The Effect of Disability Insurance Benefit Cuts on Labor Supply within the Household

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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 I focus on existing beneficiaries and effects of a benefit cut. The behavioral response on labor market participation is estimated 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. The effects for the spouse are also positive but not statistically significantly different from zero.

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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 benefit generosity should therefore lead to variation in labor supply. Since policy reforms in this sense often affect the general population, i.e. everyone faces the new rules, their effect is difficult to estimate in lack of suitable counterfactuals.

A range of studies have used 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. Autor and Duggan, 2003; Gruber, 2000; Staubli, 2011). Unlike most other 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 the intensive margin. 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 policy reforms which applied 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 Duggan *et al.* (2010) for the US). 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; 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 income effect in a Spanish setting.

When more drastic changes to existing beneficiaries by decreasing benefit generosity 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 benefits of approximately 100’000 individuals. Most recently, Moore (2011) estimates considerable employment increases by 20-30 percentage points for this population.

[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.

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 been recently 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 their husbands' enrollment into DI increases induced by relaxed eligibility criteria.

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 (for a short description of the SSDI program, see e.g.Bound and Burkhauser, 1999). 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. The working incapacity has to last at least for 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

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)

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.

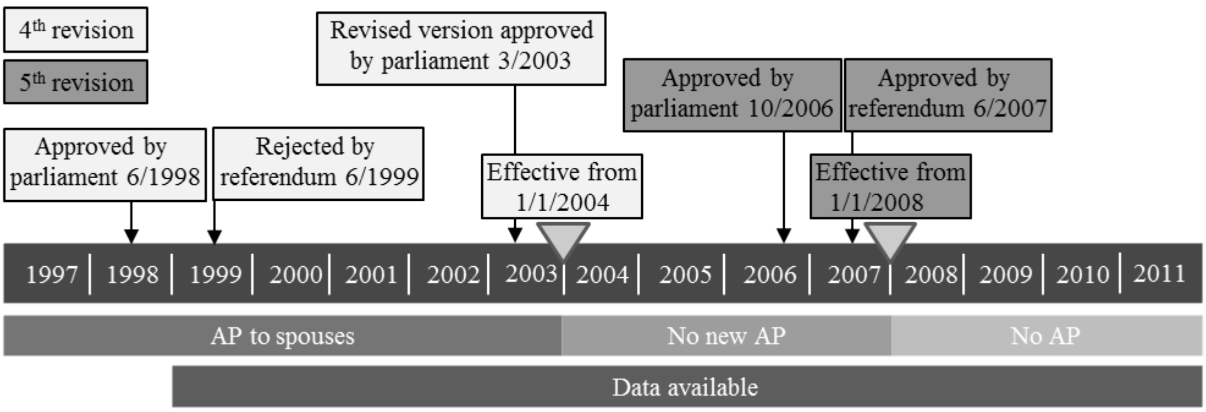
Similar to the supplemental security income (SSI) in the US, there is also a means-tested supplemental income program in Switzerland. If benefits from DI and other income fail to cover basic living costs, a Swiss resident can apply for *Ergänzungsleistungen*. Unlike in the SSI, eligibility is conditional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

Together with the Old Age and Survivors Insurance and the Unemployment Insurance, the DI and Ergänzungsleistungen 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 account 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.

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.[2] 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 1, 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.

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



[2] 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.

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 reduction of DI benefits (due to the partial DI system) or even an increase in DI outflow. The existence of a significant effect would quantify the extent of moral hazard in the DI. However, the abolition might also cause spill-over 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 data set is composed of data from 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 resulting data set 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. 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. It is also useful for my identification strategy to have observations from years before the policy change as outlined below. Therefore I use the pooled cross-section data over all available years.

4. Identification Strategy

There are two stages for the abolition of the additional pension to spouses (AP) which can be exploited as exogenous variation in benefit generosity. After the first reform in 2004, no new APs were granted but existing APs were still paid out (grandfathering). After the second reform in 2008, all remaining APs were cancelled.

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 treatment and control groups 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.[3]

4th revision

For the evaluation of the 4th revision I compare married individuals who started to receive their ordinary DI benefit two years before and after the revision with no longer married (i.e. divorced, separated or widowed) beneficiaries or alternatively with never married beneficiaries.[4] The difference is thus not taken over time but over periods of start of DI uptake. My hypothesis is that married individuals, who started to draw a DI benefit after the 4th revision and when access to APs was denied, should have a higher labor force participation (LFP) (on the extensive or intensive margin or on both) and a higher outflow relative to individuals, who started to draw a DI benefit before the revision and were thus potentially eligible for an AP. As a control I check whether these outcomes are to a similar degree

[3] 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.

[4] 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.

different between the groups of no longer married or never married beneficiaries, who were never eligible for the AP. Analytically I estimate regressions of the following type:

$$y_{it} = \alpha + \beta Treat_{it} + \gamma Period_i + \delta(Treat_{it} \times Period_i) + X_{it}'\theta + \varepsilon_{it}, \qquad (1)$$

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 person is married, 0 if person is no longer married or alternatively never married). *Period* is an indicator for the period in which the individual started to draw a DI pension (1 if start was between 2004 and 2005, 0 if start was between 2002 and 2003). X_{it} is a set of covariates, which include age, gender, foreigner status, educational attainment, number of children, regional dummies, and regional unemployment rate. For *t*, I use the years 2006 and 2007. I cannot use earlier years since I do not have any observations in earlier years when defining the variable *Period* in this way. Later years cannot be included either because it would interfere with the 5th revision implemented in 2008. I will assess the effect of the 5th revision in a separate specification below. The coefficient of interest is δ which measures the effect of no new APs on married beneficiaries relative to no longer or never married beneficiaries 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 two 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 severely 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 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 in the years 2006 and 2007 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. On a first look, this assumption might not hold because the 4th revision took almost seven years from the announcement of the law in 1997 until its implementation in 2004 (see FIGURE 1). However, by using observations from only after the revision (i.e. years 2006 and 2007), no anticipating behavior should be observed.

