The Effect of Cutting Disability Insurance Benefits on Labor Supply in Households

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Abstract

Moral hazard is inherent in the Disability insurance (DI), yet difficult to quantify, especially in lack of counterfactuals. I use exogenous variation created by the abolition of an accompanying spouse pension for a married DI beneficiary. Unlike the previous literature the focus is on existing beneficiaries rather than on the inflow into DI and on effects of a benefit cut rather than an increase. The richness of the dataset allows me to look at the behavioral response on labor market participation from the spouse and not only from the beneficiary. I estimate the effect using a difference-in-differences methodology comparing beneficiaries who started to draw DI benefits just prior to the revision to beneficiaries who started to draw DI benefits just after the revision. I find considerable employment effects for the beneficiary. These effects are robust to a number of sensitivity checks. The effects for the spouse are also positive but not statistically significantly different from zero.

Keywords: Difference-in-differences, disability insurance, labor supply, policy reform

JEL classification: H55, J21

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1. Introduction

The purpose of disability insurance (DI) is to guarantee individuals a certain standard of living if their working capacity is limited by a long lasting health-related problem. Yet, asymmetric information between the insurer and the claimant as well as the impossibility to completely dispel it leads to moral hazard. For the insurer it is not perfectly possible to assess whether a claimant is truly disabled. The DI thus distorts work incentives and people with a large disutility of work may select into the DI. Variation in DI eligibility or in benefit generosity should therefore lead to variation in labor supply (for a theoretical model, see e.g. Halpern and Hausman, 1986). Since policy reforms in this sense often affect the general population (i.e. everyone faces the new rules), their effect is generally difficult to estimate in lack of suitable counterfactuals.

A range of studies have used policies or policy reforms which applied to only a subset of the population to estimate the effect of (less) tighter eligibility criteria or of a reduction (increase) in benefit generosity on labor supply (see e.g. Gruber, 2000; Staubli, 2011). Unlike most of these studies, I will not look on the impact of a reform on the inflow into DI but on the existing beneficiaries. This paper exploits exogenous variation by a policy reform of the Swiss DI that affected only married individuals. Prior to the reform, married DI beneficiaries had the possibility to additionally request a pension for their spouse. Starting in 2004, new accompanying benefits for spouses were no longer granted. In 2008, all existing benefits for spouses were abolished. Another contribution of this study is to analyze the behavioral response not only of the beneficiary but also of other members of the household. Many decisions in a household - especially on labor force participation - are taken in consideration of all household members and are dependent on total income of the household. I apply a difference-in-differences methodology comparing beneficiaries who started to draw DI benefits just prior to the revision to beneficiaries who started to draw DI benefits just after the revision. I find considerable employment effects for the beneficiary on the extensive as well as on the intensive margin, which are robust to a number of sensitivity checks. The effects for the spouse are also positive but not statistically significantly different from zero.

The paper proceeds as follows. The next section gives information on the background, including a literature review and the institutional setting of social insurances as well as of the policy reform in Switzerland. Section 3 describes the data. In section 4 the identification strategy is outlined. Section 5 presents the results, which are further discussed in section 6. Section 7 concludes.

2. Background

2.1. Literature review

The literature review should give a broad overview of studies investigating the work disincentive effect of the DI and identify the gap which this study aims to close. The work disincentive effect of the DI has been most extensively studied in the United States. Bound (1989) was the first to argue that when using cross-sectional variation in potential DI benefits relative to previous earnings, the estimated elasticity of labor force participation with respect to benefit generosity would be inflated. The reason is that potential DI benefits are likely to be endogenous due to their relation to past earnings. Bound uses rejected disability applicants instead of non-recipients as a group to construct the counterfactual of DI recipients. As this group may still be different in many characteristics from DI recipients, he interprets the results as an upper bound for the behavior of DI recipients if those had not received DI benefits. This approach is still very popular as numerous recently published articles demonstrate (see e.g. von Wachter *et al.* (2011), Giertz and Kubik (2011), and Singleton (2012)).

Another way to quantify moral hazard in the DI is to rely on exogenous variation created by quasi-experiments in form of policies or policy reforms which apply to only a subset of the population. Some researchers estimated the effect of tighter or less tighter eligibility criteria for DI benefits or of a reduction or increase in benefit generosity on labor supply in different countries (Staubli (2011) for Austria, Campolieti (2004) and Gruber (2000) for Canada, Karlström *et al.* (2008) for Sweden, and Chen and van der Klaauw (2008) and Duggan *et al.* (2010) for the United States). Other researchers used a change or regional variation in screening stringency to evaluate the effect on labor supply (Autor and Duggan, 2003; Gruber and Kubik, 1997; Mitra, 2009). Most of them are able to quantify considerable work disincentive effects in the DI.

While all these studies look at the effect on the inflow into the DI or on employment at the time of application, studies about the effect on existing beneficiaries are rare. Outflow from DI is generally low across all OECD countries; only around 1-2% of all beneficiaries leave the DI annually for reasons other than death or retirement (OECD, 2010). Two reasons have been identified for the low outflow: (i) There may be limited access to vocational rehabilitation and employment integration measures. A couple of countries have implemented special rehabilitation and integration measures targeting DI beneficiaries. The evaluation of these projects in the US and the UK, however, delivered disappointing results indicating low take-ups and no or only small effects on outflow (Adam *et al.*, 2010; Clayton *et al.*, 2011; Kornfeld and Rupp, 2000; Thornton *et al.*, 2007). (ii) Due to means testing, many DI systems may generate considerable lock-in effects. Expanding work efforts reduces benefit levels and the implicit tax rate on labor supply can be quite a substantial disincentive for return to work. Various policies have therefore been introduced to encourage beneficiaries to return to work by reducing this implicit tax rate. In the US, for example, DI beneficiaries are also covered by health insurance. Thus, they face a tradeoff between work and combined cash as well as health coverage benefits. States were given the authority to expand health insurance coverage to include persons with disabilities at higher income levels. Yet, as Gettens (2009) shows, the effectiveness of this expansion on employment and DI benefit participation is small.[1] Campolieti and Riddell (2012) demonstrate positive effects of the introduction of an earnings disregard in Canada; increasing the propensity to work for men by 5.1 to 5.7 percentage points. The effect for women is even larger (7.9-9.5 percentage points) but not as precisely estimated. However, there is no effect on DI in- or outflow. They also evaluate the introduction of automatic reinstatement provisions whereby former recipients could remain eligible for DI when taking up work. This new measure had no effect on any of these outcomes.

Autor and Duggan (2007) provide an explanation for the low effects on existing beneficiaries: Beneficiaries may prefer leisure over labor even if work is not implicitly taxed. They exploit a change in the DI program for veterans, where veterans who served in the Vietnam War could increase DI benefits due to the inclusion of diabetes on the list of conditions. Because these benefits are not work-contingent or means tested, the estimated decrease in labor force participation is due to a pure income effect. Marie and Vall Castello (2012) are able to replicate this finding in a Spanish setting.

