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Spousal Death, Mental Health and Survivor Benefits

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Abstract

In this paper, we estimate the causal effect of social security income on mental health. We focus on widowhood, a life event associated with large and persistent mental health declines, and exploit a reform of the Dutch survivor benefits system that introduced cohort-based restrictions in benefit eligibility. Using administrative data, we find that reduced access to survivor benefits increases antidepressant use by 9%, accounting for 35% of the overall rise in antidepressant use following spousal death. A mechanism analysis shows that survivor benefits stabilize mental health by smoothing living standards, highlighting the potential welfare gains from well-targeted income support policies.

JEL classification

I12, J14, H55

Keywords

widowhood, survivor benefits, mental health, antidepressant use, income security

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1 Introduction

Mental health problems are highly prevalent in OECD countries: nearly half of the population experiences a mental health disorder at some point in life, and up to one in five individuals lives with a mental health condition at any given time (OECD, 2025). Both prevalence and recognition have increased in recent years, as reflected in a 40% rise in antidepressant prescriptions across OECD countries between 2013 and 2023 (OECD, 2025). The societal burden is substantial, with mental health disorders estimated to cost more than 4.2% of GDP through direct treatment expenditures and indirect costs from reduced employment and productivity (OECD, 2021,0). While income (security) and mental health are strongly correlated (Ridley et al., 2020; Thomson et al., 2022), identifying the direction of causality remains challenging, especially at critical life events such as spousal death, when mental health risks increase (e.g., Fadlon et al., 2026; Siflinger, 2017) and simultaneously individuals' economic resources are altered (Fadlon and Nielsen, 2021). Understanding the causal determinants of mental health, especially at such at-risk stages of the life course, is therefore of importance for policymakers.

One key channel through which policymakers can influence income (security) is the social security system. A small strand of the literature shows that social insurance programs can affect mental health, with evidence from settings such as unemployment insurance expansions or changes in pension benefits (e.g., Ahammer and Packham, 2023; Golberstein, 2015). However, in many of these contexts, policy-induced income changes are intertwined with other factors –such as prolonged unemployment, transitions out of poverty or labor supply responses– making it difficult to isolate the causal effect of income itself on mental health. In particular, endogenous labor supply adjustments complicate the interpretation of income shock as labor force participation is correlated with mental health, which can confound estimates of the causal effect of income on mental health (Fadlon et al., 2026). We contribute to this literature by estimating the mental health effects of the reduced access to survivor benefits, providing a setting in which income changes can be studied largely independently of other confounding life events or behavioral changes.[1]

In this paper we study the effect of income on mental health –measured by mental health-related prescription drug use– by exploiting a reform that changed eligibility for survivor benefits in the Netherlands. The reform generated a sharp reduction in income for widows and widowers born in 1950 or later. Our analysis draws on comprehensive Dutch administrative data that provide precise information on the timing of spousal deaths, mental health-related prescription drug use, and a rich set of socioeconomic characteristics, including detailed measures of income, wealth, and labor supply. We estimate the impact of the survivor benefits reform on mental health using a difference-in-differences

[1] In Section 5, we show that labor supply responses to the survivor benefits reform are not relevant in our context.

design that exploits cohort-based variation in benefit eligibility induced by the reform, combined with time variation arising from the timing of spousal death. To establish a benchmark for interpreting the magnitude of the reform's impact, we first estimate the effect of spousal death on mental health using an event-study framework based on the estimator proposed by Callaway and Sant'Anna (2021).

Our results document a pronounced deterioration in mental health following spousal death. Mental health-related prescription drug use increases sharply after bereavement, peaking one year after death at 30% above the level observed four years prior, and remains elevated for up to three years. On average, prescription drug use is 24% higher over the three years following spousal death. We then show that the restriction of survivor benefits substantially amplifies these adverse mental health effects. Over the three years after spousal death, individuals affected by the reform experience a 9% increase in mental health-related prescription drug use relative to the year before death, corresponding to a 12% increase per €1,000 reduction in survivor benefits. Comparing this estimate to the overall effect of spousal death implies that the reduced access to survivor benefits accounts for 35% of the post-bereavement decline in mental health.

A detailed mechanisms analysis yields two main findings. First, the results indicate that it is not income levels per se that matter, but rather the loss in standard of living. The restricted access to survivor benefits following the death of the primary earner –predominantly men in the cohorts we study– leads to substantially larger income losses, sharper declines in living standards, and, ultimately, more severe mental health consequences for the surviving spouse than the death of a secondary earner. Second, the mental health response is entirely driven by women, reflecting their predominant role as secondary earners in these cohorts. Taken together, these findings highlight the central role of public survivor insurance in stabilizing living standards after bereavement, and, in turn, mitigating mental health declines, particularly among those individuals whose spouse was the primary earner. More broadly, our results suggest that policies affecting (household) income should aim to smooth living standards in order to avoid unintended adverse effects on mental health.

We contribute to four strands of the literature. First, we add to the literature on income and mental health. A large body of work documents a bidirectional relationship between income and mental health (Ridley et al., 2020; Thomson et al., 2022). To identify causal effects of income on mental health, previous studies have exploited exogenous income shocks, such as lottery winnings (e.g., Apouey and Clark, 2015), inheritances (e.g., Kim and Ruhm, 2012), or job displacement (e.g., Browning et al., 2006). While many of these studies find that higher income improves mental health, the estimates may be confounded by concurrent changes –such as job loss– that accompany income shocks. Other studies find no significant effect of exogenous income shocks on mental health (Adda et al., 2009; Frijters et al., 2005), suggesting that broader economic conditions may matter more than idiosyncratic

shocks (Rohde et al., 2016). We contribute to this literature by isolating the causal effect of income on mental health in a setting in which individuals face an identical life event –spousal death– while income varies exogenously and is largely independent from other contemporaneous changes.

Second, we contribute to the literature on social security and mental health. Several studies exploit exogenous variation in social insurance benefits to estimate causal effects on mental health. For example, Christian et al. (2019) examine a nationwide cash transfer program in Indonesia, focusing on suicide, while Chen et al. (2019) and Golberstein (2015) study expansions in social pensions in China and the U.S. Social Security notch, respectively, using survey data to capture mental health. Most closely related to our setting, Ahammer and Packham (2023) show that extending unemployment insurance reduces antidepressant consumption, largely because unemployed individuals are able to search longer and transition into higher-quality jobs. In contrast, our setting isolates the mental health effects of an income shock generated by social insurance inducing only a negligible labor supply effect, thereby identifying a cleaner income channel.

Third, we advance the literature on survivor benefits. Existing research has primarily focused on the effects of survivor benefits on income and labor supply, documenting reductions in labor force participation and substitution toward other social insurance programs (Coyne et al., 2024; Giupponi, 2019; Rabaté and Tréguier, 2024). Other dimensions have received little attention. One exception is the marriage market, where a few papers analyze how changes in survivor benefits rules –interpreted as implicit marriage subsidies or penalties– affect marital behavior (Baker et al., 2004; Brien et al., 2004; Persson, 2020). We extend this literature by studying a previously unexplored margin: the impact of survivor benefits on mental health.

Fourth, we contribute to the literature of the effects of spousal death on mental health. It is well established that mental health deteriorates following spousal death (see, e.g., Kristiansen et al., 2019). While this topic has been extensively studied in public health and psychology, much of the existing work is descriptive and does not account for age- and time-specific trends, limiting its ability to identify causal effects.[2] Within economics, most studies –including recent contributions such as Streeter (2020) and Siflinger (2017) – rely primarily on survey data.[3] In parallel work, Fadlon et al. (2026) analyze the dynamic effects of spousal death on mental health using prescription drug use, providing an objective measure of mental health. Similar to their approach, we trace mental health responses over time using administrative prescription data. We extend this line of research by identifying the underlying mechanisms by causally identifying the impact of social security income on mental health using an

[2]Einiö et al. (2020); Luoma and Pearson (2002); Stroebe et al. (2007); Stroebe and Stroebe (1987); Turvey et al. (1999); Zunzunegui et al. (2001).

[3]See also Clark et al. (2008); Frijters et al. (2011); Lindeboom et al. (2002) for earlier studies, and Espinosa and Evans (2008); Simeonova (2013); Tseng et al. (2018); Van den Berg et al. (2011) for related evidence on mortality effects.

exogenous policy-induced change in access to survivor benefits resulting from the 1996 reform.[4]

2 Institutional Setting

2.1 The survivor insurance in the Netherlands and its reform

Insurance against spousal death. Survivor insurance in the Netherlands is provided through three main sources, commonly known as the three pillars of pension systems. The first pillar is public insurance, which provides a basic pension. The amount and eligibility of this pension do not depend on the labour market history of either spouse. The second pillar is an occupational pension scheme that depends on the work history of the deceased spouse. The rules on coverage and replacement rates vary from one pension fund to another. Finally, the third pillar refers to the self-insurance of individuals through private contracts, such as life insurance. Our analyses focus on a change in the first pillar, the public survivor insurance. The other pillars were not significantly reformed during this period.

The public survivor benefits in the past. Before 1996, under the public survivor insurance scheme, a widow(er) was eligible for survivor benefits if she or he (1) was aged 40 years or over; or (2) had at least one child under the age of 18; or (3) suffered from a work disability. If the survivor was caring for a child under the age of 18, the survivor received 100% of the gross minimum wage, otherwise 70%.[5] No means test was required. During the period of eligibility, survivors received the benefits until they reached the state pension age (65 for those born before 1948 and progressively older thereafter). After this age, the state survivor's pension was replaced by the general old-age pension (*Algemene Ouderdomswet*, AOW), which we refer to here as the AOW age, corresponding to the statutory retirement age. Benefit payments are also stopped if the survivor remarries.

The public survivor benefits 1996 reform. The 1996 reform, known as the *Algemene nabestaandenwet* (ANW), amended the existing survivor benefits rules in several ways. In what follows, we describe only the changes that are relevant for this study; a discussion of additional reform components is provided in Appendix Section A. The 1996 reform substantially restricted eligibility for survivor benefits to disabled individuals and parents of young children. Rather than being implemented immediately, these changes were phased in through a transition period that exempted two groups: (1) individuals whose spouse died between July 1996 and July 1999, and (2) individuals born before 1 January 1950. Because we observe prescriptions outcomes for the full population only after 2006, we do not exploit the 1999 discontinuity in spousal death dates in the remainder of the paper. Instead, we

[4] We discuss Fadlon et al. (2026)'s findings and how we overcome empirical challenges they face in more detail in Section 5.

[5] As of 1 July 2023, the annual gross minimum wage in the Netherlands is equal to 23,213 euros (25,487 dollars).

focus on the discontinuity in eligibility based on date of birth introduced by the reform. In summary, our main source of variation is as follows: all widow(er)s born before 1950 remain eligible for survivor benefits, whereas individuals born in 1950 or later are eligible only if they are caring for a minor child or are disabled. Using the same identification strategy, Rabaté and Tréguier (2024) show that on average the reform reduced survivor benefits by €570 per month, induced increases in labor supply and take-up of other social security benefits, and ultimately led to a net decline in total income of €399 per month. Survivors benefits are paid until the earliest of three events: the youngest child reaches age 18, the survivor reaches statutory retirement age, or the survivor enters a new marriage, registered partnership, or cohabiting relationship.[6] Although several other social security reforms were implemented around the same time, Rabaté and Tréguier (2024) show that these changes do not confound the estimated effects of the ANW reform.[7] Moreover, there is no reason to expect other reforms differentially affect individuals before and after spousal death. Consistent with this, we show in Section 4 that the treatment and control groups defined by the survivor benefits reform exhibit parallel trends in mental health prior to spousal death.

