# Survivor Benefits, Self-Insurance, and Gender Inequality: Evidence from Sweden

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**Abstract.** We study how women respond to reductions in public insurance against widowhood, with a particular focus on forward-looking adjustment before widowhood occurs. We exploit a cohort-based reform in Sweden that sharply curtailed survivor benefits in 1990. Applying a regression discontinuity design to population-wide administrative data, we study labor market outcomes, wealth, and retirement over more than three decades. Treated women delay retirement by two to three months and accumulate modestly more net wealth, but show little earnings response during prime working ages. After an early and unexpected partner death, women in treated cohorts increase employment and earnings substantially more than control cohorts, consistent with the larger income loss they face under the reformed benefit schedule. In a simulation, we show that forward-looking behavioral adjustments offset roughly two-thirds of the reform-induced decline in expected lifetime consumption, yet fall short of full compensation: the net present value of expected consumption falls by 14 percent of baseline annual income. Accounting for the disutility of additional labor supply implies welfare losses of around 50 percent of baseline annual utility. The insurance cost of increased consumption risk upon widowhood is small, in part because women self-insure through precautionary wealth accumulation.

**Keywords:** public insurance, survivor benefits, forward-looking behavior, pension, female labor supply, gender equality, welfare

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# 1 Introduction

Survivor benefits have been an important part of public insurance programs in most developed countries, protecting households—primarily women—against the loss of spousal income. Because these benefits are tied to spousal rather than own earnings, they may also discourage female labor supply and private wealth accumulation. In recent decades, fiscal pressures and a shift toward individualized social protection have led many countries to curtail or abolish survivor benefits, raising a fundamental question: how do women respond when public insurance against widowhood is reduced?

Fully answering this question is challenging because widowhood is uncertain and typically lies far in the future. Responses to reduced survivor insurance are therefore forward-looking, requiring women to adjust labor supply and saving decisions decades before widowhood is realized. Consequently, we know very little about how the design of survivor benefits shapes female labor supply and investment decisions in anticipation of their spouse's death.

Knowing the responses, however, is important, since whether and when such anticipatory adjustment occurs has direct implications for the design of social security reforms. If women compensate through higher earnings and private wealth accumulation, cuts to survivor benefits may be less costly than the mechanical reduction in transfers suggests. If adjustment is incomplete—due to imperfect foresight, adjustment frictions, or limited earning capacity—reforms risk widening gender disparities in retirement income. Women already face a pension gap of 25.6 percent on average across OECD countries (OECD, 2021), and reductions in survivor insurance may further increase old-age poverty among women (European Parliament's Policy Department, 2016).

In this paper, we exploit the 1990 abolition of long-term survivor insurance in Sweden to estimate how households adjust to a reduction in public insurance. The reform reduced expected lifetime income and increased the risk of losing spousal income, thereby altering incentives well before retirement. Forward-looking women may thus respond by increasing labor supply, accumulated earnings, and private wealth. The Swedish setting is ideally suited for three reasons. First, pre-reform survivor benefits were substantial: widows were entitled to benefits for life, and payments typically equaled 40 percent of the deceased spouse's pension.[1] The reform thus generated a sharp and salient decline in coverage. Second, the pre-reform system

[1] Prior to the reform, survivor benefits were available only to married women; after the reform, eligibility became gender neutral.

created strong disincentives to accumulate own pension entitlements, as survivor benefits were means-tested against a woman's own pension income. Higher own earnings and pension contributions therefore reduced survivor benefits, making this a useful setting in which to study the labor-supply distortions created by survivor-based pension schemes. Third, because the reform occurred more than 25 years ago, we can trace both short- and long-term behavioral responses, including labor supply adjustments at older ages and around retirement. We do so, using population-wide Swedish administrative data that provide detailed information on labor market outcomes over the life cycle, wealth, marital histories, children, and spouses. Taken together, these features make Sweden a particularly informative setting in which to study individual responses to reductions in public insurance.

The Swedish reform generates plausibly exogenous variation in survivor-benefit generosity that we exploit in a regression discontinuity design (RDD). We focus on women who remained eligible because they had been married for at least five years as of January 1, 1990,[2] but whose survivor benefits changed discontinuously at a cohort cutoff through the treatment of the husband's pension entitlements. Women born in 1944 or earlier continued to have benefits calculated using the husband's pension points accumulated up to age 64, whereas women born in 1945 or later had accumulation capped at the end of 1989. This generated a sharp, mechanically induced decline in survivor-benefit entitlements for women born in 1945 relative to otherwise similar women born in 1944. The discontinuity is especially pronounced among couples with larger pre-reform earnings gaps, for whom survivor benefits accounted for a larger share of expected retirement income.

Our paper delivers three main findings. First, we provide causal evidence on how reductions in public insurance affect women's labor supply. Although the reform substantially reduced survivor benefits, labor supply responses were modest. Earnings effects are concentrated at older ages and operate primarily through delayed retirement rather than increased labor supply earlier in life. For example, annual earnings at age 62 increase by roughly EUR 1,000, and the probability of postponing retirement rises by about 2.5 percentage points, corresponding to an average delay of approximately four months.

Second, we estimate the causal effect of reduced survivor benefits on wealth accumulation, documenting an additional margin of adjustment to lower public insurance. We find modest increases in assets, debt, and net wealth, with net wealth at age 55

[2] Women also remained eligible if they had a joint child and were married before January 1, 1990, a provision exploited by Persson (2020) to study marital responses to the reform. To avoid endogenous marital selection, we restrict attention to couples who were already married before the reform.

rising by about EUR 3,500.

Third, we quantify the implications of these behavioral responses for lifetime income and welfare in a life-cycle model. Combining the policy-induced reduction in expected survivor benefits with our empirical estimates of labor supply and wealth responses, we find that behavioral adjustments offset about two-thirds of the reform-induced decline in expected consumption, but fall short of fully compensating for the lost benefits. As a result, the net present value of expected lifetime income for treated women declines by about 14 percent of baseline annual income. Converting these income effects into utility and accounting for the disutility of additional labor supply implies a welfare loss of around 50 percent of baseline annual utility. Because precautionary wealth accumulation partly smooths consumption between the non-widowhood and widowhood states, most of this welfare loss reflects the disutility of additional labor supply rather than the loss of insurance against widowhood risk.

Our study makes several contributions to the existing literature. First, it contributes to the literature on the labor supply effects of social insurance.[3] A small subset of this literature studies survivor insurance, but it has so far focused on responses after a spouse's death. Giupponi (2024) shows that lower survivor benefits in Italy induce substantial labor supply responses after widowhood, largely offsetting the loss of benefits through greater labor force participation. The increased labor force participation mainly results from shorter time out of work among younger survivors and delayed retirement by older survivors. Similar evidence for the United States and the Netherlands also points to strong labor supply responses to the generosity of survivor benefit payments (Coyne et al., 2024, Rabaté and Tréguier, 2024). Fadlon and Nielsen, 2021 further show, using Danish data, that households partially offset large income losses if they are permanent due to spousal death, but not after well-insured nonfatal shocks. We differ from this literature by focusing on *forward-looking* adjustment. Rather than examining behavior after widowhood, we study how women adjust labor supply and wealth before a spouse's death in response to a permanent reduction in survivor insurance. This broader perspective allows us to trace labor force participation, earnings, and retirement over more than three decades, and thereby to characterize how retrenchment of public insurance affects labor supply behavior over the life cycle. Existing evidence on forward-looking responses to survivor insurance is limited to family formation: Persson (2020) shows that announced changes in the generosity of survivor benefits affected marriage behavior, underscoring the role of survivor insurance in marital decisions.

Second, our paper relates to a small but growing literature on forward-looking

[3] TBC.

responses to changes in retirement wealth. Because many public policies affect pension entitlements, understanding whether individuals adjust their behavior in anticipation of future wealth is central to evaluating their incentive and welfare effects. Artmann et al. (2023) show that a German pension reform that retroactively raised pensions for mothers aged around 50 to 55, reduced their hours worked and earnings. Other work studies forward-looking responses to changes in the statutory retirement age (Carta and De Philippis, 2024, Hairault et al., 2010) and the pension-contribution link (French et al., 2026). Our paper differs in showing that changes in the *expected* insurance value, not only *realized* changes in benefits, can affect behavior.[4]

Third, our findings contribute to a body of research that studies the interaction between public and private insurance. A central question in this literature is whether reductions in public insurance lead to offsetting increases in private coverage or self-insurance. Cutler and Gruber (1996) document substantial crowd-out of private health insurance by Medicaid expansions, while Seibold et al. (2025) shows that cuts to public disability insurance in Germany led to a sizeable but incomplete take-up of private disability coverage. In the context of permanent income shocks, Blundell et al. (2008) demonstrate that households can partially self-insure through saving and labor supply adjustment, and Fadlon and Nielsen (2021) show that households offset permanent income losses from spousal death but not well-insured temporary shocks. We complement this literature by documenting a specific self-insurance channel: women who face reduced survivor benefits accumulate a precautionary wealth buffer that partially replaces the public insurance they lose.

Finally, we contribute to the broad literature on gender inequality in the labor market (see Altonji and Blank (1999) and Bertrand (2011) for reviews). Related research emphasizes how the tax-and-transfer system shapes gender inequality, especially through the societal choice between joint and individual taxation (Bick et al., 2019, Bick and Fuchs-Schundeln, 2018, Borella et al., 2022, Bierbrauer et al., 2025). We complement this literature by showing that the design of public insurance, specifically survivor benefits, also affects women's labor supply and long-run economic outcomes.

The paper is structured as follows. Section 2 describes the institutional setting and Section 3 introduces our data. Section 4 studies women's responses in anticipation of reduced survivor benefit generosity, while Section 5 focuses on their responses after the partner's death. Both sections present results on labor and wealth, together with the corresponding identification strategy. Section 6 quantifies the welfare implications

[4] In this respect, it is closely related to Dolls et al. (2018), who show that providing salient information about expected future pension wealth increases earnings and savings, even though pension entitlements themselves are unchanged.

of our empirical findings in a simple life-cycle framework. The last section concludes.

## 2 Institutional Background

**Pre-reform survivor insurance for women in Sweden.** Before the reform, Sweden provided generous lifelong survivor benefits to widows. These benefits replaced part of the deceased husband's pension income and were paid for the remainder of the widow's life. Only women were eligible. Eligibility required that the widow be at least 36 years old and either lived with a joint child of the deceased husband or have been married to him for at least five years. The program covered the overwhelming majority of married women (Persson, 2020). Typically, the benefit equaled 40% of the husband's earnings-related public supplementary pension (*Allmä n Tillä ggspension*, ATP)[5], involving significant financial amounts.

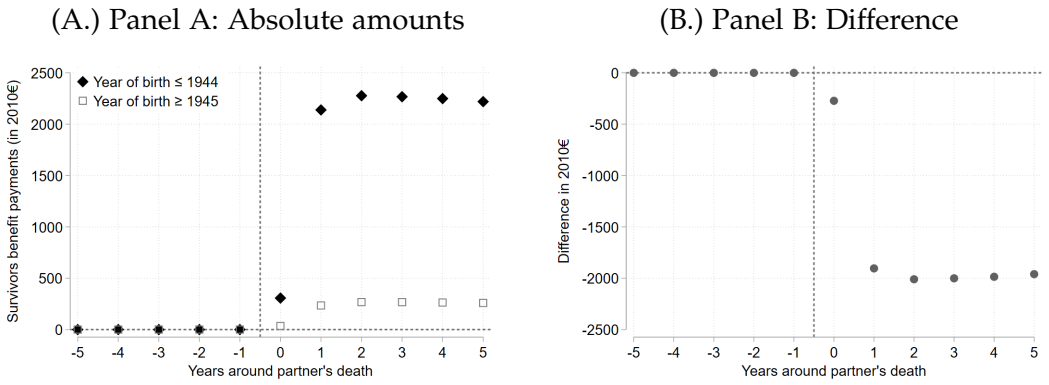
Survivor benefits were means-tested against a widow's own supplementary pension entitlement: benefits were reduced whenever the sum of her own pension and the survivor benefit exceeded 50% of the spouses' combined supplementary pension entitlements, with more generous thresholds for women born before 1935. Thus, survivor benefits increased with the husband's earnings but decreased with a widow's own pension entitlement, weakening women's incentives to accumulate pension rights of their own.

**Post-reform survivor insurance.** The Swedish Parliament debated a reform of survivor insurance in June 1988, and Sweden abolished lifelong survivor benefits on January 1, 1990. In their place, it introduced a one-year transition pension, equal to 40% of the deceased spouse's earned benefit, for both women and men. After the transition year, surviving spouses received only their own earned benefit. The reform thus sharply reduced the value of public survivor insurance for women. At the same time, grandfathering provisions preserved more generous benefits for some previously married women, creating quasi-experimental variation in survivor-insurance generosity. Women who had been married for at least five years as of December 31, 1989, in principle remained eligible for lifelong benefits.[6] Among these women, the

[5] Sweden's public pension system comprised the ATP, an earnings-related supplementary pension, and the *folkpension*, a residence-based basic pension; see Palme/Svensson (1999). For widows below retirement age, the survivor benefit was reduced from 40% to 35% if children in the household received a child pension. In addition, these younger widows received a supplement equal to 90% of the statutory base amount (*basbelopp*), or SEK 25,110 in 1989. The supplement ended when the widow turned 66.

