Intergenerational Spillovers in Disability Insurance*

Gordon B. Dahl† Anne C. Gielen‡

January 30, 2018

Abstract: Does participation in a social assistance program by parents have spillovers on their children's own participation, future labor market attachment, and human capital investments? While intergenerational concerns have figured prominently in policy debates for decades, causal evidence is scarce due to nonrandom participation and data limitations. In this paper we exploit a 1993 policy reform in the Netherlands which tightened disability insurance (DI) criteria for existing claimants, and use rich panel data to link parents to children's long-run outcomes. The key to our regression discontinuity design is that the reform applied to younger cohorts, while older cohorts were exempted from the new rules. We find that children of parents who were pushed out of DI or had their benefits reduced are 11% less likely to participate in DI themselves, do not alter their use of other government safety net programs, and earn 2% more in the labor market as adults. The combination of reduced government transfers and increased tax revenue results in a fiscal gain of 5,900 euros per treated parent due to child spillovers by 2014. Moreover, children of treated parents complete an extra 0.12 years of schooling on average, an investment consistent with an anticipated future with less reliance on DI. Our findings have important implications for the evaluation of this and other policy reforms: ignoring parent-to-child spillovers understates the long-run cost savings of the Dutch reform by between 21 and 40% in present discounted value terms.

Keywords: Peer effects, disability insurance, intergenerational links
JEL codes: I38, H53, J62

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*We thank Kate Antonovics, Prashant Bharadwaj, Julie Cullen, Roger Gordon, Olivier Marie, and Erik Plug for helpful advice, and seminar participants at several universities and conferences for useful comments and suggestions. Financial support from the Tinbergen Institute is gratefully acknowledged.

†Department of Economics, University of California San Diego; email: gdahl@ucsd.edu
‡Erasmus School of Economics, Erasmus University Rotterdam; email: gielen@ese.eur.nl

1 Introduction

Parental participation in social assistance programs could influence a child's own participation, and possibly even a child's future earnings and human capital investments. Arguments about the presence, type, and size of intergenerational spillovers have figured prominently in policy debates for decades. On the one hand, parental participation could create a cycle of government dependence and reduced employment within the family. Observing a parent out of the labor force and on public assistance could alter a child's perceptions about the relative costs, benefits, and stigma associated with the two alternatives.[1] On the other hand, intergenerational patterns could simply reflect shared negative environmental or genetic factors. Characteristics like poor health, bad neighborhoods, or reduced employment opportunities could be correlated across generations, creating mechanical intergenerational links which do not reflect a behavioral response on the part of the child. Figuring out whether the observed associations within a family are causal is crucial for understanding the reasons behind persistent participation and designing effective policies. Moreover, determining the long-term fiscal impacts of government assistance programs requires a full intergenerational accounting which includes changes in taxes paid and other transfer program receipt.

Estimating intergenerational spillovers is a difficult empirical problem because a parent's participation is not random. Credible identification requires an exogenous shock which affects a parent's participation, but does not directly affect their children. On top of this, one needs a dataset which links parents to children, contains a detailed set of outcome variables, and follows families over a long period of time. Because of these challenges, the existing evidence base on causal effects is scarce. We overcome these identification challenges by taking advantage of a policy reform which generates quasi-experimental variation in social program participation combined with rich administrative datasets.

Our setting is disability insurance (DI) in the Netherlands and a 1993 reform prompted by the rising costs of the Dutch system. In 1969, two years after its introduction, 4% of the Dutch working age population participated in DI, but by the late 1980s, participation had risen to 12% (Koning and Lindeboom, 2015). At its peak, the program cost 4.2% of GDP, and was not fiscally sustainable. Similar trends, while not always as dramatic, have occurred in most industrialized nations, including the U.S., the U.K., and other European countries (see Burkhauser, Daly, McVicar, and

[1] There could also be information transmission about how to enroll or differential child investments due to changing resource constraints.

Wilkins, 2014). Due to a series of reforms, including the one we study, DI participation in the Netherlands dropped to 7%. Dutch DI payments now constitute around 2.1% of GDP, which compares to 2.3% on average in Europe and 1.7% in the U.S.

The 1993 Dutch reform simultaneously tightened eligibility criteria and lowered payment generosity. It forced current DI recipients to be re-examined by a medical doctor and subjected to a new set of rules which made them weakly worse off. Some individuals received lower payments because their degree of disability was reduced, and others were disqualified from the program entirely. Importantly, the more stringent re-examination rules only applied to individuals less than age 45 as of August 1, 1993, since at the last minute older individuals currently on DI were grandfathered in. This differential application of the new rules creates an age discontinuity, with individuals around the cutoff being similar in all dimensions except for exposure to the stricter DI rules. Using a regression discontinuity (RD) design, Borghans, Gielen, and Luttmer (2014) find that approximately 4% of DI participants exited DI due to the more stringent rules and that annual benefits fell by around 1,000 euro, or 10%. A similar analysis, applied to our sample of parents with children, comprises the "first stage" of our paper.[2]

The goal of our paper is to explore how a parent's reduction in DI use affects their children's choices. We examine children's future participation in DI and other social assistance programs, labor market outcomes as adults, and human capital investments when young. Since the DI rule changes affected parents on both the intensive and extensive margins, we focus on the reduced form effects of the DI reform on child outcomes, but also present IV estimates which scale these effects by the parental drop in DI payments (treating exit from the program as a reduction in payments to 0). We use an RD design, where the running variable is the age of the parent and the dependent variables are child outcomes.

Our first result is that there is a strong link in DI usage between parents and children. Children whose parents are subject to the harsher DI rules are 1.1 percentage points less likely to have ever participated in DI 21 years after the reform date. This is as of 2014, when children are 37 years old on average and have an ever-participation rate of 10 percent. The corresponding IV estimate reveals that for every 1,000 euro drop in parental payments due to the reform, child participation drops by 0.9 percentage points. Using cumulative income received from DI as the dependent variable instead, children of treated parents received roughly 1,600 euros less in DI payments, which is sizable compared to the overall mean of 10,100 euros.

[2]We find slightly larger effects for our sample of parents with children, with 5.4% of parents exiting DI due to the reform and annual benefits dropping by 1,300 euro on average.

To get a fuller picture of intergenerational spillovers and fiscal impacts, we next assess whether a child's taxable earnings and participation in other social support programs change. These effects are typically ignored, but only with this information can the total spillover effect be calculated on the government's budget.[3] We find that cumulative earnings up to 2014 rise by approximately 7,200 euros, or a little less than 2%, for children of parents subject to the less generous DI rules. In contrast, we find no detectable change in cumulative unemployment insurance receipt, general assistance (i.e., traditional cash welfare), or other miscellaneous safety net programs. The estimated cumulative increase in taxes minus government transfers is approximately 3,500 euros per child. While roughly 45% of this amount can be attributed to cost savings from lower DI payments, the remaining is due to increased tax revenue resulting from higher earnings. Since parents in our sample have an average of 1.7 children living with them at the time of the reform, this implies 5,900 euros in positive intergenerational spillovers per treated parent.

To gauge the importance of these fiscal spillover effects, we compare them to the direct effects of the reform on parents. Up through 2013, when parents around the reform cutoff reach age 65, we find a large reduction in a parent's cumulative DI benefits, a modest increase in other benefit receipt, and a statistically insignificant effect in taxes paid. Compared to our child estimates up through 2013, we find that children account for 21% of the net fiscal savings of the reform in present discounted value (PDV) terms. This percentage, although sizable, understates the long term savings due to child spillovers. This is because when parents turn 65, they become subject to mandatory retirement and DI benefits cease,[4] while their children have an additional 30 years or so of eligibility and work life remaining. Extrapolating the estimated child spillovers beyond 2013, we calculate that 40% of the PDV of savings in the long run is due to children.

We then turn to children's educational attainment as a possible mechanism, and find intriguing evidence for anticipatory investments. When a parent is subject to the reform which tightened DI benefits, their child invests in a statistically and economically significant 0.12 extra years of education relative to an overall mean of 11.5 years. The largest increase occurs for the margin of upper secondary school, with a 2.2 percentage point increase in the graduation rate. Since most schooling takes place before children have entered the labor market, these findings provide suggestive evidence that children of treated parents plan for a future with less reliance on DI

[3] A similar point, although not in the intergenerational context, is made in a recent working paper by Autor, Kostol, Mogstad, and Seltzer (2017).

[4] At age 65, parents transition to state pensions, which do not depend on employment history.

in part by investing in their labor market skills. As expected, the schooling effect is concentrated on children who are less than age 18 at the time the reform was implemented, since these children have more time to alter their educational plans.

We consider several explanations for our results. We begin by ruling out various possibilities which others have postulated for intergenerational spillovers. It cannot be information about how to apply for the program, as all parents have been through the DI screening process. Likewise, reductions in stigma from seeing a parent participate seems improbable, as both treated and untreated parents have already been on DI for a long time (7.5 years on average). The explanation is also not increased investments in children due to increases in family income or parental supervision. This is because the reform caused parental leisure to decrease and work hours to increase, with total parental income changing little in the short run but declining in the long run.

Instead, the two explanations most consistent with our findings are that children experience a scarring effect or learn about formal employment. Children whose parents are kicked off of DI or have their benefits reduced may infer they cannot rely on the government to take care of them, similar to the scarring effect talked about in Malmendier and Nagel (2011) in a different context. This type of scarring can explain why treated children invest more in education and work more in the future, even though they face the same labor market and social safety net as their untreated peers. Learning about formal employment via a parental role model is also consistent with our findings; participation in the labor market rises substantially for treated parents, with over 60% of lost benefits being replaced with earnings.

Despite the importance of intergenerational spillovers in policy discussions, there is remarkably little existing causal evidence. As surveyed by Black and Devereux (2011), there are many observational studies which document intergenerational links in the use of social assistance, but few with credible research designs. There are only a handful of papers which have tried to use exogeneous sources of variation for identification. Antel (1992) uses state-level welfare benefits and net migration flows in a Heckman selection model and finds evidence for intergenerational links. Levine and Zimmerman (1996) use variation in state benefit levels and local labor market conditions and conclude that most of the intergenerational correlation in welfare use is not causal. Hartley, Lamarche, and Ziliak (2017) use variation across U.S. states in the timing of welfare reform implementation and find a mother's use of welfare significantly increases the chances her daughter will participate as well. Finally, Dahl, Kostol, and Mogstad (2014) use a random judge assignment design and find that DI participation by parents in Norway increases the chances their child will participate as well.[5]

Our paper makes several contributions to this sparse literature. First, we leverage a nationwide policy reform which tightened DI eligibility rules in a way which generates convincing quasi-experimental variation. Moreover, we follow children to an age in adulthood when DI participation is relatively common. Another contribution is that we calculate the total fiscal costs of the intergenerational spillover, including changes in a child's DI payments, taxes, and other transfers, rather than simply focusing on the participation margin. We also provide a comparison of the cumulative cost savings from each generation, documenting the importance of both for the government's long-term budget. Finally, we find robust evidence that children invest more in schooling, consistent with an anticipated future with less reliance on government assistance. These novel findings highlight the strength and nature of parent-child interactions, and the importance of considering spillover effects in policy debates about social assistance programs.[6]

More broadly, our study complements a related literature which looks at other shocks to parents which have the potential to change children's long-run outcomes.[7] There is also a related literature on disability insurance programs and their labor supply effects.[8]

The remainder of the paper proceeds as follows. The next section provides background on disability insurance in the Netherlands, the 1993 reform, and the data. Section 3 lays out our RD design and discusses threats to identification. In Section 4, we present the first stage estimates for parents. Sections 5 and 6 present our

[5]Two related papers use a bounds analysis. Pepper (2000) finds large confidence intervals, while De Haan and Schreiner (2017) bound average treatment effects to be substantially below OLS and estimates in the literature which identify local treatment effects for marginal participants.

[6]While Dahl, Kostol, and Mogstad (DKM, 2014) also studies intergenerational DI participation, our paper goes beyond it by (i) examining the effects of a nationwide policy reform (DKM looks at the 6.25% of all DI applicants who are initially denied but then appeal), (ii) following children for 21 years by which time participation reaches 10% (versus 5 years in DKM's baseline sample with 3% participation), (iii) estimating a broader set of labor market, public assistance, and education outcomes (DKM focuses on the binary participation margin due to a lack of precision for other outcomes), and (iv) calculating the total fiscal costs to the government budget, including changes in DI payments, other transfers, and taxes. Moreover, our paper contributes to a better understanding of intergenerational patterns in DI use by (v) exploiting variation which forces individuals off of DI or reduces their benefits (DKM uses variation which denies or delays entry into DI), and (vi) using a completely different quasi-experimental research design and a different country.

[7]See Chen, Osberg, and Phipps (2015), Chetty, Hendren, and Katz (2016), Dahl and Lochner (2012), Katz, Kling, and Liebman (2001), Milligan and Stabile (2011), Oreopoulos (2003), Oreopoulos, Page, and Stevens (2008), Rege, Telle, and Votruba (2011), and Stevens and Schaller (2011).

