Persistent Effects of Social Program Participation on the Third Generation*

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Abstract

Can participation in safety net programs have long-lasting negative effects across multiple generations? We take advantage of a 1993 Dutch disability insurance reform which tightened requirements and lowered benefits for participants. We study the third generation 25 years after the reform, finding that grandchildren of individuals whose DI eligibility and benefits were reduced are less likely to be born premature, have low birthweight, or experience complicated deliveries. They also have better health and schooling outcomes during early childhood. These early-life improvements are consequential, as they have been linked to better health, education, and labor market outcomes in adulthood.

Keywords: Multigenerational spillovers, disability insurance, child health
JEL codes: I38, H53, J62

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1 Introduction

A longstanding policy debate is whether participation in social support programs causes long-term reliance and economic disadvantage which persists across generations. Recent research using quasi-experimental methods has shown that parental program participation strongly impacts their offspring. That work finds curtailing the generosity of the social safety net for parents not only reduces their children's future welfare use, but also improves their children's economic outcomes as adults.[1] An open question is whether these positive causal effects of reduced dependency fade out after the second generation, or continue to impact the third generation.

Third-generation effects could arise if second-generation parents' circumstances improve enough to positively affect their own children's development. However they could also dissipate due to the passage of time and intervening life events. If sizable effects do persist across multiple generations, reforms which lessen dependence could start a virtuous cycle of reduced economic disadvantage for affected families.

In this paper, we explore how a grandparent's use of disability insurance (DI), a large component of the social safety net in most countries, affects early-life outcomes for their grandchildren. We take advantage of rich administrative data which contains biological family links over three generations. We look at grandchild health at birth and subsequent health and education in early childhood, measures which have been shown to predict key later-life outcomes. For example, premature and low-birthweight babies subsequently have lower educational attainment, reduced cognitive ability, worse health, lower earnings, and higher rates of welfare use as adults.[2]

We estimate the causal impact of DI benefit receipt over multiple generations using a regression discontinuity (RD) design, exploiting a reduction in DI benefit generosity in the Netherlands following a 1993 reform. The reform tightened eli-

[1] Dahl et al. (2014); Dahl and Gielen (2021); De Haan and Schreiner (2025); Hartley et al. (2022).

[2] See Almond and Currie (2011); Almond and Mazumder (2011); Behrman and Rosenzweig (2004); Black et al. (2007); Currie and Hyson (1999); Figlio et al. (2014); Oreopoulos et al. (2008); Royer (2009).

gibility and reduced benefit amounts for those currently on DI, but only for those under age 45 as of August 1, 1993. Participants less than 45 were re-examined and subject to a new set of rules which (weakly) reduced their degree of disability; this in turn (weakly) lowered their DI payments or completely disqualified them from the program. In contrast, those over age 45 were grandfathered in under the old rules. This reform generates an age cohort discontinuity, with current recipients near the cutoff being similar except for exposure to the stricter DI rules.

Prior work has shown that following this reform, approximately 4 percent of DI participants exited DI (a 35% increase) and that annual benefits fell by around 1,000 euros (a 10% decrease) (Borghans et al., 2014). These marginal DI participants demonstrated substantial work capacity, replacing a majority of their lost benefits with labor market earnings. The reform additionally improved outcomes for the children of these DI participants: once adults themselves, they had lower DI participation, more education and earnings, and committed fewer serious crimes (Dahl and Gielen, 2021). In sum, from a societal perspective, the reform curtailing DI benefits had positive effects on both the directly affected individuals (the first generation) and their children (the second generation). The current paper asks whether effects continue to spill over to the third generation of grandchildren. From a public-policy perspective, documenting effects for the third generation provides a more complete understanding of the costs and benefits of curtailing DI participation. Even though the second generation experienced positive outcomes, this need not imply their children will also benefit.[3]

We offer two sets of results on third-generation spillovers. The first is that there is a strong link between DI use of grandparents and their grandchildren's health at birth. Grandchildren whose grandparents were subject to the less generous DI rules

[3]For example, prior research has found mixed evidence for whether maternal schooling causally affects infant health outcomes such as low birthweight; while Currie and Moretti (2003) and Noghanibehambari et al. (2022) find positive impacts, Lindeboom et al. (2009) and McCrary and Royer (2011) find little to no impact. There is limited causal evidence on how income affects birth outcomes, with magnitudes varying based on the source of exogenous variation (see the references in Martin et al. (2024)).

are 18% less likely to be born premature, 18% less likely to have low birthweight, and 7% less likely to require specialized doctors to assist with the birth.[4] To provide a sense of potential long-term impacts, consider a back of the envelope calculation using external estimates for how low birthweight affects later-life outcomes. Having a grandparent subject to the stricter DI rules is predicted to decrease their grandchild's probability of dropping out of high school by 0.9 percentage points, increase their adult earnings by 2.7%, and prevent 2.2 years of "premature aging".[5]

Second, we find evidence for spillovers in two domains during early childhood: health and education. For grandchildren whose grandparents were exposed to the reform, there is an 8% decrease in health expenditures, a 5% decline in the use of pharmaceutical drugs, and a 13% decrease in grade repetition during elementary school.[6] This provides further evidence that social program participation has the potential to change the well-being of families over decades. In our dataset, the third generation is still relatively young. But related work suggests the improved health and education impacts we document will continue into adulthood; negative early childhood outcomes are linked to poorer health, education, and labor market outcomes as adults.[7]

Overall, the reduced reliance on DI for the first generation has a positive impact across three generations. But it is important to view these findings in context: they do not imply that social assistance will always harm family dynasties. In the years preceding the reform, 12% of the eligible Dutch population was on DI, prompting some to satirically remark there must be a "Dutch disease." The first generation compliers in our sample are the grandparents on the margin who had substantial

[4]In addition to documenting three-generation effects, these results also contribute to a literature on the causes of low birthweight, e.g., Aizer and Currie (2014) and Eshaghnia and Heckman (2023).

[5]These calculations are based on Johnson and Schoeni (2011), which finds that low birthweight increases the chances of dropping out of high school by 5 percentage points, lowers adult earnings by 15%, and ages people prematurely by 12 years (e.g., a 30 year old reporting the average health of a 42 year old).

[6]While we do not find any statistically significant effect of the reform on early child mortality, any such effect would likely bias our estimates upward, making our estimates conservative.

[7]See the review articles by Almond and Currie (2011) and Almond et al. (2018).

work capacity.[8] Viewed in this light, our findings suggest that social programs can go too far, negatively impacting succeeding generations. The key takeaway from our paper is that government policies have the power to affect the transmission of health and socioeconomic outcomes across multiple generations.

Our paper is related to three additional strands of the literature not already mentioned. The first examines intergenerational mobility, documenting substantial correlations in income and other key outcomes across generations, with recent work exploring how this varies across time, countries, and geography.[9] A second literature examines persistence in wealth, earnings, occupation, or education across multiple generations, concluding that a focus on just two generations can underestimate long-run intergenerational persistence.[10] A third literature studies how various government policies affecting parents impact their children, including the Earned Income Tax Credit, Food Stamps, Medicaid, the Mother's Pension Program, oral contraception availability, and school construction programs.[11] Relative to these literatures, our paper provides the first evidence on how a government support program causally impacts the third generation, finding that social assistance programs can play an important role in the chain of intergenerational disadvantage.

[8]In our setting of the early 1990s, DI was both relatively generous and easy to access. For example, as Koning and Van Vuuren (2007) show, firms often facilitated the inflow of employees into DI instead of the less generous and temporary UI system after a layoff. The popularity of this "substitute-UI" is consistent with both high participation rates and marginal DI participants having substantial earnings capacity as found in Borghans et al. (2014).

[9]Aaronson and Mazumder (2008); Black and Salvanes (2010); Chetty et al. (2022); Chetty and Hendren (2018a,b); Corak (2013); Corak et al. (2014); Deutscher and Mazumder (2023); Halliday et al. (2021); Lee and Solon (2009).

[10]Adermon et al. (2018, 2021); Braun and Stuhler (2018); Chan and Boliver (2013); Ferrie et al. (2021); Lindahl et al. (2015); Long and Ferrie (2018).

