CPB Netherlands Bureau for
Economic Policy Analysis

Labor Supply Effects of Survivor Insurance

Evidence from Restricted Access to Survivor Benefits in the Netherlands

This paper investigates the effects of survivor benefits (SB) on labor supply. Building on rich administrative data on the universe of Dutch residents, we use the 1996 Dutch reform that considerably tightened SB eligibility rules to causally identify the labor supply effect of SB. We show that income and employment after spousal death increase significantly (+23% and 16% respectively).

We further decompose those effects between the extensive and intensive margins, and provide evidence of program substitution of widows toward disability insurance and welfare as a result of the reform.

CPB Discussion Paper

Simon Rabaté (CPB), Julie Tréguier (Ined)

March 2022

Doi: https://doi.org/10.34932/5y30-1s64

Labor Supply Effects of Survivor Insurance: Evidence from Restricted Access to Survivor Benefits in the Netherlands*

Simon Rabaté[1] and Julie Tréguier[2]

[1]Netherlands Bureau for Economic Policy Analysis (CPB) and Institut national d'études démographiques (Ined)

[2]Institut national d'études démographiques (Ined) and EconomiX, University of Paris Nanterre.

March 2022

Abstract

This paper investigates the effects of survivor benefits (SB) on labor supply. Building on rich administrative data on the universe of Dutch residents, we use the 1996 Dutch reform that considerably tightened SB eligibility rules to causally identify the labor supply effect of SB. We use a regression discontinuity approach, leveraging the cohort-based implementation of the reform. We show that, following the tightening of SB eligibility, individual income and labor force participation after spousal death increase significantly (+23% and 16% respectively). We further decompose those effects between the extensive and intensive margins. We show that the widows who remained in the labor market due to the reform mainly did so as wage earners, and that already working widows increased their number of working hours by 8%. We also find evidence of program substitution of widows toward disability insurance and welfare as a result of the reform. Finally, our heterogeneity results suggest that the magnitude of the response to the reform depends on widows' ability to self-insure against the risk of spousal death.

*We are grateful to Jonneke Bolhaar, Carole Bonnet, Itzik Fadlon, Marion Goussé, Arthur van Soest, Marianne Tenand and Maxime Tô, as well as the participants of the Ined Séminaire café, the CPB internal seminar, the Chaire Travail PSE internal seminar and the EALE 2021 conference. All remaining errors are our own.

1 Introduction

The death of a spouse is not only a devastating life event, it is also a source of major economic risk. Subsequently, through multiple channels, it may affect the labor supply of the survivor. First, the loss itself may affect the well-being of the bereaved partner (Siflinger 2017), which, in turn, may affect the ability to work. Second, spousal death can modify individual preferences – e.g disutility from work – because of the loss of shared leisure with the deceased (Goux et al. 2014). Last but not least, a spouse's death will change the household's standard of living: some economies of scale are lost, as well as the income of the deceased spouse, triggering labor supply responses. These financial risks can be partially offset by public or private life insurance. In this paper, we focus on the latter financial determinant of the labor supply response to spousal death and study the role of publicly provided survivor benefits.

Survivor insurance against spousal death can be provided through a first-pillar public insurance system, or through second-pillar occupational based pension. Individuals can also self-insure through private contracts. Public survivor insurance systems have been widely questioned in recent years, on different grounds. Survivor insurance schemes represent a large public expenditure that is considered as *(i)* inadequate, as it has not evolved with conjugal behaviors (more separations), *(ii)* unfair, as it generates transfers from singles to couples that may not seem justified, *(iii)* inefficient, as these schemes can generate large disincentives to female employment. As a result, several countries have decided to lower the generosity or even eliminate these public schemes (OCDE 2018).

In this paper, we analyze the effect of the restricted access to the Dutch first pillar survivor insurance on survivors' labor supply. Before the reform, the Netherlands were characterized by a generous public SB scheme and low female employment rate. The reform made considerably stricter eligibility rules to SB for individuals born after January 1st, 1950. It provides a clean quasi-experimental setting as it induced a large and discontinuous drop in SB received over the lifetime. Using a regression discontinuity (RD) approach and high quality administrative data on the full universe of Dutch widows, we estimate the causal effect of survivor benefits on female income and labor force participation. We then decompose the extensive margin by type of industry, and the intensive margin response between hours worked and wage rate. To assess the overall effect of the reform on public spending, we also explore program substitution effects by analyzing the take-up of other social security benefits as a response to the reform. Finally, we examine whether the responses to the reform are heterogeneous across different groups with regards to self-insurance ability.

We find that the tightening of SB eligibility leads to an important increase in personal

income (+23%) and labor force participation (+16%) three years after spousal death. The implied income elasticity to labor force participation (-0.4) is large and roughly in line with recent findings in the literature (Giupponi 2019, Fadlon & Nielsen 2021). The extensive margin response of non-working widows is driven by wage earners. We further investigate responses according to the 1-digit classification sectors and find no evidence of differences with regards to industries. We then analyze the intensive margin response. We find that widows who have worked during the entire six-year period around spouse's death increased by 8% their number of hours worked because of the reform. We find no effect on the wage rate. Assessing insurance program substitution generated by the reform, we find evidence of an increase in the take-up of disability insurance (+1.5 pp). We also find program substitution towards welfare benefits, for widows without any source of income after losing survivor benefits (+6 pp). Finally, we show that the magnitude of the response to the reform somewhat depends on individuals' ability to self insure against the risk of spousal (income) loss. We find that individuals without a job before their spouse died react much more than others, as well as individuals whose share in the household total income was the lowest. However, we surprisingly find no evidence of a differentiated reaction to the reform by level of liquid wealth.

In general, our paper relates to the literature studying the labor supply effect of spousal death. While the widespread risk of poverty affecting elder women after widowhood has been largely described in the literature, the literature on its economic consequences on working-aged women is relatively scarce.[1] Fadlon & Nielsen (2021) show that a spouse's death induces an increase in labor supply because of the loss of the deceased spouse's income, while van der Vaart et al. (2020) show a drop in standard of living for working age women, which is due to a decrease in labor supply.[2] However, those papers do not directly assess the role of survivor insurance in the labor supply response to spousal death.

More specifically, our paper is related to the literature studying the labor supply effect of survivor insurance schemes. Despite the importance of survivor benefits, both in terms of public expenditure and their role of consumption smoothing in the difficult time of spousal

[1]Note that the question of the effect of economic consequences of widow(er)hood is conceptually and empirically close to the effect of divorce or separation. Our paper also indirectly relates to this rich literature. It has extensively been shown in different countries and various contexts that women encounters a strong decrease in their standard of living following divorce while for men the evidence is mixed (for a literature review, see Mortelmans 2020). Consequently, there is empirical evidence of an increase of female labor supply following a divorce (Bargain et al. 2012, Bonnet et al. 2021). It has also been shown evidence of anticipatory behavior, with female labor supply increases prior to the divorce (Poortman 2005). In other words, women may take up a job as a form of insurance in case of divorce, or in anticipation of divorce.

[2]More precisely, and consistent with a self-insurance reaction to compensate the income shock, Fadlon & Nielsen (2021) find that individuals with low income or low income share increase their labor supply while others decrease it. van der Vaart et al. (2020) compare the effect of a spouse's death on labor supply for widows whose spouse's death happened before a means test was introduced and for those whose spouse's death happened after. They find a decrease in labor supply for the latter group that they explain by the introduction of the means-test.

death, the literature on the topic is relatively scarce. This question has been investigated in theoretical papers, using dynamic structural life-cycle models to help understanding the role of marriage-based social insurance on household's labor supply decision. Kaygusuz (2015), Fadlon & Nielsen (2019), Nishiyama (2019), Groneck & Wallenius (2021), Borella et al. (2019) find that removing spousal and survivor benefits would increase female labor participation. These theoretical predictions have been empirically verified in recent papers that use natural experiments to study the causal effect of survivor insurance over survivors' labor supply, and find important responses. Giupponi (2019) shows that the lower generosity in Italian SB translated into an increase in labor supply. Böheim & Topf (2021) show similar results for widowers in the Austrian context but find no evidence of a significant response for those who were already working before their spouse's death. Finally, Fadlon et al. (2019), take advantage of the age discontinuity in SB eligibility in the US. They show that eligibility to SB induces a decrease in labor supply. They find a considerable gradient in responses according to income level. They highlight that liquidity constraints, rather that myopia, explains widows' response: labor supply declines are attributable to lower liquidity households who exhibit large effects, whereas the highest-liquidity households are able to fully smooth their labor supply behavior.

We also study potential spillovers between different types of insurance: reduction in generosity of one type of insurance can induce take-up of alternative insurance schemes. Such substitution effects can limit the labor supply effect of a reform, and generate negative externalities of the reform on public spending. Giupponi (2019) gives evidence of such program substitution for survivor insurance. This is also closely related to the large literature analyzing program substitution effects toward unemployment or disability programs, in the context of pension reforms, or disability insurance reforms (Duggan et al. 2007, Karlström et al. 2008, Li & Maestas 2008, Coe & Haverstick 2010, Staubli 2011, Staubli & Zweimüller 2013, Borghans et al. 2014, Atalay & Barrett 2015, Inderbitzin et al. 2016, Rabaté & Rochut 2020).

We make the following contributions to the literature. First of all, we provide a new and clean evaluation in the scarce but booming literature studying the causal effect of survivor benefits on survivors' labor supply. Our empirical strategy combines a sharp and large reform of survivor benefits with rich administrative data, which makes it possible to isolate the effect of survivor benefits from confounding factors and reforms. In line with findings of recent studies, we find large increases in the surviving spouse's labor supply as a response to a restricted access to benefits. Second, we extend on the existing literature in four ways. First, we are able to provide a precise decomposition of widows' labor supply response to the reform. This allows us to disentangle the different response's margins – income, participation, hours worked, wage rate

— and to provide a complete assessment of the reform's effect. We fill a gap in the literature displaying the precise intensive margin component of the response. Indeed, we decompose the intensive margin response between the wage rate and hours worked responses to the reform, which was done using proxies in previous papers. Second, we further decompose the extensive margin, by providing a split of the response by sector of activity, which has never been explored in the literature. We show that wage employment mainly absorbs the flow of *extra* widowed workers but find no evidence of differences in the response to the reform by industry. Third, we investigate substitution with other programs and find evidence of an increase in the take-up of other insurance schemes. This is consistent with Giupponi (2019), but not often observed in the literature for other insurance schemes. Fourth and lastly, we carry out heterogeneity analyses to investigate the hypothesis that the people most affected by the reform are those who had a low level of self-insurance, including an analysis of the labor supply response by wealth level. This latter component – for which we surprisingly find no heterogeneous effects – was not included in previous studies.

The rest of the paper is organized as follows. Section 2 describes the institutional background, section 3 describes the data and the empirical strategy, we describe the results in sections 4 and 5 and section 6 concludes.

2 Institutional context

Insurance against spousal death can be provided through three main sources in the Netherlands. A public first pillar insurance provides a basic pension of which amount and eligibility do not depend on labor market history of either of the spouses. A second pillar is the occupational based pension plan. It depends on the deceased spouse's work history, and rules for coverage and replacement rates vary from one pension fund to the other. Lastly, individuals can self-insure through private contracts (e.g life insurance).

In our analyses, we focus on the first pillar survivor insurance. The rest of this section presents its rules and their evolution. We present a somehow simplified description of the institutional context, concentrating on the elements essential to the understanding of our empirical strategy.

The public SB system in the Netherlands. Over the period covered by our data (1999-2019), the rules of the first pillar of survivor benefits (SB) are defined by the reform act which was implemented in 1996 and gave its name to the scheme (*Algemene nabestaandenwet*, Anw).[3]

[3]It replaced the General Widow and Orphans Benefits Act (AWW, *Algemene Weduwen- en Wezenwet*). This former scheme was tied to the marriage contract. A widow(er) was eligible to survivor benefits if he or she *(i)*

In terms of eligibility, married couples, registered partners and cohabitants are eligible to survivor benefits if the survivors meet any of the following conditions: *(i)* caring for a child aged less than 18 or *(ii)* suffering from inability to work (at least 45% disabled). While eligible, survivors receive benefits from spousal death up to the moment their younger child gets 18 years old or they reach the state pension age (equal to 65 for individuals born before 1948 and then progressively increasing). From that latter age on, the Anw pension is replaced by the public pension system (AOW, *Algemene Ouderdomswet*). Benefits are also cut if the survivor forms a new household.

In terms of amount, the survivor gets a flat benefit equal to 70% of a gross minimum wage, plus 20% of a gross minimum wage in the presence of a minor child (half-orphan's benefits).[4] The benefits are means tested: if the survivor has income, it is partly or fully deducted from the Anw benefits, depending on the type of income and the type of benefits. The part of SB related to the care of a minor child is not means tested, the mean test only applies to the basic part (70% of the minimum wage). Unemployment, sickness and disability benefits are fully deducted from Anw benefits. Survivor benefits from other schemes (second and third pillar) are not deducted. Finally and most importantly, labor income is partially deducted from Anw benefits. The benefits are phased-out from labor income above 50% of fulltime work at minimum wage, with a rate of 66%. Individuals with income over 155% of a gross minimum wage then do not receive any SB.

Change in eligibility criteria and restricted access to benefits. The 1996 reform did not implement Anw immediately. A transition period exempted individuals *(i)* whose spouse's death occurred before July 1999 and *(ii)* who were born before 1st, January 1950, who remained eligible to survivor benefits. As we observe labor market outcome for the full population from 1999, we do not use the 1996 discontinuity in the rest of the paper. We focus on the date of birth discontinuity in eligibility introduced by the reform. Table 1 summarizes our main source of variation: individuals born after 1950 are eligible to SB only if they care for a minor child or are disabled. Figure 1 illustrates the effect of the reform in terms of benefits, according to income level. We observe that the means-tested schedule for SB disappears for able individuals who do not care for a child, while being unaffected for the others.[5]

was 40 years old or more, *(ii)* had at least a child under the age of 18 or *(iii)* suffered from inability to work. 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 and there was no means test.

[4] As of 1st, July 2021, the yearly gross minimum wage in the Netherlands is equal to 20,412 euros (23,876 dollars).

[5] Properly speaking, individuals caring for a minor child may be affected by the reform as benefits end as soon as their youngest child gets 18 years old *vs.* at AOW-age before the reform.

