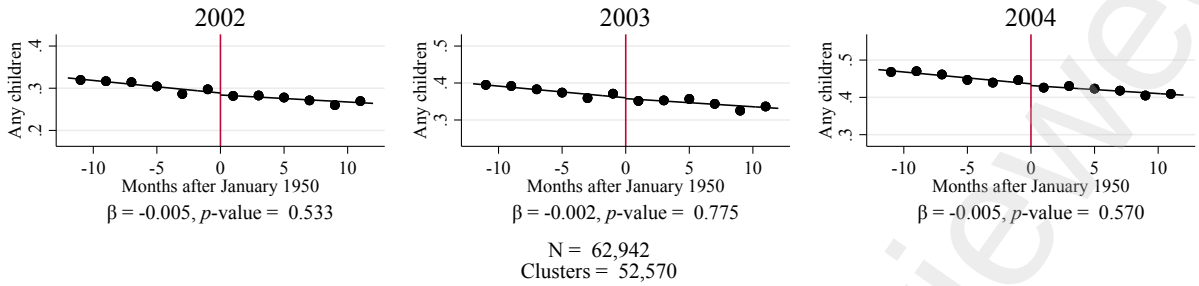
Note: Scatter plots for the mean of a variable taking value one if the woman has any children by birth month of the selected parent(-in-law). Two month bins. Point estimates refer to regression discontinuity effect estimates from a flexible linear specification.

Figure B2: Any children in years 2002-2004 in relation to women's fathers' birth dates, incl. those with children before the reform



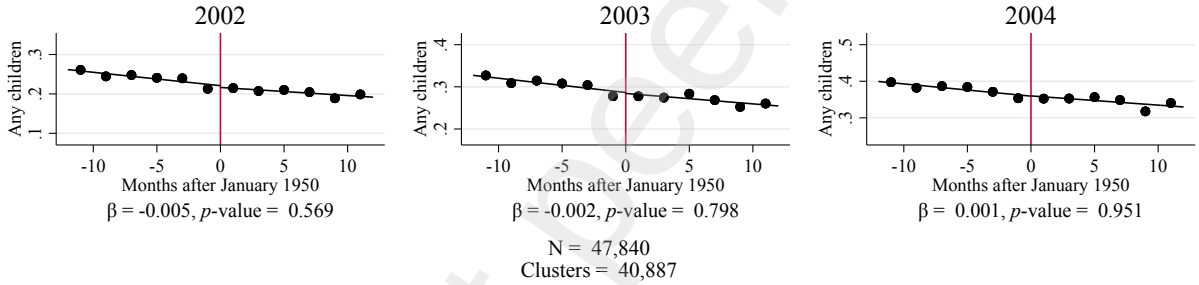
Note: Scatter plots for the mean of a variable taking value one if the woman has any children by birth month of the selected parent(-in-law). Two month bins. Point estimates refer to regression discontinuity effect estimates from a flexible linear specification.

Figure B3: Any children in years 2002-2004 in relation to partners' mothers' birth dates, incl. those with children before the reform



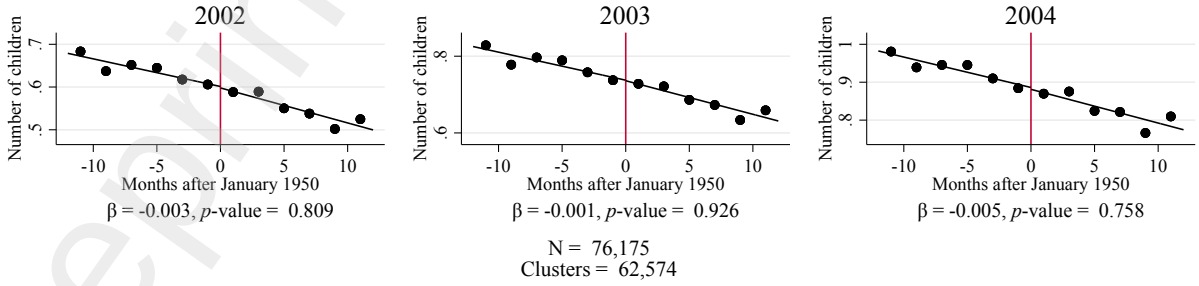
Note: Scatter plots for the mean of a variable taking value one if the woman has any children by birth month of the selected parent(-in-law). Two month bins. Point estimates refer to regression discontinuity effect estimates from a flexible linear specification.

