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ABSTRACT

Health Effects of Containing Moral Hazard: Evidence from Disability Insurance Reform*

We exploit an age discontinuity in a Dutch disability insurance (DI) reform to identify the health impact of stricter eligibility criteria and reduced generosity. Women subject to the more stringent rule experience greater rates of hospitalization and mortality. A €1,000 reduction in annual benefits leads to a rise of 4.2 percentage points in the probability of being hospitalized and a 2.6 percentage point higher probability of death more than 10 years after the reform. There are no effects on the hospitalization of men subject to stricter rules but their mortality rate is reduced by 1.2 percentage points. The negative health effect on females is restricted to women with low pre-disability earnings. We hypothesize that the gender difference in the effect is due to the reform tightening eligibility particularly with respect to mental health conditions, which are more prevalent among female DI claimants. A simple back-of-the-envelope calculation shows that every dollar reduction in DI is almost completely offset by additional health care costs. This implies that policy makers considering a DI reform should carefully balance the welfare gains from reduced moral hazard against losses not only from less coverage of income risks but also from deteriorated health.

JEL Classification: I14, H53, I38

Keywords: disability insurance, moral hazard, health, mortality, regression discontinuity

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1. Introduction

Many developed countries are experiencing an enormous increase in the number of disability insurance (DI) claimants. In the U.S., the share of individuals receiving DI benefits has more than doubled during the last three decades (Duggan and Imberman, 2009; Wise, 2012) and the share of DI spending in total social security expenditures has risen from one in ten dollars in 1988 to nearly one in five dollars in 2009 (Autor, 2011). Since 2009, the ratio of disability beneficiaries to workers in the U.S. has overtaken that of the Netherlands, which was long regarded the 'world leader' in DI dependency (Burkhauser and Daly, 2011). Much of the increase in DI roles tends to be attributed to moral hazard and governments have responded by tightening eligibility criteria and/or reducing the generosity of benefits (e.g. Autor and Duggan, 2006; Karlström *et al.,* 2008; Staubli, 2011). Such rationalization in the Netherlands has contributed to the country being knocked off the top of the DI dependency league table.

The standard welfare evaluation of DI weighs the gains from insuring income risk against the losses from moral hazard incentives to reduce work effort. Welfare improving reforms encourage the work-able to re-enter employment. Since work capacity is not perfectly observable, indeed this is the reason for the moral hazard problem, there is a risk that mistakes are made in any rationalization program or the process goes too far, such that partially incapacitated individuals for whom work takes an inordinate toll on health are forced back into employment. The health impact may operate, for example, through stress related to re-entry into the labor market, or from work tasks that are physically demanding given certain health conditions. As an example, we refer to the appalling case of Sheila Holt. In February 2014, the UK government issued an apology to the family of Sheila Holt, a woman who had been sent an official letter encouraging her to find work despite having been in a coma for two months (BBC News, 2014). Ms. Holt, who has suffered from severe bipolar disorder from childhood and had not worked for 27 years, had been threatened with a cut in her benefits unless she attended a job-seeking course that is part of a government drive to get individuals on disability insurance back into employment. Apparently, the stress she experienced as a result of participating in the work program provoked a manic episode, which resulted in Sheila's hospitalization. While in hospital, she suffered a heart attack and entered a coma.

This paper examines whether there are many other cases like that of Sheila Holt, perhaps not as extreme but similar in the sense of health deteriorating as a result of more stringent disability insurance (DI). Evidence for any negative health effects of DI reform implies that, besides the direct loss in welfare resulting from deteriorated health, health deterioration may entail health care costs that offset the savings from cuts in DI and will make the reform less beneficial from the perspective of public finances. On the other hand, health may also improve if post-reform re-employment improves the cognitive ability of individuals as is often found in the retirement literature (Rohwedder and Willis, 2010; Mazzonna and Peracchi, 2012; Bonsang *et al*., 2012).

We provide the first evidence we are aware of on the health impact of DI reform by examining the consequences of a Dutch reform in 1993 that entailed medical re-examination of DI recipients, stricter eligibility criteria and benefit cuts. Identification comes from an age discontinuity – the eligibility criteria were made significantly more stringent for disability recipients aged below 45 on August 1st, 1993. Following medical re-examination, many DI beneficiaries had their benefits cut and the average reduction was about 10 percent higher for the younger cohort subject to the more stringent criteria.[1] We exploit the discontinuity in the reform by comparing outcomes in later years of individuals younger than the 45 year-old cutoff at the time of the reform to the outcomes of individuals older than this threshold. The reform was introduced with little warning and so differences in DI generosity arising from it could not have been anticipated. To estimate the effect on health, mortality and medical spending we use administrative panel data providing information on hospitalization and vital status for the total population of Dutch DI claimants.

We find that the reform had a negative impact on the health of female DI claimants. Women subject to the stricter DI eligibility criteria had a 2.2 percentage point higher

[1] See also Borghans *et al.* (2014) for further information.

probability of being hospitalized over twelve years and a 1.4 percentage point higher probability of dying within seventeen years of the reform. There is no significant effect on the hospitalization of males, but their probability of death within seventeen years is estimated to have been reduced by a statistically significant 1.2 percentage points. We hypothesize that the gender difference in the effect is due to the reform tightening eligibility particularly with respect to mental health conditions, which are more prevalent among female DI claimants.

For women, a €1,000 reduction in the annual amount of DI benefits led to a 4.2 percentage point higher probability of being hospitalized over twelve years and a 2.6 percentage point increase in the risk of death within seventeen years. A simple back-of-the-envelope calculation suggests that each €1 reduction in spending on DI benefits was offset by an increase of €0.23 in spending on hospital care, excluding the cost of ambulatory care and prescribed medicines. In addition, using an estimate of the value of a statistical life, the effect on life expectancy arising from each €1 reduction in DI benefits is valued by €0.69. Combining the medical care costs with the value of the lost lives gives a loss almost equal in value to the public finance savings arising from the rationalization of DI for females. While the efficiency gains from the curtailment of any moral hazard effects on work incentives would need to be factored into a full welfare analysis, it would appear that health effects of tighter DI rules can be empirically important and, in addition to the impact on moral hazard, income insurance and public finances, need to be taken into account in any evaluation of the case for DI reform.

While the magnitudes of our estimated effects are obviously specific to the particular Dutch reform, the broad conclusions are relevant in a wider context. Health deterioration arising from measures that push (female) DI recipients back to work suggests that the extent of moral hazard may have been overstated. An employment response to DI eligibility rules and generosity is not sufficient to conclude that moral hazard exists. The difficulty in defining and containing moral hazard with respect to DI lies in the fact that disability is not a clear-cut medical condition. Rather it is a threshold on a continuum of work incapacity, and the pain and discomfort associated with work, that is defined by the policymaker (Diamond and Sheshinski, 1995). Reforms that tighten the eligibility criteria and so raise the threshold of work incapacity may induce individuals who can only work under excessive duress to take up employment causing health to deteriorate. This argument does not deny the existence of substantial moral hazard in DI, nor does it claim that reforms are unnecessary to reduce moral hazard.[2] Rather, the point is that setting the threshold of work incapacity involves balancing losses from the Type I errors that represent moral hazard with the losses from Type II errors that arise when individuals who can only work at an excessive cost to their health are denied DI. Given the generosity of the Dutch DI program in the nineties, one would expect the marginal individual affected by the 1993 reform to have been in better health than the marginal DI recipient in a less generous scheme. This suggests that the negative health effects of tightening DI in other countries and periods may be even greater than we estimate.

The remainder of the paper is organized as follows. In Section 2, we describe the reform used for identification. Section 3 describes the data, Section 4 describes the empirical strategy, and Section 5 presents the results. Section 6 concludes.

2. Disability Insurance in the Netherlands

2.1 Pre-reform

In the Netherlands, all employees were and are still insured against an earnings loss resulting from an illness or infirmity. In the first year after the onset of a disability individuals are entitled to sickness payments. After this first year, they become eligible to

[2] There is a substantial literature on moral hazard in DI. Bound (1989) and Chen and van der Klaauw (2008) found that labor supply of U.S. males over the age of 45 who applied for DI would have been 30 to 40 percent higher if it were not for the availability of DI benefits. Further suggestive evidence for moral hazard is found in the fact that DI applications tend to rise and fall with the unemployment rate (e.g. Bound and Burkhauser, 1999; Autor and Duggan, 2003, 2006). There is also evidence that policies that tighten DI are effective in reducing the number of claimants and bringing people back to work (Autor and Duggan, 2003; Borghans *et al.*, 2014).

receive DI benefits.[3] Note that there is allowance for partial disability and health insurance is not tied to the receipt of DI benefits.

The amount of DI benefit depends on the "degree of disability", which equals the percentage difference between earnings before disability and the potential earnings of the applicant, which prior to the 1993 reform was determined in three steps. First, the applicant was examined by a medical doctor, who compiled a list of job demands that the applicant could still meet.[4] Second, this list was compared with a dictionary of occupations that specified the job demands, as well as the required education level, for each occupation in order to identify a set of occupations the applicant would be able to perform. Only occupations that were no more than two education levels (on a scale of seven) below the level demanded in the applicant's previous occupation were considered. Finally, the applicant's potential earnings capacity was defined as the mean wage of the 5 highest paying occupations in the set of feasible ones, with the further proviso that at least 10 workers should be currently engaged in each of the occupations in the applicant's region (though there should not necessarily be that number of vacancies).[5] When 5 suitable occupations with at least 10 workers could not be identified, the potential earnings capacity was set at 0. The degree of disability was then defined by the percentage loss in earnings due to disability, i.e. the difference between prior labor earnings[6] and the potential earnings capacity divided by prior labor earnings, and grouped into 8 categories varying from 0-15% to 80-100%. The replacement rate was then determined by these categories (see Table 1).

[3] Further institutional background information on the Dutch disability act and recent trends and patterns in Disability Insurance enrollment in the Netherlands can be found in Bovenberg (2000) and García-Gómez *et al.* (2011), respectively.

[4] The complete list includes 27 physical job demands (such as "lifting," "kneeling," and "ability to deal with temperature fluctuations") and 10 psychological skills (such as "ability to work under time pressure," "ability to perform monotonous work," and "ability to deal with conflict").

[5] For this procedure, the Netherlands was divided in 5 regions and in 16 "start regions." Alternative jobs in the "start regions" had to be considered first. Only if less than 5 suitable occupations were available, alternative jobs in the neighboring regions (within one of the main 5 regions) could be considered.

[6] Prior labor earnings were subject to a cap, which was about €36,000/year in 1999.

2.2 The reform

The 1993 reform was designed to lower the generosity of disability benefits and to reduce the number of (full) disability claimants.[7] With this aim, the criteria to define the earnings capacity were changed in two respects.[8] First, eligibility was only based on objective medical information, not the doctor's judgment. This required that a functional work limitation was clearly observable, and could be linked directly to a medical diagnosis. As a result, disabilities related to mental health problems became more difficult to prove. Second, the criteria for identifying suitable occupations were relaxed, such that: (i) all education levels could be considered, (ii) only 3 occupations (rather than 5) with at least 10 workers employed were required to calculate the mean potential earnings, and (iii) the geographic region within which there should be employment in the occupations was expanded roughly threefold.[9] These changes increased the probability of finding a higher-paid occupation that the applicant could still perform, and thereby raised the potential earnings capacity. Furthermore, it became less likely that the potential earnings capacity was set at 0 because insufficient occupations could be identified. For any individual who was enrolled in DI, the potential earnings capacity under the new criteria was always higher than under the old criteria and, hence, DI benefits were lower.

