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ABSTRACT

Unearned Income and Labor Supply: Evidence from Survivor Pensions in Austria*

We study the effect of lower unearned income on labor supply. To identify the causal effect of an unexpected reduction in unearned income, we exploit a policy reform that lowered survivor pensions in Austria. Men widowed after the survivor pension reform received an approximately 34% lower survivor pension than men widowed before the reform. We follow the employment history of both groups for 150 months and estimate the reform's effect on labor supply using a regression discontinuity design. The effect of the lower pension is evident immediately after the death of their spouse, is persistent over time, becomes more pronounced over time, and is robust across model specifications. Our baseline result suggests a 3.5 to 5.4 percentage point higher employment rate for survivors in the low pension regime in the long run. The estimated effect corresponds to a labor supply elasticity at the extensive margin with respect to the changes in total income of about -0.9 to -1.3.

JEL Classification: I38, J22, J48

Keywords: labor supply, unearned income, regression discontinuity design

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1 Introduction

The elasticity of labor supply with respect to unearned income is a fundamental parameter of the economic theory of the labor market.[1] The magnitude of this elasticity is important for economic policy, as it allows researchers to assess the degree to which labor supply is affected by taxes and transfers such as universal basic income (Marinescu, 2018). Since it is empirically difficult to identify income effects, our knowledge of this elasticity is still patchy and the estimates of how unearned income affects labor supply vary widely. For example, Imbens, Rubin and Sacerdote (2001) report that lottery winners reduce their labor earnings by 11 cent per dollar won in the lottery. Gruber (2000) reports that an 8.8 percentage point increase in disability benefits results in a 2.7 percentage point lower employment rate.[2] Autor, Duggan, Greenberg and Lyle (2016) exploit an eligibility expansion to disability benefits for Vietnam veterans and estimate an income elasticity of labor force participation with respect to the changes in total income of -0.49, and an earnings reduction of 26 cent per dollar. Giupponi (2019) estimates that survivor pension recipients in Italy increased their earnings by the same amount by which pensions were reduced after a reform in 1995. Her estimate is largely driven by the extensive margin labor supply responses of women (e.g., moves from non-employment to employment). Fadlon, Ramnath and Tong (2019) report a participation elasticity of labor supply of about -0.35 as a result of becoming eligible for the United States survivor pension scheme at the age of 60. However, because the United States survivor pension scheme imposes strict rules on the maximum annual earnings above which the pension is additionally taxed, their estimated effect is a combination of an income effect and a substitution effect.[3]

[1] Two recent reviews in the large body of literature on labor supply and taxes are Keane (2011) and Saez, Slemrod and Giertz (2012).

[2] Gruber (2000) estimates a non-participation elasticity with respect to disability insurance benefits between 0.28 and 0.36.

[3] Between the age of 60 and full retirement age, the United States Social Security Administration deducts \$1 from a survivor pension for every \$2 of earnings above an earnings limit of \$18,240. From the full retirement age onward, it deducts \$1 for every \$3 above an earnings limit of \$48,600 (Social Security Administration, 2020).

We estimate the labor supply responses of men who receive a survivor pension after the death of their spouse to the changes in unearned income. In Austria, a survivor pension is a permanent source of unearned income for surviving spouses of all ages and this creates an ideal environment to assess its impact on the long-run labor supply of able-bodied adults who participate in the labor market.

We identify the causal effect of the unexpected change in unearned income by exploiting a reform of survivor pensions that took place on 1 October, 2000. Men widowed before the reform received on average €304 per month as their survivor pension and men widowed thereafter received on average €201 per month, 34% less.[4] At the time of their spouses' deaths, this implied on average a 4.8% lower monthly income for post-reform survivors (i.e., the sum of gross labor earnings and the gross survivor pension).[5]

We use a sample of male survivors widowed between April 1998 and March 2003, an interval of 60 months around the survivor pension reform of October 2000. We follow their employment histories for 150 months after widowhood and analyze the changes in employment between the two groups. One advantage of the survivor pension scheme is that survivors do not select themselves purposefully into the program. Purposeful selection is a well-known confounder in the policy evaluation of disability and unemployment benefit recipients, as it changes the composition of the treatment and control groups (Autor and Duggan, 2003; Mullen and Staubli, 2016).

Our estimations show that post-reform survivors, who received lower pensions, had higher employment rates and earned more in the years after the death of their spouse than pre-reform survivors. In the baseline model, we estimate that this lower pension led to a higher employment rate of about 3.5 to 5.4 percentage points (or 4.2% to 6.4% from the baseline employment rate). This finding suggests an extensive margin labor supply elasticity with

[4] Pensions expressed in 2005 prices.
[5] We have no information on income from capital.

respect to total income — caused by a change in unearned income — of −0.9 to −1.3. The results summarized in Marinescu (2018) suggest that the largest adjustment to the changes in unearned income is along the intensive margin, especially for small changes. However, our data do not provide working hours. As a proxy for hours worked, we analyze gross labor earnings. Post-reform widowers earned between €63 and €121 more per month over the 150 months following the death of their spouse than pre-reform widowers, which implies a marginal propensity to earn out of unearned income of −0.6 to −1.2. The earnings effect is due to responses at the extensive margin, not the intensive margin. If we only compare survivors who are employed, the earnings effect disappears.

When we divide the sample based on the median age of about 43 years of age, the estimates indicate that widowers below the age of 43 reacted more strongly to differences in survivor pensions than widowers between the ages of 43 and 50. Younger widowers reduced their labor supply by 7.9 percentage points and older widowers by only 2.7 percentage points.

Our results are not only relevant for the design of survivor pensions, but also extend to other areas of social security. Whenever (unconditional) transfers change people's options (e.g., income support and child benefit), policymakers are concerned about their labor market responses. Our results suggest that an increase in survivor pensions increases the reservation wage by about the same amount, which implies that transfers affect labor supply considerably. There is some debate on why the elasticities of labor supply in microeconomics studies, as is the case here, are typically higher than those found in macroeconomic studies. Estimates from quasi-experiments allow us to assess how the reservation wage reacts to transfers (Chetty, Guren, Manoli and Weber, 2011) and it is the reservation wage that reflects the opportunity cost of work. (The extensive margin of labor supply in macroeconomic models is often calibrated to match macroeconomic moments.) The reservation wage therefore depends critically on the social security or welfare benefit available to a person as a supplement (or alternative) to labor income.

2 Background

2.1 Legal Background

The Austrian social security system grants survivor pensions to surviving spouses and orphans. A surviving spouse is entitled to a survivor pension if the deceased paid social security contributions for at least 180 months during the last 360 months, with exceptions for early and occupational deaths.[6] If the deceased was 50 or younger, entitlement requires at least 60 months of social security contributions during the last 120 months.[7] If the deceased was 27 or younger, six months of paid contributions are sufficient. Survivor pensions after occupational deaths do not require a minimum number of months of paid contributions. If the deceased's number of contribution months is insufficient for a survivor pension, the surviving spouse receives a severance payment instead.[8]

Surviving spouses in childless marriages receive a pension either for a temporary period of 30 months or an indefinite period, depending on their duration of marriage, their age difference, the deceased's age at marriage, and whether the deceased was a pensioner at the time of the marriage.[9] If the spouses had children, a survivor pension is always granted for an

[6] Entitlement is also established if the deceased paid social security contributions (i) for at least 180 months if the deceased was employed, paid voluntary self-insurance, or was in education or training (up to 72 months) or (ii) for 300 months if the person was unemployed, sick, on parental leave (up to 48 months per child), or in military service.

[7] If the deceased was older than 50, one additional month of paid contributions is required for each month the deceased was older than 50. The period over which months are counted increases by two months for every month the deceased was older than 50. For example, if the deceased was 50 years and one month, then 61 months during the last 122 months are required.

[8] The severance payment requires at least one month of paid contributions and amounts to a maximum of six monthly salaries.

[9] A temporary pension (30 months) is granted if:

1. The survivor is younger than 35 and the marriage lasted for less than 10 years.
2. The survivor is older than 35 and the deceased was a pensioner at the time of the marriage and
   (a) The spouses' age difference is less than 20 years and the marriage lasted for less than three years,
   (b) The spouses' age difference is between 20 and 25 years and the marriage lasted for less than five years, or
   (c) The spouses' age difference is more than 25 years and the marriage lasted for less than 10 years.

indefinite period.[10] If a survivor marries anew, the entitlement to a survivor pension ends, but the survivor will receive a final payment of 35 monthly pensions. If the new marriage is dissolved, the survivor may resume the survivor pension after a waiting period of 30 months. A survivor's imprisonment results in the suspension of the survivor pension for the duration of the sentence if the sentence is longer than one month.[11]

After the spouse's death, a survivor has to file a claim with the pension insurance. If the claim is filed within six months of the spouse's death, a pension is paid from the day following death onward. If the claim is filed later, a pension is paid from the day following the application onward. A pension is paid monthly, 14 times a year, with a double payment in April and October. The amount is adjusted for inflation every January using the consumer price index, with the exception of the first year after the spouse's death. A survivor pension automatically insures the recipient with social health insurance and 4% of the pension is retained in social security contributions.[12] In Austria, a progressive income tax also applies to survivor pensions.

The legal system sees a survivor pension as a share of the deceased's pension. If the deceased died before retirement, the survivor pension is based on the disability pension which the deceased would have received at the time of death. If the deceased died after reaching the statutory retirement age, the survivor pension is based on the deceased's old age pension. The share of the deceased's pension is based on a comparison of the so-called "contribution ratio", defined as the difference between the survivor's and the deceased's previous social security contributions. The contribution ratio is the ratio of the mean of the survivor's 180

3. The survivor is older than 35 and the deceased was above the standard pensionable age without receiving a pension at the date of marriage, and the marriage lasted for less than two years.

[10] The presence of children includes pregnancy on the day of death, adopted children, and children eligible for an orphan's pension living together with the surviving spouse.

[11] Since 2010, a (survivor) pension is also not suspended if a (survivor) pension recipient is under electronically monitored house arrest (§89 ASVG).

[12] The 4% is based on the legal rules in 2000. In 2020, the health insurance contribution rate for pensioners is 5.1%.

months with the most social security contributions to the mean of the deceased's 180 months with the most social security contributions. If contributions were paid for fewer than 180 months, the average of the available months is used.[13]

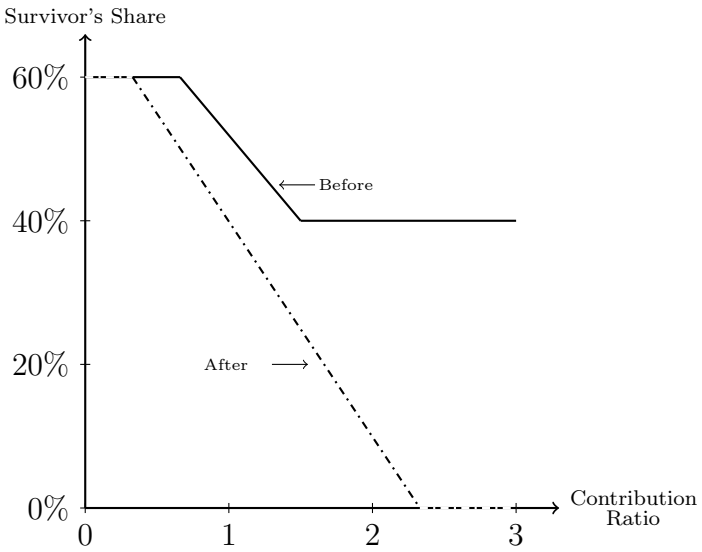
In October 2000, the formula that maps the contribution ratio into a survivor pension was modified for survivors whose pension was granted on or after 1 October, 2000; in other words, the pensions of pre-reform survivors did not change. Before October 2000, a survivor received a 52% share from the pension of the deceased if the contribution ratio equaled one (i.e., the spouses had equal earnings). For every percentage point the survivor earned more than the deceased, the share of the survivor decreased by 0.24 pp, and vice versa. The minimum share of a survivor was 40% and the maximum share was 60%. After October 2000, a survivor received a share of 40%, if the contribution ratio equaled one. For every percentage point the survivor earned more than the deceased, the share decreased by 0.30 pp, and vice versa. The minimum share was lowered to 0%, but the maximum share remained at 60%.

Figure 1 illustrates the impact of the reform on the share for different contribution ratios. We plot the resulting shares for both regimes (before and after). The reform shifted the shares downward, except for survivors with a contribution ratio below 1/3 on the left of the figure. The higher the contribution ratio, the stronger was the decrease in the resulting share. Survivors with a contribution ratio higher than 7/3, on the right of Figure 1, received 0% after the reform, while they would have received 40% beforehand.

Two refinements protect survivors from having to rely on a very low income after the death of their spouse. If a survivor's total monthly income, including the survivor pension, is below a threshold, then the survivor pension increases at most until (i) the survivor's share

[13] This describes the rules in 2000, which have since changed. Between 1 January, 2003 and the 1 January, 2004, the contribution ratio was based on a range of months between 180 and 216, which depended on a person's age. Between 1 January, 2004 and 1 July, 2004, it was based on the 480 months with the highest contribution payments. Since 1 July, 2004, the contribution ratio has been based on income in the two years before death. Since 1 January, 2006, the last two reference years of the deceased can be extended to four years if the deceased was sick and if it is favourable for the survivor.

Figure 1: Survivor's share, before and after October 2000.



Note: The two lines indicate the shares of the deceased's pension to which a survivor is entitled depending on the contribution ratio. The contribution ratio is the mean of the survivor's 180 months with the highest social security contribution to the mean of the deceased's 180 months with the highest social security contribution. Before October 2000, the entitlement was between 40% and 60% of the deceased's pension (solid line). After October 2000, the entitlement was between 0% and 60% (the dashed line).

is 60%, or (ii) the survivor's total income is equal to the threshold. Before October 2000, the monthly minimum income threshold was €1,231 and the reform increased it to €1,453, a rise of 18%.[14] The second threshold is the minimum income for Austrian retirees. If a survivor's total monthly income from a survivor pension and other income, after raising the survivor's share to 60%, is below the second threshold, then the pension increases until total income equals the second threshold. The value of the second threshold was €604 in 2000 and this was not reformed.[15]

Autor and Duggan (2007) argue that placing ceilings on earned income in disability insurance schemes may also generate substitution effects.[16] In October 2000, a maximum income threshold for survivors was introduced. If a survivor's total monthly income exceeded €6,279,

[14] The threshold can be higher if a 60% share of the deceased person's pension is above the threshold. In this case, the 60% share of the deceased person's pension is used instead of the threshold. The threshold value for 2020 is €2,031.

