Intergenerational Spillovers in Disability Insurance*

Gordon B. Dahl† Anne C. Gielen‡

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Abstract: Using a 1993 Dutch policy reform and a regression discontinuity design, we find children of parents whose disability insurance (DI) eligibility was reduced are 11% less likely to participate in DI themselves, do not alter their use of other government programs, and earn 2% more as adults. The reduced transfers and increased taxes of children account for 40% of the fiscal savings relative to parents in present discounted value terms. Moreover, children of treated parents complete more schooling, have a lower probability of serious criminal arrests and incarceration, and take fewer mental health drugs as adults.

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†Department of Economics, University of California San Diego; Norwegian School of Economics; NBER; CESifo; IZA; email: gdahl@ucsd.edu

‡Erasmus School of Economics, Erasmus University Rotterdam; email: gielen@ese.eur.nl

1 Introduction

Does a parent's use of the social safety net have spillover effects on their children? The answer to this question is important both for the welfare of the child and long-run fiscal costs. Reforms curtailing the generosity of the social safety net could harm children if the drop in parental benefit receipt leads to reduced resources or a worse home environment. On the other hand, if decreased parental participation provides a positive role model or otherwise changes perceptions about assistance versus work, this could lead to better child outcomes and reduced government expenses. The goal of this paper is to explore how a parent's use of disability insurance (DI), a large component of the social safety net in most countries, affects a broad range of children's later-life outcomes and the long-term government budget.

Arguments about the presence, type, and size of intergenerational spillovers have figured prominently in policy debates for decades. Yet convincing empirical evidence is scarce because a parent's program participation is not random. Credible identification requires an exogenous shock which affects a parent's participation, but does not directly impact 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. We overcome these challenges using a regression discontinuity design (RD) coupled with rich administrative data.

Our context is a 1993 reform in the Netherlands which simultaneously tightened DI eligibility criteria and lowered payment generosity. All new DI applicants were subject to the reform. For those currently on DI, however, the application of the new versus old rules depended on the participant's age. Current DI recipients less than age 45 as of August 1, 1993 were re-examined by a medical doctor and subjected to the new set of rules which made them weakly worse off. Some of these re-examined individuals received lower payments because their degree of disability was reduced, and others were disqualified from the program entirely. In contrast, participants over age 45 were grandfathered in under the old rules. This creates an age cohort discontinuity, with current recipients around the cutoff being similar in all dimensions except for exposure to the stricter DI rules.

Using an 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 euros, or 10% (unconditional on remaining on DI). A similar analysis applied to our sample of parents reveals slightly larger effects, with 5.4% of parents exiting DI and annual benefits dropping by 1,300 euros. Even though treated participants have been on DI for 7.5 years on average, they exhibit substantial work capacity, with over 60% of lost DI benefits being replaced with earnings.[1] In the short run, substitution to other government programs makes up another 30% of lost DI benefits, but this effect tapers off over time.

Taking advantage of the Dutch reform and intergenerational data spanning more than 20 years, we estimate how the reduction in parental DI benefits affects (i) a wide array of children's later-life outcomes (participation in DI, use of other social assistance programs, earnings and taxes, human capital investments, arrests and imprisonment, and adult mental health) and (ii) the government's budget (fiscal costs for each generation, including changes in DI payments, other social program receipt, and taxes paid). The running variable in our RD design is the age of the parent and the dependent variables are child outcomes. The DI reform affected parents on both the intensive margin (payment amounts) and extensive margin (participation). While it would be interesting to disentangle the separate effects attributable to each, this is not feasible since the reform only provides one instrument. We therefore focus on the reduced form effects, which are the policy relevant parameters from a tightening of DI eligibility in a system which allows for partial disability (see Section 3.2).

We offer four sets of results on intergenerational spillovers. Our first 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 11% less likely to have ever participated in DI (-1.1 percentage points on a base of 10%). This is as of 2014, 21 years after the reform, when children are 37 years old on average. Using cumulative income received from DI as the dependent variable instead, children of treated parents receive roughly 1,600 euros less in DI payments, which is sizable compared to the mean of 10,100 euros.

Second, we find an increase in a child's taxable earnings but no effect on participation in other social support programs. Cumulative earnings up to 2014 rise by approximately 7,200 euros, or around 2%, for children of parents subject to the less generous DI rules. In contrast, we find no change in cumulative unemployment insurance, general welfare assistance, 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.[2]

Third, we find that child spillovers have first order fiscal effects. Compared to the

[1] Both partially and fully disabled individuals demonstrate substantial work capacity. See Appendix Table A7 in Borghans et al. (2014)).

[2] The importance of accounting for broader tax and transfer effects when evaluating public programs is made in a recent paper by Autor et al. (2019).

direct effect of the reform on parents up to age 65 (when they transition off DI), we estimate that children account for 21% of the net fiscal savings in present discounted value terms (assuming a 3% discount rate). Extrapolating beyond, when parents are no longer eligible for DI but their children still are, we calculate that 40% of the budgetary savings is due to children. However, this is only a partial accounting of the fiscal savings due to children. This is because the quasi-experiment we study reduces program generosity for parents below the RD cutoff, while holding program generosity fixed at the lower post-reform level for children. The full fiscal savings of the reform for children is likely to be much larger, as we only capture the portion due to complementarity between the parents and children, but not the main effect of the reform itself on child DI use.

Fourth, we find evidence for spillovers to other domains, which helps provide a fuller picture of the reform's effects and insights into possible mechanisms. We find intriguing evidence for anticipatory educational investments, consistent with children planning for a future with less reliance on DI.[3] When a parent is subjected to the tighter DI rules, 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 completion (roughly the equivalent of High School in the U.S.), with a 2.2 percentage point rise. A second change is in the area of crime. Children of parents exposed to the reform are 15% less likely to have been arrested for a serious crime and their probability of imprisonment drops by 18%. A third improvement is in the later-life mental health of children who were young when their parents' DI benefits were scaled back. These children reduce their use of prescription drugs for mental health disorders in adulthood by 11%. The costs and benefits of these extra improvements in child well-being are more difficult to quantify in euro terms, but are also relevant for the public budget.

There are several potential explanations for our findings. We begin by ruling out various possibilities which others have postulated for intergenerational spillovers. It is 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. It also is not information about how to apply, as all parents have been through the DI screening process. Likewise, reduction in stigma associated

[3] We note the education result is also consistent with a different type of anticipation driven by the DI reform. Children could forecast their utility from being employed in the future will be higher if their parent is also employed, and that getting more education will increase their employment prospects.

with parental entry into the DI program is ruled out, as the reform affects the exit margin. Of course, if information and stigma only become salient when the child is old enough to consider applying for DI, then whether the parent is still receiving DI benefits when the child is an adult could have both information and stigma effects.

Instead, three explanations consistent with our battery of findings are that children learn about formal employment, have a better home environment, or experience a scarring effect. The sizable increase in employment for treated parents could help their children learn about the labor market as well as provide a positive role model. This explanation is consistent with increased child investments in education and reduced participation in illegal activities. A related explanation is that children grow up in a better home environment when their parent's DI use drops. This is consistent with correlational studies which document that long-term unemployment is associated with increased rates of depression and stress within the home (e.g., Björklund, 1985; Di Tella et al., 2001). Finally, children whose parents are forced off DI or have their benefits reduced may infer they cannot rely on the government to take care of them, similar to the scarring effect discussed 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.

Regardless of the underlying explanation, our results suggest the reform curtailing parental DI benefits had positive spillover effects on children, both from an individual and societal perspective. Lowering DI benefits for parents did not appear to create a resource trap which harmed children and prevented them from being self-sufficient, even though parental income did not change in the short run and declined in the long run. And from a government budget perspective, the resulting intergenerational fiscal benefits were large. It is important to recognize, however, that our setting captures the effect of lowering DI use for parents with marginal disabilities and substantial work capacity. This is arguably the most policy relevant group, but care should be taken not to extrapolate to other populations.

Despite the importance of intergenerational spillovers in policy discussions, there is surprisingly little causal evidence. As surveyed by Black and Devereux (2011), there are many observational studies, but few with credible research designs. The best observational work uses panel data and family fixed effects (e.g., Bratberg et al., 2015). Only a handful of papers have used quasi-experimental methods, with most of these exploiting variation across U.S. states and over time for identification. Antel (1992) uses state-level welfare benefits and net migration flows and finds evidence for intergenerational links. Levine and Zimmerman (1996) uses variation in state benefit levels and local labor market conditions and concludes that most of the intergenerational correlation in welfare use is not causal. Hartley et al. (2017) uses more recent temporal variation across U.S. states in AFDC, TANF, and EITC benefits and finds a mother's use of welfare significantly increases the probability her daughter will participate as well. Using a random judge design, Dahl et al. (2014) finds that when a parent is allowed on to DI on appeal in Norway, it increases the chance their child also participates as a young adult.[4]

Our paper makes several contributions to this sparse literature.[5] We leverage a nationwide reform which generates convincing quasi-experimental variation combined with high quality administrative data. The rule changes we study are highly policy relevant, as they allow us to quantify how children are affected when parents already on DI become ineligible or have their payments reduced. This margin is likely to have different effects compared to program entry (in part because a parent's direct experience with the DI program differs), which is what other work has focused on. An advantage of our study is that we can follow children to an age in adulthood when DI participation is relatively common. We also look at mothers, fathers, sons, and daughters, while most studies focus on the mother-daughter link. Importantly, we consider effects not only on children's DI participation, but rather a wide range of labor market, government support, education, crime, and mental health outcomes. This allows us to better understand mechanisms and the overall impact of intergenerational spillovers on children's future well-being. Another novel contribution is the calculation of the combined fiscal costs, including changes in a child's DI payments, taxes, and other transfers. This matters substantively, as the increase in child tax revenues exceeds the reductions in child DI payments. We also provide a comparison of the cumulative cost savings from each generation, documenting the importance of

[4]Two related papers use a bounds analysis; Pepper (2000) obtains large confidence intervals, while De Haan and Schreiner (2018) bounds average treatment effects to be substantially below most quasi-experimental estimates which identify local treatment effects. While intergenerational links are not the focus of the paper, Deshpande (2016; see Appendix Figure A7) leverages a policy change which removed low-income youth from SSI and finds no effect on the combined outcome of fertility and intergenerational SSI receipt. Another related paper is Aizer et al. (2016), which looks at the long-run impacts of the historical Mothers' Pension program on children's outcomes.

[5]More broadly, our study complements a related literature which looks at other shocks to parents and children's outcomes. See Chen et al. (2015), Chetty et al. (2016), Dahl and Lochner (2012), Katz et al. (2001), Milligan and Stabile (2011), Oreopoulos (2003), Oreopoulos et al. (2008), Rege et al. (2011), and Stevens and Schaller (2011). There is also a related literature on disability insurance programs and their labor supply effects. See Autor et al. (2016), Bound and Burkhauser (1999), Chen and van der Klaauw (2008), Campolieti and Riddell (2012), de Jong et al. (2011), Deshpande (2016), French and Song (2014), Gruber and Kubik (1997), Kostol and Mogstad (2014), Maestas et al. (2013, 2018), Mullen and Staubli (2016), and von Wachter et al. (2011).

both parent and child responses for the government's long-term budget. Taken together, our results highlight the strength and nature of parent-child interactions, and the importance of considering spillover effects in policy debates about social assistance programs.[6]

The remainder of the paper proceeds as follows. The next section provides background on DI 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 effect of the reform on parents. Section 5 presents our results on child spillovers in program participation and work and Section 6 discusses the resulting fiscal implications. Section 7 documents spillovers in education, crime, and mental health. Section 8 conducts some specification checks. We then conclude.

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 fully after their first day of employment, replaced up to 80% of wages, and included a variety of subjective illnesses. Moreover, sickness benefits replaced a worker's wages between 80 and 100% during the transition to disability insurance, and workers on sickness benefits for a full year were routinely transferred to the DI program without a serious reappraisal of their disability (Kalwij et al., 2014). These factors fueled a rapid rise in DI recipients, from 4% participation of the eligible population in 1967 to over 8% by 1980. Modest reforms in the early 1980s were enacted in an attempt to stem the rise, but were largely ineffective. Participation reached a peak of 12% in the late 1980s, with payments ballooning to 4.2% of gross domestic product.

