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The Introduction of Social Pensions and Elderly Mortality: Evidence 1870-1939

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Philipp Jäger[1]

The Introduction of Social Pensions and Elderly Mortality: Evidence 1870–1939

Abstract

The strong association between income and mortality raises the question whether more generous social security systems could improve poor people's health outcomes. Thus, in this paper, I analyze whether a major social security innovation, the introduction of social pensions targeted at poor elderly people in the late 19th–early 20th century, has reduced mortality rates of senior citizens. Therefore, I use a cross-country dataset spanning from 1870 to 1939 consisting of 13 countries of which 9 eventually implemented social pensions before World War II. Applying a difference-in-difference-in-difference as well as a regression discontinuity design, I find no evidence for a decline in elderly mortality due to the introduction of social pensions. Based on aggregate census data, I argue that social pensions have reduced elderly labor supply. The reduction is much smaller than social pension recipiency rates, though. These findings suggest that social pensions have raised elderly incomes which, however, did not translate into lower mortality.

JEL Classification: H55, I18

Keywords: Pension; social security; elderly mortality

May 2019

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1 Introduction

Over the last three centuries, life expectancy in developed countries has increased tremendously. The benefits of this evolution, however, were not equally spread. While mortality inequality was arguably limited prior to the 18th century,[1] class-dependent mortality differences are well documented from the mid 19th century onward.[2] Today, health inequalities between rich and poor households are immense. For the US, Chetty et al. (2016) calculate that the life expectancy gap between the richest and the poorest one percent amounts to 15 years for men and 10 for women. In Europe, life expectancy also varies closely with income (Mackenbach, 2006).

The strong association between income and mortality raises the question whether more generous social security systems could improve poor people's health outcomes – a highly relevant question in a time where population aging puts pressure on the functioning of social security systems. To shed light on the social security-health relationship, I analyze the mortality implications of a major social security innovation, the introduction of means-tested social pensions in the late 19th–early 20th century across Europe and North America. The historical setting allows me to estimate the full mortality effect of the establishment of a non-contributory pension system, from essentially no program to an old-age assistance scheme that covered a substantial share of the elderly population.

A priori, the effect of social pensions on elderly mortality is unclear. Lifting the income constraint by providing supplementary public transfers should increase health-beneficial consumption including more or higher quality nutrition, housing related expenses (rent, energy) or basic healthcare and hygiene products. Given that social pensions targeted poor elderly people in a time of limited public assistance, one might expect a substantial positive health effect of even modest income transfers. However, when social pensions were paid, pension recipients had lived most of their lives already. Thus, assuming that health is a stock variable that depends on previous investments (Grossman, 1972), additional health-beneficial spending late in life has only modest effects on survival rates if the elderly have already accumulated irreversible health deficits. The lack of advanced medical technologies to treat chronic and infectious diseases at the time reinforces this point. In fact, income transfers might therefore have bought only limited additional life time for elderly people.[3] The analysis is complicated by the fact that means-tested social pensions tend to induce retirement (see e.g. for the US: Fetter and Lockwood, 2018; Friedberg, 1999; Parsons, 1991), which 1) results in an ambiguous overall income effect of social pensions and 2) has potentially direct health implications by reducing the risk associated with work (e.g. accidents) or causing social isolation or cognitive decline (e.g. Fitzpatrick and Moore, 2018; Snyder and Evans, 2006). Given the theoretical ambiguity, I estimate the mortality-social pension relationship empirically. To judge the effect of social pensions on retirement, I will also examine how labor market outcomes changed after the reform.

The existing literature suggests that the introduction of social pensions in middle-income countries in the last decades e.g. in China (Huang and Zhang, 2016; Cheng et al., 2018), Mexico (Aguila et al., 2015; Galiani et al., 2016) or South Africa (Case, 2004) improved elderly health outcomes. However, little is known about the health impact of social pensions in today's developed economies. Stoian and Fishback (2010), Balan-Cohen (2008) and Emery and Matheson (2012), examining the mortality response to the introduction of

[1] For instance, Harris (2004) reports that English aristocrats had no life expectancy advantage over ordinary people before 1750.

[2] See among other Harris (2004); Antonovsky (1967) and Costa (2015).

[3] Although, today many premature deaths are caused by the consumption of tobacco, alcohol or simply to many calories (Danaei et al., 2009), these "risky" consumption habits have played a much smaller role in late 19th–early 20th century. In this period, infectious diseases were a much bigger threat than chronic diseases (Cutler et al., 2006). Furthermore, smoking tobacco was far less common than today. Thus, the introduction of social pensions has most likely not lead to a substantial increase in health-deteriorating consumption.

social pensions in the US and Canada, are notable exceptions but reach different conclusions. While Balan-Cohen (2008) finds sizable declines in elderly mortality of -22% in the US, Stoian and Fishback (2010) (for the US) and Emery and Matheson (2012) (for Canada) argue that the effect is economically zero (and statistically insignificant). In this paper, I investigate the introduction of social pensions in 9 countries including Canada and the US but mostly taking place in Northern and Western Europe for which evidence is especially scarce. The focus on more than one reform allows me to draw more general conclusions about the mortality implications of social pensions.