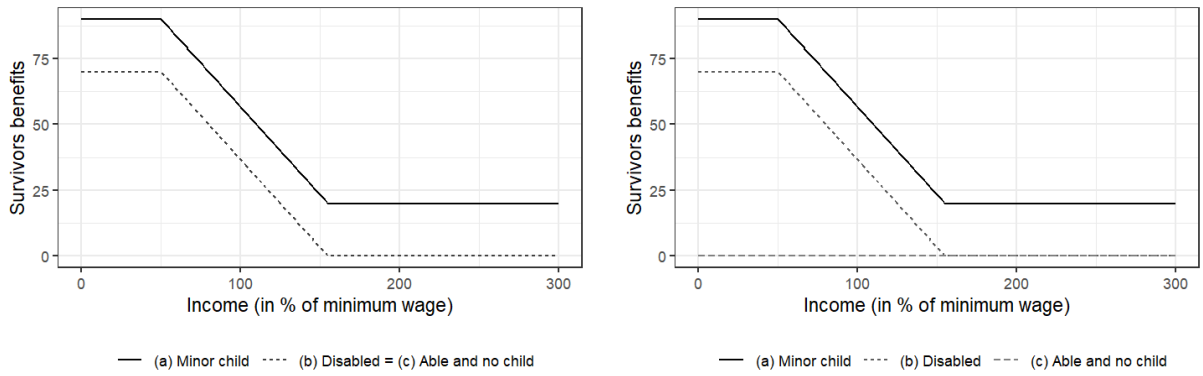
Table 1: Eligibility to SB

	Born before 1950	Born after 1950
Cares for a child	✓	✓
Is disabled	✓	✓
Does not care for a child and not disabled	✓	X

SOURCE: Legislation

Figure 1: SB amount in % of minimum wage according to date of birth

(a) Born before 1950 (b) Born after 1950



SOURCE: Legislation.

Confounding reforms. There are three confounding reforms potentially interacting with our reform of interest, since they *(i)* impact the same group of cohorts and *(ii)* can have an effect on employment outcomes of survivors. We hereby present them and discuss the implication they have on our empirical strategy.

The first one is the partner pension reform. It is a pension supplement for the older partner who has reached the AOW-age, provided that the younger one had an income below a certain amount. This supplement was removed for older partner born after December 1949, who would not receive the supplement any more. Nagore-Garcia & van Soest (2021) find only limited effect of the reform for the older spouse, for which the 1950 discontinuity applies. Based on this result, and on the fact that the surviving spouse is less likely to be the older spouse, the interaction with this reform may be of second order of importance.

The second confounding reform relates to the occupational pension reform, which was implemented in 2006 and largely reduced the generosity of early retirement schemes for individuals born on January, 1st 1950 and after. It then translated into a large negative shock on pension wealth for individuals. As a consequence, it modifies resources composition and labor supply between 60 and pension age. Atav et al. (2021) show this reform translates into an increase in employment rates for the 60-65 age. This important reform affects the cohorts with the same

discontinuity as the one of our interest. Nevertheless, focusing on widows whose spouse died before they reached 62 years old (see discussion on sample selection in the next section), while the reform mainly affected men aged near the statutory retirement age, allows us neutralizing the effects of the confounding reform.

Third and lastly, the statutory retirement age is progressively increased from 65 to 67 in 2023. In particular, it increased from 65 and 2 months to 65 and 3 months for individuals born after November 1949, which is close to the 1950 cutoff. As the first pillar SB are received until the statutory retirement age, this could interact with our reform of interest. Yet, we argue that this effect is likely to be small, based on two reasons. First, this one-month increase represents only a small change in lifetime earnings compared to the effect of the SB reform. Second, we can test that an increase in the statutory retirement age itself, without any change in the SB, does not have any significant effect on the income trajectories after spouse's death (see section 4).

All in all, there is no reason why those reforms would differently affect individuals before and after the death of the spouse. Moreover, if there were an impact of any of these reforms on widows' labor supply, it would be a positive impact (increased labor supply). Then, the absence of difference between treated cohorts and the control group before spouse's death would suggest that the interaction with the confounding reforms is not a major issue in our setting. In section 4, we give empirical evidence that for our population of interest, widows born after 1950 are not different from those born before 1950, with regards to labor supply before spouse's death.

3 Data and empirical strategy

3.1 Data presentation

Data sources. We use register data collected and maintained by Statistics Netherlands (CBS). They cover all residents living in the Netherlands. We have information on complete individual trajectories, and retrospective data on household histories. Each record contains a unique personal identifier that lets us merge datasets and get information on individuals, their civil-status and household histories (from 1995 onward), their labor, welfare or pension income from tax data (from 1999 onward), their wages, hours worked and activity sector from employers-employees data (from 2006 onward), their wealth (from 2006 onward), their partner's and children's characteristics and the survivor benefits they possibly receive (from 2005 onward). More details can be found on which datasets we have mobilized and how we have combined them to define our population of interest for the analyses in Appendix A.

Sample selection. In the rest of the analysis, we focus on widows because survivors are mostly women, due to their higher life expectancy and the age difference between spouses. We take advantage of a discontinuity according to date of birth: January, 1st 1950. We thus focus on individuals born between 1946 and 1953. In order to observe the population of interest on a six-year period around death (three years before and three years after), we define our scope as widows whose spouse died from January 2003 and before they reached age 62 (three years before the state pension age).

As we want to focus on widows that are affected by the reform, we remove from our estimation sample widows who care for a child who will be age 18 before they reach AOW-age, as they were not affected by the eligibility restriction. Disabled widows were also unaffected by the reform, but we cannot detect them in the data at our disposal.[6] As a result they are kept in our sample of analysis. In the following, 'treated individuals' as well as 'treated cohorts' will refer to individuals belonging to cohorts born after January, 1st 1950, even though the disabled are not treated. This implies that our estimates of the effect of the reform on the treated are slightly underestimated.

Table 2 presents descriptive statistics of the data and variables we use in the paper. Age at spousal death, as well as average labor supply outcomes over the pre-death period show monotonic trends, but no jumps, over the cohorts of birth. This is consistent with a relatively stable age difference between spouses and male life expectancy, and with the increasing women's participation in the labor market over the period, respectively. The last control cohort (widows born in 1949) was aged 57 at the time of their spouse's death. Among them, 44% worked during the year preceding death. Their average income over the three-years period before death amounted to 740 monthly euros, which corresponds to 31% of total household income. Overall, 39% never worked in this three-years period, 25% had an average income that is lower than SB means-test threshold in case of a spouse's death, 25% would be partially means-tested and 11% would *a priori* be fully means-tested.

3.2 Empirical strategy

The 1996 reform introduced a discontinuity in SB eligibility. From 1996, survivors are eligible to SB only if they were born before January, 1st 1950 or if they care for a child or are disabled. We take advantage of this discontinuity in the date of birth to identify the causal impact of SB elimination on labor supply. We use a regression discontinuity framework (Lee & Lemieux

[6] We could identify the disabled as individuals who benefit from disability benefits. However, disabled individuals are eligible to disability benefits from 35% incapacity while eligibility to SB is conditional on 45% incapacity. We thus are not able to identify the group of individuals who are eligible to SB with a disability criteria.

Table 2: Descriptive statistics per cohort of birth

	1946	1947	1948	1949	1950	1951	1952	1953
Nb of individuals	4,289	4,469	4,577	4,579	4,771	4,803	5,310	5,542
Age	58.3	57.9	57.5	57.0	56.6	56.2	55.8	55.5
Working status (in %)	34.6	38.8	41.0	43.9	47.1	51.3	51.7	55.0
Labor income (in euros)	521	593	676	738	808	926	972	1,071
Income share (in %)	30	22	30	31	31	27	33	33
Total wealth (in k euros)	233	241	229	248	227	231	211	211
Liquid wealth (in k euros)	292	317	302	323	299	308	296	298
Labor income (x) category (in %):								
never worked	50	46	42	39	36	32	30	26
$x \leq 0.5$MW	22	22	23	25	25	25	26	26
$0.5 < x \leq 1.55$	19	23	23	25	26	28	29	30
$x > 1.55$MW	9	9	11	11	12	15	15	17

NOTE: Age refers to age at spouse's death. Working status refers to the proportion of individuals who have had at least a period of work during the year preceding death. Labor income is expressed in monthly euros and wealth is defined on a yearly basis. Labor income category refers to average income level on the 3 years pre-death period, expressed in number of minimum wages (MW). Labor income category refers o the three-year period preceding spouse's death. In particular, the proportion of widows who never worked on the 3-years pre-death period is mechanically lower that the proportion of those who never worked during the year preceding death (*working status*). Monetary values are expressed in 2018 euros.
SCOPE: Widows born between 1946 and 1953 whose husband died after 2003, who do not care for a child and who were aged below 62 at death time.
SOURCE: CBS.

2010), in which the assignment variable is the survivor's date of birth. Individuals born before January, 1st 1950 compose the control group while the others are treated.

We estimate the following equation:

$$Y_i = \alpha + \beta.f(dob_i - 1950) + \gamma.\mathbf{1}_{dob \geq 1950} + \delta.\mathbf{1}_{dob \geq 1950} * f(dob_i - 1950) + \lambda.X_i + \epsilon_i \quad (1)$$

where Y_i is the independent variable (labor income or labor force participation), $dob - 1950$ is the cohort variable, equal to the difference between woman i's date of birth and January, 1st 1950 (in quarters in our main specification). The $f()$ function must be continuous in 0. In our baseline specifications, we include a linear trend in the running variable, $f(dob_i - 1950) = dob_i - 1950$. The specification allows for different slopes before and after the cutoff.[7] X_i is a vector of age dummies and ϵ_i is a random noise. δ is our parameter of interest and captures the SB reform on labor supply. We estimate equation (1) using OLS, separately for each quarter t between -3 and 3 years relative to spousal death.

The main identification assumptions in a regression discontinuity (RD) are that individuals

[7]For robustness checks, we also estimate equation (1) with f a second and a third order polynomials, successively, and for equal slopes before and after the cutoff. See corresponding estimates in Table 3.

cannot manipulate their treatment status, are similar on both sides of the discontinuity and that there are no other elements than the reform of interest that are changing at the discontinuity and can impact the outcomes of interest. Our running variable – date of birth of the individuals – is by essence not manipulable. We nevertheless plot the probability density function of benefit recipients by quarter of birth in Appendix Figure B.1. We verify that there is no visible discontinuity in the density around the threshold. Moreover, the McCrary test statistics reported on the graph do reject the null hypothesis of discontinuity at the threshold. To verify that individuals around the cutoff are comparable in their observable and unobservable characteristics, we perform a covariate balancing test using parametric and non-parametric RD specifications. As reported in Appendix Table B.1, covariates are balanced under both the linear and the quadratic parametric specifications, and the local linear regression specification.

Our identification strategy relies on the assumption that, absent the reform, spousal death would have had the same effect on labor supply for survivors born before and after 1950. In the next section, we provide two important results regarding the credibility of this assumption. First, we display pre-trends and show that there is no significantly different from zero difference between the cohorts before spousal death with regards to labor supply. Second, we carry out placebo analyses and show that there is no significant different response to counterfactual reforms that would have happened for 1949 or 1951 cohorts, respectively.

As discussed in section 2, the main threat to our identification strategy are the potential interactions with contemporaneous pension reforms whose implementations were also *(i)* cohort-based and *(ii)* at the vicinity of the 1950 cut-off. As explained, the occupational pension reform mainly affected men aged near the statutory retirement age. As we focus on widows whose spouse died before they reached 62 years old, it may not interact with our analysis. Most of the effect we are estimating then occur at ages that are not directly impacted by the occupational pension reform, which shifted the employment probabilities between 60 and 65 (Atav et al. 2021). We nevertheless give empirical evidence that this is indeed not the case, by showing that there is no discontinuity in labor supply for treated cohort when considering pre-spousal death period. Generally speaking, a large part of the threat is alleviated by the fact that we can observe the dynamics of the effect of the reform before and after spousal death and that there is no clear reason why the effect of the pension reforms would be correlated with the timing of spousal death. A difference in labor supply outcomes observed at the date of birth cutoff and that kicks in after the death of the spouse is then very likely to be driven by the change in the eligibility to survivor benefits we are studying.

4 Main results

4.1 Graphical evidence

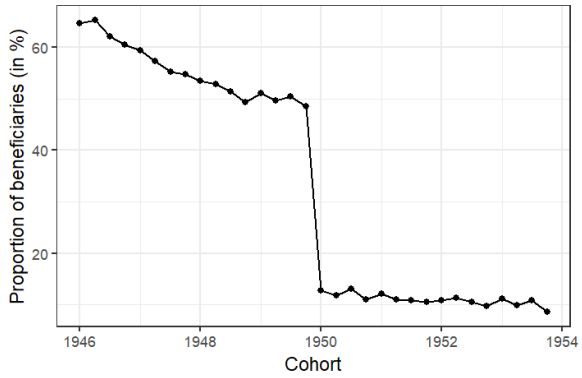
We first present graphical evidence of the effect of the change in survivor benefits' eligibility. We start with a description of the effect of the reform on survivor benefits, to verify and assess the bite and magnitude of the reform. Figure 2 presents two illustrations of the effect of the reform in terms of survivor insurance. Figure 2a shows the proportion of SB beneficiaries among widows per cohort. We verify that there is a large drop in this proportion from the 1950 cohort of birth. Among widows born before 1950, despite a decreasing trend, a large part of the population receive SB. 65% of widows born in the first quarter of 1946 benefit from SB and so do 49% of those born in the last quarter of 1949. This proportion is much lower for widows who were treated by the SB reform. Indeed, for those born in the first quarter of 1950, only 13% receive SB benefits.[8] This drop in eligibility translates into large changes in the total amount of SB received over the life-cycle, as depicted in Figure 2b. The latter, which includes zero amounts, shows the drop induced by the restricted access to benefits but also gives a estimation of the reform magnitude. Even though the impact of the reform is limited by the fact that the SB pension is only received between the time of spousal death and the retirement age, we observe a clear and large discontinuity in total SB received at the cutoff. The average total SB amount over the life-cycle is approximately equal to 30 minimum wages (corresponding to 50 thousands of 2018 euros) for widows born before 1950. On average, this corresponds to a six-year widowhood duration. For individuals born after 1950, this amount dropped to 7 minimum wages on average. The magnitude of the drop of lifetime income due to the reform – almost 40k euros – is then important, and likely to trigger large labor supply responses.

