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

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Abstract

Previous empirical literature has shown a substantial extent of work disincentives in the Disability Insurance (DI). While its focus has been on the inflow into DI and on increases in benefits, this study focuses on a partial benefit cut and on existing beneficiaries. The partial benefit reduction affected married DI beneficiaries only and was also dependent on their entry into DI. 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. Using a difference-in-differences methodology, I find no effect on labor supply or earnings for both members of the couple. If anything, there might be spillover effects into means-tested social insurance. These results indicate partially cutting benefits is not an effective policy to increase labor supply of existing DI beneficiaries.

Keywords

Disability insurance, labor supply, policy reform.

JEL Classification

H55, J21.

1 Introduction

Most developed countries have lately experienced growing numbers of beneficiaries in their disability insurance (DI) systems. Since the outflow from DI for other reasons than death or retirement is very low, on average at about 1% (OECD, 2010), the substantial pressure on public finances has mounted. Policy makers are thus in need of effective and efficient policies to control the stock of DI beneficiaries. Due to asymmetric information inherent in every insurance, DI systems can produce substantial work disincentives, so that persons without an impaired working capacity may select into or stay in DI. There is now a growing literature analyzing the extent of work disincentives in the DI in various countries. The most popular way is to analyze how changes in eligibility criteria or benefit generosity lead to changes in labor supply (Staubli (2011) for Austria, Campolieti (2004) and Gruber (2000) for Canada, Marie and Vall Castello (2012) for Spain, Karlström et al. (2008) for Sweden, and Chen and van der Klaauw (2008) as well as Duggan et al. (2010) for the United States). Most of these studies show substantial work disincentive effects, whereas Staubli (2011) and Karlström et al. (2008) are also able to identify substantial spillover effects into other social insurances (unemployment or sickness insurance). Yet, the focus of these studies is usually on the inflow into the DI or on employment at the time of application.

In this paper I take a different focus by analyzing whether a partial benefit cut leads to a significant increase in labor supply of *existing* beneficiaries. Empirical studies of reductions in DI benefits are scarce in lack of respective policy changes. A notable exception is the removal of drug

addiction as a disabling condition in the USA in 1996, which fully terminated benefits of approximately 100,000 individuals. Most recently, Moore (2011) estimates considerable employment increases by 20-30 percentage points for this population. However, to the best of my knowledge there is no study which analyzes the effect of only a partial benefit cut applying to existing beneficiaries unconditional on any health condition or type of disability.

Another contribution of this paper is to analyze the behavioral response not only of the beneficiary but also of the spouse. Today, 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. 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 labor supply. While the added worker 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.

I use a policy reform of the Swiss DI in 2008 which partially cut benefits for some married beneficiaries. Prior to the reform, married DI beneficiaries who had received DI benefits for more than four years and had fulfilled very

low eligibility criteria could draw an additional pension (AP) for their spouse, unconditional on the spouse's labor supply. Beneficiaries lost up to 23% of their DI benefits, on average about 3500 CHF per year, which is equal to about 5% of total household income. With administrative records of the full Swiss DI beneficiary population I apply a difference-in-differences methodology using married beneficiaries who had received DI benefits for less than four years as a control group. Their benefits were unaffected by the policy reform in 2008 since they had never drawn an AP.

Unlike the previous literature on inflow, the results show no effects neither on employment nor earnings for both the beneficiary and the spouse. For the main beneficiary, changes in labor supply could affect the amount of DI benefits even further. In contrast, for the spouse, the partial reduction by the abolition of the AP is a pure income effect being welfare neutral. Due to the loss in income, part of the population seems to newly qualify for means-tested supplementary benefits. These results indicate that partially cutting benefits is not an effective policy to increase labor supply of existing DI beneficiaries.

The paper proceeds as follows. The next section gives information on the institutional setting and the policy reform. Section 3 describes the data. In section 4 the empirical strategy is outlined. Section 5 presents the results, while section 6 concludes.

2 Institutional setting

2.1 The Swiss Disability Insurance as part of the Social Insurance system

The Swiss social insurance system consists of three pillars. The first pillar includes Disability Insurance (DI), Old Age and Survivors Insurance and Unemployment Insurance, which are all financed through a pay-as-you-go scheme. In order to be awarded with a disability insurance (DI) pension, an individual's working capacity has to be limited by a health impairment lasting for at least one year. Generally the applicant needs also to have worked in sustainable employment for at least one year (three years since 2008). The amount of benefits is dependent on the degree of disability which leads to a partial benefit system. If DI benefits and other income fail to cover basic living costs, a Swiss resident can apply for means-tested Supplementary Benefits (*Ergänzungsleistungen*).

The second pillar is constituted by occupational pension plans and accident insurance financed through a funded scheme. Every employed worker is required to individually contribute to an occupational pension plan through payroll taxes and which is 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 includes voluntary, tax-deductible

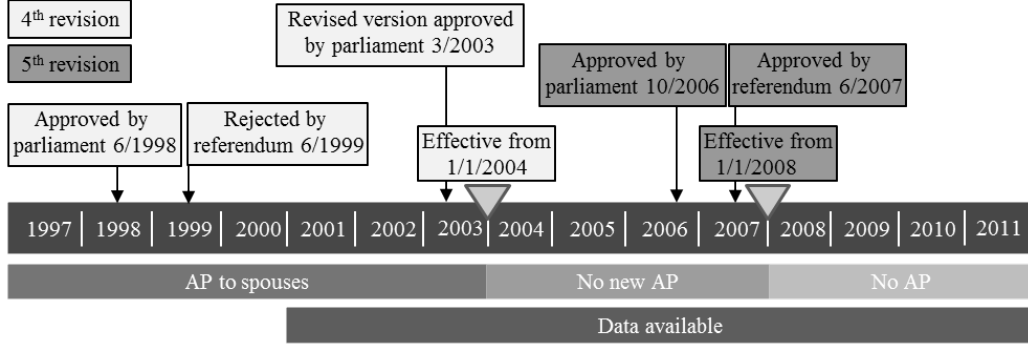
private insurance plans, whose benefits are granted in the same way as in the second pillar.

2.2 The additional pension for spouses and its abolition

Before 2008, an additional pension (AP) to a spouse of a DI beneficiary was awarded if the main beneficiary entered DI before January 1st, 2004 and fulfilled the following criteria: The person was employed prior to the onset of work incapacity and his/her spouse is not his-/herself eligible for a DI benefit. The AP was therefore granted unconditionally of the spouse's earnings. The reason for introducing this pension in the first place was the compensation of the part of the earnings which was lost due to the work incapacity by one member of the couple and was destined for the subsistence of the conjugal community. The amount of the AP was set to be an additional 30% of the DI benefit, which was on average about CHF 3500 per year or about 5% of total household income.