Starting in the 1990s, a series of reforms were implemented to control the spiraling costs of the DI system, including reductions in benefit levels, tightened eligibility criteria, changes to the sickness benefit program, and increased financing and respon-

[6] This footnote highlights the differences between the current paper and the prior work of Dahl et al. (2014). First, Dahl et al. study the intergenerational effects of making it harder for parents to get onto DI in the first place (the appeals margin), whereas our analysis looks at reducing benefit levels for parents who have already been on DI for many years. Second, the current paper has more and better measured outcomes, including the intensive margins of DI and other social program participation, cumulative earnings and taxes, crime, and mental health. The Dahl et al. paper could not look at high school spillovers and lacked precision to identify college spillovers, whereas we find strong intergenerational effects on high school completion. Third, we examine the fiscal consequences of intergenerational spillovers. Fourth, we follow children for a longer time period (up to 21 years later), including older ages when DI use is more common among the children.

sibility 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 Appendix Figure A1 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 Dutch rate of 7% is higher compared to the U.S. rate of 5%, but lower than Norway's 10%, for example. Several researchers have proposed adopting aspects of the Dutch system to reverse steeply increasing DI trends in the U.S. (Autor, 2015; Burkhauser et al., 2014).

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 et al. (2014) to study benefit substitution, we only briefly explain the most salient features of DI in the Netherlands and the 1993 reform, and refer readers to their paper for further details.

In the Netherlands, individuals receive DI payments depending 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 (in the prior year) to a constructed measure of "earnings capacity." The reform we exploit affected the calculation of earnings capacity in a way which made DI less generous.

The degree of disability is denoted in 7 categories which determine the replacement rate;[7] 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), and at the same time receive DI payments for the fraction of lost earnings.[8]

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, 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 (see footnote 7).

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.

All new applicants, regardless of age, were subject to the new rules. But for existing DI recipients, the original 1993 reform specified that those age 50 or older

[7] 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%. In our data, we observe the category, but not the continuous measure, for the degree of disability.

[8] Pre-disability annual earnings are indexed and subject to a cap (roughly 36,000 euro in 1999). If individuals exceed their capped earnings exemption, DI benefits are reduced temporarily; if earnings exceed the cap for three years, individuals are reclassified.

at the time of the reform would continue to be subject to the old rules. Since it was not logistically feasible to re-examine all existing DI participants below age 50 immediately, they were scheduled to be re-examined over the ensuing years based on their age cohort. They started with the youngest cohorts under the age of 35 as of 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.

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, and pre-disability earnings. 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 register which is used to determine eligibility and benefit amounts for all Dutch social insurance programs. Additional details on many of these variables, and how they are measured, can be found in Appendix B of Borghans et al. (2014).

We further merge in educational attainment as of 2014. The education data is complete for younger cohorts, but comprises only a sample for older cohorts.[9] Crime

[9]We have education information for approximately 70% of children. For the parent's generation,

data on arrests and incarcerations come from two different data sources, and both span 2005-2014. We also merge in information on prescriptions for mental health drugs; this data is available starting in 2005. 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.

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 (so that they have had an opportunity to finish their schooling and enter the formal labor market); 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.[10] For the education analyses, our sample is smaller since education was collected for all individuals in later cohorts, but only a subsample of earlier cohorts. For the crime, arrest, and mental health prescription outcomes, we use the same sample restrictions, but only require the child to be age 18 or older by 2014.

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.

Appendix Table A2 shows transition matrices for a parent's disability status in

we only have information on education for 22% of the sample.

[10]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).

1996 (before the re-examinations take place) versus 1999 (after the re-examinations are finished), separately for the age 40-45 and 45-50 cohorts. The younger cohort was subject to the stricter rules. But since both cohorts were re-examined, the degree of disability could adjust upwards or downwards for both cohorts. As expected, the younger cohort experienced much larger reductions in disability ratings. Take the example of parents who were classified as fully disabled prior to the reform. For the older cohort, 5.9% had their disability rating reduced (3.6 percentage point drop), whereas for the younger cohort, 18.9% had a reduction (9.6 percentage point drop). More generally, for the older cohort, 3.8% exited DI completely (either because they chose to leave or were forced off), while 2.1% remained on DI but with a lower DI rating. This contrasts with the younger cohort where 13.2% exit DI and 5.7% have a lower rating.

Turning to children, their average age is 15.6 as of the reform date. Appendix Figure A2 graphs the distribution of child ages separately for parents on each side of the age cutoff for the main sample. 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 ages of 40 to 45 were re-examined according to the new, more stringent rules. The direct effect of the reform on parental outcome y^P can be modeled in an RD framework as:

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

where a^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 corresponding reduced form model for child outcome y^C is:

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

where e^C is an error term, and h_l, and hg are unknown functions. The vector of control variables x_i are the same in both equations, and include both parent and child characteristics measured before the reform (see footnote to Table 1). The coefficient λ is the reduced form (RF) or intention to treat (ITT) effect of the reform on outcomes.

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 reform date) 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.[11]

Borghans et al. (2014) perform two empirical tests for manipulation for their sample, which includes all individuals on DI, and not just parents. They conclude there is little evidence of manipulation. We repeat their tests, but for our sample of parents and children. We first graph the histogram of parental age at the time of the reform, and find no noticeable jumps around the age 45 cutoff. 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; see Appendix Figure A3). Second, we find no systematic evidence of changes in the distribution of pre-determined characteristics around the reform date. Two out of 16 variables are significant, which is not much more than would be expected by chance. Indeed, the joint test for all 16 variables has a p-value of 0.20. (See Appendix Figures A4 and A5 and Appendix Table A3). Moreover, our RD estimates barely move when we include these characteristics in the regressions.

[11]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.

Exclusion Restriction and Monotonicity. With no manipulation or differential attrition (or any other reason which differentially affects which types of individuals are found immediately to the left or right of the cutoff), the RD design identifies the causal RF impacts. There is no need for an exclusion restriction or monotonicity. Since the RF captures the policy relevant intergenerational parameters of a tightening of eligibility in a DI system with partial disability, these estimates are interesting in their own right.

A practical reason to focus on RF instead of IV estimates is that the DI reform affected parents on both the intensive margin (payment amounts) and extensive margin (participation). Since the reform only provides one instrument, it is not feasible to disentangle the separate effects attributable to each.[12] Consider the challenge of using the extensive margin of participation as the instrument. To satisfy the exclusion restriction, the intensive margin would need to not directly affect children's outcomes. This would imply a drop in DI payments from 10,000 euros to 2,000 euros has no effect on children, and likewise that a drop in payments from 10,000 euros to 0 euros and a drop in payments from 2,000 euros to 0 euros, have the same effect. This is clearly not a reasonable assumption.

Now consider the assumption required to use the intensive margin as the instrument. To interpret the resulting IV estimates as causal requires the exclusion restriction that parental exit has no direct effect. 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. This assumption, while not perfect, is more reasonable. Therefore, to facilitate a comparison to OLS estimates, we discuss and present IV estimates which scale the effects by the parental drop in DI payments in Online Appendix A and Online Appendix Table A6.[13]

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

[12] If the effects of the reform on exit versus a drop in payments could be predicted based on predetermined characteristics, one could interact these predetermined characteristics with the reform cutoff to get a sense of the two margins. Unfortunately, there is not enough statistical power based on pre-determined characteristics to make this exercise useful.

[13] 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.

same compared to what they would have received under the old rules. Since the new rules weakly reduced payments for any individual whose situation had 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 as their disabilities worsen. The bigger problem for IV estimation is that exclusion restrictions are unlikely to hold.

4 Effect on Parents

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.

Figure 1 graphs the relationship between the reform and parent's intensive and extensive use of DI. The sample is comprised of parents who were already receiving DI benefits prior to 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 determines whether the parent is subject to the new versus old DI rules. On the y-axes are parental benefits in 1999 and parental DI exit by 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 figure, and all other RD graphs, can be found in the Appendix.

The left 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. To document the extensive margin of the DI reform, in the right panel we graph the fraction of parents who exit DI. The first pattern to notice is that exits decrease with age. More relevant for our RD design, at the cutoff there is a sizable increase in exits for parents exposed to the reform.

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. We use the same set of covariates in all of our child outcome regressions.[14]

The first specification in Table 1 looks at a parent's DI payments in the year 1999. Mirroring what was drawn in the left panel of Figure 1, there is a sizable 1,300 euro drop in benefits for parents exposed to the reform, which amounts to a 13% reduction compared to the mean. 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, which is roughly a 60% higher exit rate than otherwise would be predicted. Both the size and the precision of these first stage effects are useful for identifying spillover effects on children, as spillovers are by their nature second order effects.

As a reminder, some individuals exposed to the reform were kicked off the program (extensive margin), while others remained on DI but with lower benefits (intensive margin). While it would be interesting to disentangle the separate effects attributable to each margin, this is not feasible since the reform only provides one instrument. We therefore focus on the reduced form effects on children's outcomes, which are the policy relevant parameters for reforms of this type.

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.

[14]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.

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 2 presents RD graphs for the extensive and intensive margins of DI use. The x-axes are the same as in Figure 1, 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.

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 left graph in Figure 2 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 in the right graph. 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.

To arrive at the cost savings to taxpayers from the reduced DI use of children because of complementarity between parents and children, in Figure 3 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 (including zeros).

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.

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 right graph in Figure 3 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 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 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 left panel of Figure 4 plots the cumulative earnings of children for the 16 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.[15] The left 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

[15] We note that while child age is positively correlated with parent age, this should not be a problem, as child age is smooth through the RD cutoff. 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.

parents were subject to the reform. This is roughly a 2% increase in earnings relative to the overall mean.

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 right graph of Figure 4 plots 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.

6 Fiscal Consequences

6.1 Cumulative Fiscal Effects

To provide a more comprehensive picture of the fiscal spillover effects, we 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 subtract 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.

To provide further insight into the fiscal effects over time, Figure 5 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 5 plots the net effect of taxes minus transfers over time. It mirrors the reduction in DI payments and the rise in tax payments over

[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.

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. We note that 90% confidence intervals are shown for visual clarity in Figure 5; the cumulative DI benefits, the cumulative tax payments, and the cumulative taxes minus transfers estimates are all statistically different from zero at the 5% significance level by 2014.

6.2 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 5, 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

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

is due to children.[18] Moreover, the full fiscal savings of the reform due to children is likely to be much larger, as we only capture the portion due to complementarity between parents and children, but not the main effect of the reform itself on child DI use.

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. These rough estimates also do not include the public costs and benefits associated with the extra education, lower crime and imprisonment, and improved mental health we document in the next section. 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.

7 Spillovers in Education, Crime, and Mental Health

When viewed in isolation, the fact that children participate less in DI if a parent is exposed to the reform could be either good or bad. If children participate less in DI even though they have a debilitating condition, or if the reduction in family income harms a child's development, then children in treated families are worse off. However, children could also react positively to the parental shock. The fact that children earn more and do not change their participation in other government benefit programs suggests increased self-sufficiency. To further explore whether affected children are better or worse off, we examine three child outcomes which are key markers of future well-being: educational investments, criminal activity, and mental illness in adulthood.

7.1 Educational Investments

We first examine whether children alter their educational investments. We collected data on children's educational attainment as of 2014. In Figure 6, the left 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 estimate and standard error for years of education. There

[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 5 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.

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 right panel in Figure 6 plots the RD graph with upper secondary school completion (roughly the equivalent of High School in the U.S.) 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%.

Table 5 further reports RD estimates for other levels of schooling.[19] 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 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. The DI decision occurs in the future, after a child has grown up and entered the labor market. But it appears that children (or their parents) anticipate this lower reliance on DI and increased labor market attachment in the future, and make different investment choices while they are still young. These higher levels of education can help explain a portion of the increase in earnings and tax revenue we observe, as we discuss later in Section 8.1. More broadly, higher education levels are associated with increased life satisfaction and happiness (e.g., Di Tella et al., 2001).