Figure B4: Any children in years 2002-2004 in relation to partners' fathers' birth dates, incl. those with children before the reform



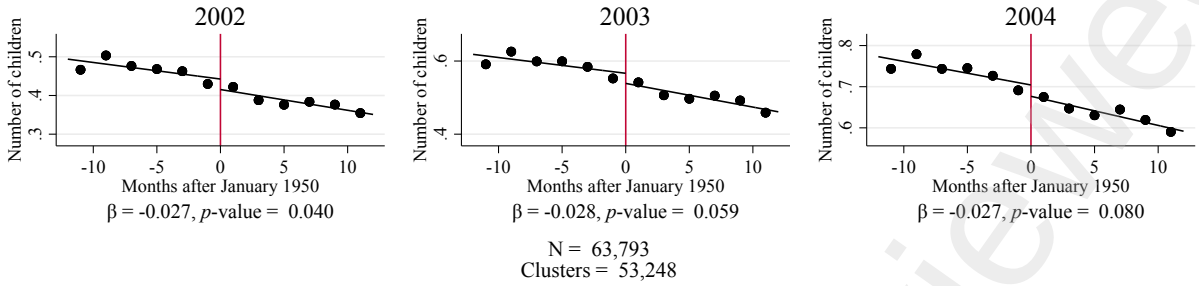
Note: Scatter plots for the mean of a variable taking value one if the woman has any children by birth month of the selected parent(-in-law). Two month bins. Point estimates refer to regression discontinuity effect estimates from a flexible linear specification.

Figure B5: Fertility in years 2002-2004 in relation to women's mothers' birth dates, incl. those with children before the reform



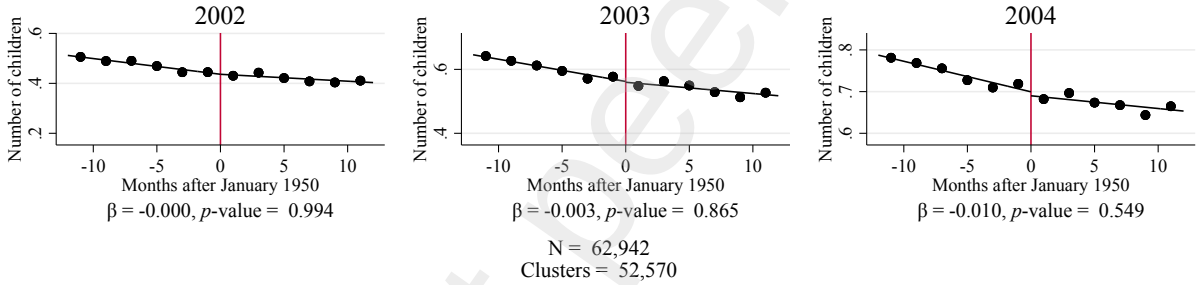
Note: Scatter plots for the mean of total number of children by birth month of the selected parent(-in-law). Two month bins. Point estimates refer to regression discontinuity effect estimates from a flexible linear specification.

Figure B6: Fertility in years 2002-2004 in relation to women's fathers', incl. those with children before the reform