Due to the disproportionally rising number of DI beneficiaries in the nineties, which led to an imbalance in the system, the Swiss federal government decided in 1997 to cut mainly on the expenditure side. The extensive 4th DI reform package, which was approved by the parliament in June 1998, included the abolition of new grants of APs to spouses. Yet, the reform package faced heavy opposition, was therefore adapted and eventually became effective not until January 1st, 2004. From this day on, no APs were granted to spouses of new entrants into the DI, while all existing AP beneficiaries kept receiving the AP (grandfathering). Soon after, the 5th revision was drafted as the imbalance

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



in the system had continued to increase. The parliament discussed the law in 2005-2006, finally passing it in October 2006. The opposition against the revision was less fierce, so that the Swiss people approved the reform in June 2007. The government announced on September 28th, 2007 that the reform was going to be implemented on January 1st, 2008. The reform 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 Data

I use administrative register data covering the full Swiss population of DI beneficiaries between the years 2001 and 2011. Yearly information in December is available for the main beneficiary and the spouse (if any) on DI benefits (including benefits for dependent children), labor market status (employed or self-employed), yearly earnings, and additional social insurance payments (i.e. Supplementary Benefits and Helplessness Allowance). Background characteristics of the main beneficiary include age, age at entry into DI, gender,

canton of residence, marital status, number of minor children, nationality, degree and type of disability (four categories: congenital, accident, mental, and other illness). Since neither information on the application process nor on the outsiders (i.e. non DI beneficiaries) is available, any analysis on the inflow into the DI (e.g. benefit denial rate or effect of self-screening) is not possible.

In the analysis I exclude individuals older than 55 at the time of their entry into DI as their labor supply behavior might be affected by the additional option to enter (early) retirement.¹ In addition, I also disregard people living not in Switzerland since they face different labor markets and institutions. The third group excluded are beneficiaries of an extraordinary pension because they may have a significantly different employability. These people receive a pension although they do not fulfill the requirement of having contributed to the system for at least a year because their working incapacity had been limited before they reached the working age. Table A1 in the appendix gives an overview of the sample selection.

4 Empirical strategy

4.1 Identification

In order to identify the effect of the partial benefit cut in 2008 on the labor supply of existing AP beneficiaries I use a difference-in-differences (DiD) approach. I compare the labor supply of the treatment group before and after 2008 with the labor supply of the control group before and after 2008. The

¹The statutory retirement age in Switzerland for men is 65, while it changed for women from 63 to 64 in 2005. The reason why many years before the statutory retirement age are excluded lies in the way treatment groups are defined (see next section).

treatment group receives the AP before 2008 but not after January 1st, 2008, while the control group never receives an AP in both periods. Formally,

$$D = \begin{cases} 1 & \text{if } AP_{t < 2008} = 1, AP_{t \geq 2008} = 0 \\ 0 & \text{if } AP_{t < 2008} = 0, AP_{t \geq 2008} = 0 \end{cases}.$$

Whether a beneficiary is in the treatment group depends on the years spent in DI. Before the year of the abolition in 2008, married DI beneficiaries with more than four years in DI started to receive DI benefits before 2004, the year when the 4th revision was enacted, so they received an AP up to January 1st, 2008. These beneficiaries therefore belong to the treatment group. On the other hand, married DI beneficiaries with less than four years in DI entered DI after 2004, so they never received an AP. These beneficiaries therefore belong to the control group. Formally, the treatment effect is

$$\begin{aligned} \theta(x) = & E(Y|X = x, T = 1, YDI > 4) - E(Y|X = x, T = 0, YDI > 4) \\ & - [E(Y|X = x, T = 1, YDI < 4) - E(Y|X = x, T = 0, YDI < 4)] \quad (1) \end{aligned}$$

where T is 1 if the year of observation is 2008 or later and 0 if the year of observation is before 2008. YDI indicates the years in DI and X are covariates.

The main identifying assumption is that the trend in labor supply of the control group represents the counterfactual situation of the treatment group

if the AP was not abolished in 2008 (common trend assumption):

$$\begin{aligned} & E(Y^d|X = x, T = 1, YDI < 4) - E(Y^d|X = x, T = 0, YDI < 4) \\ &= [E(Y^d|X = x, T = 1, YDI > 4) - E(Y^d|X = x, T = 0, YDI > 4)] \quad (2) \end{aligned}$$

where Y^d indicates the potential outcome.

Rather than using years in DI (YDI) as a parameter to separate the treatment from the control group, it might be more straightforward to use the year in which the person started to draw a DI benefit. In that sense, members of the treatment group would be beneficiaries who entered DI in 2003 or earlier, while members of the control group would be beneficiaries who entered DI in 2004 or later. These persons would then be followed over the years in a panel structure. However, identification then fails due to a non-linear decrease in the labor force participation of DI beneficiaries. Figure A1 in the appendix plots the employment rates for the treatment (entered DI in 2003) and control group (entered DI in 2005) over time. This definition might look attractive as the two lines converge after 2007 when the treatment group loses the AP. However, as Figure A2 in the appendix demonstrates, the effect which would be attributed to the reform is only a non-linear time trend. In Figure A2 the variable on the x-axis (December of year in Figure A1) is replaced by the years in DI. The employment rates of DI beneficiaries decrease in a similar non-linear way over time for both groups. This pattern is similar when other years are used for the definition of the groups.

Instead, if the years in DI are used to separate the treatment from the control group, I use cohorts in repeated cross-sections observing in every

year different beneficiaries. When using this treatment group definition, group substitution needs to be taken into account depending on the year of observation. If, for example, I define the treatment group to include married DI beneficiaries with five years in DI, I can only use these beneficiaries in the years 2008 and before. In the year 2009 and in later years, beneficiaries with five years in DI entered DI in 2004 or later and thus never received an AP. Similarly for the control group defined as persons with three years in DI: These beneficiaries can only be used from 2007 and after. Before 2007, beneficiaries with three years in DI entered DI before 2004 and were therefore eligible for the AP. This control group would then no longer be untreated. Taking these two groups together, I can only use the years 2007 and 2008 (to observe the outcome variable), rendering for each group one observation prior and one after the policy change. For all other years there is no counterfactual group available (i.e. either both groups receive the AP or both groups have never received the AP).

By altering the years spent in DI for the definition of treatment and control group I can access more pre- and post-reform years of observation. On the one hand, using beneficiaries with, for example, six years in DI instead of five allows me to additionally use the year 2009 as a post-reform year. On the other hand, altering the control group using beneficiaries with two years in DI instead of three opens the year 2006 as an additional pre-reform year. Beneficiaries with nine or more years in DI do not increase the number of post-reform years of observation since data from 2012 and later are not available. However, these persons might also have very different stocks of human capital and other unobservable characteristics than beneficiaries with

a shorter spell in DI. For this reason, all beneficiaries with more than eight years in DI are excluded from the analysis.