[15] The minimum income for retirees is paid 14 times per year. For 2020, the threshold is €966.65.)

[16] To separate the income effect from the substitution effect, Autor and Duggan (2007) study the United States' disability compensation program for veterans in which there is no maximum threshold on the income a recipient is allowed to earn and the benefits are also exempt from the income tax.

the survivor pension was reduced by €1 for every €1 a survivor's total income exceeded the threshold (i.e., an implicit tax rate of 100%). However, the threshold was about 3.5 times higher than mean earnings and few recipients were affected.[17]

2.2 Economic Background

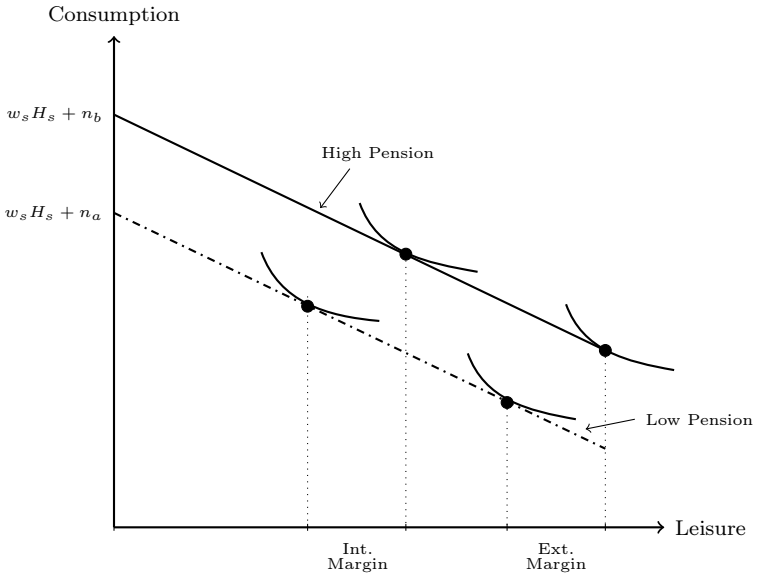
We illustrate the economic context of the reform using a standard model of labor supply (e.g., Cahuc, Carcillo and Zylberberg, 2014). Let $U(C, L)$ be a utility function, where argument C is income spent on consumption and argument L is time devoted to leisure, and both goods are normal goods. Let H be time devoted to labor and T be total time, $T = L + H$. Let w be the real wage rate per unit of time and n be unearned income. Total income is $wH + n$ and the budget constraint is $C \leq wH + n$. The reform, ceteris paribus, lowered n for most survivors. This model is defined with the following equations:

$$\begin{aligned} \max_{C,L} \quad & U(C, L) \\ \text{s.t.} \quad & C \leq wH + n \\ & T = H + L, \end{aligned} \tag{1}$$

where maximization results in the optimal choices C^* and L^*. Under standard assumptions, the model predicts that a reduction of n leads to an increase in working hours (intensive margin). For some, lowering n might result in a move from non-employment to employment (extensive margin). Figure 2 depicts this model. The solid line is the budget constraint for survivors widowed before the reform (high pension). The dashed line is the budget constraint for survivors widowed after the reform (low pension). The reform thus shifted the budget constraint downward.

[17] The maximum threshold increases on an annual based on the change in average social security contributions between $t-3$ and $t-2$. Between 2000 and 2013, the maximum threshold was based on twice the maximum social security contribution ceiling (§264/6a ASVG). Since 2013, the value has been fixed at €8,460.

Figure 2: Lower Unearned Income Increases Labor Supply.



Note: This figure illustrates the effect of the survivor pension reform in the context of a textbook model of labor supply. A survivor widowed before the reform, the solid line, receives a high pension n_b. A survivor widowed after the reform, the dashed line, receives a low pension n_a. The model predicts that less unearned income results in a greater supply of labor.

3 Data

3.1 Sample

We construct a sample of survivor pension recipients from the Austrian social security database and Austrian pension insurance database. The social security database contains information on a survivor's ID, sex, birth date, start and end dates of the survivor pension, employment history before and after widowhood, and death date (Zweimüller, Winter-Ebmer, Lalive, Kuhn, Wuellrich, Ruf and Büchi, 2009). The pension insurance database contains information on a survivor's ID, survivor pension size, the date on which the survivor pension was granted, whether the pension was granted for a temporary period, and if the pension is paid to a survivor who does not live in Austria (Sozialministerium, 2019). We select male recipients whose survivor pension started between April 1998 and March 2003, 30 months

before and after the survivor pension reform in October 2000.[18] We restrict the sample to survivors widowed before age 50. We exclude survivors who claimed their survivor pension late, who revived their survivor pension, who were widowed multiple times, who did not live in Austria, who were disabled at the time of widowhood, and who are civil servants.[19]

3.2 Descriptive Evidence

Figure 3 shows the distributions of the survivor pensions for pre-reform and post-reform survivors. On the left, we plot the survivor pension amount against time. The dots indicate individual survivor pension amounts, while the black solid line is a smoothed trend for the before and after reform period separately. On the right, we plot the distributions of the survivor pensions. The reduction in the average survivor pension amount is 34%. In the month of their spouses' death, widowers widowed after the reform have a 4.8% lower income (sum of gross labor earnings and the gross survivor pension) than those widowed before the reform.

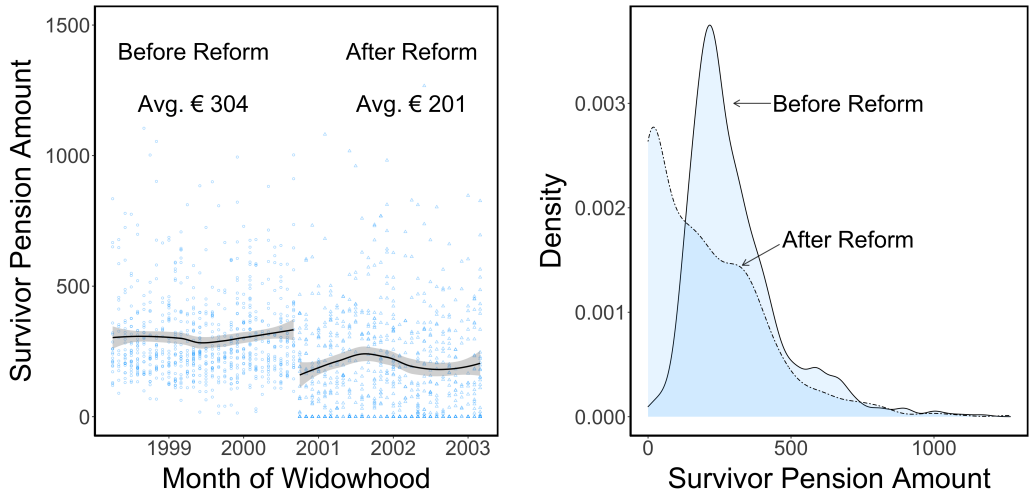
Table 1 shows the summary statistics of our sample. We observe 790 survivors before the reform and 736 thereafter.[20] Both groups are on average 42 years old at widowhood, 41.3%

[18] Social security distinguishes between the date of the spouse's death, the date on which the survivor becomes entitled to a survivor pension, the date on which the survivor pension is officially granted and calculated by the pension authority, and the date on which the first survivor pension payment is transferred. To calculate a survivor pension, the national pension authority uses the legal rules valid on the first day of the month after the spouse's death; only if the spouse died on the first day of a month does the pension authority use the legal rules valid on that day (§86 ASVG and §223 ASVG). The date of the first payment is about two months after the spouse's death. The first payment includes a retroactive payment back to the first day after the spouse's death. From then onward, the survivor pension is paid monthly. If an application is filed more than six months after the date of the spouse's death, the date of the spouse's death is replaced by the date of the application. Survivors whose spouse died on or before 1 September, 2000 were granted the survivor pension under the before reform policy regime. Survivors whose spouse died later were granted a survivor pension under the after reform policy regime. The law (BGBl. 101/2000, pp. 1092–1093) clearly states the rules for survivor pensions in September 2000.

[19] We exclude civil servants because we lack their earnings data before 2004. However, the results on the extensive margin do not change if we include them.

[20] Figure A1 in the Appendix shows the number of selected survivors between March 1998 and April 2003.

Figure 3: Survivor pensions, before and after October 2000.



Note: The figure plots the monthly survivor pensions before and after the reform. The mean group difference in the monthly survivor pension amount is €103. This amounts to a 34% reduction in survivor pension income.

and 41.3% are blue collar workers, 26.1% and 28.3% are white collar workers, 11.6% and 8.8% are self-employed, and 4.9% and 4.8% are farmers.[21]

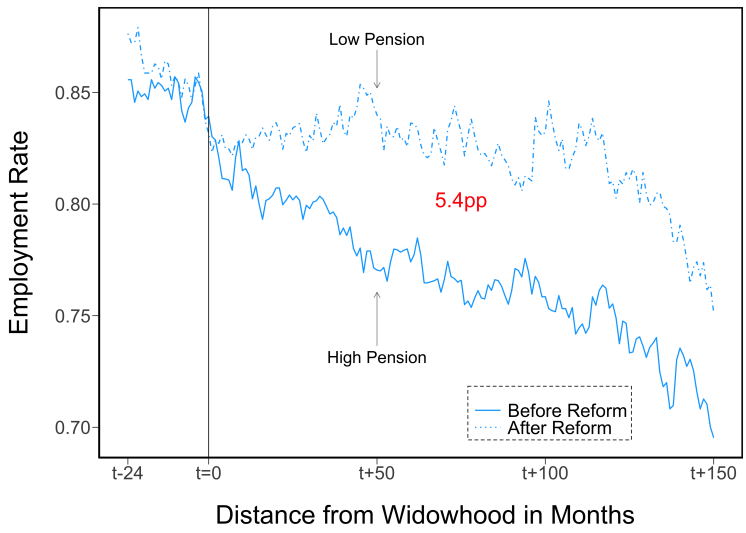
About 5.4% and 6.4% of survivors died within 150 months of widowhood, 16.6% and 15.5% retired, and 23.5% and 26.2% of survivor pensions ended for another reason (e.g., temporary pension grant, remarriage). The employment rate in the month of widowhood is 83.9% for the group before the reform and 83.1% for the group after the reform. From the month of widowhood onward, we adjust the employment rate for attrition. In each month, we drop a widower from the risk set if he died, retired or if the survivor pension ended. Over time, the employment rate for survivors in the high pension regime decreases faster. Gross labor earnings are €1,802 and €1,808 in the month of widowhood. While the gross labor earnings of survivors in the high pension regime decrease over time, those of survivors in the low pension regime increase.[22]

[21] It is possible that a survivor has more than one employment in a month. We use the status with the longest duration to assign a unique labor market status. If there are more statuses with the same duration in a month, we use the following hierarchy, (1) blue collar worker, (2) white collar worker, (3) self-employed person, and (4) farmer.

[22] This metric includes survivors not employed with €0 earnings. Conditional on being employed, no gross

Figure 4 shows the employment rate from 24 months before widowhood to 150 months thereafter. Around the month of widowhood, the employment rate falls sharply. While the employment rate for those in the high pension regime continues to decrease, that for those in the low pension regime remains on a higher trajectory. The average employment rate difference after widowhood is 5.4 percentage points.

Figure 4: Employment rate over time.



Note: This figure shows the employment rate for the before-reform group ("high pension") and after-reform group ("low pension"). It covers 24 months before widowhood and 150 months thereafter. The employment rate decreases faster for survivors with a high pension. The raw average difference after widowhood is 5.4 percentage points. We account for attrition over time.

earnings difference emerges between the groups over time.

Table 1: Summary statistics.

		Before Reform	After Reform	Difference	P-Value
Monthly Survivor Pension		304	201	−103	0.00
Employed (%)	in month $t = 0$	83.9	83.1	−0.8	0.68
	in month $t + 12$	81.3	82.9	+1.6	0.42
	in month $t + 24$	80.4	83.1	+2.7	0.20
	in month $t + 36$	79.5	83.1	+3.6	0.10
	in month $t + 96$	75.9	81.0	+5.1	0.04
	in month $t + 150$	69.5	75.1	+5.6	0.07
Earnings (€)	in month $t = 0$	1802	1808	+6	0.92
	in month $t + 12$	1793	1831	+38	0.54
	in month $t + 24$	1756	1855	+99	0.12
	in month $t + 36$	1787	1902	+115	0.09
	in month $t + 96$	1723	1899	+176	0.03
	in month $t + 150$	1545	1794	+249	0.04
Age at Widowhood (years, $t = 0$)		42.2	42.1	−0.1	0.66
Blue Collar (%, $t = 0$)		41.3	41.3	±0	0.99
White Collar (%, $t = 0$)		26.1	28.3	+2.2	0.34
Self Employed (%, $t = 0$)		11.6	8.8	−2.8	0.07
Farmers (%, $t = 0$)		4.9	4.8	−0.1	0.87
Days worked ($t - 150$ to $t - 1$)		3785	3798	+13	0.84
Benefit Duration (months, $t + 150$)		121.4	118.3	−3.1	0.21
Attrition Rate (%, in $t + 150$)		45.6	48.1	+2.5	0.32
Death Rate (%, in $t + 150$)		5.4	6.4	+1.0	0.44
Retirement Rate (%, in $t + 150$)		16.6	15.5	−1.1	0.56
Pension End Rate (%, in $t + 150$)		23.5	26.2	+2.7	0.23
Number of Survivors ($t = 0$)		790	736		

Note: This table shows the sample statistics for the two groups of male survivor pension recipients (high and low pension regimes). Survivor pensions and earnings are expressed in 2005 prices. We account for attrition over time. Figure A2 in the Appendix details the summary statistics.

4 Empirical Framework

We use a regression discontinuity framework as summarized by Lee and Lemieux (2010) in which the assignment variable is the month of widowhood. The identifying assumption of the effect of a lower pension on labor supply is that spouses' death assigns survivors as good as randomly to a high or low pension. In a regression discontinuity framework, the effect of the reform, τ, can be written as

$$\tau = E[Y_i(1) - Y_i(0)|X_i = c], \tag{2}$$

where E is the expectation operator, $Y_i(D)$ indicates individual i's outcome on being assigned to a low survivor's pension ($D = 1$) or a high survivor's pension ($D = 0$), X_i is the month of widowhood, and c is the date on which the reform entered into force (1 October, 2000). The estimand is the difference between the employment rate of the before-reform group and the employment rate of the after-reform group when the assignment variable equals the reform date.