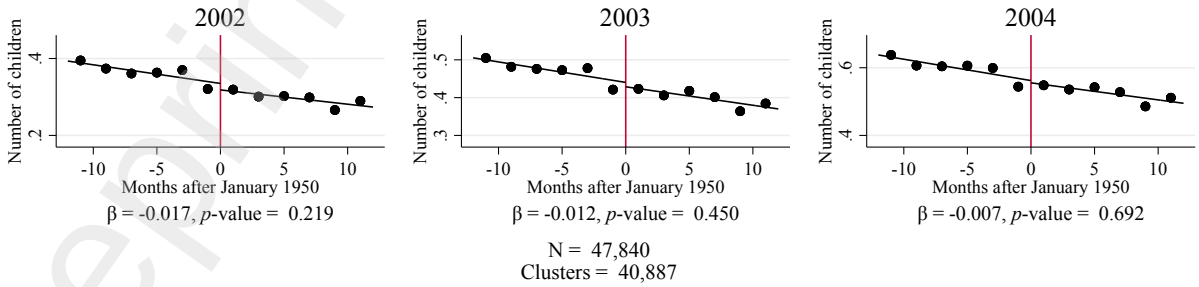
Note: Scatter plots for the mean of total number of children by birth month of the selected parent(-in-law). Two month bins. Point estimates refer to regression discontinuity effect estimates from a flexible linear specification.

Figure B7: Fertility in years 2002-2004 in relation to partners' mothers' birth dates, incl. those with children before the reform



Note: Scatter plots for the mean of total number of children by birth month of the selected parent(-in-law). Two month bins. Point estimates refer to regression discontinuity effect estimates from a flexible linear specification.

Figure B8: Fertility in years 2002-2004 in relation to partners' fathers' birth dates, incl. those with children before the reform



Note: Scatter plots for the mean of total number of children by birth month of the selected parent(-in-law). Two month bins. Point estimates refer to regression discontinuity effect estimates from a flexible linear specification.

Table B4: Balance table for women in relation to their mothers' birth dates, incl. those with children before reform

	Exempted mean	Affected mean	Cond. p-val.
Background			
Children>0 in 2004	0.542	0.492	0.709
Children in 2004	0.936	0.829	0.758
Age 2004	29.62	28.81	0.610
Partner's age 2004	32.31	31.60	0.641
Relationship duration	5.28	4.84	0.660
Other unaffected parents	2.368	2.223	0.900
Oldest parent	0.033	0.025	0.758
Distance to parent (km)	20.79	20.68	0.046
Higher education	0.278	0.275	0.711
Labor market outcomes in 2004			
Working	0.844	0.856	0.215
Income (1,000s of euros)	16.76	16.75	0.240
Couple's income (1,000s of euros)	46.35	45.65	0.854
Parent's income (1,000s of euros)	7.05	7.69	0.555
Parent working	0.449	0.482	0.980
Parent working in public sector	0.216	0.232	0.755

Comparison of females with a selected parent(-in-law) born up to 12 months before 1950 (exempted group) and females with a selected parent(-in-law) born up to 12 months after 1949 (affected group). p-values for regression discontinuity point estimates from a from a flexible linear specification using a 12-month bandwidth. Relationship duration is the number of year a couple had spent living together in 2004 since 1996. Other unaffected parents is the number of parents(-in-law) born before 1950 and alive in 2004 excluding the one in question. Oldest parent takes value 1 if the selected parent is the oldest parent(-in-law). Distance to parent (km) is the distance in a straight line between the household of the couple and the household of the parent in question in 2004. Higher education takes value 1 if the female has higher education. Income measures capture total employment and self-employment income in 2004. Work measures capture if the individual has non-zero employment income N = 76,175, cluster = 62,574 (N = 75,654, cluster = 62,142 for distance).

Table B5: Balance table for women in relation to their fathers' birth dates, incl. those with children before reform

	Exempted mean	Affected mean	Cond. p-val.
Background			
Children>0 in 2004	0.453	0.401	0.168
Children in 2004	0.740	0.636	0.080
Age 2004	28.01	27.20	0.917
Partner's age 2004	30.90	30.18	0.700
Relationship duration	4.40	3.94	0.760
Other unaffected parents	1.670	1.454	0.292
Oldest parent	0.173	0.156	0.775
Distance to parent (km)	21.19	20.60	0.885
Higher education	0.270	0.261	0.565
Labor market outcomes in 2004			
Working	0.865	0.878	0.610
Income (1,000s of euros)	16.76	16.74	0.260
Couple's income (1,000s of euros)	44.74	43.99	0.174
Parent's income (1,000s of euros)	31.13	31.59	0.840
Parent working	0.776	0.785	0.175
Parent working in public sector	0.233	0.233	0.275

