

# Spillover Effects of Old-Age Pension Across Generations: Family Labor Supply and Child Outcomes\*

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## Abstract

This paper investigates the importance of spillover effects of public policies across generations. We study the impact of grandparental retirement decisions on family members' labor supply and child outcomes by exploiting a reform in early retirement incentives in the Netherlands. This reform has led to a sharp discontinuous increase in labor supply for cohorts born since 1950 allowing us to causally estimate spillover effects using a fuzzy Regression Discontinuity design and administrative data for the Dutch population. In terms of short-run spillovers, we find that one additional hour worked per month by grandmothers causes mothers to work around half an hour less per month. This effect is strongest for adult daughters with children aged between 4 and 7. We do not find effects on adult daughters without children, with children older than age 12, or on sons or daughters-in-law. While grandfathers' labor supply responds strongly to the reform, we do not find spillover effects on their family members. Investigating long-run spillovers, we find positive test score effects on children aged 4 to 7 who experience a substitution from grandparental to maternal care, and negative test score effects on children aged 11 to 12 who experience a substitution from grandparental to formal childcare. Lastly, we show important long-run impacts on maternal labor supply and a substantial increase in the child penalty.

**JEL Codes:** J13, J22, J26, I38, D64

**Keywords:** spillover effects, retirement, grandparental childcare, maternal labor supply, child development

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# 1 Introduction

Evaluating the overall impact of public policies is complex, given a myriad of potential spillover effects, both within and across generations. Among the most important public policy changes in recent years have been changes in early retirement policies and policies related to (female) labor supply. Many OECD countries have reformed their public pension systems to reduce the incentive for early retirement and increase labor supply in old age. Recent evidence shows that such policies can have indirect effects on spouses/partners, which tends to exacerbate the direct labor supply response of the elderly generation.<sup>1</sup>

However, little is known about the cross-generational spillover effects of the old-age pension. A delay in the timing of grandparents' retirement might lead to important spillover effects across generations, because of a resulting reduction in grandparental childcare. In fact, grandparents play an essential role in childcare in many countries.<sup>2</sup> Therefore, pension reforms that increase grandparents' age of retirement might lead to a decrease in the labor supply of their adult children (in particular of daughters) and/or an increase in childcare costs. Such intergenerational spillovers might thereby counteract or even reverse the direct effects on overall labor supply. They might even have important consequences for the long-run labor market earnings of mothers, and for the implied child penalty and gender wage gap within a society. Lastly, the spillover effects might impact long-run outcomes of (grand)children due to changes in childcare modes. Despite these critical implications at the individual and societal levels, the importance of such spillover effects across generations is largely understudied.

We aim to fill this gap by investigating the importance of multigenerational spillover effects of retirement and labor supply decisions, exploiting pension reform-induced variations in the retirement age in the Netherlands. In particular, in 2006, the Dutch government abolished the tax-favored voluntary early retirement schemes for employees born in 1950 or later, which had strong early retirement incentives for the earlier cohorts. [Lindeboom and Montizaan \(2020\)](#) examine the direct effect of the same pension reform on retirement expectations. They show that the affected workers born since 1950 face a substantial loss in pension wealth and expect to work longer, validating our design. Using administrative data covering the universe of the Dutch population<sup>3</sup> and a fuzzy Regression Discontinuity (RD) design, we provide the first evidence of the spillover effects of such

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<sup>1</sup>See, for example, [Coile and Gruber \(2007\)](#), [Mastrobuoni \(2009\)](#), [Manoli and Weber \(2016\)](#), [Blundell et al. \(2016\)](#), for recent evidence on the direct effects of recent pension reforms and see, for example, [Hurd \(1990\)](#), [Coile \(2004\)](#), [Stancanelli and Van Soest \(2012\)](#), [Lalive and Parrotta \(2017\)](#) for indirect effects on spouses/partners.

<sup>2</sup>In most OECD countries, more than 45% of grandparents take care of at least one grandchild (see [OECD \(2012\)](#)). In the Netherlands (studied in this paper) this fraction is 60%. For children aged 4 to 12 who attend primary school, 20% of parents rely solely on grandparental childcare (see Section 2.4).

<sup>3</sup>We use non-public microdata from Statistics Netherlands, which –under certain conditions– are accessible for statistical and scientific research. For further information: [microdata@cbs.nl](mailto:microdata@cbs.nl).

a reform across household members and generations. We thereby shed light on the relevance of intergenerational spillover effects in terms of labor supply decisions and of the multigenerational effects of grandparents' labor supply decisions on children's educational outcomes.

First, we show that the pension reform has led to an important increase in grandparents' labor supply. We find that grandmothers increase their total hours worked (including zeros) between age 60 and 64 by 6.1 hours per month (equivalent to 18 percent), while their likelihood of still being employed increases by six percentage points (14 percent). While grandfathers are not the main focus of our analysis, we find that their hours worked per month increases by 26 hours (45 percent), and that their likelihood of being employed increases by 14 percentage points (32 percent). Throughout the analysis, we primarily focus on the total number of hours worked (including zeros) as a measure of grandparents' time availability, since it simultaneously captures the extensive and intensive margins of labor supply.

In our main analysis we employ a fuzzy RD design to investigate the importance of intergenerational labor supply spillovers. In particular, we use the first-stage reform estimate to instrument for the labor supply decision of the elderly generation affected by the pension reform. In terms of short-run labor supply spillovers, we find that one additional hour worked per month by grandmothers causes mothers to work around half an hour less. This result is robust to the inclusion of controls and sector fixed effects as well as to variation of the bandwidth and the usual validity tests provide supporting evidence for the necessary RD assumptions. While the reform has important direct effects on grandfathers' labor supply, we do not find spillover effects on their adult children.

To investigate the underlying mechanisms and to paint a more complete picture of who is affected by labor supply spillovers, we first show that the effects we find are linked to childcare needs and a reduction in grandmothers' childcare availability. In particular, we find no effect on adult daughters without children or with children older than 12. Instead, we find that results are driven by mothers with their youngest child aged 4 to 12 (with the largest effect for children aged 4 to 7) when their children are in primary school (including pre-school). This is consistent with grandparental childcare being particularly important for families with children in this age group, as 20% of families with children aged 4 to 12 use grandparents as the sole caretaker (instead of daycare or after-school care), compared to less than 10% of families with a child younger than age 4.<sup>4</sup>

In support of our findings in terms of channels, we explore heterogeneity by grandmothers' proximity to their adult daughters, the health of their partners, and the number of young maternal grandchildren. Our results strongly point to the importance of intergenerational time transfers (i.e., child care provided by grandmothers), as the main channel. We rule out other channels, including money transfers and "reminder effects", by investigating the effects on other outcomes (such as

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<sup>4</sup>These figures are based on own calculations from the Dutch LISS (Longitudinal Internet studies for the Social Sciences) panel administered by CentERdata (Tilburg University, The Netherlands). Also see Section 2.4.

gross (household) income of grandparents) or groups without childcare needs.

For a more complete picture of labor supply spillovers, we also examine the impact of grandmothers' retirement on other extended family members, such as adult sons, daughters-in-law, and sons-in-law, and the impacts of grandfathers' retirement. We find that while an increase in grandmothers' labor supply decreases the labor supply of (adult) daughters with young children between ages 4 and 12, it does not have an impact on (adult) sons or daughters-in-law with children in the same age range. This is consistent with maternal grandparents being those who provide childcare in about two-thirds of cases (own calculation based on LISS data). Interestingly, the effects on adult daughters are mirrored in the effect of the opposite sign on their husbands (i.e., the sons-in-law), who increase their labor supply, with important implications in terms of intra-household inequality, while the overall effect on their household income is zero. We do not find spillover effects of grandfathers' labor supply on their adult children, despite large direct reform effects, consistent with grandmothers being more important in providing childcare.

Next, we analyze the importance of long-run spillover effects. We first show the reform impact on (grand)children's educational performance and then investigate the dynamic spillover effects on mothers' long-run labor market outcomes and on the child penalty and gender gap. As a measure of children's educational outcomes, we use their performance on a high-stake test (*Cito* test), taken at the end of primary school to determine the school track children should attend in secondary school. The school track is critical for individuals' long-run educational and labor market outcomes, since enrolling in university is only possible with a degree from the academic track. Interestingly, we find positive reform effects on educational performance for children aged 4 to 7 when their grandmothers were between age 60 and 64. These children experienced a substitution away from grandparental care towards maternal care. Effects are particularly strong for girls, but there are also positive effects on boys' verbal skills. While we do not find effects for children aged 8 to 10, we do find adverse effects for children aged 11 to 12. These effects are driven by a strong negative impact on boys, who are 6 percentage points less likely to receive a recommendation for the highest track in secondary school. For this age group, grandmothers' time availability decreases while mothers do not change their labor supply. Using supplementary data on childcare take-up, we show that after-school care substitutes for the decrease in grandparental care (the probability of taking up after-school care increases by 2 percentage points and after-school care increases by 9 hours). In addition, some children in this age group may stay at home alone without supervision for a few hours, with negative consequences for boys in particular (consistent with findings by [Aizer \(2004\)](#)). While there is no change in formal childcare for children aged 8 to 10, there appears to be a reduction in formal childcare for children aged 4 to 7. This suggests that the substitution of grandmothers' care and formal care through maternal care positively affects children and particularly girls in this age group, which is consistent with recent evidence (e.g., [Fort et al. \(2020\)](#) find

negative effects of formal childcare for girls, albeit younger in age).

Lastly, we investigate the importance of dynamic spillover effects on mothers' labor supply and earnings and reform impacts on the child penalty and gender gap. Building on the framework by [Kleven et al. \(2019a\)](#), we show that in addition to the immediate impacts, changes in grandmothers' labor supply have dynamic long-run effects on child penalties. In particular, a significant gap in mothers' hours worked and earnings opens up between mothers with (grand)mothers who are affected versus those unaffected by the reform. While mothers who have (grand)mothers unaffected by the reform start to recover about 5 or 6 years after the birth of their child, hours worked and earnings remain at the same (low) level for mothers whose (grand)mothers are affected by the reform. The reform thus magnifies the already existing child penalty and gender gaps.

Our findings have important policy implications. First, our results show that pension reforms aimed at increasing labor market attachment for the elderly generation can have unintended and critical consequences for younger generations, including on their adult daughters and grandchildren. Spillover effects of such policy reforms might counteract or even reverse direct reform effects. Following the framework proposed by [Hendren and Sprung-Keyser \(2020\)](#), we calculate the Marginal Value of Public Funds of the Dutch reform studied in this paper. Assuming that the government only cares about income tax revenue, and the impact on maternal labor supply lasts for up to eight years (as our results suggest), we show that the loss in tax revenue from the drop in maternal labor supply outweighs the gain in tax revenue from delaying the retirement of grandmothers.<sup>5</sup> Second, our findings in terms of children's educational outcomes suggest the importance of quality of care. We show that unsupervised time at home due to a lack of grandparental care and/or low-quality formal care can negatively affect the performance of children in high-stake tests, with critical long-run implications. The positive effects we find for children aged 4 to 7 underlines the importance of high-quality care for children's cognitive development. Such high-quality care options can be made possible and shared by both parents through generous parental leave policies or by improving the quality of formal care choices.

To conclude, our results show that public policies, such as pension reforms, can trigger multi-generational spillover effects with important distributional consequences. In particular, while the reform has reached the intended goal of increasing labor supply in old age, maternal labor supply in fact decreased, with critical implications for the long-run labor market outcomes of women and for the child penalty and gender gap within a society. On the other hand, children –in particular younger children– appear to have benefited from the increase in maternal care time. Our paper thereby adds to a recent literature that estimates the long-term costs and benefits of public policies and examines how they can have opposing effects for different generations (see, e.g., [Hoynes et al. \(2016a\)](#), [Bailey et al. \(2020\)](#) and [Aizer et al. \(2022\)](#) on safety net programs).

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<sup>5</sup>See Appendix C for detailed steps of the calculation.

Our paper adds to the following four strands of literature. First, it speaks directly to the scarce literature on the spillover effects of old age pension. Apart from the spillovers effects on spouses/partners (see [Hurd \(1990\)](#), [Coile \(2004\)](#), [Stancanelli and Van Soest \(2012\)](#), [Lalive and Parrotta \(2017\)](#)) and work place peers (see [Duflo and Saez \(2002\)](#), [Duflo and Saez \(2003\)](#) and [Brown and Laschever \(2012\)](#)), the literature on spillovers of such policies across generations is extremely scarce.<sup>6</sup> One notable exception is [Bratti et al. \(2018\)](#), who study the impact of grandparental availability on maternal labor force participation by exploring an Italian pension reform. Using an instrumental-variable approach, they find that mothers of children under age 15 whose own mothers are retirement eligible have a 11% higher probability of being in the labor force than those whose mothers are ineligible. Our paper goes beyond the effects on adult daughters and provides a more complete picture of intergenerational spillover effects. To the best of our knowledge, our paper is the first to causally estimate impacts on all family members' labor supply and to show evidence of changes in childcare choices and resulting effects on children's academic performance. Moreover, the high-quality Dutch data allow us to show labor supply effects beyond the extensive margin responses and to investigate longer-run effects on the child penalty and gender gaps.

Second, we contribute to the general literature studying the responses of maternal labor supply to various care provisions, such as formal childcare (e.g., [Baker et al., 2008](#); [Fitzpatrick, 2010](#); [Bauernschuster and Schlotter, 2015](#); [Baker et al., 2019](#)) and parental leave policies (e.g. [Gruber, 1994](#); [Schönberg and Ludsteck, 2014](#); [Bana et al., 2020](#); [Kleven et al., 2020](#)). Our paper provides causal evidence of strong responses to the availability of grandparental care and thereby contributes to the limited evidence available on the effects of grandparents on maternal labor supply ([Posadas and Vidal-Fernandez, 2013](#); [Bratti et al., 2018](#); [Fenoll, 2020](#)). Moreover, we provide detailed evidence on the underlying mechanisms, including an analysis of labor supply spillovers for all extended family members. Our paper also relates to the literature on peer effects in maternal labor supply decisions (see [Nicoletti et al. \(2018\)](#) on sibling spillovers and [Olivetti et al. \(2018\)](#) on peer effects of classmates' mothers). These papers do not look at spillovers of a public policy, but use the strategy of overlapping peer groups to identify peer effects. Our contribution to this literature is to provide comprehensive insights into spillover/peer effects across generations.

Third, our paper relates to the literature on parental investments, childcare choices and skill development in childhood and adolescence, which shows that maternal time is an important determinant for children's cognitive development ([Carneiro et al., 2013](#); [Del Bono et al., 2016](#); [Francesconi and Heckman, 2016](#); [Bastian and Lochner, forthcoming](#)). Studies examining extensions in paid maternity leave (usually when the child is 0-2 years old) overall find no effects on child well-being

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<sup>6</sup>Papers examining intergenerational spillovers of other types of policies are, for example, [Dahl et al. \(2014\)](#), [Aizer et al. \(2016\)](#) and [Hoynes et al. \(2016b\)](#) on intergenerational effects of welfare programs and [Black et al. \(2005\)](#) on intergenerational effects of education policies.

(Dustmann and Schönberg, 2012; Dahl et al., 2016; Danzer and Lavy, 2018), but a positive effect of a longer period of maternity leaves for children from privileged families (Danzer and Lavy, 2018; Ginja et al., 2020). Studies on the effect of formal childcare tend to find small or no effects on children's cognitive and non-cognitive skills (e.g., Cornelissen et al., 2018; Felfe and Lalive, 2018) or even negative effects on children's skills and well-being (e.g., Baker et al., 2008; Bernal, 2008; Fort et al., 2020; Baker et al., 2019), in particular for girls and/or children in more financially advantaged families. Consistent with many of these studies, we find that an increase in maternal time spent with the children has positive effects on their cognitive skills and educational performance, in particular for girls. Our paper also directly contributes to the very few studies on the impact of grandparental care, which have inconclusive findings.<sup>7</sup> In particular, we show that a substitution from formal and grandparental care to maternal care tends to have positive effects on children's educational performance, while a substitution from grandparental care to formal care and/or no adult supervision has strong negative effects particularly for boys (consistent with the findings of Aizer (2004)).

Lastly, our paper is connected to research on gender inequality in the labor market (see reviews by, e.g., Altonji and Blank (1999); Blau and Kahn (2017) and Olivetti and Petrongolo (2016)). A large body of studies show the career costs of children (Bertrand et al., 2010; Adda et al., 2017; Lundborg et al., 2017) and the large and persistent impact of children on the gender earnings gap (Daniel et al., 2013; Angelov et al., 2016; Kleven et al., 2019a,b; Cortes and Pan, 2020; Cubas et al., 2021). We contribute to this literature by evaluating the impacts of grandmothers' labor supply on child penalties. Building on the framework developed by Kleven et al. (2019a), we show that in addition to the immediate impacts, changes in grandmothers' labor supply have dynamic long-run effects on child penalties. The pension reform, which aims to prolong the working life of the older generation, has unintended consequences on adult daughters with young children. We find suggestive evidence that the lack of care support from the grandmothers slows down the recovery of earnings and working hours to pre-birth levels of the mothers.

The paper proceeds as follows: Section 2 describes the core features of the Dutch pension system, the pension reform, and details of childcare arrangements in the Netherlands. Section 3 presents the data and the empirical design. Section 4 presents the short-run spillover effects, and Section 5 discusses the mechanisms and threats to validity. Section 6 presents the long-run reform impacts on grandchildren and the dynamic implications for the gender gap and child penalties. Finally, Section 7 concludes.

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<sup>7</sup>Of the two studies that we are aware of, Del Boca et al. (2018) shows that compared with children in formal childcare, children between ages 3 and 7 cared for by their grandparents are better at naming objects but perform worse in terms of non-verbal reasoning in the UK. Zhang et al. (2021) finds that compared with parental care, grandparental care delays the achievement of children between ages 1 and 5 in China.

## 2 Institutional Setting

### 2.1 The Dutch pension system

The Dutch pension system consists of three pillars: the Pay-as-You-Go state pensions (AOW), occupational pensions, and individual savings. The first pillar, the state pensions, provide all Dutch residents aged 65 and above a flat-rate pension.<sup>8</sup> AOW benefits depend on years of residence and are not related to earnings and contributions paid before retirement.<sup>9</sup>

The second pillar, the occupational pensions, which we focus on in our analysis, are collective pension schemes connected to a specific industry or company, capital-funded, and managed by pension funds. The majority of these schemes are of the defined benefit type. Contribution to the second pillar is mandatory, and more than 90 percent of the workers in the Netherlands contribute to a collective pension fund via their employer. The contribution rate is 14% of gross wages, of which 70% is contributed by the employers and 30% by the employees. These schemes typically aim at a replacement rate of about 80% (including the AOW benefits) of average pay after 40 years of service (Bovenberg and Gradus, 2015). Retirement before the statutory AOW claiming age is only possible through the occupational pensions, which have sectoral early pension schemes as part of the collective agreements. During our sample period, the earliest possible ages to claim the occupational pensions are between ages 55 and 60, depending on their occupational group.

The third pillar consists of non-mandatory savings. It is relatively small in the Netherlands and provides around 5% of pension income.

### 2.2 Early retirement schemes and the 2006 reform

The reform we explore in this paper is the 2006 pension reform which made early retirement less attractive (see Appendix A.1 and Euwals et al. (2010) for a summary and further details on the evolution of Dutch early retirement schemes). Retiring early was and still is only possible through the early pension (ER) scheme, which is part of the occupational pension scheme. Before 2006, early retirement was subsidized through the tax system, as contributions to the ER schemes were tax-deductible. The tax advantage amounted to about 25% of the net early retirement allowance (Euwals et al., 2010). Therefore, approximately 80% of all workers retired at the age of 62 or younger before 2006 (Netherlands, 2009).

Since January 1, 2006, the tax benefits for early retirement schemes were eliminated. The goal was to encourage labor market participation of the elderly by speeding up the transition towards

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<sup>8</sup>Since 2012, the state pension claiming age was set to gradually increase, reaching 66 in 2018 and 67 in 2021. For our baseline sample of grandmothers, their state pension claiming ages are between 65 and 2 months and 65 and 3 months.

<sup>9</sup>The AOW benefits are linked to minimum wage and are financed by income taxes (OECD, 2019b).



an actuarially fair early retirement system. Two types of individuals are exempted from the new bill. First, people, who were 55 years or older before January 1, 2005 are not affected by the reform. Thus, people born before January 1950 are exempted from the changes, while those born in or after January 1950 are no longer enjoying the tax advantages as of January 1, 2006. Second, people who have claimed early retirement before 2006 are not affected by the reform. Even though the general topic of eliminating early retirement tax benefits has been discussed since 2000, the sharp differential treatment by birth date was unexpected by the public and spurred heated public debate.<sup>10</sup>

This cohort-based reform creates a sharp discontinuous drop in early retirement incentives for people born since January 1950. Figure A.1 shows the distribution of age at exiting employment for women born in 1949 and 1950. There is a clear shift towards later retirement, with most of the changes concentrated between ages 60 and 64. Therefore, the reform-induced exogenous change in early retirement incentives of the older generations, which allows us to causally estimate the impact of grandmothers' labor supply on mothers' and children's outcomes. More specifically, we employ a Regression Discontinuity Design based on grandmothers born since January 1950, as opposed to before, and compare outcomes (of grandmothers, mothers and children) when grandmothers are aged 60 to 64.

### 2.3 Child care and primary education in the Netherlands

The amount of free public childcare increases as the child ages. In the Netherlands, children aged 0 to 3 can go to center-based childcare and informal care. Childcare centers charge an hourly rate of between 6 and 8 euros on average.<sup>11</sup> From age 4 onwards, most children start primary school (mandatory at age 5) and at age 12 they go to secondary school. Primary schools are free of charge and provide around 30 to 35 hours of free care per week. The number of hours in school increases as children grow older.<sup>12</sup> School starts at around 8 am and ends at around 2 or 3 pm and at some schools finish early on Wednesday afternoons after the lunch break. In case families take the option of after-school (also called out-of-school) care (buitenschoolse opvang, OSC), which is generally provided by center-based out-of-school care providers, they need to pay

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<sup>10</sup>The reform bill No. 29760 includes a clause to adjust fiscal policy VUT and prepension (Wet voor aanpassing fiscale behandeling VUT and pre-pension) and is sometimes referred to as the 56-plus scheme (de 56-plusregeling). For further details, see Appendix A.1 .

