

**PRELIMINARY AND INCOMPLETE**

**Social Support Shopping:  
Evidence from a Regression Discontinuity in Disability Insurance Reform**

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**Abstract**

This paper examines how a change in the generosity of one social assistance program generates spillovers onto other social assistance programs. We exploit an age discontinuity in the stringency of the 1993 Dutch disability reforms to estimate the effect of decreases in the generosity of disability insurance (DI) on reliance on other forms of social assistance. We find strong evidence of “social support shopping”: for each Euro saved in DI benefits, the government has to spend an extra 51 cents in other social assistance programs. This benefit-shifting ratio is even larger if we also take into account the response of the partners’ of those affected by the DI reform. The benefit shifting effect declines over time, but is still 21% eight years after the reform. In addition, we find a substantial degree of crowd out of labor income by disability benefits. For each Euro in lost DI benefits, individuals on average increase their earnings by 95 cents.

Key words: crowd out, spillover effects, benefit shifting, social insurance, income assistance, welfare, regression discontinuity, administrative data.

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## 1. Introduction

What are the effects of changes in the generosity or eligibility criteria of one social assistance program on participation in other social assistance programs? This question is pertinent in the U.S, where welfare reform has led many individuals to take up Supplemental Security Income rather than welfare (Schmidt and Sevak, 2004). Similarly, Autor and Duggan (2003) have shown that Disability Insurance applications have become more sensitive to local labor market shocks over the last few decades, indicating that Disability Insurance is increasingly functioning as a form of unemployment insurance. The scope for social support shopping, the phenomenon of individuals substituting social assistance programs in response to changes in their relative generosity or stringency of eligibility, is even larger in countries with a more extensive social safety net than that of the U.S.<sup>1</sup>

Evidence on the existence of social support shopping is important because it implies that policy makers cannot consider reforms to one social assistance program in isolation. Evidence on the extent of social support shopping allows policy makers to make more accurate predictions of the budgetary impact of a reform to a social assistance program by taking into account the spillover effects of the reform on participation in other programs.

In this paper, we estimate the extent of social support shopping in response to reforms to disability insurance in the Netherlands. The reforms involved medical re-examinations of existing recipients and a common outcome of the re-examination was to reduce the benefit level or terminate eligibility of a current recipient. Two features make this reform particularly suitable for studying substitution between different social assistance programs. First, we have administrative panel data on the universe of Dutch disability insurance claimants, including information on their future labor market earnings and income from other government social assistance programs. These data allow us to track for a period of nearly a decade what happens to (former) disability insurance claimants in the wake of the reform. Second, the reform contains an age

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<sup>1</sup> The fact that individuals substitute one social assistance program for another has no implications for the question of whether the recipients are “deserving.” We therefore do not intend any judgment about recipients’ deservingness by the use of the term “social support shopping.”

discontinuity: the reform was significantly more stringent for the cohort that would turn 45 after August 1<sup>st</sup>, 1993. Since we have each individual's month of birth, we exploit this discontinuity by comparing later labor market earnings and social assistance income for the cohort just below this age cutoff to these outcomes for the cohort just above the age cutoff. We scale this difference in outcomes by the discontinuity in disability benefit levels around the age cut-off. The resulting estimates yields two key ratios: (i) the benefit shifting ratio, which is the average causal effect of the more stringent DI rules on income from other social assistance as a fraction of average lost DI income, and (ii) the earnings crowd out ratio, which is the average causal effect of more stringent DI rules on earnings as a fraction of average lost DI income.

We have three main findings. First, in the short term (about 2 years after reform), we find that the more stringent DI rules increased the probability of receiving income from other forms of social assistance programs by 5 percentage points (a near doubling), and the income from these other social assistance programs replaces 51% of lost DI income. In other words, we find a significant amount of social support shopping with a short-term benefit-shifting ratio of 0.51. The more stringent rules both increased the probability of having any labor market earnings and the average amount of earnings. These additional earnings replaced 95% of foregone DI income, i.e., the earnings crowd-out ratio is 0.95. Thus, in the short term, individuals were able to replace more than all of their foregone DI income on average.

Second, over time, the benefit-shifting ratio rate clearly falls but the earnings crowd-out ratio shows a relatively moderate increase that is not statistically significant. Eight years after the reform (the last year in our data), the benefit-shifting ratio has fallen to 0.21 while the crowd-out ratio stands at 1.19. In other words, in the longer run, former DI recipients rely significantly less on other forms of social assistance, but do not significantly increase their labor market earnings.

Third, spillovers between social assistance programs operate not only through former DI recipients' own choices, but also through the decisions of their spouses. We find that the DI reform causes the spouses of (former) disability recipients to rely more strongly on social assistance. If we include the social assistance income of spouses in our

benefit-shifting calculation, the short-run benefit-shifting ratio roughly doubles in magnitude.

While the precise magnitudes of our findings are obviously specific to this particular Dutch disability insurance reform, we believe our paper offers important lessons that are widely applicable. First, our evidence demonstrates that social support shopping is not just a theoretical possibility but that it occurs at an economically meaningful scale for prime-age disability insurance recipients. Hence, a carefully designed reform of a social assistance program needs to take into account its effects on other social assistance programs. Second, the degree to which programs substitute for each other can vary substantially with time since the reform, so it is important to consider the effects of the reform over various horizons. Finally, spillover effects not only occur through decisions of the household member directly affected by the reform, but also through the amount of social assistance received by household members of the directly affected individual.

Our findings on the existence of spillover effects between different social assistance programs confirm earlier results from other contexts.<sup>2</sup> With respect to child-related benefits, Garrett and Glied (2000) show that the increase in child Supplemental Security Income (SSI) eligibility in the early 1990s led to a greater increase in SSI enrollment in states with less generous benefits for Aid to Families with Dependent Children (AFDC), suggesting that families use SSI and AFDC as substitutes. Kubik (2003) shows that the substitution of SSI for AFDC is larger in states with negative fiscal shocks, suggesting that states actively encouraged this substitution (because the state-share in SSI payments is generally lower than the state-share in AFDC payments). Duggan and Kearney (2007) examine individual-level panel data to find that households in which a child becomes eligible for SSI subsequently receive less income from AFDC, WIC, and food stamps.<sup>3</sup> With respect to early retirement, Duggan, Singleton, and Song

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<sup>2</sup> The one exception is the paper by Autor and Duggan (2008), who exploit a ruling that suddenly expanded the eligibility for Veteran's Disability Compensation (DC) for a subgroup of Vietnam Veterans. They find that the increased take-up of Veteran's Disability Compensation due to this ruling raised the receipt of Social Security Disability Insurance (SSDI) benefits. As Autor and Duggan note, this result may be explained by the fact that one needs leave the labor force to qualify for SSDI, and leaving the labor force is less costly for people who already receive DC. Thus, this institutional feature may explain the complementarity between two social assistance programs in this case.

<sup>3</sup> WIC provides nutritional assistance to low-income families with young children and pregnant women.

(2007) use differences by cohort in the generosity of Social Security retirement benefits to show that the reduction in the generosity of Social Security retirement benefits led to an increased use of Social Security disability benefits. Karlström, Palme, and Svensson (2008) use a difference-in-differences design to examine the effect of the abolition of DI as a path to early retirement for 60-64 year olds in Sweden. They find that, in the 2-3 years following the reform, this group responded by taking up other forms of social assistance rather than by increasing their labor supply. Finally, Staubli (2011) also uses a difference-in-differences approach to show that a disability insurance reform that affected 55-56 year-old males in Austria has spillover effects on their take up of unemployment insurance and sick leave.

Our paper contributes to this literature by estimating substitution between social support programs for prime-age workers, in a setting that allows us to very cleanly identify the degree of spillovers between programs. Our evidence that social support shopping operates in part through the reliance on social assistance by the partners of the individuals affected by the benefit reform is, to the best of our knowledge, a new finding in this literature. Finally, we extend the literature by examining substitution effects over longer horizons (up to eight years after the reform).

The remainder of the paper is organized as follows. In Section 2, we describe the reform in the Dutch disability act that we use for identification in this paper. Section 3 describes the data, and Section 4 presents the results. Section 5 concludes.

## **2. The 1993 Dutch Disability Insurance Reform**

To place the Dutch disability insurance reform in context, it is useful to know that the Netherlands has historically had high DI recipiency rates. Together with other countries such as Sweden (10.8%) and Denmark (7.2%), the Netherlands is in the top ranks among OECD countries in terms of the share of the working-age population receiving DI (8.3%).<sup>4</sup> The corresponding figure for the U.S. is 5.9%. Individuals in the

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<sup>4</sup> These numbers were reported in 2007 and are obtained from the OECD project “*Sickness, Disability and Work*” ([www.oecd.org/els/disability](http://www.oecd.org/els/disability)).

Netherlands are entitled to disability benefits if an illness or infirmity prevents them from earning the amount they used to earn before the onset of the disability.<sup>5</sup> The replacement rate offered by DI depends on the “degree of disability,” which is defined by the gap between the prior earnings and the remaining potential earnings capacity of the DI applicant.

In order to explain the 1993 DI reform, we first describe how the Dutch disability insurance system determined eligibility and replacement rates prior to the reform. Prior to the reform, the potential earnings capacity was determined by the following procedure. First, a medical doctor examined the applicant and compiled a list of work activities that, according to the doctor’s judgment, the applicant could still perform.<sup>6</sup> Second, using a dictionary of occupations that specified for each occupation the required education level and work activities, a list of occupations that an applicant could still perform was compiled, but occupations that were more than two “education levels” (on a 7-level scale) below the applicant’s level of education were not considered. Third, to prevent the list of suitable occupations from containing only very rare occupations, the list was further limited to occupations with at least 10 active workers in the applicant’s region.<sup>7</sup> Because it was regarded as the responsibility of the worker to obtain a position, the occupations on the list did not need to have actual vacancies. Finally, if the list contained at least 5 suitable occupations with at least 10 active workers, then the mean wage of the 5 highest paying occupations on the list was taken as the applicant’s potential earnings capacity. The loss of earnings due to the disability, measured by the difference between the prior labor earnings and the potential earnings capacity, determined the degree of disability. If it was not possible to specify 5 suitable occupations with at least 10 workers, the degree of disability was set at 100%. The measured degrees of disability were grouped in 8 categories varying from 0-15% to 80-100%, and these categories determined the replacement rate (see Table 1).