Observations from beneficiaries with two years in the DI and even more so those with one year in DI need special attention. The year in which a beneficiary shows up for the first time in the data may not correspond to the year in which she actually enters DI in the sense of receiving payments. The reason is that payments are made retrospectively up to the time of application which is not observed in the data. The age at entry into DI allows to identify in which year the applicant started to receive benefits. In that sense, observations after one year or even two years are incomplete.² As Table A2 in the appendix shows, beneficiaries who appear in the data one or two years after the start of their spell are significantly different in background characteristics from beneficiaries who are observed after three years or later. Therefore, I use beneficiaries with one or two years in DI as a control group in robustness checks only.

Analytically I estimate regressions of the following type:

$$y_{it} = \alpha + \beta Treat_{it} + \gamma Post_t + \delta(Treat_{it} \times Post_t) + X'_{it}\theta + \epsilon_{it} \quad (3)$$

where y_{it} is the outcome variable of interest for person i at time t . $Treat$ is an indicator for the treatment group. In the baseline regression, $Treat$ is 1 if the main beneficiary has drawn DI benefits for five to eight years. $Treat$ is 0 if this period is equal to three years. t is then $\in \{2007, 2008\}$. $Post$ is an

²There are only few beneficiaries who appear in the data for the first time more than three years after their estimated start of the spell. For example, from all beneficiaries who entered DI in 2005 less than 12% appear in the data for the first time more than three years later.

indicator for the year of observation (1 if 2008 and after; 0 before 2008), while X is a set of covariates, which include age, gender, foreigner status, number of minor children, cantonal dummies, and a dummy for mental illness. The coefficient of interest is always 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 persons spent on the DI rolls.

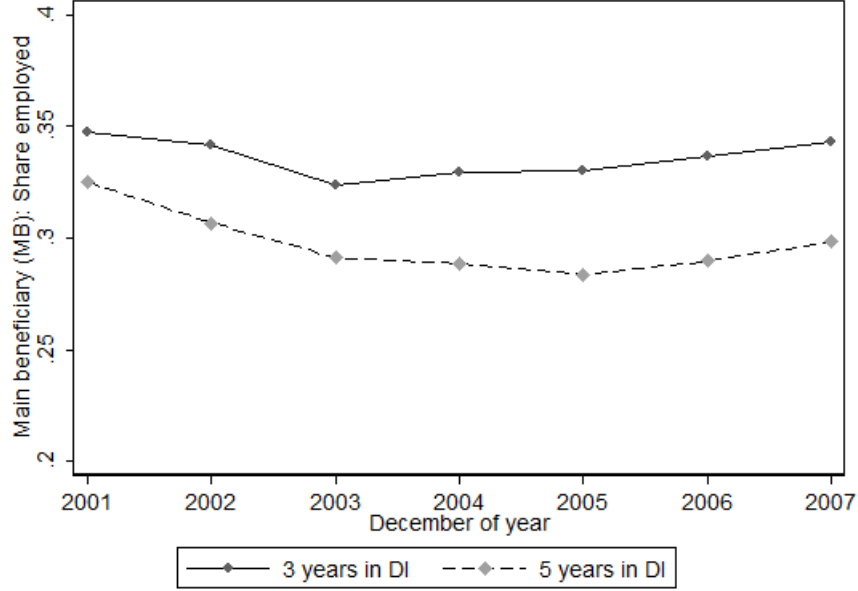
4.2 Plausibility of identifying assumptions

The main identifying assumption is that cohorts with similarly long time on the DI rolls have common labor market outcomes over time. Although this **common trend assumption** is not testable, it may gain credibility if a parallel trend prior to the reform can be observed. Figure 2 plots the employment rate of the main beneficiaries with five and three years in DI prior to the reform. The two lines decrease similarly in 2002 and 2003 and slowly recover thereafter. The two groups seem to be affected in the same way by the small recession in Switzerland after the burst of the dot-com bubble.

Another way to check the plausibility of the common trend assumption is to run placebo tests at a random point in time other than at the time of the implementation of the reform or with an unaffected group. A significant difference between the treatment and control group after this artificial policy reform would cast serious doubt on the causality of the real reform. The results of this test are discussed in section 5.3.

If the placebo date is chosen close to the date of the actual policy change, a significant difference could also be viewed as an **anticipation** effect. In

Figure 2: Employment rate of main beneficiary prior to the policy change



anticipation of the reduction in benefits, beneficiaries would start to change their labor supply even before the policy is implemented. However, this is not very likely as the transposition date was announced only three months earlier (see section 2.2). In any event, if there was any anticipation, it would result in a downward bias of the effect.

The source of another anticipation effect could be the long timespan between the announcement of the 4th revision and its implementation (see Figure 1), which I use for the definition of the treatment and control group. These groups would be confounded if individuals selected into treatment by bringing their DI application forward prior to January 1st 2004 (in order to still benefit from the AP). If selection into treatment was substantial, a significant spike in the inflow into DI should be observed in the years before 2004 and a drop-off thereafter. However, as Figure A3 in the appendix shows, the inflow

decreased from the start of the observation period in 2001. Although the decline is steeper after the revision, it is rather reflecting a general trend since the decrease is similar in the group of divorced beneficiaries which are not affected by the policy change (see picture on the right of Figure A3).

As a second check for selection into treatment, I compare the observable characteristics of beneficiaries who entered DI just prior to the policy change with those who entered just after. If the characteristics significantly differ, this would be a strong sign for an anticipating reaction of some part of the population. Yet, as Table A3 in the appendix shows there are no significant differences between beneficiaries who entered DI in 2003 with those who entered one year later, for whom no AP was granted.³ The same pattern emerges when a non-affected group (divorced) or an earlier or later cohort of married beneficiaries is compared. Although selection into treatment cannot be fully ruled out, all these analyses reveal that it is not likely to be a relevant confounder of the results.

5 Results

5.1 Main results

Table 1 reports summary statistics of the sample used for the baseline evaluation. Each variable's mean is grouped by treatment status and compared before and after January 1st 2008, the date of the policy change. The treatment group includes beneficiaries who have drawn DI benefits for five to eight years, while members of the control group have spent three years on the DI

³The only exception is mental illness. However, the share of persons with a mental illness is lower before the policy reform. Under the assumption that mental illness is easier to simulate, this share should be higher if selection into treatment was present.

rolls. Panel A shows the effect of the revision on various outcome variables. The average yearly amount of the AP, which was canceled, is 3508 CHF (1 CHF = 0.609€ in January 2008).⁴ While the amount of the ordinary DI benefit and in combination the degree of disability also decreased significantly in the treatment group, these two outcomes decreased in a similar way in the control group. Both the employment rate and earnings of the main beneficiary and the spouse increased in both groups. 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. If the couple passes a means test, it can request supplementary benefits (SB). Some DI beneficiaries might additionally qualify for helplessness allowances (HA) if they need assistance to perform activities for daily living. However, comparing the two groups before and after the policy change the same pattern as for the employment effect can be observed for the additional social insurance payments. As a side effect it is interesting to see whether the abolition of the AP led to an increase in the dissolution of marriages. However, the separation rate is rather constant over the period for both groups.