In our preferred specification, we use all survivors widowed 30 months before or after the reform ("the bandwidth").[23] Our preferred estimand is a simple difference of group means ("the local constant specification").[24] In practice, we estimate a range of different specifications:

$$Y_{it} = \alpha + \tau D_i + \beta f(X_i^n) + \gamma Z_i + \epsilon_{it}, \tag{3}$$

where Y_{it} is a binary employment indicator of individual i in month t after widowhood. D_i

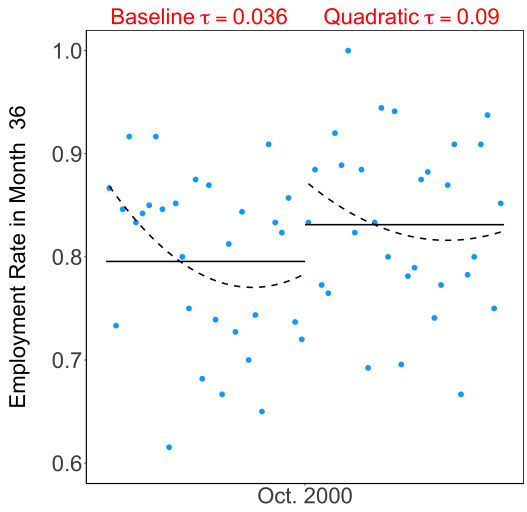
[23] We also estimate the specification for trimmed samples in which we exclude survivors farthest away from the reform date. This reduces potential biases, but increases the variance as the sample size falls.

[24] Our preferred estimand for τ comes from the simplest version of equation (3): $Y_{it} = \alpha + \tau D_i + \epsilon_{it}$. We also include covariates (Z_i) and allow for a relationship with the assignment variable (X_i) up to order 4.

equals one if a survivor was widowed after the reform. The term $f(X_i^n)$ allows for a flexible fit of polynomials up to order n for the relationship between the month in which a survivor was widowed and the employment rate, separately on the left and right sides of the cutoff.[25] We also control for a set of covariates Z_i, such as the age of a survivor at widowhood, the days a survivor worked in the 150 months before widowhood, and occupational dummy variables measured in the month of widowhood. (We do not include person fixed effects because they are collinear with the treatment indicator D_i.)

Figure 5 illustrates our approach for a month after widowhood. We pool survivors into bins of a one-month width and estimate functions using this distribution. Our baseline causal effect is given by the jump between the solid lines at the reform date. As an alternative, a quadratic trend results in the dashed lines with a greater jump at the reform date.

Figure 5: Effect of the reform on the employment rate.



Note: This figure illustrates our empirical strategy. To construct the baseline estimate, indicated by the distance between the solid lines at the cutoff in October 2000, we regress the employment indicator on a reform indicator, D_i. An alternative estimate, indicated by the distance between the dashed lines, uses a quadratic trend between the assignment variable (X) and employment indicator (Y). In month 36 after widowhood, the jump between the dashed lines ($\tau = 0.090$) is 2.5 times greater than the jump between the solid lines ($\tau = 0.036$). To construct the graph, we pool widowers into bins of a one-month width based on the month of widowhood and fit functions using this distribution. Each point in the figure consists of about 22 survivors. The figure is constructed from 675 widowers before the reform and 628 widowers thereafter.

[25] We present the results including polynomials up to order 4. However, we refer readers to the discussion in Gelman and Imbens (2019) on the use of higher-order polynomials in a regression discontinuity design.

5 Results on the Extensive Margin

5.1 Main Sample

The main sample consists of survivors widowed in a 60-month interval around the reform. Table 2 tabulates the coefficients and standard errors for different regression discontinuity specifications and outcome months after widowhood. The dependent variable is a binary employment indicator coded 1 if a survivor is employed in a given month and 0 if he is not employed. The coefficient is the estimated difference between the two groups at the reform cutoff, τ.

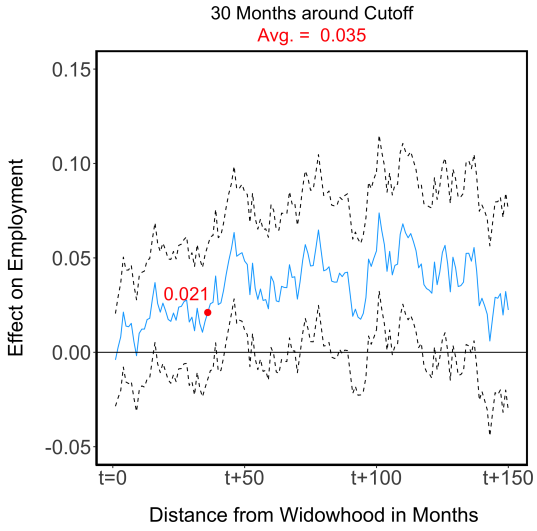
Column (1) shows the results for the baseline model in which the polynomial order is 0, the kernel is uniform, there are no covariates, and the width of the constructed bins is one month. In column (2), we add the covariates. Columns (1) and (2) are the baseline specifications. Columns (3) to (6) show the results for polynomial orders 1, 2, 3, and 4. We tabulate the coefficients and standard errors for 0, 12, 24, 36, 96, and 150 months after widowhood. In the center of the table, we report the average of the coefficients from estimating the model for each of the 150 months after widowhood. The specifications with higher-order polynomials yield higher coefficients and standard errors in most cases. On average, the estimated effect in the alternative specifications is two to three times greater than that at the baseline.

To assess the stability of the regression discontinuity specifications, Dong and Lewbel (2015) suggest reporting the treatment effect derivative (TED), which is the slope of the regression discontinuity function at the cutoff when the treatment is turned on. We report the average TED over the 150 months, separately for both sides of the cutoff. In those specifications with polynomials of orders 3 and 4, the TED values are large, which suggests high model instability (see columns (5) and (6) in Table 2).

Figure 6 plots the coefficients and 95% confidence intervals for every month after widowhood

for the baseline specification with the covariates of column (2) in Table 2. It shows that labor supply is persistently greater for survivors in the low pension regime than those in the high pension regime. The difference between the two groups is evident immediately after widowhood and increases over time. The red dot in Figure 6 indicates that the effect at 36 months after widowhood is a 2.1 percentage point higher probability of being employed for widowers who receive a low survivor pension. The average effect over all 150 months after widowhood is 3.5 percentage points.

Figure 6: Effect of the reform on the employment rate over time.



Note: This figure shows the treatment effect for the baseline specification of column (2) in Table 2 for each month after widowhood. The solid line traces the treatment effect and the dashed lines are the 95% confidence intervals. The treatment effect in month t+36 is 0.021 which means that survivors in the low pension regime have a 2.1 percentage point higher employment rate.

Table 2: Estimated effect on the employment rate.
(Standard errors in parentheses.)

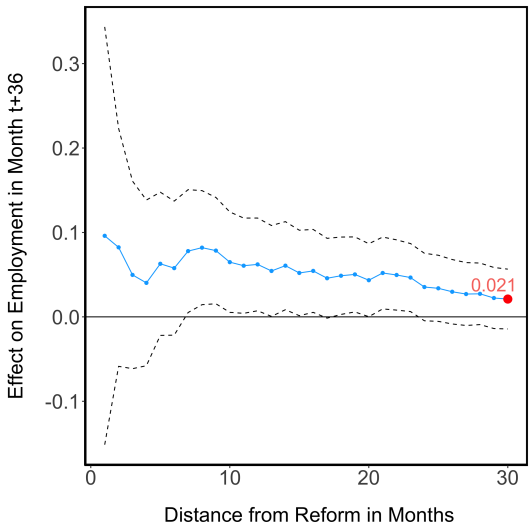
	Baseline (column 1 & 2)		Alternative (column 3 - 6)			
Employed	(1)	(2)	(3)	(4)	(5)	(6)
in month $t = 0$	-0.008	-0.005	0.010	0.013	0.022	0.025
	(0.019)	(0.009)	(0.017)	(0.022)	(0.028)	(0.036)
in month $t + 12$	0.016	0.012	0.073	0.116	0.097	0.057
	(0.02)	(0.016)	(0.032)	(0.048)	(0.065)	(0.081)
in month $t + 24$	0.027	0.017	0.105	0.103	0.104	0.144
	(0.021)	(0.017)	(0.033)	(0.049)	(0.066)	(0.082)
in month $t + 36$	0.036	0.021	0.096	0.071	0.069	0.073
	(0.022)	(0.018)	(0.036)	(0.055)	(0.075)	(0.096)
in month $t + 96$	0.051	0.029	0.090	0.050	0.058	0.035
	(0.025)	(0.022)	(0.042)	(0.064)	(0.084)	(0.102)
in month $t + 150$	0.056	0.022	0.078	0.102	-0.003	-0.008
	(0.031)	(0.027)	(0.053)	(0.078)	(0.103)	(0.124)
Mean Coefficient	0.054	0.035	0.100	0.092	0.096	0.089
	(0.024)	(0.02)	(0.039)	(0.059)	(0.080)	(0.099)
Mean TED Left	-	-	-0.087	0.009	-0.457	-0.715
	(-)	(-)	(0.049)	(0.197)	(0.492)	(0.988)
Mean TED Right	-	-	0.041	-0.100	0.784	1.452
	(-)	(-)	(0.071)	(0.279)	(0.702)	(1.410)
Polynomial Order	0	0	1	2	3	4
Full Bandwidth	Yes	Yes	Yes	Yes	Yes	Yes
Bin width	1	1	1	1	1	1
Uniform Kernel	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	No	Yes	Yes	Yes	Yes	Yes

Note: This table shows the regression results for employment rates. Column (1) shows the results for the baseline model when the polynomial order is 0, the bin width is one month, the kernel is uniform, and there are no covariates. Column (2) shows the baseline model with the covariates. Column (3) to column (6) show the regression results when the polynomial order is 1 = linear, 2 = quadratic, 3 = cubic, and 4 = quartic with the covariates. We tabulate the results for different outcome months (the rows). The dependent variable is an employment indicator coded 1 if a survivor was employed in a given month after widowhood. We tabulate the coefficients for τ (equation 3) and standard errors (in parentheses). The covariates in the specifications of column (2) to column (6) are the age of a survivor at widowhood, the days a survivor worked in the 150 months before widowhood, and occupational dummy variables measured in the month of widowhood. The middle box shows the average coefficient over all 150 months after widowhood and the average TED on both sides of the cutoff.

5.2 Alternative Samples

The estimates in Table 2 are based on survivors widowed 30 months before and after the reform ("full bandwidth"). Figure 7 describes the effect on the employment rate in month t+36 for different intervals around the reform. On the left of Figure 7, we use the smallest possible interval for the estimation, namely, survivors widowed in an interval of two months around the reform (i.e., in September or October 2000). From left to right, we increase the interval in two-month increments, one month before and after. On the right of Figure 7, we use the full 60-month bandwidth. In summary, the treatment effect decreases as the interval around the reform increases. The red dots in Figures 6 and 7 show the same estimates.

Figure 7: Baseline effect on the employment rate in month t+36, by reform distance.

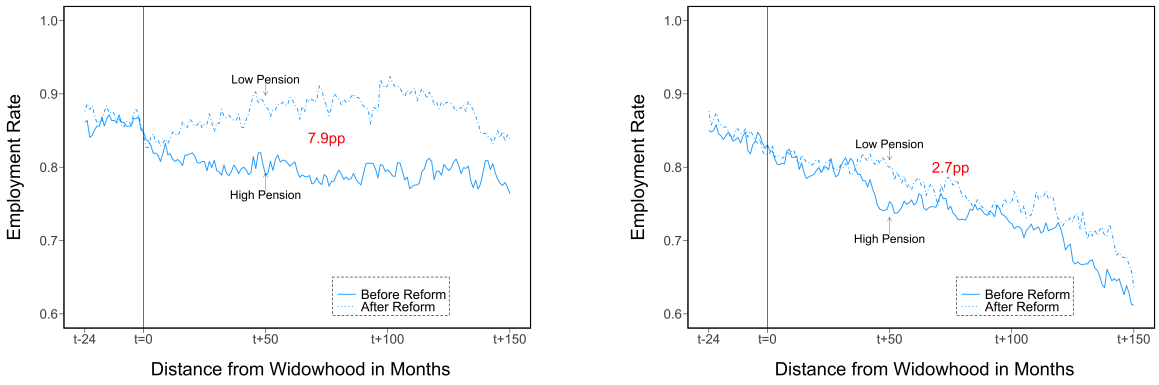


Note: This figure shows the baseline treatment effect in month $t+36$ after widowhood for different samples. On the left of the figure, the estimated treatment effect is based on a sample of survivors widowed one month before and after the reform. From left to right, the sample increases by one month on each side of the cutoff. On the right of the figure, the estimated treatment effect is based on a sample of survivors widowed in the full 60-month interval, 30 months before and after.

5.3 Young Widowers

To investigate the differences in responses, we split the sample by survivors' median age at the time of the death of their spouse. Figure 8 presents descriptive evidence on the employment rates of younger and older widowers, showing that the employment response of older widowers was modest, whereas younger widowers reacted strongly to the reform. On average, over the 150 months after widowhood, the employment rate difference is 7.9 percentage points for younger widowers and 1.9 percentage points for older widowers. (Tables A2 and A3 in the Appendix present the summary statistics for younger and older widowers.)

Figure 8: Employment rates for younger and older survivors.



(a) $21 < Age \leq 43.1$ (b) $43.1 < Age \leq 50$

Note: This figure plots the employment rates of widowers below the median age (43.1 years of age) and widowers above the median age at the time of the death of their spouse. The average differences in the employment rate are 7.9 and 2.7 percentage points.

5.4 Robustness Check

5.4.1 Attrition

All the results presented above are based on samples that account for sample attrition over time. We excluded survivors who died, retired, and those whose survivor pension ended from the month of the event onward. However, attrition may be a choice that depends on the size of the pension. For example, survivors with a high pension may choose to remarry less often because remarriage results in the loss of the survivor pension. Survivors with a high pension may also choose to retire earlier because their total old age income will depend less on their own old age pension. In this case, the composition of the two groups changes non-randomly over time and the estimated treatment effect reflects not only different labor market choices, but also differences in other related outcomes.