<sup>11</sup>In the Netherlands, mothers are entitled to fully-funded maternity leave 6 weeks before and 10 weeks after childbirth. Before 2019, partners are entitled to two days of fully paid paternity leave at the time of childbirth, and they can extend this up to 5 weeks of unpaid leave. After childbirth, each parent can take up to 26 weeks of unpaid parental leave per child. The parental leave period can be taken at any time up to the 8th birthday of the child with flexibility in terms of the exact arrangement, either in blocks or several hours per week.

<sup>12</sup>According to the overview of teaching hours on the [official Dutch government website](#), which provides information on Dutch central government policy, pupils must be taught at least 3,520 hours in the first four school years (lower secondary) compared to 3,760 hours in the last four school years (senior years).

for it. Parents who do not send their children to OSC, need to arrange other types of care. A portion of the daycare and after-school care costs is reimbursable for working parents. More specifically, the Dutch Childcare Allowance reimburses part of the childcare costs for dual-earner couples and single working parents who sent their children aged 0-12 years to registered daycare and after-school care facilities and certified childminders. Depending on gross household income, around 30 to 96 percent of the costs will be reimbursed.

At age 12, pupils at the vast majority of schools participate in an aptitude test called the Cito final test primary education (Cito Eindtoets Basisonderwijs, Cito test). Performance on the Cito test is one of the key determinants of the type of secondary education (i.e. different tracks of schools, such as vocational, technical and academic) that the child can attend.

## 2.4 Grandparents and child care

Grandparents play an essential role in childcare in the Netherlands and many other countries. In the Netherlands, 60% of grandparents take care of at least one grandchild. In the majority of OECD countries, this fraction is between 45 and 55%, while Ireland has an even higher fraction of grandparents providing care (65%) (OECD, 2012). In the US in 2011, according to the Survey of Income and Program Participation, 47% of children below age 15 receive grandparent-provided child care. For 93% of pre-school children grandparents are the primary child care arrangement (Laughlin (2010) and Rupert and Zanella (2018)).

To further illustrate the importance of different childcare modes in the Netherlands, we explore the 2008 wave of the Longitudinal Internet Studies for the Social Sciences (LISS) data.<sup>13</sup> Overall, there are four types of childcare modes: parental care, grandparental care, formal (institutionalized) childcare, and informal childcare (other than grandparental care). First, according to the LISS data – and similarly to most countries – mothers spend more time in terms of child care than fathers. Even when it is conditional on both parents working, 48% of mothers with young children state that they currently work less to care for their children compared to only 8% of fathers. Conditional on working less, mothers state that they work 14 hours less per week to care for their children, while those fathers who state that they reduce their work hours to care for their children reduce their working time by 8 hours per week.

Next, in Figure 1 we show the distribution of the different types of childcare (other than parents), in particular formal care, grandparental care, and other types of informal care. Panel (a) of Figure 1 displays what fraction of parents use a particular mode of childcare (potentially in combination with other modes), while Panel (b) displays the fraction of parents using a particular combination

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<sup>13</sup>The LISS (Longitudinal Internet studies for the Social Sciences) panel is a representative sample of Dutch individuals who participate in monthly internet surveys which are administered by CentERdata (Tilburg University, The Netherlands). We use the 2008 wave because it is the wave before our sample period. For more details see section B.1.

of childcare modes (presenting the most common combinations). According to Panel (a), around 35 to 40% of parents report using grandparental care in the past week, while 60-80% of them use some formal care. The two most common care arrangements for children younger than 4 years old are paid formal care and a combination of daycare and grandparental care. According to Panel (b) of Figure 1, children aged between 4 and 12 need less childcare overall because primary school provides a considerable amount of free care. From the perspective of the paper, it is important to note that 20% of parents with children aged 4 -12 rely *solely* on grandparental care, which is only true for less than 10% in the case of younger children below age 4. Lastly, the data show that maternal grandparents are more important in terms of care giving than paternal grandparents, in that more than 60% of the care-giving grandparents are maternal grandparents.

### 3 Data and Empirical Strategy

#### 3.1 Data

We use Dutch administrative data maintained by Statistics Netherlands (Centraal Bureau voor de Statistiek, CBS), which covers the entire Dutch population and contains information that allows us to follow families across generations and over time. These records allow for unique linkages across different registers to provide information on, among other things, labor market histories and retirement outcomes and educational performance. Birth and marriage records also enables us to link several generations and create extended family networks (for more details on specific variables and data sets, see Appendix B.1.). Using birth records we link individuals born around 1950 to their two descendant generations. We refer to this “first generation” as the *grandmothers* or *grandfathers*. Their adult children, i.e. the “middle generation”, is referred to as *mothers* or *fathers*, and the “third generation” is referred to as *children*. We also analyze effects on the partners of the middle generation and refer to them as *sons-in-law* and *daughters-in-law*. Since the pension reform affects the first generation’s labor supply mainly between ages 60 and 64 (see Figure A.1), we examine the average labor market outcomes of grandparents and their adult children (i.e., mothers and fathers), as well as childcare usage, while the elderly generation is between 60 and 64. In terms of longer-run implications, we investigate the educational performance of children affected during this time and longer-run labor market dynamics of mothers.

We are able to follow the entire Dutch population (more specifically, those individuals still alive in October 1994, when official records start being available). Basic demographics such as birth date and place, household composition, and place of residence is available since 1994. While the information on labor market participation and the main source of income is available from 1994 onwards, detailed labor market histories including working hours, employment sector, and employ-

ment contract details are available annually since 2006. For the analysis of the third generation, we exploit official records of the “Cito” test results (nationwide standardized test) and data on childcare usage (both the type and hours) related to childcare subsidies, which are available annually from 2007 onward. Data availability does not represent a constraint for us, since our main sample period is between 2009 and 2015, when the grandparents born around January 1950 were between ages 60 and 64.

*Baseline sample:* In our analysis, we start with all grandmothers born between 1948 and 1951. We exclude first-generation individuals who are migrants or who do not have at least one adult daughter. We further drop grandmothers who are unlikely to be affected by the reform, that is, grandmothers who are self-employed or family workers, because they do not rely on early retirement schemes. We also drop grandmothers who exit the labor force before age 50, those who were never active in the labor market, those who claimed disability before the age of 55, and those who died before age 65. We are thus left with 62% of the 1948-1951 generation of women (for details on the sample construction, see Appendix B.2 and Table A10, which tests sample restrictions and shows that they are not affected by the reform).

Since we aim to investigate the impact of grandmothers’ labor supply on their adult daughters’ labor supply while they have young children, the baseline sample links those grandmothers to their adult daughters who have at least one young child when the grandmothers are aged 60 to 64.<sup>14</sup> To focus on maternal labor supply, we exclude mothers who are studying (less than 1%), have incomplete employment histories due to work or study abroad (around 3%), and are at the top and bottom 1% of the income distribution. We further split this sample by the age of the youngest child. Our baseline sample consists of mothers whose youngest child is of primary school age during the sample period (i.e., aged 4-12 when grandmothers are aged 60 to 64) because grandparents are particularly important as the sole source of childcare for this age group (see Section 2.4).

Lastly, for the baseline RD analysis, we keep families with grandmothers who are within the bandwidth of 8 months around January 1950.<sup>15</sup> We end up with 23,497 mothers in our baseline sample. In addition to our baseline sample, we also analyze samples of adult daughters without children, mothers with a toddler (below age 4), mothers with an adolescent (ages 13 to 18), mothers with deceased grandmothers, and other family members.

*Summary statistics:* Table 1 presents summary statistics. Columns 1 and 2 (“All”) consist of all (extended) families (who are not necessarily living together) with grandmothers born between 1948 and 1951 who are Dutch, have worked at least one month in their lives, have not claimed disability insurance before age 55, have not exited the labor market before age 50, and are still

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<sup>14</sup>28.64% of adult daughters do not have any children, while grandmothers are aged 60 to 64. We provide results for this group in Section 4. In Section 5, we also look at sons-in-law, adult sons and daughters-in-law.

<sup>15</sup>In Section 3.2, we discuss the bandwidth choice.

alive at age 65. Columns 3 and 4 (“Full sample”) restrict the sample to (extended) families with the youngest child being aged 4 to 12 when their grandmothers are aged 60 to 64. Columns 5 and 6 (“RD sample”) are the baseline analysis sample which is the full sample (of Columns 3 and 4) restricted to families with grandmothers who were born within the bandwidth of 8 months around January 1950. In the baseline RD sample, grandmothers have on average 2.5 adult children and 1.7 adult daughters, similar to the “Full sample” and the sample “All”. The mothers in this sample are on average 38 years old, entered the labor market on average at age 25, had their first child at age 28, 66% are married, and they have on average two children. While these figures are very similar to the “Full sample” (i.e., without bandwidth restriction), compared to the “All” sample (which did not condition on mothers having a child), these mothers are somewhat older, are more likely married, were younger when they had their first child, and have two children on average instead of one.

Our main outcome variables are the labor supply of grandmothers and mothers, which are measured when the grandmothers are between ages 60 and 64. Grandmothers work on average 37 hours per month and earn 638 euros per month.<sup>16</sup> Their likelihood of employment is 42%, and 5% are employed full-time. On average, grandmothers exit the labor force at age 61 and start claiming retirement at age 63. The mothers work on average 78 hours and earn 1534 euros per month. Around 78% of them are employed, and 6% are in full-time employment.

*Children’s outcomes:* We also examine the reform effect on children’s educational outcomes. As in our main analysis, where we investigate the effect on mothers’ labor supply based on the age of their youngest child while their grandmother was between ages 60 and 64, we study children’s educational outcomes for the youngest child who was in a particular age range while their grandmother was between ages 60 and 64.

The Dutch primary education is for children aged 4 to 12 and lasts for eight years. At the end of primary education, pupils take the Cito test, which is a high-stakes standardized test used to sort students into different types of secondary school tracks (such as vocational, technical, academic tracks). We merge the children in our baseline sample with the test score data, including the final Cito score, number of correct answers overall, and number of correct answers in math and verbal skills. While the Cito test is the test used in the majority of schools to determine the secondary school track for their students, schools can opt for other test types, though we do not have data for this. Among all children aged 4 to 12 (of our baseline mothers), 50% attend schools that administer the Cito test (as opposed to alternative tests). Table A12 presents a comparison of the children in our baseline sample with the children who took the Cito test showing that the children have similar characteristics. We supplement our analysis on child outcomes with annual data on the childcare allowance that families receive for childcare usage, which contains information on the probability

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<sup>16</sup>All income measures are CPI adjusted for the year 2015.

of childcare allowance take-up, the type of childcare, and the hours requested.

### 3.2 The Fuzzy Regression Discontinuity

In this paper, we address the research question as to whether, to what extent, and through which channels grandmothers' labor supply affects the maternal labor supply of their adult daughters and the educational performance of the grandchildren. It is difficult to causally estimate the effects for two reasons. First, unobserved variables can affect the employment decisions of both grandmothers and mothers. For example, grandmothers' gender identity can be transmitted to their adult daughters (Fernández et al. (2004); Farré and Vella (2013); Kleven et al. (2019a)). Second, there might be reversed causality as grandmothers' retirement decisions can be affected by childcare decisions. The timing of grandparenthood can cause a reduction in the labor supply of grandmothers (Rupert and Zanella (2018); Frimmel et al. (2020)). We address this issue by exploring a cohort-based reform in early retirement incentives in the Netherlands.

We investigate three generations. First, we show that the reform creates a sharp discontinuous increase in labor supply for grandmothers born since 1950. The direct effect of the reform on grandmothers' outcome  $y^{GM}$  is modeled in the following Regression Discontinuity (RD) framework:

$$y_i^{GM} = \alpha_0^{GM} + \alpha_1^{GM} D_i^{GM} + \alpha_2^{GM} r_i^{GM} + \alpha_3^{GM} D_i^{GM} \times r_i^{GM} + \delta^{GM} X_i + \epsilon_i^{GM} \quad (1)$$

where  $r_i^{GM}$  is the running variable defined as the grandmothers' birth month,  $c_i^{GM}$ , centered around the cutoff  $c$ ,  $r_i^{GM} = (c_i^{GM} - c)$ .  $c$  is set to January 1950. The treatment indicator  $D_i^{GM}$  is defined as  $D = \mathbb{1}(r_i^{GM} \geq 0)$ .  $\alpha_2^{GM}$  and  $\alpha_3^{GM}$  allow for cohort trends in the outcome variables to differ by treatment status. The coefficient  $\alpha_1^{GM}$  is the first stage estimated impact of the reform on grandmothers' labor supply outcomes.  $X$  contains demographic characteristics of the grandmother and the mother, including mother's age and migration background, number of her siblings and sisters, the age at first birth, mother's and grandmother's predetermined marital status, mother's predetermined number of children, disability status of the grandmother's partner, predetermined employment probability of the grandmother, and whether mother and grandmother lived in the same district before the analysis period. We also include sector fixed effects to control for sector-specific pension rules. In the case where a grandmother may have changed her sector at some point during her employment history, we consider the one in which she was employed the longest.

For the baseline analysis, we use a bandwidth of 8 months around the cutoff, which is the average of the mean square error optimal bandwidths generated by the Calonico et al. (2017) and Calonico et al. (2018) procedure for the different outcomes we consider,<sup>17</sup> and a linear specification. For

<sup>17</sup>In the Table A8, we show the estimates using mean square error optimal bandwidths generated by Calonico et al.

consistency, we use this specification everywhere and in addition show robustness in Section 5.4.2.

Second, we investigate the middle generation: the mothers. The reform allows us to causally estimate the impact of the grandmothers' labor supply on the labor supply of the mothers. The corresponding reduced form model for mothers' outcome  $y^M$  is:

$$y_i^M = \alpha_0^M + \alpha_1^M D_i^{GM} + \alpha_2^M r_i^{GM} + \alpha_3^M D_i^{GM} \times r_i^{GM} + \delta^M X_i + \epsilon_i^M \quad (2)$$

where  $y_i^M$  is a list of mothers' labor supply outcomes. The coefficient  $\widehat{\alpha_1^M}$  is the the reduced form effect of the reform on outcomes of mothers.

The effect of grandmothers' labor supply on mothers' labor supply can be obtained as the ratio of the discontinuity in mothers' labor supply ( $\widehat{\alpha_1^M}$ ) to the discontinuity in grandmothers' labor supply ( $\widehat{\alpha_1^{GM}}$ ) (Hahn et al. (2001); Lee and Lemieux (2010)). The following local linear regression gives us the two stage least square (2SLS) fuzzy RD estimate:

$$y_i^M = \beta_0 + \beta_1 \widehat{Y}_i^{GM} + \beta_2 r_i^{GM} + \beta_3 D_i^{GM} \times r_i^{GM} + \theta X_i + \eta_i \quad (3)$$

The coefficient  $\widehat{\beta_1}$  measures the local average treatment effects (LATEs) of grandmothers' labor supply on mothers' labor supply. The fuzzy RD estimate is analogous to a two-stage least squares (2SLS) estimate with imperfect compliance.

Lastly, we look at the youngest generation - the children (or the grandchildren). We examine the reform impact on children's educational performance. The reduced form model for children' outcome  $y^C$  is

$$y_i^C = \alpha_0^C + \alpha_1^C D_i^{GM} + \alpha_2^C r_i^{GM} + \alpha_3^C D_i^{GM} \times r_i^{GM} + \delta^C X_i + \epsilon_i^C \quad (4)$$

where  $y_i^C$  is a list of children's outcomes. X includes (in addition to the controls used in the analysis of mothers) the child's birth cohort and month, and treatment duration (i.e. number of years the child is exposed to the grandmothers' labor response when aged 60-64). The coefficient  $\widehat{\alpha_1^C}$  is the estimated reform impact on children.<sup>18</sup>

### 3.3 Assumptions

*Smoothness in density:* For an RD design to be valid, individuals must not manipulate the assignment variable, which, in our case, is the grandmother's birthdate. Since the timing of grandmoth-

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(2017) for each of the outcome variables.

<sup>18</sup>We present results from regressions with cluster standard errors at primary school level. The clustering allows for correlations of test-performance within schools. These results are also robust to clustering at the mothers' level and to two-way clustering on mothers' and primary school level.

ers' birth cannot be affected by a pension reform more than 50 years later and since we are using administrative birth records from the Netherlands, there is little-to-no room for manipulation. Figures 2a and 2b show the density plot of grandmothers 24 months and 8 months around the cutoff. The bin size is the grandmothers' birth month. Figures 2c and 2d show the density plot of mothers 24 months and 8 months around the cutoff. The fluctuating pattern of the density plots are similar when we compare grandmothers and their adult daughters of our sample as well as comparing them to the pattern for women (elderly and the middle generation) without (grand)children (see Figures 2e and 2f). This suggests seasonal patterns which commonly occur in terms of birth rates (and which are not driven, for example, by the sample restriction of having a (grand)child). Moreover, Haandrikman and van Wissen (2008) and Calot and Blayo (1982) show that in the Netherlands birth rates peak in spring and are the lowest around November, which is consistent with the above described density plots.

*Smoothness in covariates:* Table A1 reports the estimated impact of grandmothers being born since January 1950 (as opposed to before) on a list of predetermined characteristics of grandmothers and mothers (using Equations 1 and 2). All variables are predetermined and refer to the time period when the grandmothers were aged 50 to 53. We show the estimated effects under different specifications: linear (column (1)) and quadratic (column (2)). All specifications use a bandwidth of 8 months. Covariates are smooth across the cutoff. In particular, there are no significant differences in the covariates above versus below the cutoff, with two exceptions out of 21 variables (significant at the 10 and 5 percent level, respectively), suggesting that the distribution of pre-determined characteristics is balanced around the cutoff.

*Instrument validity:* There are three conditions necessary to interpret the two-stage least squares estimate. First, the grandmothers' birthdate is strongly associated with the grandmothers' labor supply. We show the validity and magnitude of the first-stage relationship in Section 4.1. Second, a grandmother's birthdate only impacts her adult daughter's labor supply outcome through changes in her own labor supply. The exclusion restriction could be violated if a grandmother who was born before or since 1950 affects her adult daughters' labor supply and grandchildren's education outcomes through channels other than her own labor supply. This assumption is fundamentally untestable. We argue that the exclusion restriction assumption is reasonable because there are no other reforms with the same grandmother birthdate cutoff. Moreover, a number of results in the mechanisms analysis (Section 5) indicate that it is the changes in grandmothers' time availability that causes the mothers to work less. In Section 5.4.1, we provide a more in-depth discussion about the exclusion restriction based on further empirical findings. Third, the monotonicity condition requires that the changes in early retirement incentives always make grandmothers increase their labor supply. Suppose a grandmother was exposed to the less generous early retirement policy, in that case, she will raise her labor supply or at least maintain the same level of labor supply



compared to the level under the old regime. Given the nature of the 2006 pension reform, this condition is readily satisfied.

## **4 Short-Run Spillover Effects: Direct and Indirect Effects on Labor Supply**

In this section, we first investigate the direct reform impact on grandmothers' labor supply, then we show the indirect spillover effects on mothers with their youngest child between age 4 and 12 (baseline sample). Finally, we further divide our baseline sample by the age of the youngest child to highlight the importance of childcare responsibilities.

We hypothesize that mothers with children of primary school ages are most strongly affected by changes in the informal care provided by grandmothers. This is because the need for childcare and the compatibility of grandparental care and formal care varies according to the child's age. First, as the children grow older, the amount of care needed decreases. Children below primary school age (younger than 4 years), require more care and more intensive care, while children older than 12 years need limited or no supervision. Second, the amount of free public childcare hours increases as the children grow up. While daycare for children below age 4 is costly, from age 4 onwards children can attend primary school which is free of charge, and offers around 30 to 35 hours of care per week. According to the LISS data, grandparents provide about three half-days ( $\sim 9$  hours) per week of childcare, which – combined with the time in primary school – can free mothers from childcare responsibilities and allow them to work more during the week.

In contrast, children below age 4 do not yet attend (pre)school and thus require care full-time, which grandparents are rarely able to provide in its entirety. In fact, we find – based on own calculations from the LISS data – that for children aged 4 to 12, grandparents are the only childcare option for 20% of families, while for children below age 4, this is only the case for less than 10% of families (see discussion and further details in Section 2.4). Thus, for very young children below age 4, mothers either stay home completely or if they do work, they mostly rely on formal daycare. Therefore, grandparents' time availability is less critical for this age group and hence less likely to alter mothers' labor supply decisions.

Lastly, children above age 12 are likely to require little or no supervision after their (longer) school day. Also "mothers" (i.e., adult daughters) without children obviously require no childcare at all. We present the estimated effects for the two latter groups (children above 12 years and no children) to support our finding that the effects we find are indeed linked to time availability and the childcare provision of grandparents.

## 4.1 Effects on Grandmothers' Labor Supply: First-stage

Figure 3 provides graphical evidence of the first-stage reform impact on grandmothers' total hours worked. It shows the bin scatter plot of total monthly hours worked as a function of distance to the cutoff for grandmothers' birth month, which is January 1950. The solid lines are the linear fitted lines, and the shaded areas indicate the 95 percent confidence interval. We can see that grandmothers born between April 1949 to December 1949 work on average 33 hours per month between age 60 and 64, while grandmothers born between January 1950 and September 1950 work on average 41 hours between the same ages. Moreover, there is a clear jump at the cutoff from about 34.5 to 42.5 hours per month.

First-stage estimates of the pension reform are shown in the top panel of Table 2. Columns 1, 2, and 3 show the results for a local linear regression without controls, with controls, and with controls and sector fixed effects. All three specifications use a bandwidth of 8 months (as in all other tables, unless otherwise specified; see discussion in Section 3.2). Columns 4 and 5 show the robustness of the results using a bandwidth of 6 months and of 12 months. Standard errors are clustered at the grandmother level, since in the main analysis grandmothers are in the sample multiple times if they have several daughters with children in the relevant age range. The regression results are consistent with the graphical analysis. We find that the reform increases the grandmothers' monthly hours worked by around 6.2 hours.

## 4.2 Effects on Mothers' Labor Supply: Spillover Effects

Graphical evidence in Figure 4 show that mothers' labor supply drops sharply at the cutoff. Fuzzy regression estimates are at the bottom panel of Table 2. Columns 1, 2, and 3 show the results for a local linear regression without controls, with controls, and with both controls and sector fixed effect. All three specifications use a bandwidth of 8 months (as do all other tables, unless otherwise specified). Columns 4 and 5 show the robustness of the results using a bandwidth of 6 months and of 12 months. Standard errors are clustered at the grandmother level. We find that a one-hour increase in grandmothers' monthly working hours induces a decline in mothers' monthly hours worked of around 28 minutes (0.47 hours) at 5 percent significance level. When we change the bandwidth to 6 and 12 months, the estimate remains similar in terms of size and significance. In fact, reducing the bandwidth to 6 months even increases the estimated coefficient in absolute value to 0.63 at 5 percent significance; that is, a one-hour increase in grandmothers' monthly working hours decreases mothers' monthly hours worked by 38 minutes. The impact on other measures of labor supply shows a similar pattern. When grandmothers work more, mothers are less likely to engage in formal employment (significant at the 10 percent level). The probability of working full-time decreases (significant at 10 percent level for a bandwidth of 12 months).

