Preliminary Draft

Intergenerational Spillovers in Disability Insurance*

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Abstract: Does participation in a social program by a parent influence their child's use of public assistance, human capital investments, future labor market, and marriage outcomes? From a policy perspective, what a child learns from his or her parents about employment relative to government support could matter for the financial stability of a variety of social insurance and safety net programs. However, estimating a causal effect is difficult due to parent's nonrandom participation. In this paper we exploit a disability insurance (DI) reform in the Netherlands which tightened eligibility criteria and reduced the generosity of the program. The key to our regression discontinuity design is that the reform applied to younger cohorts, while older cohorts were exempted from the new rules. We find strong evidence that children of parents who were pushed out of DI or had their benefits reduced are affected positively on a variety of dimensions. 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, invest significantly more in their education, increase their earnings, and are more likely to marry/cohabit and have a child. Our results have important implications for the evaluation of the costs and benefits of this and other policy reforms; indeed, ignoring the spillover effects of lower government transfers and increased taxes paid by the next generation greatly underestimates the cost savings of the Dutch DI reform in the long run.

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1 Introduction

Does participation in a social program by a parent influence their child's own social program participation, human capital investments, future employment and earnings, and marriage market? Evaluating the intergenerational effects of income support programs is difficult because a parent's participation is not randomly assigned. On the one hand, observing a parent out of the labor force and on public assistance could alter a child's perceptions about the relative costs, benefits, and stigma associated with the two alternatives. There could also be information transmission or differential investment as a result of having a parent receive government transfers. On the other hand, characteristics like poor health or reduced opportunities could be correlated across generations, creating mechanical intergenerational links which do not reflect a behavioral response of the child.

Causal identification of intergenerational effects in program participation is a difficult empirical problem. Simple intergenerational correlations are unlikely to capture a causal effect, as unobservable characteristics which are correlated across generations or influence a family's environment are likley to bias the estimates. Credible identification requires an exogenous shock which affects parents' participation, but does not directly affect their children. On top of this, estimation requires a panel dataset which links parents to children, contains detailed set of outcome variables, and follows familes over a long period of time. This paper overcomes these challenges with a quasi-experimental setting, and provides causal estimates for a variety of intergenerational spillovers due to a parent's program participation.

From a policy perspective, what children learn from their parents about employment versus governmental assistance could matter for the financial stability of a variety of social insurance and safety net programs. Our setting is disability insurance (DI) in the Netherlands and a 1993 reform which was prompted by the rising costs of the Dutch system. In 1970, around 2.5% of the Dutch working age population participated in disability insurance, but by the late 1980s, participation had risen to 12%. At its peak, the program cost more than 4 percent of GDP, and was not fiscally sustainable. Similar trends, while not as dramatic, have occured in most industrialized nations, including the U.S., the U.K., and other countries in Europe (see Burkhauser, Daly, McVicar, and

Wilkins, 2013).

The 1993 reform we study simultaneously tightened eligibility criteria and lowered payment generosity. It forced current DI recipients to be re-examined by a medical doctor and subjected to a new set of rules which unambiguously made them (weakly) worse off. Some individuals recieved lower payments and other were disqualified from the program entirely. Importantly, the more stringent rules only applied to individuals less than age 45 as of August 1, 1993, since it was decided that individuals age 45 or older would be grandfathered in under the old program rules. This differential application of the new rules creates an age discontinuity in program eligibility; individuals around the cutoff should be similar in all dimensions except for the differential stringency in DI re-examinations.

Using a regression discontinuity (RD) design, Borghans, Gielen, and Luttmer (2014) find that approximately 4 percent of DI participants exited DI due to the more stringent rules and that annual benefits fell by around 1,000 euro, or 10 percent. We find slightly larger effects for a sample of parents with children still living at home at the time of the reform, with 5 percent of parents exiting DI due to the reform and annual benefits dropping by a little over 1,300 euro on average. Borghans et al's analysis also reveals a strong rebound in labor earnings of 0.62 euros on average per euro of lost DI benefits and a .30 euro increase from other social assistance programs in the short run.

The goal of our paper is to explore how the parental reduction in DI benefits, with its resulting changes in employment and social support substitution, affects children's future participation in DI and other social programs themselves, human capital investments, labor market outcomes, and marriage/cohabitation. It is important to note the DI rule changes affected parents on both the intensive and extensive margins: some parents had their DI payments reduced while others were kicked off the program entirely. We therefore present both reduced form effects of the DI reform on child's outcomes and well as IV estimates which scale these effects by the drop in DI payments (treating exit from the program as a reduction in payments to 0). We use an RD design, where the running variable in the reduced form is the age of the parent and the dependent variables are various outcomes for their children.

We begin by looking at the DI participation of children as adults. We find an increase in children's DI participation in both the medium and long run. Children whose parents exit DI or have their benefits reduced are 1.7 percentage points less likely to have ever participated in DI 6 years after the reform (in 1999) and 1.3 percentage points less likely 19 years after the reform (in 2012). Thinking about cumulative usage of the program, children are on DI 43 fewer days by 2012 if they had a parent subject to the new rules. Using cumulative income recieved from DI as the dependent variable instead, children recieved roughly 1,400 euros less in DI payments (including zeros), which is sizable relative to the overall mean of 8,300 euros in cumulative DI receipt.

This first finding documents a significant and causal link in DI usage by parent and child. But to get a fuller picture of intergenerational spillovers and fiscal impacts, it is important to estimate whether a child's earnings and participation in other social support programs also change. Only with this information can one calculate the spillover effect on the government's budget net of taxes and transfers for children of affected parents. We find that cumulative taxable earnings rise by approximately 6,000 euros (a little less than 2%) for children of parents subject to the less generous DI rules, with over half of this increase coming through higher self-employment income. In contrast, we find almost no change in cumulative unemployment insurance, general assistance (i.e., traditional cash welfare), and all other miscellaneous transfer programs. The estimated increase in taxes, net of transfers and including DI, is approximately 3,300 euros per child. It is important to note that while 1,400 euros of this amount is due to cost savings from lower DI payments, the remaining 1,900 is due to increased tax revenue resulting from higher earnings.

We next turn to children's educational investments. When a parent is forced off of DI or has their benefits reduced, their child invests in a statistically and economically significant 0.13 extra years of education. The largest increase occurs for the margin of graduation from upper secondary school (the equivalent of High School), although parental DI exit also raises the probability of attending college. Interestingly, the increase in children's education seems to be driven almost entirely by fathers, with mothers having little effect. Using a rate of return to schooling of 8% per year, the extra schooling induced by the reform can account for roughly half the induced increase in cumulative earnings.

Finally, we explore a few other outcomes which could be affected by a parent's DI participation.

Examining marriage and fertililty choices, we find that at least for daughter, the probabilities of marriage/cohabitation and having a child go up if their parent was exposed to the new, less generous DI rules. Looking at the criminal activity of children, we find mostly negative, but statistically insignificant effects due to a parent being subject to the new rules.