2.2 Antidepressant use in the Netherlands

In the Netherlands, mental healthcare is delivered within a mandated and subsidised private insurance system in which general practitioners (GPs) serve as the primary point of contact. They assess mental health problems, prescribe medications such as antidepressants, and refer patients to specialised services when necessary. Psychopharmacological treatments, that is mental health medication, including antidepressants, are widespread: around 10% of the population receives such medications each year, reflecting both the broad accessibility of healthcare and the central role of GPs in managing mild to moderate mental health conditions.

In this paper, we primarily focus on a specific category of psychopharmacological treatments: antidepressants (ATC code N06A), as they are primarily prescribed for depression and anxiety, and their use closely reflects underlying psychological distress. Appendix Table B.1 shows that antidepressant dispensations are strongly correlated with survey-based measures of mental health, such as the Kessler-10 scale, and that this relationship remains stable before and after spousal death. Individuals reporting higher psychological distress are substantially more likely to use mental healthcare, and this correspondence between subjective distress and treatment is particularly strong in the Dutch context (Kolstad et al., 2024). While some degree of non-take-up inevitably persists, antidepressant dispensations constitute a reliable and consistent proxy for poor mental health.

Access to mental healthcare is broadly equalised across the income distribution (Kolstad et al., 2024),

[6] Beneficiaries are legally required to report any changes in their civil status.
[7] Appendix Section A.2 provides an overview of other relevant reforms.

and even individuals who default on health insurance premia continue to receive care (Roos et al., 2021). As a result, unmet medical needs among low-income households are exceptionally low: only 0.4%, compared with much higher rates in countries such as the United States (Danesh et al., 2024). Consistent with the institutional context, antidepressant consumption slightly decreases with household income –both before and after spousal death–, making prescription drug data a particularly suitable measure for studying population mental health responses to widowhood and survivor benefits (Appendix Figure B.1).

Finally, and crucially for the interpretation of our findings, our analysis focuses on relative changes rather than absolute levels of mental health. The main results measure changes in drug consumption relative to a pre-widowhood baseline, and the effect of the reform is itself expressed relative to the effect of widowhood.

3 Data and empirical strategy

3.1 Data

Data sources. We use administrative register data collected and maintained by Statistics Netherlands (CBS), which cover the entire resident population and provide detailed individual life histories along with retrospective information on household composition. Each record includes a unique personal identifier that enables us to merge datasets and retrieve the following information for each individual: civil and household status (available since 1995), drug consumption (since 2006), and income based on tax records (since 1999). The same information is available for their partners. In addition, we draw on survey data to assess the correlation between the objective measure of mental health—proxied by drug consumption—and self-assessed mental health. Appendix C describes in detail the datasets used and how they were combined to define the population of interest for our analyses.

The drug consumption data contain the name of every medication that requires a prescription in the Netherlands. They cover the universe of Dutch prescriptions, including both inpatient and outpatient treatments. We focus on antidepressants (ATC code N06A) as an objective measure of mental health status. As common in the literature (e.g., Ahammer and Packham, 2023; Fadlon et al., 2026), we assess mental health at the extensive margin: an individual is considered to have poor mental health if he or she consumes antidepressants.[8] Consequently, we can study individuals' responses to the survivor benefits reform at the extensive margin – that is, whether they switch from non-consumer to consumer status (or vice versa). Since the reform may have affected mental health both by inducing some individuals to start antidepressant treatment and by increasing the dosage among existing users,

[8] We do not observe changes in the intensive margin (dosage).

our estimates likely represent a lower bound of the true effect. We also analyze the overall group of psychoanaleptics (ATC code N06, including antidepressants), opioid medications (ATC codes N02A and N01AH) and non-opioid painkillers (ATC code N02B) as complementary indicators.[9]

Sample selection. In our main analyses, we investigate the effect of survivor benefits on mental health. To identify this effect, we exploit the discontinuity in eligibility to survivor benefits introduced by the reform. Then, to assess the magnitude of the effect, we further normalize it by the effect of widowhood itself on antidepressant consumption. In the analysis of the effects of widowhood on mental health, we focus exclusively on individuals affected by the reform for two reasons: (i) to ensure that all individuals share the same baseline eligibility status with respect to survivor benefits; and (ii) because, for this group, the ratio between the reform effect and the widowhood effect can be interpreted as the share of the total mental health decline following widowhood that is attributable to the reform. As a result, we define two analytical samples:

Sample 1 (used to analyze the effect of widowhood on mental health) consists of widows and widowers born after 1 January 1950. This restriction ensures that all individuals in the sample are subject to the same survivor benefits eligibility rules. Survivor benefits are paid until individuals reach the state pension age, the age at which they become eligible for first-pillar retirement pension benefits. Many individuals stop working upon reaching this age, which was 65 for cohorts up to 1948 and gradually increases thereafter. We therefore restrict the sample to widows and widowers younger than 65.

Sample 2 (used to analyze the effect of the survivor benefits reform on mental health) consists of widows and widowers born around the eligibility cutoff (1 January 1950). For these individuals, we construct a balanced panel of work and benefit histories covering the period from three years before to three years after the spouse's death. Because drug consumption data are available only from 2006 onward, we focus on widowhood events occurring after January 2009 to ensure that drug consumption is observed three years prior to the spouse's death for all individuals. As in Sample 1, we restrict the sample to individuals younger than 65, which here implies that they must have been under age 62 at the time of their spouse's death in order to observe three post years before retirement eligibility. As a result of these restrictions, the analytical sample includes only widows and widowers born in 1948 or later.

3.2 Descriptive statistics

Table 1 reports individual-level summary statistics for our outcome variables and selected socioeconomic characteristics, shown for the full Sample 2 as well as separately by treatment status and by gender. All means are measured in the year prior to spousal death in the respective samples.

[9]For reference on ATC codes, see here.

Table 1: Descriptive statistics

Variable	All	Treatment	Control	Men	Women
Total income	30522.746 (31286.703)	30696.345 (31310.230)	27582.667 (30741.914)	53979.758 (34021.220)	19934.643 (23190.357)
Age at death	57.916 (2.723)	57.752 (2.713)	60.688 (0.463)	57.901 (2.721)	57.923 (2.724)
Wealthy	0.499 (0.500)	0.495 (0.500)	0.572 (0.495)	0.500 (0.500)	0.499 (0.500)
Income decrease	0.484 (0.500)	0.494 (0.500)	0.333 (0.471)	0.259 (0.438)	0.591 (0.492)
Main Breadwinner	0.407 (0.491)	0.408 (0.491)	0.382 (0.486)	0.861 (0.346)	0.200 (0.400)
Partner total income	36345.633 (33484.864)	36380.452 (33443.090)	35755.925 (34184.685)	18345.130 (20891.226)	44470.759 (34901.002)
Sudden death	0.009 (0.097)	0.009 (0.097)	0.008 (0.091)	0.008 (0.090)	0.010 (0.099)
Female	0.689 (0.463)	0.689 (0.463)	0.690 (0.462)	0.000 (0.000)	1.000 (0.000)
Age	56.916 (2.723)	56.752 (2.713)	59.688 (0.463)	56.901 (2.721)	56.923 (2.724)
Birth year	1954.899 (3.040)	1955.269 (2.706)	1948.642 (0.480)	1954.874 (3.031)	1954.911 (3.044)
N06A	0.099 (0.298)	0.099 (0.299)	0.091 (0.287)	0.074 (0.261)	0.110 (0.313)
N06	0.100 (0.300)	0.101 (0.301)	0.091 (0.288)	0.075 (0.264)	0.111 (0.314)
N02B	0.029 (0.169)	0.029 (0.168)	0.030 (0.172)	0.019 (0.136)	0.034 (0.181)
Opioids	0.077 (0.267)	0.078 (0.268)	0.069 (0.254)	0.063 (0.244)	0.083 (0.276)
Observations	55871	52756	3115	17376	38495

NOTE: Summary statistics of certain characteristics and outcome variables of the full sample 2 (column 1), the treatment group (column 2), the control group (column 3), men (column 4) and women (column 5). The sample is based on our sample 2, including all widows and widowers born between 1948 and 1959. Displayed are means computed one year before spousal death. Standard errors are shown in parenthesis. Total income = individual earnings + social benefits, age at death = age at spousal death, wealthy = 1 if above age-specific median wealth (banking assets), income decrease = 1 if equivalized household size decreases after spousal death, main breadwinner = 1 if primary earner, partner total income = partner's earnings + social benefits, female = 1 if women, Age = age, Birth year = birth year, N06A = 1 if N06A prescription (antidepressant), N06 = 1 if N06 prescription (all psychoanaleptics), N02B = 1 if N02B prescription (non-opioid painkiller), Opioids = 1 if opioid prescription.
SOURCE: CBS, own calculations.

First, comparing treatment and control groups shows that –by construction– individuals in the control group are older than those in the treatment group. In several dimensions, the groups appear similar: in both samples, roughly two-thirds of respondents are women, and sudden deaths (accidents) represent only a very small share of cases (0.9%). In contrast, the groups differ in income and wealth: individuals in the control group are wealthier but have lower (partner) earnings. Additionally, the control group is less likely to experience an income decrease after spousal death and is less likely to be the main breadwinner. Drug consumption also differs, with slightly lower antidepressant use observed in the control group.

Second, comparing men and women shows that they are similar in age at the time of their spouse's death, but men have higher income –consistent with the predominance of the male breadwinner model in these cohorts– while women have higher antidepressant use. This pattern aligns with existing evidence that mental health problems are more prevalent, or at least more frequently diagnosed, among women (e.g., Yang et al., 2024).

3.3 Identification strategy

We study the effect of survivor benefits on antidepressant consumption. To assess the magnitude of this effect, we aim to quantify the share of the total decline in mental health following widowhood that can be attributed to the reform. To do so, we adopt a two-step strategy. In the first step, we estimate the change in antidepressant consumption following widowhood. In the second step, we identify the effect of survivor benefits on antidepressant consumption by exploiting the discontinuity in eligibility introduced by the 1996 reform.