Comparison of females with a selected parent(-in-law) born up to 12 months before 1950 (exempted group) and females with a selected parent(-in-law) born up to 12 months after 1949 (affected group). p-values for regression discontinuity point estimates from a from a flexible linear specification using a 12-month bandwidth. Relationship duration is the number of year a couple had spent living together in 2004 since 1996. Other unaffected parents is the number of parents(-in-law) born before 1950 and alive in 2004 excluding the one in question. Oldest parent takes value 1 if the selected parent is the oldest parent(-in-law). Distance to parent (km) is the distance in a straight line between the household of the couple and the household of the parent in question in 2004. Higher education takes value 1 if the female has higher education. Income measures capture total employment and self-employment income in 2004. Work measures capture if the individual has non-zero employment income N = 63,793, cluster = 53,248 (N = 63,095, cluster = 52,653 for distance).

Table B6: Balance table for women in relation to their partners' mothers birth dates, incl. those with children before reform

	Exempted mean	Affected mean	Cond. p-val.
Background			
Children>0 in 2004	0.456	0.419	0.570
Children in 2004	0.745	0.672	0.549
Age 2004	28.64	27.98	0.046
Partner's age 2004	30.23	29.49	0.121
Relationship duration	4.45	4.07	0.368
Other unaffected parents	2.030	1.845	0.879
Oldest parent	0.057	0.050	0.754
Distance to parent (km)	19.36	18.41	0.421
Higher education	0.272	0.263	0.570
Labor market outcomes in 2004			
Working	0.870	0.877	0.533
Income (1,000s of euros)	17.06	16.94	0.379
Couple's income (1,000s of euros)	45.23	44.26	0.350
Parent's income (1,000s of euros)	7.03	7.52	0.843
Parent working	0.442	0.475	0.488
Parent working in public sector	0.208	0.223	0.112

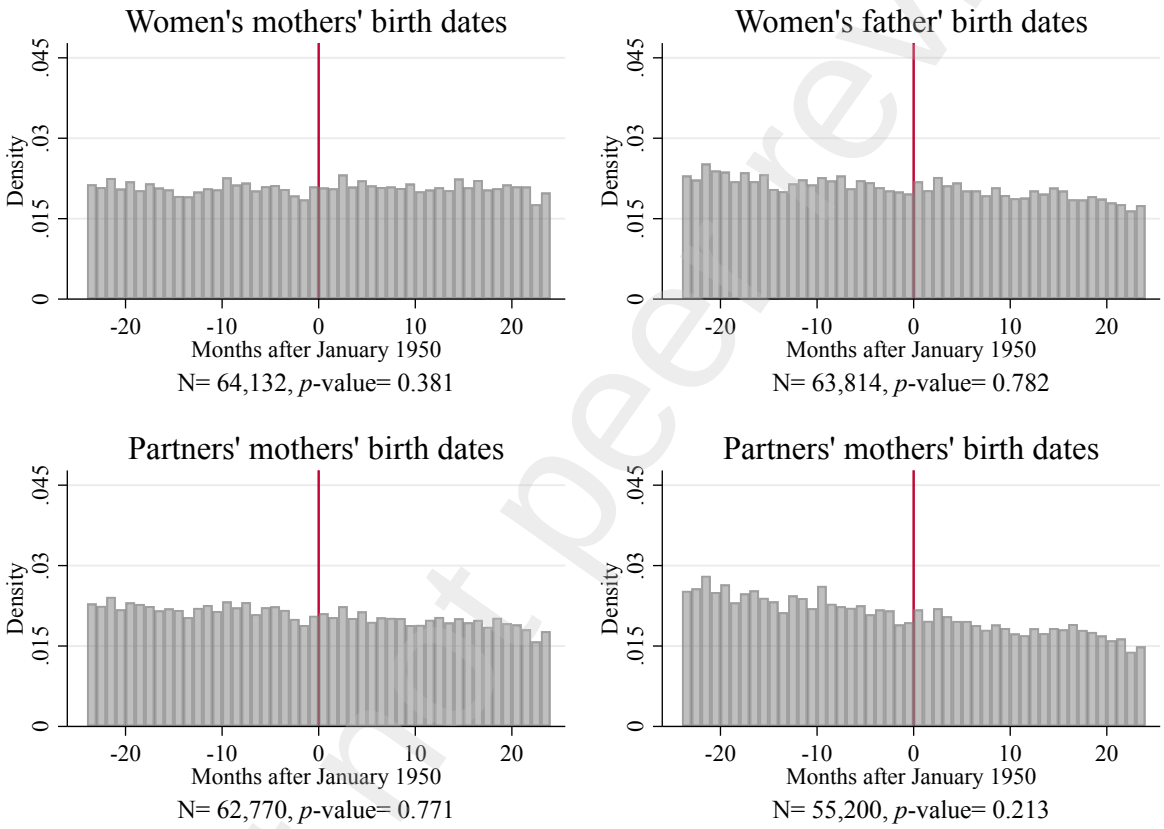
Comparison of females with a selected parent(-in-law) born up to 12 months before 1950 (exempted group) and females with a selected parent(-in-law) born up to 12 months after 1949 (affected group). p-values for regression discontinuity point estimates from a from a flexible linear specification using a 12-month bandwidth. Relationship duration is the number of year a couple had spent living together in 2004 since 1996. Other unaffected parents is the number of parents(-in-law) born before 1950 and alive in 2004 excluding the one in question. Oldest parent takes value 1 if the selected parent is the oldest parent(-in-law). Distance to parent (km) is the distance in a straight line between the household of the couple and the household of the parent in question in 2004. Higher education takes value 1 if the female has higher education. Income measures capture total employment and self-employment income in 2004. Work measures capture if the individual has non-zero employment income N = 62,942, cluster = 52,570 (N = 62,499, cluster = 52,190 for distance).

