



**HAL**  
open science

# Survivors Benefits and Conjugal Behavior. Evidence from the Netherlands

Julie Tréguier, Simon Rabaté

► **To cite this version:**

Julie Tréguier, Simon Rabaté. Survivors Benefits and Conjugal Behavior. Evidence from the Netherlands. 2021. halshs-03156317

**HAL Id: halshs-03156317**

**<https://shs.hal.science/halshs-03156317>**

Preprint submitted on 2 Mar 2021

**HAL** is a multi-disciplinary open access archive for the deposit and dissemination of scientific research documents, whether they are published or not. The documents may come from teaching and research institutions in France or abroad, or from public or private research centers.

L'archive ouverte pluridisciplinaire **HAL**, est destinée au dépôt et à la diffusion de documents scientifiques de niveau recherche, publiés ou non, émanant des établissements d'enseignement et de recherche français ou étrangers, des laboratoires publics ou privés.

# Survivors Benefits and Conjugal Behavior

## Evidence from the Netherlands

Julie Tréguier \*

Simon Rabaté †

**PRELIMINARY VERSION, PLEASE DO NOT CITE**

### 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 of the 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 from the cut in survivors benefits.

---

\*Institut national d'études démographiques (Ined), Institut des politiques publiques (IPP) and EconomiX, University of Paris Nanterre.

†Centraal Plan Bureau (CPB) Netherlands Bureau for Economic Policy Analysis.

# 1 Introduction

Survivors benefits (SB) are the benefits served to a survivor following their spouse's death. These benefits are usually linked to the deceased spouse's pension benefits or the pension benefits they would have benefited from if they had not died.<sup>1</sup>

Several economic mechanisms are at stake when talking about survivors benefits. First, as SB are linked to marriage contract – you have to be married to be eligible – it generates a financial incentive to marry. 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. 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. Given the potential importance of SB schemes in marital behavior, their reforms can have indirect and perhaps unattended consequences over individual consequences over conjugal behavior. This is the question we are studying in this paper.

More precisely, we exploit the 1996 Dutch survival 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 this 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.

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 labour 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 labour 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 survival 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 time of reform announcement. She shows couples advanced their marriage date in order to be eligible to

---

<sup>1</sup>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.

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.

The last part of the paper relates to another scarce but booming literature studying the effect of SB and their reforms on survivors' labour 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 labour 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 labour force participation. Böheim & Topf (2021) also find a large increase in labour 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' labour 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. More importantly, they study the effect of the whole reform – means test and eligibility restriction – when we focus on the latter dimension. We are then able to identify a pure income effect, when the papers mentioned above study reforms which can also generate substitution effects, as the implementation of means tests reduce the return to work for surviving spouses.

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 Data and institutional background

### 2.1 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 labour, 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 1 presents descriptive statistics of the data and variables we use in the paper.

Table 1: Descriptive statistics per cohort of birth

	1947	1948	1949	1950	1951	1952
Total number of individuals	6162502	5831594	5670236	5600603	5545445	5739665
Married at 42 women						
Number of individuals	97875	91623	87698	85622	84107	85975
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	97342	91623	88039	86400	85075	87262
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.

### 2.2 Institutional context

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.<sup>2</sup> First pillar is the old age pension benefits (AOW, *Algemene Ouderdomswet*). It provides a flat amount for all pensioners reaching AOW-age: 70 % of gross minimum wage for singles and 100 % for a couple.<sup>34</sup> Beyond AOW-age, individuals are no longer eligible for benefits from other social insurance programs like survivors insurance.

The 1996 reform replaced the General Widow and Orphans Benefits Act by the General Survivors Act. The two acts are different in the benefits offered and in the population they cover. The new act was enacted in July, 1st 1996.<sup>5</sup> A transition period allows individuals born before January, 1st 1950 to benefit from the new survivors benefits even if they do not meet any criteria.

**Pre-reform survivors insurance.** The General Widow and Orphans Benefits Act (AWW, *Algemene Weduwen- en Wezenwet*) was enacted in 1959. Initially dedicated to women, men became eligible in 1988. Survivors insurance was tied to the marriage contract.<sup>6</sup> A widow(er) was eligible to survivors' benefits if (i) he or she was 40 years old or more, (ii) had at least a child under the age of 18 or (iii) suffered from inability to work. In some cases, survivors were eligible to AWW benefits even if they had divorced from the deceased. The condition was to be eligible to AWW benefits both at the time of the divorce and at the time of death.

Following the death of a spouse, the survivor got 100 % of a gross minimum wage for those caring for a child under the age of 18 or 70 % a of gross minimum wage otherwise. Benefits were cut as soon as the survivor got 65 (become eligible to AOW) or he or she remarried.

**The reform.** Several objectives led to the development of the new act. First, the evolution of conjugality have made the AWW obsolete. The 1959 act was in line with the clear division of labour between men and women of these times. Since then, the breadwinner model is outdated, fewer people marry and more couples are cohabitants. Moreover, there is a move towards an individualization of pension rights objective. Finally, policy makers have a saving money purpose.

The General Survivors Act (Anw, *Algemene Nabestaandenwet*) was adopted on December, 21st 1995, enacted on July, 1st 1996 and replaced AWW.

**Post-reform survivors insurance.** 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

---

<sup>2</sup>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).

<sup>3</sup>The AOW-age was fixed at 65 up to and including 2012, it started increasing stepwise from 2013 onward ([Atav et al. 2019](#)).

<sup>4</sup>The full benefits are served to individuals who have been living at least 50 years in the Netherlands and is reduced by 2 % for each missing year.

<sup>5</sup>It nevertheless has been modified twice between 1996 and 1998.

<sup>6</sup>Civil-unions and cohabitants were not eligible to any survivors' benefits.

inability to work (at least 45 % disabled). Divorcees are eligible to Anw benefits 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.

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). Benefits are cut as soon as the survivor gets AOW-age, or the child gets 18 or the survivor forms a new household. Moreover, 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.<sup>7</sup> 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 labour 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. For former AWW beneficiaries – i.e. individuals whose spouse died before July, 1st 1996 – a higher release apply, namely 70 % of minimum wage plus one third of the excess. Moreover, these beneficiaries are always entitled to at least 30 % of gross minimum wage. Before 2013, the 70 % of a gross minimum wage part of survivors benefits was means tested while the half-orphan's benefits was fully served, no matter the beneficiary's level of income. These benefits were abolished with the law of 26, March 2013. In return, survivors' benefits were increased to 90 % of gross minimum wage for beneficiaries caring for a child. This translates into a means-test half-orphan's benefit. 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.

**Partial exemptions.** The conditions to be entitled to survivors insurance have become very strict and few people are eligible under Anw. A derogation was then granted for individuals born before January, 1st 1950. They remained eligible to survivors benefits even if they do not meet any criteria. However, they are subject to the new means test. 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). A series of transitional rules are also enacted (appendix A).

Table 2 summarizes the main differences between the two acts. The first column presents the pre-reform situation (AWW), the second and third present the final legislation (Anw). Details on transition legislation can be found in appendix A.

**Confounding reforms.** We identify three confounding reforms. The first one is the partner pension reform (Tyros et al. 2018). In 1996 was announced the cut of the partner supplement, that

---

<sup>7</sup>Until January, 1st 2011, only labour 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).

Table 2: Survivors' benefits legislation

		AWW		ANW	
				born before 1950*	born after 1950
<b>Eligibility</b>	<b>Matrimonial status</b>	marriage	marriage or partnership or cohabitation		marriage or partnership or cohabitation
	<b>Individual charac.</b>	+40 y.o.			minor child
<b>Benefits</b>	<b>If minor child</b>	100 % min. wage	70% + 20% min. wage	90% min. wage	
	<b>If no child</b>	70% min. wage	70% min. wage		
	<b>Means test</b>	No	Yes**		Yes**
	<b>Stop when</b>	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

came into effect on April, 1st 2015. Until then, if at the time the older partner reached AOW-age, the younger one had an income below a certain amount, then the first would receive an AOW supplement on top of his or her AOW pension. After April, 1st 2015, the older partner would not receive the supplement any more. As the change in law could potentially lead to a substantial drop in income for some households, the Dutch government decided to provide a large announcement period before the law was enacted.<sup>8</sup> The second confounding reform relates to the occupational pension reform. It closed early retirement i.e large negative shock on pension wealth before/after date of birth January, 1st 1950. Finally, by 1996, AOW benefits were indexed on wages.

**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 divorce 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.

<sup>8</sup>There was a 20 years period between the law was announced and came into effect, as the loss for household could reach a maximum of 1,411 monthly 2015 euros perceived on average during seven years.

### 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.<sup>9</sup>

After the reform, survivors benefits eligibility become conditional on caring for a minor child and is means tested. 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.

#### 3.2 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

---

<sup>9</sup>We focus on women because widows are mostly women, because of their higher life expectancy and age difference with their spouse.

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.<sup>10</sup> The second population is composed of married at 42 years old women and we look at their divorce probability between 43 and 64.<sup>11</sup> Our two panels are balanced.

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  $\Delta_d$  the reform effect on Dutch divorce probability.

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

where  $D^{t_0}$  is divorce probability before the reform,  $D^{t_1}$  is divorce probability after the reform and  $T$  is 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:

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

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.

$\delta$  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 labour supply.<sup>1213</sup>

---

<sup>10</sup>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.

<sup>11</sup>Age 42 is chosen as corresponding to age reached by the eldest cohort during year 1990.