We then present graphical evidence of the effect of spousal death on labor supply, according to individuals' date of birth (before and after 1950). Figure 3 presents widows' labor force participation and monthly income level according to distance to spouse's death for treated and non treated cohorts. For non treated cohorts, we observe a continuous decline in labor force participation and in labor income. In particular, there is no discontinuity at spouse's death. Conversely, for individuals born after 1950, we observe a stabilization of labor supply following the spouse's death. Even if the pattern we observe are partly tampered by the different age composition of the groups, it does suggest that labor supply increased as a response to the restricted access to public SB. As suggested in Appendix Figure B.2, this may be the consequence of the large drop in average public SB income after spouse's death, while no change in average

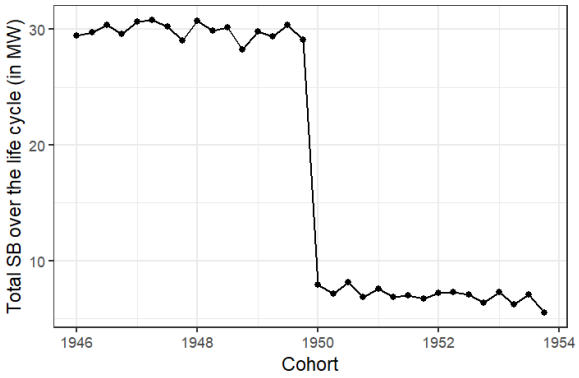
[8] The remaining beneficiaries in the post-reform group is likely to be driven by disabled widows, since they are not affected by the reform but cannot be detected in our data (see section 3.1).

Figure 2: Proportion of SB beneficiaries and total SB over the life-cycle

(a) Proportion of beneficiaries



(b) Total SB over the life-cycle



NOTE: MW stands for minimum wage. To compute total SB over the life-cycle, some 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: Widows born between 1946 and 1953 whose husband died after 2003, who do not care for a child and who were aged below 62 at death time.
SOURCE: CBS.

occupational SB income, which is not fully compensated by the increase in other social security programs average income.

Figure 3: Average labor supply according to distance to spouse's death

(a) Labor force participation

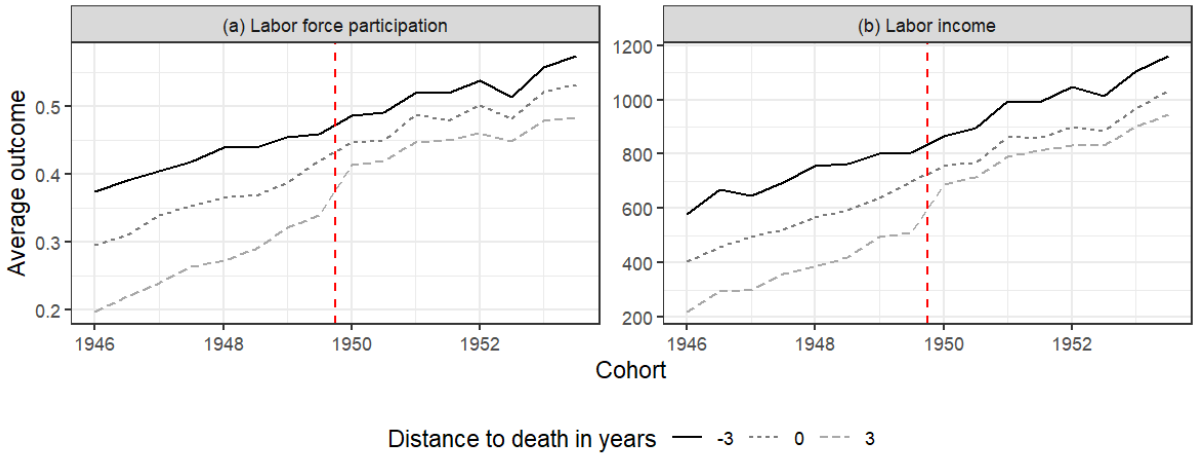
(b) Labor income

NOTE: Labor force participation is a dummy equal to 1 if labor income > 0 and 0 otherwise. Labor income refers to wage income, profits from self-employment and income from other activity.
SCOPE: Widows born between 1946 and 1953 whose husband died after 2003, who do not care for a child and who were aged below 62 at death time.
SOURCE: CBS.

Finally, we give more direct graphical evidence of the causal effect of SB elimination on labor supply in Figure 4. It presents the average labor force participation and labor income by cohort of birth, for different values of the distance to spousal death event in years (-3; 0; +3). For the pre-death period (distance to death = -3), labor force participation and labor income follow a smooth and upward trend over cohorts of birth. On the contrary, three years after death,

labor supply follows an upward trend over the cohorts with a big jump for 1950 cohort of birth. Individuals born after 1950 thus seem to have increased their labor supply at spouse's death in reaction to the SB elimination. These patterns are at the heart of our RD empirical strategy: we observe a jump in labor supply between widows born on both sides of the 1950 cutoff, and this jump is observable from the moment of spousal death, when the reform kicks in. This makes a strong case for the identification of a causal effect of the SB reform on labor supply of widows.

Figure 4: Average LFP and LI according to cohort of birth and distance to death



NOTE: Labor force participation is a dummy equal to 1 if labor income > 0 and 0 otherwise. Labor income refers to wage income, profits from self-employment and income from other activity.
SCOPE: Widows born between 1946 and 1953 whose husband died after 2003, who do not care for a child and who were aged below 62 at death time.
SOURCE: CBS.

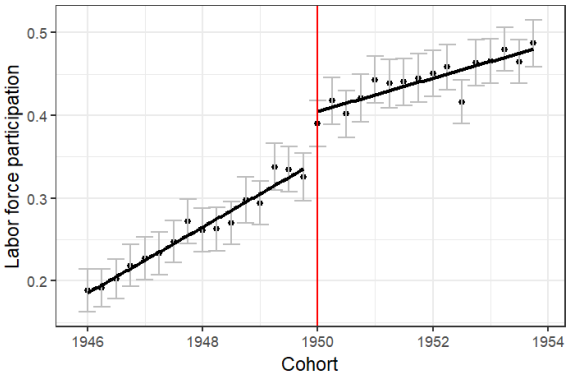
4.2 Estimation results

Before turning to regression results, Figure 5 isolates the $t = 3$ line of Figure 4 and presents, for labor force participation and labor income, the average level by cohort of birth three years after spousal death, along with confidence intervals and a linear fit from each side the cutoff. This gives a clearer view of the discontinuous increase in labor supply between widows born before and after the 1950 cutoff.

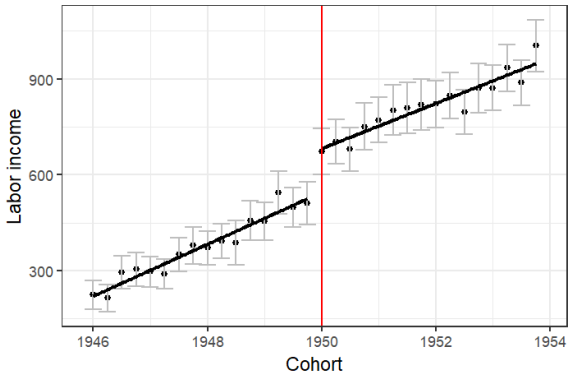
Table 3 shows the coefficients for estimation of equation (1) three years after spousal death and for different specifications of polynomial $f()$: three for a polynomial of order 1, 2 and 3, respectively, and one of order 1 with slopes set equal on each side of the discontinuity. For labor force participation, RD estimates take values ranging from 0.043 to 0.062. It means that three years after spouse's death, treated individuals have a 4.3 to 6.2 percentage points higher labor force participation than control group. All coefficients are significantly different from 0. In terms of labor income, estimates vary between 110 and 140 monthly euros. In the rest of the

Figure 5: Average participation and income by date of birth, three years after spousal death

(a) Labor force participation



(b) Labor income



NOTE: Labor force participation is a dummy equal to 1 if labor income > 0 and 0 otherwise. Labor income refers to wage income, profits from self-employment and income from other activity. Both outcomes are measured three years after the death of the spouse. Confidence intervals are displayed at the 95% level.
SCOPE: Widows born between 1946 and 1953 whose husband died after 2003, who do not care for a child and who were aged below 62 at death time.
SOURCE: CBS.

paper, we keep the first specification as our preferred specification, with a first order polynomial and different slope on each side of the discontinuity.

Table 3: Regression discontinuity estimates

	(a) Labor force participation				(b) Labor income			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
δ	0.060	0.054	0.043	0.062	140	130	110	140
Std. err.	0.010	0.015	0.021	0.010	24	37	52	24
p value	0.000	0.000	0.040	0.000	0.000	0.000	0.044	0.000
Parametric	✓	✓	✓	✓	✓	✓	✓	✓
Poly. order	1	2	3	1	1	2	3	1
Equal slopes	X	X	X	✓	X	X	X	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Nb obs.	38,340	38,340	38,340	38,340	38,340	38,340	38,340	38,340

NOTE: Labor force participation is a dummy equal to 1 if labor income > 0 and 0 otherwise. Labor income refers to wage income, profits from self-employment and income from other activity. All estimates are based on a 8-years symmetric bandwidth. Estimations are carried out on a quarterly time basis.
SCOPE: Widows born between 1946 and 1953 whose husband died after 2003, who do not care for a child and who were aged below 62 at death time.
SOURCE: CBS.

Our results are robust to estimation time basis and presence of controls, as shown in Table 4. We run the estimation for different bandwidths around the discontinuity. We make bandwidth varying from 0.5 to 3.5 around the 1950 threshold. In other words, we estimate the RD equation for individuals born a quarter around January, 1st 1950, then for individuals born six months

around January, 1st 1950 and so on until individuals born three years around the threshold. Appendix Figure B.3 shows the estimated effects of the reform according to the different bandwidths. We verify the estimates are relatively stable along the bandwidths, in the range 4.1 to 7.9 pp for labor force participation and 110 to 190 euros for labor income.

Table 4: Regression discontinuity estimates, robustness checks

	(a) Labor force participation				(b) Labor income			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
δ	0.060	0.062	0.059	0.060	140	146	137	137
Std. err.	0.010	0.010	0.010	0.010	24	25	24	24
p value	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Controls	✓	✓	✓	X	✓	✓	✓	X
Time basis	quarter	semester	month	quarter	quarter	semester	month	quarter
Nb obs.	38,340	37,674	38,745	38,340	38,340	37,674	38,745	38,340

NOTE: Labor force participation is a dummy equal to 1 if labor income > 0 and 0 otherwise. Labor income refers to wage income, profits from self-employment and income from other activity. Controls are age dummies. All estimates are based on a 8-years symmetric bandwidth.
SCOPE: Widows born between 1946 and 1953 whose husband died after 2003, who do not care for a child and who were aged below 62 at death time.
SOURCE: CBS.

Dynamics of the effect. We then estimate equation (1) for each quarter t between -3 and +3 years relative to the date of spousal death. Figure 6 shows the estimated δ_t coefficients, along with their 95% confidence intervals. The goal of this approach is twofold. First, we can assess the timing of the labor supply response to the reform. Second, it is a way to check that the interactions with the contemporaneous pension reforms are of limited magnitude.[9]

In terms of dynamics, we observe that the increase in labor force participation (panel (a)) and labor income (panel (b)) kicks in immediately with a large labor supply response from $t = 0$. The effect then progressively increases over time, and approximately doubles from 6 months to 3 years after the death of the spouse. As the effect seems to continue to increase beyond our three year window, it is likely that we do not observe the full dynamics of the effect. In Giupponi (2019), the labor supply responses to a restricted access to survivor benefits steeply increase from $t = 0$ to $t = 7$ years before they relatively stabilize. This suggests that we may underestimate the long term responses to the reform in our setting.

Figure 6 also shows that there is no detectable effect of the SB reform before spousal death.

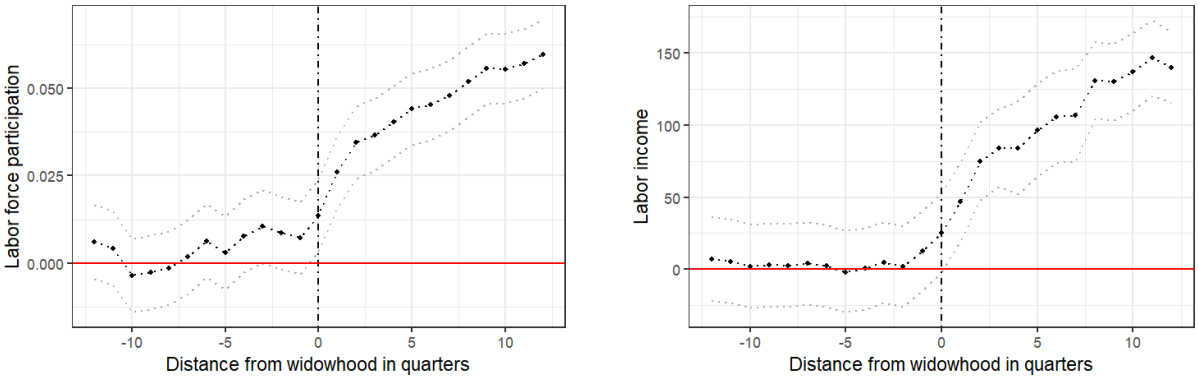
[9]Indeed, if any effect of the confounding reforms on labor supply for our population of interest, our RD estimates should show pre-trends before spouse's death. In other words, we would find significant difference between widows born before 1950 and those born after, with regards to labor supply, before spouse's death. If not, and because there is no reason why the effects of the confounding reforms would be correlated with the timing of spousal death, we could conclude to the limited effect of the reforms on our estimates.

This first suggests that there is no anticipation effect of the reform before spousal death. This could be explained by the fact that we consider relatively young individuals (around age 50-60) for which spousal death may not be anticipated. Secondly, the absence of effect before $t = 0$ suggests that our estimation of the effect of the SB reform is not biased by the effect of the contemporaneous pension reforms affecting individuals in the vicinity of the 1950 cutoff (see section 2). As there is no clear reason why those reforms would affect individuals deferentially before and after the death of the spouse, the absence of effect before death suggest that the interaction with the pension reforms is not a major issue in our setting.

Figure 6: Effect of the reform on the labor supply over time

(a) Labor force participation

(b) Labor income



NOTE: Labor force participation is a dummy equal to 1 if labor income > 0 and 0 otherwise. Labor income refers to wage income, profits from self-employment and income from other activity. All estimates are based on a 8-years symmetric bandwidth. Dotted lines represent 95% confidence intervals.
SCOPE: Widows born between 1946 and 1953 whose husband died after 2003, who do not care for a child and who were aged below 62 at death time.
SOURCE: CBS.