[8]For a sampling, see Autor et al. (2016), Bound and Burkhauser (1999), Chen and van der Klaauw (2008), Campolieti and Riddell (2012), de Jong, Lindeboom, and van der Klaauw (2011), Deshpande (2016), French and Song (2014), Gruber and Kubik (1997), Kostol and Mogstad (2014), Maestas, Mullen, and Strand (2013), and von Wachter, Song, and Manchester (2011).

main results on child spillovers in program participation, work, and education and discusses the resulting fiscal implications. Section 7 conducts some heterogeneity and robustness analyses and compares our results to OLS. Section 8 concludes.

2 Background and Data

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. The program was generous compared to other countries, as it covered all workers with no waiting period, 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, de Vos, and Kapteyn, 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. The trends over time are documented in Figure 1 and discussed in more detail by Koning and Lindeboom (2015).

The current state of DI in the Netherlands is that payments now total around 2.1% of GDP (as of 2016). This compares to 2.3% in other European countries, and 1.7% in the U.S. In terms of participation, the 2012 Dutch rate of 7% is higher compared the U.S. rate of 5%, but lower than Norway's 10%, for example. One interesting contrast is that the U.S. rate continues to rise and is projected to reach 7% by 2018 (Burkhauser and Daly, 2012), while the Dutch rate is continuing to fall. Because of this, some have proposed adopting several aspects of the Dutch system to reverse the steeply increasing DI trends in the U.S. (Autor, 2015).

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; Autor and Duggan, 2003).

2.2 1993 Reform

Many changes are 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, Gielen, and Luttmer (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 the degree of their disability, which is based on the calculated income loss due to a disability. Calculated income loss is determined by comparing pre-disability earnings to a constructed measure of "earnings capacity." The reform that we exploit affected the calculation of this "earnings capacity," making it less generous to both current and new DI claimants.

The degree of disability is denoted in 8 categories; which category an individual belongs to is determined by the ratio of pre-disability earnings minus earnings capacity to pre-disability earnings. Individuals can continue to work and earn up to their remaining earnings capacity (pre-disability earnings minus earnings capacity) after a disability, and at the same time receive DI payments for the fraction of lost earnings.[9]

To explain the cohort discontinuity, we first need to describe how earnings capacity and benefits were determined before and after the 1993 reform. Prior to 1993, a medical doctor examined applicants and created a subjective list of work activities the applicant could still perform, based on a set of 27 physical activities (e.g., lifting, kneeling) and 10 psychological abilities (e.g., the ability to work under time pressure). This work activity list, in conjunction with the applicant's education level,

[9] Pre-disability annual earnings are indexed and subject to a cap (roughly 36,000 euro in 1999). If individuals earn more than their capped earnings exemption, their DI benefits are reduced temporarily, with a reclassification of the degree of disability only happening if an individual exceeds the cap for three years.

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 calculated degree of disability was then binned into categories which determined the replacement rate. Replacement rates varied from 0 to 70% of prior earnings.[10]

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, (i) the list of suitable occupations was expanded by no longer taking education level into account, (ii) only 3 suitable occupations were used to calculate earnings capacity, and (iii) the geographic region of 10 active workers was expanded 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, as long their disability has not gotten worse since their last re-examination.

A feature of the 1993 reform is that it specified all individuals age 50 or older at the time of the reform would be subject to the old rules and not re-examined at all. For individuals below age 50 as of January 8, 1993, the new rules affected both new applicants and existing DI participants. Since it was not logistically feasible to re-examine all DI participants immediately, they were scheduled to be re-examined over the ensuing years based on their age cohort, starting with the youngest cohorts under the age of 35 on August 1, 1993. The 35 to 40 year old cohort was scheduled to be re-examined in 1995, the 41-45 year old cohort between 1996-1997, and the 45-50 year old cohort between 1997-2001. However, on November 12, 1996 the Dutch Parliament passed a motion grandfathering the 45-50 year age group 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.

[10] For a degree of disability between 80-100% the replacement rate is 70%, for 65-80% it is 50.75%, for 55-65% it is 42%, for 45-55% it is 35%, for 35-45% it is 28%, for 25-35% it is 21%, for 15-25% it is 14%, and for less than 15% it is 0%.

2.3 Data

Our analysis uses several data sources that we can link through a unique identifier assigned to all individuals in the Netherlands. We combine administrative data from several sources on the universe of children of DI recipients for the time period we study. The disability administrative records begin in 1996 and are observed as late as 2014. The records include information on the start and end dates of a spell, the binned disability rating, DI payments received, pre-disability earnings, and the reason a spell ends. The records do not contain the medical doctor's diagnosis, the list of work activities the individual could still perform, or the set of suitable occupations.

We merge in data from a variety of administrative records for the period 1999 to 2014. We use data from Statistics Netherlands for earnings, self-employment, and unemployment insurance which is compiled using information from three different tax and social insurance record sources. This data starts in 1999. Unemployment insurance in the Netherlands can last up to 5 years depending on prior work history.

Data on general assistance (traditional cash welfare) and miscellaneous benefit programs come from the various organizations that administer the programs. As opposed to the U.S., general assistance has no time limit in the Netherlands and does not require dependents, although it is means tested. There are a variety of miscellaneous benefit programs during our time period, most of which are small in terms of benefit amounts and the size of the eligible population. This information comes from the "Polisadministratie" register, which is used to determine eligibility and benefit amounts for all Dutch social insurance programs.

We further merge in educational attainment as of 2014, as well as family structure in 2014. The education data is complete for younger cohorts, but comprises only a sample for older cohorts. Crime data on arrests and incarcerations come from two different data sources, and both span 2005-2014. Finally, we use municipal registry files for basic demographics. One advantage of this rich dataset merged from several sources is that we can study a variety of spillover effects across generations. Further details on most of these variables, and how they are measured, can be found in Appendix B of Borghans, Gielen, and Luttmer (2014).

Our data window focuses on parents who were between the ages of 40 and 50 and on DI as of the reform date of August 1, 1993. Due to data availability, our sample is limited to children of parents who were receiving DI benefits on August 1, 1993 and who were still on DI in 1995. It is important to realize this sample limitation should not create any biases. The reason is that 1995 is still before the DI re-examinations took place for the age 40-45 cohort and before the passage of the DI rule change exempting the age 45-50 cohort. Starting with 1995 affects the interpretation of our estimates, but not their validity. We also require the child to be living at home around the time of the reform and to be at least 25 by 2014; as an extra specification, we estimate effects for children not living at home at the time of the reform date. After imposing these restrictions, we have a sample of 116,356 children.[11] For the education analyses, our sample is smaller (N=79,924) since education was collected for all individuals in later cohorts, but only a subsample of earlier cohorts.

Summary statistics for both parents and children can be found in Appendix Table A1. The first column displays sample means for parents who were between the ages of 40 to 50 and on DI as of the cutoff date, and still on DI as of 1995. The other two columns show means for subsamples on each side of the 45 year-old age cutoff. On average, parents have been on DI for almost 7.5 years as of the reform cutoff date, with the older sample having approximately an extra half year of participation. Fifty-eight percent of parents are classified as fully disabled. Older parents are 10 percentage points more likely to be fully disabled, while younger parents have higher rates of low-level disability. Parents in our sample are predominantly male, married, and native Dutch.

Turning to the children, their average age is 15.6 as of the reform date. Appendix Figure A1 graphs the distribution of child ages separately for parents on each side of the age cutoff. There is substantial overlap in the two distributions. The fact that we have a sample of somewhat older children is due to two factors related to our sampling frame. First, few parents between the ages of 40 and 50 have young children, as fertility is highest when individuals are in their twenties and early thirties. Second, children in the Netherlands commonly live with their parents during their early years in the labor market and while attending college.

3 Model and Identification

3.1 Regression Discontinuity Design

The discontinuity we exploit arises from the fact that the reform affected some DI participants, but not others, based on their age. Parents who were age 45 to 50 as of August 1, 1993 were subject to the old DI rules, while parents between the the ages of 40 to 45 were re-examined according to the new, more stringent rules. The

[11] We drop parents of Turkish and Moroccan origin, as birthdate is often incorrectly registered for these individuals, and parents from the East Indies, as immigration rules were changing over time. We further drop children whose mother was less than age 18 at the time of their birth, children with missing covariates, and children with two parents on DI where one parent is treated and the other is not (we include children with two parents on DI if both parents have the same treatment status).

direct effect of the reform on parental outcome y^P can be modeled in a regression discontinuity (RD) framework as:

$$y_i^P = \alpha^P + 1[t_i^P \geq c](g_l(t_i^P - c) + \theta) + 1[t_i^P < c]g_r(c - t_i^P) + \delta^P x_i + e_i^P \qquad (1)$$

where t^P is the age of the parent on August 1, 1993, c is the cut-off age of 45, x is a vector of pre-determined parental and child characteristics, e^P is an error term, and g_l, and g_r are unknown functions. The coefficient θ is the first stage coefficient for the associated parental outcome (DI payment amount, or alternatively, DI participation).

The reduced form model for our RD design can be implemented as:

$$y_i^C = \alpha^C + 1[t_i^P \geq c](h_l(t_i^P - c) + \lambda) + 1[t_i^P < c]h_r(c - t_i^P) + \delta^C x_i + e_i^C \qquad (2)$$

where y^C is the relevant child outcome variable, x is a vector of pre-determined parental and child characteristics, e^C is an error term, and h_l, and hg are unknown functions. The coefficient λ is the reduced form (RF) or intention to treat (ITT) effect of the reform on outcomes. In the absence of covariates, the IV estimate is simply the ratio of the RF estimate of λ to the relevant first stage estimate of θ.

3.2 Threats to Identification

Manipulation. The validity of an RD design requires that individuals cannot manipulate the assignment variable, which in our setting is the parent's age at the time of the reform. Since parents cannot change their actual or officially recorded age easily in the Netherlands, there is little chance for this type of direct manipulation.

Since the DI data is not available until 1995, a similar threat to validity is that the reform caused differential attrition around the age 45 cutoff. As a reminder, our sample includes parents who were receiving DI benefits on August 1, 1993 (the date the reform went into effect for new applicants and the youngest cohort of existing claimants) and who were were still on DI in 1995. In other words, we can only observe whether an individual was receiving DI at the time of the initial implementation of the reform if they remained on DI until 1995. While the reform likely caused some claimants to exit DI in anticipation that they would be re-examined, it is unlikely to have caused a jump in exits around the age 45 cutoff. The reason is the re-examinations for individuals age 40-45 did not start until after 1995 and it was not until November 1996 that Parliament decided the 45-50 year old cohort would be grandfathered in to the old, more generous rules.[12]

Borghans et al. (2014) perform two empirical tests for manipulation for their

[12] While 40 year olds were initially scheduled to be re-examined at the end of 1995, the re-examinations took longer than initially expected. In conversations with the disability insurance office, we learned that few 40 year olds were re-examined before 1996.

sample, which includes all individuals on DI, and not just parents. They first graph the histogram of age at the time of the reform, and find no noticeable jumps around the age 45 cutoff. We find a similar result for our sample of parents: using a McCrary (2008) test, we do not reject the null hypothesis of a smooth density around the 45 year old cutoff (p-value=0.25). Second, they find no systematic evidence of changes in the distribution of pre-determined characteristics around the reform date. Using our sample of parents, we similarly find that almost all of the pre-determined characteristics do not jump significantly at the 45 year old cutoff. Moreover, the point estimates are small in magnitude and our RD estimates barely move when we include these characteristics in the regressions.

Exclusion Restriction. As long as parents cannot manipulate their age and there is no differential attrition around the age cutoff, the RD design will identify the ITT effects for children. That is, we can estimate the causal impacts on children of the 1993 DI reform which tightened DI generosity for some parents but not others. To scale these reduced form effects, we will be using parental DI payments as the first stage outcome. Interpreting the resulting IV estimates as the causal effect of a drop in parental DI payments requires an exclusion restriction: whether a parent was exposed to the 1993 reform should affect their child's outcomes only through the drop in parental DI payments, and not directly in any other way.

The drop in DI payments may not be a sufficient statistic for how the program changes affected children. For parents remaining on the program, the reform (weakly) decreased DI payments, whereas for parents kicked off DI or choosing to leave voluntarily, the reform reduced their payment to zero. Parental DI payments will capture both the intensive and extensive margins of the reform under the assumption that total DI payments are what matters. For the exclusion restriction to hold, therefore, parental participation versus non-participation cannot directly affect children except through the reduction in payments to zero. This implies, for example, that a parental reduction in benefits from 10,000 to 7,000 euros has the same effect as a parent who previously received 3,000 euros exiting the program and receiving 0 euros. Since this may not be the correct functional form for how the new stricter rules affected children, we focus more on the reduced form estimates throughout.

The 1993 reform may also have triggered a variety of changes for exposed parents, such as changes in parental labor supply, available family income, or even family structure. It is important to note these changes do not violate the exclusion restriction. Instead, they are potential mechanisms through which a shock to parental DI generosity affects children.