The new criteria for determining the potential earnings capacity applied both to new applicants and to existing claimants aged younger than 50 at the time of the reform (August 1st, 1993). Existing claimants were to be re-examined, which was scheduled to take place by age cohort from 1994 onwards: those aged <35 on the 1st of August 1993 in 1994, 35-40 in 1995, 41-44 in 1996/1997, and 45-49 in 1997-2001. However, shortly before the re-examinations of the oldest cohort started, political pressure led to a parliamentary motion passed in November 1996 ruling that this cohort would be assessed on the pre-reform, more lax criteria. This created a discontinuity in DI stringency

[7] The formal name of the 1993 DI reform is "Terugdringing Beroep op Arbeidsongeschiktheidsverzekeringen (TBA)," which roughly translates as "Reducing claims on disability insurance".
[8] Another important change of the 1993 DI reform was the introduction of an age- and duration-dependent benefit for new DI applicants. However, these changes did not apply to those already receiving disability benefits as of August 1993, i.e. the group that we study here.
[9] Now, rather than occupations in only one of the 16 "start regions", all available jobs within the main region where the individual was residing (out of 5 main regions) could be considered.

between the cohort under the age of 45 and those aged 45-49 that we exploit to estimate the effects of reduced generosity on health outcomes.[10]

3. Data

3.1 Data sources

We extract our data from several large administrative databases maintained by Statistics Netherlands. Individuals can be matched across the various data files by a Random Identification Number (RIN), which is an encrypted Dutch equivalent of the U.S. Social Security number.[11]

We use administrative data on all disability benefits recipients aged 15-64 in the Netherlands for the period 1995-2005. These data originate from the organizations that administered the disability benefits at the time. They include information on entry and exit dates of a disability spell, the degree of disability (in categories), labor earnings prior to DI entry and disability benefit payments. Unfortunately, there is no reliable or consistent information about the medical condition that led to enrollment.

Demographic characteristics of the claimants are obtained from the municipal registries. These contain information on date of birth (year and month), marital status, number of children, nationality, and place of residence for all residents of the Netherlands.

Information on other sources of income are obtained by merging five different administrative datasets: earnings from paid employment of all employees, self-employment earnings, unemployment benefits, general assistance, and receipt of other types of social assistance (from about 30 relatively minor programs). Information about income from paid employment and self-employment comes from the tax authorities and

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[10] We cannot exploit the discontinuity at age 50 since the re-examinations of those ages 48 and 49 were gradually phased out because of the long backlogs that arose at the disability office.

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

social insurance records, whereas the data on social assistance are provided by the organizations that administer these programs. All these files are available from 1999 onwards.

Last, we obtain information on health outcomes from registries of mortality and hospitalizations covering the entire Dutch population. Mortality records are available for the period 1996-2010 from the reports filed by a medical examiner or pathologist. Furthermore, we have information available about all hospitalizations for the period 1996-2005 with medical and administrative details for all patients who have had inpatient hospital treatment in the Netherlands.[12] We distinguish between hospitalizations for various types of diseases based on ICD-9 classification.[13] We estimate the effect on the probability of a hospitalization since 1996, and every year until 2005, as well as on the cumulative number of hospitalizations from 1996 up till a given year.

3.2 Sample definition

Our baseline sample consists of all individuals who (i) received DI benefits on August 1st, 1993, (ii) were aged between 40 and 50 on that date, and (iii) were still on DI as of January 1st, 1996. The first restriction follows from the fact that the discontinuity in DI generosity only applies to disability claimants enrolled at the time the reform went into effect. The second restriction is made to create a sample of individuals who are relatively comparable in all other respects except for the assessment of earnings capacity made on re-examination and the consequent difference in DI generosity. The standard criterion by Imbens and Kalyanaraman (2012) for setting the window of a regression discontinuity

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[12] From 2006 onwards, the hospital registry continued but participation became voluntary for hospitals. Therefore, from 2006 onwards the number of observations drops substantially and the sample is no longer representative for the Dutch population.

[13] International statistical classification of diseases and related health problems. Admissions for the following diseases and conditions are distinguished: circulatory system, musculoskeletal system and connective tissue, neoplasm, nervous system, sense organs, respiratory system, digestive system, genitourinary system, symptoms and ill-defined conditions, injury and poisoning, blood and blood-forming organs, dermatological and subcutaneous tissue, congenital anomalies, nutritional disorders and mental disorders. Note that mental illnesses are not very well covered in the hospital data, as people with mental illness are not very likely to be hospitalized.

(RD)[14] would suggest taking an estimation sample only including claimants aged between 43 and 47. However, with this age window there is insufficient power: at most 0.30 for males and 0.25 for females. Therefore we have extended the age window to claimants aged 40 to 50. Point estimates with the smaller sample are not different from the ones for the wider sample (see section 5.3 for more details). The last restriction is imposed by the fact that data on DI status are available only from 1996. From those observed on DI in January 1996, we select those whose records show they had been claiming at least since 1st January 1993. Given that the re-examinations of the individuals in our sample did not start before 1996 and the parliamentary motion excluding the cohort aged 45 and above from the more stringent criteria was only passed in November 1996, we do not expect any differential attrition to have taken place around the age of 45 prior to January 1st, 1996.[15] This assumption is supported by Appendix Figure B1 where we show that there is no differential exit out of DI for individuals aged below and above 45 in 1995.

We drop all non-native Dutch from our sample because of problems with certain groups of immigrants that potentially invalidate the identification strategy. First, more than a proportional share of Turkish immigrants is registered as being born in January, which would appear to reflect inaccurate birth registration and is problematic for our identification based in date of birth.[16] Second, there are large differences in the share of immigrants from the East Indies across cohorts either side of the threshold determining exposure to the more stringent test.[17] We cannot rule out that there are similar problems with other groups of immigrants. We decide to take a conservative approach and drop all non-native individuals from our sample given that there are differences in health and life-expectancy between individuals from different ethnic backgrounds. However, we will

[14] More information about our empirical strategy follows in section 4.

[15] Please note that throughout this paper we refer to cohorts by their age at the time the reform went into effect (i.e., as of August 1st, 1993).

[16] It was common for families in rural areas of Turkey not to register new-born children immediately after they were born, but rather wait until more children were born and then register them all at once. Frequently, parents did not remember then the exact birth date of their children, and registered them as being born on January 1st.

[17] Directly after the Second World War (WWII) a large inflow of immigrants from the East Indies who fought in the Royal Netherlands East Indies Army came to the Netherlands and had children shortly after arrival. These people are aged 47 at the time of the reform, which creates important differences in the composition of the group aged 40-44 compared to the group aged 45-49.

show in section 5.3 that the results are not changed when non-native Dutch are included in the sample. After these sample restrictions, our baseline sample in 1996 contains 91,090 males and 46,669 females in 1996.[18]

3.3 Summary statistics

Table 2 presents a summary of our variables of interest. Panel A shows the characteristics of the individuals in our sample as of 1st January 1996 before they were re-examined. On average, the claimants in our sample have spent 7-8 years on disability insurance at the time the reform was implemented. The majority of the sample is fully disabled (between 80 and 100% earnings reduction). This holds in particular for women.

Panel B presents the means of our key outcome variables. In the interest of space, we only present the health indicators for the last year of our observation period. By the year 2005, which is the last year for which we have information on hospitalizations, more than half of the claimants had been in hospital at least once. On average, individuals in our sample have been in hospital about twice by 2005. For women the number of hospitalizations was slightly higher than for men. On the other hand, the mortality rate was higher for men. By 2010, 15 percent of the male, but only 11 percent of the female, DI claimants had died.

[18] About 3 percent of the individuals in our sample appear in more than one disability record in a given month. These observations are dropped because it is not clear whether they are administrative/coding errors or whether they arise from individuals who are entitled to multiple DI benefits because they were employed in multiple jobs before the onset of disability. Since there is no discontinuity at age 45 in the probability of someone having multiple disability records, we believe that omitting/including those with multiple records would not substantively change our results. Furthermore, in order to take into account that hospitalizations are only observed for people that are still alive, we censor all observations for people deceased in the hospitalization measures. Hence, the number of observations for hospitalizations for 1997 - 2005 is slightly lower than in 1996. Note that our results do not change if we would not censor observations for deceased individuals.

4. Empirical Strategy

The 1993 DI reform in the Netherlands has had a significant impact on the average benefits that a DI claimant received. On average, disability benefits were reduced by €1,302 and €535 for men and women under the age of 45, respectively (see also Table 3, first stage). Borghans *et al.* (2014) evaluate the employment and income effects of the same DI reform and also find – using a similar identification strategy - that the more stringent re-examinations have reduced the generosity of the DI program for individuals under the age of 45. For this younger cohort the reform caused the annual DI benefits to drop by about 10 percent more than for individuals aged 45 and older. Furthermore, the replacement rate was 5.9 percentage points lower and exit out of disability was 3.8 percentage points higher for the affected cohort at the discontinuity.[19] We exploit these effects to examine whether the reform had any impact on health.

We estimate separate models for the various health outcomes. Treatment is indicated by a dummy equal to 1 for those subject to the stricter re-examinations (i.e. those aged under 45). Our set of controls include age in months as of the 1st of August 1993, an interaction between the treatment indicator and age measured in months younger/older than turning 45 in August 1993 to allow the age trend to differ either side of the discontinuity that defines treatment, 6 dummies for the degree of disability in 1st January 1996, pre-DI earnings, a dummy for being married in 1996, 39 regional dummies, and duration on DI at the start of the reform. To control for seasonal patterns in mortality by month of birth (Doblhammer and Vaupel, 2001), we also control for 12 month of birth dummies. Finally, we include a dummy for those born between May 1945 and January 1946, and a dummy for those born between February 1946 and January 1947 to allow for potential differences in the composition of the baby boom generation and for the possibility that those born shortly after World War II differ in health status from other cohorts because of conception during the last months of the war that were particularly harsh in the Netherlands (Scholte *et al.*, 2012).

[19] Further information on these first stage effects can be found in Borghans *et al.* (2014).

The key identifying assumption is that the stringency of the criteria applied in the DI re-examinations is the only discontinuous change relevant to the health outcomes at the cutoff age of 45. Although the data do not allow us to test this, we are not aware of any other policy change that would create a discontinuity around this age. We have regressed all covariates used in the analysis on the treatment indicator in a reduced-form RD specification to check for any discontinuities around age 45. Under the null hypothesis that there is no effect, from 51 placebo regressions we would expect to find a significant coefficient for only 3 (at 5%) or 5 (at 10%) of the covariates. We find a significant effect for only 3 of them.[20]

5. Results

5.1 Impact on mortality

Figure 1 shows the probability of death by 2010 for the individuals in our data. There is an upward jump in the male mortality rate at the age of 45, which corresponds to the age from which the more generous DI reassessment applied, but a downward jump in female mortality. Men aged below 45 that were subject to the stricter regime had a 1.2 percentage point lower probability of having died by 2010 than older men, controlling for age. Younger women that experienced the tighter reassessment had a 1.4 percentage point higher probability of death. This corresponds to a relative effect of about 13 percent. Estimating mortality in earlier years (Figure 2) we find a consistently positive effect on mortality for women, and a consistently negative effect for men, where the magnitude of both increases over time and reaches significance in the last two years (see also Appendix Table B2 for the results). Interpreting these estimates as causal effects would suggest that tightening the eligibility criteria and reducing benefits raised the mortality rate of women, but reduced that of men. In Section 5.3. we show that these results are robust to various

[20] The full results are available in Table B1 in the Appendix

robustness checks,[21] and consider possible explanations for this gender difference in section 5.4.

As a result of the re-examination, some individuals lost all their DI benefits because they were considered ineligible under the new criteria, whereas others experienced a marginal reduction in their benefits. In order to give an impression of the average effect on mortality for those who were affected by the reform, we use an IV standard fuzzy RD specification to scale our reduced-form estimates by the amount of the reduction in disability benefits. The IV estimates of Table 3 show that a €1,000 reduction in DI benefits led to an increase in the cumulative mortality rate by 2010 among women of 2.6 percentage points, which is a relative increase of 23 percent.[22] For men we find that a €1,000 reduction in DI benefits has led to a 0.9 percentage point (i.e. 6 percent) reduction in mortality by 2010. Note that the average reduction in DI benefits due to this reform was €1,302 for men and €535 for women.

The consistent effects of this DI reform on mortality for men and women imply an important welfare cost to society. In order to give an impression of the magnitude of this cost, we try to monetize this health impact using the value of a statistical life in the Netherlands, which is valued at €2.2 million in 2001 prices (Blaeij, 2003). Using a simple back-of-the-envelope calculation, the mortality effects from Table 3 suggest that a €1,000 reduction in disability benefits implies an additional total welfare loss of about €1,023 million in 2010 prices.[23] Total budgetary savings would be €1,486 million in 2010 prices. This suggests that every euro reduction on DI benefits due to the reform led to an additional welfare loss of about €0.69. Note that this is a very simple calculation not taking into account indirect budgetary effects, such as savings on future old-age pension

[21] One may argue that our analyses involve running many regressions which increases the probability of a Type 1 error. However, our significant effects do not appear randomly: we find quite persistent significant effects for certain types of hospitalizations over time, whereas others are never significant. This suggests that what we find are true health effects rather than Type 1 errors.
[22] Unfortunately, we have insufficient observations for each cause of death to investigate whether there are particular causes of death driving this result.
[23] This includes both the effect on men and women. More information about this calculation can be found in the Appendix section A1.

benefits and future health care expenditures for those deceased.[24] Therefore, the numbers should be interpreted with caution. Nevertheless, this simple calculation shows that that the reform – which aimed to reduce government spending on disability – was less successful from a welfare point of view due to the substantial effects on later mortality. In the next section, we investigate whether there were any further health effects, which can account for the mortality effects.

