EconomiX

Survivors Benefits and Conjugal Behavior. Evidence from the Netherlands

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Abstract

This paper investigates the impact of survivors insurance on marital behavior. We study the 1996 Dutch reform which considerably tightened eligibility rules to survivors' benefits. Exploiting a discontinuity in date of birth eligibility to survivors insurance and using a rich and exhaustive Dutch population administrative dataset, we carry out a regression discontinuity design and we find no evidence of the reform on divorce probability. Exploring possible explanations for our zero-effect result, we study how labor supply responses can compensate the income drop the reform induced. We find a strong increase in the labor force participation of widows after the reform. However, this response does not completely offset the decrease in income generated form the cut in survivors benefits.

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1 Introduction

Survivors benefits (SB) are the benefits served to a survivor following their spouse's death.[1] Several economic mechanisms are at stake when talking about survivors benefits. First, if SB are removed from marriage, marital surplus falls and this induces some married couples to divorce (Persson 2020). Second, SB are served on a monthly basis, during several years, so it can be significant part of life-cycle income. It can in turns affect labor force participation (Böheim & Topf 2021, Giupponi 2019). Third, SB are conditional on individuals characteristics like age or income level. It makes specialisation possible for couples and then can affect spouse's bargaining power (Persson 2020). Given the potential importance of SB schemes in marital behavior, their reforms can have indirect and perhaps unattended consequences over conjugal behavior. This is the question we are studying in this paper.

More precisely, we analyze the causal impact of (losing) survivors insurance on exit from marriage. We exploit the 1996 Dutch survivors insurance reform that tightened eligibility rules to SB (it became conditional on caring for a child), introduced a means test and extended eligibility to partners and cohabitants. Overall, the reform largely cut the amount of SB distributed to survival spouses over the life-cycle. During the phase-in of the reform, individuals born before January, 1st 1950 were partially exempted from the reform. We build on the cohort-based variations in SB levels to study the effect of SB on divorce probability, using a regression discontinuity approach, comparing marital behavior for women born before and after 1950.[2]

We establish the following results. First, we find a precisely estimated zero-effect of the reform on divorce probability. Even when targeting the most impacted women – building a life-cycle indicator, *survivors benefits wealth* (SBW), in order to identify the reform treatment intensity – we find no evidence of an effect of the reform. We then consider potential channels explaining the absence of impact of the reform on marital behavior, focusing on labor supply responses. We provide causal evidence that the decrease in public benefits is partially compensated by private income by means of an increase of labor force participation. We carry out a difference-in-differences analysis and our results suggest a 5.1 percentage point higher employment rate for survivors and a 550 semi-annual euros increase in labor income, corresponding to a 10.8 % and a 11.5 % variations, respectively. We also show a response gradient in wealth, in line with individuals' (in)ability to smooth standards of living at death time.

Literature on the effect of survivors benefits on conjugal behavior is scarce and mixed. Persson (2020) exploits the elimination of SB in Sweden to show a boom in the number of marriages at the

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[1]In the rest of the paper, we say *she* for the survivor and *he* for the deceased because survivors are mostly women, both because of their higher life expectancy and age gap in couples.

[2]Due to reform design, we are not able to clearly identify the causal impact of the reform on marriage behavior. By 1996, most of born around 1950 women who choose to get married have already done so. We nevertheless would have expected a decrease in marriage rate. Indeed, before the reform, only married individuals were eligible to survivors insurance while, after the reform, cohabitants and registered partners are also entitled to these benefits. By 1996, marriage has no comparative advantage over other forms of couples.

time of reform announcement. She shows that couples advanced their marriage date in order to be eligible to SB. She also finds a decrease in marriage rate, a raise of divorce probability and greater assortativeness among couples, in terms of educational attainment. A series of papers evaluate the effect of the 10 years-marriage condition to remain eligible to SB in case of divorce in the USA (Dillender 2016, Goda et al. 2007, Dickert-Conlin & Meghea 2004). They find small or not significant effects on the timing of divorce. Finally, regarding remarriages, Baker et al. (2004) and Brien et al. (2004) show that the removal of remarriage penalty (cut of SB in case of remarriage) had an effect on widows' marriage rate and on remarriage timing, respectively. This question more generally relates to the effect of other social protection schemes on conjugal behaviors, for which the literature is also mixed. Concerning marriage decision, Frimmel et al. (2014) and coauthors exploit a suspension of a cash-on-hand marriage subsidy to show it resulted in a marriage boom and that extra marriages were less stable. Conversely, Bitler et al. (2004) and Fitzgerald & Ribar (2004) find no effect on marriages when exploiting a linked to marriage welfare reform in the USA.

In economics of the household literature focusing on exit from marriage field, quasi-natural experiments mainly relates to divorce reforms instead of reforms that modified marriage surplus. They rely on the fact that a reduction in the cost of divorce affects propensity to divorce for couples (Becker et al. 1977). Wolfers (2006), Friedberg (1998) have, for example, focused on unilateral divorce (*vs.* mutual consent) in the U.S. However, Weiss & Willis (1997, 1993) showed that a one-to-one correspondence exists between divorce probability and the size of the marriage surplus. In other words, a negative shock lowers marital surplus and induces some married couples to divorce. Only Persson (2020) shows an increase in divorce probability, due to survivors insurance elimination.

The last part of the paper relates to another scarce but booming literature studying the effect of SB and their reforms on survivors' labor supply using quasi-natural experiments. Recent papers found sizable effect of SB on labor force participation and income. Fadlon et al. (2019) exploit age discontinuity in the U.S. eligibility to SB and observe a significant drop in labor force participation as the immediate post-shock consequence of receiving SB. Studying an Italian reform introducing a means test largely reducing the amount of SB distributed, Giupponi (2019) shows that the drop in SB was entirely compensated by an increase in labor force participation. Böheim & Topf (2021) also find a large increase in labor force participation for men following to a SB amount reduction in Austria, coupled with a means-testing implementation. Finally, and more directly related to our setting, van der Vaart et al. (2020) study the impact of widowhood on the income position of surviving spouses of the 1996 reform we study. They show that the introduction of a means test and of stricter eligibility conditions positively affected widow(er)s' labor supply. Compared to van der Vaart et al. (2020) we use administrative data on the full universe of the Dutch population that makes it possible to exhibit a strong wealth-gradient in the response to the reform.

Our paper contributes to this nascent literature by providing a clean estimation of a recent and

major reform of survivors benefits, relying on rich and exhaustive administrative data. Exploiting the discontinuity according to date of birth in eligibility, we find no increase on divorce probability. We nevertheless find large increases in the surviving spouse's labor supply suggesting that the social planner may face a trade-off between social insurance, on the one hand, and distorting labor market decisions, on the other.

The rest of the paper is organized as follows. Section 2 presents the data and describes the institutional background, section 3 lays out the identification strategy, section 4 presents the results, we discuss our results in 5 and section 6 concludes.

2   Institutional background

In this paper, we are interested in survivors insurance, that is to say the benefits paid to a survivor spouse, deriving from the pension system's first pillar. In particular, we do not deal with survivors' pensions deriving from occupational pension plans.[3] The 1996 reform tightened eligibility to SB and introduced a means-test. A transition period however allows individuals born before January, 1st 1950 to benefit from the SB even if they do not meet any criteria. Conversely, eligibility to SB was extended to partners and cohabitants.

The public SB system in the Netherlands.   By 1996, married couples, registered partners and cohabitants are eligible to survivors' benefits if they *(i)* care of a child aged less than 18 or *(ii)* suffer from inability to work (at least 45 % disabled). Divorcees are eligible to Anw benefits only if they were eligible at the time of the divorce and at the time of their ex-spouse's death and if the divorce resulted in the payment of alimony. Benefits are cut as soon as the survivor gets AOW-age, or the child gets 18 or the survivor forms a new household. Following a partner's death, the survivor gets 70 % of a gross minimum wage plus 20 % of a gross minimum wage in the presence of a minor child (half-orphan's benefits). For eligible divorcees, the amount of Anw benefits is equal to the alimony if it was less than the amount of the Anw, or the amount of the Anw otherwise. The benefits are means tested: if the survivor has income, it is partly or fully deducted from the Anw benefits. Unemployment, sickness and disability benefits are fully deducted from Anw benefits.[4] Employment income is partially deducted from Anw benefits. Survivors' benefits from other schemes are not deducted. More precisely, survivors benefits amount is equal to the difference between the reference amount and the income, knowing that 50 % of labor income plus 1/3 of the excess is disregarded. In other words, individuals with income over 31/20 of a gross minimum wage do not receive any SB. Figure 1 illustrates the SB means test

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[3]The mandatory Dutch pension system is composed of two pillars: the government provided basic old age pension scheme (50 % of total benefits) and the occupational pensions schemes (30 % of total benefits). A third pillar consists of individual savings for retirement (20% of total benefits).

[4]Until January, 1st 2011, only labor income was deducted from Anw benefits. Nevertheless, there was a transition period and the deduction of other sources of income applied July, 1st 2013. This translated in a sensible (approximately 5,000) decrease of Anw beneficiaries (Doove et al. 2018).

and presents SB amount served to a survivor according to their level of income and whether if they car for a minor child or not.

Figure 1: SB amount in % of minimum wage according to income level
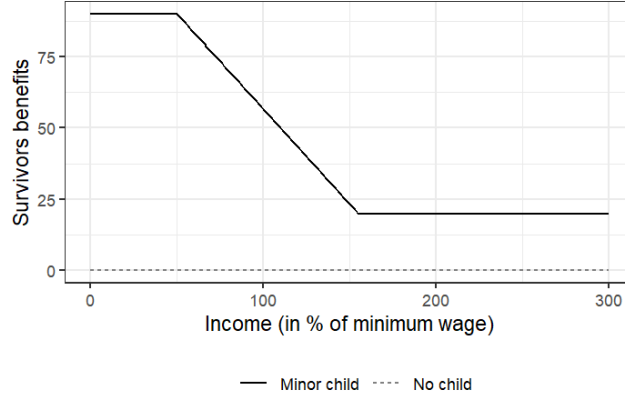
The reform. In 1996, partners and cohabitants became, like married individuals before, eligible to survivors insurance. The 1996 reform has nevertheless made eligibility to SB criteria stricter. While widows aged 40 years old or more were eligible under former scheme, only those caring for a minor child or disabled are now eligible for benefits. However, partial exemption was granted to individuals born before January, 1st 1950. These individuals remained eligible to survivors benefits even if they do not meet any criteria. As of 2015, there are no people left in *transition* since those born on January, 1st 1950 have reached AOW-age (65 for this cohort of birth). The 1996 reform also introduced a means-test. In particular, all beneficiaries became subject to this means-test, included former beneficiaries as well as widow(er)s born before January, 1st 1950. In the following, we do not analyze this latter part of the reform.

In the rest of the paper, we focus on the SB reform that introduced a discontinuity in eligibility, according to survivors' date of birth. Individuals born after 1950 are eligible to SB only if they care for a minor child (table 1). Figure 2 illustrates the change in terms of benefits, according to income level. Table A.1 in appendix A summarizes the differences between pre- and post-reform schemes.

Table 1: Eligibility to SB

	Born before 1950	Born after 1950
Cares for a child	✓	✓
Does not care for a child	✓	✗

Confounding reforms. There are two confounding reforms potentially interacting with our reform of interest, since they (i) impact the same group of cohorts and (ii) can have an effect on employment

Figure 2: SB amount in % of minimum wage according to date of birth

(a) Born before 1950   (b) Born after 1950
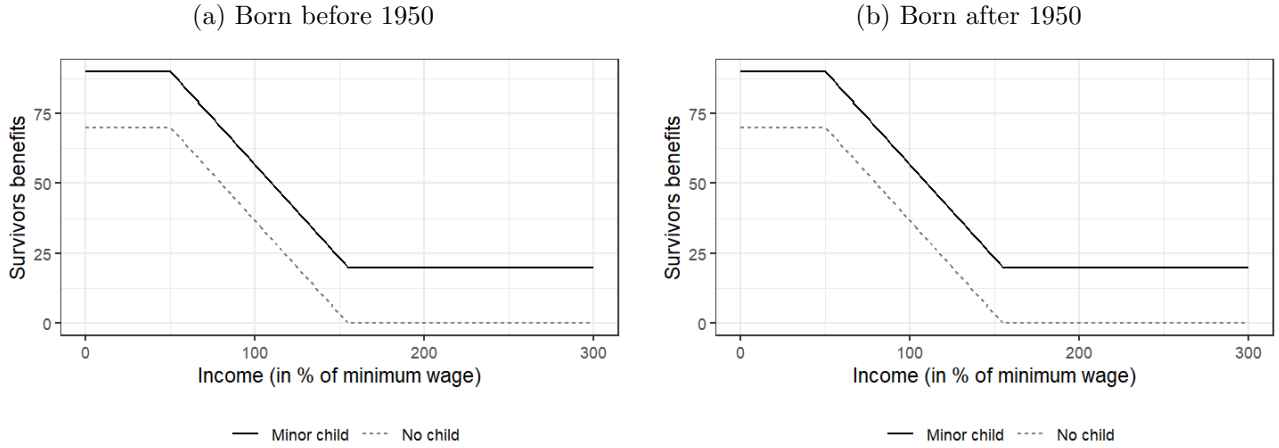
outcomes of women. The first one is the partner pension reform. It is a pension supplement for the older partner who has reached the AOW-age, provided that the younger one had an income below a certain amount. This supplement was removed from April, 1st 2015, the older partner would not receive the supplement any more. Individuals born before January, 1st 1950 were thus exempted. Nagore-Garcia & van Soest (forthcoming) find small but significant effects of the reform on female labor force participation. The second confounding reform relates to the occupational pension reform, which was implemented in 2006 and closed early retirement schemes for individuals born on January, 1st 1950 and after, and then translated into a large negative shock on pension wealth for individuals. As a consequence, it modifies resources composition and labor supply between 60 and pension age. Atav et al. (2021) show this reform translated into a increase in 60 to 65 years old employment rates. These two reforms affect the cohorts with the same discontinuity as the one of our interest. However, if any effect of these reforms on divorce were to be expected (due to the negative income shock), it would be in the same direction as that of the SB reform. But we find no effect. Conversely, with regards to the last part of the paper, these confounding reforms may have increased survivors' labor force participation. However, there is no reason why the effects should occur at the time of the spouse's death.

Marriage market in the Netherlands. The basic principles underlying marriage and divorce in the Netherlands are similar to other developed countries (Kabátek 2018). No-fault divorced was introduced in 1971 and replaced the law granting divorce only on grounds of adultery, cruelty or other pre-specific issues. Registered partnership was introduced in 1998. In 2001, same-sex marriages was legalized and same-sex registered partners became eligible to marry. Conversely, between 2001 and 2009, it was possible for married couples to convert their marriage into a registered partnership. Their partnership could then be annulled without having to go to court and thirty thousand couples

separated through so-called flash divorce ([Loozen & van Huis 2010](#)). Legal distinction between marriage and registered partnership principally concerns children: registered partners are excluded from international adoptions and they do not automatically become legal parents when a child is born to their spouse. Moreover, unlike married couples, registered partners can file for administrative divorce.