Monotonicity. If the effect of the drop in parental DI payments is constant for each child outcome, then the absence of manipulation combined with the exclusion restriction are sufficient for consistent IV estimation. With heterogeneous effects, however, monotonicity is also needed. In our setting, monotonicity requires that if a parent was exposed to the new, more stringent DI rules, they must receive DI payments which are lower or the same compared to what they would have received under the old rules. Monotonicity ensures that IV identifies the local average treatment effect (LATE) of a drop in parental DI payments, that is, the average effect among the subgroup of children whose parent's DI payments would have been lowered if they were exposed to the new versus old rules.

Since the new rules weakly reduced payments for any individual whose situation has not changed, monotonicity holds by construction for most of the sample. The one exception is that if a parent's illness has worsened, re-examination under the new, stricter rules could still result in a higher degree of disability classification (and hence a higher DI payment). Comparing the 40-45 age cohort, which was exposed to the stricter rules, with the 45-50 age cohort reveals this is unlikely to be an important issue. For the 40-45 age cohort, 5.8% of the sample had their degree of disability rating increase between 1996 and 1999, whereas for the 45-50 age cohort, 6.6% had their rating increase. This comparison indicates that any margin for non-monotonicity to matter is small, even taking into account that rating increases are expected to occur somewhat more often for older individuals.

4 First Stage Parental Estimates

This section documents the effect of the reform on parents using an RD design. An advantage of RD is that results can be presented graphically, which provides a transparent way of showing how the intergenerational spillovers are identified. Throughout the paper, we will begin with a graphical depiction of key outcomes before turning to a more detailed regression-based analysis. The figures will include outcomes aggregated into parental age bins, as well as separate linear trends on each side of the cutoff estimated using the underlying data and baseline regression specification. The regression lines best illustrate the trends in the data and the size of the jump, whereas the binned means provide a sense of the underlying variability in the data.

The top panel of Figure 2 graphs the relationship between parental DI payments and the reform. The sample is comprised of parents who were already receiving DI benefits before the reform. The running variable is the parent's age as of the reform date of August 1, 1993 and the cutoff age of 45 years old determines whether the parent is subject to the new versus old DI program rules. On the y-axis is parental DI benefits in 1999; we use 1999 since this is after all the re-examinations have taken place. Our age variable is recorded at the monthly level; each observation in the graph is the average DI payment for parents in six-month age bins. Three-month age bins for this graph, and all other RD graphs, can be found in the Appendix.

The figure reveals that DI benefit payments rise with age, largely reflecting the fact that older individuals have higher degree of disability ratings on average and therefore higher DI payments. More importantly, there is a sharp drop in payments for individuals just to the left of the cutoff. This is as expected, since parents less than age 45 were subject to the stricter DI program rules. DI payments drop by around 1,300 euros, which is a reduction of 13% compared to the average.

To document the extensive margin of the DI reform by itself, in the bottom panel we graph the fraction of parents who exit DI completely. The running variable and cutoff are the same as in the top panel. Each observation in the graph is the fraction of parents in a six-month bin who have exited DI by 1999. The first pattern to notice is that exits decrease with age. More relevant for our RD design, at the cutoff there is a sizable 5 percentage point increase in exits for parents exposed to the reform, which is roughly a 60% higher exit rate than otherwise would be predicted.

In Table 1 we present regression results corresponding to these figures. Our baseline specification, here and in what follows, regresses the relevant outcome on a dummy for the reform cutoff and separate linear trends in parental age to the left and the right of the cutoff. We use triangular weights so that observations nearer the cutoff will have more influence. Although the coefficients are not reported, we also include a variety of covariates for both the parent and the child which are measured as of January 1, 1996 and listed in the footnote to the table.[13]

The first specification in Table 1 looks at a parent's DI payments in the year 1999, after all re-examinations have taken place. Mirroring what was drawn in the top panel of Figure 2, the first stage RD estimate is a sizable 1,300 euro drop in benefits for parents exposed to the reform. This first stage point estimate is more than 13 times its standard error. Both the size and the precision of this estimate are important for identifying spillover effects on children, which by their nature are second order effects. The second specification uses exit from DI by 1999 as the outcome, and finds a large and precisely estimated 5.4 percentage point drop at the cutoff.

As a reminder, some individuals exposed to the reform were kicked off the program,

[13] January 1, 1996 is before the passage of the law exempting the 45-50 age cohort from the new, less generous DI rules and before the re-examinations have occurred for the 40-45 age cohort, so these controls should be exogenous to the cutoff.

while others remained on DI but with lower benefits. Given the reform had both an extensive and intensive margin, we focus primarily on the reduced form estimates when analyzing children's outcomes. But to provide a sense of scale, we also use the total drop in parental DI payments (including drops to zero) as a first stage variable to construct an IV estimate.

As a result of the reform, other parental outcomes changed as well. Borghans et al.'s (2014) analysis finds a strong rebound in labor earnings of 0.62 euros on average per euro of lost DI benefits and a 0.30 euro substitution to other social assistance programs in the short run. These effects diminish in magnitude over time, so that financial resources decline in the long run. We find similar patterns for our sample of parents. These other effects are important to keep in mind when interpreting the child spillovers we estimate in the paper.

5 Spillovers in Program Participation and Work

5.1 Child DI Participation

We begin our investigation of intergenerational spillovers by exploring the linkage in DI participation between parents and their children. Figure 3 presents RD graphs for the extensive and intensive margins of DI use. The x-axes in both graphs are the same as in Figure 2, with the running variable being the age of the parent as of the reform date and the cutoff age of 45 being marked with a vertical line. But now the y-axis plots the child's participation in DI, rather than the parent's. Each observation in the graph is an average for six-month age bins; three-month bins can be found in the Appendix.

An advantage of our long panel is that we can measure outcomes when the children are much older, after they have had a chance to live on their own, enter the labor market, and participate in the DI program. For our main child outcomes, we measure cumulative effects as of 2014, which is 21 years after the reform cutoff date. By this time, children are 37.4 years old on average, with the range of child ages spanning from 28 years old at the 10th percentile to 40 years old at the 90th percentile. Between 1999 and 2014, over 10% of children in our sample have participated in DI at some point, with an average number of 298 days spent on the program (including zeros).

The top graph in Figure 3 looks at whether a child has ever participated in DI between 1999 and 2014. There is a noticeable jump in child DI participation at the parental age cutoff of 45. Likewise, there is a noticeable jump in the cumulative number of days a child has been on DI. Table 2 presents the reduced form estimates corresponding to these graphs. For the extensive margin of participation, there is a statistically and economically significant 1.1 percentage point drop for children if their parent was exposed to the reform. This is an 11% effect relative to the mean. Likewise, children participate in DI for 47 fewer days if their parent was subject to the stricter DI rules, which represents a 16% drop relative to the mean.

The table also presents IV estimates to provide a sense of scaling. We use the total drop in parental DI payments, including drops to zero, as the first stage outcome variable (see panel A in Table 1).[14] Applying this scaling, a parental drop of 1,000 euros results in a 0.9 percentage point lower probability a child will be on DI and 38 fewer cumulative days on DI.

To arrive at the cost savings to taxpayers from the reduced DI use of children, in Figure 4 we plot an RD graph with the dollar amount of cumulative DI receipt as the outcome. There is a drop of approximately 1,600 euros in cumulative child DI benefits between 1999 and 2014. As reported in Table 3, this is a sizable effect relative to the mean of 10 thousand euros in DI receipt on average (including zeros). The IV estimate suggests that when a parent's DI benefits fall by a thousand euros in 1999, a child's cumulative DI income is roughly 1,300 euros lower.

5.2 Other Government Transfer Programs

We next look at other government transfer programs. This is important, because if children are simply shifting from one social assistance program to another, the cost savings to the government from children's reduced DI use will be overstated. Indeed, Borghans et al. (2014) document that while the reform lowered DI participation and benefits for those directly affected, a sizable portion of this loss was replaced by increased participation in other social assistance programs in the short run. Similar program substitution occurs for the directly affected parents in our sample as well.

With this motivation in mind, we pool together all of the miscellaneous benefit programs (besides DI) which are part of the social safety net in the Netherlands, and see if a child's receipt of these other benefits is affected by having a parent subject to the harsher DI rules. The bottom graph in Figure 4 reveals no noticeable change in other benefit receipt at the cutoff. Table 3 confirms that the point estimate is small and statistically insignificant. The table breaks things down further by separately reporting RD estimates for UI income, general assistance (traditional cash welfare), and the remaining miscellaneous benefit programs. For each type of benefit category, the estimates are small and insignificant.

These results stand in stark contrast to those of their parents, who themselves

[14]Note that the IV estimates will have the opposite sign compared to the reduced form, as the first stage estimate is negative.

had substantial substitution to these other programs in the short run (in particular to the UI program). This means that a parent's increased reliance on these other transfer programs, including any accrued knowledge and experience, did not transfer to their children. Any learning and spillover effects are apparently linked to the DI program itself. The conclusion is that the cost savings from the next generation due to lower DI use is not offset by increased participation in other programs.

5.3 Labor Market Earnings and Taxes Paid

We now turn to labor market earnings and taxes paid by children. The top panel of Figure 5 plots the cumulative earnings of children for the 15 year period from 1999 to 2014. Cumulative earnings includes wage income as well as income from self employment. In this graph, we plot the residuals from a regression of child earnings on child age. The reason to plot residuals is that children's cumulative earnings have a steep own-age profile and child age increases on average with their parent's age as of the reform date. This makes the range of the y-axis so wide with raw data that it is difficult to zoom in on the RD jump at the cutoff. We note that while child age is positively correlated with parent age, this should not be a problem, as child age appears to be smooth through the RD cutoff.[15]

The top figure shows a jump in cumulative child earnings at the parental age cutoff. Turning to Table 4, the RD estimate is an increase of a little over 7 thousand euros in earnings for children whose parents were subject to the reform. This is roughly a 2% increase in earnings relative to the overall mean. Stated somewhat differently, the IV scaling suggests that for each 1,000 euro drop in parental DI benefits due to the reform, children's cumulative earnings increase by around 5,700 euros.

While earnings changes are inherently interesting, what matters for the government's balance sheet is taxes minus transfers. We therefore calculate predicted taxes for children from 1999 to 2014.[16] The bottom graph of Figure 5 plots child cumulative child tax payments versus the running variable of parental age. As we did for earnings, we first regress out a child's age for this graph. Table 4 documents a large and statistically significant reduced form effect on taxes: estimated taxes paid rise by two thousand euros, which is a little under 2% of the mean. The IV estimate which

[15]Using child age as the outcome variable, and parent's age as the running variable, yields a small, and statistically insignificant jump of -.044 (s.e.=.066) at the cutoff.

[16]We calculate taxes using the relevant tax brackets for each year. We allow individuals to carry losses backward and forward, as specified by the Dutch tax code. The rules specify that losses are first used to offset positive income in the last three years, with further losses being carried forward for up to nine years. Since our income data begins in 1999, we are limited in applying carrybackward losses until 2002. As an alternative, we also tried using a variable which ignored the ability to offset losses. The results using this alternative tax measure are similar.

provides a scaling is also sizable, but loses statistical significance at the 10 percent confidence level.

5.4 Cumulative Fiscal Effects

To provide a comprehensive picture of the fiscal spillover effects, we now estimate the cumulative change in taxes minus transfers up through 2014. Policy makers should ultimately be concerned with this net effect, since this is what matters for the government's budget. To do this, we create a variable which combines DI and all other government transfer program payments and subtracts this from taxes paid by a child. As shown in Table 4, we find that taxes minus transfers increase by 3,483 euros (s.e.=1,271) for children of parents who were subject to the stricter DI rules. The scaling provided by our IV estimate implies that for each 1,000 euro drop in parental DI benefits around the time of the reform, the government's budget improved by almost 2,800 euro per child by 2014.

To provide further insight into the fiscal effects over time, Figure 6 plots year-by-year RD estimates for cumulative DI benefits, cumulative other transfers, and cumulative tax payments over time. There is a small, but statistically significant savings in DI payments in the first five years, and this effect grows larger over time. In contrast, other cumulative transfers are close to zero and insignificant for the entire period. Cumulative tax payments, plotted in the upper left graph, start out small and rise little in the first 5 years. This makes sense, as many of the children are still in school and have not yet begun working full time in the early years of our data. But the increase in estimated tax payments rises with time, so that by 2006 the effect becomes statistically significant.

The lower right panel in Figure 6 plots the net effect of taxes minus transfers over time. It mirrors the reduction in DI payments and the rise in tax payments over time, as expected. It is interesting to note that by 2014, increased taxes account for a slightly larger fraction of the net savings to the government's budget compared to the reduction in DI payments. This highlights the limitation of looking at DI in isolation, without considering other possible fiscal spillovers.