<sup>12</sup>Income level is equal to income at 52 as the youngest age at each information is available for the eldest cohort of our analysis.

<sup>13</sup>Descriptive statistics of the control variable for each population of interest are presented in table 1.

## 4 Results

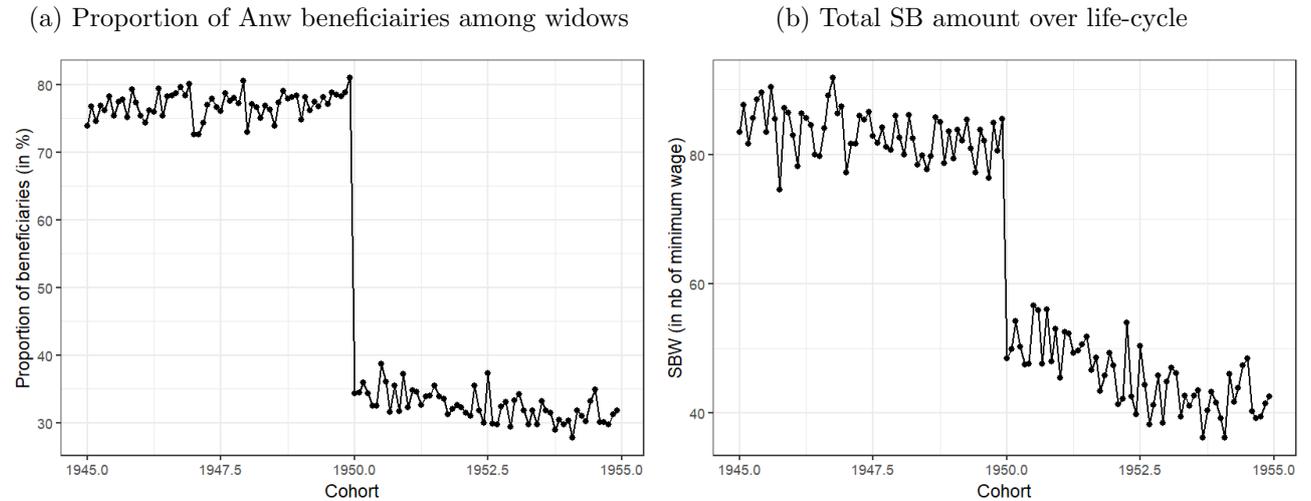
### 4.1 Graphical evidence

Figures 1a and 1b give graphical evidence of the effect of the reform in terms of survivors insurance.

Figure 1a shows the proportion of SB beneficiaries among widows per cohort. We verify there is a large drop in this proportion from 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 1b, 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 1: Information on survivors insurance beneficiaires, per cohort



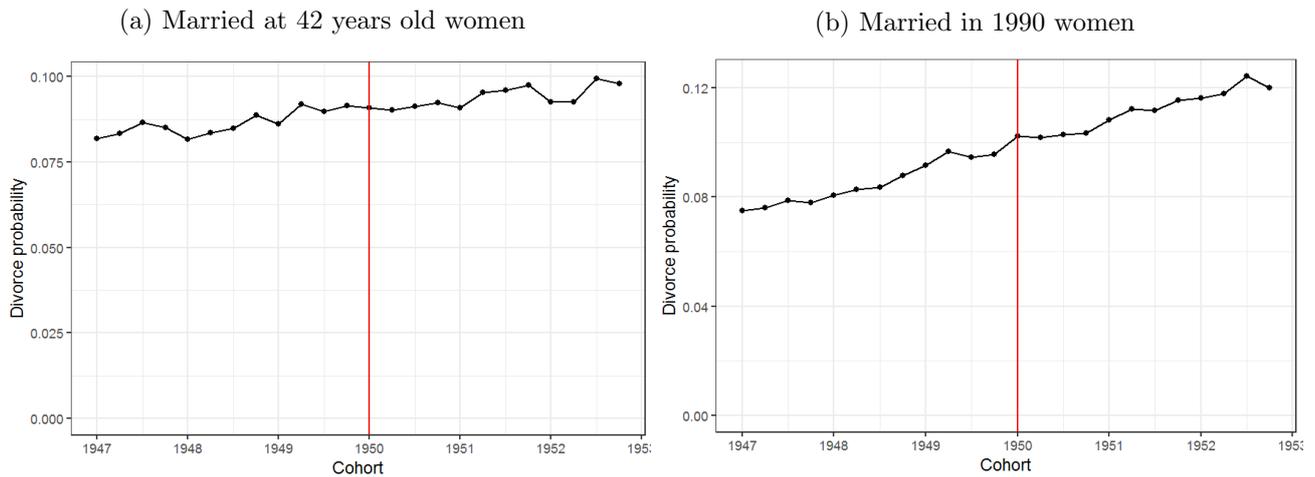
NOTE: SBW stands for survivors benefits wealth, defined as total SB amount over the life-cycle.

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 beneficiaires born between 1945 and 1955.

SOURCE: CBS.

Figures 2a and 2b 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 across cohorts, consistent with the divorce probability upward trend over years at this period. Divorce probability between 43 and 64 years old for women who were married at 42 is equal to 8 % for 1947 cohort and is equal to 10 % for 1952 cohort. We conversely do not see any discontinuity at the 1950 cohort threshold.



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.

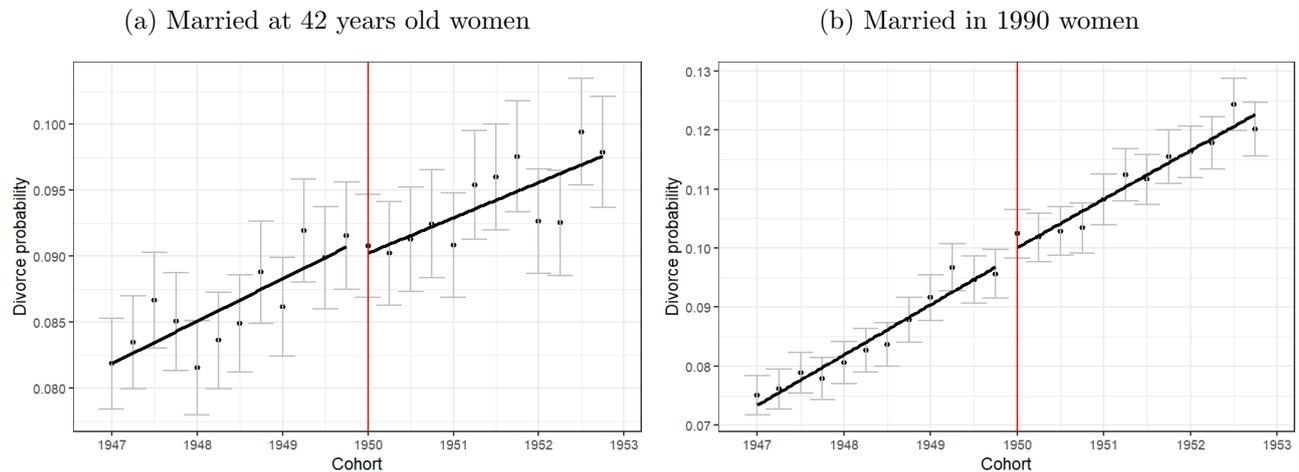
SCOPE: Married at 42 years old women and married in 1990 women, respectively.

SOURCE: CBS.

## 4.2 Empirical results

RD-graphs are presented on figures 3a and 3b. For each of our populations of interest, they present divorce probability according to cohort of birth, with confidence intervals. 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 3: RD-graphs for divorce probability



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.

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)
$\delta_d$	-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 quarterly 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 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 than 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 than 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 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 [D.1](#) and [D.2](#) in appendix [C.1](#) 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 [D.1](#) in appendix [C.1](#), results also show zero-effect estimates. We nevertheless go further, looking at robustness and heterogeneity.

Table 4: Regression discontinuity estimates with controls

	Married at 42 years old	Married in 1990
$\delta_d$	-0.00098 (0.00158)	0.00154 (0.00163)
Age difference between spouses	0.00303 (0.00011)	0.00314 (0.00011)
Cares for a minor child	0.00609 (0.00087)	0.00079 (9e-04)
Marriage duration	0.00085 (8e-05)	0.00133 (9e-05)
Income level	0.03157 (0.00044)	0.03579 (0.00045)
Number of obs.	524679	528459

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.

### 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  $\kappa$  % chance of producing an impact estimate that is statistically significant at the  $\alpha$  level (Bloom 1995).  $\kappa$  is the statistical power, usually equal to 20 % in the literature and  $\alpha$  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  $\delta_d$  (Bloom 1995):

$$MDE = M \cdot \hat{\sigma}_\delta \quad (3)$$

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  $\delta_d$  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 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	524679	-0,00177	-1,99
Married in 1990	0,00154	0,00163	528459	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 treatment 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-cycle indicator, which takes into account each of these three dimensions: we compute women's survivors benefits wealth (SBW).<sup>14</sup>

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

where  $SBW_i$  is individual  $i$ 's survivors benefits wealth,  $t_d$  is husband's date of death,  $\beta$ : 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 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

<sup>14</sup>As individuals' income is available from 1999 only, in practice we compute SBW at 52 as it is the age of the elder cohort in 1999. In particular, we do not compute SBW at each age.

SBW at period  $p$ , taking the partner's death probability, according to his age, into account.

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

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,

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

and

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

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

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,

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

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

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 D.3 and D.4 in appendix C.2 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 D.5 and D.6 in appendix C.3 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.<sup>15</sup>

Table D.2 in appendix C.4 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 idea 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 D.7 in appendix C.4 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 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.

