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December 2010

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ABSTRACT

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

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 causal effect of exit from disability insurance (DI) on participation in other social assistance programs. We find strong evidence of “social support shopping”: 43 percent of those induced to leave DI due to the reform receive an alternative form of social assistance two years after the implementation of the reform. As a result, for each Euro saved in DI benefits, the government has to spend an extra 60 cents in other social assistance programs. This crowd-out rate grows from 60% to 69% if we also take into account the response of the partners' of those affected by the DI reform. The crowd-out effect declines over time, but is still 25% eight years after the reform.

JEL Classification: H53, J22, I38

Keywords: crowd-out, spillover effects, 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.¹

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. 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 participation in any 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 discontinuity: the reform was significantly more stringent for those who would turn 45 after August 1st, 1993. Since we have each individual's month of birth, we exploit this discontinuity by

¹ 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."

comparing later labor market earnings and social assistance income for those just below this age cutoff to these outcomes for those just above the age cutoff. We scale this difference in outcomes by the discontinuity in disability exit rates around the age cut-off. The resulting estimates represent the average causal effect of the reform on those who were induced to exit by the more stringent rules but who would have remained on disability insurance under the less stringent rules. We refer to this group as “marginal DI leavers.”

We have three main findings. First, in the short term (about 2 years after reform), we find that the reform increased the probability among marginal DI leavers of receiving income from other social assistance programs by 43%, and the income from these other social assistance programs replaces 60% of the foregone DI income. In other words, we find a significant amount of social support shopping with a short-term crowd-out rate of 60%. The reform increased the probability of having any labor market earnings by 48%, and these earnings replaced 116% of foregone DI income, i.e., the crowd-in rate is 116%. Thus, in the short term, marginal DI leavers were able to replace more than all of their foregone DI income on average.

Second, over time, the crowd-out rate falls but the crowd-in rate remains roughly constant. Eight years after the reform (the last year in our data), the crowd-out rate has fallen to 25% while the crowd-in stands at 128%. 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 marginal DI leavers to become significantly more likely to receive social assistance. If we include the social assistance income of spouses in our crowd-out calculation, the point estimate of the short-run crowd-out rate increases from 60% to 69%.

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 its effects on other social assistance programs into account. 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 take-up decisions of 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.² 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.³ With respect to early retirement, Duggan, Singleton, and Song (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. Finally, 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.

² 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.

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

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 benefit take-up decisions of 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 reciprocity 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%).⁴ The corresponding figure for the U.S. is 5.9%. Individuals in the 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.⁵ 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,

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

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

according to the doctor's judgment, the applicant could still perform.⁶ 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.⁷ 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).

The DI reforms of 1993 tightened this procedure in two respects.⁸ 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

⁶ 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").

⁷ 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).

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

than 5), and (iii) the geographic region in which these occupations had to exist with at least 10 active workers was expanded roughly threefold.⁹ 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 number of fully disabled, and increasing the number of ineligible applicants and partial disability claimants. A recent study by Garcia Gomez 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 1st, 1993). Because re-examinations of existing claimants are time consuming, these re-examinations were scheduled to take place by age group over a period of several years. Disability claimants who were age 34 or younger on the 1st of August 1993 were re-examined in 1994, those aged 35-40 in 1995, those aged 41-44 in 1996/1997, and those aged 45-50 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 to that the 45-50 age group would be re-examined based on the previous and more generous procedure for determining replacement rates 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 1st, 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.

⁹ 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.”

Another important change of the 1993 DI reform was the introduction of an age and duration dependent benefit for new applicants.¹⁰ 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.¹¹

3. Data

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

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.

¹⁰ 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.

¹¹ 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.

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

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

Sample definition

In our baseline analysis, we restrict the sample to all individuals who (i) received disability benefits on August 1st, 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 1st, 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.¹³ 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 reform 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.¹⁴ 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 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 others. 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.

¹³ 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. In Appendix Table A.2, we show that our key results are robust to using different bandwidths.

¹⁴ 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 1st, 1993).

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 sample is 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 and 2005 in the interest of space. We define labor force status in year t by the following four mutually exclusive and exhaustive categories:

Still on DI (original spell): Everyone who receives a DI benefit at the start of year t from a DI-spell that started before August 1st, 1993. Note that some of these people may have other sources of income (including labor income) in addition to receiving the DI benefit. We still consider them DI recipients because they may already have had earnings in addition to their DI benefit at the time of the re-examination, and hence their working status may not be the result of stricter eligibility criteria.¹⁵

Employed: Everyone with income in year t from either paid employment or self-employment but without income from DI or other social assistance programs.

¹⁵ We cannot observe whether this is the case, because we have no information about employment for the years before 1999.

Social assistance (other than original DI spell): Everyone who, in year t , receives any non-DI social assistance (i.e., unemployment benefits or general assistance) or a DI benefit resulting from a DI spell that started after August 1st, 1993.

Out of the labor force (OLF): Everyone who does not receive any income in year t from employment, self-employment, DI, unemployment benefits, or general assistance (and hence is missing in all the administrative files).

In 1999, so about 1 to 3 years after the re-examinations took place for the age cohorts in our sample, 91% of those on DI in 1993 are still on DI. 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. Of those who left DI, about half are working, with the remaining half roughly divided between receiving another form of social assistance and being out of the labor force. By 2005, the share of individuals still on their original DI spell has declined to 79%. Of those who exited DI between 1999 and 2005, the majority left the labor force while a minority found employment.

4. Results

4.1 Effects of the reform on exit from DI

We start by examining the effect of the 1993 DI reform on subsequent exit out of DI. The variation in exit rates due to the age discontinuity allows us to examine our key interest, namely the degree to which people substitute one social assistance program for another. In particular, the effects of the DI reform on exit will serve as the first-stage regression for the IV results that will we show later.

The first panel of Figure 2 shows the relationship between age (measured as of the DI reform date) and the probability of leaving DI before January 1st, 1999. The figure reveals a clear discontinuity in the exit rate at age 45, the age below which the DI re-examinations were performed based on the new and stricter protocol. Those who were just younger than 45 in August 1993 were 3.6 percentage points more likely to have

exited DI than those who had just turned 45 on that date. This 3.6 percentage point increase is highly statistically significant and represents a 45% upward jump in the hazard rate of leaving DI. Under the assumption that there are no other factors that had a discontinuous effect on exit rates at age 45 as of August 1, 1993 (and we are not aware of any such possible factors), the estimate of 3.6 percentage points can be interpreted as the causal effect of the stricter DI benefit determination rules on exit out of DI between January 1st, 1995 and January 1st, 1999.

The next two panels of Figure 2 show the exit rate separately for men and women. In each case, the discontinuity is obvious from the graph and highly statistically significant. In terms of magnitude, the effect for women (5.3 percentage points) is about twice as large as that for men (2.8 percentage points). It is not clear what drives this difference, 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 all our 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. We calculate these rates as fractions of DI claimants in our sample on January 1st, 1995, so the rates add up to the exit rate reported in Figure 2. 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, 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.

4.2 Causal effects on employment and take-up of other social assistance

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.

We start by analyzing the reduced-form effects of the DI reform on labor force status. The first panel of Figure 4 shows a clear discontinuity at age 45 in the probability that observations from our full sample are working in 1999, where working is defined as having labor earnings but no income from DI or other social assistance programs. The discontinuity at age 45 is a highly statistically significant 1.7 percentage points, indicating that the stricter re-examinations caused a 1.7 percentage point increase in the probability of working. The next two panels show similar discontinuities for men and women separately.

Figure 5 shows the analogous figures for the probability that by 1999 our sample members are claiming other social assistance benefits including DI benefits from a DI spell that started after the DI reforms. As the figure shows, there is a clear upward jump in the probability of receiving other social assistance benefits; those subjected to the stricter re-examinations are 1.6 percentage points more likely to be claiming other social assistance benefits, and this discontinuity is noticeably larger among women than men. In other words, we find clear evidence of substitution of other forms of social assistance for DI benefits.

Finally, Figure 6 shows that, stricter re-examinations had no statistically significant impact on the probability of being out of the labor force in 1999 among the entire sample or for the male subsample. For women, there is some evidence that the reform increased the likelihood of being out the labor force. The RD estimate is statistically significant, but the discontinuity is not as compelling visually.

Because the DI reform cannot have affected the labor force status of those still on DI (by definition) and because the DI reform logically should not affect those who would have exited DI regardless of the reform, the effects of the reform on labor force status must have operated through the exits induced by the reform. In other words, we believe the exclusion restriction for treating the age discontinuity in the reform as an instrument for exit is satisfied. Second, given that the re-examination was in all respects more

stringent for those below the cutoff age, the monotonicity assumption should be satisfied as well. In other words, we assume that the probability of exit weakly decreases for all individuals at the cutoff age. If, in addition, there are no omitted factors that have a discontinuous impact at age 45 on labor force status, all the identifying assumptions for allowing us to estimate the causal effect of DI exit on labor force status using the standard “fuzzy RD design” are satisfied.¹⁶ The resulting causal estimate should be interpreted as a local average treatment effect for individuals around the cutoff age – it is the average effect of the DI exit on those induced to leave DI because they were subject to the stricter re-examination rules (i.e., the “compliers”).

Panel A of Table 3 presents the causal estimates of induced DI exit on labor force status. We find that 48% of individuals who were induced to exit DI have found paid work roughly 2 years later, whereas 43% are receiving social assistance. Both estimates are highly statistically significant. We estimate that 9% of those induced to exit DI are out of the labor force, although this estimate is only marginally significant. In short, our estimates show a roughly even split of marginal leavers finding work and relying on social assistance. Women and men are roughly equally likely to rely on other forms of social assistance, but women are significantly more likely than men to leave the labor force, while men are twice as likely as women to find paid work. Our finding that a substantial fraction of individuals who were induced to exit DI are able to subsequently find paid employment contributes to the extensive literature that estimates 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).