To investigate the mortality-social pension relationship empirically, I substantially extend the cross-country analysis in the first part of Huang and Zhang (2016)[4], who, by comparing elderly and middle age mortality trends in 10 countries, argue that the introduction of social pensions has reduced elderly mortality. To do so, I first add a control group–the elderly in countries without a pension reform–which should control for the effect of medical progress (e.g. insulin and sulfa drugs) and public health interventions (e.g. vaccination, sanitation, pasteurizing milk) on elderly mortality. Moreover, I also control for a range of potentially confounding variables as well as of events happening around the introduction of social pensions, e.g. wars, GDP per capita, education, democratization and other welfare reforms. Secondly, I make use of the detailed age-specific mortality data set and estimate whether there is evidence for a mortality jump around the legal retirement age. Thirdly, I focus on the period before World War II (WW II) because of its mortality implications and the significant welfare state expansion that followed and potentially confounds the estimates. Last but not least, to my knowledge, I also provide the first cross-country evidence on labor market reactions to the introduction of social pensions.

Thus, in this paper, I apply two complementing research strategies, a difference-in-difference-in-difference (DDD) estimator combined with an event-study design and a regression discontinuity (RD) approach. Using the DDD estimator, I compare the change in mortality rates of senior citizens (age $\geq$ legal retirement age) after the implementation of social pensions with the mortality change of two control groups: 1) middle-aged citizens (45+) in "treated" countries and 2) their same age counterparts in "non-treated" countries. In the RD-framework, I investigate the mortality outcomes around the legal retirement age in "treated" countries before and after the implementation of social pensions.

In contrast to Huang and Zhang (2016), I find no evidence for an economically significant decline in total elderly mortality. Using aggregate census data for 6 of the 9 countries[5] that introduced social pensions, I also show that average labor force participation of the elderly relative to working-age adults declined substantially after social pensions were introduced. The share of people receiving these pensions by far exceeds any plausible labor force participation decline, though. These findings suggest that social pensions have raised elderly incomes, which, however, did not translate into lower mortality.

In the next section, I describe the concept of social pensions and their implementation across the sample countries. Section 3 introduces the data as well as the empirical approach, while the baseline results and robustness checks for the mortality specification are presented in sections 4 and 5. I discuss the reasons for the absence of a positive mortality effect in chapter 6. In this context, I also provide evidence for labor market responses. Finally, section 7 concludes.

[4]This is not a critique of Huang and Zhang (2016). The authors mainly focus on the impact of social pensions in China and thus devote little space to the cross-country analysis.

[5]Comprehensive data is missing for the remaining 3 countries.

2 Social pensions

Public pension schemes can be broadly categorized into insurance- and assistance-based programs. In this paper, I focus on the assistance-based schemes, which I call "social pensions" throughout the text. These pensions were first introduced in Denmark in 1891. In contrast to the pension system introduced in Germany under Chancellor Bismarck in 1889, which was constructed as a mandatory contribution-based public insurance scheme, Denmark set-up a pension model that provided tax-financed, means-tested minimum pensions for elderly people above the legal retirement age. In the following years, both pension models, the Bismarckian and the social pension model, spread across Europe and Northern America. Table 1 shows the 9 sample countries that introduced social pensions before WW II. Many elderly people in the sample countries benefited from social pensions, on average around 35% of the eligible age groups received these pensions. However, recipiency rates differed across countries. Average replacement rates (pensions relative to average wages) were rather modest ranging between 8% and 33% and covered only very basic needs. In contrast to the insurance type pension models that relied on previous worker contributions, social pensions were typically rewarded right after their implementation. More details about the pension reforms in the 9 affected countries as well as the situation in the 4 "control" countries (Finland, Italy, Spain and Switzerland) that have not introduced social pensions before WW II are provided in Appendix A.

Table 1: **Social pension characteristics**

Country	Belgium	Canada	Denmark	France	Netherlands	Norway	Sweden	UK	USA
Implementation	1901	1929	1891	1907	1913	1937	1913	1909	1935
Retirement age	65	70	60	70	70	70	67	70	65
Recipiency rate[a]	40%	17%	23%	20%	(50%)	80%	40%	45%	18%
Replacement rate	>8%*	18%	13-33%*	8-16%*	21%*	15%	14-21%	22%	>21%*

[a]Recipiency rates for the closest available year after the implementation, however, maximum 3 years after implementation. *Calculated assuming 250 workdays. If average wages are missing, the replacement rate is calculated against the lowest and highest available wage group. Sources: Belgium: Brenner et al. (1991); de Bernonville (1911) Canada: Palme (1990); Denmark: de Bernonville (1911); Khaustova and Sharp (2015) ; France: de Bernonville (1911); Levasseur (1909) ; Netherlands: Dorrestijn and Kingma (2008); Radhakrishnan (1993); Smits et al. (2000), recipiency rates might have been considerably lower, if numbers in Dorrestijn and Kingma (2008) refer to overall and not annual recipients; Norway: Palme (1990); Sweden: Hagen (2013); UK: Old-age Pension Act (1908); de Bernonville (1911); Feinstein (1990); USA: Balan-Cohen (2008); Friedberg (1999); Huberman and Minns (2007)