5.2 Impact on hospitalizations

Figure 3 shows the effect of the reform on the probability of being hospitalized between 1996 and 2005. In moving from older to younger cohorts, there is an upward jump in the hospitalization probability for women below the age of 45 who were subject to the more stringent re-examinations. There is no clear discontinuity for males. A graph of the cumulative number of hospitalizations until 2005 shows a similar pattern.[25] The reduced-form estimates are presented in Figure 4. By 2005, "treated' women (i.e. subject to the stricter re-examinations) on average faced a 2.2 percentage points higher probability of being hospitalized and 0.25 additional hospitalizations compared to older "control" women (i.e. subject to the weaker re-examination regime).[26] Furthermore, the effects on hospitalizations are not only apparent in the long run but already from 1996. In addition, while the effect on admission probability in the short-run is almost as large as in the long-run, there is an upward trend in the effect on the number of hospitalizations. This suggests that health worsens shortly after the decrease in DI benefits, and that it does not recover over time, but deteriorates even further. This is not inconsistent with our finding of mortality effects only in the long-run.

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[24] Also, one may argue that the value of a statistical life of an average individual in The Netherlands exceeds that of a disabled individual, since the life expectancy of this unhealthy population is on average lower and the value of a statistical life may decrease as life expectancy decreases (Viscusi and Aldi, 2003). However, there are no existing estimates neither about the value of a statistical life of our relevant population group, or about to what extent the value for a healthy individual should be reduced to obtain the value for a disabled individual, or the difference in life-expectancy between our population group and the average population in order to use a life expectancy-adjusted value of statistical life. Hence, we have not adjusted the value of a statistical life in our simple calculation.

[25] Results not shown but available from the authors.

[26] The estimation results can also be found in Appendix Table B2. Note that the number of observations decreases over time due to the fact that hospitalizations for deceased are censored.

Table 4 presents the results from the IV regressions in which the reduced-form estimates are scaled by the amount of the reduction in disability benefits. The results indicate that for every thousand euro reduction in DI benefits, women experience a 4.2 percentage points higher probability of being hospitalized. This is a relative increase of about 6.9%. Furthermore, every thousand euro reduction in DI benefits leads to about 0.46 additional hospitalizations by 2005 (18% increase).

In order to better understand how the reform had an impact on health, we investigate the probability that women are hospitalized by type of diagnose from 1996 to 2005.[27] Figure 5 presents the IV estimates over time for those types of admission diagnoses where the reform had a significant impact over most of the years.[28] We find that the increased probability of hospitalization is mostly due to mental, musculoskeletal, nervous and digestive problems. These are typically diseases with a large occupation-related component (Krause *et al.*, 2001) and hence they may be the result of re-employment after the re-examinations. This possibility will be investigated in more detail in section 5.4. In addition, it is noteworthy the large relative increase of the probability of being hospitalized by 2005 by a mental health problem (68%) or an illness associated to the nervous system (36%) after a €1,000 reduction in the amount of DI.

The fact that the effects on hospitalizations remain in the long run suggests that the budgetary savings on DI spending are partly offset by health care spending on hospitalizations. We use information on the average costs of hospitalization by type of treatment, and calculate that health care spending on hospital care increases by €346 million (see Appendix section A2 for more information on the calculation). Given that total savings were €1,486 million, this implies that for every €1 saving on DI health care expenditures on hospitalizations went up by €0.23 cents. This is a very rough estimate and a lower bound of the true effect, since we only take into account whether or not an individual has been hospitalized in the period 1996-2005 and not the total number of

[27] Unfortunately, we do not have reliable information on the medical condition that led to enrollment in DI.
[28] The IV estimates for all types of hospitalizations up till 2005 and their mean prevalence are shown in the Appendix Table B3.

hospitalizations. In addition, other health care costs are excluded from the analysis as we do not have information on outpatient visits or pharmaceutical consumption. Hence, our findings suggest that the budgetary consequences of the health effects following the DI reform are an important and non-negligible side-effect.

5.3 Robustness of the results

Table 5 presents the results of various sensitivity analyses. In Panel A we present the results of estimations without any control variables. In Panel B we include a quadratic in age in the model. The results remain mostly unchanged when leaving out the controls or including a quadratic in age in the model. Panel C shows the outcomes when immigrants are included in the sample. The results for women remain unchanged. For men, the significant mortality effects disappear. This is consistent with differences in the share of immigrants (at least from East Indies) between age groups, and lower life expectancy of non-natives compared to natives. In addition, measurement error in the month and year of birth would bias our estimates towards zero. Panel D shows the results of estimations when we use a different definition of hospitalizations in which only hospitalizations that last at least two nights are considered. One could think that the previous results on hospitalizations for females were driven by women going more often to the hospital trying to get diagnosed to go back into DI. However, the effects on the probability of hospitalization by 2005 are similar when we use a more stringent definition of hospitalizations, and therefore the Dutch DI reform has had true health effects for women.

As mentioned in section 4, we choose a slightly larger bandwidth than would be optimal based on the Imbens-Kalyanaraman criterium given the lack of power for the smaller bandwidth. One may be concerned that the health effects are therefore driven by the inclusion of individuals further away from the threshold so they are the result of age differences. For our results for women we argue that it is unlikely to be the case as treated individuals are younger and this would imply lower mortality and hospitalizations for the treated group. However, for men we cannot rule out that this is the case. However, in Figures 6 and 7 we investigate how sensitive our results are to the chosen bandwidth size.

The figures illustrate that the point estimates of our models are quite robust to bandwidth size.[29] More specifically, the results remain stable after the optimal bandwidth of 24 months according to the Imbens-Kalyamaran procedure. The standard errors of the estimates, however, are sensitive to the bandwidth, and the figures show that as the power increases (larger bandwidth includes larger number of observations), the estimates also become more precise.

5.4 Are the effects driven by changes in employment or total income?

Our results show that changes in the DI generosity have important health effects on DI recipients. The negative health effects among women are likely to be driven by increased stress related to new employment situation or reduced income, and/or occupational diseases if the person enters into paid employment. The reduced generosity of this reform has been found to have effects both on employment and total income. Borghans *et al.* (2014) have shown that participation in employment went up by 2.9 percentage points after the reform. Also total income changed, albeit to a lesser extent: on average, for every euro lost on DI benefits, about 92 cents is compensated through increased labor earnings or other benefit income. We exploit our information on employment status and total income to disentangle the importance of both in explaining the total health effects.

We re-estimate our IV models using two additional scaling variables: a dummy for whether the individual is in paid employment and individual total income in 1999 (i.e. after the re-examinations were finished). Table 6 Panel A shows that conclusions are similar if we use employment status as the scaling variable and the treatment dummy as the instrument. There are health costs (gains) for those women (men) that get into paid employment due to the stricter eligibility criteria. On the other hand, income changes driven by the reform do not seem to explain changes in mortality or hospitalizations (Panel B). For women, the reform did not have an effect on total income (no first stage effect). This is in line with the finding by Borghans and co-authors (2014) that individuals are able to (nearly) fully compensate a drop in DI benefits via higher labor

[29] Only for very small bandwidths they are quite different, but one should be careful interpreting these point estimates since also the standard errors are very large.

market earnings and/or other social security benefits. For men, the reform had an effect on total income, but a €1,000 reduction in total income is not associated with a statistically significant mortality effect. Therefore, the previous health effects for men and women must be driven by individuals who left DI and moved into employment due to the more stringent DI criteria, and not by those who stayed on DI with reduced benefits.

5.5 Why did the reform only harm the health of women?

Our results have shown clear gender differences in the health effects of the Dutch DI reform. It may be that the reform affected men and women in a different way. One of the implications of the new criteria is that disabilities related to mental health problems became more difficult to prove than physical health problems (see section 2). Women are more likely to suffer from mental problems (37% of females vs. 28% of males), while men are more likely to suffer from physical health, like musculoskeletal (31% of males and 26% of females) and cardiovascular (8% of males vs. 3% of females) problems (UWV, 2006).[30] Then, one would expect the average man who left DI to be healthier than the average woman who left DI, as it is more likely that the reclassification was driven by a health recovery among men. This could explain the gender differences as employment may be more hazardous when initial health is poorer. Similarly, one might also expect heterogeneous effects by type of health condition. Unfortunately, information about type of illness is missing in our data so we cannot test for this.

In order to ascertain what can explain the gender differences, we pool observations for men and women, and make a distinction by three other variables that may drive these results: degree of disability (full/partial), disability duration and earnings level. The results are shown in Table 7.

First, women are more likely to be *fully* disabled compared to men (see Table 2). One would expect that fully disabled would suffer more from a DI reform that decreases the

[30] Numbers are averages calculated for period after the reform (1994-2005).

amount of benefits and encourages disabled individuals to go back to work than partially disabled, as the labor market opportunities for the former are expected to be worse. The DI reform raises the probability of mortality by 2010 for both fully and partial disabled women, while it decreases the probability of mortality by 2010 only for fully disabled men (Panel A of Table 7). In addition, the increase in both the probability and the number of hospitalizations by 2005 only appear for fully disabled women. In addition, the estimated effect is statistically different for males and females in the two subgroups (partial and fully disabled). This suggests that the gender-specific findings cannot (only) be attributed to differences in the level of disability between genders.

Second, we investigate the time spent on DI at the time the reform came into effect. On the one hand, individuals who have been on DI for longer may suffer more from forced labor market re-entry as they have been detached from the labor market for longer. On the other hand, the health of long-term disabled may have improved while on DI, therefore they may well be able to find re-employment without this having adverse health effects. From Table 2 we know that women have slightly shorter DI spells than men. Panel B of Table 7 shows separate results by time spent in DI. We find that the mortality effects for men are concentrated among long-term disabled, whereas there are negative effects for women who are either long-term or short-term disabled. Moreover, the difference in the direction of the effect between males and females remains among the long-term disabled. This suggests that gender-specific findings can neither (only) be driven by differences in the time spent on DI between genders.

There are large differences in the labor market history and opportunities between men and women, as women tend to be low wage earners. Therefore, our results could be well driven by lower wage earners (mainly women) having to return to more health hazardous jobs compared to higher earners (mainly men). In Panel C of Table 7 we separately estimate the effect of the reform for those below and above the median level[31] of labor market earnings (measured before entry in DI). We find that the negative health effects are indeed concentrated among low earning women, whereas the positive health effects

[31] Note that we use the overall median wage, not gender specific. When using gender specific medians, we could not separate the gender effects from the earnings effect.

for men are concentrated among high earning men. Figure 8 shows the reduced form effects on mortality over time for the four different groups (low earnings females, high earnings females, low earnings males, and high earnings males). We find that the positive health effects on high earning males affected by the reform are apparent in the short-run (from 1998). Moreover, the estimated effect on high earning females is similar in both size and sign, and not statistically different, to the estimated effect on high earning males, although it is never statistically significant. The lack of statistical significance can be due to the low number of females that are among the high earners, as only 24% of females have pre-DI earnings above the median. The differences between high earning males and low earning females are clear since the early 2000s. This finding suggests that part of the gender differences is driven by differences in the skill level between males and females in our sample. However, this cannot be the only explanation as the reform did not have any effect on low earning males. In addition, these results pose some equity concerns on the effects of the DI reform, as the health gains are concentrated among the high-earners while the losses are concentrated among the low-earners.

Furthermore, gender differences in occupations may be responsible for these differential health effects. LaMontagne *et al.* (2008) have shown that job strain related to depression is mostly concentrated in low skilled occupations, and national statistics show that female recipients in the relevant age group are about 10 percentage points more likely to suffer from mental health problems compared to males (UWV, 2006). Hence, this suggests that the combination of the type of health problems and the occupational position makes women more vulnerable to the reform. Unfortunately, type of illness is not registered in our data and we cannot test this hypothesis directly.