Taken together, these results suggest that children respond strongly when a parent exits DI or has their benefits reduced. Understanding the Dutch context is key for interpreting these intergenerational effects. Parental leisure decreased and work hours increased substantially, with total parental income changing little in the short run but declining in the long run. Less parental supervision due to increased work hours or lower income in the long run could both hurt children, but at least for measurable outcomes, this is not the case. Instead, the main findings all point to a greater focus by children on formal employment: children increase their education and labor force participation, decrease their DI use, and reduce their criminal activity. This is consistent with forward-looking children anticipating they will rely relatively less on government assistance in the future. When we break down our results by the gender of the parents and children, we find that both mothers and fathers have an important influence on their children, but along different dimensions.

Understanding the intergenerational effects of social program participation is important for policy, but to date, there is remarkably little empirical evidence about causal effects. As summarized by Black and Devereux (2011) in the Handbook of Labor Economics, "while the intergenerational correlations in welfare receipt are clear, there is much less evidence that a causal relationship exists." One exception is a recent paper by Dahl, Kostol, and Mogstad (DKM, 2014), which uses a random judge assignment design and finds that DI participation by parents in Norway increases the chances their child will participate as well. While that paper presents some of the first causal evidence of intergenerational participation in a social program, more work on this important question is needed.

Relative to DKM's paper, we make several contributions towards a better understanding of intergenerational spillovers in program participation. First, DKM focus on take up of DI across generations, while we explore a broader set of labor market, public assistance, human capital investment, and demographic outcomes. This is mostly because they cannot estimate spillover effects on education or employment with enough precision; our estimation sample is 8 times larger and uses a large shock to parents. Our second contribution is related to the first. We are able to calculate the net effect to the government budget due to intergenerational spillovers, including changes in taxes and transfers, and not just DI payments. This is empirical relevant, as the net effect is more than twice as large as the intergenerational savings based on reduced DI payments alone. Third, we are able to follow children for a longer time period, including older ages when DI use is more common among the children.[1] Third, we document interesting patterns by gender of the parents and child, which is possible due to our relatively large sample size. Fourth, we examine the consequences of pushing individuals off of DI or reducing their benefits, whereas DKM study the effects of making it harder to get onto DI in the first place. There is no a priori reason to expect the intergenerational effects will be the same for the entry and exit margins, and any differences matter when considering different public policy reforms. And finally, we use a completely different, but equally compelling, quasi-experimental research design for this important, but largely unanswered, question.

Our study also complements a related literature which looks at shocks to parents which have the potential to change children's long-run outcomes. Oreopoulos, Page and Stevens (2008) find that after a parental job loss due to a firm closure, children have lower earnings and higher UI and social assistance participation. Likewise, Rege, Telle, and Votruba (2011) find that children perform worse in school after a job loss due to a plant closure, particularly if the father was affected. Stevens and Schaller (2011) find similar effects using observational data on job loss. A series of papers look at the effects of income shocks, such as those due to child tax benefits (Dahl and Lochner, 2012; Milligan and Stabile, 2011) or DI changes (Chen, Osberg, and Phipps, 2015) and generally find increases in academic achievement and child well-being. A different type of shock is having a parent move to a better neighborhood. Analyses of adults and older youth from the Moving to Opportunity (MTO) experiment find no effect on earnings or employment (e.g., Katz, Kling, and Liebman 2001; Oreopoulos 2003). However, Chetty, Hendren, and Katz (forthcoming) find large effects from this experiment for young children at the time of the move, and conclude that better neighborhoods have

[1] In DKM, cumulative DI participation is 3 percent on average for their baseline estimates. In contrast, cumulative DI participation is just under 10 percent for our baseline estimates.

the potential to reduce the intergenerational persistence of poverty.[2]

The remainder of the paper proceeds as follows. The next section provides background on disability insurance in the Netherlands, the 1993 reform, and our data. Section 3 lays out our RD design and discusses threats to identification. In Section 4, we present our main results and several heterogeneity and robustness findings. Section 5 interprets our findings and simulates the long-run effects of the reform. Section 6 concludes.

2 Background and Data

2.1 Disability Insurance in the Netherlands

The modern Dutch DI program was created in 1967 by merging two existing programs covering workplace-induced injuries and disabilities unrelated to employment. The program was generous compared to other countries, as it covered all workers with no waiting period, replaced up to 80 percent of wages, and included a variety of subjective illnesses. Moreover, sickness benefits replaced a worker's wages between 80 and 100% during the transition to disability insurance, and workers on sickness benefits for a full year were routinely transferred to the DI program without a serious reappraisal of their disability (Kalwij, de Vos, and Kapteyn, 2014). These factors fueled a rapid rise in DI recipients, from 4 percent participation of the eligible population in 1967 to over 8 percent 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% of eligibles in the late 1980s, with payments ballooning to 4.2% of gross domestic product at the peak.

Starting in the 1990s, a series of reforms were implemented to control the spiraling costs of the Dutch DI system, including reductions in benefit levels, tightened eligibility criteria, changes to the sickness benefit program, and increased financing and responsibility transfered to individual employers. The cumulative effect of these reforms was that by 2012 the participation rate had fallen

[2] There is also a related literature on disability insurance programs and their labor supply effects. See Autor and Duggan (2006), Autor (2011), Bound (1989), Bound and Burkhauser (1999), Chen and van der Klaauw (2008), Campolieti and Riddell (2012), De Jong, Lindeboom, and van der Klaauw (2011), French and Song (2014), Gruber (2000), Gruber and Kubik (1997), Kostol and Mogstad (2014), Maestas, Mullen, Singleton (2012), Strand (2013), and von Wachter, Song, and Manchester (2011). Most of these studies find sizable labor supply responses to DI benefit generosity.

to just over 7% of the eligible population. Going forward, the participation rate is predicted to fall even further as the stock of older recipients transitions out of the DI program and on to the retirement pension program. The trends over time are documented in Figure 1 and discussed in more detail by Koning and Lindeboom (2015).

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

Before continuing, we note several differences between the 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 a binary variable. Second, health insurance and other benefits are unrelated to DI receipt in the Netherlands, but directly linked to DI receipt 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; Duggan, 2003).

2.2 1993 Reform

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

In the Netherlands, individuals receive DI payments based on the degree of their disability, which is based on the calculated income loss due to a disability. Calculated income loss is determined by comparing pre-disability earnings to a constructed measure of "earnings capacity." The reform that we exploit affected the calculation of this "earnings capacity," making it less generous to DI claimants.

The degree of disability is denoted in 8 categories; which category an individual belongs to is determined by the ratio of pre-disability earnings minus earnings capacity to pre-disability earnings. Individuals can continue to work and earn up to their remaining earnings capacity (pre-disability earnings minus earnings capacity) after a disability, and at the same time receive DI payments for the fraction of lost earnings.[3]

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.[4] 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 percent of prior earnings.[5]

The 1993 reform altered this process in two ways. First, it mandated the doctor create a list of work activities based on an 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,

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

[4] If there are not initially 5 suitable occupations within an applicants narrow region, the region is expanded.