Table 6: Regression discontinuity estimates according to treatment intensity

	Married at 42 years old				Married in 1990			
	Q1	Q2	Q3	Q4	Q1	Q2	Q3	Q4
$\delta_d$	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.

### 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

<sup>15</sup>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.

were allowed to take out an Anw gap pension insurance when available in their firm.<sup>16</sup> This Anw gap insurance insured the employee’s surviving 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 D.3 in appendix C.4 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 D.8 in appendix C.4 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 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 7. All estimates are close to 0 and not statistically different from 0.

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
$\delta_d$	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.

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

<sup>16</sup>According to the National Remuneration Survey 2014, approximately 2/3 of firms were insured for the Anw gap.

## 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 \(2020\)](#), which 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, 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 labour 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 in more depth this last channel, and try to find evidence of an income substitution to SB loss, by mean of higher labour force participation or program substitution.

### 5.2 Labor supply effect of the reform

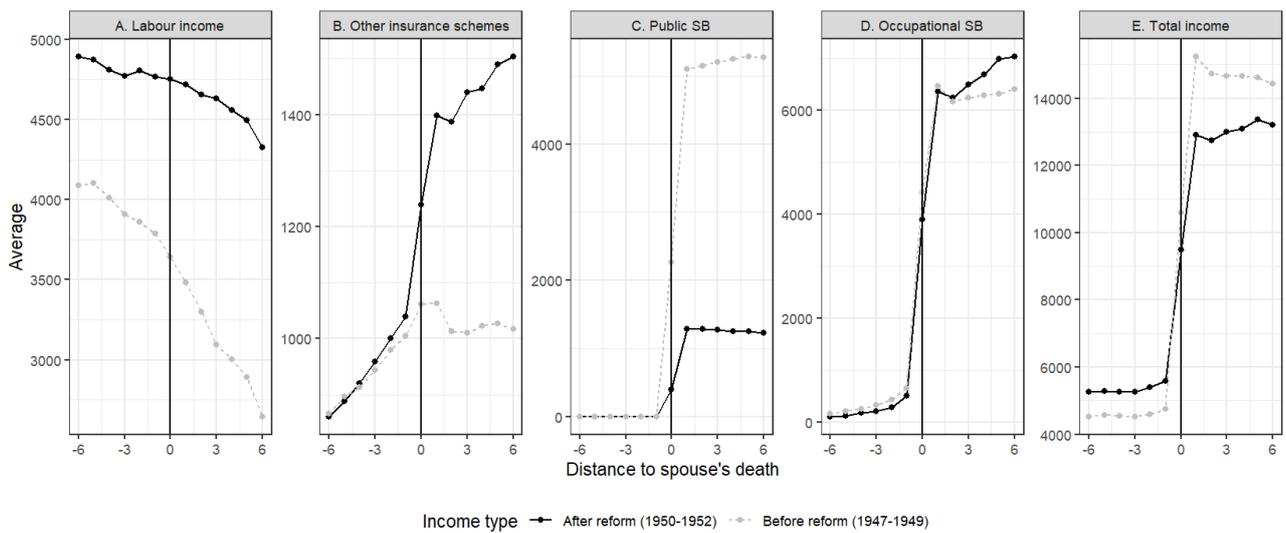
#### 5.2.1 Graphical evidence

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 4](#) presents widows' income level according to distance to spouse's death for different types of income. Labour 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, labour and welfare income are higher for treated cohorts after spouse’s death. Following spouse’s death, labour income decreases in a lower proportion for treated cohorts than for the non-treated ones, by comparison to the pre-death situation. Overall, higher labour 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 4: Income level according to distance to spouse death

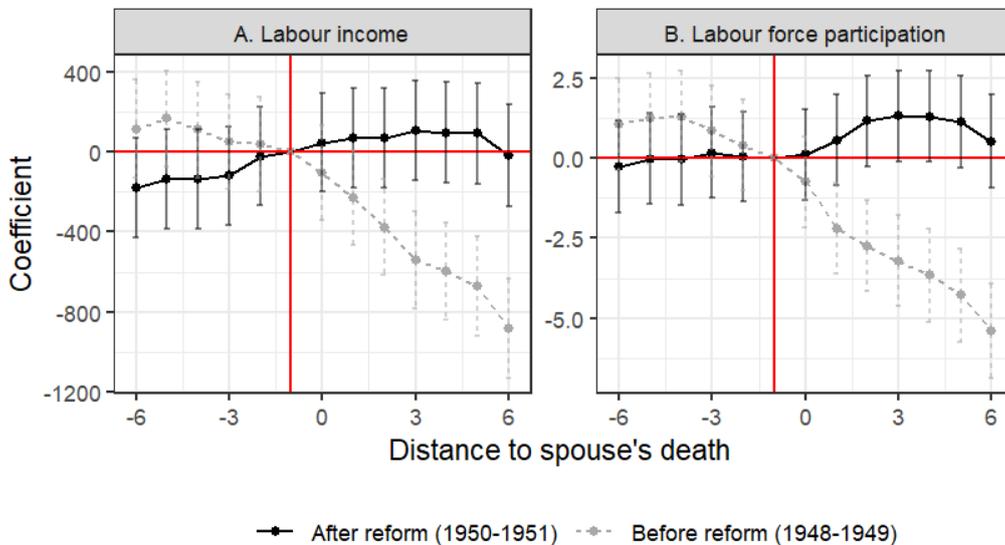


NOTE: Labour income refers to wage income, profits from self-employment and income from other activity, occupational SB are computed as the difference between public SB (Anw) and total pension, other insurance schemes income refers to social welfare benefits, unemployment benefits and sickness/disability 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 5 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 labour supply, both in terms of income (panel A.) and labour force participation (panel B.). Three years after spouse death, widows born before 1950 have reduced their labour 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 labour 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 5: Linear model estimates according to distance to spouse death



NOTE: Estimation is carried out on a semi-annual basis.

Labour income refers to wage income, profits from self-employment and income from other activity, welfare income refers to social welfare benefits, unemployment benefits and sickness/disability benefits 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 were aged below 62 at death time.

SOURCE: CBS.

## 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 labour supply outputs between a treated group (individuals born after 1950) and a control group (individuals born before 1950). We successively use two independent variables: labour income and labour force participation.

Labour 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:

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

where  $Y_{its}$  is the independent variable (labour income and labour 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,  $X_{it}$  is a vector of age and date controls and  $\epsilon_{its}$  is a random noise.  $\delta_{e1}$  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 labour supply on the period, we also estimate a simple two periods difference-in-differences model in some specifications:

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

where  $Y_{it}$  is the independent variable (labour income or labour 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,  $X_{it}$  is a vector of age and date controls and  $\epsilon_{it}$  is random noise.  $\delta_{e2}$  captures the effect of the reform on the treated individuals.

**Main results.** Figure 6 presents the results of the estimation of equation (4) using OLS, for labour income and labour force participation. As expected, before death event, estimates are non significantly different from zero. After spouse death, labour income and labour force participation are significantly higher than reference level. Three years after death, labour income has increased by 700 euros while labour 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, labour income increased by 550 euros as a result of the SB reform while labour 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 event, SB reform effect represents a 11.5 % increase of labour income and a 10.8 % increase in labour force participation.

The effects we estimate are of slightly larger magnitude than in related literature. Giupponi (2019) finds a 7 % increase of labour 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 labour force participation as the immediate

Figure 6: Event study estimates for labour income and labour force participation



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. 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.

post-shock consequence of receiving SB in the U.S.<sup>17</sup> We measure a pure income shock because we estimate the effect of eligibility restriction to individuals born before 1950 compared to the others. Means test applies to the whole population and we are thus free of substitution effect.<sup>18</sup> It may explain the differences with [Böheim & Topf \(2021\)](#) paper in which reform also made the means test stricter. Indeed, [van der Vaart et al. \(2020\)](#) give evidence of a substitution effect induced by the introduction of the means test in the Netherlands: the 1996 reform discouraged working, particularly for women with a high pre-widowhood income. In other words, we do not take into account substitution effect, that would otherwise have lowered reform income shock effect on labour force participation. [Giupponi \(2019\)](#) gives evidence that in her setting, there is no substitution effect. Nevertheless, and conversely to ours, she carries out her analyses on a restricted sample of the population: widows whose income is lower than the SB means test threshold. [Fadlon et al. \(2019\)](#), who estimate the effect eligibility age on labour 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 labour 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 period, while [Giupponi \(2019\)](#) identifies effects up to 15 years after the spouse's death and [Böheim & Topf \(2021\)](#)

<sup>17</sup>[Fadlon et al. \(2019\)](#) estimate a 2.87 percentage points increase of labour 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.

<sup>18</sup>SB means test discourages working, especially for individuals with high income who benefit more from reducing their labor supply. They moreover have more financial possibilities to reduce labor supply.

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
$\delta_e$	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.<sup>19</sup> 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 labour 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 labour income for treated group and a 5.5 percentage points increase on labour force participation.

Finally, we build two placebo analyses. In the first one (placebo 1) we consider 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 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, labour supply adjustment can be attributable to a lack of liquidity at spouse death and then an inability to smooth standards of living at death other than investing labour market (Fadlon et al. 2019).

<sup>19</sup>Fully-interacted DiD graphs for robustness checks and placebo analyses are shown in figures D.10, D.11 and D.12 in appendix C.5.