6. Conclusion

In this paper, we investigate whether disability insurance reform aiming to contain moral hazard may have additional welfare effects via changes in people's health. The standard assessment of welfare effects of DI balances the gains from insuring incomes against the losses from moral hazard. Welfare improving reforms aim to encourage the work-able to become re-employed. However, when this is pushed too far and even less work-able are forced back to work this may come at a long term cost to their health. Such adverse health effects are not only undesirable from a human perspective, but also entail additional health care expenses which offset the savings from reduced expenditure on DI. Little is known about health effects of DI reforms and a better understanding of these effects is crucial for designing effective public policy.

We exploit a cohort discontinuity in the stringency of the 1993 Dutch disability reforms to obtain causal estimates of the effects of decreased generosity of disability insurance (DI) on health outcomes of existing DI recipients. We find that the reform in Dutch DI has had important health effects which last for more than 15 years after the reform. Women aged 44 and younger which are subjected to the stricter re-examinations experience more hospitalizations over time. Furthermore, their mortality is 1.4 percentage points higher by 2010 than for the cohort aged at least 45. For men, on the other hand, mortality rates were reduced by 1.2 percentage points. Therefore, there were clear gender differences in the effect of the reform. A plausible explanation is that the reform affected men and women in a different way, as mental health problems, which were more prevalent among women, became more difficult to prove than physical health problems. This would imply that women leaving DI were on average in worse health than men, and therefore more sensitive to the occupational risks. Our data do not allow us to test for this.

We investigate if other factors, which are correlated with gender, may be responsible for the gender differences. Our results show that the gender-specific findings cannot (only) be attributed to differences in the level of disability neither to differences in the time in DI between genders. However, earnings differences play an important role. We find that the health gains are concentrated among high earning males, while the health costs are paid by low earning females. Job strain related to depression seems to be concentrated in low skilled occupations (LaMontagne, 2008). This suggests that the combination of the type of health problems and the occupational position makes women more vulnerable to the reform.

Regardless of the specific driving mechanisms behind these gender differences, our results illustrate that there were clear winners and losers from this reform. We believe this is a relevant lesson applicable to other institutional settings and DI reforms. On the one hand, tightening of the eligibility criteria to reduce moral hazard can not only increase labor force participation but also improve population health if targeted to the appropriate groups. On the other hand, if the criteria do not allow benefits entitlement to individuals in some vulnerable groups, the incentives to go back to work may have irreversible damages to their health. In order to advance in the identification of the groups that can benefit from a DI reform and the groups that society needs to protect, future research should focus on better identification of the effects of DI policies on individuals with different health conditions and in different occupational groups.

A simple back-of-the-envelope calculation shows that every €1 reduction on DI benefits was offset by €0.23 additional health care expenses on hospitalizations. Furthermore, the welfare loss of deteriorated health through increased mortality resulting from every €1 reduction on DI was valued as €0.69. Borghans *et al* (2014) evaluate the effects of this reform on social support substitution, and find that for every €1 reduction on DI, individuals collect 31 cents more from other social security programs. These results should change the moral hazard debate. They show that the discussion should not only focus on the tradeoff between providing disability insurance and making people go back to work, but that there are other important behavioral effects that may not only compensate the savings from reduction in the DI generosity, but entail additional costs in other government budgets and the society as a whole.

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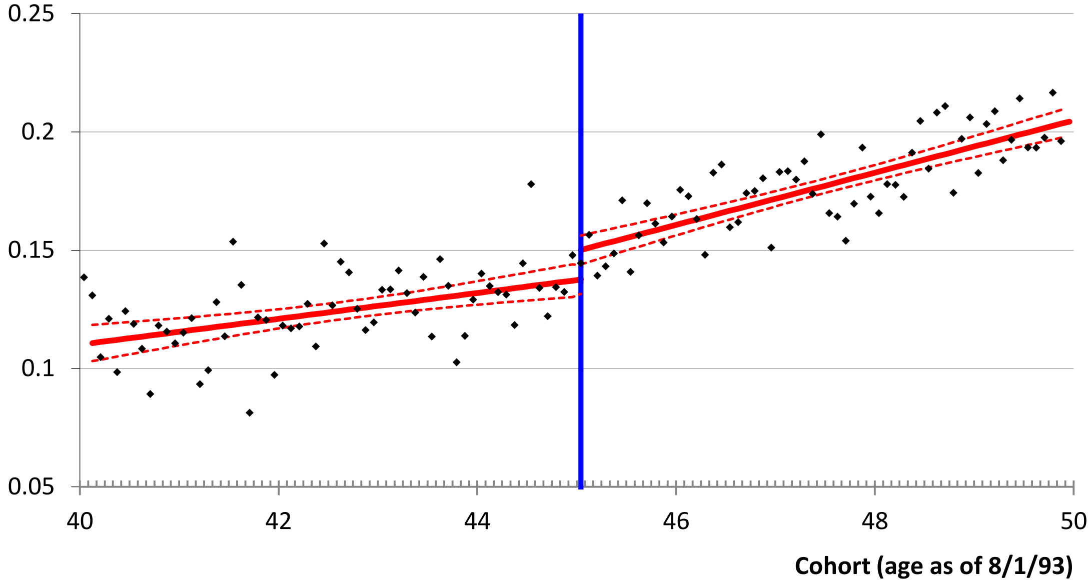
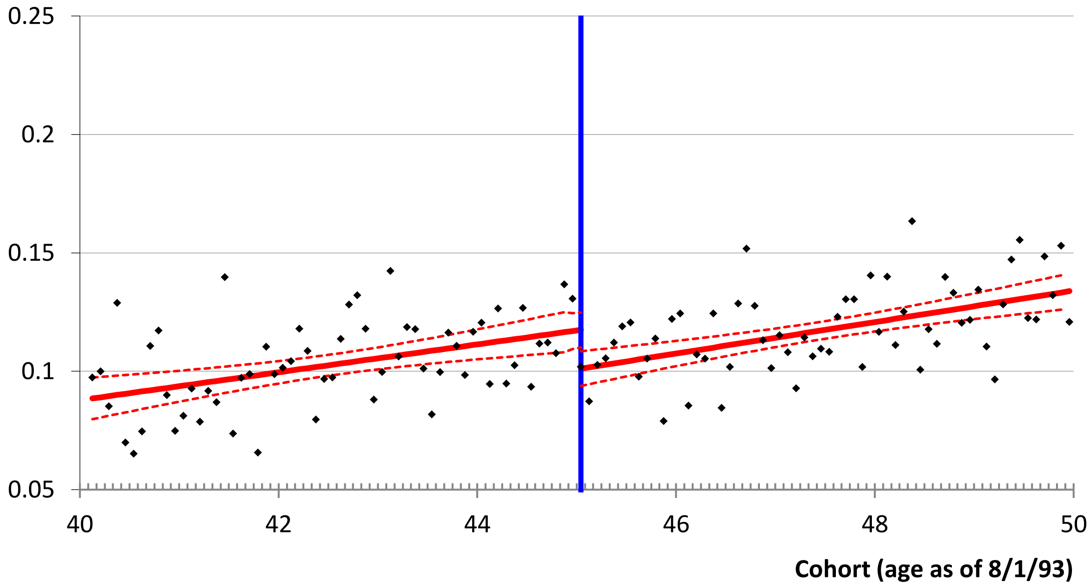
Figures and Tables

Figure 1: Effect of DI Reform on mortality by 2010
A. Males

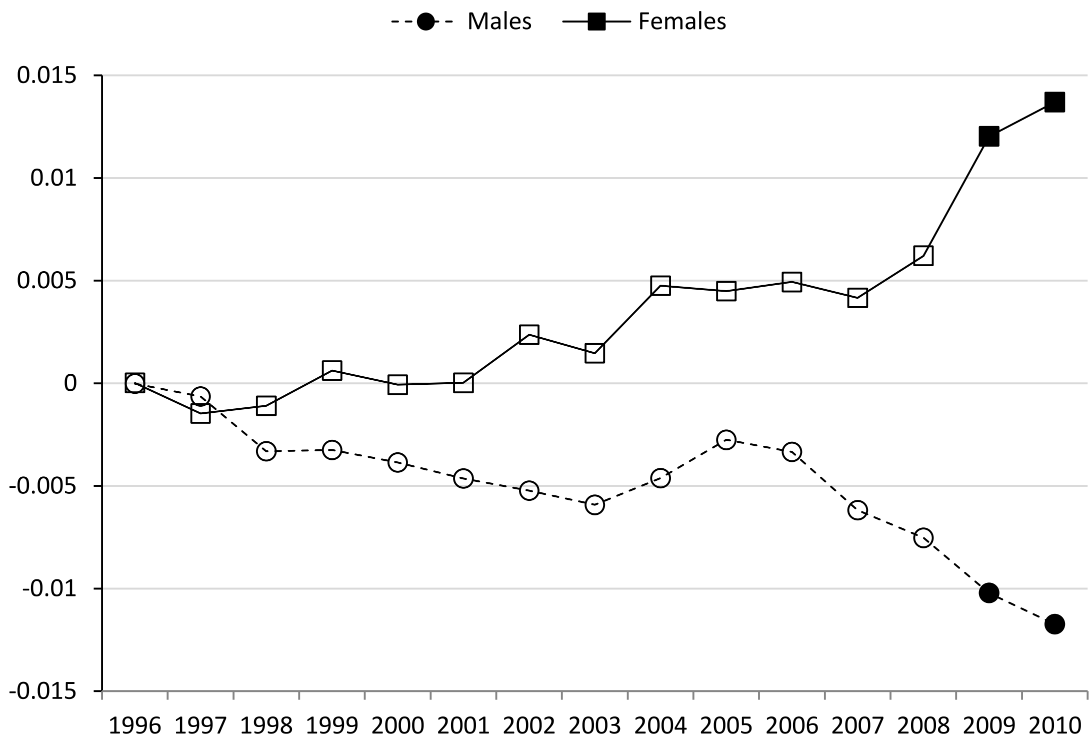


B. Females



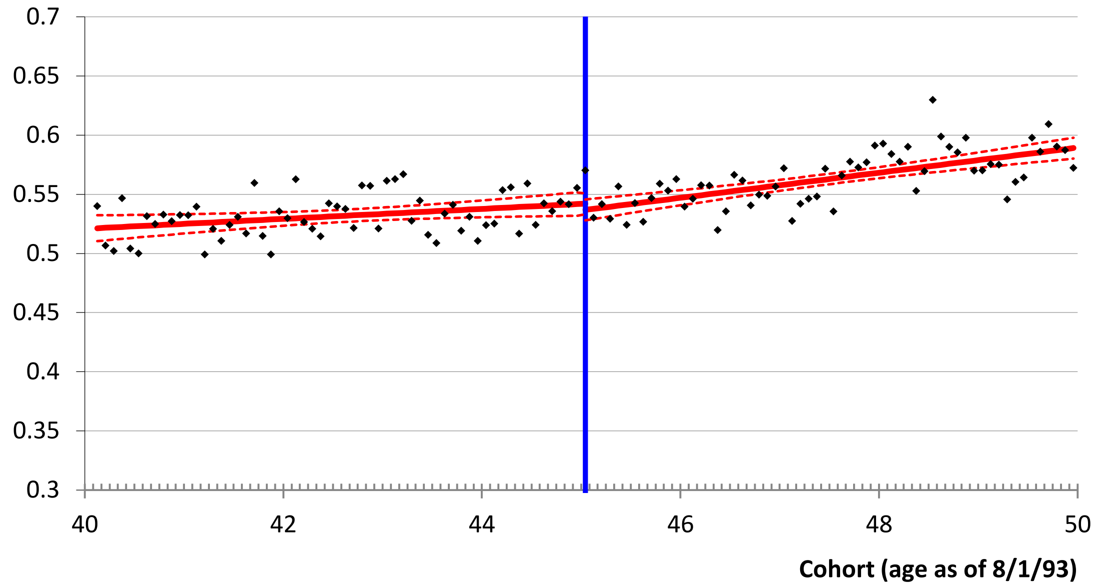
Note: Dashed lines represent the 95% confidence intervals. Regression estimates come from RD reduced-form regressions with the following controls: a dummy for being subject to the strict re-examinations (treatment), its interaction with [age-45], and age in months as of 8/1/93.