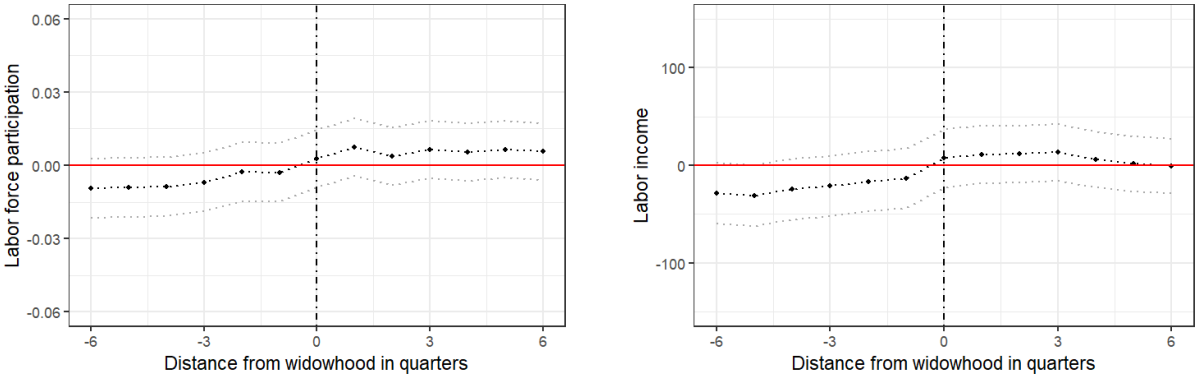
Placebo analyses. As last checks for the validity of our identification strategy, we implement the following placebo tests. We estimate equation (1) with two alternative samples and placebo cutoffs, where no changes in SB were implemented. In the first (resp. second) placebo test, we consider individuals born between January 1946 and December 1951 (resp. January 1948 and December 1953) and a placebo cutoff in January 1949 (resp. 1951). We expect no significant change in labor supply at the cutoffs. Figures 7 and 8 present the estimated coefficients of the RD model (1) for each quarter t three years before and after spousal death. Reassuringly, we do not find much effect of those pseudo-reforms, except for the pre-death estimates of the second placebo, which exhibits some significant but very small positive effects.

Interestingly, the first placebo is also a validity check for the interaction of the SB reform we study with the change in the state pension age. Recall that the SB reform is almost contemporaneous with a 1 month increase in the state pension age occurring in 2013. A similar change

was implemented for the individuals born in December 1948 (from 65 and 1 month to 65 and 2 months), which is very close to the cutoff of our first placebo. The null effects found for this placebo also suggests that our main analyses' estimates are not tampered with the effects of the increase in the state pension age.

Figure 7: Effect of the reform on the labor supply (placebo 1949)

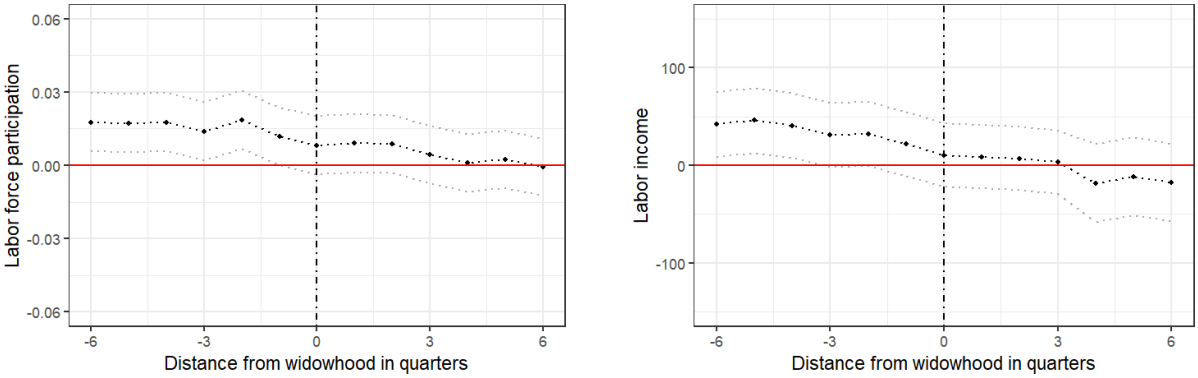
(a) Labor force participation (b) Labor income



NOTE: Labor force participation is a dummy equal to 1 if labor income > 0 and 0 otherwise. Labor income refers to wage income, profits from self-employment and income from other activity. In this placebo specification, we set 1949 as the counterfactual discontinuity date of birth. All estimates are based on a 8-years symmetric bandwidth. Dotted lines represent 95% confidence intervals.
SCOPE: Widows born between 1946 and 1951 whose husband died after 2003, who do not care for a child and who were aged below 62 at death time.
SOURCE: CBS.

Figure 8: Effect of the reform on the labor supply (placebo 1951)

(a) Labor force participation (b) Labor income



NOTE: Labor force participation is a dummy equal to 1 if labor income > 0 and 0 otherwise. Labor income refers to wage income, profits from self-employment and income from other activity. In this placebo specification, we set 1951 as the counterfactual discontinuity date of birth. All estimates are based on a 8-years symmetric bandwidth. Dotted lines represent 95% confidence intervals.
SCOPE: Widows born between 1948 and 1953 whose husband died after 2003, who do not care for a child and who were aged below 62 at death time.
SOURCE: CBS.

4.3 Magnitude and interpretation

In this section, we discuss the magnitude of our results and compare it to the effects found in the literature. In order to make our results comparable to other studies, we relate the estimated labor supply effect to *(i)* the pre-reform level of employment and *(ii)* the magnitude of the financial incentives generated by the reform.

Table 5 displays the percent change in labor force participation and labor income induced by the SB reform. It is defined as the estimated effect of the reform on the outcome of interest divided by the baseline before the reform was enacted. We use our RD design equation (1) three years after spouse's death:

$$Y_i = \alpha + \beta.f(dob_i - 1950) + \gamma.\mathbf{1}_{dob \geq 1950} + \delta.\mathbf{1}_{dob \geq 1950} * f(dob_i - 1950) + \lambda.X_i + \epsilon_i$$

Recover that δ is the effect of the reform on Y_i, taken to be labor force participation or labor income. In this specification, α captures the baseline level for individuals belonging to non treated cohorts, β captures the underlying trend and λ is a vector of the estimated parameters for control variables. We estimate the treatment effect of SB elimination as the full-exposure impact for cohort 1951. The baseline for this population then equals $\alpha + \beta_{left}.m(1951 - 1950) + \lambda\bar{X}_{i \in 1951} = \alpha + \beta_{left} + \lambda\bar{X}_{i \in 1951}$, as we chose an order 1 polynomial for $f()$ and different slopes on each side of the discontinuity in our main specification. More precisely, β_{left} refers to the slope to the left of the discontinuity The full exposure effect of the reform is then equal to:

$$effect = \frac{\delta}{\alpha + \beta_{left} + \lambda\bar{X}_{i \in 1951}}$$

As shown in Table 5, labor force participation baseline for cohort 1951 is 38% so the 6 pp effect of the SB corresponds to a 15.82% increase. Similarly, labor income baseline is 611 monthly euros, so the reform translated in a 23% increase for 1951 cohort.

In order to be compared with other studies, the labor supply response to the reform must be re-scaled to the magnitude of the financial incentives it embedded. We thus compute a labor supply elasticity at the extensive margin with respect to the change in total income to compare our results to the related literature. This corresponds to normalizing the reform effect on labor force participation by the percent change in total income that is attributed to survivor benefits. As we do not observe this latter value for the 1951 cohort, we extrapolate the SB share of total income for the non treated cohorts. This indicator happens to be flat over the cohorts born before 1950 and is equal to 42%. We then consider that the SB reform induced a -42% variation in treated individuals total income. We end up with an elasticity of labor force participation

Table 5: Main RD estimates and average effect

	(a) Labor force participation	(b) Labor income
δ	0.06	140
Baseline	0.38	611
Effect (in %)	15.82	22.90
Nb obs.	38,340	38,340

NOTE: Labor force participation and is a dummy equal to 1 if labor income > 0 and 0 otherwise. Labor income refers to wage income, profits from self-employment and income from other activity. RD estimate is computed on the full population while baseline and reform effect focus on the 1951 cohort of birth. All estimates are computed three years after spouse's death and are based on a 8-years symmetric bandwidth.
SCOPE: Widows born between 1946 and 1953 whose husband died after 2003, who do not care for a child and who were aged below 62 at death time.
SOURCE: CBS.

with respect to SB income equal to -0.38.

Our results are roughly in line with the other estimates available in the literature. Our estimated elasticity is slightly smaller than the one found by Giupponi (2019) (participation elasticity of -0.6) and very close to the one computed by Fadlon et al. (2019), equal to -0.35. However, we must remain cautious when comparing the different elasticities as external validity of each study is likely to be limited, for at least four reason.

First, we estimate a combination of income and substitution elasticity. Indeed, following a spouse's death, survivors may decrease their labor supply in order not to be means-tested and benefit from full SB (van der Vaart et al. 2020). The elimination of the Dutch SB scheme thus may have translated into a stabilization of labor supply for some individuals, compared to the decrease it would have induced if the reform would not have been enacted. However, in our setting, the benefits are phased-out from labor income above 50% of the minimum wage, with a rate of 66%. In particular, we can see that only the first kink point in the budget set (50% of minimum wage threshold, Appendix Figure B.4) may be an incentive to bunching (Saez 2010). We nevertheless do not find any evidence of such behavior among widows (Appendix Figure B.5). We therefore believe that the substitution effect is limited in our setting. Giupponi (2019) faces the same issue, and focuses on high income widow(er)s so as to neutralize the substitution effect of the reform she exploits.

Second, the populations on which are based the analyses may differ between papers. Since different populations are also likely to respond differently to SB reforms (as shown in section 5.3), this could generate differences between the estimated elasticities. We focus on widows born between 1946 and 1953, whose husband died from January 2003, before they reached 62 of age and who do not care for a child. Giupponi (2019) focuses on high-income survivors, she

restricts her sample to individuals aged 55 and under at the time of their spouse's death. Her population is therefore younger as ours. Our population of analysis is much more similar to the one in Fadlon et al. (2019), since they focus on the age 60 discontinuity for eligibility to the US scheme.

Third, since the SB reform responses exhibit a dynamic pattern (cf. Figure 6), consistent elasticities must be computed on similar time windows. We measure our elasticity in $t = 3$ years after spouse's death, which is not in line with Giupponi (2019) and Fadlon et al. (2019). The former compute an elasticity based on the average response over the 15-years period following death and the latter measure an elasticity based on the average response over the two-years period following death.

Finally, the response to the reform can depend on the salience and timing of the reform. In our case as in the other papers, we stress that the individuals were aware of the reform. In particular, for the Dutch context, the reform was enacted in July 1996 but had been discussed since March 1991 in the Parliament and December 1992 in the Senate. Moreover, it was decided that the reform would apply to widows born after January 1st, 1945 (instead of 1950) until very late (this was still the case in the memorandum of explanation of the law, in April 1996). Those affected by the reform have therefore had plenty of time to anticipate its effects.

5 Decomposition of the labor supply response

In this section, we provide additional results to understand the mechanisms underlying widows' response to SB elimination. We decompose our results into an intensive and an extensive margins, we provide evidence of program substitution effects toward other social security programs and we show response heterogeneity.

5.1 Intensive and extensive margin responses

Widow's response to the SB reform in terms of labor supply may be the combination of different effects, among which a change in the number of hours worked or a switch towards a different wage rate job. We first decompose the intensive margin response between hours worked and wage rate responses. Then, we investigate the activity sector composition of the job positions in which widows remained in work following their spouse's death.

Intensive margin: hours worked and hourly wage. Our dataset provides precise information on the number of hours worked and wage rate, on a monthly basis, for the whole widowed population, three years after their spouse's death. This allows for a direct estimation of intensive

margin responses, while they were proxied using working days in previous literature (Giupponi 2019, Böheim & Topf 2021).

We focus on the population of widows who have always been working on the six-year period around their spouse's death and look at whether they adjusted the number of hours worked or the wage rate following the death. Column (a) in Table 6 shows that the reform increased the number of monthly working hours by 7. Compared to the baseline, it represents a 8% increase. The results then suggest that not only surviving spouses remained in the labor market, but also already working widows moved from part-time to full-time or at least increased their workload. We find no significant effect on the wage rate (column (b)).

Table 6: Intensive margin RD estimates three years after spouse's death and average effect

	(a) Hours worked	(b) Wage rate
δ	7	0.45
Std. err.	2	0.30
Baseline	85	16.90
Effect (in %)	8.29	2.67
Nb obs.	11,130	11,130

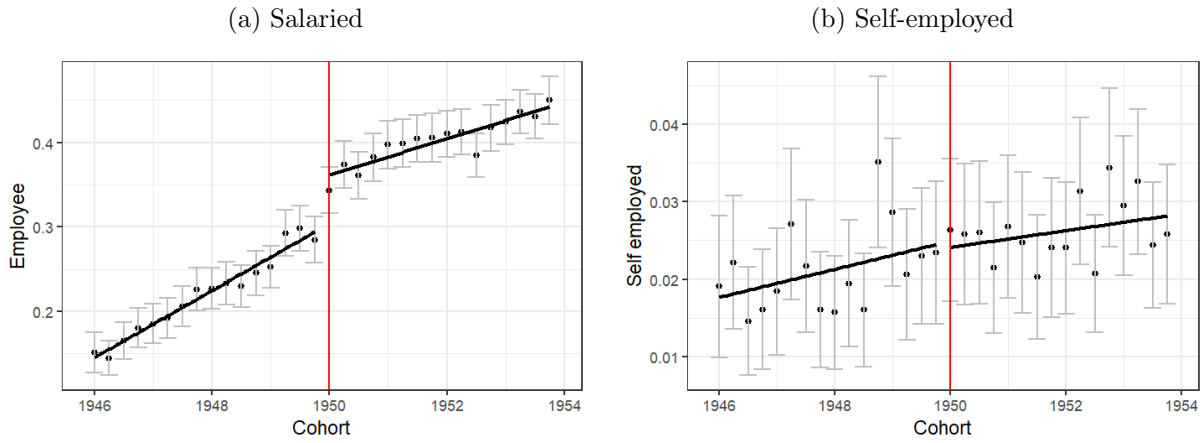
NOTE: Intensive margin response is conditional on having always been working on the six-year perod around spouse's death. All estimates are computed three years after spouse's death and are based on a 8-years symmetric bandwidth.
SCOPE: Widows born between 1946 and 1953 whose husband died after 2003, who do not care for a child and who were aged below 62 at death time.
SOURCE: CBS.

Extensive margin: decomposition by sector of activity. In the previous section, we showed that the elimination of survivor benefits induced an important response at the extensive margin (labor force participation). We now investigate the activity sector composition of the job positions in which widows remained in work following their spouse's death. Our dataset provides complete information on the 5-digits classification sectors occupied by working widows. It lets us shed light on a dimension that has never been looked at in the literature: the nature of the activity sectors in which the flow of *extra* widowed workers remained after their spouse's death.

We first examine labor force participation response according to whether individuals chose a wage employment or to become self employed. As shown in Figure 9, almost all widows who decided to remain in the labor market after their spouse died, did so as wage earners. The SB reform increased wage employment by 6pp three years after spouse's death. We find no significantly different from zero response to the SB reform on labor force participation as a

self-employed.