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 is 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.

5th revision

When analyzing the effect of the 5th revision, I no longer need to compare married with non-married individuals. Instead the treatment group now includes all married individuals who started to receive their ordinary DI pension between 2002 and 2003. 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. In this evaluation the second difference is over time, so I expect members of the treatment group to increase their LFP (on the extensive or intensive margin or on both) after the revision was implemented relative to members of the control group. In contrast to the 4th revision I can also look at the behavioral response of the spouse. Analytically I estimate regressions of the following type:

$$y_{it} = \alpha + \beta Treat_{it} + \delta(Treat_{it} \times Post_t) + \mu_t + X_{it}'\theta + \varepsilon_{it}, \qquad (2)$$

where in contrast to equation (1) *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. Again δ is the coefficient of interest which measures the effect of the abolition of APs on beneficiaries relative to beneficiaries who had never drawn an AP using variation over time. Standard errors are again clustered on the individual level to account for correlation within observations across the years.

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

$$y_{it} = \alpha + \beta Treat_{it} + \sum_{m=2007}^{2011} \delta_m (s_m \times Treat_{it}) + \mu_t + X'_{it}\theta + \varepsilon_{it}, \qquad (3)$$

where s_m is a dummy equal to 1 in year m and 0 otherwise. A significant effect (δ) in the year 2007 (i.e. prior to the reform) would hint to an anticipation effect. Yet, in contrast to the 4th revision the process from the announcement to the implementation took much less time for the 5th revision (see FIGURE 1).

I can use no longer 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 observationally equivalent group which is not affected by the cancellation of the AP. The regression framework then is

$$y_{it} = \alpha + \beta Treat_{it} + \gamma Married_{it} + \delta_1 (Treat_{it} \times Married_{it}) + \delta_2 (Treat_{it} \times Post_t) + \delta_3 (Married_{it} \times Post_t) + \lambda (Treat_{it} \times Married_{it} \times Post_t) + \mu_t + X'_{it}\theta + \varepsilon_{it} \qquad (4)$$

where *Married* is equal to 1 if the person is married and 0 if the person is no longer married. The coefficient of interest now is λ. Similarly to equation (3) 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

4th revision

TABLE 2 reports summary statistics of the sample used for the evaluation of the 4th revision. Each variable is grouped by civil status and set in comparison to beneficiaries who started to draw an ordinary DI benefit before January 1st 2004 with beneficiaries who started to draw an ordinary DI benefit after the date of the policy change. The upper panel includes various outcome variables. For married beneficiaries, measures of labor supply both on the intensive as well as on the extensive margin are significantly higher for those who are not eligible for an AP because they started to draw a DI benefit in the years 2004 or 2005. Using an unaffected population as comparison, the labor supply of no longer married individuals is marginally statistically lower for more "recent" beneficiaries, i.e. those who are on the DI rolls for a shorter time. However, if never married individuals are used to control for the

TABLE 2: Sample statistics by marital status for the analysis of the 4th revision

Year of start of DI benefit receipt	Married		No longer married		Never married	
	2002-03 (1)	2004-05 (2)	2002-03 (3)	2004-05 (4)	2002-03 (5)	2004-05 (6)
Outcome variables						
Being in the labor force	0.268	0.391***	0.447	0.296^	0.449	0.688†††
Worked for pay last week	0.193	0.302***	0.376	0.259	0.362	0.571††
Being employed	0.232	0.352***	0.424	0.278^	0.391	0.623†††
Being unemployed	0.031	0.089***	0.059	0.056	0.014	0.065
Weekly hours worked	6.602	9.688**	10.775	6.706^	10.152	17.471†††
Weekly hours worked (in categories of 5h)	1.358	2.050**	2.282	1.352^	2.159	3.714†††
Amount of DI benefit[1]	1213.2	1182.4	1370.6	1264.8	1314.5	1208.4
Outflow	0.013	0.043*	0.013	0	0.016	0.029
Backgrounds						
Age	49.205	48.872	52.706	50.5^^	42.884	40.662
Female	0.441	0.419	0.635	0.685	0.565	0.442
Foreigner	0.665	0.620	0.471	0.444	0.304	0.325
Number of children	1.315	1.034**	0.247	0.389	0.072	0.117
Educational dummies						
Lower secondary or lower	0.374	0.447	0.306	0.204	0.159	0.156
Higher secondary	0.492	0.441	0.494	0.685^^	0.710	0.662
Tertiary	0.134	0.112	0.200	0.111	0.130	0.182
Regional dummies						
Leman	0.181	0.123	0.176	0.148	0.203	0.182
Mittelland	0.130	0.173	0.200	0.130	0.159	0.130
Northwest	0.150	0.112	0.176	0.222	0.217	0.221
Zurich	0.122	0.134	0.118	0.056	0.174	0.117
East	0.181	0.201	0.176	0.130	0.043	0.169††
Central	0.083	0.112	0.059	0.130	0.014	0.065
Ticino	0.154	0.145	0.094	0.185	0.188	0.117
Regional un-employment rate in %	3.252	3.037	3.195	3.185	3.477	3.201
Number of observations	254	179	85	54	69	77

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. [1] Amount of DI benefit in CHF for the individual, not the household, therefore not including any AP. ***,**,*: statistically different from column 1 at 1, 5, and 10 percent, respectively. ^^,^: statistically different from column 3 at 5 and 10 percent, respectively. †††,††: statistically different from column 5 at 1 and 5 percent, respectively. *Source*: Own calculations based on SESAM data (waves 2006 & 2007).

counterfactual, we observe an even bigger increase in labor supply than for married individuals.