5.5 Budget Savings from Children versus Parents

To gauge the importance of child spillovers, we compare the budget savings of the reform, including all transfers and taxes, due to children versus their parents. Borghans et al. (2014) estimate direct effects on parents from 1999 to 2005. We extend their analysis to calculate a measure of the cumulative fiscal costs for parents until mandatory retirement at age 65, which occurs in 2013 for parents at the reform cutoff.

Mandatory retirement complicates this calculation, as once parents within the estimation window start reaching age 65, we can no longer use an RD design. This is because parents over age 65 are no longer eligible for DI benefits and instead automatically begin to collect their government provided pension (which is a fixed amount and does not depend on work history).

To deal with this, we estimate the cumulative fiscal effects using an RD design for each year from 1999 to 2008, before any parents in our estimation window reach age 65. It turns out the increase in cumulative net taxes minus all transfers is remarkably linear in years; a regression of the estimated RD coefficients on a year trend has a slope coefficient of 1,167.7 euros (s.e.=21.4) and an R-squared of 0.997. We then extrapolate this linear trend for the years 2009 to 2013. Assuming a discount rate of 3% per year, we calculate a PDV budgetary savings of 12,999 euros per parent exposed to the reform up through 2013. Using the RD estimates for children from Figure 6, we calculate a PDV budgetary savings of 3,485 euros from children per exposed parent (taking into account that some parents have more than one child).[17]

These calculations imply the child spillover effects account for 21% of the fiscal benefits of the reform by 2013. This is likely an underestimate going forward in time, however. This is because while the parents are no longer eligible to work or participate in DI, their children have an average of 30 years of DI eligibility and work life remaining. Extrapolating the estimated child spillovers beyond 2013, we calculate that 40% of the present discounted value of the savings in the long run is due to children.[18]

Projections about future DI use and taxes paid by both parents and children should be viewed as suggestive, in part because the economic and policy environment is likely to change over time.[19] But the basic point remains: fiscal spillovers from the next generation are nontrivial, and ignoring their effects greatly understates the cost savings of the reform in the long run.

[17]All figures are indexed to be in 2014 euros.

[18]We use a linear extrapolation based on the RD estimates for taxes minus transfers for 2005-2014. We exclude 1999-2004, since the lower right panel of Figure 6 reveals a different trend when children are finishing school and beginning their work life. A regression of the estimated RD coefficients on a year trend has a slope coefficient of 255.6 euros (s.e.=7.8) and an R-squared of 0.992.

[19]These rough estimates also do not include the public costs associated with the increased education we document in the next section.

6 Spillovers in Education and Possible Mechanisms

6.1 Educational Investments

So far, we have examined how parents influence their children's participation in DI, other government benefit programs, and earnings from work. These child outcomes mostly occur in the future, after a child has grown up and entered the labor market. Is it possible that children anticipate this lower reliance on DI and increased labor market attachment in the future, and make different investment choices while they are still young?

One way to get at this question is to see if children increase their educational investments in response to having a parent exposed to the harsher DI rules. We collected data on children's educational attainment as of 2014.[20] In Figure 7, the top graph plots child years of education against the running variable of the parent's age as of the reform date. While most children will be done with their formal education by 2014, not all are. Indeed, one can see in the figure that education trends slightly upward in the graph as a function of parental age, which is correlated with child age. Table 5 reports the corresponding RF estimate and standard error for years of education. There is a significant jump at the reform cutoff, with children of reform-exposed parents getting 0.12 years more education, relative to a mean of 11.5 years. The IV estimate suggests that a one thousand euro loss in parental benefits results in an increase of roughly one tenth of a year of education.

The bottom panel in Figure 7 plots the RD graph with upper secondary school completion (roughly the equivalent of High School) as the outcome variable. There is a significant jump of 2.2 percentage points at the reform cutoff, as documented in Table 5. This is a modestly sized, but economically significant, effect relative to the overall mean of 78 percent.

Table 5 further reports RD estimates for other levels of schooling.[21] We find no effect of a parent's exposure to the DI reform on their children's completion of lower secondary school. This is as expected, since most children are too old to be affected, and most children complete this minimal level of schooling anyway due to compulsory schooling laws. In contrast, children of reform-exposed parents are not only more likely to complete upper secondary school, but they are also more likely to obtain higher education. This could be in part because admittance to college requires

[20] The sample size is somewhat smaller than for the analyses in Section 5, because for earlier cohorts, education is only available for a subsample of observations.

[21] As background, from the ages of 4 or 5 to 12 or 13, children attend elementary school. Further education in secondary school is split into three tracks, and takes an additional 4 to 6 years depending on whether the student enrolls in a vocational or college preparatory program.

completion of upper secondary school.

These results are intriguing, because they provide some of the first well-identified and precisely estimated evidence documenting anticipatory investments by children as a result of parental program participation. These higher levels of educational investment have the potential to increase future earnings, lower unemployment spells, and hence increase government tax revenue.

6.2 Other Outcomes: Crime and Marriage

We briefly explore spillovers for two other sets of outcomes. We start by looking at whether children's crime is affected, since the opportunity cost of committing crime should rise as children work and earn more in the formal labor market. As Appendix Table A2 documents, we find a reduction in the chances a child is incarcerated if their parent was exposed to the reform. There is a statistically significant 0.3 percentage point drop in incarceration relative to an overall mean of 1.8%, or a 16% reduction. However, we find no significant evidence for a decline in arrests.

Marriage could also be affected by a parent's DI use, as children with higher earnings and extra education should be more attractive marriage partners. We find some evidence that having a parent whose DI benefits are reduced increases the probability a child will get married. There is a 1.1 percentage point increase in marriage, relative to a base of 46%, for children of reform-exposed parents. In contrast, cohabitation which includes a child in the relationship goes the other direction, although it is not statistically significant. Insofar as marriage represents a more stable type of union compared to cohabitation, these are potentially positive spillovers.

6.3 Possible Mechanisms

Before continuing, we consider several explanations for our findings. We start by ruling out two possibilities which have been hypothesized as reasons for intergenerational program participation. It cannot be information about how to apply for the program, as all parents have been through the DI screening process. Similarly, reductions in stigma from observing a parent participate seems improbable, as parents have already been on DI for a long time prior to the reform (almost 7.5 years on average). While such learning and stigma channels may be important in other contexts, they play at most a minor role here.

Understanding how the new DI policy affected parents is key for interpreting the intergenerational effects. As a result of the reform, parental leisure decreased and work hours increased on average, with total parental income changing little in the short run but declining in the long run. In theory, less parental supervision due to increased work hours or lower income in the long run could result in reduced investments in children, harming their attachment to the labor force as adults. Based on our estimates, this is not the case, with the findings all pointing to a greater focus by children on future employment.

Instead, the two explanations most consistent with our findings are that children experience a scarring effect or learn about formal employment. The scarring mechanism we have in mind is that when a child observes a parent being forced off of DI or having their DI payment cut, the child infers they cannot rely on the government to take care of them in the future. This is similar to the scarring effect talked about in Malmendier and Nagel (2011) in the context of macroeconomic shocks and future risk taking. The learning mechanism is that affected parents transmit information about the labor market or provide a positive role model due to their increased employment. While we cannot test these two explanations directly, both can explain why children of treated parents increase their educational investments when young, earn more in the labor market as adults, and decrease their future DI use.[22]

7 Heterogeneity, Robustness, Placebo Tests, and OLS

7.1 Effects by Child Age

To better understand the intergenerational spillovers just documented, in this section we break up the estimated effects by child age as of November 1996. The reason to focus on child age as of this date is that it is when the Dutch Parliament decided the 45-50 year old cohort would be grandfathered in under the old DI rules. It is also the approximate time when the re-examinations for the 40-45 year old cohort began, and hence when children began to be differentially affected by the reform. We split children into two roughly equally-sized groups: those who are 18 and younger versus 19 and older as of November 1996.[23] While it would be interesting to also look at even younger age splits, the sample of parents around the reform cutoff are old enough that they do not have many young children.

Table 6 reports separate RD estimates for our main outcomes split by child age. Looking at the DI spillovers in specifications A through C, the effects are all large and statistically significant for the younger group. The estimated effects for the older group, while going in the same direction, are smaller.

[22]Interestingly, we find no evidence that children participate more in other safety net programs, like unemployment insurance, even though their parent's use rises modestly. One explanation is that the increased focus on future employment dominates any effects from these other programs.

[23]As a reminder, we limit our sample to children still living at home at the time of the reform announcement, including children living at home while attending college.

For other social programs besides DI, we find no effect for either age group. But when we turn to earnings, we find relatively large and marginally significant effects for the older group. These increased earnings also translate into higher taxes paid, although the estimate is not statistically significant at conventional levels. For the younger group, the effects are the same sign, but smaller. This apparent puzzle, given the opposite pattern found for DI participation by child age, has a simple explanation which we return to after discussing the education outcomes.

Specifications G and H estimate the spillover effects on child education. The IV estimate for the younger group implies an increase of 0.16 years of schooling for each thousand dollar reduction in parental DI benefits. In contrast, there is no statistically significant spillover in years of education for those age 19 and older. Looking at upper secondary school completion, we again find larger effects for the younger age group. Upper secondary school is usually completed by age 18 or 19, so for the older group, there is less time to affect this schooling margin.[24]

How do all of these results fit together, particularly the stronger DI effect for the younger group and the larger earnings effect for the older group? First, it is important to recognize that because of their age, the older group has had over three more prime-age years to work in the labor market; indeed, mean cumulative earnings for the older group are 50% higher. On top of this, the younger group gets more education, which delays the start of their prime earnings years. Accounting for this education-induced absence from the workforce can more than explain the difference in the earnings effects found for the two age groups.[25] Education-induced absences from the workforce can also help explain the stronger DI result for younger children, as individuals cannot be enrolled full time in school and concurrently on DI.

In summary, the pattern of results in Table 6 indicates that younger children are more strongly affected by their parents. A natural set of explanations is that younger children are more impressionable, have a longer period to observe their parent's DI experiences, and have more time to alter their educational plans.

[24] A small number of children complete their education at older ages if they are either on a 6 year educational track or have previously repeated a grade.

[25] To make a comparison, we concentrate on the IV estimates to account for the differences in parental first stages. Treated children in the younger group receive an extra 0.162 years of education for each thousand dollar reduction in parental DI, compared to 0.045 for the older group. A reasonable estimate of earnings in prime age years can be taken from the difference in average cumulative earnings for the two groups (448,788-290,500 euros) divided by the average age difference between the groups (3.41 years). Assuming individuals do not work while in school, this implies a loss of 5,431 in earnings for treated children. Adding this to the IV estimate of -4,274 for the younger group (specification E) equals -9,705, which is over 70% larger in absolute value compared to the IV estimate of -5,640 for the older group.

7.2 *Robustness*

Appendix Table A3 reports a variety of specification checks for our main outcomes. For simplicity, the table only reports the reduced form estimates. The first row repeats our baseline estimates for ease of comparison. In Specification B, we allow separate quadratic trends on each side of the cutoff; the estimates are larger, but the standard errors also increase. The next two specifications remove the triangular weights and the control variables from the regression, and yield similar findings to the baseline.

In specifications E and F we narrow the RD estimation window. As we shrink the window down to 45 or 30 months on each side of the cutoff, the estimates become somewhat larger, but the standard errors increase as well. All estimates remain statistically significant (except for cumulative other transfers, which is never significant). As an alternative set of specifications, we estimate local linear regressions. Depending on the bandwidth, some of the estimates become insignificant, but the point estimates are broadly similar to the baseline.

In specification J, we run a regression where the sample only includes children not living at home at the time of the reform. This can occur if the child has moved out or because a child does not live with their biological parent (for example, if the parent is divorced). With this sample, we find no significant effect for most of the outcomes. In specification K we cluster the standard errors by parental age and find it makes little difference. The final two specifications exclude non-native Dutch and children whose parents left DI by 1995. The RD estimates remain similar to the baseline.

7.3 *Placebo Tests*

To further explore the validity of our estimates, we conduct placebo tests for our main outcomes. To do this, we collected a completely different sample of children: those whose parents were *not* on DI as of 1995. Since these parents are all subject to the new DI examination rules (regardless of their age), they should not be treated differentially. As a result, there should be no discontinuity at the 45 year old age cutoff. Indeed, we find no evidence of a first stage for this sample.

Table 7 replicates our baseline reduced form specifications for child outcomes, with the only exception being the different, and much larger, sample. There is no evidence of an effect for any of these outcomes, with the point estimates being uniformly small and statistically insignificant. This provides reassurance our results are being driven by the change in DI strictness, and not other policies which differentially affected parents at a similar age cutoff.

7.4 Comparison to OLS

As a final exercise, we compare our quasi-experimental estimates to OLS. To construct our OLS sample, we take all parents between the ages of 40 to 50 at the time of the reform who have at least one child still living at home with them. This sample includes parents who were on DI as of the reform date, but additionally includes parents who were not on DI as of the reform date. To make sure differential re-examinations for those under versus over the age 45 cutoff do not drive our OLS estimates, we split this sample into two groups: parents between the ages of 40 and 45, and parents between the ages of 45 and 50. We estimate separate OLS regressions for children whose parents are in the younger versus older parental age groups.