Figure 9: RD graph per professional status



NOTE: For each professional status, the RD graph represents the extensive margin response to the reform (our variable of interest is a dummy indicating whether labor supply is > 0 within the professional status considered) three years after spouse's death.
SCOPE: Widows born between 1946 and 1953 whose husband died after 2003, who do not care for a child and who were aged below 62 at death time.
SOURCE: CBS.

We go further into the decomposition and we study whether an activity sector is favored by widows who decide to keep working in the labor market after the death of their spouse. Appendix Figure B.7 displays RD estimates and effects of the reform for each industry, following the 1-digit classification. Only significantly different from zero RD estimates are compared to the baseline in order to compute reform effects. The corresponding RD graphs are also presented, in Appendix Figure B.8. Except for the care sector, which account for approximately one third of all wages earners three years after death, the analysis suffer from small sample size. We find no clear evidence of differences in responses to the SB reform by industry.

We finally display RD estimation heterogeneity results for participation to caring sector according to working status before spouse's death. We want to distinguish between widows who were already working before their spouse's death and decided to switch activity sectors, those who were already working in this sector and those who just entered the labor market (after a three-year stop). We focus on the care sector for sample size reason, as it is the sector which account for the larger share of wage earners. As shown in Appendix Figure B.9, caring sector participation as a response to SB elimination is mainly driven by widows who did not reduced their labor force participation. We find no significant evidence of a switching towards caring sector behavior. We finally find significant evidence of widows who entered the care sector after their spouse's death, while not on the labor force participation before.[10]

[10]This last result nevertheless does not exclude the possibility that the widows entering the care sector after their spouse's death had already worked in this sector in the past (at least three years before, as defined in our

5.2 Program substitution responses

The elimination of SB may induce widows into taking up more of other insurance schemes, either to increase their disposable income or as a consequence of the negative health effect following the wealth drop after spouse's death. Such program substitution effects would limit the labor supply responses of the SB reform, and generate negative externalities of the reform on public spending. We investigate whether the SB reform had an impact on the number of welfare, disability or unemployment beneficiaries among the widowed population. Disability benefits are provided to former workers that are at least 35% incapacitated for work. Unemployment benefits are served to individuals who used to be employed or are employed under a certain workload and who do not receive disability benefits. Welfare benefits are a safety net for individuals with income below a certain threshold who do not receive disability nor unemployment benefits.

From a financial point of view, we expect widows to react to the reform with an increase in the take-up of other social security schemes. Indeed, SB are one-to-one reduced with welfare, disability or unemployment benefits. Before the reform, individuals whose SB are higher than expected welfare, disability or unemployment benefits may decide not to claim. Conversely, after the reform, many individuals loose eligibility to SB and therefore have a higher financial incentive to claim other social security benefits.

A second order possible response to the SB reform is a direct health effect of spousal death on widows is less mitigated without SB. Indeed, without SB, there is less compensation of the negative wealth shock following spouse's death. It may translate into a lower health status and work capabilities for the widows, which could in turn translate into an increase in the take-up of disability or unemployment insurances.

Figure 10 provide the RD estimates for being a disability, unemployment or welfare beneficiary, respectively, for each event during the six-year period around spouse's death. The proportion of widows who receive welfare benefits mechanically increased following the SB reform (panel (a)). Indeed, without survivor benefits, more of them were living below a certain income threshold. They then became eligible to welfare benefits and it translated in a 6 pp increase in the proportion of beneficiaries three years after spouse's death (while the baseline was almost null).

Secondly, we find empirical evidence of an increased proportion of widows receiving disability benefits (+1.5 pp compared to the 12.7% baseline). We believe the mechanism is the following. We suppose there is a non zero cost c of claiming disability insurance (assessment, administrative process, etc.). One decides to claim disability insurance only if it increases total income from

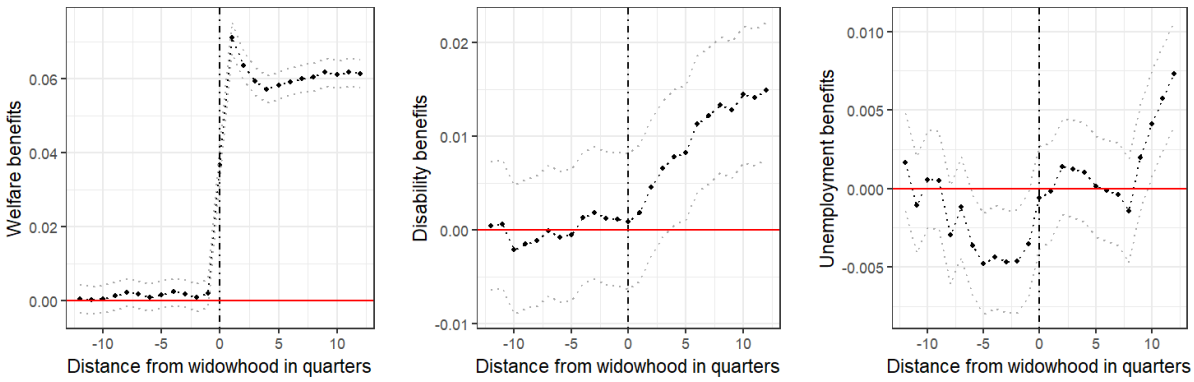
analyses).

more than c. Before the reform, survivor benefits were one-to-one reduced with disability benefits. So there should not have any disability insurance claiming as soon as expected disability benefits are lower than $SB + c$. In particular, individuals who are eligible to SB and eligible to disability insurance with a low level of benefits may decide not to claim. After the reform, many individuals loose eligibility to SB. Individuals with low disability benefits now have more incentive to claim it. Consistent with this interpretation, Appendix Figure B.10 shows that disability benefits are low for a large part of the population. Before spousal death, most widows born before 1950 (who were not impacted by the reform) have disability benefits below the minimum wage. It suggests that the threshold $SB + c$ can be reached by a significant part of the population eligible to disability insurance. Furthermore, Appendix Figure B.11 shows that the effect of the reform is concentrated on low level of disability benefits. The RD estimate reaches +1.5pp in the take-up of disability insurance three years after death for individuals with benefits below the minimum wage, while we do not find any significant response for above the minimum wage. This is in line with the assumption that only the former group has incentive to change his take-up behavior when access to survivor benefits is restricted.

Finally, we do not find any evidence of an increased proportion of unemployment beneficiaries following the SB reform (panel (c)).

Figure 10: Effect of the reform on other social security programs

(a) Welfare benefits (b) Disability benefits (c) Unemployment benefits



NOTE: Disability benefits, unemployment benefits and welfare benefits are dummies equal to 1 if disability, unemployment or welfare benefits respectively are > 0 and 0 otherwise. All estimates are based on a 8-years symmetric bandwidth. Dotted lines represent the 95% confidence intervals.
SCOPE: Widows born between 1946 and 1953 whose husband died after 2003, who do not care for a child and who were aged below 62 at death time.
SOURCE: CBS.

5.3 Heterogeneity in labor supply response

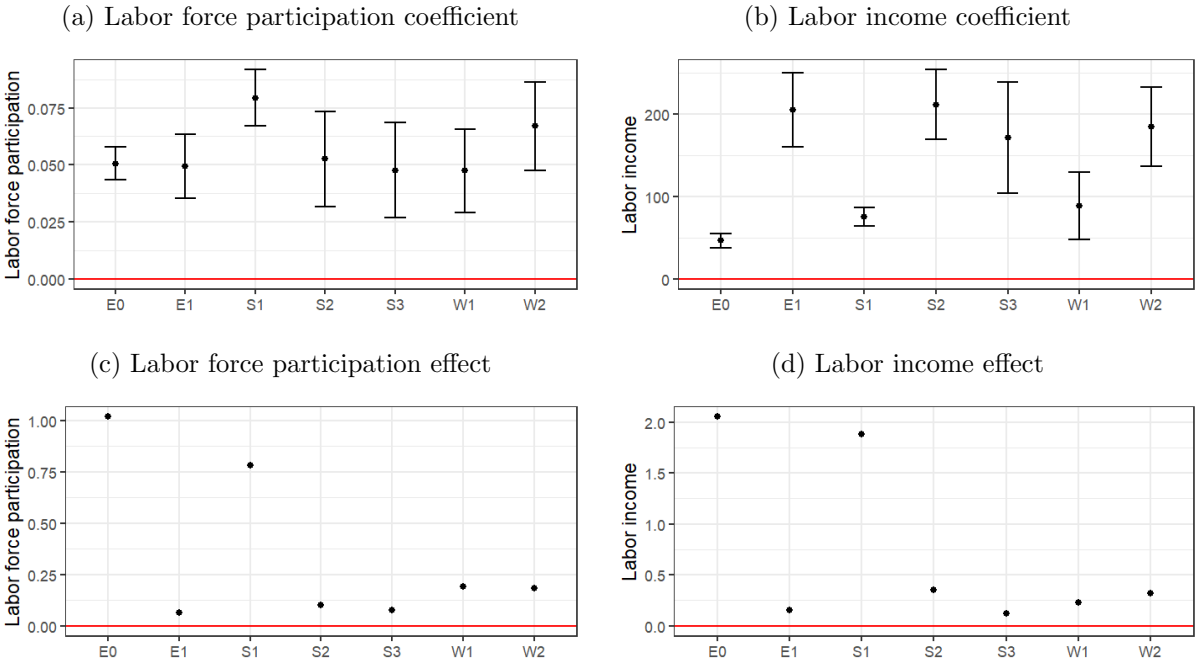
The overall labor supply effects we estimate may hide substantial heterogeneity across subgroups. We study how different the responses to the reform are, according to self-insurance level before spouse's death. Indeed, the necessity, efficacy, and cost-effectiveness of government welfare policies depends on the risks that households face and the actions that they can take to self-insure, for instance by adjusting their saving and labor supply. Said differently, larger labor supply responses – which correspond to a stronger need to self-insure – imply a greater scope for welfare-improving social insurance due to lack of adequate formal insurance. We distinguish individuals according to their working status before death, the share in the total household income they hold and their level of liquid wealth.

We first carry out the RD analyses separately on two sub-population according to whether the widow worked during the year preceding spouse's death or not.[11] On one hand, one would expect that widows who were already in the labor market react more to the SB elimination than others, as it is easier to remain in than to enter the labor force. On the other hand, one would expect that widows who were not working at the time of their spouse's death react more because their ex-spouse's income was probably their main source of income. In that case, labor supply response is a direct mean to self-insure. Figure 11 shows evidence for our second possible interpretation. Indeed, both sub-populations (E0 for non working widows and E1 for the others) show a 5 pp increase in labor force participation following the reform. Compared to the baselines, this translates into a 102% and a 7% increase, respectively. In terms of labor income, the SB reform induced a 47 monthly euros and a 205 monthly euros increase, that is to say a 205% and a 16% increase, respectively. RDD estimates according to distance to spouse's death graphs are shown in Appendix Figure B.12.

According to OCDE (2018), SB pursue the main objective of smoothing standard of living after the partner's death. At spouse's death, household standard of living varies with the losses of the partner resources and economies of scales. Accepted the modified OECD scale, a single-person household requires 33% less household income to have the same standard of living as an otherwise comparable couple without children. In other words, all other things being equal, a survivor whose share of total household income was below $\frac{2}{3}$ decreases her standard of living following a spouse's death and increases it otherwise. One should then expect that widows whose income share is low to react more than others in order to limit the decrease in standard of living. Given the fact that very few women have a total household income share above $\frac{2}{3}$, we

[11] The estimations based on the widows who did not work during the year preceding spouse's death are different from those that distinguish between intensive and extensive margins. Indeed, in the paper, the previous analyses on extensive margin (labor force participation) were carried out on the full population – i.e. widows who do not participate in the labor market are included, as well as those who were already in the market.

Figure 11: Response heterogeneity



NOTE: Labor force participation is a dummy equal to 1 if labor income > 0 and 0 otherwise. Labor income refers to wage income, profits from self-employment and income from other activity.
E0 and E1 refer to employment status. Employment status is a dummy equal to 1 if widow worked at least once during the year preceding spouse's death (E1) and 0 otherwise (E0). S1 to S3 refer to income share tercile, which is the tercile of the widows' total household income share during the three-years preceding spouse's death. W1 and W2 refer to liquid wealth group, W1 being the group below the liquid wealth distribution median and W2 the above median group. All estimates are computed three years after spouse's death and are based on a 8-years symmetric bandwidth. Confidence intervals are displayed at the 95% level.
SCOPE: Widows born between 1946 and 1953 whose husband died after 2003, who do not care for a child and who were aged below 62 at death time.
SOURCE: CBS.

divide the widowed population into three terciles of income share. We empirically find 8% and 33% as first and second thresholds. The estimates for the separate RD analyses are displayed in Figure 11 (S1 for first, S2 for second and S3 for third income share terciles). Consistent with the smoothing standard of living objective, we find that widows belonging to the lowest income share category respond more to the SB reform than others. The results even show a negative gradient in response magnitude with regards to income share level. Indeed, widows belonging to the S1 sub-population increased their labor force participation by 78% *versus* 10% for S2 and 8% for S3 (corresponding to a 7.9 pp 5.3 pp and 4.8 pp increase, respectively). In terms of labor income, the SB reform translated into a 188%, 35% and 13% increase for S1 to S3 sub-populations, respectively. RDD estimates according to distance to spouse's death graphs are shown in Appendix Figure B.13.

Finally, we investigate response heterogeneity according to liquid wealth. We define liquid wealth as total wealth excluded housing wealth. Indeed, we believe that the latter is more diffi-

cult to use for consumption smoothing when facing an economic shock. Conversely, individuals can quite easily smooth their standard of living through dissaving. One would expect widows with a high liquid wealth level to react less to the SB reform because they are able to smooth their standard of living using financial wealth without adjusting labor supply. We separately carry out the RD analysis on two sub-populations defined according to liquid wealth level (below and above the median). Surprisingly, as shown in Figure 11, we do not find any evidence of a wealth gradient in response to the SB reform.

6 Conclusion

This paper provides novel insights on the causal effects of public survivor insurance on labor supply outcomes. We take advantage of the 1996 Dutch reform that enacted a very strict restricted access to SB for individuals born after January, 1st 1950, using high quality administrative data. Consistent with previous literature, and based on a regression discontinuity design, we show that a decrease in SB increases labor supply. We thus provide empirical evidence that SB schemes are a disincentive to female employment. We find a stronger labor supply response for widows with less ability to self-insure against the loss of income following spousal death, as well as program substitution towards disability insurance.