The 4th revision could deter persons from applying for disability benefits due to the lower amount of benefits granted. In order to control for this non-endogenous non-entry after the revision, the lower panel of TABLE 2 illustrates the socio-economic background characteristics of the different groups. With the exception of the number of children, which is lower for more recent beneficiaries, these characteristics do not differ significantly between the two groups for married beneficiaries. It is therefore rather unlikely that a particular group of people decided not to apply for DI benefits due to the change in benefit generosity. For the unaffected population, there is no significant change in the composition of the groups either, except that no longer married beneficiaries with a more recent start year of DI benefits are on average two years younger and have a different educational attainment. Never married beneficiaries only differ significantly in one region of residence.

Results of the OLS estimation of equation (1) are summarized in TABLE 3. The dependent variable is a dummy for being in the labor force (employed or unemployed). The results show that the choice of the group which controls for the counterfactual is crucial. While there is a large significant positive effect on labor force participation when compared to no longer married beneficiaries, the effect is even negative, yet not significantly, when compared to never married beneficiaries. The effects are fairly robust to including covariates (see column (1) vs. (2), and (3) vs. (4), respectively), which have the expected sign but are not reported. When no longer married DI beneficiaries are used to control for the counterfactual, married DI beneficiaries who no longer have the possibility to draw an AP have an LFP which is on average 25.9 percentage points higher than the LFP of married DI beneficiaries who still can draw an AP. The results are similar, both in magnitude and statistical significance, when different measures for labor supply are used. They are reported in TABLE A1 in the appendix, as well as results on the intensive margin. Married beneficiaries increased their hours of work by 6-7 hours per week when compared to no longer married beneficiaries. Part of the increase in labor force participation might be explained by an increase in unemployment. Yet, the increase in unemployment is not significantly different from zero. The effect on outflow is not significant, either. Note that for this estimation I included only beneficiaries who were observed over at least two years. There is hardly any effect on the amount of DI benefit of the

TABLE 3: Impact of 4th revision on labor force participation

	No longer married		Never married	
	(1)	(2)	(3)	(4)
Being in the labor force				
Treat × Period	0.274**	0.259**	-0.116	-0.099
	(0.111)	(0.110)	(0.112)	(0.108)
Treat (=1 if married)	-0.179**	-0.136*	-0.182**	-0.056
	(0.075)	(0.075)	(0.081)	(0.092)
Period (=1 if start of DI	-0.151	-0.150	0.239**	0.215**
uptake in 2004 or 2005)	(0.096)	(0.094)	(0.096)	(0.093)
Covariates	No	Yes	No	Yes
R^2	0.022	0.107	0.079	0.154
Mean	0.336	0.336	0.383	0.383
N	572	572	579	579

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

beneficiary.[5] So the increased labor supply did not reduce the degree of disability which would have resulted in a reduced DI benefit.

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 define an arbitrary cutoff date some years prior to the actual policy change. As a first date I choose January 1st 2000, so that the first group includes persons who started to draw DI benefits in the years 1998 or 1999, while persons in the other group started to draw DI benefits in the years 2000 or 2001. I observe their labor force participation in 2002 and 2003. When no longer married beneficiaries are used to control for the counterfactual, the effect of the placebo policy change is no longer statistically significantly different from zero and has even a different sign (see panel A of TABLE A2 in the appendix). There is no change when never married beneficiaries are used to control for the counterfactual. The effect remains negative and not significantly different from zero. In additional placebo regressions, I use two cutoff dates (January 1st 2001 and 2002, respectively) with the same setup for the two groups. The results are very similar both quantitatively and qualitatively compared to the first

[5] This variable does not include any AP but only the benefit for the individual. The amount of DI benefit per household (including any AP) decreased.

placebo date. 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 also husbands were eligible. The results from the placebo regressions illustrate that the difference in LFP in the years 2006 and 2007 seems to buck the trend as the LFP of more recent married DI beneficiaries is usually lower than or similar to the LFP of less recent married DI beneficiaries. This is a strong sign of evidence for a causal effect of the 4th revision.

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. Panel A of TABLE A3 in the appendix reports the results of the regression when increasing the window size to three years. The variable *Period* in Equation (1) has then the value of 1 if the start of DI uptake was between 2004 and 2006, and 0 if it was between 2001 and 2003. The magnitude of the effect changes only when no longer married beneficiaries are used to control for the counterfactual (7.5 percentage points lower). The results remain on this level when I increase the window size further to four years (see Panel B) but they are more precisely estimated. Further increasing the window size to five or more years is not only impossible because it would interfere with the 5th revision. It is also questionable to compare beneficiaries on the DI rolls for up to ten years with those on the DI rolls for only one year since their employment prospects might differ substantially. Finally, when I decrease the window size from the baseline setup of two years to one, the results decrease even further when no longer married beneficiaries are used to control for the counterfactual and increase in absolute terms with never married beneficiaries as the control group. However, the results are now no longer significantly different from zero for both control groups (see panel C of TABLE A3).