[11]Aizer et al. (2016); Akee et al. (2021); Ananat and Hungerman (2012); Bailey et al. (2019, 2020); Bastian and Michelmore (2018); Bastian et al. (2021, 2022); East et al. (2023); Lefgren et al. (2022); Madestam et al. (2020); Mazumder et al. (2021).

2 Institutional Background

2.1 Disability insurance in the Netherlands

The modern Dutch DI program was created in 1967 by merging two existing programs covering workplace-induced injuries and disabilities unrelated to employment.[12] The program was generous compared to other countries, as it covered all workers fully after their first day of employment, replaced up to 80% of wages, and included a variety of subjective illnesses. Moreover, sickness benefits replaced a worker's wages between 80 and 100% during the transition to disability insurance, and workers on sickness benefits for a full year were routinely transferred to the DI program without a serious reappraisal of their disability (Kalwij et al., 2014). These factors fueled a rapid rise in DI recipients, from 4% participation of the eligible population in 1967 to over 8% by 1980. Modest reforms in the early 1980s were enacted in an attempt to stem the rise, but were largely ineffective. Participation reached a peak of 12% in the late 1980s, with payments ballooning to 4.2% of gross domestic product.

Starting in the 1990s, a series of reforms were implemented to control the spiraling costs of the DI system, including reductions in benefit levels, tightened eligibility criteria, changes to the sickness benefit program, and increased financing and responsibility transferred to individual employers. The cumulative effect of these reforms was that by 2012 the participation rate had fallen to just over 7% of the eligible population. Going forward, the participation rate is predicted to fall even further as the stock of older recipients transitions out of the DI program and on to the retirement pension program.[13]

The current state of DI in the Netherlands is that payments now total approximately 2.8% of GDP (as of 2019). This compares to 2.2% in other European Union

[12] The discussion in Section 2 borrows from Dahl and Gielen (2021), often using the same description but abbreviated, since the Dutch DI setting for the two papers is the same.

[13] The trends over time are discussed in more detail by Koning and Lindeboom (2015).

countries, and 1.0% in the U.S (OECD, 2023). In terms of participation, the Dutch rate of 7.5% (as of 2018) is higher compared to the U.S. rate of 6.2%, but lower than Norway's 10.8%, for example (OECD, 2022).

Before continuing, we note several differences between the current Dutch and U.S. programs. First, in the Netherlands, individuals can receive payments for a partial disability and therefore continue to work and earn benefits simultaneously, while in the U.S. disability determination is binary. Second, health insurance and other benefits are unrelated to DI receipt in the Netherlands, but directly linked in the U.S. Third, benefits do not depend on family size in the Netherlands, while they do in the U.S. Fourth, the replacement rate in the Netherlands is not a function of tenure, with all workers being covered 100% the first day on the job. Finally, the replacement rate of 70% for complete disability in the Netherlands is higher than the average U.S rate of 40 to 50% (see Borghans et al. (2014)).

2.2 1993 reform

Many changes were responsible for the reduction in DI expenditures in the Netherlands; in this paper we take advantage of a 1993 reform which generates a discontinuity in program generosity based on age. As this is the same cohort discontinuity used by Borghans et al. (2014) to study benefit substitution, we only briefly explain the most salient features of DI in the Netherlands and the 1993 reform, and refer readers to their paper for further details.

In the Netherlands, individuals receive DI payments based on their degree of disability, which is calculated as their "income loss" divided by pre-disability earnings. Binned categories for the degree of disability determine the replacement rate, which varies from 0 to 70% of prior earnings.[14] Income loss is defined as pre-disability earnings minus a constructed measure of "earnings capacity." The reform we exploit affected the calculation of earnings capacity in a way which made DI less generous.

Prior to 1993, a medical doctor examined applicants and created a subjective list

[14] In our data, 49% of claimants are classified as "fully" disabled and receive 70% of prior earnings.

of work activities the applicant could still perform, which in conjunction with the applicant's education level, was used to create a list of suitable occupations from a dictionary of occupational requirements. The applicant's earnings capacity was then defined as the average wage in the 5 highest paying suitable occupations which had at least 10 active workers in the applicant's geographic region. If 5 suitable occupations could not be found, earnings capacity was set to 0.

The 1993 reform altered this process in two ways. First, it mandated the doctor create a list of work activities based on a more objective medical diagnosis which could be directly linked to functional work limitations. Second, the list of suitable occupations was expanded by no longer taking education level into account, only using 3 suitable occupations to calculate earnings capacity, and expanding the geographic region of 10 active workers to be roughly three times larger. Each of these changes weakly reduce the degree of disability for an applicant compared to the old criteria, as remaining earnings capacity can only rise. Moreover, the new rules make it more likely that enough suitable occupations can be found, reducing the chances of total disability. The end result is that fewer individuals qualify for DI and benefit levels are weakly reduced for those who continue to qualify.

All new applicants, regardless of age, were subject to the new rules. But for existing DI recipients, those age 45 or older were grandfathered into the old, more generous rules. This grandfathering creates a sharp cutoff in the generosity of DI based on an individual's age, a feature we exploit for identification.

3 Data and Descriptive Statistics

3.1 Administrative data

Our analysis uses a variety of administrative data sources, each of which contains the personal identification number assigned to individuals living in the Netherlands. The personal identification number not only facilitates the merging of information for a given individual across different datasets, but also allows biological grandparents,

parents, and grandchildren to be linked together.

The disability administrative records begin on January 1, 1996.[15] The records include information on the start and end dates of a spell, the binned disability rating, DI payments received, and pre-disability earnings. Municipal registry files contain information on basic demographics. We merge in data for the third generation from a variety of administrative sources, with the last year being 2019 for all but the health care expenditure outcome, which ends in 2018 (which means our data stops before the covid pandemic hits). We use birth record data from Statistics Netherlands for health at birth. This data is collected by PERINED[16] starting in 1999, and includes information on gestational age, birthweight, and other health conditions at birth. We further merge in information on health care expenditures paid for by Netherland's basic health insurance and information on all drug prescriptions; this data is available starting in 2006. Basic health insurance covers all necessary health care, is free of charge for children, and has no deductible for children under age 18. We also merge in education information; starting in 2008 we can observe a child's grade level, allowing us to deduce whether they repeat a grade.

Our data span three generations, which we denote as G1 (grandparents), G2 (parents), and G3 (grandchildren). Age is measured in months. We limit our sample window for grandparents to those between the ages of 40 and 50 and on DI as of the reform date of August 1, 1993, similar to Dahl and Gielen (2021). Our estimation sample is further limited to G2 parents who were (i) living at home at the start of 1996 and (ii) between the ages of 14 and 18 on November 12, 1996 (when the the cutoff was unexpectedly changed to 45+ instead of the originally scheduled 50+).[17] This

[15]For everyone on DI in 1996, their record includes information on whether they were receiving DI benefits in 1993. There should not be differential attrition around the age 45 cutoff (measured as of August 1993 for existing claimants) when the DI records begin in 1996. This is because the cutoff to be exempt from the new, stricter DI rules for existing claimants was unexpectedly altered on November 12, 1996 to be 45+ instead of the originally scheduled 50+. See Borghans et al. (2014) or Dahl and Gielen (2021) for details.

[16]See www.perined.nl for more details.

[17]We make the first restriction because G2 who had already left home were empirically not affected by the reform (Dahl and Gielen, 2021). The second restriction relates to G2 birth timing:

yields a baseline sample of 19,895 grandparents and 23,294 parents. For G3-birth outcomes, we use grandchildren of any parity since there is no evidence the reform affected fertility on either the extensive or intensive margin.[18] There are 44,398 of these children with birth records recording gestational age. For early childhood outcomes, we restrict the sample to first-born G3 since they are older on average and hence have better data coverage for grade repetition and health expenditures.

We make one final sample restriction: we limit the analysis to G3 grandchildren that have exactly one grandparent in the reform window. This only excludes 5.6% of all grandchildren, as very few grandchildren have multiple grandparents on DI. This restriction simplifies the implementation and interpretation of the RD estimator, and allows for correct inference when clustering standard errors at the grandparent level.