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)
$\delta_e$	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)
$\delta_e$	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)
$\delta_e$	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.

We divide our population into four groups according to wealth quartile. We expect the richest to

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)
$\delta_e$	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, where individuals born before 1950 are the non-treated group. In (placebo 1) specification, estimation is carried out on 1946 to 1949 cohorts where individuals born before 1948 are the non-treated group. In (placebo 2) specification, estimation is carried out on 1950 to 1953 cohorts where 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.

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 D.13 in appendix C.5. Both for labour income and labour 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 labour income, SB reform increased average income by 600 euros and 790 euros for first and second quartile, respectively. Individuals belonging to the fourth quartile have increased their average labour income by 480 euros following the reform while those belonging to the third one increased their income by 390 euros only. For labour 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 labour income by more than 23 % and their labour 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 are partially exempted. They remain eligible even if they do not care for a child but their benefits are

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)
$\delta_e$	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.

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 labour 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 labour 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 labour 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.

## References

- Atav, T., Jongen, E., Rabaté, S. et al. (2019), ‘The effects of the increase in the retirement age in the netherlands.’, *CPB Discussion Papers* .
- Baker, M., Hanna, E. & Kantarevic, J. (2004), ‘The married widow: Marriage penalties matter!’, *Journal of the European Economic Association* **2**(4), 634–664.
- Bitler, M. P., Gelbach, J. B., Hoynes, H. W. & Zavodny, M. (2004), ‘The impact of welfare reform on marriage and divorce’, *Demography* **41**(2), 213–236.
- Bloom, H. S. (1995), ‘Minimum detectable effects: A simple way to report the statistical power of experimental designs’, *Evaluation review* **19**(5), 547–556.
- Böheim, R. & Topf, M. (2021), ‘Unearned income and labor supply: Evidence from survivor pensions in austria’.
- Brien, M. J., Conlin, S. & Weaver, D. A. (2004), ‘Widows waiting to wed? (re)marriage and economic incentives in social security widow benefits’, *Journal of Human Resources* **39**(3), 585–623.
- Dickert-Conlin, S. & Meghea, C. (2004), ‘The effect of social security on divorce and remarriage behavior’, *Boston College, Center for Retirement Research Working Paper* **2004-09**.
- Dillender, M. (2016), ‘Social security and divorce’, *The BE Journal of Economic Analysis & Policy* **16**(2), 931–971.
- Doove, S., van Anandel, W., Gommans, F. & Slager, T. (2018), *Inkomenspositie anw-ontvangers en overige nabestaanden*, Technical report, Statistics Netherlands.
- Fadlon, I., Ramnath, S. & Tong, P. (2019), ‘Market inefficiency and household labour supply: Evidence from social security’s survivor benefits’, *NBER Working Paper Series* (25586).
- Fitzgerald, J. M. & Ribar, D. C. (2004), ‘Transitions in welfare participation and female headship’, *Population Research and Policy Review* **23**(5-6), 641–670.
- Frimmel, W., Halla, M. & Winter-Ebmer, R. (2014), ‘Can pro-marriage policies work? an analysis of marginal marriages’, *Demography* **51**(4), 1357–1379.
- Gilbert, N. (2017), *Targeting social benefits: International perspectives and trends*, Routledge.
- Giupponi, G. (2019), ‘When income effects are large: labor supply responses and the value of welfare transfers’.
- Goda, G. S., Shoven, J. B. & Slavov, S. N. (2007), *Social security and the timing of divorce*, Technical report, National Bureau of Economic Research.

- Imbens, G. & Kalyanaraman, K. (2012), 'Optimal Bandwidth Choice for the Regression Discontinuity Estimator', *Review of Economic Studies* **79**(3), 933–959.
- Kabátek, J. (2018), 'Divorced in a flash: The effect of the administrative divorce option on marital stability in the netherlands'.
- Loozen, S. & van Huis, M. (2010), Thirty thousand couples separated through flash divorce, Technical report, Statistics Netherlands.
- Persson, P. (2020), 'Social insurance and the marriage market', *Journal of Political Economy* **128**(1), 000–000.
- Tyros, S., Stevens, R. & van Ewijk, C. (2018), 'Retirement planning and financial incentives: The impact of announced cuts in pensions'.
- van der Vaart, J., Alessie, R. & van Ooijen, R. (2020), 'Economic consequences of widowhood', *Netspar Design Paper* (160).

## A Institutional background complements

**The transition.** AWW beneficiaries were transferred to Anw. However, transition rules were enacted to ensure that people do not suddenly lose their benefits.<sup>20</sup>

- AWW beneficiaries become Anw beneficiaries and :
  - benefits are not cut even if there is no incapacity for work or a child under 18 years of age ;
  - the means test apply from 01.01.1998 for survivors born before 31.12.1940 ;
  - the means test only applies from 01.01.1998 for those born after 01.01.1940 and then 30 % of the benefits are eventually deducted from benefits.
- If the survivor form a new household, the benefits are cut, but as of 01.01.1998.
- For all concerned (above) survivors who have a child under 18 years of age, the amount of benefits is increased to 80 % (instead of 70 %, which makes a total of 100 % with the orphan benefits, and brings the total to the same amount of 100 % of the minimum wage, as for AWW benefits). The total amount of benefits is nevertheless not uprated until it reaches the current total rate of 90 %.

In 1996 and 1998, two adjustments took place.<sup>21</sup> According to [Gilbert \(2017\)](#), 'benefits to middle and higher-income groups were severely cut; these groups put pressure on political parties to relax some of the restrictive conditions; the government conceded and altered the rules on some exemptions in response to these pressures'. The exemption concerns survivors whose spouse died within the three years following the law and for those who were not insurable on private markets.

### **04.07.1996 amendment:**

- Addition of article 66bis:
  - A survivor whose spouse dies within the 3 years following the law (July, 1st 1996), is considered as born before January, 1st 1950.
  - The person is considered as a surviving spouse if:
    - \* she was born between January, 1st 1950 and July, 1st 1956.
    - \* was married to the deceased the day he died.
    - \* if the death had occurred the day before the law was put in place, the person would have been eligible to the AWW.
- Amendment to article 67: If the survivor form a new household, the benefits are not cut as of January, 1st 1998 if the survivor was born before January, 1st 1940 but only reduced by 30 % of the minimum wage.

---

<sup>20</sup>Chapter 8, Articles 67 to 74 of 21.12.1995 law.

<sup>21</sup>The 04.07.1996 amendment and the 24.06.1998 decree.

**24.06.1998 decree:** A category of eligibility to the Anw is added: surviving partners born after January, 1st 1950 and before July, 1st 1956, married before July, 1st 1996 and at the time of death, and whose spouse died before July, 1st 1999. Of these survivors, those who are not insurable (on private markets) in the period between July, 1st 1996 and July, 1st 1999 (because they were seriously ill, for example) and who can prove it are eligible for Anw.

## 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 pseudonymized). 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

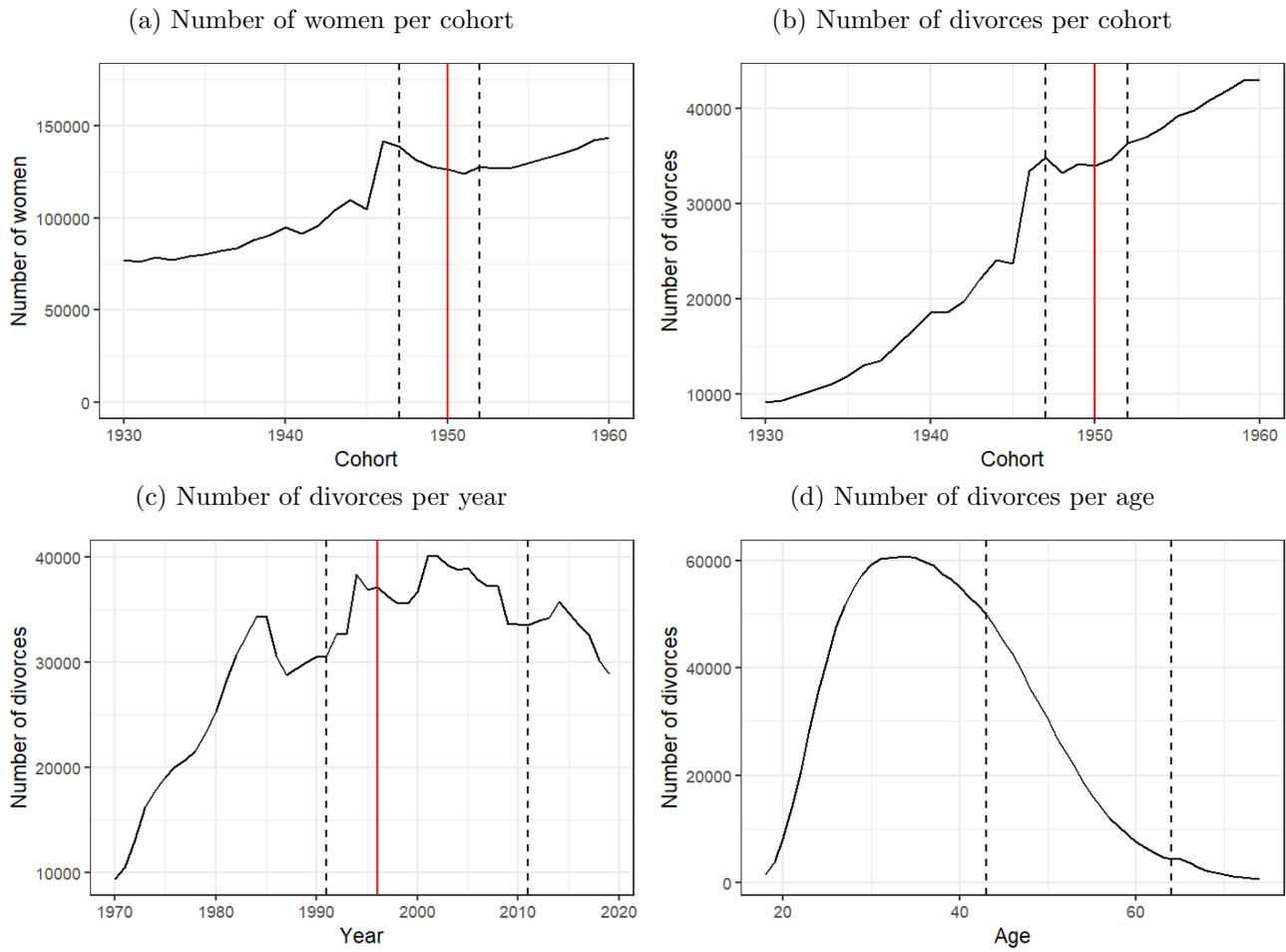
Content	Name of dataset	Source	
Date of birth and gender	GBAPERSON2019TAB (V1)	Population registers	
Death	GBAOVERLIJDENTAB2019TAB (V1)	Death records	
Civil status and date	GBABURGERLIJKESTAAT2019BUS (V1)	Population registers	
Households characteristics	GBAHUISHOUDENS2019BUS (V1)	SSB	
Linkage parent-child	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	SECMZLFMNCBDRAGBUSV20181	SSB	
Income from other activity	SECMOVACTMNCBDRAGBUSV20181	SSB	
Social welfare benefits	SECMBIJSTMNCBDRAGBUSV20181	SSB	
UI benefits	SECMWERKLMNCBDRAGBUSV20181	SSB	
DI and sickness benefits	SECMZIEKTAOMNCBDRAGBUSV20181	SSB	
Other social security benefits	SECMSOCVOORZOVNCBDRAGBUSV20181	SSB	
Pension income	SECMPEMNSIOENMNCBDRAGBUSV20181	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