It could be argued that married DI beneficiaries who then still drew an AP could already increase their LFP in the year 2007 anticipating the abolition of the AP with the 5th revision by January 1st 2008. Therefore I drop observations from the year 2007 in a sensitivity check. The effects are again only somewhat smaller (about five percentage points) and only when no longer married beneficiaries are used to control for the counterfactual. However, the precision suffers from excluding half of the observations and the effect is no longer significantly different from zero. Anticipation bias from the 5th revision might therefore be of limited importance.

As a summary for the 4th revision, I find a significant and robust positive effect on labor supply when I control for the counterfactual with no longer married beneficiaries. There is no or even a negative effect when I use never married beneficiaries to control for the counterfactual. Therefore either never married beneficiaries are not a valid group to control for the counterfactual or the positive effect when no longer married beneficiaries are used to control for the counterfactual is spurious and not due to the policy change. An argument in favor of the former interpretation is that no longer married beneficiaries might be more similar in observed and unobserved characteristics to married beneficiaries than never married beneficiaries to married beneficiaries.

5th revision

As outlined in the identification strategy part, I can rely solely on married beneficiaries when analyzing the 5th revision. The reason is that both groups of married beneficiaries can be observed before and after the policy change. (One group includes those who lose their AP, while the other includes those who never had an AP). Similar to TABLE 2, TABLE 4 reports summary statistics of the sample used for the evaluation of the 5th revision. Each variable is grouped by treatment status and compared before and after January 1st 2008, the date of the policy change. The upper panel includes various outcome variables. For treated beneficiaries (i.e. married DI beneficiaries who started to draw DI benefits in the year 2002 or 2003 and lose the AP by January 2008) all outcome variables (except for being unemployed) are higher 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.[6] Background characteristics of the sampled beneficiaries are reported in the lower panel of TABLE 4. The two groups do not differ significantly between the two periods, so non-endogenous non-entry should again not bias the results.

[6] A notable exception is a remarkable high fraction that claims to be unemployed in the pre-policy period. Evidence from other cohorts shows it is not uncommon for very recent DI beneficiaries to have an unemployment rate above the average.

TABLE 4: Sample statistics by treatment group 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)
Outcome variables				
Being in the labor force	0.268	0.332*	0.391	0.340
Worked for pay	0.193	0.257*	0.302	0.268
Being employed	0.232	0.289	0.352	0.308
Being unemployed	0.031	0.020	0.089	0.020^^^
Weekly hours worked	6.602	6.996	9.688	7.845
Weekly hours worked (in categories of 5h)	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 of 5h)	4.102	4.514	4.363	4.259
Amount of DI benefit	1213.2	1293.9	1182.4	1180.8
Outflow	0.013	0.020	0.043	0.029
Backgrounds				
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^^
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 1, 5, and 10 percent, respectively. *Source*: Own calculations based on SESAM data (waves 2006-2011).

TABLE 5: Impact of 5th revision on labor force participation

Dependent variable	Being in the labor force		Spouse: Being employed	
	(1)	(2)	(3)	(4)
Treat × Post	0.116**	0.131**	0.060	0.062
	(0.057)	(0.055)	(0.061)	(0.060)
Treat	-0.123***	-0.114***	-0.017	-0.003
	(0.046)	(0.044)	(0.057)	(0.056)
Covariates	No	Yes	No	Yes
R^2	0.007	0.112	0.008	0.081
Mean	0.329	0.329	0.644	0.644
N	1181	1181	1136	1136

Notes: Standard errors in parentheses are clustered at the individual level. ***,**: statistically different at 1 and 5 percent level, respectively. *Source*: See Table 4.

Results of the OLS estimation of equation (2) are summarized in TABLE 5. The dependent variable in columns (1) and (2) is again a dummy for being in the labor force (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 12-13 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. Part of the lower increase might be explained that a high fraction of spouses (62%) had already been employed before the revision. This is about twice the initial value of DI beneficiaries before the revision. TABLE A5 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. Part of the increase in labor force participation can be explained by a significant increase in unemployment. The effect 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 effect on outflow is also positive but not statistically significant. However, as in the 4th revision, 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.[7]

[7] Again (see footnote 5), the effect on the amount of DI benefit per household significantly decreased as a result of the abolition of the AP.