3.2 Grandchild outcomes

In this section, we define our third-generation grandchild outcomes, and present related summary statistics. Panel A in Table 1 reports on variables measured at birth. One commonly used indicator of health at birth is gestational age. The medical literature often focuses on premature births, or those born prior to 37 weeks, since adverse outcomes often accompany these births (see footnote 2). Another focus is on early births, defined as those which occur prior to 38 weeks, as these births are also in the tail of the gestational age distribution and are associated with developmental delays.

The first column in the table reports means for the entire sample of grandchildren whose grandparents were between the ages of 40 to 50 at the time of the reform. The next two columns report means for those just below the reform cutoff (age 44), and hence subject to the stricter DI rules which reduced DI eligibility and benefits, versus

those older than 18 often give birth prior to 1999, which is when our G3 birth records start; and those younger than 14 are more likely to give birth after our data ends.

[18] Using the baseline RD specification described in Section 4, the estimated effect of the reform on the probability G2 has any children is 0.010 (s.e.=.010) and on the number of children is .036 (s.e.=.023). Likewise, the reform did not affect age in days at first birth for the G2 generation (estimate=0.74, s.e.=1.42).

those just above the age cutoff (age 45). Using this narrow age comparison, the table reveals that 8.5% of births linked to grandparents experiencing more generous DI rules are born premature compared to only 6.9% of births linked to stricter DI rules. A similar gap is found for early births (15.9% versus 13.6%).

A second commonly used indicator for health at birth is birthweight, with a focal cutoff of low birthweight (<2,500 grams). Newborns whose grandparents were exposed to the reform are almost 1 percentage point more likely to be low birthweight (6.7% versus 5.8%).[19]

As a third measure of health at birth, we use whether specialized pediatric care was involved surrounding the birth of the child. This measure is a proxy for a more involved or at-risk birth; a pediatric specialist is involved in roughly 28% of births in our sample. The table points to reform-exposed individuals having grandchildren experiencing more complicated births, with a 1.7 percentage point increase.

Panel B reports on measures observed during early childhood. We limit the sample to first-born children, as they are old enough for the outcomes to be meaningful: while first-borns are 8.7 years old on average when we measure child outcomes, later-borns are only 6.3 years old. The first early childhood variable is whether the grandchild has repeated any grade during primary school. Repetition is relatively high at around 19%, with a 2.8 percentage point lower rate for grandchildren of reform-exposed grandparents. There is also evidence for a gap in early childhood health which follows the same pattern. Our first proxy for the general health of a child is whether they had any drug prescribed to them by a doctor in 2018 and our second is their cumulative health expenditures up to 2018.[20] Treated grandchildren are 2.8 percentage points more likely to have a drug prescribed.

From these means, there is already preliminary evidence that the grandchildren

[19]The medical literature also focuses on very low birthweight (<1,500 grams). We do not explore this outcome because it is extremely rare in our Dutch data, and hence we do not have enough of these births around the cohort discontinuity.

[20]We use a single year to measure whether any drug has been prescribed, rather than a cumulative measure, as almost all children have had a prescription at some point, especially in their first year or two of life.

of those exposed to the DI reform are better off. In the next section we refine the analysis using a regression discontinuity design.

4 Empirical Model

Our RD design leverages the age cohort discontinuity in how the Dutch DI reform affected grandparents. Grandparents on DI who were age 45 to 50 as of August 1, 1993 were subject to the old DI rules, while those between 40 to 45 were re-examined based on the new, more stringent rules. The effect of the reform on the third generation can be modeled as:

$$y_i = \alpha + I[a_i \geq c](f_r(a_i - c) + \theta) + I[a_i < c]f_l(c - a_i) + \gamma x_i + e_i \tag{1}$$

where y_i is the relevant outcome for grandchildren, a_i is the grandparent's age at the time of the reform, c is the age cutoff, x_i is a vector of immutable grandparent, parent, and grandchild characteristics (listed in the footnote to the table), e_i is an error term, and f_r and f_l are separate functions of the normalized running variable on each side of the cutoff. The coefficient θ captures the effect of the reform on the third generation.

The validity of the reduced form design requires that individuals cannot manipulate the assignment variable, which in our setting is the grandparent's age at the time of the reform. Since age is taken from administrative records, there is little chance for this type of direct manipulation. We provide three tests supporting the absence of manipulation. First, Appendix Table A.1 reports balancing tests for a variety of grandparent, parent, and grandchild characteristics using the baseline RD specification of Table 2. Out of 16 estimates, one is significant at the 5% level, roughly what would be expected due to chance. As a second test, we find no evidence of a discontinuous jump in the density of grandparent age around the cutoff in our sample using a McCrary (2008) test (p-value = 0.801). As a final test, we will show in our robustness table that the inclusion of pre-determined control variables has little effect on the RD estimates.

5 Results

5.1 First Stage

We begin by showing how the reform affected the first generation's (G1) use of DI. The reform affected both eligibility and benefit levels. Therefore, for these grandparents, we have two "first stage" regressions which have a similar specification to equation (1): one for the the extensive margin (DI participation) and one for the intensive margin (DI payment amount).

Figure 1 illustrates how the reform affected a grandparent's DI for each of these margins as of 1999. In both graphs, the running variable on the x-axis is the age of the grandparent at the time of the reform, with the reform cutoff of age 45 denoted with a dashed vertical line. The dots are 6 month binned values of the outcome variable, with the solid lines showing regression lines based on unbinned data.[21]

The effect of the reform on the extensive margin is shown in panel (a). There is a sharp increase in grandparent exit from DI use before the cutoff age of 45. Regression results based on equation (1) indicate a 4.8 percentage point rise (s.e.=1.1) in DI exit for those exposed to the reform, which is a 35% increase relative to the mean. Panel (b) shows how the reform affects DI benefit amounts, where DI benefit amounts are set to 0 for individuals who exit DI. There is a clear jump at the cutoff; regression estimates reveal the reform reduced the amount of DI benefits by 1,267 euros (s.e.=249), which translates to an 13% drop relative to the mean.[22] In other words, the reform had a large impact on both participation and benefits received.

While it would be interesting to disentangle the separate effects of the extensive and intensive margin for the first generation (G1) on third generation (G3) outcomes, this is not feasible, since there is only one reform.[23] For this reason, we focus on the reduced form estimates for the third generation, which capture the policy-

[21] Appendix Figure A.1 shows first stage graphs using 3 month bins.

[22] These estimates use the same specification as the baseline specification in Table 2.

[23] IV estimates for a single margin would require a strong exclusion restriction: either that G1 participation is all that matters but not G1 benefit levels, or visa versa.

relevant parameters of tightening eligibility requirements in a DI system across three generations.

5.2 Third generation effects

5.2.1 Graphical evidence

We now turn to effects on the third generation, which is the primary focus of this paper. The third generation is still relatively young, which is why we look at health at birth and early childhood outcomes. We start with a visual representation of our main RD estimates in Figure 2. In each of the graphs, the outcomes are constructed so that a lower value represents a better outcome. These graphs plot averages for six-month bins; three-month and one-month bins can be found in Appendix Figures A.2 and A.3.

There are several patterns which are common across all seven graphs in Figure 2. First, the regression lines in each of the figures slope downwards, indicating that G3 outcomes improve in the age of their grandparent at the time of the reform. While it is difficult to know exactly what drives these age patterns, similar age patterns also exist for a placebo sample unaffected by the reform, as discussed in Section 5.2.4. Hence, the negative cohort slopes appear to be a general feature of the data. These slopes imply that a simple comparison of average outcomes to the left and right of the age cutoff would understate the true effect, highlighting the need to use an RD design.

More importantly, there is a clear upward jump at the cutoff for each outcome, indicating better birth outcomes and health and education in early childhood for children of reform-treated grandparents. In other words, grandchildren whose grandparents had lower DI participation and benefits on average (those to the left of the cutoff) are better off compared to those whose grandparents had higher DI use on average (those to the right of the cutoff).