3 Identification strategy

3.1 Hypotheses

In order to understand how survivors insurance impacted marital decisions, we make hypotheses on conjugal behavior modifications as a result of the 1996 reform.

Accepted the marriage market theory of Becker, marriage and divorce decisions can be predicted by economic opportunities. Conditional on spouse's death, SB provide benefits to the survivor that renders utility deriving from marriage, called marriage *surplus*. A SB reform modifies the threshold for marriage or divorce decision. In other words, if the surplus deriving from SB and linked to marriage decreases because of a reform, individuals who are at the edge of divorce switch to divorce because, for them utility deriving from marriage becomes lower than utility deriving from divorce. In the following, we focus on married heterosexual women deciding to divorce or remain married.[5]

After the reform, survivors benefits eligibility became conditional on caring for a minor child. The advantage deriving from marriage, to eventually benefit from survivors insurance is therefore lowered after 1996. In other words, the surplus deriving from marriage is, by 1996, eliminated for individuals born after 1950 without children or with too high income. Indeed, eligibility to SB in case of divorce is conditional on being eligible at divorce time. This lead us to our **hypothesis: married women are more likely to divorce under current legislation**.

From a theoretical point of view, our hypothesis depends on whether marriage was also affected by the reform. Indeed, the reform, if it has an effect on marriage rate, changes the pool of cohabitants, partners or married individuals, which in itself has an effect on the average probability of divorce. Following to the reform, if marriage rate decreased, individuals who marry are those who, on average, value the marriage surplus more than those in a situation without reform did. The new married individuals population therefore has a lower risk of divorcing than the population of married people in a situation in which the reform would not have been enacted. In our empirical strategy, we account for this selection process when estimating the effects on divorce probability. More precisely, to test our hypothesis, we focus on individuals that were already married at the time of the reform. It enables us to distinguish the reform effect on divorce probability from the composition effect we have just highlighted.

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[5]We focus on women because widows are mostly women, because of their higher life expectancy and age difference with their spouse.

3.2 Data presentation

The data we use in this paper is administrative data of the universe of the Dutch population. The register data are maintained by Statistics Netherlands (CBS) and cover all residents living in the Netherlands between years 1995 and 2018. We have information on complete individual trajectories, and retrospective data on household histories. Each record contains a unique personal identifier that let us merge datasets and get information on individuals, their civil-status and household histories, their partner's and child's characteristics, their labor, welfare or pension income (from 1999 onward), their sector of activity, wages and hours worked (from 2006 onward), their wealth (from 2007 onward) and the survivors benefits they possibly receive (from 2005 onward). More details can be found on which datasets we have mobilised and how we have combined them to define our population of interest for the analyses in appendix B. Table 2 presents descriptive statistics of the data and variables we use in the paper.

Table 2: Descriptive statistics per cohort of birth

	1947	1948	1949	1950	1951	1952
Total number of individuals	6,162,502	5,831,594	5,670,236	5,600,603	5,545,445	5,739,665
Married at 42 women						
Number of individuals	97,875	91,623	87,698	85,622	84,107	85,975
Age difference with spouse	-2.28	-2.24	-2.29	-2.4	-2.4	-2.48
Marriage duration at 42	18.72	18.73	18.65	18.65	18.54	18.44
Having a minor child (in %)	68.16	68.96	69.87	70.85	71.33	72.43
Income level (in % of min. wage)	48.25	52.66	57.36	60.56	65.19	68.37
Having divorced between 43 and 64	8.32	8.36	8.87	8.99	9.35	9.4
Married in 1990 women						
Number of individuals	97,342	91,623	88,039	86,400	85,075	87,262
Age difference with spouse	-2.27	-2.24	-2.3	-2.4	-2.42	-2.51
Marriage duration in 1990	19.5	18.6	17.63	16.74	15.77	14.82
Having a minor child (in %)	68.09	68.96	69.95	71	71.65	72.81
Income level (in % of min. wage)	48.15	52.66	57.43	60.81	65.52	68.54
Having divorced between 1991 and 2011	7.61	8.26	9.34	10.12	11.02	11.77

NOTE: Income level corresponds to income at 54, younger age at which information is available for the eldest cohort.
SCOPE: Married at 42 years old women and married in 1990 women, respectively.
SOURCE: CBS.

3.3 Empirical strategy

We focus on married women, to analyze the causal impact of survivors insurance on divorce decision.

Cohorts born before January, 1st 1950 were partially exempted from the reform. They remained eligible to SB benefits even if they do not care for a minor child. We run a regression discontinuity design (RDD) exploiting the discontinuity in date of birth in the reform design.

Our variable of interest, divorce probability, depends on year (there is an upward trend over the

period) and on age (see figure B.1 in appendix B). In order to make cohorts comparable, we define two populations and run two analyses each time. The first population is composed of married in 1990 women and we look at their divorce probability over the 1991-2011 period.[6] The second population is composed of married at 42 years old women and we look at their divorce probability between 43 and 64.[7] Our two panels are balanced.[8][9]

Our control group is composed of women born in 1947-1949 and our treated group is composed of women born in 1950-1952. Cohorts are defined on a monthly basis.

Given the nature of our assignment variable (date of birth), it cannot be manipulated and it is smooth around the threshold. We estimate Δ_d the reform effect on Dutch divorce probability.

$$\Delta_d = \mathbb{E}[D^{t_1} - D^{t_0}|T = 1] - \mathbb{E}[D^{t_1} - D^{t_0}|T = 0] \tag{1}$$

where D^{t_0} is divorce probability before the reform, D^{t_1} is divorce probability after the reform and T is the treatment variable, equal to 1 for individuals born after 1949 and 0 otherwise.

In practice, this estimator corresponds to the OLS estimator in the following equation:

$$d_i = \alpha + \beta m(dist_i) + \gamma \mathbf{1}_{T_i=1} + \delta_d m(dist_i) * \mathbf{1}_{T_i=1} + \epsilon_i \tag{2}$$

where d_i is a dummy variable equal to 1 if woman i divorces and 0 otherwise, $dist_i$ is distance to treatment variable, equal to the difference between woman i date of birth and January, 1st 1950, $m()$ is a polynomial function, and T_i is a dummy variable equal to 1 if woman i was born after 1950 and 0 otherwise. δ captures the reform effect on Dutch divorce probability.

In a specification we also include a vector of characteristics that is not necessary for identification but that reduces the standard errors. The control variables we add in the model are age difference between spouses, marriage duration in 1990 (or at 42 years old), a dummy variable indicating whether woman i cares for a minor child in 1990 (or at 42 years old) or not and income level deriving from labor supply.[10][11]

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[6]Year 1990 is chosen as the pre-reform situation because the 1996 reform was discussed a few years before it was enacted. Year 2011 is chosen as corresponding to the year during which the eldest cohort reaches 64 years old.

[7]Age 42 is chosen as corresponding to age reached by the eldest cohort during year 1990.

[8]Legal procedure of a divorce takes time and that we probably account for divorce which were decided before the reform was enacted. Nevertheless, there is no reason why treated individuals (born just after 1950) would show a jump or a drop in divorce probability, compared to control group (individual born just before 1950). Then, we believe that duration of legal procedures does not affect our results.

[9]Divorce probability is defined as the proportion of women who divorced at least once between 43 and 64 years old or over the 1991-2011 period. Over long periods, there may have changes in preferences and behaviors as well as labor market opportunities for individuals. In the following, we provide evidence of the robustness of our results with regards to divorce probability definition. We carry out analyses in which divorce probability is defined as probability to divorce at least once between 43 and 50 years old (for pension reform neutralization) and we find similar results (table C.1 in appendix C).

[10]Income level is equal to income at 52 as the youngest age at each information is available for the eldest cohort of our analysis. We nevertheless believe it is not a problem since our population of interest is aged over 42. Indeed, the salary hierarchy does not change too much from one age to another, after a certain age (Sicherman & Galor 1990).

[11]Descriptive statistics of the control variable for each population of interest are presented in table 2.

4 Results

4.1 Graphical evidence

Figures 3a and 3b give graphical evidence of the effect of the reform in terms of survivors insurance. Figure 3a shows the proportion of SB beneficiaries among widows per cohort. We verify there is a large drop in this proportion from the 1950 cohort of birth. Among widows born before 1950, approximately 75 % benefit from Anw benefits while it is the case of approximately 30 % of born after 1949 widows. As a consequence, the total amount of SB received all widowhood long also show a clear discontinuity according to cohort of birth. As shown on figure 3b, average total amount over life-cycle is approximately equal to 131 thousands of 2019 euros (corresponding to 80 minimum wages) for widows born before 1950 while it varies between 40 and 50 minimum wages for widows born after 1949. Among widows born after 1949, are eligible those who care for a minor child (mainly), leading to higher benefits than widows without children. This explains that the ratio between born after 1949 and born before 1950 widows is lower in terms of amount over life-cycle than in proportion of beneficiaries.

Figure 3: Information on survivors insurance beneficiaires, per cohort

(a) Proportion of Anw beneficiairies among widows

(b) Total SB amount over life-cycle
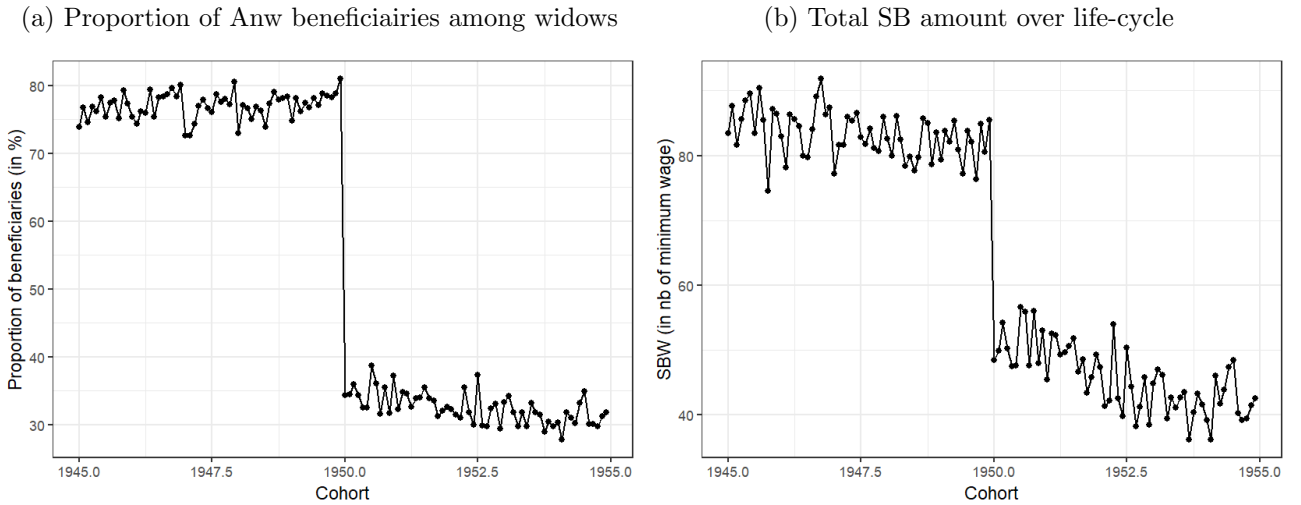
NOTE: SBW stands for survivors benefits wealth, defined as total SB amount over the life-cycle.
To compute SBW, SB amounts were backcasted from the earliest observation (as a proportion of the minimum wage) between the year after the widowhood date and the first year for which an amount is observed.
SCOPE: Female Anw beneficiaries born between 1945 and 1955.
SOURCE: CBS.

Figures 4a and 4b provide graphical evidence of the reform effect on divorce probability. Series of divorce probability according to cohort of birth are represented for the two populations of interest. We see an upward trend over the cohorts, consistent with the upward trend in divorce probability over years. Divorce probability between 43 and 64 years old for women who were married at 42 is equal to 8 % for the 1947 cohort and is equal to 10 % for the 1952 cohort. Divorce probability over the 1991-2011 period for women who were married in 1990 is equal to 6 % for the 1947 cohort and is

equal to 12 % for the 1952 cohort. We do not see any discontinuity at the 1950 cohort threshold.

Figure 4: Divorce probability according to cohort of birth
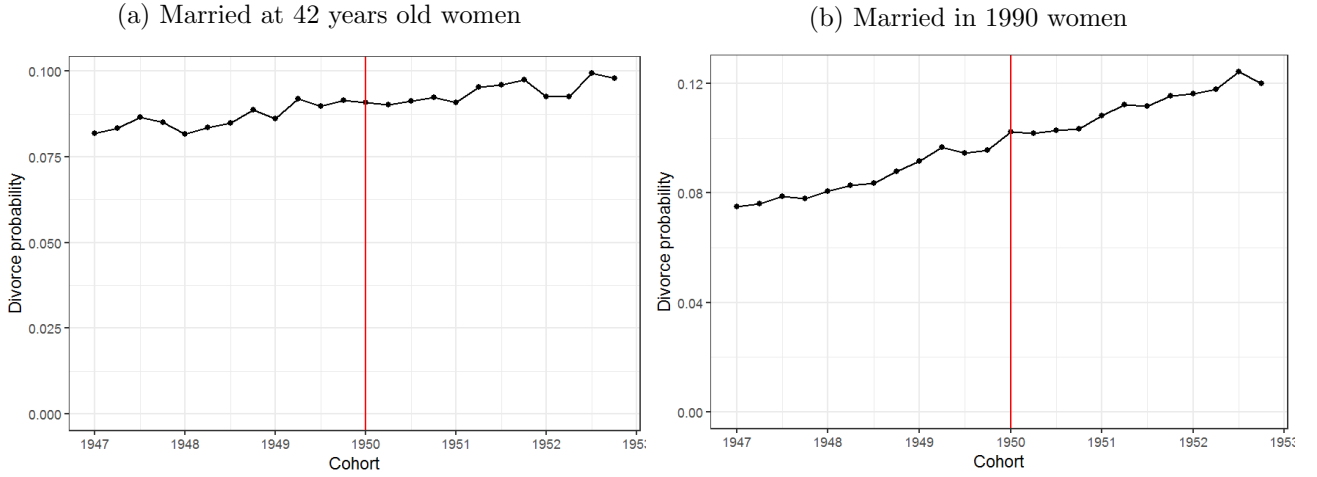
(a) Married at 42 years old women

(b) Married in 1990 women

4.2 Empirical results

RD-graphs are presented on figures 5a and 5b. For each one of the populations of interest, they show divorce probability according to cohort of birth, with confidence intervals to the 5 % significance level. We can see that individuals who were born just before January, 1st 1950 have a divorce probability that is not significantly different from the one of those born just after December, 31st 1949.