7.2 Crime

We next turn to an examination of criminal activity. Crime could decrease for two reasons. First, the opportunity cost of committing crime should rise as children work and earn more in the formal labor market. Second, if the home environment improves or parents become better role models after reducing their reliance on DI (and increasing their employment), this could help children stay out of trouble. On the other hand, children's crime could increase since treated parents have less time to supervise their children once their employment increases. To evaluate intergenerational

[19] 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.

spillovers in crime, we examine both arrests and incarceration.

Panel A in Table 6 reports results for having ever been arrested between 2005 and 2014, broken down by different categories of crime. The first entry looks at arrests for any crime type, and finds a negative, but statistically insignificant effect. The next two specifications split the crime types into minor versus serious crimes. Minor and serious crimes are defined based on whether the crime an individual is arrested for is associated with an above or below median probability of imprisonment.[20] Minor crimes include arrest categories such as shoplifting, threats, and traffic violations, while serious crimes include arrest categories such as rape, residential burglary, and arson (see Appendix Table A4 for a full listing). Using this breakdown, we find small and insignificant effects for minor crimes. In contrast, arrests for serious crimes drop by 0.54 percentage points relative to a mean of 3.63, which translates into a 15% reduction in arrests (see Table 7 for the corresponding RD graph). To provide additional insight, we further break down serious crimes into serious violent versus serious property crimes. There are sizable drops in both subsets of serious crime, with 33% reduction for serious violent crime and a 12% decrease for serious property crime.[21]

Panel B reports results for having ever been imprisoned between 2005 and 2014. Children of parents exposed to the reform have a 0.29 percentage point lower probability of being sent to prison, a result which is significant at the ten percent confidence level. Relative to an incarceration rate of 0.165, this represents an 18% drop in imprisonment. Imprisonment occurs for more serious crimes, so it is interesting to see that the incarceration result lines up with the arrest result for serious crimes (i.e., those likely to result in imprisonment). We note the arrest and imprisonment data come from two separate data sources, each collected by different governmental agencies, so the similar results are not mechanical.

[20]While we have arrest and incarceration information for each individual broken down by crime type, we cannot ascertain which arrest is linked with which incarceration. Moreover, the crime types listed in the arrest records are somewhat different compared to those in the incarceration records. So to define crime severity, we take everyone who was arrested for a specific crime in 2014 (e.g., assault) and then calculate the probability these individuals are incarcerated in 2014 for any reason. Serious and minor crimes are defined as an incarceration probability above or below the median across the 48 arrest categories using the Standard Crime Classification of Statistics Netherlands.

[21]We do not have enough power to precisely estimate effects for each of the 48 crime categories separately, but we note that almost all of the individual estimates are negative. One exception is traffic crime, which is relatively common and has a large positive effect. This could reflect the fact that traffic crimes require a car, which may be more likely if a person works and has higher earnings.

7.3 Mental Health

Finally, we explore whether a child's mental health in adulthood (when the data first become available) is affected by a parent's exposure to the DI reform. Our measure of mental health is based on having had a prescription for one of several drugs. The prescription drug categories are antipsychotics, anxiolytics, hypnotics and sedatives, antidepressants, and psychostimulants. For Table 7, we limit the sample to children age 14 or younger around the time their parents were exposed to the reform. We focus on this age range because it corresponds to the critical period identified by the World Health Organization, which notes that "Mental health evolves throughout the life-cycle... The early stages of life present a particularly important opportunity to promote mental health and prevent mental disorders, as up to 50% of mental disorders in adults begin before the age of 14 years" (WHO, 2013). In the robustness section, we report results for children of other ages as well.

In the first specification of Table 7, we find that adult prescriptions for any type of mental health drug fall by 2.6 percentage points for children who were young when their parents were exposed to the reform. This is relative to the average of 23% of children having been prescribed a mental health drug by between 2005 and 2014, or an 11% drop. Figure 8 graphs the corresponding RD figure. When we break up the analysis into the 5 types of mental health drugs, the estimates are all negative and economically meaningful, with effect sizes of 23%, 13%, 30%, 11%, and 7% relative to their respective means. When interpreting these estimates, it is important to remember that we are using exposure at a young age to measure mental health drug use as adults. One interpretation of our findings is that children experience a more stable and less stressful home environment if their parent relies less on DI and more on earnings from work.[22] Children could also experience less stigma and stress by having a parent in the labor force at this formative age. Another possible interpretation is that the observed increases in education and earnings of the children themselves contribute to more positive mental health.

8 Specification Checks

8.1 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

[22] We do not, however, find any evidence for a change in parental divorce in response to the reform (RD estimate = -.002, s.e. = .005).

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] To look at younger ages, we also present results for children less than age 14 at the time of the reform. While it would be interesting to look at even younger children, the sample of parents around the reform cutoff of age 45 are too old for such an analysis.

Table 8 reports separate RD estimates for our main outcomes by child age. Looking at the DI spillovers in specifications A through C, the effects are all large and statistically significant for the younger groups. The estimated effects for the older group, while going in the same direction, are smaller, especially compared to the sample means. For other social programs in specification D, we find no effect for any of the age groups, in line with what we found for the entire sample. When we turn to earnings, we find a relatively large and marginally significant effect for the older group. These increased earnings also translate into higher taxes paid, although the estimate is marginally insignificant at conventional levels. For the younger groups, 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 estimate for the age 18 and younger group shows an increase of 0.17 years of schooling associated with the reform, with the age 14 and younger group having a slightly larger increase. 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 groups. 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 these results fit together, particularly the stronger DI effect for the younger groups and the larger earnings effect for the older group? First, it is important to recognize that because of their age, the 19 and older group has had over three more prime-age years to work in the labor market compared to the 18 and younger group; indeed, mean cumulative earnings for the older group are over 50% higher. On top

[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.

[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.

of this, the younger groups get 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 across the 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.

Turning to our final two outcomes, specification I reports results for serious crime. The estimate is larger for the ≤18 split, but relative to their means, the >19 split is roughly the same percent size and close to significant at the 10% level. Finally, the evidence on mental health drugs is concentrated among the youngest age group, with little evidence of an effect for either of the other age categories. While most of the outcomes in specifications A-I are associated with statistically significant effects for the entire sample of children, and often for the age subsamples, the mental health result only shows up for the ≤14 age group. This could be due to the youngest group being particularly vulnerable for the onset of mental health conditions (WHO, 2013).

Overall, the pattern of results in Table 8 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 and work experiences, and have more time to alter their educational and work plans. When younger children delay entry into the workforce to get more education, they improve their future earnings prospects, but this reduces their ability to help out the household finances of their parents immediately after the DI reform.

8.2 Effects by Parent and Child Gender

We next explore whether there are heterogeneous spillover effects by either parent or child gender. We begin by splitting the sample into children whose fathers versus mothers were affected by the reform. Results are found in Table 9. We find that mothers have larger intergenerational spillovers on their children's DI use and earnings. For example, children of mothers affected by the reform reduce their cumulative use of DI by 74 days, compared to 39 days for children of fathers. Likewise, the intergenerational spillover effects on child earnings are four times larger for mothers

[25] To make a comparison, focus on the age>19 versus age≤18 split. Treated children in the younger group receive an extra 0.171 years of education, compared to 0.065 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 an education-induced loss of 5,431 in earnings for treated children. Adding this to the estimate of 4,744 for the younger group (specification E) equals 10,175, which is larger compared to the estimate of 7,998 for the older group.

than fathers. In contrast, fathers appear to be more influential for children's education, crime, and mental health outcomes. For example, children of fathers affected by the DI reform are 2.4 percentage points more likely to complete upper secondary school (i.e., the equivalent of high school in the U.S.), versus 1.3 percentage points for the mother sample. Similarly large gaps exist for a child's arrests and use of mental health drugs. While the mother and father estimates are often statistically different from zero, they are generally not statistically different from each other. This is in part because of smaller samples sizes, particularly for mothers, who comprise only one fourth of the total sample.

This auxiliary evidence for gender-specific spillovers depends on the child outcome, in line with prior research. The results on mother's effects on child DI use and earnings are consistent with longitudinal data which shows that having a working mother while a teenager influences future participation in the labor market (e.g., Stinson and Gottschalk, 2016). The larger effects for fathers on education, crime, and health are consistent with research which finds that fathers are particularly important for a child's cognitive and emotional development (e.g., Cabrera et al., 2007).

It would be interesting to explore heterogeneity by the interaction of child gender with parent gender, since role model effects may be stronger when gender is the same. Unfortunately, we do not have the precision to break up the data this finely. But we can look at the intergenerational spillover effects on sons versus daughters, which we do in the later columns of Table 9. Many of the interventions in the U.S. welfare context find larger, and generally more positive, effects among girls than boys (e.g. Hoynes et al., 2016). Relative to their sample means, we find estimated spillover effects in DI use and earnings which are similar for sons and daughters. For education there is a roughly 50% larger spillover effect for sons, although the difference is not statistically significant. Sons drive the large decrease in arrests, consistent with them being responsible for most of the crime in our data. Turning to the use of mental health drugs, where daughters have a 50% higher usage rate, the effect of the reform is concentrated among daughters. Our broad conclusion from these results is that children of both genders experience sizeable intergenerational spillovers, with most effect sizes being a function of the gender-specific means of the outcomes.

8.3 Robustness

Appendix Table A5 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 limit the sample to only include 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 (e.g., if the parent is divorced). With this sample, we find no significant effect for most outcomes, which suggests that first-hand exposure is required for the intergenerational effects to materialize. 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.

8.4 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.[26] 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 10 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.

[26] As a reminder, November 1996 is when the Dutch Parliment passed the motion to grandfather in the 45-50 years olds under the old DI rules. The grandfathering was contingent on being continuously on DI from before the reform date in August 1993.

9 Conclusion

Mapping out the nature and breadth of intergenerational DI spillovers is crucial for understanding long-term child well-being and budgetary impacts. But the endogeneity of parental participation makes this a difficult task. To obtain causal estimates, we take 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, with wide-ranging personal and societal effects. 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, invest significantly more in their education, commit less crime, and have better mental health as adults.

As the reform only provides one instrument, we cannot disentangle how these various causal outcomes influence and interact with each other. But we can perform a simple accounting exercise which suggests the increase in education can account for 48% of the increase in earnings for children of parents affected by the reform (assuming an 8% return for each extra year of education). Likewise, the reduction in DI participation can account for roughly 43% of the increase in earnings.[27] Three explanations consistent with our findings are that children learn about the labor market from their parent's increased employment, have a better home home environment, or experience a scarring effect where they infer they cannot rely on government support.

From a policy perspective, our study serves as an important lesson for the evaluation of costs and benefits from reforms to the social safety net. 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 40 percent in the long run. Additional benefits which are harder to quantify in euros, but which are nonetheless important, include children's increased education, decreased criminal activity, and improved mental health as adults.

Taken together, our results indicate the reform curtailing parental DI benefits had positive spillover effects on children, both from an individual and societal perspective. Lowering DI benefits for parents did not appear to create a resource trap which harmed children and prevented them from being self-sufficient, even though parental

[27] This calculation is based on estimates from Table 6 in French and Song (2014). Using the upper and lower bound estimates from footnote 51 in Maestas, Mullin, and Strand (2013) instead implies it accounts for between 24 and 110%. One caution is the estimates from these papers may not be directly comparable to the Dutch system and the complier types for our reform.

income did not change in the short run and declined in the long run. And from a government budget perspective, the resulting intergenerational fiscal benefits were large. It is important to recognize, however, that our setting captures the effect of lowering DI use for parents with marginal disabilities and substantial work capacity. This is arguably the most policy relevant group, but care should be taken not to extrapolate to other populations. In future work it would be interesting to explore intergenerational spillovers in other settings, including instances with more severely disabled parents or policy reforms where social assistance becomes more, rather than less, generous.