[5] 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%.

and (iii) the geographic region of 10 active workers was expanded to be three times larger. Each of these changes weakly reduce the degree of disability for an applicant compared to the old criteria, as remaining earnings capacity can only rise. Moreover, the new rules make it more likely that enough suitable occupations can be found, reducing the chances of total disability. The end result is that fewer individuals qualify for DI and benefit levels are weakly reduced for those who continue to qualify, as long their disability has not gotten worse since their last re-examination.

A feature of the 1993 reform is that it specified all individuals age 50 or older at the time of the reform would be subject to the old rules and not re-examined at all. For individuals below age 50 as of January 8, 1993, the new rules affected both new applicants and existing DI participants. Since it was not logistically feasible to re-examine all DI participants immediately, they were scheduled to be re-examined over the ensuing years based on their age cohort, starting with the youngest cohorts under the age of 35 on August 1, 1993. The 35 to 40 year old cohort was scheduled to be re-examined in 1995, 41-44 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 2012. The records include information on the start and end dates of a spell, the binned disability rating, DI payments received, pre-disability earnings, and the reason a spell ends. It does 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 2012. We use data from Statistics Netherlands for earnings, self-employment, and unemployment insurance which they compiled using information from three different tax and social insurance record sources. This data starts in 1999, which is why we start our empirical analysis of parental and child outcomes with 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 about 30 miscellaneous benefit programs, most of which are small in terms of benefit amounts or the eligible population.

We further merge in educational attainment as of 2012, as well as family structure in 2012. The education data is complete for younger cohorts, but incomplete for the older cohorts. Crime data on arrests and incarcerations come from two different data sources, and both span 2005-2012. These data contain information about the type of crime committed. Finally, we use municipal registry files for basic demographics for all individuals, including birthdate, marital status, number of children, country of birth, and place of residence. One advantage of this rich dataset merged from several sources is that we can study a variety of spillover effects across generations. Further details on these variables (except for education and crime) and how they are measured can be found in Appendix B of Borghans, Gielen, and Luttmer (2014).

We limit our sample to children of parents who were between the ages of 40 and 50 as of the reform date of August 1, 1993. We also require the child to still be living at home on August 1, 1993; as an extra specification, we estimate effects for children not living at home at the time of the reform. Parents with multiple children appear more than once in the sample, while children with two parents on DI are dropped from the sample. Due to data availability, our sample is limited to parents who were receiving DI benefits on August 1, 1993 and who were were still on DI as of January 1, 1996 (the earliest date for which we have DI records). It is important to realize that even though we do not have DI records before 1996, this should not create any biases. The reason is that 1996 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 1996 changes the interpretation of our estimates, but not the validity. After imposing all of these restrictions, we have a sample of 101,125 child observations. For the education analyses, our sample is smaller (N=65,208) since education was not collected for all but a small sample of individuals until later cohorts.

A summary statistics table showing the characteristics of parents and children will be added to future versions of this paper.

3 Model

3.1 Regression Discontinuity Design

The discontinuity we exploit arises from the fact that the 1993 reform affected some DI participants, but not others, based on their age. Parents who were age 45 to 50 as of August 1, 1993 were subject to the old DI rules, while parents between the the ages of 40 to 45 were re-examined according to the new, more stringent rule. The direct effect of the reform on parental outcome y^P can be modeled in a regression discontinuity (RD) framework without additional covariates as:[6]

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

where t 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.

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

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

where y^C is the relevant child outcome variable, t 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^C is an error

[6]See Imbens and Lemieux (2008) and Lee and Lemieux (2010) for details on the implementation and assessment of RD designs.

term, and h_l, and hg are unknown functions. The coefficient λ is the reduced form (RF) or intention to treat (ITT) effect of the reform on outcomes. In the absence of covariates, the IV estimate is simply the ratio of the reduced form estimate of λ to relevant first stage estimate of θ.

3.2 Threats to Identification

3.2.1 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 reported age easily in the Netherlands, there is little chance for this type of direct manipulation.

Since the DI data is not available until 1996, another threat to validity is that the reform caused differential attrition around the age 45 cutoff. As a reminder, our sample includes parents who were receiving DI benefits on August 1, 1993 (the date the reform went into effect for existing claimants) and who were were still on DI as of January 1, 1996. In other words, we can only observe whether an individual was receiving DI at the time of the reform if they remained on DI until at least January 1, 1996. 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 40-44 did not start until after January 1, 1996 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.[7]

Figure 2 graphs the histogram of parental age at the time of the reform in our sample. While there is some seasonal variation, there are no noticeable jumps in parental age 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=.33). The drop in the histogram around age 47.5 is due to the "hunger winter" famine which took part in the German-occupied portions of the Netherlands in

[7]While 40 years old 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 of the 40 year old cohorts were re-examined until after 1996.

World War II, and therefore is not a behavioral response to the DI reform.

As a final test, we can explore whether there are changes in the distribution of pre-determined characteristics of parents or children around the reform dates. Appendix Table 1A reveals there is little evidence of any jumps around the cutoff in pre-determined characteristics. More importantly, the point estimates are small in magnitude and our RD estimates barely move when we include these characteristics in the regressions. This appendix will be added to future drafts of the paper.

3.2.2 Exclusion Restriction

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

It is unlikely parental exposure to the 1993 reform affected children directly except through the reduced generosity of the DI program. However, the drop in DI payments may not be a sufficient statistic for how generosity changed at the cutoff. For parents remaining on the program, the reform reduced or held constant DI payments, whereas for parents kicked off DI or choosing to leave voluntarily, the reform reduced their payment to zero. Parental DI payments will capture both the intensive and extensive margins of the reform under the assumption that total DI payments are what matters. For the exclusion restriction to hold, therefore, parental participation versus non-participation cannot directly affect children except through the reduction in payments to zero. This implies, for example, that a reduction in benefits from 20,000 to 15,000 euros has the same effect as a parent who previously received 5,000 euros exiting the program. Since this may not be the correct functional form for how the new stricter rules affected parents, we also present reduced form estimates throughout.

The 1993 reform may 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.

3.2.3 Monotonicity

If the effect of the drop in parental DI payments is constant for each child outcome, then the absence of manipulation and differential attrition combined with the exclusion restriction are sufficient for causal estimation in an RD setting. With heterogeneous effects, however, monotonicity is also needed. In our setting, monotonicity requires that if a parent was exposed to the new, more stringent DI rules, they must receive DI payments which are lower or the same compared to what they would have received under the old rules. Since the new rules weakly reduced payments for any individual, this holds almost by construction. The one exception is that if a parent's illness has worsened, a re-examination under the stricter rules could result in a higher payment compared to what they would have received had they not been re-examined. We can directly asses how often this happens for the 40-45 age cohort, since for this group we observe both the initial and re-examination degree of disability. It happens rarely, which suggests that non-monotonicity is not a major issue for this group, and is unlikely to be a problem for the 45-50 cohort either. Monotonicity ensures that IV identifies the local average treatment effect (LATE) of a drop in parental DI payments; that is, the average effect among the subgroup of parents whose payment would have been lowered if the were exposed to the new versus old rules.