To understand whether the change in mothers' labor supply is related to childcare responsibilities, we further divide our baseline sample based on the childcare needs of the mother. In particular, we divide mothers (middle generation) with the youngest child in need of care (between age 0 to 12) into three different categories: 0 to 3, 4 to 7, and 8 to 12. Children aged 0 to 3 require the largest amount of care (that is, full-time care), since the child does not yet attend school. Starting at age 4, children attend school (which for the first two years is pre-school, see discussions in Section 2.3) and are thus taken care of for around 6 to 7 hours per day. A mother working part-time thus requires only a few additional hours of help. Hence, the grandmothers' availability can potentially fill this gap and allow mothers to work more. The hours attended in school increase with age and at some point, children are able to spend some time unsupervised. Thus, we also present the results for children aged 8 to 12. We also show results for mothers with a youngest child aged between 13 to 18 and without children, since in these cases no (or only limited) supervision of children is necessary.<sup>19</sup>

Table 3 shows the estimates. As hypothesized, we find the strongest effects of grandmothers' increase in hours worked (due to the pension reform) on the labor supply of mothers with a primary school-aged child between 4 and 7. In particular, a one-hour increase in grandmothers' monthly working hour induces a decline in mothers' monthly hours worked of around 32 minutes (0.53 hours), which is significant at the five percent level. In contrast we do not find effects on mothers with a child aged 0 to 3, consistent with mothers either staying home or making use of full-time daycare,<sup>20</sup> so that a change in grandmothers' time availability makes less of a difference. Furthermore we do not find significant effects on mothers' labor supply when the youngest child is aged 8 to 12. Lastly, we analyze effects for mothers whose youngest child is above 12 years and adult daughters without children, who have little or no need of childcare/supervision, and thus act as a sort of placebo group. As expected, we find no effect on those two groups, strongly supporting our interpretation that the changes in mothers' labor supply is indeed related to the time availability of grandmothers and their childcare responsibilities. We will provide further evidence on the mechanisms underlying our results in the next section.

## 5 Short-Run Spillover Effects: Mechanisms and Validity

The goal of this section is to further investigate the mechanisms underlying our results discussed above. In particular, we aim to provide additional evidence that the exogenous changes in pension

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<sup>19</sup>See the hypothesis discussion at the beginning of Section 4 for detailed reasons for the age group partition. Moreover, we show in Table A11 that the probability of having the youngest child(ren) in different age groups is not affected by the reform.

<sup>20</sup>In the Netherlands, there is a strong social norm to stay at home with the children when they are below school age (Swart et al. (2019), Rabaté and Rellstab (2021)).

incentives affect mothers' labor supply through changes in the time grandmothers can devote to take care of their grandchildren. The first piece of evidence supporting the "time transfer channel" is based on the results for different age groups (younger than 12 versus older than 12 or no kids), which suggests that the negative effects on mothers' labor supply is indeed related to childcare needs and thus strongly points towards a time transfer channel (instead of, say, a monetary transfer channel, since this should also affect mothers with older children or adult daughters without children).

In principle, it could be that grandparents support their adult children (who themselves have young children) via monetary transfers for formal child care. While we do not have direct data on monetary gift exchanges and we can not directly test the "monetary transfer channel", we can test whether the gross income of grandmothers changes in the first place. However, we show in Table A2 that grandmothers' monthly gross income and household gross income are not affected by the reform (i.e., grandparents do not have more money available), which suggests that the monetary transfer channel is unlikely. In any case, this channel would not explain why after the reform when grandmothers work more, mothers work less.

If the time transfer channel plays a critical role, we have the following three hypotheses. First, we expect a smaller impact if grandmothers have other care responsibilities. Since those grandmothers have little time for childcare already in the absence of the reform, the reform should have little impact on grandmothers' availability for childcare. Second, we expect grandmothers who live far away to have a smaller or no impact, as they are unlikely to provide care regularly. If, on the other hand, grandmothers support childcare via monetary transfers, the residential location should be irrelevant. Third, we expect grandmothers with only one young maternal-grandchild to have a larger impact, as their time is not shared with other grandchildren and other daughters. We test these hypotheses in Table 4, as discussed in the upcoming section.

Furthermore, we test whether there is a similar effect for grandfathers on their daughters and study the impact of grandmothers' labor supply on other family members in the middle generation, including daughters, sons-in-law, sons, and daughters-in-law. We have seen in Section 2.4 that the majority of grandparents providing child care are maternal grandparents (more than 60%). Thus, if the effects we find are due to a decrease in time availability, we should observe larger effects for maternal grandparents and thus smaller effects on the daughters-in-law of grandmothers.

## 5.1 Heterogeneity by Health, Proximity and Family Composition

Table 4 shows heterogeneous effects by health status of the grandmother's partner, the proximity of the grandmothers, and by family composition. Columns 1 and 2 of Table 4 show the results for grandmothers whose partner (mostly the grandfather) is unhealthy or healthy, respectively. We

define the grandfather to be healthy if he has not claimed any disability insurance before age 50. We find that grandmothers with sick partners have small and insignificant effects on maternal labor supply. Note that, the first-stage F-statistic is below 10 for this subgroup; that is, grandmothers' labor supply is not affected in the first place. In contrast and as expected, for grandmothers with healthy partners we find significant effects (which are somewhat larger than the baseline estimates).

Columns 3 and 4 show the results by the proximity of the grandmothers. We find that one additional hour worked per month by grandmothers living nearby causes the mothers to work about 50 minutes (0.82 hours) less. In addition, the probability of employment declines significantly. At the same time, we find much smaller and insignificant effects when the grandmother lives in a different municipality to her daughter. The differences between these two subgroups are statistically significant at the 10 percent level for employment probability. Thus, the finding that grandmothers living close have large impacts on their daughters' labor supply, while those that live in a different municipality (and thus were unlikely to provide regular childcare even in the absence of the reform) do not, strongly supports the time transfer channel.

Columns 5 and 6 explore the dimension of competition for grandmothers' time. For this purpose we compare (among grandmothers with at least one daughter who has a child) grandmothers with exactly one maternal-grandchild aged between 4 and 7 with the remainder of grandmothers. We focus on maternal grandmothers, since they are most relevant in terms of taking care of grandchildren (see evidence based on LISS survey data discussed in Section 2.4; also see our discussion below where we compare the effects of grandmothers' labor supply on daughters/sons/daughters-in-law/sons-in-law, and where we investigate the effect of grandfathers' labor supply). Moreover, we focus on grandchildren aged 4 to 7, since we find the strongest effects of changes in grandmothers' labor supply for this subsample, and we know from the LISS data that grandparents play a particularly large role as sole childcare providers for primary school-aged children (Section 2.4).

Indeed, as hypothesized, we find that grandmothers who have exactly one maternal grandchild between ages 4 and 7 have a large and significant impact. One additional hour worked per month by grandmothers causes the mothers to work about 0.83 hours (50 minutes) less. In contrast, the impact on the other group is small and insignificant.

## **5.2 Grandfathers' Effect**

We provide further evidence for the time transfer channel by looking at grandfathers. The reform affects both genders, therefore grandfathers are also affected. In fact first-stage results on the reform impact on grandfathers' labor supply are even stronger than on grandmothers due to their stronger attachment to the labor force (see Table A4). While both grandparents can provide childcare support, previous studies show that grandmothers are more likely than grandfathers to

be engaged in childcare activities (Jappens and Van Bavel, 2012; Janta, 2014). Therefore, if it is the “time transfer” channel, we expect to see grandfathers having smaller impacts on daughters’ labor supply. If it is the “monetary transfer” channel, we expect to see a similar impact by the grandfathers, as they could provide money equally as well as grandmothers.

In Table 5, we compare the effect on (adult) daughters’ labor supply of grandfathers (Column 1) versus grandmothers (Column 3). We find that grandfathers have a very limited impact on daughters’ labor supply. The impacts on hours worked, employment probabilities and probability of full-time employment are an order of magnitude smaller than for grandmothers and are all insignificant, with the exception of daughters’ employment probabilities, with a coefficient that is also only one-third compared to the effect of grandmothers. A one-hour increase in grandfathers’ monthly working hours decreases daughter’s employment probability by 0.1 percent.<sup>21</sup>

In summary, even though grandfathers’ labor supply is strongly affected by the pension reform, we find that grandmothers’ labor supply and time availability causes significant changes in daughters’ employment, while grandfathers have little impact. These findings provide further supportive evidence for the time transfer channel; that is, the change in grandmothers’ time engagement in childcare activities is the underlying factor that drives our findings.

### 5.3 Impacts on Other Family Members

To understand further potential mechanisms at play, we look at the impact of grandmothers’ labor supply on other family members in the middle generation, including daughters, sons-in-law, sons, and daughters-in-law. First, we expect maternal grandmothers to have a larger impact, if the underlying mechanism of our findings is a time transfer channel. Survey evidence from LISS data (as discussed in Section 2.4), as well as the existing literature show that maternal grandparents are more likely to provide childcare support than paternal grandparents (see, e.g., Danielsbacka et al. (2019) who find that maternal grandmothers matter more in childcare provision using the Survey of Health, Ageing and Retirement in Europe (SHARE) and 11 countries in Europe). Moreover, this exercise provides a complete picture of the intergenerational spillover effect on all extended family members. Thereby it helps us understand the full scope of the unintended fiscal costs of pension reform.

Table 6 compares the impacts of grandmothers’ labor supply on her daughters, sons-in-law, sons, and daughters-in-law, for whom their youngest child is aged between 4 and 12. We find no

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<sup>21</sup>One reason for finding only a small effect of grandfathers’ labor supply on daughters’ employment probability might be that in response to grandfathers’ increased labor supply their partners (i.e, grandmothers) work more, which is consistent with the existing literature (Hurd, 1990; Coile, 2004; Stancanelli and Van Soest, 2012; Lalive and Parrotta, 2017). Therefore, grandmothers are in turn less available to provide childcare. Columns 2 and 4 of Table 5 suggest that this might be the case, since an increase in grandfathers’ labor supply has some effect on their partner but not vice versa.

significant impacts on sons and daughters-in-law. Only maternal grandmothers matter.

As for the core families with the youngest child aged 4 to 12, we have shown that in response to grandmothers' labor supply increases, mothers work less. Table 6 shows that their husbands work more, potentially to compensate for the loss in their wives' labor earnings.<sup>22</sup> Indeed, we find that there is no impact on overall household income. However, the fact that mothers of young children reduce their labor supply in response to the reform, while their husbands' labor supply increases, has important implications both in terms of intra-household inequality and in terms of gender inequality in the society.

## 5.4 Threats to Validity

### 5.4.1 Exclusion Restriction

The exclusion restriction requires that a grandmother's birthdate only impacts her adult daughter's labor supply outcome through changes in her labor supply. In principle, the pension reform could lead to an increase in grandmothers' labor income because of delaying retirement, which in turn might have an effect on monetary transfers to her children. However, while the reform increases grandmothers' monthly labor supply, monthly gross income and gross household income remain unchanged (see Table A2),<sup>23</sup> suggesting that changes in monetary transfers in response to the reform are unlikely. Moreover, as discussed before, if the reform led to a change in monetary transfers, this would not explain why only mothers with young children aged 4 to 12 (who thus have childcare needs) respond with changes in their labor supply (see Table 3), but not mothers with older children. It would also not explain why there is only an effect on adult daughters, but not on sons or daughters-in-law (see Table 6).

Another potential channel through which the pension reform could affect adult daughters' labor supply outcome is the "reminder effect". Adult children of mothers affected by the pension reform might become more aware of the insufficient future public pension and thus save more and work more. However, again, this channel affects maternal labor supply in the opposite direction. Moreover, there is no reason why this channel should only affect mothers with the youngest child aged between 4 and 12. Women without children or with older children should also be affected as should be the case for sons and daughters-in-law. These outcomes further support the validity of the exclusion assumption.

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<sup>22</sup>See Section 2.4 for supporting evidence by the LISS panel, that in the majority of cases it is the mother who reduces her work hours to care for young children instead of the father (48% of mothers versus 8% of fathers report reducing their hours due to childcare needs and, conditional on this reduction, mothers/fathers report reducing their work hours by 14 hours versus 8 hours).

<sup>23</sup>The fact that individuals born since 1950 are entitled to less generous pension benefits helps explain why we do not find a change in the average total income.

Lastly, in 2006, the Dutch government introduced the “Life course savings” (Levensloopregeling, LCS) program. This tax-facilitated savings program allows workers to save for periods of unpaid leave or early retirement and was perceived as a way out of the labor market for people affected by the 2006 reform. While the LCS program was introduced at the same time as the early retirement reform, both the treated and control can use this new tax-facilitated saving scheme. Moreover, if anything, the availability of the LCS plan could make our first stage estimates smaller, but does not pose a threat to the validity of our estimates.<sup>24</sup>

#### 5.4.2 Placebo Tests and Robustness Checks

Two placebo exercises further support the credibility of our estimates. First, we show the reduced-form impact of having grandmothers born since 1950 using a sample of mothers with deceased grandmothers. We do not expect the pension reform to affect women with deceased mothers (grandmothers) born around the cutoff. Table A5 shows the estimated reform impacts on the labor supply of adult daughters (mothers) whose mothers (grandmothers) deceased before age 50. As expected, none of the estimates are significant and the coefficient size is an order of magnitude smaller (see Table A3). The results suggest that the estimated changes in mothers’ labor supply in our baseline analysis are not caused by any other changes at the cutoff or unobserved characteristics of mothers that differ for those whose mothers are alive (i.e., the grandmothers) born on the dates around the cutoff.

Second, we show the validity of our results by using placebo cutoffs 10-months prior and 10-months ahead at a bi-monthly frequency. Figure A.2 shows that the F-statistics is only above 10 at the actual cutoff. Table A6 shows the reduced-form estimates and Figure A.3 plots the coefficients with 95% confidence intervals. We find no significant effects of grandmothers’ labor supply on maternal labor supply at these placebo cutoffs.

We also test the robustness of the estimation results by varying the choice of polynomial orders and bandwidth. Table A7 shows the estimates in response to a one-hour increase in grandmothers’ monthly working hours for a linear and a quadratic specification. The Aikake Information Criterion (AIC) and Bayesian information criterion (BIC) and AICc (AIC with a correction for small sample sizes) with correction are also reported. According to the AIC, BIC and AICc criteria, in terms of total monthly hours worked, the linear specification fits the best.

Table A8 displays the estimated impacts and bandwidth for all relevant outcomes using the mean square error optimal bandwidths generated by the [Calonico et al. \(2017\)](#) and [Calonico et al.](#)

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<sup>24</sup>Moreover, in practice, only some high-wage workers manage to retire early using the LCS plan. [Lindeboom and Montizaan \(2020\)](#) show that only around 15% of the 1950 cohort participated in the LCS plan, among which only 16% managed to counteract the effect of the early retirement reform and maintain their previously planned retirement dates. See Appendix A.2 for further details about the Life course savings program.



(2018) procedure. We show the estimates and optimal bandwidths for both linear and quadratic specifications. Notice that the estimated impact on the total monthly hours worked is around -0.65 hours using a bandwidth of 7.25, which is higher than the baseline result. Therefore, if anything, our baseline result is rather conservative. We show results for a bandwidth of 8 to avoid switching between different bandwidths (and thus samples) for different outcomes and for different subgroups.

### 5.4.3 Fertility Effects

In our main analysis we focus on mothers with the youngest child between age 4 and 12. However, given that grandparental child care is important, it might also affect the fertility decision of their adult daughters. The size and sign of this effect in the literature are mixed (see [Battistin et al. \(2014\)](#); [Aparicio-Fenoll and Vidal-Fernandez \(2015\)](#); [Eibich and Siedler \(2020\)](#)).<sup>25</sup>

The question of whether grandparents' retirement and labor supply decisions affect their adult daughters' fertility is interesting and policy relevant. Moreover, it is important for us to test the fertility responses, since if the pension reform affects fertility we might end up with a selected sample (for example, mothers with grandmothers born since January 1950 might be less likely to have children or may time the births differently.)

We investigate this question by performing the following analyses. We estimate the impact of having grandmothers born since 1950 on the fertility outcomes of their adult daughters (using a different sample than the baseline sample, in particular, one that is not conditional on adult daughters having children). Table A9 shows the effect on a number of different outcomes. In terms of total fertility, we look at the probability of ever having a child, the total number of children, and the probability of having at least two children. For fertility timing, we examine age at the first birth, age at the last birth, the average age gap between children, the average age gap between children born after grandmothers turned age 55, and the probability of having their first child after grandmothers turned age 55. None of these measures of fertility are affected by the reform.

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<sup>25</sup>[Battistin et al. \(2014\)](#) and [Aparicio-Fenoll and Vidal-Fernandez \(2015\)](#) exploit the pension reforms in Italy and find contradictory results. [Aparicio-Fenoll and Vidal-Fernandez \(2015\)](#) find that even though grandmothers who delay retirement provide less childcare, their daughters are more likely to have children due to an increase in family income. Instead, [Battistin et al. \(2014\)](#) shows that delaying retirement causes their daughters to delay and reduce fertility. Similarly, [Eibich and Siedler \(2020\)](#) finds that grandparents' retirement affects the timing of fertility. Adult children seem to time the childbirth to coincide with their parents' retirement.

## 6 Long-Run Spillover Effects

### 6.1 Effects on Grandchildren’s Educational Achievement

We have shown that an (exogenous) increase in grandmothers’ labor supply (due to the pension reform) leads to a decrease in mothers’ labor supply when they have young children between ages 4 and 12. This suggests a substitution effect away from grandparental care to maternal care for these children raising the interesting question of whether this change affects children’s educational performance.

To analyze whether the change in childcare mode from grandmaternal to maternal care affects children’s educational outcomes, we make use of data on the performance of children on the Cito test, which is a high-stake test at the end of primary school designed to assign children to different tracks in secondary school (vocational, technical, academic). The performance on the test and the resulting track assignment has important long-run implications in terms of the likelihood of enrolling/completing university (which requires completing the academic track in secondary school), earnings and family formation outcomes (see, e.g., [Dustmann et al. \(2017\)](#) on the longterm effects of early track choice and [Kaufmann et al. \(2021\)](#) on the marriage market effects of university education).

More specifically, we examine the impact on the overall Cito score as well as on the number of correct answers (total) and the number of correct answers in math and on the verbal component of the test (separately). The Cito test is taken at the end of primary school (at around age 12), which is in spring, and is before the children enter secondary school after the summer break. Since we find effects on mothers’ labor supply that depend on the age of the *youngest* child in the family, we investigate reform spillovers on the performance of the youngest child.

Table 7 presents the RD estimates of the reform spillovers on children’s test scores. Panel (A) shows the results for children aged 4 and 12 years old during the relevant years (i.e., when the grandmother was 60 to 64, as in our entire analysis), since this is when the grandmothers, and, as a result, the mothers are affected and change their labor supply behavior. We find that the reform has led to an overall improvement in children’s Cito performance, in particular, for the math component of the test.<sup>26</sup>

In Panel (B) of Table 7, we split the sample by age groups. We find that changes in grandmothers’ labor supply has particularly strong effects for children aged 4 to 7. Moreover, children above age 10 years might be able to spend some time unsupervised, while at the same time this is a critical age for the Cito test in determining their secondary education track. For this reason, we investigate the effect on children’s educational performance for three different age groups: 4 to 7 years, 8 to

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<sup>26</sup>Table A13 lends support to the smoothness condition in that covariates for the Cito sample are balanced across the cutoff.

10 years, and 11 to 12 years.<sup>27</sup>

The first row presents results for children who were aged between 4 and 7 during the relevant years. We find that these children whose grandmothers' labor supply increased due to the reform, while their mothers' labor supply decreased, perform significantly better on the Cito test. Their overall Cito score increases, as well as the number of correct answers on both the verbal and the mathematical component (significant at the one percent level). The overall number of correct answers increases by 3.4 percent and the number of correct answers on the math test increases by 5.5 percent. When we split the sample by gender (see Table 8), we find particularly strong positive effects on girls suggesting that girls in particular benefit from the increased interaction with their mothers.<sup>28</sup>

The second row of Table 7 shows regression estimates for children aged 8 to 10 during the relevant years. For this age group, the children of treated grandmothers do not perform significantly differently from children with untreated grandmothers and this is true for both boys and girls (see Table 8). For these older children (aged 8 to 12), we did not find significant effects on mothers' labor supply.

The third row of Table 7 presents RD estimates for children aged 11 to 12. We find that these children perform significantly worse on the Cito test, in that their overall Cito score as well as the number of correct answers on the verbal and the mathematical components decreases (the number of correct answers in math decreases by 6.3 percent and on the verbal component by 3.2 percent). One potential explanation might be that part of the grandmaternal supervision time (which decreases in response to the reform) is substituted for by unsupervised time for these children aged 11 to 12 (compare this to Aizer (2004), who finds that a lack of adult supervision after school can have important consequences for human capital development). Our findings in Table 8 lend some support to this interpretation in that the negative effects on children aged 11 to 12 are entirely driven by boys. The differences between boys and girls are all statistically significant. It is particularly striking that for boys even the likelihood of receiving a recommendation for the highest (academic) track decreases by 6.7 percentage points. Thus, while girls are generally more conscientious and thus might study for the high-stake test even if unsupervised, boys' study behavior at this age depends more strongly on adult supervision.<sup>29</sup> The reform thus has very negative spillover effects on

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<sup>27</sup>Table A14 shows that there is no selection into taking the Cito test for the different age groups.

<sup>28</sup>This is consistent with recent evidence by Fort et al. (2020) who find –for younger children– that attending daycare instead of spending more time with their mother has a negative effect on girls' abilities.

<sup>29</sup>As discussed in Bertrand and Pan (2013), boys are known to perform worse than girls on many noncognitive dimensions. Boys have well documented attention and behavioral difficulties (see Beaman et al. (2006), Gilliam (2005) and Ready et al. (2005)), have lower levels of inhibitory control and perceptual sensitivity (Else-Quest et al., 2006), are less able to delay gratification (Silverman, 2003), and are more likely to be diagnosed with attention deficit hyperactivity disorder (see, for example, Szatmari et al. (1989)). Related to these findings, boys' educational performance is more strongly affected by negative shocks/environments, such as poor school quality, living in a single-parent household etc (see, for example, Bertrand and Pan (2013) and Autor et al. (2016)).

boys aged 11 to 12, since it decreases their likelihood of getting into the academic track in secondary school with important consequences for their long-term performance outcomes (reducing their likelihood to attend and complete university, their labor earnings, etc.).