5.2.2 Birth outcomes

Table 2 presents the reduced-form RD estimates corresponding to Figure 2. We use an RD model with separate linear trends on each side of the cutoff, a bandwidth of 60 months on each side, and triangular weights. These regressions also include the predetermined or immutable G1, G2, and G3 controls described in the table note. All standard errors are clustered at the grandparent level.

Panel A reports effects on the third generation at birth. We find statistically and economically significant improvements for treated grandchildren whose grandparents received less support from DI. The probability of being born premature (<37 weeks) drops by an economically and statistically significant 1.5 percentage points. For early births (<38 weeks), the drop is 2.2 percentage points. Relative to their respective means, these are sizable 19% and 14% decreases, respectively.

Table 2 reveals the improvements in gestational age are also reflected in a reduced probability of low birthweight. Grandchildren of those subject to the stricter DI rules are 1.1 percentage points less likely to be low birthweight, which represents an 17% drop relative to the mean. For context, this effect is the same order of magnitude as the effect of a 10% negative economic shock, but smaller than the effect of maternal smoking.[24]

Effects for a broader range of gestational age and birthweight cutoffs are plotted in Figure 3. Panel (a) plots RD estimates using our baseline specification, but in addition to reporting estimates for premature birth (<37 weeks) and early birth (<38 weeks), it plots similar estimates for all cutoffs between 35 and 42 weeks. The figure shows decreases at every gestational age cutoff of 39 weeks or less, with all but the 35 week cutoff being statistically significant.[25] In other words, having a grandparent

[24]Bozzoli and Quintana-Domeque (2014) find a reduction in economic activity of 0.1 log points increases the probability of low birthweight by 9-10 percent. Lien and Evans (2005) find that maternal smoking doubles the chance of having a baby with low birthweight.

[25]When interpreting magnitudes, it is important to take into consideration that a smaller fraction of births occur at earlier gestational ages. Hence, while the lower cutoffs have smaller percentage point changes (as indicated in the figure), they generally have larger percent changes relative to their means.

exposed to the reform, and hence eligible for fewer DI benefits, increases gestational age at birth up through 39 weeks.

Panel (b) performs a similar exercise for different birthweight cutoffs in addition to the low birthweight cutoff (<2,500 grams). The figure reveals statistically significant drops in birthweights less than 3,000 grams, 2,500 grams, and 2,000 grams. In other words, grandparents subject to the stricter DI rules had grandchildren who were less likely to be underweight, consistent with the reduction in low gestational age.

Returning to Table 2, we investigate one other birth outcome in panel A: whether the birth included specialized pediatric care. This is a proxy for a more involved or at-risk birth, and occurs in 28% of births in our dataset. We find a substantial reduction in the probability of a more involved or at-risk birth. Grandchildren whose grandparent was exposed to the reform are 2.1 percentage points less likely to have specialized pediatric care present at their birth. This is a reduction of roughly 8% relative to the mean.

5.2.3 Early childhood outcomes

Table 2, panel B reports outcomes in early childhood, where due to the age of children we focus on firstborns. The results on health during early childhood are consistent with those observed at birth. There is a 2.7 percentage point drop in drug prescriptions for children whose grandparents were treated (i.e., subject to less generous DI benefits), which is a 5% decline relative to the mean.

We next look at cumulative health expenditures, which serves as another proxy for the health of a child. We take the inverse hyberbolic sine of expenditures, rather than the natural log, so that we can include the small fraction (<1%) of individuals with no expenditures. We find that health expenditures drop by 7%, suggesting improvements in G3 childhood health if their grandparent had access to less generous DI benefits.

We also observe a positive effect of reduced first generation DI use on grandchildren's educational performance in primary school. Our measure is whether the child has ever repeated a grade, which roughly 1 in 5 children do in our sample. Having a grandparent exposed to the stricter DI rules reduces the chances of repeating a grade by 3.0 percentage points, or 15% relative to the mean. Grade repetition not only signals poor performance in school, but could also matter if it increases later dropout probabilities or uses up more educational resources (Eide and Showalter, 2001; Giano et al., 2022; Jacob and Lefgren, 2009).

Taken together, the combined evidence from panels A and B all points in the same direction. Grandchildren whose grandparents who were subject to removal from DI or having their DI benefits reduced experience significantly better birth outcomes and early childhood outcomes.

To help interpret these findings, it is useful to summarize prior research for the first two generations. Borghans et al. (2014) show that directly affected G1 individuals experienced a strong rebound in earnings, replacing almost two-thirds of their lost DI benefits with labor market income. In the short run, they substituted towards other social assistance programs, but in the longer run, total income from all sources fell. Dahl and Gielen (2021) show that G2 individuals whose G1 parents were subject to the harsher DI rules were themselves less likely to participate in DI, had higher labor market earnings (and overall income), invested in more years of schooling, and experienced lower arrest rates. So while the improved outcomes for G3 grandchildren cannot be attributed to an increase in G1 grandparent income, they could reflect the improved situation of their G2 parents.

5.2.4 Robustness and placebo tests

Appendix Table A.2 reports robustness checks for third generation outcomes. The results are robust to using quadratic trends, not using triangular weights, omitting the control variables, using different windows, and using local linear regression.

To further probe the validity of our estimates, we conduct placebo tests. To do this, we construct a placebo sample of G3 grandchildren whose G1 grandparents were age 40-50 at the time of the reform, but who were not on DI at the time of the reform. While some of these G1 individuals may go on to DI later, all of them will be subject to the new DI rules, regardless of their age. Hence, there is no age-cohort discontinuity at 45.

In Appendix Table A.3, we report RD estimates similar to Table 2, but using our placebo sample instead. As long as there are no other policies or shocks which differentially affected G1 individuals over the age of 45, there should be no jump in G3 child outcomes at the cutoff. Indeed, this is what we find. The appendix table reveals that the estimates for all of our outcomes are small, precisely estimated, and statistically insignificant. Appendix Figure A.4 plots the RD graphs for this placebo analysis. For all of the outcomes, there are negative slopes as a function of the running variable, although less pronounced compared to the main estimation sample. More importantly, there is no visual evidence of a jump at the placebo cutoff age.

As a final exercise, in Appendix Table A.4 we estimate effects for G3 grandchildren whose G2 parents were age 19 or older at the time of the reform and still living at home. In prior work (Dahl and Gielen (2021)), we found smaller impact on the second generation for this older group. In the current paper, the effect of the DI reform on their G3 children is likewise muted, with estimates close to zero and only 1 out of 9 being statistically significant at the 10% level (which is roughly what would be predicted by chance). This provides some suggestive evidence that the effect of the reform is primarily working through its impacts on G2 parents, rather than the original impacts on G1 grandparents spilling directly over onto the G3 kids.

6 Conclusion

This paper provides the first causal evidence that participation in a social support program can have long-lasting effects across three generations. We find that grandchildren whose grandparents were forced off of DI or had their benefits reduced are less likely to be born premature, have low birthweight, or have complicated births.

Moreover, these grandchildren also have better health in early childhood, as proxied by lower health care expenditures and lower use of prescription drugs, and are less likely to repeat a grade in primary school. These birth and early childhood outcomes have been shown to have important long-lasting consequences.

While these results are striking, we caution that they should be interpreted in context. Around the time of our reform, 12% of the Dutch working age population was on DI, and the program consumed 4.2% of GDP at its peak. We study the consequences of tightening DI eligibility and benefits for marginal participants with substantial work capacity. Our results suggest that safety net programs can go too far and have the unintended effect of harming future generations. Of course, social program participation in other contexts could alternatively help families escape poverty traps, with third generation effects moving in the opposite direction. Our work highlights the key role government programs can play in shaping outcomes across multiple generations. In future work it would be interesting to study the long-term impacts of public programs in other countries and settings.