Figure 2: Reduced-form effects of DI Reform on mortality 1996-2010

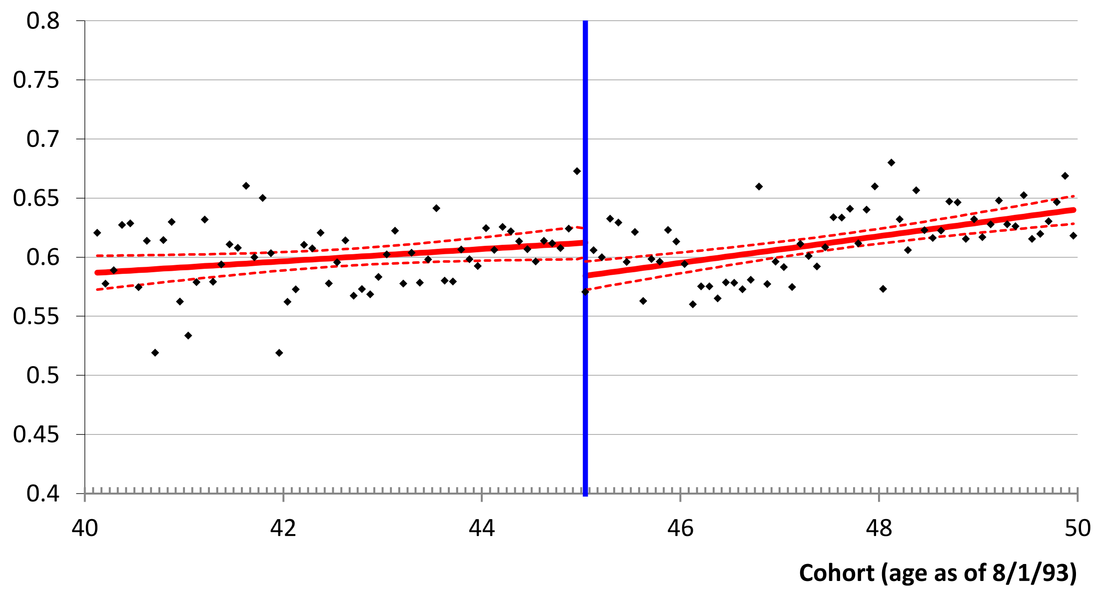


Note: Unfilled markers refer to coefficients not significantly different from zero at a 10% level. Black markers are significant at a 5% level, and grey markers are significant at the 10% level. Each dot in the figure comes from a separate regression. The following controls are used in the regressions: a dummy for being subject to the strict re-examinations (treatment), its interaction with [age-45], age in months as of 8/1/93, 6 dummies for degree of disability in 1996, pre-DI earnings, a dummy for being married in 1996, a dummy for being conceived during and born shortly after WWII (May 1945 – February 1946), a dummy for being conceived and born after WWII (February 1946 – January 1947), 39 regional dummies, 11 month of birth dummies, and duration in DI at the start of the reform.

Figure 3: Effect of DI reform on probability of being hospitalized by 2005
A. Males



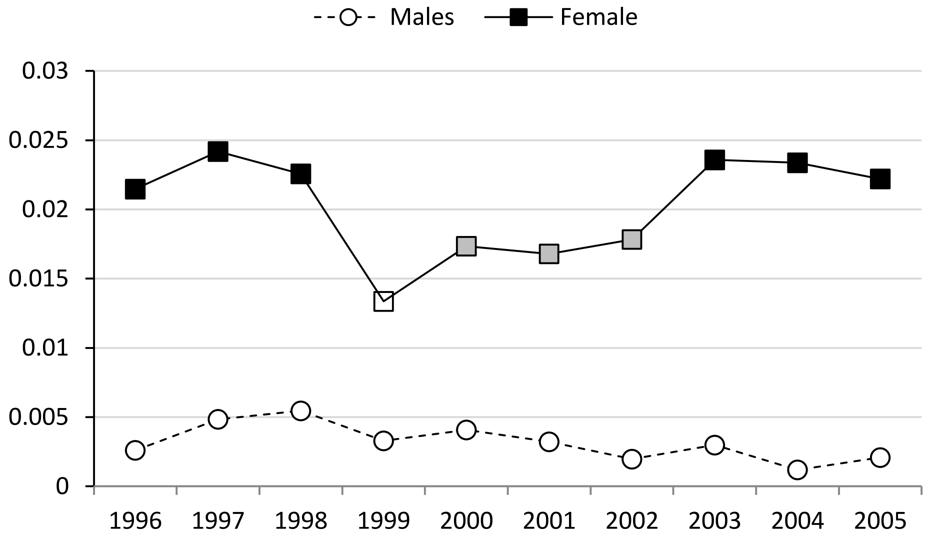
B. Females



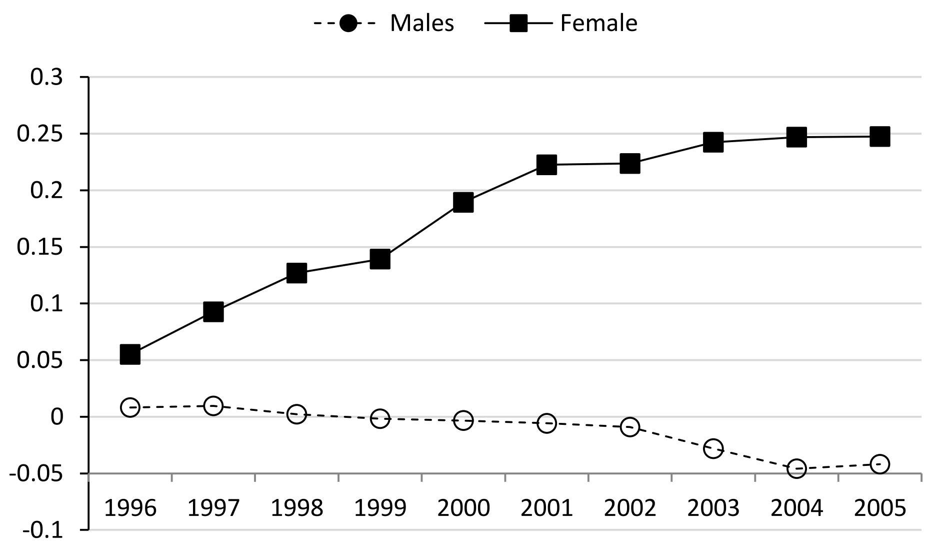
Note: Dashed lines represent the 95% confidence intervals. Regression estimates come from RD reduced-form regressions with the following controls: a dummy for being subject to the strict re-examinations (treatment), its interaction with [age-45], and age in months as of 8/1/93.

Figure 4: Reduced-form effects of DI reform on hospitalizations 1996-2005

A. Probability of being hospitalized

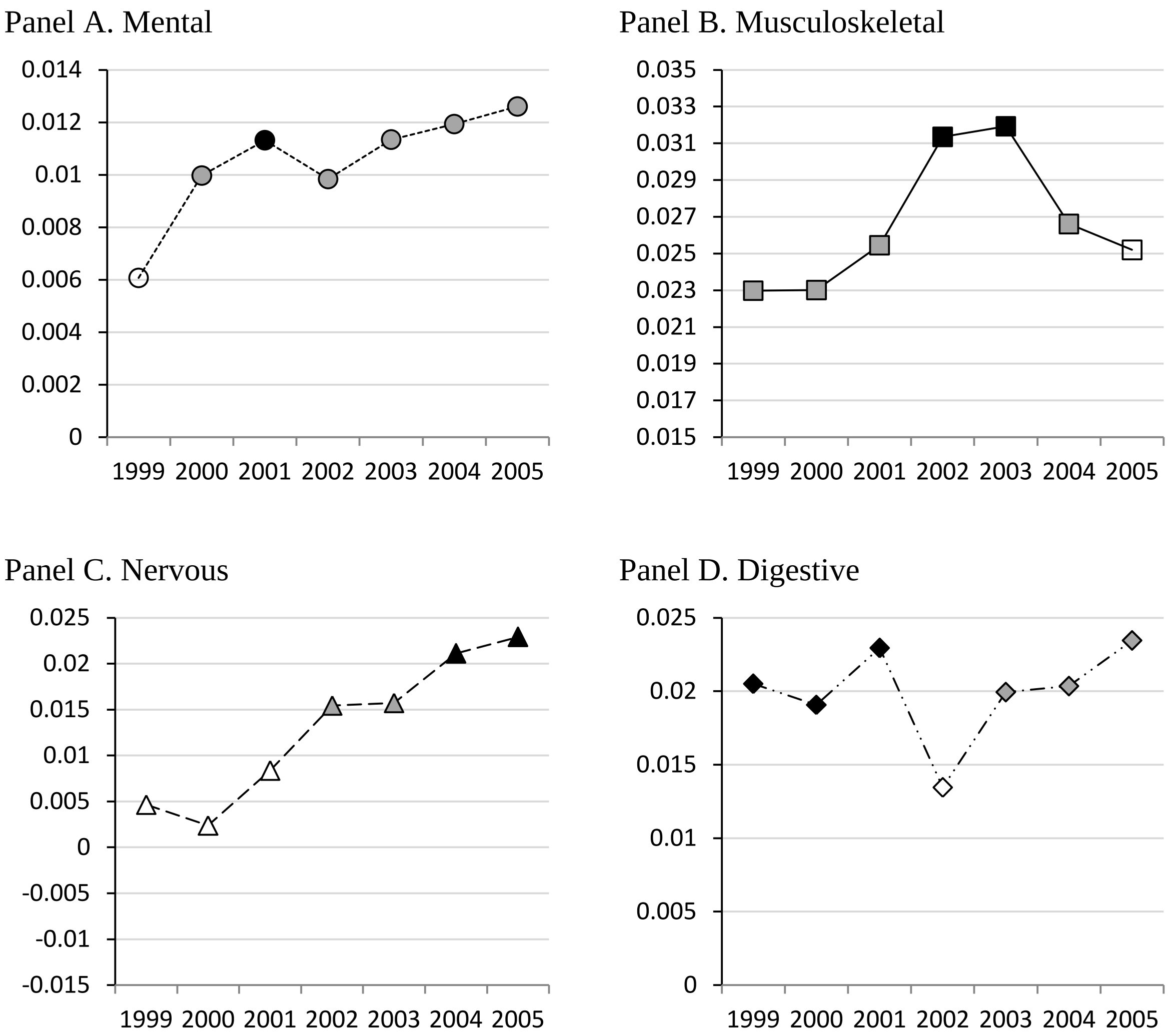


B. Cumulative number of hospitalizations



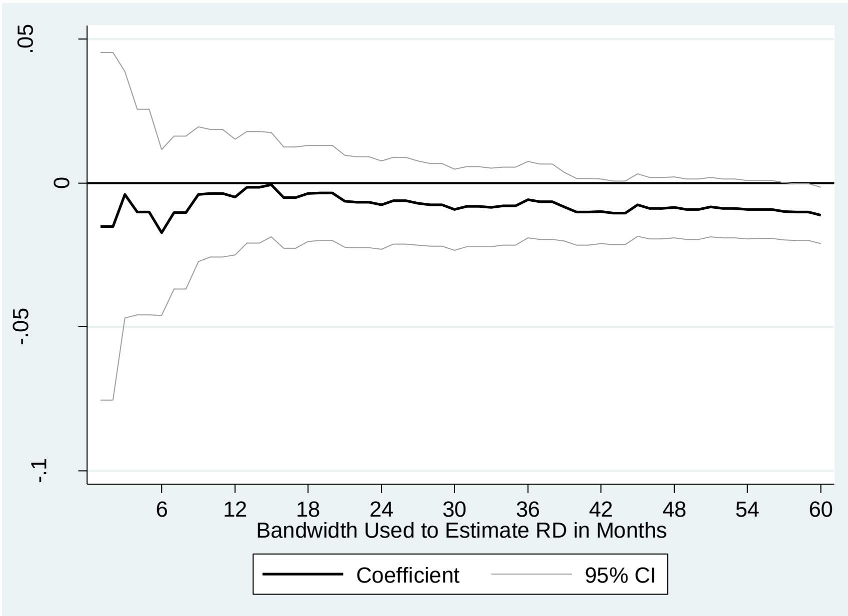
Note: Unfilled markers refer to coefficients not significantly different from zero at a 10% level. Black markers are significant at a 5% level, and grey markers are significant at the 10% level. Each dot in the figure comes from a separate regression. See the note to Figure 2 for the demographic controls included in the regressions.