[6] Women also remained eligible if they had a joint child and were married before January 1, 1990. A shown by **Persson2020**, this transition rule affected marriage behavior. Because our focus is on labor

Figure 1: Reform-induced drop in survivor benefits




This figure plots the mean survivor benefits in absolute amounts (panel A) and the difference (panel B), both in 2010 Euros. The figure includes all eligible married women in Sweden born in 1942–47, who are 65 years or older at the time of their spouse's death.

reform generated exogenous variation in benefit generosity: among eligible women, those born before 1944 were treated more generously, because the deceased spouse's pension entitlements were counted up to his age 64, whereas for women born in 1945 or later, the husband's pension entitlements were counted only through 1989. This generated a sharp and plausibly unanticipated drop in survivor benefits for women born in 1945 relative to those born in 1944, especially in couples with larger spousal earnings gaps.

Figure 1 displays yearly survivor benefits among eligible women born in 1944 or earlier (black diamonds) and 1945 or later (hollow squares), both in absolute amounts (expressed in Euro 2010, panel A) and as the difference between the two groups (panel B). The payments as a share of a widow's disposable income are depicted in Figure B.1. Annual survivor benefits payments declined on average by around 2000 Euros (11% of disposable income) due to the reform. The reduction in benefit payments is particularly large for women whose pensionable income is considerably lower than that of their deceased spouse (cf. Figure B.2). Given its large financial consequences for future widows, the reform was highly salient in Sweden and attracted substantial media coverage (see Persson, 2020, Figure 1B).

In the next section, we exploit the sharp drop in survivor benefits to identify the effects of publicly provided survivor insurance on labor supply and private capital accumulation.

supply and saving responses, we exclude women with children on January 1, 1990, and therefore abstract from this marriage channel.

# 3 Data

## 3.1 Data Sources

We use linked administrative panel data covering the universe of residents in Sweden from 1985. The data combine several population-wide registers maintained by Statistics Sweden and are linked at the individual level using personal identification numbers.

Our primary data source is the firm and establishment register (*Registerbaserad arbetsmarknadsstatistik*) which contains information on individuals' employment spells and earnings. We also use the LISA (*Longitudinell integrationsdatabas för Sjukförsäkrings- och Arbetsmarknadsstudier*, available from 1990) database provided by Statistics Sweden, which contains detailed information on individual income as well as pension and survivor benefit payments for all residents aged 16 and older. We complement these sources with several additional administrative registers. Information on income components, taxable income, and wealth comes from the Income and Tax Register (*Inkomst och Taxeringsregistret*, available from 1999 to 2006). Fertility histories are drawn from the birth register (*Födelseregistret*), mortality information from the cause-of-death register (*Dödsorsaksregistret*), and marital status histories from the civil status register (*Civilståndsregistret*). The population censuses (*Folk- och Bostadsräkningar*) between 1960 and 1990 contain additional demographic information. Together, these registers provide comprehensive longitudinal information on individuals' employment, earnings, retirement behavior, wealth, and demographic characteristics.

Our analysis focuses on women born between 1942 and 1947. This range includes three cohorts born before 1945, who faced more generous survivor-insurance rules because the deceased husband's pension entitlements were counted up to age 64, and three cohorts born in 1945 or later, whose benefits were based only on pension entitlements accrued through 1989. We further restrict the sample to women who had been married for at least five years as of December 31, 1989. This restriction guarantees stable pre-treatment marital histories and ensures that assignment to treatment is determined solely by birth cohort rather than by marriage duration at the time of the reform. The final sample contains approximately 40,000 women per cohort and is broadly representative of married women in Sweden. Finally, we use information on marriage histories, children, and, where available, family identifiers to match women to their spouses and to the spousal earnings.[7] Table A.1 depicts the summary statistics of selected variables for the birth cohorts 1944 and 1945 of our estimation sample.

[7] Applying this procedure, we match 94% of women to their corresponding spouse.

# 4 Anticipatory Behavior

This section presents the causal effects of reduced survivor benefits on women's anticipatory behavior, i.e., before her partner dies. Subsection 4.1 presents the empirical model. In Subsection 4.2, we present the effects on earnings, zoom in on retirement behavior, and discuss robustness. Section 4.3 turns to the effects on wealth accumulation.

## 4.1 Empirical Model

We focus on women whose marriage had lasted at least five years as of December 31, 1989, and who, therefore, remained eligible for survivor benefits. For this group, we exploit the cohort-based cutoff in survivor benefit generosity described in Section 2 in a regression discontinuity design (RDD): Women born before January 1, 1945 retained the more generous benefit formula, under which husbands' pension points accumulated up to age 64. For women born on or after that date, pension point accumulation was instead truncated at the end of 1989. This generates a sharp, mechanically induced reduction in expected survivor benefits for women born just after the cutoff relative to those born just before.

We estimate the following RDD specification separately for each year $t \in \{1985, \ldots, 2018\}$:

$$Y_{it} = \alpha + \beta\, T_i + \gamma_1 (X_i - c) + \gamma_2\, T_i \cdot (X_i - c) + \epsilon_{it}, \tag{1}$$

where $Y_{it}$ denotes the outcome of individual $i$ in year $t$ (e.g., annual earnings, employment, or wealth); $X_i$ is the individual's date of birth measured in days; $c$ denotes the cutoff date of January 1, 1945; and $T_i = \mathbb{1}[X_i \geq c]$ is a treatment indicator equal to one for women born on or after the cutoff. The parameter of interest is $\beta$, which captures the local average treatment effect of reduced survivor benefit generosity on the outcome at the cutoff. The model includes separate linear trends in the running variable on either side of the cutoff.

We implement the RDD using the bias-corrected robust inference procedures proposed by Calonico et al. (2014), which select optimal bandwidths in a data-driven manner while adjusting confidence intervals for the bias inherent in local polynomial estimation. By estimating equation (1) year by year, we trace out the dynamic evolution of the treatment effect over the life cycle. This allows us to distinguish pre-reform years—where we expect no effect—from post-reform years in which forward-looking individuals may begin to adjust labor supply and savings. We present the resulting

treatment effects $\hat{\beta}_t$ in a series of event-study-style graphs.

**Identifying assumptions.** The validity of the RDD rests on two standard conditions. First, individuals must be unable to manipulate their assignment to treatment. Since the relevant birth-year cutoff was determined more than 40 years before the reform was first debated in June 1988, manipulation is implausible. We provide supporting evidence by showing that the density of births is smooth around the cutoff (see Appendix Figure B.3).[8] Second, continuity of potential outcomes requires that all determinants of the outcome other than treatment evolve smoothly at the cutoff. We verify this by testing for discontinuities in predetermined characteristics and pre-treatment outcomes, including earnings and employment in 1985 (five years before the reform) as well as years of schooling and immigrant status in 1990. Appendix Figure B.4 presents the results and shows no significant discontinuities at the cutoff for any of these variables.

**Identification challenges and checks.** A potential confounder is a Swedish school reform that extended compulsory education for birth cohorts from the late 1940s, gradually implemented across municipalities (see Nybom and Stuhler, 2024 for details). We address this concern in a sensitivity analysis by residualizing earnings by educational attainment and show that the results are unchanged.

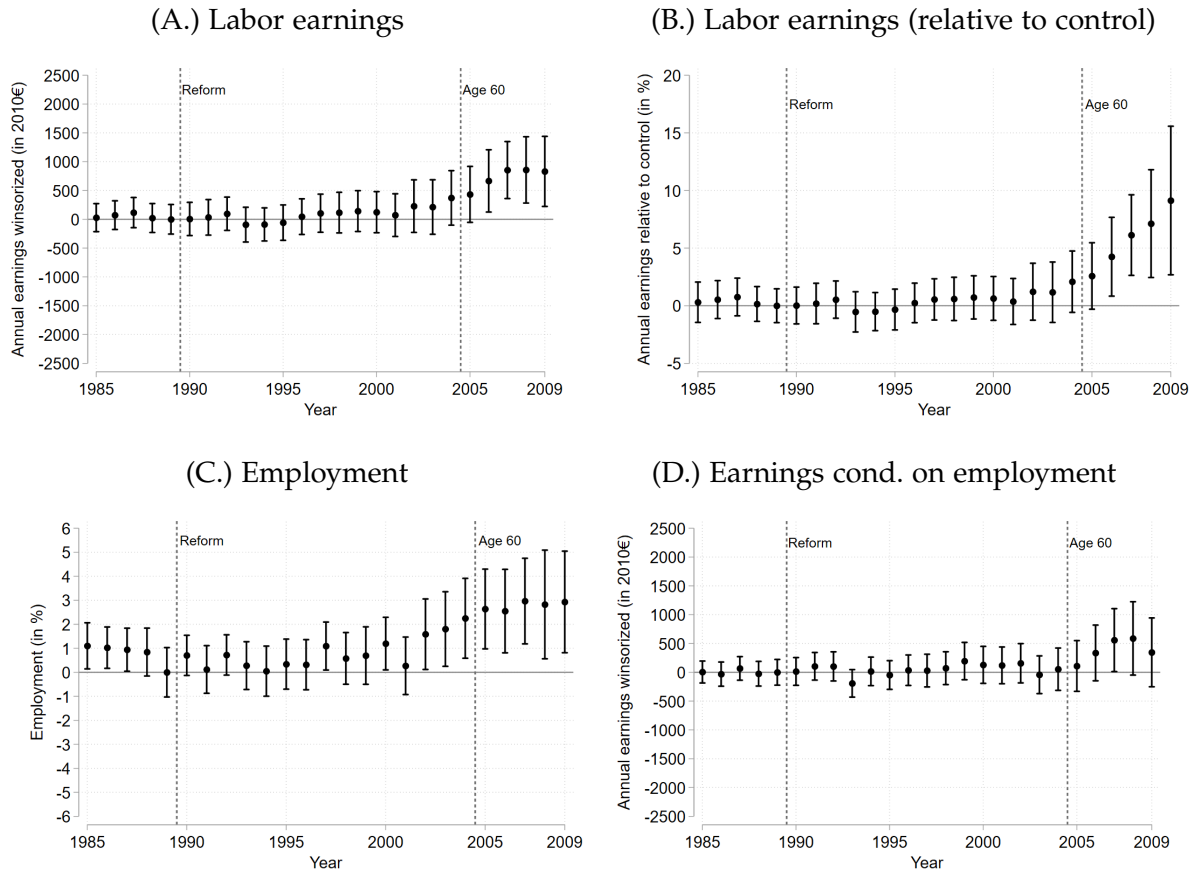
Another relevant institutional change is the 1994 pension reform, which linked pensions to lifetime earnings rather than the best 15 years of earnings. Individuals born between 1938 and 1953 belonged to transition cohorts, with the share of benefits calculated under the old system declining linearly by 1/20 per birth year, from 16/20 for the 1938 cohort to 1/20 for the 1953 cohort; cohorts born in 1954 or later were fully covered by the new system. We address this potential confounder by estimating a triple-difference specification that absorbs differential exposure to the reform across birth cohorts.

## 4.2 Effects on Labor

**Baseline.** Figure 2 presents the year-by-year RDD estimates of the reform effect on annual earnings and employment from 1985 to 2009. Panel A displays absolute earnings differences at the cutoff between treated women (born in 1945 or later) and control women (born before 1945). Several patterns emerge. In the pre-reform period (1985–1989), estimated treatment effects are close to and statistically indistinguishable

[8] We implement the density test proposed by Cattaneo et al. (2020).

Figure 2: Anticipatory effects of reduced survivor benefits on annual earnings




*Sample:* Women born 1942–47, married for at least five years as of December 31, 1989. *Method:* Robust RDD with optimal bandwidth (Calonico et al., 2014). Estimates show mean yearly differences in outcomes between women just above and below the January 1, 1945 cutoff, with 95% confidence intervals. *Outcomes:* Panel A reports annual total earnings (EUR 2010), winsorized at p1 and p99. Panel B reports the treatment effect relative to mean earnings in the control group. Panel C reports the employment rate. Panel D reports annual total earnings conditional on employment, winsorized at p1 and p99.

from zero, supporting the absence of pre-existing differences. After the reform, a small but growing earnings gap appears, with treated women earning modestly more than women in the control group. Importantly, these differences remain small throughout prime working ages. The gap widens substantially and significantly after 2005, when women in the sample approach ages 60–65, suggesting that the labor supply response operates primarily through the timing of retirement rather than through behavioral responses during the prime career. Averaged across all post-reform years, the treatment increased annual earnings by approximately EUR 248 in 2010 prices. Panel B presents these treatment effects relative to the control group's mean earnings. The average annual earnings increase is 1.81 percent, driven by substantial increases of women approaching retirement age.