Figure 5: RD-graphs for divorce probability
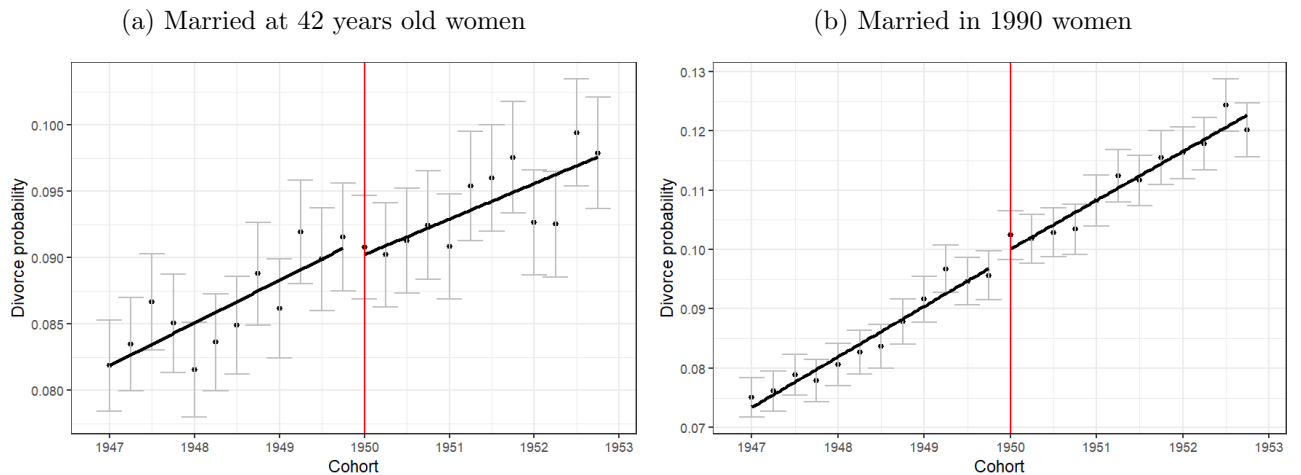
(a) Married at 42 years old women

(b) Married in 1990 women

The results of our estimation of equation (2) are presented in table 3. The different columns correspond to the different specifications we have tested. For married at 42 years old women, RD estimates take values ranging from –0.004 to –0.001, i.e. a decrease in divorce probability between 43 and 64 years old of 0.1 to 0.4 percentage points or a 1.2 to 4.0 % decrease in divorce probability, but all of them are not statistically different from 0. For the married in 1990 women sample, results are similar. RD-estimates vary between 0 and 0.005 according to the specification and all are not statistically different from 0.

Table 3: Regression discontinuity estimates for divorce probability

	Married at 42 years old							Married in 1990						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(1)	(2)	(3)	(4)	(5)	(6)	(7)
δ_d	-0,001	-0,003	-0,004	-0,001	-0,001	-0,001	-0,003	0,001	0,000	0,005	0,002	0,001	0,001	0,001
Std. err.	0,002	0,002	0,003	0,002	0,002	0,002	0,002	0,002	0,002	0,003	0,002	0,002	0,002	0,002
p value	0,467	0,191	0,244	0,536	0,468	0,452	0,182	0,449	0,992	0,151	0,347	0,448	0,475	0,721
Parametric	✓	✓	✓	✓	✓	✓		✓	✓	✓	✓	✓	✓	
Poly. order	1	2	3	1	1	1		1	2	3	1	1	1	
Controls				✓							✓			
Equal slopes					✓							✓		
Time basis	m	m	m	m	m	q	m	m	m	m	m	m	q	m
Nb obs.	525620	525620	525620	524679	525620	525620	525620	528461	528461	528461	528459	528461	528461	528461

NOTE: Control variables are marriage duration (at 42 years old or in 1990), spouses' age difference, a dummy that indicates whether the married woman is caring for a minor child (at 42 years old or in 1990) and normalized income at 52 years old (first age at which we have information for the elder cohort).
Regarding time basis, m stands for monthly basis and q for quaterly basis.
Column 7 correspond to the non-parametric specification, using Imbens & Kalyanaraman (2012) bandwidth.
For married at 42 women, probability of divorce is computed as the probability to divorce at least once between 43 and 64 years old. For married in 1990 women, probability of divorce is computed as the probability to divorce at least once between 1991 and 2011.
SCOPE: Married at 42 years old women and married in 1990 women, respectively.
SOURCE: CBS.

Comparing orders of magnitude in the specification with controls (table 4), the RD-estimate for married at 42 years old women sample is equal to –0.00098 and is comparable in magnitude to the effect of one additional year of marriage duration (0.00085) or three times as lower as an additional comparable in magnitude to the effect of one additional year of difference between spouses (0.00303). Similarly, the RD-estimate for married in 1990 women sample is equal to –0.00154 and is comparable in magnitude to the effect of one additional year of marriage duration (0.00133) or three times as lower as an additional comparable in magnitude to the effect of one additional year of difference between spouses (0.00314). Overall, this makes us confident that substantial effects of the SB reform on divorce probability can be ruled out.

We nevertheless run sensitivity to bandwidth and placebo tests on the non-parametrical specification in order to assess the validity of our results. Figures C.1 and C.2 in appendix C confirm our zero-effect result. We also run RD analysis on divorce probability until age 60 instead of age 65 in order to neutralize effect of confounding pension reform. As shown in table C.1 in appendix C, results also show zero-effect estimates. We nevertheless go further in our analyses and look at robustness and

Table 4: Regression discontinuity estimates with controls

	Married at 42 years old	Married in 1990
δ_d	-0.00098	0.00154
	(0.00158)	(0.00163)
Age difference between spouses	0.00303	0.00314
	(0.00011)	(0.00011)
Cares for a minor child	0.00609	0.00079
	(0.00087)	(9e-04)
Marriage duration	0.00085	0.00133
	(8e-05)	(9e-05)
Income level	0.03157	0.03579
	(0.00044)	(0.00045)
Number of obs.	524,679	528,459

NOTE: Income level is normalized by minimum wage income at 52 years old (first age at which we have information for the elder cohort). For married at 42 women, probability of divorce is computed as the probability to divorce at least once between 43 and 64 years old. For married in 1990 women, probability of divorce is computed as the probability to divorce at least once between 1991 and 2011.
SCOPE: Married at 42 years old women and married in 1990 women, respectively.
SOURCE: CBS.

heterogeneity.

4.3   Minimum detectable effect

In each population, we investigate whether the non-significant result can be interpreted as an absence of link between the SB reform and divorce probability or a lack of power. To this aim, we compute minimum detectable effects (MDE). The MDE of an experiment is the smallest effect that, if true, has a κ % chance of producing an impact estimate that is statistically significant at the α level (Bloom 1995). κ is the statistical power, usually equal to 20 % in the literature and α is the statistical significance, usually equal to 5 % in the literature. In other words, a MDE of x means that with a non-significant coefficient lower than x, we cannot conclude the absence of association between the SB reform and divorce probability.

MDE is equal to an appropriate multiple M of the estimated standard error $\hat{\sigma_\delta}$ of the parameter of interest δ_d (Bloom 1995):

$$MDE = M.\hat{\sigma_\delta} \tag{3}$$

where, in a two-tailed test, M is approximately equal to:

$$M \approx \begin{cases} t_{\frac{\alpha}{2}} + t_{1-\kappa} & \text{if } \hat{\delta_d} > 0; \\ t_{\frac{\alpha}{2}} - t_{1-\kappa} & \text{if } \hat{\delta_d} < 0. \end{cases}$$

where $t_{\frac{\alpha}{2}}$ and $t_{1-\kappa}$ are the $\frac{\alpha}{2}$ and $(1-\kappa)$ quantiles of a Student distribution and δ_d is the SB reform effect on divorce probability.

Table 5 presents the MDE for our two populations of interest, for a two-sided hypothesis test at 5 % significance level and 20 % statistical power. The minimum detectable effect for women married at 42 years old is –0.00177. It is higher in absolute value than our estimate in the specification with control variables ($\hat{\delta}_d = -0.00098$) so the effect is not detectable. The minimum detectable effect for women married in 1990 is 0.00456. It is higher than our estimate ($\hat{\delta}_d = 0.00154$) so the effect is not detectable.

Finally, if there is an effect of the SB reform on divorce probability, it is lower in absolute value than 0.177 percentage point and lower than 0.456 percentage point. We cannot conclude between absence of effect and lack of statistical power. However, our MDE estimates suggest that is there is an effect on divorce probability, it very small in magnitude (equal to a 1.99 % decrease or a 2.48 % increase).

Table 5: Minimum detectable effect on divorce probability

Population	$\hat{\delta}_d$	SE	n	MDE	Variation (in %)
Married at 42 years old	-0.00098	0.00158	524,679	-0.00177	-1.99
Married in 1990	0.00154	0.00163	528,459	0.00456	4.89

NOTE: $\hat{\delta}_d$ is our estimate, SE is the corresponding estimated standard error, n is the sample size, MDE stands for minimum detectable effect and variation gives the corresponding to MDE (in percentage points) divorce probability variation with regards to the last non-treated (1949) cohort of birth.
SCOPE: Married at 42 years old women and married in 1990 women, respectively.
SOURCE: CBS.

4.4 Robustness and heterogeneity analyses

Robustness to treatment intensity. Reform intensity varies from a woman to another according to her income level, age of the child they possibly care of and spouse's probability of death. We assume that intensively treated women will change their behavior with regards to divorce probability consequently to SB reform while others do not. These intensively treated women are not easy to identify. They are those who will not have to care for a minor child for many years, those with low income (for others, benefits are anyway partially or entirely means tested), and those with a high spouse's death probability. We thus choose to adopt a life-cyle indicator, which takes into account each of these three dimensions: we compute women's survivors benefits wealth (SBW).[12]

$$SBW = \sum_{t=t_d}^{AOW\_age} \beta^{t-t_d} . SB_{it}(\tau_w) . S(t_d, t)$$

where SBW_i is individual i's survivors benefits wealth, t_d is husband's date of death, β is the discount factor (97 %), SB_{it} is individual i's SB at time t and $S(t_d, t)$: probability of being alive at t conditionally

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[12]As individuals' income is available from 1999 only, in practice we compute SBW at 52 as it s the age of the elder cohort in 1999. In particular, we do not compute SBW at each age.

on being alive at t_d.

Given the uncertainty of the date of death of one's spouse, we compute $\mathbb{E}[SBW(p)]$, the expected SBW at period p, taking the partner's death probability, according to his age, into account.

$$\mathbb{E}[SBW(p)] = (1 - S_P(p+1, p)). \sum_{t=p}^{AOW\_age} \beta^{t-p}.SB_{it}(\tau_w).S(p, t)$$

where $S_P(t+1, t)$ is the probability for partner to be alive in period $t+1$ conditionally on being alive at t.

We then define treatment intensity (TI) as $TI = SBW_{49}$ - SBW_{50} where:

SBW_{49} is SBW computed with legislation applying to individuals born before 1950 and;

SBW_{50} is SBW computed with legislation applying to individuals born after 1950.

More precisely,

$$SBW_{49it} = \sum_{t=t_d}^{AOW\_age} \beta^{t-t_d}.S(t_d, t). \left(\mathbf{1}_{child_{td}}.cb_t + max \left(0, sb_t - \frac{2}{3} \left(max(0, y_{it} - 0.5MW_t) \right) \right) \right)$$

and

$$SBW_{50it} = \sum_{t=t_d}^{AOW\_age} \beta^{t-t_d}.\mathbf{1}_{child_t}.SB_{it}(\tau_w).S(t_d, t)$$

$$= \sum_{t=t_d}^{t_{cm}} \beta^{t-t_d}.S(t_d, t).\mathbf{1}_{child_{td}}. \left(cb_t + max \left(0, sb_t - \frac{2}{3} \left(max(0, y_t - 0.5MW_t) \right) \right) \right)$$

where $\mathbf{1}_{child_t}$ is a dummy variable indicating whether if woman i cares for a child at t, sb_t is the reference amount of SB for individuals who do not care for a child (70 % of minimum wage), cb_t is the reference amount for half-orphans benefits (20 % of minimum wage), y_{it} is woman i income at t and MW_t stands for minimum wage at t.

Then,

$$TI_{it} = \mathbf{1}_{child_{td}}. \left(\sum_{t=t_{cm}+1}^{AOW\_age} \beta^{t-t_d}.S(t_d, t).cb_t \right)$$

$$+ (1 - \mathbf{1}_{child_{td}}). \left(\sum_{t=t_d}^{AOW\_age} \beta^{t-t_d}.S(t_d, t).max \left(0, sb_t - \frac{2}{3} \left(max(0, y_t - 0.5MW_t) \right) \right) \right)$$

If woman i cares for a minor child at time of death, the decrease in her benefits is equal to the discounted sum of half-orphans benefits received from the first year of child majority until AOW-age. If woman i does not care for a minor child, the decrease in her benefits is equal to the discounted sum of SB perceived all widowhood long. Figures C.3 and C.4 in appendix C give a theoretical representation of treated individuals according to their income and child caring characteristics. As we deal with

expected SBW at period p, women in couple with elder husband are also more treated than those with in couple with a younger one. Figures C.5 and C.6 in appendix C attest of the accuracy of our simulations.

We compute treatment intensity for each woman of our sample at 52 (minimum age at which income information is available for the eldest cohort) and we split the dataset into four groups according to treatment intensity's quartiles.[13] Table C.2 in appendix C shows individual characteristics according to treatment intensity quartile. We verify women are similar from a quarter to another regarding age difference between spouses and marriage duration. Conversely, women in the first two quartiles (the less treated) are more likely to care for a minor child than in the other two quartiles. This is consistent with the fact that SB will stop at child majority. First quartile women have a much higher income level, consistent with the fact they are less treated because means tested anyway.

We run our RD-analysis independently on the four sub-populations with regards to treatment intensity. Figure C.7 in appendix C give graphical evidence of the results. Divorce probability slightly increases over the cohorts of birth but there is no discontinuity around the 1950 threshold, no matter the treatment intensity. There is no graphical evidence of an effect of 1996 reform on married women probability to divorce, no matter treatment intensity. Finally, RD-estimates are presented in table 6. All estimates are close to 0 and not statistically different from 0. Even relatively more impacted women do not seem to react to the reform, meaning that our zero-effect result is robust to reform treatment intensity.

We believe women may respond differently to the reform according to their experience on the labor market. The rationale is that a woman aged over 40 is less likely to participate in the labor market if she had never worked before or had few partial time job or long career interruptions. Thus she will remain in marriage. Unfortunately, we do not have the information to build the entire labor market careers of the population of interest. Nevertheless, one could consider income at 52 as a pretty good proxy of women's labor market integration. As shown in appendix C.2, there is a gradient in treatment intensity according to income level. Thus, we believe that the heterogeneity results with regards to SBW give us information about the responses heterogeneity with regards to women's labor market integration. We conclude that there is no effect of the SB reform on divorce probability, no matter the experience on the labor market.