For children aged between 4 and 7, while we know that there has been a substitution away from grandmaternal care to maternal care, it is less clear what has happened in the case of children aged between 8 and 12. For these children, mothers' labor supply did not decrease in response to an increase in grandmothers' labor supply. However, we find a strong negative reform effect on children aged 11 to 12, suggesting that there was a change in the mode of supervision and a likely decrease in grandmaternal care. This raises the question as to whether substitution has taken place towards formal after-school care or other types of informal care. We therefore supplement our analysis with data on whether parents applied for childcare subsidies and for which type (day care for children aged 4 to 7 or out-of-school care for children aged 4 to 12), for how many hours and for which child.

Table A15 presents the estimated reform impact on the probability to take up daycare subsidies, hours of daycare, the probability to take up out-of-school care subsidies and hours of out-of-school care. For children aged 4 to 7, we do not find significant reform effects on the likelihood of taking up daycare subsidies or after-school-care subsidies. However, the number of hours in daycare decreases somewhat (by 6 hours per month). Thus, in addition to the substitution from grandmaternal care to maternal care, we see that part of the increase in maternal care time comes from a decrease in formal daycare hours. It appears that mothers who reduce their labor supply in response to the decrease in grandmothers' availability also send their young child to daycare for slightly fewer hours.

While we do not find reform effects in terms of after-school care for children aged 8 to 10, we find a significant increase in the the probability of taking up subsidies for after-school care, as well as in the hours of after-school care (9 hours per months more). Thus, for children aged 11 to 12, there appears to have been some substitution away from grandmother supervision towards after-school-care. It is not clear whether this increase fully makes up for the reduction in time availability of grandmothers and it is possible that for the remaining time children aged 11 and older are at home unsupervised for a few hours in the afternoon, as hypothesized above.

Interpreting the results in terms of Cito test for children of different ages points to the following conclusions: children who are aged 4 to 7 when their grandmothers are affected by the reform benefit from the fact that the mother spends more time with them (in particular, at young ages when starting (pre-)school), substituting away from formal childcare and grandmothers' care. This is consistent with the growing literature studying the relationship between childcare options/parental inputs and child development (e.g., Baker et al., 2008; Bernal, 2008; Fort et al., 2020; Baker et al., 2019). For children aged 8 to 10, we find no effect in terms of test scores, which is consistent with

the fact that there are no significant changes in terms of childcare modes. For children aged 11 to 12, we find a negative effect on test scores (for boys in particular), suggesting that the substitution away from grandmothers' care towards either after-school care or no adult supervision (for a few hours after school) during the one to two years prior to the high-stake test has important negative effects on the performance of boys. This is consistent with the fact that especially boys at this age need adult supervision to practice for this very important test. Previous studies also show that a lack of out-of-school adult supervision has important consequences for their human capital development (Aizer, 2004). Moreover, the quality of childcare in the Netherlands could be one potential reason for the test score results. Both expenditures and teacher-child ratio of both pre-primary school and out-of-school care in the Netherlands were below OECD average and at a similar level compared to the U.S. during our sample period (OECD, 2017).

## 6.2 Dynamic Effects on Mothers' Labor Supply and Child Penalty

Reducing gender inequality in the labor market is high on the policy agenda. The existing literature has shown that children have a large and persistent impact on the gender gap in labor market outcomes (Kleven et al. (2019a,b, 2020)). The Netherlands faces a similar situation as the U.S. and other developed countries. In particular, the monthly gender wage gap in 2014 was 41.8% (women earn EUR 580 to every EUR 1,000 earned by a man), and thereby is among the OECD countries with a large gender gap (OECD, 2019a). In this section, we connect our empirical findings to this debate and aim to address the question as to whether grandmothers' retirement decisions affect the gender gap and child penalties. First, we estimate the causal long-run reform impact by comparing the effect of having a child on the labor market trajectories of mothers with treated grandmothers to mothers with untreated grandmothers. Second, we compare the differences in child penalties between mothers with treated grandmothers and untreated grandmothers.

We build on the framework developed by Kleven et al. (2019a) and estimate the following regression separately by gender ( $g$ ) and treatment status ( $d$ ):

$$Y_{ist}^{gd} = \sum_{j \neq -1} \alpha_j^{gd} I[t = j] + \sum_k \beta_k^{gd} I[age = k] + \sum_s \gamma_s^{gd} I[year = s] + \nu_{ist}^{gd} \quad (5)$$

Hereby  $Y_{ist}^{gd}$  denotes the labor market outcome of individual  $i$ , in calendar year  $s$ , at event time  $t$ . The first term captures a full set of event time dummies, where event time  $t = 0$  marks the birth of the first child. We exclude  $t = -1$  so that the coefficients measure the impact of the first child relative to the year before birth. To control for life-cycle and time trends, the second and third term include sets of dummies for the age of individual  $i$  and calendar year, respectively. Conditional on age and year, there is variation in the age at first childbirth, which identifies the effects of all three

sets of dummies (see [Kleven et al. \(2019a\)](#) for details of the method).

Since our main interest lies in measuring changes in total labor supply (total monthly hours worked), we keep zeros (i.e., non-participation), and we specify Equation 5 in levels. First, we estimate the effect of children on men and women separately by converting estimated level effects into percentages:

$$P_t^{gd} = \frac{\hat{\alpha}_t^{gd}}{E[\tilde{Y}_{ist}^{gd}|t]}$$

with  $\tilde{Y}_{ist}^{gd}$  capturing the predicted labor market outcome without the contribution of the event time dummies (i.e., excluding the first term from Equation 5). This transformation allows to interpret  $P_t^{gd}$  as the percentage loss of average labor market outcomes due to having a child that individual  $i$  of gender  $g$  with treatment status  $d$  experiences.

Second, to compare penalties between women and men, we calculate the relative child penalty,  $P_t^d$ , measuring the relative loss women experience at event time  $t$  due to children:

$$P_t^d = \frac{\hat{\alpha}_t^{md} - \hat{\alpha}_t^{wd}}{E[\tilde{Y}_{ist}^{wd}|t]}$$

The child penalty results are based on a sub-sample of parents for whom we observe labor market outcomes in all years from four years prior to seven years after birth. This yields a balanced sample of women and men with their first child born between the years 2009 and 2013.<sup>30</sup> We further exclude teenage births by dropping observations with first birth before age 20 and exclude late entry into parenthood after the age of 40.

We choose the optimal bandwidth of the total monthly hours worked and take women and men with (grand)mothers born in the seven months around the January 1950 cutoff. Since we only found negative effects on daughters' labor supply, we focus on the middle generation that has a direct relation to the treatment generation, meaning only (adult) daughters and sons (with a young child) that have a (grand)mother born around January 1950.

While the literature on gender gaps and child penalty shows whether and to what extent women's labor market outcomes converge with men's outcomes, we are interested in whether the pension reform causally leads to a *slower* convergence due to its spillovers on maternal labor supply. Such effects are easiest to detect for women who have exactly one child, since otherwise there can be slower convergence due to having a second child some time after the birth of the first as well as being due to the longer-run effects of the first child or due to the reform impact, which introduces additional noise. Therefore, in our main part of this analysis, we focus on women and men with only one child, which allows us to investigate the child penalty without the effects of potential

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<sup>30</sup>Detailed hours worked data are available from 2006 onward, as discussed in Section 3.1.

additional births after the first one.<sup>31</sup>

Figure 5 compares the evolution of mothers' total hours worked around the birth of their child (marked in the figure as event time 0) for women with treated (blue dots) and untreated grandmothers (black triangles), i.e.,  $P_t^{wd}$ . Both the 90 and 95 percent confidence intervals are shown by the shaded areas. We find that women experience a sharp drop in their monthly hours worked within the first year after birth, which amounts to a 21 percent drop relative to their pre-birth work hours.

However, women with treated grandmothers recover more slowly than do women with untreated grandmothers. This difference becomes significant at 5 percent starting around four years after birth, which is consistent with our main findings in Section 4. In other words, the reform did not only lead to short-run effects on maternal labor supply but also had dynamic effects in that it led to a slower recovery of the working hours of mothers with young children.

Figure 6 compares the estimated child penalties for women (blue dots) relative to men (black triangles) separately for treated (left panel) and control groups (right panel): i.e.,  $P_t^d$ . We also calculate the long-run relative child penalty faced by women seven years after birth and report it at the bottom of each panel. We observe that the gender gap in total hours worked starts to converge and reduces to 11 percent at seven years after birth for the control group (i.e., with grandmothers not treated by the reform).<sup>32</sup> In contrast, for the treatment group the gap remains wide, and the long-run earnings gap remains at 30 percent seven years after giving birth to their first child.<sup>33</sup>

Overall, we find that the changes in grandmothers' labor supply decisions do not only affect maternal labor supply in the short run, but there are also dynamic spillover effects in the long run. The lack of childcare, through grandmothers delaying their retirement, leads to a significant negative impact on mothers' long-run labor supply and a substantial increase in the child penalty.

## 7 Conclusion

In recent years it has become more and more common for researchers and policy makers to propose incentivizing workers to delay their retirement with the goal of ensuring the solvency of the public pension system. However, such pension reforms may lead to spillover effects across generations that might counteract or even reverse the original effects in terms of total labor supply. Further, the reforms might have other indirect effects, including on (grand)children's educational performance. Exploiting a cohort-based reform in early retirement incentives in the Netherlands, this

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<sup>31</sup>In the Appendix Figure A.6, we show that also without this restriction we find dynamic treatment effects of the reform, albeit smaller.

<sup>32</sup>Note that this is a sample with only one child. The convergence of hours worked for the control group comes from the fact that we shut down the impacts from additional child births during the 7 years.

<sup>33</sup>Similarly, we also find that the reform leads to a slower recovery of monthly labor earnings. The dynamic treatment effects and the relative child penalty are smaller but have similar patterns for labor earnings, see Appendix Figures A.4 and A.5.

paper causally estimates the impact of grandparents' labor supply on the labor supply of different family members, most importantly the one of their adult daughters. We show that grandmothers working one hour per month more causes their adult daughters with young children to work half an hour less. In contrast, the change in grandmothers' labor supply does not affect adult daughters without child or with older children, nor does it affect sons or daughters-in-law. While grandfathers' labor supply responds strongly to the reform, there are no spillover effects on adult children.

While our results on extended family members (other than adult daughters with young children) already points to the importance of childcare needs and time availability of the grandmother, the richness of our Dutch data allows us to investigate the underlying mechanisms even further. We show that the proximity of grandmothers and the competition for grandmothers' time indeed matter. Grandmothers living closer and grandmothers with only one young maternal grandchild have more substantial impacts on maternal employment. Moreover, we show that grandmothers' and grandparents' overall household income is not affected by the reform. Our results thus point to the importance of intergenerational time transfers.

In addition to short-run spillover effects, we investigate long-run effects on grandchildren and the dynamic effects on maternal labor market outcomes and the child penalty. In particular, we analyze the spillover effects of the reform on grandchildren's performances in the Cito test at the end of primary school, which determines the track choice in secondary school. Children's performance on this test, therefore, has critical implications for their opportunities to enroll in college and for later labor and marriage market outcomes. We find important positive effects of the reform on the Cito test performance of children aged 4 to 7, who have experienced a substitution away from grandparental to maternal care. In particular, girls perform substantially better on the mathematical component of the test and the overall results, while boys' verbal performance improves. While we do not find effects for children aged 8 to 10, we find substantial negative effects on children aged 11 to 12, which are predominantly driven by boys who perform worse on the test and are less likely to receive recommendation for the academic track in secondary school. Supplementing our data with information on childcare take-up, we find that for this age group take-up and hours in after-school care increases. On the other hand, mothers' labor supply remains unchanged for this age group, suggesting a substitution away from grandparental care to formal after-school care and possibly time unsupervised at home.

The reform also has important dynamic effects on maternal labor market outcomes. Mothers of young children whose (grand)mothers were treated by the reform (and whose time availability for childcare has decreased) recover much more slowly in terms of hours worked and labor earnings than mothers whose (grand)mothers could retire earlier. The reform therefore has critical implications for the long-term labor market outcomes of women who are often still at the beginning of their career and for the implied child penalty and gender wage gap.



Our findings have two important policy implications. First, our results show that pension reforms aimed at increasing the labor market attachment for the elderly can have unintended and critical consequences for younger generations, including their adult daughters and grandchildren. Spillover effects of such policy reforms might reinforce or even offset direct reform effects. Following the framework proposed by [Hendren and Sprung-Keyser \(2020\)](#), we calculate the Marginal Value of Public Funds of the Dutch reform studied in this paper. Assuming the government only cares about income tax revenue, and the impact on maternal labor supply lasts for up to eight years (as our results on the dynamic effects suggest), we show that the loss in tax revenue from the drop in maternal labor supply outweighs the gain in tax revenue from delaying the retirement of the grandmothers.<sup>34</sup> Second, our findings in terms of children’s educational outcomes suggest the importance of quality of care. We show that unsupervised time at home due to the lack of grandparental care and/or low-quality formal care can negatively affect the performance of high-stake tests with critical long-run implications. The positive effects we find for children aged 4 to 7 underlines the importance of high-quality care for children’s cognitive development. Such high-quality care options can be made possible and shared by both parents through generous parental leave policies or by improving the quality of formal childcare choices.

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<sup>34</sup>See Appendix C for detailed steps of the calculation

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## 8 Tables and Figures

### List of Tables

1	Summary statistics . . . . .	41
2	Fuzzy RD estimates - impact on mothers' labor supply . . . . .	42
3	Fuzzy RD estimates - by age of youngest child . . . . .	43
4	Mechanisms I: Heterogeneous Effects . . . . .	44
5	Mechanisms II: Grandfathers' vs. grandmothers' effect . . . . .	45
6	Mechanisms III: Impacts on other family members . . . . .	46
7	Effects on children's educational performance (reduced-form) . . . . .	47
8	Effects on children's educational performance by gender (reduced-form) . . . . .	48
A1	Impacts on covariates (reduced-form) . . . . .	10
A2	Impacts on grandmothers' labor supply (reduced-form) . . . . .	11
A3	Impacts on mothers' labor supply (reduced-form) . . . . .	12
A4	Impacts on grandfathers' labor supply (reduced-form) . . . . .	13
A5	Placebo test - impacts on labor supply of mothers with deceased grandmothers (reduced-form) . . . . .	14
A6	Placebo tests using other birth dates as cutoffs (reduced form) . . . . .	15
A7	Fuzzy RD estimates by polynomial orders . . . . .	16
A8	Fuzzy RD estimates by CCT optimal bandwidth and polynomial orders . . . . .	17
A9	Impacts on mother's fertility outcomes (reduced-form) . . . . .	18
A10	Impacts on sample selection . . . . .	19
A11	Impacts on sub-sample selection . . . . .	20
A12	Summary statistics of children . . . . .	21
A13	Smoothness of children's covariates (reduced-form) . . . . .	22
A14	Impacts on CITO-sample selection . . . . .	23
A15	Impacts on subsidy take-up (reduced-form) . . . . .	24

### List of Figures

1	Survey Evidence on Childcare Modes . . . . .	49
2	Smoothness of the density around the cutoff . . . . .	50
3	Grandmothers' labor supply relative to the cutoff (First-stage) . . . . .	51
4	Mothers' labor supply relative to the cutoff (Reduced-form) . . . . .	52
5	Dynamic Treatment Effects on Monthly Working Hours . . . . .	53
6	Relative child penalty by treatment status . . . . .	54
A.1	Distribution of age at exiting employment for women by treatment status . . . . .	25
A.2	Placebo test: F-statistics as a function of the location of cutoff . . . . .	26
A.3	Placebo test: placebo estimates as a function of the location of cutoff . . . . .	27
A.4	Dynamic Treatment Effects on Monthly Labor Earnings . . . . .	28
A.5	Relative child penalty by treatment status . . . . .	29
A.6	Dynamic Treatment Effects on Monthly Working Hours (incl. multiple births) . . . . .	30

Table 1: Summary statistics

Variables	All		Full sample		RD sample	
	Mean	s.d.	Mean	s.d.	Mean	s.d.
<b>Grandmothers' characteristics</b>						
Birth cohort	1949.51	[1.121]	1949.48	[1.117]	1949.53	[0.499]
Age	62.516	[0.292]	62.878	[1.110]	62.957	[1.123]
Number of adult children	2.510	[1.016]	2.475	[0.982]	2.469	[0.983]
Number of adult daughters	1.729	[0.800]	1.712	[0.785]	1.702	[0.780]
<b>Mothers' characteristics</b>						
Age	35.254	[4.521]	37.884	[2.938]	37.924	[2.952]
Age at first child birth	29.691	[4.200]	28.337	[3.477]	28.378	[3.518]
Age at first employment	23.589	[3.916]	24.855	[3.888]	24.726	[3.720]
Married	0.466	[0.470]	0.661	[0.460]	0.657	[0.461]
Age gap to partner	2.663	[4.395]	2.809	[3.995]	2.786	[3.987]
Number of children	1.246	[1.087]	2.015	[0.763]	2.017	[0.767]
<b>Outcomes: grandmothers' labor supply</b>						
Monthly hours worked	44.023	[48.245]	37.230	[48.712]	37.315	[48.628]
Prob (Employed)	0.474	[0.419]	0.415	[0.446]	0.417	[0.447]
Prob (Full-time employed)	0.067	[0.212]	0.055	[0.204]	0.053	[0.200]
Monthly labor income	803.52	[1019.71]	638.99	[947.15]	637.76	[935.73]
Monthly gross income	1635.15	[1552.12]	1419.83	[1344.81]	1395.29	[1315.91]
Monthly HH labor earnings	1725.80	[1966.94]	1307.63	[1739.54]	1287.62	[1713.83]
Age at exiting employment	61.103	[4.304]	60.812	[4.388]	60.786	[4.442]
Age at claiming pension	63.039	[3.171]	62.954	[3.230]	62.912	[3.189]
<b>Outcomes: mothers' labor supply</b>						
Monthly hours worked	97.255	[51.482]	78.498	[47.458]	78.176	[47.388]
Prob (Employed)	0.816	[0.334]	0.784	[0.377]	0.782	[0.378]
Prob (Full-time employed)	0.232	[0.355]	0.063	[0.209]	0.061	[0.205]
Monthly labor income	1844.71	[1219.59]	1531.13	[1188.53]	1533.70	[1193.38]
Monthly HH labor income	4280.80	[2477.81]	4525.05	[2677.43]	4546.31	[2697.09]
Obs. Mothers	147858		66252		23497	
Obs. Grandmothers	106036		55055		19548	

*Notes:* Table 1 reports means and standard deviations. Columns 1 and 2 consist of all (extended) families – not necessarily living in the same household – with grandmothers born between 1948 and 1951 who are Dutch, have worked at least one month in their lives, have not claimed disability insurance before age 55, and who are still alive by age 65. Columns 3 and 4 restrict the sample to (extended) families with grandmothers with the youngest grandchild aged 4-12 when the grandmother is between 60 and 64. Columns 5 and 6 are the RD sample, which is the sample of Columns 3 and 4 restricted to families with grandmothers born within a bandwidth of 8 months before and after January 1950. Grandmothers and mothers' labor supply is measured when the grandmother is between age 60 and 64. All income measures are CPI adjusted for the year 2015.

*Source:* Authors' calculations from the CBS data.

Table 2: Fuzzy RD estimates - impact on mothers' labor supply

	Fuzzy RD estimates					Means at cutoff
	(1)	(2)	(3)	(4)	(5)	
<b>First-stage estimates - impact on GM's total labor supply</b>						
Total monthly hours worked	6.801*** [1.493]	6.831*** [1.392]	6.174*** [1.340]	5.947*** [1.556]	5.398*** [1.086]	34.418 [47.608]
<b>Impact on mothers' labor supply</b>						
Total monthly hours worked	-0.469** [0.219]	-0.413** [0.204]	-0.465** [0.229]	-0.630** [0.298]	-0.405* [0.207]	78.876 [47.744]
<i>Other labor supply measures</i>						
Prob(employed)	-0.003* [0.002]	-0.003* [0.002]	-0.003* [0.002]	-0.004* [0.002]	-0.003* [0.002]	0.785 [0.378]
Prob(full-time employed)	-0.001 [0.001]	-0.001 [0.001]	-0.001 [0.001]	-0.001 [0.001]	-0.002* [0.001]	0.066 [0.214]
F-stat	20.75	24.09	21.22	14.62	24.69	
Obs. (Mothers)	23497	23497	23497	17930	34592	4018
Obs. (Grandmothers)	19548	19548	19548	14959	28739	
Bandwidth	8	8	8	6	12	
Controls	NO	YES	YES	YES	YES	
Sector FE	NO	NO	YES	YES	YES	

*Notes:* Table 2 shows the coefficient estimates of grandmothers' total monthly hours worked on mothers' labor supply. The top panel reports first-stage estimates and the bottom panel reports 2SLS fuzzy RD estimates. An indicator for the grandmother being born since January 1950 serves as the instrument for grandmother's total monthly hours worked. Columns 1, 2, and 3 show the results without controls, with controls, and with both controls and sector fixed effects, respectively. We use local linear regressions with a bandwidth of 8 months (optimal bandwidth, generated by the [Calonico et al. \(2017\)](#) and [Calonico et al. \(2018\)](#) procedure). Columns 4 and 5 show local linear regressions with a bandwidth of 6 and 12 months, respectively. Sample means at the cutoff (measured in the three months before the cutoff) are reported in Column 6. All outcomes are measured when the grandmothers are between ages 60 and 64. Robust standard errors clustered at grandmothers' level are in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

*Source:* Authors' calculations from the CBS data.