Figure 9 plots the employment rates for different subsamples. First, we exclude all survivors who died during the follow-up period from the sample (90 out of the 1,526 survivors). In the next robustness analysis, we exclude all survivors who died or retired (335 out of the 1,526 survivors). Finally, we exclude all survivors who died, retired or whose survivor pension ended within 150 months of becoming a widower (714 out of the 1,526 survivors). These different samples yield similar employment differences of 5.5, 6.0, and 7.2 percentage points. Table 3 details the attrition rates over time by different causes, showing only minor differences between the groups. Survivors in the high pension regime remarry slightly less often and retire slightly more often in the follow-up period than those in the low pension regime.

Figure 9: Accounting for attrition.

Note: The estimates are robust to different forms of accounting for attrition. This figure plots the employment rates if we exclude survivors who leave the sample within 150 months from the sample.

Table 3: Attrition rates by cause (in %).

	Total		Dead		Retired		Ended	
	Before	After	Before	After	Before	After	Before	After
$t+0$	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0
$t+12$	2.5	3.0	0.5	0.4	0.4	0.7	1.6	1.9
$t+24$	6.3	7.7	1.0	0.5	0.8	1.6	4.6	5.6
$t+36$	14.6	14.7	1.5	0.5	1.3	1.2	11.8	12.9
$t+96$	27.8	31.9	3.3	3.7	3.7	5.3	20.9	23.0
$t+150$	45.6	48.1	5.4	6.4	16.6	15.5	23.5	26.2

Note: This table shows the attrition rates for the sample of survivor pension recipients over time. Before (after) refers to survivors widowed before (after) October 2000.

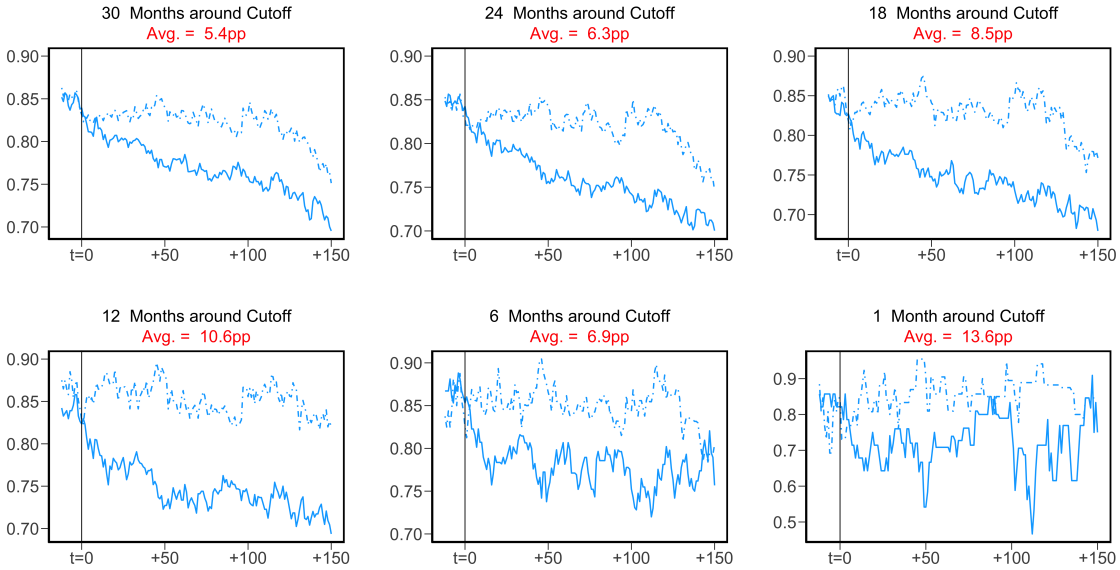
5.4.2 Bandwidth

In our preferred sample, the survivors farthest from the reform cutoff were widowed in a 60-month interval around the reform. We choose this interval to balance the size of the sample (variance) and confounding variation (bias). An extension of the 60-month interval would have introduced further difficulties because there were additional pension reforms before 1998 and after 2003.

Figure 10 shows the employment rates over time for narrower sample choices. At the top left, we show the employment rate for the main sample of 30 months on each side ("full bandwidth"). From the top left to the bottom right, we narrow the bandwidths to 24, 18, 12, six, and one month(s) around the cutoff. Independent of the choice of the sample, the employment rate is persistently higher for survivors in the low pension regime than in the high pension regime (see the dashed lines in Figure 10).

Figure 11 accounts for potential selection around the reform cutoff ("donut sample"). In the left figure, we exclude from the sample survivors widowed in a 12-month interval around the reform. In the right figure, we further exclude survivors widowed more than 24 months from the reform cutoff. The average treatment effect in these samples is similar to our baseline result, 4.8 and 5.8 percentage points.[26]

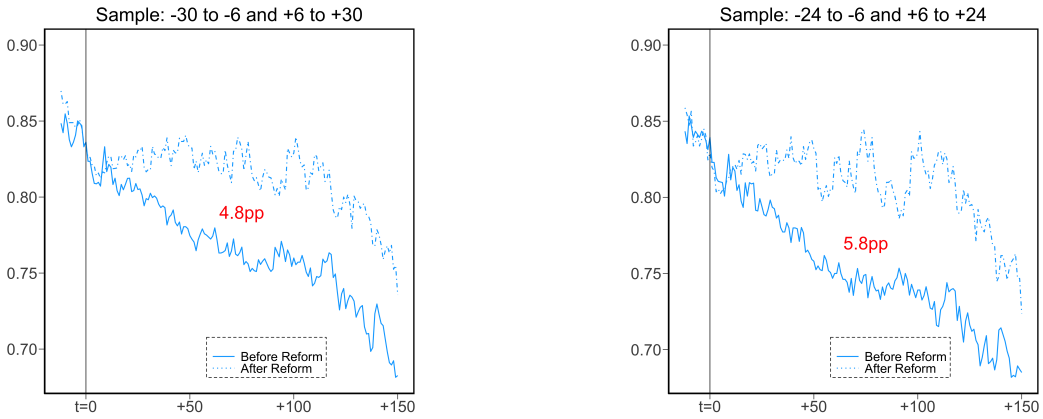
Figure 10: Employment rate for different sample choices around the reform.



Note: The employment difference is greater when the distance of widowhood to the reform is smaller. The top left graph shows the employment rates for the preferred sample. The other graphs shows the employment rates for smaller sample intervals. The solid line traces the employment rate for widowers before the reform ("high pension") and the dashed line traces the employment rate for widowers thereafter ('low pension").

[26] The law is precise about whether a survivor's pension was calculated according to the rules before or after the reform. If the deceased died before 2 September, 2000, the before-reform rules applied. If the deceased died on or after 2 September, the after-reform rules applied.

Figure 11: Employment rate for the donut samples.



Note: In this figure, we account for potential selection around the reform cutoff. The left figure plots the employment rates of survivors widowed 30 to six months before the reform and six to 30 months thereafter. The right figure plots the employment rates of survivors widowed 24 to six months before the reform and six to 24 months thereafter.

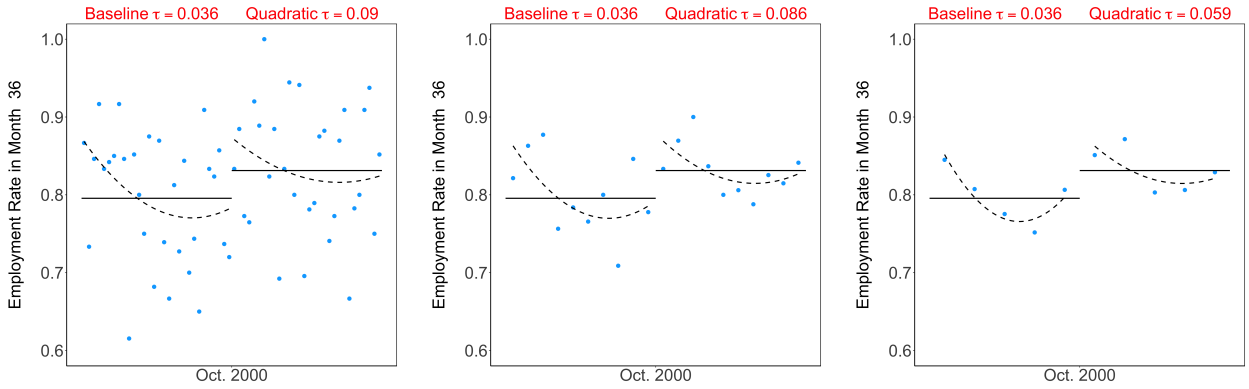
5.4.3 Bin Width

In our preferred sample, we pool survivors into bins of a one-month width and estimate the regression functions on the left and right sides of the cutoff. We further explore how the estimate changes when we alter the width of the bins to three and six months. Figure 12 shows that changing the width of the bins has little impact on the treatment effect when using the quadratic specification. (The baseline specification does not depend on the width of the bins.)

5.4.4 Data-driven Bandwidth Selection

Next, we estimate the treatment effect using the optimal bandwidth selection methods as carried out by Calonico, Cattaneo and Titiunik (2014), Calonico, Cattaneo and Titiunik (2015), Calonico, Cattaneo and Farrell (2017). These methods choose an optimal bandwidth based on minimizing the mean square error tradeoffs between bias and variance (MSE). We apply three methods, (1) we minimize the difference between the two bias and variance

Figure 12: Changes to the width of the bins.



Note: This figure shows that changing the width of bins has little impact on our estimate.

terms, one term for each side of the cutoff (MSEone); (2) we minimize the bias and variance terms separately on each side (MSEtwo); and (3) we minimize the sum of the two bias and variance terms (MSEsum).

We use a local constant specification and uniform kernel and estimate the regressions for each month after widowhood. Table 4 reports the results. Column (1) contains the baseline result for comparison purposes (i.e., using the full bandwidth from −1.00 to 1.00 on each side) and columns (2) to (4) contain the results using the above automatic selection methods. The bold lines at the bottom highlight the average suggested optimal bandwidth over the 150 months after widowhood, which ranges between 0.10 and 0.12 (i.e., between three and four months to the left and right of the cutoff). We estimate that the coefficients and standard errors are, on average, about two to three times higher in the specifications that use a bandwidth selection method. We report further results on the bandwidth selection for different specifications in the Appendix (Tables A4 to A9).[27]

[27] For example, the selected bandwidth using a polynomial order of 1 is between 0.22 and 0.33 and the selected bandwidth for a polynomial order of 2 is between 0.32 and 0.38. However, the fluctuation of the treatment effect between months is strong, especially when greater weight is placed on survivors close to the cutoff (i.e., triangular kernel).

Table 4: Effects on the employment rate using bandwidth selectors.
(Standard errors in parentheses.)

Employed	Baseline (1)	MSEone (2)	MSEtwo (3)	MSEsum (4)
in month $t = 0$	-0.005	0.022	0.028	0.034
	(0.009)	(0.017)	(0.016)	(0.024)
in month $t + 12$	0.012	0.057	0.079	0.099
	(0.016)	(0.046)	(0.059)	(0.060)
in month $t + 24$	0.017	0.063	0.095	0.095
	(0.017)	(0.040)	(0.052)	(0.052)
in month $t + 36$	0.021	0.040	0.048	0.050
	(0.018)	(0.049)	(0.055)	(0.056)
in month $t + 96$	0.029	0.047	0.046	0.025
	(0.022)	(0.060)	(0.065)	(0.068)
in month $t + 150$	0.022	-0.007	-0.035	0.00
	(0.027)	(0.082)	(0.074)	(0.075)
Mean Coefficient ($t + 1$ to $t + 150$)	0.035	0.071	0.078	0.070
Mean Standard Error ($t + 1$ to $t + 150$)	(0.020)	(0.058)	(0.063)	(0.064)
Polynomial Order	0	0	0	0
Bandwidth Left	1.00	0.12	0.10	0.10
Bandwidth Right	1.00	0.12	0.12	0.10
Bin width	1	1	1	1
Uniform Kernel	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes

Note: This table compares the baseline result in column (1) with the bandwidth selection methods MSEone, MSEtwo and MSEsum in columns (2) to (4). *MSEone* minimizes the difference between the two bias/variance terms, one term on each side of the cutoff, *MSEtwo* minimizes the bias/variance terms separately on each side, *MSEsum* minimizes the sum of the two bias/variance terms (see Calonico et al. (2014)). The selected bandwidths are relatively narrow, as they range between 0.10 and 0.12. The smaller samples selected by these methods produce higher coefficients and standard errors than in the baseline specification. We report additional tables for different specifications in the Appendix (i.e., Tables A2 to A7).

5.5 Statistical Power Analysis

We illustrate the statistical power of our analysis using the raw baseline coefficients, as reported in column (1) of Table 2. For example, the coefficient estimated for month 36 after widowhood is 0.036 with a p-value of 0.10. In the statistical power analysis, we are interested in the smallest sample size with which we could estimate the coefficient of 0.036 with a p-value of 0.05. Our original sample size in month 36 after widowhood is 1,303 survivor pension recipients. We calculate that a sample size of at least 1,837 survivor pension recipients would be required to estimate a coefficient of 0.036 with a p-value of 0.05. (The sample size would have to be 41% larger for this month.) Table 5 shows the results of the statistical power analysis. Whenever the coefficient is less than 0.053, the estimate is not statistically significant at the 95% level. (Because our sample size decreases over time, the size of the coefficient sufficient to estimate a statistically significant effect increases over time.)

In the raw baseline model, 100 out of the 150 estimates for each month after widowhood are statistically significant with a p-value of less than 0.05 and 13 additional estimates are statistically significant with a p-value of between 0.05 and 0.10. In the covariate-adjusted baseline model 58 out of the 150 estimates have a p-value below 0.05 and 18 additional months have a p-value between 0.05 and 0.10. Our results thus consistently show the positive effect of the reform on labor supply. From the statistical power analysis, we conclude that traditional statistical significance at a 95% confidence level cannot be obtained for small effects since the size of our sample is insufficient in such months.

Table 5: Statistical power analysis (in %).

Month	Coefficient	P-value	Obs. Actual	Obs. P-value 0.05
$t+0$	-0.008	0.69	1,526	35,802
$t+12$	0.016	0.42	1,484	8,624
$t+24$	0.027	0.20	1,419	3,252
$t+36$	0.036	0.10	1,303	1,837
$t+96$	0.051	0.04	1,071	1,071
$t+150$	0.056	0.07	812	979

Note: This table shows the outcome month, coefficient, p-value, and sample size of the raw baseline model in columns (1) to (4), as reported in Table 2. Column (5) reports the sample size needed to estimate a given coefficient with a p-value of 0.05. For example, in month t+36 the sample size would have to be 1,837 instead of 1,303, or 41% larger, to estimate a coefficient of 0.036 with a p-value of 0.05.