When more drastic changes in the form of reductions of benefits for existing beneficiaries are analyzed, the effect is different. Empirical evidence is similarly scarce and has focused on the removal of drug addiction as a disabling condition in the US in 1996, which terminated

[1] The UK "Pathways to Work" program also included a financial incentive to return to work in addition to job assistance services. Exploiting regional variation the results show that the program has accelerated the outflow from DI benefits, but only for those individuals who would have left benefit roles in less than a year in any case (Adam *et al.*, 2010). It is unclear however, which aspect of the program has contributed to this decline.

benefits of approximately 100’000 individuals. Most recently, Moore (2011) estimates considerable employment increases by 20-30 percentage points for this population.

Additional (self-)insurance against work-limiting disability can be provided by the spouse through the added worker effect. As a result of a negative income shock the spouse might increase her labor supply. While the added work effect has mainly been studied in the unemployment literature (see e.g. Cullen and Gruber, 2000), the relationship between DI and spousal labor supply has only recently been analyzed. Spousal labor force participation might be higher in absence of a DI. Using longitudinal data, Chen (2012) shows that spousal labor force participation decreases in the long term as soon as their husbands are granted DI benefits. Using quasi-experimental variation, Duggan *et al.* (2010) also find a reduction in spousal labor supply if relaxed eligibility criteria induce an increase in the propensity of their husbands to enroll into DI.

The contribution of this study is the combination of two topics which have not yet gained much empirical attention. First, I look at the effect from a partial reduction of DI benefits on existing beneficiaries. Second, I observe the labor supply response to this reduction not only from the beneficiary but also from the spouse. The evaluation of a reduction in benefits allows me to check whether the spousal effect is symmetric, while the previous literature has only analyzed increases in benefits.

2.2. Institutional setting

The Swiss Disability Insurance as part of the Social Insurance

This section will give an overview of the Swiss Disability Insurance (DI) which is a major part of the Swiss Social Insurance system. It is important to be acquainted with the particular features of the system as some may also be affected by the revision, which is described in the next section, through spillover effects.

The Swiss DI program is similar to the social security disability insurance (SSDI) program in the United States.[2] Both are mainly financed through payroll taxes and pay benefits not only to the disabled worker, but also to dependent children and in some cases also to the spouse. In Switzerland, there are three conditions for eligibility to benefits from the DI program: health impairment, working incapacity and a causal relationship between the two

[2] The SSDI program has received most of the attention in the economic literature. For a short description of it, see e.g. Bound and Burkhauser (1999).

(BSV, 2003). The working incapacity has to last for at least a year. Unlike in the US, the Swiss program differs between ordinary and extraordinary benefits. In order to be awarded with an ordinary pension, the applicant must have worked at least one year (three years since 2008) in sustainable employment. Extraordinary pensions are granted mainly for individuals with a congenital condition who were never able to work and to contribute to the pension system. Another important difference to most other DI programs is the method the amounts of benefits are calculated, which is dependent on the degree of disability and leads to a partial benefit system. The degree of disability is calculated by comparing hypothetical and reasonable earnings without a disability with those with a disability in any job. TABLE 1 illustrates the type of disability pension and minimum and maximum amounts of benefits with respect to the degree of disability. If the difference of the two earnings is smaller than 40%, the claimant does not receive any benefits. If it is bigger than 70%, he receives a full pension. The minimum and maximum amounts depend on how many years the person has been insured and on her past earnings. Between a degree of disability of 40% and 70% the type of pension is leveled in steps of ten percentage points.

Similar to the supplemental security income (SSI) in the US, there is also a means-tested supplemental income program in Switzerland. If benefits from DI and other income fail to cover basic living costs, a Swiss resident can apply for *Ergänzungsleistungen* (Supplementary Benefits). Unlike in the SSI, eligibility is conditional on DI benefit.

Together with the Old Age and Survivors Insurance and the Unemployment Insurance, the DI and Supplementary Benefits form the first pillar of the Swiss social insurance. The second pillar is constituted by occupational pension plans and accident insurance. Every employed worker is required to individually contribute to an occupational pension plan through payroll taxes. This occupational insurance account is filled over the workers lifetime and managed by a private insurance company. In case of disability onset or retirement, a monthly benefit is granted, whose amount depends on the stock of funds as well as on the degree of disability assessed by the DI. In addition, a similar system exists for accident insurance. Those who are not employed pay their premiums through health insurance. The goal of policy makers is to achieve a replacement rate of 60% with benefits from the first and second pillar. The third pillar in Switzerland includes voluntary private insurance plans, whose benefits are granted in the same way as in the second pillar.

TABLE 1: Degree of disability and amounts of benefits

degree of disability	type of pension	amount of monthly pension in CHF	
		minimum	maximum
<40%	no pension		
40-49%	quarter pension	277	553
50-59%	half pension	553	1105
60-69%	three quarter pension	829	1658
>70%	full pension	1105	2210

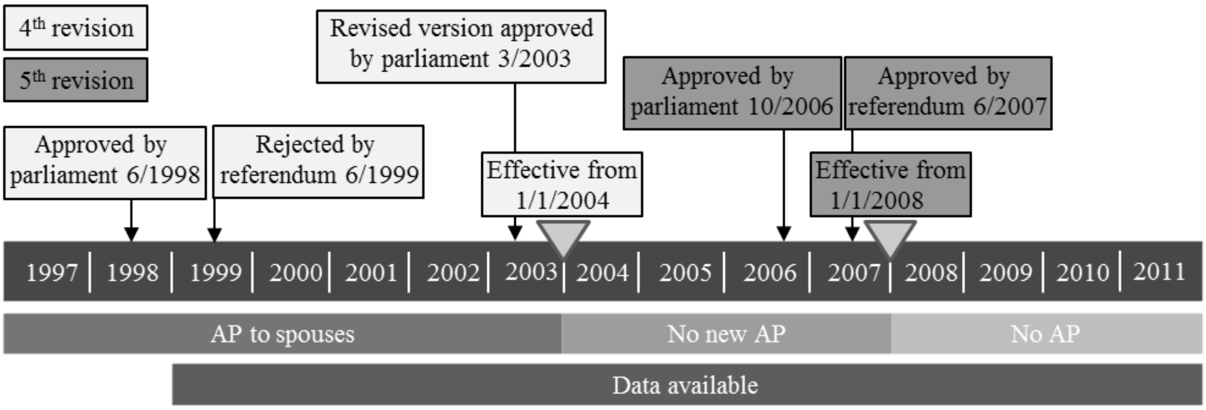
Notes: Amounts reported are effective from January 2007 and are gradually adjusted for inflation. 1CHF = 1.61€ in January 2007.
Source: BSV (2008)