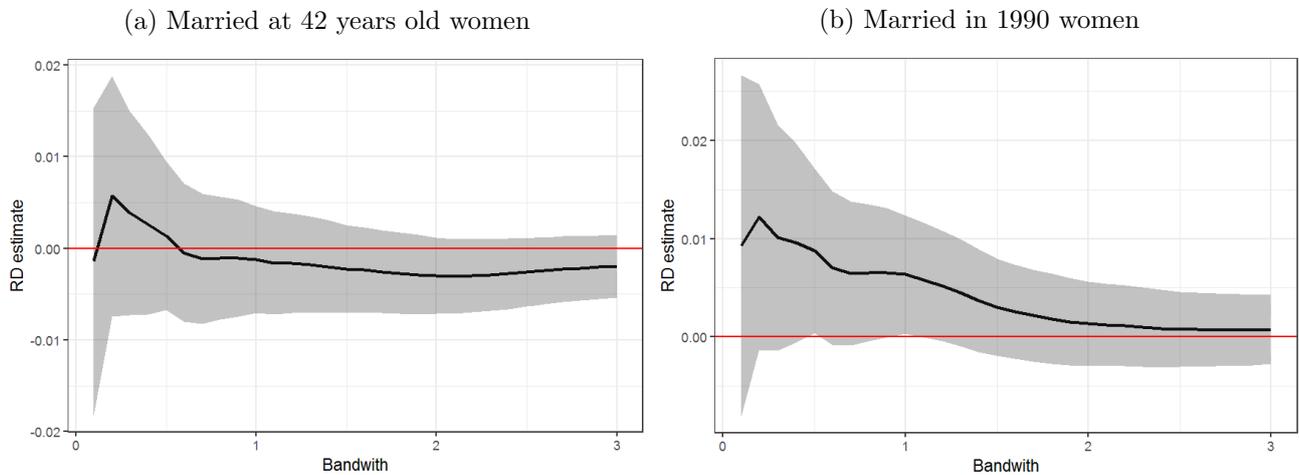


SOURCE: CBS.

## C Additional results

### C.1 Divorce probability analyses robustness check

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

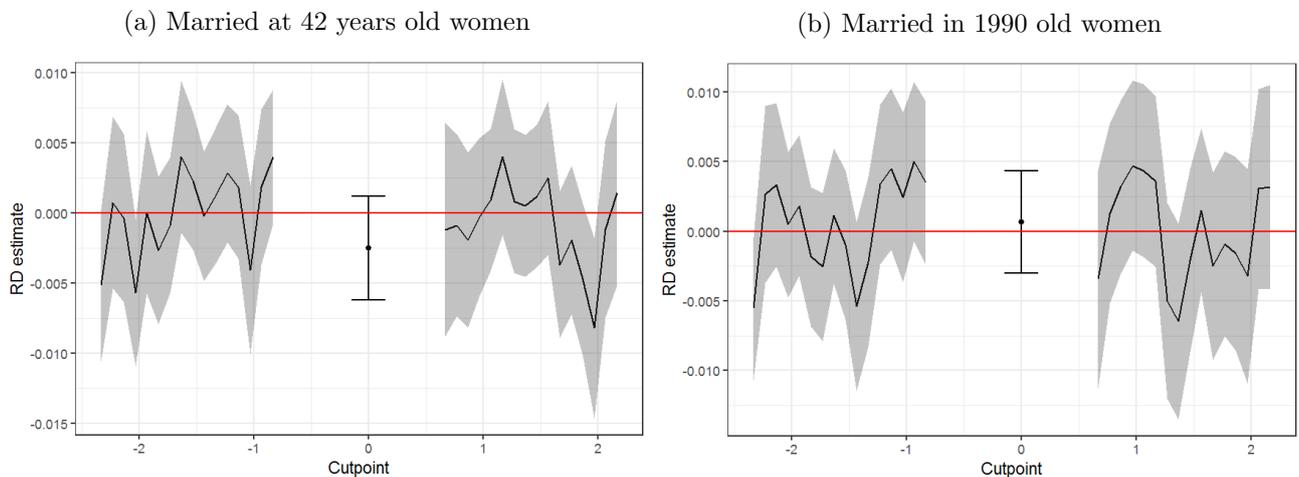


NOTE: For married at 42 women, probability of divorce is computed as the probability to divorce at least once between 43 and 64 years old. For married in 1990 women, probability of divorce is computed as the probability to divorce at least once between 1991 and 2011.

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

SOURCE: CBS.

Figure D.2: Placebo test for divorce probability



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 D.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)
$\hat{\delta}_d$	-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 D.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 D.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 differentiated according to

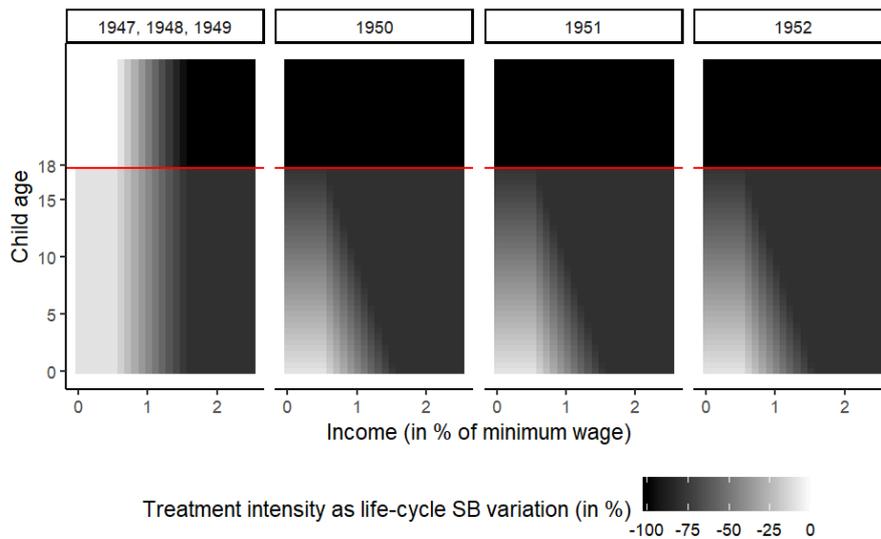
date of birth set up of the reform. F

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)

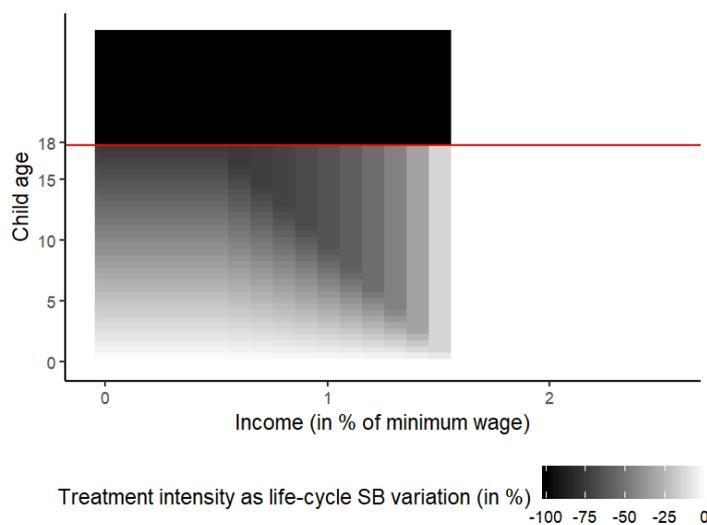
Overall, among individuals caring for a child, the most intensively treated individuals are those who have both an older child and moderate income level.