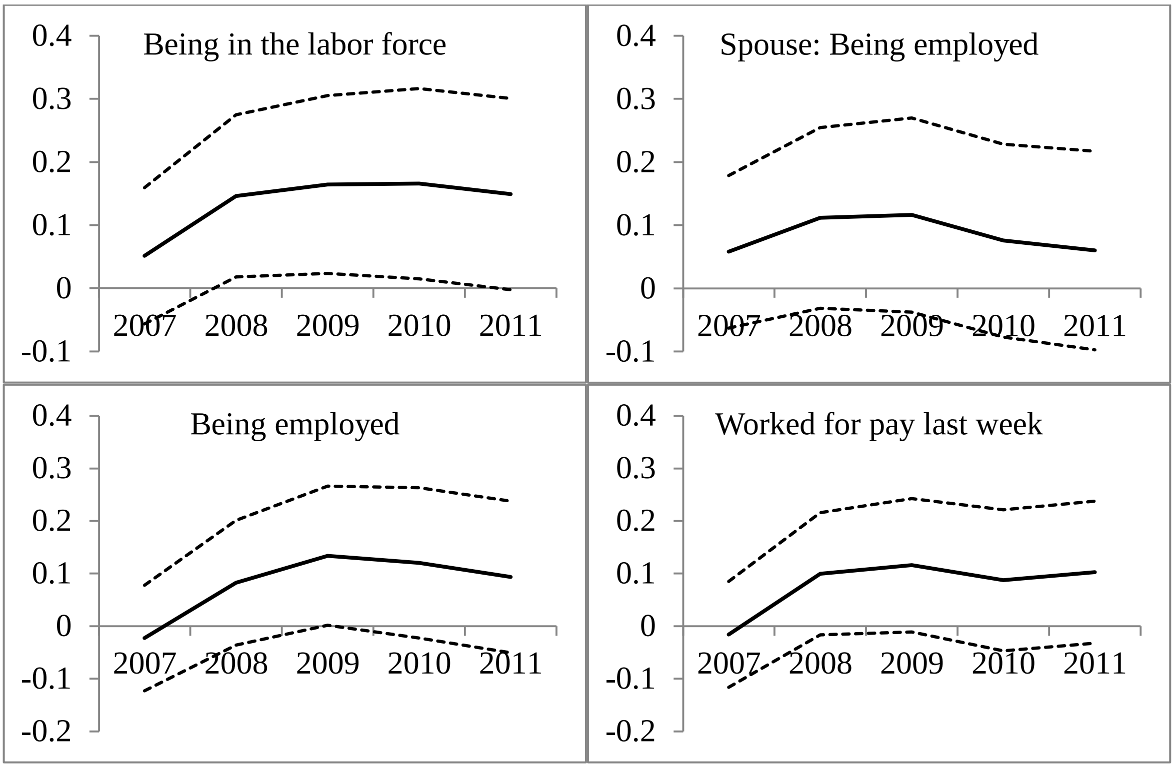
Similarly to the analysis of the 4th revision I run placebo regressions where I choose an artificial date of revision to define treatment and control groups. A significant effect of this artificial revision would cast doubt on the causality of the results of the 5th revision. I use all possible dates in the period between the years 1997 and 2003, which was not affected by any actual revision. The first artificial date of revision is 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 year 1999 or 2000. In contrast to the placebo regression of the 4th revision, I do not change the years of observations which remain between the years 2006 and 2011. As the upper left panel of TABLE A6 in the appendix shows, the effect of this placebo revision is virtually zero, especially 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 gains borderline statistical significance. However, the sign of the effect is reversed compared to the one from the 5th revision. Thus, the positive coefficient in TABLE 5 seems to reflect a causal effect from the 5th revision.

As a next sensitivity check, I alter the window size used to define the groups in the same way as in the 4th revision. TABLE A7 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 with borderline significance when the window size is increased to four years. When the window size is decreased to one year, the sign of the effect is reversed, yet not significantly different from zero.

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 (3) 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 the upper left panel, when the effect on being in the labor force is analyzed.

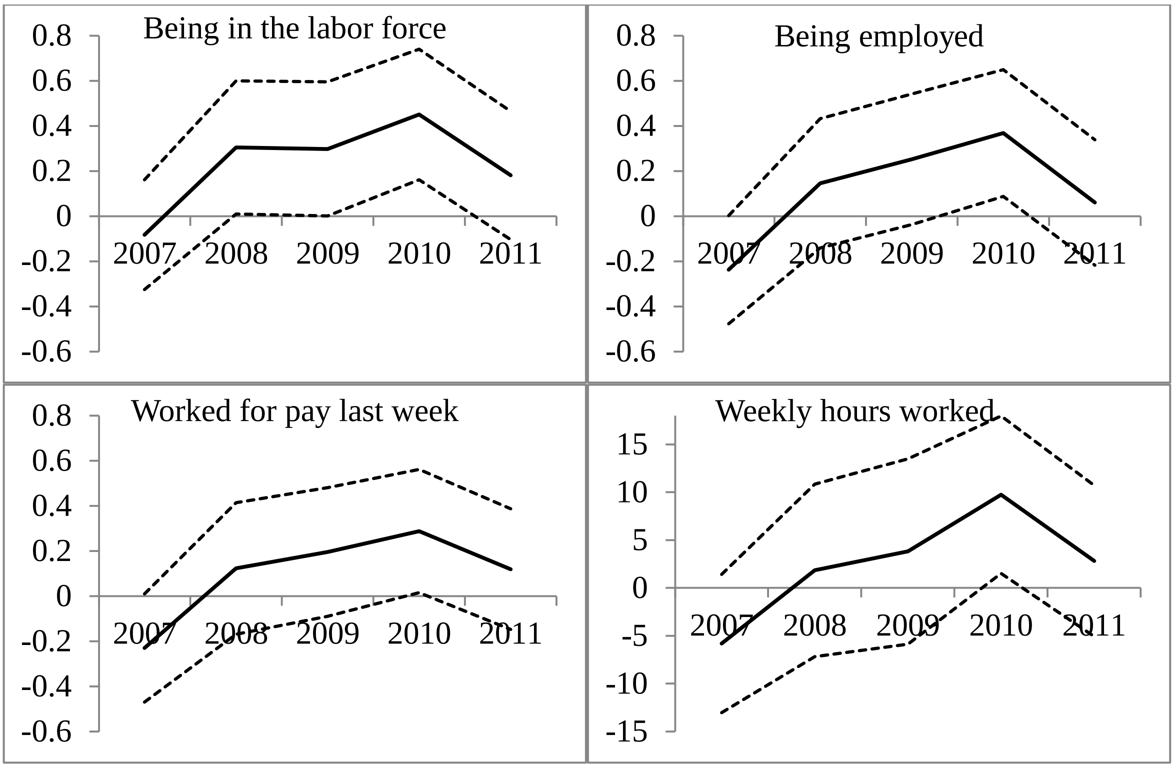
I now use no longer married beneficiaries as an additional control group. This group is similar to married beneficiaries but unaffected by the policy change. Including this group in a Difference-in-Difference-in-Differences (DiDiD) set up should cancel out changes in employment rates of the treatment group which are unrelated to the policy change. TABLE 6 reports the coefficients of the triple interaction effect in Equation (4) for various outcome measures. The cancellation of the AP has again a 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 neither on unemployment nor on outflow. Note that 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 (4). The pattern is very similar to FIGURE 2 with the exception that the maximum is now reached in 2010.