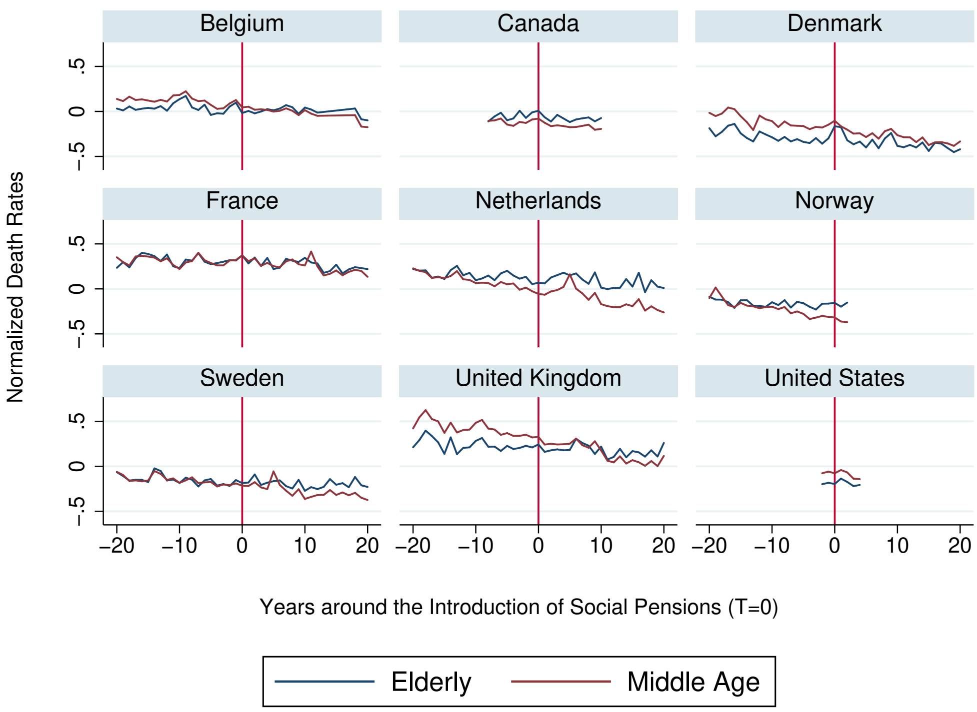
Table 1 also shows the striking time variation in the adoption of social pensions. So far, the empirical literature has not yet identified economic, political or demographic variables that robustly predict why some countries have introduced public pensions earlier than others.[6] I will argue in section 5 that the introduction of social pensions is not driven by mortality trends. Public pension programs have often been discussed well in advance of their implementation but either faced strong opposition from certain interest groups (e.g. friendly societies in the UK, the liberal party in Norway etc.) or were delayed because one could not agree on the specific design of the system (insurance vs. assistance). Similarly, the choice of the legal retirement age was typically not derived from specific needs, but mostly based on the expected costs (e.g. in Canada: Emery and Matheson, 2012; Norway: Bjørnson, 2001; UK: Casson, 1908).

[6]Palme (1990) investigates the role of industrialization, the age composition of the population and the level of working class mobilization and finds some evidence that higher working class mobilization (voting for left parties, union density) is associated with a more generous pension system in the year 1930. However, working class mobilization, as well as all other tested variables, have only very limited explanatory power for the cross-country differences in pension generosity. Lindert (1994) uncovers that public pension expenditures between 1880 and 1930 are positively related to the share of elderly people relative to the working age-population and negatively to the share of people working in agriculture. In contrast, Cutler and Johnson (2004) do not find that the share of the elderly or the share of people working in industry or cities predicts which countries introduce public pensions earlier than others. Moreover, in line with Palme (1990), none of the other factors tested (e.g. income differences, democracy, ethnic-diversity, religion) explain the cross-country differences in the adoption of public pension systems very well.

3 Empirical strategy and data

One simple way to quantify the health effect of social pensions would be to compare mortality rates of the affected age groups (age $\geq$ legal retirement age) before and after these pensions have been introduced. Figure 1 shows that, in most countries, mortality trends of eligible age groups have changed very little after the reform. Elderly mortality rates in some countries such as Denmark and the UK seem to have reacted to the reform, though. A simple before and after comparison, however, is only valid if we believe that mortality changes are solely driven by the introduction of social pensions, an assumption unlikely to be true especially during a time characterized by rapid industrialization and groundbreaking medical inventions. Figure 1 also shows that the drop in elderly mortality in the UK for instance is mimicked by a decline in middle-aged (45 $\leq$ age $<$ legal retirement age) mortality, even though these age groups are not directly affected by the introduction of social pensions. The mortality decline is therefore likely driven by something else than the pension reform.

Figure 1: **Age-specific mortality around the introduction of social pensions**



Normalized death rates = $log(mortality)$ minus average $log(mortality)$.

3.1 The difference-in-difference-in-difference model

My DDD strategy accounts for potential confounding factors by using two control groups and augmenting the regression by a range of control variables. The first control group consists of middle-aged people, i.e. age groups not yet eligible for social pensions but older than 45, in countries that introduced social pensions. This control group is intended to capture general mortality fluctuations in "treated" countries that are not due to the pension reform. Given that I remove all age-specific mortality variation that is common across countries (e.g. by including age*year fixed effects), the analysis will not be biased by the fact that average middle-aged mortality has declined faster than average elderly mortality. One crucial assumption behind the use of this control group, which I will discuss in more detail in Appendix C, is that middle age mortality is not (or only to a very limited degree) affected by the introduction of social pensions. The second control group, elderly people in not "treated" (or not yet "treated") countries, should account for global health shocks that specifically affected elderly people.