Panel B of Table 1 reveals whether the groups differ in observable background characteristics. Remember that every of the four columns represents different individuals. Except for the fact that the share of people with a mental illness has significantly increased in 2008 in the treatment group compared to the control group, changes between the two periods in all other characteristics are not significant.

⁴Note that 7% of beneficiaries in the control group also received an AP in 2007. This is because the age at entry into DI is not precise in the administrative data but estimated only.

Table 1: Descriptive statistics by treatment status

	Treated (5-8 years in DI)		Control (3 years in DI)		
	Before 2007	After 2008	Before 2007	After 2008	Difference in Differences
A. Outcomes					
Yearly amount of AP in CHF	3508	0***	339.1	0***	-3169†††
Yearly amount of DI in CHF	16238	15602***	15230	14638***	-43.89
Degree of disability	76.56	76.01***	74.16	73.73	-0.130
Outflow from DI	0.036	0.038	0.019	0.024*	0.003
Main beneficiary (MB): Employed	0.277	0.282	0.343	0.359	-0.010
MB: Yearly earnings in CHF	6518	6709	8441	9403**	-771.8†
Spouse: Employed	0.646	0.659***	0.754	0.758	0.010
Spouse: Yearly Earnings in CHF	35473	36442**	40684	40921	733.1
Household (HH): Share receiving Supplementary Benefits	0.166	0.181***	0.137	0.146	0.006
HH: Yearly amount of Supplementary Benefits in CHF	3191	3439***	2718	2867	98.63
HH: Share receiving Helplessness Allowance	0.045	0.048	0.051	0.051	0.003
HH: Yearly amount of Helplessness Allowance in CHF	34.40	35.91	37.01	38.32	0.206
No longer married	0.067	0.067	0.029	0.026	0.003
B. Covariates					
Age	51.78	51.90*	48.90	48.93	0.086
Female	0.441	0.442	0.427	0.419	0.010
Foreigner	0.432	0.434	0.428	0.426	0.004
Number of minor children	0.729	0.720	0.883	0.891	-0.016
Mental illness	0.333	0.346***	0.378	0.369	0.022††
N	30,024	29,248	5341	4290	68,903

Notes: All amounts in CHF are adjusted for inflation. ***, **, *, : statistically different from *Before* at 1, 5 and 10 percent, respectively. ^{†††}, ^{††}, [†]; statistically different at 1, 5 and 10 percent, respectively.

I now turn to the regression framework outlined in section 4.1. Results from the OLS estimation of equation (3) are summarized in Table 2. The dependent variable is the employment rate of the main beneficiary. The top panel of the table reports the results from the baseline regression where treatment and control groups are defined as in Table 1. Remember that in this setup only one post reform year can be observed. If beneficiaries with five years in the DI are excluded from the treatment group, an additional post reform year becomes available. The first column of the second panel of Table 2 therefore repeats the same regression for the diminished sample as a consistency check, while the second column now reports the result for the post period of 2008 and 2009. The third and fourth panel decrease the treatment group each by another year, respectively, so even more long-term effects can be analyzed. All regressions include the covariates mentioned, which are not reported due to space constraints. Overall, the effect on the employment rate of the main beneficiary is close to zero, which is rather consistent for the different treatment groups and over time.

Although there is no effect on the employment rate on the extensive margin, effects on earnings may partly reflect changes on the intensive margin. Table A4 in the appendix therefore reports the same analysis for yearly earnings as the dependent variable.⁵ Again, the results are consistent and not significantly different from zero.

⁵Earnings are set to 0 if the person is not working. Results are similar when only persons who work are considered. Furthermore, all money amounts are adjusted for inflation.

Table 2: Impact on employment rate of main beneficiary

Dependent variable:	Number of years post reform			
MB: Employed	1 (2008)	2 (2008-09)	3 (2008-10)	4 (2008-11)
Treated: 5-8 years in DI - Control: 3 years in DI				
Treat $\times$ Post	-0.007 (0.010)			
Treat	-0.072*** (0.007)			
Post	0.014 (0.009)			
Mean (Treated)	0.277			
N	68,903			
R ²	0.090			
Treated: 6-8 years in DI - Control: 3 years in DI				
Treat $\times$ Post	-0.006 (0.010)	-0.009 (0.009)		
Treat	-0.082*** (0.007)	-0.082*** (0.007)		
Post	0.014 (0.009)	0.014* (0.008)		
Mean (Treated)	0.269			
N	54,475	79,491		
R ²	0.093	0.089		
Treated: 7-8 years in DI - Control: 3 years in DI				
Treat $\times$ Post	-0.006 (0.011)	-0.009 (0.009)	-0.014 (0.009)	
Treat	-0.090*** (0.007)	-0.090*** (0.007)	-0.090*** (0.007)	
Post	0.014 (0.009)	0.014* (0.008)	0.018** (0.008)	
Mean (Treated)	0.264			
N	39,349	57,628	74,494	
R ²	0.095	0.090	0.086	
Treated: 8 years in DI - Control: 3 years in DI				
Treat $\times$ Post	-0.007 (0.012)	-0.011 (0.010)	-0.015 (0.010)	-0.016* (0.009)
Treat	-0.097*** (0.008)	-0.096*** (0.008)	-0.096*** (0.008)	-0.096*** (0.008)
Post	0.014 (0.009)	0.014* (0.008)	0.019** (0.008)	0.015** (0.007)
Mean (Treated)	0.262			
N	24,180	35,008	45,247	54,302
R ²	0.097	0.094	0.089	0.085

Notes: All regressions include covariates (age, gender, foreigner status, number of minor children, cantonal dummies, and a dummy for mental illness). Number of pre-reform years is always one (2007). Robust standard errors in parentheses. ***,*: statistically different at 1 and 10 percent, respectively.

5.2 Reasons for no reaction in labor supply from main beneficiary

One reason for a missing reaction in labor supply from the main beneficiary may lie in the peculiarity of the DI benefit system. The amount of benefits depends on the degree of disability, which is determined by the difference of previous earnings without the disability and the hypothetical as well as reasonable earnings with a disability in any job as a fraction of the previous earnings. The type of DI pension is grouped into four categories determined by sharp thresholds of the degree of disability. For example, a beneficiary with a disability degree of 50% receives half a pension, while someone with a degree of 49% receives only a quarter pension, i.e. 50% less. The former therefore loses half of the pension if the labor supply is only marginally increased (thereby reducing the degree of disability). The abolition of the AP might therefore have a different effect on beneficiaries directly at the edge of the step compared to beneficiaries just beneath the step for whom a marginal increase in labor supply would not lead to a reduction in DI benefits. However, a stratified analysis does not reveal significant differences in earnings between beneficiaries with a degree of disability just above and below the thresholds (results are not reported).