In Table 8, we estimate the effect of parental DI benefit amounts in 1996 on each of our main child outcomes. The OLS estimates are most directly comparable to the IV estimates shown previously, as both are measured on the same scale. The OLS estimates for the younger versus older parent samples are generally quite similar, but diverge sharply from the IV estimates.

The first row in Table 8 uses whether the child was ever on DI by 2014 as the outcome variable. The OLS estimate implies an extra 1,000 euros in parental DI payments increases a child's probability of participating in DI by 0.3 percentage points for both the older and younger parent samples. This compares to the IV estimate of 0.9 percentage points in Table 2. Looking at days on DI, income from DI, earnings, taxes, and education, the IV estimate is similarly between 2 and 4 times larger compared to OLS. Interestingly, the OLS estimate for cumulative total benefits from other social assistance programs is large and significant. In contrast, the IV estimate is close to zero.

Why are the IV estimates substantially larger in general? There are several possible explanations. First, the reasons for differential DI participation and payment amounts are likely not the same in the two samples. For example, in the OLS sample, if a child observes a parent is "gaming" the system, they may be inclined to do the same, whereas if they see their parent is truly disabled, it may have little effect on them unless they experience the same health condition. In contrast, the IV estimate compares parents whose job prospects and health conditions are presumably similar, but whose DI payments change due to differential exposure to the new, stricter DI rules. Being forced off of DI or having one's payments reduced could represent a large shock to a parent expecting to remain on DI for the long term, and children's views about the ability to rely on government support could change markedly in response.

A second reason is that IV estimates a local average treatment effect (LATE) for compliers, and the intergenerational spillovers could be different for the complier sample compared to the general population. To better understand who the compliers are in the RD regressions, Appendix Table A4 calculates the average characteristics of compliers.[26] The table then compares these averages to the characteristics of all children in the OLS sample whose parents are between 44.5 and 45.5 years old as of the reform date. The biggest difference is the degree of parental disability. Fifty-seven percent of individuals in the OLS sample are fully disabled, while only 48% of compliers are fully disabled. Compliers have also been on DI for a longer time period, with durations which are 10 months longer on average.

On a related point, it is important to recognize the Dutch reform affected marginal DI participants who had more work capacity on average. The intergenerational spillover effects for these marginal cases could be quite different compared to cases where a parent has little or no work capacity. Fortunately, the marginal participants we study are the most policy relevant, as they are the ones who presumably would be targeted by most reforms.

8 Conclusion

Whether a parent's participation in a social assistance program influences their child's use of public assistance, employment, and human capital investments is a difficult question to answer due to the nonrandom nature of program participation and the likelihood that unobserved factors driving participation are correlated across generations. Yet the impact of parental DI participation on children's later life outcomes could matter for the financial stability of a variety of social insurance and safety net programs.

To obtain causal estimates of intergenerational spillovers, this paper takes advantage of a DI reform in the Netherlands combined with high quality register data. Our results indicate that children respond strongly when a parent exits DI or has their benefits reduced. Children whose parents were exposed to the reform are less likely to participate in DI themselves as adults, do not increase their participation in other public assistance programs, increase their earnings and taxes paid, and invest significantly more in their education. Two explanations consistent with our findings are that children experience a scarring effect, inferring that they cannot rely on government support, and that children learn about the labor market from their parent's increased employment.

From a policy perspective, our study serves as an important lesson for the eval-

[26] For details on how to calculate the complier averages, see Borghans et al. (2014).

uation of costs and benefits of social assistance programs. Considering current participants only, without accounting for the long-run effects within families, would be a mistake. We find that ignoring intergenerational spillovers underestimates the cost savings of the Dutch reform by between 21 and 40 percent in the long run.

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Figure 1: DI Stocks and Inflows as a Percentage of Insured Workers, 1968-2016.

Notes: Data come from the Dutch Employee Insurance Agency (Uitvoeringsinstituut Werknemersverzekeringen), as used in Koning and Lindeboom (2015). Estimates of the number of insured workers are used to calculate receipt and award percentages for 2014 to 2016.

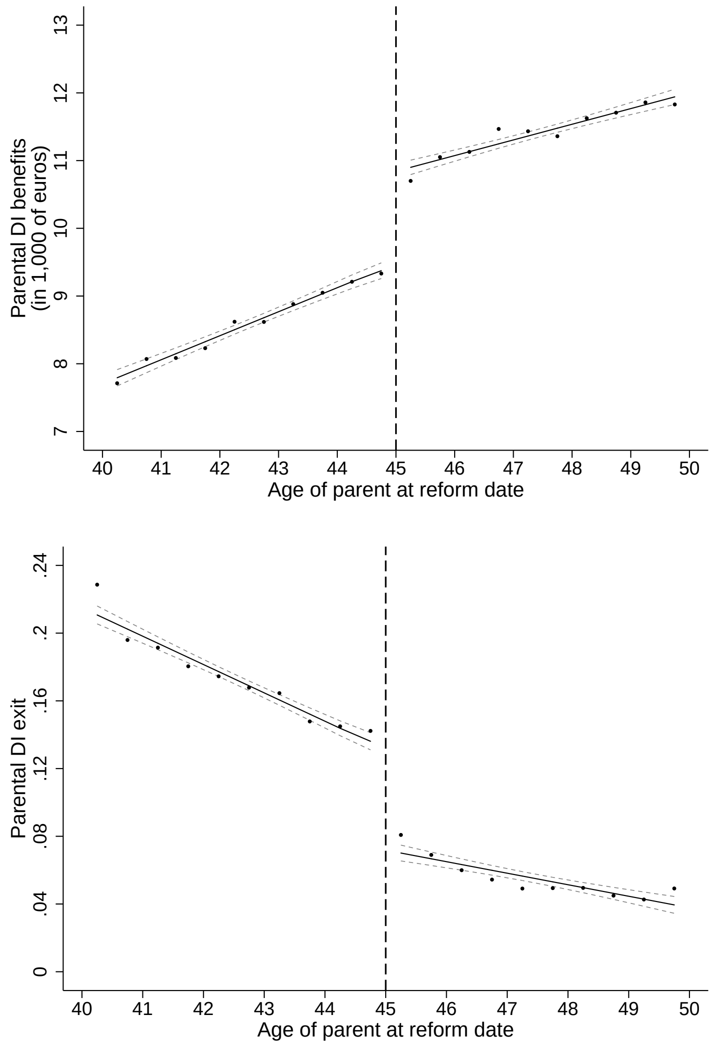


Figure 2: Effects of the Reform on Parents

Notes: Each observation represents average parental DI receipt in 1999 (top panel) or average parental DI exit by 1999 (bottom panel) in 6 months age bins, based on the parent's age as of the reform date of August 1993. The dashed vertical lines denote the reform cutoff of age 45. The solid trend lines are based on regressions using unbinned data, with dotted lines indicating pointwise 90 percent confidence intervals.

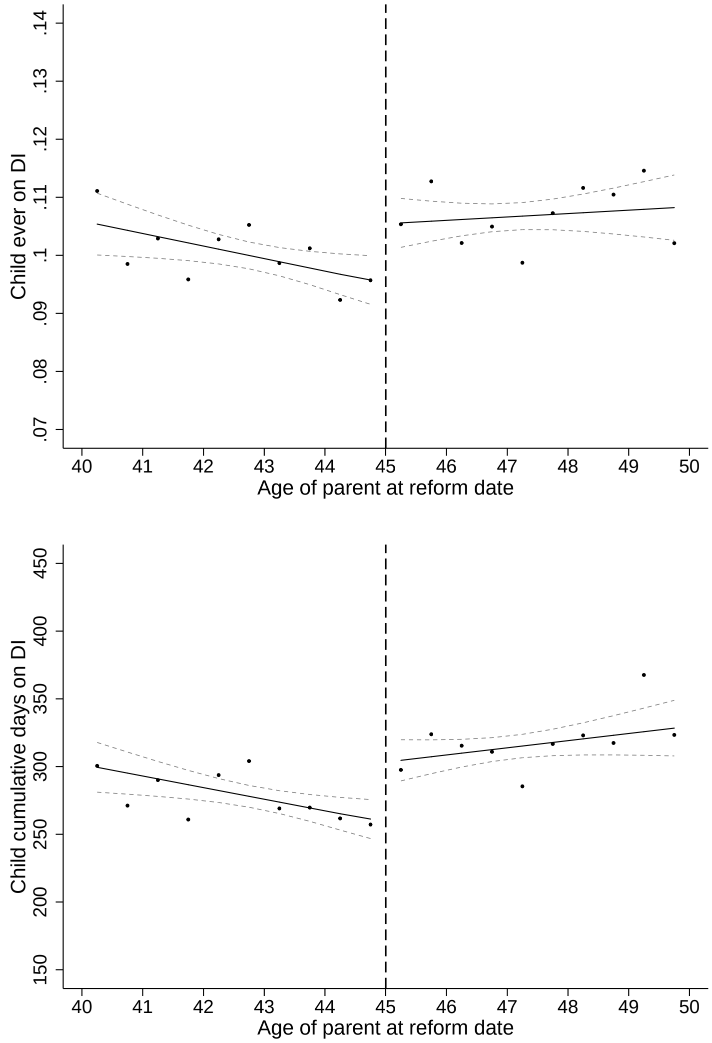


Figure 3: Child DI Participation

Notes: Each observation represents average child DI participation by 2014 (top panel) or average cumulative child days on DI by 2014 (bottom panel) in 6 months age bins, based on the parent's age as of the reform date of August 1993. The dashed vertical lines denote the reform cutoff of age 45. The solid trend lines are based on regressions using unbinned data, with dotted lines indicating pointwise 90 percent confidence intervals.

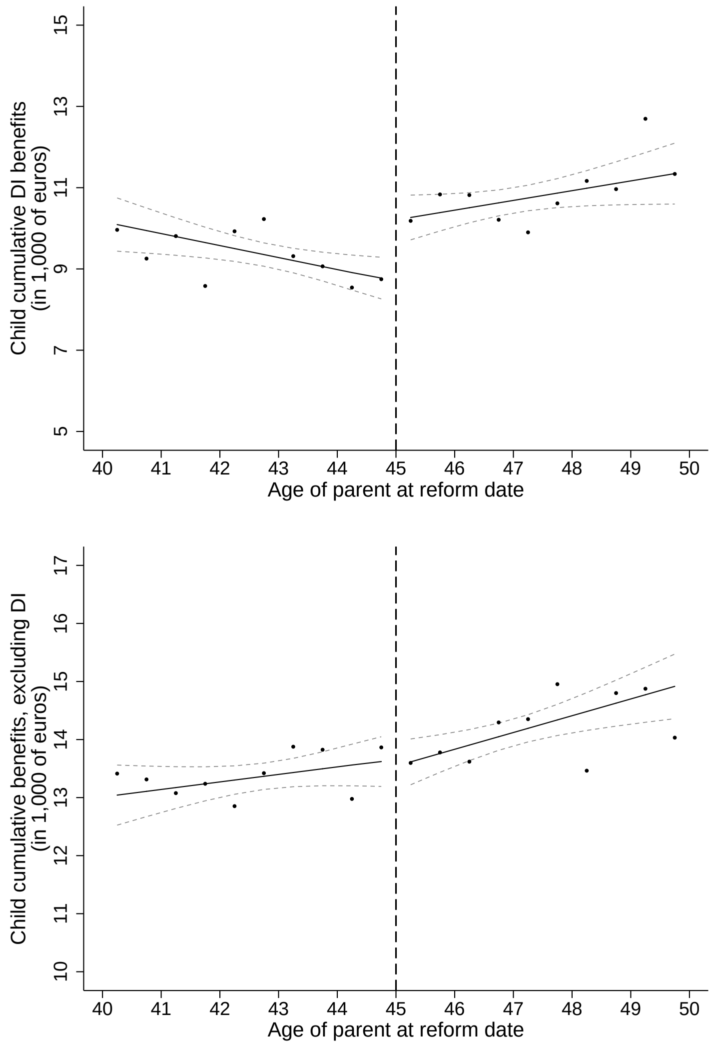


Figure 4: Child DI and Other Benefit Receipt

Notes: See Table 3 and notes to Figure 3.