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<sup>5</sup> Also see Bovenberg (2000), who provides useful institutional background information on the Dutch disability act.

<sup>6</sup> The list includes 27 physical activities (such as “lifting,” “kneeling,” and “ability to deal with temperature fluctuations”) and a list of 10 psychological abilities (such as “ability to work under time pressure,” “ability to perform monotonous work,” and “ability to deal with conflict”).

<sup>7</sup> The Netherlands was divided up in 5 regions and in 16 “start regions”. Alternative jobs had to be found in the “start regions” first. Only if none were available, the labor market expert could look for jobs in the neighboring regions (within one of the main 5 regions).

The DI reforms of 1993 tightened this procedure in two respects.<sup>8</sup> First, the determination of disability had to be based on objective medical information (rather than just the doctor's judgment). In other words, the applicant needed to have a clearly observable functional work limitation, and a direct relationship between the functional work limitation and the medical diagnosis had to be plausible. Disabilities due to mental health problems became more difficult to prove than physical health problems. Second, the criteria for the list of suitable alternative occupations were relaxed: (i) occupations more than two "education levels" below the applicant's education level were included from now on, (ii) the list only needed to contain 3 suitable alternative occupations (rather than 5), and (iii) the geographic region in which these occupations had to exist with at least 10 active workers was expanded roughly threefold.<sup>9</sup> With the relaxed criteria for the list with suitable alternative occupations, it became less likely that the list would not contain at least the minimum number (now 3) of occupations and that the applicant would be declared fully disabled as a consequence. By changing the criteria for what constituted suitable alternative employment, the reform aimed to lower the generosity of disability benefits and to reduce the number of claimants. A recent study by García-Gómez et al. (2009) provides some descriptive evidence that the recent reforms in Dutch DI (as of 2003) were effective in reducing the number of DI applicants.

The new procedure for determining benefits was applied to new DI applicants as well as to existing DI claimants who were 50 or younger at the time of the reform (August 1<sup>st</sup>, 1993). Because re-examinations of existing claimants are time consuming, these re-examinations were scheduled to take place by cohort over a period of several years. Disability claimants who were age 34 or younger on the 1<sup>st</sup> of August 1993 were re-examined in 1994, the 35-40 year-old cohort in 1995, the 41-44 year-old cohort in 1996/1997, and the 45-50 year-old cohort were to be re-examined in 1997-2001. However, shortly before the re-examinations for this latter age group started, political pressure led the government to decide that the 45-50 age cohort would be re-examined based on the previous and more generous procedure for determining replacement rates

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<sup>8</sup> The formal name of the 1993 UI reforms is "Terugdringing Beroep op Arbeidsongeschiktheidsverzekeringen (TBA)," which roughly translates as "Reducing claims on disability insurance."

<sup>9</sup> Now all available jobs within the main region where the individual was residing (out of 5 main regions) could be used to calculate the potential earnings capacity, rather than just jobs in one of the 16 "start regions."

rather than the new and more strict procedure. While re-examinations tended to lower benefits, this was not necessarily the case for every claimant. Some disability claimants saw their benefits rise, for example because their medical condition had deteriorated.

In this paper, we exploit the fact that disability claimants aged 44 or younger as of August 1<sup>st</sup>, 1993 were re-examined according to the new and stricter criteria, while for those aged 45 and above the old criteria applied. This allows us to investigate the effects of the reform by comparing outcomes for those slightly younger than 45 on the date of reform to those of individuals aged slightly older than 45 on that date.

Another important change of the 1993 DI reform was the introduction of an age and duration dependent benefit for new applicants.<sup>10</sup> For those already receiving a disability benefits as of August 1993, i.e. the group that we are studying here, these changes did not apply and the benefit level remained a function of the (indexed) last earned wage.

Figure 1 shows the number of DI recipients as a percentage of the labor force for the period of 1950 to 2009. Until 1993, the percentage claimants increased steadily. There is a clear dip in the percentage DI recipients for about 3 years following the 1993 reform, after which the number again starts to rise until around 2002, when a new round of reforms was introduced to further reduce the use of disability benefits.<sup>11</sup>

### **3. Data**

#### *3.1 Data sources*

This paper relies on administrative data that Statistics Netherlands has assembled from several sources. Information from these various sources is merged at the individual

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<sup>10</sup> De Jong, Lindeboom, and Van der Klaauw (forthcoming) analyze the effects on new applicants of an experiment in the Netherlands in which caseworkers in 2 (out of 26) regions were instructed to screen DI applicants more stringently. They find that stricter screening reduced DI applications, increased resumption of work (after a period of sickness), and did not affect UI take up.

<sup>11</sup> Initially the reforms only concerned the inflow into DI. In 2004, a new round of re-examinations started for all people on DI younger than 50. The complete sample in this paper was older than 50 at that time.

level by using a so-called RIN-number (which is a coded version of the Dutch equivalent of the U.S. Social Security number).<sup>12</sup>

First, we have administrative data on all disability benefits recipients aged 15-64 in the Netherlands for the period 1995-2005. The data were collected by the organizations responsible for administering disability benefits. The information from these administrative records include the start and end dates of a disability spell, the degree of disability (in categories), industry information, and the reason for ending of the disability spell, but does not contain reliable or consistent information about the medical condition that gave rise to the disability spell.

We obtain the demographic characteristics of the disability claimants from the municipal registries (“GBA”), which contain all residents of the Netherlands. This database includes information on each person’s month and year of birth, marital status, number of children, race, and place of residence, and the identification numbers (RIN-codes) of household members.

Finally, we obtain information on labor market earnings and other sources of social assistance income by merging four administrative datasets: earnings of all employees, self-employment earnings, unemployment benefits (“WW”), and general assistance (“Bijstand”). Data about social assistance come from the organizations that administer these programs. Information about the earnings from paid labor and self employment are gathered by Statistics Netherlands using information from the tax authorities and social insurance records. All these files are available from 1999 onwards, which is why 1999 is the start year for our empirical analysis of consequences of the reform on labor market outcomes and receipt of other forms of social assistance. Unemployment insurance covers any income loss due to unemployment for a duration of up to 5 years, where the duration depends on one’s work history. General assistance is unlimited in duration and does not require dependents (unlike the U.S. welfare program). Apart from the programs mentioned here, there are no additional social assistance programs in the Netherlands that are relevant for individuals in the age range of our

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<sup>12</sup> These data can be accessed via a remote-access computer after a confidentiality statement has been signed.

sample. Appendix Table A.1 provides detailed definitions of the variables used in this paper.

### *3.2 Sample definition*

In our baseline analysis, we restrict the sample to all individuals who (i) received disability benefits on August 1<sup>st</sup>, 1993, (ii) who were between the ages of 42.5 and 47.5 at that date, and (iii) who were still on DI on January 1<sup>st</sup>, 1995. The first restriction is necessary because the discontinuity in benefit rules only applies to existing claimants on the date of the reform. The second restriction limits the sample to those who are close to age 45, where the discontinuity in benefit rules occurs. We selected this bandwidth based on the Imbens-Kalyanaraman (2009) test.<sup>13</sup> The last restriction is driven by data availability. Our data on disability starts in 1995. Thus, we can only observe individuals who were on disability at the time of the passage the reform legislation if they remained on disability until 1995 or later. We believe it is highly unlikely that differential attrition occurred around the age discontinuity prior to 1995 because the re-examinations for the individuals in our sample did not start until 1996 and the government decided only in 1997 that those aged 45 or older would not be subject to the new, stricter criteria.<sup>14</sup> Moreover, the McCrary (2008) density test shows no evidence of a discontinuity in the density of disability claimants around the cutoff age of 45 (p-value = 0.126).

Our data extends until 2005, when the oldest members of our sample approach age 60. This means that we do not examine the interaction of DI with retirement decisions. Autor and Duggan (2008), and Karlström, Palme, and Svensson (2008) show that for individuals nearing retirement, retirement benefits substitute for other forms of social insurance.

We exclude all individuals that appeared on more than one disability record in our data in a given month (about 3 percent of the sample). We exclude these observations because it is not clear whether they reflect administrative/coding errors or whether they

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<sup>13</sup> The Imbens-Kalyanaraman criterion yields different optimal bandwidths for different outcome variables. Rather than changing the sample for each outcome variable, we selected a bandwidth in the middle of the optimal bandwidths suggested by the Imbens-Kalyanaraman criterion, and applied this bandwidth to all our specifications.

<sup>14</sup> Please note that the age as presented in this paper always refers to the age (in years and months) at the time of the reform (i.e., as of August 1<sup>st</sup>, 1993).

truly concern individuals who are entitled to two (or more) different disability insurance benefits because they were employed in two (or more) jobs before they became disabled. In the latter case it is hard to understand why we observe that in many of these cases there has been a health improvement (i.e., a reduction in the degree of disability) during a particular period for one of the benefit claims, but not for the other. We have checked that no discontinuity occurs at age 45 in the likelihood that an individual has more than one disability record, and are therefore not concerned that the omission or inclusion of the 3 percent of observations with multiple records would substantively affect our results. After these sample restrictions, our baseline sample contains 90,818 observations.