The AP abolition might have different effects depending on the background characteristics of the beneficiaries. Moral hazard might be more present in hard-to-diagnose cases like mental illness or for beneficiaries with only a partial disability. Similarly, beneficiaries with helplessness allowance may have a more limited working capacity so that they could not adapt their labor supply, while beneficiaries without may very well do so. To check

for effect heterogeneities I split the sample into two groups estimating the treatment effect for them separately. However, there is no effect heterogeneity neither between people with a full benefit and persons with partial benefits nor between beneficiaries with or without a mental illness. There are also no significant differences between beneficiaries below and above the median income or between beneficiaries with or without helplessness allowance (results are not reported).

Another reason for a lack of reaction in labor supply from the main beneficiary might be that the partial income loss of the household is compensated by an increase in labor supply from the healthier member of the household. Tables A5 and A6 in the appendix report the results for the employment rate and earnings of the spouse, respectively. However, there is again no significant difference between households who lost their AP and those who did not. The lack of reaction from the spouse could be due to the already high labor force participation rate prior to the revision. The baseline employment rate is more than twice as high for spouses than for the main beneficiary (about 64% vs. 27%). Yet, an analysis stratified by the spouse's previous earnings does not show that spouses who had earnings below the median prior to the revision reacted differently than spouses with earnings above the median (results are not reported).

If both members of the household did not react to the partial benefit cut by altering labor supply, households may react by applying for supplementary benefits (SB). SB are granted if the household passes a means test. Table A7 in the appendix shows the effect on the share of DI beneficiaries additionally drawing SB. While the change in this share is close to zero in all different

treatment groups in the year after the abolition, the effect is stronger and becomes significant in the years after 2008 as the screening of the application may need time.⁶ Comparing these effects to the baseline share before the policy change of about 17%, they are sizeable. The increase of 1.5 percentage points corresponds to an increase in the share of about 9%. There is no effect on helplessness allowance (results not reported), which is expected as applicants in order to be eligible for it, would need to prove a sudden change in health status newly requiring assistance in performing daily activities.

The impact on the share of beneficiaries receiving SB could also be heterogeneous with respect to the background characteristics of the beneficiary. In fact, the positive effect seems to be almost entirely driven by foreigners (see Table A8 in the appendix replicating Table A7 for foreigners and non-foreigners). A possible explanation may be that foreigners do not have the same social network as natives on which they can rely on if they risk of falling into poverty.

There could be more spillover effects into other social insurances, for which information in the data is lacking. 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 abolition if it has been previously capped to prevent overcompensation. Such an increase would potentially inhibit an effect on labor supply. The data do not include any information on benefits from the second pillar of social insurance. Information from the Swiss Household Panel, a representative data set of

⁶Statistical significance is not upheld when only beneficiaries with eight years in DI are used as the treatment group but the size of the effect is hardly reduced.

the Swiss population, reveals that about a third of all DI beneficiaries draw an additional pension from an occupational insurance (OI) plan. To get a sense of how many OI benefits increased due to the abolition of the AP, I contacted two insurance companies which had approximately 350 beneficiaries listed with an AP from DI on their rolls by January 1st, 2008. Since no single benefit has been revised, significant confounding effects through the second pillar are very implausible. Spillover effects into unemployment insurance (UI) can not be assessed with the data, either. However, beneficiaries need to have contributed to the system prior to claiming benefits. Therefore it is also rather unlikely that the inflow into UI considerably increased for the beneficiaries losing the AP.

5.3 Robustness checks

The results so far may be driven by the particularity of the control group which is restricted to include beneficiaries with three years in the DI. As mentioned in section 4.1, beneficiaries with one or two years in the DI have been excluded because they seem to differ significantly in background characteristics from later inflows. However, as a robustness check the upper panel of Table A9 in the appendix reports employment effects from regressions with control groups which include beneficiaries with two to three or one to three years in DI. For the sake of comparison, the first column reports the baseline estimate from Table 2 when only beneficiaries with three years in DI are used as a control group. Remember that increasing the number of years in DI in the control group allows for including more than one pre-reform year. The effects are

very similar for both control groups and with more pre-reform years. Results with other treatment groups (six to eight, seven to eight, and eight years in DI) are not reported as the results are very similar as well. The lower panel of Table A9 repeats the same exercise for the share of beneficiaries with supplementary benefits. The effects are slightly bigger and significant if the control group is increased and more pre-reform years are included.

As mentioned in section 4.1, placebo tests, either with an unaffected group or at a random point in time other than at the time of the implementation of the reform, may help to substantiate the plausibility of the identifying assumptions of the DiD approach. If 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 have different outcomes at another point in time or from an unaffected group, the difference between the years 2007 and 2008 might not be caused by the revision but is rather reflecting a general trend. Table A10 in the appendix reports placebo treatment effects for a policy change that would have abolished all remaining AP by January 1st, 2006 using various treatment groups. The effects are not significantly different from zero for any of the three outcomes (employment rate of main beneficiary or spouse, share with SB). Note that only one post-reform year (i.e. 2006) can be observed because more years would interfere with the true reform. Members of the artificial control group with three years in DI in 2007 would differ from previous years since they would have never had access to an AP.⁷ This issue does not evolve when using another group which was not affected by the policy change, namely divorced beneficiaries. The upper (lower) panel of Table A11

⁷Note that in this placebo test both groups should receive an AP at all times.

in the appendix therefore reports treatment effects as in Table 2 (A7) but for divorced beneficiaries so that neither the treatment nor the control group has ever drawn an AP. The effects for both outcomes (employment rate and share with SB) switch sign but are not significant. Since divorced beneficiaries have no spouse by definition, I cannot run placebo regressions for the outcomes of the spouse.

The two revisions of the DI act included other changes which could potentially interfere with the analysis of the abolition of the AP. The 4th revision included a duplication of Helplessness Allowance as well as the introduction of medical screening services and of the three-quarter pension. Helplessness Allowance is granted when a person needs help in performing daily activities. Since the amount was also duplicated for existing beneficiaries, both treatment and control group are affected by this change in the same way. The medical screening services may have an effect on the inflow after its introduction. The introduction of the three-quarter pension, where some beneficiaries received more benefits while some received less, could have an effect on the inflow and on the labor force participation. However, while differences in background characteristics at the time of inflow have already been analyzed and ruled out (see Table A3), it is highly unlikely that differences in labor supply would emerge exactly four years after the revision and therefore coincide with the 5th revision. Concerning other changes within the 5th revision, additional incentives for reintegration into the labor market were introduced. Yet, these incentives are available unconditional on the time in DI so that treatment and control group were affected by this change in the same way. The remaining changes applied only to the inflow into DI, which is no longer an issue when

focusing on existing beneficiaries.