The additional pension for spouses and its abolition

Up to the 4th revision of the DI act, the DI could award an additional pension (AP) to a spouse if the person eligible for an ordinary DI pension was employed prior to the onset of work incapacity and if the spouse is not his-/herself eligible for a DI benefit. The amount of the AP was set to be an additional 30% of the DI benefit. Due to the disproportionally rising number of DI beneficiaries in the nineties, which led to an imbalance in the system, the federal government decided in 1997 to cut mainly on the expenditure side. The policy reform, which was approved by the parliament in 1998, included the abolition of the quarter pension and no new grants of APs to spouses.[3] Yet, lobbyists were able to request a referendum and the Swiss people disapproved the reform in 1999 in a vote with an unusual high no-share of 70%. There was wide consensus that the failure of the reform was mainly due to the planned abolition of the quarter pension level. Following the referendum, the federal government adapted the reform accordingly and the parliament passed the law in March 2003. Moreover, the parliament added the introduction of a three-quarter pension. Since no referendum was requested, the reform became effective starting January 1st, 2004. Soon after, the 5th revision was drafted as the imbalance in the system had continued to increase. The parliament discussed the law in 2005-2006, finally passing it in October 2006. Again, a referendum was requested but the Swiss people approved the reform in June 2007 with a yes-share of 59%. The reform was implemented on January 1st, 2008. It contained the abolition of all still existing APs for spouses. As a summary, FIGURE 1 illustrates the timeline of the abolition of the AP.

[3] Another incentive to abolish the additional pension for spouses was the fact that it was the only benefit left that was conditional on marital status.

FIGURE 1: Timeline of the abolition of the additional pension (AP)



Couples affected by the abolition of the AP can close the income gap caused by the abolition by individually or jointly increasing their labor force participation (LFP) on the intensive or extensive margin. If the effect on LFP is strong enough, this may lead to a further reduction of DI benefits (due to the partial DI system) or even an increase in DI outflow. However, the abolition might also cause spillover effects into other parts of the social insurance system. These possible effects are discussed in section 6.

3. Data

I use the SESAM dataset (*Syntheseerhebung soziale Sicherheit und Arbeitsmarkt*) to analyze the effect of the revision. The dataset is a combination of SAKE (Swiss Labor Force Survey) linked with information from different social insurance registers of the first pillar, i.e. old age, survivors', disability, and unemployment insurance. Up to 2009, SAKE was a rotating household panel with a yearly sample size of approximately 45'000 persons representing the permanent Swiss residents aged 15 and older. Households were randomly sampled from the telephone number register. Each household stayed in the sample for five consecutive years. So every year, a fifth of the panel was replaced by a new sample. In the first interview, a randomly chosen target person was interviewed in each household. In the following years, the same person was re-interviewed. The interview was held in the second quarter of each year. The most detailed information is available for this target person, while some basic information is provided for all other household members. Administrative register data is provided for the target person only. Information on other household members is only provided if it directly relates to the target person (as it is the case for the AP). Beginning in 2010, the methodology for SAKE has been revised. Households now stay in the sample for five consecutive quarters and are interviewed four times. Yet, the SAKE continues to be linked to administrative data only once in a year, so the available dataset includes one observation per household and year. Data are available for the years 1999 to 2011.

In the analysis I exclude individuals older than 59 as their labor supply behavior might be affected by the additional option to enter (early) retirement. In addition, I cannot use the panel feature of the data set to a full extent for various reasons. First, because effects of the revision might be long-term or could take time to substantiate, they might not be detectable when people stay in the sample for a maximum of only five years. Second, it is useful for my identification strategy to have observations from years before the policy change as outlined below. Third, the sample size would be severely reduced threatening the power of the tests (see section 5). Therefore I use the pooled cross-section data over all available years.

4. Identification Strategy

The empirical strategy exploits the fact that not every DI beneficiary was affected by the policy change of the AP abolition. I can therefore use the unaffected DI beneficiaries to control for the counterfactual situation in a difference-in-differences (DiD) approach. The approach relies heavily on the assumption that both the treatment and control group follow a similar trend so that in the absence of the policy reform, the two groups would evolve in the same way (common trend assumption). It is therefore critical to define the control group as similar as possible to the treatment group. This is not an easy task, especially in the field of disability research.[4]

There are two stages for the abolition of the additional pension to spouses (AP) which can be exploited as exogenous variation in benefit generosity. However, I restrict my analysis to the 5th revision when on January 1st 2008 all remaining AP beneficiaries lost their benefits resulting in a reduction of 23% of DI benefits. The main reason why I do not analyze the effect of the 4th revision is that with the way I define treatment and control groups I would have to compare married with non-married beneficiaries. For the 5th revision I can rely on married beneficiaries only because I observe both groups before and after the revision. Using only married beneficiaries the crucial assumption in the DiD approach of a common trend is

[4] Gupta and Larsen (2008; 2010) evaluate the introduction of the Danish Flexjob scheme using a DiD approach. To be eligible for Flexjob, individuals must have a long-term disability (at least three years) and a reduction in working capacity. In the earlier draft (2008) the general population was used as the control group, finding only modest employment effects. In the later version (2010), when long-term disabled without reduction in working capacity and short-term disabled individuals (based on self-classification) were used as control groups, the authors find a substantial positive employment effect.

much more plausible. The other major concern in the 4th revision is the possibility of changes in behavior in anticipation of the policy change. The process from the announcement of the law until its implementation took almost seven years (see FIGURE 1) leaving much time for individuals to change their behavior before the AP was finally no longer awarded for new DI beneficiaries.

The treatment group is defined to include all married individuals who started to receive their ordinary DI pension between 2002 and 2003.[5] These individuals were eligible for an AP before 2008 but no longer after January 1st 2008. The members of the control group are married individuals who started to receive their ordinary DI benefit between 2004 and 2005, so they have never been eligible for an AP. I expect members of the treatment group (DI beneficiaries and/or their spouse) to increase their labor force participation LFP (on the extensive or intensive margin or on both) after the revision was implemented relative to members of the control group. Analytically I estimate regressions of the following type:

$$y_{it} = \alpha + \beta Treat_{it} + \delta(Treat_{it} \times Post_t) + \mu_t + X'_{it}\theta + \varepsilon_{it}, \qquad (1)$$

where y_{it} is the outcome variable of interest for person *i* at time *t*. *Treat* is an indicator for the treatment group (1 if start was between 2002 and 2003, 0 if start was between 2004 and 2005). *Post* is an indicator for the year of observation (1 if between 2008 and 2011; 0 if between 2006 and 2007). μ is a vector for year fixed effects, while X_{it} is a set of covariates, which include age, gender, foreigner status, educational attainment, number of children, regional dummies, regional unemployment rate, and type of disability. The coefficient of interest is δ which measures the effect of the abolition of APs on beneficiaries relative to beneficiaries who had never drawn an AP using variation over time when the persons started to draw an ordinary DI benefit. Standard errors are clustered on the individual level to account for correlation within observations across the years.

By using time windows of two years before and after the revision to define the size of the groups, I believe the vital common trend assumption to be fulfilled. I need to exclude any DI beneficiaries who have been on the DI rolls for a longer time because their employability may be substantially different from DI beneficiaries with less time on the DI rolls. The fulfillment of this assumption can gain more credibility if I run placebo tests at a random point in time other than at the time of the implementation of the reform to check whether a similar

[5] The start year of DI benefit is calculated using the age of a person when she first received DI benefits or her latest change in DI status.

difference in LFP exists between beneficiaries who are for a shorter time on the DI rolls compared to those who are on the DI rolls for a slightly longer period. If so, the difference between the years 2006-2007 and 2008-2011 might not be caused by the revision but is reflecting a general trend.