Effect of spousal death on mental health. We use an event-study design to identify the causal effect of spousal death on survivors' antidepressant consumption. Since we do not observe individuals' drug consumption in the absence of the event, we use later-treated individuals as a control group to construct counterfactual trajectories. Due to non-eligibility for survivor benefits from age 65 (statutory retirement age), we restrict our analysis of the effects of survivor benefits to individuals losing their spouse before age 65. For comparability, to identify the causal effect of spousal death on antidepressant consumption, we exploit the exogenous timing of the event and compare outcomes between treated individuals and not-yet-treated individuals who will experience spousal death a few years later, while still under 65. Our identification strategy relies on the assumption that the timing of spousal death is not correlated with outcomes prior to the event. We assess the validity of this assumption by (i) formally testing for parallel pre-trends and (ii) conducting a falsification test in which the date of spousal death is randomly assigned to individuals. Our main specification is as follows:

$$Y_{it} = \alpha_i^d + \lambda_t^d + \sum_{k,k\neq -4} \beta_k^d \cdot \mathbb{1}(EventTime_{t=k}) + \sum_g \gamma_g^d \cdot \mathbb{1}(G_i = g) + \varepsilon_{it}^d, \tag{1}$$

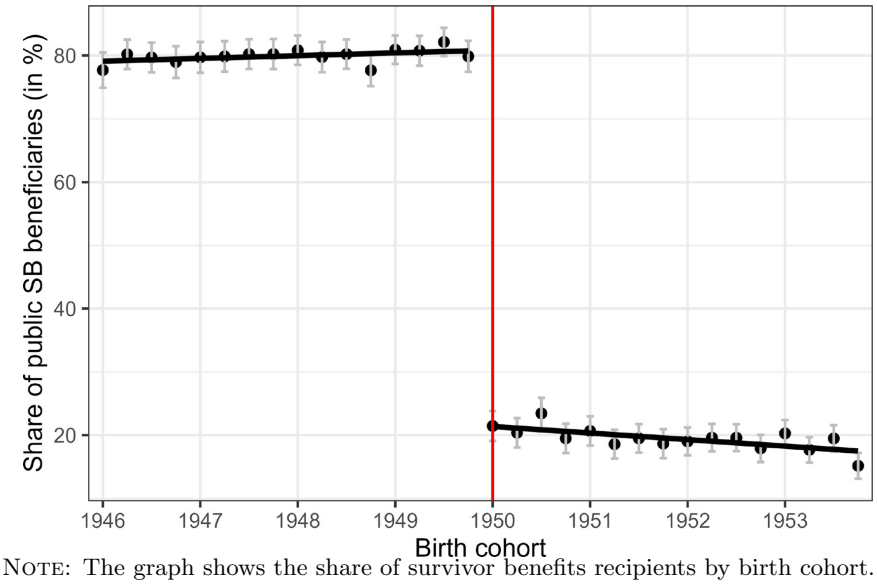
where Y_{it} denotes the outcome variable (dummy equal to 1 if individual i consumes antidepressants and 0 otherwise) for individual i at time t; α_i^d and λ_t^d are individual and time fixed effects, respectively. The indicator variable $\mathbb{1}(EventTime_{t=k})$ equals 1 if i is observed k periods before or after death and 0 otherwise. The variable $\mathbb{1}(G_i = g)$ is an indicator for membership in cohort g, defined as individuals whose spouse dies at time g; γ_g^d denotes the corresponding cohort fixed effect. The error term is represented by ε_{it}^d. The coefficients β_k^d are our parameters of interest. They measure the effect of spousal death on drug consumption at event time k, relative to the reference period four years before widowhood. We choose $k = -4$ as the reference period to allow for potential anticipation effects. The coefficients for $k = -3$ to $k = -1$ capture such anticipatory responses. In addition, we perform a heterogeneity analysis distinguishing between sudden and non-sudden deaths to verify that, by definition, no anticipation effects arise in the case of sudden deaths. As pointed out by recent studies (Callaway and Sant'Anna, 2021; De Chaisemartin and d'Haultfoeuille, 2020), estimating event studies with staggered treatment timing requires particular caution to avoid biased results due to forbidden comparisons or contamination from early-treated cohorts. To estimate overall effects and their dynamics, we thus use the estimator proposed by Callaway and Sant'Anna (2021).

Effect of survivor benefits on mental health. The 1996 reform introduced a discontinuity in eligibility for public survivor benefits. After 1996, survivors were eligible for public survivor benefits only if they were born before January 1, 1950, caring for a child, or disabled. We exploit this discontinuity in date of birth to identify the causal effect of the reduction in survivor benefits on antidepressant

consumption.

Our empirical strategy is a difference-in-differences design.[10] The treated group consists of individuals born in 1950 or later, while the control group includes those born before 1950. Figure 1 illustrates the rationale for this approach: it plots the proportion of survivor benefits recipients among widows by birth cohort, showing a pronounced drop for the 1950 cohort. Among widows born before 1950, 80% received public survivor benefits, whereas this proportion sharply declines to 21% among those born in the first quarter of 1950. Because of this sharp and exogenous decline in the probability of receiving public survivor benefits, we use the survivor's date of birth as the treatment variable to estimate the causal effect of the reform-induced reduction in survivor benefits on antidepressant consumption.

Figure 1: Survivor benefits recipients



NOTE: The graph shows the share of survivor benefits recipients by birth cohort. The share declined from 80% in the last non-treated cohort (widows born in the last quarter of 1949) to 21% in the first treated cohort (widows born in the first quarter of 1950). The sample is composed of widows born between 1946 and 1953 whose husband died after 2002, and who were aged below 62 at death time. Being a public SB beneficiary is a dummy that is equal to 1 if public survivor benefits are > 0 and 0 otherwise. The outcome is measured at the three-year mark following spousal death.
SOURCE: Rabaté and Tréguier (2024).

Our identification strategy relies on the parallel trends assumption: in the absence of the reform, widows and widowers born before and after 1950 would have followed similar trajectories of drug consumption. We assess the plausibility of this assumption by (i) providing descriptive evidence of parallel trends prior to widowhood, (ii) formally testing for pre-trend using the dynamic difference-in-differences specification, and (iii) conducting a placebo test using a false cutoff, comparing individuals born between 1950–1954 and those born between 1955–1959. Specifically, we implement the following specification:

[10] One might consider a regression discontinuity design; however, due to data limitations, the number of observations before the cutoff is too small to yield precise estimates.

$$Y_{it} = \alpha_i + \lambda_t + \sum_{k=-3,\, k\neq -1}^{3} \delta_k \big(\text{Treated}_i \times \text{EventTime}_{t=k}\big) + \varepsilon_{it}, \tag{2}$$

where Y_{it} is the the outcome variable (dummy equal to 1 if individual i consumes antidepressants and 0 otherwise) for individual i at time t; α_i and λ_t are individual and time fixed effects; $Treated_i$ is a dummy equal to 1 if individual i belongs to the treated group (born 1950 and after) and 0 otherwise; $EventTime_{t=k}$ is a dummy equal to 1 at period k, and 0 otherwise; ε_{it} is the error term. δ_k are our parameters of interest. They capture the dynamics of the survivor benefits reform's effect on average antidepressant use from three years before to three years after widowhood, relative to the reference period, one year before widowhood.

We also estimate the overall effect of the reform over the three years following spousal death in a pooled DiD specification:

$$Y_{it} = \alpha_i + \lambda_t + \tau \big(\text{Treated}_i \times \text{Post}_{it}\big) + \varepsilon_{it}, \tag{3}$$

where $Post_{it}$ is a dummy variable equal to 1 in the year of spousal death and thereafter, and 0 otherwise.

The reduced form displayed in Equation (3) assesses the causal impact of the survivor benefits reform on drug consumption. It tells us the extent to which Dutch widows and widowers born after 1950 have changed their drug consumption in response to the stricter eligibility for the scheme. To put the results into context, we use the same identification strategy in an IV-specification to quantify the change in antidepressant use induced by a €1,000 reduction in survivor benefits. More details on this approach are provided in Appendix D.

4 Main results

Our main analysis focuses on the effects of survivor benefits on mental health. To set the stage, and to establish a benchmark for interpreting the magnitude of the reform's impact, we begin by estimating the effect of spousal death on mental health.

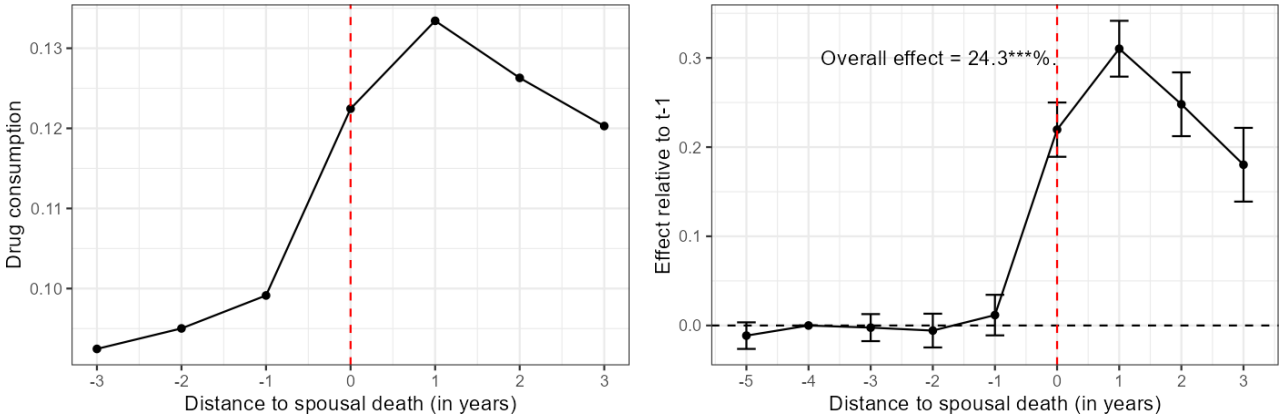
4.1 Spousal death and mental health

We begin with descriptive evidence. Figure 2, panel (a) shows average mental health medication use around the time of spousal death. Approximately 10% of individuals use mental health medication in the year preceding their spouse's death, and this share follows a smooth upward trend over time. Medication use increases sharply at the time of spousal death and remains elevated for at least three

years thereafter. However, these descriptive patterns do not account for age or time trends.

Figure 2: Effect of spousal death on antidepressant use

(a) Antidepressant consumption, raw mean (b) Antidepressant consumption, treatment effects



NOTE: Panel (a) displays the raw mean of drug use classified under ATC code N06A, relative to spousal death. Panel (b) displays the event study estimates based on Equation 1. The average share of individuals consuming antidepressants one year before spousal death is equal to 10%. The causal effect of spousal death on antidepressant consumption one year after spousal death is equal to a 24.3% increase. The sample is composed of widows and widowers born between 1950 and 1959, under age 64, who became widowed after 2009.
SOURCE: CBS, own calculations.

In Figure 2, panel (b), we plot the estimates from Equation 1 for each event time, along with the overall effect over the three years following spousal death. The results show that losing a spouse leads to a sharp increase in antidepressant use, peaking one year after death, when usage is about 30% higher than fours years prior to spousal death. By three years after death, antidepressant use has decreased but is still well-above pre-death levels (about 17% increase). On average, the increase over the first three years following spousal death corresponds to a 24% rise in use. Heterogeneity analyses show that women, individuals with lower income/wealth and individuals who lose their partner unexpectedly (to an accident) experience slightly larger increases in antidepressant use than men, higher-income/wealth individuals and individuals whose partner dies less sudden following the death of a partner (Figures B.2 and B.3).

As outlined in Section 3.3, the parallel trend assumption needs to hold to ensure that the approach in Equation 1 generates valid estimates. By examining the pre-trends in Figure 2, it can be observed that the coefficients at period $k = -5$ to $k = -1$ are close to zero and statistically insignificant. This provides evidence supporting the plausibility of the assumption that mental health for treated and control individuals would have evolved in parallel in absence of spousal death. As an additional plausibility check, we conduct a falsification test. To do so, we randomly assign spousal death events to individuals.[11] Figure B.4 shows that the line with randomly assigned spousal death is consistently flat and there are no noticeable changes around spousal death.

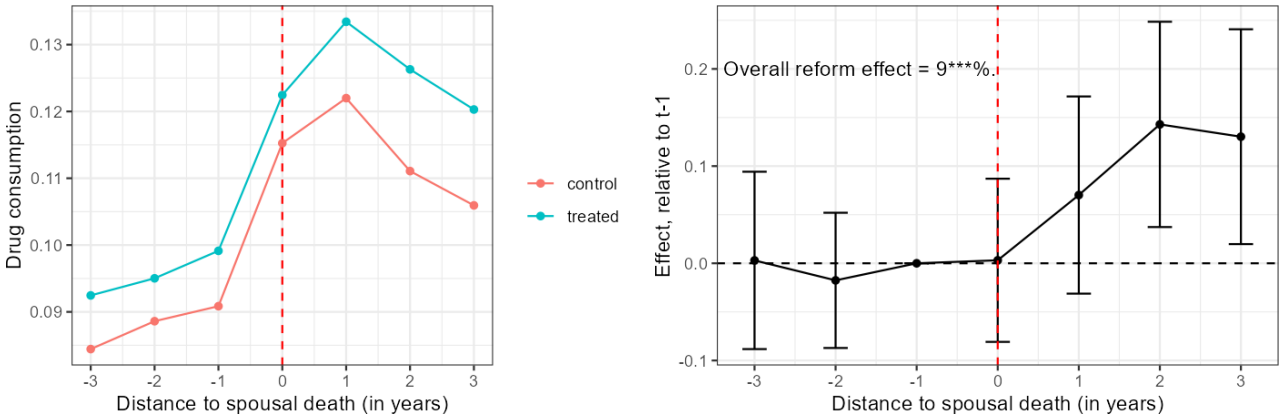
[11] Specifically, we draw the fake spousal death date from a uniform distribution spanning 2009 to 2023.