Heterogeneity according to spouse's activity sector. As the reform only concerns pension system's first pillar, we make the hypothesis that occupational plans have differently compensated the SB loss between sectors. In the private sector, employees were allowed to take out an Anw gap pension insurance when available in their firm.[14] This Anw gap insurance insured the employee's surviving

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[13]First quartile of treatment intensity corresponds to the 25 % of women whose treatment intensity is the lowest while fourth quartile of treatment intensity corresponds to the 25 % of women whose treatment intensity is the highest.

[14]According to the National Remuneration Survey 2014, approximately 2/3 of firms were insured for the Anw gap.

Table 6: Regression discontinuity estimates according to treatement intensity

	Married at 42 years old				Married in 1990			
	Q1	Q2	Q3	Q4	Q1	Q2	Q3	Q4
δ_d	0	-0.003	0.002	-0.003	0.005	0	0.002	-0.002
Std. err.	0.004	0.003	0.003	0.003	0.004	0.003	0.003	0.003
p value	0.92	0.422	0.559	0.336	0.176	0.959	0.536	0.601
Observations	135961	135277	129742	124638	137342	135976	130174	124967

NOTE: Q1 to Q4 are quartiles of treatment intensity, computed as the quartile of the evolution of life-cycle SB between actual and counterfactual legislation. Q1 corresponds to the married women that were treated with the lowest treatment and Q4 with the highest treatment.
For married at 42 women, probability of divorce is computed as the probability to divorce at least once between 43 and 64 years old. For married in 1990 women, probability of divorce is computed as the probability to divorce at least once between 1991 and 2011.
SCOPE: Married at 42 years old women and married in 1990 women, respectively.
SOURCE: CBS.

spouse against an additional to the normal survivors insurance monthly payment. The benefits were determined by the pension fund but generally did not exceed 1/3 of minimum wage and were dedicated to individuals born before January, 1st 1950. We assume women in couple with a husband working in the private sector react more than the others, because the differential treatment between born before 1950 and born after 1950 in this sector is greater than in other sectors.

Table C.3 in appendix C shows individual characteristics according to spouse's activity sector. We verify women are roughly similar from one group to another regarding marriage duration. We nevertheless note that age difference between spouses is higher for husbands working in the private sector. Women whose spouse works in the public sector are more likely to have a minor child than in the other groups. Women whose spouse works in the private sector have a lower income level than in the other groups.

We run our RD-analysis independently on the three sub-populations with regards to spouse's sector. Figure C.8 in appendix C give graphical evidence of the results. Divorce probability slightly increases over the cohorts of birth but there is no discontinuity around the 1950 threshold, no matter the spouse's activity sector. In other words, there is no graphical evidence of an effect of 1996 reform on married women probability to divorce, no matter the spouse's activity sector. Finally, RD-estimates are presented in table 7. All estimates are close to 0 and not statistically different from 0.

We conclude that overall there is no evidence of an effect of 1996 reform on divorce probability among Dutch married women.

Table 7: Regression discontinuity estimates according to spouse's activity sector

	Married at 42 years old			Married in 1990		
	Private	Public	Subsidized	Private	Public	Subsidized
δ_d	0.004	-0.002	0.009	0.006	-0.002	0.007
Std. err.	0.005	0.002	0.008	0.005	0.002	0.008
p value	0.398	0.431	0.233	0.211	0.316	0.389
Observations	48919	199945	21817	48673	199538	21721

NOTE: The different columns refer to spouse's activity sector.

For married at 42 women, probability of divorce is computed as the probability to divorce at least once between 43 and 64 years old. For married in 1990 women, probability of divorce is computed as the probability to divorce at least once between 1991 and 2011.

SCOPE: Married at 42 years old women and married in 1990 women, respectively.

SOURCE: CBS.

5 Labor supply responses

5.1 Potential explanations for the absence of effect on divorce

In this section we try to understand why the Dutch survivors insurance reform had no effect on divorce probability. We consider the following potential explanations for our zero-effect result.

A first range of explanations relates to individuals' perception and preferences. It is possible that individuals underestimate the risk of widowhood and necessity to save money to insure one's living standard. They may also strongly discount the potential income drop in the future. With a large preference of the present, the surplus deriving from marriage is so too far away in time to be accounted for at the time of decision making.

Second, the reform we are studying may be too small to trigger a behavioral response. In Persson (2020), who shows significant effect of the Swedish SB elimination on marriage market, the magnitude of the reform is bigger than in our case. In the Netherlands, survivors insurance is conditional on widow(er)hood before age 65, which is a much lower probability than the probability of spouse's death. Moreover, the amount at stake (discounted SB sum over life-cycle) is of lower magnitude than in Persson (2020) because benefits stop at pension-age. Finally, conversely to the Swedish case, on the one hand, survivors insurance reform impacted all Dutch residents with introduction of the means test and, on the other hand, has not definitively deprived anyone of benefits, since people caring for a minor child remain eligible. Overall, the variation in financial incentive deriving from survivors insurance is of much lower magnitude following the Dutch reform than it was in the Swedish case. This may explain the different findings between the papers.

Third and lastly, the decrease in public benefits may be compensated by an increase in private income, which would cushion the effect of the reform. Individuals may entirely compensate the SB loss with higher labor income or private survivors insurance to insure their standard of living and thus have no reason to react in terms of marriage or divorce decisions to the SB reform. In this section,

we explore more in depth this last channel, and try to find evidence of an income substitution to SB loss, by mean of higher labor force participation or program substitution.

5.2 Labor supply effect of the reform

5.2.1 Graphical evidence

We first present graphical evidence of the effect of the death of the husband on labor supply of the widow, and how this was affected by the SB reform. Figure 6 presents widows' income level according to distance to spouse's death for different types of income. Labor income refers to wage income, profits from self-employment and income from other activity; other insurance schemes refer to social welfare benefits, unemployment benefits and sickness/disability benefits; public SB refers to first pillar survivors insurance; and occupational SB refers to second pillar survivors pension plans.

We verify that at spouse's death, first pillar SB become positive. After spouse's death and on average, first pillar SB are equal to 70 % of minimum wage for the non-treated cohorts (those born before 1950) while they are only equal to 20 % for the treated ones, which illustrates the important financial effect of the reform as previously shown. Conversely, labor and welfare income are higher for treated cohorts after spouse's death. Following spouse's death, labor income decreases in a lower proportion for treated cohorts than for the non-treated ones, by comparison to the pre-death situation. Overall, higher labor income for youngest cohorts does not compensate SB loss and total income after spouse's death is lower for these cohorts than for the eldest ones (panel E., total income)

Figure 6: Income level according to distance to spouse death
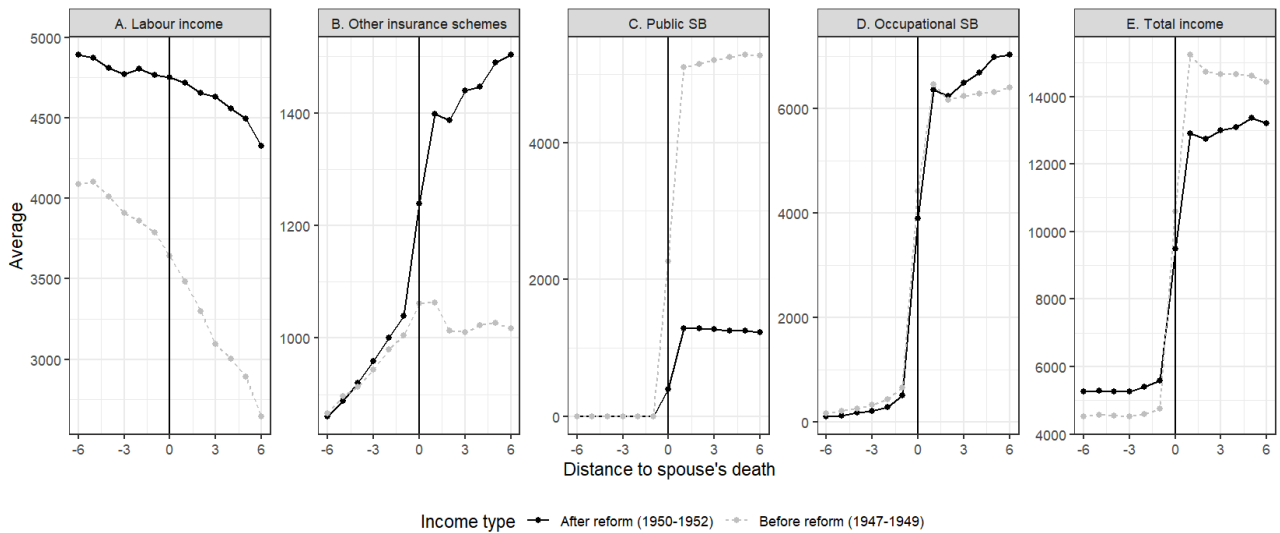
NOTE: Labor income refers to wage income, profits from self-employment and income from other activity, occupational SB are computed as the difference between public SB (Anw) and total pension, other insurance schemes income refers to social welfare benefits, unemployment benefits and sickness/disability benefits.
SCOPE: Widows born between 1947 and 1952.
SOURCE: CBS.

Those simple averages then exhibit evidence of labor supply response to the reform: the decrease

in labor incomes after the death of the spouse seems to be less pronounced after the reform. The difference is however slightly blurred by the decreasing trend in labor income over time, and the difference in level between the pre- and post-treatment groups. As an intermediate step before the difference-in-differences approach implemented in the next subsection, figure 7 shows the linear model estimates according to distance to the event, with the addition of controls for time and age. With those controls, the evolution of labor force participation before and after the death of the spouse seems to be largely modified by the reform. Relatively to a pre-reform baseline, non-treated individuals decrease their labor supply, both in terms of income (panel A.) and labor force participation (panel B.). Three years after spouse death, widows born before 1950 have reduced their labor income by 800 euros in average compared to individuals who were treated by the reform. After the reform, the drop in labor force participation after the spouse's death virtually disappears. We interpret this evolution of the causal effect of the SB cut implemented by the reform, that is partially compensated by an increase in the labor supply of the widow. This is in line with van der Vaart et al. (2020), who show that labor income after death slightly increase after the SB reform. In the next subsection we quantify directly the magnitude of this effect using a difference-in-differences approach.

Figure 7: Linear model estimates according to distance to spouse death

5.2.2 Estimation results

Empirical strategy. We carry out a difference-in-differences estimation in order to catch the differentiated effects of the SB reform on labor supply outputs between a treated group (individuals born

after 1950) and a control group (individuals born before 1950). We successively use two independent variables: labor income and labor force participation.

Labor supply information is available from 1999 onward. We thus restrict our scope of analysis to widows whose husband died after 2003 and who were aged below 62 at death time so as we observe all individuals at each time of the three-years around death period. We more precisely estimate a fully interacted differences-in-differences specification so as to distinguish the treatment effect from pre-event trends. Let define $S_{it} = t - P_{it}$ the *relative event time*. It indicates, for each widow i the relative distance to her spouse's death P_{it}. The dynamic effect of the event on the observed variable of interest can be estimated from:

$$Y_{its} = \alpha + \sum_{s=-\infty}^{+\infty} \beta \mathbf{1}_{s=S_{it}} + \gamma \mathbf{1}_{T=1} + \sum_{s=-\infty}^{+\infty} \delta_{e1} \mathbf{1}_{s=S_{it}} \mathbf{1}_{T=1} + \lambda X_{it} + \epsilon_{its} \tag{4}$$

where Y_{its} is the independent variable (labor income and labor force participation), S_{it} are the event time dummies, T is the treatment variable, equal to 1 for individuals born after 1950 and 0 otherwise, $X_i t$ is a vector of age and date controls and ϵ_{its} is a random noise. δ_{e1} captures the pre-event trends ($s < 0$) and the treatment effect ($s > 0$). The reference group is the non-treated one. We expect estimates for the treated cohorts not to be significantly different from zero before spouse death and to give an estimation of the reform effect after spouse death on the different outcome variables.

In order to summarise the average SB reform effect on labor supply on the period, we also estimate a simple two periods difference-in-differences model in some specifications:

$$Y_{it} = \alpha + \beta \mathbf{1}_{t>0} + \gamma \mathbf{1}_{T=1} + \delta_{e2} \mathbf{1}_{t>0} * \mathbf{1}_{T=1} + \lambda X_i t + \epsilon_{it} \tag{5}$$

where Y_{it} is the independent variable (labor income or labor force participation), t is distance to death, T is the treatment variable, equal to 1 if individual i was born after 1950 and 0 otherwise, $X_i t$ is a vector of age and date controls and ϵ_{it} is random noise. δ_{e2} captures the effect of the reform on the treated individuals.

Main results. Figure 8 presents the results of the estimation of equation (4) using OLS, for labor income and labor force participation. As expected, before death event, estimates are non significantly different from zero. After spouse death, labor income and labor force participation are significantly higher than reference level. Three years after death, labor income has increased by 700 euros while labor force participation is 6 percentage points higher than reference level.

Table 8 present the results for the two-periods difference-in-difference. Over the three-years period following the spouse death, labor income increased by 550 euros as a result of the SB reform while labor force participation increased by 5 percentages points compared to a counterfactual situation in which the reform had not been enacted. Compared to the treated group baseline before the death

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Figure 8: Event study estimates for labor income and labor force participation

NOTE: Labor income refers to wage income, profits from self-employment and income from other activity and labor force participation is a dummy variable equal to 1 if individual has positive labor income and 0 otherwise.
Confidence intervals are computed at the 95 % level.
SCOPE: Widows born between 1947 and 1952 whose husband died after 2003 and were aged below 62 at death time.
SOURCE: CBS.

event, SB reform effect represents a 11.5 % increase of labor income and a 10.8 % increase in labor force participation.

The effects we estimate are of slightly larger magnitude than in related literature. Giupponi (2019) finds a 7 % increase of labor force participation as a result of the decrease in Italian SB amount introduced by reform in 1995. Böheim & Topf (2021) results suggest a 4.2 to 6.4 % decrease from the baseline employment rate following the decrease in Austrian SB amount enacted by the 2000 reform. Finally, Fadlon et al. (2019) estimate a 4.7 to 8.1 % drop in labor force participation as the immediate post-shock consequence of receiving SB in the U.S.[15] Nevertheless institutional contexts are different from ours and literature comparison should rely on elasticities instead of comparison. We plan to make these calculations for the next version of the paper.