Table B7: Balance table for women in relation to their partners' fathers birth dates, incl. those with children before reform

	Exempted mean	Affected mean	Cond. p-val.
Background			
Children>0 in 2004	0.380	0.346	0.951
Children in 2004	0.602	0.527	0.692
Age 2004	27.39	26.71	0.720
Partner's age 2004	28.79	28.03	0.103
Relationship duration	3.71	3.30	0.171
Other unaffected parents	1.292	1.057	0.181
Oldest parent	0.337	0.313	0.504
Distance to parent (km)	19.64	19.30	0.367
Higher education	0.263	0.251	0.222
Labor market outcomes in 2004			
Working	0.885	0.891	0.716
Income (1,000s of euros)	17.01	16.63	0.589
Couple's income (1,000s of euros)	43.58	42.26	0.222
Parent's income (1,000s of euros)	31.28	31.73	0.116
Parent working	0.773	0.783	0.576
Parent working in public sector	0.228	0.235	0.173

Comparison of females with a selected parent(-in-law) born up to 12 months before 1950 (exempted group) and females with a selected parent(-in-law) born up to 12 months after 1949 (affected group). p-values for regression discontinuity point estimates from a from a flexible linear specification using a 12-month bandwidth. Relationship duration is the number of year a couple had spent living together in 2004 since 1996. Other unaffected parents is the number of parents(-in-law) born before 1950 and alive in 2004 excluding the one in question. Oldest parent takes value 1 if the selected parent is the oldest parent(-in-law). Distance to parent (km) is the distance in a straight line between the household of the couple and the household of the parent in question in 2004. Higher education takes value 1 if the female has higher education. Income measures capture total employment and self-employment income in 2004. Work measures capture if the individual has non-zero employment income N = 47,840, cluster = 40,887 (N = 47,317, cluster = 40,431 for distance).