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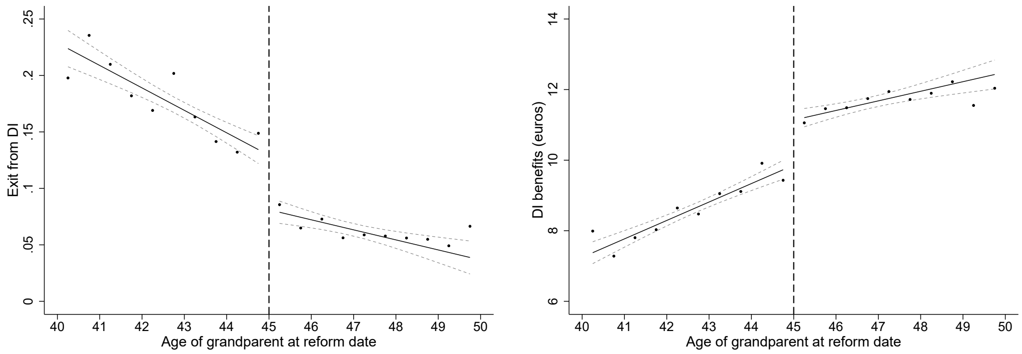
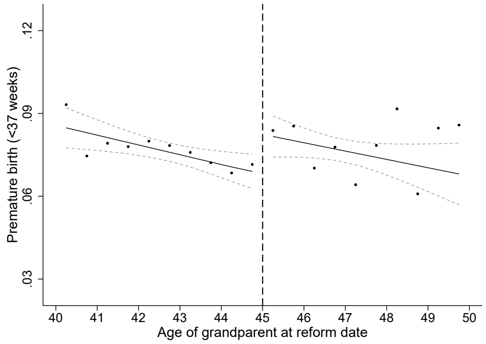


(a) Exit from DI

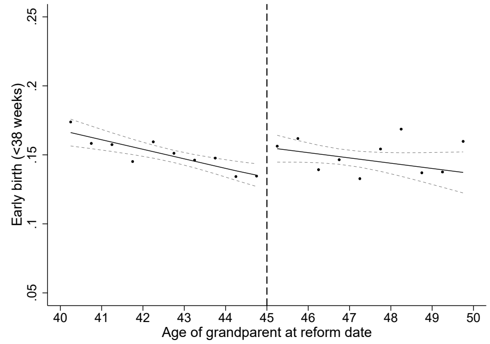
(b) DI benefit amount

Figure 1: First stage RD graphs for grandparent DI participation and benefits

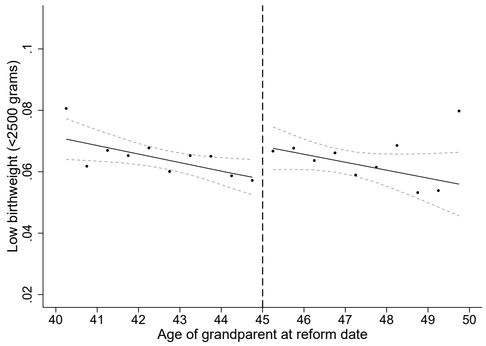
Notes: Averages for six-month age bins, based on grandparent's age as of the reform date, for (a) DI participation and (b) DI benefit amount in euros. Dashed vertical lines denote the reform cutoff. Solid trend lines are based on regressions using unbinned data, with dotted lines indicating pointwise 90 percent confidence intervals.



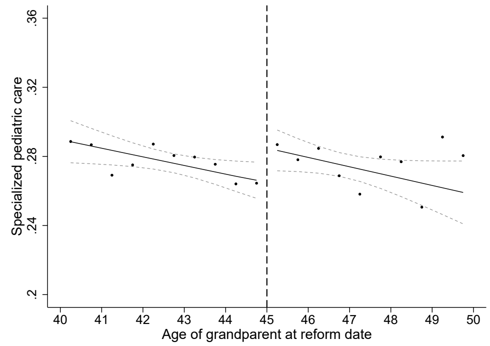
(a) Premature birth



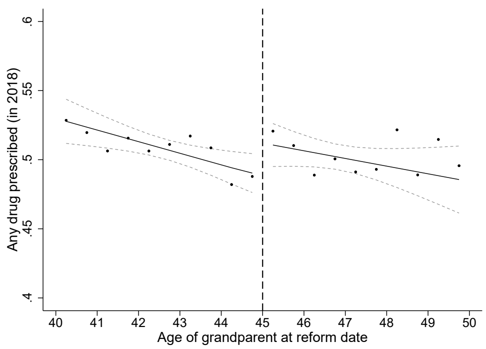
(b) Early birth



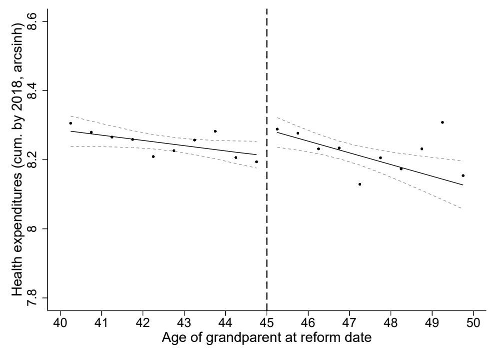
(c) Low birthweight



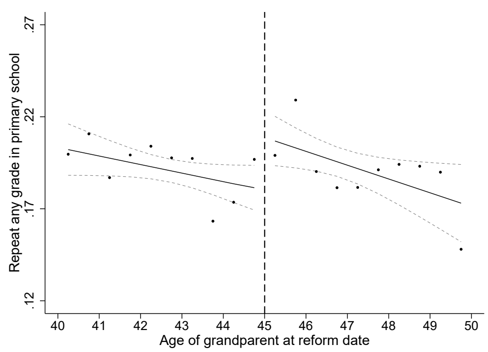
(d) Specialized pediatric care



(e) Drug Prescription



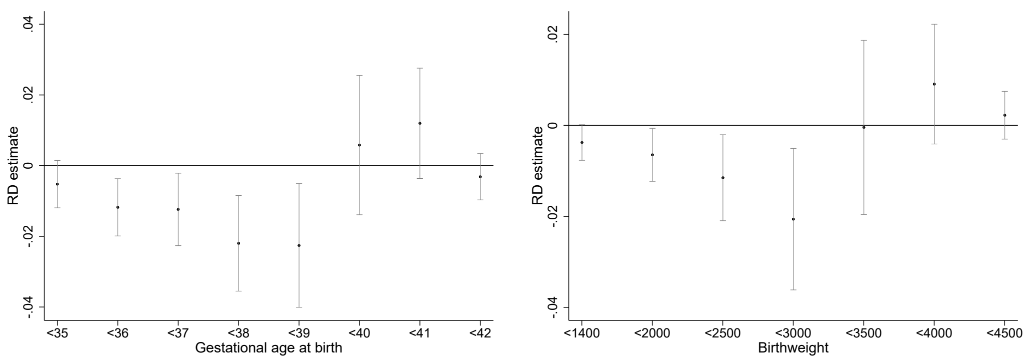
(f) Health expenditures



(g) Grade repetition

Figure 2: RD graphs for third generation outcomes

Notes: Averages for six-month age bins, based on grandparent's age as of the reform date. Dashed vertical lines denote the reform cutoff. Solid trend lines are based on regressions using unbinned data, with dotted lines indicating pointwise 90 percent confidence intervals.



(a) Gestational age at birth (b) Birthweight

Figure 3: RD estimates for different gestational age and birthweight cutoffs

Notes: Figure displays RD estimates using the baseline RD specification in Table 2, but for different gestational age and birthweight cutoffs. Vertical bars denote 90 percent confidence intervals.