It is however worth noting that Böheim & Topf (2021) paper focuses on widowers and that Giupponi (2019) she carries out her analyses on a restricted sample of the population. She focuses on widows whose income is lower than the SB means test threshold. Fadlon et al. (2019), who estimate the effect eligibility age on labor supply both for total population and a subsample composed of individuals below the similar U.S. threshold, find a larger response on the total population sample (8.1 % decrease in labor force participation *versus* 4.7 % for the subsample). This composition effect participates in the explanations for our higher magnitude effects. Finally, we estimate the reform effect on a three-years

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[15]Fadlon et al. (2019) estimate a 2.87 percentage points increase of labor force participation for total population, to compare to a 61.62 % baseline and a 2,42 percentage points increase for individuals whose income is below the means test threshold, to compare to the 30 % baseline.

period, while Giupponi (2019) identifies effects up to 15 years after the spouse's death and Böheim & Topf (2021) estimates reform effects over the 150 months following the death of their spouse. We believe our results would have been of higher magnitude if identified on a longer period of time.

Table 8: Difference-in-differences estimator for main specification

	A. Labour income	B. Labour force participation
δ_e	549.117	0.051
Std. err.	57.444	0.003
p value	0.000	0.000
Effect (in %)	11.517	10.806
Nb obs.	364754	364754

NOTE:Labour income refers to wage income, profits from self-employment and income from other activity and labour force participation is a dummy variable equal to 1 if individual has positive labour income and 0 otherwise.
SCOPE: Widows born between 1947 and 1952 whose husband died after 2003 and who were aged below 62 at death time.
SOURCE: CBS.

Robustness. We implement a wide set of robustness tests to check the validity of our results. To do so, we use a more tractable 2 period DiD approach.[16]. Overall, our results are robust to time basis and presence of controls in our specifications, as shown in tables 9 and 10. We also carry out the analysis on a narrower period in order to neutralize the possible effects of the 2006 pension reform of the second pillar pension, which impacted the same cohorts. More precisely, we analyse the SB reform effects on labor supply outcomes until 60 years old instead of 65 years old, so as to neutralize variation in income due to early-retirement schemes. Table 11 shows similar even if slightly higher estimates. SB reform has a 730 euros increase effect on labor income for treated group and a 5.5 percentage points increase on labor force participation.

Finally, we build two placebo analyses. In the first one (placebo 1) we consider the 1946 to 1949 cohorts of birth and we define as non-treated the individuals born before 1948 while treated the others. In the second one (placebo 2), we consider the 1950 to 1953 cohorts and we define as non-treated the individuals born before 1952 while treated the other. The main estimation is estimated on a four-cohorts of birth restricted sample: individuals born between 1948 and 1951. As expected, estimates are found not to be significantly different from zero for none of the events, for the two placebo analyses.

Effect by wealth group. We try to go further in understanding the effects of the reform by studying the heterogeneous effects according to individuals' wealth. Indeed, labor supply adjustment can be attributable to a lack of liquidity at spouse death and then an inability to smooth standards of living

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[16]Fully-interracted DiD graphs for robustness checks and placebo analyses are shown in figures C.10, C.11 and C.12 in appendix C.

Table 9: Robustness checks for labour income difference-in-differences estimator

	Semi-annual				Quarterly				Monthly			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
δ_e	549.117	549.117	549.117	549.117	263.743	263.743	263.743	263.743	90.497	90.497	90.497	90.497
Std. err.	57.506	57.470	57.490	57.490	20.412	20.398	20.406	20.406	4.046	4.043	4.045	4.045
p value	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Effect (in %)	11.517	11.517	11.517	11.517	5.532	5.532	5.532	5.532	1.898	1.898	1.898	1.898
Time basis	s	s	s	s	q	q	q	q	m	m	m	m
Controls	X	age	date	✓	X	age	date	✓	X	age	date	✓
Nb obs.	364754	364754	364754	364754	733475	733475	733475	733475	2200366	2200366	2200366	2200366

NOTE: s stands for semi-annual basis, q for quarterly basis and m for monthly basis. For controls, ✓ means both age and date controls.

SCOPE: Widows born between 1947 and 1952 whose husband died after 2003 and who were aged below 62 at death time.

SOURCE: CBS.

Table 10: Robustness checks for labour force participation difference-in-differences estimator

	Semi-annual				Quarterly				Monthly			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
δ_e	0.051	0.051	0.051	0.051	0.049	0.049	0.049	0.049	0.050	0.050	0.050	0.050
Std. err.	0.003	0.003	0.003	0.003	0.002	0.002	0.002	0.002	0.001	0.001	0.001	0.001
p value	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Effect (in %)	10.806	10.806	10.806	10.806	10.482	10.482	10.482	10.482	10.607	10.607	10.607	10.607
Time basis	s	s	s	s	q	q	q	q	m	m	m	m
Controls	X	age	date	✓	X	age	date	✓	X	age	date	✓
Nb obs.	364754	364754	364754	364754	733475	733475	733475	733475	2200366	2200366	2200366	2200366

NOTE: s stands for semi-annual basis, q for quarterly basis and m for monthly basis. For controls, ✓ means both age and date controls.

SCOPE: Widows born between 1947 and 1952 whose husband died after 2003 and who were aged below 62 at death time.

SOURCE: CBS.

Table 11: Robustness test (pension reform) for difference-in-differences estimators

	A. Labour income				B. Labour force participation			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
δ_e	728.401	728.401	728.401	728.401	0.055	0.055	0.055	0.055
Std. err.	116.000	116.000	116.000	116.000	0.006	0.006	0.006	0.006
p value	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Effect (in %)	15.278	15.278	15.278	15.278	11.775	11.775	11.775	11.775
Time basis	s	s	s	s	s	s	s	s
Controls	X	age	date	✓	X	age	date	✓
Nb obs.	115739	115739	115739	115739	115739	115739	115739	115739

SCOPE: Widows born between 1947 and 1952 whose husband died after 2003 and who were aged below 62 at death time.

SOURCE: CBS.

at death other than investing labor market (Fadlon et al. 2019).

Table 12: Placebo tests for difference-in-differences estimators

	A. Labour income			B. Labour force participation		
	(main)	(placebo 1)	(placebo 2)	(main)	(placebo 1)	(placebo 2)
δ_e	760.642	205.738	-176.070	0.057	0.000	-0.004
Std. err.	137.416	261.735	132.460	0.007	0.014	0.007
p value	0.000	0.432	0.184	0.000	0.999	0.605
Effect (in %)	15.954	4.315	-3.693	12.085	0.002	-0.812
Nb obs.	79339	36777	78962	79339	36777	78962

NOTE: In (main) specification, estimation is carried out on 1948 to 1952 cohorts, were individuals born before 1950 are the non-treated group. In (placebo 1) specification, estimation is carried out on 1946 to 1949 cohorts were individuals born before 1948 are the non-treated group. In (placebo 2) specification, estimation is carried out on 1950 to 1953 cohorts were individuals born before 1952 are the non-treated group.

SCOPE: Widows born between 1947 and 1952 whose husband died after 2003 and who were aged below 62 at death time.

SOURCE: CBS.

We divide our population into four groups according to wealth quartile. We expect the richest to react less than the poorest for the reasons outlined above.

Table 13 presents the simple difference-in-differences estimates while fully-interacted difference-in-differences graphs are shown in figure C.13 in appendix C. Both for labor income and labor force participation, estimates are higher for the two first quartiles of wealth than the others, while estimates for the last quartile are slightly higher than for the third quartile of wealth. Regarding labor income, SB reform increased average income by 600 euros and 790 euros for firth and second quartile, respectively. Individuals belonging to the fourth quartile have increased their average labor income by 480 euros following the reform while those belonging to the third one increased their income by 390 euros only. For labor force participation, effects are equal to 5.8, 6.7, 3.5 and 4.3 for the first to the last quartile, respectively. However, relative to the pre-reform baseline, the estimated effect of the SB reform show a gradient in individual wealth, from first to third quartile, while fourth one remains at the third quartile response level. Poorest increase their labor income by more than 23 % and their labor force participation *versus* 17 and 14 % respectively for second quartile, 4.8 and 6.1 %, respectively for the third quartile and 6.9 and 7.6 % respectively for the last one. As expected, the labor supply response largely decrease with the level of wealth, as wealthy widows can smooth the effect of the SB cut out of their wealth.

6   Conclusion

In this paper we investigate the impact of survivors insurance on marital behavior. We take advantage of the 1996 Dutch reform, which considerably tightened eligibility rules to survivors' benefits.

After the reform, survivors benefits become conditional on child caring and means tested. By 1996, expected surplus deriving from marriage is lowered. Individuals born before January, 1st 1950

Table 13: Difference-in-differences estimators per quartile of wealth

| | A. Labour income | | | | B. Labour force participation | | | |
	(Q1)	(Q2)	(Q3)	(Q4)	(Q1)	(Q2)	(Q3)	(Q4)
δ_e	648.675	794.341	285.972	480.406	0.058	0.067	0.035	0.043
Std. err.	87.474	97.153	72.041	168.061	0.007	0.007	0.006	0.006
p value	0.000	0.000	0.000	0.004	0.000	0.000	0.000	0.000
Effect (in %)	22.915	17.205	4.815	6.944	16.981	13.728	6.058	7.574
Nb obs.	90181	88452	84994	94328	90181	88452	84994	94328

NOTE: Q1 to Q4 are wealth quartiles. Q1 corresponds to the 25 % of individuals with the lowest wealth.
SCOPE: Widows born between 1947 and 1952 whose husband died after 2003 and who were aged below 62 at death time.
SOURCE: CBS.

are partially exempted. They remain eligible even if they do not care for a child but their benefits are means tested. Exploiting the discontinuity in date of birth, we carry out a regression discontinuity design on divorce probability. We find a no significantly different from zero effect of the 1996 reform on divorce probability. We build a life-cycle indicator, *survivors benefits wealth* (SBW), in order to identify the reform treatment intensity. We nevertheless find no evidence of an effect of the reform on the most treated group. Our heterogeneity analysis by spouse's activity sector does not give evidence of an effect of the reform on divorce probability either. We provide evidence that the decrease in public benefits is partially compensated by private income by means of an increase of labor force participation. We carry out a difference-in-differences analysis and our results suggest a 5.1 percentage point higher employment rate for survivors and a 550 euros increase in labor income, corresponding to a 10.8 % and a 11.5 % variations, respectively. We show a response gradient in wealth, in line with individuals' (in)ability to smooth standards of living at death time.

We obviously miss determinants other than economic opportunities for divorce decisions, even if we control our estimations by age difference between spouses, caring for a minor child, marriage duration and income level. In particular, it may be more relevant to adopt a household point of view, and more especially for those in which there is a housewife. The reform effect probably depends on income difference between the members of a couple, meaning that very specialized couples in which the wife is a housewife probably react more than others because they have no alternative on the labor market. We believe the latter could react more to the reform.

Future research will let us investigate several possible explanations for our no-effect result, such as risk under-estimation of one's spouse death, rate of time preference and low value for future expected earning and heterogeneity per resources distribution among households.

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A Additional information on institutional context

Table A.1: Survivors' benefits legislation before and after 1996 reform

		Before reform	After reform	
			born before 1950*	born after 1950
Eligibility	**Matrimonial status**	marriage	marriage or partnership or cohabitation	marriage or partnership or cohabitation
	Individual charac.	+40 y.o.		minor child
Benefits	**If minor child**	100 % min. wage	70% + 20% min. wage	90% min. wage
	If no child	70% min. wage	70% min. wage	
	Means test	No	Yes**	Yes**
	Stop when	reach AOW-age or remarriage	reach AOW-age or new household	reach AOW-age or child gets 18 or new household

* Survivors born before january, 1st 1956 and whose partner deceased before July, 1st 1999 are considered as if they were born before January, 1st 1950.
** Means test only applies on the 70% of minimum wage while the 20% of minimum wage linked to the care of a minor child are not until 2013. Means test apply on all benefits (90% of minimum wage) from 2013 onward.
SOURCE: Legislation

B Data

B.1 Complements on data presentation

The data used in this study for divorce probability analyses are individual-level or household-level data provided by Statistics Netherlands (CBS). There are accessible via a remote access environment in a set of different datasets. In a dataset, each individual is identified by a unique number (which has been pseudomyzed). The linkage of the different datasets is performed using the individual identifier, and is thus exact.

Table B.1 provides the list of the microdata used in this research. The civil-status histories cover each resident's past and present partnership, marriage, separation, divorce and widow(er)hood. For each event, the data indicate the exact date of happening. Leveraging the spousal and child identifiers, we are able to link individuals to their spouses and children and then get information on these. Administrative files of Anw beneficiaries are available from 2005. We link information on survivors' benefits nature and amount to individuals belonging to the Dutch registers. We mobilize individual income sources. These are available from 1999 and give information, on a monthly basis, on nature and amount of income. We also use the employees database to get information on activity sector, wages and hours worked (from 2006 onward). Finally, we mobilize wealth database to get information on wealth (from 2007 onward). We can distinguish liquid wealth (financial assets) from total wealth.
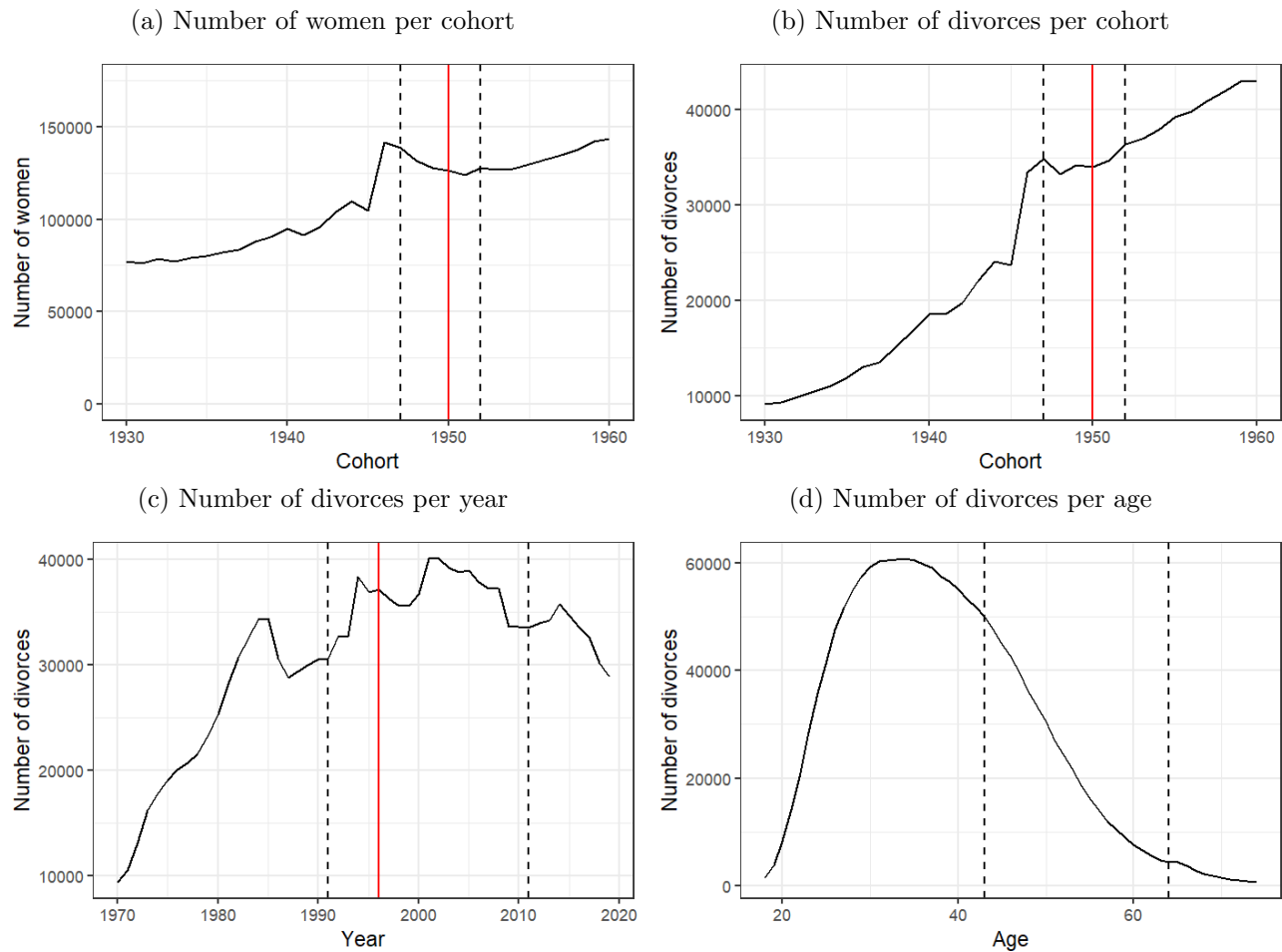
Table B.1: Datasets used