Figure B9: Distributions of birth dates for women's parents and parents-in-law

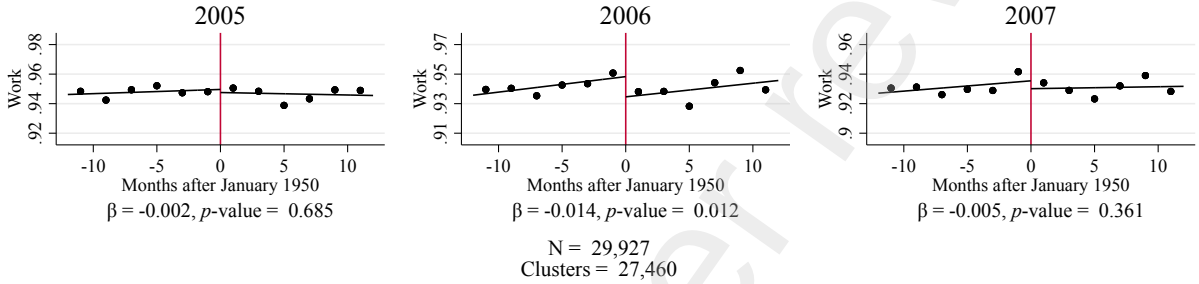


Note: Distributions of birth dates for women's parents and parents-in-law in my main sample. p-values from a test for manipulation of the running variable following Cattaneo et al. (2020) implemented with default parameters described in Cattaneo et al. (2018). 36 moth bandwidth used for model selection.

Appendix C

Here I present additional analyses of my labor market effects. Results from my main specification indicate a negative effect on female labor force participation in the year of the reform that runs through partners' fathers. Figure C1 presents the probability of a woman having non-zero labor income in years 2005, 2006, and 2007 by their partner's father's birth month. The result for 2006 appears to be driven by outliers. The estimated effect is not robust to changes in the bandwidth or the running variable function. The results for other labor market outcomes are similar (not reported).

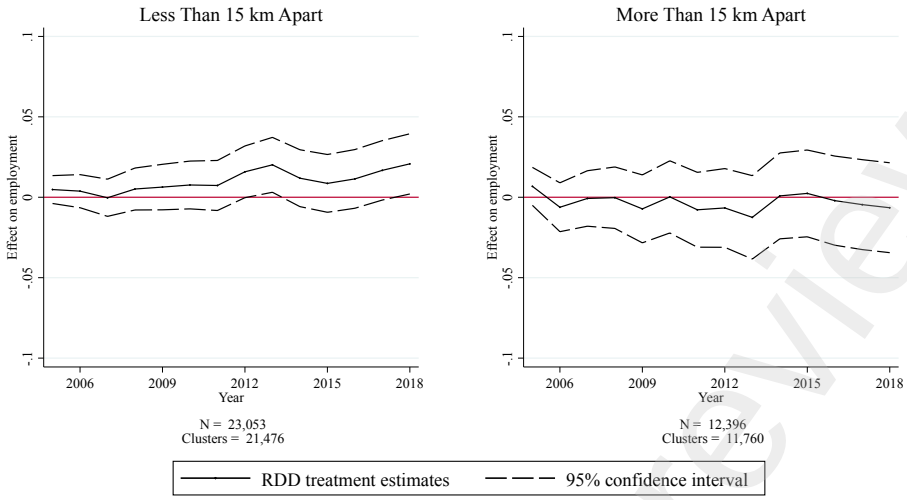
Figure C1: Working in 2005-2007 in relation to partners' fathers' birth dates



Note: Scatter plots for the probability of working by birth month of the selected parent. Two month bins. Point estimates refer to regression discontinuity effect estimates from a flexible linear specification.