Panel C of Figure 2 shows that the earnings effects operate primarily through the extensive margin. Treated cohorts are more likely to be employed than control cohorts,

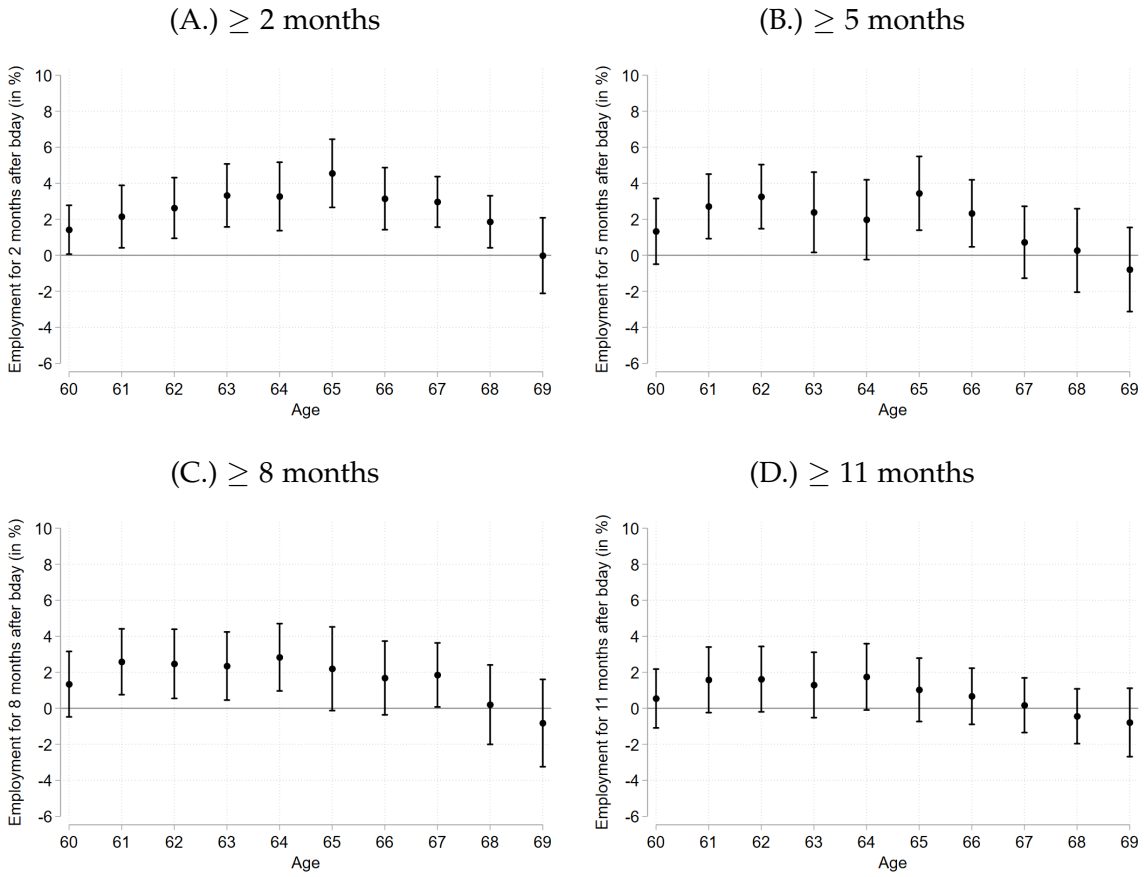
with differences remaining small through most of women's working life and opening up only in their late 50s. Panel D shows a positive, but less pronounced, effect on earnings, conditional on working. For the population of workers employed in the public sector, and a subsample of workers employed in the private sectors we observe weekly working hours and monthly wages. In line, with the less pronounced earnings response, Appendix Figure B.5 shows no statistically significant response in hours worked or in wages conditional on employment. Although hours worked increase slightly in the early years, these estimates are very small (about 15 minutes per week) and statistically insignificant.

**Retirement behavior.** The strong earnings effect at older ages points to postponed retirement as the key margin of adjustment. A challenge in identifying retirement is that pension data are available only at the annual level, which can distort the measurement of retirement timing for women born in different months.[9] We address this issue by moving to a monthly measure and defining the outcome as an indicator for positive labor income in $x$ consecutive months following a woman's birthday at ages 60 to 69. We focus on $x \in \{2, 5, 8, 11\}$.

Figure 3 presents the results. Under the two-month definition, treated women are about 2.5 percentage points more likely to remain employed than control women. This implies a substantial delay in retirement: treated women remain more likely to work for at least two additional months, and this pattern is visible throughout ages 60 to 69. The effect is somewhat smaller under the five- and eight-month definitions, though the estimates are not statistically distinguishable. Under the eleven-month definition, it declines to about 0.7 percentage points. The pattern suggests that many treated women delayed retirement by a few months, while fewer postponed retirement by longer periods. In addition, we estimate the effect of the reform on the sum of women's monthly earnings, conditional on them being employed for at least $x$ consecutive months following their birthday. The results are presented in Appendix Figure B.6. Only for women who were employed for at least eleven consecutive months, we find a significant positive effect at the intensive margin. Finally, we find no evidence that women reenter the labor force once retired (results available upon request). Overall, these results are consistent with labor supply mainly responding at the extensive margin around retirement, and less so during the prime career.

[9] To see this, consider two women who both work one additional month. In annual data, a woman born in December appears to delay retirement by a full year, whereas a woman born in January does not.

Figure 3: Postponed retirement: probability of continued employment after birthday




*Sample:* Women born 1942–47, married for at least five years as of December 31, 1989. *Method:* Robust RDD with optimal bandwidth (Calonico et al., 2014). Estimates show mean yearly differences in outcomes between women just above and below the January 1, 1945 cutoff with 95% confidence intervals. *Outcomes:* Probability of having positive labor income in $x$ consecutive months following a woman's birthday, for ages 60–69. Panel A: $x = 4$; Panel B: $x = 7$; Panel C: $x = 11$.

**Part-time vs full-time.** Part-time work may constrain adjustment on the intensive margin. If women are already working part-time, increasing hours may require switching to a full-time position—a transition that is often difficult due to limited availability, caregiving obligations, or employer constraints. We estimate the effect on women's earnings separately for full-time and part-time workers, defined by their weekly working hours, if available, in 1989. The results indicate that the reform's positive effect on women's earnings between 60 and 65 is more pronounced for women who initially worked full-time (cf. Figure B.7).

**Labor earnings vs small business income.** We decompose total earnings into employed labor income and self-employment income from small businesses. If frictions are more binding in dependent employment (e.g., due to rigid working-hour schedules), we expect a larger response among self-employed women who have greater control over their work intensity. The estimated point estimates for labor earnings are

very similar to those of total earnings, while we do not find any statistically significant effect on small business income (cf. Figure B.8).

**Partners' responses.** To come.

**Sensitivity.** We run a comprehensive set of robustness checks to assess the sensitivity of our baseline earnings estimates.

(1) *Winsorizing.* We show that results are robust to using non-winsorized earnings, alleviating concerns that findings are driven by outliers (cf. Figure B.9).

(2) *Heterogeneity by shock size.* We split the sample at the median of pre-reform accumulated pensionable income, which proxies for the expected benefit loss. The results do not substantially differ between the two groups (cf. Figure B.10).

(3) *Conservative sample.* Excluding women whose partner died within three years of the observation ensures that the estimates capture anticipatory adjustments rather than responses to imminent spousal death. Results are unchanged (cf. Figure B.11).

(4) *School reform.* A Swedish compulsory schooling reform affected cohorts born around the mid-1940s, potentially confounding the RDD. In fact, the pre-determined years of schooling show a small increase around the cutoff (see Panel C of Figure B.4. Residualizing earnings by educational attainment (using the Sun2000 classification) leaves the results unchanged (cf. Figure B.12).

(5) *Bandwidth.* We re-estimate the RDD using fixed bandwidths of one year and six months, as well as using June earnings only within a five-month window. Results are stable across these specifications (cf. Figure B.13).

(6) *Difference-in-Discontinuities.* We estimate the RDD around the January 1 cutoffs for 1944, 1945, and 1946 separately. We use a 180-day window on either side of the cutoff date to prevent individuals from being included in multiple estimation samples. The difference-in-discontinuities estimator is computed as the difference between the 1945 RDD estimate and the average of the 1944 and 1946 RDD estimates. Standard errors are constructed using the delta method. The point estimates for the effect of the reform on women's earnings are similar to our baseline estimates (cf. Figure B.15).

## 4.3 Effects on Capital

Women may respond to reduced survivor benefits not only by adjusting labor supply but also by accumulating more private wealth as a form of self-insurance. We, therefore, estimate the RDD for wealth outcomes over the period 1999–2006, the years for which individual-level wealth data are available in the Swedish administrative records. We

examine three categories: total assets, total debt, and net wealth (defined as assets minus debt). Because the wealth data contain extreme outliers, we winsorize all variables at the 1st and 99th percentiles in our baseline specification.

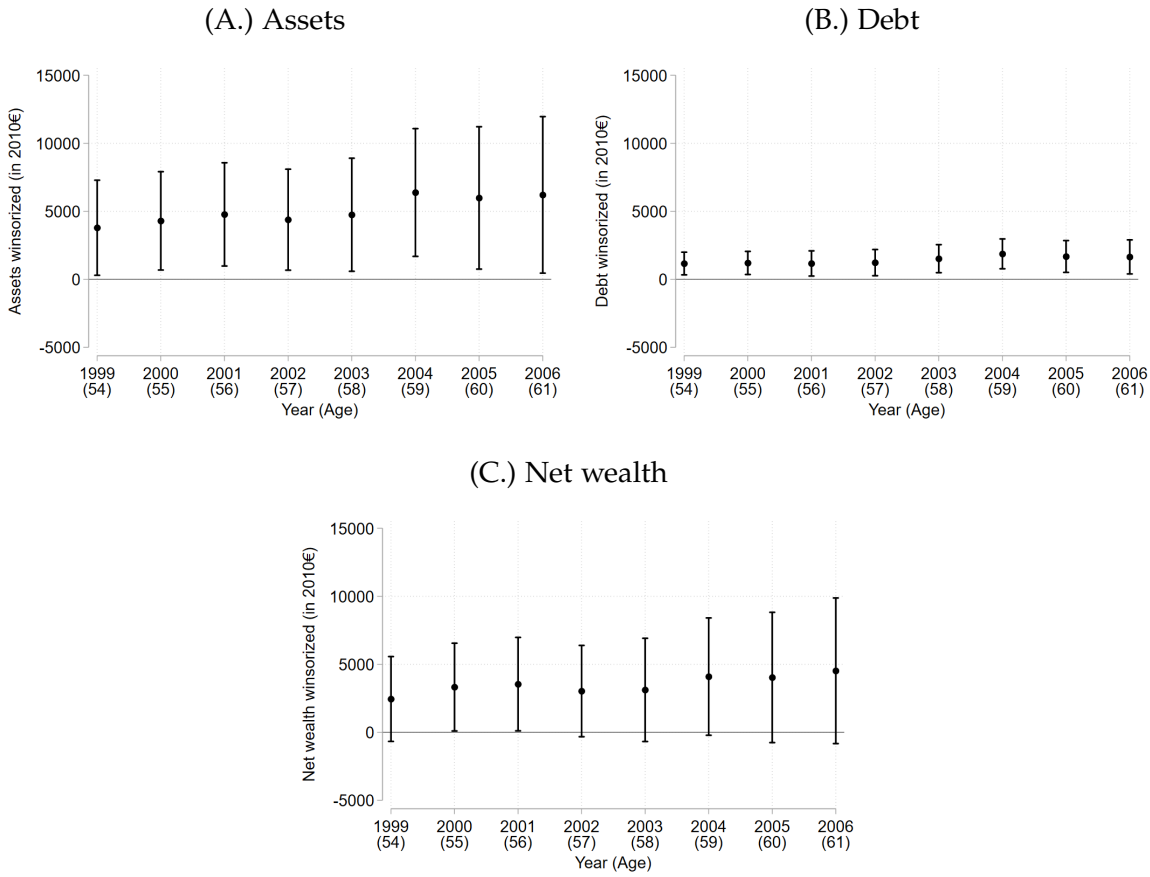
Figure 4 presents the results. We find a statistically significant increase in total assets of approximately EUR 5,068 for treated relative to control women over the 1999–2006 period, with the gap rising from 1764 EUR in 1999 to 6562 EUR in 2006. At the same time, debt increases by roughly EUR 1,432, possibly reflecting mortgage-financed housing investments. Overall, this leads to an increase in net wealth by approximately EUR 3,512 (4.8%). The magnitude of the wealth response, however, is modest relative to the expected loss in survivor benefits—a pattern we quantify more formally in the conceptual framework that follows.

**Effects on private pensions.** To come.

**Effects on private life insurance.** To come.

**Sensitivity.** Results using raw wealth measures are qualitatively similar but less precisely estimated (see Appendix Figure B.16)

Figure 4: Anticipatory effects of reduced survivor benefits on wealth




*Sample:* Women born 1942–47, married for at least five years as of December 31, 1989. *Method:* Robust RDD with optimal bandwidth (Calonico et al., 2014). Estimates show mean yearly differences in outcomes between women just above and below the January 1, 1945 cutoff with 95% confidence intervals. *Outcomes:* Panel A shows total assets, Panel B total debt, and Panel C net wealth (assets minus debt). All outcomes measured in EUR 2010 and winsorized at p1 and p99. Wealth data are available for 1999–2006.