Figure D.3: Treated individuals according to characteristics



SOURCE: Authors' calculations.

Figure D.4: Treatment intensity according to characteristics

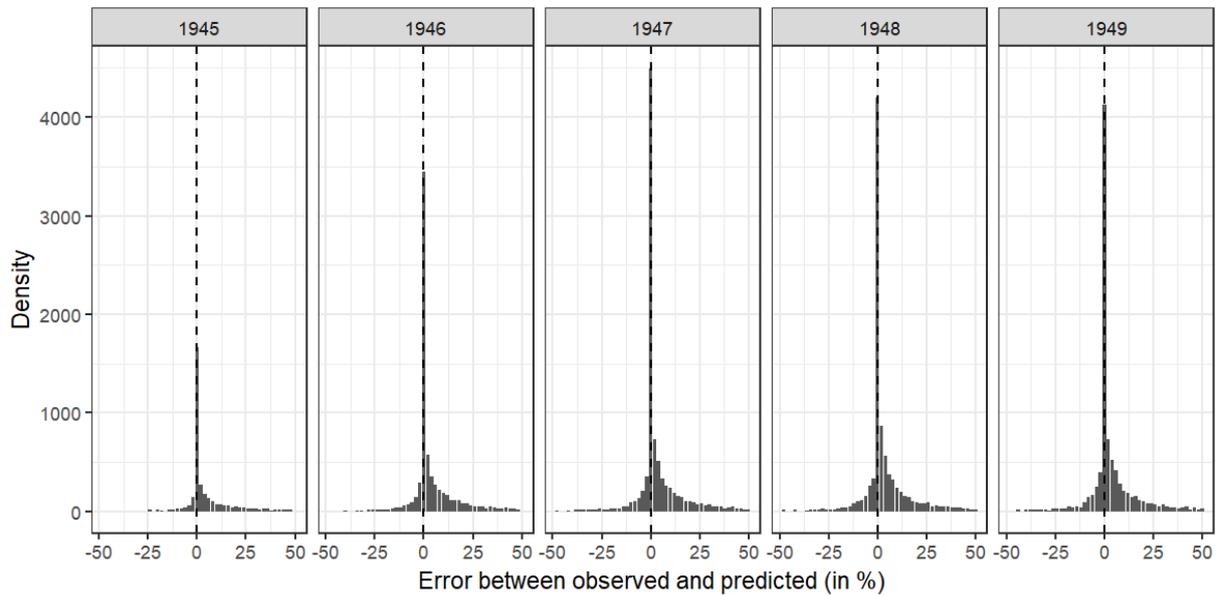


SOURCE: Authors' calculations.

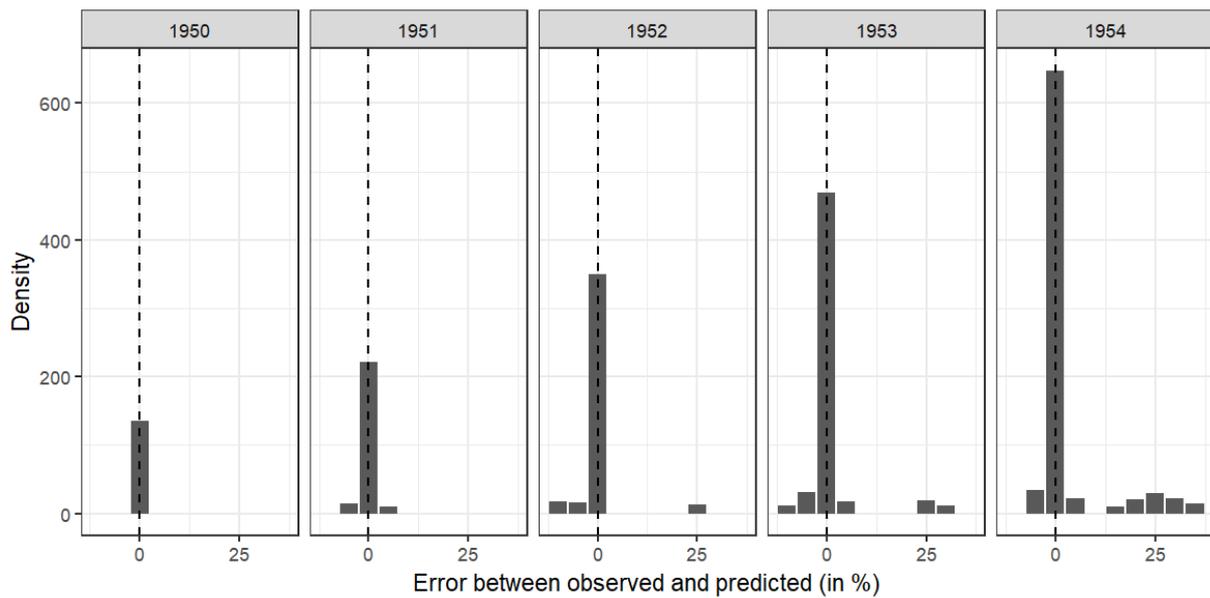
### C.3 Survivors benefits simulator assessment

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

(a) Spouses' benefits



(b) Spouses' and half-orphans' benefits

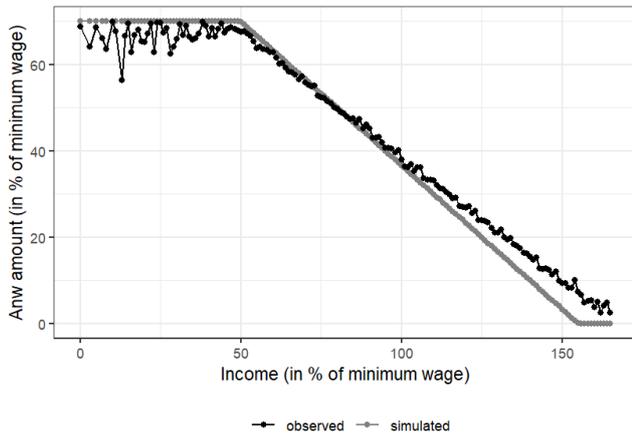


SCOPE: For spouses' benefits computation only, the scope is composed of female Anw beneficiaries born between 1945 and 1949 who do not care for a minor child. For spouses' and orphans' benefits together, the scope is composed of female Anw beneficiaries born between 1950 and 1954 who care for a minor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

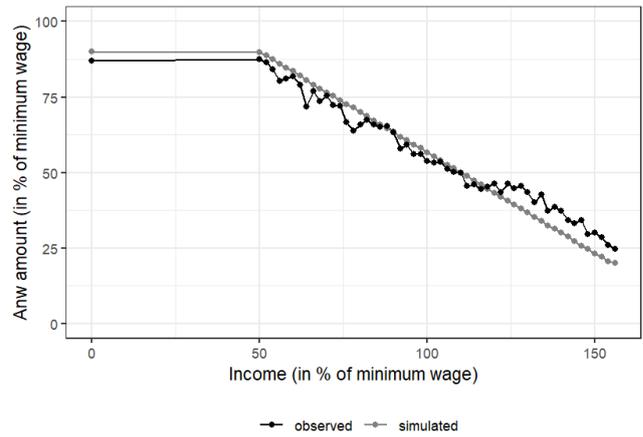
SOURCE: CBS.

Figure D.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 D.2: Descriptive statistics per treatment intensity group

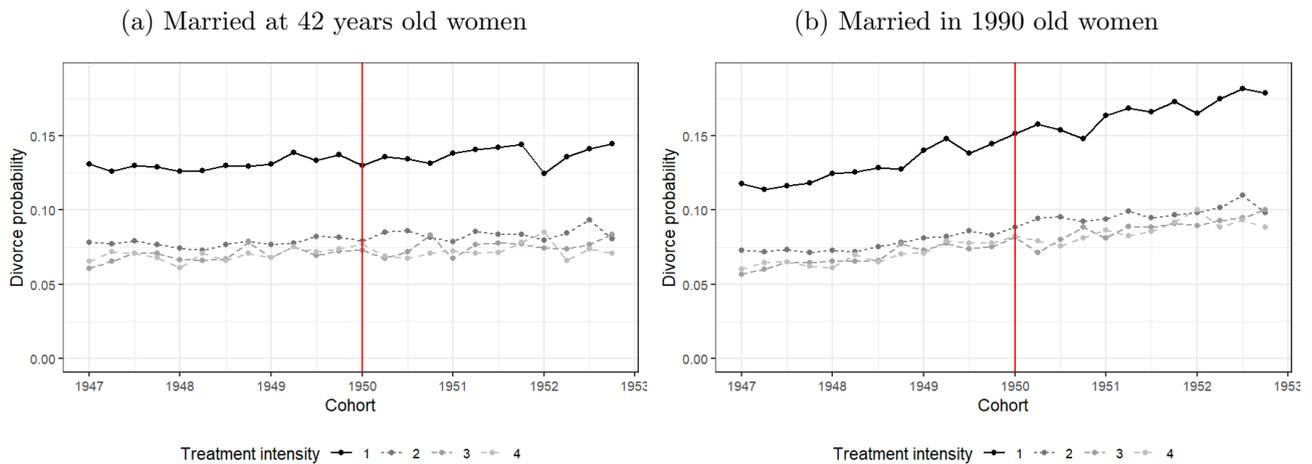
	Q1	Q2	Q3	Q4
<b>Married at 42 women</b>				
Number of individuals	137803	133074	131005	131015
Age difference with spouse	-2.149	-2.253	-2.497	-2.495
Marriage duration at 42	17.041	18.746	19.398	19.391
Having a minor child (in %)	73.227	74.643	66.524	66.186
Income (in % of min. wage)	119.534	41.756	34.73	34.688
Having divorced between 43 and 64	13.233	7.941	7.075	6.989
<b>Married in 1990 women</b>				
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 D.7: Divorce probability per cohort of birth and treatment intensity