4.2 Survivor benefits and mental health

We begin with descriptive evidence of the effect of public survivor insurance on mental health. Figure 3, panel (a) shows average mental health medication use for individuals affected by the reform (treated) and those exempt from the new restrictive rules (control). The two groups exhibit parallel trends from three years prior to spousal death up to the time of death. After spousal death, medication use begins to diverge, with a larger increase among the treated group than among the control group. This divergence suggests that the reform amplified mental health medication use following spousal death.

Figure 3: Effect of survivor benefits on antidepressant use

(a) Antidepressant consumption, raw mean (b) Antidepressant consumption, treatment effects



NOTE: Panel (a) shows the raw means of drug use classified under ATC code N06A, relative to spousal death for treatment and control group. Panel (b) shows the event study estimates based on Equation 2. The treated group is composed of individuals born in 1948 or 1949, and the control group is composed of individuals born between 1950 and 1959. The average share of individuals using antidepressants one year before spousal death is 10% in the treated group and 9% in the control group. The causal effect of survivor benefits on antidepressant use one year after spousal death is a 7% increase. The sample is composed of widows and widowers born between 1948 and 1959 who became widowed after 2009 and were aged 61 or younger at the time of the spouse's death.
SOURCE: CBS, own calculations.

In Figure 3, panel (b), we present the DiD estimates from Equation 2. The x-axis shows time in years relative to spousal death, with one year prior serving as the reference period. We additionally report the pooled DiD estimate from Equation 3. The results show that widows exposed to the reform –those no longer eligible for survivor benefits– experience a statistically significant increase in antidepressant use. The pooled estimate corresponds to a 9% increase relative to the usage one year before spousal death. These findings indicate that the adverse mental health consequences of bereavement are amplified by the accompanying negative income shock.

For a complete picture, we investigate other common drugs. Specifically, Figure B.5 shows that the increase in general psychoanaleptics use is entirely driven by antidepressants since the effect size shown in panel (a) corresponds the effect size in Figure 3, panel (b). Additionally, we provide suggestive evidence for an increase in opioids usage (not statistically significant) while non-opioids painkiller consumption remains at pre-death levels.

As outlined in Section 3.3, our identification strategy is based on the assumption that, in the absence of the reform, spousal death would have had the same effect on the use of mental health medication of survivors born before and after 1950. We conduct three sets of checks to assess the plausibility of this assumption. The event-study estimates in Figure 3, panel (b) show that the coefficients for periods $k = -3$ and $k = -2$ are close to zero and statistically insignificant supporting the common trend assumption. We also implement a placebo test by shifting the cutoff cohort to 1955 and comparing individuals born between 1950–1954 to those born between 1955–1959. As shown in Figure B.6, the estimated effects around the placebo cutoff are flat, with no discernible break in trends. Finally, we carry out a robustness check on treatment group definition, to show that our result is not driven by the fact that we include more cohorts of birth in the treatment group than in the control group. Figure B.7 shows that even if less precise, the effect of survivor benefits on antidepressant consumption remains stable as we successively add cohorts of birth to our treatment group. Taken together, these findings provide strong support for the validity of our identification strategy.

4.3 Discussion

Our benchmark specification yields results that are qualitatively and, to a large extent quantitatively consistent with the main findings of Fadlon et al. (2026), despite differences in the setting. Similar to our results, they document a sharp increase in mental health medication use following spousal death, with effects peaking one year after the event and remaining significantly elevated for up to three years. In terms of magnitude, we estimate an increase in antidepressant use of about 3 percentage points –corresponding to a 30% rise relative to baseline– one year after death. In comparison, Fadlon et al. (2026) report a peak effect of 19.5 percentage points, equivalent to an increase of about 80% relative to baseline (see Figure 1, panel (D), in their paper).

Two main differences between the studies likely explain this discrepancy in effect sizes. First, we focus exclusively on antidepressants (ATC N06A), whereas Fadlon et al. (2026) also include psycholeptics (ATC N05). We exclude psycholeptics because they carry a higher risk of dependence and are often prescribed on a short-term or intermittent basis.[12] Second, we include all causes of spousal death, whereas Fadlon et al. (2026) restrict attention to sudden deaths, focusing on households in which one spouse experiences a heart attack or stroke and dies within one year. Consistent with their findings, Figure B.2 shows that sudden deaths trigger substantially larger mental health responses; specifically, we estimate that sudden death increases antidepressant use by approximately 70%.

We then turn to our main contribution: identifying the causal effect of social income on mental health. Exogenous variation in income is essential for precisely estimating this relationship, as spousal death

[12]Moreover, a 2009 reform in the Netherlands restricted health insurance reimbursement for psycholeptics (ATC N05), leading to a sharp decline in N05 prescriptions and introducing a clear structural break in these series.

itself induces behavioral responses that affect both income and mental health. In particular, surviving spouses often increase their labor supply following bereavement (Fadlon and Nielsen, 2021). This response has two important implications. First, observed changes in living standards reflect not only the initial income loss but also endogenous labor supply adjustments, complicating the interpretation of the income shock. Second, labor force participation is correlated with mental health medication use, which can confound estimates of the causal effect of income on mental health. The survivor benefits reform allows us to compare individuals who have all experienced spousal death, isolating differences in antidepressant use that arise solely from variation in survivor benefits receipt. As shown in Section 5, the labor supply response induced by the reform is minimal and, if anything, would only slightly bias our estimates downward.

We find that the reform led to a 9 percentage-point increase in antidepressant consumption over the three-year period following spousal death, corresponding to a 9% increase relative to baseline. Quantitatively, the removal of survivor benefits accounts for 35% of the post-bereavement decline in mental health.[13]

To contextualize the magnitude, we also scale the effect to a €1,000 reduction in survivor benefits. Using an IV approach, we first regress our running variable (date of death) on survivor benefits and then reinject the estimate into the reduced-form equation (2), as described in Appendix D. We find that a €1,000 drop in benefits corresponds to a 12% increase in antidepressant use (Appendix Table D.1).

5 Mechanisms

5.1 The effect is not driven by a labor supply response

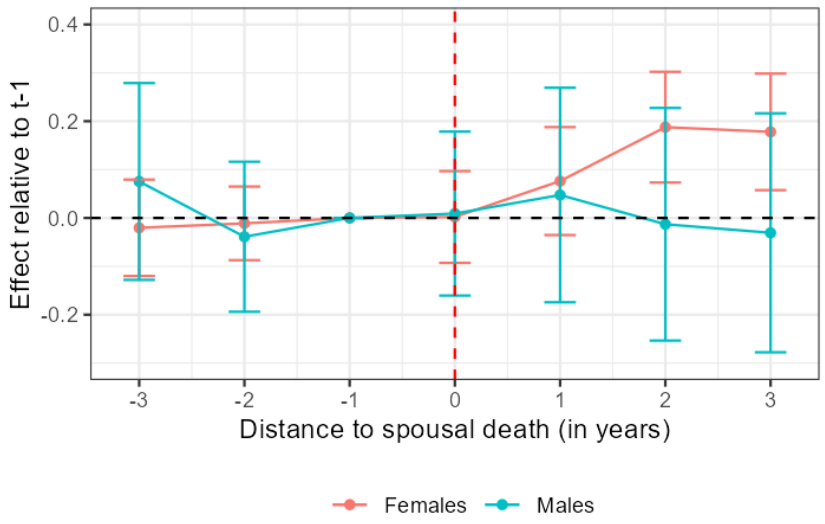
In this section, we examine the mechanisms underlying the negative mental health effects of restricting access to survivor benefits. We begin by disentangling whether these effects are driven by the income shock induced by the reform or by the labor supply response induced by this reform, as documented by Rabaté and Tréguier (2024). Rabaté and Tréguier (2024) show that the reform increased total labor income, partly through higher labor force participation. Since both income and labor supply may influence mental health, we decompose their respective contributions following Heckman et al. (2013). Our decomposition indicates that, if anything, labor supply has a protective effect on mental health. Assuming away the labor supply response would increase the estimated mental health impact of the reform by 4.6%. Thus, we can clearly attribute our findings to the income shock rather than to behavioral adjustments in labor supply. Further details on the decomposition analysis are provided in Appendix Section E.

[13]This share is calculated as the ratio of the second-stage estimate (0.085) to the first-stage estimate (0.243).

5.2 Income (in)security

Having established the income channel, we next investigate whether particular subgroups are differentially affected by the reform. Figure 4 reports separate estimates for men and women and shows that the increase in antidepressant use is entirely driven by women. The question that arises is whether these gender differences reflect a genuine gender-specific response or simply the fact that women in the studied cohorts typically had substantially lower personal income than their husbands. As a result, widows face a much larger relative financial loss when survivor benefits are eliminated, which could account for the observed gender gap in mental health responses. To disentangle whether the effect is driven by gender or by income, we conduct several heterogeneity analyses.

Figure 4: Effect of survivor benefits by sex



NOTE: Event study estimates based on Equation 2 separately for men and women. The causal effect of survivor benefits on antidepressant use three years after spousal death is a 19% increase for females and no increase for males. The sample is composed of widows and widowers born between 1948 and 1959 who became widowed after 2009 and were aged 61 or younger at the time of the spouse's death. SOURCE: CBS, own calculations.

We first examine heterogeneity by individual total income measured two years prior to spousal death. Individual income includes labor earnings as well as social security benefits, such as unemployment, disability, and welfare benefits. Low-income individuals are defined as those with income below the age-specific median. Table 2[14] shows that the effect of the reform is entirely driven by individuals in the low-income group (14% vs. no increase in antidepressant usage for the high-income group). We then assess whether household income, rather than individual income, explains this heterogeneity. When individuals are classified according to their position in the household income distribution two years prior to spousal death, taking both partners' incomes into account, we find no differential effects.

These results indicate that baseline income levels per se do not explain the observed heterogeneity.

[14]Event study estimates for the subgroup analyses are shown in Appendix Figure B.8

Table 2: Overall effect of public survivor benefits by subgroups

	Coefficient	SE	p-value	Observations
Full sample	0.0085	(0.0040)	0.033	391,097
Gender				
Females	0.0124	(0.0050)	0.013	269,465
Males	0.0001	(0.0065)	0.983	121,632
Individual income				
high income	0.0035	(0.0054)	0.521	198,268
low income	0.0135	(0.0058)	0.020	192,829
Household income				
high household income	0.0087	(0.0052)	0.092	229,215
low household income	0.0101	(0.0062)	0.104	161,882
Partner income				
high partner income	0.0026	(0.0058)	0.653	197,533
low partner income	0.0137	(0.0055)	0.013	193,564
Breadwinner status				
secondary earner	0.0126	(0.0054)	0.021	223,972
main breadwinner	0.0035	(0.0060)	0.562	153,482
Standard of living variation				
decrease std	0.0214	(0.0072)	0.003	174,104
increase std	0.0026	(0.0051)	0.606	185,332
Wealth (banking assets)				
high banking assets	0.0018	(0.0053)	0.733	177,842
low banking assets	0.0140	(0.0070)	0.044	178,402

NOTE: Pooled DiD estimates based on Equation 3. The causal effect of survivor benefits on antidepressant use over the three year period following spousal death is a 12% increase for females and no increase for males. Individual income is classified as low if earnings plus social benefits two years before the spouse's death fall below the age-specific median, and as high otherwise. Household income is classified as low if total household income two years before the spouse's death falls below the age-specific median, and as high otherwise. Breadwinner status is defined as main breadwinner if the individual contributed at least 50% to the total household income two years before spousal death and secondary earner otherwise. Standard of living variation is measured in equivalized household income, comparing two years before the event to one year after. Partner earnings is classified as low if earnings two years before the death fall below the age-specific median, and as high otherwise. Wealth is classified as low if banking assets two years before the death fall below the age-specific median, and as high otherwise. The sample is composed of widows and widowers born between 1948 and 1959 who became widowed after 2009 and were aged 61 or younger at the time of the spouse's death.
SOURCE: CBS, own calculations.