6 Conclusion

A partial benefit reduction for existing DI beneficiaries in Switzerland has not been successful in raising neither the employment rate nor the earnings of married beneficiaries. Similarly, there is no reaction in employment or earnings from the other healthy member of the married couple. If anything, this policy change may have led to spillover effects into other social insurances as some beneficiaries newly qualify for additional means-tested supplementary benefits.

These results are in contrast to the results from studies which look at the effect of tighter eligibility criteria on applicants and find significant increases in employment rates. The reason why the effects are not symmetric for existing beneficiaries may be twofold: (i) The beneficiaries' stock of human capital cannot be maintained on a level to still find employment after some years on the DI rolls. (ii) But the fact that not even the healthy person of the couple reacts may call for a different explanation. For the spouse the partial benefit cut is a pure income effect. DI benefits of the couple are unaffected by an increase in the labor supply of the spouse. These couples may thus value leisure much more than their working peers (Autor and Duggan, 2007). Another explanation could be that there are similar demand side effects so that spouses are willing to increase their labor supply but fail to find employment.

The policy implication from this study is that partially cutting DI ben-

efits of existing beneficiaries is not an effective measure to increase their labor supply or outflow from DI rolls. When reforming one social insurance, policy makers need to take into account possible spillover effects into other (non-)concurrent social insurances.

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Appendix

Figure A1: Employment rates of alternative treatment and control group

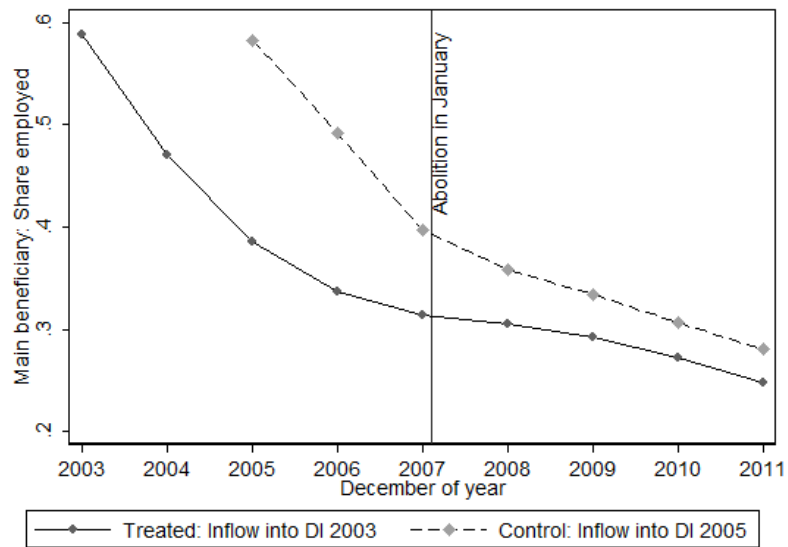


Figure A2: Employment rates of alternative treatment and control group with alternative x-axis

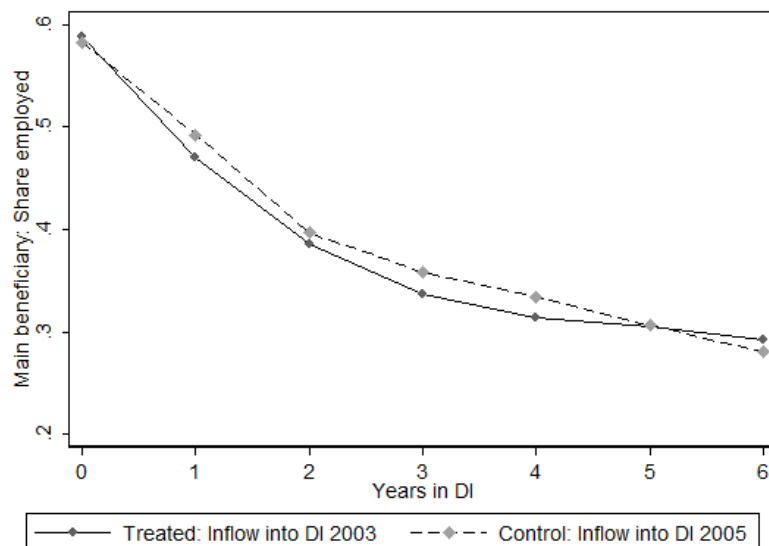
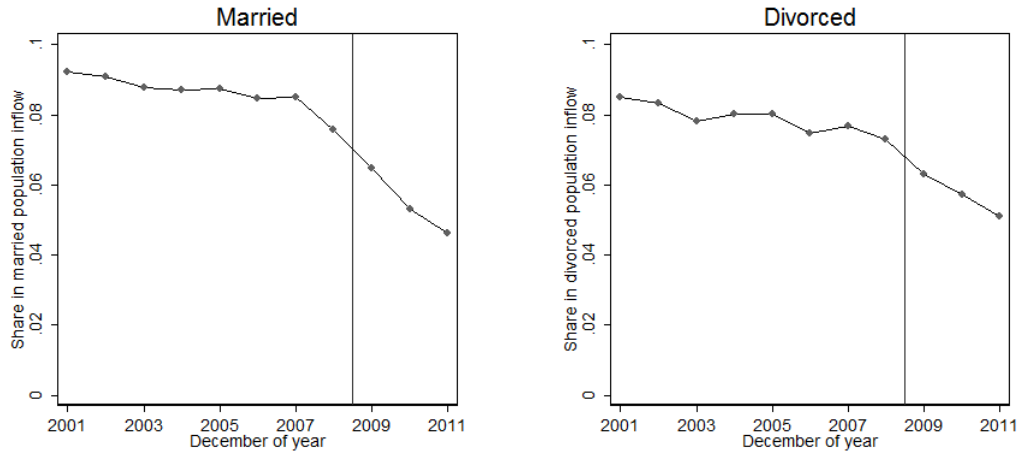


Figure A3: Share of inflow into DI of married and divorced beneficiaries with five years in DI



Notes: The time of inflow should not be measured before the beneficiary spent at least three years in DI (see the discussion about Table A2 in section 4.1). The y-axis therefore reports the following ratio: $\frac{\text{Married (Divorced) population with 5 years in DI}}{\text{Married (Divorced) population in DI}}$. If selection into treatment was present, the inflow should increase prior to the year 2009 because 2008 is the last year when people with five years in DI entered DI in the regime where they still receive the AP (before 2004).