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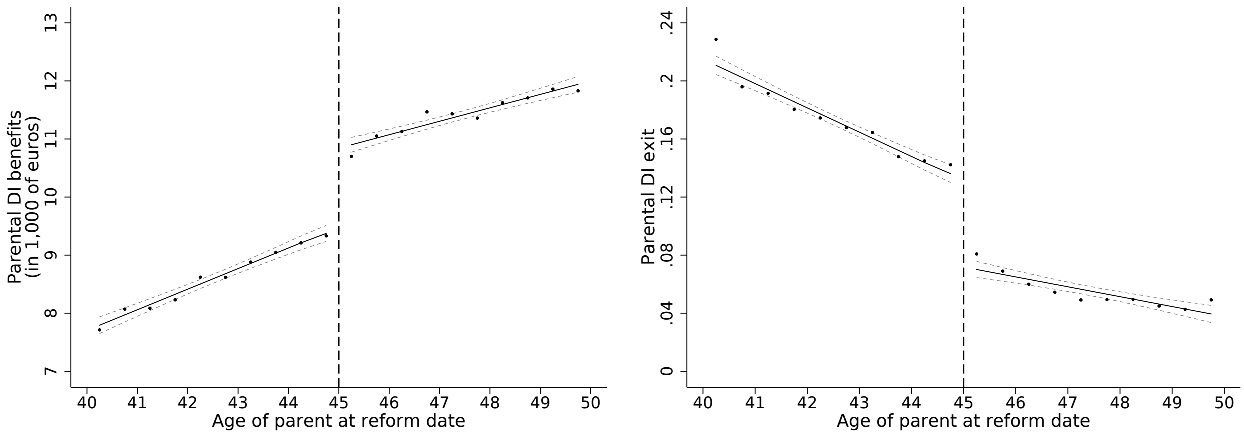


Figure 1: Effects of the Reform on Parents

Notes: Each observation represents average parental DI receipt in 1999 (left panel) or average parental DI exit by 1999 (right 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.

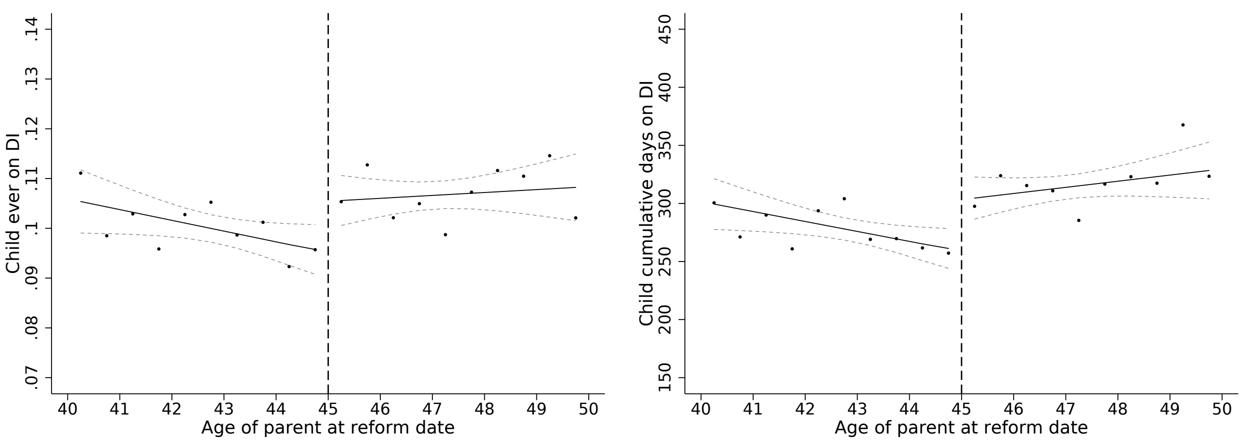


Figure 2: Child DI Participation

Notes: Each observation represents average child DI participation by 2014 (left panel) or average cumulative child days on DI by 2014 (right 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.

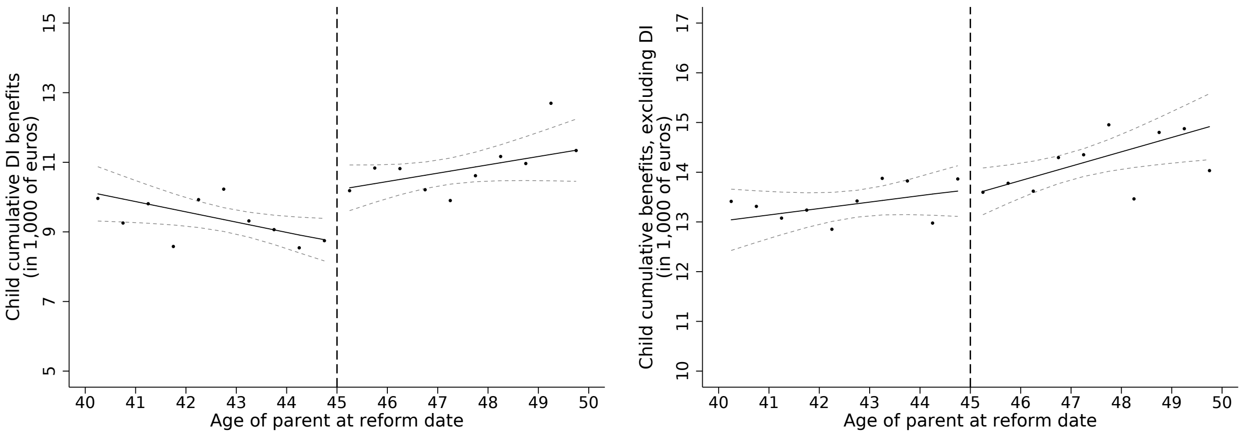


Figure 3: Child DI and Other Benefit Receipt

Notes: See Table 3 and notes to Figure 2.

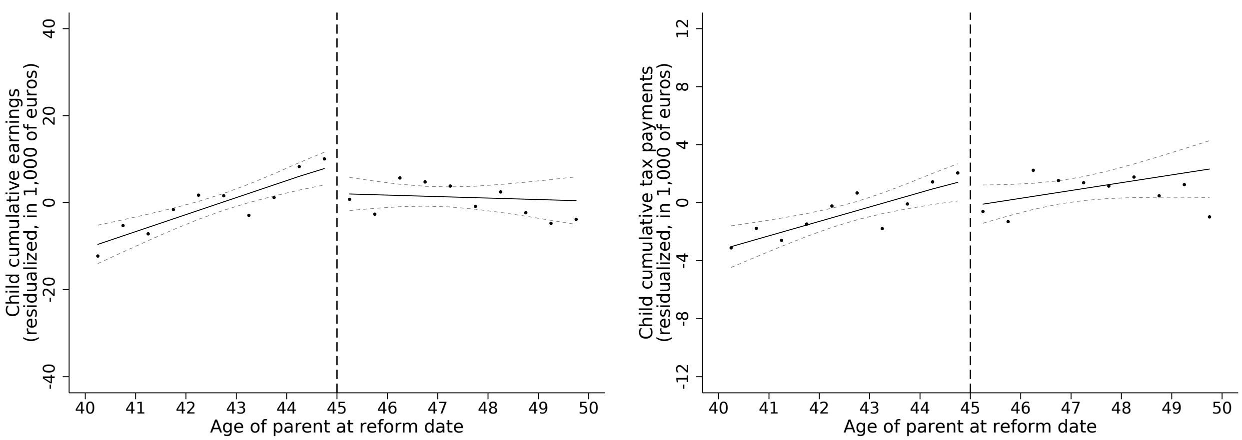


Figure 4: Residualized Child Earnings and Taxes

Notes: See Table 4 and notes to Figure 2. In these graphs, we first regress out child age to keep the range of the y-axis from being too large.

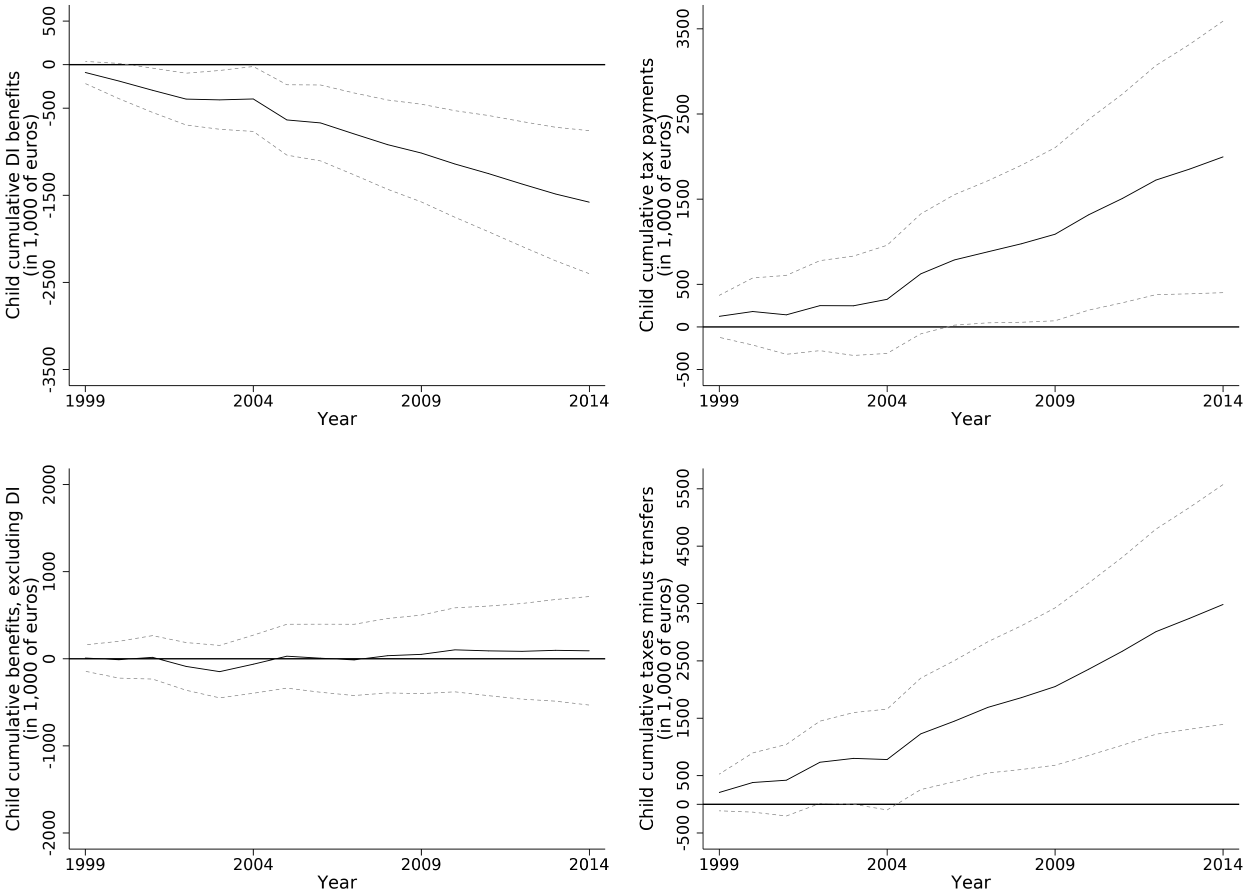


Figure 5: Cumulative Fiscal Effects Over Time for Children

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.

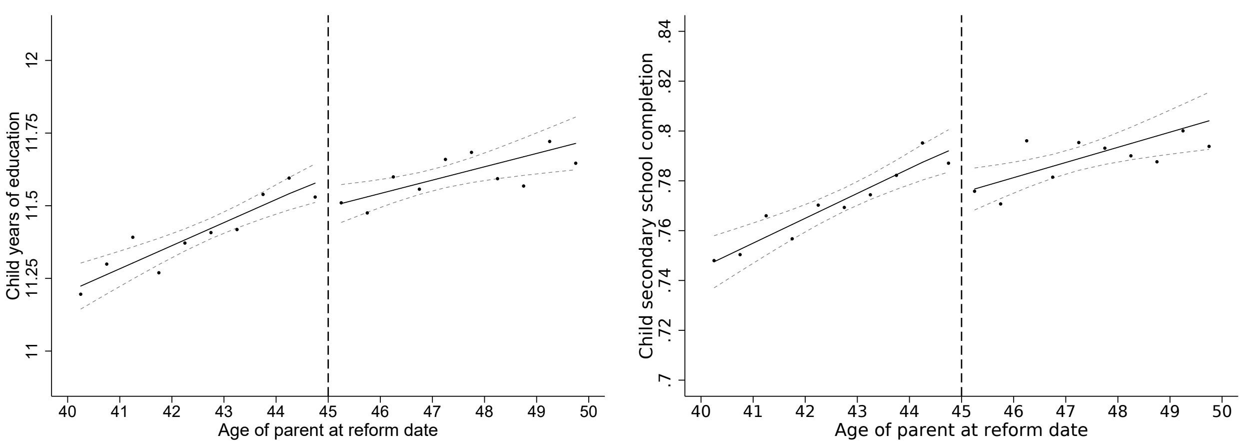


Figure 6: Child Educational Attainment

Notes: See Table 5 and notes to Figure 2.