Content	Name of dataset	Source
Date of birth and gender	GBAPERSOON2019TAB (V1)	Population registers
Death	GBAOVERLIJDENTAB2019TAB (V1)	Death records
Civil status and date	GBABURGERLIJKESTAAT2019BUS (V1)	Population registers
Households characteristics	GBAHUISHOUDENS2019BUS (V1)	SSB
Linkage parent-child	KINDOUDER2019TAB (V1)	Population registers
Anw benefits	090106 ANW 2005 (V1)	SSB
	090109 ANW 2006 (V1)	SSB
	090112 ANW 2007 (V1)	SSB
	090112 ANW 2008 (V1)	SSB
	100208 ANW 2009 (V1)	SSB
	110316 ANW 2010 (V1)	SSB
	120535 ANW 2011 (V1)	SSB
	130206 ANW 2012 (V1	SSB
	140324 ANW 2013 (V1)	SSB
	ANWUITKERING1ATAB2014MM (V2)	SSB
	ANWUITKERING1ATAB2015MM (V2)	SSB
	ANWUITKERING1ATAB2016MM (V1)	SSB
	ANWUITKERING1ATAB2017MM (V1)	SSB
	ANWUITKERING1ATAB2018MM (V1)	SSB
	ANWUITKERING1ATAB2019MM (V1)	SSB
Individual income		
Wage income	SECMWERKNDGAMNBEDRABUSV20181	SSB
Profits from self-employment	SECMZLFMNDBEDRAGBUSV20181	SSB
Income from other activity	SECMOVACTMNDBEDRAGBUSV20181	SSB
Social welfare benefits	SECMBIJSTMNDBEDRAGBUSV20181	SSB
UI benefits	SECMWERKLMNDBEDRAGBUSV20181	SSB
DI and sickness benefits	SECMZIEKTAOMNDBEDRAGBUSV20181	SSB
Other social security benefits	SECMSOCVOORZOVMNDBEDRAGBUSV20181	SSB
Pension income	SECMPENSIOENMNDBEDRAGBUSV20181	SSB
Activity sector, wage and hours worked	POLISBUS 2006 (V2)	SSB
	POLISBUS 2007 (V1)	SSB
	POLISBUS 2008 (V1)	SSB
	POLISBUS 2009 (V1)	SSB
	SPOLISBUS 2010 (V1)	SSB
	SPOLISBUS 2011 (V1)	SSB
	SPOLISBUS 2012 (V1)	SSB
	SPOLISBUS 2013 (V2)	SSB
	SPOLISBUS 2014 (V1)	SSB
	SPOLISBUS 2015 (V3)	SSB
	SPOLISBUS 2016 (V3)	SSB
	SPOLISBUS 2017 (V2)	SSB
	SPOLISBUS 2018 (V5)	SSB
	SPOLISBUS 2019 (V5)	SSB
Total wealth, liquid wealth	VEHTAB 2007 (V2)	SSB
	VEHTAB 2008 (V2)	SSB
	VEHTAB 2009 (V2)	SSB
	VEHTAB 2010 (V2)	SSB
	VEHTAB 2011 (V5)	SSB
	VEHTAB 2012 (V5)	SSB
	VEHTAB 2013 (V5)	SSB
	VEHTAB 2014 (V5)	SSB
	VEHTAB 2015 (V5)	SSB
	VEHTAB 2016 (V2)	SSB
	VEHTAB 2017 (V2)	SSB

NOTE: SSB stands for *Sociaal Statistisch Bestand* (Social Statistical Database).

SOURCE: CBS microdata catalogue.

B.2 Additional descriptive statistics
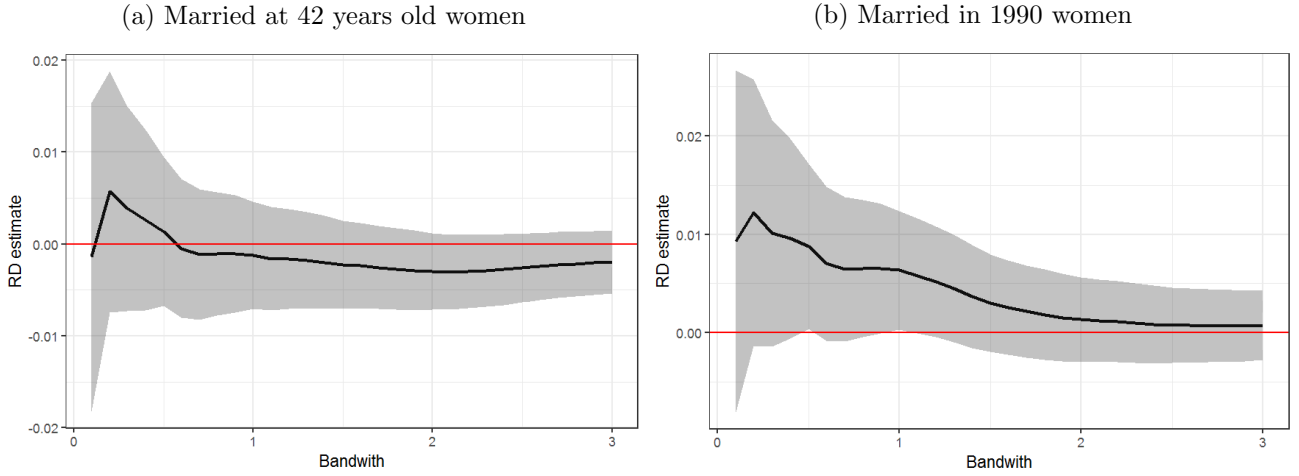
Figure B.1: Number of women and divorces

(a) Number of women per cohort

(b) Number of divorces per cohort

(c) Number of divorces per year

(d) Number of divorces per age

Source: CBS.

C Additional results

C.1 Divorce probability analyses robustness check

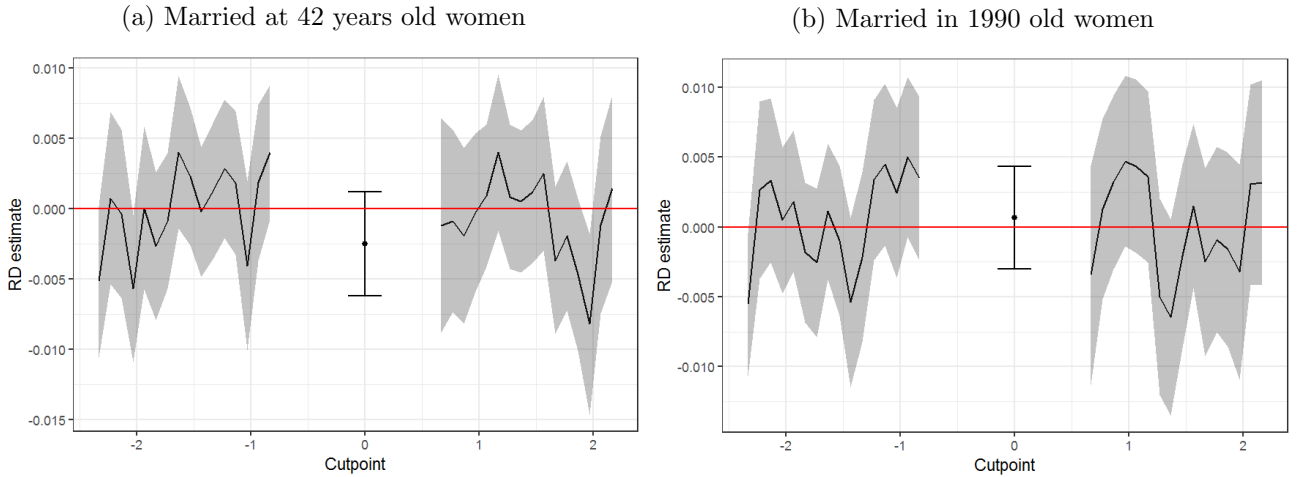Figure C.1: Sensitivity to bandwidth test for divorce probability

(a) Married at 42 years old women   (b) Married in 1990 women

NOTE: For married at 42 women, probability of divorce is computed as the probability to divorce at least once between 43 and 64 years old. For married in 1990 women, probability of divorce is computed as the probability to divorce at least once between 1991 and 2011.
SCOPE: Married at 42 years old women and married in 1990 women, respectively.
SOURCE: CBS.

Figure C.2: Placebo test for divorce probability

(a) Married at 42 years old women   (b) Married in 1990 old women

NOTE: For married at 42 women, probability of divorce is computed as the probability to divorce at least once between 43 and 64 years old. For married in 1990 women, probability of divorce is computed as the probability to divorce at least once between 1991 and 2011.
SCOPE: Married at 42 years old women and married in 1990 women, respectively.
SOURCE: CBS.

Table C.1: Regression discontinuity estimates for divorce probability before pension reform

| | Married at 42 years old | | | | | | | Married in 1990 | | | | | | |
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(1)	(2)	(3)	(4)	(5)	(6)	(7)
δ_d	-0,001	-0,004	-0,003	-0,001	-0,001	-0,001	-0,003	0,001	-0,001	0,005	0,002	0,001	0,001	0,001
Std. err.	0,002	0,002	0,003	0,002	0,002	0,002	0,002	0,002	0,002	0,003	0,002	0,002	0,002	0,002
p value	0,396	0,101	0,373	0,456	0,398	0,397	0,105	0,403	0,762	0,107	0,322	0,398	0,442	0,768
Parametric	✓	✓	✓	✓	✓	✓		✓	✓	✓	✓	✓	✓	
Poly. order	1	2	3	1	1	1		1	2	3	1	1	1	
Controls				✓							✓			
Equal slopes					✓							✓		
Time basis	m	m	m	m	m	q	m	m	m	m	m	m	q	m
Nb obs.	525620	525620	525620	524679	525620	525620	525620	528461	528461	528461	528459	528461	528461	528461

NOTE: Control variables are marriage duration (at 42 years old or in 1990), spouses' age difference, a dummy that indicates whether the married woman is caring for a minor child (at 42 years old or in 1990) and normalized income at 52 years old (first age at which we have information for the elder cohort). For married at 42 women, probability of divorce is computed as the probability to divorce at least once between 43 and 59 years old. For married in 1990 women, probability of divorce is computed as the probability to divorce at least once between 1991 and 2006.
SCOPE: Married at 42 years old women and married in 1990 women, respectively.
SOURCE: CBS.

C.2 Theoretical representation of treatment intensity according to characteristics

Figure C.3 presents the life-cycle SB variation induced by the 1996 SB reform, according to individuals' characteristics.

Individuals born before 1950 and who do not care for a minor child (child age over 18 on the y-axis) remain eligible to SB up to 70 % of gross minimum wage but are subject to the means test. The first 50 % of minimum wage are disregarded. Thus, individuals whose income is lower than 50 % of minimum wage do not suffer from any SB decrease. Treatment intensity is null (upper left corner white rectangle). Then, their life-cycle SB decrease compared to the pre-reform situation along the income distribution, because of the means test. Individuals with income higher than 31/20 do not receive any SB. They are 100 % treated (upper right corner black rectangle).

Individuals born before 1950 and caring for a minor child are eligible to 70 % of gross minimum wage subject to means test plus 20 % of gross minimum wage (child benefits). For individuals with income under 50 % of minimum wage, the loss in terms of life-cycle SB is equal to 10 % because SB amount before 1996 was equal to 100 % of gross minimum wage. Then, life-cycle SB decrease along the income distribution and reach a minimum equal to 20 % of gross minimum wage per month (the non means-tested part) at 31/20 minimum wage.

Individuals born after 1950 are eligible to SB only if they care for a minor child. Otherwise they do not have any SB benefits and the variation in terms of life-cycle SB is equal to -100 % (top black rectangle).

For those caring for a minor child, means test applies on the 70 % part of minimum wage only (same mechanisms along the income distribution as seen previously) but SB stop at child majority *vs.* AOW-age, before 1996. In terms of life-cycle SB, the marginal variation (due to date of birth) is proportional to the difference between age at child majority and and AOW-age and then depends on date of birth and child age. In other words, for a same income level between 0.5 and 1.5 % of gross minimum wage, the elder the child age, the higher the life-cycle SB decrease compared to the pre-reform situation.

In order to identify an effect of a SB reform on divorce probability, we take advantage of the treatment discontinuity along birth cohorts. Figure C.4 presents theoretical treatment intensity as the difference between life-cycle SB for born in 1949 individuals compared to life-cycle SB for born in 1950 individuals. We compare individuals born in 1950 (treated) to individuals born in 1949 (reference).