Table 1: Summary statistics for third generation outcomes

	Grandparent age			
	40-50	44	45	Diff 44-45
A. At birth				
Pregnancy term				
Gestational age (in days)	275.75	276.18	275.57	0.61
Premature (born <37 weeks, %)	7.75	6.85	8.47	-1.62**
Early birth (born <38 weeks, %)	15.07	13.59	15.90	- 2.31**
Birthweight				
Weight (in grams)	3401.88	3414.45	3399.22	15.23
Low weight (<2500g, %)	6.47	5.78	6.74	-0.96**
Health conditions at birth				
Specialized pediatric care (%)	27.67	26.50	28.23	-1.73**
B. Early childhood				
Health				
Any drugs prescribed (in 2018, %)	50.66	48.79	51.59	2.80**
Health expenditures (cum. by 2018, arcsinh)	8.24	8.21	8.28	0.07
Educational performance				
Repeat any grade in primary school (%)	19.28	18.50	21.29	-2.79**

Notes: Number of observations in panel A, first column, is 44,398 for gestational age, premature, and early birth; 44,489 for weight and low weight; 44,518 for specialized pediatric care. Number of observations in panel B, first column, is 23,903 for any drugs prescribed and health expenditures; 19,754 for repeat any grade in primary school.
** p<.05, * p<.10

Table 2: RD estimates for the third generation

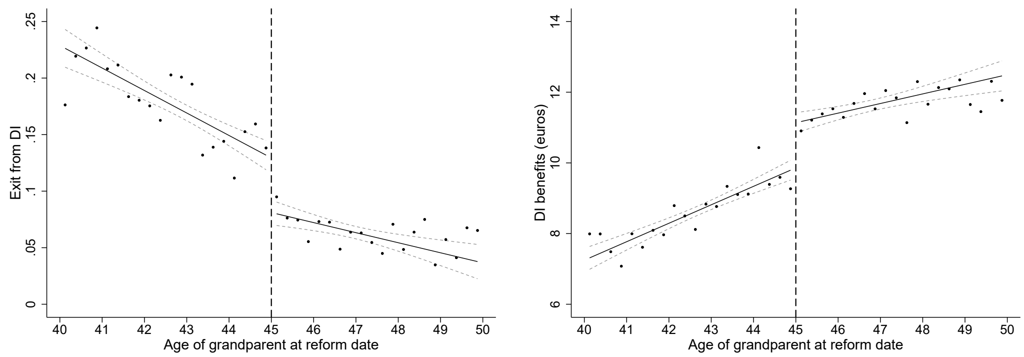
	Effect	Std error	Mean
A. At birth			
Pregnancy term			
Gestational age (days)	0.575	(0.356)	276
Premature (born <37 weeks)	-0.015**	(0.006)	0.078
Early birth (born <38 weeks)	-0.022**	(0.008)	0.151
Birthweight			
Weight (grams)	14.620	(15.729)	3,402
Low weight (<2500 grams)	-0.011*	(0.006)	0.065
Health conditions at birth			
Specialized pediatric care	-0.021**	(0.010)	0.277
B. During early childhood			
Health			
Any drug prescribed (in 2018, %)	-0.027**	(0.014)	0.507
Health expenditures (cum. by 2018, arcsinh)	-0.069*	(0.036)	8.243
Educational performance			
Repeat any grade in primary school (%)	-0.030**	(0.012)	0.193

Notes: See Table 1 for sample sizes. Estimates based on an RD model with separate linear trends on each side of the cutoff and triangular weights. Grandparent control variables include age, birth month dummies, a gender dummy, a cubic in predisability earnings, a dummy for no predisability earnings, six dummies for degree of disability, a cubic in DI duration, a dummy for native Dutch, a marriage dummy, and number of children in the household; parent control variables include age and a gender dummy; grandchild control variables include a gender dummy. The reported effect for health expenditures transforms the arcsinh coeffient $\hat{\theta}$ using the suggestion found in Bellemare and Wichman (2020) (i.e., $e^{\hat{\theta}-1}$) and uses the delta method to transform the standard error. Standard errors in parentheses, clustered at the grandparent level.
** p<.05, * p<.10

Online Appendix

Persistent Effects of Social Program
Participation on the Third Generation

by Gordon B. Dahl and Anne C. Gielen

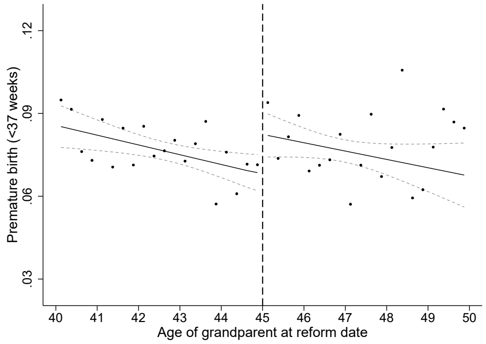


(a) Exit from DI

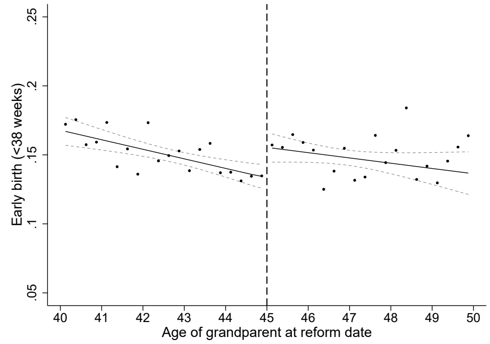
(b) DI benefit amount

Figure A.1: First stage RD graphs for grandparent DI participation and benefits: 3 month bins

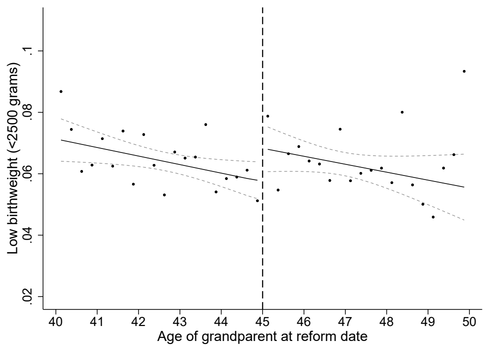
Notes: Averages for three-month age bins, based on grandparent's age as of the reform date, for (a) DI participation and (b) DI benefit amount in euros. Dashed vertical lines denote the reform cutoff. Solid trend lines are based on regressions using unbinned data, with dotted lines indicating pointwise 90 percent confidence intervals.