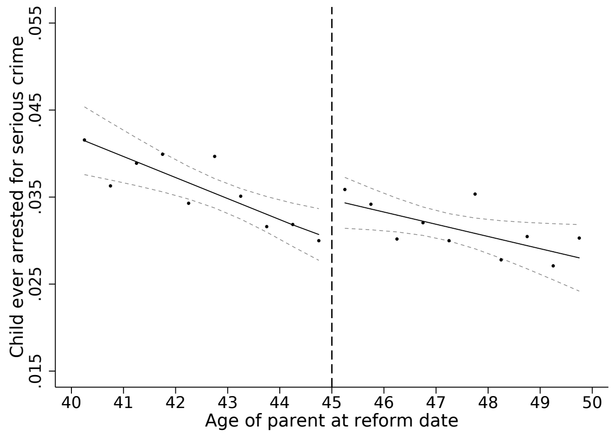


Figure 7: Child Serious Crime

Notes: See Table 6 and notes to Figure 2.

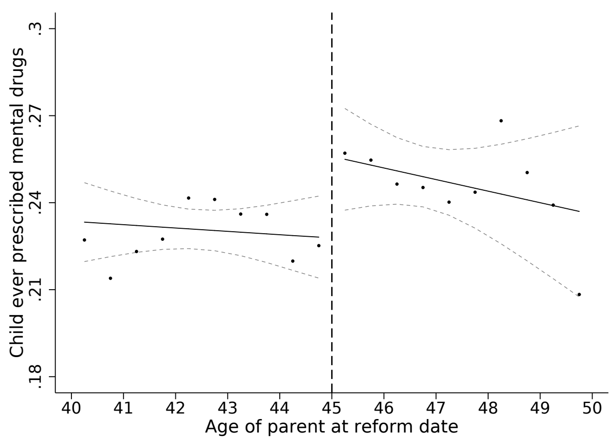


Figure 8: Child Mental Health

Notes: See Table 7 and notes to Figure 2. This graph is limited to children who were age 14 or younger at the time of implementation (November 1996).

Table 1: RD Estimates of the Reform on Parental DI

Dependent variable	First Stage	Mean
A. Parental DI benefits	-1.300**	10.063
(in 1,000 euros)	(.095)	
B. Parental exit from DI	.054**	.114
	(.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.
$^{**}p<.05$, $^{*}p<.10$

Table 2: RD Estimates of Child DI Participation

Child outcome in 2014	RF	Mean
A. Ever on DI	-.011**	.104
	(.004)	
B. Cumulative days on DI	-47.2**	298
	(13.9)	
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.
$^{**}p<.05$, $^{*}p<.10$

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

Child outcome in 2014 (in 1,000 euros)	RF	Mean
A. DI benefits		
A1. Cumulative DI income	-1.578**	10.107
	(.499)	
B. Other benefits		
B1. Cumulative total benefits, excluding DI (B2+B3+B4)	.092	13.746
	(.379)	
B2. Cumulative UI income	-.067	5.639
	(.162)	
B3. Cumulative general assistance income (traditional cash welfare)	.092	4.432
	(.266)	
B4. Cumulative misc. benefit income (all other government safety net programs)	.067	3.675
	(.145)	
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<.05, *p<.10*

Table 4: RD Estimates of Child Earnings and Taxes

Child outcome in 2014 (in 1,000 euros)	RF	Mean
A. Cumulative income from work	7.178**	371.282
	(2.836)	
B. Cumulative estimated taxes	1.997**	109.565
	(.969)	
C. Cumulative taxes minus transfers (taxes - DI benefits - other benefits)	3.483**	85.712
	(1.271)	
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<.05, *p<.10*

Table 5: RD Estimates of Child Educational Investments

Child outcome in 2014	RF	Mean
A. Years of education	.117**	11.49
	(.050)	
B. Lower secondary school or more	-.001	.95
	(.003)	
C. Upper secondary school or more	.022**	.78
	(.007)	
D. Bachelor degree or more	.017**	.33
	(.008)	
E. Master degree or more	.009*	.10
	(.005)	
F. Advanced degree or more	-.001	.01
	(.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<.05, *p<.10*

Table 6: RD Estimates of Child Crime

Child outcome in 2014	RF	Mean
A. Ever arrested		
A1. Any crime	-.0023	.1370
(A2|A3)	(.0043)	
A2. Minor crime	-.0016	.1256
(low prob of prison)	(.0042)	
A3. Serious crime	-.0054**	.0363
(high prob of prison; A4|A5)	(.0023)	
A4. Serious violent crime	-.0030**	.0090
	(.0012)	
A5. Serious non-violent crime	-.0037*	.0306
	(.0021)	
B. Ever imprisoned		
B1. Any crime	-.0029*	.0165
	(.0016)	
Observations	123,186	

Notes: See notes to Table 1; the sample differs in this table because the data covers 2005-2014 and because individuals are required to be 18 or older by 2014. Minor and serious crime are defined based on whether the crime an individual is arrested for is associated with an above or below median probability of imprisonment. See Appendix Table A4 for definition details and a listing of minor versus serious crimes. Standard errors in parentheses, clustered at the parent level.

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

Table 7: RD Estimates of Child Mental Health

	Children 14 or younger around implementation	
Child outcome in 2014	RF	Mean
A. Ever prescribed		
A1. Any mental health drug (A2|A3|A4|A5|A6)	-.026**	.234
	(.013)	
A2. Antipsychotics	-.011*	.047
	(.006)	
A3. Anxiolytics	-.014	.109
	(.009)	
A4. Hypnotics and sedatives	-.015**	.050
	(.006)	
A5. Antidepressants	-.014	.132
	(.010)	
A6. Psychostimulants	-.003	.041
	(.006)	
Observations	27,218	

Notes: See notes to Table 1; the sample differs in this table because the data covers 2005-2014 and because individuals are required to be 18 or older by 2014. Moreover, we limit the sample to children who were age 14 or younger at the time their parents were exposed to the reform in November 1996. Standard errors in parentheses, clustered at the parent level.
$^{**}p<.05$, $^{*}p<.10$

Table 8: RD Estimates by Age of Child

	Child age around implementation					
	age ≤14		age ≤18		age>19	
Child outcome in 2014	RF	Mean	RF	Mean	RF	Mean
A. Ever on DI	-.022**	.091	-.019**	.093	-.006	.114
	(.010)		(.006)		(.006)	
B. Cumulative days on DI	-66.4**	255	-64.9**	265	-36.4*	330
	(32.0)		(19.3)		(20.0)	
C. Cumulative DI income (in 1,000 euro)	-1.687*	7.979	-2.184**	8.677	-1.293*	11.480
	(1.014)		(.650)		(.747)	
D. Cum. total benefits, excl. DI (in 1,000 euro)	-.536	8.036	-.318	11.553	.332	15.851
	(.600)		(.467)		(.584)	
E. Cumulative income from work (in 1,000 euro)	5.314	189.613	4.744	290.500	7.998*	448.788
	(3.762)		(3.080)		(4.417)	
F. Cumulative estimated taxes (in 1,000 euro)	1.208	48.861	.944	80.228	2.462	137.714
	(1.042)		(.973)		(1.577)	
G. Years of education	.185*	11.33	.171**	11.57	.065	11.39
	(.102)		(.067)		(.069)	
H. Upper secondary school or more	.031**	.744	.028**	.775	.015	.783
	(.015)		(.009)		(.010)	
I. Ever arrested for serious crime	-.0066	.0590	-.0068*	.0438	-.0047	.0281
	(.0068)		(.0037)		(.0029)	
J. Ever prescribed mental drugs	-.026**	.234	-.009	.262	.008	.288
	(.013)		(.008)		(.008)	
Observations (A-F)	20,388		56,974		59,382	
Observations (G, H)	19,765		45,913		34,011	
Observations (I, J)	27,218		63,804		59,382	

Notes: See notes to Tables 1-7. 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<.05, *p<.10*

Table 9: RD Estimates by Gender of Parent and Child

	Parents				Children			
	Father		Mother		Son		Daughter	
Child outcome in 2014	RF	Mean	RF	Mean	RF	Mean	RF	Mean
A. Ever on DI	-.009**	.101	-.018**	.111	-.010**	.090	-.013**	.121
	(.004)		(.008)		(.005)		(.006)	
B. Cumulative days on DI	-38.7**	291	-74.1**	320	-41.1**	269	-54.5**	336
	(15.7)		(29.8)		(17.3)		(21.7)	
C. Cumulative DI income (in 1,000 euro)	-1.298**	9.712	-2.477**	11.339	-1.194*	9.149	-2.052**	11.347
	(.550)		(1.136)		(.619)		(.785)	
D. Cum. total benefits, excl. DI (in 1,000 euro)	.071	13.328	.143	15.051	.406	13.780	-.328	13.703
	(.426)		(.820)		(.485)		(.578)	
E. Cumulative income from work (in 1,000 euro)	4.217	368.158	17.424**	381.158	8.222**	441.648	4.800	280.250
	(3.115)		(6.547)		(4.007)		(3.533)	
F. Cumulative estimated taxes (in 1,000 euro)	1.202	107.326	4.827**	116.551	2.987**	135.808	.324	75.616
	(1.051)		(2.309)		(1.421)		(1.117)	
G. Years of education	.153**	11.36	-.001	11.90	.139**	11.26	.089	11.78
	(.057)		(.101)		(.064)		(.069)	
H. Upper secondary school or more	.024**	.768	.013	.812	.025**	.759	.017*	.802
	(.008)		(.012)		(.009)		(.009)	
I. Ever arrested for serious crime	-.0059**	.0362	-.0037	.0366	-.0080**	.0557	-.0014	.0114
	(.0027)		(.0047)		(.0038)		(.0019)	
J. Ever prescribed mental drugs	-.030**	.233	.002	.235	.005	.189	-.057**	.281
	(.014)		(.029)		(.016)		(.018)	
Observations (A-F)	88,114		28,242		65,627		50,729	
Observations (G, H)	60,072		19,852		43,739		36,185	
Observations (I)	93,557		29,629		69,066		54,120	
Observations (J)	21,261		5,957		13,853		13,365	

Notes: See notes to Tables 1-7.
$^{**}p<.05$, $^{*}p<.10$

Table 10: Placebo Tests – RD Estimates for Parents Not on DI in 1995

Child outcome in 2014	RF	Mean
A. Ever on DI	-.0002	.057
	(.0009)	
B. Cumulative days on DI	-2.126	158
	(3.232)	
C. Cumulative DI income	-.150	5.346
(in 1,000 euro)	(.117)	
D. Cumulative total benefits, excluding DI	.042	8.761
(in 1,000 euro)	(.092)	
E. Cumulative income from work	-.041	378.393
(in 1,000 euro)	(.940)	
F. Cumulative estimated taxes	-.178	110.906
(in 1,000 euro)	(.360)	
G. Years of education	.010	12.56
	(.015)	
H. Upper secondary school or more	.000	.87
	(.002)	
I. Ever arrested for serious crime	-.0007	.0233
	(.0006)	
J. Ever prescribed mental drugs	.001	.169
	(.003)	
Observations (A-F)	1,286,355	
Observations (G, H)	971,599	
Observations (I)	1,393,368	
Observations (J)	415,157	

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 1-7 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<.05, *p<.10*

Online Appendix

"Intergenerational Spillovers in Disability Insurance"

Gordon B. Dahl and Anne C. Gielen

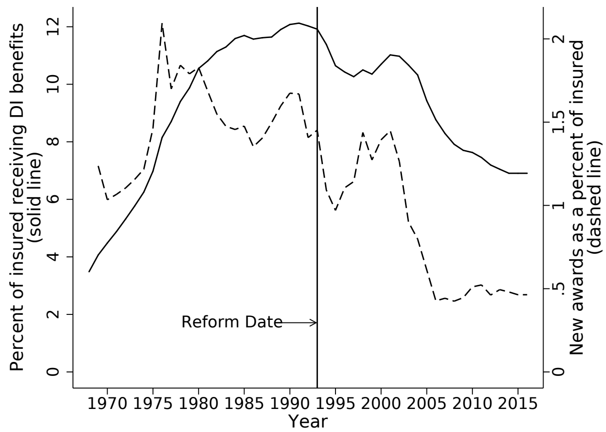
Appendix A: IV versus OLS Estimates

As a supplementary exercise, we compare OLS to similarly scaled IV estimates. To construct OLS estimates, we use 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 reexaminations 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 the effect of parental DI benefits in 1996 on each of our main child outcomes separately for each group.