Next, we explore whether induced DI leavers have different outcomes than those who would leave DI in any case or even under the less stringent re-examinations. Panel B shows our estimates of the outcomes of those who exited and were exactly 45 years old at the reform date, i.e., outcomes for those just to the right of the age-discontinuity. We estimate these outcomes by regression-adjusting our observed outcomes for age using the

¹⁶ 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).

sample of those aged 45.0 to 47.5 on the reform date.¹⁷ We find that of the people who exit through “natural variation” (as opposed to being induced by the stricter rules) 50% find paid work, 25% receive other social assistance, and another 25% leave the labor force. Thus, compared to natural exit from DI, those induced to leave DI were about as likely to find paid work, but were more likely to rely on social assistance and less likely to leave the labor force. Panel C shows that these differences are statistically significant for the full sample and for the subsample of men. In other words, observing the outcomes of those who “naturally” leave DI would provide misleading predictions for the later outcomes of those who leave DI because of a tightening of the benefit rules.

Table 4 examines whether the destinations of marginal DI leavers depend on their degree of disability or their duration on DI. We find that female DI leavers with a high degree of disability ($\geq 55\%$ of potential earnings capacity lost) are significantly less likely to find paid work and significantly more likely to rely on other forms of social assistance than women with a lower degree of disability. Similarly, female DI leavers who have been more than 5 years on DI are significantly less likely to find paid work and significantly more likely to rely on other forms of social assistance than women with a shorter disability spell. For male DI leavers, however, there are no significant differences in destinations by degree of disability or duration on disability; even men whose degree of disability exceeds 55% or who have been on disability for 5 years or more have a probability of finding paid work that exceeds 60%. For neither gender, we find significant differences by degree of disability or duration on DI in the probability of leaving the labor force.¹⁸

So far, we used three exhaustive and mutually exclusive categories as the destinations of DI leavers. A limited number of categories gives a better overview and helps with statistical power, but it hides some detail. In Table 5, we present a more detailed set of destination classifications. In particular, we now distinguish between

¹⁷ Specifically, each entry in Panel B comes from an OLS regression of labor force status on exit from DI, age, and the interaction of exit with age. These regressions are estimated on the sample of individuals not subject to the more stringent re-examinations (so age 45.0-47.5 as of August 1993). Each entry in panel B reports the regressions’ estimated effect for someone aged exactly 45.0 of exit from DI on the outcome listed.

¹⁸ The results are similar when we distinguish between partially disabled (15-80%) and fully disabled (80-100%). We measure the degree of disability before the re-examinations at the time of the start of the DI spell.

working for an employer (“paid employment”) and being self-employed.¹⁹ On the benefit side, we now distinguish between unemployment benefits, general assistance, and re-entry into DI, and add a category for those with both some form of benefits and income from employment.²⁰

Table 5 shows that most of the marginal DI leavers who moved into paid employment became employees though a small, but statistically significant fraction, became self-employed. Of the marginal DI leavers who exclusively rely on benefits, the majority (about 60%) flow into unemployment, a sizeable minority (about 30%) rely on general assistance, while only a small minority (about 10%) re-enter into DI. About a third of the marginal DI leavers that receive social assistance also receive earnings. In Table 3, we classified those with both social assistance income and earnings in the social assistance category. If we had reclassified this group as employed, the causal effect of exit on employment would increase from 48% to 61% and the causal effect on social assistance receipt would fall from 43% to 30%. Thus, the results from Table 3 are somewhat sensitive to how these dual earnings/benefits recipients are classified. We avoid the need to make such classification decisions when we examine the effects of exit on income by source, shown in Table 6 below.

Do those flowing into employment find jobs that provide a meaningful income? Do former DI recipients who receive social assistance rely heavily on this assistance or do they only receive very minimal amounts? Similarly, from a public finance perspective, it matters not only whether marginal DI leavers flow into employment or social assistance but also how much income DI leavers received from each source. To answer these questions, Table 6 examines the amounts of income from different sources rather than the individual’s labor force status. Each entry in the first seven rows of the table shows an IV regression that estimates the effect of exit out of DI on the amount of income received from a particular source. As before, exit out of DI is instrumented using the age discontinuity around age 45 in the stringency of the re-examination procedure. Under the exclusion restriction that the re-examinations did not affect the incomes of those remaining on DI, the estimates can be interpreted as the average causal effect of

¹⁹ People with income from both paid employment and self-employment are classified into the category with the highest income.

²⁰ Again, in case of more than one source for benefits, the highest income determines the category.

exit out DI on the amount of income of the listed source for marginal DI leavers.²¹ We topcoded all amounts at the 95th percentile to prevent outliers from driving the results and reducing the statistical power.²²

On average, the marginal DI leaver loses 12,400 Euros per year in DI benefits, but gains 14,400 Euros in labor income and 7,400 Euros in other social assistance benefits (mostly UI). Thus, for every Euro in lost DI income, the individual makes up 60 cents (7,400/12,400) by receiving other forms of social assistance. We refer to this as a crowd-out rate of 60%; a government not taking this crowd-out into account would overestimate the cost savings from tightening the DI eligibility rules by a factor of more than 2. On the other hand, the individual also gains 116 cents (14,400/12,400) in labor income for every Euro of lost DI benefits. We refer to this as a crowd-in of 116%. If the average tax rate is 40%, this means the government's budget gains an additional 46 cents ($=116 \times 0.4$) in revenue for every Euro of DI benefit cuts. The final row of Table 6 shows that, on average, marginal DI leavers see their total income rise by 76 cents per Euro of lost DI benefits. Note that if even the exclusion restriction does not hold (so the reforms also caused changes in income among those staying on DI), the crowd-out, crowd-in, and total income estimates remain valid. In this case, they should be interpreted as the effect of reduced DI income on other sources of income, where the effect is driven by the behavior of both DI leavers and those remaining on DI.²³

The second and third columns reveal noteworthy differences in the effects of the DI reform on men and women. While both genders experience significant declines in their income from DI and significant increases in income from other forms of social assistance, only men experience significant increases in labor income (and significantly more so than women). As a result, we find significant crowd-out for both men and women, but only find significant crowd-in for men. As noted above, it hard to determine what drives the differences in the effects of the DI reform on men and women, but we

²¹ We cannot rule out that re-examinations also affected income sources of those remaining on DI, so the exclusion restriction is a strong assumption.

²² If we do not topcode the amounts, the results are similar but noisier.

²³ To see this, note that the crowd-out, crowd-in, and total income estimates remain the same whether they are calculated as ratios of IV coefficients or ratios or ratios of reduced-form coefficients. The reduced-form regressions do not rely on an exclusion restriction; they only rely on the age discontinuity.

suspect differences in initial DI benefit levels, differences in types of disabilities, and differences in opportunities in market and household production are likely explanations.

4.3 Responses of Partners of DI recipients

Until now, our analysis did not take potential responses of the partners of DI recipients into account. Moreover, partner responses matter for our estimates of total crowd out and total crowd in. To the extent that exit out of DI induces the partner to take up social assistance, the total crowd out is larger than what we found in Table 6. Similarly, if exit out of DI causes the partner to find paid work, then total crowd in is larger than the individual-level crowd in from Table 6.

Table 7 reports the causal effect of the more stringent reform on the partners of (former) DI recipients. Panel A reports reduced-form RD regressions. We find that partners of DI recipients that were just below the age cutoff (so subject to the more stringent reforms) are significantly more likely to receive social assistance and significantly less likely to be out of the labor force than partners of DI recipients just above the age cut-off. Partners of male (former) DI recipients are significantly more likely to take up social assistance than are partners of female (former) DI recipients, and this former group drives the overall response. We do not find a significant reaction in the probability of doing paid work among partners of (former) DI recipients.

In Panel B, we scale the reduced-form estimates by the effect of the reform on exit from DI. Thus, Panel B reports the same specification as our baseline fuzzy RD design (from Table 3, panel A), except that now the outcome variable is the partner's labor force status. We find that exit of DI causes a statistically significant 35% increase in the probability that the partner of the former DI recipient receives social assistance, and that this increase is completely driven by partners of male former DI recipients.

In Table 8, we provide estimates of crowd out and crowd in that account for the responses of partners of (former) DI recipients. We find that our point estimates of both crowd in and crowd out are larger once we take the partner's response into account: crowd out increases from 60% to 69% and crowd in increases from 116% to 173%. The increase for crowd-out, while substantial in economic terms, is not statistically significant for the full sample, though for the subsample of males the increase in crowd out is

statistically significant at the 5-percent level. The increase in crowd in may seem surprising given that in Table 7 we did not find a significant effect of induced DI exit on the partner's probability of having paid work. However, partners' responses need not only occur on the extensive margin, but can also occur on the intensive margin. Apparently, partners who were already working when the individual was still on DI increased their labor market earnings in response to the individual's exit from DI. This finding 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. Overall, we believe Tables 7 and 8 clearly demonstrate that partner responses can be economically meaningful. As a result, any analysis that ignores these effects could easily produce faulty predictions or policy advice.

4.3. Responses over time

Responses to induced exit out of DI 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 1st, 2004. All workers in our sample were older than 50 at that time. These reforms therefore do not affect our estimates.