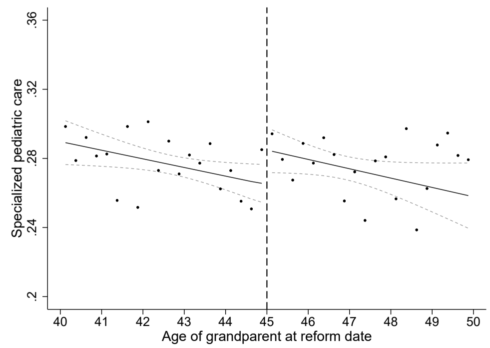
(a) Premature birth



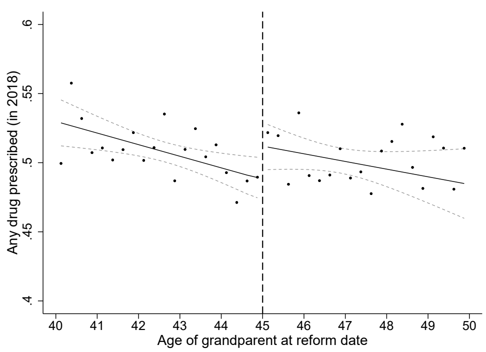
(b) Early birth



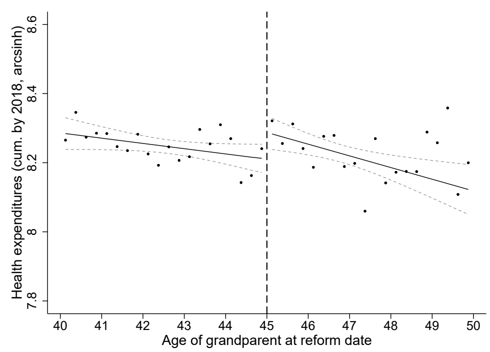
(c) Low birthweight



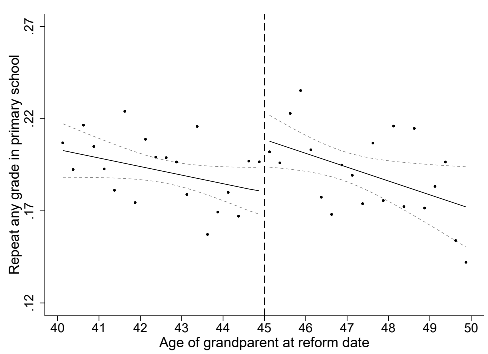
(d) Specialized pediatric care



(e) Drug Prescription



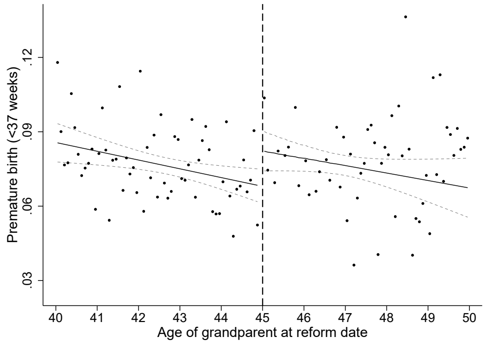
(f) Health expenditures



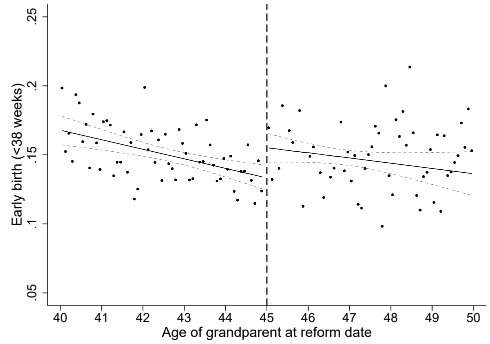
(g) Grade repetition

Figure A.2: RD graphs for third generation outcomes: 3 month bins

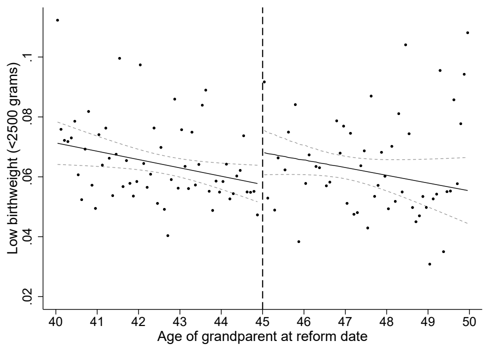
Notes: Averages for three-month age bins, based on grandparent's age as of the reform date. Dashed vertical lines denote the reform cutoff. Solid trend lines are based on regressions using unbinned data, with dotted lines indicating pointwise 90 percent confidence intervals.



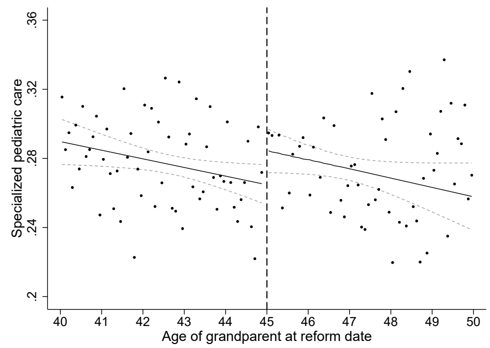
(a) Premature birth



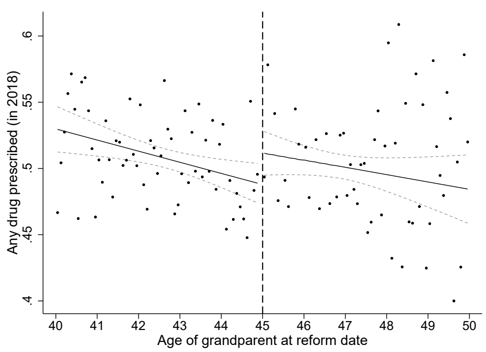
(b) Early birth



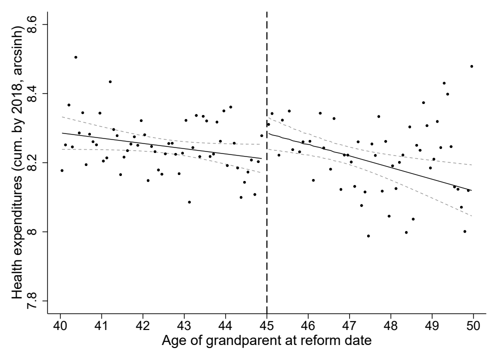
(c) Low birthweight



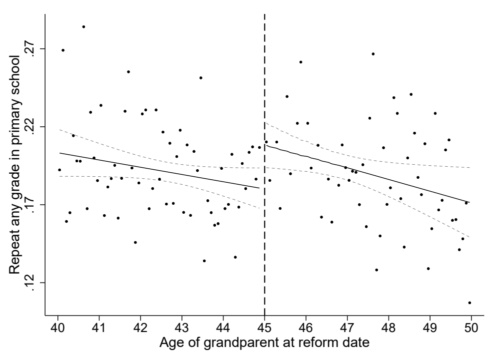
(d) Specialized pediatric care



(e) Drug Prescription



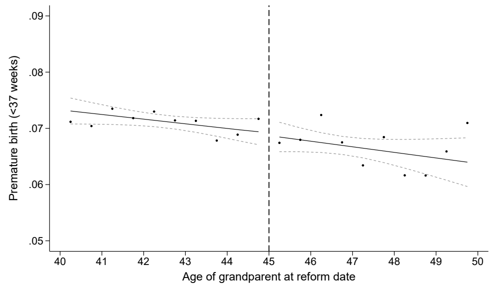
(f) Health expenditures



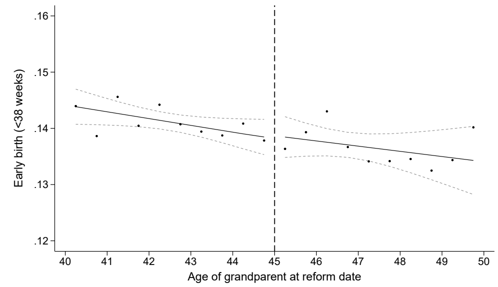
(g) Grade repetition

Figure A.3: RD graphs for third generation outcomes: 1 month bins

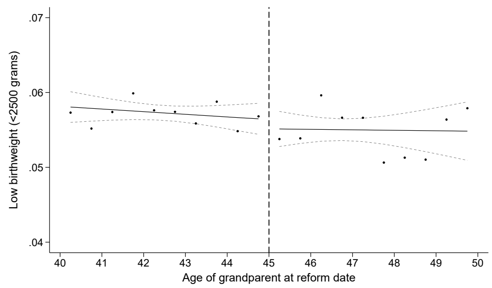
Notes: Averages for one-month age bins, based on grandparent's age as of the reform date. Dashed vertical lines denote the reform cutoff. Solid trend lines are based on regressions using unbinned data, with dotted lines indicating pointwise 90 percent confidence intervals.



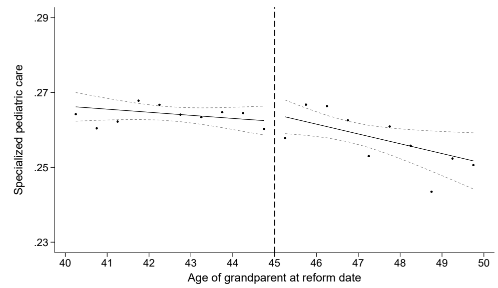
(a) Premature birth



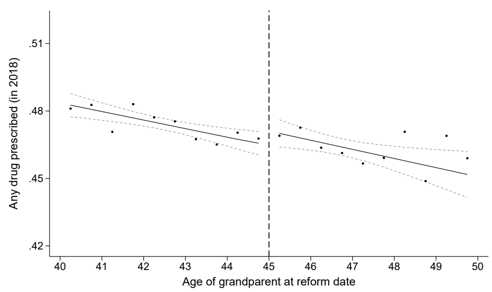
(b) Early birth



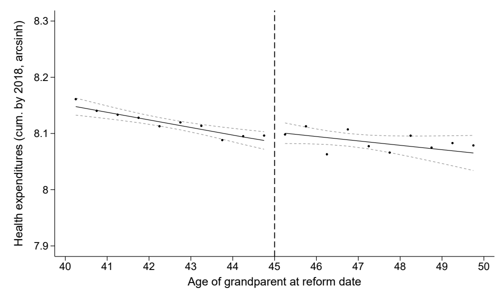
(c) Low birthweight



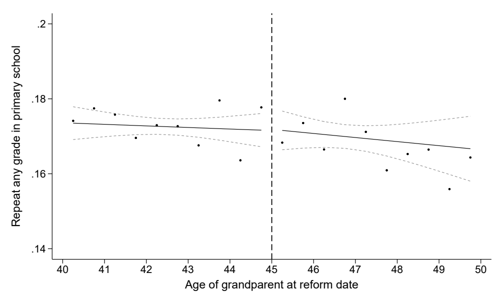
(d) Specialized pediatric care



(e) Drug Prescription



(f) Health expenditures



(g) Grade repetition

Figure A.4: Placebo RD graphs using grandparents not on DI at the time of the reform

Notes: See Table A.3 for placebo estimates. Averages for six-month age bins, based on grandparent's age as of the reform date. Dashed vertical lines denote the reform cutoff. Solid trend lines are based on regressions using unbinned data, with dotted lines indicating pointwise 90 percent confidence intervals.