To construct IV estimates, we employ our RD design and use the total drop in parental DI payments, including drops to zero, as the first stage outcome variable (see panel A in Table 1). To be valid, one must assume the exclusion restriction that there is no direct effect of exit from DI (see Section 3.2), an assumption which is unlikely to hold. With this caveat in mind, we report IV estimates to provide some type of comparison to OLS.

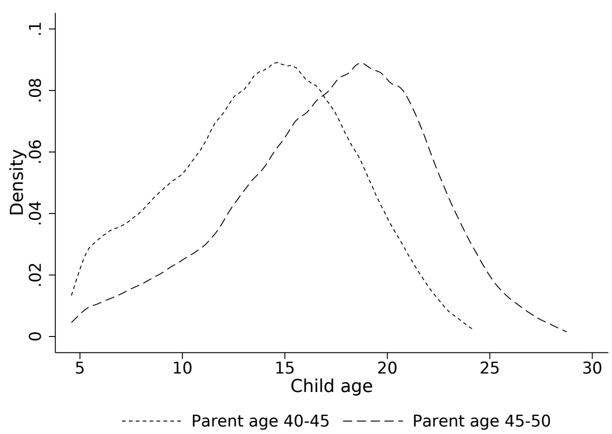
Appendix Table A6 presents the OLS and IV estimates. The OLS estimates for the younger versus older parent samples are generally quite similar, but diverge sharply from the IV estimates. Specification A uses whether the child was ever on DI by 2014 as the outcome variable. The OLS estimates imply an 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 contrasts with the larger IV estimate of 0.9 percentage points. Likewise, looking at days on DI, income from DI, earnings, taxes, education, crime, and mental health the IV estimate is roughly between 2 and 4 times larger compared to OLS. Interestingly, the OLS estimates for cumulative total benefits from other social assistance programs is large and significant, while the IV estimate is close to zero.

Why are the IV estimates substantially larger in general? There are several possible explanations. First, the mean DI participation rate is higher in the IV versus OLS samples (10.4% versus approximately 6.5%). Second, OLS could be biased due to nonrandom parental changes in DI participation and payment amounts. For example, in the OLS sample, a parent may be choosing to voluntarily exit because their health has improved or their payments may be falling because they have found part-time employment. In contrast, the IV estimates compare parents whose health conditions and job prospects are presumably similar, but whose DI payments involuntarily change due to an unexpected shock. A third reason is that the exclusion restriction could be violated as mentioned above. A final reason is that IV estimates a local average treatment effect (LATE) for compliers, while OLS estimates an average treatment effect (ATE) for the whole population. The reform reduced DI benefits for marginal participants who were deemed to have substantial work capacity. In contrast, OLS includes parents with more severe disabilities as well as parents with little attachment to the DI program. This difference is emphasized by De Haan and Schreiner (2018) when discussing how to compare intergenerational ATEs estimated using their bounding assumptions to LATEs estimated using quasi-experimental methods.



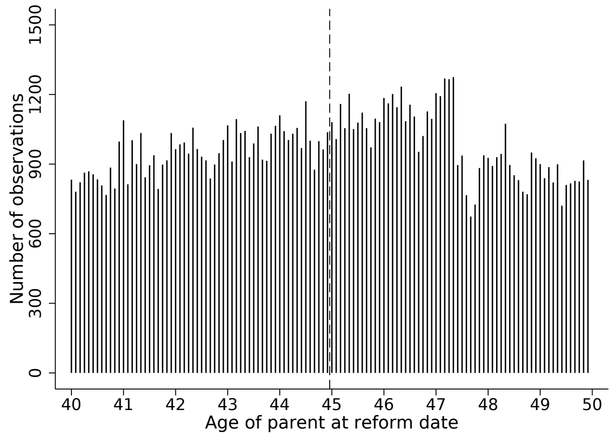
Appendix Figure A1: DI Stocks and Inflows as a Percentage of Insured Workers, 1968-2016.

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.



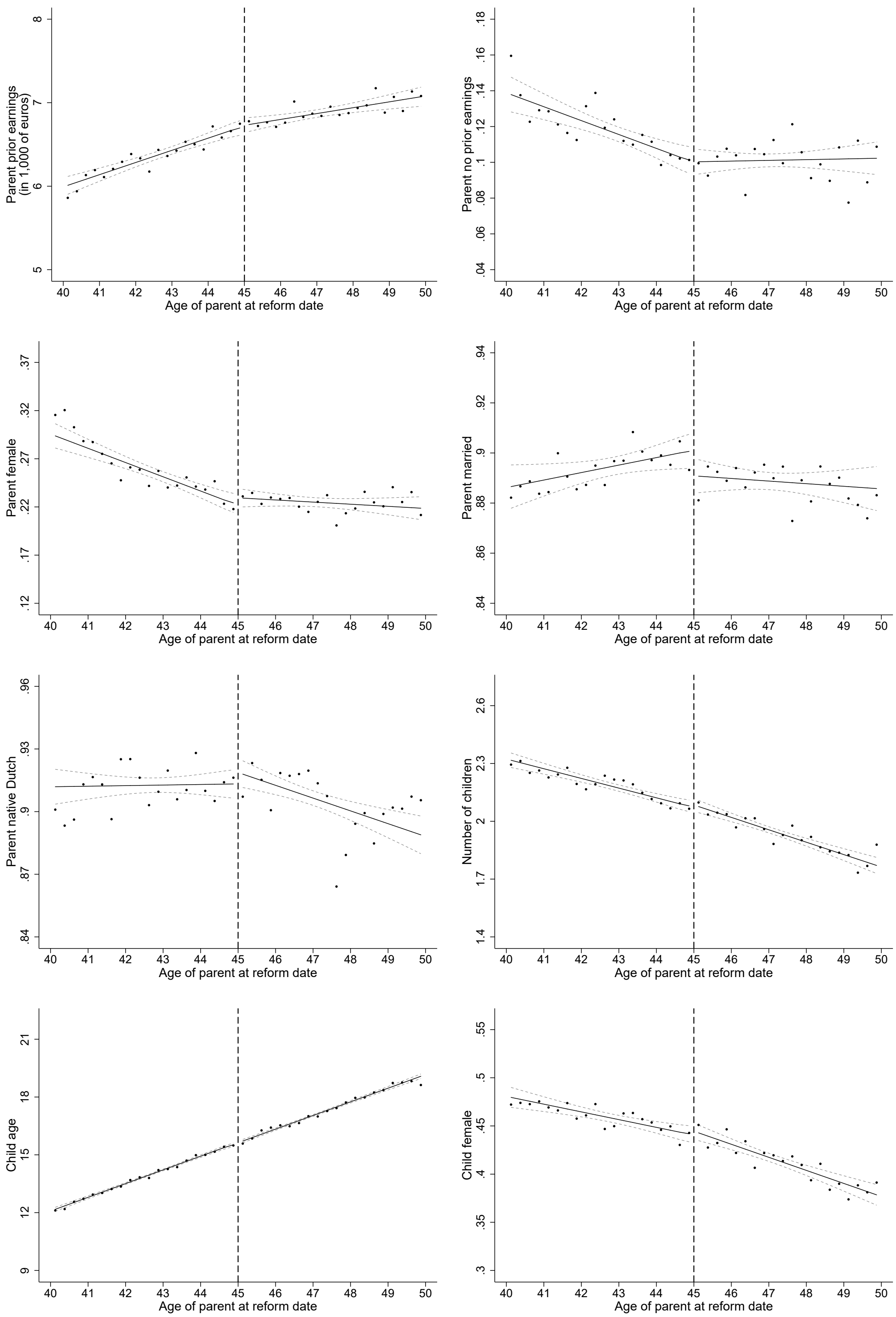
Appendix Figure A2: Child Age as of the Reform Date of August 1993

Notes: Kernel density estimates of child age, trimmed to exclude .3 percent of the data for visual clarity.



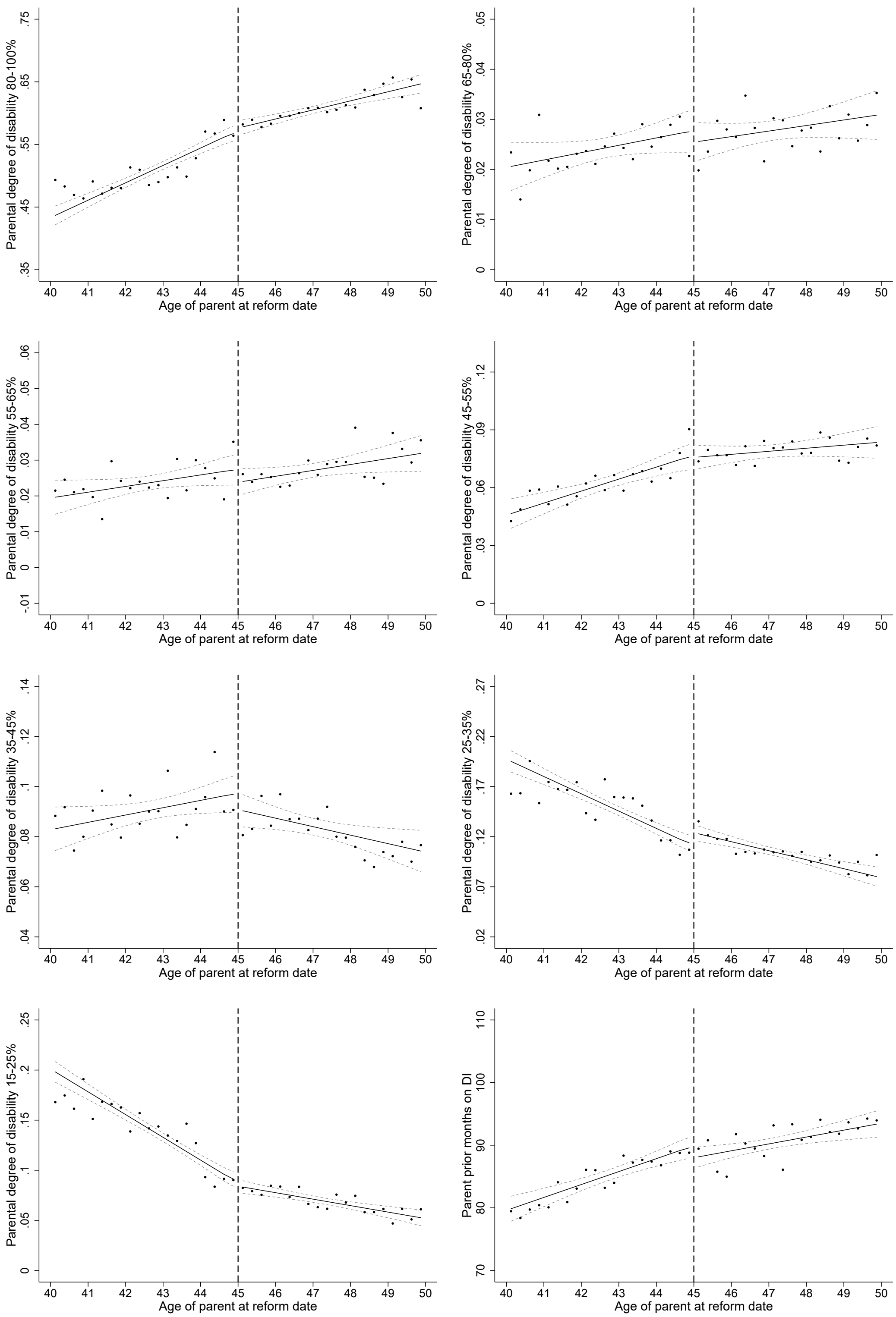
Appendix Figure A3: Number of Observations by Cohort

Notes: The McCrary density test is insignificant (discontinuity estimate=.027, s.e.=.023, p-value=0.25). The large trough and spike around ages 47 and 48 are the effects of WWII (the "hunger winter of 1944") and the subsequent baby boom.