6 Results on the Intensive Margin

Because the information on work hours needed to assess the effects on the intensive margin of labor supply are missing, we instead use gross monthly labor earnings.[28] As the Social Security Administration collects earnings information up to the maximum contribution ceiling, the measure of gross labor earnings is capped at the top.[29] About 11% of the survivors in our sample earned more than the top cap. Labor earnings are also censored from below (i.e., at the marginal earnings threshold for work), below which no social security contributions have to be paid.[30]

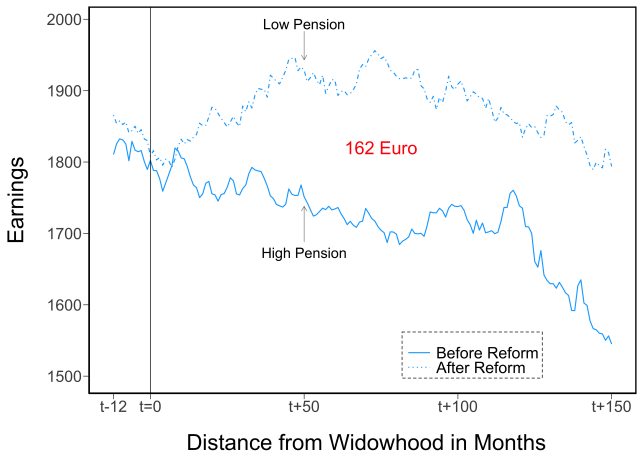
Figure 13 shows gross monthly labor earnings over time for the low and high pension group. In the month of widowhood, the earnings of the low and high pension group are about the same. In the following 150 months, however, the low pension group earns more than the high pension group. On average, the earnings difference after widowhood is €162.

[28] The part-time rate for men in Austria was about 4% in 2000 (Statistik Austria, 2020). By 2010, it had increased to about 9% (Statistik Austria, 2020). Huemer (2017) suggests that part-time work for men is relevant at younger ages and shortly before retirement.

[29] We have no information on the level of earnings above the top cap. If there was no top cap, the measured average earnings level would increase for both groups. As a consequence, the percentage point difference between the groups decreases and the elasticity increases.

[30] In the month of widowhood, 24 out of the 1,526 male survivors (1.6%) were marginally employed and 15 of those 24 were marginally employed in addition to another employment relationship. We consider the remaining nine people who are "only" marginally employed as not employed. There are no differences across the reform groups.

Figure 13: Gross labor earnings over time.



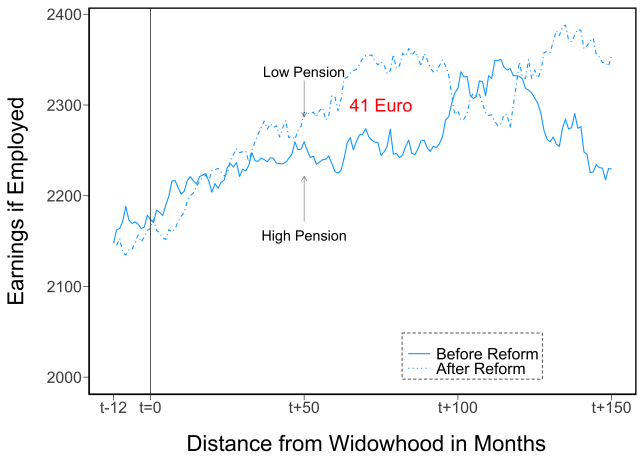
Note: Gross labor earnings in 2005 prices. Survivors in the "low pension" group earn on average €162 more per month in the follow-up period.

Figure 14 shows gross monthly labor earnings over time, conditional on being employed. The low pension group earns €41 more on average in the months after widowhood; however, in contrast to the estimates on the extensive margin of labor supply, the effects on the intensive margin are not robust over time. The difference between the lines in Figure 14 is mainly driven by gaps between months $t + 50$ and $t + 100$ as well as after month $t + 125$. Neither of these gaps has a clear causal connection to the reform. As a result, we do not attribute the €41 to the reform, and it has to be subtracted from the unconditional gross earnings effect of €162. (More generally, we subtract Figure 14 from Figure 13.) This provides an adjusted gross earnings difference of €121. In the baseline specification with the covariates, we obtain an adjusted gross earnings difference of €63. Both these adjusted gross earnings differences are due to extensive margin responses, not intensive margin responses.

Figure 15 illustrates our estimation approach for gross labor earnings in month 36 after widowhood. As before, the jump between the solid lines at the cutoff, τ, represents the baseline effect. The jump between the dashed lines represents a quadratic alternative. The quadratic function is highly curved, particularly on the left side of the reform. In most of our cases, the specifications with higher-order polynomials are unstable, producing a large

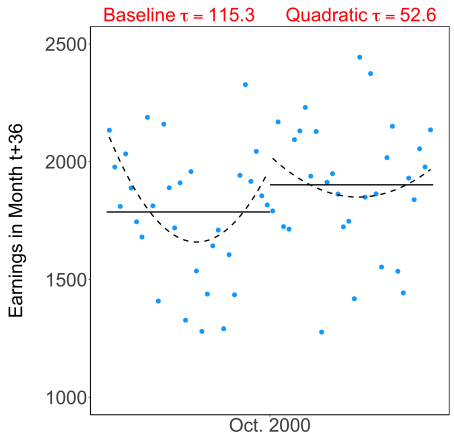
amount of volatility across outcome months.

Figure 14: Gross labor earnings over time, conditional on being employed.



Note: Earnings in 2005 prices. Employed survivors in the "low pension" regime earn on average €41 more per month in the follow-up period. However, we do not attribute this effect to the reform because the difference does not emerge immediately and the pattern is not robust over time.

Figure 15: Effect of the reform on gross labor earnings.



Note: This figure illustrates our empirical strategy. To construct the baseline estimate, the jump between the solid lines, we regress gross monthly labor earnings on a reform dummy (D_i). An alternative estimate, the jump between the dashed lines, uses a quadratic trend. The jump between the solid lines is 115.3 and the jump between the dashed lines is 52.6; however, the dashed lines show a steep slope at the cutoff (i.e., the TED).

Table 6 details the regression results for gross monthly labor earnings for survivors employed and not employed. If a person is not employed, we use €0 earnings. Table 7 provides the regression results for gross monthly labor earnings, conditional on being employed (earnings > 0). In both tables, we account for attrition over time. The tables show the causal effect,

τ, for different months after widowhood (0, 12, 24, 36, 96, and 150) and the standard errors in parentheses. In the center of these tables, we report the average effect after estimating the model for each of the 150 months separately. We further report the TED. The results from the alternative model specifications are volatile, particularly those in column (3) to column (6). We prefer the baseline models in both tables, that is, columns (1) and (2). In the alternative specifications, the estimated causal effect is sensitive to the choice of the functional form in combination with the choice of the bandwidth, particularly for functional forms with a high curvature. The baseline specifications, on the contrary, are relatively insensitive.

Figure 16 details the effect for all months after widowhood. The effect is increasing over time. Figure 17 shows that the treatment effect in month 36 does not depend on the sample period around the reform. However, if the sample is selected from the months close to the reform, the magnitude of the effect is zero. Figure 18 indicates that younger survivors responded stronger to the reform than older survivors. (See the summary statistics for older and younger survivors in the Appendix.) Figures 19 to 21 show the robustness checks for different forms of attrition, different sample choices around the reform, and donut samples. Although the magnitude of the effect varies, the direction is robust.

In summary, the baseline estimate suggests that survivors in the low pension group earn about €63 to €121 more than those in the high pension group over the follow-up period. The long-run marginal propensity to earn out of unearned income can be calculated by dividing the difference in earnings (i.e., €63 or €121) by the difference in survivor benefits (i.e., €103). The marginal propensity to earn out of unearned income is thus -0.6 and -1.2.[31]

[31] After adjusting for standard errors, the range of the marginal propensity to earn is between -1.5 and $+0.3$.

Table 6: Estimated effect on gross earnings.
(Standard errors in parentheses.)

	Baseline		Alternative			
Gross Earnings	(1)	(2)	(3)	(4)	(5)	(6)
in month $t = 0$	5.9	-19.7	-39.2	-88.4	-291.5	-171.6
	(58.6)	(38.7)	(75.8)	(109.7)	(143.1)	(177.2)
in month $t + 12$	38.1	8.0	29.1	-47.2	-236.9	-134.9
	(62.3)	(47.3)	(91)	(136.8)	(182.9)	(227.4)
in month $t + 24$	99.5	52.4	149.1	-8.9	-124.1	130.7
	(64.6)	(50.7)	(100.2)	(152.4)	(205.8)	(257.8)
in month $t + 36$	115.3	50.5	163.9	42.2	-53.9	16.8
	(68.6)	(55)	(107.9)	(163)	(221.5)	(281.6)
in month $t + 96$	176	89.1	217.2	11.4	177.8	351.6
	(80.5)	(67.7)	(140.8)	(216.5)	(285.5)	(355.5)
in month $t + 150$	248.8	119.3	164.4	42.2	-244.8	149.2
	(94.9)	(80.4)	(160.7)	(244.6)	(324.5)	(405.2)
Mean Coefficient	162.1	85.2	202.8	93.4	103.6	282.6
	(76.2)	(62.7)	(125.7)	(190.3)	(255.4)	(322.7)
Mean TED Left	-	-	-160.1	738.2	130.7	-2361.2
	(-)	(-)	(152.6)	(611.9)	(1526.3)	(3066.0)
Mean TED Right	-	-	81.0	-1068.4	37.9	1470.7.
	(-)	(-)	(219.3)	(865.0)	(2177.0)	(4374.4)
Polynomial Order	0	0	1	2	3	4
Full Bandwidth	Yes	Yes	Yes	Yes	Yes	Yes
Bin width	1	1	1	1	1	1
Uniform Kernel	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	No	Yes	Yes	Yes	Yes	Yes

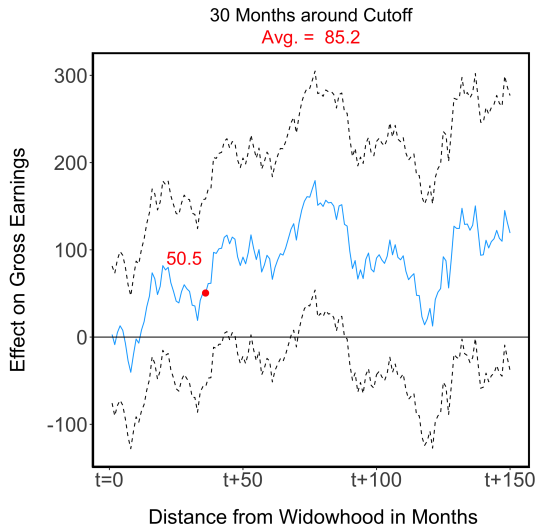
Note: This table shows the regression results for gross labor earnings (dependent variable). Column (1) shows the results for the baseline model when the polynomial order is 0, the bandwidth is 30 months before and after the reform, the bin width is one month, the kernel is uniform, and there are no covariates. Column (2) shows the baseline model with the covariates. Column (3) to column (6) show the regression results when the polynomial order is 1 = linear, 2 = quadratic, 3 = cubic, and 4 = quartic with the covariates. We tabulate the results for different outcome months (the rows). The coefficient is the estimate for τ, the jump at the reform cutoff, as shown in equation (3). The covariates are the age of a survivor at widowhood, the days a survivor worked in the 150 months before widowhood, and occupational dummy variables measured in the month of widowhood.

Table 7: Estimated effect on gross earnings, conditional on being employed. (Standard errors in parentheses.)

	Baseline		Alternative			
Gross Earnings	(1)	(2)	(3)	(4)	(5)	(6)
in month $t = 0$	-9.8	-29.1	-116.7	-159.5	-314.5	-214.5
	(50)	(41.3)	(80.4)	(117.4)	(154.4)	(191.5)
in month $t + 12$	-22.5	-17.2	-54.9	-266	-408.4	-176
	(53.6)	(45.1)	(85.9)	(128.7)	(175.8)	(224.1)
in month $t + 24$	4.5	15.9	-31.7	-234	-386.8	-167.9
	(55.5)	(46.9)	(91.8)	(136.6)	(184)	(230.6)
in month $t + 36$	33.5	19.6	-20.3	-63	-138.7	4.8
	(59.7)	(51.4)	(98.9)	(149.9)	(204.8)	(259.3)
in month $t + 96$	59.4	31.7	80.9	-118.5	53.3	241.9
	(72)	(63.3)	(129.1)	(195.4)	(264.2)	(336.8)
in month $t + 150$	125.4	75.1	76.1	-37.8	-82.9	363.1
	(85)	(75.2)	(150.3)	(228.7)	(304.9)	(379.3)
Mean Coefficient	40.8	22.6	20.5	-115.8	-131.2	85.9
	(67.2)	(58.736)	(115.9)	(175.0)	(238.9)	(304.0)
Mean TED Left	-	-	9.5	872.7	1309.6	-778.7
	(-)	(-)	(144.8)	(589.8)	(1493.7)	(2976.4)
Mean TED Right	-	-	-15.3	-929.9	-1635.1	-1739.4
	(-)	(-)	(203.9)	(815.1)	(2056.4)	(4101.3)
Polynomial Order	0	0	1	2	3	4
Full Bandwidth	Yes	Yes	Yes	Yes	Yes	Yes
Bin width	1	1	1	1	1	1
Uniform Kernel	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	No	Yes	Yes	Yes	Yes	Yes

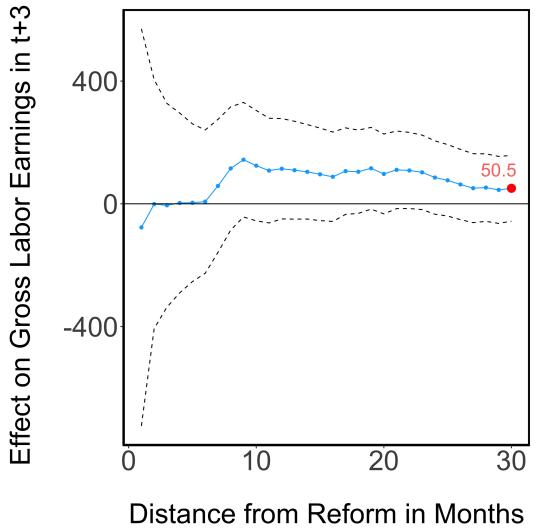
Note: This table shows the regression results for gross labor earnings, conditional on being employed (dependent variable). Column (1) shows the results for the baseline model when the polynomial order is 0, the bandwidth is 30 months before and after the reform, the bin width is one month, the kernel is uniform, and there are no covariates. Column (2) shows the baseline model with covariates. Column (3) to column (6) show the regression results when the polynomial order is 1 = linear, 2 = quadratic, 3 = cubic, and 4 = quartic with the covariates. We tabulate the results for different outcome months (the rows). The coefficient is the estimate for τ, the jump at the reform cutoff, as shown in equation (3). The covariates are the age of a survivor at widowhood, the days a survivor worked in the 150 months before widowhood, and occupational dummy variables measured in the month of widowhood.