### *3.3 Summary statistics*

Table 2 presents summary statistics for our key variables. Panel A shows the characteristics of our sample as measured at the date of the DI reform, i.e., before the re-examinations took place. About a third of disability claimants are female and about two thirds are married. The average DI spell started in 1986, so at the time of the reform, the average claimant in our sample had been on DI for about seven years, and about a third of the our sample had been on DI for more than 10 years. Finally, about two thirds of the sample are considered to be fully disabled (having lost more than 80% of their earnings capacity) and are therefore eligible for a replacement rate of 70%. Only about 4% of the sample are considered to have lost between 55% and 80% of their earnings capacity. The remaining 30% of the sample are considered to have lost between 15% and 55% of their earnings capacity and is eligible for replacement rates between 14% and 35%. The prevalence of spells exceeding 10 years is considerably higher for female claimants, and female claimants are also markedly more likely to be fully disabled.

Panel B presents the means of our key outcome variables. While we have these variables for all years from 1999 through 2005, we only present the values for 1999 in the interest of space. In 1999, so about 1 to 3 years after the re-examinations took place for the age cohorts in our sample, 82% of those on DI in 1993 are still on DI, where being on DI in 1999 is defined as having received positive income from DI in 1999. In short, the re-examinations cannot have had a dramatic effect on DI participation, though our next section will show evidence of a clear discontinuity in exit rates around the age cutoff.

About a third of our sample is working, defined as having positive earnings (including from self-employment) in 1999. Of those who have left DI, 49% are employed, whereas 30% of those on DI are employed, so a considerable number combine DI receipt with work. The fraction of men working (42%) is more than twice as high as the fraction of females with positive labor earnings (19%). Seven percent of our sample also had social assistance income (other than from the original DI spell) in 1999. Another seven percent are not observed in any of our administrative files. Most of these individuals did not have any formal labor or social assistance income in 1999 though some of them may have been deceased or may be emigrated.

## 4. Results

### 4.1 Magnitude of the reform

To what extent did the more stringent re-examinations reduce the generosity of the DI program for the under 45 cohort? The answer to this question allows us to interpret the magnitude of the effects of the reform on earnings and on receipt of other forms of social assistance. Figure 2 shows three measures by which to gauge the magnitude of the reform: the effect on benefit amounts, the effect on replacement rates, and the effect on participation in the DI program.

Panel A plots annual disability benefit amounts in 1999, including zeros for those who have exited, by cohort. There is a clear discontinuity at the cutoff age, indicating that the more stringent examinations for the younger cohort reduced their annual DI benefits by €533 or about 12%. Panel B shows that the replacement rate, including zeros for those who exited, is 6.3 percentage points lower for the affected cohort at the discontinuity.<sup>15</sup> The average replacement rate for those who just escaped the more stringent re-examinations is 0.52, so the 6.3 percentage point drop represents a 12 percent decline. Panel C shows that the fraction of the sample that is still on the original DI spell in 1999 falls discontinuously by 5.2 percentage points at the age cutoff.

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<sup>15</sup> The data do not contain the post-reform replacement rate for those who exited from DI. Based on discussions with the DI administration, our impression is that most exits occurred for those who were no longer eligible for DI, but we cannot rule out that some of those who exited were still eligible for a positive replacement rate.

In general, our design does not allow us to distinguish the effects of the reform on those staying on DI from those leaving DI because our data does not include outcome or benefit measures from before the reform. However, because the maximum replacement rate is 0.7, we know that at most 3.6 percentage point ( $=0.7*0.052$ ) of the 6.3 percentage point drop in the replacement rate can be attributed to those who left DI. Thus, the reforms must also have led to a reduction in replacement rates of the affected cohort remaining on DI. In short, Figure 2 shows that the more stringent re-examinations roughly translate into a 12% benefit reduction. The effects of the reform on labor supply and other benefit receipt should be viewed in light of this magnitude.

The reforms led to somewhat larger reductions in benefits and replacement rates for men (reductions by 13%) than for women (reductions by 9%), but induced 7.2 percent of female recipients to exit but only 4.2 percent of male recipients. It is not clear what drives these differences, but many factors (types of jobs, types of disabilities, outside options) obviously differ by gender, and it is therefore plausible that the reform had a differential impact by gender. In light of this differential impact, we will split out our key results by gender.

As a partial check on our identifying assumption that no factors besides the DI re-examinations had a discontinuous impact at age 45, Figure 3 reports the DI exit rates separately for 1995, 1996/97, and 1998. Exit is defined as the date for the end of the original DI spell, as recorded in the administrative data file, occurring during the year in question.<sup>16</sup> We calculate these rates as fractions of DI claimants in our sample on January 1<sup>st</sup>, 1995. Since no re-examinations took place in 1995 for DI claimants aged 40 or older, a discontinuity at age 45 in the 1995 exit rate would invalidate our identifying assumption. Reassuringly, the 1995 exit rate shows no sign of a discontinuity at age 45. In 1996 en 1997, all individuals aged 40-44 were re-examined as well as some of the 45 year olds. Exactly in these years, the discontinuity at age 45 becomes apparent. In 1998,

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<sup>16</sup> This means that exit is not equal to one minus the DI participation rate recorded in Figure 2C because for DI participation we also required that the person received a positive amount of DI benefits in the year in question. Some former DI recipients are still in the administrative records because they are repaying excess DI received in the past. Because we do not have DI amounts for 1995, we cannot define our participation variable for 1995.

the remainder of the 45 year olds and some of the 46 year olds were re-examined, which explains the small but statistically significant discontinuity in the opposite direction.<sup>17</sup>

#### *4.2 Reduced-form impacts on labor market and social assistance outcomes*

To what extent did individuals who left DI because of the reform end up in other social assistance programs and to what extent did they find paid work? The answer to this question is critical for judging the effectiveness of the reform. In the former case, the reform shuffles individuals across programs and budgetary savings only occur to the extent that benefits in other programs are lower than DI benefits. In the later case, not only can the government reduce DI expenditures, but it will also collect additional tax revenue. In this subsection, we examine labor market and social assistance outcomes in 1999, which is the first year for which we have the required data and which is about two years after the re-examinations took place. In subsection 4.6, we will examine the effects of a longer horizon.

We start by analyzing the reduced-form effects of the DI reform on labor market outcomes. The first panel of Figure 4 plots labor earnings (including self-employment income) in 1999 by cohort. The figure shows a discontinuity in earnings at the cutoff age but the discontinuity is not as visually compelling as in the earlier figures due to the higher variance in earnings. The RD regression, however, estimates that at the cutoff age, earnings are €506 per year higher for those who were subject to the more stringent re-examinations, and this estimate is highly significant. The €506 increase represents an 8 percent increase in earnings. This figure establishes our qualitative finding that disability income crowds out labor income. We will discuss the economic magnitude of our crowd-out estimate in the next subsection.

Because we do not have earnings for prior years, we cannot precisely determine to that extent the average increase in earnings stems from non-workers finding employment (extensive margin) and from workers increasing their earnings (intensive margin). However, at least some of the increase comes from the extensive margin because the second panel of Figure 4 shows a clear discontinuity in the fraction of individuals with

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<sup>17</sup> It would be instructive to do similar checks on the identification strategy with our key outcome variables: labor income and income from other social assistance programs. Unfortunately, we do not have data on these variables prior to 1999.

strictly positive income from wages or self-employment. The RD regression estimates that the more stringent re-examinations caused the fraction working to increase by 2.6 percentage points. To explain the observed increase in earnings in the absence of an intensive-margin labor supply response, average earnings for those who started working again would need to be €19,500 ( $=506/0.026$ ) per year, which is higher than the observed average earnings for those with positive earnings (€17,500/yr). It therefore seems likely that some of the response also occurred along the intensive margin. Our finding that the reduction in the generosity of DI increases labor supply contributes to the extensive literature on the labor supply disincentive effects of disability insurance (see, e.g., Parsons 1980, Bound 1989, or, Gruber 2000; see Bound and Burkhauser 1999 for an overview; see Chen and Van der Klaauw 2008 for estimates based on an RD design).

As Figure 4 showed, the reform caused an increase in labor supply, but it may also have shifted DI recipients to other forms of social assistance. The first panel of Figure 5 plots income from social assistance (including income from new DI spells, but excluding income from the original DI spell) by cohort. The figure shows a clear upward jump in income from other social assistance for the cohort that underwent the more stringent re-examinations. In fact, the RD regression estimates that the reform increased other social assistance income by a €272 per year. The second panel shows that the fraction receiving social assistance income from a source other than the original DI spell discontinuously increases by 4.9 percentage points at the age cutoff for the more stringent re-examinations. Both increases are highly significant and represent about a doubling of amounts and participation rates. In other words, we find clear evidence of substitution of other forms of social assistance for DI benefits.

#### *4.3 Crowd-out and Social Support Shifting*

Figures 4 and 5 established that DI benefits crowd out labor income and that people substitute between DI income and other forms of social assistance. We now turn to the economic magnitudes of earnings crowd out and shifting of social assistance. In the first column of Table 3, we scale our outcome measures by the amount by which disability benefits from the original spell decrease at the age discontinuity whereas in the second column we scale our outcome measures by the discontinuity in the replacement

rate. We implement this scaling by running IV regressions following the standard “fuzzy RD” specification.<sup>18</sup> Given that the re-examination was in all respects more stringent for those below the cutoff age, the monotonicity assumption required for the fuzzy RD design should be satisfied; being subject to a re-examination following the more stringent new protocol rather than the old protocol weakly decreases the benefit amount for everyone and weakly decreases the replacement rate for everyone. The estimate in the first row of column 1 of panel A indicates that per Euro of benefits decrease caused by the reform, the reform induces individuals to increase earnings by €0.95 in 1999. In other words, we find an earnings crowd-out ratio of 0.95: a Euro of DI benefits crowds out 95 cents of labor earnings. The second row of Panel A examines the extensive margin response and shows that, per €1000 of disability benefits decrease caused by the reform, the probability of being employed in 1999 increases by 4.8 percentage points. The second column presents the analogous estimates, but now scaled by the change in replacement rates caused by the more stringent re-examinations. We find that for a 10-percentage point decrease in replacement rates, earnings increased by €800 per year (or about 13 percent) and the probability of employment increases by 4.1 percentage points. All four estimates in panel A are highly statistically significant and establish that the degree to which DI benefits crowd out labor market earnings and participation is economically meaningful.