Therefore, I estimate the following empirical model:

$$log(mortality)_{itjg} = \delta(pension_{it} * R_{ij}) + \phi pension_{it} + \theta_t(R_{ij} * b_t) + \omega R_{ij} + \beta \mathbf{X}_{it} + \lambda_{itjg} + \varepsilon_{itjg} \quad (1)$$

where i denotes country, t time, j age group and g gender. The dependent variable is the natural logarithm of the mortality rate, which is simply the number of deaths relative to the size of the respective age group. The two main dummy variables are $pension_{it}$, equaling one after social pensions have been introduced, and the retirement age dummy R_{ij}, which is one if the respective age group is eligible for social pensions. The DDD mortality effect of social pensions is given by δ, the coefficient on the interaction between the social pension and retirement age dummy. I control for the evolution of mortality rates of elderly people in non-treated countries by the interaction of the retirement age dummy with year fixed effects ($R_{ij} * b_t$). In addition, I include country, age, gender and time fixed effects (λ_{itjg}) as well as all combinations of age-gender-country to allow for a different intercept for every age-gender-country pair, thus essentially estimating a fixed effects model that is identified using the variation over time. Moreover, I add age-year fixed effects to allow age-specific mortality rates to vary every year. Further combinations such as age-gender-year or country-year are included as a robustness check. Moreover, I include controls $\mathbf{X}_{it}$ such as average years of schooling, GDP per capita and a war dummy, which equals one if annual battle deaths exceed 0.1% of the male population in the age group 18 to 49[7]. I allow the effect of these controls to vary by age group as a robustness check. To define which age groups belong to the control group, I use a hypothetical retirement age for the never-treated countries of 70 because it is the most common retirement age in the sample. In the Appendix, I also report the results for a hypothetical retirement age of 65 (see Table C.1). The retirement age choice does not affect the overall conclusion, though.

The underlying assumption of the DDD design is that mortality in the treatment group, which consists of eligible age groups in countries that introduced social pensions, would have behaved like the control groups in absence of a reform. To show the plausibility of this assumption, I also estimate an event-time regression which allows me to visualize the co-movement of treatment and control group mortality in the past. I employ an event-time model with a relatively narrow time window— 5 years on both sides around the introduction of social pensions— because limited observations are available for the reforms that were implemented at the end of the sample period. Moreover, in this case the event-study can also serve as an additional robustness check because the estimates are less confounded by other events happening years after the implementation of social pensions.

For the event-time study, I substitute the $pension_{it}$ dummy by event-time dummies π^e_{it} (event-time: e) that are defined by their temporal distance to the introduction of social pensions, which I interact with a retirement age dummy and estimate:

[7] Since the Correlates of War database does not include annual death tolls, I divide the number of deaths by the duration of the respective war to obtain annual figures. The sample countries were only engaged to a very limited extent in the first year of WW II, hence I assign the war dummy the value zero in 1939.

$$log(mortality)_{itjg} = \sum_{e=-6}^{6} \delta_e(\pi_{it}^e * R_{ij}) + \sum_{e=-6}^{6} \phi_e \pi_{it}^e + \theta_t(R_{ij} * b_t) + \omega R_{ij} + \beta \mathbf{X}_{it} + \lambda_{itjg} + \varepsilon_{itjg} \quad (2)$$

I include 13 event-time dummies, which comprise of 5 pre- and 5 post-reform year dummies, a dummy for the year of the introduction as well as two dummies to bin-up end-points in the spirit of McCrary (2007). Hence, the event-time dummy π_{it}^{-2} equals one 2 years before a social pension is implemented, while π_{it}^{+2} is one 2 years after the introduction. The dummy π_{it}^{-6} is one for all years before the reform, while π_{it}^{+6} is one for all years after the reform. Thus, the dummies capture mortality differences, across all included age groups, between "treated" (exposed to pension reforms) and "non-treated" countries (never exposed or not yet exposed to the reform). The coefficients δ_e should therefore capture the evolution of elderly mortality rates in treated countries relative to the two control groups before as well as after the introduction of social pensions. Additionally to the general DDD estimate, I also estimate the mortality effect using only x years around the pension reform by the following formula:

$$\frac{1}{x}\sum_{e=1}^{+x} \hat{\delta}_e - \frac{1}{x}\sum_{e=-x}^{-1} \hat{\delta}_e \quad (3)$$

I report results for each time window x between 1 and 5, because choosing an appropriate bandwidth is necessarily a trade-off. Using a narrow time window comes with the benefit that the estimates are less polluted by alternative events occurring around the implementation date that are not captured by the controls. Moreover, I have a balanced dataset in event-time data only up to the 2 year bandwidth, because data for either the US and/or Norway is missing. However, relying on a tight bandwidth makes the estimates also more vulnerable to outliers (e.g. a flu that especially affected elderly in some treatment countries) because fewer observations are used to calculate the effect. In line with most event-time studies, I do not include the year of the implementation (event-time=0) in the calculation because of potential teething problems (administrative, legal etc.).

3.2 The regression discontinuity design

The DDD design is only credible under the assumption of common underlying trends. Even though, the event-time framework allows to judge whether common trends have prevailed in the past, the assumption that these trends will continue is ultimately untestable. Moreover, although unlikely as I will argue in Appendix C, the results might be theoretically driven by an effect of the social pensions on the control group. For this reason, I complement the DDD strategy by a RD design that is less dependent on the common trend assumption because it puts more weight on the comparison of age groups that are very close to the legal retirement age and thus arguably more similar. Therefore, I estimate the following specification twice, for the period before as well as after the introduction of social pensions:

$$log(mortality)_{ierg} = f(r) + \gamma * R_{ir} + \varepsilon_{ierg} \quad (4)$$

where i denotes country, e event-time, r the age group relative to the legal retirement age and g gender. The dependent variable remains the natural logarithm of the mortality rate as in the DDD specification. The function $f(r)$ is a polynomial that controls for the age-mortality relationship below and above the legal retirement age. I allow the relationship to vary on either side of the discontinuity. R_{ir} is the legal retirement age dummy, which is one if the age group is theoretically eligible for social pensions. I am mainly interested in the change of parameter γ — which represents the mortality change at the age cut-off— over time. If nothing else changed around the legal retirement age during the time social pensions were introduced, the change in γ gives a credible estimate of the effect of social pensions on mortality rates of the affected age groups. Similar to the DDD event-time design, I zoom in on the time-window 5 years around the introduction of social pensions. Therefore, I compare the change in γ by first estimating equation 4 using the data for the 5 event-time years before the reform, and second using the 5 event-time years after the reform. To account for general age-specific mortality trends that might result in a "biologically natural" discontinuity around the legal retirement age, I also estimate equation 4 using detrended dependent variables.