4 Results

4.1 Graphical Evidence

An advantage of RD is that results can be presented graphically, which provides a transparent way of showing how the intergenerational spillovers are identified. We begin our presentation of results with a graphical depiction of a few key results before turning to a more detailed regression-based analysis.

The top panel of Figure 3 graphs the first stage relationship between parental DI payments and the reform. The running variable is the parent's age as of the reform date of August 1, 1993 and the cutoff date of 45 years old determines whether the parent is subject to the new versus old DI program rules. On the y-axis is parental DI payments in 1999; we use 1999 since this is after all the re-examinations have taken place. Each observation in the graph is the average DI payment for parents in one-month age bins. The figure reveals that DI benefit payments rise with age, largely reflecting the fact that older individuals have higher disability ratings and therefore less earnings capacity and higher DI payments on average. More importantly, there is a sharp drop in payments for individuals just to the left of the cutoff. This is as expected, since parents less than age 45 were subject to re-examination under the stricter DI program rules. DI payments drop by around 1,400 euros, which is a drop of about 10 percent compared to the average payment at the cutoff.

To document the extensive margin of the DI reform by itself, in panel B we graph the fraction of parents exiting DI completely. The running variable and cutoff are the same as in panel A. Each observation in the graph is the fraction of parents in a one-month bin who have exited DI by 1999. The first pattern to notice is that exits are higher for younger individuals, with over 20% of 40 year olds exiting compared to around 5% of 50 year olds. This pattern is linear in age to start, but flattens out for older ages. At the cutoff, there is a sizeable 5 percentage point drop in exits right at the cutoff. This drop mostly reflects individuals being kicked off of DI under the stricter re-examination rules, but also includes any voluntary exits.

Figure 4 illustrates the reduced form model for two of our most important outcomes. In both graphs, the running variable of parental age as of the reform date (August 1, 1993) is on the x-axis. Each observation is the average over a one-year bin of residuals from a regression controlling for pre-determined parent and child covariates.[8] The graph also includes separate quadratic trend lines on each side of the cutoff date along with pointwise 95% confidence intervals. These trend lines are based on the underlying, unaggregated data for a parent's age.

The top graph looks at whether a child has ever participated in DI by 2012 as the outcome. By 2012, the children in our sample are on average 40 years old, and therefore have had many years to work and participate in the DI program. Indeed, the age range of children by 2012 is beginning to approach the their parent's age at the time the reform was introduced. Just under 10% of children have been on DI at some point or another by 2012 (the residualizing centers this fraction to zero in the graph). The reduced form estimate is captured by the jump in child DI participation at the parental age cutoff. There is a jump of 1.3 percentage points at the cutoff, which is large relative to the sample mean.

The bottom graph in Figure 4 mirrors the top graph, except that it uses a child's years of education in 2012 as the outcome variable. The mean education level for the children in our sample is 11.5 years. While most children will be done with their formal education by 2012, not all are. Indeed, one can see in the figure that education trends slightly upward in the graph as a function of parental age. The jump in years of education at the cutoff is around one-seventh of a year, which is over a 10 percent increase relative to the sample mean.

In the appendix, we provide visual evidence for the reduced form effects for a variety of other child's outcomes. These will be added to future drafts of the paper, along with updated RD graphs in the main paper.

4.2 First Stage Estimates

We now present regression results in table form. Table 1 shows first stage estimates for how the reform affected the amount of DI benefits received by parents. It regresses DI benefits received by

[8] We use one-year bins and residualize to help make the RD picture clearer; using less aggregated bins and not residualizing shows a similar, but somewhat noisier, picture.

parents in the year 1999 on a dummy for the reform cutoff and separate linear trends in a parents age to the left and the right of the cutoff. We use triangular weights in our regressions so that observations nearer the cutoff will have more influence. Although the coefficients are not shown, we also include a variety of covariates for both the parent and the child which are measured as of 1996. Note that 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 occured for the 40-45 age cohort, so these controls should be exogenous to the cutoff. Note also that 1999 is after the law exemption and after the re-examinations.

The first specification in the tables uses the entire sample. Mirroring what was drawn in the top panel of Figure 2, the first stage RD estimate is a 1,300 euro drop in benefits for parents exposed to the reform. As discussed earlier, the reform mandated the 40-45 age group be re-examined and subjected to rules which lowered the calculated degree of disability and therefore the replacement rate. This meant that while some individuals exposed to the reform were kicked off the program, others remained on the DI but with lower benefits. In constructing an IV estimate, if one were to simply use parental exit from the program as the first stage outcome, the exclusion restriction would clearly be violated since the reform also clearly affected the payment levels for parents remaining on the program. Given the reform had both an extensive and intensive margin, we use the total drop in DI payments to the parents for our first stage outcome. If exit has a direct affect beyond the reduction in DI benefits to zero, the exclusion restriction will not hold. For this reason, we present both reduced form and IV estimates: the RF estimates will be valid regardless, while the IV estimates help give some idea of the scale of the effects.

While the effect of the reform directly affected DI payments, it also triggered a variety of parental responses. As documented in Borghans, Gielen, and Luttmer (2014), the reform caused parents to increase their earnings by 0.62 euros per euro of lost benefits, collect 0.30 euro more from other social assistance programs per euro of lost benefits, and a small increase in spousal labor supply. The earnings increase persists over time, while the benefit substitution declines over time. We find similar spillover effects in our sample of parents with children. These additional parental outcome variables are likely to have spillover effects on children. While they do not violate the exclusion restriction, they are important to keep in mind when interpreting our child outcome estimates.

The remaining specifications present first stage estimates for subsamples based on the parent's and child's gender. These first stages will be used to estimate heterogeneous effects later in the paper. The estimated drop in parental benefits is somewhat larger for fathers, but not very different for sons versus daughters. This makes sense, as pre-disability earnings are larger on average for fathers, while child gender is not strongly correlated with average DI benefit amounts.

4.3 Reduced Form and IV Estimates

We now examine a variety of intergenerational spillovers. Table 2 looks at the intergenerational link in DI participation. As the outcome variable, specification A uses a dummy variable for whether the child has ever participated in the DI program by 2012. This specification mirrors the reduced form plotted in the top panel of Figure 3; the RF point estimate is -.013 and is statistically significant. Scaling the reduced form by the first stage presented in Table 1, the estimated effect of a 1,000 euro decrease in parental DI benefits is a 1 percentage point drop in child DI participation. Note the IV estimate will always have the opposite sign compared to the reduced form, as the first stage estimate is negative. Specifications B and C measure DI participation as the cumulative number of days on DI and the cumulative amount of DI payments, including zeros. The estimated effect of a 1,000 euro drop in parental DI benefits is 27.5 fewer days spent on DI and 908 fewer euros received under the program. Relative to the sample means, these represent about a 10% drop.