Instead, a key determinant of post-bereavement mental health responses is likely to be the individual's degree of income insurance. Following spousal death, resources are no longer pooled, and the mechanical income change depends on the survivor's contribution to total household income prior to death. Primary earners retain their own income, while secondary earners lose access to their partner's resources. We therefore expect stronger effects among secondary earners. Table 2 confirms this prediction: the effect of the reform is entirely driven by individuals who were secondary earners prior to spousal death (13% vs. no increase).

Income security may also depend on other adjustment margins. Survivors may increase labor supply or

rely on social transfers to smooth living standards. In addition, economies of scale within households imply that a surviving spouse requires more than half of pre-death household income to maintain a comparable standard of living. Using the OECD-modified equivalence scale, a surviving spouse would need approximately 66 percent of the income of a two-person household. Table 2 therefore distinguishes individuals who experience a decline in their standard of living from those whose standard of living remains stable or increases. Standard of living is measured as equivalenced household income, comparing two years before the event to one year after. The results are unambiguous: the effect of the reform is entirely driven by individuals experiencing a decline in living standards (21% vs. no increase).

These findings highlight the central role of survivor benefits in smoothing income around spousal death. In the Netherlands, survivors may also receive private survivor benefits through occupational pension plans (see Section 2.1). While eligibility rules vary across employers, both access to and generosity of these benefits are closely tied to the deceased partner's earnings history. Accordingly, Panel (e) distinguishes individuals based on whether their deceased partner's labor earnings was above or below the median, serving as a proxy for high versus low (or zero) private survivor benefits. We find larger effects of the reform among individuals whose partner had lower pre-death income.

Finally, we examine heterogeneity along a last margin of self-insurance: dissaving. We focus on banking assets, which are highly liquid and therefore well suited to smoothing consumption following negative income shocks. Table 2 shows that the effect of the reform is entirely driven by individuals with low levels of banking assets, who are less able to rely on wealth to maintain their standard of living.

Taken together, these results show that public survivor insurance plays an important role in mitigating post-bereavement declines in mental health by limiting reductions in living standards. They further suggest that survivor benefits should primarily target individuals who are most exposed to losses in living standards –namely, those whose deceased partner was the primary earner– and that benefit generosity should account for both the deceased partner's prior income and the survivor's own resources. Our *causal* evidence on the effects of social security income supports correlations documented in previous studies (e.g., Fadlon et al., 2026) on the optimal design of social income policies. More broadly, our findings underscore the importance of accounting for unintended mental health consequences when designing changes to social income programs aimed at supporting vulnerable populations.

6 Conclusion

This paper examines the causal effect of social income on mental health, leveraging a 1996 reform in the Netherlands that restricted access to survivor benefits for certain cohorts. Using detailed administrative data on spousal deaths and mental health-related prescription drug use, we show that bereavement leads to a pronounced deterioration in mental health, with prescription drug use peaking one year after spousal death and remaining elevated for several years. By restricting access to survivor benefits, the reform significantly amplified these adverse effects: over the three years following spousal death, affected individuals show a 9% increase in antidepressant use, accounting for 35% of the overall rise in antidepressant use after spousal death. Mechanism analysis indicates that these effects are driven by the loss in standard of living, with the largest impacts on women, who are predominantly secondary earners in the cohorts studied.

These results carry important implications for policy design. Public survivor benefits serve not only to stabilize household income but also to protect mental health during periods of economic vulnerability. Policies should therefore prioritize those most exposed to income losses and ensure adequate protection for economically vulnerable populations. More generally, our findings highlight that understanding the mental health consequences of social income changes is essential for designing policies that effectively support both financial security and well-being, helping policymakers leverage social income as a tool to mitigate the broader societal burden of mental health problems.

Appendices

A The public survivor benefits 1996 reform

A.1 Additional reform components beyond restricted access to survivor benefits

In addition to the changes in eligibility, the reform also changed the amount of the benefits. From 1996, the survivor receives a flat-rate benefit of 70% of the gross minimum wage, plus an additional 20% of the gross minimum wage if she or he is caring for a minor child (half-orphan benefit). A means test was also introduced. Any income received by the survivor is partially or fully deducted from the survivor benefits, depending on the nature of the income and the type of benefit. The means test does not apply to the half-orphan's benefit, but only to the flat-rate benefit of 70% of the minimum wage. Unemployment, sickness and disability benefits are fully deducted from public survivor benefits. Survivor benefits from other schemes (second and third pillar) are not deducted. Importantly, labour income is also partially deducted from public survivor benefits. For any labour income above 50% of full-time work at the minimum wage, benefits are deducted at a rate of 66%. As a result, individuals with incomes above 155% of the gross minimum wage do not receive any survivor benefits. This part of the reform, which affected all widows from 1996, is not covered in this paper.

A.2 Other reforms

After the 1996 ANW reform, three additional reforms were implemented: the 2006 second-pillar pension reform, the 2012 statutory retirement age reform, and the 2015 partner pension reform. We acknowledge that these reforms could, in principle, interact with our reform of interest for two reasons: (1) they affect the same cohorts, and (2) they may influence survivors' antidepressant consumption. However, these reforms primarily altered individuals' labour supply, and Rabaté and Tréguier (2024), whose research design we follow, provide clear evidence that these reforms do not confound the effects on labour supply and social insurance income. More importantly, there is no clear reason why these confounding reforms should affect individuals differently before and after the spouse's death. As shown in Section 4, we find no evidence of pre-trends prior to widowhood, which supports the view that interactions with these reforms are unlikely to bias our results.

The occupational pension reform. The first confounding reform relates to the 2006 reform of the occupational pension system, which substantially reduced early retirement benefits for people born on or after 1 January 1950. This then translated into a large negative shock to individual pension wealth, which could have implications for antidepressant consumptions.

The statutory retirement age reform. The second reform gradually increased the statutory retirement age from 65 to 67 between 2013 and 2023. In particular, it increased from 65 and 2 months to 65 and 3 months for those born after November 1949, which is close to the 1950 cut-off. As the first pillar of the survivor benefits is paid up to the statutory retirement age, this could interact with our reform of interest.

The partner pension reform. The third confounding factor is the 2015 reform of the partner pension, which abolished the pension supplement for the older partner who has reached AOW age, provided that the younger partner's income is below a certain threshold. This supplement was abolished for older partners born after December 1949. It should be noted that this reform had only a limited impact on the older spouse for whom the 1950 discontinuity applies.

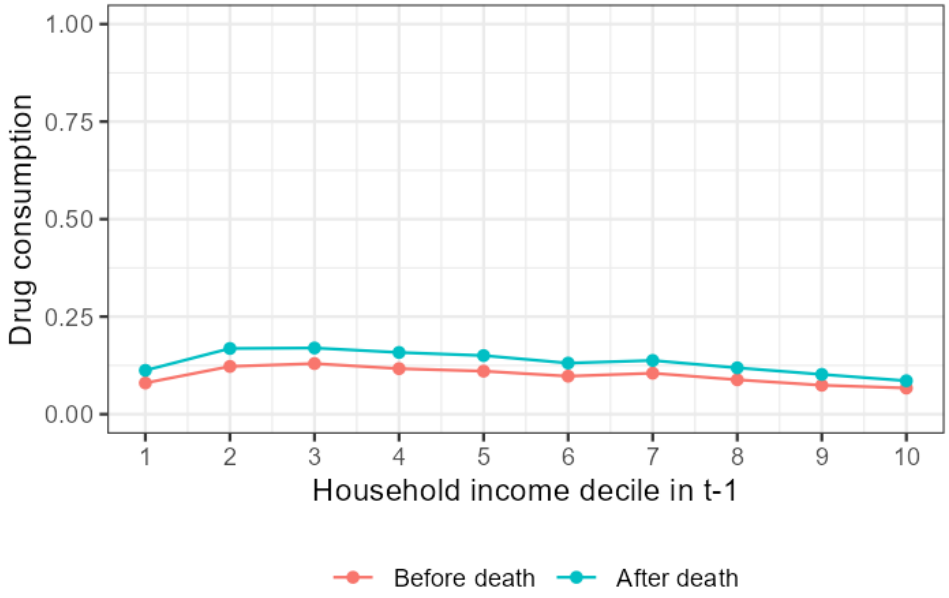
B Additional results

Table B.1: Antidepressant use *vs.* risk of anxiety or depression

	Full sample	Before death	After death
Correlation coeff	0.320***	0.293***	0.340***
	(0.017)	(0.027)	(0.022)
Nb of observations	5,885	2,672	3,213

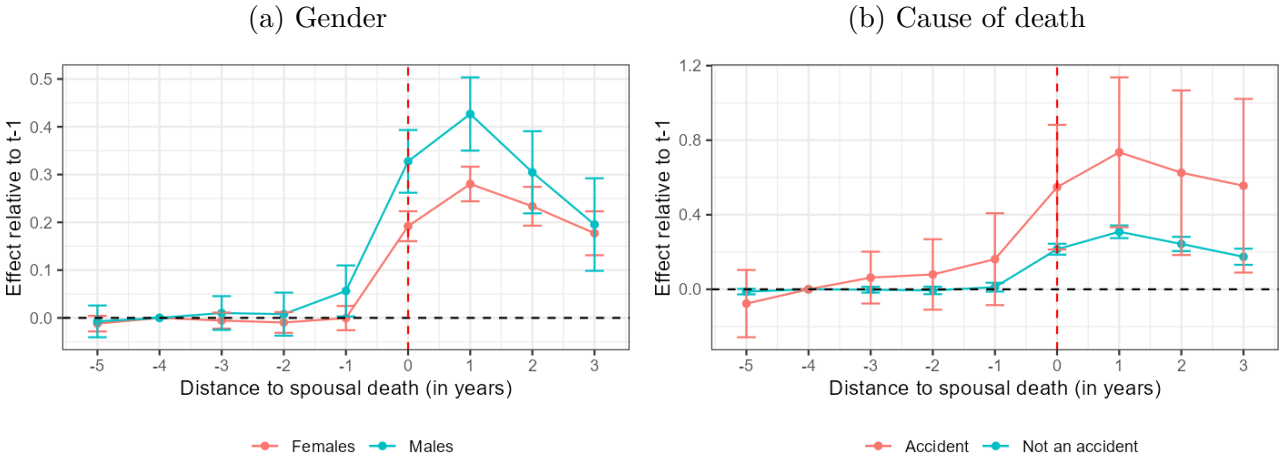
NOTE: The table shows correlations between drug use classified under ATC code N06A and Kessler-10 score for the full sample (column 1), before spousal death (column 2) and after spousal death (column 3). The correlation between antidepressant use and the Kessler-10 score is a significant 32% in the full sample. The antidepressant use variable is tested against a variable which states whether an individual has a high risk of anxiety or depression. This indicator is based on the Kessler-10 (K10) questionnaire, a standard screening tool for psychological distress. Respondents rate the frequency of ten symptoms (e.g., nervousness, hopelessness, fatigue, restlessness, sadness) experienced over the past four weeks on a 5-point scale from always to never. Scores range from 10 to 50, with higher values indicating greater distress. Individuals scoring 30 or above are classified as being at high risk of anxiety or depression. The sample is composed of widows and widowers born between 1950 and 1959, under age 64, who became widowed after 2009.
SOURCE: CBS and GGDs.