Figure 7 shows the effect of induced DI exit on the probability that the DI leaver is working or receiving social assistance 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 Panel A 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 the probability of having paid work is rather constant over time. There is no significant trend in these estimates. In contrast, the probability of receiving social assistance declines from 43% in 1999 to 23% in 2005, and this downward trend is statistically significant. The decline in social assistance receipt is offset by an increase in the number of individuals who are out of the labor force. Thus, over time, those who were induced to exit from DI and find work, seems to find work within about two years. Those who rely on other forms of social assistance gradually lose these rights but do not return to work. Thus, reintegration into the labor market primarily takes place in the first years after the loss of DI.

In Figure 8, we present the individual-level crowd-out and crowd-in figures over time; so the figure presents the estimates of Table 6 over time. We view the individual-level crowd-out and crowd-in estimates as reflecting the labor market status of the DI leaver, but where outcomes are weighted by the money amounts involved. The patterns in Figure 8 mirror those of Figure 7. In other words, we find no trends in crowd in over time but find a significant downward trends in crowd out.

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 crowd-in and 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 60 cents. Thus, ignoring this crowd-out of 60% would lead one to over-estimate the cost savings of the DI reform by factor of more than 2. Because the reform also causes the partners of affected DI recipients to become more likely to claim social assistance, the crowd-out rate is even higher (69%) when we define crowd out to include the partner response. While the crowd-out rate decreases over time, the individual-level crowd-out still stands at 25% about 8 years after the implementation of the reform for our sample.

Crowd-out estimates would obviously be different in different settings, but the direction in which the estimates would change is not clear. Our crowd-out figure may 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 exit from DI. Thus, the marginal DI leavers, whose behavior drives our crowd-out estimate, were likely in better health and had better labor market prospects than would have been the case if the reform had been more stringent. This means that our crowd-out estimate may be lower than would be the case for more far-reaching reforms.

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 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 spillover effects. 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 crowd-out rate. Finally, we show that the crowd-out rate varies with time passed since the reform. An

estimate of crowd-out 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 crowd out. We see the estimation of these complementary spillover effects as an important area for further research.

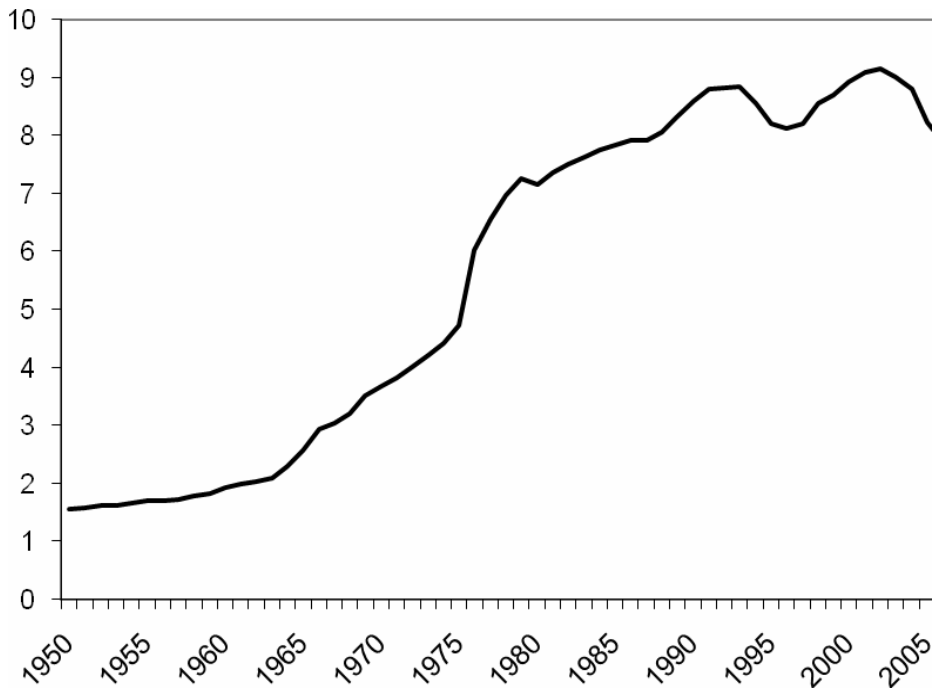
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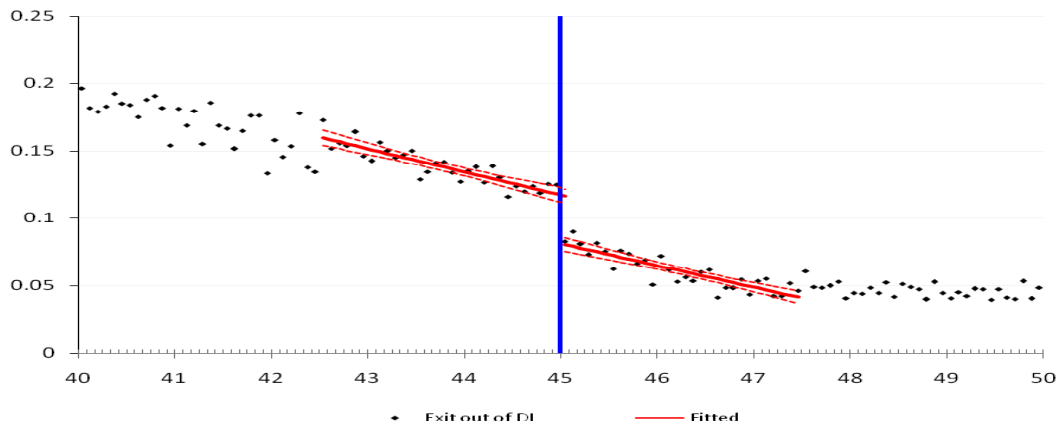
Figure 1: Percent of the labor force receiving DI benefits, 1950-2009.



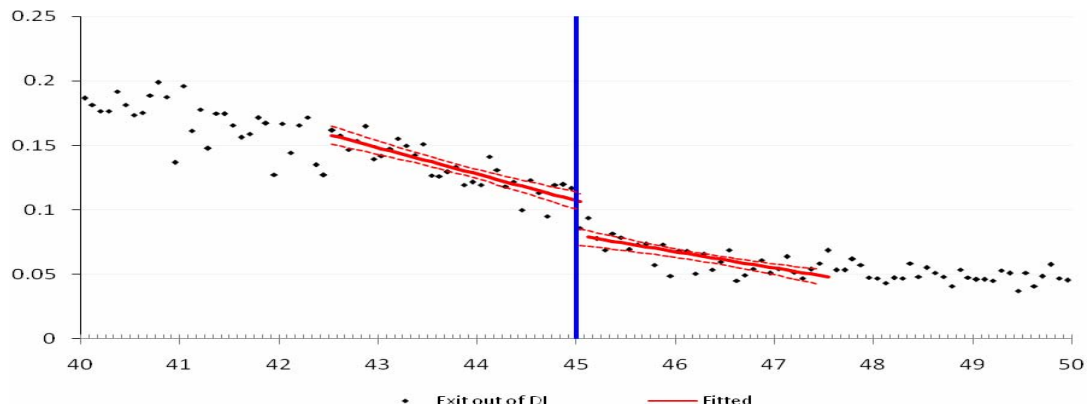
Source: Statline Statistics Netherlands

Figure 2: Exit rate out of DI (between January 1st, 1995 and January 1st, 1999)

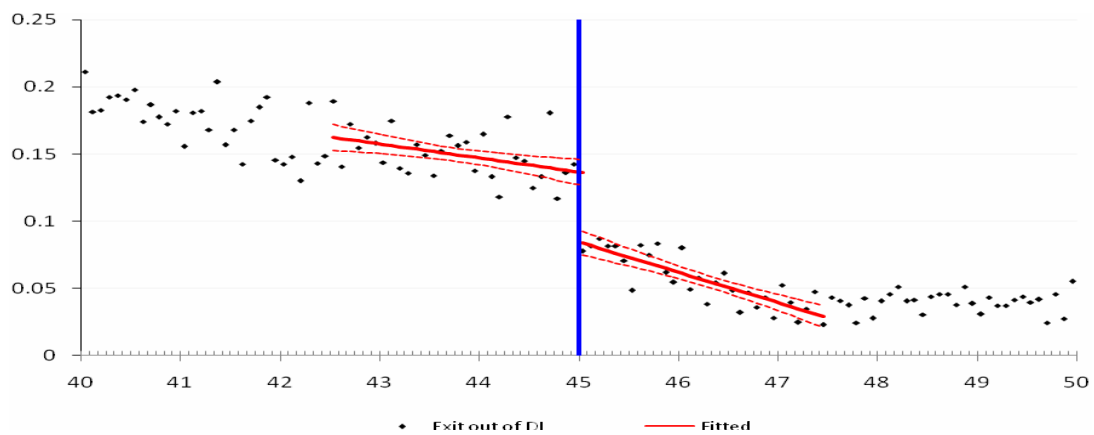
Panel A: Full sample. Estimate of the discontinuity: 0.036 (0.004)***