Panel B of Table 3 examines to what extent the reduced generosity of disability benefits causes individuals to shift to other forms of social assistance (including new DI spells). The first row of column 1 of panel B shows that for each Euro in decreased disability benefits, individuals receive €0.51 more from other social assistance program in 1999. Thus, the benefit shifting ratio is 0.51.<sup>19</sup> A government not taking this shifting into account would overestimate the reduction in government expenditure from tightening the DI eligibility rules by a factor of 2. The second row shows that per €1000 per year decrease in DI benefits caused by the more stringent rules, the probability that an individual receives income from another social assistance program increases by 9.2

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<sup>18</sup> Excellent discussions of the theoretical underpinnings and the practical application of RD methods can be found in Hahn, Todd, and Van der Klaauw (2001), Imbens and Lemieux (2008), and Lee and Lemieux (2010).

<sup>19</sup> Most of the shifting happens towards unemployment insurance, a sizeable fraction occurs through general assistance, while only a small fraction is due to re-entry into DI.

percentage points. Another way of scaling the degree of shifting between social assistance programs is provided in the second column, which shows that for a 10-percentage point reduction in disability replacement rates, income from other social assistance programs increases by about € 400 per year (nearly tripling) and the probability of participation in other social assistance programs increases by 7.8 percentage points. The estimates of panel B show that benefit shifting is not only statistically significant but also large in economic terms.

Panel C presents the combined effect of crowd out and shifting. The estimate in the first row and column indicates that individuals increased income from other social assistance and work by €1.46 per Euro of DI benefits lost. In other words, the point estimate indicates that individuals more than completely offset the decrease in DI benefits by increased income from other sources, though we cannot reject the hypothesis that the offset was less than full (p-value 0.104). More than full offset is consistent with the fact that individuals have direct and opportunity costs associated with employment. The second row shows the effect on a dummy for working or receiving income from a social assistance program other than the original DI spell. We find that per €1000 decrease in DI, an individual is 10.9 percentage points more likely to obtain income from some other source. The fact that this estimate is less than the sum of the estimates in rows 2 of panels A and B indicates that the reform induced some individuals to both work and draw income from other forms of social assistance. In particular, per €1000 decrease in DI, individuals became 3.1 ( $=4.8+9.2-10.9$ ) percentage points more likely to have both income from other social assistance programs and labor income in 1999.

#### *4.4 Effects by Gender and Degree of Disability*

Table 4 splits out the results of the first column of Table 3 by gender. While the estimates for men and women are never different at the 5-percent significance level, the table does indicate that earnings crowd out appears to be stronger for men and benefit-shifting effects appear to be stronger for women. In particular, the point estimate of the crowd-out effect is much larger for men than for women (1.21 vs. 0.17) and this difference is marginally statistically significant (p-value 0.089). Conversely, benefit shifting seems to be more important for women. Per €1000 decrease in disability

benefits, the probability of participating in another social assistance program increases by 13.4 percentage points for women but only by 8.2 percentage points for men, and this difference is marginally significant (p-value 0.086). As noted above, it is hard to determine what drives the differences in the effects of the DI reform on men and women, but we suspect differences in initial DI benefit levels, differences in types of disabilities, and differences in opportunities in market and household production are likely explanations.

Table 5 analyzes earnings crowd out and benefit shifting by degree of disability. The degree of disability is measured in 1995, so before the re-examinations took place. We find high rates of labor crowd out both for partially disabled and fully disabled recipients. The point estimate of crowd out is slightly higher for the partially disabled than for the fully disabled (0.85 vs. 0.75), but this difference is not statistically significant. The high degree to which the fully disabled are able to replace foregone disability income with labor income is striking, though it should be kept in mind that degree of disability depended on the availability of suitable jobs in the applicant's region and that the applicant was classified as fully disabled if not enough of such jobs were found.

Table 5 further finds that the degree of benefit shifting is much lower among the partially disabled than among the fully disabled. For each Euro of decreased DI benefits, the partially disabled collect 20 cents more in other forms of social assistance. In contrast, this figure is 79 cents for the fully disabled, and statistically significantly higher. Because about two thirds of the partially disabled have some earnings but only about 15 percent of fully disabled do, the fully disabled are more likely to be able to qualify for means-tested alternative sources of social assistance, such as general assistance.

#### *4.5 Responses of Partners of DI recipients*

In Table 7, we provide estimates of earnings crowd out and benefit shifting that account for the responses of partners of (former) DI recipients. We find that our point estimates of crowd out and benefit shifting are larger once we take the partner's response into account: crowd out increases from 0.95 to 1.57 and benefit shifting increases from 0.51 to 1.13. The increase for crowd out, while substantial in economic terms, is not statistically significant. An earnings response of partners is consistent with Cullen and

Gruber (2000) who estimate that increased UI benefits paid to unemployed males are largely offset by decreased labor market earnings of their wives. The increase in benefit shifting is statistically significant, but almost too large to be plausible. In ongoing work, we are further investigating this surprising result. Overall, we believe Table 6 indicates that partner responses can be economically meaningful. As a result, any analysis that ignores these effects could easily produce faulty predictions or policy advice.

#### *4.6 Responses over time*

Responses to reductions in DI benefits are likely to vary over time because it can take time to find the right match in the labor market or because certain forms of social assistance have time limits. Hence, focusing only 1999, the first year that re-examinations are completed for individuals near each side of the age discontinuity, yields an incomplete picture of the consequences of exit. We therefore repeated our main analyses for all years until 2005, which is the last year in our dataset. Incidentally, looking beyond 2005 is less informative because at that point some members of our sample are starting to reach an age where early retirement is quite common in the Netherlands. While additional DI reforms took place in 2002 and 2004, these reforms had no impact on the people in our sample. The first reform only affected new entrants while the second reform led to a re-examination of people on DI who were younger than 50 on July 1<sup>st</sup>, 2004. All individuals in our sample were older than 50 at that time. These reforms therefore do not affect our estimates.

Figure 6 shows our estimates of the earnings crowd-out ratio and the benefit-shifting ratio for each of the years from 1999 to 2005. In other words, the figure plots the coefficients from the same fuzzy RD IV regression that we presented in the first row of Panels A and B of Table 3, but now for all years until 2005. The top panel shows the results for the entire sample while the bottom two panels split out the results by gender. We find that both the earnings crowd-out ratio and benefit-shifting ratio are positive and statistically significant for the full sample in each year. The degree to which individuals replace foregone DI benefits with labor earnings increases slightly over time, but this slight upward trend is not statistically significant. In contrast, the benefit-shifting ratio declines from 0.51 in 1999 to 0.21 in 2005, and this clear downward trend is statistically

significant. The decline of the benefit-shifting is consistent with the fact that unemployment assistance is only available for a limited duration. General assistance, however, has no time limits, which explains why the benefit-shifting ratio can remain positive even in the long term. The separate figures for men and women roughly display the same patterns as the overall figure.

## **5. Conclusion**

In this paper, we investigate the consequences of a reduction in the generosity of one social support program when that program is part of a larger system of social assistance programs. Especially in the case of social assistance to people in their prime age, it was unknown to what extent reduced generosity of one program induces them to increase labor supply and to what extent it leads them to use other social assistance programs instead. This question is of obvious policy relevance in many countries. While existing studies have investigated spillover effects among programs for children or for people close to retirement, this paper examines benefit shifting and earnings crowd-out effects for people on DI in their mid 40s. Moreover, it is important to recognize that spillovers from a reform to one program can be partly driven by responses by the partners of people affected by the reform and that the spillovers may vary with the amount of time passed since the reform.

The combination of access to extensive administrative panel data and the presence of an age-discontinuity in a reform law allows us to produce causal estimates of the effect of the 1993 Dutch disability insurance reform on the participation in other social assistance programs. We find economically meaningful and statistically significant evidence of social support shopping. About 2 years after the implementation of the DI reform for our sample members, each Euro of reduced DI benefits causes an increase in income drawn from other social assistance programs by 51 cents. Thus, ignoring this benefit-shifting effect of 51% would lead one to overestimate the cost savings of the DI reform by factor of 2. Because the reform also causes the partners of affected DI recipients to become more likely to claim social assistance, the benefit-shifting ratio is even higher when we define benefit shifting to include the partner response. While the

benefit-shifting ratio decreases over time, the individual-level benefit-shifting ratio still stands at 21% about 8 years after the implementation of the reform for our sample.

Benefit shifting estimates would obviously be different in different settings, but the direction in which the estimates would change is not clear. Our benefit-shifting figure may be higher than it would be in other countries because the Netherlands has a relatively generous system of alternative social assistance programs. On the other hand, the reform we analyzed concerned a relatively minor reduction of DI generosity. Thus, many of those affected by the reform may not have qualified for means-tested alternative forms of social assistance, or alternative forms of social assistance may still have been less attractive than DI (despite the reduction in DI generosity).