Individuals who do not care for a child do not receive SB if born in 1950. Their treatment intensity is then equal to -100 %. Nevertheless, individuals with income higher than 31/20 would have anyway been means tested, so there is no variation in their life-cycle SB due to the differenciated according to

date of birth set up of the reform. F

For individuals caring for a minor child, treatment intensity depends on two dimensions:

1. child age: for a given income level, the older the child, the higher the treatment intensity (because SB stop at child majority *vs.* AOW-age)

2. income level : for a given child age, the higher the income, the lower the treatment intensity (because SB are anyway means tested)
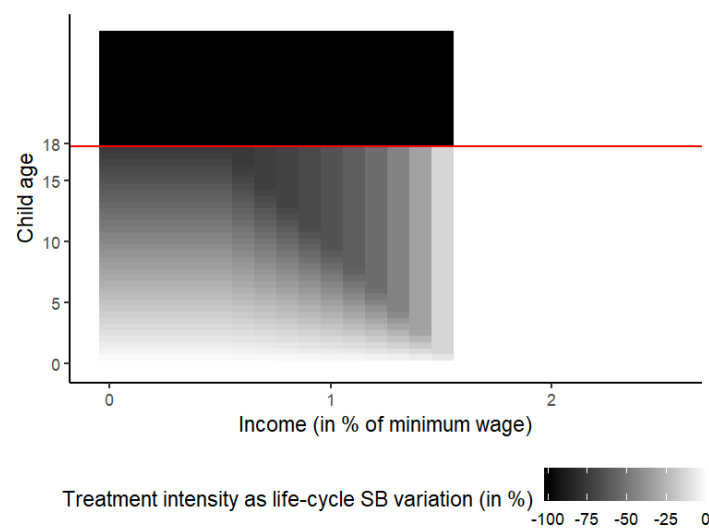
Overall, among individuals caring for a child, the most intensively treated individuals are those who have both an older child and moderate income level.

Figure C.3: Treated individuals according to characteristics
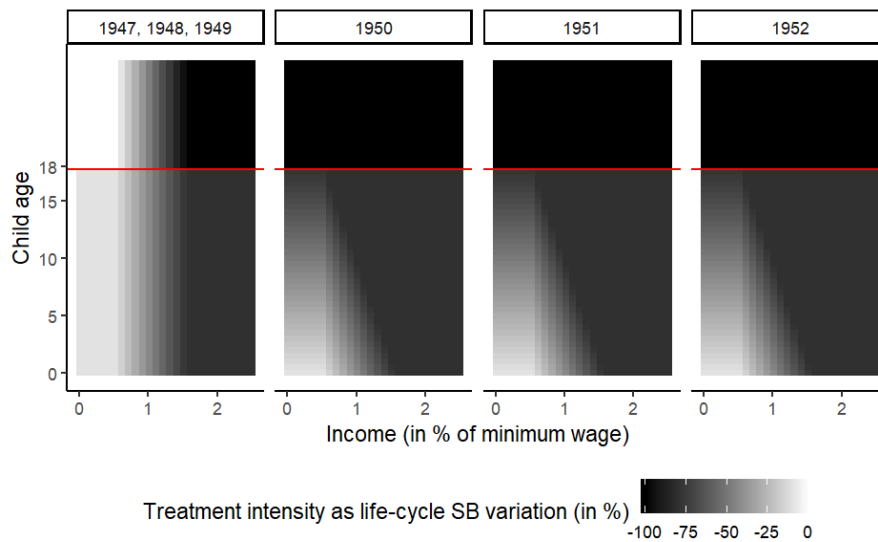
SOURCE: Authors' calculations.

Figure C.4: Treatment intensity according to characteristics

SOURCE: Authors' calculations.

C.3 Survivors benefits simulator assessment

Figure C.5: Errors of simulation for survivors' benefits

(a) Spouses' benefits
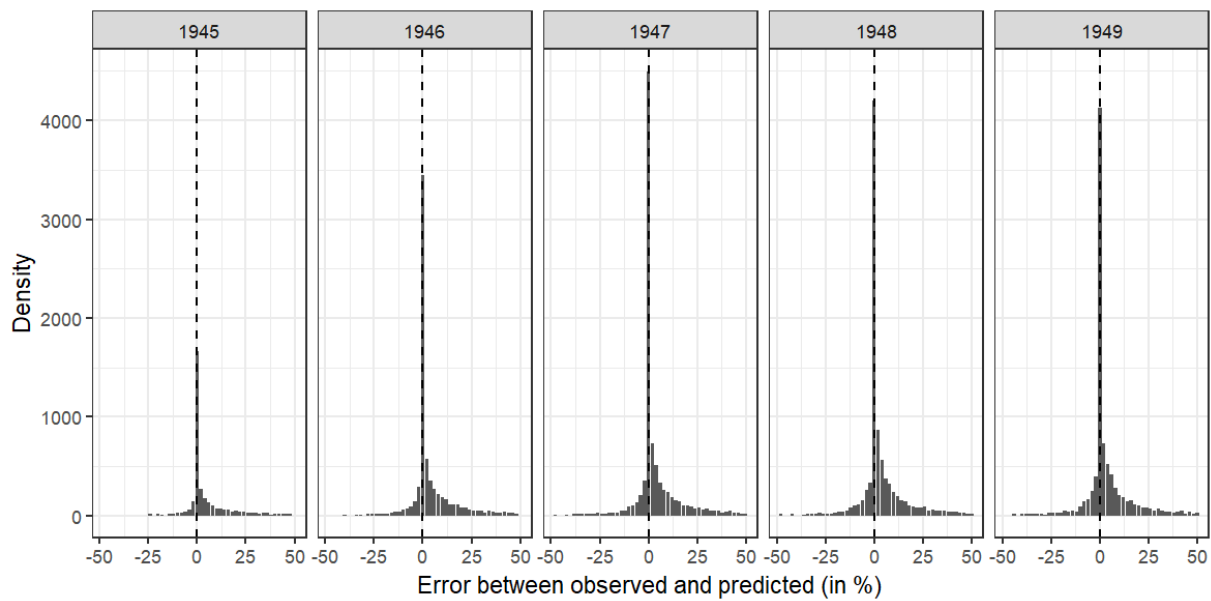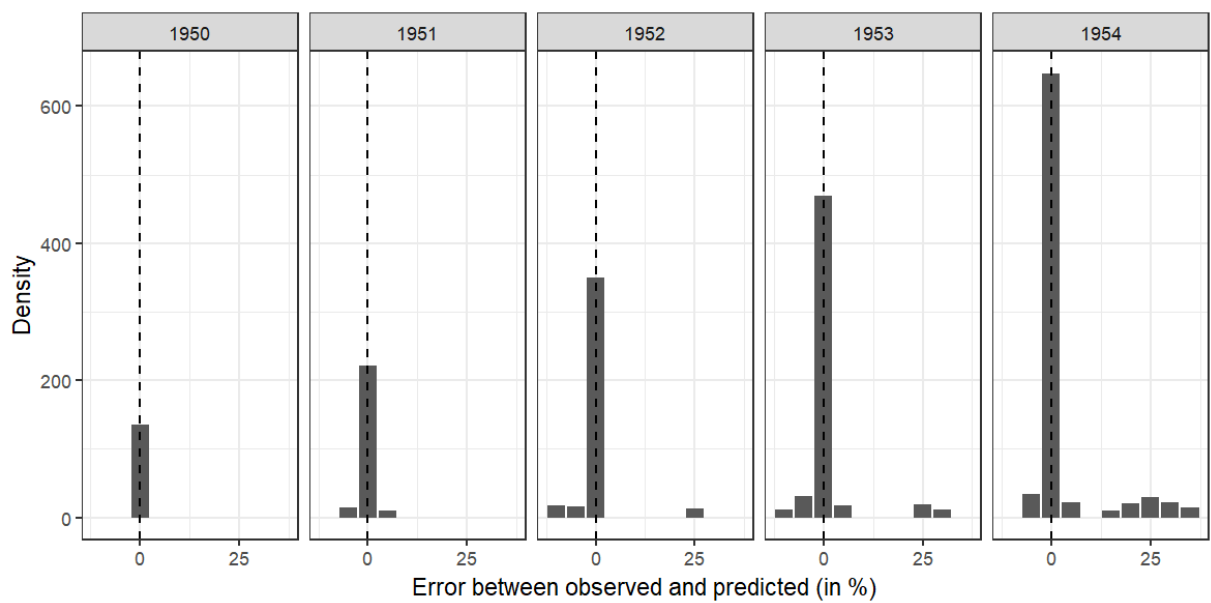
(b) Spouses' and half-orphans' benefits

SCOPE: For spouses' benefits computation only, the scope is composed of female Anw beneficiaries born between 1945 and 1949 who do not care for a minor child. For spouses' and orphans' benefits together, the scope is composed of female Anw beneficiaries born between 1950 and 1954 who care for a minor child.
SOURCE: CBS.

Figure C.6: Observed vs. simulated SB according to income

(a) Spouses' benefits
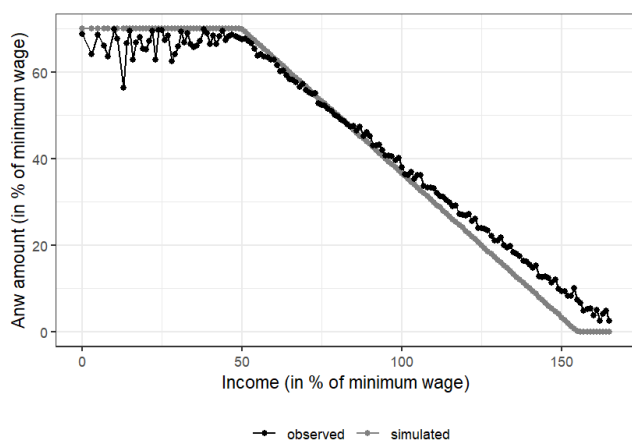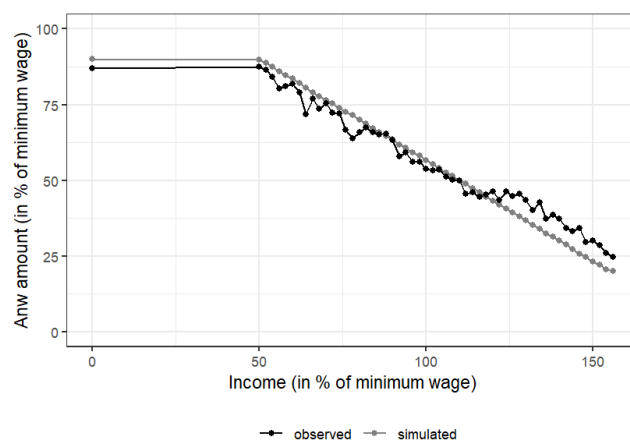
(b) Spouses' and half-orphans' benefits

SCOPE: For spouses' benefits computation only, the scope is composed of female Anw beneficiaries born between 1945 and 1949 who do not care for a minor child. For spouses' and orphans' benefits together, the scope is composed of female Anw beneficiaries born between 1950 and 1954 who care for a minor child.
SOURCE: CBS.

C.4 Heterogeneity analyses

Table C.2: Descriptive statistics per treatment intensity group

	Q1	Q2	Q3	Q4
Married at 42 women				
Number of individuals	137803	133074	131005	131015
Age difference with spouse	-2.149	-2.253	-2.497	-2.495
Marriage duration at 42	17.041	18.746	19.398	19.391
Having a minor child (in %)	73.227	74.643	66.524	66.186
Income (in % of min. wage)	119.534	41.756	34.73	34.688
Having divorced between 43 and 64	13.233	7.941	7.075	6.989
Married in 1990 women				
Number of individuals (2)	138696	133961	131513	131568
Age difference with spouse (2)	-2.176	-2.252	-2.503	-2.494
Marriage duration in 1990	15.577	17.33	18.073	18.063
Having a minor child (in %) (2)	73.35	74.808	66.656	66.33
Income (in % of min. wage) (2)	119.711	41.938	34.833	34.772
Having divorced between 1991 and 2011	14.62	8.522	7.576	7.547

NOTE: Q1 to Q4 are quartiles of treatment intensity, computed as the quartile of the evolution of life-cycle SB between current and counterfactual legislation. Q1 corresponds to the married women that were treated with the lowest treatment intensity and Q4 with the highest treatment intensity. Income is income at 54, younger age at which we have information for the eldest cohort.
SCOPE: Married at 42 years old women and married in 1990 women, respectively.
SOURCE: CBS.

Figure C.7: Divorce probability per cohort of birth and treatment intensity

(a) Married at 42 years old women   (b) Married in 1990 old women

NOTE: For married at 42 women, probability of divorce is computed as the probability to divorce at least once between 43 and 64 years old. For married in 1990 women, probability of divorce is computed as the probability to divorce at least once between 1991 and 2011.
Treatment intensity are the quartiles of the difference between SBW computed under born before 1950 legislation and SBW computed under born after 1949 legislation.
SCOPE: Married at 42 years old women and married in 1990 women, respectively.
SOURCE: CBS.

Table C.3: Descriptive statistics per spouse's activity sector group

	Private	Public	Subsidized
Married at 42 women			
Number of individuals	201948	49081	22045
Age difference with spouse	-1.43	-1.02	-1.1
Marriage duration at 42	18.75	17.99	18.09
Having a minor child (in %)	74.88	83.05	74.93
Income (in % of min. wage)	57.18	86.21	73.69
Having divorced between 43 and 64	6.69	6.76	8.6
Married in 1990 women			
Number of individuals (2)	201541	48835	21949
Age difference with spouse (2)	-1.45	-1.04	-1.12
Marriage duration in 1990	17.11	16.23	16.31
Having a minor child (in %) (2)	74.98	83.25	75.08
Income (in % of min. wage) (2)	57.13	86.01	73.68
Having divorced between 1991 and 2011	6.76	6.84	8.82

NOTE: The different columns refer to spouse's activity sector. Income is income at 54, younger age at which we have information for the eldest cohort.
SCOPE: Married at 42 years old women and married in 1990 women, respectively.
SOURCE: CBS.

Figure C.8: Divorce probability per cohort of birth and spouse's activity sector
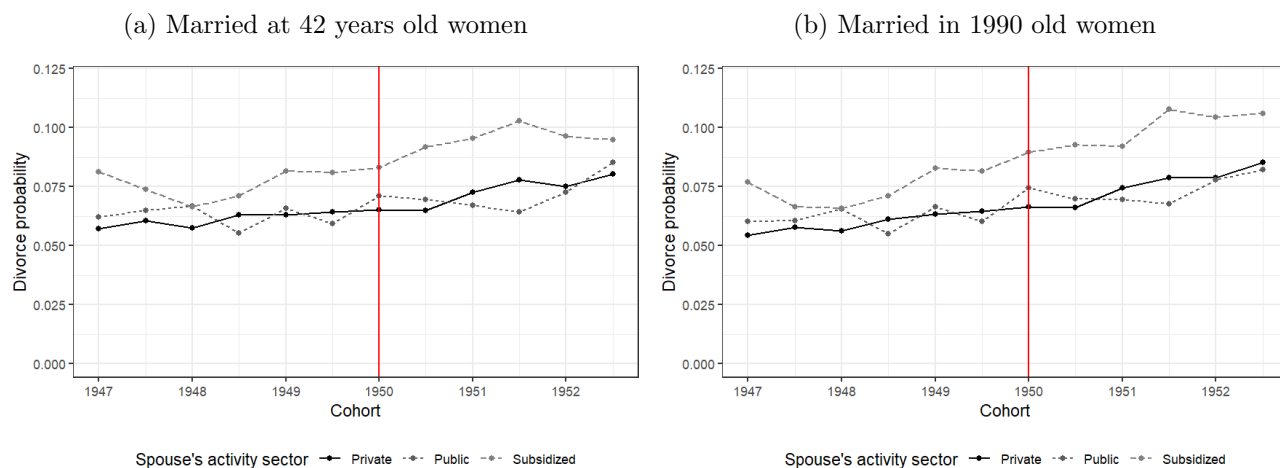
(a) Married at 42 years old women | (b) Married in 1990 old women

NOTE: For married at 42 women, probability of divorce is computed as the probability to divorce at least once between 43 and 64 years old. For married in 1990 women, probability of divorce is computed as the probability to divorce at least once between 1991 and 2011.
SCOPE: Married at 42 years old women and married in 1990 women, respectively.
SOURCE: CBS.