FIGURE 2: Dynamic impact of 5th revision on labor supply with DiD



Notes: Coefficients of the treatment × year interactions in Equation (3). The dotted lines represent the 90% confidence interval. *Source*: See Table 4.

FIGURE 3: Dynamic impact of 5th revision on labor supply with DiDiD



Notes: Coefficients of the treatment × married × year interactions in Equation (4). The dotted lines represent the 90% confidence interval. *Source*: See Table 4.

TABLE 6: DiDiD estimates for various outcomes of 5th revision

Dependent variable	**Treat × Married × Post**
Being in the labor force	0.332***
	(0.115)
Worked for pay last week	0.288***
	(0.110)
Being employed	0.308***
	(0.112)
Being unemployed	0.071
	(0.056)
Weekly hours worked	7.077**
	(3.386)
Weekly hours worked in categories of 5h	1.654**
	(0.678)
Amount of DI benefit	229.97
	(155.11)
N	1637
Outflow	0.033
	(0.031)
N	1150

Notes: Standard errors in parentheses are clustered at the individual level. ***,**: statistically different at 1, and 5 percent level, respectively. Number of observations is 1150 for the outflow regression. Source: See Table 4.

As a summary to the 5th revision, I find a significant positive employment effect on the extensive and the intensive margin for married individuals who lost part of their DI benefits after January 1st 2008. The effect is substantial but not large enough to have a significant impact on the outflow from the DI. The effect on the labor supply of the spouse is also positive but not significant.

6. Discussion

We have already seen that part of the increase in labor supply might be explained by a concurrent increase in unemployment. Persons then switch from one social insurance to another with probably low net gain to the funding state. In addition, there might be spillover effects of the abolition of the AP into other social insurances which could have an effect on the decision whether to change the labor supply and thus could affect the results. (1) If the couple passes a means test, they can request supplementary benefits (*Ergänzungsleistungen*). I am able to control for this effect since their receipt is included in the SESAM data. However, estimations where the receipt of supplementary benefits is used as the outcome variable show that the fraction of people drawing supplementary benefits actually decreased. (2) If the DI beneficiary receives an additional benefit from an occupational or accidental insurance plan, the amount of this pension increases if it has been previously capped to prevent overcompensation. Unfortunately, the SESAM data do not include information on income from this source but only total household income from all possible sources. Instead 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 benefit. 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 4th and 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 they affected the inflow into DI (new beneficiaries) and not the existing beneficiaries. The only exception was the introduction of the three-quarter pension, which could lower DI benefits if the degree of disability was between 66.67 and 69.99 percent. If the beneficiary is younger than 50, his full pension is then reduced to a three-quarter pension (see TABLE 1). The analysis of my sample shows that this applied to only ten persons. This concern is therefore negligible.

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. About a third of all married DI beneficiaries in the data did not draw an AP. Yet, given that eligibility was assessed by the DI, I assume the take-up rate of the AP to be very close to 100%.

7. Conclusion

Results from both the 4th and the 5th revision of the DI act, which cancelled the additional pension for spouses in two steps, 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.

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Appendix

TABLE A1: Impact of 4th revision on additional outcome variables

	No longer married		Never married	
	(1)	(2)	(3)	(4)
Worked for pay last week				
Treat × Period	0.226**	0.211**	-0.100	-0.086
	(0.105)	(0.103)	(0.112)	(0.106)
Treat	-0.184***	-0.129*	-0.169**	-0.003
	(0.071)	(0.091)	(0.078)	(0.084)
Period	-0.117	-0.119	0.209**	0.182*
	(0.092)	(0.091)	(0.099)	(0.095)
Covariates	No	Yes	No	Yes
R^2	0.024	0.141	0.073	0.179
Mean	0.297	0.297	0.297	0.297
Being employed				
Treat × Period	0.265**	0.258**	-0.112	-0.098
	(0.109)	(0.107)	(0.113)	(0.109)
Treat	-0.191***	-0.130*	-0.159**	0.006
	(0.074)	(0.073)	(0.080)	(0.089)
Period	-0.146	-0.144	0.232**	0.213**
	(0.094)	(0.092)	(0.099)	(0.095)
Covariates	No	Yes	No	Yes
R^2	0.025	0.137	0.072	0.170
Mean	0.302	0.302	0.340	0.340
Being unemployed				
Treat × Period	0.061	0.061	0.007	0.009
	(0.053)	(0.050)	(0.045)	(0.044)
Treat	-0.027	-0.041	0.017	-0.008
	(0.036)	(0.039)	(0.018)	(0.033)
Period	-0.003	-0.006	0.050	0.048
	(0.046)	(0.045)	(0.036)	(0.035)
Covariates	No	Yes	No	Yes
R^2	0.012	0.043	0.016	0.039
Mean	0.056	0.056	0.052	0.052

TABLE A1 (continued)