While our specific coefficient estimates only directly apply to this particular Dutch DI reform, we believe our paper offers three general lessons that are widely applicable. First, our paper provides strong evidence that spillover effects between social assistance programs can be substantial, also for prime-aged individuals. Thus, any analysis of a reform of a social assistance program would be wise to consider the possibility of benefit shifting. Second, we show that the spillover effects in part operate through the partners of the individuals directly affected by the reform. It is therefore important to consider possible reactions by partners when estimating the benefit-shifting ratio. Finally, we show that the degree of benefit shifting varies with time passed since the reform. An estimate of benefit shifting based on behavior shortly after the reform can therefore provide a misleading estimate of the cost of the spillover effects over the medium to long run.

Because the discontinuity in the stringency of disability reform applies to existing recipients, we can only examine social support shopping among those already receiving disability insurance at the time of the reform. Our setting does not allow us to estimate spillover effects stemming from people who would have claimed disability insurance under less stringent rules, but who take up another form of social assistance because of the increased stringency of the DI benefit rules. We view such spillover effects as complementary to our estimates of benefit shifting. We see the estimation of these complementary spillover effects as an important area for further research.

## Literature

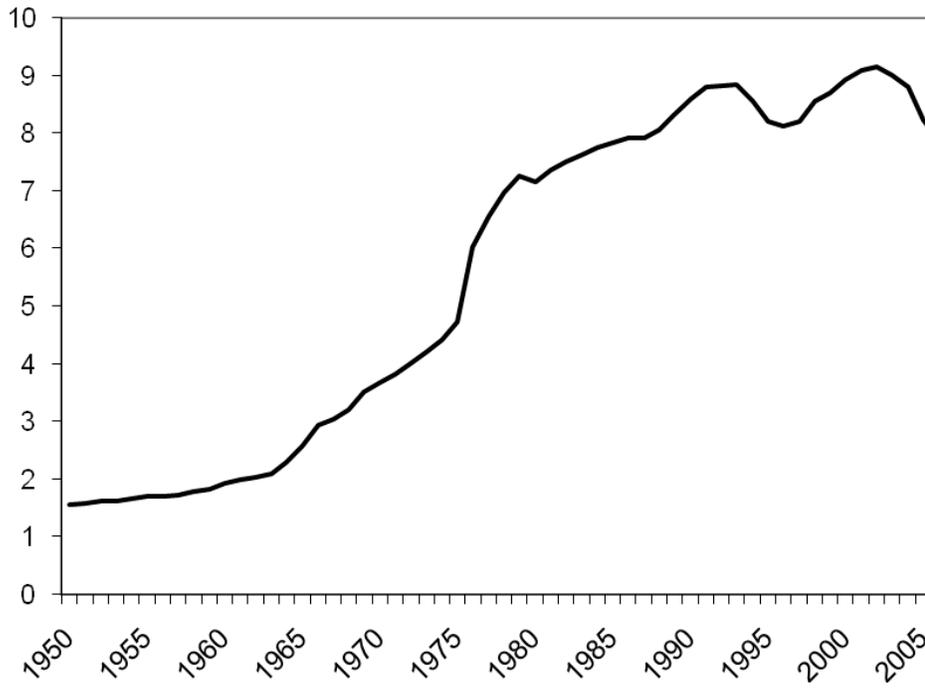
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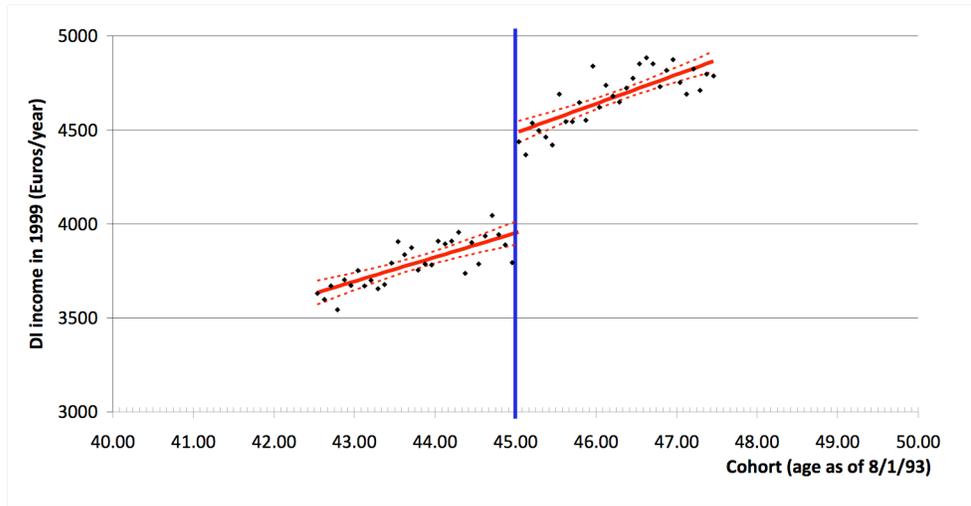
**Figure 1: Percent of the labor force receiving DI benefits, 1950-2009.**



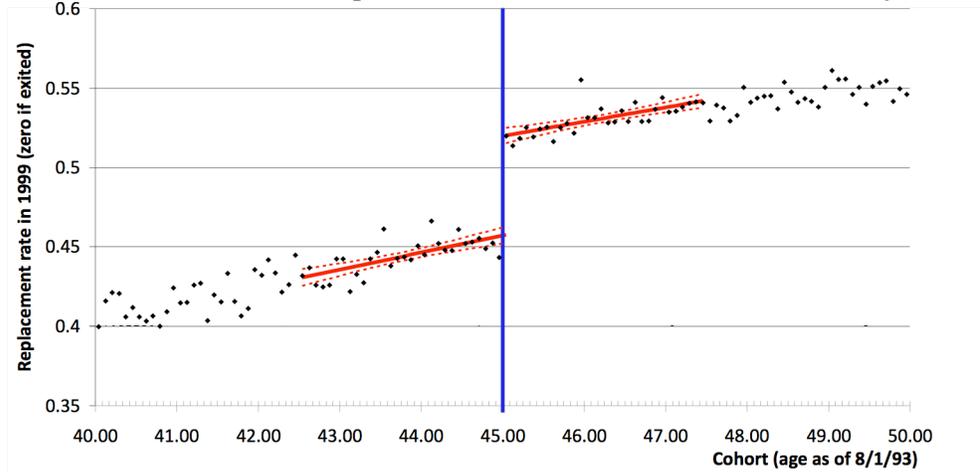
Source: Statline Statistics Netherlands

**Figure 2: Magnitude of the reform**

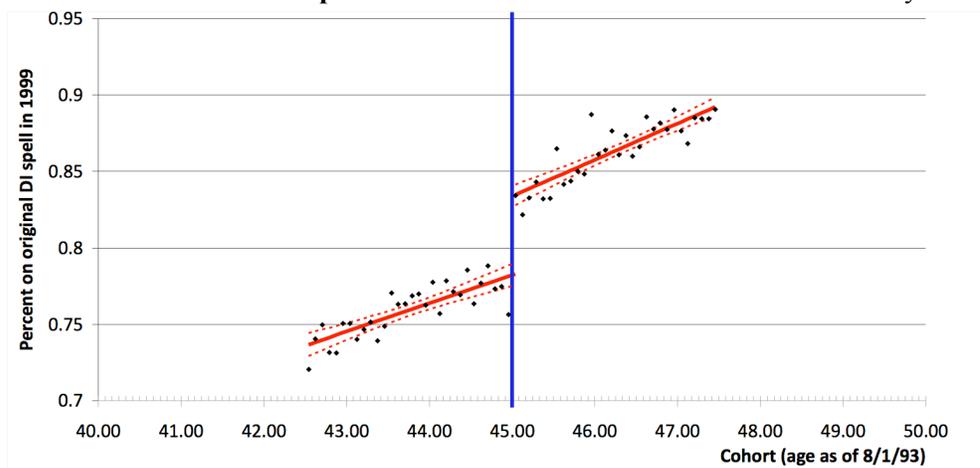
**Panel A: Effect on DI Benefit Amounts.** Estimate of the discontinuity: -533 (44)\*\*\*



**Panel B: Effect on the DI Replacement Rate.** Estimate of the discontinuity: -0.063 (0.004)\*\*\*



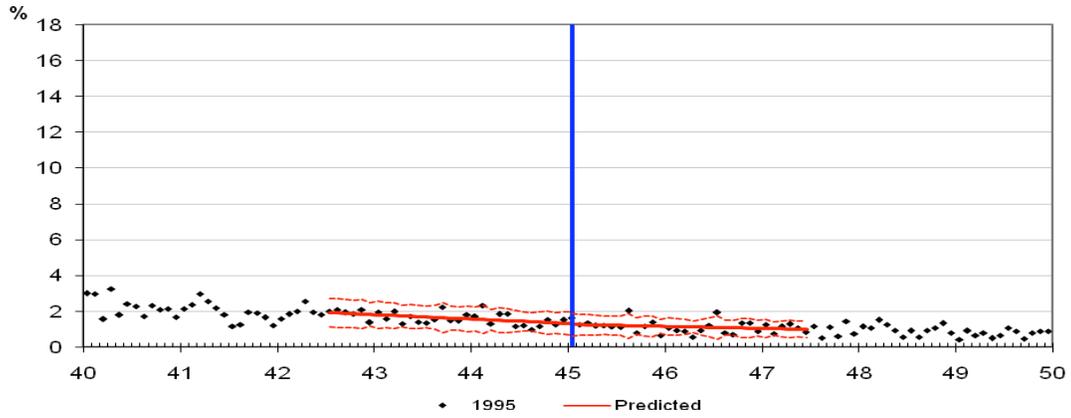
**Panel C: Effect on Participation in DI in 1999.** Estimate of the discontinuity: -0.052 (0.005)\*\*\*