Figure B.1: Antidepressant use across household income deciles



NOTE: The average share of individuals using antidepressants one year before spousal death is 10% in the fifth income decile and 7% in the tenth income decile. Income deciles are based on total household income and are computed separately by gender and age. The sample is composed of widows and widowers born between 1950 and 1959, under age 64, who became widowed after 2009.
SOURCE: CBS, own calculations.

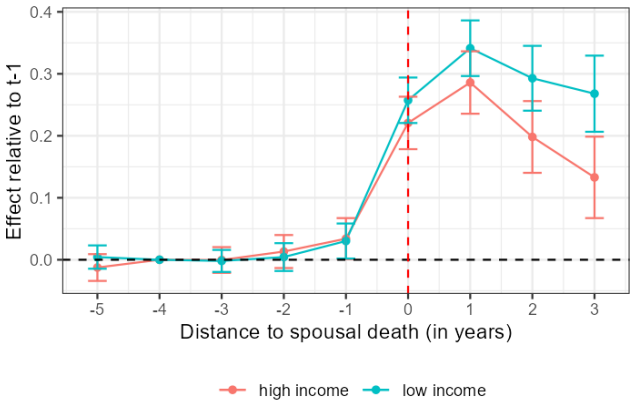
Figure B.2: Effect of spousal death on antidepressant use by subgroups (1/2)



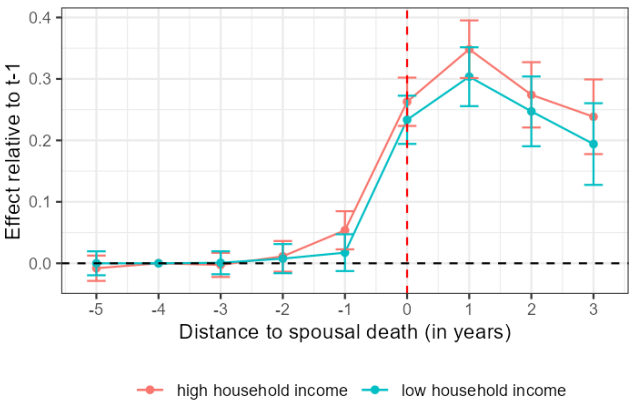
NOTE: Event study estimates based on Equation 1 by subgroups. The treated group is composed of individuals born in 1948 or 1949, and the control group is composed of individuals born between 1950 and 1959. The sample is composed of widows and widowers born between 1950 and 1959, under age 64, who became widowed after 2009.
SOURCE: CBS, own calculations.

Figure B.3: Effect of spousal death on antidepressant use by subgroups (2/2)

(a) Individual income

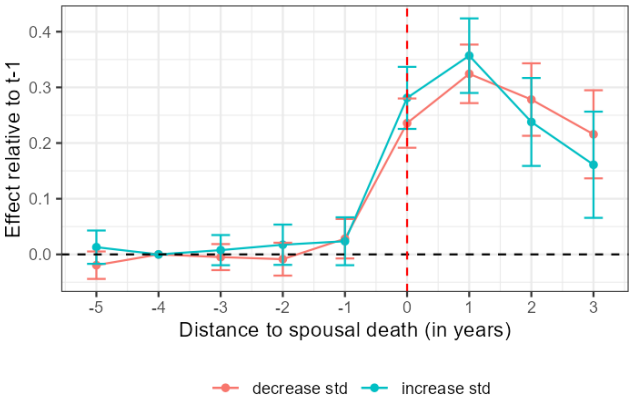


(b) Household income

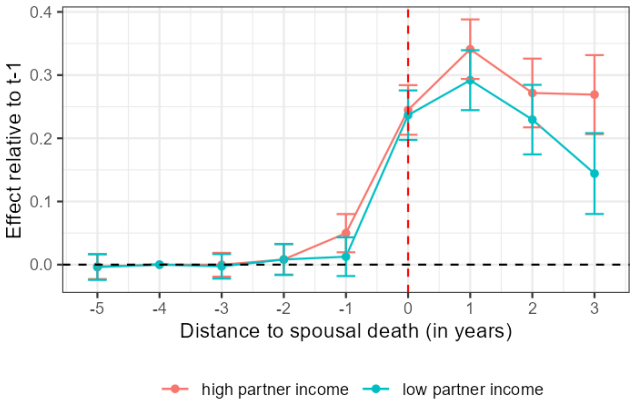


(c) Breadwinner status

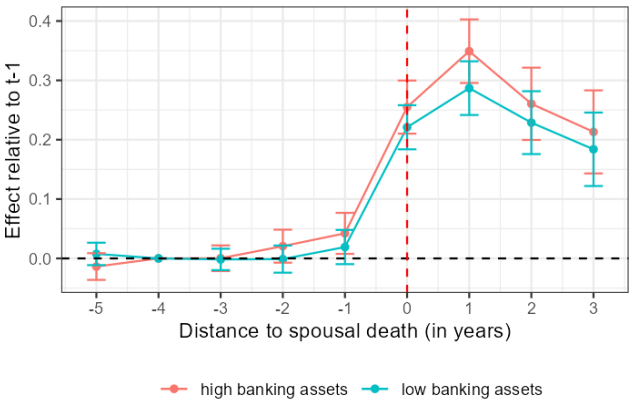
(d) Standard of living variation



(e) Partner earnings

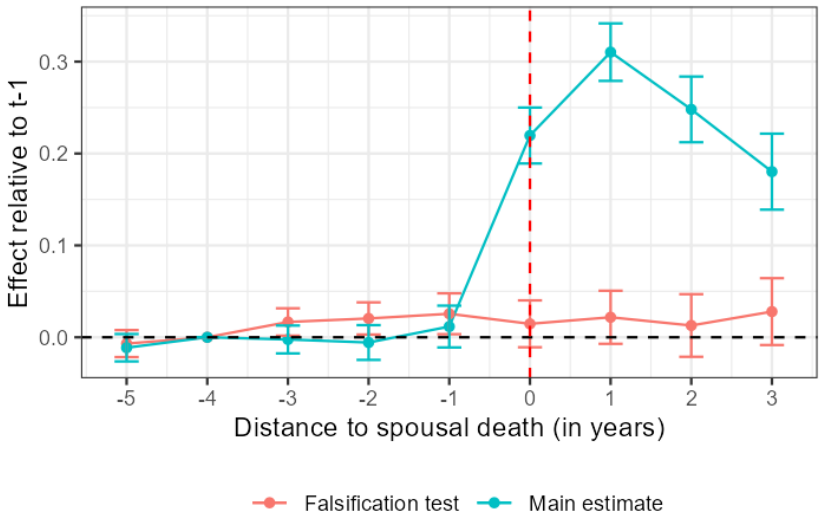


(f) Wealth (banking assets)



NOTE: Event study estimates based on Equation 1 by subgroups. Individual income is classified as low if earnings plus social benefits two years before the spouse's death fall below the age-specific median, and as high otherwise. Household income is classified as low if total household income two years before the spouse's death falls below the age-specific median, and as high otherwise. Breadwinner status is defined as main breadwinner if the individual contributed at least 50% to the total household income two years before spousal death and secondary earner otherwise. Standard of living variation is measured in equivalized household income, comparing two years before the event to one year after. Partner earnings is classified as low if earnings two years before the death fall below the age-specific median, and as high otherwise. Wealth is classified as low if banking assests two years before the death fall below the age-specific median, and as high otherwise. The sample is composed of widows and widowers born between 1950 and 1959, under age 64, who became widowed after 2009.
SOURCE: CBS, own calculations.

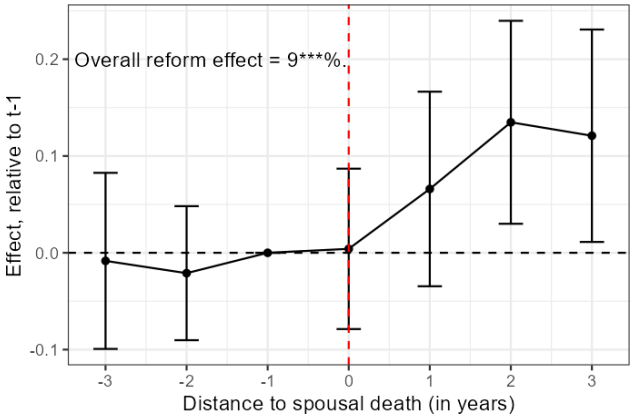
Figure B.4: Falsification test



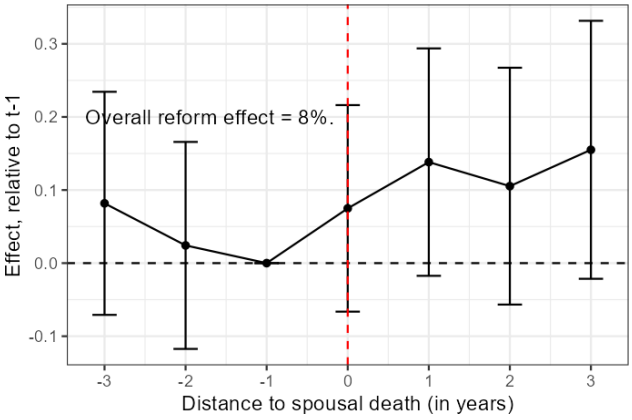
NOTE: Event study estimates based on Equation 1. The fake date of death is randomly assigned from among all dates of death observed in the sample. The sample is composed of widows and widowers born between 1950 and 1959, under age 64, who became widowed after 2009.
SOURCE: CBS, own calculations.