# 5 Behavior Upon Partner Death

After having characterized how women adjust to the reform over the life cycle in anticipation of a partner's death in Section 4, we next present causal evidence on women's behavior at and after partner death, that is, on ex post adjustment. Subsection 5.1 introduces the empirical model. Subsection 5.2 presents the empirical results.

## 5.1 Empirical Model

We estimate an event study model that compares the labor market behavior of treated and untreated women around the date of the partner's death. Denoting the individual-

specific year of partner death as $d_i$, the empirical model is given by:

$$Y_{it} = \sum_{j=-5}^{9} \alpha_j \cdot \mathbb{1}[t = d_i + j] + \sum_{j=-5}^{9} \beta_j \cdot \mathbb{1}[t = d_i + j] \cdot T_i + \gamma \cdot (AgeAtDeath_i \cdot \mathbb{1}[t = Year]) + \mu_i + \epsilon_{it}, \quad (2)$$

where $Y_{it}$ is the labor market outcome of woman $i$ at event time $t$ relative to the year of partner death, $\alpha_j$ captures the effect of partner death on outcome $Y_{it}$ from five years before the death to nine years after. We normalize effects at $j = -1$. Variable $T$ indicates the treatment group and is defined as follows

$$T_i = \begin{cases} 0 & \text{if } birthyear_i \in [1942, 1944] \\ 1 & \text{if } birthyear_i \in [1945, 1947]. \end{cases}$$

Hence, the vector of coefficients $\beta_j$ measures the differential impact of partner death for women in the treated group, i.e., with less generous survivor benefits, compared to women in the control group, who were not affected by the 1990 reform. We include individual fixed effects $\mu_i$ and year fixed effects $\mathbb{1}[t = Year]$ interacted with age-at-partner-death fixed effects.

**Identification.** The estimation equation (2) is as a triple-differences model. The first difference compares widows with non-widows, the second difference compares periods before and after the partner's death, and the third compares treated and control cohorts. To interpret $\beta_j$ causally, the following identifying assumption must hold: The effect of partner death on the labor market behavior of surviving women in the control cohorts must coincide with the counterfactual effect of treated women in the absence of the 1990 reform. An influential literature shows that partner death is not random, and that, in the context of survivor benefits, it is crucial to focus on unexpected deaths (Fadlon and Nielsen, 2021). We follow this approach by restricting the sample to early partner deaths occurring before a partner turned 60.[10] In addition, specification 2 controls for age-at-partner-death-by-year fixed effects.

**Matching non-widows.** To improve comparability between widows and non-widows, we match widows to comparable non-widows. First, we select women who become widows and whose partner dies at age 60 or younger. Second, for these ever-widows,

[10] In the near future, we will have access to information on the causes of death and can further refine the approach and assess its sensitivity.

we match based on characteristics observed five years before partner death. Third, we identify the best match among never-widows by matching exactly on the women's year of birth, the partner's year of birth, and calendar year, and by choosing the nearest neighbor in the woman's earnings, her employment status, and the partner's earnings. Fourth, we assign each matched never-widow the counterfactual partner age-at-death of her matched widow. We then follow ever-widows and matched never-widows from five years before to nine years after the actual or assigned partner's death.

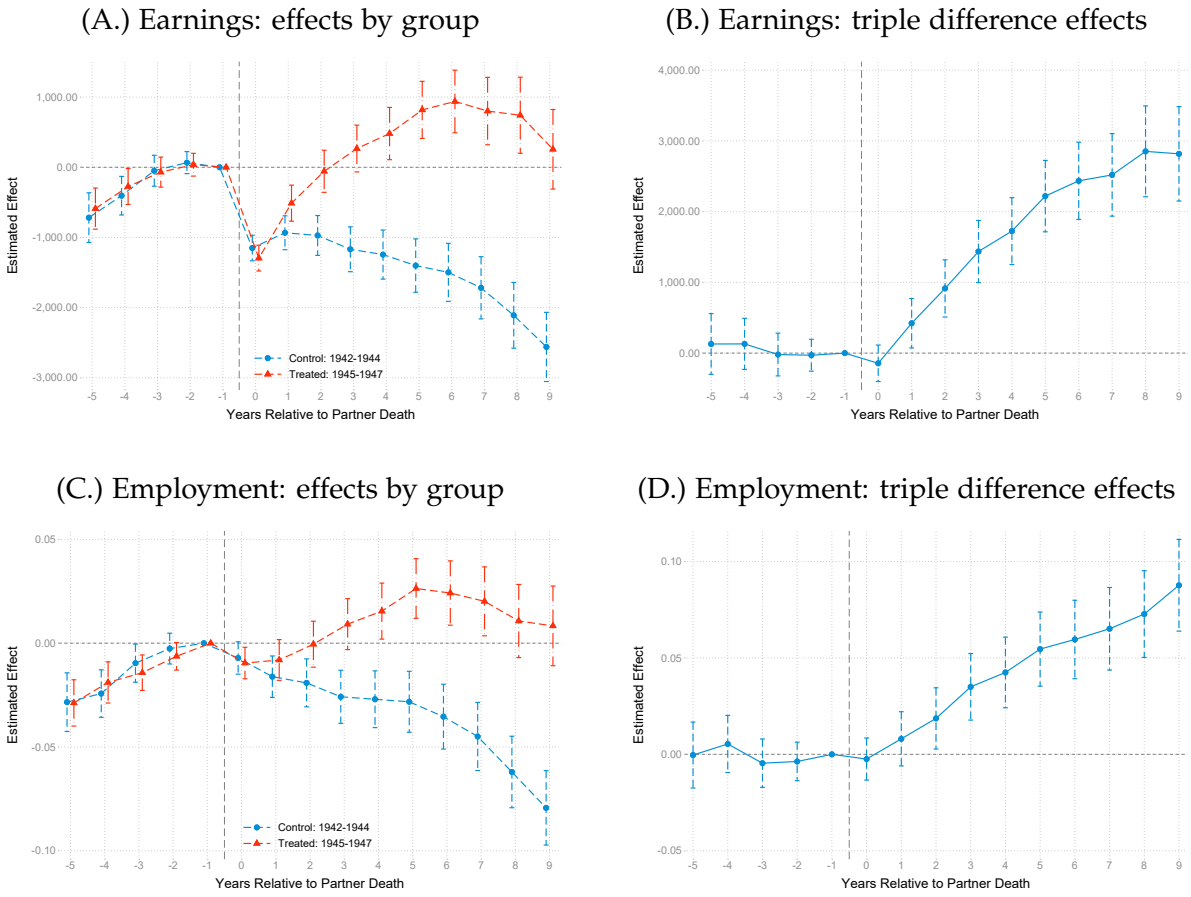
## 5.2 Results

Figure 5 presents the results of the event study. The top row panels show effects on women's earnings, and the bottom row panels report effects on their employment probability. The left panels depict separate event studies for treated and control cohorts around the partner's death. The right panels show the triple-differences estimates, captured by the set of $\beta_j$ coefficients from equation (2), that measure whether cohorts exposed to the cut in survivor benefits respond differently to partner death than untreated cohorts.

Panel A shows that to-be-widows in both the treatment and control groups experience a gradual increase in earnings in the years leading up to the partner's death. In the year of partner death (period 0), earnings drop sharply and by similar amounts in both groups. The drop amounts to roughly 1,000 euros, or about 8% of annual earnings. After this initial drop, however, the earnings paths diverge significantly. Cohorts exposed to the cut in survivor benefits increase earnings sharply and return to their pre-death trajectory within 2-3 years. In contrast, control cohorts that remained under the more generous pre-reform regime continue to reduce their earnings. Panel B depicts this difference in earnings dynamics directly. Pre-treatment effects are flat and statistically indistinguishable from zero. Starting in the first year after the partner's death, treated cohorts show a significant and growing positive earnings effect relative to control cohorts. Nine years after partner death, treated cohorts earn almost 3,000 euros more than control cohorts, corresponding to a gap of about 33 percent.

Panels C and D of Figure 5 reveal a similar pattern for employment. One year after partner death, employment probabilities in the treatment and control cohorts start to diverge. Nine years after partner death, women exposed to the less generous survivor benefit regime are 9 percentage points more likely to be employed, corresponding to a relative increase of about 15 percent.

Figure 5: Post-widowhood effects of reduced survivor benefits




*Sample:* Eligible married women born 1942–47, married for at least five years as of December 31, 1989, aged $\leq 60$ at the time of the spouse's death ($t = 0$). *Method:* Event study around spouse's death as specified in equation (2), including individual and calendar year $\times$ age-at-death fixed effects. Estimates show differences relative to $t = -1$ with 95% confidence intervals. *Outcomes:* Panels A and B show annual earnings (EUR 2010); Panels C and D show the probability of positive labor earnings. Left panels show separate event-study estimates for control women (born 1944 or earlier) and treated women (born 1945 or later). Right panels show the treatment–control difference.

# 6 Welfare Effects of the Reform

We use a simple life-cycle framework to quantify the welfare implications of the reform based on our empirical estimates. First, we focus on the monetary impact by comparing the net present value (NPV) of income gains from behavioral responses to the NPV of income losses from reduced survivor benefits (Subsection 6.1). Second, we move from income to welfare by accounting for the disutility of additional labor supply and the insurance value of survivor benefits (Subsection 6.2). Last, we provide direct evidence on welfare using women's health and mortality outcomes (Subsection 6.3).

## 6.1 Income Effects

We compute the change in expected NPV lifetime consumption for a representative married woman born in 1945 (treated) relative to one born in 1944 (control). Panel A of Table 1 decomposes this change into four components: reduced survivor benefits, additional labor income, annuity income from a precautionary wealth buffer, and the saving cost of building that buffer. All values are expressed as a percentage of average baseline annual income, measured as 1990 earnings of the 1944 cohort in 2010 euros (EUR 17,500).

**Survivor benefit loss.** The reform reduced annual survivor benefits by approximately EUR 1,960 for widows aged 65 and older (in 2010 prices). Because the loss accrues only upon widowhood, we weight the annual benefit reduction by age-specific mortality probabilities for the husband, using data from the Human Mortality Database for Swedish male cohorts born around 1942. At each age, we also account for the woman's own survival probability and discount at $r = 0.02$. Cumulative male mortality rises from approximately 5 percent by the time the woman reaches age 53 to 20 percent by age 69 and over 50 percent by age 80. The expected NPV of the benefit loss amounts to 48 percent of baseline annual income (EUR 8,400).

**Labor income gain.** The earnings responses documented in Section 4.2 generate additional labor income that accrues with certainty—regardless of whether the husband dies. This makes labor supply the dominant adjustment margin. The NPV of additional earnings, computed from year-by-year RDD estimates using the same discount rate and survival weighting, amounts to 38 percent of baseline annual income (EUR 6,700).

**Wealth buffer and saving cost.** Treated women also accumulate additional net wealth, as documented in Section 4.3. We model this wealth as a precautionary buffer against widowhood. While the husband is alive, capital income is reinvested, and the buffer is not consumed. Upon the husband's death, the buffer is converted into a survival-adjusted fair annuity over the woman's remaining lifetime.

We model the accumulation path as follows. In the reform year (1990), 45-year-old women have zero additional wealth ($W_{45} = 0$). During working ages (46–69), the wealth stock grows through a constant annual saving flow and capital returns: $W_t = (1 + r_a) \cdot W_{t-1} + s$, with $r_a = 0.03$. After retirement (age 70+), no further saving occurs, and the stock grows passively: $W_t = (1 + r_a) \cdot W_{t-1}$. Since our wealth data cover 1999–2006, the RDD identifies wealth effects only for ages 54–61. We calibrate $s$

to minimize the sum of squared deviations between the model-implied and observed RDD wealth at these ages, yielding $s \approx$ EUR 230 per year.[11]

If the husband dies, the accumulated buffer is annuitized over the woman's remaining life expectancy. The annuity is small for early widowhood and grows with both the buffer stock and the shorter remaining horizon. In NPV terms, expected annuity income amounts to 18 percent of baseline annual income (EUR 3,100). However, building the buffer requires saving that reduces consumption while the husband is alive. With saving weighted by the probability that the husband is still alive at each age, the NPV of the saving cost is 22 percent of baseline annual income (EUR 3,900). The net effect of the wealth channel is thus $-4$ percent—the saving cost exceeds the expected annuity income. Despite its negative expected monetary value, the buffer has value because it smooths consumption across states of the world, a mechanism that is captured in the welfare analysis of Section 6.2.

**Net consumption effect.** Figure 6 shows how these four components accumulate over the life cycle. The labor income gain materializes early and grows steadily, while the expected survivor benefit loss accelerates after age 65 as cumulative death probabilities rise. The saving cost increases linearly during working life and levels off at retirement; expected annuity income rises gradually as widowhood becomes more likely. The four components sum to the net consumption effect at every age.