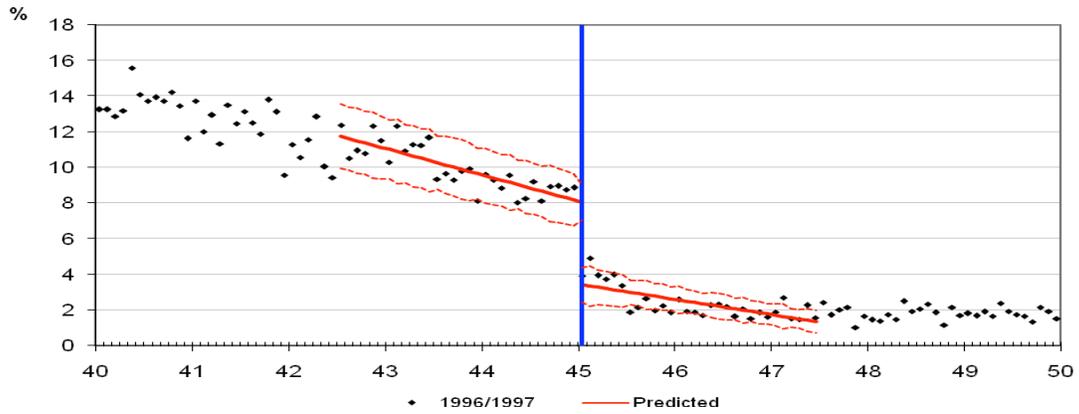
Note: Each figure is based on 90,818 observations. The dotted lines represent the 95% confidence intervals. Standard errors are in parentheses.

**Figure 3: Exit rate out of DI in 1995, 1996/7 and 1998 – full sample**

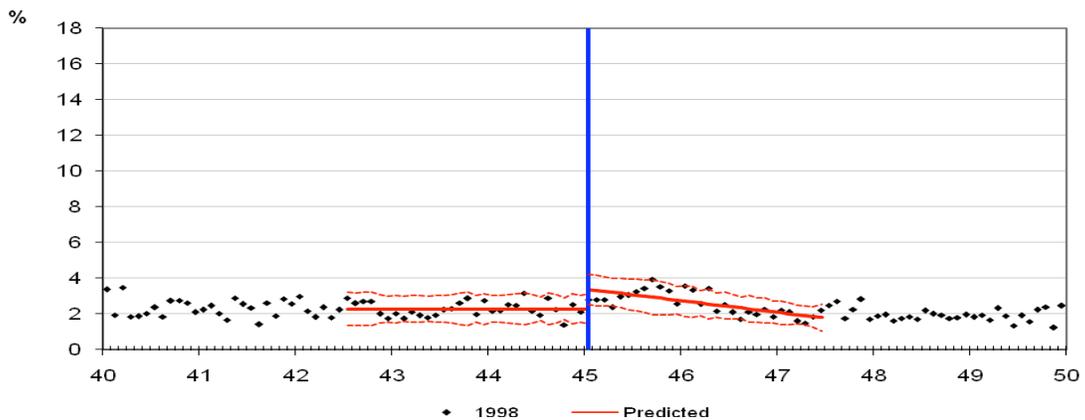
**Panel A: Exit in 1995.** Estimate of the discontinuity: 0.001 (0.002)



**Panel B: Exit in 1996/1997.** Estimate of the discontinuity: 0.046 (0.003)\*\*\*



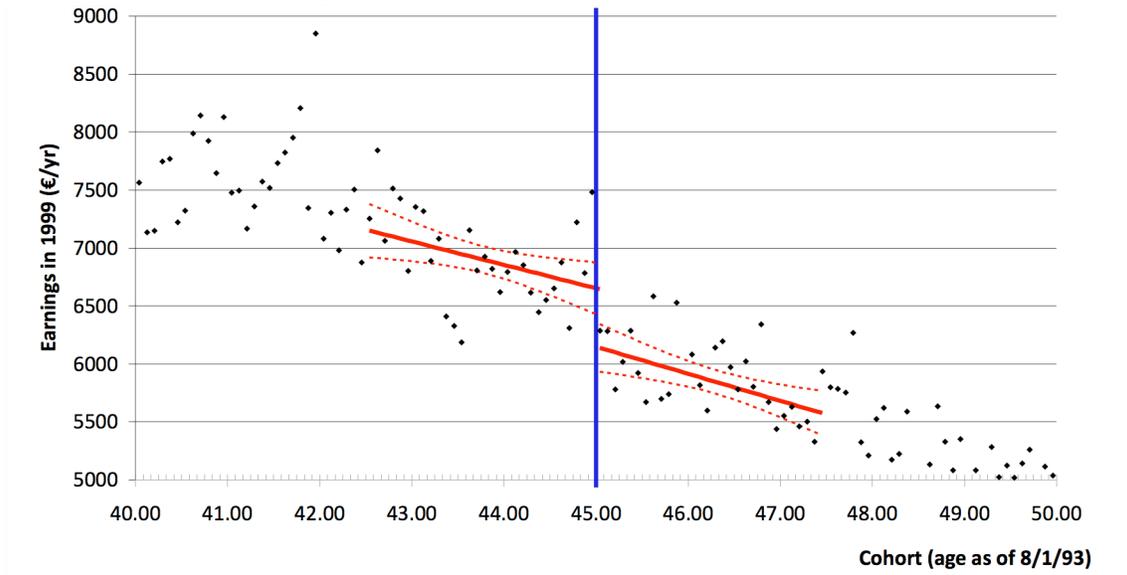
**Panel C: Exit in 1998.** Estimate of the discontinuity: -0.011 (0.002)\*\*\*



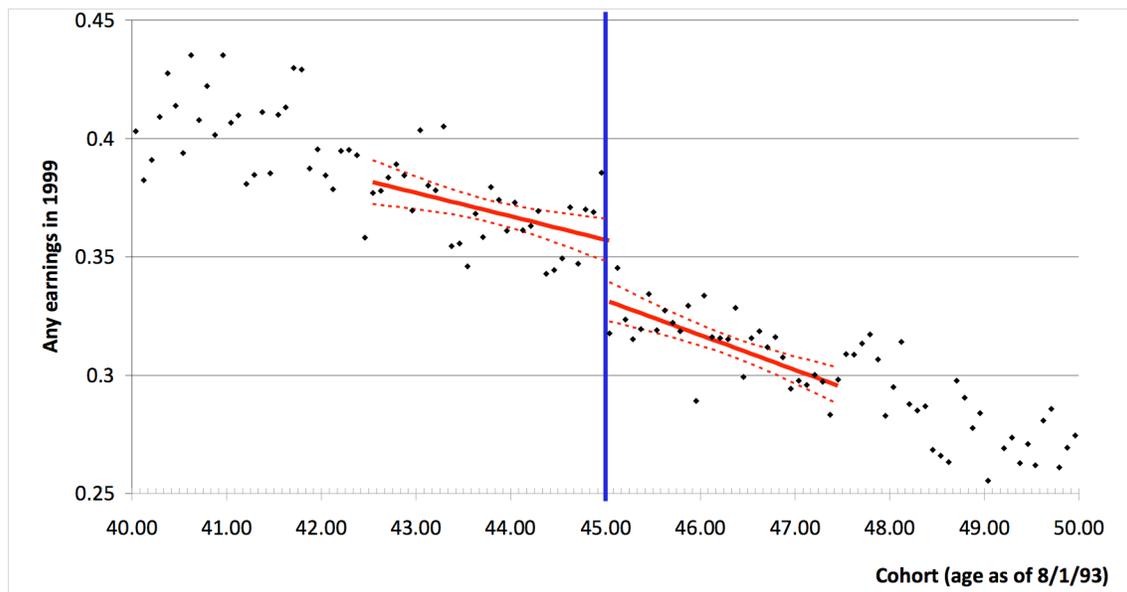
Note: The figures are based on 90,818 observations. The dotted lines represent the 95% confidence intervals. The exit rate is defined as a fraction of our sample in 1995. Standard errors are in parentheses.

**Figure 4: Effects of DI Reform on Labor Market Outcomes**

**Panel A: Effect on Earnings in 1999.** Estimate of the discontinuity: 506 (158)\*\*\*



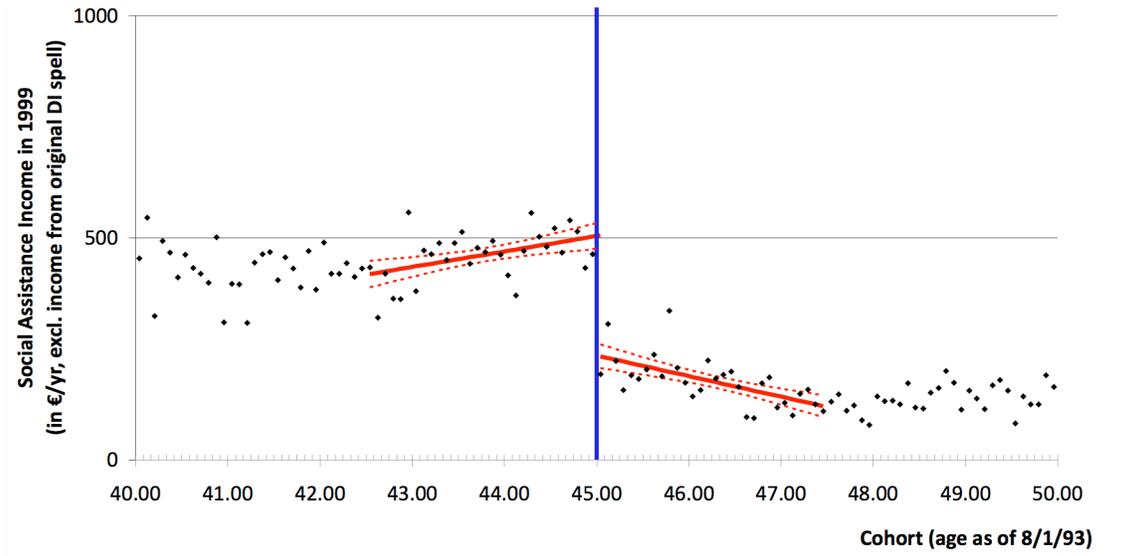
**Panel B: Effect on Employment in 1999.** Estimate of the discontinuity: 0.026 (0.006)\*\*\*