Another critical assumption when using a DiD strategy is the absence of any anticipating behavioral responses prior to the implementation of the policy reform. In contrast to the 4th revision the process from the announcement of the law until its implementation took much less time for the 5th revision (see FIGURE 1). The way the treatment and control group are defined, anticipating behavior with respect to bringing forward a DI application (in order to still benefit from the AP) would have had to occur at least four years before the abolition on January 1st 2008 (i.e. prior to January 1st 2004). In addition, since there is a waiting period of one year when applying for DI benefits, anticipating effects might even be less likely.[6] Still, I can partly check whether this anticipation is relevant by comparing the socio-economic characteristics of my sample. If beneficiaries significantly differ in them between before and after the revision, it is a strong sign for an anticipating reaction of some part of the population. In addition, I can run placebo tests prior to the revision which would reveal anticipating behavior if they turn out to be significant.

The remaining identifying assumptions for a DiD strategy are the following: (i) If covariates are included in the identification to increase precision, these covariates must be exogenous, i.e. unrelated to the policy reform. The covariates I use are exogenous since most are determined prior to the reform. (ii) A more fundamental assumption applying to all typical microeconometric evaluation strategies is the stable unit treatment value assumption (SUTVA). The policy reform must not have any general equilibrium effect in the sense that the employment probability of an individual should be independent of the treatment status of other individuals. SUTVA is likely to hold as the policy reform affected only a small group of persons relative to the general population. Although jobs for people with a disability might not be abundant, the persons in the two groups are not geographically clustered, so they are likely to not compete on the same local labor market.

It is interesting to see whether an effect of the reform is rather short-term or long-term. The effect might also need time to emerge. To investigate on the impact over time, I replace the

[6] There should not be any anticipating effects with respect to a change in civil status, either. The AP could still be awarded if the marriage was registered after the ordinary DI benefit had been awarded. Therefore, there was no incentive to bring a marriage forward. Similarly, an AP was still paid out after a divorce if the spouse (1) did not remarry or (2) was not eligible for disability, old age, or widow benefits or (3) had dependent children.

rather static ($Treat_{it} \times Post_t$) interaction term with a full set of treatment times year interaction terms:

$$y_{it} = \alpha + \beta Treat_{it} + \sum_{m=2007}^{2011} \delta_m (s_m \times Treat_{it}) + \mu_t + X'_{it}\theta + \varepsilon_{it}, \qquad (2)$$

where s_m is a dummy equal to 1 in year m and 0 otherwise. Another advantage of this approach is that a significant effect (δ) in the year 2007 (i.e. prior to the reform) would hint to an anticipation effect so that beneficiaries would alter the labor supply prior to the abolition of the AP.

I can use non-married beneficiaries as an additional control group to set up a Difference-in-Difference-in-Differences (DiDiD) estimator. In this way, I can level out changes in labor supply for an additional observationally equivalent group which is not affected by the cancellation of the AP. The price to pay for this additional control is that changes in spousal labor supply can no longer be assessed because there is no information about a partner for non-married beneficiaries. The regression framework in the DiDiD is

$$y_{it} = \alpha + \beta Treat_{it} + \gamma Married_{it} + \delta_1(Treat_{it} \times Married_{it}) + \delta_2(Treat_{it} \times Post_t) + \delta_3(Married_{it} \times Post_t) + \lambda(Treat_{it} \times Married_{it} \times Post_t) + \mu_t + X'_{it}\theta + \varepsilon_{it} \qquad (3)$$

where *Married* is equal to 1 if the person is married and 0 if the person is no longer married.[7] The coefficient of interest is now λ. Similarly to equation (2) I can replace the ($Treat_{it} \times Married_{it} \times Post_t$) interaction term with a full set of treatment times civil status times year interaction terms to explore the dynamics of the effect.

5. Results

TABLE 2 reports summary statistics of the sample used for the evaluation of the 5th revision. Each variable's mean is grouped by treatment status and compared before and after January 1st 2008, the date of the policy change. Panel A shows the effect of the revision on the DI. The average amount of the AP which was cancelled is 250 CHF. Interestingly, the amount of the ordinary DI benefit did not decrease significantly in the treatment group but instead if anything rather increased slightly. The total amount of DI benefits a household receives significantly decreased for the treatment group, while it increased somewhat for the control group. Note that this number also includes any DI benefits for dependent children. The

[7] No-longer-married beneficiaries include divorced, separated or widowed individuals. Instead, I could also use never married beneficiaries. The comparison of these two control groups is the focus of another paper. (Kauer, 2013)

outflow from DI is defined as receiving DI benefits in the previous year but not in the year of observation. Therefore, I can include only beneficiaries who were observed over at least two years. The outflow increased marginally in the treatment group, while it decreased for the control group. However, these changes are not significantly different from zero.

The abolition of the AP might induce spillover effects into other social insurances which might have an effect on the decision whether to change the labor supply and thus could affect the results. If the couple passes a means test, they can request supplementary benefits (*Ergänzungsleistungen*). Yet, as Panel B of TABLE 2 illustrates both the share receiving Supplementary Benefits and the amount received decreased for the treatment group, while it increased in the control group. Some DI beneficiaries might additionally qualify for helplessness allowances (HA) if they need assistance to perform activities for daily living. Again both the share receiving and the amount of HA decreased, even in both groups, yet not significantly. I can also check whether the spouse may select into early retirement as a consequence of losing the AP. If anything, both the share of spouses receiving old age pension and the amount received only slightly increased in the treatment group, while the increase is statistically significant in the control group. In summary, spillover effects into other social insurances for which data are available do not seem to be a significant factor. Still, there could be more spillover effects into other insurances for which I do not have data. I will discuss them in section 6.

Panel C of TABLE 2 includes various variables measuring labor force participation and earnings. All work related variables are higher for treated beneficiaries after the abolition of the AP. Yet, these results are not or only barely significantly different from zero. For married beneficiaries who are used to control for the counterfactual because they were never eligible for an AP, all outcome variables are lower in the post-policy period. Again, the differences are statistically non-distinguishable from zero.