Table 3: Fuzzy RD estimates - by age of youngest child

	Childcare need				
	more			no/less	
	Age of the youngest child				
	0-3 (1)	4 - 7 (2)	8-12 (3)	13 - 18 (4)	No child (5)
<b>Impact on mothers' labor supply</b>					
Total monthly hours worked	-0.004 [0.161]	-0.534** [0.245]	-0.410 [0.281]	0.118 [0.482]	0.046 [0.353]
<i>Other labor supply measures</i>					
Prob (Employed)	0.001 [0.001]	-0.003* [0.002]	-0.004 [0.002]	-0.002 [0.004]	0.001 [0.002]
Prob (Full-time employed)	-0.001 [0.001]	-0.001 [0.001]	-0.001 [0.001]	0.003 [0.003]	0.001 [0.004]
F-stat	30.01	19.25	14.82	5.75	6.27
Obs. (Mothers)	25450	20540	11378	4983	12289
Obs. (Grandmothers)	20987	17519	10145	4583	10878

*Notes:* Table 3 shows the coefficient estimates of grandmothers' total monthly hours worked on the mother's labor supply by the age of the youngest child (Fuzzy RD estimates). An indicator for the grandmother being born since January 1950 serves as the instrument for grandmother's total monthly hours worked. Columns 1 - 3 show the results for families with childcare need based on the age of the youngest child (0-3, 4-7, 8-12, respectively). Columns 4 and 5 show results for families with little or no childcare need (youngest child aged 13 - 18 and without children, respectively). All outcomes are measured when the grandmothers are between ages 60 and 64. All specifications use local linear regression with a bandwidth of 8 months including controls and sector fixed effects. Robust standard errors clustered at grandmothers' level are in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

*Source:* Authors' calculations from the CBS data.

Table 4: Mechanisms I: Heterogeneous Effects

Subgroups	Grandmother's partner is		Grandmother's residence municipality is		Number of maternal grandchildren aged 4-7	
	unhealthy	healthy	different	same	only one	more or none
	(1)	(2)	(3)	(4)	(5)	(6)
<b>Impact on mothers' labor supply</b>						
Total monthly hours worked	-0.051 [0.499]	-0.535** [0.257]	-0.162 [0.251]	-0.820** [0.413]	-0.829* [0.459]	-0.321 [0.259]
test p-value	0.386		0.170		0.335	
<i>Other labor supply measures:</i>						
Prob (Employed)	0.003 [0.005]	-0.004** [0.002]	-0.000 [0.002]	-0.007** [0.003]	-0.007* [0.004]	-0.001 [0.002]
test p-value	0.160		0.082		0.183	
Prob (Full-time employed)	-0.004 [0.003]	-0.001 [0.001]	-0.000 [0.001]	-0.002 [0.002]	-0.001 [0.002]	-0.002 [0.001]
test p-value	0.438		0.281		0.818	
F-stat	3.34	17.45	15.48	9.63	7.90	13.75
Obs. (Mothers)	1761	21734	10385	13112	7452	16045

*Notes:* Table 4 shows heterogeneous effects of grandmothers' total monthly hours worked on mothers' labor supply outcomes (Fuzzy RD estimates). Columns 1 and 2 show the results by the health status of the grandmother's partner. Partners are defined as healthy if they haven't claimed any disability insurance before age 54. Columns 3 and 4 show the results by the proximity of adult daughters (mothers) to grandmothers. We define the grandmother to be nearby when mother and grandmother live in the same municipality. Columns 5 and 6 show the results by the number of maternal grandchildren aged 4-7. All outcomes are measured when the grandmothers are between ages 60 and 64. All specifications use local linear regression with a bandwidth of 8 months including controls and sector fixed effects. Robust standard errors clustered at grandmothers' level are in parentheses. The p-values are from a test of the hypothesis that the coefficients are equal. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

*Source:* Authors' calculations from the CBS data.

Table 5: Mechanisms II: Grandfathers' vs. grandmothers' effect

Family member	Grandfathers'		Grandmothers'	
	Daughter (1)	Partner (2)	Daughter (3)	Partner (4)
<b>Impact of grandparent's total monthly hours worked on family members' labor supply</b>				
Total monthly hours worked	-0.066 [0.048]	0.039 [0.053]	-0.465** [0.229]	-0.148 [0.279]
<i>Other labor supply measures:</i>				
Prob (Employed)	-0.001** [0.000]	0.000 [0.000]	-0.003* [0.002]	-0.003 [0.002]
Prob (Full-time employed)	0.000 [0.000]	0.000* [0.000]	-0.001 [0.001]	0.001 [0.002]
F-stat	184.31	172.10	21.22	20.40
Obs. (Partners/ Daughters)	23609	19840	23497	16224
Obs. (Grandparents)	19766	19753	19548	16182

*Notes:* Table 5 shows the coefficient estimates of grandparents' total monthly hours worked on their partners' and adult daughters' (mothers') labor supply (Fuzzy RD estimates). An indicator for the grandparent being born since January 1950 serves as the instrument for the grandparent's total monthly hours worked. All outcomes are measured when the grandparent affected by the reform is between age 60 and 64. All specifications use local linear regression with a bandwidth of 8 months including controls and sector fixed effects. Robust standard errors clustered on the grandparent's level are in parenthesis. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

*Source:* Authors' calculations from the CBS data.

Table 6: Mechanisms III: Impacts on other family members

Grandmothers' family member:	Youngest child aged 4-12			
	Daughters (1)	Sons-in-law (2)	Sons (3)	Daughters-in-law (4)
<b>Impact on family members' labor supply</b>				
Total monthly hours worked	-0.465** [0.229]	0.648** [0.310]	0.581 [0.407]	-0.038 [0.290]
<i>Other labor supply measures:</i>				
Prob (Employed)	-0.003* [0.002]	0.004** [0.002]	0.002 [0.002]	-0.001 [0.002]
Prob (Full-time employed)	-0.001 [0.001]	0.002 [0.002]	0.002 [0.003]	-0.000 [0.001]
HH labor income		14.491 [11.685]		14.886 [14.154]
F-stat	21.22	22.09	10.87	11.59
Obs. (Family members)	23497	21530	16773	16531
Obs. (Grandmothers)	19548	18128	14374	14192

*Notes:* Table 6 shows the coefficient estimates of grandmothers' total monthly hours worked on other family members' labor supply, namely adult sons, daughters-in-law, and sons-in-law of the grandmothers (Fuzzy RD estimates). An indicator for the grandmothers being born since January 1950 serves as the instrument for the grandmother's total monthly hours worked. Household income is only considered for daughters/ sons with a partner, i.e., for this outcome the number of observations and F-statistics of column (2)/(4) apply. All outcomes are measured when the grandmothers are between ages 60 and 64. All specifications use local linear regression with a bandwidth of 8 months, including controls and sector fixed effects. All income measures are CPI adjusted for the year 2015. Robust standard errors clustered at grandmother's level are in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

*Source:* Authors' calculations from the CBS data.

Table 7: Effects on children’s educational performance (reduced-form)

RD estimates	Cito	Number of correct answers			High	Obs. (Children)
	score (1)	Verbal (2)	Math (3)	Overall (4)	track (5)	
<b>Panel A: All youngest children</b>						
Age between <b>4 - 12</b>	0.591 [0.433]	0.801 [0.747]	1.040* [0.632]	2.148 [1.318]	0.007 [0.016]	8436
Means at cutoff	534.296	83.872	50.302	142.901	0.161	
<b>Panel B: By age groups</b>						
Age between <b>4 - 7</b>	1.634*** [0.527]	2.557*** [0.986]	3.152*** [0.833]	5.665*** [1.676]	0.032 [0.021]	5500
Means at cutoff	534.818	89.546	54.644	144.568	0.185	
Age between <b>8 - 10</b>	0.734 [0.560]	1.313 [0.965]	0.807 [0.796]	2.469 [1.687]	0.012 [0.020]	5585
Means at cutoff	533.693	85.384	50.227	145.841	0.158	
Age between <b>11 - 12</b>	-1.270* [0.768]	-2.256** [1.076]	-2.625*** [0.912]	-4.082* [2.120]	-0.037 [0.025]	2868
Means at cutoff	533.114	72.298	41.286	139.104	0.116	

*Notes:* Table 7 shows reduced-form impacts on education outcomes of children who are aged 4 - 12 when their grandmothers were between ages 60 and 64. Panel A shows results for all youngest children of a family aged 4 - 12. Panel B presents results separately for the youngest children aged 4 - 7, 8 - 10, and 11 - 12. Column 1 shows the impact on the overall Cito score. Columns 2 - 4 report effects on the number of correct answers in the verbal part, mathematical part, and in the overall test, respectively. Column 5 shows the impact on the probability of obtaining a secondary school recommendation for the highest (academic) track (VWO). All specifications use local linear regression with a bandwidth of 8 months and include controls. Robust standard errors (clustered by the primary school the child attends) are in parenthesis.. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

*Source:* Authors’ calculations from the CBS data.

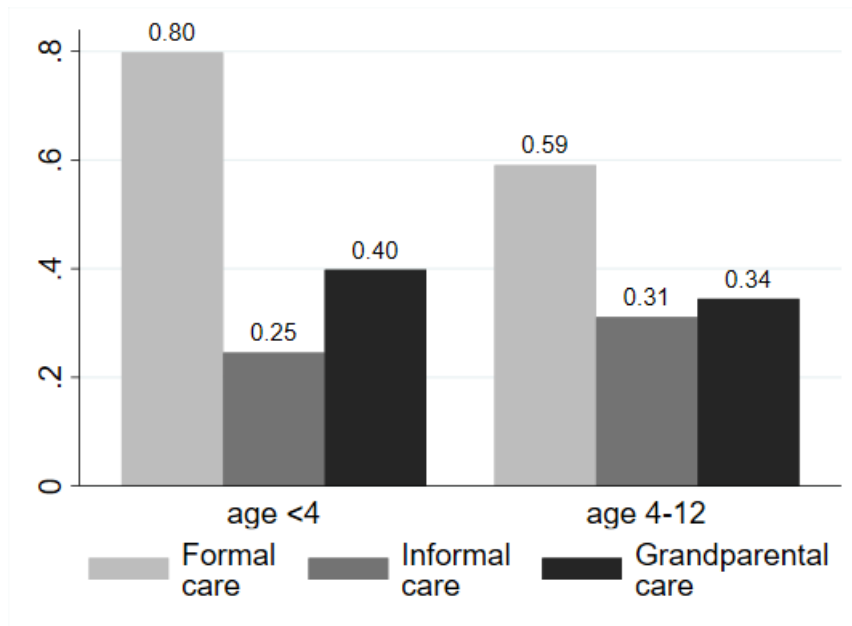


Table 8: Effects on children's educational performance by gender (reduced-form)

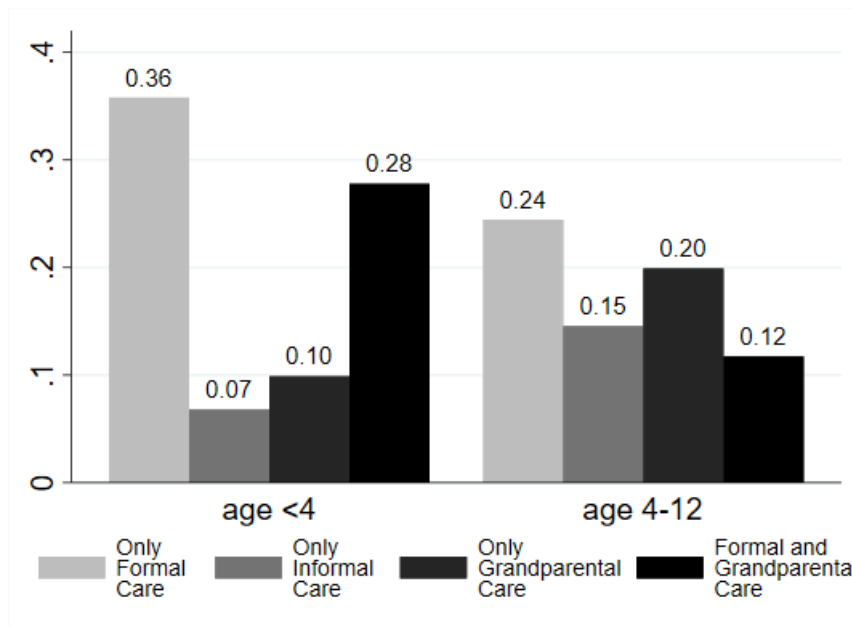
RD estimates	Cito score (1)	Number of correct answers			High track (5)	Obs. (Children)
		Verbal (2)	Math (3)	Overall (4)		
<b>Age between 4 - 7</b>						
Girls	1.866*** [0.729]	1.852 [1.317]	4.287*** [1.180]	6.301*** [2.330]	0.046 [0.029]	2785
Boys	1.217 [0.794]	3.177** [1.502]	1.663 [1.211]	4.546* [2.517]	0.012 [0.031]	2715
p-value	0.125	0.035	0.170	0.071	0.692	
<b>Age between 8 - 10</b>						
Girls	0.983 [0.776]	1.340 [1.320]	1.743 [1.130]	3.350 [2.362]	0.044 [0.028]	2847
Boys	0.375 [0.811]	0.875 [1.396]	-0.050 [1.132]	1.262 [2.428]	-0.023 [0.029]	2738
p-value	0.644	0.531	0.965	0.603	0.432	
<b>Age between 11 - 12</b>						
Girls	-1.128 [1.078]	-1.768 [1.462]	-1.878 [1.323]	-3.420 [2.983]	-0.017 [0.036]	1459
Boys	-2.024* [1.146]	-4.361*** [1.590]	-3.313** [1.343]	-6.473** [3.156]	-0.067* [0.036]	1409
p-value	0.077	0.006	0.014	0.040	0.058	

Notes: Table 8 shows reduced-form impacts on education outcomes by gender (for children aged 4 - 7, 8 - 10, and 11 - 12 when their grandmothers were between ages 60 and 64). Column 1 shows the impact on the overall Cito score. Columns 2 - 4 report effects on the number of correct answers in the verbal part, mathematical part, and overall test, respectively. Column 5 shows the impact on the probability of obtaining a secondary school recommendation for the highest (academic) track (VWO). All specifications use local linear regression with a bandwidth of 8 months and include controls. Robust standard errors (clustered by the primary school the child attends) are in parenthesis. The p-values are from a test of the hypothesis that the coefficients for girls and boys are equal. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

Source: Authors' calculations from the CBS data.



(a) Distribution of childcare modes

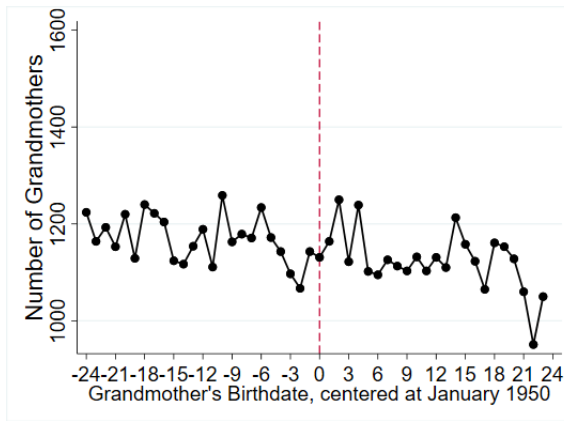


(b) Distribution of care mix

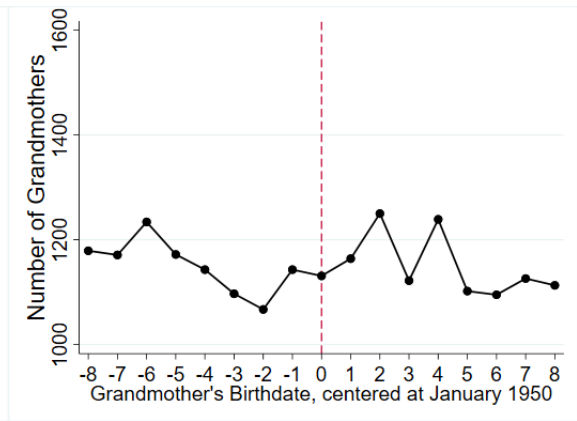
### Figure 1: Survey Evidence on Childcare Modes

*Notes:* Figure 1 shows child care modes employed by parents in the LISS panel. Parents are asked separately for their children below 4 and between ages 4 and 12 whether they make regular use (at least once a week) of various types of child care. Panel (a) shows childcare take-up allowing for multiple answers so that the three categories are not mutually exclusive. Panel (b) shows the four most common combinations of child care modes with mutually exclusive categories.

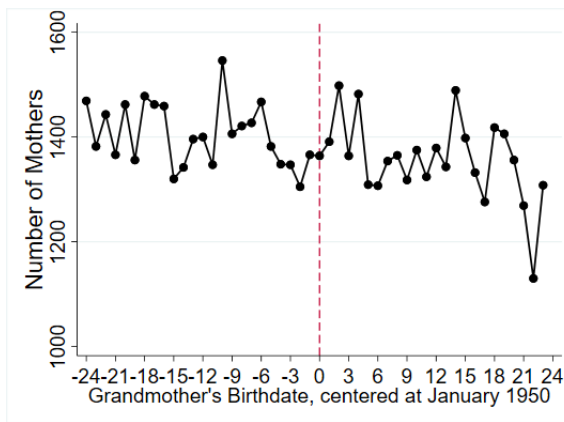
*Source:* Authors' own calculations from LISS panel administered by CentERdata (Tilburg University, The Netherlands).



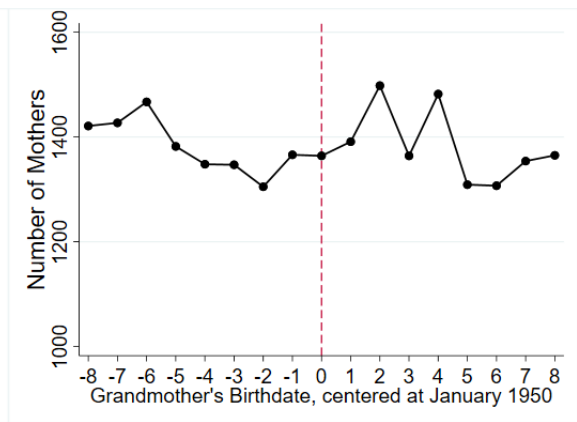
(a) Density of Grandmothers (24 months)



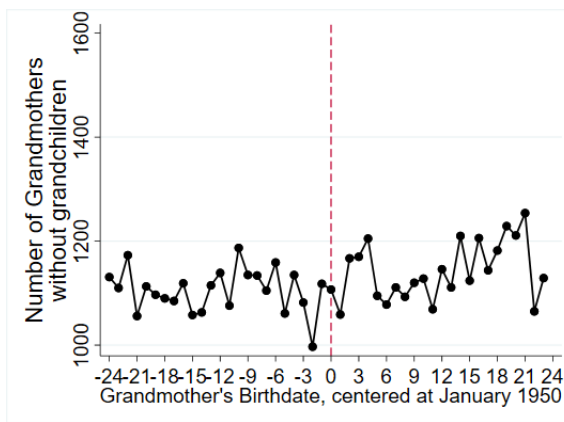
(b) Density of Grandmothers (8 months)



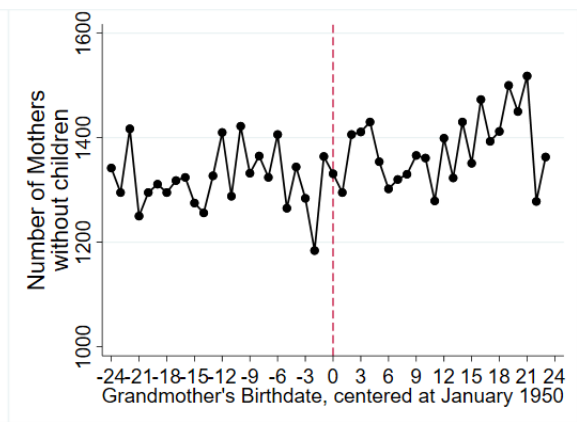
(c) Density of Mothers (24 months)



(d) Density of Mothers (8 months)



(e) Density of Grandmothers without Grandchildren



(f) Density of Mothers without Children

### Figure 2: Smoothness of the density around the cutoff

Notes: The (bin size/running variable) in Figure 2 is grandmothers' birth date/months. Figure 2a and 2b show the density plot of grandmothers 24 months and 8 months around the cutoff. Figure 2c and 2d show the density plot of mothers whose mothers' ('grandmother') birth month is 24 months and 8 months around the cutoff. Figure 2e and 2f show that the fluctuating patterns of the density plots for grandmothers and mothers of our baseline sample are not unique but a pattern that also shows up for 'grandmothers' and 'mothers' without (grand)children.

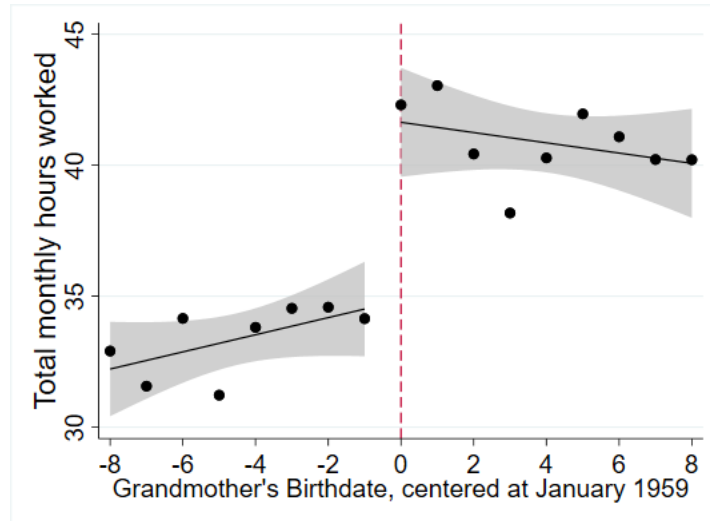
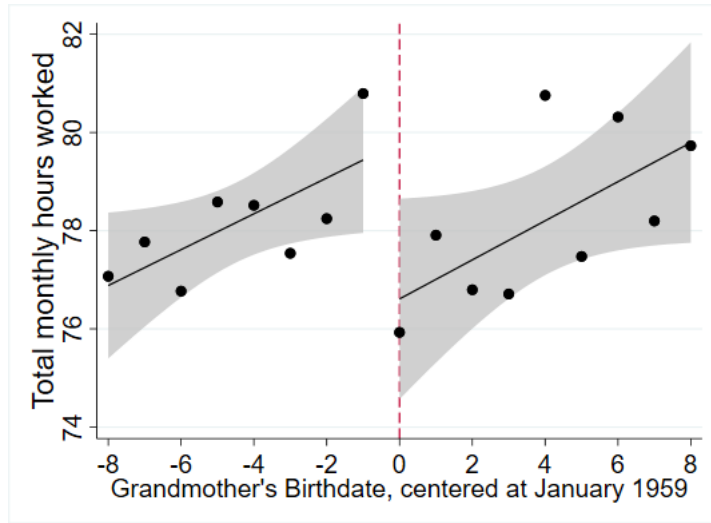
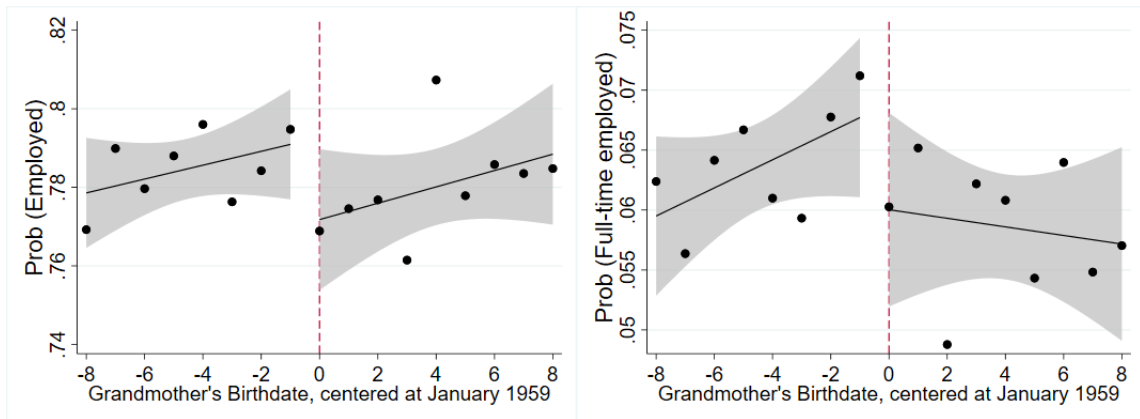


Figure 3: Grandmothers' labor supply relative to the cutoff (First-stage)

*Notes:* Figure 3 shows the scatter bin plot of grandmother's total monthly hours worked as a function of distance to the cutoff, which is grandmothers' birth month being January 1950. The solid lines are the linear fitted lines. The shaded areas indicate the 95 percent confidence interval.