Figure B.5: Effect of survivor benefits on other outcomes

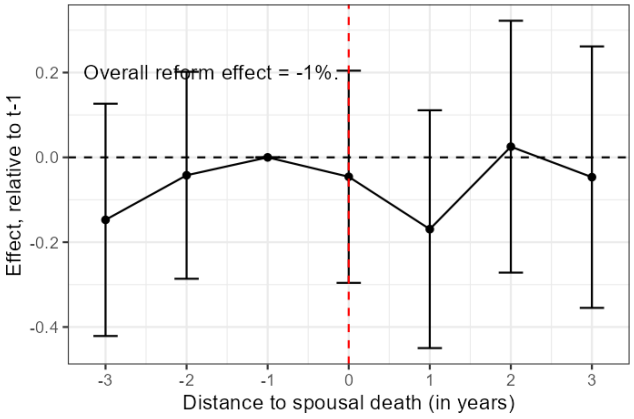
(a) All psychoanaleptics



(b) Opioids

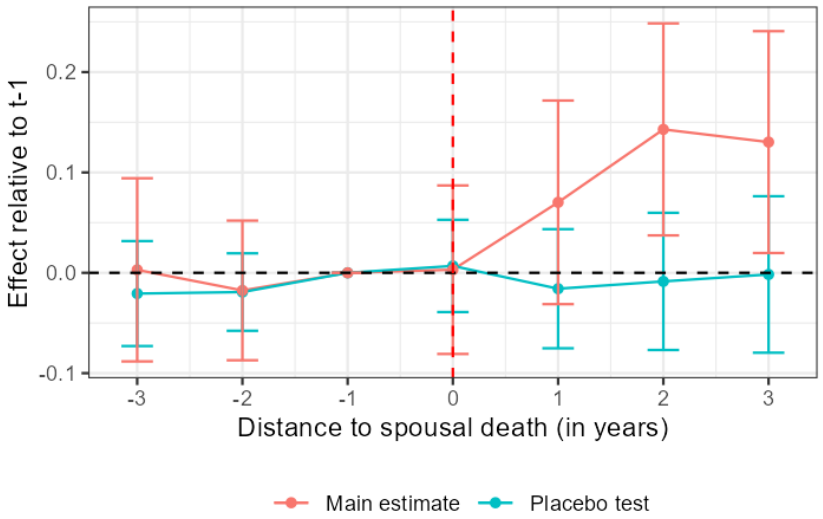


(c) Non-opioid painkillers



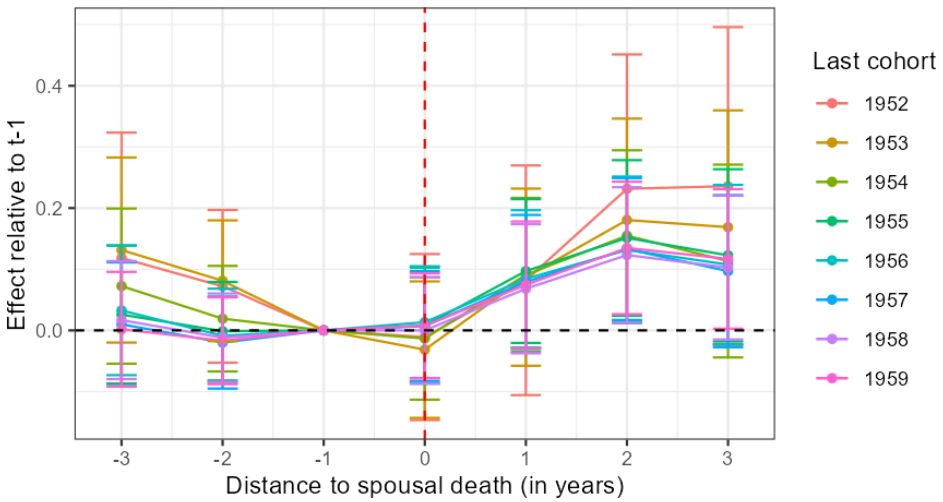
Note: Event study estimates based on Equation 2. All psychoanaleptics correspond to drugs with ATC code N06; opioids correspond to drugs with ATC codes N02A and N01AH; and non-opioid painkillers correspond to drugs with ATC code N02B. The sample is composed of widows and widowers born between 1948 and 1959 who became widowed after 2009 and were aged 61 or younger at the time of the spouse's death.
Source: CBS, own calculations.

Figure B.6: Placebo test



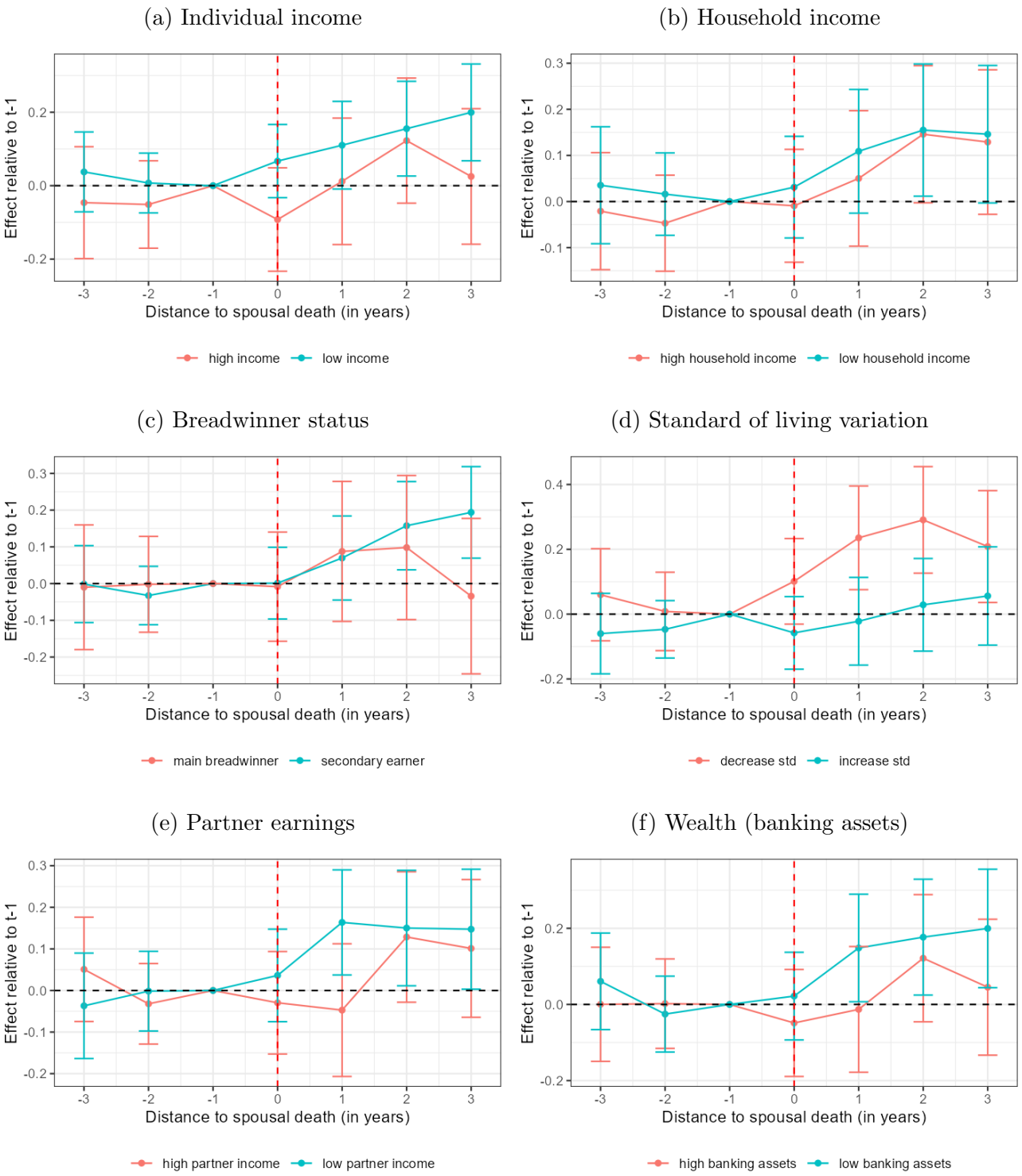
NOTE: Event study estimates based on Equation 2. The cutoff cohort is shifted to 1955 and comparing individuals born between 1950–1954 to those born between 1955–1959. The sample is composed of widows and widowers born between 1948 and 1959 who became widowed after 2009 and were aged 61 or younger at the time of the spouse's death.
SOURCE: CBS, own calculations.

Figure B.7: Robustness to last treated cohort



NOTE: Event study estimates based on Equation 2 adding cohorts to the treatment group one by one. The sample is composed of widows and widowers born between 1948 and 1959 who became widowed after 2009 and were aged 61 or younger at the time of the spouse's death.
SOURCE: CBS, own calculations.

Figure B.8: Effect of public survivor benefits on antidepressant use by subgroups



NOTE: Event study estimates based on Equation 2 by subgroups. Individual income is classified as low if earnings plus social benefits two years before the spouse's death fall below the age-specific median, and as high otherwise. Household income is classified as low if total household income two years before the spouse's death falls below the age-specific median, and as high otherwise. Breadwinner status is defined as main breadwinner if the individual contributed at least 50% to the total household income two years before spousal death and secondary earner otherwise. Standard of living variation is measured in equivalized household income, comparing two years before the event to one year after. Partner earnings is classified as low if earnings two years before the death fall below the age-specific median, and as high otherwise. Wealth is classified as low if banking assests two years before the death fall below the age-specific median, and as high otherwise. The sample is composed of widows and widowers born between 1948 and 1959 who became widowed after 2009 and were aged 61 or younger at the time of the spouse's death.
SOURCE: CBS, own calculations.

C Data

The data used in this study come from individual- and household-level records provided by Statistics Netherlands (CBS) and are accessible through a remote access environment across multiple datasets. Each dataset assigns a unique pseudonymized identifier to individuals, allowing us to link information across datasets. Civil-status histories cover past and present partnerships, marriages, separations, divorces, and widowhood for each resident. For each event, the data include the exact date of occurrence, enabling us to identify widowhood events precisely. We also use individual-level data on drug consumption, available from 2006, which include the ATC codes of medications consumed by each individual in a given year. Using household data, we linked individuals to their spouses to obtain additional information about them. Information on cause of death is available from 1995. Income data, available from 1999, provide monthly details on both the type and amount of income. Finally, we use the Health Monitor Survey (waves 2016 and 2020) to compare our objective measure of mental health—antidepressant consumption—with subjective mental health, as reported in the survey using the Kessler-10 scale. Table C.1 lists all the sources we use, and we subsequently provide more detailed information about each dataset. Information about the administrative on the micro datasets can be found on the CBS website (only available in Dutch).[15]

gbapersoontab[16]

This dataset contains demographic background data, such as gender, year of birth, and migration history, for the entire Dutch population who have been registered in the Basic Register of Persons (BRP) since 1 October 1994.

gbaoverlijdentab[17]

This dataset includes the date of death of all persons who were registered in the Basic Register of Persons (BRP) at the time of death since 1 October 1994. It also includes the date of death of non-residents who were once residents of the Netherlands after 1 October 1994, and whose death information is available in the Register of Non-Residents (RNI). The dataset is primarily sourced from the municipal registries (Gemeentelijke Basisadministratie Persoonsgegevens, GBA). We primarily use this dataset to ensure that our sample includes only individuals who are alive at the time of observation.

[15]Link to the CBS microdata information website.
[16]Link to gbapersoontab documentation in Dutch.
[17]Link to gbaoverlijdentab documentation in Dutch.

gbaburgerlijkestaatbus[18]

This dataset provides civil-status information for all individuals who have been registered in the Basisregistratie Personen (BRP) (or its predecessor) since 1 October 1994. For each person, it records changes in marital or partnership status — such as single, married, registered partnership, divorced (after marriage or partnership), and widowed (after marriage or partnership) — along with start and end dates for each status spell. Because the file uses a "BUS" structure, individuals may have multiple rows over time (one per status spell), which allows reconstructing a full civil-status history for each person. In practice, this dataset is used to assign a civil-status attribute to individuals from other datasets (e.g. demographic or household data) by linking on their unique identifier.

medicijntab[19]

This dataset records the use of prescription drugs covered by the basic (mandatory) health insurance. Each observation corresponds to the use of one type/class of prescription drug by one individual in the calendar year. The data are available from 2006 onward. In the original data, one type of drug is defined as a 4-position code of the ATC (anatomical therapeutic chemical) classification. The data can be aggregated at a higher level (e.g. 2nd level = anatomic subgroup, 3 position code).
For each category of drug we only know whether there was any use in the year, not its intensity . If a drug becomes reimbursed (or is removed from the list of reimbursed drugs) from one year to the other, its use will show in the data of one year but not in the data from the other year. By use we mean pick up of a prescription drug (the drug was prescribed by a physician and then delivered by a health care professional, but we do not know for sure whether the individual has taken the pills).

gbahuishoudensbus[20]

For the full universe of the Dutch population (as defined in the gbapersoontab), it contains information about the household composition: their place in the household, and the details of the household they belong to (e.g couple or not, married or not, with or without children, etc.). Retrospective information is available, as the data are presented as spells (one additional line when one characteristic of the household changes). The main source of information for this dataset is the municipal registries (Gemeentelijke Basisadministratie Persoonsgegevens, GBA).