Figure 16: Effect of the reform on gross labor earnings over time.



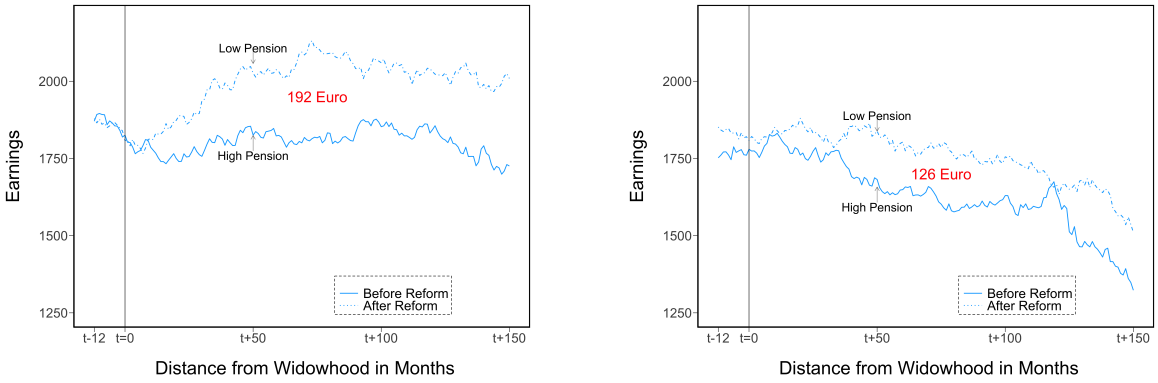
Note: This figure shows the treatment effect on gross monthly labor earnings for the baseline specification over time. The solid line traces the treatment effect for each month after widowhood and the dashed lines show the 95% confidence interval. The covariates are a survivor's age at widowhood, the total number of days a survivor worked in the 150 months before widowhood, and dummies for a survivor's worker type in the month of widowhood. The average effect is reported at the top of each graph.

Figure 17: Baseline effect on gross labor earnings in month $t + 36$, by reform distance.



Note: This figure shows the treatment effect on labor earnings depending on the choice of bandwidth. Close to the reform date, the effect on gross labor earnings disappears.

Figure 18: Gross labor earnings for younger and older survivors.

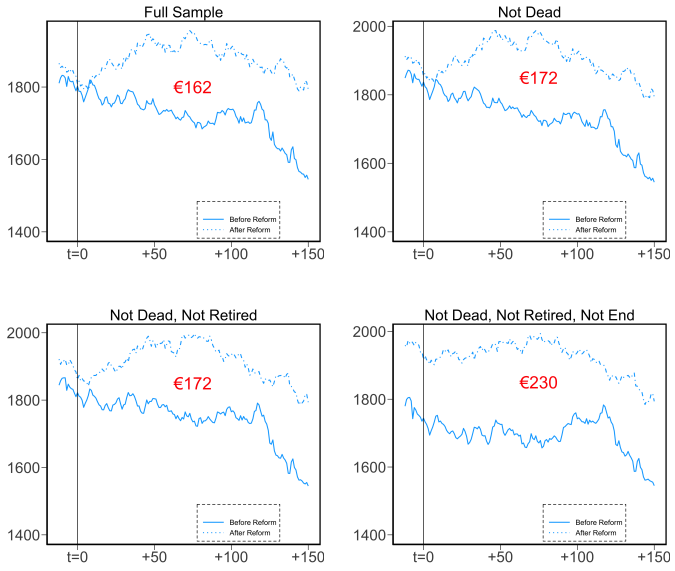


(a) Younger. (b) Older.

Note: This figure plots the gross labor earnings of survivors below the median age (43.1 years of age) and survivors above the median age at the time of the death of their spouse. The average difference between gross labor earnings is €192 for younger survivors and €126 for older survivors.

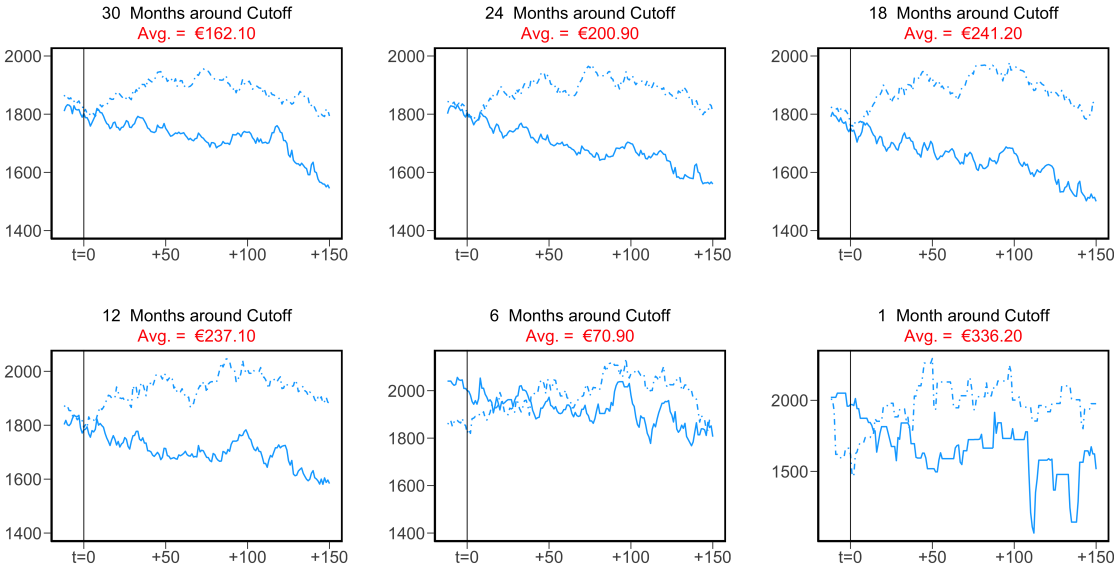
6.1 Robustness Check

Figure 19: Accounting for attrition, earnings.



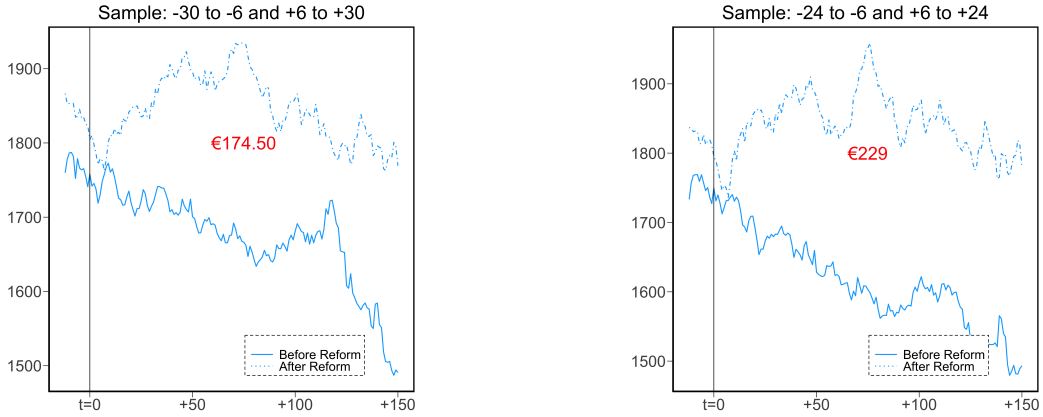
Note: We account for different forms of attrition. This figure plots employment rates if we exclude survivors who leave the sample within 150 months from the sample.

Figure 20: Different sample choices around the reform, earnings.



Note: This figure shows gross labor earnings over time for different sample intervals around the reform. The top left graph shows the earnings for our preferred sample. The solid line traces the earnings for survivors before the reform ("high pension") and the dashed line the earnings for survivors thereafter ("low pension").

Figure 21: Donut samples, earnings.

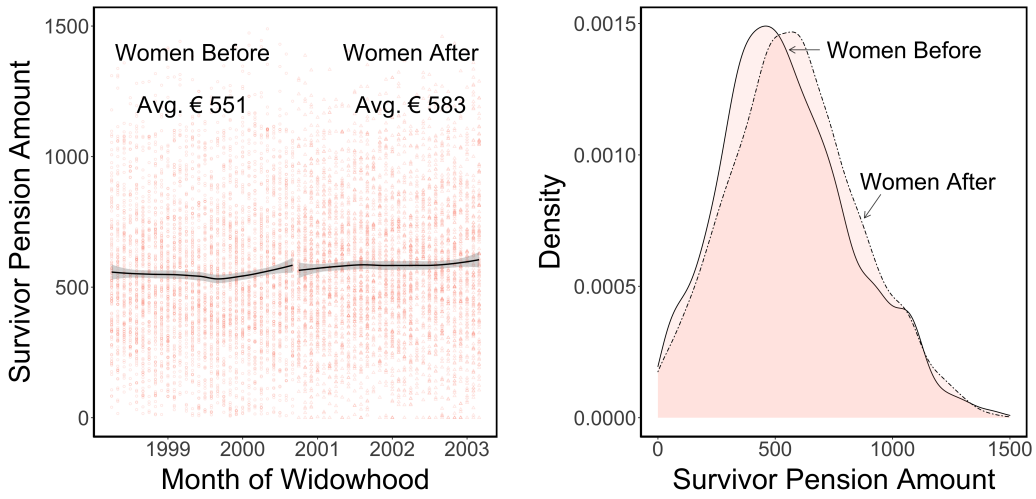


Note: We account for potential selection around the reform using donut samples. The left figure plots the earnings of survivors widowed 30 to six months before the reform and six to 30 months thereafter. The right figure plots the earnings of survivors widowed 24 to six months before the reform and six to 24 months thereafter.

7 Effect of the Reform on Women

We also analyze female survivors below age 50. The sample of women is about five times larger than the sample of men (8,104 compared with 1,526 observations). Figure 22 illustrates that female survivors, unlike male survivors, did not experience a reduction in their survivor pension, as the reform affected low income survivors differently than high income survivors and, on average, women have a lower income than men. Low income survivors were less affected because their replacement rates changed by less and the minimum income threshold was also raised. (The lowest income group was actually better off after the reform.) Figure 23 compares the employment rates of women before and after the reform. Although there is an absolute difference in employment, it remains roughly constant over time. The difference before widowhood is similar in magnitude and the employment rates do not diverge after widowhood.[32]

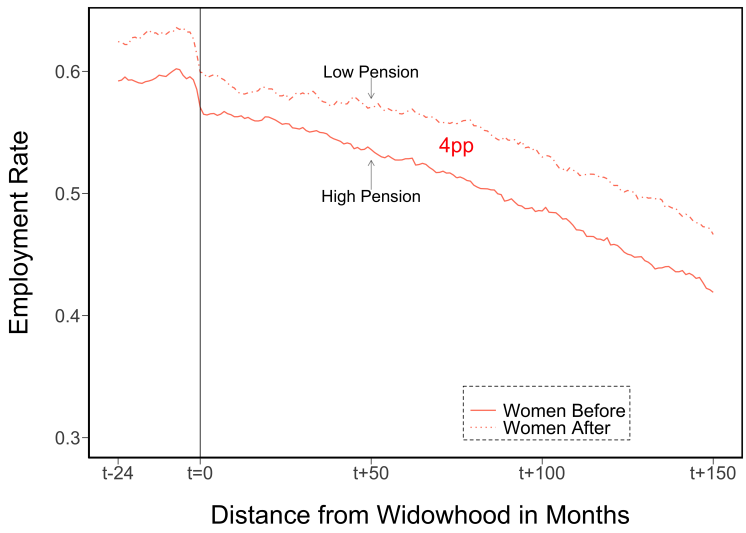
Figure 22: Survivor pensions of women, before and after October 2000.



Note: Women did not experience a reduction in their survivor pension because the reform affected low income survivors differently than high income survivors and women, on average, have a lower income than men. Pensions expressed in 2005 prices.

[32] Female survivors live abroad more more often than male survivors, this increased after the reform. We do not observe the labor market status of such individuals and exclude them from our sample.

Figure 23: Employment rates of women over time.



Note: The female employment rate of the before and after group does diverge over time (i.e., the distance between the dashed and solid lines is roughly constant before and after widowhood).

8 Discussion

Accurately estimating the impact of unearned income on the supply of labor is an important input for designing social security and welfare schemes. Economic theory predicts that unearned income reduces the supply of labor, but the extent of this reduction is an empirical question. Policymakers require precise estimates to balance the desired levels of social protection with undesired consequences for the labor market. For example, the popularity of the idea of a universal basic income has risen in recent years, but there is scant evidence on labor supply responses, especially when unearned income is significant and permanent. Labor supply responses to unearned income are difficult to quantify because experimental settings or quasi-experimental situations are rarely observed.

We study the labor supply of survivor pension recipients in Austria who receive a significant amount of unearned income — permanently. Our natural experiment is a policy reform that randomly allocated survivors into a high and a low pension regime depending on the date of their spouses' death. We thus identify the causal effect of an unexpected change in unearned

income. We analyze employment histories obtained from high quality administrative data and follow the groups over a long period.[33]

Our results suggest that changes to unearned income affect employment on the extensive margin, but not on the intensive margin. Our main estimates imply a 3.5 to 5.4 percentage point higher employment rate for survivors in the low pension regime over the 150 month follow-up period. This corresponds to a labor supply elasticity at the extensive margin of about -0.9 to -1.3. We estimate a change in gross earnings of about €63 to €121, which implies a marginal propensity to substitute unearned income with earned income of -0.6 to -1.2. To estimate effects along the intensive margin of labor supply, we use gross labor earnings, conditional on being employed, as an outcome variable. We find no causal effect of the reform on the labor earnings of survivors who are employed. We conclude that the effect of unearned income on earnings is driven by the decision of whether to work rather than how much to work.