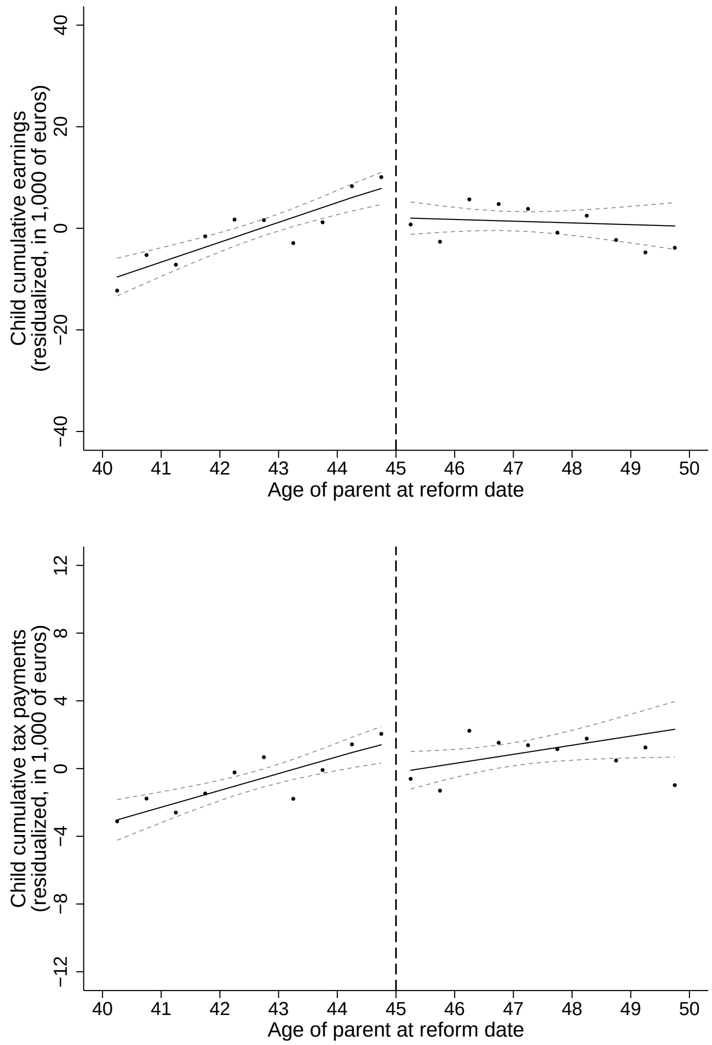


Figure 5: Residualized Child Earnings and Taxes

Notes: See Table 4 and notes to Figure 3. In these graphs, we first regress out child age to keep the range of the y-axis from being too large.

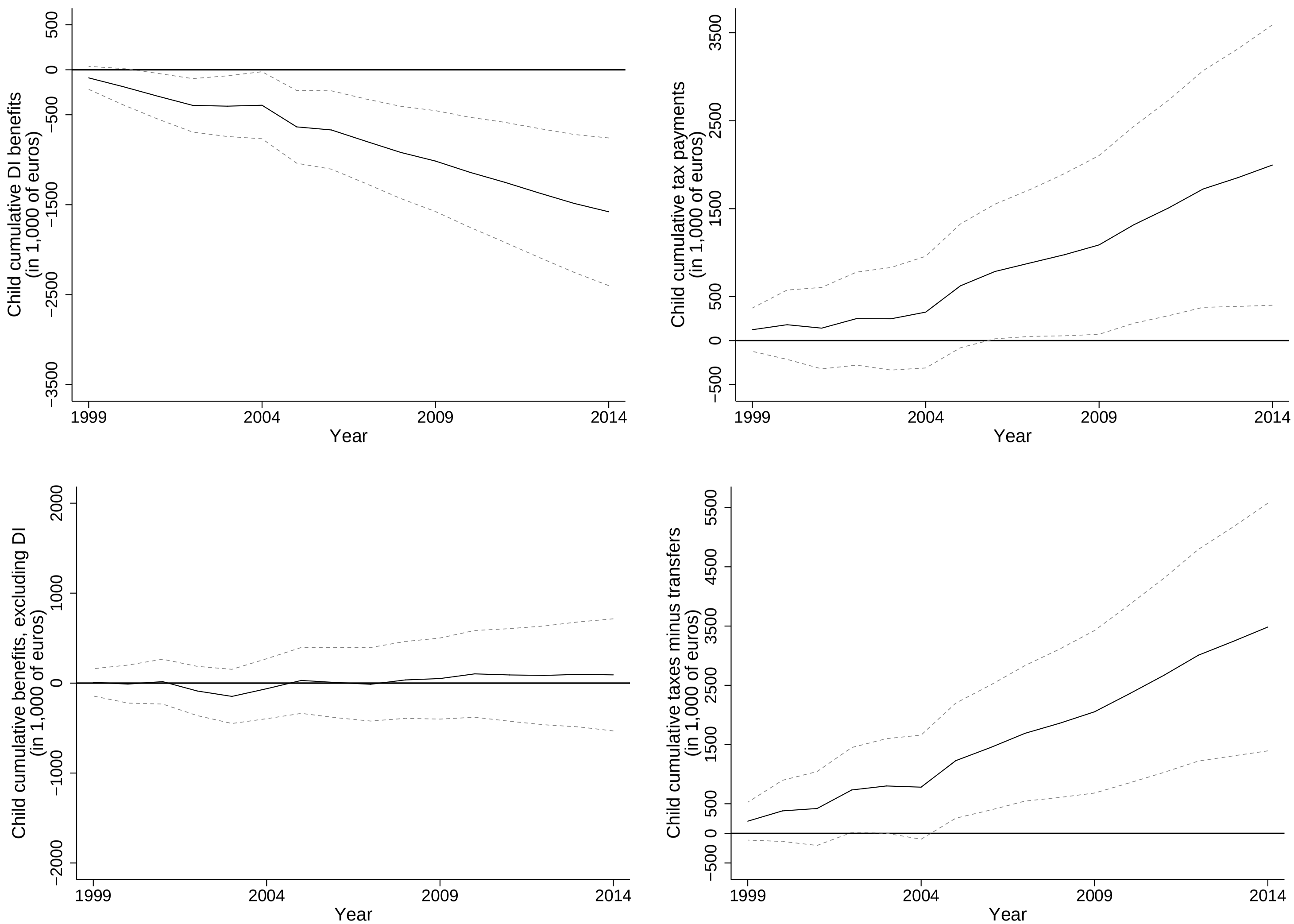


Figure 6: Cumulative Fiscal Effects Over Time for Children

Notes: Each graph plots year-by-year RD estimates of cumulative effects, using the specifications of Tables 2-4. Dotted lines indicate pointwise 90 percent confidence intervals.

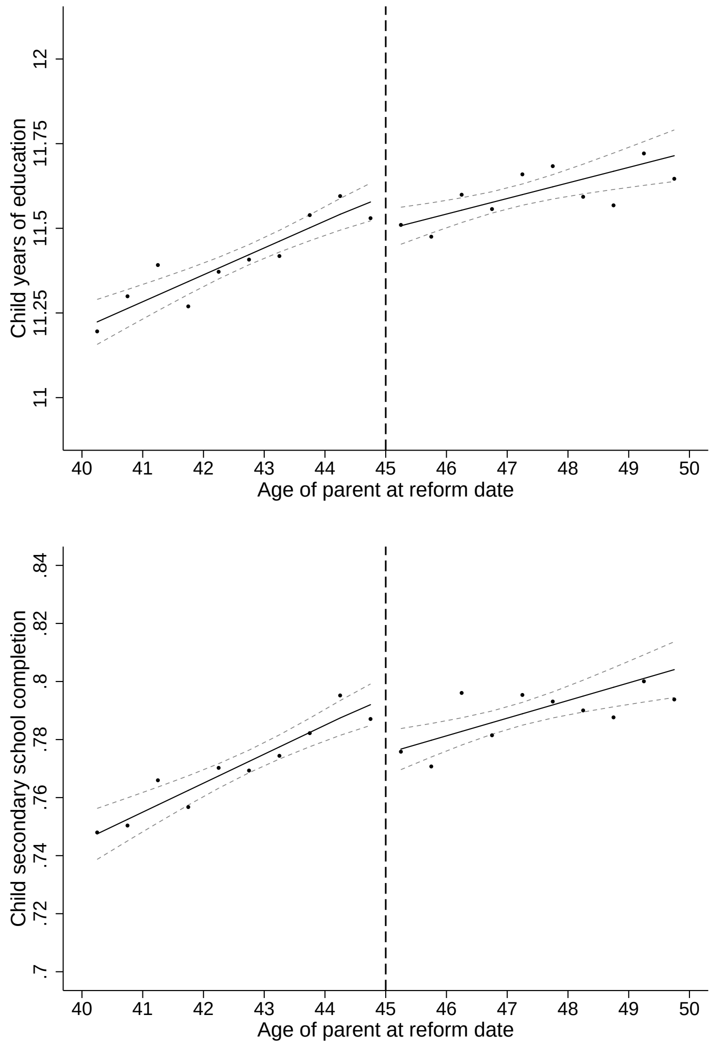


Figure 7: Child Educational Attainment

Notes: See Table 5 and notes to Figure 3.

Table 1: First Stage RD Estimates of the Reform on Parental DI

Dependent variable	Mean	First Stage
A. Parental DI benefits (in 1,000 euros)	10.063	-1.300**
		(.095)
B. Parental exit from DI	0.114	0.054**
		(0.005)
Observations		116,356

Notes: The sample is parents between the ages of 40-50 and on DI as of the reform date of August 1, 1993, who were still on DI in 1995, and had children living at home around the time of the reform. Parental DI benefits measure payments received in 1999, indexed to the year 2014. Parental exit measures whether the parent has exited DI by 1999. All coefficients are estimated using an RD model with separate linear trends on each side of the cutoff and triangular weights. Parent control variables are measured as of January 1, 1996 and include age, birth month dummies, a gender dummy, a cubic in pre-disability earnings, a dummy for no pre-disability 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; child control variables include age and a gender dummy. Parents appear more than once if they have more than one child. Standard errors in parentheses, clustered at the parent level.
$^{**}p<0.05$, $^{*}p<0.10$

Table 2: RD Estimates of Child DI Participation

Child outcome in 2014	Mean	RF	IV
A. Ever on DI	.104	-.011**	.009**
		(.004)	(.004)
B. Cumulative days on DI	298	-47.2**	37.5**
		(13.9)	(14.6)
Observations	116,356		

Notes: See notes to Table 1. The independent variables measure whether a child ever participated in DI between 1996 and 2014 and the cumulative number of days on DI between 1996 and 2014. Standard errors in parentheses, clustered at the parent level.
$^{**}p<0.05$, $^{*}p<0.10$

Table 3: RD Estimates of Child Benefits from DI and Other Government Programs

Child outcome in 2014 (in 1,000 euros)	Mean	RF	IV
A. DI benefits			
A1. Cumulative DI income	10.107	-1.578**	1.256**
		(.499)	(.522)
B. Other benefits			
B1. Cumulative UI income	5.639	-.067	.053
		(.162)	(.167)
B2. Cumulative general assistance income (traditional cash welfare)	4.432	.092	-.073
		(.266)	(.272)
B3. Cumulative misc. benefit income (all other government safety net programs)	3.675	.067	-.053
		(.145)	(.149)
B4. Cumulative total benefits, excluding DI (B1+B2+B3)	13.746	.092	-.073
		(.379)	(.388)
Observations	116,356		

Notes: See notes to Table 1. Independent variables measure cumulative amounts between 1996 and 2014, indexed to the year 2014. Standard errors in parentheses, clustered at the parent level.
$^{**}p<0.05$, $^{*}p<0.10$

Table 4: RD Estimates of Child Earnings and Taxes

Child outcome in 2014 (in 1,000 euros)	Mean	RF	IV
A. Cumulative income from work	371.282	7.178**	-5.711*
		(2.836)	(2.951)
B. Cumulative estimated taxes	109.565	1.997**	-1.589
		(.969)	(1.008)
C. Cumulative taxes minus transfers (taxes - DI benefits - other benefits)	85.712	3.483**	-2.772**
		(1.271)	(1.325)
Observations	116,356		

Notes: See Table 3 and notes to Table 1. Independent variables measure cumulative amounts between 1996 and 2014, indexed to the year 2014. Standard errors in parentheses, clustered at the parent level.
$^{**}p<0.05$, $^{*}p<0.10$

Table 5: RD Estimates of Child Educational Investments

Child outcome in 2014	Mean	RF	IV
A. Years of education	11.49	.117**	-.096**
		(.050)	(.050)
B. Lower secondary school or more	.95	-.001	.001
		(.003)	(.003)
C. Upper secondary school or more	.78	.022**	-.018**
		(.007)	(.007)
D. Bachelor degree or more	.33	.017**	.-014*
		(.008)	(.008)
E. Master degree or more	.10	.009*	-.008
		(.005)	(.005)
F. Advanced degree or more	.01	-.001	.001
		(.001)	(.001)
Observations	79,924		

Notes: See notes to Table 1. Education is measured as of 2014. Upper secondary school or more includes both academic and vocational tracks. The sample size in this table is smaller, as education data is a census for younger cohorts, but a sample for older cohorts. Standard errors in parentheses, clustered at the parent level.
$**p<0.05$, $*p<0.10$