[18]Link to gbaburgerlijkestaatbus documentation in Dutch.
[19]Link to medicijntab documentation in Dutch.
[20]Link to gbahuishoudensbus documentation in Dutch.

doodoorztab[21]

This dataset records the underlying cause(s) of death for all persons who at the time of death were registered in the basic population register (BRP) in the Netherlands. For each death, the dataset includes the coded cause of death (according to the international classification used by World Health Organization, WHO), as well as the year of death and the location of death. In cases of non-natural death (e.g. accidents), the dataset additionally records the main injury and the location of the incident.

secm datasets[22]

The secm datasets provide detailed information on various types of monthly income received since 1999: employment wage (SECMWERKNDGAMNBEDRABUS), profit (SECMZLFMNDBEDRAGBUS), other activities (SECMOVACTMNDBEDRAGBUS), unemployment benefits (SECMWERKLMNDBEDRAGBUS), disability benefits (SECMZIEKTAOMNDBEDRAGBUS), other benefits (SECMSOCVOORZOVMNDBEDRAGBUS) welfare benefits (SECMBIJSTMNDBEDRAGBUS) and pension income (SECMPENSIOENMNDBEDRAGBUS).

These datasets are compiled by Statistic Netherlands using various administrative data sources, including taxes, social security, and pension funds. The initial format of the dataset is spell data, which provides information on the start and end dates of each income source, as well as the associated monthly amount. For each change in an individual's monthly income, a new line is added to the dataset. The secmbus dataset consolidates the different sources into a single dataset that contains the main source of income and the associated amount for each spell.

gemon[23]

The GEMON dataset is a large-scale survey of the Dutch adult population (aged 19 and over in 2016) that provides data on health, lifestyle, and social circumstances. It pools information from the national health survey conducted by Centraal Bureau voor de Statistiek (CBS) and health surveys run by all regional public health services (GGD) in the Netherlands in collaboration with RIVM, resulting in a harmonized, weighted dataset representative of the adult population. The dataset covers a wide set of domains including self-assessed health, chronic conditions, functional limitations, height & weight, physical activity, and (for many respondents) psychosocial factors such as mental health (e.g. anxiety and depression), loneliness, social support, smoking, alcohol use, and other lifestyle and social-environment variables.

[21]Link to doodoorztab documentation in Dutch.
[22]Link to secm documentation in Dutch.
[23]Link to gemon documentation in Dutch.

Table C.1: Datasets used

Content	**Name of dataset**	**Years**
Date of birth and gender	GBAPERSOONTAB	1995-2024
Death	GBAOVERLIJDENTABTAB	1995-2024
Civil status and date	GBABURGERLIJKESTAATBUS	1995-2024
Drug consumption	MEDICIJNTAB	2006-2024
Households characteristics	GBAHUISHOUDENSBUS	1995-2024
Cause of death	DO & DOODOORZTAB	1995-2024
Individual income	SECMxxxBUS	1999-2024
Health Monitor Survey	GEMON	2016; 2020

Source: CBS microdata catalogue.

D Fuzzy design specification

Because the probability of treatment changes discontinuously at the cutoff—but not from 0 to 1—we also estimate a fuzzy difference-in-differences (fuzzy-DID) model. Specifically, we implement a two-stage least squares specification as follows:

For the first stage, we estimate the following equation:

$$SB_{it} = \alpha_i^f + \lambda_t^f + \sum_{k=-3,\, k\neq -1}^{3} \delta_k^f \big(\text{Treated}_i \times \text{EventTime}_{t=k}\big) + \varepsilon_{it}^f, \tag{D.1}$$

where SB_{it} is individual i's survivor benefits t years after spousal death; α_i^f and λ_t^f are individual and time fixed effects; $Treated_i$ is a dummy equal to 1 if individual i belongs to the treated group (born 1950 and after) and 0 otherwise; $EventTime_{t=k}$ is a dummy equal to 1 at period k, and 0 otherwise; ε_{it}^f is the error term. δ_k^f are our parameters of interest. They capture the dynamics of the survivor benefits reform's effect on average survivor benefits from three years before to three years after widowhood, relative to the reference period, one year before widowhood.

The reduced-form equation is equivalent to equation (D.1) with Y_i as outcome variable:

$$Y_{it} = \alpha_i^d + \lambda_t^d + \sum_{k=-3,\, k\neq -1}^{3} \delta_k^d \big(\text{Treated}_i \times \text{EventTime}_{t=k}\big) + \varepsilon_{it}^d, \tag{D.2}$$

where Y_{it} is the the outcome variable (dummy equal to 1 if individual i consumes antidepressants and 0 otherwise) for individual i at time t. δ_k^d are our parameters of interest. They capture the dynamics of the survivor benefits reform's effect on average drug consumption from three years before to three years after widowhood, relative to the reference period, one year before widowhood.

The reduced form Equation (D.2) (or Equation (2) in the main text, which is our main specification) assesses the causal impact of the survivor benefits reform on drug consumption. It tells us the extent to which Dutch widows and widowers born after 1950 have changed their drug consumption in response to the stricter eligibility for the scheme. By reinjecting the estimate obtained in the first stage [Equation (D.1)] into the reduced-form equation [Equation (D.2)], we estimate the change in drug consumption induced by a €1,000 change in survivor benefits (fuzzy-DID estimate).

Finally, we also estimate the overall effect of the reform over the three years following spousal death. Specifically, we first estimate the average impact of the reform on survivor benefits:

$$SB_{it} = \alpha^o i + \lambda^o t + \tau^o \big(\text{Treated}_i \times \text{Post}_{it}\big) + \varepsilon^o it, \tag{D.3}$$

and then the corresponding effect on drug consumption:

$$Y_{it} = \alpha_i + \lambda_t + \tau\big(\text{Treated}_i \times \text{Post}_{it}\big) + \varepsilon_{it} \tag{D.4}$$

The parameters τ^o and τ capture the average impact of the reform on survivor benefits and on antidepressant use, respectively, over the three-year post-widowhood period. Combining both equations, we obtain the fuzzy-DID estimate of the effect of survivor benefits on drug consumption:

$$\beta_{fuzzy} = \frac{\hat{\tau}}{\hat{\tau^o}}, \tag{D.5}$$

which measures the average change in antidepressant use associated with a €1,000 decrease in survivor benefits following widowhood.

Table D.1: Effect of a €1,000 decrease in survivor benefits on antidepressant use by subgroups

	Coefficient	SE	p-value	Observations
Full sample	0.0116	(-0.0041)	0.005	391,097
Gender				
Females	0.0146	(-0.0044)	0.001	269,465
Males	0.0003	(-0.0108)	0.978	121,632
Individual income				
high income	0.0064	(-0.0076)	0.401	198,268
low income	0.0152	(-0.0049)	0.002	192,829
Household income				
high household income	0.0109	(-0.0049)	0.027	229,215
low household income	0.0145	(-0.0068)	0.033	161,882
Partner income				
high partner income	0.0034	(-0.0055)	0.538	197,533
low partner income	0.0191	(-0.0060)	0.002	193,564
Breadwinner status				
secondary earner	0.0148	(-0.0048)	0.002	223,972
main breadwinner	0.0070	(-0.0094)	0.455	153,482
Standard of living variation				
decrease std	0.0313	(-0.0079)	0.000	174,104
increase std	0.0041	(-0.0060)	0.502	185,332
Wealth (banking assets)				
high banking assets	0.0023	(-0.0050)	0.648	177,842
low banking assets	0.0206	(-0.0076)	0.007	178,402

NOTE: IV estimates based on Equation D.4. Individual income is classified as low if earnings plus social benefits two years before the spouse's death fall below the age-specific median, and as high otherwise. Household income is classified as low if total household income two years before the spouse's death falls below the age-specific median, and as high otherwise. Breadwinner status is defined as main breadwinner if the individual contributed at least 50% to the total household income two years before spousal death and secondary earner otherwise. Standard of living variation is measured in equivalized household income, comparing two years before the event to one year after. Partner earnings is classified as low if earnings two years before the death fall below the age-specific median, and as high otherwise. Wealth is classified as low if banking assests two years before the death fall below the age-specific median, and as high otherwise. The sample is composed of widows and widowers born between 1948 and 1959 who became widowed after 2009 and were aged 61 or younger at the time of the spouse's death.
SOURCE: CBS, own calculations.

E Decomposition

We decompose the overall treatment effect into two components: a labor market component and a residual component. The rationale is as follows. We examine the effect of the survivor benefits reform on antidepressant consumption. However, the reform also triggered labor market responses, in particular, an increase in total labor income by 23%, partly driven by higher labor force participation. Since labor force participation is correlated with antidepressant consumption, we seek to assess whether the observed effect operates primarily through changes in labor supply. In other words, whether our estimated effect is (mostly) driven by the increase in labor market activity induced by the reform.

To do so, we decompose our difference-in-differences treatment effect following Heckman et al. (2013). We consider a set of J post-treatment mediators $\theta = (\theta^1, ..., \theta^J)$. Heckman et al. show that the treatment effect on the outcome Y can be decomposed as follows:

$$E(Y^1 - Y^0) = \tau + \sum_j \alpha^j E(\theta_1^j - \theta_0^j), \tag{E.1}$$

where the θ and α_j can be recovered from an augmented switching regression that incorporates the mediators θ:

$$Y_{it} = \alpha_i + \lambda_t + \sum_j \alpha^j \theta^j + \tau\big(\text{Treated}_i \times \text{Post}_{it}\big) + u_{it}, \tag{E.2}$$

and the term $E(\theta_1^j - \theta_0^j)$ is the effect of the treatment $\text{Treated}_i \times \text{Post}_{it}$ on mediator j which we estimate using

$$\theta^j = \lambda^j\big(\text{Treated}_i \times \text{Post}_{it}\big) + \nu. \tag{E.3}$$

Assuming that the α^j and λ^j are consistently estimated, we can obtain percentage contributions of mediator j by calculating

$$\theta^j_{\%} = \big(\frac{\hat{\alpha^j}\hat{\lambda^j}}{\hat{\tau} + \sum_j \hat{\alpha^j}\hat{\lambda^j}}\big).100, \tag{E.4}$$

and the percentage contribution of the unmeasured mediators is

$$\tau_{\%} = \big(\frac{\hat{\tau}}{\hat{\tau} + \sum_j \hat{\alpha^j}\hat{\lambda^j}}\big).100. \tag{E.5}$$

In our analysis, we consider two mediators: labor force participation and labor income. The results are presented in Table E.1. We find that if labor supply acts as a mediator in antidepressant use, its effect operates in the opposite direction to that of the reform. The reform increases labor supply, which in turn is associated with lower antidepressant consumption. Taking this into account, the total

estimated response would be 4.56% higher. Overall, we can conclude that (1) **the labor supply response does not drive our main results on antidepressant consumption**, and (2) **if labor supply does play a role, it biases our estimates downward**.

Table E.1: Decomposition of the treatment effect

	(E.3) coefficients α_j and τ	(E.4) coefficients θ_j	Contribution (%) $\theta^j_{\%}$ and $\tau_{\%}$
Labor force participation	-0.006	0.048	-3.17
Labor income	-0.000	2773	-1.39
Unexplained	0.009		104.56

NOTE: Decomposition results based on Equations E.3 and E.4. The sample is composed of widows and widowers born between 1948 and 1959 who became widowed after 2009 and were aged 61 or younger at the time of the spouse's death.
SOURCE: CBS, own calculations.

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