Overall, the expected NPV of lifetime consumption for treated women falls by 14 percent of baseline annual income (EUR 2,500). Behavioral responses—higher labor supply and precautionary wealth accumulation—thus offset roughly 70 percent of the reform-induced loss in expected survivor benefits, but do not fully compensate for it.

[11] At younger ages, this saving exceeds the woman's own additional earnings, implying that either women's total consumption decreases (modelled) or that part of the saving is funded from other sources, like (additional) partner income. We plan to incorporate estimates of spousal labor supply responses in future work.

Table 1: Welfare effects of the reform

| | % of baseline annual income | EUR 2010 (NPV) |
|---|---|---|
| *Panel A: Income effects* | | |
| Loss from reduced survivor benefits | -47.9 | -8,374 |
| Gain from labor supply response | 38.3 | 6,707 |
| Gain from annuitized wealth buffer | 17.7 | 3,092 |
| Cost of wealth accumulation (saving) | -22.2 | -3,879 |
| **Net consumption effect** | **-14.0** | **-2,454** |
| *Panel B: Welfare effects* | | |
| Consumption utility change | -14.8 | |
| Disutility of additional labor | -36.5 | |
| **Net utility (certainty equivalent)** | **-51.4** | |
| Cost of increased consumption risk | -0.8 | |
| **Net utility (risk-adjusted)** | **-52.2** | |
| Bequest value of wealth buffer | 3.9 | |
| **Net utility (incl. bequest)** | **-48.2** | |

This table decomposes the welfare effects of the 1990 survivor benefit reform for a representative treated woman (born 1945) relative to a control woman (born 1944). Panel A reports changes in the net present value (NPV) of consumption, expressed as % of baseline annual earnings (EUR 17,500) and 2010 EUR. Wealth buffer is modeled as precautionary saving: capital income is reinvested; saving is deducted from consumption while the husband is alive. Upon the husband's death, the buffer is annuitized over the woman's remaining lifetime. Panel B reports utility from a CRRA model ($\sigma = 1.1$). Labor disutility is decomposed into an intensive ($\eta = 2$, Hicksian elasticity $= 0.5$) and an extensive margin component ($\kappa = .075$/yr, Seibold 2021). Insurance cost is defined as $(E[c]) - E[U(c)]$. $r = 0.02, r_a = 0.03$. Mortality: Swedish Human Mortality Database (female cohort 1945, male cohort 1942; Gompertz extrapolation).

Figure 6: Income effects of the reform over the life cycle




This figure shows the cumulative survival-weighted net present value of the four consumption components of the reform, expressed as a percentage of average baseline annual income (EUR 17,500). Blue solid line: gain from additional labor income. Red dashed line: expected loss from reduced survivor benefits, weighted by the cumulative probability that the husband has died by each age. Green short-dashed line: expected annuity income from the precautionary wealth buffer upon widowhood. Orange dash-dotted line: saving cost of building the wealth buffer, weighted by the probability that the husband is still alive. Black solid line: net consumption effect, equal to the sum of the four components at every age. Parameters: $r = 0.02$, $r_a = 0.03$, $s = 231$ EUR/yr. Survival probabilities from the Swedish Human Mortality Database (female cohort 1945, male cohort 1942).

## 6.2 From Income to Welfare (work in progress)

The consumption effects documented above understate the welfare cost of the reform because additional labor supply entails a disutility of work. To quantify this cost, we calibrate a standard life-cycle model with CRRA preferences over consumption and the disutility of labor, feeding in our reduced-form estimates. We currently calibrate the model with parameters from the existing literature rather than estimating it structurally, and the results are sensitive to the assumed curvature of the disutility function.

**Utility function.** We use an additively separable utility function (e.g., MaCurdy, 1981, Chetty, 2006, Keane and Rogerson, 2012) with both intensive and extensive labor supply margins:

$$U(c_t, h_t, P_t) = \frac{c_t^{1-\sigma}}{1-\sigma} - P_t \cdot \left( \phi \cdot \frac{h_t^{1+\eta}}{1+\eta} + F \right), \tag{3}$$

where $c_t$ denotes consumption, $h_t$ hours worked, and $P_t \in \{0, 1\}$ is a participation indicator. The first disutility term captures the intensive margin—the cost of working more hours conditional on employment—following the isoelastic specification standard in the labor supply literature (MaCurdy, 1981, Keane and Rogerson, 2012). The second term captures the extensive margin—the utility cost of remaining employed rather than retiring—as is common in the discrete choice labor supply literature (Soest, 1995).

**Calibration.** We set $\sigma = 1.1$, close to log utility and consistent with Chetty (2006), who shows that labor supply evidence bounds the coefficient of relative risk aversion below 2 with a central estimate near unity. We set $\eta = 2$, implying a Frisch elasticity of $1/\eta = 0.5$, following the calibration in Chetty et al. (2013). With $\sigma = 1.1$, the implied Hicksian elasticity is $1/(\eta + \sigma) \approx 0.32$, closely matching their preferred intensive-margin Hicksian estimate of 0.33. The disutility weight $\phi$ is normalized to unity, which follows from the first-order condition at the baseline: at the optimum, the marginal disutility of the last hour worked equals the marginal utility of the wage income it generates.[12] For the extensive margin, we calibrate $F$ using Seibold (2021), who estimates the willingness to pay for retirement leisure from bunching at pension eligibility thresholds. His estimates imply that workers value a year of retirement at approximately 5–10 percent of annual consumption; we set $F = 0.075$.

[12] We hold $\phi$ constant across ages. Allowing $\phi$ to grow—capturing the increasing cost of work at older ages (e.g., Gustman and Steinmeier, 1986, French, 2005)—would increase the disutility of labor and thus the welfare loss. We take the constant-$\phi$ specification as our baseline.

**Mapping estimates to the model.** Our empirical results show that the labor supply response operates along both margins. Panel A of Figure B.5 documents modest increases in weekly hours of approximately 0.5 hours during prime working ages, rising to 1–1.5 hours near retirement, relative to a baseline of roughly 32 hours per week. We express these changes as a fraction of baseline hours and map them into the intensive-margin disutility. Figure 3 shows that treated women delay retirement by 2–3 months at ages 60–68. We translate these shifts into expected additional months of employment at each age and value them at $F$.

**Results.** Panel B of Table 1 reports the welfare decomposition. The change in consumption utility ($-14.8$ percent) reflects the net consumption effect from Panel A, as evaluated by the concave CRRA utility function. The disutility of additional labor amounts to $-36.5$ percent of baseline annual utility, of which roughly three-quarters is due to the intensive margin and one-quarter to the extensive margin.

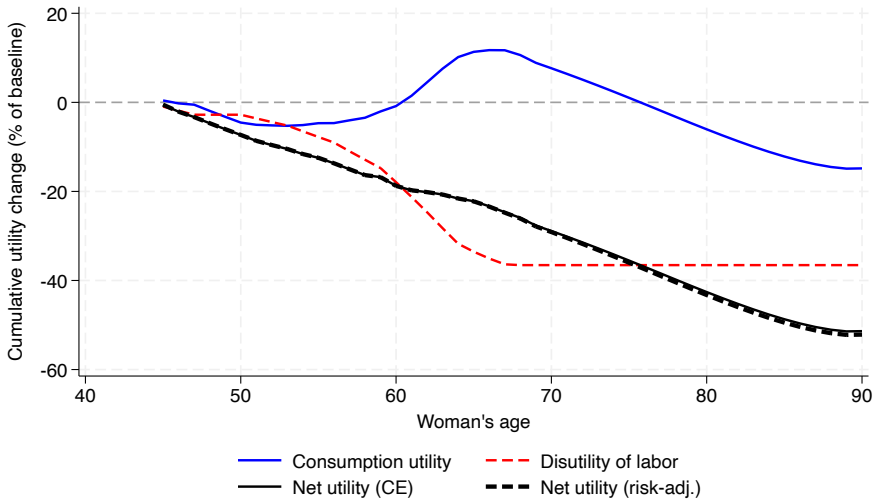
The intensive-margin disutility is large because the isoelastic disutility function with $\eta = 2$ is convex in hours: even modest increases (a 3 percent rise in weekly hours, from 32 to 33 hours) generate substantial welfare costs through the cubic curvature. We view this as an upper bound. Combining consumption utility and disutility, the certainty-equivalent net utility amounts to $-51.4$ percent.

**Insurance value.** The certainty-equivalent calculation feeds expected consumption—averaging across alive and widowhood states—into the CRRA utility function. By Jensen's inequality, this overstates welfare: $U(E[c]) > E[U(c)]$. To quantify the insurance value of survivor benefits, we compute utility separately in each state. In the alive state, consumption falls because treated women save into the wealth buffer. In the widowhood state, consumption falls sharply due to the benefit reduction, partially offset by the annuitized buffer.

The cost of increased consumption risk amounts to $-0.8$ percent of baseline annual utility, raising the net welfare loss from $-51.4$ to $-52.2$ percent under the structural specification. This insurance cost is modest because the wealth buffer partially narrows the consumption gap between states. Without the buffer, the consumption drop upon widowhood would be larger, and the insurance cost substantially higher. In this sense, the net cost of the wealth channel documented in Panel A ($-4$ percent in expected monetary terms) reflects the premium women pay for self-insurance; its benefit appears here as the modest insurance cost.

Figure 7 shows the evolution of welfare effects over the life cycle. Consumption utility from higher earnings is partially offset by the disutility of labor throughout

Figure 7: Utility effects of the reform over the life cycle




This figure shows the cumulative, survival-weighted net present value of utility changes induced by the reform, expressed as a percentage of baseline annual utility. The blue line shows the change in consumption utility from a CRRA function ($\sigma = 1.1$) applied to the state-weighted expected consumption change. The red dashed line shows the disutility of additional labor supply, decomposed into an intensive margin ($\phi = 1$, $\eta = 2$; hours from Figure B.5) and an extensive margin ($F = 0.075$ per year; retirement delay from Figure 3). The black solid line shows net utility (certainty equivalent); the black dashed line shows risk-adjusted net utility, incorporating the welfare cost of consumption variance across alive and widowhood states. Parameters: $r = 0.02$, $r_a = 0.03$, $s = 231$ EUR/yr. Bequest ($\theta = 0.5$) is included in the table but not in the figure.

working life, so that net utility is negative from the start. After age 65, the welfare loss accelerates as expected survivor benefit losses accumulate—the full annual reduction applies, and cumulative male mortality rises steeply. The disutility of labor stabilizes after the late 60s as earnings responses fade, but the insurance loss continues to grow.

**Bequest value.** If the woman dies before her husband, the wealth buffer is bequeathed. We model the bequest as valued at $\theta = 0.5$ of its consumption equivalent—that is, the woman values leaving one euro to her surviving husband at 50 cents.[13] The bequest contributes $+3.9$ percent of baseline annual utility, partially offsetting the insurance cost and reducing the net welfare loss from $-52.2$ to $-48.2$ percent (Table 1).

**Summary.** Our welfare simulation focuses on the treated woman's perspective, and does not account for potential gains to other members of the insurance pool. The reform reduces her welfare by approximately 48 percent of baseline annual utility under our structural specification.[14]

[13] This is a reduced-form warm-glow specification. The bequest is weighted at each age by the woman's conditional mortality probability and the probability that the husband is still alive.

[14] An alternative sufficient-statistics approach, which prices the intensive margin via the Harberger approximation $\frac{1}{2}\Delta h^2/\varepsilon^H$ with $\varepsilon^H = 0.33$ (Chetty et al., 2013), yields a substantially smaller disutility of labor ($-8$ percent instead of $-37$ percent) and a total welfare loss of approximately 20 percent.

### 6.3 Life Expectancy / health

In this section, we provide direct reduced-form evidence on welfare effects by estimating the impact of the reform on women's health outcomes and mortality. A growing literature documents a strong positive relationship between income and longevity.[15] If reduced survivor benefits increase financial or physical stress, affected women may experience adverse health outcomes. On the other hand, increased labor force participation may have beneficial effects through social engagement, which is not captured by the standard welfare framework in Subsection 6.2.

**To come:** We estimate mortality differences between women in the treated and control cohorts by computing the probability of death by various ages ($x = 50, 55, \ldots, 70$) and, separately, by stress-related causes of death. These estimates provide a non-monetary welfare measure that complements our model-based anayslsis and does not rely on its parametric assumptions.

## 7 Conclusion

[15] See, e.g., Chetty et al. (2016) for US evidence.