Figure 5: IV results for probability of being hospitalized by cause - Females

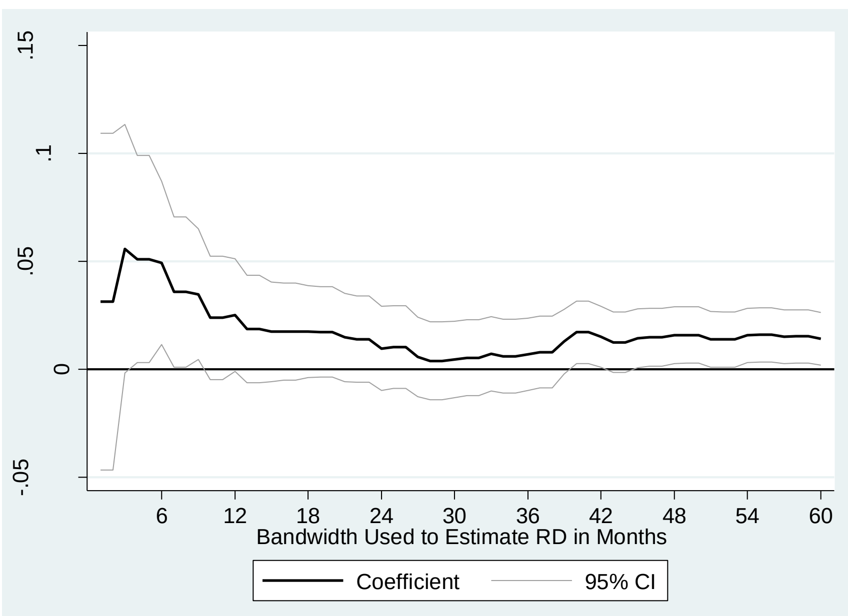


Note: Unfilled markers refer to coefficients not significantly different from zero at a 10% level. Black markers are significant at a 5% level, and grey markers are significant at the 10% level. Each dot in the figure comes from a separate regression. In the IV regressions the variable that is instrumented is the amount of DI, so the coefficients can be interpreted as effect size per €1,000/year decrease in DI. The instrument itself is the treatment dummy (aged under 45 as of 8/1/93). The following controls are used in the regressions: a dummy for being subject to the strict re-examinations (treatment), its interaction with [age-45], age in months as of 8/1/93, 6 dummies for degree of disability in 1996, pre-DI earnings, a dummy for being married in 1996, a dummy for being conceived during and born shortly after WWII (May 1945 – February 1946), a dummy for being conceived and born after WWII (February 1946 – January 1947), 39 regional dummies, 11 month of birth dummies, and duration in DI at the start of the reform.

Figure 6: Sensitivity to choice of bandwidth of the reduced-form estimates of the effect of the DI reform on mortality by 2010.
A. Males



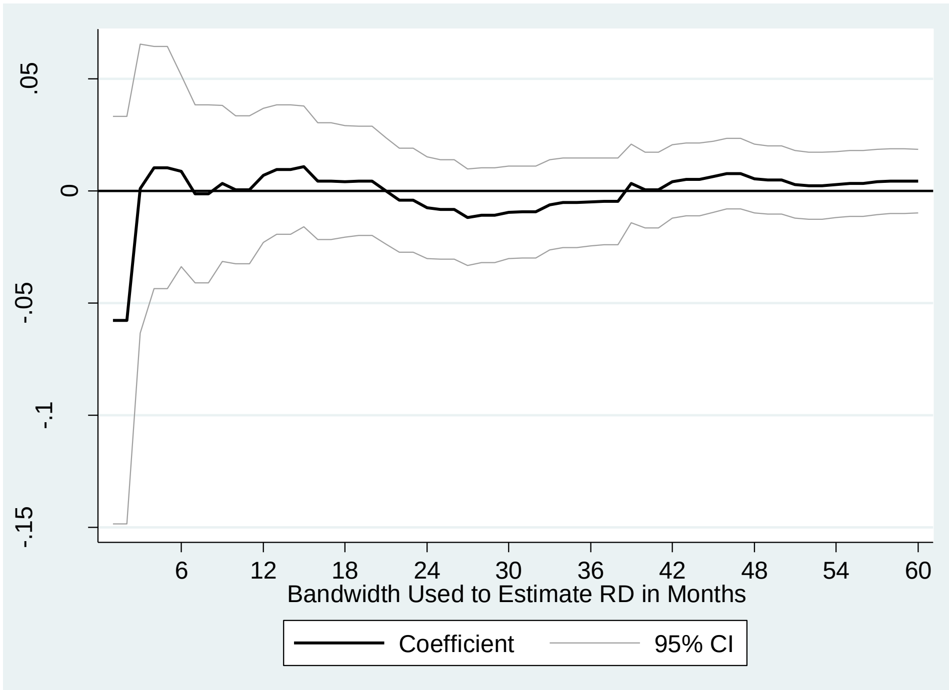
B. Females



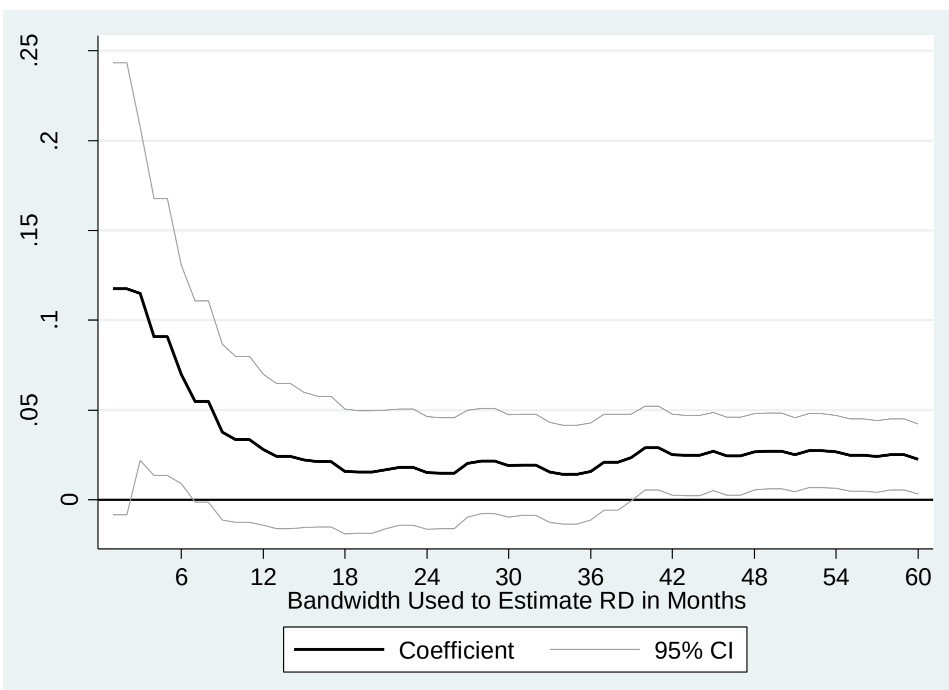
Note: Estimates come from set of RD reduced-form regressions in which the bandwidth is increased gradually by one-month from 1 to 60 months. The following controls are used in the regressions: a dummy for being subject to the strict re-examinations (treatment), its interaction with [age-45], age in months as of 8/1/93, 6 dummies for degree of disability in 1996, pre-DI earnings, a dummy for being married in 1996, a dummy for being conceived during and born shortly after WWII (May 1945 – February 1946), a dummy for being conceived and born after WWII (February 1946 – January 1947), 39 regional dummies, and duration in DI at the start of the reform.

Figure 7: Sensitivity to choice of bandwidth of the reduced-form estimates of the effect of the DI reform the probability of being hospitalized by 2005.

A. Males

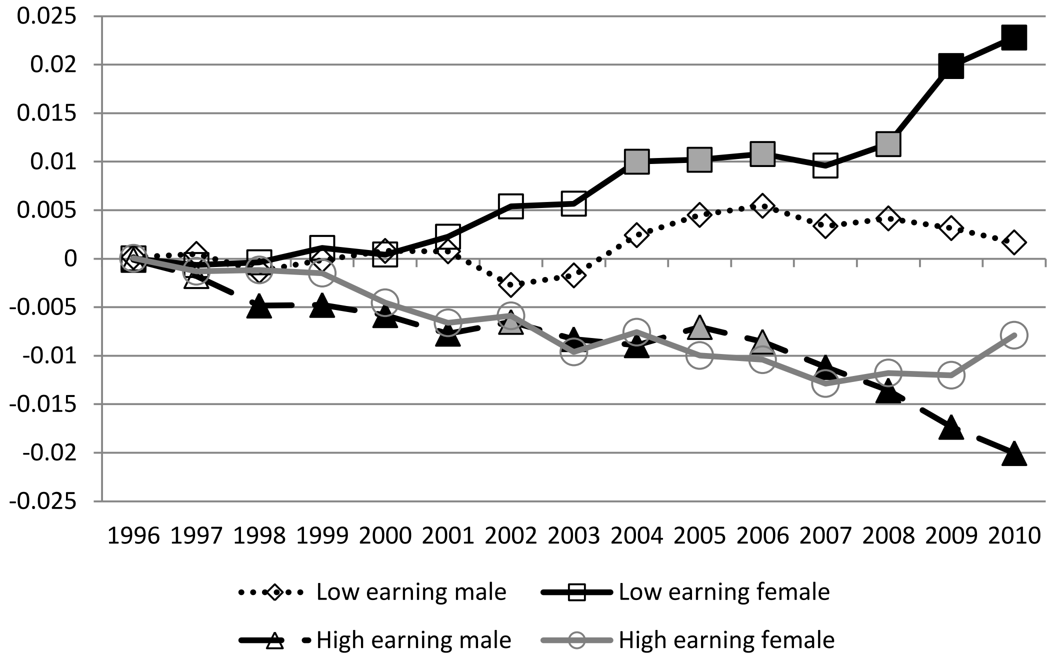


B. Females



Note: Estimates come from set of RD reduced-form regressions in which the bandwidth is increased gradually by one-month from 1 to 60 months. See the note to Figure 6 for the demographic controls included in the regression.

Figure 8: Reduced-form effects of DI Reform on mortality 1996-2010, by previous earnings



Note: Unfilled markers refer to coefficients not significantly different from zero at a 10% level. Black markers are significant at a 5% level, and grey markers are significant at the 10% level. Estimates for each year come from a separate regression. The following controls are used in the regressions: a dummy for being subject to the strict re-examinations (treatment), its interaction with each of the four subgroups, its interaction with [age-45], age in months as of 8/1/93, a different age trend for each of the four subgroups, 6 dummies for degree of disability in 1996, pre-DI earnings, a dummy for being married in 1996, a dummy for being conceived during and born shortly after WWII (May 1945 – February 1946), a dummy for being conceived and born after WWII (February 1946 – January 1947), gender, 39 regional dummies, 11 month of birth dummies, and duration in DI at the start of the reform.

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

	Males			Females		
	All	40.0-44.9	45.0-49.9	All	40.0-44.9	45.0-49.9
Panel A: Sample characteristics, measured prior to re-examination						
Age on August 1st, 1993	45.49	42.64	47.51	45.47	42.59	47.52
Duration in DI on August 1st, 1993 (months)	101.37	94.95	103.36	90.28	91.14	89.68
Degree of disability (% of earnings capacity lost):						
15-25	7.4	13.5	6.3	4.8	5.5	4.4
25-35	11.5	14.4	10.1	5.0	5.7	4.5
35-45	9.0	8.4	8.1	3.5	3.9	3.2
45-55	7.1	5.1	6.9	4.9	4.9	4.9
55-65	2.4	2.1	2.5	1.6	1.7	1.5
65-80	2.5	2.2	2.5	1.2	1.2	1.2
80-100	60.1	54.3	63.6	79.0	77.1	80.4
Panel B: Health outcomes after re-examination						
Hospitalizations by 2005:						
Probability of being hospitalized 1996-2005 (%)	55.0	52.6	55.9	60.7	60.0	61.3
Cumulative number of hospitalizations to 2005	2.04	1.88	2.10	2.56	2.59	2.55
N	82921	34824	48097	43687	18241	25446
Mortality (%):						
By 2010	15.6	12.5	17.6	11.2	10.3	11.8
N	91090	37547	53543	46669	19385	27284

Note: Since we have information available from 1996 onwards, the degree of disability is recorded in January 1996 (before the re-examinations).