I estimate the RD-model using fixed bandwidths of 15, 10 and 5 age groups around the cut-off. Moreover, I also use a "data driven" bandwidth selection algorithm implemented by Calonico et al. (2014a,b) that minimizes the mean squared error of the RD point estimate. To end up with the same bandwidth for both models, I use the bandwidth that is selected for the period before the introduction of social pensions also for the period afterwards. Furthermore, to avoid overfitting, I estimate equation 4 using two low-order polynomials: linear and quadratic. Last but not least, in addition to an uniform kernel I also estimate the model using a triangular kernel that places more weight on the observations close to the cut-off.

3.3 Data

I use mortality data from the Human Mortality Database (HMD), which provides high-quality, comparable long-run age-specific mortality rates[8] on a range of developed countries. My sample focuses on 13 countries from Western and Northern Europe as well as North America (see Table B.1). Except for Canada (starting in 1921), Spain (1908) and the US (1933), mortality information is available for almost the entire sample period. Moreover, I only include age groups that are relatively close to the legal retirement age, ranging from 45 to 85 (41 age groups per gender-country-year combination) in the benchmark specification. The age groups are chosen so that they are not more than 15 age group-years away from the lowest (60) or highest (70) retirement age in the sample.

Given that my sample includes 13 countries and almost 70 years, I end up with more than 60'000 observations for the DDD regression. The number of treated age groups within 2 years around the introduction of social pensions amounts to 334 (9 countries * on average 18.55 age groups above the retirement age * 2 genders) in each event-year. Five years after the reform, this number falls to 260 because the US and Norway drop out of the sample given that they introduced social pensions at the end of the sample period. Similarly, the US drops out in event-year -3 to -5, resulting in 292 age groups in this period, because the US only enters the sample in 1933. GDP per capita stems from The Maddison-Project (2013), years of schooling from Murtin (2012)[9] and war data comes from the Correlates of War database (Sarkees and Wayman, 2010). Summary statistics are provided in the appendix.

The RD design is estimated based on the 9 "treated" countries only. Therefore, I can use 18 observations per age group for every event-time year (9 countries * 2 genders). Given that I focus on the 5 event-years before and after the introduction, I end up with 84 (before) and 82 (after) observations per age group in the

[8] Specifically, I use death rates directly provided by the HMD.
[9] Given that schooling data is only available at a 10 year interval, I linearly interpolate the data in order to obtain annual figures.

RD design.[10] In line with the DDD estimate, I also restrict the sample to ± 15 age group-years around the legal retirement age.

4 Results

DDD results

Social pensions have not reduced elderly mortality according to the DDD model (Table 2). Irrespective of the specification, elderly mortality in treated countries increased by around 3% relative to the control age groups, an estimate that is not statistically significant, though. Zooming in on the years surrounding the introduction of social pensions confirms the absence of a positive health effect. Figure 2, plotting the estimated coefficients $\hat{\delta}_e$ from the DDD event-time model shows that elderly mortality compared to middle-aged mortality has behaved similarly in treated and non-treated countries prior to the introduction of social pensions, which supports the common trend assumption. After social pensions were introduced elderly mortality reacted very little, if anything mortality increased. In fact, the average mortality change is positive for every bandwidth ranging from 1.0% to 2.3% (Table 3). Again the estimates are mostly not statistically significant. Overall, the DDD estimates suggest that elderly health outcomes have not substantially benefited from the introduction of social pensions.

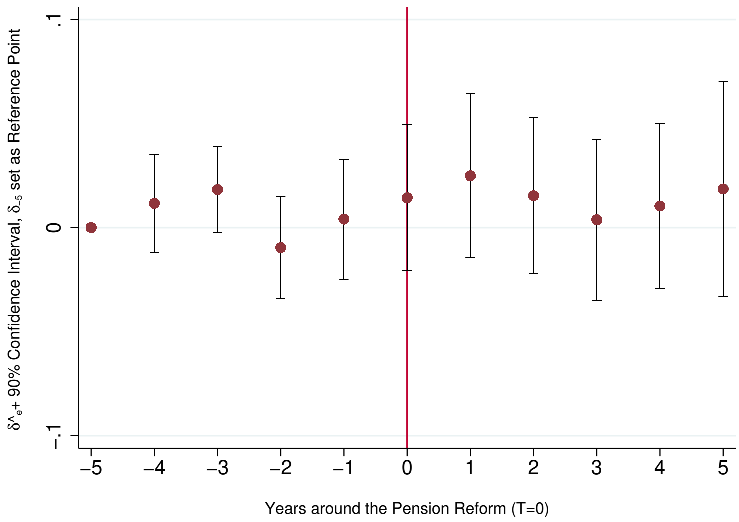
Table 2: **DDD results**

	1	**2**	**3**	**4**	**5**	**6**
DDD-estimate	2.9% (0.047)	2.9% (0.047)	3.1% (0.047)	3.3% (0.048)	3.3% (0.049)	3.0% (0.045)
Age-gender-country f.e.	No	Yes	Yes	Yes	Yes	Yes
Controls	No	No	Yes	Yes	Yes	Yes
Age-year f.e.	No	No	No	Yes	Yes	Yes
Gender-age-year f.e.	No	No	No	No	Yes	No
Country-year f.e.	No	No	No	No	No	Yes
N	60,434	60,434	60,434	60,434	60,434	60,434

Standard errors in parentheses and adjusted for clustering at the country level. Specification 1 includes age, gender, year and country fixed effects without interactions as well as the interaction between the retirement age dummy with the country and year dummies. The coefficients are multiplied by 100 to arrive at the %-interpretation. Given the small size of the coefficients, I consider this approximation appropriate.