Panel B: Males. Estimate of the discontinuity: 0.028 (0.005)***



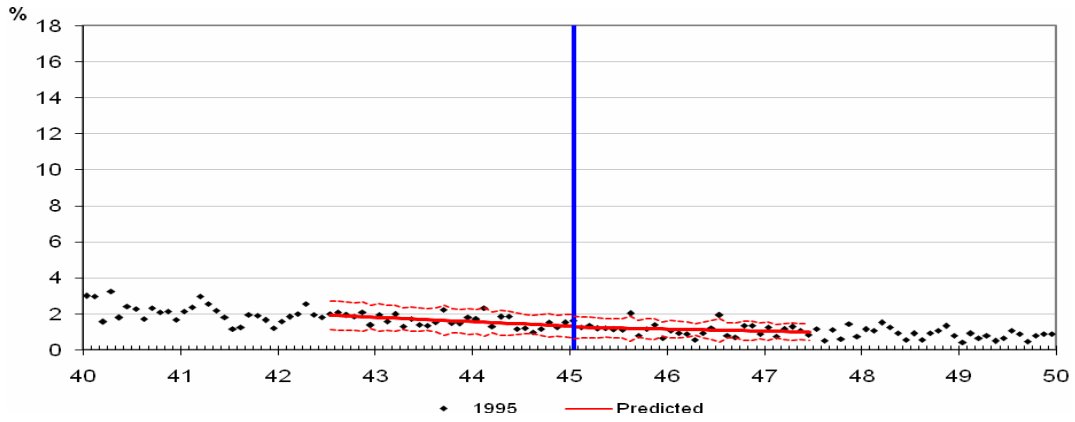
Panel C: Females. Estimate of the discontinuity: 0.053 (0.007)***



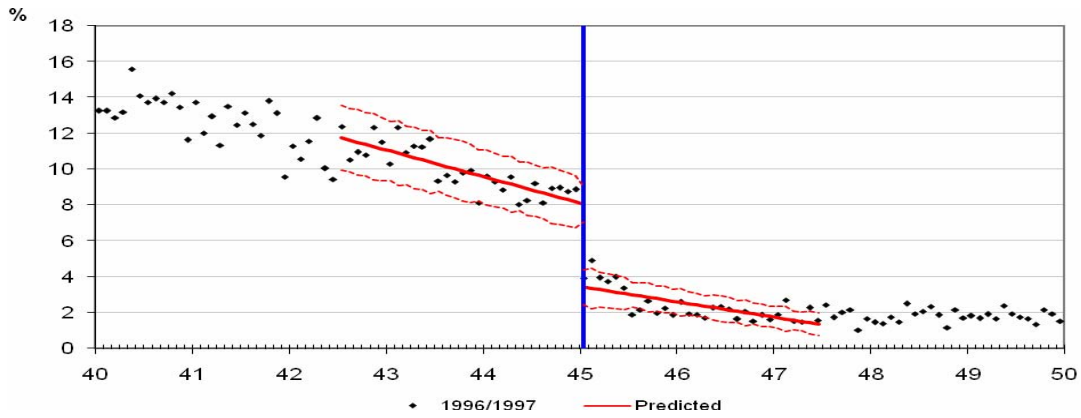
Note: The top figure is based on 90,818 observations, while the figures for males and females are based on 59,726 and 31,092 observations, respectively. The dotted lines represent the 95% confidence intervals. The exit rate is defined as a fraction of our sample in 1995. Standard errors are in parentheses.

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

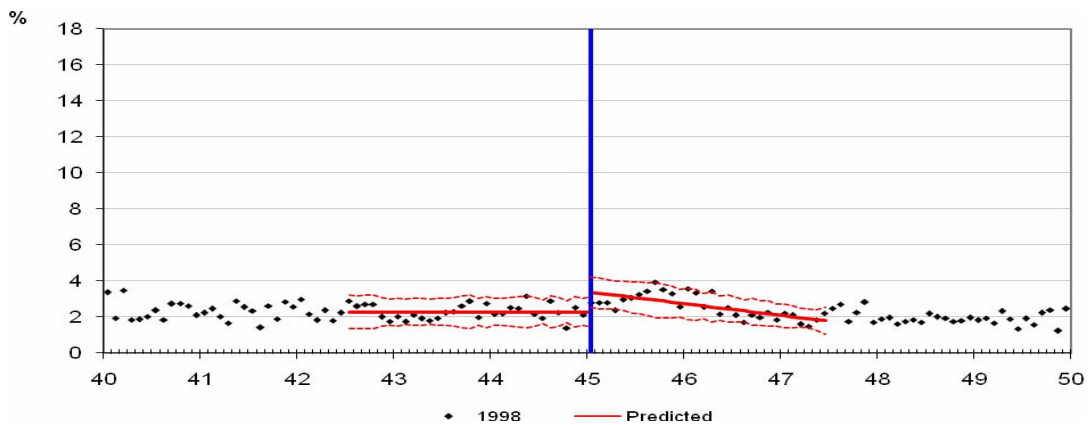
Panel A: Exit in 1995. Estimate of the discontinuity: 0.001 (0.002)



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



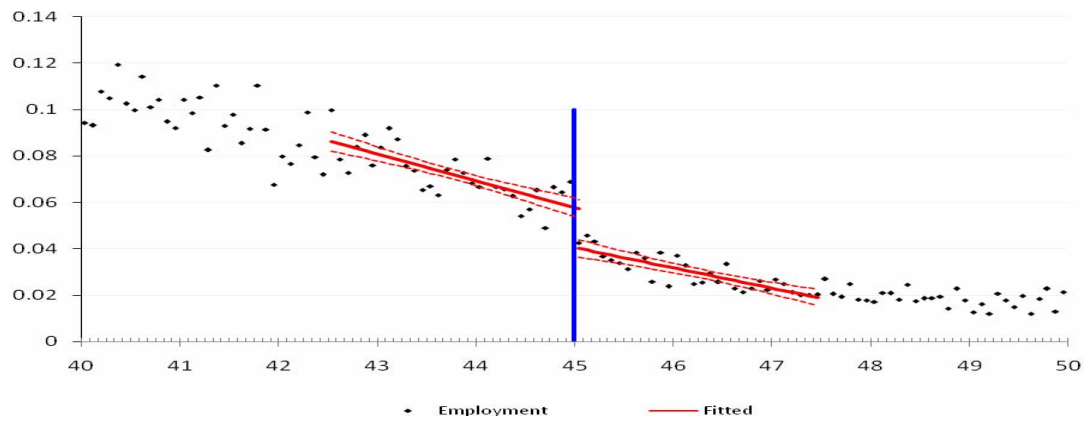
Panel C: Exit in 1998. Estimate of the discontinuity: -0.011 (0.002)^{***}



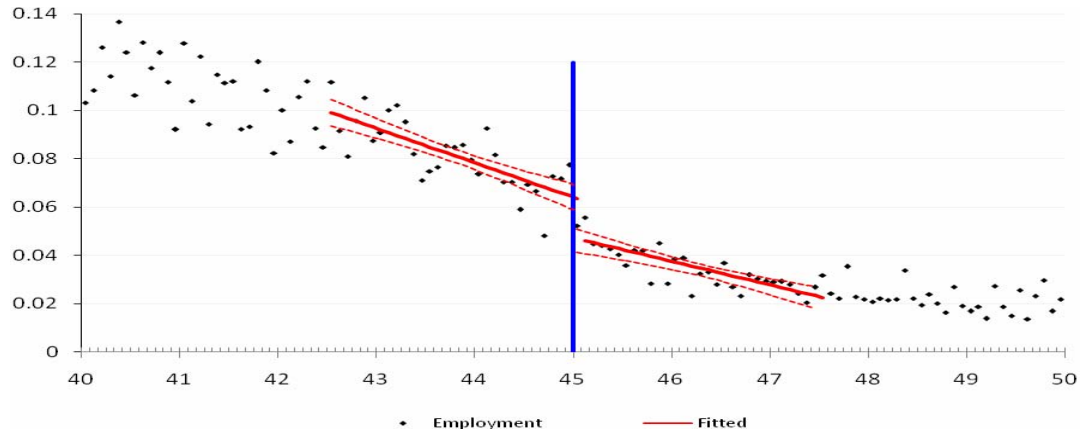
Note: The figures are based on 90,818 observations. The dotted lines represent the 95% confidence intervals. The exit rate is defined as a fraction of our sample in 1995. Standard errors are in parentheses.

Figure 4: Fraction employed in 1999 by age as of August 1993

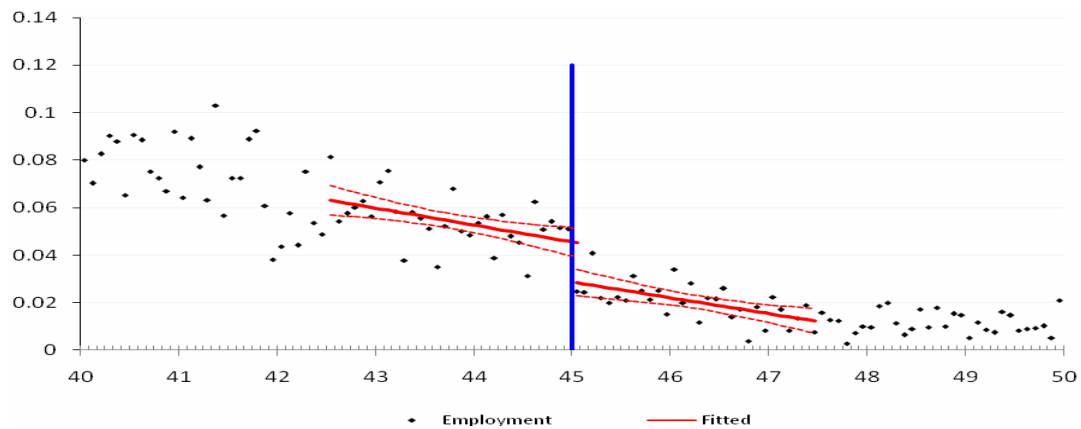
Panel A: Full sample. Estimate of the discontinuity: 0.017 (0.003)***