Table A1: Sample selection of DI beneficiaries

Beneficiaries entering DI	before 2004	2004-2007
Total	346,604	74,714
Non-married	177,358	37,509
Married	169,246	37,205
Older than 55	31,239	13,695
55 and younger	138,007	23,510
Living abroad	28,966	3,476
Living in Switzerland	109,041	20,034
Extraordinary Benefits	1,765	59
Ordinary Benefits	107,276	19,975

Notes: All characteristics are measured in the year of first appearance in the data.

Table A2: Background characteristics by time of appearance in the data

Year of inflow	2005			2006			2004	2003	2002
Year of first appearance	3 years or later	within 2 years	Difference	3 years or later	within 2 years	Difference	Difference	Difference	Difference
Age at entry	43.51	45.33	1.83***	43.72	45.12	1.39***	2.04***	1.65***	3.11***
Age	48.24	47.45	-0.800***	48.33	47.35	-0.979***	-0.841***	-1.33***	-0.793***
Female	0.434	0.411	-0.024	0.421	0.417	-0.003	0.014	0.023	-0.060***
Foreigner	0.470	0.424	-0.046***	0.443	0.410	-0.032*	-0.064***	-0.057***	-0.044**
Number of minor children	0.950	0.949	-0.001	0.964	0.987	0.024	-0.049	-0.024	-0.051
Mental illness	0.391	0.347	-0.043***	0.392	0.367	-0.025	-0.032**	-0.020	-0.060***
N	1372	3836		1046	3302				

Notes: ***, **, *: statistically different at 1, 5 and 10 percent level, respectively.

Table A3: Descriptive statistics by year of entry into DI

Year of entry and year of observation									
	Married			Divorced			Married		
							2001 vs. 2002	2005 vs. 2006	
	2003	2004	Difference	2003	2004	Difference	Difference	Difference	
Σ	Age	45.93	45.77	-0.159	47.54	47.24	-0.306	0.311	-0.257
	Female	0.402	0.415	0.013	0.604	0.585	-0.019	-0.003	-0.008
	Foreigner	0.402	0.415	0.013	0.182	0.194	0.012	0.031	-0.035
	Number of minor children	0.929	0.920	-0.009	0.480	0.534	0.055	0.032	0.031
	Mental illness	0.286	0.328	0.042**	0.466	0.475	0.009	0.015	0.015
	Main Beneficiary (MB): Employed	0.599	0.567	-0.033	0.610	0.594	-0.159	0.039**	-0.029
	Spouse: Employed	0.814	0.817	0.002	-	-	-	0.014	0.000
	MB: Yearly earnings	14665	16511	1845	14711	15043	331.4	980.8	-3527
	Spouse: Yearly earnings	41802	41639	-163.8	-	-	-	372.9	2009
	MB: Monthly DI benefit	1292	1284	-8.08	1380	1368	-11.6	-13.2	56.7*
	Degree of disability	78.24	77.48	-0.764	78.22	77.50	-0.724	0.614	1.41
	Share with supplementary benefits	0.025	0.026	0.002	0.098	0.078	-0.020	0.002	0.003
	Share with helplessness allowance	0.041	0.040	-0.001	0.019	0.027	0.008	-0.007	-0.000
	N	1302	1112		369	335		1364/1359	904/515

Notes: **, *: statistically different at 5 and 10 percent level, respectively.

Table A4: Impact on earnings of main beneficiary

Dependent variable:	Number of years post reform			
MB: Yrly Earnings	1 (2008)	2 (2008-09)	3 (2008-10)	4 (2008-11)
Treated: 5-8 years in DI - Control: 3 years in DI				
Treat $\times$ Post	-599.9 (411.7)			
Mean (Treated)	6518			
R ²	0.069			
Treated: 6-8 years in DI - Control: 3 years in DI				
Treat $\times$ Post	-600 (418.5)	-395.0 (356.1)		
Mean (Treated)	6339			
R ²	0.070	0.067		
Treated: 7-8 years in DI - Control: 3 years in DI				
Treat $\times$ Post	-547.3 (431.8)	-373.6 (366.7)	-557.3 (346.7)	
Mean (Treated)	6116			
R ²	0.073	0.070	0.070	
Treated: 8 years in DI - Control: 3 years in DI				
Treat $\times$ Post	-407.4 (473.0)	-289.5 (403.0)	-436.8 (380.2)	-349.1 (364.1)
Mean (Treated)	5918			
R ²	0.078	0.073	0.072	0.071

Notes: See Table 2. All amounts in CHF and adjusted for inflation. ***, **, *: statistically different at 1, 5 and 10 percent, respectively.

Table A5: Impact on employment rate of spouse

Dependent variable:	Number of years post reform			
Spouse: Employed	1 (2008)	2 (2008-09)	3 (2008-10)	4 (2008-11)
Treated: 5-8 years in DI - Control: 3 years in DI				
Treat $\times$ Post	0.011 (0.009)			
Mean (Treated)	0.646			
R ²	0.056			
Treated: 6-8 years in DI - Control: 3 years in DI				
Treat $\times$ Post	0.011 (0.010)	0.006 (0.008)		
Mean (Treated)	0.631			
R ²	0.059	0.060		
Treated: 7-8 years in DI - Control: 3 years in DI				
Treat $\times$ Post	0.011 (0.010)	0.008 (0.009)	0.012 (0.008)	
Mean (Treated)	0.615			
R ²	0.065	0.064	0.064	
Treated: 8 years in DI - Control: 3 years in DI				
Treat $\times$ Post	0.007 (0.012)	0.007 (0.010)	0.011 (0.010)	0.019** (0.009)
Mean (Treated)	0.605			
R ²	0.071	0.071	0.069	0.067

Notes: See Table 2. ***, **: statistically different at 1 and 5 percent, respectively.

Table A6: Impact on earnings of spouse

Dependent variable:	Number of years post reform			
Spouse: Yrly Earnings	1 (2008)	2 (2008-09)	3 (2008-10)	4 (2008-11)
Treated: 5-8 years in DI - Control: 3 years in DI				
Treat $\times$ Post	730.5			
	(906.7)			
Mean (Treated)	35473			
R ²	0.130			
Treated: 6-8 years in DI - Control: 3 years in DI				
Treat $\times$ Post	522.2	-414.2		
	(916.0)	(797.9)		
Mean (Treated)	34542			
R ²	0.144	0.139		
Treated: 7-8 years in DI - Control: 3 years in DI				
Treat $\times$ Post	456.0	-569.1	-143.9	
	(954.7)	(829.7)	(777.1)	
Mean (Treated)	33641			
R ²	0.148	0.143	0.135	
Treated: 8 years in DI - Control: 3 years in DI				
Treat $\times$ Post	776.9	-622.9	-238.5	227.9
	(1063)	(925.3)	(866.8)	(837.1)
Mean (Treated)	32942			
R ²	0.155	0.151	0.148	0.144

Notes: See Table 2. All amounts in CHF and adjusted for inflation. ***, **, *: statistically different at 1, 5 and 10 percent, respectively.