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

	Private	Public	Subsidized
<b>Married at 42 women</b>			
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
<b>Married in 1990 women</b>			
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

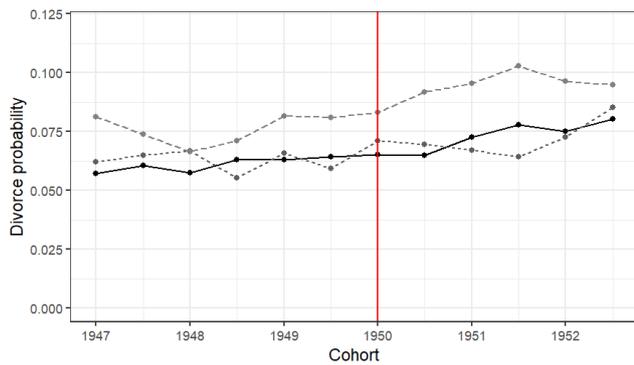
NOTE: The different columns refer to spouse's activity sector. Income is income at 54, younger age at which we have information for the eldest cohort.

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

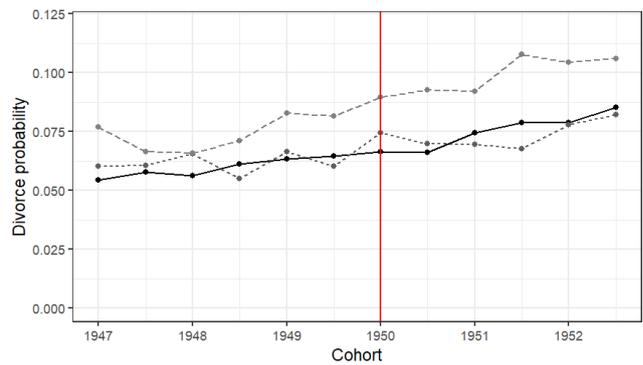
SOURCE: CBS.

Figure D.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



Spouse's activity sector — Private — Public — Subsidized

Spouse's activity sector — Private — Public — Subsidized

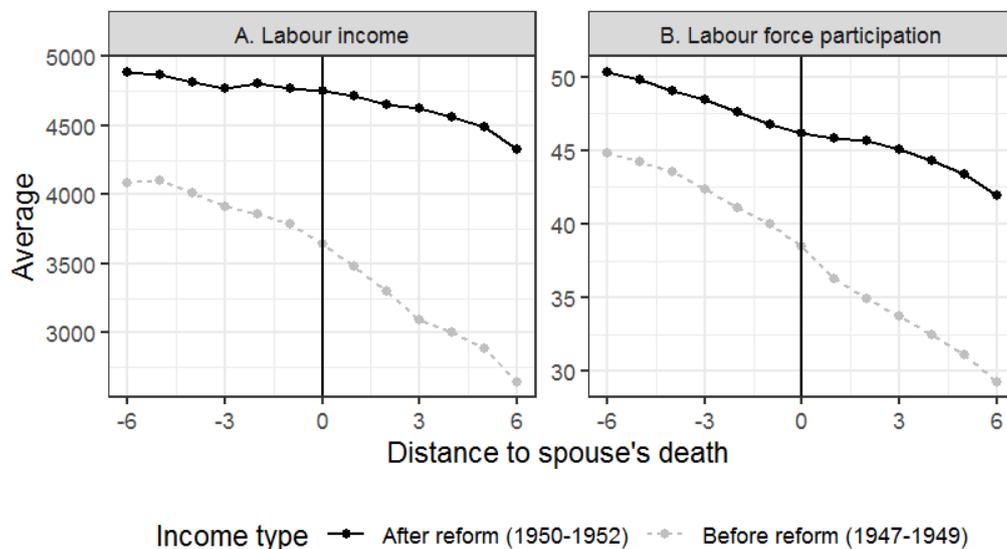
NOTE: For married at 42 women, probability of divorce is computed as the probability to divorce at least once between 43 and 64 years old. For married in 1990 women, probability of divorce is computed as the probability to divorce at least once between 1991 and 2011.

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

SOURCE: CBS.

## C.5 Income substitution

Figure D.9: Average labour supply output according to distance to spouse death



NOTE: Estimation is carried out on a semi-annual basis.

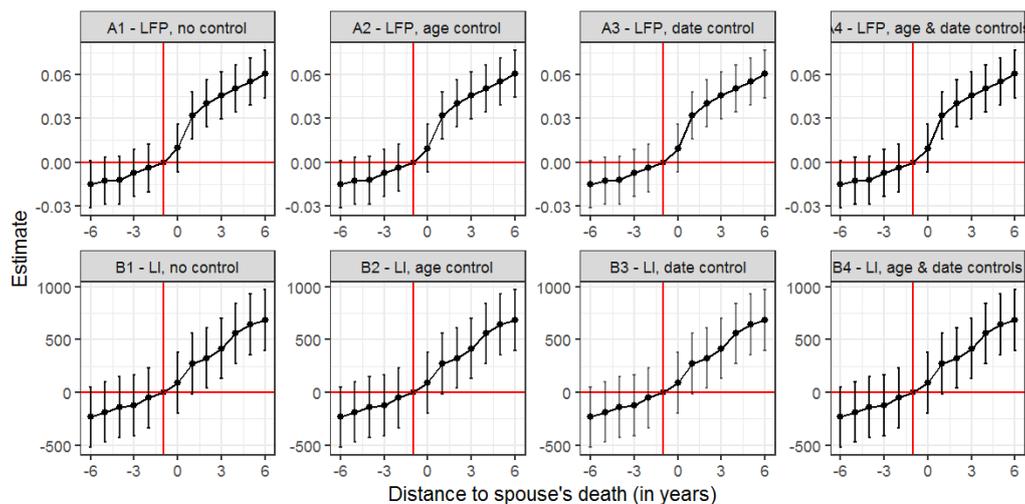
Labour income refers to wage income, profits from self-employment and income from other activity, welfare income refers to social welfare benefits, unemployment benefits and sickness/disability benefits and labour force participation is a dummy variable equal to 1 if individual has positive labour income and 0 otherwise.

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

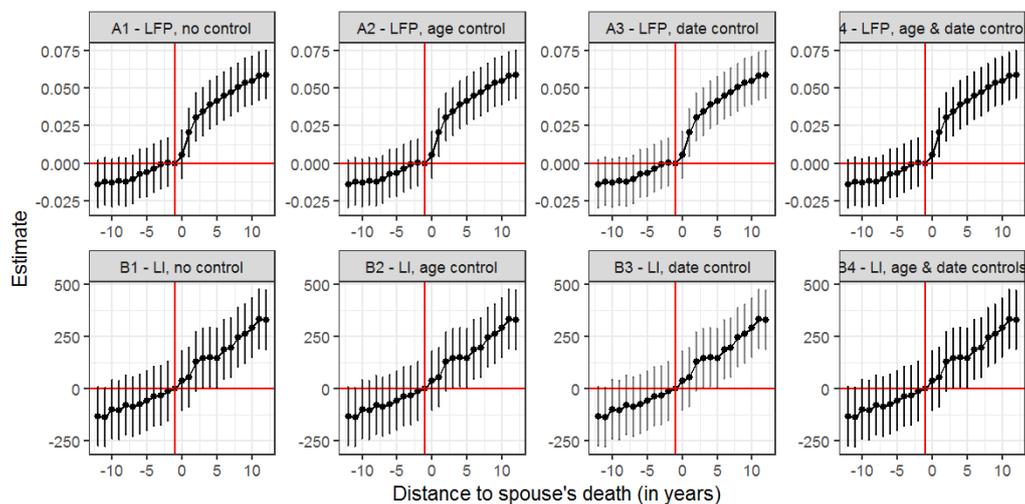
SOURCE: CBS.