	No longer married		Never married	
	(1)	(2)	(3)	(4)
Weekly hours worked				
Treat × Period	7.155**	6.336**	-4.234	-3.501
	(3.227)	(3.100)	(3.541)	(3.384)
Treat	-4.173*	-2.443	-3.550	1.180
	(2.265)	(2.632)	(2.422)	(2.663)
Period	-4.070	-3.363	7.319**	6.245**
	(2.710)	(2.632)	(3.076)	(2.937)
Covariates	No	Yes	No	Yes
R^2	0.014	0.128	0.053	0.148
Mean	8.198	8.198	9.425	9.425
Amount of DI benefit				
Treat × Period	74.99	73.86	75.26	101.11
	(152.56)	(149.87)	(148.52)	(151.23)
Treat	-157.40	-220.7**	-101.30	-37.04
	(101.31)	(106.95)	(97.79)	(110.51)
Period	-105.77	-124.50	-106.05	-142.96
	(130.62)	(127.87)	(125.69)	(132.80)
Covariates	No	Yes	No	Yes
R^2	0.010	0.117	0.004	0.073
Mean	1231.82	1231.82	1215.11	1215.11
N	572	572	579	579
Outflow				
Treat × Period	0.043	0.044	0.017	0.018
	(0.027)	(0.028)	(0.040)	(0.041)
Treat	0.000	0.007	-0.003	0.008
	(0.016)	(0.020)	(0.018)	(0.023)
Period	-0.013	-0.014	0.013	0.016
	(0.013)	(0.014)	(0.032)	(0.036)
Covariates	No	Yes	No	Yes
R^2	0.011	0.038	0.007	0.035
Mean	0.021	0.021	0.024	0.024
N	522	522	531	531

Notes: Standard errors in parentheses are clustered at the individual level. ***,**,*: statistically different at 1, 5, and 10 percent level, respectively. *Source*: See Table 2.

TABLE A2: Placebo tests for the 4th revision

Dependent variable:	No longer married		Never married	
Being in the labor force	(1)	(2)	(3)	(4)
A				
Start of DI uptake: 1998-99 vs. 2000-01 Years of observation: 2002 & 2003				
Treat × Period	-0.119	-0.061	-0.076	-0.038
	(0.095)	(0.091)	(0.093)	(0.092)
Treat	0.051	0.075	-0.207***	-0.071
	(0.071)	(0.069)	(0.067)	(0.072)
Period	0.153*	0.111	0.111	0.099
	(0.083)	(0.080)	(0.080)	(0.081)
Covariates	No	Yes	No	Yes
R^2	0.008	0.141	0.052	0.151
Mean	0.344	0.344	0.404	0.404
N	726	726	743	743
B				
Start of DI uptake: 1999-2000 vs. 2001-02 Years of observation: 2003 & 2004				
Treat × Period	-0.059	-0.045	-0.009	-0.021
	(0.084)	(0.082)	(0.092)	(0.094)
Treat	-0.086	-0.013	-0.273***	-0.122*
	(0.589)	(0.059)	(0.064)	(0.069)
Period	0.129*	0.118	0.079	0.094
	(0.074)	(0.072)	(0.082)	(0.085)
Covariates	No	Yes	No	Yes
R^2	0.021	0.134	0.065	0.143
Mean	0.317	0.317	0.352	0.352
N	897	897	870	870

TABLE A2 (continued)

Dependent variable:	No longer married		Never married	
Being in the labor force	(1)	(2)	(3)	(4)
C				
Start of DI uptake: 2000-01 vs. 2002-03 Years of observation: 2004 & 2005				
Treat × Period	-0.080	-0.080	-0.055	-0.084
	(0.091)	(0.093)	(0.100)	(0.096)
Treat	-0.072	-0.002	-0.236***	-0.040
	(0.064)	(0.071)	(0.069)	(0.076)
Period	0.116	0.106	0.091	0.110
	(0.080)	(0.083)	(0.090)	(0.087)
Covariates	No	Yes	No	Yes
R^2	0.033	0.089	0.056	0.143
Mean	0.304	0.304	0.334	0.334
N	831	831	794	794

Notes: Standard errors in parentheses are clustered at the individual level. ***,*: statistically different at 1, and 10 percent level, respectively. *Source*: Own calculations based on SESAM data (waves 2002-2005).

TABLE A3: Changing window size for the 4th revision

Dependent variable: Being in the labor force	No longer married (1)	(2)	Never married (3)	(4)
Window size: 3 years -> Start of DI uptake: 2001-03 vs. 2004-06				
Treat × Period	0.227**	0.184*	-0.093	-0.108
	(0.098)	(0.098)	(0.094)	(0.092)
Treat	-0.126**	-0.066	-0.170***	-0.015
	(0.063)	(0.065)	(0.094)	(0.074)
Period	-0.092	-0.072	0.229***	0.220***
	(0.085)	(0.084)	(0.080)	(0.078)
Covariates	No	Yes	No	Yes
R^2	0.018	0.107	0.070	0.141
Mean	0.326	0.326	0.374	0.374
N	764	764	789	789
Window size: 4 years -> Start of DI uptake: 2000-03 vs. 2004-07				
Treat × Period	0.234***	0.191**	-0.071	-0.093
	(0.091)	(0.091)	(0.088)	(0.086)
Treat	-0.132***	-0.078	-0.192***	-0.036
	(0.051)	(0.055)	(0.054)	(0.066)
Period	-0.087	-0.072	0.218***	0.212***
	(0.078)	(0.078)	(0.074)	(0.071)
Covariates	No	Yes	No	Yes
R^2	0.021	0.105	0.072	0.142
Mean	0.317	0.317	0.360	0.360
N	936	936	933	933
Window size: 1 year -> Start of DI uptake: 2003 vs. 2004				
Treat × Period	0.117	0.144	-0.189	-0.182
	(0.150)	(0.141)	(0.154)	(0.137)
Treat	-0.114	-0.069	-0.217*	-0.054
	(0.104)	(0.094)	(0.111)	(0.118)
Period	-0.172	-0.190	0.134	0.121
	(0.130)	(0.123)	(0.135)	(0.119)
Covariates	No	Yes	No	Yes
R^2	0.015	0.140	0.086	0.197
Mean	0.302	0.302	0.366	0.366
N	301	301	309	309