Table 3: IV estimates. Effect of reform per 1000 €/year decrease in amount of original DI. Results for mortality 1996-2010

	Males	Females
First Stage	-1.302 (0.075)****	-0.535 (0.076)***
1999	-0.002 (0.002)	0.001 (0.005)
2000	-0.003 (0.002)	<0.001 (0.006)
2001	-0.004 (0.002)	<0.001 (0.007)
2002	-0.004 (0.003)	0.004 (0.007)
2003	-0.005 (0.003)*	0.003 (0.008)
2004	-0.004 (0.003)	0.009 (0.008)
2005	-0.002 (0.003)	0.008 (0.009)
2006	-0.003 (0.003)	0.009 (0.010)
2007	-0.005 (0.003)	0.008 (0.010)
2008	-0.006 (0.004)	0.012 (0.011)
2009	-0.008 (0.004)**	0.023 (0.011)**
2010	-0.009 (0.004)**	0.026 (0.012)**
N	91090	46669

Note: Standard errors are in parentheses. Significance levels: * 10 percent; ** 5 percent; *** 1 percent. Each entry in the table comes from a separate regression. In the IV regressions the variable that is instrumented is the amount of DI, so the coefficients can be interpreted as effect size per €1000/year decrease in DI. The instrument itself is the treatment dummy (age less than 45 as of 8/1/93). The following controls are used in the regressions: a dummy for being subject to the strict re-examinations (treatment), its interaction with [age-45], age in months as of 8/1/93, 6 dummies for degree of disability in 1996, pre-DI earnings, a dummy for being married in 1996, a dummy for being conceived during and born shortly after WWII (May 1945 – February 1946), a dummy for being conceived and born after WWII (February 1946 – January 1947), 39 regional dummies, 11 month of birth dummies, and duration in DI at the start of the reform.

Table 4: IV estimates. Effect of reform per 1000 €/year decrease in amount of original DI. Results for probability of being hospitalized in 1996-2005 and number of hospitalizations in 1996-2005

	Males			Females		
	Probability of being hospitalized between 1996 and year	Cumulative number of hospitalizations	N	Probability of being hospitalized between 1996 and year	Cumulative number of hospitalizations	N
1999	0.003 (0.005)	-0.001 (0.025)	88767	0.025 (0.018)	0.260 (0.105)**	45819
2000	0.003 (0.005)	-0.003 (0.029)	87906	0.032 (0.019)*	0.354 (0.127)***	45486
2001	0.002 (0.005)	-0.004 (0.034)	87012	0.031 (0.019)	0.416 (0.143)***	45142
2002	0.002 (0.005)	-0.007 (0.040)	86043	0.033 (0.020)*	0.418 (0.163)**	44787
2003	0.002 (0.006)	-0.022 (0.046)	85034	0.044 (0.020)**	0.453 (0.186)**	44443
2004	0.001 (0.006)	-0.035 (0.053)	83959	0.044 (0.020)**	0.462 (0.213)**	44077
2005	0.002 (0.006)	-0.032 (0.061)	82921	0.042 (0.020)**	0.463 (0.237)**	43687

Note: Standard errors are in parentheses. Significance levels: * 10 percent; ** 5 percent; *** 1 percent. Each entry in the table comes from a separate regression. In the IV regressions the variable that is instrumented is the amount of DI, so the coefficients can be interpreted as effect size per €1000/year decrease in DI. The instrument itself is the treatment dummy (age less than 45 as of 8/1/93). See the note to Table 3 for the demographic controls included in the regression.

Table 5: Robustness of reduced form results

	Males	Females
Panel A: No controls		
Mortality by 2010	-0.012 (0.005)**	0.014 (0.006)**
N	91090	46669
Cum. # of hospitalizations by 2005	-0.037 (0.080)	0.247 (0.121)**
Prob. of hospitalizations by 2005	0.002 (0.007)	0.022 (0.010)**
N	82921	43687
Panel B: Quadratic age trend		
Mortality by 2010	-0.012 (0.005)**	0.014 (0.006)**
N	91090	46669
Cum. # of hospitalizations by 2005	-0.053 (0.081)	0.228 (0.123)*
Prob. of hospitalizations by 2005	0.001 (0.007)	0.027 (0.010)***
N	82921	43687
Panel C: Sample with immigrants		
Mortality by 2010	-0.006 (0.005)	0.012 (0.005)**
N	108271	56872
Cum # of hospitalizations by 2005	-0.015 (0.072)	0.243 (0.103)**
Prob. of hospitalizations by 2005	0.004 (0.007)	0.019 (0.009)**
N	98650	53379
Panel D: Hospitalizations with at least 2 nights		
Prob. of hospitalizations by 2005	-0.003 (0.007)	0.022 (0.010)**
N	82921	43687

Note: Standard errors are in parentheses. Significance levels: * 10 percent; ** 5 percent; *** 1 percent. Each entry in the table comes from a separate regression. See the note to Table 3 for the demographic controls included in the regression. Estimates in Panel C also include 10 national origin dummies.

Table 6: Results for different scaling variables

	Males	Females
Panel A: Instrument employment dummy (IV)		
First stage	0.036 (0.005)***	0.029 (0.005)***
	Effect of being re-employed in 1999 after the re-examination	
Mortality by 2010	-0.322 (0.143)**	0.426 (0.195)**
N	91090	58048
Cum. # of hospitalizations by 2005	-1.149 (2.188)	8.487 (3.706)**
Prob. of hospitalizations by 2005	0.057 (0.200)	0.817 (0.327)**
N	82921	54491
Panel B: Instrument total income (in €1000) (IV)		
First stage	-0.315 (0.155)**	0.134 (0.110)
	Effect of 1000€/year decrease in total income in 1999	
Mortality by 2010	-0.037 (0.025)	
N	91090	
Cum. # of hospitalizations by 2005	-0.133 (0.261)	
Prob. of hospitalizations by 2005	0.007 (0.023)	
N	82921	

Note: Standard errors are in parentheses. Significance levels: * 10 percent; ** 5 percent; *** 1 percent. Each entry in the table comes from a separate regression. See the note to Table 3 for the demographic controls included in the regression.

Table 7: Different heterogeneous effects. Reduced-form effects of DI Reform

	Mortality by 2010	Cum. # of hospitalizations by 2005	Prob. of being hospitalized by 2005
Panel A: By DI-level in January 1996			
$\beta_{Full, Male}$	-0.017 (0.006)***	-0.141 (0.102)	-0.001 (0.009)
$\beta_{Full, Female}$	0.014 (0.007)**	0.297 (0.120)**	0.025 (0.011)**
$\beta_{Partial, Male}$	-0.006 (0.007)	0.051 (0.001)	0.006 (0.010)
$\beta_{Partial, Female}$	0.022 (0.012)*	0.196 (0.199)	0.016 (0.017)
$\beta_{Full, Male} = \beta_{Full, Female}$	[<0.001]	[0.003]	[0.042]
$\beta_{Partial, Male} = \beta_{Partial, Female}$	[0.025]	[0.493]	[0.581]
$\beta_{Full, Male} = \beta_{Partial, Male}$	[0.210]	[0.192]	[0.587]
$\beta_{Full, Female} = \beta_{Partial, Female}$	[0.522]	[0.634]	[0.612]
Panel B: By time in DI at August 1st, 1993			
$\beta_{>=5yr, Male}$	-0.020 (0.005)***	-0.133 (0.093)	-0.001 (0.008)
$\beta_{>=5yr, Female}$	0.022 (0.008)***	0.291 (0.136)**	0.023 (0.012)*
$\beta_{<5yr, Male}$	<0.001 (0.007)	0.061 (0.110)	0.005 (0.010)
$\beta_{<5yr, Female}$	0.008 (0.008)	0.264 (0.141)*	0.025 (0.012)**
$\beta_{>=5yr, Male} = \beta_{>=5yr, Female}$	[<0.001]	[0.008]	[0.090]
$\beta_{<5yr, Male} = \beta_{<5yr, Female}$	[0.429]	[0.242]	[0.205]
$\beta_{>=5yr, Male} = \beta_{<5yr, Male}$	[0.006]	[0.119]	[0.534]
$\beta_{>=5yr, Female} = \beta_{<5yr, Female}$	[0.179]	[0.873]	[0.876]
Panel C: By earnings before DI			
$\beta_{Low, Male}$	0.002 (0.007)	-0.183 (0.111)	-0.019 (0.010)*
$\beta_{Low, Female}$	0.023 (0.007)***	0.253 (0.120)**	0.029 (0.011)***
$\beta_{High, Male}$	-0.020 (0.005)***	0.009 (0.093)	0.012 (0.008)
$\beta_{High, Female}$	-0.007 (0.011)	0.356 (0.183)*	0.008 (0.016)
$\beta_{Low, Male} = \beta_{Low, Female}$	[0.024]	[0.006]	[<0.001]
$\beta_{High, Male} = \beta_{High, Female}$	[0.645]	[0.086]	[0.788]
$\beta_{Low, Male} = \beta_{High, Male}$	[0.003]	[0.123]	[0.004]
$\beta_{Low, Female} = \beta_{High, Female}$	[0.004]	[0.597]	[0.220]

Note: Reduced form results. Standard errors are in parentheses. P-values in square brackets. Significance levels: * 10 percent; ** 5 percent; *** 1 percent. Each set of estimates per outcome and panel in the table comes from a separate regression. The following controls are used in the regressions: a dummy for being subject to the strict re-examinations (treatment), its interaction with each of the four subgroups, its interaction with [age-45], age in months as of 8/1/93, a different age trend for each of the four subgroups, 6 dummies for degree of disability in 1996, pre-DI earnings, a dummy for being married in 1996, a dummy for being conceived during and born shortly after WWII (May 1945 – February 1946), a dummy for being conceived and born after WWII (February 1946 – January 1947), gender, 39 regional dummies, 11 month of birth dummies, and duration in DI at the start of the reform.

Appendix A: Back-of-the-envelope calculation of effectiveness of DI reform

A.1. Welfare effect

	Total	**Males**	**Females**
Number of individuals	137,759	91,090	46,669
Average savings per individual per year		1,000	1,000
Savings per year	138M	91M	47M
Total savings of a €1,000 reduction in DI benefits by 2010 (corrected for 2% inflation)	1,486M	983M	503M
Value of a statistical life in 2010	2.6M	2.6M	2.6M
Estimated mortality effect of a €1,000 reduction in DI benefits		-0.9 p.p.	2.6 p.p
Percentage effect of a €1,000 reduction in DI benefits (estimated effect/average mortality)		-5.769	23.214
Total deaths by 2010		14,210	5,227
Additional number of deaths due to a €1000 reduction in DI benefits		-820	1,213
Total loss of a €1,000 reduction in DI benefits by 2010	1,023M	-2,132M	3,155M
Average additional loss per euro saved on DI benefits	€0.69		

Gains

If DI benefits were reduced by €1,000 annually for each of the 46,669 women and 91,090 men in the age group in this study, this implies that annual DI benefits were reduced by €138 million per year from 1999 onwards (i.e. the first year for which all re-examinations were completed for the relevant age group). Hence, by 2010, using an inflation correction of 2% per year and assuming no attrition from DI due to death or other reasons, this implies a total saving of €1.49 billion.

Losses

According to Blaaeij (2003), the value of a statistical life in the Netherlands is €2.2 million in 2001 prices, which amounts to €2.6 million in 2010 prices. For every €1,000 reduction in DI benefits mortality for men is decreased by 5.8%. Given 91,090 men and an average mortality rate of 15.6% this implies 820 less fatalities due to the reform, which have a total value of €2,132 million.

For women, on the other hand, for a €1,000 reduction in DI benefits, mortality is increased by 23%. Given 46,669 women and an average mortality rate of 11.2% this implies 1,213 additional fatalities due to the reform, which have a total value of €3,155 mln.

Net welfare effect of DI reform

A simple back-of-the-envelope calculation shows that for every €1 saved on DI benefits there is on average an additional welfare loss of €0.69 due to mortality effects.[32] Although this is just a rough estimate, it does point to the fact that a DI reform aimed at reducing moral hazard may have substantial negative welfare effects for some individuals. Furthermore, the additional costs coming from the increased number of hospitalizations for women are not taken into account here. This will be done in section A2.

[32] Note that in this calculation we do not aim to provide an overall budgetary effect of the reform. We only take into account the *direct* monetary savings on DI due to the reform, which we compare with the *direct* health effects. There are other indirect monetary savings, e.g. additional income tax revenues on re-employment labor income, but also other indirect monetary costs, e.g. additional spending on other social security benefits, where the latter are likely to outweigh the former (see Borghans *et al.* (2014) for more details).

A.2. Budgetary effect

Budgetary gains

The savings on DI are similar as to those calculated in section A1, i.e. €1.49 billion.