Does the drop in DI use translate into increased income from work or other social programs? In Table 3 we answer these questions. We begin by looking at cumulative income from any type of work, including wage employment and self-employment. The IV estimate translates into a 5,300 euro increase in cumulative work income for every 1,000 euro drop in parental DI benefits. In interpreting this number, it is important to keep in mind the child variable is a cumulative measure with a mean of 331,000 euros, while the parent variable is an immediate, annual change in benefits due to the reform. Breaking work income into wage and self-employment income reveals an especially large response for self-employment income relative to its mean, with more than a 10% increase in self-employment income due to the reform.

Borghans, Gielen, and Luttmer (2014) found substantial program substitution for parents in response to the reform. Parents replaced about 30% of lost DI income with benefits from other programs in the short run. However, we find no significant effects on their children's use of these same governmental programs. Specification D reveals that parental DI benefits have virtually no effect on cumulative income from unemployment insurance. Likewise, there is no evidence for an effect on general assistance (traditional cash welfare) or combined income from all other government transfer programs.

Table 4 turns to child educational investments. For this table, we have fewer observations, as schooling is only observed for the entire population for younger cohorts.[9] Using children's years of education in 2012 as the outcome, we find a substantial effect of parental DI receipt. Children of parents exposed to the reform increased their education by a statistically significant 0.13 years. Scaled in terms of parental benefits, each 1,000 euro decrease in a parent's benefits increased child investments in education by one-tenth of a year on average. The reform appears to primarily affect children's completion of upper secondary school (roughly equivalent to High School in the U.S.) and a college degree. Separate regressions using these educational attainment thresholds as child outcomes reveal strong and statistically significant effects.

An interesting question is how much of the the increase in income from work in Table 3 can be accounted for by the increase in education in Table 4. Assuming that an additional year of schooling has an 8% return, the estimated increase in schooling from the reduced form translates into just over a 1% increase in cumulative earnings. Specification A of Table 3 found a 2.1% increase in earnings due to the reform. So a simple calculation reveals that almost half of the increase in earnings is due to the increased educational investment by children.[10]

We break things up into subsamples in Table **??** based on the parent's and child's gender. For

[9] In this table, we also limit the sample to children who are young enough to not have already completed their education at the time of the reform.

[10] While it would be interesting to estimate how much of the increase in earnings can be attributed to hours, this variable is unfortunately not available.

simplicity, we only present the reduced form estimates in this table. The first two columns show results for the sample of fathers and the sample of mothers for some of the most interesting outcomes in the prior three tables. For the ever on DI and cumulative DI outcomes, the reduced form estimates remains statistically significant for both subsamples, but the effects are larger for children whose mothers were exposed to the reform compared to fathers. Since the first stage estimates are also larger for fathers in Table 1, the implied IV estimates are even farther apart for the two subsamples. A similar patterns holds for income, whether from wages or self-employment, with mother's exerting a stronger effect. In contrast, for the education outcomes, it is the father's exposure to the reform which affect children, with no measurable impact from mothers. This finding that fathers matter more for education is broadly consistent with the work by Behrman and Rosenzweig (2002) using twins and Plug (2004) using adoptees, which finds a strong link between a father's and their children's education level, but no link for mothers. Due to relatively large standard errors, however, we cannot claim that the subsample results are statistically different from each other.

Continuing on to Table 6 we look at whether parental DI benefits affect a child's criminal activity. For this table we also focus on the reduced form effect. We look at whether a child has ever been arrested or incarcerated for a crime by 2012. While the two estimates for all crimes combined are negative, they are not statistically significant. When we break crimes into specific types (here we focus just on the more commonly commited crimes), there is a statistically significant negative effect of having a parent exposed to the reform on arrests for theft and a positive and statistically significant effect on arrests for traffic violations. The positive effect for traffic violations might have to do with the fact that these are very different types of crimes compared to the other categories. The remaining entries in the table are generally negative, for both arrests and incarcerations, but not statistically significant. While we do not have sufficient precision to rule out small effects, if anything, it appears that reducing a parent's DI benefits had a socially positive effect on a child's criminal activity, and not a negative one. One might have worried that a reduction in DI benefits would leave children less supervised, since parents returned to work in response to the reform. Likewise, it lowered family income in the long run. But neither of these effects seem to have harmed children for any of the outcomes we can measure.

Our final set of outcomes is child marriage / cohabitation and fertility choices. Since the most interesting patterns are gender specific, in Table 7 we again split the sample by gender of the parent and child. For the child outcome of ever being married, we find a 3 percentage point increase in the probability for the mothers sample, a finding which is statistically significant. Using cumulative months married as the outcome instead, there is a statistically significant 3.5 month increase in marriage for children whose mothers were exposed to the reform. In the Netherlands, as in many European countries, cohabitation is becoming increasingly common. So we next create a variable combines both marriage and cohabitation. The results tell a similar story for the mothers sample, but not the effect for the daughters samples also comes into sharper focus and the coefficients become statistically significant. Turning to fertility, we again find strong effects for both the mothers sample and the daughters sample. Both sample reveal a 2.7% increase in the probability of having at least one child by 2012 due to parental exposure to the reform. In contrast, none of these child family outcomes are statistically significant for either the fathers or sons samples. While it would be interesting to further analyze the data using subsamples based on the interaction of parent and child gender, this finer cut of the data results in less precise estimates.

4.4 Robustness

To be written.

4.5 Comparison to OLS

To be written.

5 Interpretation and Simulation

To be written.

6 Conclusion

Does participation in a social program by a parent influence their child's use of public assistance, human capital investments, future labor market, and marriage outcomes? This is a difficult question to answer due to the nonrandom nature of program participation and the likelihood that unobserved factors driving participation are correlated across generations. Yet what a child learns from his or her parents about employment relative to government support could matter for the financial stability of a variety of social insurance and safety net programs.

In this paper we take advantage of a disability insurance (DI) reform in the Netherlands which simultaneously tightened eligibility criteria and reduced the generosity of the program. The key to our regression discontinuity design is that the reform applied to younger cohorts, while older cohorts were exempted from the new rules. We find strong evidence that children of parents who were pushed out of DI or had their benefits reduced are affected positively on a variety of dimensions. 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, invest significantly more in their education, increase their earnings, and are more likely to marry/cohabit and have a child.

Our analysis provides an important lesson for the evaluation of the costs and benefits associated with policy reforms. Considering just the direct effects on current and future participants, without accounting for peer effects within families or other networks, would be a mistake. In our setting, the intergenerational spillover effects are substantial. Children of affected parents not only reduce their own participation in the DI program (without an increase in participation in other transfer programs), but they simultaneously increase their education and earn more in the future. This results not only in fewer government benefits being paid out, but also in an increase in taxes paid. Ignoring the spillover effects on the next generation would greatly underestimate the cost savings of the Dutch DI reform in the long run.