As mentioned in the previous section, the 4th revision could have induced people to bring forward their DI application to still benefit from the AP before its abolition. Similarly it could deter persons from applying for disability benefits due to the lower amount of benefits granted after the abolition. In order to control for this anticipation effect before the revision and for non-endogenous non-entry after the revision, TABLE 3 illustrates the socio-economic background characteristics of the different groups. The two groups do not differ significantly

TABLE 2: Outcome means by marital status for the analysis of the 5th revision

	Treated (Start of DI 02-03)		Control (Start of DI 04-05)	
	Before: 2006-07 (1)	After: 2008-11 (2)	Before: 2006-07 (3)	After: 2008-11 (4)
A. Disability benefits				
Spouse: Amount AP	249.2	0***	0	0
Amount of DI benefit	1213.2	1293.9	1182.4	1180.8
Household: Amount DI pension	2158.3	1944.1**	1636.9	1722.5
Outflow from DI	0.013	0.020	0.043	0.029
B. Other social insurances				
Share receiving Supplementary Benefits (SB)	0.106	0.077	0.061	0.110^
Amount SB	2307.7	1646.5	1261.5	2220.6
Share receiving Helplessness Allowances (HA)	0.047	0.035	0.073	0.052
Amount HA	39.4	25.3	61.7	48.7
Spouse: Share receiving old age pension	0.028	0.037	0.006	0.035^^
Spouse: Amount old age pension	55.1	58.9	11.2	59.5^
C. Work				
Being in the labor force	0.268	0.332*	0.391	0.340
Worked for pay last week	0.193	0.257*	0.302	0.268
Being employed	0.232	0.289	0.352	0.308
Looked for work last week	0.035	0.042	0.045	0.032
Weekly hours worked	6.602	6.996	9.688	7.845
Weekly hours worked (in categories)	1.358	1.444	2.050	1.573^
Spouse: Being employed	0.615	0.674	0.632	0.637
Spouse: Weekly hours worked (in categories)	4.102	4.514	4.363	4.259
Net earnings last year	7815.5	9328.8	14115.0	11550.2
Number of observations	254	401	179	347

Notes: Start of DI benefit receipt is calculated using the age of a person when she first received DI benefits or her latest change in DI status. ***,**,*: statistically different from column (1) at 1, 5 and 10 percent, respectively. ^^,^: statistically different from column (3) at 5 and 10 percent, respectively. *Source*: Own calculations based on SESAM data (waves 2006-2011).

TABLE 3: Socio-economic characteristics by marital status for the analysis

	Treated (Start of DI 02-03)		Control (Start of DI 04-05)	
	Before: 2006-07 (1)	After: 2008-11 (2)	Before: 2006-07 (3)	After: 2008-11 (4)
Age	49.205	49.860	48.872	48.842
Female	0.441	0.459	0.419	0.444
Foreigner	0.665	0.611	0.620	0.530^^
Number of children	1.315	1.137*	1.034	1.121
Educational attainment				
Lower secondary or lower	0.374	0.421	0.447	0.424
Higher secondary	0.492	0.476	0.441	0.458
Tertiary	0.134	0.102	0.112	0.118
Regional dummies				
Leman	0.181	0.170	0.123	0.156
Mittelland	0.130	0.145	0.173	0.156
Northwest	0.150	0.185	0.112	0.179^^
Zurich	0.122	0.157	0.134	0.130
East	0.181	0.150	0.201	0.190
Central	0.083	0.095	0.112	0.086
Ticino	0.154	0.100**	0.145	0.104
Regional unemployment rate in %	3.252	3.319	3.037	3.281^^
Type of disability				
Mental illness	0.362	0.374	0.358	0.432^
Accident	0.043	0.087**	0.034	0.066
Number of observations	254	401	179	347

Notes: Year of start of DI benefit receipt is calculated using the age of a person when she first received DI benefits or her latest change in DI status. **,*: statistically different from column 1 at 5 and 10 percent, respectively. ^^,^: statistically different from column 3 at 5 and 10 percent, respectively. *Source*: See Table 2.

between the two periods. For the rare cases that there is a significant change in one group, the change goes in the same direction (but not significantly) for the other group.[8] It is therefore rather unlikely that a particular group of people decided to speed up with the application before the revision or not to apply for DI benefits after the revision due to the change in benefit generosity.

[8] The only exception is the number of children. But there the significance is only borderline.

TABLE 4: Impact on labor force participation

Dependent variable	**Being in the labor force**		**Spouse: Being employed**	
	(1)	(2)	(3)	(4)
Treat × Post	0.114**	0.120**	0.062	0.060
	(0.057)	(0.054)	(0.063)	(0.062)
Treat	-0.123***	-0.115***	-0.017	-0.004
	(0.046)	(0.044)	(0.057)	(0.055)
Covariates	No	Yes	No	Yes
R^2	0.007	0.136	0.008	0.093
Mean	0.330	0.330	0.644	0.644
N	1182	1182	1137	1137

Notes: Standard errors in parentheses are clustered at the individual level. ***,**: statistically different at 1 and 5 percent level, respectively. *Source*: See Table 2.

I now turn to the regression framework outlined in section 4. Results of the OLS estimation of equation (1) are summarized in TABLE 4. The dependent variable in columns (1) and (2) is a dummy for being in the labor force (i.e. employed or unemployed). The effect of the policy change is positive and significant. It is also fairly robust to including covariates (see column (1) vs. (2)), which have the expected sign but are not reported. The LFP increased for married beneficiaries who lost their AP compared to married beneficiaries who were never awarded an AP by 12 percentage points. When the employment status of the spouse is used as the outcome, the effect is also positive and about half in size but not precisely estimated.[9] Part of the lower increase might be explained by a high fraction of "treated" spouses (62%) already being employed before the revision. This is about twice the initial value of DI beneficiaries before the revision. TABLE A1 in the appendix shows the results when different outcome variables are used. The results are similar, yet slightly smaller in magnitude when other measures of the labor supply on the extensive margin of the DI beneficiary are used. The effects on the intensive margin are also positive for both the DI beneficiary and the spouse. It is however only statistically significant for the DI beneficiary and when measured in categories of five hours. The effects on looking for work and on outflow are also positive but not statistically significant. However, the increased labor supply did not reduce the degree of disability so that the amount of DI benefit of the beneficiary would have decreased. In fact, if anything the amount slightly increased.

[9] The variable "Being in the labor force" is not available for spouses.

A valid objection for these results is the reasoning that more recent married beneficiaries could always have a higher labor force participation compared to less recent married beneficiaries. The observed increase might therefore not be a one-time phenomenon and due to the policy change but regularly appearing. To check for this concern I run placebo regressions where I choose an artificial date of revision to define treatment and control groups. A significant effect of this artificial revision would cast doubt on the causality of the results. I do not use placebo cutoff dates which would result in groups that span over the year 1997 in order not to intervene with possible effects from an additional policy change to the AP. Prior to that year, only wives of DI beneficiaries were eligible for the AP. From 1997 onward, gender discrimination was removed so that husbands were also eligible. As a first artificial date of revision I choose January 1st 1999, so the artificial treatment group includes married individuals who started to draw DI benefits in the year 1997 or 1998, while the artificial control group includes married individuals who started to draw DI benefits in the year 1999 or 2000. As the upper left panel of TABLE A2 in the appendix shows, the effect of this placebo revision is not significant. It actually changes sign and is virtually zero when covariates are included. I now move the artificial date of revision stepwise forward by one year. The results hardly change when January 1st 2000 is used as the artificial date of revision. When I use the beginning of the two following years as artificial cutoffs, the effect increases in absolute terms. Since the effect is negative and never statistically significantly different from zero at all placebo cutoffs, the positive coefficient in TABLE 4 is very likely to reflect a causal effect.