Taken together, the effects we find raise the question of whether survivor insurance schemes are still needed (OCDE 2018), or should just be reformed (James et al. 2009). Some countries decided to lower SB and introduced a means-test while others, like Sweden, eliminated the scheme and replaced it with temporary benefits (Monticone et al. 2008). As SB schemes represent 13% of old-age benefits in average in OECD countries (OCDE 2018), and contribute to reducing the gender pension gap, their modification remains very sensitive. The reform implemented in the Netherlands actually consists in an elimination of the public SB scheme for all individuals who are not disabled and whose children have reached majority. Then, overall, our results suggest that the elimination of the survivor benefits schemes neutralizes the disincentive to female employment, but constrains some widows to take up other social security benefits. Disincentive to work is only one aspect of the overall effect of survivor insurance. A complete evaluation of the scheme would also require to investigate the protective role of survivor benefits for the survivors.

The results presented in this paper could be extended in several directions. First, even if our findings support labor supply as a key self-insurance mechanism against the income shock following a spouse's death, we do not find evidence of the importance of liquid wealth in the

labor supply response. This is at odds with expectations and requires further investigation. A way to shed light on this question is to investigate the economic consequences of a spouse's death on labor supply and wealth des-accumulation, with regards to standard of living variation at the time of death. Another possible indirect but important channel for the long run effect of SB elimination is its health effects. As the death of a spouse has an impact on the survivors' mental health (Siflinger 2017) and this mental health is correlated with income (Stewart-Brown et al. 2015), we can expect some effects of the large income drop generated by the SB reform. Finally, the drop in SB induced by the reform modified the gain from marriage. Indeed, according to the marriage market theory of Becker (1973) and conditional on the spouse's death, SB provide benefits to the survivor that renders utility deriving from marriage, called marriage *surplus*. As shown by Persson (2020) in the Swedish case, a SB reform can modify the marriage and divorce decisions. Analyzing the effect of a restricted access to SB on the health of surviving spouses on and on conjugality, are therefore interesting avenues for future research.

References

Atalay, K. & Barrett, G. F. (2015), 'The impact of age pension eligibility age on retirement and program dependence: Evidence from an Australian experiment', *Review of Economics and Statistics* **97**(1), 71–87.

Atav, T., Jongen, E. & Rabaté, S. (2021), 'Increasing the effective retirement age: Key factors and interaction effects', *IZA Discussion Paper* **14150**.

Bargain, O., González, L., Keane, C. & Özcan, B. (2012), 'Female labor supply and divorce: New evidence from Ireland', *European Economic Review* **56**(8), 1675–1691.

Becker, G. S. (1973), 'A theory of marriage: Part 1', *Journal of Political Economy* **81**(4), 813–846.

Böheim, R. & Topf, M. (2021), 'Unearned income and labor supply: Evidence from survivor pensions in Austria', *IZA Discussion Paper* **13994**.

Bonnet, C., Garbinti, B. & Solaz, A. (2021), 'The flip side of marital specialization: the gendered effect of divorce on living standards and labor supply', *Journal of Population Economics* **34**(2), 515–573.

Borella, M., De Nardi, M. & Yang, F. (2019), 'Are marriage-related taxes and Social Security benefits holding back female labor supply?', *NBER working paper* **26097**.

Borghans, L., Gielen, A. C. & Luttmer, E. F. (2014), 'Social support substitution and the earnings rebound: Evidence from a regression discontinuity in disability insurance reform', *American economic Journal: Economic Policy* **6**(4), 34–70.

Coe, N. & Haverstick, K. (2010), 'Measuring the spillover to disability insurance due to the rise in the full retirement age', *Boston College Center for Retirement Research Working Paper* **21**.

Duggan, M., Singleton, P. & Song, J. (2007), 'Aching to retire? The rise in the full retirement age and its impact on the social security disability rolls', *Journal of Public Economics* **91**(7-8), 1327–1350.

Fadlon, I. & Nielsen, T. H. (2019), 'Household labor supply and the gains from social insurance', *Journal of Public Economics* **171**, 18–28.

Fadlon, I. & Nielsen, T. H. (2021), 'Family labor supply responses to severe health shocks: Evidence from Danish administrative records', *American Economic Journal: Applied Economics* **13**(3), 1–30.

Fadlon, I., Ramnath, S. P. & Tong, P. K. (2019), 'Market inefficiency and household labor supply: Evidence from Social Security's survivors benefits', *NBER Working Paper* **25586**.

Giupponi, G. (2019), 'When income effects are large: labor supply responses and the value of welfare transfers', *CEP Discussion Paper* **1651**.

Goux, D., Maurin, E. & Petrongolo, B. (2014), 'Worktime regulations and spousal labor supply', *American Economic Review* **104**(1), 252–76.

Groneck, M. & Wallenius, J. (2021), 'It sucks to be single! Marital status and redistribution of social security', *The Economic Journal* **131**(633), 327–371.

Inderbitzin, L., Staubli, S. & Zweimüller, J. (2016), 'Extended unemployment benefits and early retirement: Program complementarity and program substitution', *American Economic Journal: Economic Policy* **8**(1), 253–88.

James, E. et al. (2009), 'Rethinking survivor benefits', *World Bank, Social Protection and Labor Discussion Paper* **928**.

Karlström, A., Palme, M. & Svensson, I. (2008), 'The employment effect of stricter rules for eligibility for DI: Evidence from a natural experiment in Sweden', *Journal of Public Economics* **92**(10-11), 2071–2082.

Kaygusuz, R. (2015), 'Social security and two-earner households', *Journal of Economic Dynamics and Control* **59**, 163–178.

Lee, D. S. & Lemieux, T. (2010), 'Regression discontinuity designs in economics', *Journal of economic literature* **48**(2), 281–355.

Li, X. & Maestas, N. (2008), 'Does the rise in the full retirement age encourage disability benefits applications? evidence from the health and retirement study', *Michigan Retirement Research Center Research Paper No. 2008-198* **198**.

Monticone, C., Ruzik, A. & Skiba, J. (2008), 'Women's pension rights and survivors' benefits: A comparative analysis of EU member states and candidate countries', *ENEPRI Research Reports* **53**.

Mortelmans, D. (2020), conomic consequences of divorce: A review, *in* M. K. Trappe, ed., 'Parental Life Courses after Separation and Divorce in Europe', Springer, Cham, pp. 23–41.

Nagore-Garcia, A. & van Soest, A. (2021), 'How does an allowance for a non-working younger partner affect the retirement behaviour of couples?', *Journal of pension economics and finance* **31**(1).

Nishiyama, S. (2019), 'The joint labor supply decision of married couples and the US Social Security pension system', *Review of Economic Dynamics* **31**, 277–304.

OCDE (2018), 'Are survivor pensions still needed?', *OECD Policy Brief* **1**.

Persson, P. (2020), 'Social insurance and the marriage market', *Journal of Political Economy* **128**(1), 252–300.

Poortman, A.-R. (2005), 'Women's work and divorce: A matter of anticipation? a research note', *European Sociological Review* **21**(3), 301–309.

Rabaté, S. & Rochut, J. (2020), 'Employment and substitution effects of raising the statutory retirement age in France', *Journal of Pension Economics & Finance* **19**(3), 293–308.

Saez, E. (2010), 'Do taxpayers bunch at kink points?', *American Economic Journal: Economic Policy* **2**(3), 180–212.

Siflinger, B. (2017), 'The effect of widowhood on mental health-an analysis of anticipation patterns surrounding the death of a spouse', *Health Economics* **26**(12), 1505–1523.

Staubli, S. (2011), 'The impact of stricter criteria for disability insurance on labor force participation', *Journal of Public Economics* **95**(9-10), 1223–1235.

Staubli, S. & Zweimüller, J. (2013), 'Does raising the early retirement age increase employment of older workers?', *Journal of Public Economics* **108**, 17–32.

Stewart-Brown, S., Samaraweera, P. C., Taggart, F., Kandala, N.-B. & Stranges, S. (2015), 'Socioeconomic gradients and mental health: implications for public health', *The British Journal of Psychiatry* **206**(6), 461–465.

van der Vaart, J., Alessie, R. & van Ooijen, R. (2020), 'Economic consequences of widowhood', *Netspar Design Paper (160)* .

A Data

The data used in this study for divorce probability analyses are individual-level or household-level data provided by Statistics Netherlands (CBS). There are accessible via a remote access environment in a set of different datasets. In a dataset, each individual is identified by a unique number (which has been pseudomyzed). The linkage of the different datasets is performed using the individual identifier. 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 survivor 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 A.1 lists all the sources we use, and we then provide more detailed information about each of the data we use. Information about the administrative micro data sets can be found at https://www.cbs.nl/nl-nl/onze-diensten/maatwerk-en-microdata/microdata-zelf-onderzoek-doen/catalogus-microdata (available in Dutch only)

gbapersoontab[12]

It contains demographic background data (e.g. gender, year of birth, migration background) for the universe of the Dutch population, that is all persons who appear in the registered in the population register (Basic Register of Persons, BRP) since 1 October 1994.

gbaoverlijdentab[13]

Contains the date of death of all persons who have died since 1 October 1994 and were registered in the population register (Basic Register of Persons, BRP) at the time of death. It also contains the date of death of persons who are not residents but were once residents of the Netherlands since 1 October 1994 and whose information about the death is received in the Register of Non-Residents (RNI). The main source of information for this dataset is the municipal registries (Gemeentelijke Basisadministratie Persoonsgegevens, GBA).

[12] Link to gbapersoontab documentation in Dutch
[13] Link to gbaoverlijdentab documentation in Dutch

polisbus[14] **and spolisbus**[15]

It contains information on the full universe of job in the Netherlands, available from year 2006. There is one line by employment spells, with information on both the individual (wage, hours worked, contributions, etc) and the firm (sector, collective agreement, etc).

betab[16]

This file contains data on the economic activity, the size class and the municipality of establishment of firms. The data comes from the Polis administration (SPOLISBUS) and the ABR. File are available on a yearly basis the period 1999-2021.

secm datasets[17]

The secm datasets contain monthly information on the income receive each month from year 1999 for different types of incomes: employment wage (SECMWERKNDGAMNBEDRABUS), profit (SECMZLFMNDBEDRAGBUS), other activities (SECMOVACTMNDBEDRAGBUS), unemployment benefits (SECMWERKLMNDBEDRAGBUS), disability benefits (SECMZIEKTAOMNDBEDRAGBUS), other benefits (SECMSOCVOORZOVMNDBEDRAGBUS) welfare (SECMBIJSTMNDBEDRAGBUS) and pension income (SECMPENSIOENMNDBEDRAGBUS).

These datasets are constructed by Statistic Netherlands using different administrative data sources (taxes, social security, pension funds). The initial form of the dataset is spell data, and contains a date of beginning, a date of end and an associated monthly amount. A new line is added for a given individual everytime the monthly amount she perceives changes. The secmbus dataset combines the different sources mentioned above in a single dataset containing the main source of income and associated amount for each spell.

ANW datasets[18]

Record on anw benefits (benefits for surviving relatives, half-orphans and orphans). The figures are based on the registration of the Sociale Verzekeringsbank (SVB). Database available per month for the period 200409-202112.

[14] Link to polis documentation in Dutch
[15] Link to spolis documentation in Dutch
[16] Link to betab documentation in Dutch
[17] Link to secm documentation in Dutch
[18] Link to documentation in Dutch

vehtab[19]

The vehtab data provide information about the wealth of the full universe of the Dutch household. It is available from year 2006, and contains on a yearly basis the value of asset and debt owned, for different types of wealth (e.g financial assets, business assets, housing). The vehtab data do not cover all wealth in the national accounts, as pension wealth is not included. Depending on the type of wealth, the value is either observed (from tax data) or computed by Statistic Netherlands.

Table A.1: Datasets used

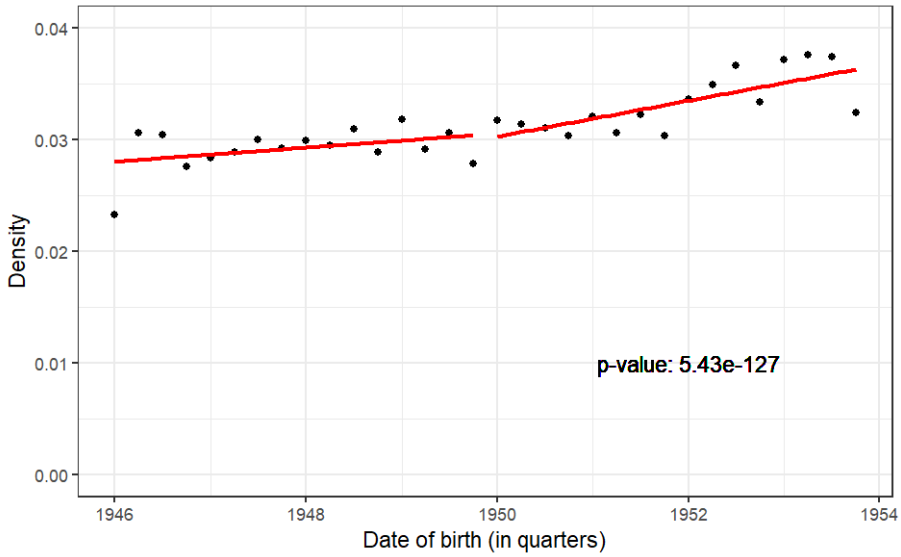
Content	**Name of dataset**	**Years**
Date of birth and gender	GBAPERSOON2019TAB (V1)	2019
Death	GBAOVERLIJDENTAB2019TAB (V1)	2019
Civil status and date	GBABURGERLIJKESTAAT2019BUS (V1)	2019
Households characteristics	GBAHUISHOUDENS2019BUS (V1)	2019
Linkage parent-child	KINDOUDER2019TAB (V1)	2019
Anw benefits	Anwuitkeringtab	2005-2019
Individual income		
Wage income	SECMWERKNDGAMNBEDRABUSV20181	2018
Profits from self-employment	SECMZLFMNDBEDRAGBUSV20181	2018
Income from other activity	SECMOVACTMNDBEDRAGBUSV20181	2018
Social welfare benefits	SECMBIJSTMNDBEDRAGBUSV20181	2018
UI benefits	SECMWERKLMNDBEDRAGBUSV20181	2018
DI and sickness benefits	SECMZIEKTAOMNDBEDRAGBUSV20181	2018
Other social security benefits	SECMSOCVOORZOVMNDBEDRAGBUSV20181	2018
Pension income	SECMPENSIOENMNDBEDRAGBUSV20181	2018
Total wealth, liquid wealth	VEHTAB	2006-2018
Activity sector, wage and hours worked	POLISBUS and SPOLISBUS	2006-2019
Activity sector (1digit classification)	BETAB	2006-2019

NOTE: SSB stands for *Sociaal Statistisch Bestand* (Social Statistical Database).
SOURCE: CBS microdata catalogue.