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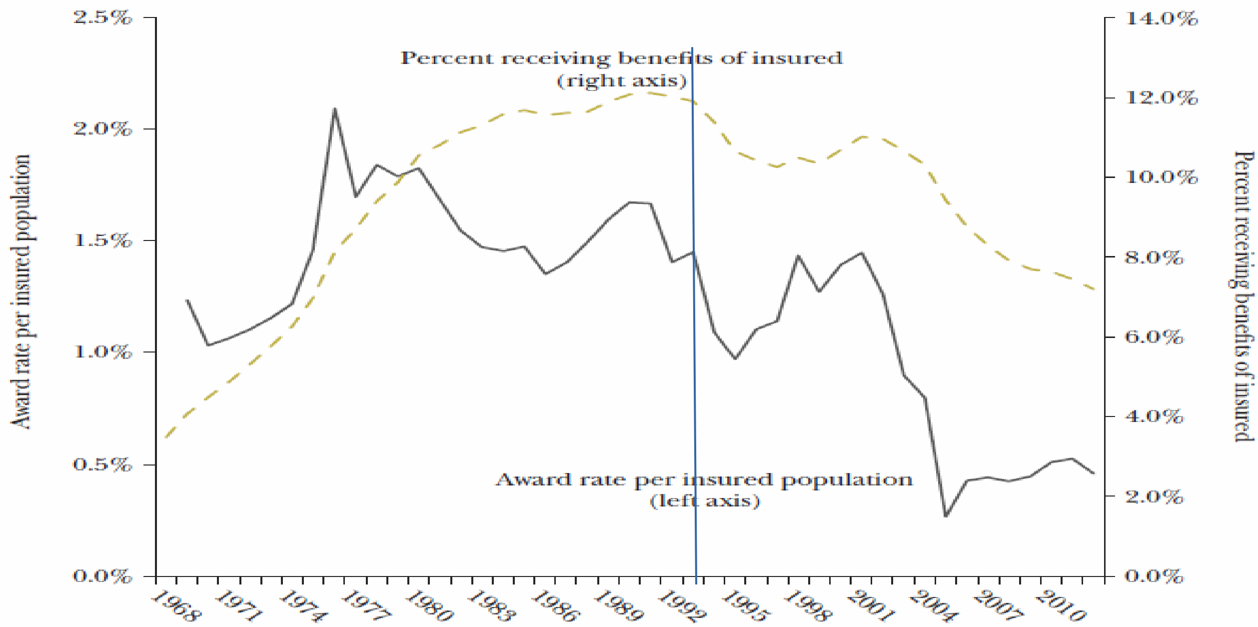
Figure 1
Disability Insurance Award and Enrollment Rates per Insured Worker in the Netherlands, 1968–2012



Source: UWV (2012).
Note: The Disability Insurance award rate is the share of the insured population that started to receive disability payments in a given year.

Figure 1. DI award and enrollment rates over time

Note: The blue line marks August 1, 1993, which is the start date of the reform we study.

Source: Figure 1 from Koning and Lindeboom (2015).

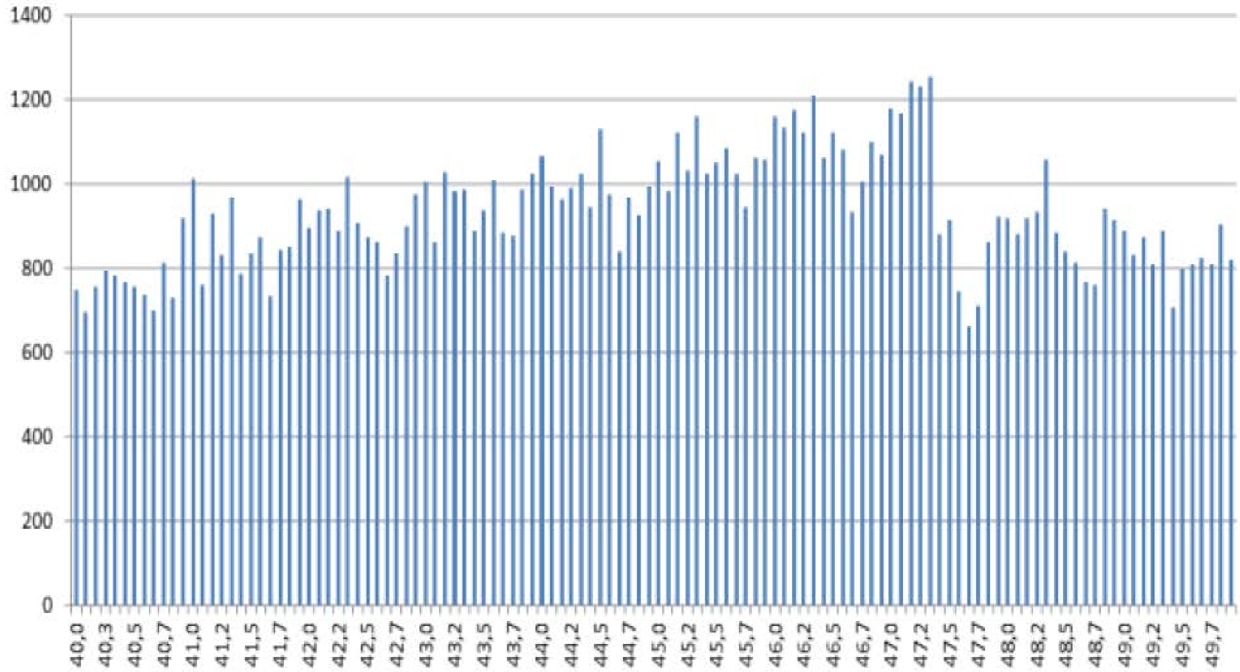
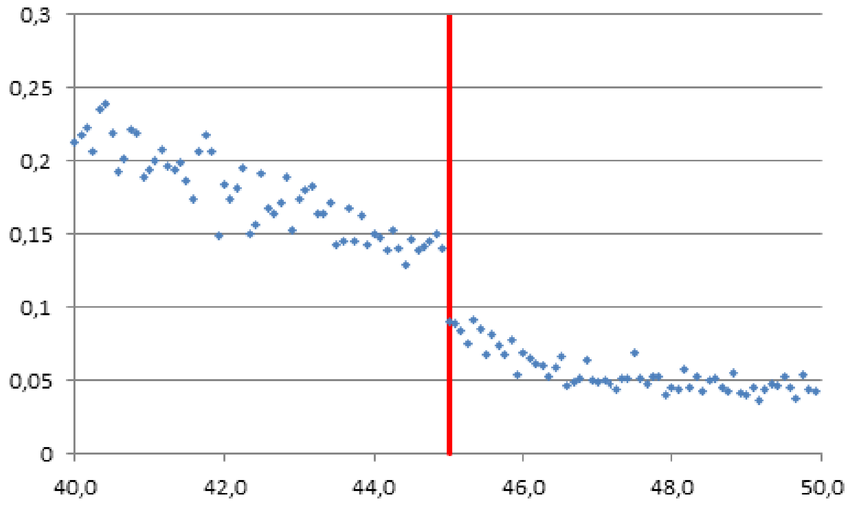


Figure 2. Density of parental age at the time of the reform

Notes: The reform differentially affected individuals age 45 or older at the time of the reform. The drop around age 47.5 is due to the "hunger winter" famine in German-occupied portions of the Netherlands in World War II.