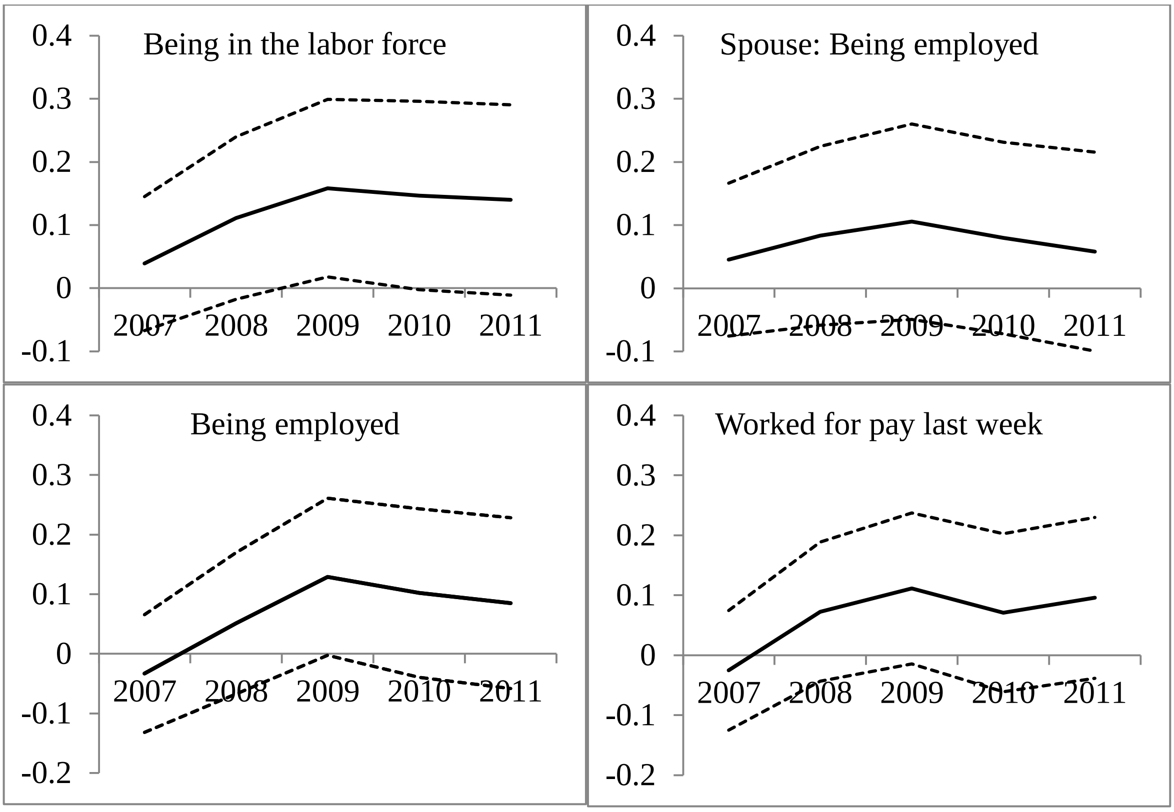
In the baseline setup I use time windows of two years to define the size of the groups. In this way, I compare persons who have been on the DI rolls for three to four years with persons who have been on the DI rolls for one to two years. In a sensitivity check, I change the window size to see whether the results are sensitive to this choice. TABLE A3 in the appendix summarizes the results when increasing the window size from two years to three and four years as well as decreasing it to one year. The results hardly change at all when the treatment (control) group is defined to include married individuals who started to draw DI benefits between 2001 (2004) and 2003 (2006). The effect is smaller and no longer significant when the window size is increased to four years. Further increasing the window size to five or more years is not only impossible because it would span over the date of the second policy change on January 1st 2008. It is also questionable to compare beneficiaries on the DI rolls for up to ten years (or more) with those on the DI rolls for only one year since their employment prospects might differ substantially. The last result with a window size of four years already may point to the fact that a person on the DI rolls for eight years might be quite different and probably should not be compared to one who entered the DI just the previous year. On the other hand, when the window size is decreased to one year, the effect is even smaller and its sign is reversed, yet not significantly different from zero.

The revision might have a different effect depending on the background characteristics of the beneficiaries. To check for effect heterogeneities I split the sample into two groups for six different background characteristic and estimate the treatment effect for them separately. TABLE A4 in the appendix reports only the coefficients of the treatment effect (δ) from these twelve regressions in the form of equation (1). The effect of the revision is stronger for beneficiaries who are 50 or older compared to their younger counterparts. This difference is almost statistically significant (p-value from a Chow test: 0.12). The other effect heterogeneities are smaller and even less precisely estimated, either. If anything, the treatment effect is stronger for men, foreigners, beneficiaries without any dependent children, beneficiaries with low education and with a disability other than a mental illness.

As outlined in the section 4, it is interesting to check for dynamics in the effect of the revision over the available years. FIGURE 2 plots the estimated coefficients of the interaction terms (solid line) in Equation (2) for four outcome variables. The 90-percent confidence interval is shown by the dotted lines. The pattern is similar in all four panels: The effect is close to zero in 2007 before the implementation of the revision. It increases considerably in the first year of implementation, while reaching its maximum in the second year (2009) and leveling off in later years. Unfortunately, the respective coefficients are only statistically significant at the 90%-level in one year in the upper left panel, when the effect on being in the labor force is analyzed.