Budgetary losses

Information on the total costs on hospitalizations in The Netherlands is taken from the National institute for Public Health and the Environment (www.kostenvanziekten.nl). The costs for the relevant hospitalizations are presented in the second column (*A*) of the Table below. Column 3 (*B*) presents the effect of the reform on the cumulative probability of a particular type of hospitalization for a €1,000 reduction in DI benefits. The percentage effect of a €1,000 reduction in DI benefits due to the reform is shown in the fourth column (*D* = *B/C*), which is then used to calculate the additional health spending on hospitalizations by 2005 (*E* = *D**A*).

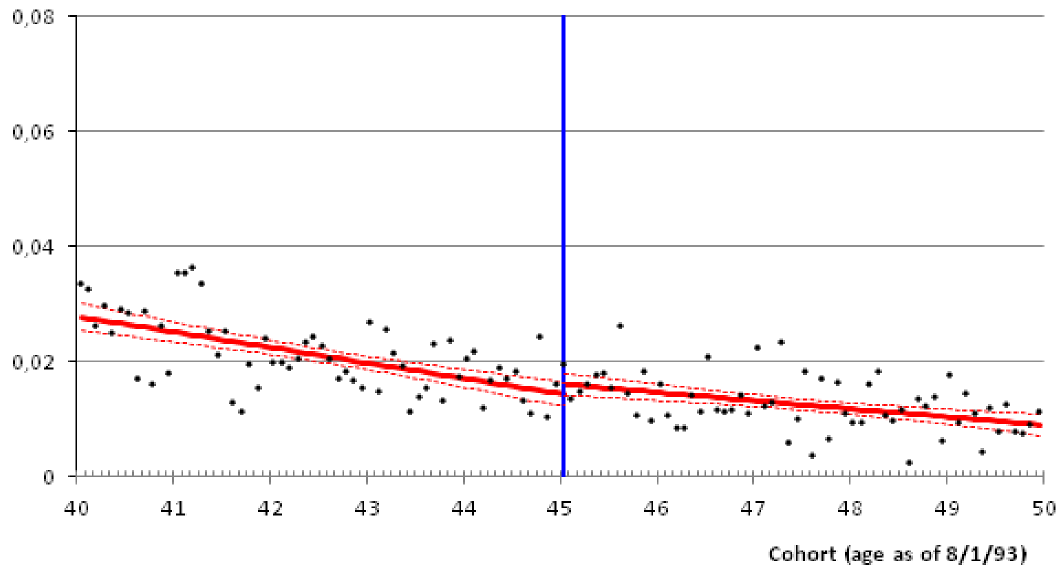
	Costs (M € 2005) *A*	Effect of reform 2005 *B*	Average Prob. of hosp. in 2005 *C*	Effect *D*	Additional health costs *E*
Mental	524.1	0.013	0.028	0.464	243.332
Musculoskeletal	157.3	0.025	0.204	0.123	19.277
Nervous	118.6	0.023	0.064	0.359	42.604
Digestive	211.4	0.023	0.118	0.195	41.205
Total costs					€ 346.418M
Total savings					€ 1,486M
Average additional hospitalization costs per euro saved on DI benefits					€0.23

Budgetary effect

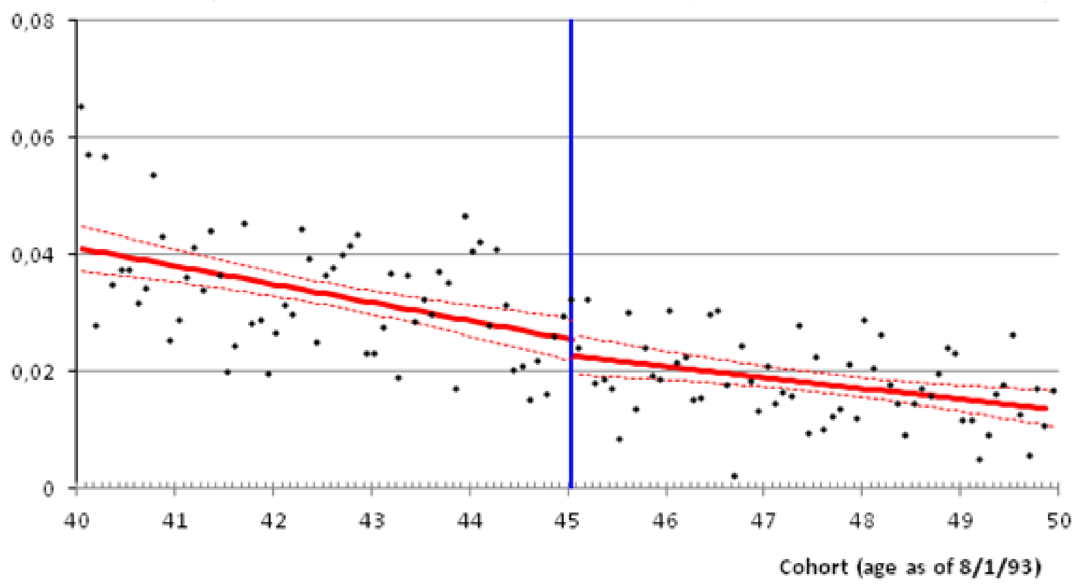
Based on the direct monetary savings on DI and the direct additional spending on hospitalizations, we can conclude that every euro saved on DI benefits is offset by about €0.23 on additional hospitalization expenses. Note that this is an underestimation of the actual costs, since we only take into account whether or not someone has been hospitalized in the period 1996-2005 and not the number of hospitalizations per individual.

Appendix B: Additional figures and tables

Figure B1: Exit from DI in 1995
A. Males



B. Females



Note: Immigrants are not excluded from this Figure as we cannot identify the country of origin for those who left DI in 1995. Regression estimates come from RD reduced-form regressions with the following controls: a dummy for being subject to the strict re-examinations (treatment), its interaction with [age-45], age in months as of 8/1/93.

Table B1. Placebo tests. Reduced-form results for the effect of DI reform on different characteristics

Variable	Effect of the reform		Variable	Effect of the reform	
Degree of disability			*Corop region dummies*		
Dummy(15-25%)	0.003	(0.003)	Corop1	-0.002	(0.001)
Dummy(25-35%)	-0.005	(0.003)	Corop2	-0.001	(0.001)**
Dummy(35-45%)	-0.001	(0.003)	Corop3	0.000	(0.002)
Dummy(45-55%)	-0.002	(0.003)	Corop4	0.001	(0.002)
Dummy(55-65%)	0.002	(0.002)	Corop5	-0.001	(0.001)
Dummy(65-80%)	0.002	(0.002)	Corop6	0.001	(0.001)
Dummy(80-100%)	0.001	(0.005)	Corop7	0.000	(0.001)
			Corop8	0.001	(0.001)
Previous labor earnings	31.788	(38.017)	Corop9	0.000	(0.001)
No prev. lab. earn. info.	-0.003	(0.003)	Corop10	-0.002	(0.002)
			Corop11	-0.002	(0.001)
Dummy(married)	0.002	(0.005)	Corop12	0.001	(0.002)
			Corop13	-0.003	(0.002)
Duration in DI	0.594	(0.849)	Corop14	0.001	(0.002)
			Corop16	-0.001	(0.001)
			Corop17	0.000	(0.003)
			Corop18	-0.001	(0.002)
			Corop19	-0.002	(0.001)
			Corop20	-0.003	(0.001)**
			Corop21	-0.001	(0.001)
			Corop22	-0.002	(0.001)
			Corop23	-0.003	(0.003)
			Corop24	-0.002	(0.001)
			Corop25	0.000	(0.001)
			Corop26	-0.002	(0.002)
			Corop27	0.000	(0.001)
			Corop28	0.000	(0.001)
			Corop29	-0.003	(0.003)
			Corop30	0.000	(0.002)
			Corop31	0.001	(0.001)
			Corop32	0.001	(0.001)
			Corop33	0.000	(0.002)
			Corop34	0.002	(0.002)
			Corop35	0.002	(0.002)
			Corop36	0.004	(0.002)*
			Corop37	0.002	(0.001)
			Corop38	0.002	(0.001)
			Corop39	0.008	(0.003)***
N	137759		Corop40	0.001	(0.002)

Note: Standard errors are in parentheses. Significance levels: * 10 percent; ** 5 percent; *** 1 percent. Each entry in the table comes from a separate regression. See the note to Table 3 for the demographic controls included in the regression.

Table B2: Reduced-form results for the effect of DI reform on mortality 1996-2010, probability of being hospitalized by 1996-2005 and number of hospitalizations 1996-2005

	Males								Females							
	Mortality		N	Probability of being hospitalized between 1996 and year		Cumulative number of hospitalizations		N	Mortality		N	Probability of being hospitalized between 1996 and year		Cumulative number of hospitalizations		N
1996	<0.001	(<0.001)	91090	0.003	(0.004)	0.008	(0.011)	91087	<0.001	(<0.001)	46669	0.021	(0.007)***	0.055	(0.018)***	46667
1997	<0.001	(0.001)	91090	0.005	(0.005)	0.010	(0.018)	90392	-0.001	(0.001)	46669	0.024	(0.008)***	0.093	(0.029)***	46412
1998	-0.003	(0.002)*	91090	0.005	(0.006)	0.002	(0.025)	89619	-0.001	(0.002)	46669	0.023	(0.009)**	0.127	(0.040)***	46134
1999	-0.003	(0.002)	91090	0.003	(0.006)	-0.002	(0.032)	88767	<0.001	(0.003)	46669	0.013	(0.010)	0.139	(0.052)***	45819
2000	-0.004	(0.003)	91090	0.004	(0.007)	-0.003	(0.038)	87906	<0.001	(0.003)	46669	0.017	(0.010)*	0.189	(0.062)***	45486
2001	-0.005	-0.003)	91090	0.003	(0.007)	-0.006	(0.044)	87012	<0.001	(0.004)	46669	0.017	(0.010)*	0.222	(0.069)***	45142
2002	-0.005	(0.003)	91090	0.002	(0.007)	-0.009	(0.052)	86043	0.002	(0.004)	46669	0.018	(0.010)*	0.224	(0.081)***	44787
2003	-0.006	(0.003)*	91090	0.003	(0.007)	-0.028	(0.060)	85034	0.001	(0.004)	46669	0.024	(0.010)**	0.242	(0.092)***	44443
2004	-0.005	(0.004)	91090	0.001	(0.007)	-0.046	(0.070)	83959	0.005	(0.005)	46669	0.023	(0.010)**	0.247	(0.108)**	44077
2005	-0.003	(0.004)	91090	0.002	(0.007)	-0.042	(0.080)	82921	0.004	(0.005)	46669	0.022	(0.010)**	0.247	(0.121)**	43687
2006	-0.003	(0.004)	91090						0.005	(0.005)	46669					
2007	-0.006	(0.004)	91090						0.004	(0.005)	46669					
2008	-0.008	(0.005)	91090						0.006	(0.006)	46669					
2009	-0.010	(0.005)**	91090						0.012	(0.006)**	46669					
2010	-0.012	(0.005)**	91090						0.014	(0.006)**	46669					

Note: Standard errors are in parentheses. Significance levels: * 10 percent; ** 5 percent; *** 1 percent. Each entry in the table comes from a separate regression. See the note to Table 3 for the demographic controls included in the regression.

Table B3: IV results for probability of being hospitalized by cause until 2005

	Females	
	Mean prevalence	Effect of reform per 1000 €/year decrease in amount of original DI
Circulatory	0.116	0.012 (0.012)
Mental	0.028	0.019 (0.007)*
Musculoskeletal	0.204	0.025 (0.016)
Neoplasm	0.097	0.012 (0.012)
Nutritional	0.030	0.003 (0.007)
Nervous	0.064	0.023 (0.010)**
Sense	0.074	0.015 (0.010)
Respiratory	0.051	0.013 (0.009)
Digestive	0.118	0.023 (0.013)*
Genitourinary	0.128	0.007 (0.013)
Symptoms	0.131	0.016 (0.013)
Injury	0.089	0.020 (0.011)*
Blood	0.008	0.003 (0.004)
Dermatological	0.025	0.003 (0.006)
Congenital	0.005	0.003 (0.003)
N	43687	43687

Note: Standard errors are in parentheses. Significance levels: * 10 percent; ** 5 percent; *** 1 percent. Each entry in the table comes from a separate regression. In the IV regressions the variable that is instrumented is the amount of DI, so the coefficients can be interpreted as effect size per €1000/year decrease in DI. The instrument itself is the treatment dummy (age less than 45 as of 8/1/93). See the note to Table 3 for the demographic controls included in the regression.