Table 6: RD Estimates by Age of Child

	Child age: 18 and younger			Child age: 19 and older		
Child outcome in 2014	Mean	RF	IV	Mean	RF	IV
A. Ever on DI	.093	-.019**	.017**	.114	-.006	.004
		(.006)	(.006)		(.006)	(.005)
B. Cumulative days on DI	265	-64.9**	58.5**	330	-36.4*	25.7
		(19.3)	(19.3)		(20.0)	(16.4)
C. Cumulative DI income (in 1,000 euro)	8.677	-2.184**	1.968**	11.480	-1.293*	.912
		(.650)	(.748)		(.747)	(.613)
D. Cum. total benefits, excl. DI (in 1,000 euro)	11.553	-.318	.287	15.851	.332	-.234
		(.467)	(.515)		(.584)	(.472)
E. Cumulative income from work (in 1,000 euro)	290.500	4.744	-4.274	448.788	7.998*	-5.640
		(3.080)	(3.080)		(4.417)	(3.616)
F. Cumulative estimated taxes (in 1,000 euro)	80.228	0.944	0.851	137.714	2.462	-1.736
		(.973)	(1.072)		(1.577)	(1.295)
G. Years of education	11.57	.171**	-.162**	11.39	.065	-.045
		(.067)	(.080)		(.069)	(.052)
H. Upper secondary school or more	.775	.028**	-.026**	.783	.015	-.011
		(.009)	(.011)		(.010)	(.007)
I. First Stage: Parental DI benefits (in 1,000 euro, for A-F)		-1.110**			-1.418**	
		(.132)			(.110)	
J. First Stage: Parental DI benefits (in 1,000 euro, for G, H)		-1.052**			-1.452**	
		(.144)			(.139)	
Observations (A-F)	56,974			59,382		
Observations (G, H)	45,913			34,011		

Notes: See notes to Tables 1-5. Child age is measured as of November 1996, which is when the Dutch Parliament passed the motion to grandfather in the 45-50 year olds under the old DI rules.
***p<0.05, *p<0.10*

Table 7: Placebo Tests – RD Estimates for Parents Not on DI in 1995

Child outcome in 2014	Mean	RF
A. Ever on DI	.057	-.0002
		(.0009)
B. Cumulative days on DI	158	-2.126
		(3.232)
C. Cumulative DI income	5.346	-.150
(in 1,000 euro)		(.117)
D. Cumulative total benefits, excluding DI	8.761	.042
(in 1,000 euro)		(.092)
E. Cumulative income from work	378.393	-.041
(in 1,000 euro)		(.940)
F. Cumulative estimated taxes	110.906	-.178
(in 1,000 euro)		(.360)
G. Years of education	12.56	.010
		(.015)
H. Upper secondary school or more	0.87	.0000
		(.0016)
Observations (A-F)	1,286,355	
Observations (G, H)	971,599	

Notes: The placebo sample is comprised of children whose parents were ***not*** *on DI as of 1995. Since these parents are all subject to the new DI rules (regardless of their age), there should be no discontinuity at the cutoff in any of the child outcomes. See notes to Tables 1-5 for details on the RD estimator, the included control variables, and the child outcome variables. Standard errors in parentheses, clustered at the parent level.*
***p<0.05, *p<0.10*

Table 8: OLS Estimates

	Indep. var.: Parental DI payments in 1996 (in 1,000 euros)			
	Parent age: 40-45		Parent age: 45-50	
Child outcome in 2014	Mean	OLS	Mean	OLS
A. Ever on DI	.060	.003**	.070	.003**
		(.000)		(.000)
B. Cumulative days on DI	164	8.6**	203	8.4**
		(.3)		(.3)
C. Cumulative DI income (in 1,000 euro)	5.495	.293**	7.039	.289**
		(.009)		(.010)
D. Cumulative total benefits, excl. DI (in 1,000 euro)	9.160	.230**	10.707	.235**
		(.007)		(.008)
E. Cumulative income from work (in 1,000 euro)	347.254	-2.383**	442.195	-2.964**
		(.062)		(.080)
F. Cumulative estimated taxes (in 1,000 euro)	99.245	-.668**	134.848	-.933**
		(.023)		(.032)
G. Years of education	12.39	-.052**	12.47	-.050**
		(.001)		(.001)
H. Upper secondary school or more	.85	-.005**	.87	-.005**
		(.000)		(.000)
Observations (A-F)	498,378		421,731	
Observations (G, H)	387,264		287,799	

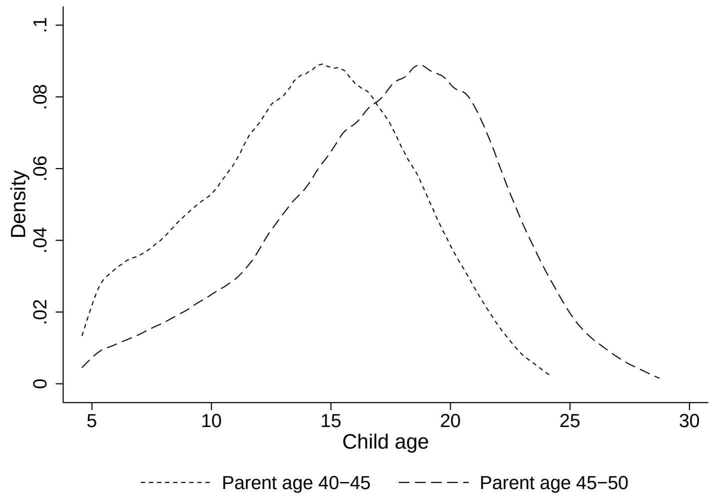
Notes: Sample includes children still living at home for all parents between the ages of 40-50, regardless of whether the parent was on DI as of the reform date. The sample is split into two parental age groups (40-45 and 45-50 as of the reform date) to ensure that the stricter DI rules for those parents under versus over the age 45 cutoff do not drive the OLS estimates. Parent control variables are measured as of January 1, 1996 and include age, birth month dummies, a gender dummy, a dummy for Native Dutch, a marriage dummy, and number of children in the household; child control variables include age and a gender dummy. Standard errors in parentheses, clustered at the parent level.

$^{**}p<0.05$, $^{*}p<0.10$

For Online Publication: Appendix Figures and Tables

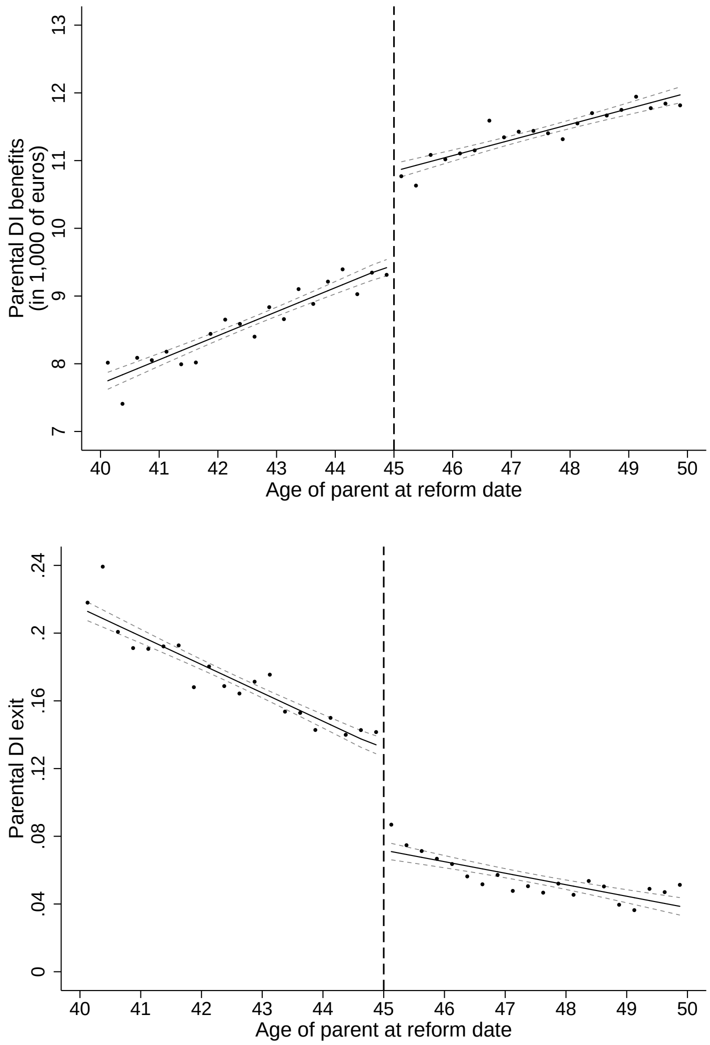
"Intergenerational Spillovers in Disability Insurance"

Gordon B. Dahl and Anne C. Gielen



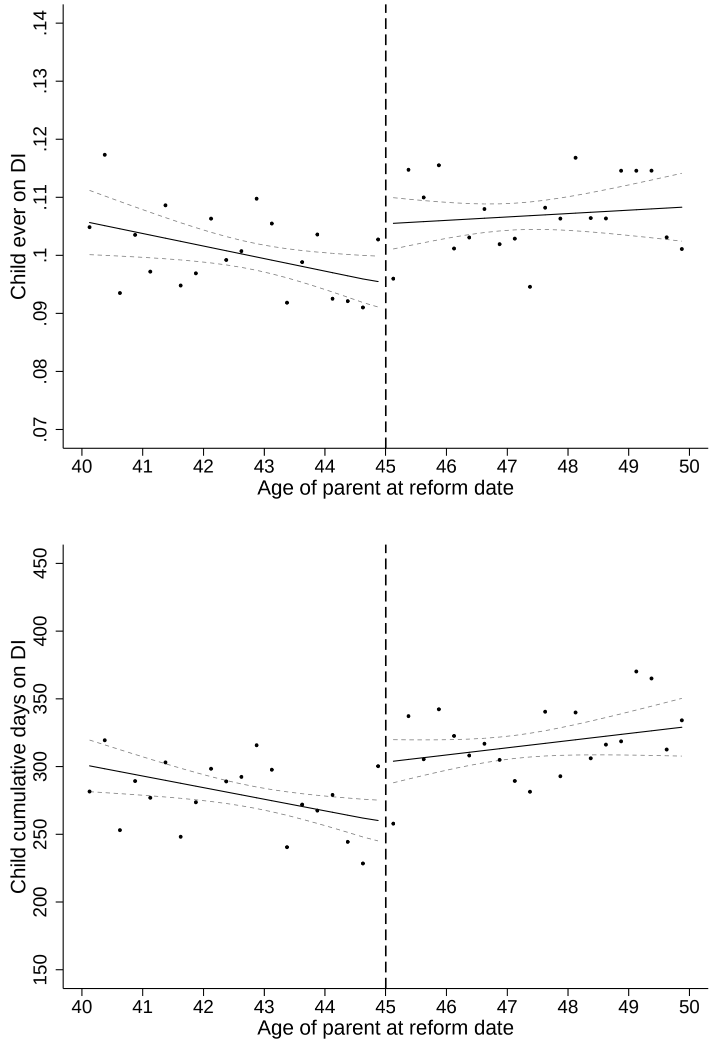
Appendix Figure A1: Child Age as of the Reform Date of August 1993

Notes: Kernel density estimates of child age, trimmed to exclude 0.3 percent of the data for visual clarity.



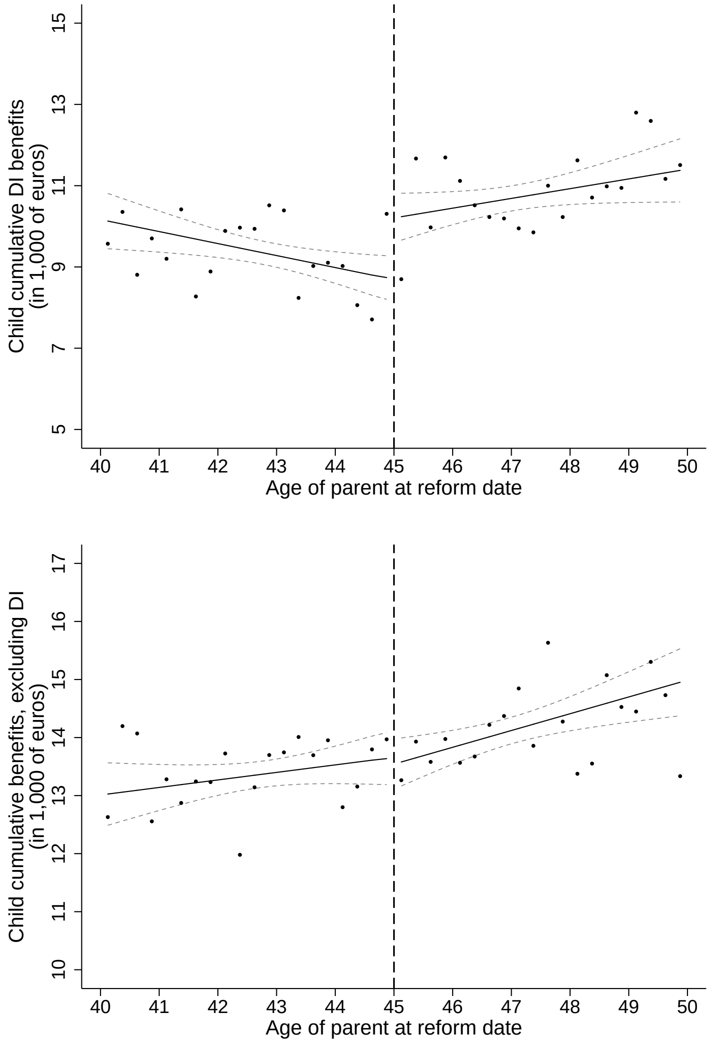
Appendix Figure A2: Effect of the Reform on Parents

Note: Graphs mirror those in Figure 2, but with 3 month age bins.