C.5 Income substitution additional results

Figure C.9: Average labor supply output according to distance to spouse death
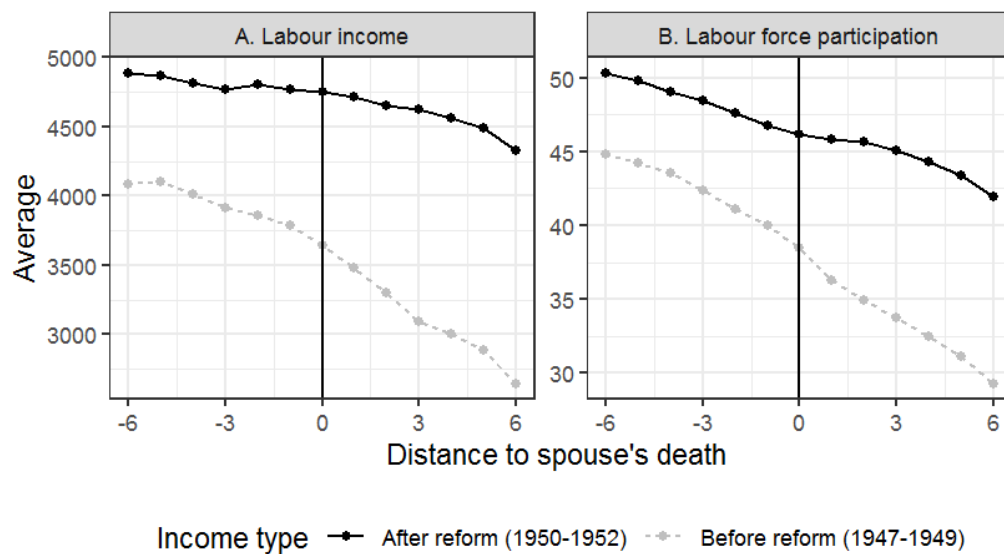
Figure C.10: Event study estimates for labor income and labor force participation
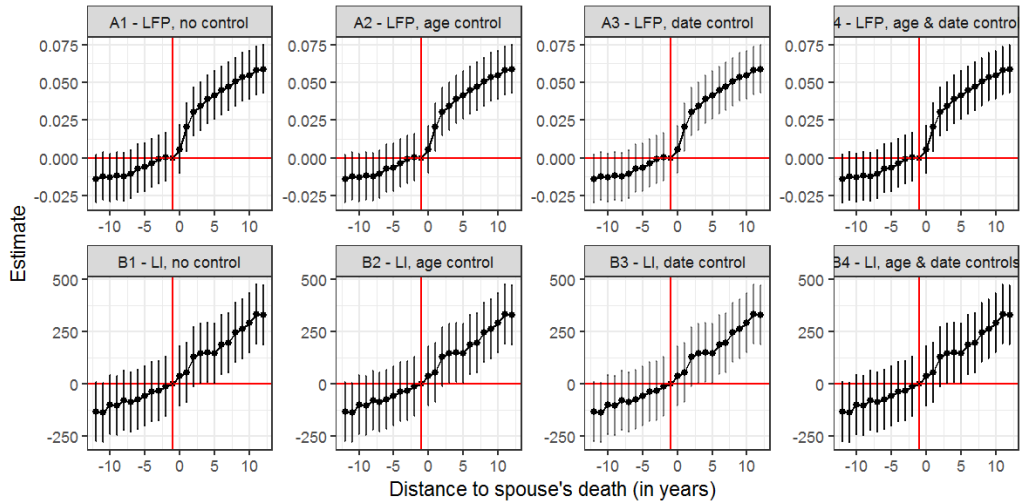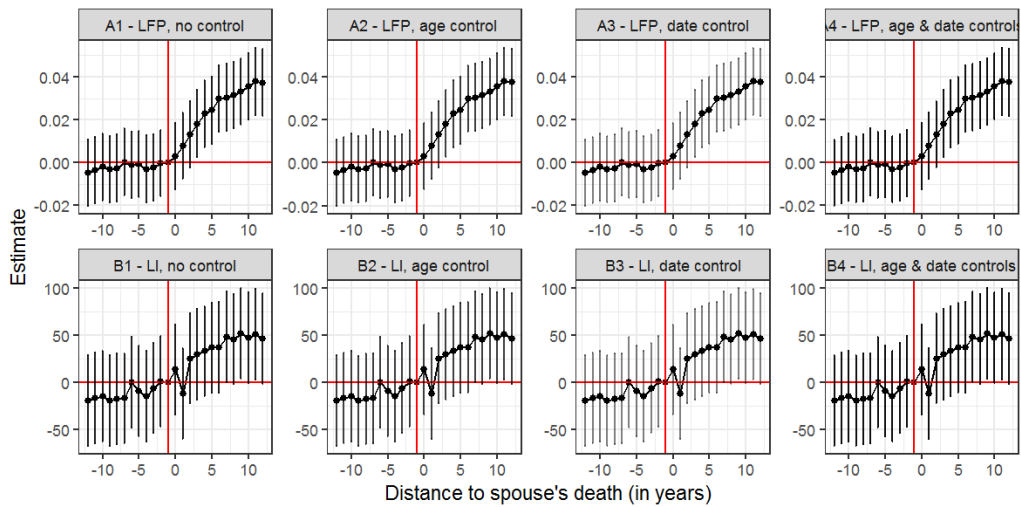
(a) Semi-annual basis
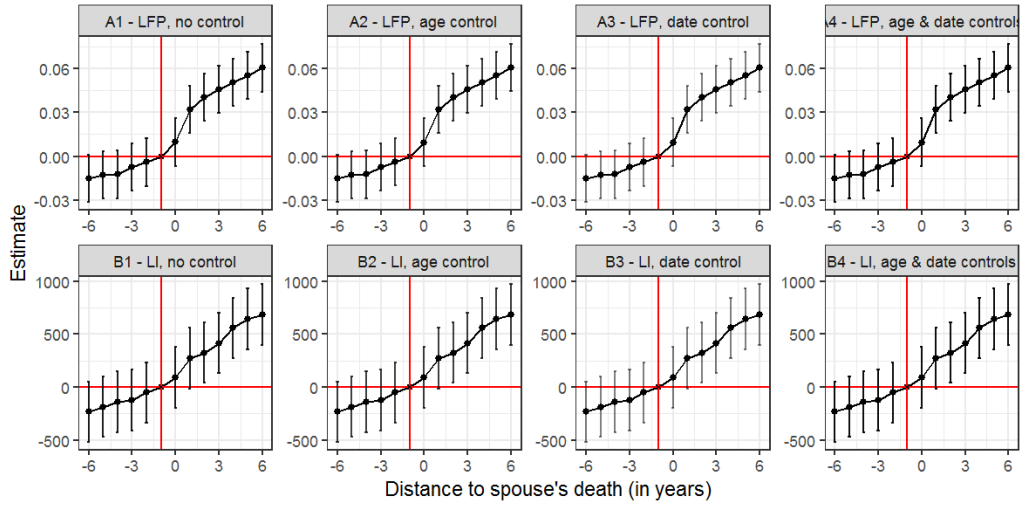
(b) Quarterly basis
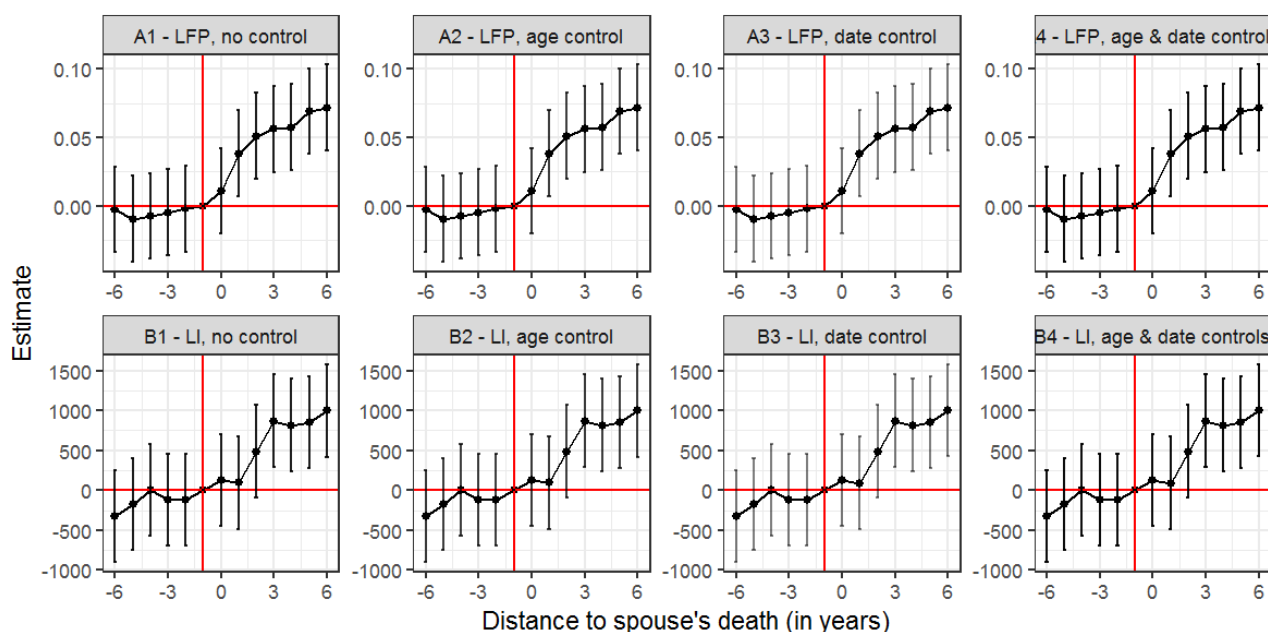

(c) Monthly basis

NOTE: Labor income refers to wage income, profits from self-employment and income from other activity and labor force participation is a dummy variable equal to 1 if individual has positive labor income and 0 otherwise.
SCOPE: Widows born between 1947 and 1952 whose husband died after 2003 and were aged below 62 at death time.
SOURCE: CBS.

Figure C.11: Event study estimates for robustness test (pension reform)
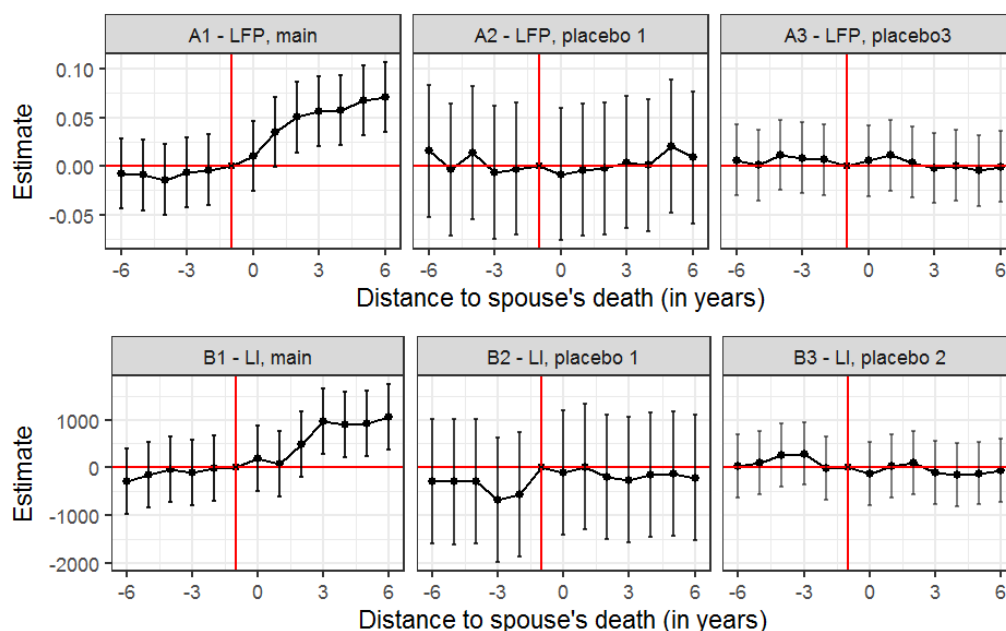
NOTE: Labor income refers to wage income, profits from self-employment and income from other activity and labor force participation is a dummy variable equal to 1 if individual has positive labor income and 0 otherwise.
SCOPE: Widows born between 1947 and 1952 whose husband died after 2003 and were aged below 62 at death time.
SOURCE: CBS.

Figure C.12: Event study estimates for placebo analyses

NOTE: LI stands for labor income and LFP stands for labor force participation. Labor income refers to wage income, profits from self-employment and income from other activity and labor force participation is a dummy variable equal to 1 if individual has positive labor income and 0 otherwise.
In (main) specification, estimation is carried out on the 1948 to 1952 cohorts, were individuals born before 1950 are the non-treated group. In (placebo 1) specification, estimation is carried out on the 1946 to 1949 cohorts were individuals born before 1948 are the non-treated group. In (placebo 2) specification, estimation is carried out on the 1950 to 1953 cohorts were individuals born before 1952 are the non-treated group.
SCOPE: Widows born between 1947 and 1952 whose husband died after 2003 and were aged below 62 at death time.
SOURCE: CBS.

Figure C.13: Event study estimates per quartile of wealth

NOTE: LI stands for labor income and LFP stands for labor force participation. Labor income refers to wage income, profits from self-employment and income from other activity and labor force participation is a dummy variable equal to 1 if individual has positive labor income and 0 otherwise.
Q1 to Q4 are wealth quartiles. Q1 corresponds to the 25 % of individuals with the lowest wealth.
SCOPE: Widows born between 1947 and 1952 whose husband died after 2003 and were aged below 62 at death time.
SOURCE: CBS.