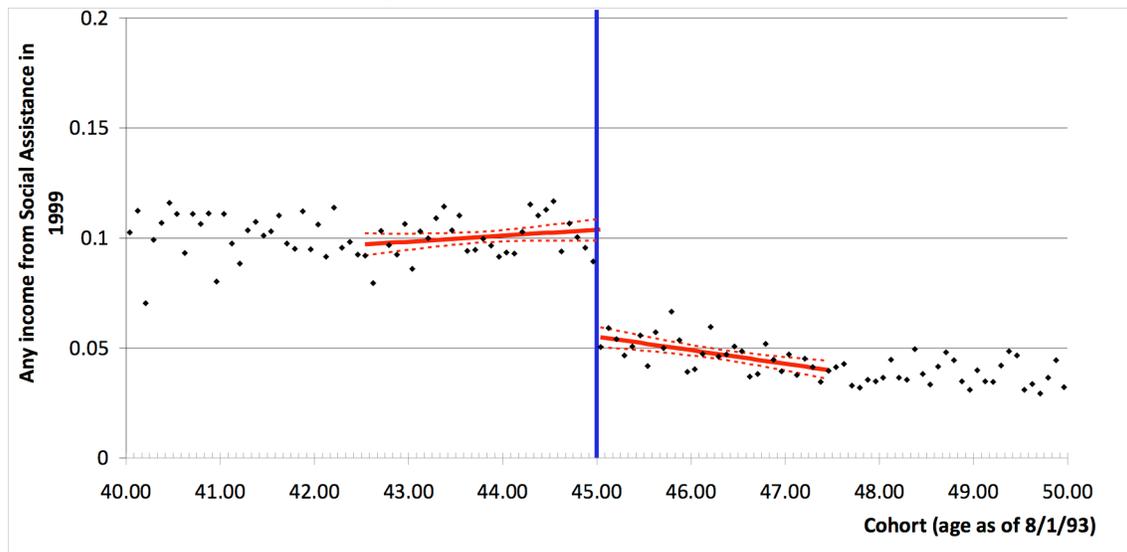
Note: Figures are based on 90,818 observations. The dotted lines represent the 95% confidence intervals. Employment is defined as having positive earnings from employment or self-employment. Standard errors are in parentheses.

**Figure 5: Effects of DI Reform on Other Social Assistance**

**Panel A: Social Assistance Income in 1999.** Estimate of the discontinuity: 272 (21)\*\*\*



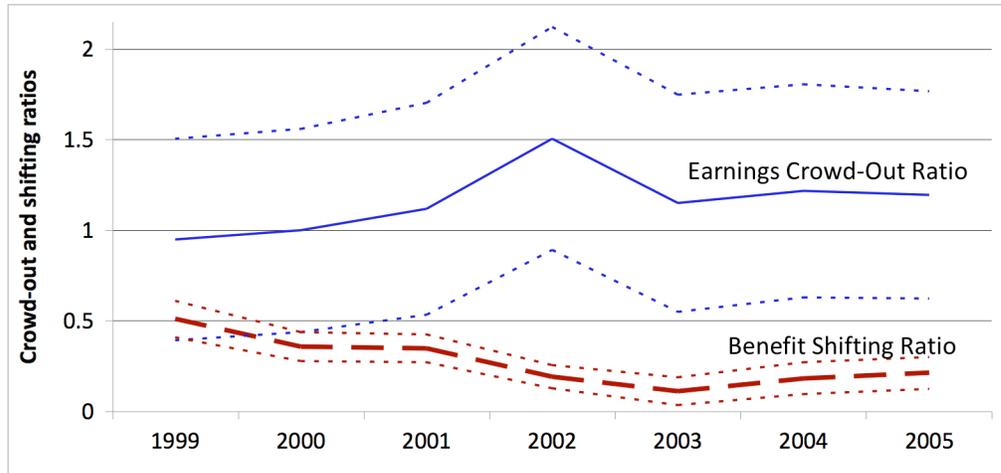
**Panel B: Social Assistance Reciprocity in 1999.** Estimate of the discontinuity: 0.049 (0.003)\*\*\*



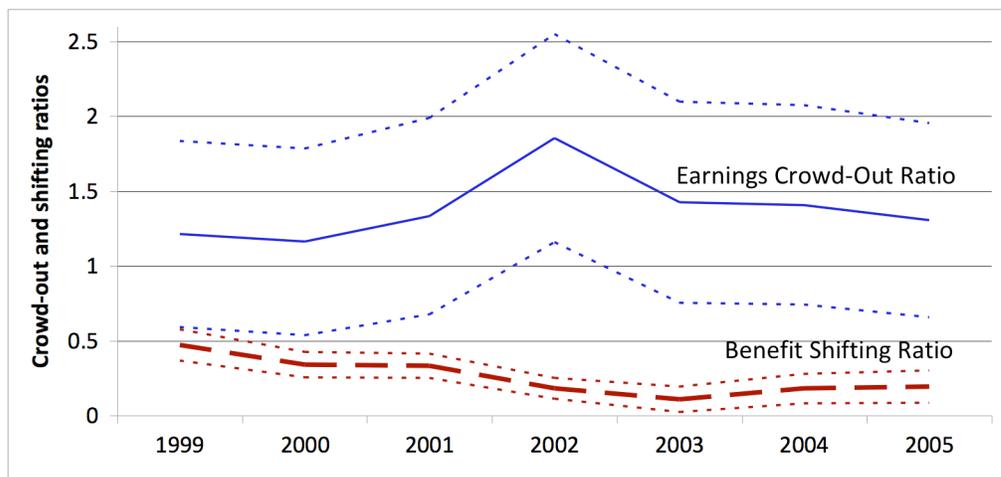
Note: Figures are based on 90,818 observations. The dotted lines represent the 95% confidence intervals. Benefits from the original DI spell are not included in social assistance income and reciprocity rates exclude the original DI spell. Standard errors are in parentheses.

**Figure 6: Earnings Crowd Out and Benefit Shifting over Time**

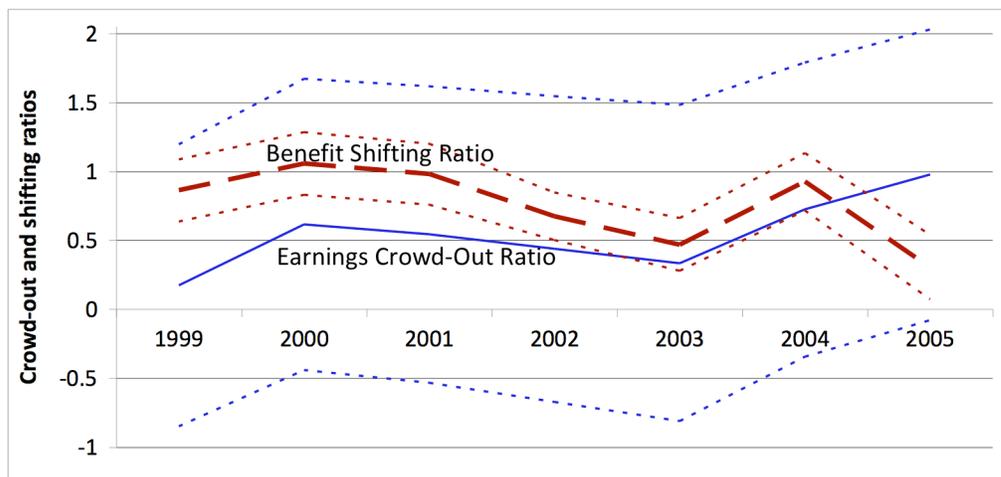
**Panel A: Full Sample**



**Panel B: Males**



**Panel C: Females**



Note: The dotted lines represent the 95% confidence intervals.

**Table 1: Relation between Degree of Disability and Replacement Rates**

Degree of disability:	Replacement rate (% of last earned wage):
80 – 100 %	70 %
65 – 80 %	50.75 %
55 – 65 %	42 %
45 – 55 %	35 %
35 – 45 %	28 %
25 – 35 %	21 %
15 – 25 %	14 %
Less than 15 %	0 %

Source: UWV (2006). UWV is the abbreviation of the agency that administers all social insurance for employees in the Netherlands. See text for a description of how the degree of disability is determined. Disability insurance benefit levels are determined as a percentage of the last earned wage and adjusted for inflation over time.