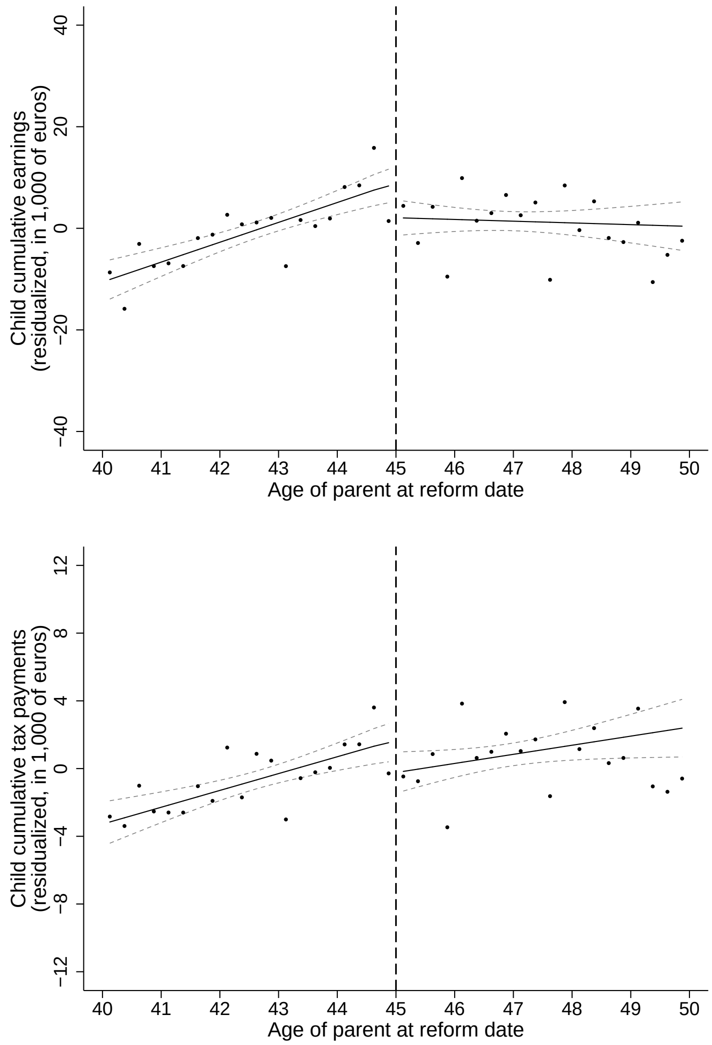
Appendix Figure A3: Child DI Participation

Note: Graphs mirror those in Figure 3, but with 3 month age bins.



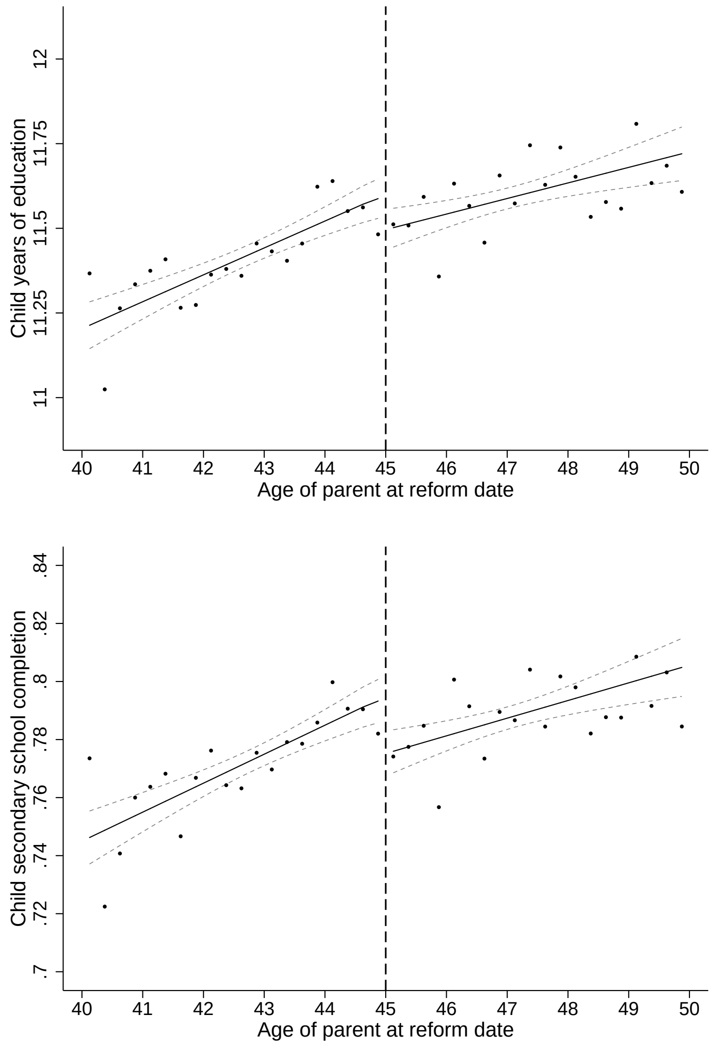
Appendix Figure A4: Child DI and Other Benefit Receipt

Note: Graphs mirror those in Figure 4, but with 3 month age bins.



Appendix Figure A5: Residualized Child Earnings and Taxes

Note: Graphs mirror those in Figure 5, but with 3 month age bins.



Appendix Figure A6: Child Educational Attainment

Note: Graphs mirror those in Figure 7, but with 3 month age bins.

Appendix Table A1: Summary Statistics

	Overall	Parent age: 40-45	Parent age: 45-50
A. Parents			
Female	0.27	0.29	0.26
Married	0.87	0.87	0.87
Age (Aug 1993)	45.17	42.58	47.36
Duration DI (months)	88.38	85.20	91.08
Degree of disability			
15-25%	0.10	0.14	0.07
25-35%	0.12	0.14	0.10
35-45%	0.08	0.09	0.08
45-55%	0.07	0.06	0.08
55-65%	0.02	0.02	0.03
65-80%	0.02	0.02	0.03
80-100% (Full disability)	0.58	0.53	0.63
Pre-DI earnings (euros)	6,529	6,249	6,766
Native Dutch	0.91	0.91	0.91
Number of kids in HH	1.71	1.87	1.58
Parent observations	70,319	32,279	38,040
B. Children			
Female	0.44	0.46	0.41
Age (Aug 1993)	15.60	13.86	17.27
Child observations	116,356	57,028	59,328

Notes: The sample in panel A is parents between the ages of 40-50 and on DI as of the reform date of August 1, 1993, who were still on DI in 1995, and had children living at home around the time of the reform. The sample in panel B is the children of these parents. A degree of disability between 0-15% does not qualify for DI benefits. Variables are measured as of January 1, 1996, unless otherwise indicated.
$^{**}p<0.05$, $^{*}p<0.10$

Appendix Table A2: RD Estimates of Child Crime and Marriage Outcomes

Child outcome in 2014	Mean	RF	IV
A. Ever arrested	.135	-.0021	.0016
		(.0043)	(.0044)
B. Ever incarcerated	.018	-.0030*	.0024
		(.0017)	(.0017)
C. Ever married	.458	.0107*	-.0084
		(.0062)	(.0063)
D. Ever cohabiting (with a child)	.300	-.0049	.0038
		(.0058)	(.0059)
Observations	123,186		

*Notes: See notes to Table 1. Standard errors in parentheses, clustered at the parent level. **p<0.05, *p<0.10*

Appendix Table A3: Robustness Tests for Main Child Outcomes (Reduced Form Models)

Specification	Ever on DI	Cum. days on DI	Cum. DI income	Cum. other transfers	Cum. earnings	Cum. taxes	Years educ.	Upper second. school
A. Baseline	-.011**	-47.151**	-1.579**	.092	7.178**	1.997**	.117**	.022**
	(.004)	(13.921)	(.499)	(.379)	(2.836)	(.969)	(.050)	(.007)
B. Quadratic trends	-.016**	-57.876**	-2.076**	.346	10.469**	3.120**	.130**	.026**
	(.006)	(20.334)	(.729)	(.562)	(4.165)	(1.415)	(.073)	(.010)
C. No triangular weights	-.010**	-36.897**	-1.175**	.021	5.282**	1.313	.099**	.018**
	(.004)	(13.054)	(.469)	(.354)	(2.642)	(.908)	(.046)	(.006)
D. No control variables	-.010**	-44.400**	-1.515**	.098	5.877**	1.548	.100**	.019**
	(.004)	(13.822)	(.497)	(.385)	(3.242)	(1.117)	(.052)	(.007)
E. 45 month window	-.012**	-52.179**	-1.776**	.128	8.028**	2.248**	.120**	.022**
	(.004)	(15.094)	(.541)	(.411)	(3.084)	(1.051)	(.054)	(.007)
F. 30 month window	-.018**	-69.737**	-2.440**	.264	11.077**	2.941**	.142**	.026**
	(.005)	(18.142)	(.651)	(.495)	(3.684)	(1.254)	(.065)	(.009)
G. Local linear regression	-.010**	-37.323**	-1.227**	-.019	3.923	.676	.076	.015**
bandwidth 60 months	(.004)	(12.737)	(.459)	(.345)	(3.040)	(1.029)	(.048)	(.006)
H. Local linear regression	-.010**	-46.517**	-1.589	.954	6.116*	1.641	.091*	.018**
bandwidth 45 months	(.004)	(14.526)	(.543)	(.412)	(3.424)	(1.196)	(.055)	(.007)
I. Local linear regression	-.016**	-67.382**	-2.338**	.308	8.993**	2.298	.147**	.025**
bandwidth 30 months	(.005)	(17.858)	(.629)	(.501)	(4.161)	(1.416)	(.067)	(.009)
J. Sample of children not	-.007	-17.811	-.333	-.200	6.038	1.665	.122**	.013
living at home	(.006)	(20.977)	(.812)	(.712)	(4.179)	(1.470)	(.066)	(.009)
K. Cluster s.e.'s	-.011**	-47.151**	-1.578**	.091	7.178**	1.997**	.117**	.022**
by parental age	(.003)	(13.007)	(.513)	(.332)	(2.576)	(.792)	(.043)	(.005)
L. Excluding non-natives	-.011**	-42.131**	-1.375**	.034	8.126**	2.229**	.124**	.023**
	(.004)	(14.629)	(.521)	(.387)	(2.959)	(1.019)	(.052)	(.007)
M. Excluding children whose	-.012**	-49.767**	-1.694**	.014	7.172**	1.837**	.126**	.022**
parents left DI in 1995	(.004)	(14.144)	(.506)	(.384)	(2.866)	(.977)	(.050)	(.007)

Notes: See notes to Tables 1-5. Standard errors in parentheses, clustered at the parent level.
$**p<0.05$, $*p<0.10$

Appendix Table A4: Characteristics of Compliers

	Compliers	Parent age: 44.5-45.5	Difference
A. Parents			
Female	.208**	.226**	-.018
	(.016)	(.003)	(.016)
Married	.927**	.896**	.032**
	(.013)	(.002)	(.012)
Duration DI (months)	98.697	88.943**	9.754**
	(2.804)	(.432)	(2.760)
Degree of disability			
15-25%	.100**	.086**	.014
	(.014)	(.002)	(.014)
25-35%	.077**	.118**	-.040**
	(.015)	(.002)	(.015)
35-45%	.166**	.094**	.072**
	(.014)	(.002)	(.013)
45-55%	.099**	.076**	.023*
	(.012)	(.002)	(.012)
55-65%	.035**	.026**	.010
	(.008)	(.001)	(.008)
65-80%	.041**	.027**	.015**
	(.007)	(.001)	(.007)
80-100% (Full disability)	.481**	.574**	-.093**
	(.020)	(.003)	(.020)
Pre-DI earnings	6,586.082**	6,723.887**	-137.806
	(151.404)	(22.464)	(149.599)
Native Dutch	.910**	.916**	-.006
	(.011)	(.002)	(.011)
Number of kids in HH	2.051**	2.078**	-.027
	(.042)	(.006)	(.041)
B. Children			
Female	.450**	.442**	.007
	(.021)	(.003)	(.021)
Age (Aug 1993)	15.710**	15.634**	.076
	(.182)	(.028)	(.179)

Notes: See notes to Table A1. For details on how to calculate the complier averages, see Borghans et al. (2014).

$**p<0.05$, $*p<0.10$