Panel B: Males. Estimate of the discontinuity: 0.017 (0.004)***



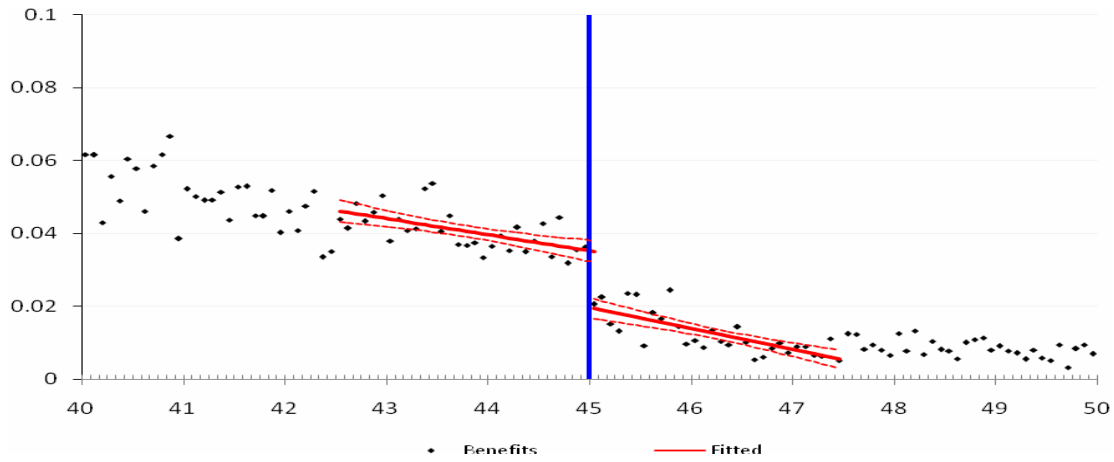
Panel C: Females. Estimate of the discontinuity: 0.017 (0.004)***



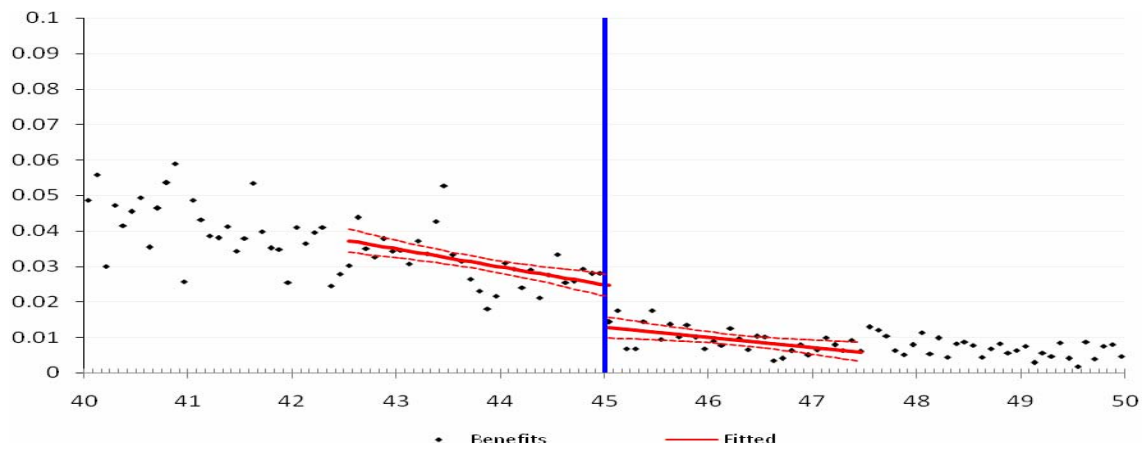
Note: The top figure is based on 90,818 observations, while the figures for males and females are based on 59,726 and 31,092 observations, respectively. The dotted lines represent the 95% confidence intervals. All individuals with labor income, but without social assistance income, in 1999 are considered employed. Standard errors are in parentheses.

Figure 5: Fraction receiving social assistance in 1999 by age as of August 1993

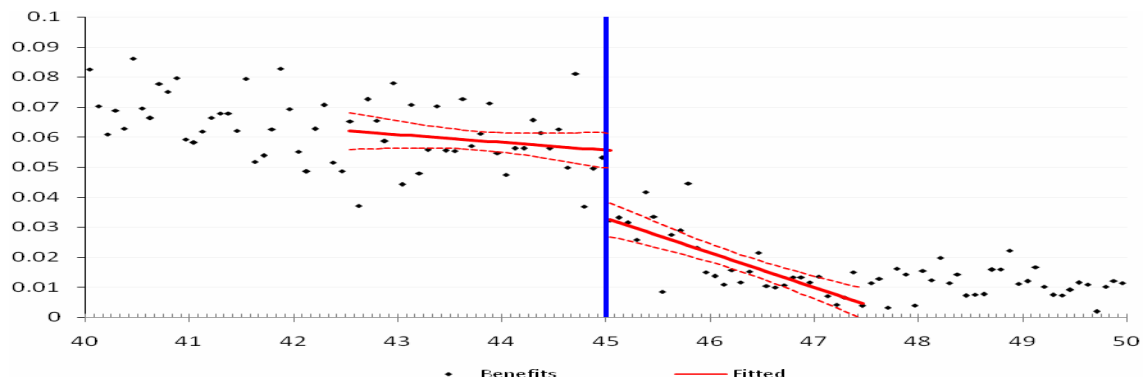
Panel A: Full sample. Estimate of the discontinuity: 0.016 (0.002)***



Panel B: Males. Estimate of the discontinuity: 0.012 (0.002)***



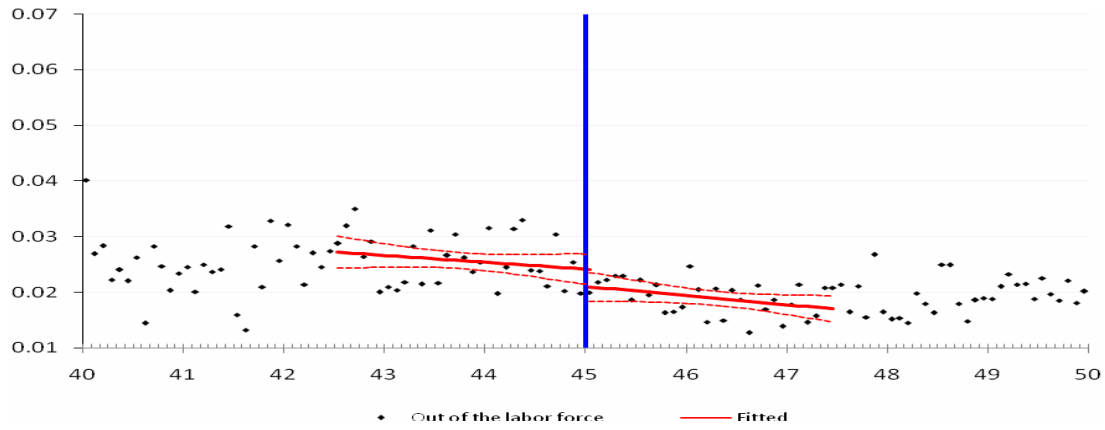
Panel C: Females. Estimate of the discontinuity: 0.023 (0.004)***



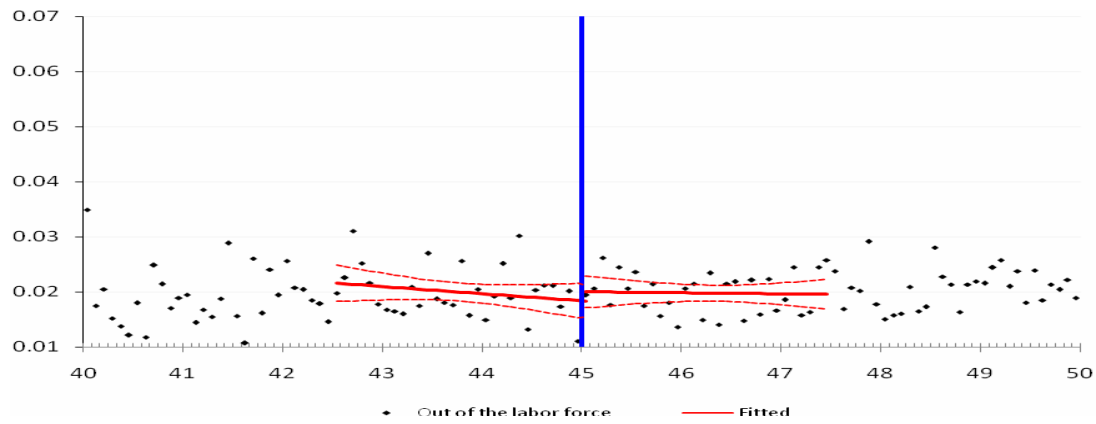
Note: The top figure is based on 90,818 observations, while the figures for males and females are based on 59,726 and 31,092 observations, respectively. The dotted lines represent the 95% confidence intervals. Individuals are counted as receiving social assistance if they receive any social assistance income in 1999 other than income associated with their original DI spell. Standard errors are in parentheses.