[19] Link to vehtab documentation in Dutch

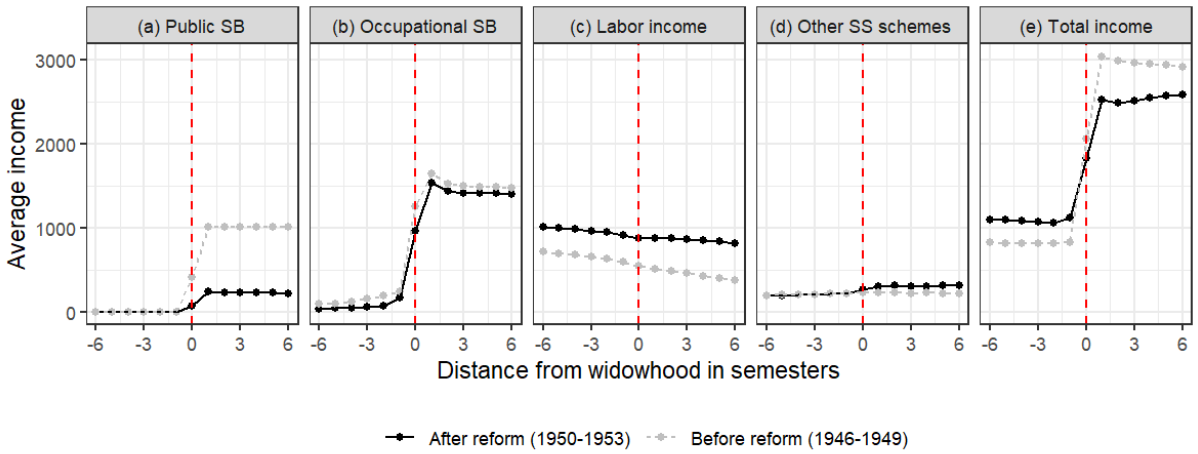
B Additional Tables and Figures

Figure B.1: Distribution of widows by date of birth and McCrary test



NOTE: The graphs plot the empirical probability density function of widows by quarterly date of birth. It also reports the test statistics and associated standard error in parenthesis of a McCrary test of the discontinuity in the probability density function of the running variable at the January 1950 threshold.
SCOPE: Widows born between 1946 and 1953 whose husband died after 2003, who do not care for a child and who were aged below 62 at death time.
SOURCE: CBS.

Figure B.2: Income level according to distance to spouse's death



NOTE: SS stands for social security. Public SB refers to first pillar survivor insurance. Occupational SB refers to second pillar survivor pension plans. It is computed as the difference between public SB and total pension. Labor income refers to wage income, profits from self-employment and income from other activity. Other social security schemes income refers to social welfare benefits, unemployment benefits and sickness/disability benefits.
SCOPE: Widows born between 1946 and 1953 whose husband died after 2003, who do not care for a child and who were aged below 62 at death time.
SOURCE: CBS.

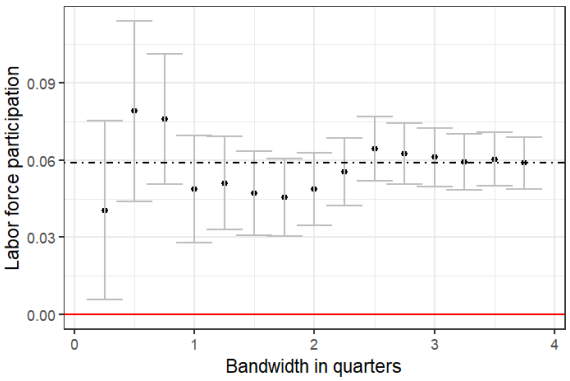
Table B.1: Covariate balancing tests

	Regression discontinuity					Mean
	(1)	(2)	(3)	(4)	(5)	(6)
Age at death	-0.10*	0.00	-0.04	0.00	-0.04	59
	(0.056)	(0)	(0.086)	(0)	(0.07)	
Working status (in pp)	0.009	0.009	0.012	0.013	0.010	0.40
	(0.01)	(0.01)	(0.016)	(0.016)	(0.011)	
Income share (in pp)	-0.005	-0.003	0.006	0.005	0.001	0.19
	(0.005)	(0.005)	(0.008)	(0.008)	(0.007)	
Labor income (in euros)	9.29	11.27	42.36	61.13	22.65	661
	(80)	(82)	(124)	(125)	(87)	
Wealth (in k euros)	-1.24	4.91	-8.31	-5.23	-3.81	229
	(9.94)	(10.35)	(15.31)	(15.45)	(10.7)	
Nb obs.	38,340	38,340	38,340	38,340	38,340	38,340

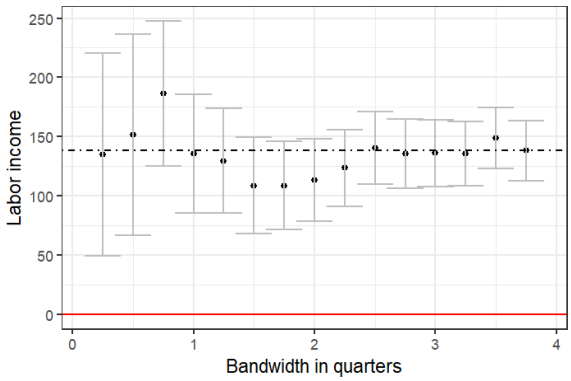
NOTE: The table reports the coefficient δ from estimating equation (1) for different outcome variables. Robust standard errors are reported in parenthesis. P-value: *** p<0.01. ** p<0.05, * p<0.1. Columns (1) and (2) are based on a linear parametric specification, without and with controls respectively. Columns (3) and (4) are based on a quadratic parametric specification, without and with controls respectively. Column (5) is based on non-parametric local linear regression. Column (6) reports the mean of the outcome variable in the control group. Working status refers to the proportion of individuals who have had at least a period of work during the year preceding death. All estimates are based on a 8-years symmetric bandwidth.
SCOPE: Widows born between 1946 and 1953 whose husband died after 2003, who do not care for a child and who were aged below 62 at death time.
SOURCE: CBS.

Figure B.3: Effect of the reform: sensitivity to bandwidth

(a) Labor force participation

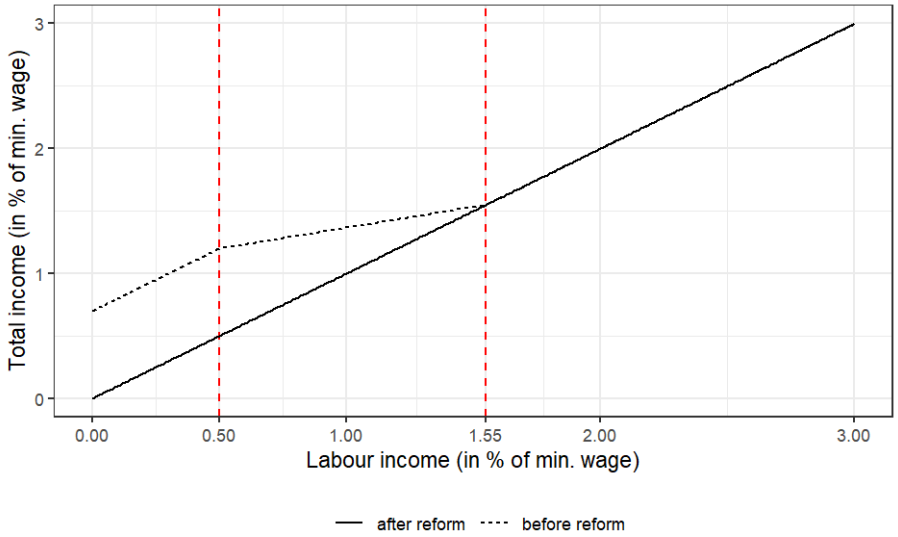


(b) Labor income



NOTE: Labor force participation is a dummy equal to 1 if labor income > 0 and 0 otherwise. Labor income refers to wage income, profits from self-employment and income from other activity. All estimates are computed three years after spouse's death. Confidence intervals are displayed at the 95% level.
SCOPE: Widows born between 1946 and 1953 whose husband died after 2003, who do not care for a child and who were aged below 62 at death time.
SOURCE: CBS.

Figure B.4: Survivor benefits budget set



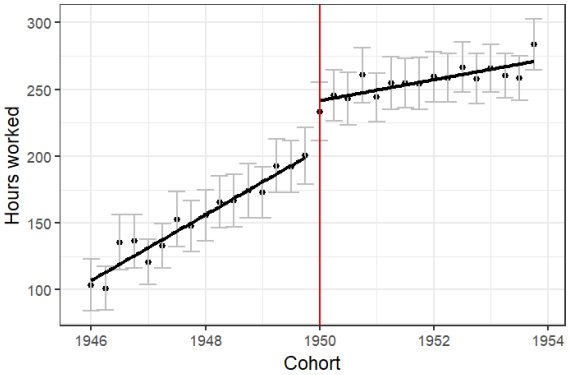
Source: Legislation.

Figure B.5: Total income three years after spouse's death

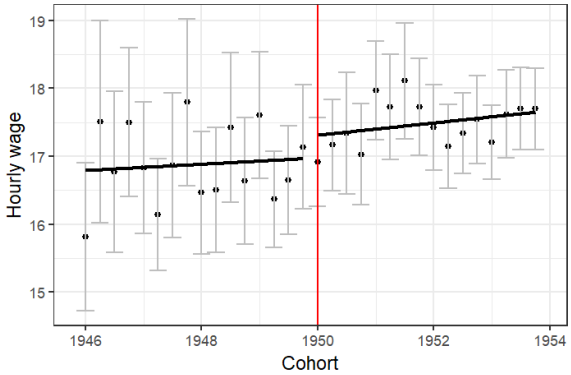
Note: Total income includes *(i)* labor income that refers to wage income, profits from self-employment and income from other activity, *(ii)* survivor benefits and *(iii)* other insurance schemes income that refers to social welfare benefits, unemployment benefits and sickness/disability benefits.
Scope: Widows born between 1946 and 1953 whose husband died after 2003, who do not care for a child and who were aged below 62 at death time. Observations with null total income are filtered.
Source: CBS.

Figure B.6: Effect of the reform on the intensive margin

(a) Hours worked



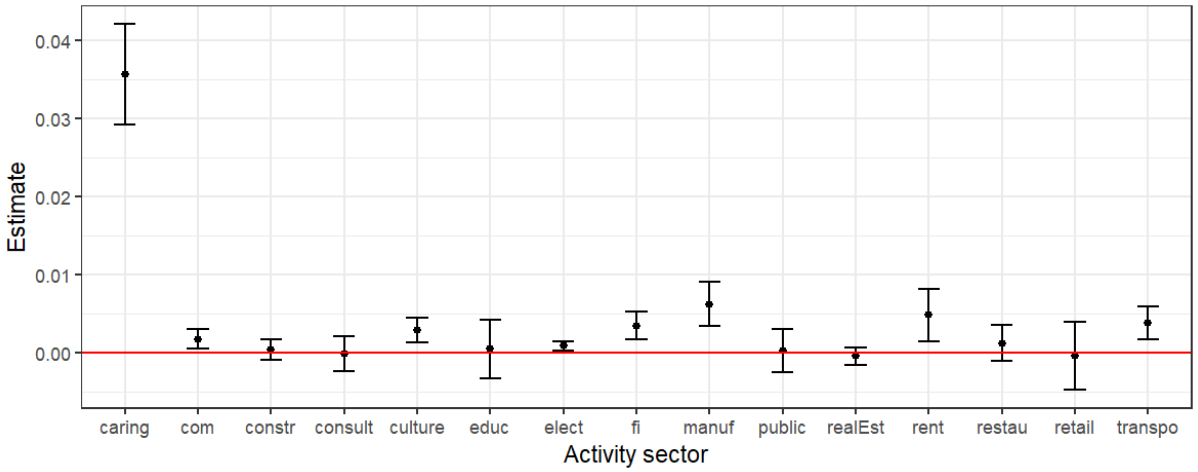
(b) Hourly wage



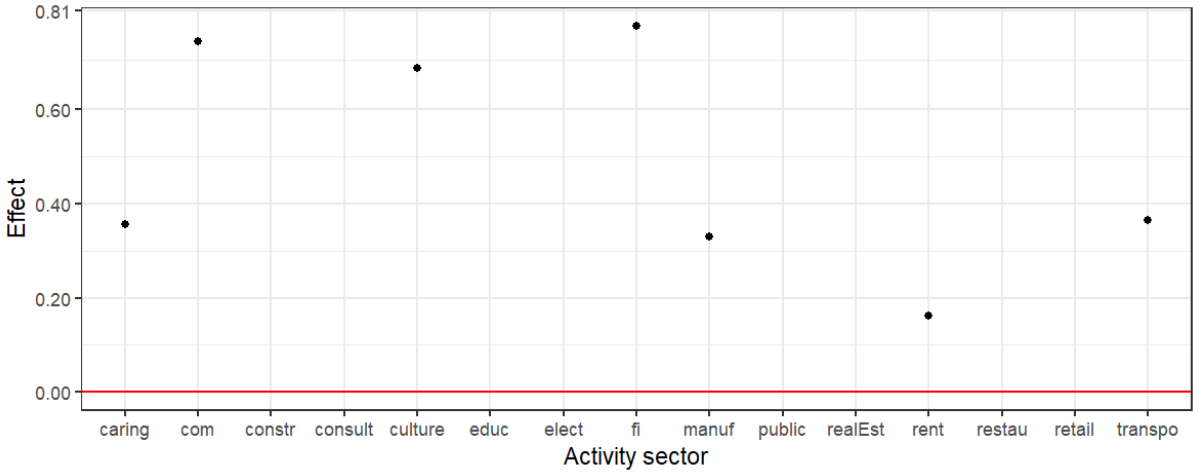
NOTE: Hourly wage is defined as wage income divided by the number of hours worked.
SCOPE: Widows born between 1946 and 1953 whose husband died after 2003, who do not care for a child and who were aged below 62 at death time.
SOURCE: CBS.