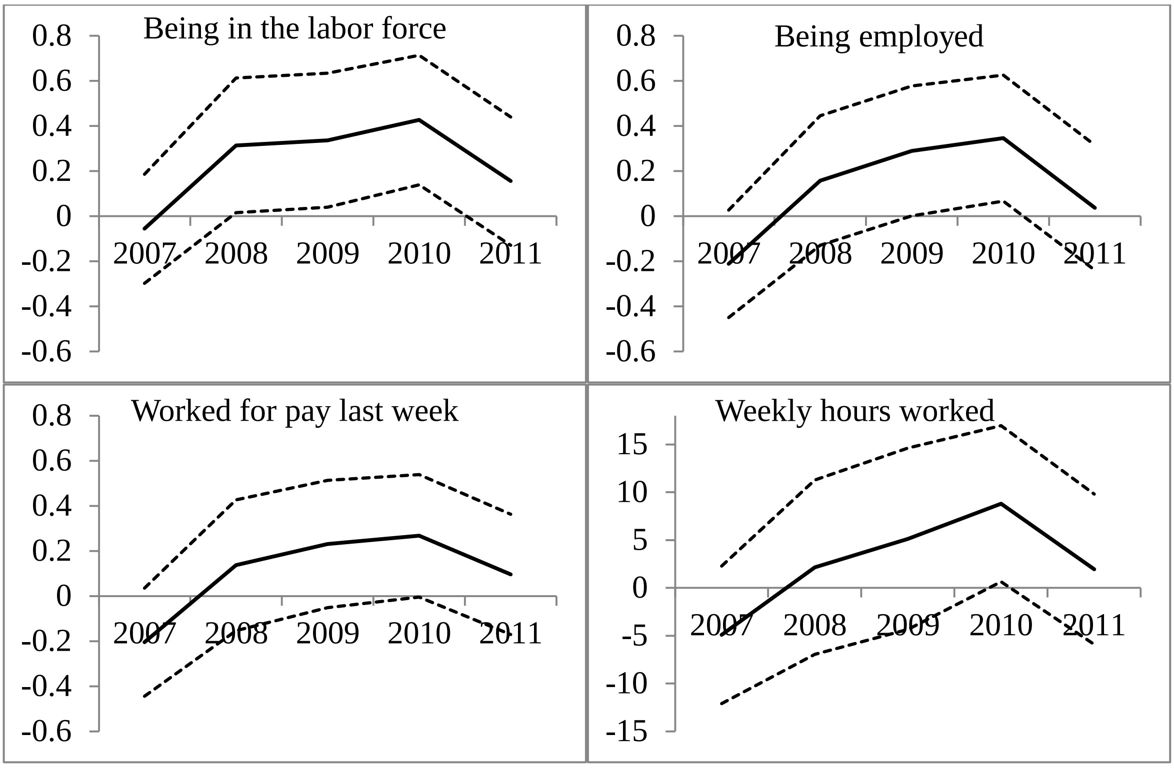
I now use no longer married beneficiaries as an additional control group. This group is similar to married beneficiaries but unaffected by the policy change. Including this group in a Difference-in-Difference-in-Differences (DiDiD) set up should cancel out changes in employment rates of the treatment group which are unrelated to the policy change. TABLE 5 reports the coefficients of the triple interaction effect in Equation (3) for various outcome measures. The cancellation of the AP has an even larger positive and significant effect on all three measures of labor supply on the extensive margin as well as on the two measures of labor supply on the intensive margin. There is no significant effect either on the propensity to look for work, on the amount of DI benefit the individual receives or on outflow. Note that

FIGURE 2: Dynamic impact on labor supply with DiD



Notes: Coefficients of the treatment × year interactions in Equation (2). The dotted lines represent the 90% confidence interval. *Source*: See Table 2.

FIGURE 3: Dynamic impact on labor supply with DiDiD



Notes: Coefficients of the treatment × married × year interactions in Equation (3). The dotted lines represent the 90% confidence interval. *Source*: See Table 2.

TABLE 5: DiDiD estimates for various outcomes

Dependent variable	**Treat × Married × Post**
Being in the labor force	0.313***
	(0.117)
Being employed	0.290**
	(0.114)
Worked for pay last week	0.272**
	(0.112)
Looking for work last week	0.028
	(0.042)
Weekly hours worked	6.406*
	(3.405)
Weekly hours worked in categories of 5h	1.519**
	(0.680)
Amount of DI benefit	245.74
	(160.04)
N	1638
Outflow	0.044
	(0.035)
N	1240

Notes: Standard errors in parentheses are clustered at the individual level. ***,**: statistically different at 1, and 5 percent level, respectively. *Source*: See Table 2.

when using this additional control group, I can no longer analyze the effect on spousal labor supply since no longer married beneficiaries have by definition no spouses. To assess the dynamics of the effect, Figure 3 plots the estimated coefficients of the yearly interaction terms, which substitute the single post interaction term in Equation (3). The pattern is very similar on a higher scale to Figure 2 with the exception that the maximum is now reached in 2010.

6. Discussion

This section discusses potential concerns which could bias the results I have presented. For each potential concern I present some evidence to strengthen the argument that these concerns do not cause serious bias.

Spillover effects of the revision into other social insurances have already been tested for which the SESAM dataset provides information, namely Supplementary Benefits, Helplessness Allowances, and retirement. The results show that if anything the inflow rather decreased than increased. Yet, detailed information on benefits from the second pillar of social insurance is missing in the SESAM dataset. If the DI beneficiary receives an additional benefit from an occupational or accidental insurance plan from the second pillar, the amount of this pension increases after the revision if it has been previously capped to prevent overcompensation. SESAM only provides information on total household income from all possible sources (including third pillar and interest payments). As a second-best option, I use the Swiss Household Panel, wherein information on benefits from an occupational insurance plan is included. Since this also a representative data set of the Swiss population, I assume as an arguably crude measure that the share of married DI beneficiaries below the age of 60, who additionally receive benefits from an occupational insurance plan, in this data set should be similar to the one in my sample. The analysis shows that about a third draw an additional pension from an occupational insurance plan. Although I am not able to completely dispel any spillover effects, if they exist, they would only bias my results downwards. The results can therefore be seen as a lower bound.

The 5th revision of the DI act included other changes to the DI. However, confounding effects from these other changes should not be a problem as its focus lied on (reducing) the inflow into DI (new beneficiaries) and not on the existing beneficiaries.

It could be argued that the results are only partially valid because I actually cannot observe whether a person is eligible for an AP. For that I would need to know whether the person eligible for an ordinary DI pension was employed prior to the onset of work incapacity and whether the spouse was not his-/herself eligible for a DI benefit at that date. Unfortunately, this information is not available in the data. Yet, prior to the abolition less than a quarter of all married DI beneficiaries in the data did not draw an AP. Since eligibility was assessed by the DI and not upon the initiative of the beneficiary, I assume the take-up rate of the AP to be very close to 100%.

Although I do not use the panel structure of the data to a full extent, attrition might be a potential concern if the attrition rate is different between the treatment and control group (so that attrition might be correlated with the treatment). Attrition is also problematic if it is non-random, i.e. attrition is correlated with the outcome variable. Identifying attrition is not trivial in this dataset since the structure of the panel was changed in 2010. As mentioned in section 3, people were sampled to stay in the panel for five consecutive years up to 2009. In 2010 the periodicity was changed so that people stayed in the panel for five consecutive quarters. Only part of the sample newly introduced in 2008 and 2009, respectively was selected to be followed-up in 2010. Therefore, some respondents dropped out of the data involuntarily and cannot be counted as attritors. Since I can observe which individuals were not selected, I can account for this involuntary attrition. I exclude the newly sampled observations from the last year (2011) because I cannot observe whether they would attrite in later years or not. Although the attrition rate is fairly high in the remaining sample (32.5%), there is no difference between the treatment and control group (32.7% vs. 32.3%, p-value of t-test: 0.91). Following Fitzgerald *et al.* (1998) I run two tests whether attrition is random. In the first test I estimate the probability of being an attritor explained by the baseline values of the socio-economic background variables. These probit regressions are run separately for every outcome variable used in TABLE 4 and TABLE A1, while every outcome variable is included as an explanatory variable. In all eleven probit regressions the pseudo R-squared is never greater than eight percent.[10] As more than 92% of the variation in the attrition cannot be explained by these explanatory variables, it is quite safe to conclude from this first test that attrition is random. As a second test I use the methodology by Becketti *et al.* (1988), commonly known as the BGLW test. An outcome variable is regressed on the socio-economic background variables, an attrition dummy, and this dummy interacted with the background variables. If the coefficient on the attrition dummy and its interaction coefficients are jointly significant, the two samples of non-attritors and attritors differ from each other, so that attrition is likely to be non-random. Only two of the eleven p-values of F-tests for joint significance are slightly below the 10% level.[10] From these two tests I conclude that the attrition in my sample is most likely to be random. Most importantly, the attrition rate does not differ at all between the treatment and control group. Therefore, my results should not be biased by attrition.