A. Parental exit from DI by January 1999



B. Annual DI benefit in thousands of euros as of January 1999

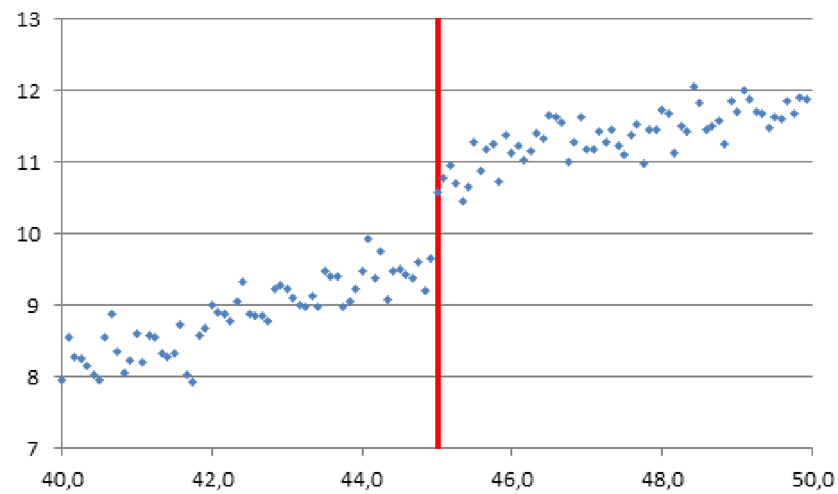
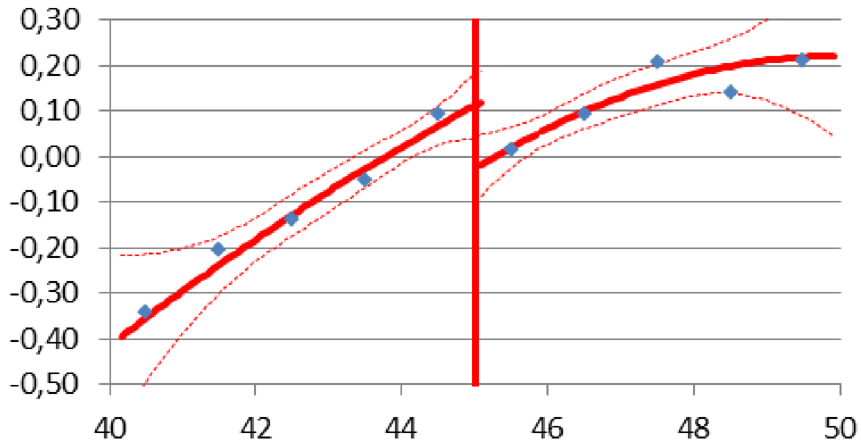


Figure 3. Effect of the reform on parents' DI participation and DI benefit amounts

Notes: The age of the parent as of August 1, 1993 (the reform date) is on the x-axis, and the parental outcome is on the y-axis. Each observation is the average over a one month bin in age. The vertical bar denotes the 45 year-old cutoff associated with the reform: individuals less then age 45 as of the reform date where subject to new, stricter DI rules, while individuals 45 or older were grandfathered in under the old, more generous DI rules.

A. Child years of education by 2012



B. Child ever on DI by 2012

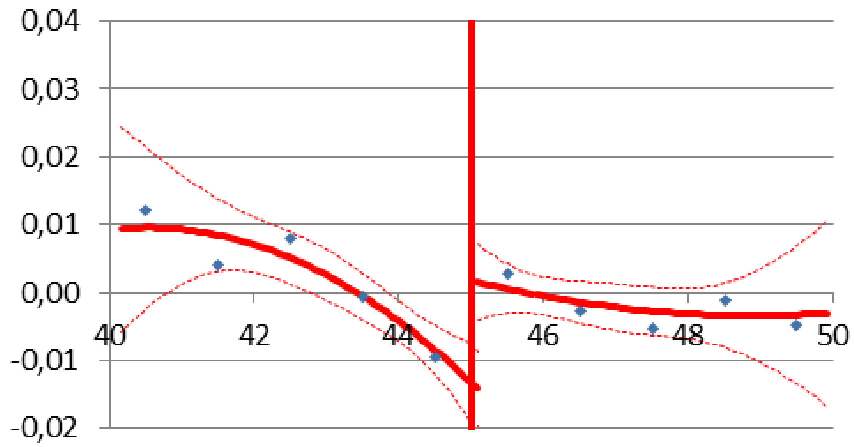


Figure 4. Reduced form effects of the reform on children

Notes: The age of the parent as of August 1, 1993 (the reform date) is on the x-axis, and the child outcome is on the y-axis. Each observation is the average over a one year bin of residuals from a regression controlling for pre-determined parent and child covariates. The vertical bar denotes the the 45 year-old cutoff associated with the reform: parents less then age 45 as of the reform date where subject to new, stricter DI rules, while those 45 or older were grandfathered in under the old, more generous DI rules. Solid lines are second order polynomials with 95 percent confidence intervals based on the underlying monthly data.

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

Dep var: Parental DI Benefits	First Stage
A. Full Sample	-1.306**
	(.096)
B. Father	-1.372**
	(.117)
C. Mother	-1.018
	(.136)
D. Son	-1.268**
	(.113)
E. Daughter	-1.354**
	(.124)
Observations	101,125

Notes: Parental DI benefits measure DI payments received in 1999, in thousands of euros. The sample is parents age 40-50 on August 1, 1993, receiving DI benefits on August 1, 1993, with children living at home on August 1, 1993, and who where still on DI as of January 1, 1996 (the earliest date for which we have DI records, but still before the DI re-examinations for the age 40-45 cohort and the passage of the law exempting the age 45-50 cohort). Parents with multiple children appear more than once in the sample, while children with two parents on DI are dropped from the sample. All coefficients are estimated using a linear RD model and triangular weights. The running variable in the RD is the age of the parent as of August 1, 1993 and the cutoff is age 45 as of August 1, 1993. Control variables for the parent are measured as of 1996 and include age, birth month dummies, a gender dummy, a cubic in pre-disability earnings, six dummies for degree of disability, a cubic in DI duration, national origin dummies; control variables for the child are measured as of 1996 and include age, birth month dummies, and a gender dummy. Standard errors in parentheses, clustered at the parent level.

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

Table 2: Intergenerational RD Estimates: Child DI Participation

Child outcome variable in 2012	Mean	RF	IV
A. Ever on DI	.097	-.013**	.010**
		(.004)	(.004)
B. Cumulative days on DI	250	-36.06**	27.49**
		(12.73)	(12.63)
C. Cumulative DI income	8.303	-1.192**	.908**
(in 1,000 euros)		(.451)	(.447)
Observations	101,125		

Notes: See notes to Table 1 for the sample definition, the RD estimator, and the included control variables. The three independent variables measure whether a child ever participated in DI between 1996 and 2012, the cumulative number of days on DI between 1996 and 2012, and the cumulative DI payments received between 1996 and 2012. Included control variables for the parent are measured as of 1996 and include age, birth month dummies, a gender dummy, a cubic in pre-disability earnings, six dummies for degree of disability, a cubic in DI duration, national origin dummies; control variables for the child are measured as of 1996 and include age, birth month dummies, and a gender dummy. Standard errors in parentheses, clustered at the parent level.