Our estimated extensive margin elasticity is higher than the elasticity of -0.49 for disabled veterans in Autor, Duggan, Greenberg and Lyle (2016). Our marginal propensity to substitute unearned with earned income is greater than that for lottery winners (e.g., Imbens et al. (2001), Cesarini, Lindqvist, Notowidigdo and Östling (2017), Picchio, Suetens and van Ours (2018)), but comparable to that of Giupponi (2019), who reports a marginal propensity of -1.0 for survivor pension recipients in Italy. A possible explanation for the large responses among survivors is that their preferences for leisure and consumption change more than those of lottery winners and veterans. For example, survivors may have to care for bereaved children, which might lead to different utility from non-working time. Younger survivors respond more strongly to the reform than older survivors.

Our work contributes to the scarce empirical literature that aims at estimating the causal

[33] By comparison, the Finnish basic income experiment (Kangas, Jauhiainen, Simanainen and Ylikännö, 2020) studies unemployed people for one year.

effect of unconditional cash transfers on labor supply. The analysis of the effects of survivor pensions on other outcomes, for example, on health, is left to further research.

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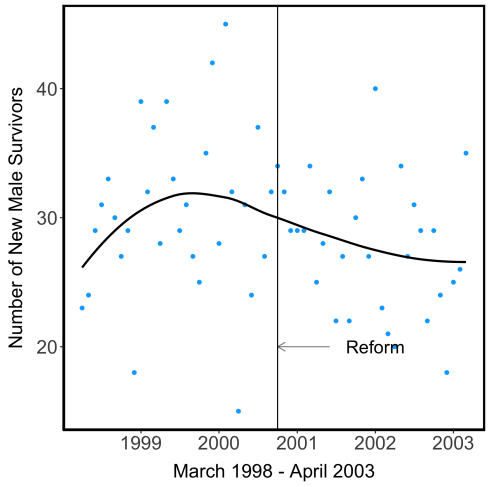
9 Annex

Table A1: Legal references.

Social Security Law Worker Type Coverage	ASVG Employees (1)	GSVG Self Employed (2)	BSVG Farmers (3)
	Admission of a Case		
Qualifying Date of Death	§223	§113	§104
Application Date	§86	§55	§51
Federal Law Gazette	30/1998	30/1998	30/1998
	Minimum Insurance Duration (of Deceased)		
Minimum Insurance Duration	§236 (1)	§120 (3)	§111 (3)
Reference Period for Insurance Duration	§236 (2)	§120 (4)	§111 (4)
if Deceased Age < 27	§236 (4)	§120 (2)	§111 (2)
if Occupational Death	§235 (3)	§120 (2)	§111 (2)
Federal Law Gazette	30/1998	30/1998	30/1998
	Severance Payment		
if Deceased not enough Insurance Months	§269	§148a	§137 (1)
	Entitlement & Duration		
Entitlement	§258 (1)	§136 (1)	§127 (1)
Temporary Pension	§258 (2)	§136 (2)	§127 (2)
Temporary Pension Exemption	§258 (3)	§136 (3)	§127 (3)
Divorced Survivors	§258 (4)	§136 (4)	§127 (4)
Federal Law Gazette	138/1998	139/1998	140/1998
	Calculation & Replacement Rate		
Pension Basis for Calculation	§264 (1)	§145 (1)	§136 (1)
Pension Replacement Rate	§264 (2)	§145 (2)	§136 (2)
Contribution Survivor	§264 (3)	§145 (3)	§136 (3)
Contribution Deceased	§264 (4)	§145 (4)	§136 (4)
Contribution Types	§264 (5)	§145 (5)	§136 (5)
Minimum Income Threshold	§264 (6+7)	§145 (6+7)	§136 (6+7)
Minimum Pension Threshold	§293	§150	§141
Maximum Income Threshold	§264 (6a+7a)	§145 (6a+7a)	§136 (6a+7a)
Alimony	§264 (8+9)	§145 (8+9)	§136 (8+9)
Alimony Exemption	§264 (10)	§145 (10)	§136 (10)
Federal Law Gazette (Before Reform)	139/1997	139/1998	139/1997
Federal Law Gazette (After Reform)	101/2000	101/2000	101/2000
	Remarriage & Revival		
Remarriage Severance Payment	§265 (1)	§146 (1)	§137 (1)
Revival if Remarriage Dissolves	§265 (2)	§146 (2)	§137 (2)
Revival Waiting Period	§265 (3)	§146 (3)	§137 (3)
Revival Alimony	§265 (4+5)	§146 (4+5)	§137 (4+5)
Federal Law Gazette	411/1996	412/1996	413/1996

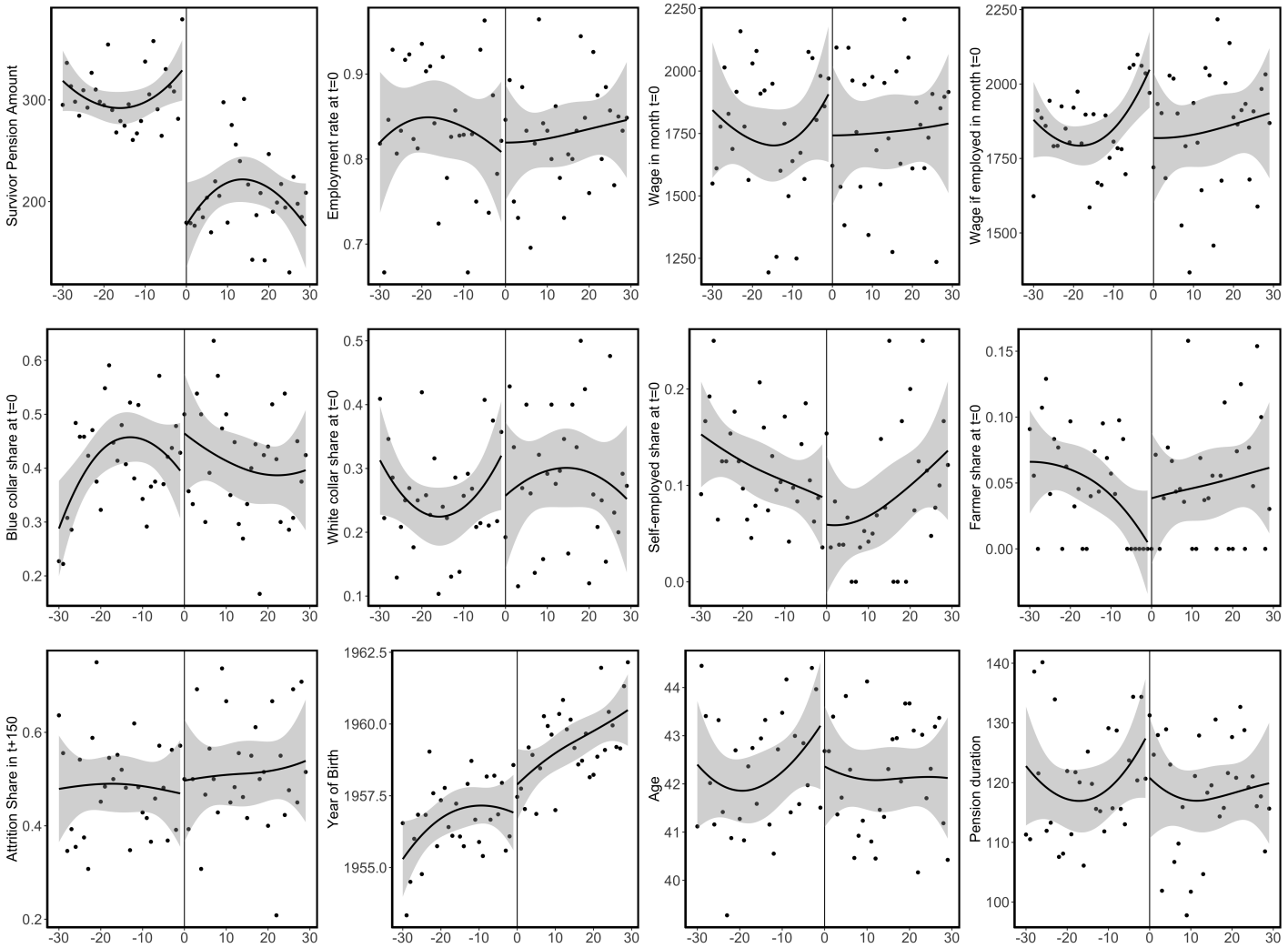
Note: This table shows the references of Austrian survivor pension law. Austrian social security law is divided into three frameworks for employees, the self-employed, and farmers. With respect to survivor pensions, the legal texts are in principle identical. All references can be verified at *www.sozdok.at*, a website for Austrian social security law. Content is only available in German.

Figure A1: Number of survivors over time.



Note: This figure shows the selected number of male survivor pension recipients below age 50 between April 1998 and March 2003 and a smoothed trend for the whole time period (no regression discontinuity design).

Figure A2: Selected variables, by distance from the reform in months.



Note: This figure shows the averages for selected dependent and independent variables (y-axis) by the distance from the survivor pension reform in months. We pool survivors into bins of a one-month width, scatter the means, and fit a quadratic polynomial on each side.

Table A2: Summary statistics — Younger widowers ($21 \leq$ Age < 43.1).

		Before Reform	After Reform	Difference	P-Value
Monthly Survivor Pension		299.3	203.7	−95.6	0
Employed (%)	in month $t = 0$	84.8	84.4	−0.5	0.86
	in month $t + 12$	81.1	84.8	+3.8	0.17
	in month $t + 24$	80.8	85.4	+4.6	0.1
	in month $t + 36$	79.4	86.7	+7.3	0.01
	in month $t + 96$	78.5	87.6	+9.1	0.01
	in month $t + 150$	76.4	83.9	+7.5	0.04
Earnings (€)	in month $t = 0$	1825.4	1808.1	−17.2	0.83
	in month $t + 12$	1767	1832.1	+65.1	0.45
	in month $t + 24$	1754.8	1860.3	+105.6	0.24
	in month $t + 36$	1825	1998.7	+173.7	0.07
	in month $t + 96$	1855.7	2053.6	+197.9	0.07
	in month $t + 150$	1725.6	2009.3	+283.7	0.02
Year/Month of Birth		1962/01	1964/08	+31	0
Age at Widowhood (years, $t = 0$)		37.3	37.3	0	0.9
Blue Collar (%, $t = 0$)		44.2	41.8	−2.4	0.36
White Collar (%, $t = 0$)		27.2	29.6	+2.4	0.49
Self-Employed (%, $t = 0$)		9.5	8.6	−0.9	0.5
Farmers (%, $t = 0$)		3.9	4.3	+0.5	0.46
Days worked ($t - 150$ to $t - 1$)		3680.1	3727.6	+47.4	0.6
Survivor Pension Duration (in $t + 150$)		118	113.2	−4.8	0.19
Attrition Rate (%, in $t + 150$)		39.1	41.5	+2.4	0.67
Death Rate (%, in $t + 150$)		3.1	3.8	+0.7	0.75
Survivor Retirement Rate (%, in $t + 150$)		3.9	2.4	−1.4	0.6
Survivor Pension Ended (%, in $t + 150$)		32.1	35.3	+3.2	0.26
Number of Survivors (in $t = 0$)		389	371	−18	0.36

Note: Survivor pensions and earnings in 2005 prices are paid 14 times per year. The employment rate aggregates the shares of blue collar workers, white collar workers, the self-employed, and farmers. During the follow-up period, we exclude survivors from the analysis who retired, died or whose survivor pension ended from the month of the event onward.

Table A3: Summary Statistics — Older widowers ($43.1 \leq$ Age ≤ 50).

		Before Reform	After Reform	Difference	P-Value
Monthly Survivor Pension		309.5	198.6	−110.9	0
Employed (%)	in month $t = 0$	83	81.9	−1.1	0.68
	in month $t + 12$	81.5	81	−0.6	0.84
	in month $t + 24$	80.1	80.7	+0.7	0.83
	in month $t + 36$	79.7	79.8	+0.1	0.97
	in month $t + 96$	73.6	74.4	+0.8	0.82
	in month $t + 150$	61.1	63.6	+2.5	0.63
Earnings (€)	in month $t = 0$	1779.8	1808.2	+28.3	0.74
	in month $t + 12$	1819.3	1831	+11.7	0.9
	in month $t + 24$	1756.8	1850.2	+93.3	0.31
	in month $t + 36$	1751	1812.8	+61.9	0.52
	in month $t + 96$	1599	1743.5	+144.5	0.23
	in month $t + 150$	1323.2	1510.4	+187.2	0.22
Year/Month of Birth		1952/07	1954/12	+29	0
Age at Widowhood (years, $t = 0$)		46.9	46.9	0	0.99
Blue Collar (%, $t = 0$)		38.4	40.8	+2.4	0.07
White Collar (%, $t = 0$)		24.9	26.8	+1.9	0.42
Self-Employed (%, $t = 0$)		13.7	9	−4.7	0.5
Farmers (%, $t = 0$)		6	5.2	−0.8	0.55
Days worked ($t - 150$ to $t - 1$)		3887.2	3868.9	−18.3	0.83
Survivor Pension Duration (in $t + 150$)		124.7	123.5	−1.2	0.72
Attrition Rate (%, in $t + 150$)		51.9	54.8	+2.9	0.04
Death Rate (%, in $t + 150$)		7.7	9	+1.3	0.64
Survivor Retirement Rate (%, in $t + 150$)		28.9	28.8	−0.2	0.51
Survivor Pension Ended (%, in $t + 150$)		15.2	17	+1.8	0.96
Number of Survivors (in $t = 0$)		401	365	−36	0.51

Note: Survivor pensions and earnings in 2005 prices are paid 14 times per year. The employment rate aggregates the shares of blue collar workers, white collar workers, the self-employed, and farmers. During the follow-up period, we exclude survivors from the analysis who retired, died or whose survivor pension ended from the month of the event onward.