Notes: Standard errors in parentheses are clustered at the individual level. ***,**,*: statistically different at 1, 5, and 10 percent, respectively. *Source*: See Table 2.

TABLE A4: Restricting observations to one year for the 4th revision

	No longer married		Never married	
Year of observation: 2006	(1)	(2)	(3)	(4)
Being in the labor force				
Treat × Period	0.220*	0.209	-0.106	-0.098
	(0.111)	(0.132)	(0.132)	(0.127)
Treat	-0.132*	-0.067	-0.156*	-0.009
	(0.080)	(0.084)	(0.093)	(0.107)
Period	-0.080	-0.081	0.246**	0.231**
	(0.115)	(0.115)	(0.115)	(0.111)
Covariates	No	Yes	No	Yes
R^2	0.020	0.116	0.074	0.177
Mean	0.323	0.323	0.368	0.368
N	294	294	291	291

Notes: Standard errors in parentheses are clustered at the individual level. **,*: statistically different at 5, and 10 percent level. *Source:* Own calculations based on SESAM data (wave 2006).

Table A5: Impact of 5th revision on additional outcome variables

Dependent variable	Worked for pay last week		Being employed		Being unemployed	
	(1)	(2)	(1)	(2)	(1)	(2)
Treat × Post	0.095*	0.109**	0.100*	0.119**	0.058**	0.055**
	(0.056)	(0.052)	(0.058)	(0.055)	(0.028)	(0.028)
Treat	-0.108**	-0.098**	-0.118**	-0.115**	-0.058**	-0.055**
	(0.051)	(0.048)	(0.055)	(0.052)	(0.026)	(0.026)
Covariates	No	Yes	No	Yes	No	Yes
R^2	0.008	0.126	0.007	0.127	0.021	0.034
Mean	0.253	0.253	0.292	0.292	0.033	0.033
Dependent variable	**Weekly hours worked**		**Weekly hours worked (in categories of 5h)**		**Spouse: Weekly hours worked (in categories of 5h)**	
Treat × Post	2.282	2.849	0.574	0.680*	0.532	0.596
	(1.891)	(1.763)	(0.379)	(0.354)	(0.502)	(0.472)
Treat	-3.105*	-3.042*	-0.696*	-0.678**	-0.252	-0.232
	(1.755)	(1.669)	(0.360)	(0.341)	(0.473)	(0.431)
Covariates	No	Yes	No	Yes	No	Yes
R^2	0.006	0.130	0.008	0.130	0.005	0.130
Mean	7.569	7.569	1.555	1.555	4.328	4.328
N	1181	1181	1181	1181	1181	1181
Dependent variable	**Outflow**					
Treat × Post	0.023	0.021				
	(0.023)	(0.024)				
Treat	-0.030	-0.030				
	(0.024)	(0.023)				
Covariates	No	Yes				
R^2	0.009	0.036				
Mean	0.025	0.025				
N	850	850				

Notes: Standard errors in parentheses are clustered at the individual level. **,*: statistically different at 5, and 10 percent level, respectively. *Source*: See Table 4.

TABLE A6: Placebo tests for the 5th revision

Start of DI uptake:	**1997-98 vs. 1999-2000**		**1998-99 vs. 2000-01**	
	(1)	(2)	(3)	(4)
Treat × Post	-0.024	-0.002	-0.030	-0.005
	(0.055)	(0.052)	(0.055)	(0.052)
Treat	0.089	0.072	-0.003	-0.038
	(0.053)	(0.052)	(0.051)	(0.051)
Covariates	No	Yes	No	Yes
R^2	0.010	0.127	0.003	0.087
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	-0.086*	-0.055	-0.080
	(0.053)	(0.050)	(0.055)	(0.052)
Treat	-0.053	-0.054	-0.023	0.014
	(0.046)	(0.044)	(0.048)	(0.045)
Covariates	No	Yes	No	Yes
R^2	0.018	0.124	0.008	0.109
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 4.

TABLE A7: Changing window size for the 5th revision

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	0.119**	0.120**	0.080	0.085*	-0.080	-0.060
	(0.053)	(0.051)	(0.050)	(0.048)	(0.079)	(0.075)
Treat	-0.134***	-0.108**	-0.145***	-0.114**	0.055	0.055
	(0.050)	(0.048)	(0.048)	(0.047)	(0.074)	(0.070)
Covariates	No	Yes	No	Yes	No	Yes
R^2	0.008	0.105	0.011	0.110	0.006	0.108
Mean	0.328	0.328	0.316	0.316	0.314	0.314
N	1632	1632	2026	2026	611	611

Notes: Standard errors in parentheses are clustered at the individual level. ***,**,*: statistically different at 1, 5 and 10 percent, respectively. *Source*: See Table 4.