Figure D.10: Event study estimates for labour income and labour force participation

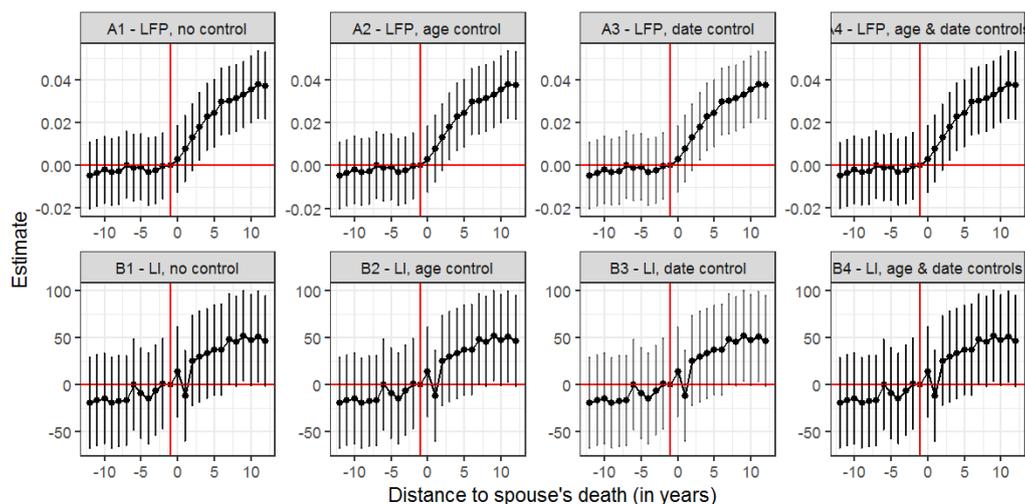
(a) Semi-annual basis



(b) Quarterly basis



(c) Monthly basis

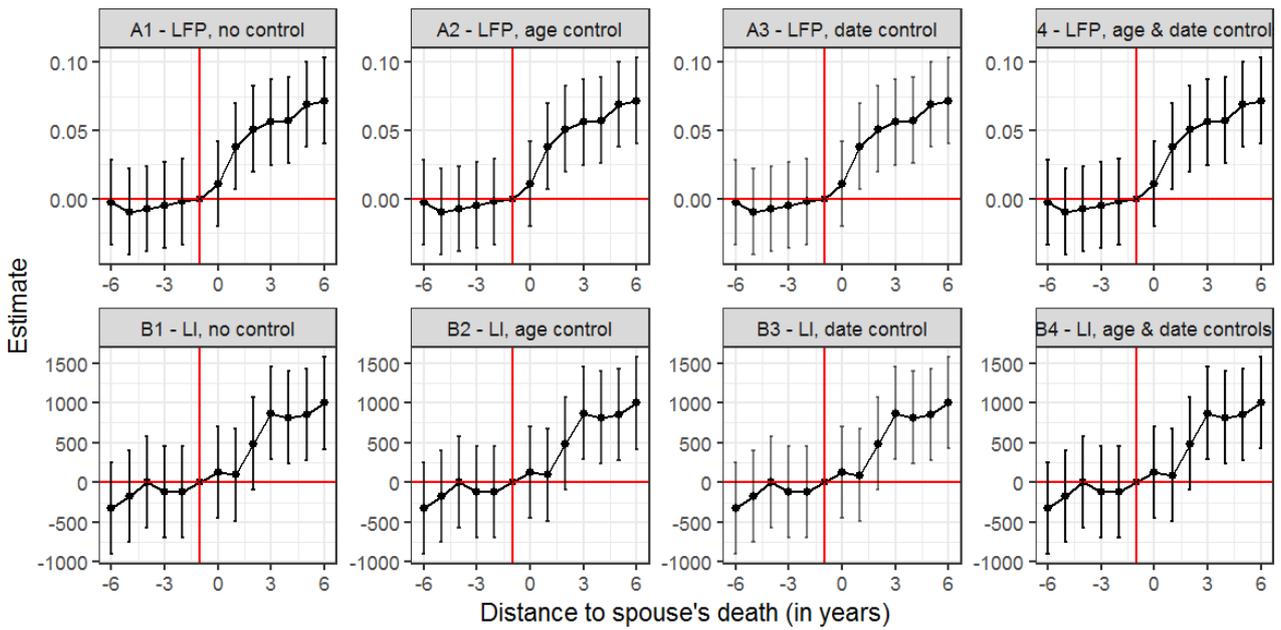


NOTE: Labour income refers to wage income, profits from self-employment and income from other activity and labour force participation is a dummy variable equal to 1 if individual has positive labour income and 0 otherwise.

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

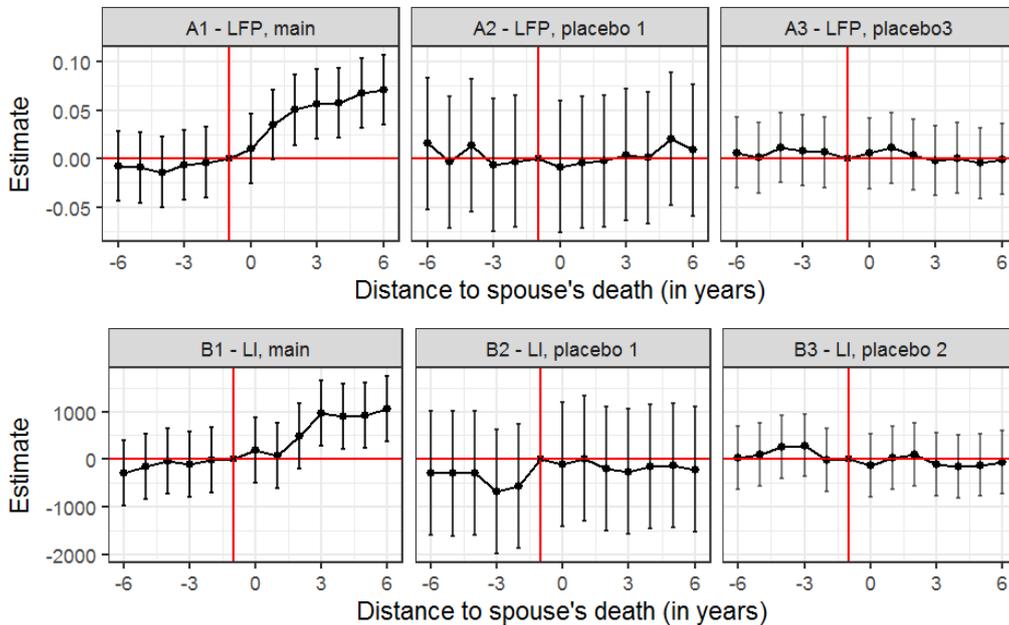
SOURCE: CBS.

Figure D.11: Event study estimates for robustness test (pension reform)



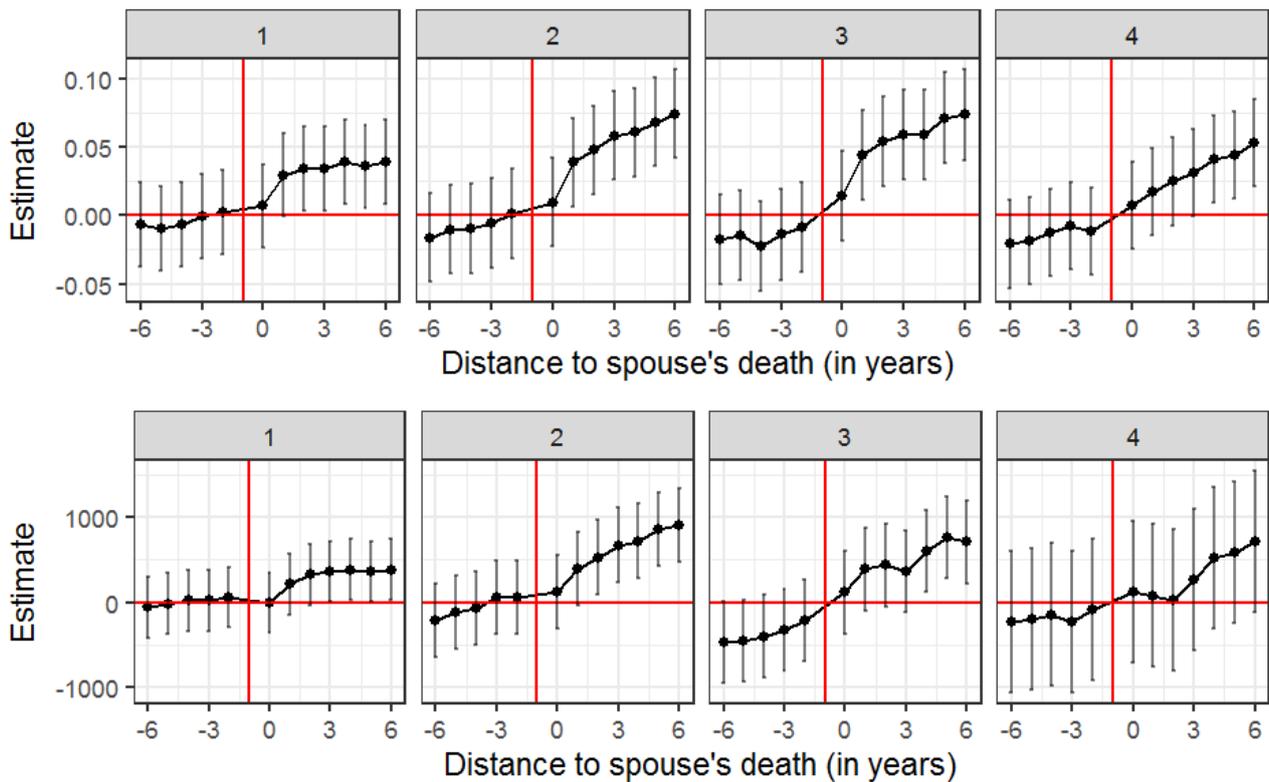
NOTE: Labour income refers to wage income, profits from self-employment and income from other activity and labour force participation is a dummy variable equal to 1 if individual has positive labour income and 0 otherwise.  
 SCOPE: Widows born between 1947 and 1952 whose husband died after 2003 and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

Figure D.12: Event study estimates for placebo analyses



NOTE: LI stands for labour income and LFP stands for labour force participation. 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.  
 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 were aged below 62 at death time.  
 SOURCE: CBS.

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



NOTE: LI stands for labour income and LFP stands for labour force participation. 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.

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.