**p<0.05, *p<0.10

Table 3: Intergenerational RD Estimates: Child Labor Market and Other Social Assistance

Child outcome variable in 2012	Mean	RF	IV
A. Cumulative income from work	330.855	6.950**	-5.297*
(wage + self-employment)		(2.840)	(2.477)
B. Cumulative wage income	309.874	4.012	-3.058
		(2.519)	(2.477)
C. Cumulative self-employment income	20.981	2.938**	-2.239*
		(1.310)	(1.283)
D. Cumulative UI income	4.205	-.056	.043
		(.142)	(.139)
E. Cumulative general assistance income	3.324	.014	-.010
(traditional cash welfare)		(.235)	(.230)
F. Cumulative misc. transfer program income	2.976	.014	-.010
		(.130)	(.127)
Observations	101,125		

Notes: See notes to Table 1 for the sample definition, the RD estimator, and the included control variables. All of the independent variables measure cumulative income amounts between 1996 and 2012 for the child, measured in 1,000 euros. Standard errors in parentheses, clustered at the parent level.
***p<0.05, *p<0.10*

Table 4: Intergenerational RD Estimates: Child Educational Investments

Child outcome variable in 2012	Mean	RF	IV
A. Years of education	11.52	.129**	-.105**
		(.054)	(.053)
B. Lower secondary school or more	.95	-.001	.001
		(.004)	(.004)
C. Upper secondary school or more	.78	.025**	-.020**
		(.007)	(.007)
D. Bachelor degree or more	.34	.0178**	.-015*
		(.009)	(.008)
E. Master degree or more	.10	.008	-.006
		(.005)	(.005)
F. Advanced degree or more	.01	-.001	.001
		(.002)	(.001)
Observations	65,208		

Notes: See notes to Table 1 for the sample definition, the RD estimator, and the included control variables. Education is measured as of 2012. Upper secondary school or more includes voacational school. The sample size in this table is smaller, as education data is complete for younger cohorts, but incomplete for older cohorts. Standard errors in parentheses, clustered at the parent level.
***p<0.05, *p<0.10*

Table 5: Intergenerational RD Estimates by Parent and Child Gender

	Reduced Form			
Child outcome variable in 2012	Fathers	Mothers	Sons	Daughters
A. Ever on DI	-.011**	-.018**	-.010**	-.016**
	(.004)	(.008)	(.005)	(.006)
B. Cumulative DI income	-1.141**	-2.087**	-1.036**	-1.801**
	(.477)	(.993)	(.537)	(.687)
C. Cumulative wage income	.286	10.641**	-.059	5.761**
	(2.687)	(5.515)	(3.441)	(3.035)
D. Cumulative self-employment income	2.810**	4.610**	6.144**	-.596
	(1.430)	(2.410)	(2.027)	(1.041)
E. Years of education	.165**	.003	.166**	.081
	(.059)	(.104)	(.066)	(.072)
F. Upper secondary school or more	.028**	.008	.031**	.014
	(.008)	(.013)	(.009)	(.0100)

Notes: See notes to Table 1 for the sample definition, the RD estimator, and the included control variables. The child outcome variables are described in Tables 2, 3, and 4. Standard errors in parentheses, clustered at the parent level.

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

Table 6: Intergenerational RD Estimates: Child Criminal Activity

Child ever arrested/jailed for specified crime by 2012	Arrests		Incarceration	
	Mean	RF	Mean	RF
A. Any crime	.099	.005	.015	-.0020
		(.004)		(.0020)
B. Theft	.031	-.004**	.004	.0008
		(.002)		(.0008)
C. Crime against person	.054	-.003	.005	-.0001
		(.003)		(.0009)
D. Vandalism	.020	-.000	.0003	-.0002
		(.002)		(.0002)
E. Drugs	.017	-.001	.003	.0008
		(.002)		(.0002)
F. Traffic	.040	.005**	.003	-.0009
		(.002)		(.0006)
Observations	101,125			

Notes: See notes to Table 1 for the sample definition, the RD estimator, and the included control variables. Arrest and incarceration data come from two separate datasets. Standard errors in parentheses, clustered at the parent level.
***p<0.05, *p<0.10*

Table 7: Intergenerational RD Estimates: Child Marriage and Fertility Choices

	Reduced Form			
Child outcome variable in 2012	Fathers	Mothers	Sons	Daughters
A. Ever married	.004	.030**	.012	.006
	(.007)	(.012)	(.008)	(.009)
B. Cumulative months married	.175	3.540**	1.254	.521
	(.752)	(1.319)	(.805)	(1.003)
C. Ever married / cohabiting	.009	.020	.010	.012**
	(.007)	(.013)	(.008)	(.009)
D. Cumulative months married / cohabiting	.125	5.164**	.854	1.848**
	(.795)	(1.552)	(.936)	(1.032)
E. Fertility: at least one child	-.010	.027**	-.010	.027**
	(.007)	(.013)	(.007)	(.013)
Observations	101,125			

Notes: See notes to Table 1 for the sample definition, the RD estimator, and the included control variables. The columns limit the estimation sample to fathers on DI, mothers on DI, sons with a parent on DI, and daughters with a parent on DI, respectively. Standard errors in parentheses, clustered at the parent level.
$**p<0.05$, $*p<0.10$

Table 8: Intergenerational OLS Estimates: Child DI Participation

	Age of parent			
Dep var: Child ever on DI by 2012	40-45		45-50	
A. Change in parental DI payments	.0003*	-.0000	.0008**	.0003
(in 1,000 euros)	(.0002)	(.0002)	(.0002)	(.0002)
B. Parent exited DI by 1999	-0.004	.001	-.012*	-.001
	(0.003)	(.003	(.005)	(.006)
Controls		X		X
Observations	53,699	53,699	58,154	58,154

Notes: The sample is children of parents who were receiving DI benefits on August 1, 1993 and who where still on DI as of January 1, 1996 (the earliest date for which we have DI records). We split the data into two, based on a parent's age, so that the discontinuity in the DI rules cannot influence the estimates. In specification A, the independent variable is the change in a parent's DI payments between 1999 and 1996. In specification B, the independent variable is a dummy for whether parents have exited DI by 1999. Control variables for the parent are measured as of 1996 and include age, birth month dummies, a gender dummy, a cubic in pre-disability earnings, six dummies for degree of disability, a cubic in DI duration, national origin dummies; control variables for the child are measured as of 1996 and include age, birth month dummies, and a gender dummy. Standard errors in parentheses, clustered at the parent level.
$**p<0.05$, $*p<0.10$