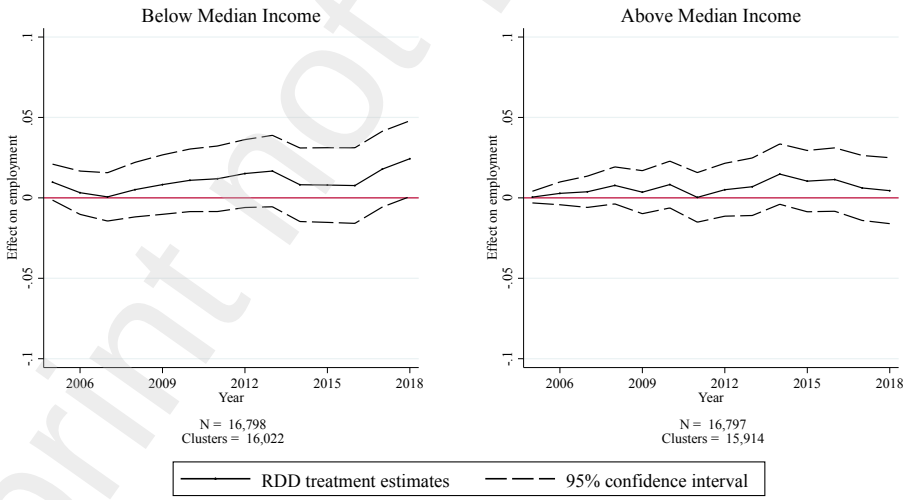
In my main analysis, I test for heterogeneity in the effect on labor force participation in women's age. Here I test for heterogeneity in the effect on employment in other dimensions in which I found heterogeneous fertility effects. Figure C2 presents the effect on working after I split the sample into women who lived within 15 kilometers of their mothers (in 2004) and those who lived further away. Figure C3 presents the effect on working after I split the sample into women who were working and earning less than the median (in 2004) and those who working and were earning more. I find no heterogeneity in the effect on working in either dimension. Notably, as in the main analysis, I find no labor market effects among women who did not reduce their fertility. This provides further support that my result of no labor market effects is not driven by an offsetting effect that runs through reduced fertility.

Figure C2: Effect on employment through women's mothers, heterogeneity by distance between households



Note: Regression discontinuity effect estimates from a flexible linear specification. 12-month bandwidth. Sample split into women who were living closer than 15 kilometers to their mothers in 2004 and women who were living further away. Controls include quadratic functions of woman's and their partner's age, quadratic functions of woman's and their partner's labor incomes in 2004, dummies for having non-zero labor income in 2004 for the woman and their partner, dummies for the number of years the couple had spent cohabiting before the reform and the number of woman's parents(-in-law) exempted from the reform (excluding the one we study in this specification). Standard errors clustered at the parent level.

Figure C3: Effect on employment through women's mothers, heterogeneity by women's income



Note: Regression discontinuity effect estimates from a flexible linear specification. 12-month bandwidth. Sample split into women who were earning less than the median in 2004 and women who were earning more. Controls include quadratic functions of woman's and their partner's age, quadratic functions of woman's and their partner's labor incomes in 2004, dummies for having non-zero labor income in 2004 for the woman and their partner, dummies for the number of years the couple had spent cohabiting before the reform and the number of woman's parents(-in-law) exempted from the reform (excluding the one we study in this specification). Standard errors clustered at the parent level.

Appendix D

Here I discuss the measure I construct to estimate the effect of the reform on individuals' lifetime income. Income data that is sufficiently detailed to compare individuals subject to different pension systems and (as a result) with different labor market profiles is only available from 2011 onward. Nonetheless, I believe that my measure can provide valuable insight on the effect of the reform because it covers the period for which I expect the largest effect on income: the years when most of those born before January 1, 1950 retired while those born from January 1, 1950 onward delayed retirement.

Since my goal is to capture income that could potentially be transferred to the subsequent generation, I build the most comprehensive measure of such income possible. Notably, my measure includes expenses that might be ignored when computing disposable income (such as private insurance premiums) and also includes components that are not directly linked to retirement but that might be affected by other aspects of individuals' income or labor supply decisions (for example, rental allowance). Specifically, I add up employment income, unemployment benefits, redundancy pay, sickness benefits, disability benefits, benefits from private insurances for medical expenses and incapacity to work, old-age pension benefits, survivor pension benefits, private pension benefits, reimbursements of income insurance premiums, withdrawals from the Life-course Savings account, work and social assistance benefits, reimbursements of healthcare insurance premiums, rent allowance, and miscellaneous benefits. Then, I subtract income taxes, premiums for private illness/disability, incapacity to work, and old-age insurances, national old-age and survivor pension premiums, deposits into the Life-course Savings account, health insurance premiums, national exceptional medical expenses insurance premiums, unemployment insurance premiums, and private pension premiums.