Table A.1: RD estimates for covariate balance

	Effect	Std error	Mean
A. Grandparent (G1) characteristics			
Female	0.011	(0.012)	0.20
Married	0.007	(0.009)	0.91
Duration DI (months)	-0.035	(2.092)	86.9
Degree of disability			
15-25%	0.013	(0.009)	0.12
25-35%	-0.026**	(0.010)	0.14
35-45%	-0.008	(0.009)	0.10
45-55%	0.004	(0.008)	0.08
55-65%	0.004	(0.005)	0.03
65-80%	0.001	(0.005)	0.03
80-100%	0.012	(0.015)	0.51
Pre-DI earnings (1,000 Euros)	-0.066	(0.108)	6.76
Native Dutch	-0.009	(0.008)	0.92
Number of kids in household	-0.037	(0.029)	2.16
B. Parent (G2) characteristics			
Female	-0.010	(0.014)	0.53
Age (as of Nov 1996)	-0.038	(0.036)	16.7
C. Child (G3) characteristics			
Female	-0.005	(0.010)	0.49

Notes: Each row is a separate RD estimate of the effect of the DI reform on a predetermined or immutable outcome. Regression includes separate linear trends on each side of the cutoff and uses triangular weights. Number of grandparent observations is 19,895; number of parent observations is 23,294; number of child observations is 44,398. Standard errors in parentheses, clustered at the grandparent level.
** $p<.05$, * $p<.10$

Table A.2: Robustness checks

	Baseline	Quadratic trends	No weights	No controls	45 month window	30 month window	Alt. clustering	LLR	LLR alt. c.i.
A. At birth									
Pregnancy term									
Premature (born <37 weeks)	-0.015**	-0.025**	-0.011*	-0.014**	-0.016**	-0.022**	-0.015**	-0.014**	-0.014
	(0.006)	(0.009)	(0.006)	(0.006)	(0.007)	(0.008)	(0.005)	(0.006)	[-.004,-.025]
Early birth (born <38 weeks)	-0.022**	-0.033**	-0.018**	-0.022**	-0.022**	-0.033**	-0.022**	-0.022**	-0.022
	(0.008)	(0.012)	(0.008)	(0.008)	(0.009)	(0.011)	(0.006)	(0.008)	[-.008,-.036]
Birthweight									
Low weight (<2500 grams)	-0.011*	-0.016*	-0.008	-0.011*	-0.011*	-0.013*	-0.011**	-0.011*	-0.011
	(0.006)	(0.009)	(0.005)	(0.006)	(0.006)	(0.008)	(0.005)	(0.006)	[-.001,-.020]
Health conditions at birth									
Specialized pediatric care	-0.021**	-0.033**	-0.016	-0.020*	-0.023**	-0.033**	-0.021**	-0.020*	-0.020
	(0.010)	(0.015)	(0.010)	(0.010)	(0.011)	(0.014)	(0.008)	(0.010)	[-.002,-.038]
B. During early childhood									
Health									
Any drug prescribed	-0.027**	-0.047**	-0.022*	-0.024*	-0.029*	-0.042**	-0.027**	-0.024*	-0.024
	(0.014)	(0.020)	(0.013)	(0.014)	(0.015)	(0.018)	(0.013)	(0.014)	[.003,-.051]
Health expenditures	-0.069*	-0.102**	-0.054	-0.072**	-0.080*	-0.095**	-0.069**	-0.072**	-0.072
	(0.036)	(0.050)	(0.033)	(0.036)	(0.038)	(0.045)	(0.030)	(0.036)	[-.000,-.139]
Educational performance									
Repeat any grade (%)	-0.030**	-0.029*	-0.031**	-0.028**	-0.029**	-0.035**	-0.030**	-0.028**	-0.028
	(0.012)	(0.018)	(0.011)	(0.012)	(0.013)	(0.016)	(0.011)	(0.012)	[-.004,-.052]

Notes: See notes to Table 2. Alt. clustering column clusters at the level of the running variable rather than the grandparent level. LLR column estimates local linear regression using a bandwidth of 60 months. LLR alt. c.i. estimates LLR but using the the approach of Kolesár and Rothe (2018) to calculate 95% confidence intervals (reported in brackets). To estimate the tuning parameter which reflects the possible curvature, for each outcome we use our placebo sample (see Section 5.2.4 and Appendix Table A.3), a local linear regression, and a second-order polynomial to estimate the curvature around the placebo cutoff of age 45. Standard errors in parentheses, clustered at the grandparent level (except for the Alt. clustering column).

** p<.05, * p<.10

Table A.3: Placebo tests: RD estimates using grandparents not on DI at the time of the reform

	Effect	Std error	Mean
A. At birth			
Pregnancy term			
Gestational age (days)	0.020	(0.130)	276
Premature (born <37 weeks)	0.001	(0.002)	0.070
Early birth (born <38 weeks)	-0.000	(0.003)	0.140
Birthweight			
Weight (grams)	5.213	(5.752)	3,434
Low weight (<2500 grams)	0.001	(0.002)	0.056
Health conditions at birth			
Specialized pediatric care	-0.002	(0.004)	0.262
B. During early childhood			
Health			
Any drug prescribed (in 2018, %)	-0.006	(0.005)	0.472
Health expenditures (cum. by 2018, arcsinh)	-0.015	(0.016)	8.112
Educational performance			
Repeat any grade in primary school (%)	0.000	(0.004)	0.172

Notes: RD estimates using the baseline specification of Table 2, but for a placebo sample of G3 grandchildren whose G2 parents satisfy the same age and living arrangement restrictions we make for our main sample and whose G1 grandparents were age 40-50 at the time of the reform, but **not on DI** at the time of the reform. Since all of these G1 individuals were subject to the new rules, there is no age-cohort discontinuity at age 45. Number of observations is 331,756 for gestational age, premature, and early birth; 332,302 for weight and low weight; 332,609 for specialized pediatric care; 181,442 for any drugs prescribed and health expenditures; 142,944 for repeat any grade in primary school. Standard errors in parentheses, clustered at the grandparent level.
** p<.05, * p<.10

Table A.4: RD estimates using parents over the age of 19 at the time of the reform and still living at home

	Effect	Std error	Mean
A. At birth			
Pregnancy term			
Gestational age (days)	0.259	(0.296)	276
Premature (born <37 weeks)	-0.002	(0.005)	0.082
Early birth (born <38 weeks)	-0.004	(0.007)	0.152
Birthweight			
Weight (grams)	10.585	(13.006)	3,398
Low weight (<2500 grams)	-0.001	(0.005)	0.071
Health conditions at birth			
Specialized pediatric care	-0.002	(0.008)	0.266
B. During early childhood			
Health			
Any drug prescribed (in 2018, %)	0.021*	(0.011)	0.540
Health expenditures (cum. by 2018, arcsinh)	0.018	(0.031)	8.202
Educational performance			
Repeat any grade in primary school (%)	-0.005	(0.009)	0.175

Notes: RD estimates using the baseline specification of Table 2, but for a sample of third generation children whose second generation parents were over the age of 19 at the time of the reform and still living at home. Number of observations in panel A is 66,395 for gestational age, premature, and early birth; 66,621 for weight and low weight; 66646 for specialized pediatric care. Number of observations in panel B is 37,886 for any drugs prescribed and health expenditures; 34,191 for repeat any grade in primary school. Standard errors in parentheses, clustered at the grandparent level.
** p<.05, * p<.10