(a) Total hours worked by mothers

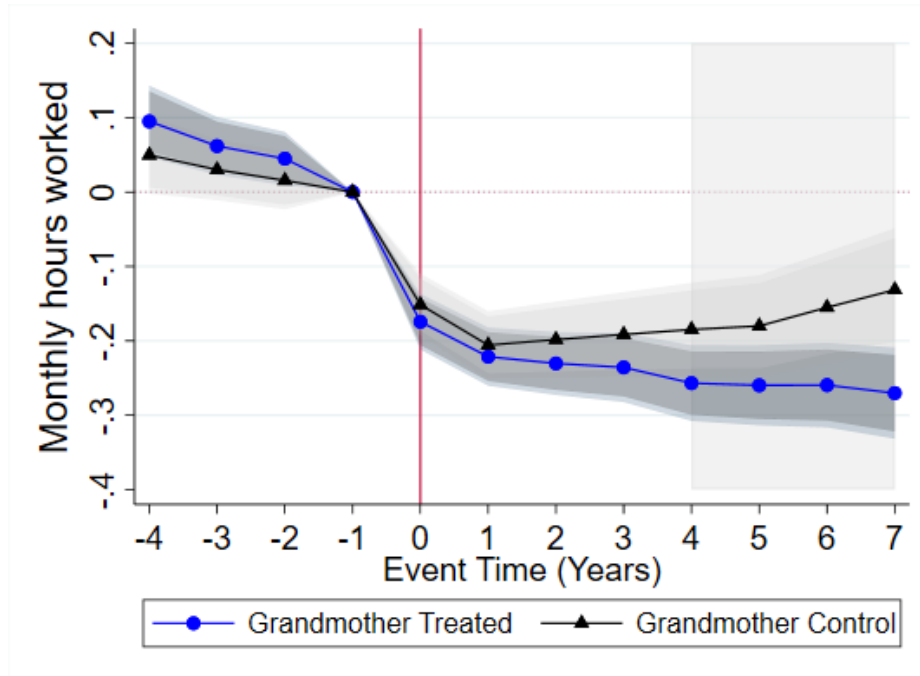


(b) Mothers' probability of employment

(c) Mothers' probability of full-time employment

### Figure 4: Mothers' labor supply relative to the cutoff (Reduced-form)

Notes: Figure 4 shows the scatter bin plots of mother's labor supply as a function of distance to the cutoff, which is grandmothers' birth month being January 1950. The solid lines are the linear fitted lines. The shaded areas indicate the 95 percent confidence interval.



**Figure 5: Dynamic Treatment Effects on Monthly Working Hours**

*Notes:* Figure 5 shows the evolution of mothers' total monthly hours worked from four years before and to seven years after they gave birth to their first child. It compares the monthly working hours of treated (blue dots) mothers, whose (grand)mothers are born since January 1950 and thus treated by the pension reform, to those of control (black triangles) mothers (with untreated (grand)mothers). Event time 0 marks the birth of the first child. The value at  $t = -1$  is normalized to zero so that coefficients measure the impact of the first child relative to the year before birth. The shaded area between event times 4 and 7 indicates the main reform spillover window on the second generation. To capture the extensive and intensive margin of labor supply simultaneously, total hours worked include zeros for women not working. Different tones of shaded areas indicate the 90 and 95 percent confidence intervals.

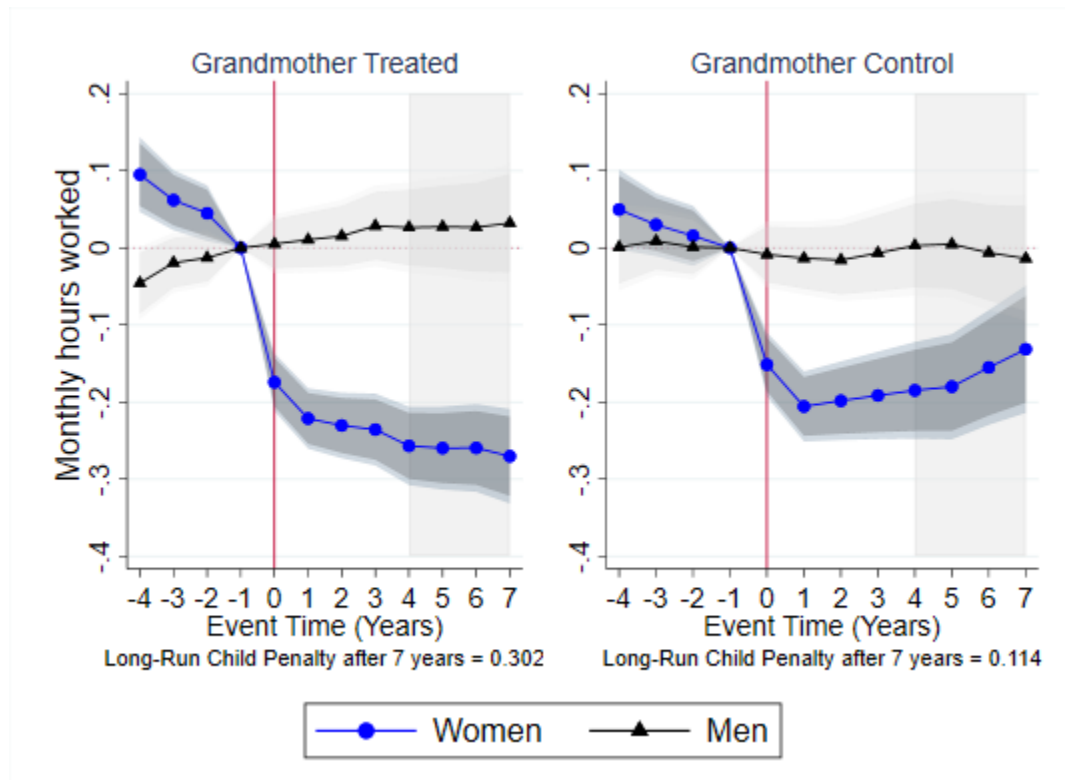


Figure 6: Relative child penalty by treatment status

*Notes:* Figure 6 depicts the child penalty in total monthly working hours (including zeros) by treatment status. The left panel presents the child penalty for treated grandmothers, i.e. for individuals whose (grand)mothers were born since January 1950 and thus treated by the pension reform, and the right panel for control grandmothers, i.e. for individuals whose (grand)mothers were born before January 1950 and thus not treated by the reform. In each of the two panels, blue dots document women's and black triangles indicate men's monthly working hours, the difference between which represents the child penalty. The long-run relative child penalty after 7 years (i.e., the relative loss women experience compared to men) is reported below each sub-graph. Event time 0 marks the birth of the first child. The value at  $t = -1$  is normalized to zero so that coefficients measure the impact of the first child relative to the year before birth. The shaded area between event times 4 and 7 indicates the main reform spillover window on the second generation. Different tones of shaded areas indicate the 90 and 95 percent confidence intervals.

# Spillover Effects of Old-Age Pension Across Generations: Family Labor Supply and Child Outcomes

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## Online Appendix

### A Additional Details on Institution

#### A.1 The evolution of Dutch early retirement schemes

The early retirement (ER) schemes are part of the collective labor agreements, which constitute the basis of the second pillar occupational pension schemes. In the Netherlands, the early retirement schemes were first introduced in the 1970s at a flat-rate and were financed on a pay-as-you-go basis. These schemes were initially designed to reduce youth unemployment. The replacement rates vary by sectors and even by firms within sectors but are generally considered financially attractive. The average replacement rate is 80 percent of previous gross earnings. The flat-rate ER schemes were attractive and not actuarially fair.<sup>1</sup> In the early 1990s, the Dutch social partners started to replace the flat-rate ER schemes with actuarially adjusted schemes due to concerns about the long-run financial sustainability. The ER scheme started transitioning from the generous and actuarially unfair pay-as-you-go VUT schemes towards capital-funded, actuarially fair, and less generous schemes. Under the new ER schemes, workers receive lower pension benefits if they retire earlier than the statutory retirement age. However, contributions to the ER schemes were tax-deductible. The tax advantage amounted to about 25% of the net early retirement allowance (Euwals et al. (2010)). Therefore, retiring early was common even under the new more actuarially fair ER schemes. Approximately 80% of all workers retired at the age of 62 or younger before 2006 (Netherlands (2009)).

The general plan to eliminate early retirement tax advantages was announced in 2000 by the first Balkenende cabinet. The goal was to encourage labor market participation of the elderly by speeding up the transition towards an actuarially fair early retirement system. The second Balkenende cabinet made several proposals to speed up the cancellation of the favorable tax treatment of the ER schemes in 2004, which has entailed one of the largest union demonstrations in Dutch history in October 2004. In November 2004, the proposal of bill No. 29760 was passed by the House of Representatives and adopted by the Senate in February 2005. The bill was published in the Official Gazette 115 of March 10, 2005. From that date onwards, all sectors and industries introduced new pension schemes that are more actuarially fair and flexible. For example, the Dutch government announced to replace the pre-pension with a new pension scheme called the ABP Flexible Pension

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<sup>1</sup>The flat-rate ER schemes were also called “VUT schemes.” In Dutch, VUT stands for “Vervroegde Uittreding” in Dutch, which means “early retirement”.



Scheme in July 2005. More generally, the reform bill no. 29760 includes a clause to adjust fiscal policy VUT and prepension (Wet voor aanpassing fiscal behandeling VUT and pre-pension) and is sometimes referred to as the 56-plus scheme (de 56-plusregeling). People, who were 55 years or older before January 1, 2005 (i.e. born before 1950) are not affected by the reform, while for people who turned 55 since January 1, 2005 (i.e. who were born since January 1, 1950) tax benefits for early retirement schemes were eliminated.

The reform bill no. 29760 includes a clause to adjust fiscal policy VUT and prepension (Wet voor aanpassing fiscal behandeling VUT and pre-pension) and is sometimes referred to as the 56-plus scheme (de 56-plusregeling). People, who were 55 years or older before January 1, 2005 are not affected by the reform.

## **A.2 The life course savings program**

In 2006, the Dutch government introduced the "Life course savings" (Levensloopregeling, LCS) program. This tax-facilitated savings program allows workers to save for periods of unpaid leave or early retirement. Employees can save up to 210 percent of their last wages, which equates to around two years of full income or two years with 70% of previous income. Each year employees can save up to 12% of annual earnings. This life-course savings program was abolished in 2012. However, people who started participating in the program prior to 2012 were still able to save tax-free in life course savings programs until 2021.

All individuals in our sample were eligible to participate in the life course savings program, which means both the grandmothers born before 1950 and since 1950 can use this new tax-facilitated saving scheme. However, individuals who were at least 50 years old but not yet 55 on 1 January 2005 (born since 1950) could save more than 12% per year. The policy intention was to provide a slight advantage for people aged 50 to 55 in 2005 to save quicker. This favorable treatment might wane the reform-induced rise in grandmother's labor supply because it was perceived as a way out of the labor market for the ones affected by the 2006 reform. However, we are not worried about the LCS plan as a confounding factor. First, both treated and control can use this new tax-facilitated saving scheme. If anything, the availability of the LCS plan makes our first stage estimates smaller. Moreover, in practice, only some high-wage workers manage to retire early using the LCS plan. [Lindeboom and Montizaan \(2020\)](#) shows around 15% of the 1950 cohort participated in the LCS plan, among which only 16% managed to counter the reform effect and maintain their previously planned retirement dates.

## **B More Details on Data**

### **B.1 Data sources**

Below we describe the different data sources used in the analysis. All datasets used are provided by Statistics Netherlands (CBS). Documentation for each of the files shortly described below can be found at the link provided below. Please note that these are only available in Dutch.

#### **gpapersoontab**

This file is updated yearly and provides information on the demographic background of the Dutch population that appears in the Personal Records Database (BRP) since 1 October 1994. Informa-

tion includes gender, date and country of birth, migration background.

For details see: [Official documentation of gpapersontab](#)

#### **kindoudertab**

This file links children to their legal parents and includes all registered individuals if the parents could be identified. The file refers to legal parents and thus includes biological as well as adoptive parents.

For details see: [Official documentation of kindoudertab](#)

#### **gbaoverlijdentab**

This file contains the date of death of all persons who have died since 1 October 1994 and who were registered in the Personal Records Database (BRP) on the date of death. The date of death of persons who are not residents but were once residents of the Netherlands since 1 October 1994 and whose death information has been registered in the Register Non-Residents (RNI) are also included.

For details see: [Official documentation of gbaoverlijdentab](#)

#### **gbahuishoudenbus**

This file provides information on all individuals who appear in the Basic Registration of Persons (BRP) from 1 October 1994. Information includes their position in the household and details about the family to which they belong/ belonged, such as family composition, number of children, marital status. The data is provided in the form of spells indicating each household's start and end date and individuals belonging to the family.

For details see: [Official documentation of gbahuishoudenbus](#)

#### **gbaadresobjectbus**

This file contains encrypted information on the addresses of persons who are or have been registered since 1 January 1995.

For details see: [Official documentation of gbaadresobjectbus](#)

#### **vslgwbt**

This file includes all objects from the Key Register of Addresses and Buildings (BAG) and all objects that were in the SSB before 1-1-2012 (historical objects). Information on the municipality in which the object is placed is provided on an annual basis.

For details see: [Official documentation of vslgwbt](#)

#### **secmbus**

This file contains monthly data on the socioeconomic category (SECM) of individuals registered since 1 January 1995. Information on separate sources of income, on which the SECM definition is based, is indicated for each relevant period. Also, the file indicates whether a person was registered as a student in the given month.

For details see: [Official documentation of secmbus](#)

#### **integraal persoonlijk inkomen**

This file contains the annual income of individuals belonging to the population of the Netherlands on 31st December of the study year. The main data supplier are the tax authorities. All individuals that appear in the basic tax register are included. Note that this file was replaced from 2011 on by INPATAB due to the revision of the income statistics 2017.

For details see: [Official documentation of integraal persoonlijk inkomen](#)

#### **CITOTab**

This file contains information on the performance on a high-stake test taken at the end of primary school, the Cito Primary School Final Test, used to assign children to different tracks in secondary

school. Until the school year of 2013/14, only the Cito Primary School Final Test was provided. Since 2014/15 schools could choose between three compulsory final tests, of which the test designed by Cito on behalf of the Board for Tests and Exams is included in our data. The data includes information on the test results including separate components (such as points on the verbal and mathematical part).

For details see: [Official documentation of CITOTab](#)

### **kinderopvang**

In each year, we observe information on the type and number of childcare hours in a reporting year for each child that used childcare under the Childcare Act.

For details see: [Official documentation of kinderopvang](#)

### **LISS panel**

The LISS panel is an online household panel. The panel consists of some 5000 households in the Netherlands, comprising approximately 7500 individuals over the age of 16. The panel is based on a true probability sample of households drawn from the population register by Statistics Netherlands. Every year, a longitudinal survey is fielded in the panel, covering many domains, including health, work, education, income, housing, time use, political views, values, and personality. More information about the LISS panel can be found at: [www.lissdata.nl](http://www.lissdata.nl)

We use the first wave collected in 2008 and restrict our focus on parents (i.e., individuals with children) whose own mother (i.e., the grandmother) is still alive. Parents are asked about child care arrangements separately for their children below age four and children aged 4-12 who do not attend secondary school yet. Parents of children were asked the following four questions based on which we calculate child care statistics.

“The following questions are about your living-at-home children born from 2004 onwards. This concerns your [CHILD/CHILDREN] [NAMES AND BIRTH YEARS CHILDREN BORN FROM 2004 ONWARDS]. For [THIS/THESE] [CHILD/CHILDREN], do you make regular use, that is at least once a week, of the following childcare options? If so, of which? More than one answer possible.” Parents could indicate for each of the following options whether they make regular use of it or not:

- a toddler playgroup, nursery school
- b child daycare center ('kindergarten' – also half-day childcare)
- c pre-school childcare
- d after-school childcare
- e host parent where the child goes to (arranged through a host parent agency)
- f paid child sitter, where the child goes to
- g paid child sitter, that comes to your home
- h unpaid child sitter (for instance family/friends/neighbors/acquaintances)
- i other childcare

j no, no childcare

If parents indicated to make use of an “unpaid child sitter” (option h), they were asked in addition: “Who is generally this unpaid child sitter? If more than one option applies, please indicate the person that you call on most often.” The answer could be one among the following options:

- i own parent (child’s grandparent)
- ii partner’s parent (child’s grandparent)
- iii neighbour
- iv friend/acquaintance
- v brother or sister (own or partner’s)
- vi other own (step/adoptive/foster) child
- vii someone else

Analogously, information on child care arrangement for children aged 4-12, who potentially attend primary or another type of school but do not attend secondary school yet, were collected.

“This concerns your living-at-home children born from 1995 onwards that do not attend secondary school: your [CHILD/CHILDREN] [NAMES CHILDREN BORN FROM 1995 ONWARDS THAT DO NOT ATTEND SECONDARY SCHOOL]. Do you make regular use, that is at least once a week, of the following childcare options for your children? If so, of which? More than one answer possible.” Again, parents could indicate for each of the following options whether they made regular use of it or not:

- a pre-school childcare
- b after-school childcare
- c between-school childcare
- d host parent where the child goes to (arranged through a host parent agency)
- e paid child sitter, where the child goes to
- f paid child sitter, that comes to your home
- g unpaid childcare (for instance family/friends/neighbors/acquaintances)
- h other childcare
- i no childcare

For those making regular use of a “unpaid childcare” (option g), a second question on the unpaid care giver followed.

“Who is generally this unpaid child sitter? If more than one option applies, please indicate the person that you call on most often.” The answer could be one among the following options:

- i own parent (child’s grandparent)
- ii partner’s parent (child’s grandparent)
- iii neighbour
- iv friend/acquaintance
- v brother or sister (own or partner’s)
- vi other own (step/adoptive/foster) child
- vii someone else

Additionally, we make use of two questions regarding parents’ work and child care arrangements. In case parents report to perform paid work (“even if is it just for one or several hours per week or for a brief period”), they are asked:

“Are you currently working less hours in order to care for your children? If you have children and you are on parental leave, then consider the hours that you have additionally started working less (so in addition to the parental leave)”

For those who indicated to currently work less, a follow-up question asked about the reduction in hours:

“How many hours per week did you start working less on account of the care for your children? Do not include the hours that you have possibly taken as your parental leave.” The answer could be any number of hours ranging from zero to 40.

## **B.2 Sample selection**

In Table [A10](#) we illustrate our sample construction step-by-step and show that our sample restrictions are smooth around the RD cutoff. Starting with all native Dutch grandmothers born eight months around January 1950 with at least one adult daughter, we show that exiting the labor market before age 50 is smooth around the cutoff and not very common with a likelihood of 38% (step 1). We exclude inactive grandmothers and test in step 2 whether the probability of living up to age 65 differs by treatment. Among our sample, the death rate before age 65 is 2% and does not differ between treated and control grandmothers. We exclude the small fraction of deceased grandmothers. In step 3, we test whether there is evidence of self-selection based on restrictions in terms of health status. Among both treated and non-treated grandmothers, 8% claim disability insurance before age 55. After excluding grandmothers claiming disability before age 55, sample restrictions based on grandmothers’ characteristics are complete.

To ensure the focus on the relevant sample, we make two additional restrictions based on mothers’ characteristics. Step 4 shows that almost 60% of mothers have a youngest child aged 4-12 when the grandmother is aged 60-64. Keeping only mothers with a youngest child aged 4-12 gives us a baseline sample of 23,497 mothers (and 19,548 grandmothers).

### B.3 Linkages to CITO data

At the end of primary school children have to take a standardized test, the *Cito test*, to determine the secondary school track they are admitted to. Since the academic year 2014/15, schools can choose between three different tests, the most important of which is the central final test administered by Cito. It is important to note that the schools, not the parents or children, select the type of test. The data includes information on the Cito test and only for children from schools that permitted Cito to pass on data to Statistics Netherlands. Overall 50% of our sample of children aged 4 to 12 can be matched.

The Cito-sample uses the youngest children aged 4-12 (when the grandmother is 60-64) to mothers of our baseline sample that can be matched to their Cito test results. Table A12 compares characteristics of all children aged 4-12 (see Columns (1) and (2)), to characteristics of the youngest children aged 4-12 (Columns (3) and (4)), and to the characteristics of the children in the Cito-sample (Columns (5) and (6)). Compared to all children of our baseline mothers, children in the Cito-sample are less likely to be firstborn and somewhat more likely to have parents who are married (when the grandmother was aged 50-53). Compared to all youngest children aged 4-12, these differences disappear as we focus on the youngest child in a family. In terms of other characteristics, the Cito-sample is comparable to all children and even more to all youngest children aged 4-12 of the baseline mothers. Comparing the average year of birth across the three samples, children in the Cito-sample are somewhat older than all children aged 4-12 due to the fact that since 2014/15 only part of the schools still rely on the Cito test which we have data on. Table A14 tests whether the Cito-sample restrictions and matching rates differ by treatment status. We show that among all children aged 4-12 (when the grandmothers are 60-64), the likelihood of being the youngest child in the given age range and matched to Cito results are not affected by grandmothers' treatment status. These results show that the restricted data availability of test scores does not constrain our analysis.