Figure 6: Fraction out of the labor force in 1999 by age as of August 1993

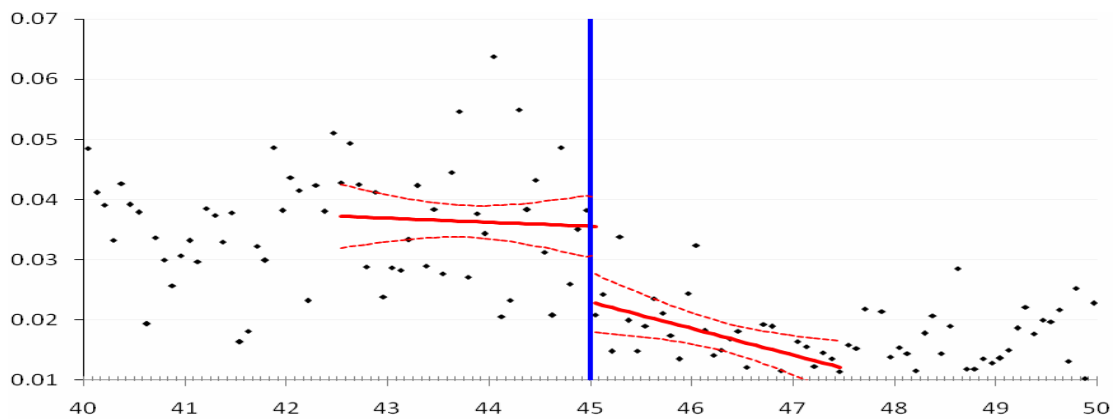
Panel A: Full sample. Estimate of the discontinuity: -0.002 (0.001)*



Panel B: Males. Estimate of the discontinuity: -0.002 (0.002)

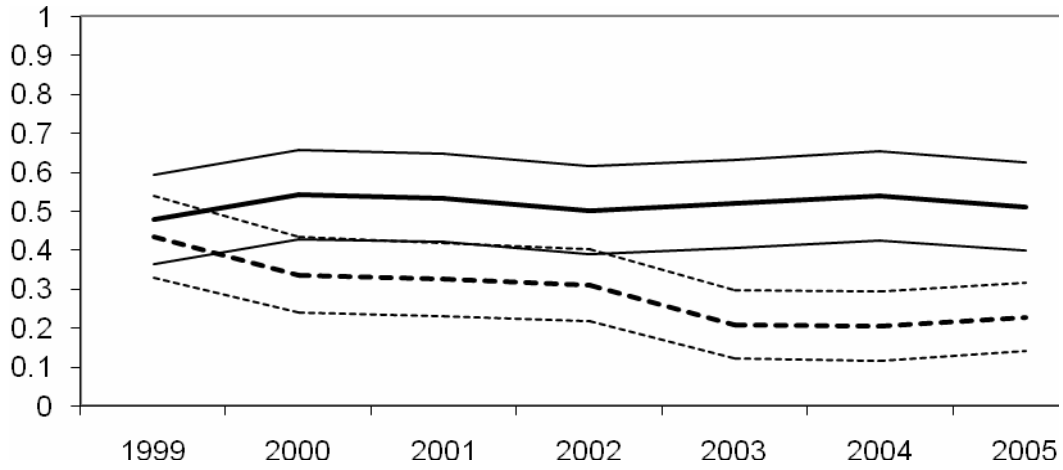


Panel C: Females. Estimate of the discontinuity: 0.013 (0.004)***

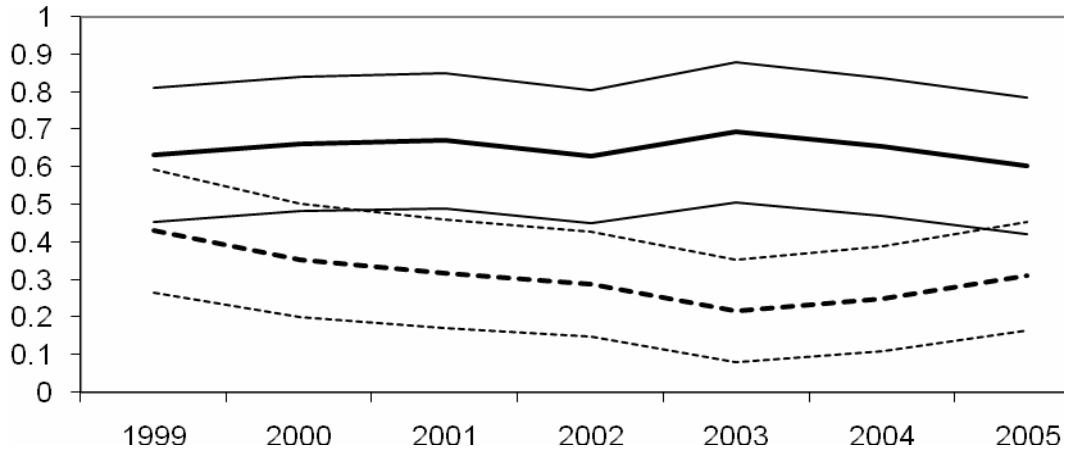


Note: The top figure is based on 90,818 observations, while the figures for males and females are based on 59,726 and 31,092 observations, respectively. The dotted lines represent the 95% confidence intervals. Those who receive neither social assistance income nor labor income are considered out of the labor force. Standard errors are in parentheses.

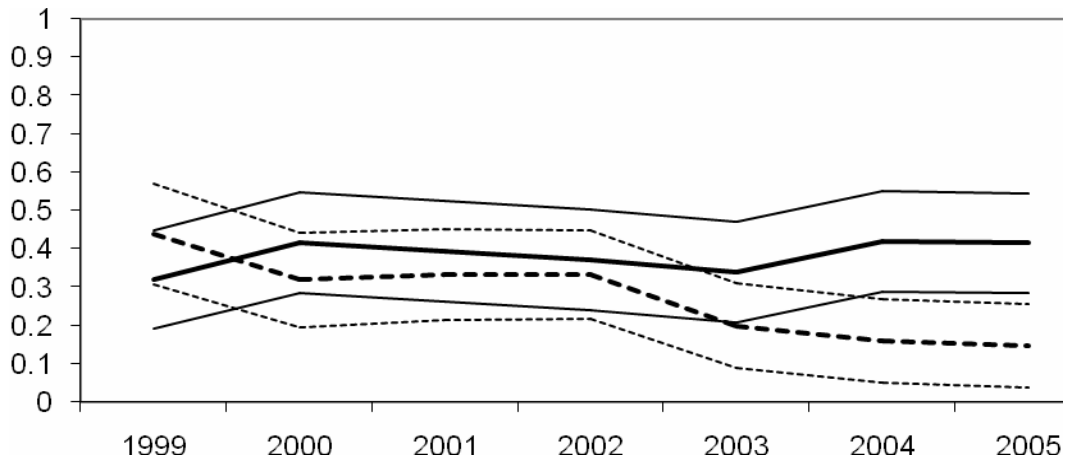
Figure 7: Estimated effects of induced DI exits on employment status over time
Panel A: Full Sample



Panel B: Males

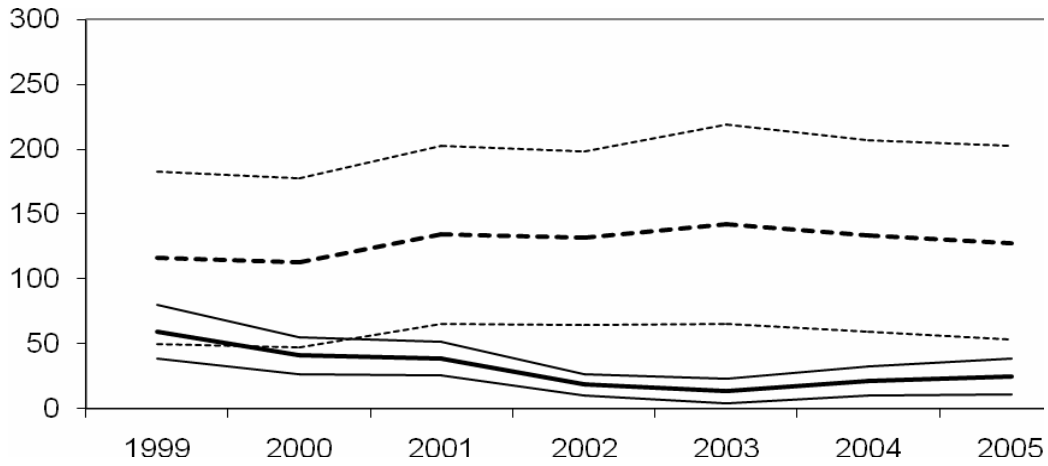


Panel C: Females

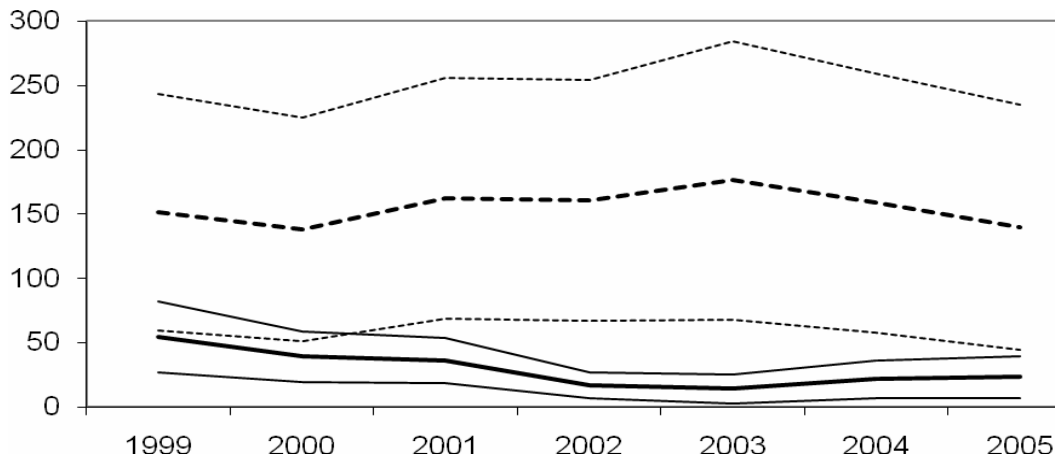


_____ : Fraction working (and 95% Confidence interval)
 : Fraction receiving social assistance (and 95% Confidence interval)