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# SUPPLEMENTAL APPENDIX

## A Data Appendix

Table A.1: Descriptive statistics

| | Cohort 1944 | | | | Cohort 1945 | | | |
|---|---|---|---|---|---|---|---|---|
| | 1990 | | 2000 | | 1990 | | 2000 | |
| | mean | sd | mean | sd | mean | sd | mean | sd |
| *Panel A: Women's characteristics* | | | | | | | | |
| Earnings (EUR 2010) | 18026.472 | 9983.488 | 19073.260 | 13876.216 | 17995.856 | 9886.681 | 19849.240 | 13780.580 |
| Employed | 90.767 | 28.950 | 80.315 | 39.762 | 91.210 | 28.316 | 82.397 | 38.085 |
| Weekly working hours | 32.713 | 8.499 | 31.662 | 12.108 | 32.683 | 8.538 | 32.143 | 11.783 |
| Establishment size | 316.465 | 856.433 | 324.801 | 1386.940 | 321.511 | 858.874 | 351.858 | 1493.063 |
| Years of schooling | 10.971 | 3.077 | 10.971 | 3.077 | 11.090 | 3.033 | 11.090 | 3.033 |
| Immigrant share | 0.084 | 0.278 | 0.084 | 0.278 | 0.090 | 0.286 | 0.090 | 0.287 |
| Number of children | 2.299 | 0.910 | 2.300 | 0.909 | 2.286 | 0.889 | 2.286 | 0.888 |
| Year of birth | 1944.000 | 0.000 | 1944.000 | 0.000 | 1945.000 | 0.000 | 1945.000 | 0.000 |
| *Panel B: Partner's characteristics* | | | | | | | | |
| Year of birth | 1941.424 | 3.964 | 1941.432 | 3.951 | 1942.500 | 3.854 | 1942.504 | 3.847 |
| Earnings (EUR 2010) | 28326.866 | 18874.421 | 24793.361 | 29631.391 | 28259.058 | 18285.554 | 26293.854 | 28533.284 |
| Observations | 42088 | | 41270 | | 42411 | | 41630 | |

*Note*: The table depicts the mean and standard deviation of selected variables for the birth cohorts 1944 and 1945 of our estimation sample. All values are depicted for the years 1990 and 2000. Earnings are winsorized at the 1st and 99th percentile and stated in 2010 Euros.

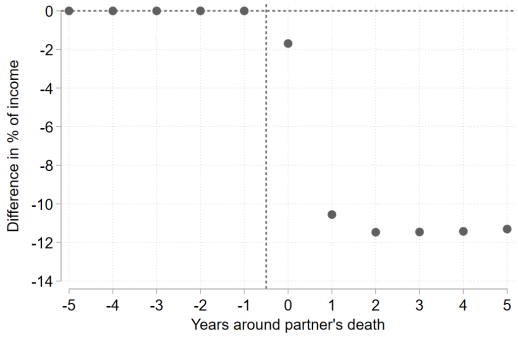
# B Additional results

Figure B.1: Survivor benefits as share of disposable income

(A.) Share of disposable income




(B.) Difference

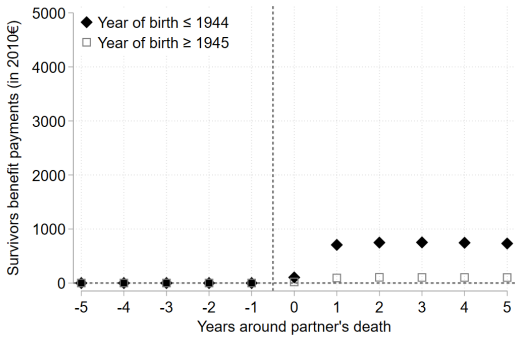



This figure plots the mean survivor benefits in absolute amounts (subfigure A) and the difference (subfigure B), both in % of disposable income. The figure includes all eligible married women in Sweden born in 1942–47, who are 65 years or older at the time of their spouses death.

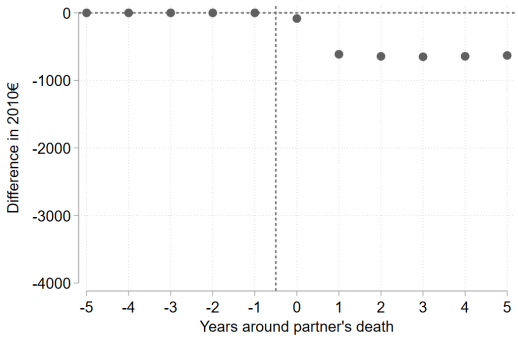
Figure B.2: Heterogeneity: Survivor benefits by within household difference in pensionable income

*Panel A: Difference in sum of pensionable income in 1989 $\leq$ 25th percentile*

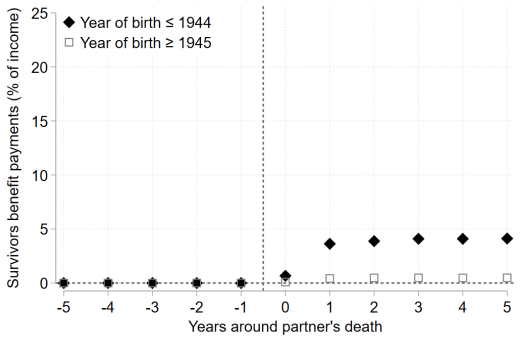
(A.) Absolute amounts



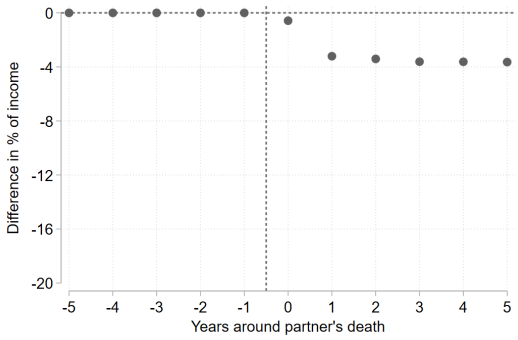

(B.) Difference




(C.) Share of disposable income

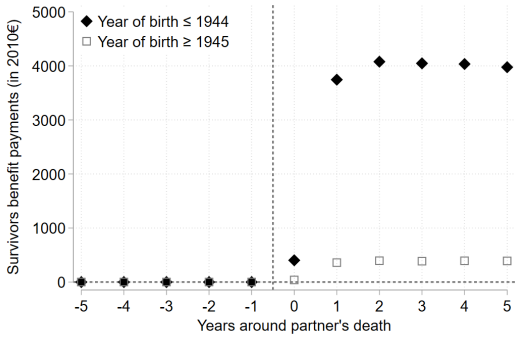



(D.) Difference

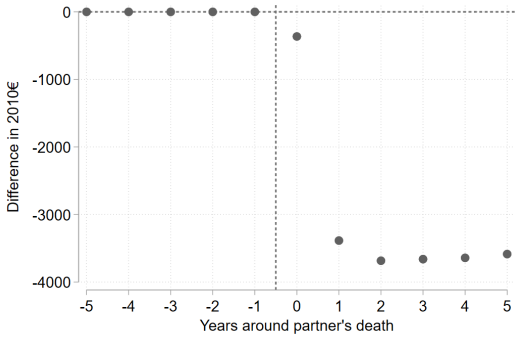



*Panel B: Difference in sum of pensionable income in 1989 $\geq$ 75th percentile*

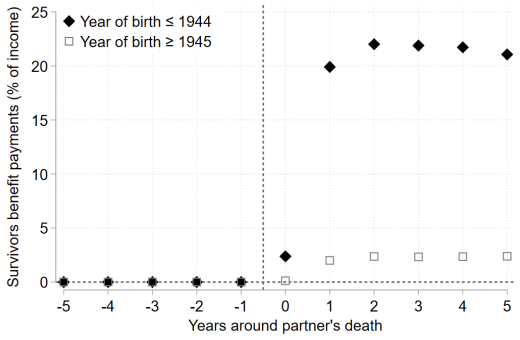
(E.) Absolute amounts



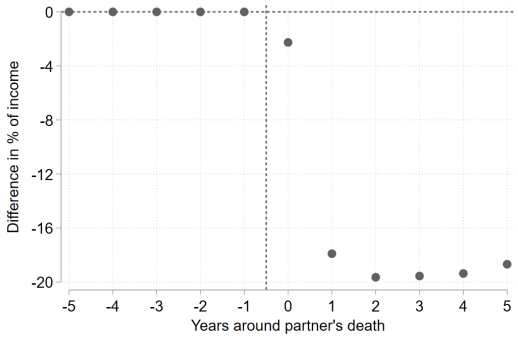

(F.) Difference




(G.) Share of disposable income



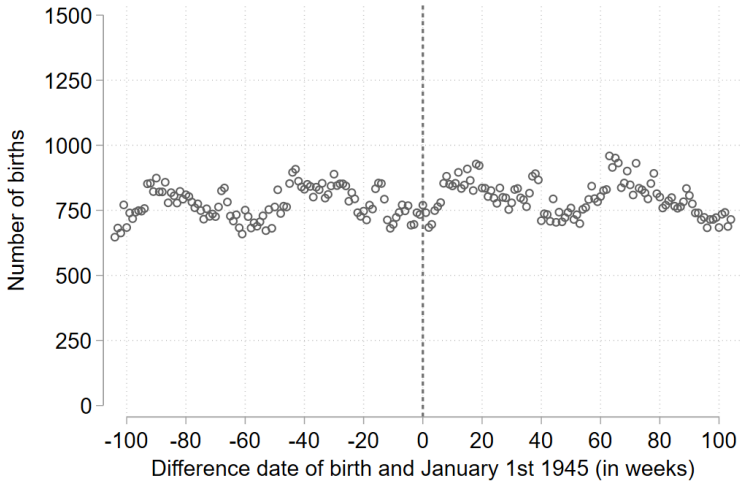

(H.) Difference





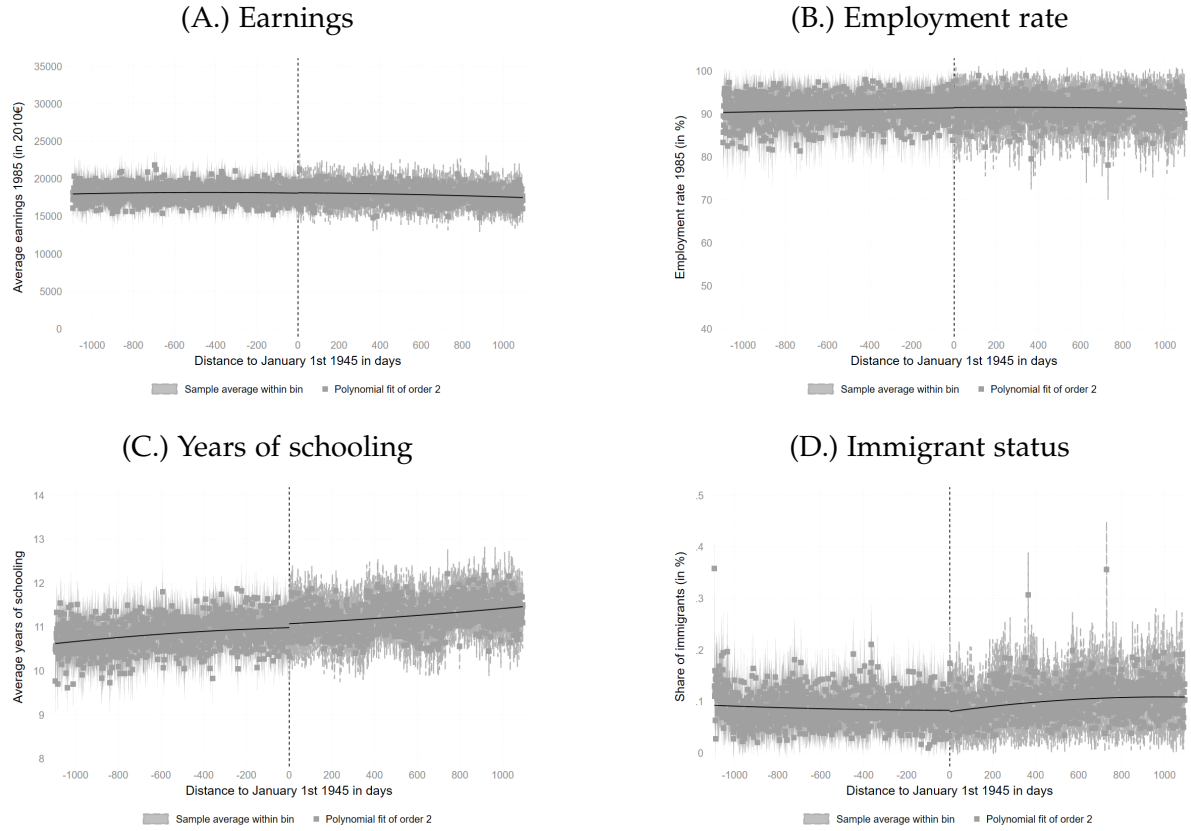

This figure plots the mean survivor benefits for different subgroups around a spouse's death. Panel A depicts the benefits payments for women whose within household difference in accumulated pensionable income in 1989 is at the 25th percentile or lower. Panel B depicts the benefits payments for women whose within

Figure B.3: No manipulation of the running variable




This figure shows the weekly number of births around January 1, 1945. The sample consists of women born 1942–47, married for at least five years as of December 31, 1989. The vertical dashed line indicates the cutoff date. No discontinuity in the density of the running variable is visible at the cutoff.