7. Conclusion

Results from 5th revision of the DI act, which cancelled the additional pension (AP) for spouses, show positive and significant employment effects on the extensive and intensive margin for the DI beneficiary. Effects on spousal labor supply and on outflow from the DI are also positive but not statistically significantly different from zero. Yet, I find no significant change in the degree of disability which would be coupled with a change in the amount of DI benefit of the individual. Beneficiaries seem to work at the margin, i.e. they take up work or increase hours only up to the level, where there is no reduction in their disability degree which would result in a reduction in DI benefits. Thus, from a policy perspective the abolition of the

[10] Detailed results from these regressions are available from the author upon request.

AP seems not to have resulted in all the effects originally intended. As a direct effect the cancellation of AP saves money per se. As an indirect effect, the policy maker may hope that beneficiaries or spouses will react with increasing labor supply, which would in turn decrease DI benefits and public spending even more. As another indirect effect, the government could further benefit from increasing tax revenues due to higher labor force participation. There is no evidence for the first indirect effect in my analysis, while there is some for the second. Significant spillover effects into other social insurances, which for the government would only result in shifting money between different accounts and would not save money, are not apparent, either.

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Appendix

TABLE A1: Impact on additional outcome variables

Dependent variable	Being employed		Worked for pay last week		Looking for work last week	
	(1)	(2)	(1)	(2)	(1)	(2)
Treat × Post	0.098	0.109**	0.093	0.100*	0.022	0.016
	(0.060)	(0.055)	(0.057)	(0.053)	(0.026)	(0.025)
Treat	-0.118**	-0.116**	-0.108**	-0.099**	-0.010	-0.004
	(0.055)	(0.051)	(0.051)	(0.047)	(0.022)	(0.021)
Covariates	No	Yes	No	Yes	No	Yes
R^2	0.007	0.147	0.008	0.143	0.003	0.019
Mean	0.293	0.293	0.254	0.254	0.038	0.038
	Weekly hours worked		**Weekly hours worked (in categories)**		**Spouse: Weekly hours worked (in categories)**	
Treat × Post	2.195	2.448	0.557	0.600*	0.545	0.560
	(1.969)	(1.795)	(0.389)	(0.356)	(0.518)	(0.485)
Treat	-3.105*	-3.083*	-0.696*	-0.686**	-0.252	-0.237
	(1.755)	(1.629)	(0.360)	(0.333)	(0.473)	(0.430)
Covariates	No	Yes	No	Yes	No	Yes
R^2	0.006	0.159	0.008	0.160	0.005	0.140
Mean	7.596	7.596	1.561	1.561	4.324	4.324
N	1182	1182	1182	1182	1182	1182
	Net earnings last year		**Outflow**		**Amount of DI benefit**	
Treat × Post	4070.9	4580.0	0.015	0.012	80.54	106.56
	(3630.4)	(3415.1)	(0.024)	(0.023)	(83.00)	(79.35)
Treat	-6301.8*	-6621.4**	-0.030	-0.030	32.10	48.99
	(3256.1)	(2981.2)	(0.024)	(0.023)	(79.24)	(73.42)
Covariates	No	Yes	No	Yes	No	Yes
R^2	0.009	0.139	0.008	0.049	0.009	0.118
Mean	10426.6	10426.6	0.025	0.025	1225.38	1225.38
N	1182	1182	915	915	1182	1182

Notes: Standard errors in parentheses are clustered at the individual level. **,*: statistically different at 5 and 10 percent level, respectively. *Source*: See Table 2.

TABLE A2: Placebo tests

Start of DI uptake:	**1997-98 vs. 1999-2000**		**1998-99 vs. 2000-01**	
	(1)	(2)	(3)	(4)
Treat × Post	-0.024	-0.006	-0.030	-0.006
	(0.056)	(0.052)	(0.056)	(0.053)
Treat	0.089*	0.065	-0.003	-0.041
	(0.053)	(0.050)	(0.051)	(0.050)
Covariates	No	Yes	No	Yes
R^2	0.010	0.137	0.003	0.094
Mean	0.227	0.227	0.242	0.242
N	972	972	1038	1038
Start of DI uptake:	**1999-2000 vs. 2001-02**		**2000-01 vs. 2002-03**	
Treat × Post	-0.080	-0.083	-0.055	-0.083
	(0.054)	(0.052)	(0.056)	(0.052)
Treat	-0.053	-0.060	-0.023	0.012
	(0.046)	(0.044)	(0.048)	(0.044)
Covariates	No	Yes	No	Yes
R^2	0.024	0.131	0.008	0.128
Mean	0.248	0.248	0.281	0.281
N	1151	1151	1237	1237

Notes: Standard errors in parentheses are clustered at the individual level.
*: statistically different at 10 percent level. *Source*: See Table 2.

TABLE A3: Changing window size

Dependent variable: Being in the labor force	Window size: 3 years -> Start of DI uptake: 2001-03 vs. 2004-06		Window size: 4 years -> Start of DI uptake: 2000-03 vs. 2004-07		Window size: 1 year -> Start of DI uptake: 2003 vs. 2004	
	(1)	(2)	(1)	(2)	(1)	(2)
Treat × Post	0.118**	0.112**	0.078	0.079	-0.084	-0.047
	(0.054)	(0.052)	(0.051)	(0.049)	(0.082)	(0.076)
Treat	-0.134***	-0.109**	-0.145***	-0.115**	0.055	0.025
	(0.050)	(0.048)	(0.048)	(0.046)	(0.074)	(0.069)
Covariates	No	Yes	No	Yes	No	Yes
R^2	0.008	0.122	0.012	0.125	0.006	0.141
Mean	0.329	0.329	0.317	0.317	0.315	0.315
N	1633	1633	2028	2028	612	612

Notes: Standard errors in parentheses are clustered at the individual level.
***,**: statistically different at 1 and 5 percent, respectively. *Source*: See Table 2.

TABLE A4: Effect heterogeneities

Sample splitted by	Yes	No	p-value of difference
50 or older	0.195	0.016	0.124
N	673	509	
Female	0.088	0.128	0.728
N	525	657	
Foreigner	0.135	0.108	0.821
N	710	472	
At least one dependent child	0.076	0.180	0.379
N	708	474	
Education: Lower secondary or lower	0.148	0.092	0.628
N	491	691	
Mental illness	0.063	0.145	0.479
N	456	726	

Notes: The coefficients represent δ from a separate regression in the form of (2) with "Being in the labor force" as the dependent variable. The sample includes only those beneficiaries who fulfill the given criteria. The p-values are from a Chow test comparing the two coefficients. *Source*: See Table 2.