[10] Without missings, I would end up with 90 observations per age group (9 countries * 2 genders * 5 years) Since some years for the US and Norway are missing (as described above) overall sample size is slightly lower.

Figure 2: **DDD event-time model**



Event-time model estimated using specification 4 from Table 2 including age-gender-country f.e., controls and age-year f.e. For the ease of visualization, δ_{-5} serves as reference category.

Table 3: **DDD Event-time model results**

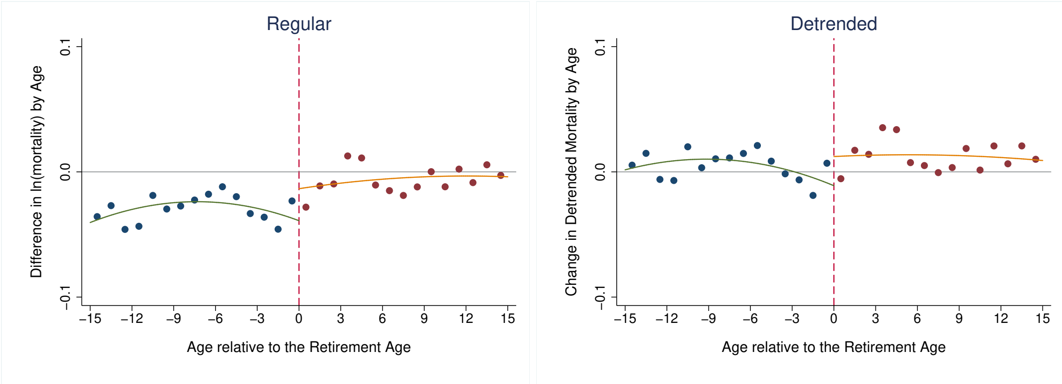
Time window (in years)	**5**	**4**	**3**	**2**	**1**
Benchmark	1.0% (0.015)	0.7% (0.012)	1.0% (0.011)	2.3% (0.010)	2.1% (0.013)
$CI_{90\%}$	[-1.7% , 3.7%]	[-1.5% , 2.9%]	[-1.0% , 3.0%]	[0.5% , 4.1%]	[-0.2% , 4.4%]

Coefficient depending on time window x:$\frac{1}{x}\sum_{e=1}^{+x}\hat{\delta}_e - \frac{1}{x}\sum_{e=-x}^{-1}\hat{\delta}_e$. Event-timel model estimated using specification 4 from Table 2 including age-gender-country f.e., controls and age-year f.e. Standard errors in parentheses and adjusted for clustering at the country level. To account for the low number of clusters, critical values are calculated using the number of clusters minus 2. The coefficients are multiplied by 100 to arrive at the %-interpretation. Given the small size of the coefficients, I consider this approximation appropriate.

RD results

Figure 3, plotting the mortality change by age group, supports the findings from the DDD model. Focusing on the 5 years around the introduction of social pensions, mortality for age groups slightly below the retirement age has declined faster than for the age groups slightly above the legal retirement age. This pattern remains true if the age-specific mortality rates are detrended to account for different age-specific mortality trends. The graphical evidence is supported by the empirical results presented in Table 4. Most specifications show an increase in mortality after social pensions were introduced, with the only exception being the estimate using the regular mortality rate with a data-driven bandwidth of 8 age groups and a quadratic polynomial. The mortality decline is not statistically different though as the confidence intervals of the γ-before and γ-after coefficients overlap. However, this is also true for the other specifications. The results are similar if other combinations of the mortality rate (regular or detrended), age group bandwidth, order of polynomial and kernel function are estimated (see Table C.4 in the appendix). The majority of the specifications show an insignificant mortality increase above the age cut-off or a very small insignificant decrease. The appendix provides graphical evidence for the results of Table 4 (before & after separated: D.1).

Figure 3: **Mortality change after the implementation of social pensions by age group**



Each dot represents the mortality change after the introduction of social pensions for the 9 treated countries (both genders). The change is calculated by taking the average mortality rate for the 5 years after the reform (Event-time: 1 to 5) and subtracting the average mortality rate of the 5 years before (Event-time: -5 to -1). Detrended mortality rates = residuals e_{itjg} from the regression: $ln(mortality)_{itjg} = age_i + time\ trend_t + age_i * time\ trend_t + e_{itjg}$. Regular mortality refers to $ln(mortality)$ not detrended.

Table 4: **Regression discontinuity results**

Specification	γ-before	γ-after	After−Before
Regular mortality, 15 age groups, quadratic, uniform kernel	-0.8% (0.039)	1.2% (0.040)	2.0%
Detrended mortality, 15 age groups, quadratic, uniform kernel	-1.6% (0.022)	0.4% (0.022)	2.0%
Regular mortality, optimal bandwidth (8 age groups), linear, triangular kernel	0.1% (0.040)	1.8% (0.043)	1.7%
Regular mortality, optimal bandwidth (8 age groups), quadratic, triangular kernel	0.4% (0.065)	-0.8% (0.069)	-1.2%
Detrended mortality, optimal bandwidth (9 age groups), linear, triangular kernel	-2.4% (0.021)	-0.5% (0.021)	1.9%
Detrended mortality, optimal bandwidth (10 age groups), quadratic, triangular kernel	-3.1% (0.031)	-2.7% (0.031)	0.4%

$log(mortality)_{ierg} = f(r) + \gamma * R_{ir} + \varepsilon_{ierg}$ estimated for the 5 years before and 5 years after the introduction of social pensions. Detrended mortality rates = residuals e_{itjg} from the regression: $ln(mortality)_{itjg} = age_i + time\ trend_t + age_i * time\ trend_t + e_{itjg}$. Regular mortality refers to $ln(mortality)$ not detrended. Standard errors in parentheses. All regressions are weighted based on a triangular kernel using the rdrobust command (Calonico et al., 2014a,b). The coefficients are multiplied by 100 to arrive at the %-interpretation. Given the small size of the coefficients, I consider this approximation appropriate.