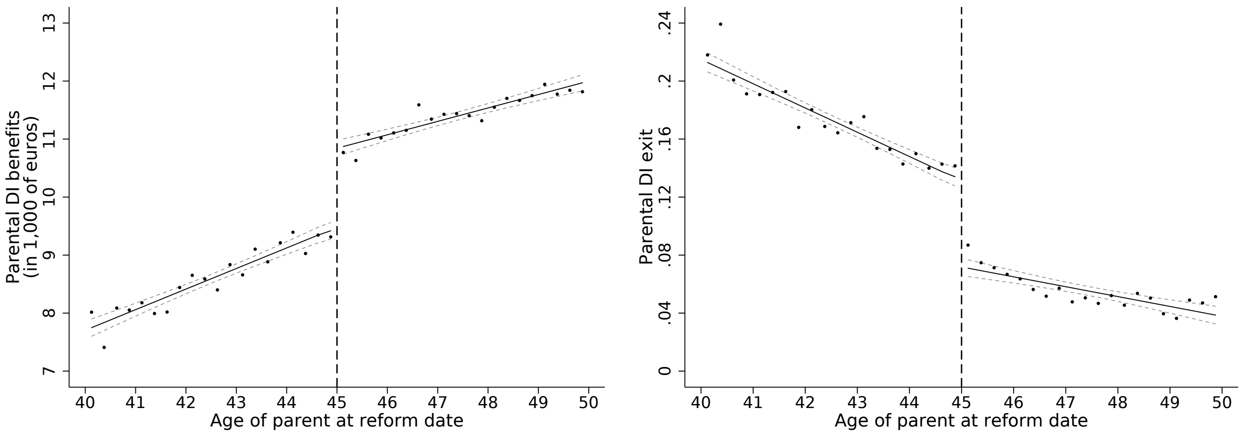
Appendix Figure A4: Covariate Balance, Parental and Child Characteristics.

Note: Graphs mirror those in Figure 2, but use pre-existing parental and child variables as the outcome variables.



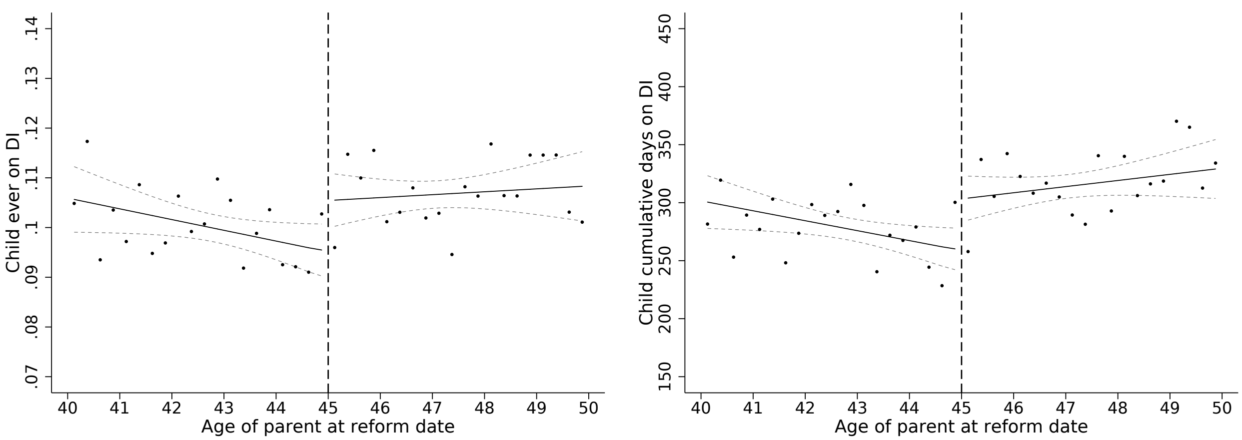
Appendix Figure A5: Covariate Balance, Parental Degree of Disability and Prior Months on DI.

Note: Graphs mirror those in Figure 2, but use pre-existing parental degree of disability bins and prior months on DI as the outcome variables.



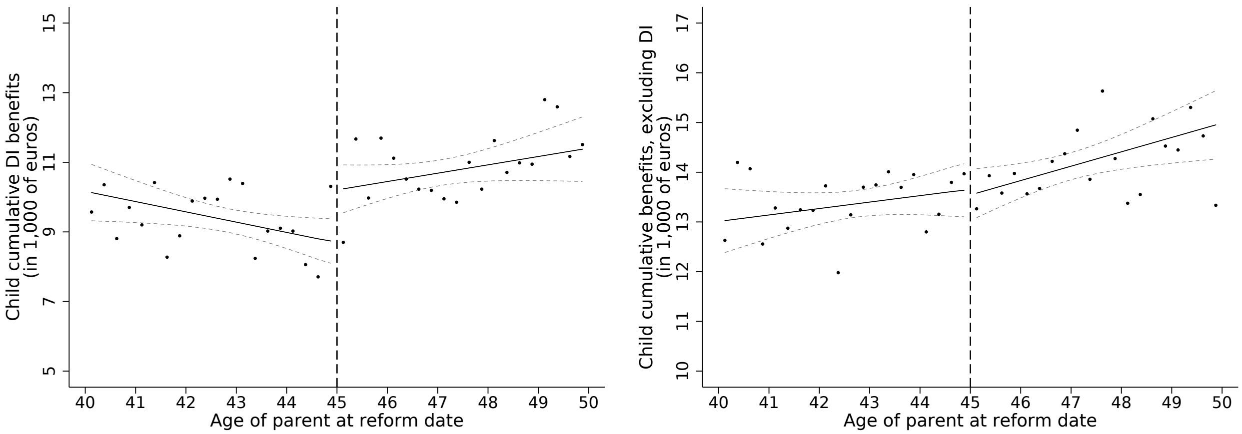
Appendix Figure A6: Effect of the Reform on Parents

Note: Graphs mirror those in Figure 1, but with 3 month age bins.



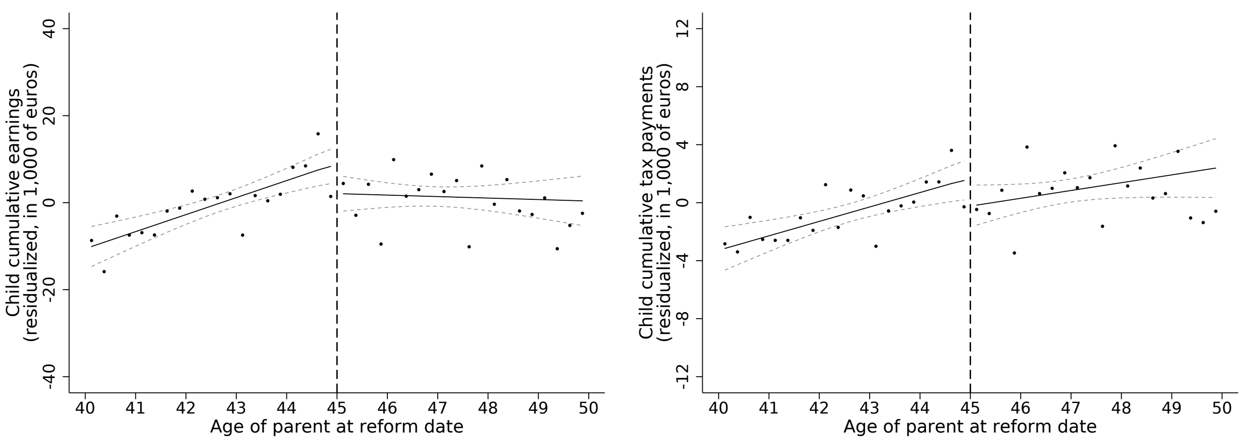
Appendix Figure A7: Child DI Participation

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



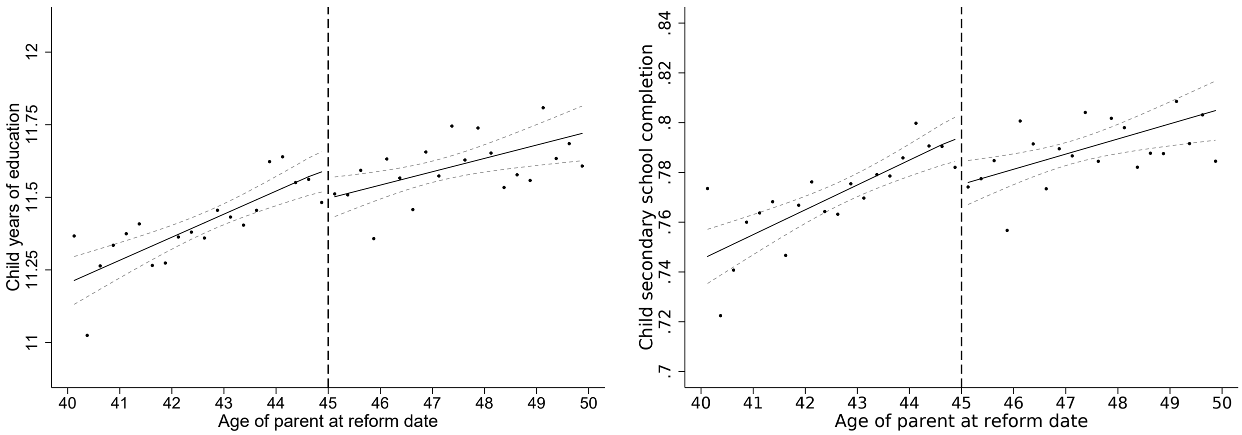
Appendix Figure A8: Child DI and Other Benefit Receipt

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



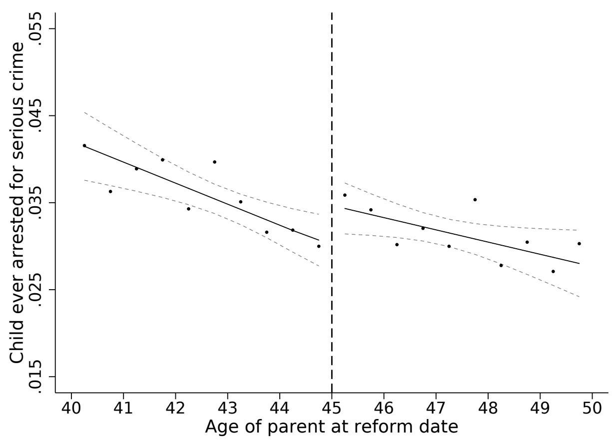
Appendix Figure A9: Residualized Child Earnings and Taxes

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



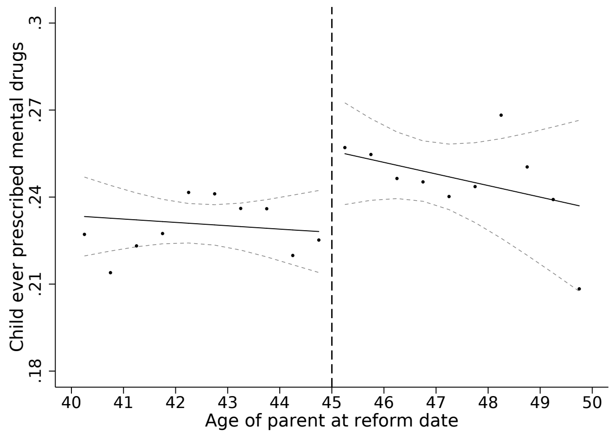
Appendix Figure A10: Child Educational Attainment

Note: Graphs mirror those in Figure 6, but with 3 month age bins.