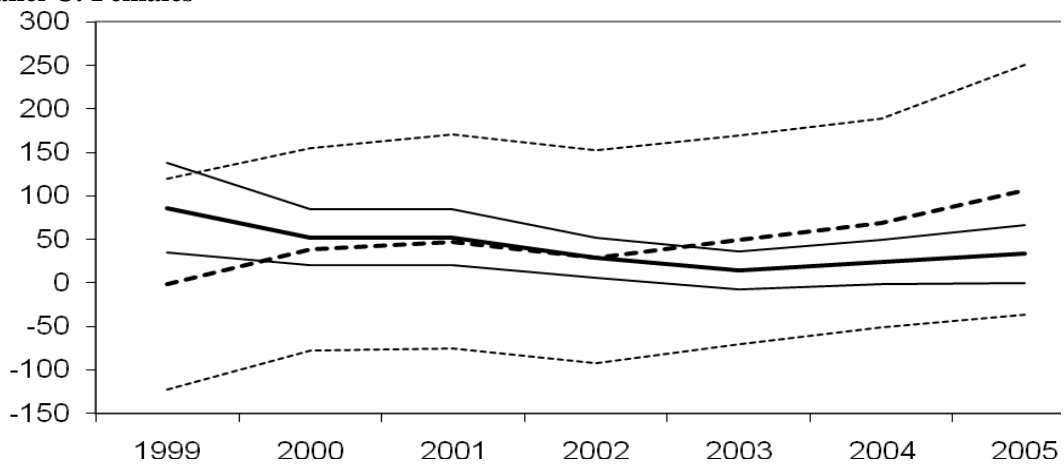
Figure 8: Individual-level crowd out and crowd in over time.
Panel A: Full sample



Panel B: Males



Panel C: Females



_____ : Crowd-out effect (and 95% Confidence interval)
 : Crowd-in effect (and 95% Confidence interval)

Table 1: Relation between degree of disability and benefit levels

Degree of disability:	Benefit level (% 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
Panel A: Sample characteristics before re-examination			
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 st , 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
Panel B: Outcomes after re-examination			
<i>Labor market status in 1999 (%):</i>			
Still on DI (on the original spell)	90.55	90.70	90.27
Employed	4.79	5.45	3.53
Social assistance (other than original DI spell)	2.47	1.87	3.62
Out of the labor force	2.19	1.98	2.58
<i>Labor market status in 2005 (%):</i>			
Still on DI (on the original spell)	79.25	78.54	80.60
Employed	7.71	8.64	5.95
Social assistance (other than original DI spell)	2.40	2.27	2.65
Out of the labor force	10.63	10.55	10.80
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: Effects of induced DI exits on labor force status in 1999

	Full sample	Males	Females	P-value gender dif.
Panel A: Causal estimate (fuzzy RD design)				
Employed	0.479 (0.058) ^{***}	0.632 (0.091) ^{***}	0.320 (0.066) ^{***}	0.006
Social assistance	0.434 (0.054) ^{***}	0.430 (0.084) ^{***}	0.438 (0.067) ^{***}	0.941
Out of labor force	0.087 (0.051) [*]	-0.062 (0.090)	0.242 (0.061) ^{***}	0.005
Panel B: Natural variation (not causal)				
Employed	0.501 (0.004) ^{***}	0.589 (0.005) ^{***}	0.332 (0.007) ^{***}	0.000
Social assistance	0.247 (0.003) ^{***}	0.163 (0.004) ^{***}	0.408 (0.007) ^{***}	0.000
Out of labor force	0.252 (0.004) ^{***}	0.248 (0.005) ^{***}	0.260 (0.007) ^{***}	0.163
Panel C: Difference between panel B and C				
Employed	-0.022 (0.058)	0.043 (0.091)	-0.012 (0.066)	0.781
Social assistance	0.187 (0.054) ^{***}	0.267 (0.089) ^{***}	0.030 (0.067)	0.002
Out of labor force	-0.164 (0.051) ^{***}	-0.309 (0.089) ^{***}	-0.018 (0.060)	0.002

Note: Standard errors are in parentheses. Significance levels: * 10 percent; ** 5 percent; *** 1 percent. The dependent variable is the labor market status in 1999 (employed, receiving social assistance, or being out of the labor force). Each entry in Panel A comes from a separate IV regression based on the fuzzy RD design. The regressions in Panel A are based on 90,818 observations for the full sample; for males and females we have 59,726 and 31,092 observations, respectively. Each entry in Panel B comes from an OLS regression of the outcome listed on exit from DI, age, and the interaction of exit with age. These regressions are estimated on the sample of individuals not subject to the more stringent re-examinations (so age 45.0-47.5 as of August 1993). Each entry in panel B reports the regression's estimated effect for someone aged exactly 45.0 of exit from DI on the outcome listed. The regressions in Panel B are based on 90,818 observations for the full sample; for males and females we have 59,726 and 31,092 observations, respectively.

Table 4: Heterogeneity in the effects of induced DI exits on labor force status in 1999

	Full sample	Males	Females	P-value gender dif.
Panel A: Employment				
1. By degree DI				
<i>Low (15 – 55%)</i>	0.607 (0.070)***	0.609 (0.115)***	0.603 (0.076)***	0.965
<i>High (55 – 100%)</i>	0.319 (0.083)***	0.650 (0.151)***	-0.003 (0.108)	0.000
P-value on difference	0.008	0.822	0.000	
2. By duration on DI (as of August 1993)				
<i>Less than 5 years</i>	0.458 (0.093)***	0.486 (0.165)***	0.437 (0.103)***	0.801
<i>More than 5 years</i>	0.493 (0.075)***	0.719 (0.115)***	0.172 (0.083)**	0.000
P-value on difference	0.770	0.247	0.045	
Panel B: Receipt of Social Assistance				
1. By degree DI				
<i>Low (15 – 55%)</i>	0.251 (0.057)***	0.313 (0.099)***	0.183 (0.060)***	0.261
<i>High (55 – 100%)</i>	0.657 (0.101)***	0.518 (0.147)***	0.728 (0.130)***	0.454
P-value on difference	0.000	0.134	0.000	
2. By duration on DI (as of August 1993)				
<i>Less than 5 years</i>	0.363 (0.083)***	0.552 (0.172)***	0.224 (0.102)***	0.101
<i>More than 5 years</i>	0.497 (0.074)***	0.358 (0.095)***	0.692 (0.102)***	0.017
P-value on difference	0.228	0.323	0.001	
Panel C: Out of labor force				
1. By degree DI				
<i>Low (15 – 55%)</i>	0.142 (0.049)***	0.078 (0.073)	0.214 (0.062)***	0.156
<i>High (55 – 100%)</i>	0.023 (0.101)	-0.232 (0.205)	0.274 (0.108)***	0.029
P-value on difference	0.289	0.140	0.614	
2. By duration on DI (as of August 1993)				
<i>Less than 5 years</i>	0.179 (0.075)**	-0.039 (0.147)	0.339 (0.091)***	0.029
<i>More than 5 years</i>	0.010 (0.074)	-0.077 (0.115)	0.135 (0.091)	0.148
P-value on difference	0.109	0.839	0.113	

Note: Standard errors are in parentheses. Significance levels: * 10 percent; ** 5 percent; *** 1 percent. The dependent variable is the labor market status in 1999 (employed, receiving social assistance, or being out of the labor force). Each entry comes from a separate IV regression based on the fuzzy RD design. Degree DI is the degree of disability as measured by the percentage loss of prior earnings capacity. See the text for a description of how degree DI is determined.

Table 5: Effects of induced DI exits, by more narrowly specified destinations in 1999

	Full sample	Males	Females	P-value gender dif.
Employment				
Paid employment	0.423 (0.057) ^{***}	0.553 (0.092) ^{***}	0.289 (0.065) ^{***}	0.019
Self employment	0.055 (0.027) ^{**}	0.079 (0.047) [*]	0.031 (0.024)	0.363
Social Assistance				
Unemployment benefits	0.178 (0.033) ^{***}	0.145 (0.040) ^{***}	0.212 (0.050) ^{***}	0.295
General assistance benefits	0.087 (0.022) ^{***}	0.127 (0.038) ^{***}	0.045 (0.024) [*]	0.068
Re-entry into DI	0.035 (0.029)	0.013 (0.042)	0.058 (0.040)	0.438
Employment and benefits	0.134 (0.032) ^{***}	0.145 (0.049) ^{***}	0.123 (0.041) ^{***}	0.731
Out of the labor force	0.087 (0.051) [*]	-0.062 (0.090)	0.242 (0.061) ^{***}	0.005

Note: Standard errors are in parentheses. Significance levels: * 10 percent; ** 5 percent; *** 1 percent. The dependent variable is the labor market status in 1999. Each entry comes from a separate IV regression based on the fuzzy RD design. The regressions for the full sample are based on 90,818 observations; for males and females we have 59,726 and 31,092 observations, respectively. Definition of the categories follows. Paid employment: income from paid employment larger than income from self-employment and no income from social assistance. Self employment: income from self employment larger than income from paid employment and no income from social assistance. Unemployment benefits: income from unemployment benefits larger than income from general assistance and no labor income. General assistance benefits: income from general assistance benefits larger than income from unemployment and no labor income. Re-entry into DI: income from a new spell of DI and no labor income. In the data there is nobody who combines income from a new DI spell with other social assistance. Employment and benefits: any combination of income from work and social assistance. Out of the labor force: no income from work and no social assistance.