Figure B.7: Participation to each sector

(a) RD estimates

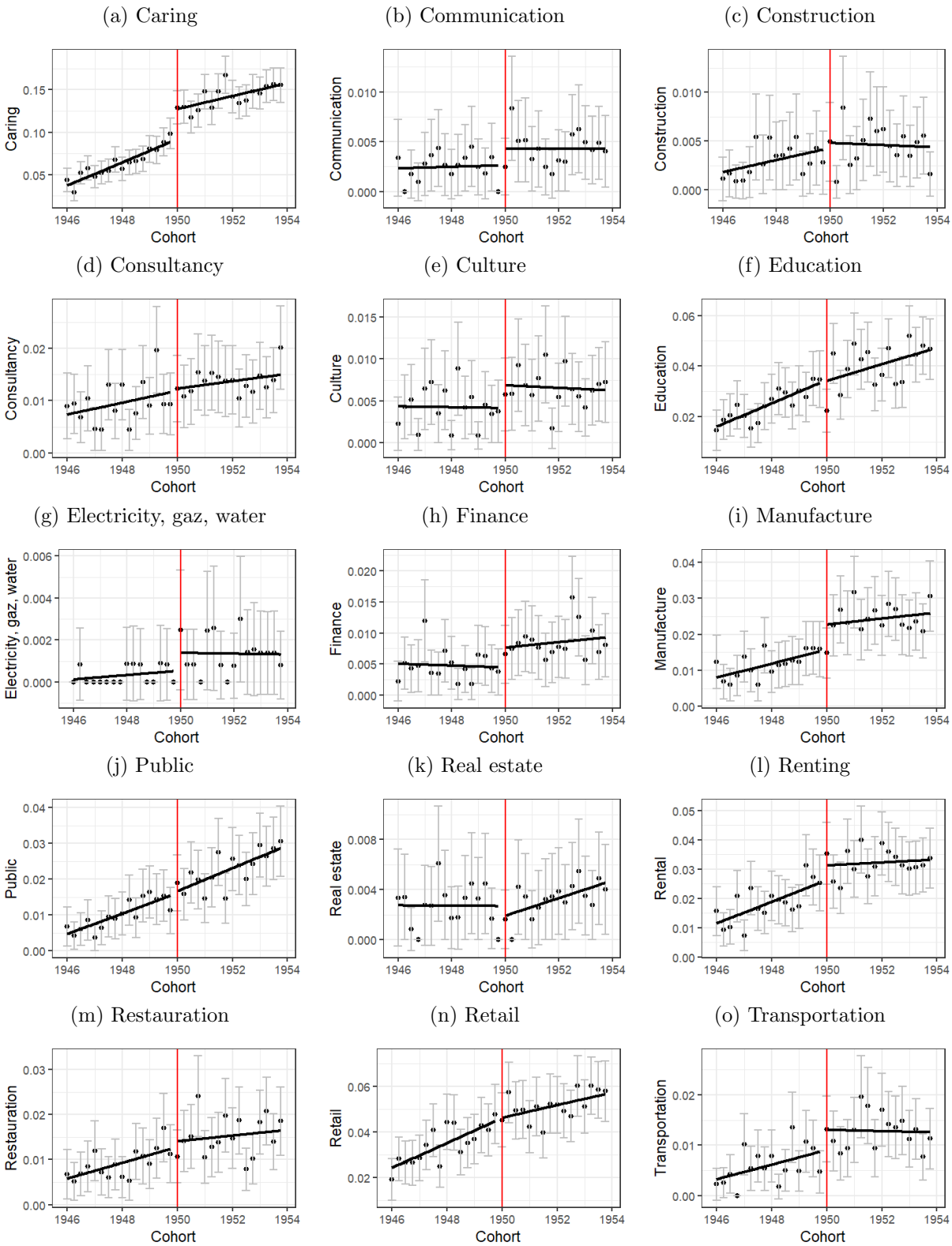


(b) Reform effects



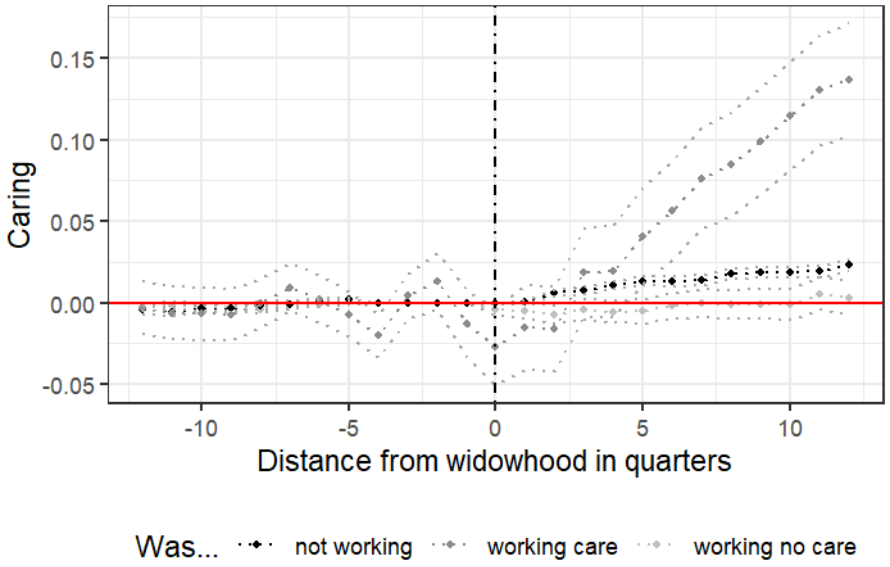
NOTE: For each activity sector, the RD graph represents the extensive margin response to the reform (our variable of interest is a dummy indicating whether labor supply is > 0 within the activity sector considered). All estimates are based on a 8-years symmetric bandwidth. Confidence intervals are displayed at the 95% level.
SCOPE: Widows born between 1946 and 1953 whose husband died after 2003, who do not care for a child and who were aged below 62 at death time.
SOURCE: CBS.

Figure B.8: RD graph per activity sector



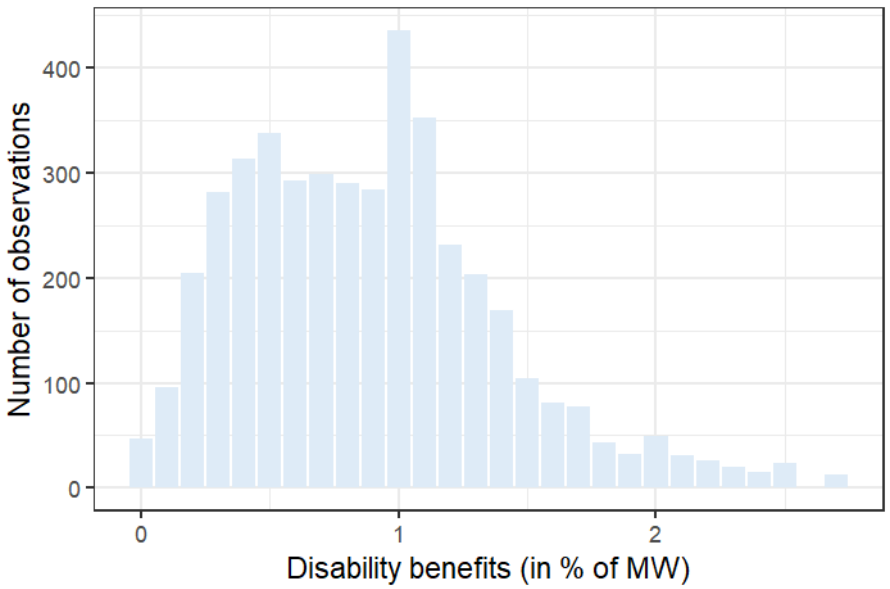
NOTE: For each activity sector, the RD graph represents the extensive margin response to the reform (our variable of interest is a dummy indicating whether labor supply is > 0 within the activity sector considered).
SCOPE: Widows born between 1946 and 1953 whose husband died after 2003, who do not care for a child and who were aged below 62 at death time.
SOURCE: CBS.

Figure B.9: RD coefficients for participation to caring sector



Note: Caring is a dummy equal to 1 if widow works in the caring sector and labor income > 0 and 0 otherwise. We look at response to the reform according to whether the widow was not working during the year preceding spouse's death or if she was working but in a different from caring activity sector. All estimates are based on a 8-years symmetric bandwidth. Dotted lines represent the 95% confidence intervals.
Scope: Widows born between 1946 and 1953 whose husband died after 2003, who do not care for a child and who were aged below 62 at death time.
Source: CBS.

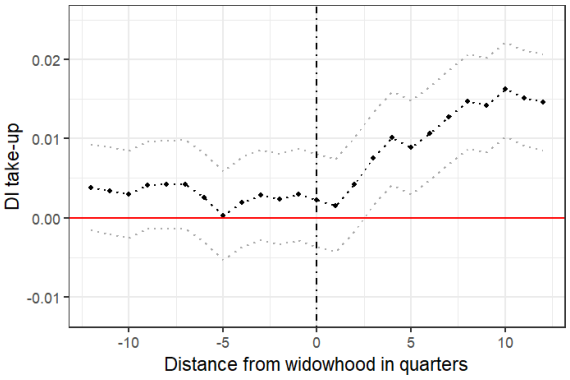
Figure B.10: Average disability benefits distribution



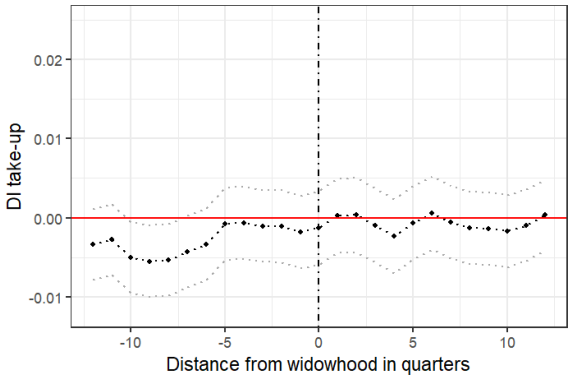
Note: MW stands for minimum wage.
Scope: Widows born between 1946 and 1953 whose husband died after 2003, who do not care for a child and who were aged below 62 at death time.
Source: CBS.

Figure B.11: RD coefficients for the take-up of DI, according to DI benefits level

(a) Low DI benefits



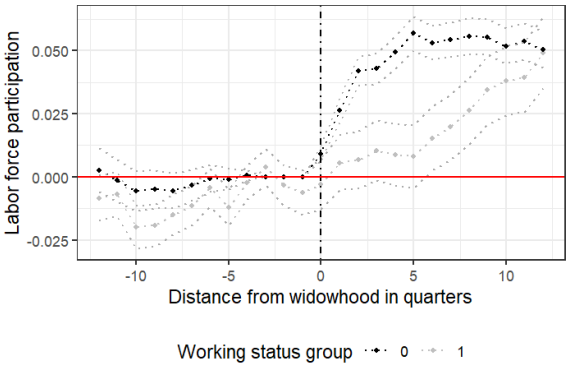
(b) High DI benefits



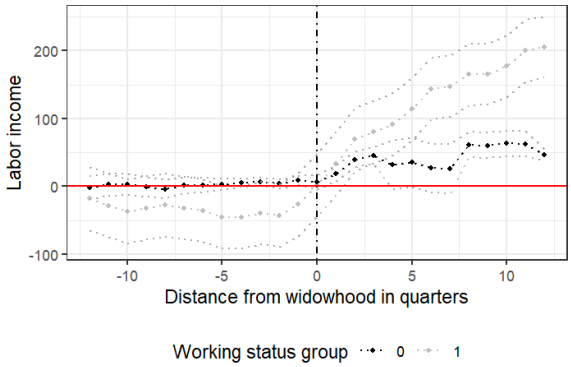
NOTE: Disability benefits is a dummy equal to 1 if disability benefits are > 0 and 0 otherwise. Low DI benefits are disability benefits below the minimum wage and high DI benefits are disability benefits equal to or above the minimum wage.
SCOPE: Widows born between 1946 and 1953 whose husband died after 2003, who do not care for a child and who were aged below 62 at death time.
SOURCE: CBS.

Figure B.12: RDD estimates per working status

(a) Labor force participation



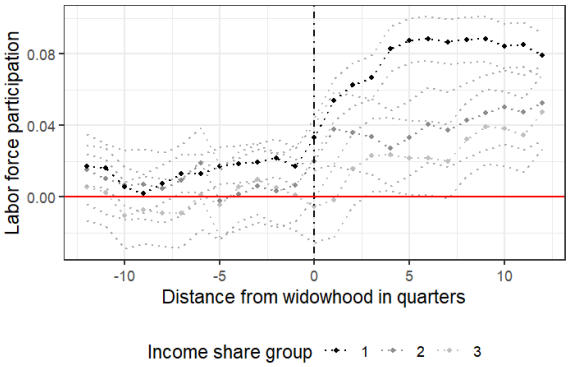
(b) Labor income



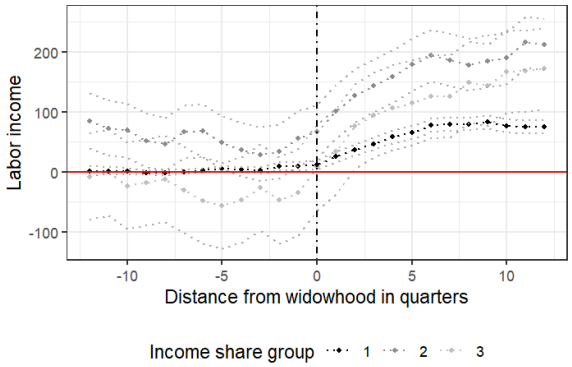
NOTE: Labor force participation is a dummy equal to 1 if labor income > 0 and 0 otherwise. Labor income refers to wage income, profits from self-employment and income from other activity. Working status is a dummy equal to 1 if widow worked at least once during the year preceding spouse's death and 0 otherwize.
SCOPE: Widows born between 1946 and 1953 whose husband died after 2003, who do not care for a child and who were aged below 62 at death time. All estimates are based on a 8-years symmetric bandwidth. Dotted lines represent the 95% confidence intervals.
SOURCE: CBS.

Figure B.13: RDD estimates per income share tercile

(a) Labor force participation



(b) Labor income



NOTE: Labor force participation is a dummy equal to 1 if labor income > 0 and 0 otherwise. Labor income refers to wage income, profits from self-employment and income from other activity. Income share tercile is the tercile of the widows' total household income share during the three-years preceding spouse's death.
SCOPE: Widows born between 1946 and 1953 whose husband died after 2003, who do not care for a child and who were aged below 62 at death time. All estimates are based on a 8-years symmetric bandwidth. Dotted lines represent the 95% confidence intervals.
SOURCE: CBS.

Table B.2: Elasticity heterogeneity

	(a) Working status		(b) Income share tercile		
	(0)	(1)	(1)	(2)	(3)
δ	0.05	0.05	0.08	0.05	0.05
Effect (in %)	102	6.5	78	10.2	7.8
Elasticity	-2.09	-0.22	-1.40	-0.28	-0.33
Nb obs.	20,687	17,599	12,022	9,336	9,486

NOTE: Working status is a dummy equal to 1 if widow worked at least once during the year preceeding spouse's death and 0 otherwize. Income share tercile is the tercile of the widows' total household income share during the three-years preceeding spouse's death. δ refers to the labor force participation RD estimate. All estimates are computed three years after spouse's death and are based on a 8-years symmetric bandwidth.
SCOPE: Widows born between 1946 and 1953 whose husband died after 2003, who do not care for a child and who were aged below 62 at death time.
SOURCE: CBS.