Summary

Neither the DDD nor the RD models suggest that the introduction of social pensions has reduced elderly mortality, which is surprising given the scale of the programs with average recipiency rates of around 35% shortly after introduction. Unfortunately, however, the standard errors are too big to rule out relevant health effects in most specifications. The DDD event-time model, however, is more precisely estimated. According to the 90-percent confidence interval (Table 3), I can reject the hypothesis that mortality declined by more than 1.7% (in absolute terms) for the 5 year bandwidth. The value is even smaller (also in absolute terms) for the other bandwidths. Based on a simple back-of-the-envelope calculation using the 35% pension recipiency rate, the 1.7% overall elderly mortality decline translates into a 4.9 $(1.7 * \frac{1}{0.35})$% decline for the pension-receiving elderly. This calculation is based on the assumption that there is no mortality effect of social pensions on non-receiving elderly and that the original mortality rate of the treated is the same as for the non-treated. Given that mortality rates of treated, hence poor, elderly people were almost certainly higher than mortality rates of non-treated elderly people— the summary by Antonovsky (1967) shows that poor elderly excess mortality in this time period ranges from more than 400% to around 20%[11]— the 4.9% decline constitutes, in absolute terms, an upper bound estimate. Thus, I conclude that the substantial mortality decline of -22% estimated by Balan-Cohen (2008) for the US or the -11% decline in China according to Huang and Zhang (2016) cannot be generalized.

[11] Estimates depend on the region, available data, the poor/rich cut-off and the exact time period.

5 Identification issues & robustness checks

In this section, I discuss the most relevant identification problems. Further discussions and robustness checks are presented in the Appendix C.

Reverse causality

My identification strategy is flawed if the decision to introduce social pensions is driven by elderly health trajectories. In other words, if social pensions are implemented because elderly mortality shifts, I wrongly attribute the underlying mortality change to the implementation of social pensions. However, I do not consider reverse causality as a major threat for my analysis for three reasons. First, high or increasing elderly mortality was not officially stated as a rationale for implementing pensions. Second, using a simple Cox- proportional hazard model, I find no evidence that the level of elderly mortality predicts the timing of social pension introduction. If anything, countries with lower elderly mortality introduce social pensions earlier, the result is not statistically significant, though. Third, elderly mortality relative to middle-aged mortality has not increased substantially in the 5 years before the reform as already shown in Figure 2. Hence, the research design would only be flawed if social pensions are a reaction to expected future shifts in elderly mortality. I doubt that policy makers had sufficient information to anticipate such changes.

World War I and the Spanish flu

My sample period includes two major events that have heavily affected mortality patterns in the beginning of the 20th century, WW I (1914-1918) and the Spanish flu (1918-1920). To make sure that my results are not driven by these two events, I have estimated the DDD model 1) using data before 1913 and thus including 4 reforms only (Denmark 1891, Belgium 1901, France 1907, UK 1908) and 2) without countries heavily affected by the war (Belgium, France, Italy and the UK). Moreover, as an additional robustness check, I have added a Spanish flu dummy which I interact with the country and age fixed effects, to allow for country and age-specific mortality effects of the flu. The results are broadly identical. Similarly, the RD results do not change substantially if I exclude war and Spanish flu years.

Heterogeneous effects

To understand if subgroups of people or countries drive the results, I have conducted a range of heterogeneity checks. Therefore, I interact the DDD estimate with subgroup dummies. First, I analyze whether male mortality reacts differently than female mortality. Second, I examine whether the results are driven by small pension reforms. Therefore, I add a high-recipiency dummy which is one if recipiency rates, as reported in Table 1, exceed 40% (the 40%-threshold divides the sample most evenly). In a similar vein, I check whether the legislated retirement age makes a difference by adding a "retirement age 70" dummy. Furthermore, I include a public health insurance and accident insurance dummy which is one after a country has implemented these social security programs.[12] None of the results suggest that the overall DDD estimate hides relevant mortality declines of certain subgroups. Interestingly, the estimates suggest that the mortality increase is even stronger after high-recipiency reforms.

[12]Data for the introduction of public health insurance stems from Cutler and Johnson (2004) and from the Social Security Administration for public accident insurance.

6 Why did elderly mortality not decline?

Labor supply response

Labor market adjustments might be responsible for the absence of a positive health effect, because they 1) affect the size of the overall income change and 2) potentially have a direct health effect. Given that evidence on the labor supply effect of social pensions is lacking for all of the sample countries except the US (Fetter and Lockwood, 2018; Friedberg, 1999; Parsons, 1991), I collected aggregate age-specific labor force participation rates for 6 of the 9 sample countries (Canada, France, Netherlands, UK, USA, Sweden) for which comprehensive data is available.[13] Figure 4 presents the average cross-country change in participation rates of the elderly relative to working-age adults for the two censuses before as well as one census wave after the introduction of social pensions. Since participation rates differ dramatically by gender, I report results separately for women and men. For the ease of interpretation, I also set the differential to zero in the first census wave (T_{-2}: two waves before the introduction of social pensions). In fact, average elderly labor force participation was already substantially lower— 32.5 percentage points for men, and 15.0 percentage points for women— in the first census wave. In contrast to the mortality data, the participation rates are only available for broad age groups, differ by definition and vary by countries (see Table 5 for the age groups and census years used).