Table A13 reports the estimated impact of a grandmother being treated (i.e. born since January 1950) on a list of predetermined characteristics of the Cito-sample. We show results using a linear and quadratic specification with a bandwidth of 8 months around the cutoff. All covariates are smooth, so that treated and non-treated grandchildren are comparable in terms of children's covariates, such as, among other things, birth year and month, gender, birth order, and family situation.

## C Calculation of Marginal Value of Public Fund

To provide a comprehensive assessment of the benefits of pension reforms incentivizing later retirement relative to the costs, we follow the framework proposed by [Hendren and Sprung-Keyser \(2020\)](#) to calculate the Marginal Value of Public Funds (MVPF). The MVPF is the ratio of society's willingness to pay for incentivizing later retirement to the net cost to the government of implementing this policy.

At first glance, it seems unnecessary to calculate the MVPF for the policy of incentivizing later retirement. That is because the government's budget constraint is expanded mechanically by a less generous pension and behaviourally by the resulting prolonged working life. However, we find that adult daughters reduce their labor supply due to the pension reform, which could potentially

offset the gain in the government's budget. Therefore, in the following, we calculate the MVPF for the policy of incentivizing later retirement, taking into account the grandmothers' and mothers' labor supply responses.

The mechanical net cost of incentivizing later retirement is the changes in pension generosity. The 2006 reform changed pension replacement rates between age 60 and 64 from 70% to 64% (Lindeboom and Montizaan (2020) Table A.1) For a typical woman with average labor earnings of 727 euro and an average duration of pension claim of 17 years,<sup>2</sup> we calculate that the government saves about 8900 euro per person. If the government only cares about income tax and ignores the reduction in social security spending, the mechanical cost is zero.

The behavioral costs consist of the direct impact on grandmothers and the indirect spillover effects on mothers. First, we calculate the net cost of pension reform from the direct impact on grandmothers. The reduced form estimates show that grandmothers earn 106 euros more per month between age 60 and 64 (Table A2). The average monthly labor earning around the cutoff is 727 euros. Using the [Dutch Income Tax Calculator](#), we find that the government makes 484 euro more tax income during those four years. Moreover, people delay retirement and claim fewer years of pension benefits. Here, we provide a conservative measure and ignore the reduction in government spending due to delayed pension claiming.

Second, we include the spillover impacts on mothers' labor supply. The reduced form estimates show that mothers with youngest children aged between 4 and 12 earn 58 euros less (Table A3). Their average monthly labor earning around the cutoff is 2064 euros. This suggests that the government loses 264 euro tax income during those four years because mothers work less.

We could also include the spillover impacts on grandchildren. However, we find that children treated when they are young (ages 4-7) perform better, and children treated when older (ages 11-12) perform worse. The reduced-form estimates have similar magnitudes but opposite signs. Therefore, we do not include the spillovers on grandchildren on tax revenue. Based on the early childhood development literature, we know that return to investment on younger children is higher and can have a long-term impact on their lifetime earnings. Therefore, if we do include the spillover effects on grandchildren, we expect the impact on government tax revenue, in the long run, to be positive or at least non-negative due to the reform.

Finally, we calculate the net government costs of incentivizing later retirement is negative, approximately minus 8152 euros. If we assume the government only cares income tax revenue, the net government costs are minus 224. Although grandmothers work more has negative impacts on mothers' labor supply, this policy of incentivizing later retirement still pays for itself due to the significant cut in government spending on pension payments.

Note that we only consider the immediate short-run impact on mothers' labor supply. Section 6.2 shows that the reform has a long-run dynamic effect on mothers' lifetime income. Assuming the government only cares about income tax revenue and the impact on maternal labor supply lasts for up to eight years, we show that the loss in tax revenue from the drop in maternal labor supply outweighs the gain in tax revenue from delaying the retirement of the grandmother.

Of course, the net government costs may remain negative if we include the tax revenue gain from grandfathers working longer and the spillover effects on sons-in-laws' labor supply. Nevertheless, our MVPF exercise highlights the importance of spillover effects across generations in cost-benefit

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<sup>2</sup>The duration of pension claim is the length of the period between pension claim age (65 years old) and death (life expectancy of 82 years old).

analysis and the optimal design of public policies.

## **D Appendix Tables and Figures**



Table A1: Impacts on covariates (reduced-form)

	RD estimates		Mean at cutoff
	(1)	(2)	
<b>Grandmothers' characteristics</b>			
Age	0.044 [0.031]	0.027 [0.051]	62.934 [1.144]
Number of adult children	0.055* [0.032]	0.022 [0.051]	2.451 [0.892]
Number of adult daughters	0.024 [0.027]	-0.003 [0.043]	1.712 [0.761]
Prob (Employed)	-0.006 [0.011]	0.024 [0.0174]	0.793 [0.338]
Prob (Married)	-0.008 [0.011]	0.012 [0.018]	0.835 [0.355]
Prob (Cohabit)	0.003 [0.006]	0.005 [0.009]	0.036 [0.166]
Prob (Partner disabled)	0.016** [0.008]	0.016 [0.012]	0.062 [0.235]
Birthcohort of partner	0.093 [0.129]	-0.169 [0.214]	1947.50 [3.849]
<b>Mothers' characteristics</b>			
Age	0.124 [0.081]	0.091 [0.132]	37.867 [2.899]
Native	0.007 [0.006]	0.019* [0.010]	0.953 [0.211]
Birth cohort	-0.074 [0.091]	-0.065 [0.147]	1974.45 [3.211]
Prob (Married)	0.006 [0.012]	-0.000 [0.020]	0.369 [0.434]
Prob (Employed)	-0.005 [0.009]	-0.008 [0.015]	0.772 [0.333]
Live in same municipality as GM	0.003 [0.013]	0.037* [0.021]	0.552 [0.461]
Age at first child birth	0.080 [0.010]	0.123 [0.160]	28.268 [3.609]
Age of youngest child	-0.042 [0.071]	0.084 [0.111]	2.059 [2.022]
Age of oldest child	-0.083 [0.117]	-0.086 [0.190]	3.790 [3.164]
Number of children	0.002 [0.027]	-0.025 [0.044]	0.842 [0.969]
Age of first employment	0.060 [0.105]	0.032 [0.173]	24.891 [3.809]
Obs. (Mothers)	23497	23497	4018
Obs. (Grandmothers)	19548	19548	
Polynomial	linear	quadratic	

*Note:* We test the impact of grandmothers being born since January 1950 on a list of the grandmothers' and mothers' characteristics. All variables are predetermined and refer to times when the grandmother was aged 50 to 53. Prob(employed) refers to formal employment only. All specifications use local linear regression with a bandwidth of 8 months. Robust standard errors clustered at grandmother's level are in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. *Source:* Authors' calculations from the CBS data.

Table A2: Impacts on grandmothers' labor supply (reduced-form)

	RD estimates			Means at cutoff
	(1)	(2)	(3)	
<b>First-Stage RD estimates</b>				
Total monthly hours worked	6.801*** [1.493]	6.831*** [1.392]	6.174*** [1.340]	34.418 [47.608]
<i>Other labor supply measures:</i>				
Prob (Employed)	0.063*** [0.014]	0.063*** [0.013]	0.054*** [0.012]	0.387 [0.438]
Prob (Full-time employed)	0.007 [0.006]	0.006 [0.006]	0.007 [0.006]	0.054 [0.202]
<i>Other income measures:</i>				
Monthly labor earnings	121.767*** [27.798]	123.340*** [26.076]	106.286*** [24.961]	573.065 [887.669]
Monthly HH labor earnings	126.754** [50.983]	142.272*** [50.071]	122.952** [49.318]	1211.88 [1647.46]
Monthly gross income	64.678 [40.259]	66.114* [36.270]	45.148 [34.101]	1361.66 [1304.62]
Monthly gross HH income	15.901 [66.725]	49.140 [64.097]	28.525 [62.482]	4082.91 [2144.97]
Obs. (Mothers)	23497	23497	23497	4005
Obs. (Grandmothers)	19548	19548	19548	
Controls	NO	YES	YES	
Sector FE	NO	NO	YES	

Notes: Table A2 shows the first-stage reduced form impacts on grandmothers' labor supply and income measures. Columns 1, 2, and 3 show the results without controls, with controls, and with both controls and sector fixed effects, respectively. All specifications use local linear regression with a bandwidth of 8 months. Robust standard errors clustered at grandmother's level are in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

Source: Authors' calculations from the CBS data.

Table A3: Impacts on mothers' labor supply (reduced-form)

	RD estimates			Means at cutoff
	(1)	(2)	(3)	
<b>Mothers' labor supply outcomes</b>				
Total monthly hours worked	-3.193** [1.300]	-2.818** [1.259]	-2.871** [1.259]	78.876 [47.744]
<i>Other labor supply measures:</i>				
Prob (Employed)	-0.021** [0.010]	-0.019* [0.010]	-0.020* [0.010]	0.785 [0.378]
Prob (Full-time employed)	-0.009 [0.006]	-0.009 [0.006]	-0.009 [0.006]	0.066 [0.214]
N Mothers	23497	23497	23497	4018
N Grandmothers	19548	19548	19548	
Controls	NO	YES	YES	
Sect FE	NO	NO	YES	

*Notes:* Table A3 shows the reduced form impacts of grandmothers being born since January 1950 on labor market outcomes on their adult daughters (mothers). Columns 1, 2, and 3 show the results without controls, with controls, and with both controls and sector fixed effects, respectively. All specifications use local linear regression with a bandwidth of 8 months. Robust standard errors clustered at grandmother's level are in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

*Source:* Authors' calculations from the CBS data.

Table A4: Impacts on grandfathers' labor supply (reduced-form)

	RD estimates			Means at cutoff
	(1)	(2)	(3)	
<b>First-Stage RD estimates</b>				
Total monthly hours worked	28.058*** [2.052]	28.276*** [2.025]	26.233*** [1.932]	58.951 [64.226]
<i>Other labor supply measures:</i>				
Prob (Employed)	0.151*** [0.013]	0.153*** [0.013]	0.137*** [0.012]	0.430 [0.422]
Prob (Full-time employed)	0.144*** [0.012]	0.145*** [0.012]	0.137*** [0.012]	0.267 [0.373]
<i>Other income measures:</i>				
Hourly wage rate	4.311*** [0.371]	4.335*** [0.367]	3.967*** [0.352]	9.483 [11.104]
Monthly labor earnings	728.209*** [54.897]	731.704*** [54.178]	683.573*** [52.019]	1303.44 [1666.96]
Monthly gross income	388.721*** [60.231]	382.745*** [59.366]	355.081*** [55.937]	3826.74 [1831.90]
Obs. (Grandfathers)	23609	23609	23609	4026
Controls	NO	YES	YES	
Sector FE	NO	NO	YES	

*Notes:* Table A4 shows the first-stage reduced form impacts on grandfathers' labor supply. Columns 1, 2, and 3 show the results without controls, with controls, and with both controls and sector fixed effects, respectively. All specifications use local linear regression with a bandwidth of 8 months. Robust standard errors clustered at grandfather's level are in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

*Source:* Authors' calculations from the CBS data.

Table A5: Placebo test - impacts on labor supply of mothers with deceased grandmothers (reduced-form)

	RD estimates		Mean at cutoff
	(1)	(2)	
<b>Mothers' labor supply outcomes</b>			
Total monthly hours worked	0.415 [4.923]	0.161 [4.827]	68.467 [49.236]
<i>Other labor supply measures:</i>			
Prob (Employed)	-0.001 [0.042]	-0.005 [0.041]	0.699 [0.421]
Prob (Full-time employed)	0.006 [0.019]	0.005 [0.019]	0.055 [0.193]
N Mothers	1858	1858	312
Controls	NO	YES	

*Notes:* Table A5 shows the second-stage reduced form impacts on adult daughters (mothers) whose mothers (grandmothers) deceased before age 50. Columns 1 and 2 show results without and including controls, respectively. All specifications use local linear regression with a bandwidth of 8 months. Robust standard errors clustered at grandmother's level are in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

*Source:* Authors' calculations from the CBS data.

Table A6: Placebo tests using other birth dates as cutoffs (reduced form)

Distance to actual cutoff	-10	-8	-6	-4	-2	0	2	4	6	8	10
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
<b>Mothers' labor supply outcomes</b>											
Total monthly hours worked	0.774 [1.243]	-0.357 [1.235]	0.305 [1.234]	0.088 [1.245]	1.139 [1.253]	-2.871** [1.259]	-0.84 [1.247]	1.670 [1.255]	1.581 [1.266]	-0.156 [1.285]	-0.594 [1.284]
<i>Other labor supply measures:</i>											
Prob (Employed)	0.007 [0.010]	0.001 [0.010]	0.005 [0.010]	-0.003 [0.010]	0.001 [0.010]	-0.020* [0.010]	0.002 [0.010]	0.021 [0.010]	0.005 [0.010]	-0.005 [0.010]	-0.002 [0.010]
Prob (Full-time employed)	0.004 [0.005]	0.002 [0.006]	0.007 [0.005]	-0.001 [0.006]	0.006 [0.006]	-0.009 [0.006]	-0.011 [0.005]	-0.0000 [0.005]	0.009 [0.005]	0.006 [0.006]	-0.003 [0.006]
Obs. (Mothers)	23853	23643	23753	23861	23730	23497	23342	23196	23333	23392	23331

*Notes:* In Table A6 we test the validity of our results by using placebo cutoffs of 10 months back and 10 months ahead at a bi-monthly frequency. The second-stage reduced form impacts of grandmothers being born since January 1950 are reported in the table. All specifications use local linear regression with a bandwidth of 8 months including controls and sector fixed effect. Robust standard errors clustered at grandmother's level are in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

*Source:* Authors' calculations from the CBS data.

Table A7: Fuzzy RD estimates by polynomial orders

Outcomes	poly	estimates	AIC	BIC	AICc	Obs
<b>Impact of GM's total monthly hours worked on mothers' labor supply</b>						
Total monthly hours worked	1	-0.465** [0.229]	251525	251638	251526	23497
	2	-0.850* [0.473]	259250	259379	259250	23497
<i>Other labor supply measures:</i>						
Prob (Employed)	1	-0.003* [0.002]	23495	23608	23495	23497
	2	-0.004 [0.003]	25453	25582	25453	23497
Prob (Full-time employed)	1	-0.001 [0.001]	-5550	-5437	-5550	23497
	2	-0.002 [0.002]	-3550	-3421	-3550	23497

*Notes:* Table A7 shows the 2SLS estimates of grandmothers' total monthly hours worked on mothers' labor supply outcomes by polynomial orders. An indicator for grandmothers being born since January 1950 serves as the instrument for the grandmothers total monthly hours worked. We show local linear and quadratic regressions with a bandwidth of 8 months. Robust standard errors clustered at the grandmother's level are in parentheses. We also report the AIC, its sample equivalent (AICc), and BIC criteria for each regression. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

*Source:* Authors' calculations from the CBS data.

Table A8: Fuzzy RD estimates by CCT optimal bandwidth and polynomial orders

Outcomes	Poly	estimates	s.e.	optimal BW	Obs
<b>Impact of GM's total monthly hours worked on mothers' labor supply</b>					
Total monthly hours worked	1	-0.645**	[0.285]	7.252	20711
	2	-0.822**	[0.404]	9.986	26221
<i>Other labor supply measures:</i>					
Prob (Employed)	1	-0.004*	[0.002]	7.698	20711
	2	-0.004*	[0.003]	10.942	29142
Prob (Full-time employed)	1	-0.002	[0.001]	8.780	23497
	2	-0.002	[0.002]	10.091	29142
Average of optimal BW				9.1248	
Average of optimal BW (linear)				7.9099	
Average of optimal BW (quadratic)				10.3398	

*Notes:* Table A8 shows the 2SLS estimates of grandmothers' total monthly hours worked on mothers' labor supply outcomes using the mean square error optimal bandwidths generated by the Calonico et al. (2017) and Calonico et al. (2018) procedure (the CCT bandwidths). The optimal bandwidths are generated separately for each outcome. Standard errors are in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

*Source:* Authors' calculations from the CBS data.



Table A9: Impacts on mother’s fertility outcomes (reduced-form)

	RD estimates		Mean at cutoff
	(1)	(2)	
<b>Mothers’ fertility outcomes</b>			
Prob (Ever child)	0.002 [0.006]	0.004 [0.009]	0.768 [0.422]
Prob (At least 2 children)	-0.007 [0.007]	-0.007 [0.011]	0.611 [0.488]
Total number of children	-0.003 [0.016]	-0.002 [0.026]	1.610 [1.147]
Age at first birth	0.018 [0.072]	-0.052 [0.117]	29.309 [4.430]
Age at last birth	-0.052 [0.063]	-0.104 [0.103]	32.685 [4.059]
Average agegap of children	-0.023 [0.030]	0.020 [0.050]	3.092 [1.763]
Average agegap after GM age 55	-0.060 [0.040]	0.000 [0.065]	3.264 [2.045]
Prob (First child after GM age 55)	-0.001 [0.007]	-0.004 [0.011]	0.470 [0.499]
Obs. (Mothers)	100369	100369	16923
Obs. (Grandmothers)	69628	69628	
Polynomial	linear	quadratic	

*Notes:* Table A9 tests the impact of grandmothers being born since January 1950 on the adult daughters’ (mothers’) fertility outcomes. All specifications use local linear regression with a bandwidth of 8 months. Robust standard errors clustered at grandmother’s level are in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

*Source:* Authors’ calculations from the CBS data.

Table A10: Impacts on sample selection

	RD estimates		Mean at cutoff
	(1)	(2)	
<b>Restrictions by Grandmothers' characteristics</b>			
<i>Step 1: reform relevance</i>			
Exit labor force before age 50	-0.000 [0.009]	0.001 [0.015]	0.385 [0.487]
Obs. Mothers	72924	72924	12307
Obs. Grandmothers	54912	54912	
<i>Step 2: alive during treatment period</i>			
Dead before age 65	0.001 [0.004]	0.002 [0.006]	0.023 [0.148]
Obs. Mothers	44903	44903	7564
Obs. Grandmothers	34085	34085	
<i>Step 3: health status/ relevance for care responsibility</i>			
Claim disability before age 55	0.001 [0.007]	-0.001 [0.011]	0.081 [0.272]
Obs. Mothers	43810	43810	7394
Obs. Grandmothers	33253	33253	
<b>Restrictions by Mothers' characteristics</b>			
<i>Step 4: Keep by relevance of child care</i>			
Youngest 4-12 sample	-0.014 [0.011]	0.003 [0.017]	0.595 [0.491]
Obs. Mothers	40160	40160	6799
Obs. Grandmothers	30447	30447	
<b>Baseline Sample</b>			
Obs. Mothers		23497	
Obs. Grandmothers		19548	
Polynomial	linear	quadratic	

*Note:* Table A10 tests the impact of grandmothers being born since January 1950 on a list of sample selection variables. Step 1 is based on all women (grandmothers) born 8 months around the January 1950 cutoff who have at least one daughter. In step 2, we show that for all grandmothers with at least one adult daughter and still in the labor force by age 50, the probability of death before age 65 is smooth around the RD cutoff. Each further step builds on the previous one. Steps 1-3 test groups to drop from the sample and step 4 tests for groups to keep in the baseline sample. Columns 1 and 2 show results based on local linear and quadratic regressions with a bandwidth of 8 months, respectively. Robust standard errors clustered at grandmother's level are in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

*Source:* Authors' calculations from the CBS data.

Table A11: Impacts on sub-sample selection

	RD estimates		Mean at cutoff
	(1)	(2)	
<b>Sub-sample selection criteria</b>			
<i>Restrictions by age of youngest child</i>			
Youngest aged 0-3	-0.008 [0.010]	-0.029* [0.017]	0.647 [0.478]
Youngest aged 4-7	-0.002 [0.011]	0.003 [0.018]	0.525 [0.499]
Youngest aged 8-12	-0.003 [0.010]	0.016 [0.016]	0.293 [0.455]
Youngest aged 12-18	-0.006 [0.007]	0.000 [0.012]	0.130 [0.336]
Obs. Mothers	39293	39293	6663
Obs. Grandmothers	29921	29921	
Polynomial	linear	quadratic	

*Note:* Table A11 tests the impact of grandmothers being born since January 1950 on a list of sub-sample selection variables. All regressions are based on the sample selected after completing Steps 1 to 4 displayed in Table A10. Columns 1 and 2 show results based on local linear and quadratic regressions with a bandwidth of 8 months, respectively. Robust standard errors clustered at grandmother's level are in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

*Source:* Authors' calculations from the CBS data.

Table A12: Summary statistics of children

	Baseline Sample Children					
	All		Youngest		CITO - sample	
	Mean	s.d.	Mean	s.d.	Mean	s.d.
Birth month	6.549	[3.404]	6.488	[3.386]	6.608	[3.346]
Birth year	2004.27	[3.175]	2005.27	[3.047]	2003.37	[2.460]
Girl	0.489	[0.500]	0.490	[0.500]	0.508	[0.500]
Children in Household	2.281	[0.815]	2.049	[0.775]	2.001	[0.747]
Birthorder	1.652	[0.750]	1.996	[0.737]	1.951	[0.721]
Prob (First-born child)	0.493	[0.500]	0.238	[0.426]	0.256	[0.436]
Prob (Parents married)	0.409	[0.439]	0.397	[0.438]	0.533	[0.447]
Live is same municipality as GM	0.539	[0.463]	0.551	[0.462]	0.553	[0.469]
Parents' age difference	2.776	[3.084]	2.822	[3.963]	2.912	[3.865]
High SES (disp. income of GM)	0.446	[0.497]	0.429	[0.495]	0.391	[0.488]
Age of mother (when GM aged 60-64)	38.094	[2.808]	38.058	[2.924]	38.482	[2.820]
Native mother	0.960	[0.196]	0.957	[0.204]	0.955	[0.207]
Number of aunts and uncles	2.526	[1.043]	2.463	[0.976]	2.434	[0.990]
Number of aunts	1.730	[0.809]	1.698	[0.779]	1.685	[0.783]
Mother's age first child	28.019	[3.289]	28.133	[3.455]	27.483	[3.251]
Obs. Children	41685		21443		8445	
Obs. Mothers	22828		22828		5487	

*Note:* Table A12 reports means and standard deviations. Columns 1 and 2 consist of all children aged 4-12 of mothers in the baseline sample (i.e., with grandmothers born 8 months before and after January 1950). Columns 3 and 4 restrict the sample in addition to the youngest children aged 4-12. Columns 5 and 6 summarize characteristics of the youngest children aged 4-12 that can be matched to their Cito outcomes (Cito-sample used for the long-run analysis).