**Table 2: Descriptive Statistics**

	Full sample	Males	Females
<b>Panel A: Sample characteristics before re-examination</b>			
Female (0=no; 1=yes)	0.34	0.00	1.00
Married (0=no; 1=yes)	0.66	0.69	0.60
Age on August 1 <sup>st</sup> , 1993	45.16	45.17	45.14
Start date or DI spell	1986.06	1985.69	1986.76
<i>Duration on DI (as of August 1993):</i>			
Less than 5 years	30.75	33.46	25.56
5-10 years	24.67	25.28	23.51
10+ years	44.57	41.26	50.94
<i>Degree of disability (% of earnings capacity lost):</i>			
15-25	6.92	7.91	5.0
25-35	9.38	11.64	4.96
35-45	7.19	9.13	3.39
45-55	6.19	6.8	5.0
55-65	2.01	2.21	1.61
65-80	1.91	2.35	1.05
80-100	66.4	59.96	78.99
<b>Panel B: Outcomes after re-examination</b>			
<i>Labor market status in 1999 (%):</i>			
Still on DI (on the original spell)	81.84	82.15	81.24
Employed	33.74	41.62	18.60
Social assistance (other than original DI spell)	7.05	6.58	7.96
Zero income (dummy for no formal income)	6.63	5.71	8.38
<i>Income by source in 1999, €/yr (including zeros):</i>			
DI from original DI spell	4,292	4,674	3,559
Earnings	6,306	8,210	2,647
Social assistance (other than original DI spell)	301	294	313
<i>Income by source in 1999, €/yr (if non-zero):</i>			
DI from original DI spell	5,255	5,702	4,385
Earnings	17,427	18,295	13,598
Social assistance (other than original DI spell)	4,411	4,647	4,035
N	90,818	59,726	31,092

Note: Since we have information available from 1995 onwards, both marital status and degree of disability are recorded in January 1995 (before the re-examinations).

**Table 3: Earnings Crowd Out and Benefit Shifting**

	Effect scaled by decrease in amount of original DI (in '000 €/yr)	Effect scaled by decrease in the replacement rate (fraction)
<b>Panel A: Labor market outcomes in 1999</b>		
Earnings	0.948 (0.284) <sup>***</sup>	8.083 (2.353) <sup>***</sup>
Employment dummy	0.048 (0.012) <sup>***</sup>	0.411 (0.093) <sup>***</sup>
<b>Panel B: Other social assistance in 1999</b>		
Income from other social assistance	0.510 (0.051) <sup>***</sup>	4.347 (0.371) <sup>***</sup>
Participation dummy	0.092 (0.009) <sup>***</sup>	0.782 (0.062) <sup>***</sup>
<b>Panel C: Total</b>		
Income except from original DI spell	1.458 (0.282) <sup>***</sup>	12.430 (0.227) <sup>***</sup>
Dummy for work or other soc. asst.	0.109 (0.013) <sup>***</sup>	0.931 (0.089) <sup>***</sup>

Note: Standard errors are in parentheses. Significance levels: \* 10 percent; \*\* 5 percent; \*\*\* 1 percent. Each entry in the table comes from a separate IV regression based on the fuzzy RD design. The dependent variable is listed in the rows. Other social assistance only includes disability income from re-entry in to disability (so it excludes disability income from the original spells). The variable that is instrumented (endogenous explanatory variable) is listed in the columns. The instrument itself is the discontinuity at the cutoff age (45 as of 8/1/93). Earnings and income are measured in thousands of Euros per year. The replacement rate is expressed as a fraction. Each regression is based on 90,818 observations.

**Table 4: Earnings Crowd Out and Benefit Shifting by Gender**

	Effect of reform per 1000 €/yr decrease in amount of original DI		P-value gender difference
	Males	Females	
<b>Panel A: Labor market outcomes in 1999</b>			
Earnings	1.213 (0.318)***	0.174(0.522)	0.089
Employment dummy	0.046 (0.012)***	0.068(0.028)*	0.464
<b>Panel B: Other social assistance in 1999</b>			
Income from other social assistance	0.472 (0.053)***	0.688(0.155)***	0.188
Participation dummy	0.082 (0.009)***	0.134(0.029)***	0.086
<b>Panel C: Total</b>			
Income except from original DI spell	1.685 (0.313)***	0.862(0.522)*	0.176
Dummy for work or other soc. asst.	0.100 (0.012)***	0.158(0.037)***	0.136

Note: Standard errors are in parentheses. Significance levels: \* 10 percent; \*\* 5 percent; \*\*\* 1 percent. Each entry in the table comes from a separate IV regression based on the fuzzy RD design. The dependent variable is listed in the rows. Income and earnings are measured in thousands of Euros per year. The variable that is instrumented (endogenous explanatory variable) is the amount of DI, so all coefficients can be interpreted as effect size per €1000/yr decrease in DI. The instrument itself is the discontinuity at the cutoff age (45 as of 8/1/93). The regressions are based on 59,726 and 31,092 observations for males and females, respectively.

**Table 5: Earnings Crowd Out and Benefit Shifting by Degree of Disability**

	Effect of reform per 1000 €/yr decrease in amount of original DI			P- value gender dif.
	Full sample	Males	Females	
<i>I. Partially Disabled in 1995</i>				
<b>Panel A: Labor market outcomes in 1999</b>				
Earnings	0.850 (0.489)*	1.066 (0.571)*	0.042 (0.802)	0.298
Employment dummy	0.031 (0.016)*	0.022 (0.017)	0.065 (0.036)*	0.273
<b>Panel B: Other social assistance in 1999</b>				
Income from other social assistance	0.203 (0.052)***	0.147 (0.059)**	0.404 (0.117)***	0.000
Participation dummy	0.059 (0.011)***	0.054 (0.012)***	0.074 (0.023)***	0.444
<b>Panel C: Total</b>				
Income except from original DI spell	1.053 (0.482)**	1.213 (0.562)**	0.446 (0.792)	0.430
Dummy for work or other soc. asst.	0.066 (0.015)***	0.055 (0.016)***	0.102 (0.038)***	0.255
N	31,060	24,306	6,754	
<i>II. Fully Disabled in 1995</i>				
<b>Panel A: Labor market outcomes in 1999</b>				
Earnings	0.747 (0.265)***	0.979 (0.308)***	0.278 (0.526)	0.250
Employment dummy	0.052 (0.015)***	0.053 (0.016)***	0.070 (0.036)*	0.677
<b>Panel B: Other social assistance in 1999</b>				
Income from other social assistance	0.788 (0.101)***	0.795 (0.107)***	0.934 (0.314)***	0.675
Participation dummy	0.123 (0.015)***	0.113 (0.015)***	0.186 (0.060)***	0.233
<b>Panel C: Total</b>				
Income except from original DI spell	1.535 (0.293)***	1.770 (0.330)***	1.212 (0.610)**	0.421
Dummy for work or other soc. asst.	0.138 (0.020)***	0.130 (0.020)***	0.207 (0.067)***	0.273
N	59,758	35,420	24,338	

Note: Standard errors are in parentheses. Significance levels: \* 10 percent; \*\* 5 percent; \*\*\* 1 percent. Each entry in the table comes from a separate IV regression based on the fuzzy RD design. The dependent variable is listed in the rows. Income and earnings are measured in thousands of Euros per year. The variable that is instrumented (endogenous explanatory variable) is the amount of DI, so all coefficients can be interpreted as effect size per €1000/yr decrease in DI. The instrument itself is the discontinuity at the cutoff age (45 as of 8/1/93). Degree of disability is as determined by the disability administration (see text for the description of the procedure for the determination of degree of disability).

**Table 6: Earnings Crowd Out and Benefit Shifting Including Partner Responses**

	Effect of reform per 1000 €/yr decrease in amount of original DI			P- value gender dif.
	Full sample	Males	Females	
<b>Panel A: Labor market outcomes in 1999</b>				
Earnings	1.572 (0.475) <sup>***</sup>	1.557 (0.435) <sup>***</sup>	1.654 (1.616)	0.954
<b>Panel B: Other social assistance in 1999</b>				
Income from other social assistance	1.129 (0.132) <sup>***</sup>	1.069 (0.128) <sup>***</sup>	1.409 (0.432) <sup>***</sup>	0.450
<b>Panel C: Total</b>				
Income except from original DI spell	2.700 (0.487) <sup>***</sup>	2.626 (0.446) <sup>***</sup>	3.063 (1.666) <sup>*</sup>	0.800

Note: Standard errors are in parentheses. Significance levels: \* 10 percent; \*\* 5 percent; \*\*\* 1 percent. Each entry in the table comes from a separate IV regression based on the fuzzy RD design. The dependent variable is listed in the rows. Income and earnings are measured in thousands of Euros per year. The variable that is instrumented (endogenous explanatory variable) is the amount of DI, so all coefficients can be interpreted as effect size per €1000/yr decrease in DI. The instrument itself is the discontinuity at the cutoff age (45 as of 8/1/93). The regressions are based on 90,818 observations for the full sample, and on 59,726 and 31,092 observations for males and females, respectively.

## Appendices

**Tabel A1: Description of the relevant variables**

<i>Variable</i>	<i>Definition</i>	<i>Timing</i>
<b>DI-files</b>		
Sector	Industry (3-digit)	Start of disability spell
StartDI $i$	Start of disability spell (yyyymm)  Index $i$ indicates the number of changes in the degree of disability since the start of the DI spell. <i>StartDI1</i> always equals the start of the disability spell. For each subsequent change in the degree of disability that was observed, a new <i>startDI2</i> , <i>startDI3</i> etc. was constructed.	Start of disability spell
EndDI	End of disability spell (yyyymm)	End of disability spell
DegreeDI $i$	Degree of disability  Index $i$ indicates the number of changes in the degree of disability since the start of the DI spell. <i>DegreeDI1</i> always equals the degree of disability at the start of the disability spell. For each subsequent change in the degree of disability that was observed, a new <i>DegreeDI2</i> , <i>DegreeDI3</i> etc. was constructed.	At the time of the spell
Age	Age as of August 1 <sup>st</sup> , 1993  Constructed from birth year and birth month.	
Exit	Dummy exit out of disability (excluding diseased, retirement, emigration) before January 1999	
<b>GBA-files</b>		
Sex	Gender (1=male; 0=female)	
Marital status	Marital status	In 1995
<b>SSB-files</b>		
Work	Having any income from paid work or self-employment in 1999.	1999, end of the year

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**Other  
social  
benefits**

UI	Dummy for receiving monthly earnings from (any time in) 1999 unemployment benefit
General assistance	Dummy for receiving monthly earnings from general (any time in) 1999 assistance

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