Figure B.4: Continuity of pre-treatment outcomes and predetermined characteristics at the cutoff



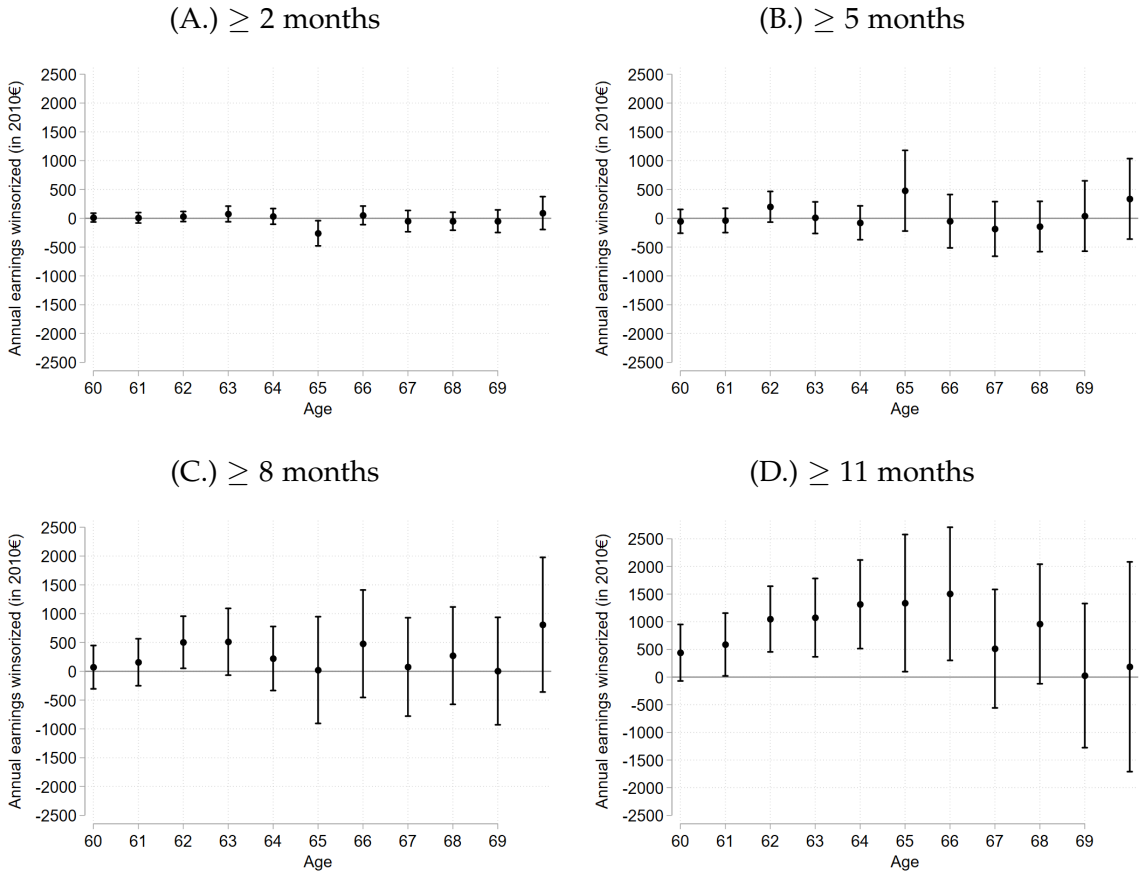

*Sample:* Eligible married women born 1942–47, married for at least five years as of January 1, 1990. Each panel shows an RD plot of a pre-treatment outcome against the running variable (date of birth in days relative to January 1, 1945). Panels A and B plot earnings and employment rates measured in 1985, five years before the reform. Panel C plots years of schooling and Panel D immigrant status both measured in 1990. None of the outcomes exhibits a discontinuity at the cutoff, supporting the identifying assumption that potential outcomes vary smoothly at the threshold.

Figure B.5: Causal effects of reduced survivor benefits on working hours and wages



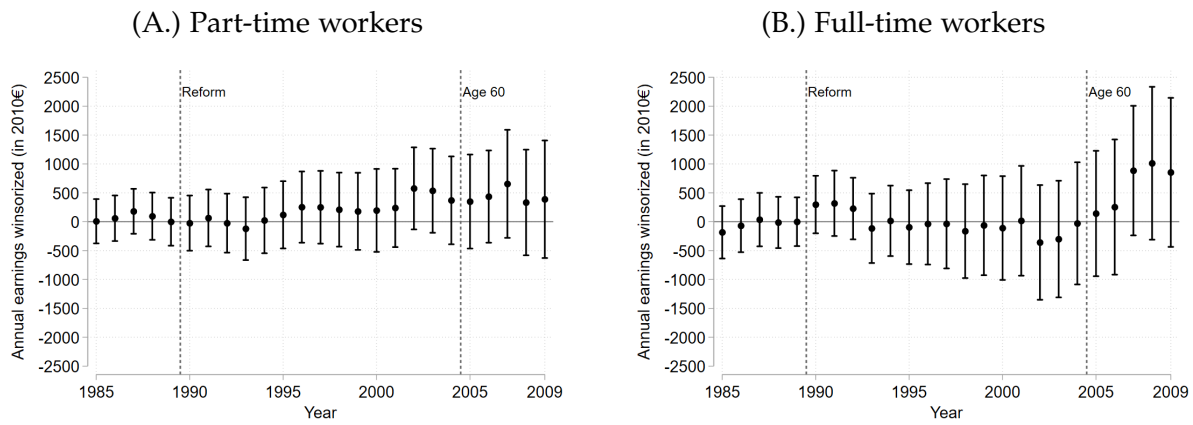

*Sample:* Women born 1942–47, married for at least five years as of December 31, 1989. *Method:* Robust RDD with optimal bandwidth (Calonico et al., 2014). Estimates show mean yearly differences in outcomes between women just above and below the January 1, 1945 cutoff; whiskers denote 95% confidence intervals. *Outcomes:* Panel A shows weekly working hours. Panel B shows monthly wages conditional on working (in 2010 Euros).

Figure B.6: Postponed retirement: sum of monthly earnings conditional on being employed



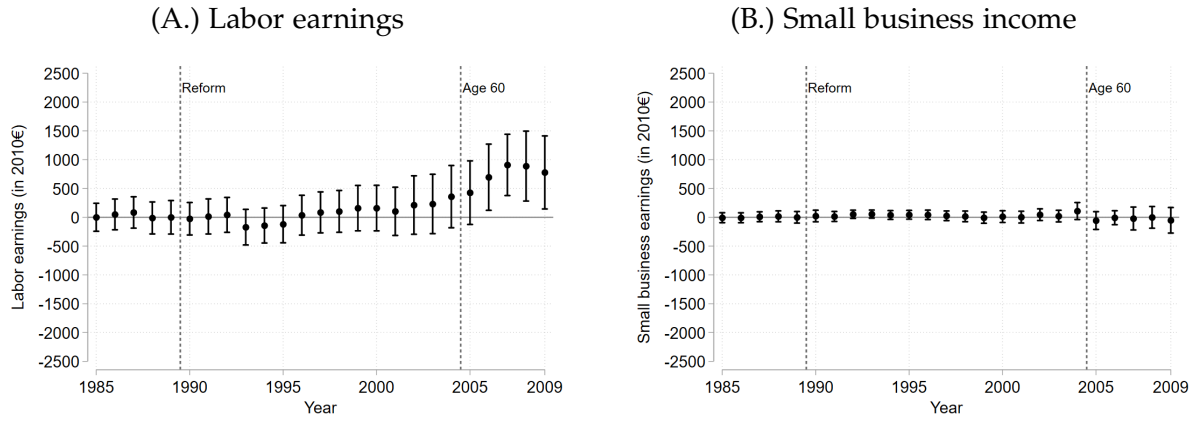

*Sample:* Women born 1942–47, married for at least five years as of December 31, 1989. *Method:* Robust RDD with optimal bandwidth (Calonico et al., 2014). Estimates show mean yearly differences in outcomes between women just above and below the January 1, 1945 cutoff with 95% confidence intervals. *Outcomes:* Sum of earnings for $x$ consecutive months following a woman's birthday, for ages 60–69 and conditional on being employed for $x$ consecutive months. Panel A: $x = 4$; Panel B: $x = 7$; Panel C: $x = 11$.

Figure B.7: Heterogeneity: Part-time vs full-time workers




*Sample:* Women born 1942–47, married for at least five years as of December 31, 1989 and for who information on weekly working hours in 1989 is available. *Method:* Robust RDD with optimal bandwidth (Calonico et al., 2014). Estimates show mean yearly differences in outcomes between women just above and below the January 1, 1945 cutoff, with 95% confidence intervals. *Outcomes:* Panel A reports the treatment effect on annual total earnings (EUR 2010) for part-time workers. Panel B reports the treatment effect on annual total earnings (EUR 2010) for full-time workers. Full-time workers are defined as women whose weekly working hours in 1989 are equal or larger than the full-time equivalent. Part-time workers are defined as women whose weekly working hours in 1989 are smaller than the full-time equivalent

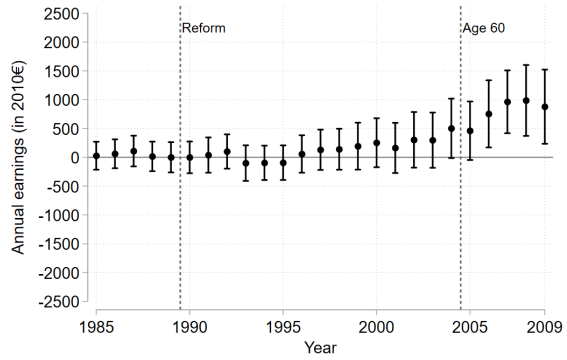
Figure B.8: Causal effects of reduced survivor benefits on labor earnings and small business income



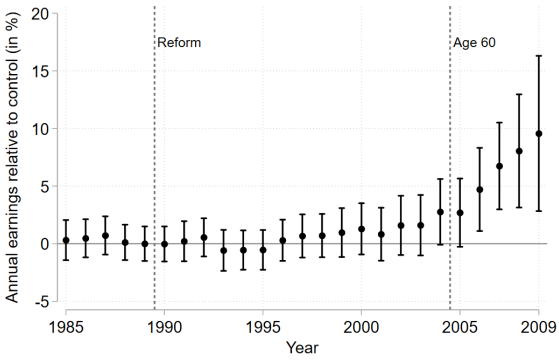

*Sample:* Women born 1942–47, married for at least five years as of December 31, 1989. *Method:* Robust RDD with optimal bandwidth (Calonico et al., 2014). Estimates show mean yearly differences in outcomes between women just above and below the January 1, 1945 cutoff, with 95% confidence intervals. *Outcomes:* Panel A reports the treatment effect on annual labor earnings (EUR 2010). Panel B reports the treatment effect on small business income (EUR 2010).

## Figure B.9: Sensitivity: No winsorizing

(A.) Absolute earnings



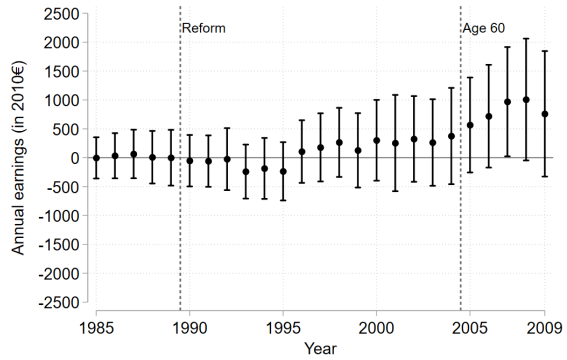

(B.) Relative earnings



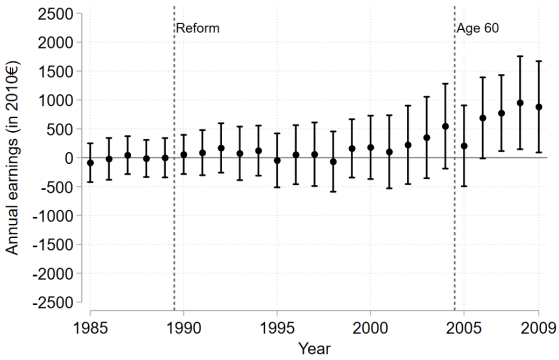

*Sample:* Eligible married women born 1942–47, married for at least five years as of January 1, 1990. *Method:* Robust RDD with optimal bandwidth (Calonico et al., 2014). Estimates show mean yearly differences in outcomes between women just above and below the January 1, 1945 cutoff with 95% confidence intervals. *Outcomes:* Panel A shows annual total earnings (EUR 2010) without winsorizing. Panel B shows the treatment effect relative to the mean earnings of the control group.