*Source:* Authors' calculations from the CBS data.

Table A13: Smoothness of children’s covariates (reduced-form)

	RD estimates		Mean at cutoff
	(1)	(2)	
Birth month	0.026 [0.156]	0.130 [0.250]	6.565 [3.375]
Birth year	-0.062 [0.113]	-0.003 [0.186]	2003.35 [2.437]
Girl	0.028 [0.023]	0.008 [0.037]	0.500 [0.500]
Children in Household	-0.030 [0.035]	-0.039 [0.058]	2.029 [0.736]
Birthorder	-0.036 [0.033]	-0.064 [0.054]	1.978 [0.707]
Prob (First-born child)	0.018 [0.019]	0.044 [0.030]	0.231 [0.422]
Prob (Parents married)	0.003 [0.020]	-0.041 [0.034]	0.540 [0.445]
Live is same municipality as GM	-0.002 [0.021]	-0.004 [0.034]	0.554 [0.467]
Parents’ age difference	-0.279 [0.183]	-0.196 [0.293]	2.954 [3.794]
High SES (disp. income GM)	-0.035 [0.022]	-0.005 [0.036]	0.403 [0.491]
Age of mother (when GM aged 60-64)	0.075 [0.125]	0.014 [0.205]	38.391 [2.755]
Native mother	0.013 [0.010]	0.049*** [0.017]	0.945 [0.227]
Number of aunts and uncles	0.054 [0.044]	-0.065 [0.069]	2.441 [0.896]
Number of aunts	0.053 [0.037]	-0.076 [0.056]	1.702 [0.769]
Mother’s age first child	0.183 [0.152]	0.350 [0.243]	27.312 [3.449]
Obs. Children	8436	8436	1521
Obs. Mothers	8221	8221	
fit	linear	quadratic	

*Note:* Table A13 tests the impact of grandmothers being born since January 1950 on a list of children’s characteristics. Regressions are based on all children aged 4-12 when the grandmother is aged 60-64 who can be matched to their CITO results. Columns 1 and 2 show results based on local linear and quadratic regressions with a bandwidth of 8 months, respectively. Robust standard errors clustered at the mother’s level are in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

*Source:* Authors’ calculations from the CBS data.

Table A14: Impacts on CITO-sample selection

	RD estimates		Mean at cutoff
	(1)	(2)	
<b>Restrictions by Age</b>			
<i>Relative age of child within family and test score availability</i>			
Youngest child 4-7 and cito available	-0.013	-0.027	0.218
	[0.010]	[0.016]	[0.413]
Obs. Children	29019	29019	4887
Obs. Mother	18645	18645	
Youngest child 8-10 and cito available	-0.019	-0.018	0.267
	[0.012]	[0.019]	[0.442]
Obs. Children	21807	21807	3732
Obs. Mother	15377	15377	
Youngest child 11-12 and cito available	-0.019	0.004	0.221
	[0.014]	[0.023]	[0.415]
Obs. Children	12882	12882	2222
Obs. Mother	9758	9758	
fit	linear	quadratic	

*Note:* Table A14 tests the impact of grandmothers being born since January 1950 on a list of Cito-sample selection variables. Regressions are based on all children aged 4-7, 8-10, and 11-12 when the grandmother is aged 60-64, respectively. Columns 1 and 2 show results based on local linear and quadratic regressions with a bandwidth of 8 months, respectively. Robust standard errors clustered at the mother's level are in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

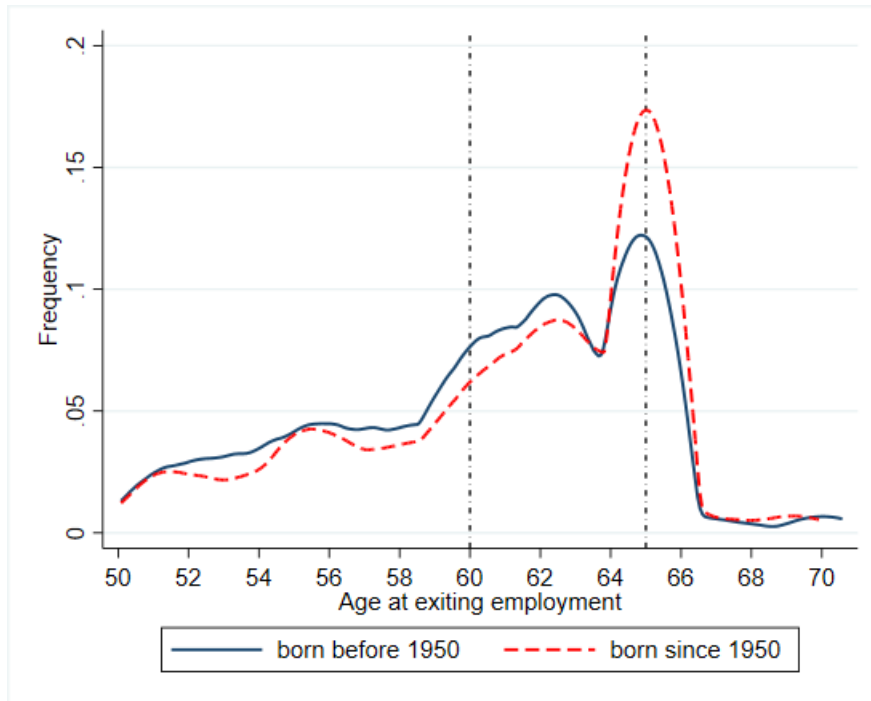
*Source:* Authors' calculations from the CBS data.

Table A15: Impacts on subsidy take-up (reduced-form)

RD estimates	Daycare		Out-of-school care		N (mothers)
	Prob. (1)	Hours (2)	Prob. (3)	Hours (4)	
Age between 4-7	-0.007 [0.006]	-5.934* [3.051]	-0.009 [0.014]	1.301 [6.807]	18683
Means at cutoff	0.092	38.847	0.321	122.516	
Age between 8-10	-	-	0.014 [0.015]	10.677 [7.193]	10243
Means at cutoff	-	-	0.180	66.916	
Age between 11-12	-	-	0.021* [0.011]	8.850* [4.931]	5292
Means at cutoff	-	-	0.048	16.114	

*Notes:* Table A15 shows reduced-form reform impacts on childcare subsidy take-up in families with the youngest child aged 4 - 7, 8 - 10, and 11 - 12 when the grandmothers are at age 60 and 64. Subsidy take-up is shown for any child within the indicated age range, which means that a mother with multiple children in official child care will show up in multiple age groups. Columns 1 and 2 show effects on the probability of daycare take-up and the average hours of daycare usage, respectively. Columns 3 and 4 show effects on the probability of out-of-school care take-up and the average hours of out-of-school care usage, respectively. All specifications use local linear regression with a bandwidth of 8 months and include controls. Robust standard errors clustered by grandmother's id are in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

*Source:* Authors' calculations from the CBS data.

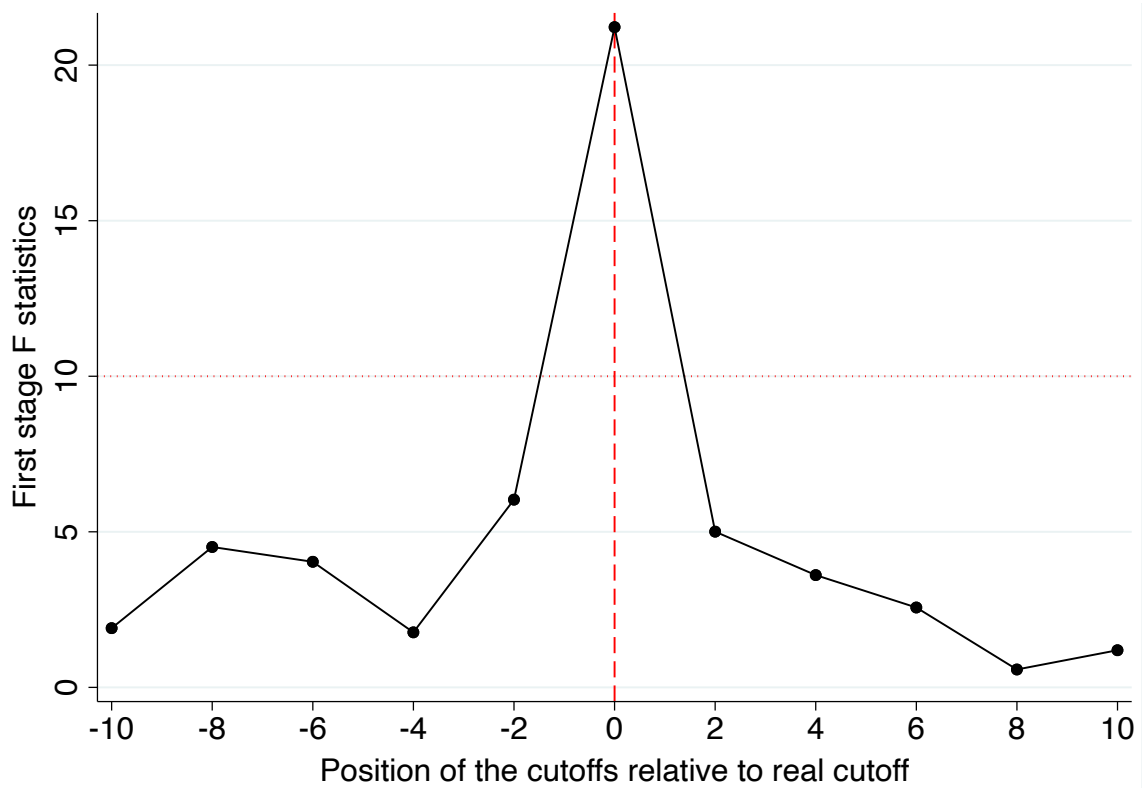


**Figure A.1: Distribution of age at exiting employment for women by treatment status**

*Notes:* Figure A.1 shows the distribution of age at exiting employment for the cohorts born before and since 1950 in baseline sample (i.e., 8 months around the cutoff). We can clearly see a shift towards later retirement for the treated cohorts.

*Source:* Authors' calculations from the CBS data.

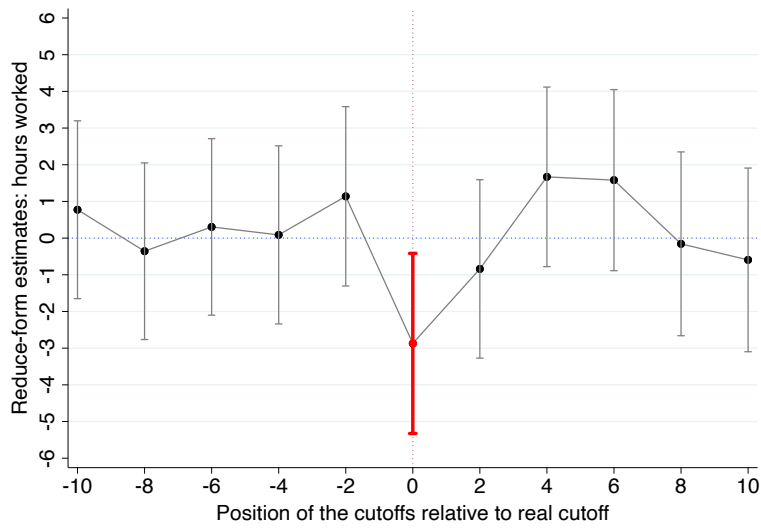




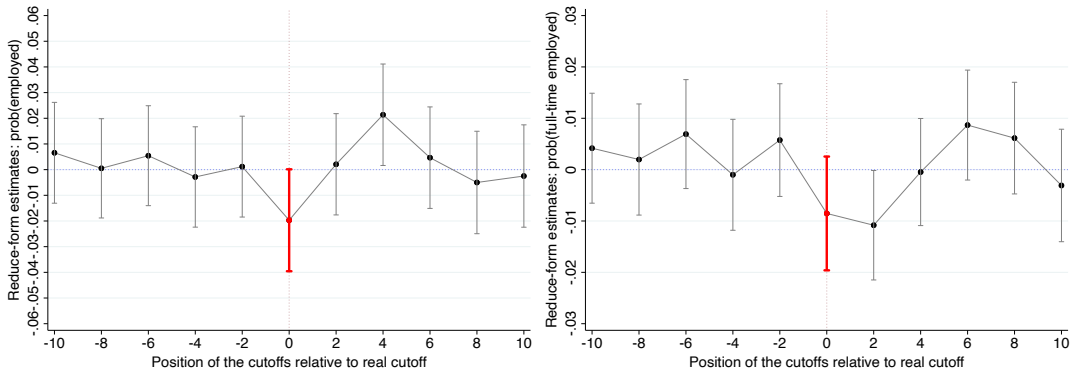
**Figure A.2: Placebo test: F-statistics as a function of the location of cutoff**

*Notes:* Figure A.2 shows F-statistics when the cutoff is placed at “placebo” locations around the real cutoff. We estimate the reduced-form impacts on mothers’ labor supply with placebo cutoffs ranging from -10 to 10.

*Source:* Authors’ calculations from the CBS data.



(a) Hours worked



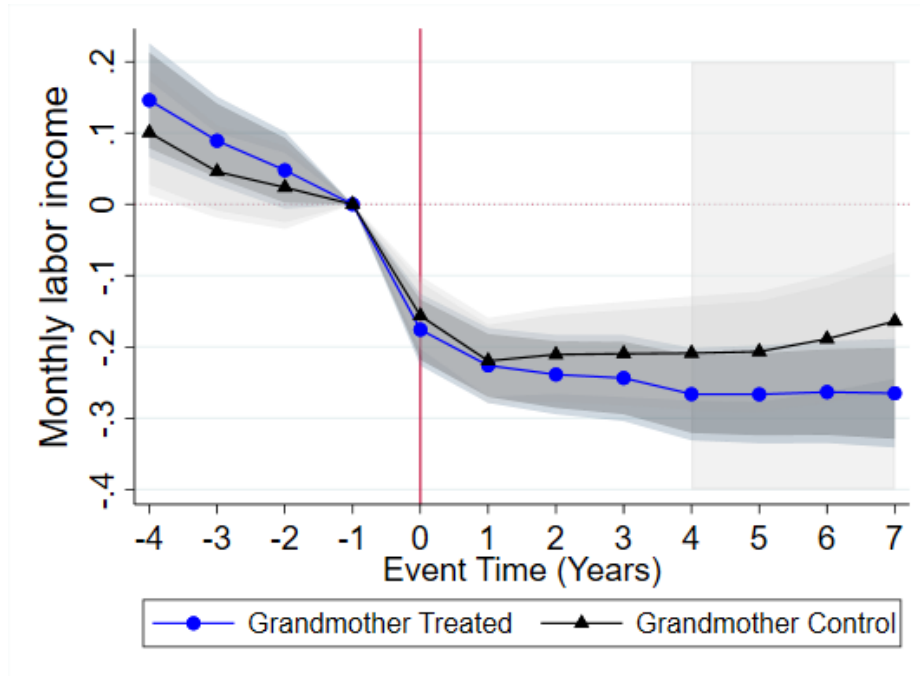
(b) Prob(employed)

(c) Prob(full-time)

Figure A.3: Placebo test: placebo estimates as a function of the location of cutoff

Notes: Figure A.3 plots reduced-form estimates and 95% confidence intervals by replacing the true cutoff (normalized to zero) with placebo cutoff locations ranging from -10 to 10. The red dashed line indicates the actual cutoff. Table A6 lists the regression estimates.

Source: Authors' calculations from the CBS data.



**Figure A.4: Dynamic Treatment Effects on Monthly Labor Earnings**

*Notes:* Figure A.4 shows the history of monthly labor income of women for a window of four years before and up to seven years after they gave birth to their first child. The effect on monthly labor income is estimated unconditional on employment status to capture the impact on total labor supply. Monthly labor income of treated (blue dots) and control (black triangles) women are compared. Treated refers to women (mothers) with a grandmother born since January 1950 (using a bandwidth of 7 months). Event time 0 marks the birth of the first child. The shaded area between event times 4 and 7 indicates the main reform spillover window on the second generation. The value at  $t = -1$  is normalized to zero so that coefficients measure the impact of the first child relative to the year before birth. Different tones of shaded areas indicate the 90 and 95 percent confidence intervals.

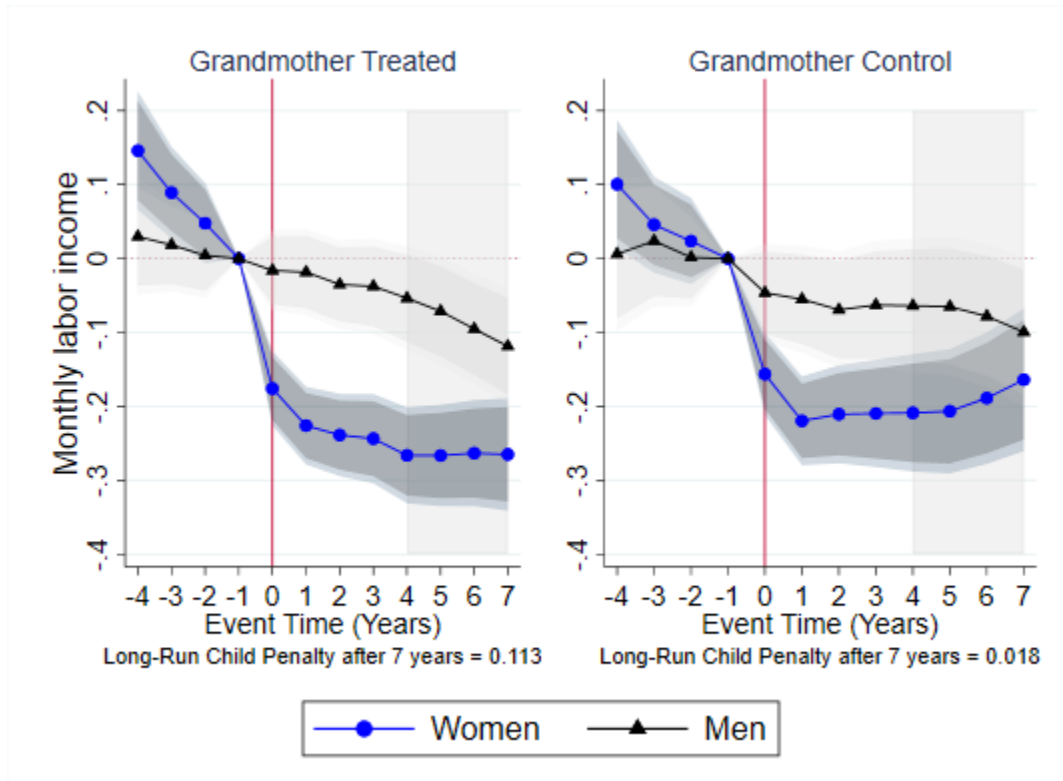


Figure A.5: Relative child penalty by treatment status

Notes: Figure A.5 shows the child penalty in total labor earnings (including zeros) by treatment status. Blue dots document women's and the black triangles indicate men's monthly labor income. Treated refers to women/ men (i.e., adult daughters/ sons in our analysis) with a grandmother born after and including January 1950 (using a bandwidth of 7 months). Event time 0 marks the birth of the first child. The shaded area between event times 4 and 7 indicates the main reform spillover window on the second generation. The value at  $t = -1$  is normalized to zero so that coefficients measure the impact of the first child relative to the year before birth. Different tones of shaded areas indicate the 90 and 95 percent confidence intervals. The long-run relative child penalty after 7 years (i.e., relative loss women experience compared to men) is reported below each sub-graph.

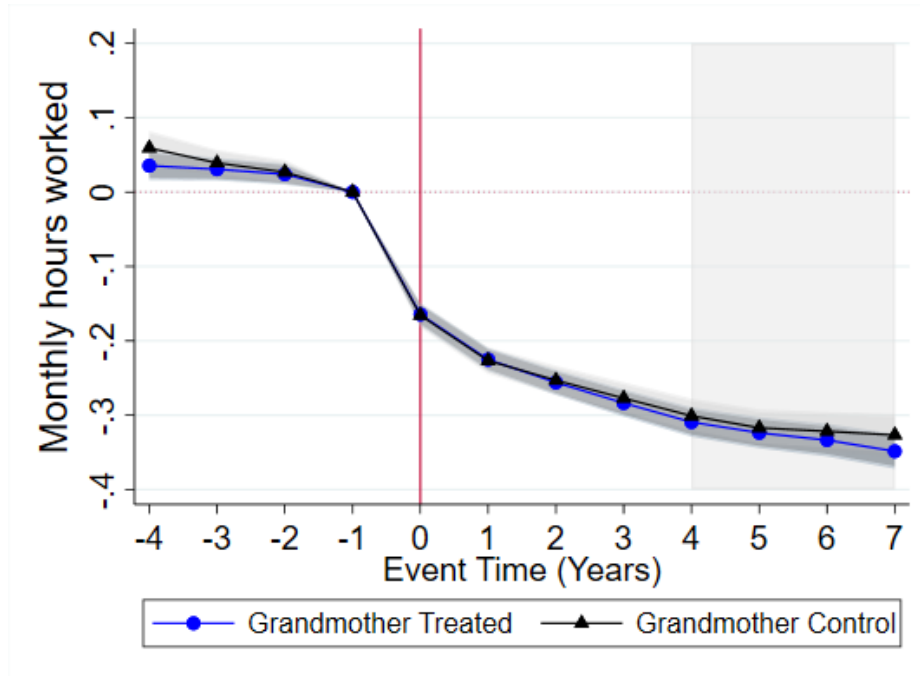


Figure A.6: Dynamic Treatment Effects on Monthly Working Hours (incl. multiple births)

Notes: Figure A.6 shows the history of total monthly hours worked of women for a window of four years before and up to seven years after they gave birth to their first child. The effect on total hours worked is estimated unconditional on employment status to capture the impact on total labor supply. Monthly working hours of treated (blue dots) and control (black triangles) women are compared. Treated refers to women (mothers) with a grandmother born since January 1950 (using a bandwidth of 7 months). Event time 0 marks the birth of the first child. No restriction is made concerning additional children born between the depicted event times zero and seven. The shaded area between event times 4 and 7 indicates the main reform spillover window on the second generation. The value at  $t = -1$  is normalized to zero so that coefficients measure the impact of the first child relative to the year before birth. Different tones of shaded areas indicate the 90 and 95 percent confidence intervals.