Appendix Figure A11: Child Serious Crime

Note: Graph mirrors Figure 7, but with 3 month age bins.



Appendix Figure A12: Child Mental Health

Note: Graph mirrors Figure 8, but with 3 month age bins.

Appendix Table A1: Summary Statistics

	Overall	Parent age: 40-45	Parent age: 45-50
A. Parents			
Female	.27	.29	.26
Married	.87	.87	.87
Age (Aug 1993)	45.17	42.58	47.36
Duration DI (months)	88.38	85.20	91.08
Degree of disability			
15-25%	.10	.14	.07
25-35%	.12	.14	.10
35-45%	.08	.09	.08
45-55%	.07	.06	.08
55-65%	.02	.02	.03
65-80%	.02	.02	.03
80-100% (Full disability)	.58	.53	.63
Pre-DI earnings (euros)	6,529	6,249	6,766
Native Dutch	.91	.91	.91
Number of kids in HH	1.71	1.87	1.58
Parent observations	70,319	32,279	38,040
B. Children			
Female	.44	.46	.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.

Appendix Table A2: Transition Matrices in Parental Disability Rating

A. Parent age: 40-45

		Disability rating in 1999 (post re-examinations)								
		Exit DI	15-25%	25-35%	35-45%	45-55%	55-65%	65-80%	80-100%	All
	15-25%	4.7	8.0	.3	.1	.1	.0	.0	.8	14.1
	25-35%	3.3	.8	9.2	.5	.1	.1	.1	1.1	15.0
Disability rating	35-45%	1.3	.3	.8	5.5	.3	.1	.1	.7	9.1
in 1996	45-55%	1.0	.1	.2	.4	3.7	.1	.1	.6	6.2
(pre re-examinations)	55-65%	.3	.1	.0	.1	.1	1.4	.1	.3	2.4
	65-80%	.2	.0	.1	.1	.1	.1	1.6	.4	2.4
	80-100%	6.7	.5	.7	.5	.4	.3	.4	41.3	50.9
	All	17.4	9.8	11.4	7.2	4.7	2.0	2.4	45.1	100.0

B. Parent Age: 45-50

		Disability rating in 1999 (post re-examinations)								
		Exit DI	15-25%	25-35%	35-45%	45-55%	55-65%	65-80%	80-100%	All
	15-25%	1.1	4.5	.3	.1	.1	.0	.0	.7	6.9
	25-35%	1.0	.4	7.1	.5	.2	.1	.1	1.0	10.5
Disability rating	35-45%	.5	.1	.3	5.9	.2	.1	.1	1.0	8.2
in 1996	45-55%	.4	.0	.1	.3	5.8	.2	.1	1.0	7.9
(pre re-examinations)	55-65%	.2	.0	.0	.1	.1	2.0	.1	.3	2.8
	65-80%	.1	.0	.0	.0	.0	.1	2.1	.5	2.8
	80-100%	2.3	.1	.2	.2	.2	.2	.3	57.3	60.9
	All	5.7	5.3	8.1	7.1	6.8	2.6	2.8	61.7	100.0

Notes: The sample is as described in Appendix Table A1. "Exit DI" captures both voluntary exit and being forced off of DI.

Appendix Table A3: RD Estimates for Covariate Balance

	Treatment dummy: age<45	Mean
A. Parents		
Female	-.007 (.007)	.27
Married	.010** (.005)	.87
Duration DI (months)	1.778 (1.215)	88.38
Degree of disability		
15-25%	.005 (.005)	.10
25-35%	-.012** (.006)	.12
35-45%	.007 (.005)	.08
45-55%	.001 (.005)	.07
55-65%	.004 (.003)	.02
65-80%	.002 (.003)	.02
80-100% (Full disability)	-.006 (.009)	.58
Pre-DI earnings (euros)	-17.205 (63.779)	6,529
No pre-DI earnings	.0001 (.005)	.01
Native Dutch	-.005 (.005)	.91
Number of kids in HH	-.008 (.023)	1.71
Parent observations	70,319	
B. Children		
Female	-.003 (.006)	.44
Age (Aug 1993)	-.044 (.066)	15.60
Child observations	116,356	
Joint F-test [p-value]	0.69 [.202]	

Notes: Each row is a separate RD regression, and uses the same sample and specification as Table 2, except that each regression includes no additional covariates besides the running variable. Standard errors in parentheses, clustered at the parent level.
$**p<.05$, $*p<.10$

Appendix Table A4: Serious and Minor Crimes by Arrest Category

A. Serious crimes

Mugging, Theft of a car, Theft of a motorcycle, Extortion, Burglary in a school, Theft of items from a car, Burglary in a shed/garage, Murder, Vandalism of a public building, Rape, Burglary in a sports complex, Burglary in a residence, Theft of a bicycle, Pickpocketing, Commerical theft, Trespassing, Arson, Possession of stolen goods, Fraud, Assault, Other public disturbance or trespass, Gun offense, Violation of court order

B. Minor crimes

Cybercrime, Drunk driving, Sexual acts with a minor, Other traffic violation, Leaving the scene of an accident, Disorderly conduct, Miscellaneous civil offense, Public indecency, Maltreatment, Stalking, Other sexual offense, Drug offense, Vandalism of a car, Miscellaneous criminal offense, Disrespecting public authority, Other violent offense, Other financial crime, Driving with a suspended license, Forgery, Shoplifting, Other theft or burglary, Kidnapping, Other Vandalism, Threats

Notes: These are translations of the 48 arrest categories used in the Standard Crime Classification of Statistics Netherlands. To categorize serious versus minor crime, we take everyone who was arrested for a specific crime in 2014 (e.g., assault) and then calculate the probability that these individuals are incarcerated in 2014 for any reason. Serious and minor crimes are defined as an incarceration probability above or below the median across the 48 arrest categories, respectively.

Appendix Table A5: 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	Serious crime	Mental drugs
A. Baseline	-.011**	-47.151**	-1.579**	.092	7.178**	1.997**	.117**	.022**	-.0055**	-.026**
	(.004)	(13.921)	(.499)	(.379)	(2.836)	(.969)	(.050)	(.007)	(.0023)	(.013)
B. Quadratic trends	-.016**	-57.876**	-2.076**	.346	10.469**	3.120**	.130**	.026**	-.0075**	-.046**
	(.006)	(20.334)	(.729)	(.562)	(4.165)	(1.415)	(.073)	(.010)	(.0034)	(.018)
C. No triangular weights	-.010**	-36.897**	-1.175**	.021	5.282**	1.313	.099**	.018**	-.0042**	-.020**
	(.004)	(13.054)	(.469)	(.354)	(2.642)	(.908)	(.046)	(.006)	(.0022)	(.012)
D. No control variables	-.010**	-44.400**	-1.515**	.098	5.877**	1.548	.100**	.019**	-.0049**	-.028**
	(.004)	(13.822)	(.497)	(.385)	(3.242)	(1.117)	(.052)	(.007)	(.0024)	(.013)
E. 45 month window	-.012**	-52.179**	-1.776**	.128	8.028**	2.248**	.120**	.022**	-.0058**	-.031**
	(.004)	(15.094)	(.541)	(.411)	(3.084)	(1.051)	(.054)	(.007)	(.0025)	(.014)
F. 30 month window	-.018**	-69.737**	-2.440**	.264	11.077**	2.941**	.142**	.026**	-.0080**	-.044**
	(.005)	(18.142)	(.651)	(.495)	(3.684)	(1.254)	(.065)	(.009)	(.0031)	(.016)
G. Local linear regression	-.010**	-37.323**	-1.227**	-.019	3.923	.676	.076	.015**	-.0039**	-.022*
bandwidth 60 months	(.004)	(12.737)	(.459)	(.345)	(3.040)	(1.029)	(.048)	(.006)	(.0022)	(.011)
H. Local linear regression	-.010**	-46.517**	-1.589	.954	6.116*	1.641	.091*	.018**	-.0048*	-.027**
bandwidth 45 months	(.004)	(14.526)	(.543)	(.412)	(3.424)	(1.196)	(.055)	(.007)	(.0025)	(.013)
I. Local linear regression	-.016**	-67.382**	-2.338**	.308	8.993**	2.298	.147**	.025**	-.0075**	-.044*
bandwidth 30 months	(.005)	(17.858)	(.629)	(.501)	(4.161)	(1.416)	(.067)	(.009)	(.0031)	(.016)
J. Sample of children not	-.007	-17.811	-.333	-.200	6.038	1.665	.122**	.013	.0035	-.006
living at home	(.006)	(20.977)	(.812)	(.712)	(4.179)	(1.470)	(.066)	(.009)	(.0031)	(.007)
K. Cluster s.e.'s	-.011**	-47.151**	-1.578**	.091	7.178**	1.997**	.117**	.022**	-.0054**	-.026**
by parental age	(.003)	(13.007)	(.513)	(.332)	(2.576)	(.792)	(.043)	(.005)	(.0019)	(.011)
L. Excluding non-natives	-.011**	-42.131**	-1.375**	.034	8.126**	2.229**	.124**	.023**	-.0040*	-.033**
	(.004)	(14.629)	(.521)	(.387)	(2.959)	(1.019)	(.052)	(.007)	(.0023)	(.014)
M. Excluding children whose	-.012**	-49.767**	-1.694**	.014	7.172**	1.837**	.126**	.022**	-.0059**	-.026**
parents left DI in 1995	(.004)	(14.144)	(.506)	(.384)	(2.866)	(.977)	(.050)	(.007)	(.0024)	(.013)

Notes: See notes to Tables 1-7. Standard errors in parentheses, clustered at the parent level.
***p<.05, *p<.10*

Appendix Table A6: OLS versus IV Estimates

	Independent variable: Parental DI payments in 1996 (in 1,000 euros)		
	OLS		IV
	Parent age:		Parent age:
Child outcome in 2014	40-45	45-50	40-50
A. Ever on DI	.003**	.003**	.009**
	(.000)	(.000)	(.004)
B. Cumulative days on DI	8.6**	8.4**	37.5**
	(.3)	(.3)	(14.6)
C. Cumulative DI income (in 1,000 euro)	.293**	.289**	1.256**
	(.009)	(.010)	(.522)
D. Cumulative total benefits, excl. DI (in 1,000 euro)	.230**	.235**	-.073
	(.007)	(.008)	(.388)
E. Cumulative income from work (in 1,000 euro)	-2.383**	-2.964**	-5.711*
	(.062)	(.080)	(2.951)
F. Cumulative estimated taxes (in 1,000 euro)	-.668**	-.933**	-1.589
	(.023)	(.032)	(1.008)
G. Years of education	-.052**	-.050**	-.096**
	(.001)	(.001)	(.050)
H. Upper secondary school or more	-.005**	-.005**	-.018**
	(.000)	(.000)	(.007)
I. Ever arrested for serious crime	.0009**	.0009**	.0042*
	(.0000)	(.0000)	(.0024)
J. Ever prescribed mental drugs	.004**	.004**	.019*
	(.000)	(.000)	(.011)

Notes: OLS samples include children still living at home, regardless of whether the parent was on DI as of the reform date. The OLS samples are split into two parental age groups to ensure the stricter DI rules for those parents under versus over the age 45 cutoff do not contribute to the estimates. See notes to Table 1 for a list of control variables. Samples sizes for the three columns, in order, are: A-F 498,378; 421,731; 116,356, G, H 387,264; 287,799; 79,924, I 923,119; 612,885; 123,186, J 368,372; 85,202; 27,218. The IV estimates scale the RF estimates using the drop in DI payments, assuming exit itself has no effect; see Tables 1-7. Standard errors in parentheses, clustered at the parent level.

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