## Figure B.10: Heterogeneity: By shock size

(A.) Below median shock size

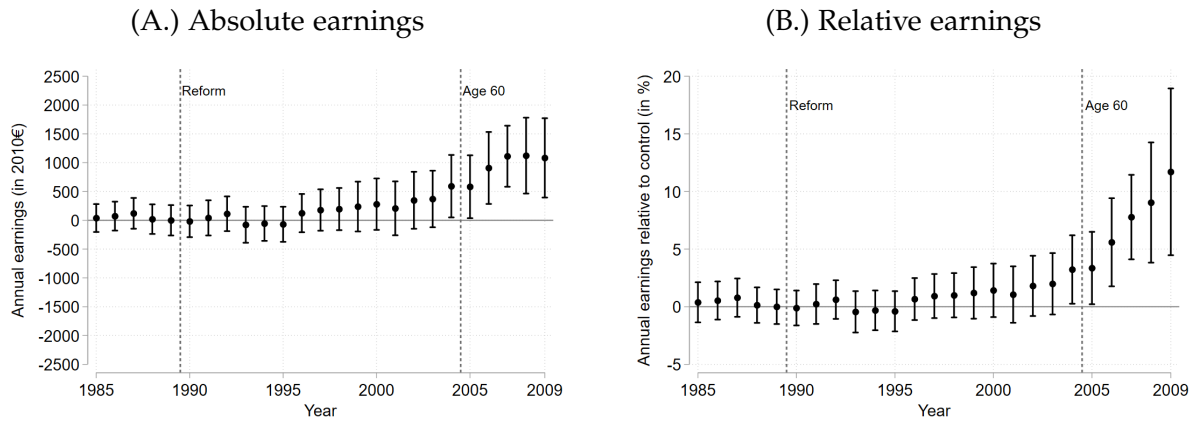



(B.) Above median shock size



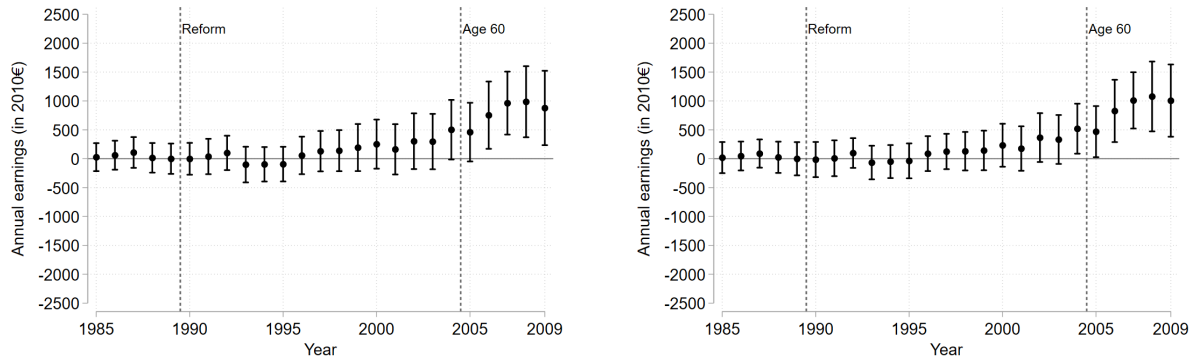

*Sample:* Eligible married women born 1942–47, married for at least five years as of January 1, 1990. *Method:* Robust RDD with optimal bandwidth (Calonico et al., 2014). Estimates show mean yearly differences in outcomes between women just above and below the January 1, 1945 cutoff with 95% confidence intervals. *Outcomes:* The dependent variable is the annual earnings in 2010 Euros. Panel A shows the treatment effect for women with a below median shock size. Panel B shows the treatment effect for women with a above median shock size. The shock size is approximated by the within couple difference in pre-reform accumulated pensionable income.

Figure B.11: Sensitivity: Excluding women with imminent partner death




*Sample:* Eligible married women born 1942–47, married for at least five years as of January 1, 1990, whose partner is alive in year $t + 3$. *Method:* Robust RDD with optimal bandwidth (Calonico et al., 2014). Estimates show mean yearly differences in outcomes between women just above and below the January 1, 1945 cutoff with 95% confidence intervals. *Outcomes:* Panel A shows annual total earnings (EUR 2010), winsorized at p1 and p99. Panel B shows the treatment effect relative to the mean earnings of the control group.

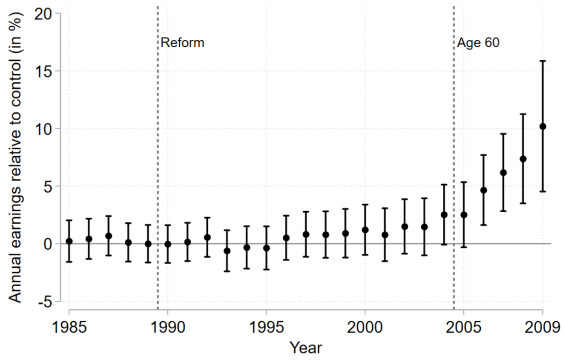
Figure B.12: Sensitivity: Controlling for the Swedish compulsory schooling reform



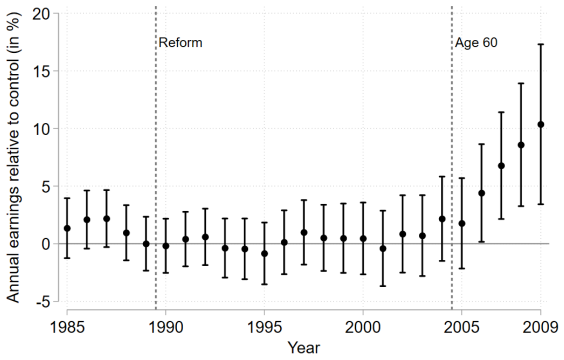

*Sample:* Eligible married women born 1942–47, married for at least five years as of January 1, 1990. *Method:* Robust RDD with optimal bandwidth (Calonico et al., 2014). Estimates show mean yearly differences in outcomes between women just above and below the January 1, 1945 cutoff with 95% confidence intervals. *Outcomes:* Panel A reproduces the baseline using annual total earnings (EUR 2010) without residualization. Panel B residualizes earnings by educational attainment (Sun2000 classification) to account for a compulsory schooling reform that affected cohorts born around the mid-1940s.

## Figure B.13: Sensitivity: Alternative bandwidths

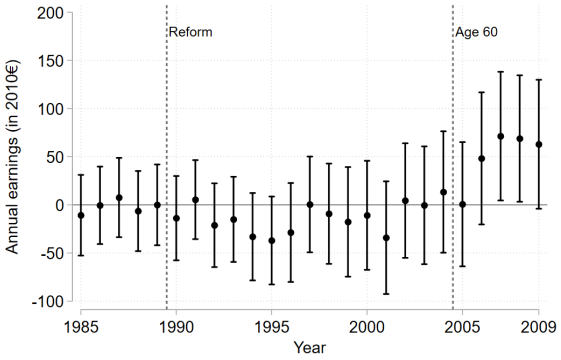
(A.) 1-year bandwidth




(B.) 6-month bandwidth



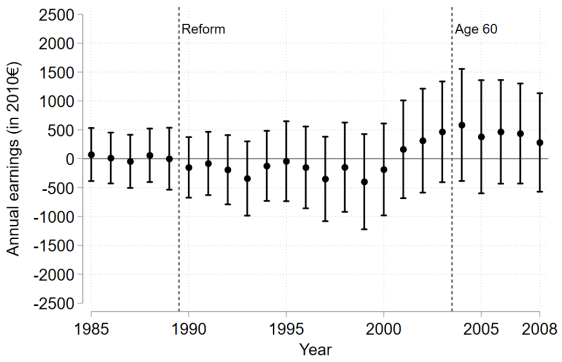

(C.) June earnings, 5-month bw.



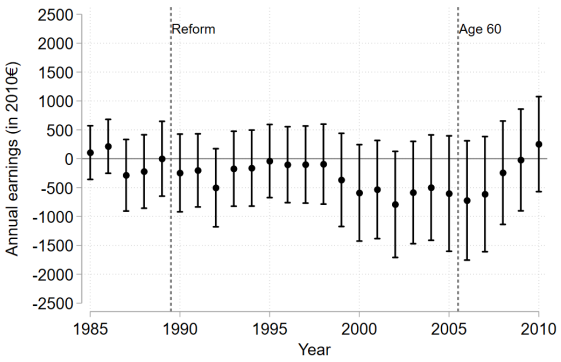

*Sample:* Eligible married women born 1942–47, married for at least five years as of January 1, 1990. *Method:* Robust RDD with fixed bandwidth. Estimates show mean yearly differences in outcomes between women just above and below the January 1, 1945 cutoff with 95% confidence intervals. *Outcomes:* Panels A and B show relative annual earnings (EUR 2010) using fixed bandwidths of one year and six months, respectively. Panel C shows earnings in June only with a five-month bandwidth.

## Figure B.14: Sensitivity: Placebo RDD at adjacent-year cutoffs

(A.) Placebo cutoff: January 1, 1944



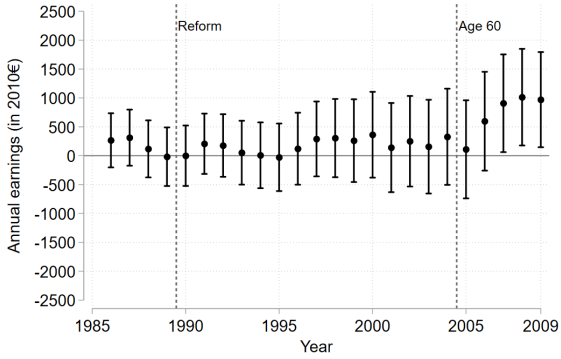

(B.) Placebo cutoff: January 1, 1946




*Sample:* Eligible married women born 1942–47, married for at least five years as of January 1, 1990. *Method:* Robust RDD with optimal bandwidth (Calonico et al., 2014). Estimates show mean yearly differences in outcomes between women just above and below the respective cutoff with 95% confidence intervals. *Outcomes:* Panel A estimates the RDD at the January 1, 1944 cutoff; Panel B at the January 1, 1946 cutoff. Neither year features a discontinuity in survivor benefit generosity.

## Figure B.15: Sensitivity: Difference-in-Discontinuities

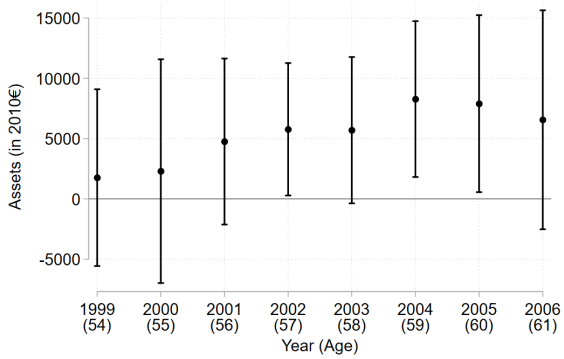
(A.) Total earnings



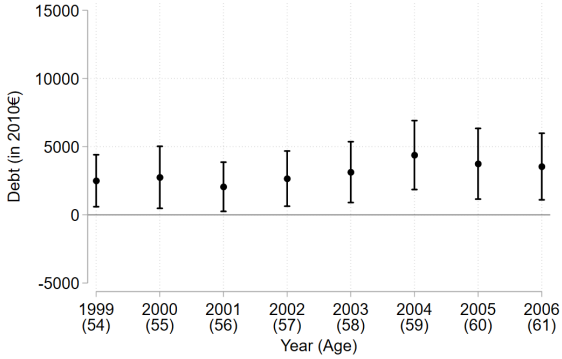

*Sample:* Eligible married women born 1942–47, married for at least five years as of January 1, 1990. *Step 1:* Estimate RDD with a bandwidth of 180 days left and right around January 1st of 1944, 1945, and 1946, separately. *Step 2:* Compute difference between the 1945 RDD estimate and the average of the 1944 and 1946 estimates. Standard errors are constructed applying the delta method. Estimates show mean yearly differences in outcomes between women just above and below the respective cutoff with 95% confidence intervals. *Outcomes:* Panel A reports annual total earnings (EUR 2010).

## Figure B.16: Sensitivity: Effects on wealth without winsorizing

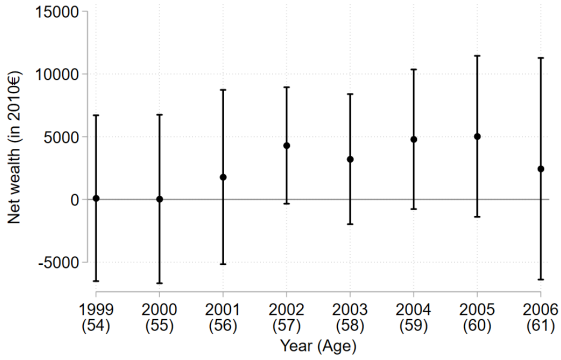
(A.) Assets




(B.) Debt




(C.) Net wealth




*Sample:* Eligible married women born 1942–47, married for at least five years as of January 1, 1990. *Method:* Robust RDD with optimal bandwidth (Calonico et al., 2014). Estimates show mean yearly differences in outcomes between women just above and below the January 1, 1945 cutoff with 95% confidence intervals. *Outcomes:* Panel A shows total assets (EUR 2010) without winsorizing. Panel B shows total debt (EUR 2010) without winsorizing. Panel C shows net wealth, defined as assets minus debt (EUR 2010), without winsorizing. Wealth data are available for 1999–2006.