Table A4: Effects on the employment rate using bandwidth selectors.
Polynomial order = 0, kernel = uniform
(standard errors in parentheses)

	Excl. Covariates				Incl. Covariates			
	bl	one	two	sum	bl	one	two	sum
Employed	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
in month $t = 0$	-0.008	-0.022	0.011	-0.022	-0.005	0.022	0.028	0.034
	(0.019)	(0.046)	(0.049)	(0.046)	(0.009)	(0.017)	(0.016)	(0.024)
in month $t + 12$	0.016	0.049	0.042	0.055	0.012	0.057	0.079	0.099
	(0.02)	(0.051)	(0.055)	(0.06)	(0.016)	(0.046)	(0.059)	(0.06)
in month $t + 24$	0.027	0.064	0.071	0.056	0.017	0.063	0.095	0.095
	(0.021)	(0.047)	(0.051)	(0.051)	(0.017)	(0.04)	(0.052)	(0.052)
in month $t + 36$	0.036	0.056	0.047	0.034	0.021	0.04	0.048	0.05
	(0.022)	(0.067)	(0.063)	(0.06)	(0.018)	(0.049)	(0.055)	(0.056)
in month $t + 96$	0.051	0.06	0.06	0.06	0.029	0.047	0.046	0.025
	(0.025)	(0.07)	(0.07)	(0.07)	(0.022)	(0.06)	(0.065)	(0.068)
in month $t + 150$	0.056	0.065	0.065	0.038	0.022	-0.007	-0.035	0
	(0.031)	(0.094)	(0.094)	(0.086)	(0.027)	(0.082)	(0.074)	(0.075)
Mean Coefficient	0.054	0.086	0.081	0.084	0.035	0.071	0.078	0.07
Mean Standard Error	(0.024)	(0.068)	(0.067)	(0.065)	(0.02)	(0.058)	(0.063)	(0.064)
Polynomial Order	0	0	0	0	0	0	0	0
Bandwidth Left	1.00	0.13	0.14	0.14	1.00	0.12	0.1	0.1
Bandwidth Right	1.00	0.13	0.13	0.14	1.00	0.12	0.12	0.1
Bin width	1	1	1	1	1	1	1	1
Uniform Kernel	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	No	No	No	No	Yes	Yes	Yes	Yes

Note: This table compares our baseline results (column (1) and column (5)) with the three mean square error (MSE) bandwidth selection methods: (i) MSEone in columns (2) and (6), (ii) MSEtwo in columns (3) and (7), and (iii) MSEsum in columns (4) and (8). These methods are implemented in the *rdrobust* package (Calonico et al., 2015).

Table A5: Effects on the employment rate using bandwidth selectors.
Polynomial order = 0, kernel = triangular
(standard errors in parentheses)

	Excl. Covariates				Incl. Covariates			
	bl	one	two	sum	bl	one	two	sum
Employed	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
in month $t = 0$	-0.008	-0.01	-0.006	-0.005	0	0.023	0.025	0.023
	(0.022)	(0.046)	(0.048)	(0.049)	(0.01)	(0.017)	(0.015)	(0.017)
in month $t + 12$	0.034	0.095	0.095	0.124	0.032	0.078	0.096	0.099
	(0.023)	(0.065)	(0.064)	(0.074)	(0.018)	(0.047)	(0.056)	(0.056)
in month $t + 24$	0.055	0.094	0.129	0.137	0.045	0.118	0.135	0.121
	(0.024)	(0.051)	(0.064)	(0.066)	(0.019)	(0.053)	(0.057)	(0.054)
in month $t + 36$	0.058	0.057	0.054	0.065	0.045	0.06	0.062	0.058
	(0.025)	(0.055)	(0.059)	(0.063)	(0.021)	(0.047)	(0.051)	(0.053)
in month $t + 96$	0.072	0.069	0.066	0.074	0.047	0.049	0.046	0.044
	(0.029)	(0.063)	(0.066)	(0.069)	(0.025)	(0.05)	(0.055)	(0.056)
in month $t + 150$	0.074	0.085	0.064	0.083	0.04	0.011	0.005	0.014
	(0.035)	(0.089)	(0.084)	(0.088)	(0.03)	(0.067)	(0.065)	(0.07)
Mean Coefficient	0.075	0.099	0.102	0.111	0.055	0.08	0.085	0.08
Mean Standard Error	(0.027)	(0.065)	(0.069)	(0.071)	(0.023)	(0.054)	(0.059)	(0.059)
Polynomial Order	1	1	1	1	1	1	1	1
Bandwidth Left	1	0.19	0.18	0.16	1	0.17	0.15	0.15
Bandwidth Right	1	0.19	0.17	0.16	1	0.17	0.18	0.15
Bin width	1	1	1	1	1	1	1	1
Triangular Kernel	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	No	No	No	No	Yes	Yes	Yes	Yes

Note: This table compares our baseline results (column (1) and column (5)) with the three mean square error (MSE) bandwidth selection methods: (i) MSEone in columns (2) and (6), (ii) MSEtwo in columns (3) and (7), and (iii) MSEsum in columns (4) and (8). These methods are implemented in the *rdrobust* package (Calonico et al., 2015).

Table A6: Effects on the employment rate using bandwidth selectors.
Polynomial order = 1, kernel = uniform
(standard errors in parentheses)

	Excl. Covariates				Incl. Covariates			
	bl	one	two	sum	bl	one	two	sum
Employed	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
in month $t = 0$	-0.008	-0.042	-0.049	-0.042	0.01	0.04	0	0.04
	(0.039)	(0.066)	(0.068)	(0.066)	(0.017)	(0.027)	(0.024)	(0.027)
in month $t + 12$	0.072	0.075	0.057	0.08	0.073	0.07	0.07	0.086
	(0.04)	(0.082)	(0.078)	(0.089)	(0.032)	(0.063)	(0.064)	(0.067)
in month $t + 24$	0.113	0.09	0.084	0.075	0.105	0.075	0.087	0.099
	(0.041)	(0.081)	(0.079)	(0.077)	(0.033)	(0.064)	(0.062)	(0.067)
in month $t + 36$	0.103	0.1	0.08	0.015	0.096	0.027	0.083	0.027
	(0.043)	(0.098)	(0.095)	(0.091)	(0.036)	(0.078)	(0.083)	(0.078)
in month $t + 96$	0.116	0.099	0.114	0.024	0.09	0.028	0.052	0.057
	(0.049)	(0.11)	(0.107)	(0.089)	(0.042)	(0.076)	(0.078)	(0.08)
in month $t + 150$	0.112	0.011	0.126	0.038	0.078	-0.03	-0.03	-0.007
	(0.061)	(0.129)	(0.137)	(0.116)	(0.053)	(0.101)	(0.101)	(0.09)
Mean Coefficient	0.118	0.126	0.138	0.1	0.1	0.09	0.084	0.086
Mean Standard Error	(0.047)	(0.103)	(0.103)	(0.094)	(0.039)	(0.079)	(0.08)	(0.08)
Polynomial Order	1	1	1	1	1	1	1	1
Bandwidth Left	1	0.23	0.23	0.28	1	0.25	0.25	0.25
Bandwidth Right	1	0.23	0.25	0.28	1	0.25	0.26	0.25
Bin width	1	1	1	1	1	1	1	1
Uniform Kernel	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	No	No	No	No	Yes	Yes	Yes	Yes

Note: This table compares our baseline results (column (1) and column (5)) with the three mean square error (MSE) bandwidth selection methods: (i) MSEone in columns (2) and (6), (ii) MSEtwo in columns (3) and (7), and (iii) MSEsum in columns (4) and (8). These methods are implemented in the *rdrobust* package (Calonico et al., 2015).

Table A7: Effects on the employment rate using bandwidth selectors.
Polynomial order = 1, kernel = triangular
(standard errors in parentheses)

	Excl. Covariates				Incl. Covariates			
	bl	one	two	sum	bl	one	two	sum
Employed	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
in month $t = 0$	0.003	0.038	0.025	0.039	0.011	0.024	0.016	0.029
	(0.042)	(0.078)	(0.078)	(0.079)	(0.016)	(0.023)	(0.022)	(0.022)
in month $t + 12$	0.098	0.173	0.154	0.139	0.09	0.079	0.091	0.094
	(0.045)	(0.104)	(0.099)	(0.096)	(0.035)	(0.057)	(0.063)	(0.066)
in month $t + 24$	0.122	0.235	0.217	0.19	0.102	0.108	0.119	0.12
	(0.046)	(0.103)	(0.1)	(0.096)	(0.036)	(0.059)	(0.065)	(0.066)
in month $t + 36$	0.099	0.056	0.094	0.056	0.086	0.06	0.061	0.06
	(0.047)	(0.088)	(0.092)	(0.088)	(0.04)	(0.07)	(0.072)	(0.069)
in month $t + 96$	0.103	0.067	0.081	0.085	0.071	0.05	0.051	0.052
	(0.054)	(0.087)	(0.1)	(0.101)	(0.047)	(0.089)	(0.087)	(0.073)
in month $t + 150$	0.133	0.117	0.144	0.117	0.085	-0.003	-0.003	-0.001
	(0.065)	(0.135)	(0.138)	(0.135)	(0.056)	(0.086)	(0.087)	(0.088)
Mean Coefficient	0.122	0.141	0.157	0.137	0.095	0.088	0.091	0.087
Mean Standard Error	(0.052)	(0.105)	(0.108)	(0.104)	(0.043)	(0.077)	(0.078)	(0.076)
Polynomial Order	1	1	1	1	1	1	1	1
Bandwidth Left	1	0.26	0.22	0.26	1	0.3	0.29	0.31
Bandwidth Right	1	0.26	0.33	0.26	1	0.3	0.32	0.31
Bin width	1	1	1	1	1	1	1	1
Triangular Kernel	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	No	No	No	No	Yes	Yes	Yes	Yes

Note: This table compares our baseline results (column (1) and column (5)) to three mean square error (MSE) bandwidth selection methods: (i) MSEone in columns (2) and (6), (ii) MSEtwo in columns (3) and (7), and (iii) MSEsum in columns (4) and (8). These methods are implemented in the *rdrobust* package (Calonico et al., 2015).

Table A8: Effects on the employment rate using bandwidth selectors.
Polynomial order = 2, kernel = uniform
(standard errors in parentheses)

	Excl. Covariates				Incl. Covariates			
	bl	one	two	sum	bl	one	two	sum
Employed	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
in month $t = 0$	0.018	0.003	0.001	-0.011	0.013	0.005	0.004	0.026
	(0.058)	(0.092)	(0.089)	(0.084)	(0.022)	(0.032)	(0.03)	(0.035)
in month $t + 12$	0.137	0.19	0.124	0.142	0.116	0.093	0.143	0.093
	(0.063)	(0.118)	(0.105)	(0.106)	(0.048)	(0.078)	(0.082)	(0.078)
in month $t + 24$	0.135	0.144	0.118	0.077	0.103	0.119	0.14	0.118
	(0.063)	(0.102)	(0.101)	(0.09)	(0.049)	(0.071)	(0.074)	(0.078)
in month $t + 36$	0.09	0.118	0.101	0.024	0.071	0.109	0.127	0.109
	(0.064)	(0.122)	(0.118)	(0.115)	(0.055)	(0.118)	(0.118)	(0.118)
in month $t + 96$	0.08	0.16	0.086	0.051	0.05	0.019	0.046	0.073
	(0.074)	(0.133)	(0.132)	(0.122)	(0.064)	(0.103)	(0.121)	(0.127)
in month $t + 150$	0.162	0.19	0.104	0.084	0.102	-0.001	0.038	0.053
	(0.091)	(0.179)	(0.159)	(0.154)	(0.078)	(0.121)	(0.118)	(0.123)
Mean Coefficient	0.124	0.175	0.161	0.143	0.092	0.096	0.108	0.114
Mean Standard Error	(0.071)	(0.132)	(0.129)	(0.126)	(0.059)	(0.099)	(0.104)	(0.106)
Polynomial Order	2	2	2	2	2	2	2	2
Bandwidth Left	1	0.32	0.32	0.35	1	0.36	0.34	0.32
Bandwidth Right	1	0.32	0.35	0.35	1	0.36	0.33	0.32
Bin width	1	1	1	1	1	1	1	1
Uniform Kernel	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	No	No	No	No	Yes	Yes	Yes	Yes

Note: This table compares our baseline results (column (1) and column (5)) to three mean square error (MSE) bandwidth selection methods: (i) MSEone in columns (2) and (6), (ii) MSEtwo in columns (3) and (7), and (iii) MSEsum in columns (4) and (8). These methods are implemented in the *rdrobust* package (Calonico et al., 2015).

Table A9: Effects on the employment rate using bandwidth selectors.
Polynomial order = 2, kernel = triangular
(standard errors in parentheses)

	Excl. Covariates				Incl. Covariates			
	bl	one	two	sum	bl	one	two	sum
Employed	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
in month $t = 0$	0.001	0.108	0.104	0.069	0.019	-0.006	0.008	-0.008
	(0.062)	(0.112)	(0.108)	(0.097)	(0.021)	(0.028)	(0.029)	(0.026)
in month $t + 12$	0.116	0.245	0.249	0.18	0.107	0.167	0.17	0.167
	(0.069)	(0.134)	(0.131)	(0.113)	(0.052)	(0.099)	(0.09)	(0.1)
in month $t + 24$	0.121	0.331	0.321	0.195	0.102	0.211	0.173	0.186
	(0.068)	(0.131)	(0.129)	(0.106)	(0.053)	(0.093)	(0.082)	(0.088)
in month $t + 36$	0.068	0.119	0.123	0.082	0.07	0.054	0.05	0.055
	(0.07)	(0.123)	(0.118)	(0.11)	(0.06)	(0.091)	(0.092)	(0.091)
in month $t + 96$	0.063	0.123	0.132	0.109	0.05	0.051	0.062	0.046
	(0.079)	(0.131)	(0.125)	(0.127)	(0.068)	(0.101)	(0.099)	(0.102)
in month $t + 150$	0.088	0.239	0.227	0.151	0.051	0.117	0.09	0.025
	(0.097)	(0.178)	(0.173)	(0.156)	(0.082)	(0.128)	(0.124)	(0.111)
Mean Coefficient	0.104	0.202	0.198	0.171	0.09	0.123	0.123	0.115
Mean Standard Error	(0.077)	(0.136)	(0.132)	(0.127)	(0.064)	(0.104)	(0.104)	(0.102)
Polynomial Order	2	2	2	2	2	2	2	2
Bandwidth Left	1	0.34	0.35	0.38	1	0.36	0.38	0.38
Bandwidth Right	1	0.34	0.37	0.38	1	0.36	0.37	0.38
Bin width	1	1	1	1	1	1	1	1
Triangular Kernel	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	No	No	No	No	Yes	Yes	Yes	Yes

Note: This table compares our baseline results (column (1) and column (5)) to three mean square error (MSE) bandwidth selection methods: (i) MSEone in columns (2) and (6), (ii) MSEtwo in columns (3) and (7), and (iii) MSEsum in columns (4) and (8). These methods are implemented in the *rdrobust* package (Calonico et al., 2015).