Table 6: Effects of induced DI exits on income by source in 1999

	Full sample	Males	Females	P-value gender dif.
Panel A: Income amount by source				
1. Paid employment	10.9 (3.6) ^{***}	22.7 (6.8) ^{***}	-0.0 (2.8)	0.002
2. Self employment	3.5 (1.3) ^{***}	6.9 (2.7) ^{***}	-0.0 (0.9)	0.015
3. Unemployment benefits	6.7 (0.8) ^{***}	9.6 (1.8) ^{***}	3.7 (0.6) ^{***}	0.002
4. General assistance	0.7 (0.2) ^{***}	1.1 (0.3) ^{***}	0.4 (0.3)	0.111
5. Disability benefits	-12.4 (1.6) ^{***}	-19.6 (7.5) ^{***}	-4.7 (1.2) ^{***}	0.001
6. Total earnings: (1)+(2)	14.4 (3.8) ^{***}	29.6 (3.3) ^{***}	-0.1 (2.9)	0.000
7. Total benefits: (3)+(4)+(5)	-5.0 (1.4) ^{***}	-8.9 (2.5) ^{***}	-0.6 (1.3)	0.003
Panel B: Implied crowd-out and crowd-in rates				
8. Crowd out (%): $-\frac{[(3)+(4)]}{(5)}$	59.6 (10.5) ^{***}	54.4 (14.0) ^{***}	86.5 (26.3) ^{***}	0.281
9. Crowd in (%): $-\frac{(6)}{(5)}$	116.1 (33.9) ^{***}	151.3 (46.9) ^{***}	-1.4 (61.7)	0.049
10. Total effect (%): (8)+(9)	75.8 (31.1) ^{**}	105.7 (39.6) ^{***}	-14.9 (62.1)	0.102

Note: Standard errors are in parentheses. Significance levels: * 10 percent; ** 5 percent; *** 1 percent. The dependent variable is the amount of income from the respective sources (wage income, self-employment income, unemployment benefits, general assistance, disability benefits, and summations of these) in 1999 Euros per year ($\times 1000$). Income amounts in rows 1 through 5 have been top coded at the 95th percentile. In 1999, the GDP per capita in the Netherlands was 22,250 Euro per year. In the first seven rows, each entry comes from a separate IV regression based on the fuzzy RD design. The regressions are based on 90,818 observations; for males and females we have 59,726 and 31,092 observations, respectively.

Table 7: Effects on partners' labor force status in 1999

	Full sample	Partners of males	Partners of females	P-value gender dif.
Panel A. Reduced form estimates				
Employed	0.004 (0.008)	-0.003 (0.010)	0.019 (0.015)	0.222
Social assistance	0.016 (0.007)**	0.025 (0.008)***	-0.006 (0.014)	0.055
Out of labor force	-0.020 (0.007)***	-0.022 (0.010)**	-0.013 (0.007)*	0.461
Panel B. IV estimates				
Employed	0.095 (0.181)	-0.074 (0.282)	0.280 (0.213)	0.316
Social assistance	0.346 (0.161)**	0.704 (0.250)***	-0.085 (0.206)	0.015
Out of labor force	-0.441 (0.165)***	-0.632 (0.288)**	-0.195 (0.108)*	0.155

Note: Standard errors are in parentheses. Significance levels: * 10 percent; ** 5 percent; *** 1 percent. The dependent variable is the labor market status of the partner in 1999 (employed, receiving social assistance, or being out of the labor force). Each entry in panel A is the reduced-form RD estimate of the effect of the more stringent re-examination on the partners' labor force status. Each entry in Panel B comes from a separate IV regression based on the fuzzy RD design. The regressions are based on 60,563 observations; for males and females we have 41,562 and 19,001 observations, respectively.

Table 8: Effects of induced DI exits on household income by source in 1999

	Full sample		Males		Females		P-value gender dif.
Panel A: Income amount by source							
1. HH paid employment	17.9	(5.8) ^{***}	28.5	(9.2) ^{***}	6.9	(7.5)	0.069
2. HH self employment	3.5	(2.4)	10.1	(4.2) [*]	-3.2	(2.9)	0.009
3. HH unemployment benefits	6.8	(1.0) ^{***}	9.9	(1.9) ^{***}	3.5	(1.0) ^{***}	0.003
4. HH general assistance	1.2	(0.3) ^{***}	1.9	(0.6) ^{***}	0.4	(0.3)	0.025
5. HH disability benefits	-11.8	(1.7) ^{***}	-18.8	(3.5) ^{***}	-4.5	(1.7) ^{***}	0.000
6. Own disability benefits	-12.4	(1.6) ^{***}	-19.6	(7.5) ^{***}	-4.7	(1.2) ^{***}	0.001
7. Total HH earnings	21.5	(6.1) ^{***}	38.6	(10.4) ^{***}	3.6	(7.8)	0.007
8. Total HH benefits	-3.9	(1.6) ^{**}	-6.9	(2.6) ^{***}	-0.6	(1.9)	0.050
Panel B: Implied crowd-out and crowd-in rates							
9. HH crowd out (%): $-\frac{-(3)+(4)+(5)-(6)}{(6)}$	68.9	(13.9) ^{***}	64.6	(17.2) ^{***}	87.3	(39.7) ^{**}	0.600
10. HH crowd in (%): $-\frac{(7)}{(6)}$	173.2	(54.1) ^{***}	197.5	(63.6) ^{***}	77.6	(166.5)	0.501
11. Total HH income (%): $\frac{(9)+(10)}{(6)}$	142.1	(51.2)	162.1	(58.2) ^{***}	65.0	(160.4)	0.569

Note: Standard errors are in parentheses. Significance levels: * 10 percent; ** 5 percent; *** 1 percent. The dependent variable is the amount of income of both the former DI recipient and his/her partner from the respective sources (wage income, self-employment income, unemployment benefits, general assistance, disability benefits, and summations of these) in 1999 Euros per year ($\times 1000$). Income amounts in rows 1 through 6 have been top coded at the 95th percentile. In 1999, the GDP per capita in the Netherlands was 22,250 Euro per year. In the first eight rows, each entry comes from a separate IV regression based on the fuzzy RD design. The regressions are based on 90,818 observations; for males and females we have 59,726 and 31,092 observations, respectively.

Appendices

Tabel A1: Description of the relevant variables

<i>Variable</i>	<i>Definition</i>	<i>Timing</i>
DI-files		
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 st , 1993 Constructed from birth year and birth month.	
Exit	Dummy exit out of disability (excluding diseased, retirement, emigration) before January 1999	
GBA-files		
Sex	Gender (1=male; 0=female)	
Marital status	Marital status	In 1995
SSB-files		
Work	Having any income from paid work or self-employment in 1999.	1999, end of the year

**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

Appendix Table A2: Robustness of the baseline results (baseline = Table 3, Panel A)

	Full sample		Males		Females		P-value gender dif.
Panel A: Employed							
1. Baseline	0.479	(0.058) ^{***}	0.632	(0.091) ^{***}	0.320	(0.066) ^{***}	0.006
2. 2-year age window	0.524	(0.107) ^{***}	0.624	(0.187) ^{***}	0.426	(0.112) ^{***}	0.364
3. 10-year age window	0.478	(0.030) ^{***}	0.642	(0.045) ^{***}	0.281	(0.037) ^{***}	0.000
4. quadratic age term	0.556	(0.092) ^{***}	0.666	(0.161) ^{***}	0.460	(0.098) ^{***}	0.274
Panel B: Receiving social assistance							
1. Baseline	0.434	(0.054) ^{***}	0.430	(0.084) ^{***}	0.438	(0.067) ^{***}	0.941
2. 2-year age window	0.504	(0.106) ^{***}	0.658	(0.223) ^{***}	0.389	(0.112) ^{***}	0.281
3. 10-year age window	0.393	(0.027) ^{***}	0.346	(0.039) ^{***}	0.450	(0.037) ^{***}	0.053
4. quadratic age term	0.394	(0.084) ^{***}	0.515	(0.161) ^{***}	0.295	(0.098) ^{***}	0.243
Panel C: Out of the labor force							
1. Baseline	0.087	(0.051) [*]	-0.062	(0.090)	0.242	(0.061) ^{***}	0.005
2. 2-year age window	-0.028	(0.103)	-0.282	(0.234)	0.186	(0.103) [*]	0.067
3. 10-year age window	0.129	(0.026) ^{***}	0.011	(0.041)	0.269	(0.033) ^{***}	0.000
4. quadratic age term	0.049	(0.084)	-0.180	(0.179)	0.245	(0.089) ^{***}	0.034

Note: Standard errors are in parentheses. Significance levels: * 10 percent; ** 5 percent; *** 1 percent. The dependent variable is the labor market status in 1999 (employed, receiving social assistance, or being out of the labor force). Each entry comes from a separate IV regression based on the fuzzy RD design. The regressions in rows 1 and 4 are based on 90,818 observations for the full sample; for males and females we have 59,726 and 31,092 observations, respectively. The regressions in row 2 are based on 35,348 observations for the full sample; for males and females we have 23,322 and 12,026 observations, respectively. The regressions in row 3 are based on 172,000 observations for the full sample; for males and females we have 112,355 and 59,645 observations, respectively. Row 1 in each panel repeats the baseline regressions presented in Panel A of Table 3. This baseline regression uses a 5-year age window (ages 42.5 – 47.5 as of August 1993) and a linear term in age. In row 2, we limit our sample to a 2-year age window (ages 44.0 – 46.0 as of August 1993). In row 3, we expand our sample to a 10-year age window (ages 40.0 – 50.0 as of August 1993). In row 4, we allow for a quadratic term in age.