Table A7: Impact on share of supplementary beneficiaries

Dependent variable:	Number of years post reform			
Share SB	1 (2008)	2 (2008-09)	3 (2008-10)	4 (2008-11)
Treated: 5-8 years in DI - Control: 3 years in DI				
Treat $\times$ Post	0.006 (0.008)			
Mean (Treated)	0.166			
R ²	0.066			
Treated: 6-8 years in DI - Control: 3 years in DI				
Treat $\times$ Post	0.007 (0.008)	0.012* (0.007)		
Mean (Treated)	0.168			
R ²	0.067	0.068		
Treated: 7-8 years in DI - Control: 3 years in DI				
Treat $\times$ Post	0.009 (0.008)	0.015** (0.007)	0.014** (0.007)	
Mean (Treated)	0.168			
R ²	0.069	0.069	0.070	
Treated: 8 years in DI - Control: 3 years in DI				
Treat $\times$ Post	0.003 (0.009)	0.011 (0.008)	0.011 (0.008)	0.007 (0.007)
Mean (Treated)	0.172			
R ²	0.070	0.070	0.071	0.072

Notes: See Table 2. ***,**,*: statistically different at 1, 5 and 10 percent, respectively.

Table A8: Effect heterogeneities with respect to foreigners

Dependent variable: HH: Share with SB	Number of years post reform			
	1 (2008)	2 (2008-09)	3 (2008-10)	4 (2008-11)
Foreigners				
Treated: 5-8 years				
Treat $\times$ Post	0.018 (0.013)			
Mean (Treated)	0.241			
N	29,754			
Treated: 6-8 years				
Treat $\times$ Post	0.020 (0.014)	0.027** (0.012)		
Mean (Treated)	0.245			
N	23,482	32,932		
Treated: 7-8 years				
Treat $\times$ Post	0.023 (0.014)	0.030** (0.013)	0.023** (0.012)	
Mean (Treated)	0.248			
N	16,882	23,776	29,991	
Treated: 8 years				
Treat $\times$ Post	0.023 (0.016)	0.034** (0.014)	0.028** (0.014)	0.020 (0.013)
Mean (Treated)	0.248			
N	10,337	14,407	18,167	21,441
Non-Foreigners				
Treated: 5-8 years				
Treat $\times$ Post	0.001 (0.009)			
Mean (Treated)	0.109			
N	39,149			
Treated: 6-8 years				
Treat $\times$ Post	0.002 (0.009)	0.005 (0.008)		
Mean (Treated)	0.110			
N	30,993	46,559		
Treated: 7-8 years				
Treat $\times$ Post	0.002 (0.009)	0.007 (0.008)	0.011 (0.007)	
Mean (Treated)	0.109			
N	22,467	33,852	44,503	
Treated: 8 years				
Treat $\times$ Post	-0.010 (0.010)	-0.003 (0.009)	0.004 (0.008)	0.002 (0.008)
Mean (Treated)	0.117			
N	13,843	20,601	27,080	32,861

Notes: Every estimate represents the coefficient of Treat $\times$ Post from a separate regression including covariates. The control group always includes beneficiaries with 3 years in DI, so that the number of pre-reform years is always one (2007). Robust standard errors in parentheses. **, *: statistically different at 5 and 10 percent, respectively.

Table A9: Sensitivity to different control groups

Dependent variable:	Number of years pre reform			
MB: Employed	1 (2007)	1 (2007)	2 (2006-07)	3 (2005-07)
Treated: 5-8 years in DI				
Control:	3 years	2-3 years		
Treat × Post	-0.007 (0.010)	-0.005 (0.008)	-0.005 (0.007)	
Mean (Treated)	0.277			
N	68,903	75,794	116,791	
R ²	0.090	0.093	0.092	
Control:	1-3 years			
Treat × Post		-0.012 (0.007)	-0.009 (0.006)	-0.006 (0.006)
Mean (Treated)				
N		79,676	123,165	169,720
R ²		0.097	0.097	0.096
Dependent variable:	Number of years pre reform			
HH: Share SB	1 (2007)	1 (2007)	2 (2006-07)	3 (2005-07)
Treated: 5-8 years in DI				
Control:	3 years	2-3 years		
Treat × Post	0.006 (0.008)	0.011* (0.006)	0.012** (0.005)	
Mean (Treated)	0.166			
N	68,903	75,794	116,791	
R ²	0.066	0.065	0.064	
Control:	1-3 years			
Treat × Post		0.012** (0.005)	0.016*** (0.005)	0.015*** (0.004)
Mean (Treated)				
N		79,676	123,165	169,720
R ²		0.065	0.064	0.062

Notes: All regressions include covariates. Number of post-reform years is always one (2008). ***, **, *: statistically different at 1, 5 and 10 percent, respectively.

Table A10: Placebo tests with policy change in 2006

Treated	MB: Employed	Spouse: Employed	HH: Share with SB
5-8 years	-0.002 (0.008)	0.004 (0.008)	0.007 (0.006)
6-8 years	-0.004 (0.009)	0.004 (0.008)	0.005 (0.006)
7-8 years	-0.002 (0.009)	0.004 (0.009)	0.008 (0.007)
8 years	-0.014 (0.010)	0.010 (0.011)	0.003 (0.008)

Notes: Every estimate represents the coefficient of $\text{Treat} \times \text{Post}$ from a separate regression including covariates. The control group always includes beneficiaries with 3 years in DI. Robust standard errors in parentheses.

Table A11: Placebo tests with divorced beneficiaries

Dependent Variable	Number of years post reform			
	1 (2008)	2 (2008-09)	3 (2008-10)	4 (2008-11)
MB: Employed				
5-8 years	0.015 (0.018)			
6-8 years	0.017 (0.019)	0.024 (0.016)		
7-8 years	0.011 (0.019)	0.020 (0.017)	0.020 (0.016)	
8 years	0.003 (0.021)	0.013 (0.019)	0.016 (0.018)	0.020 (0.017)
HH: Share with SB				
5-8 years	-0.015 (0.018)			
6-8 years	-0.014 (0.018)	-0.024 (0.016)		
7-8 years	-0.021 (0.019)	-0.026 (0.017)	-0.029* (0.016)	
8 years	-0.003 (0.022)	-0.018 (0.019)	-0.020 (0.018)	-0.023 (0.017)

Notes: Every estimate represents the coefficient of $\text{Treat} \times \text{Post}$ from a separate regression including covariates. The control group always includes beneficiaries with 3 years in DI. Robust standard errors in parentheses. *: statistically different at 10 percent.