Regardless of the data limitations, Figure 4 strongly suggests that elderly labor force participation of men fell due to the implementation of social pensions. In the two census waves preceding the introduction of social pensions, participation rates of the elderly and people in working-age moved in tandem; between T_{-2} and T_{-1}, the participation gap changed by less than 0.2 percentage points. After social pensions were introduced (between T_{-1} and T_{+1}), elderly participation rates (relative to the control age groups) declined by 6.2 percentage points, which is in line with the 8.5 percentage point reduction estimated by Fetter and Lockwood (2018) for the US. Moreover, the decline was universal across countries, i.e. elderly participation decreased faster in every country after the reform. The effect is less clear for women which might be partly driven by the fact that female labor force participation rates were not adequately recorded.[14] The female elderly participation rate also decreased by 2.3 percentage points after the implementation of social pensions, however, the participation rates of older women already declined by 1.5 percentage points between T_{-2} and T_{-1}.

The analysis suggests that the implementation of social pensions has indeed induced (male) retirement. Thus, on average, incomes of the pension-receiving elderly have increased by less than the amount of social pensions. For at least three reasons, however, the labor supply response most likely does not fully explain the absence of a positive health effect. First, pension recipiency was manifold higher than any plausible decline in elderly labor force participation. This remains true if we consider that a part of the pension-receiving elderly might have also responded by working fewer hours. Second, even for people that retired or worked less, absolute incomes did not necessarily decline if previous earnings were low. Fetter

[13] I have also collected employment figures for the 13 sample countries from a range of sources mostly following Madsen (2009) and estimated an event-time model to investigate the overall employment response to the introduction of social pensions. I find that overall employment within 5 years after the introduction of social pensions has hardly changed. Given that elderly people constituted only a small fraction of the employed workforce and elderly labor force participation was lower than for the rest of the workforce, however, even an economically significant decrease in elderly employment would translate into very small changes in total employment and therefore might be masked by fluctuations in non-elderly employment. This problem is aggravated by the fact that the historical employment figures are often based on interpolated series, and hence do not always represent the employment situation in a given year adequately. Therefore, I decided to focus on the change in elderly participation rates derived from census data.

[14] Labor force participation before 1940 was mainly based on the concept of gainful employment. Married women that worked for no pay in a family business or a farm might have recorded no occupation and thus are not considered as part of the labor force.

and Lockwood (2018) show for the US that indeed persons with low earnings potential were more likely to stop working and that nearly half of the reduction in labor force participation was due to exits from unemployment or work relief programs. Third, since working was riskier before WW II (for the US: e.g. Costa and Kahn, 2004), the health effect of retirement itself was arguably more positive than today and most likely outweighed the risks of cognitive decline and social isolation.

Figure 4: **Labor supply response to the introduction of social pensions**

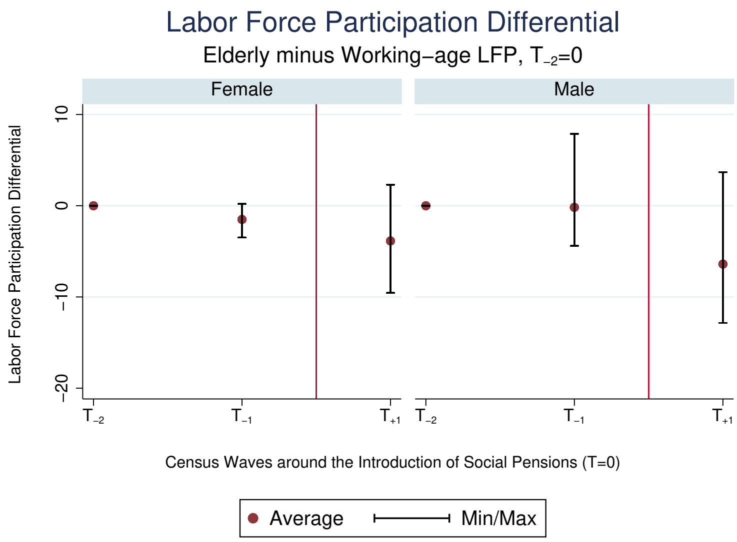


Table 5: **Labor force participation: Treatment and Control age groups by country**

Country	**Census years**	**Treatment**	**Control**	**Source**
Canada	1911, 1921, 1931	65+	25-64	Canada Statistical Yearbook (1922, 1943)
France	1901, 1906, 1911	65+	45-64	Bairoch (1969)
Netherlands	1899, 1909, 1920	70+	64-69	Statistik van Nederland (1899, 1909, 1920)
UK	1891, 1901, 1911	65+	25-64	Matthews et al. (1982)
USA	1920, 1930, 1940	65+	45-64	Bairoch (1969)
Sweden	1900, 1910, 1920	70+	50-59	IPUMS international and Kohli et al. (1991)

Netherlands before 1920: 71+ vs. 66-70, 1920: 70+ vs. 65-69.

Further explanations

If the decline in elderly labor force participation was not solely responsible, what other mechanisms have prevented a positive mortality effect? The displacement of other (public or private) transfers probably played a role, but has unlikely been decisive. The absence of a comprehensive social security system indicates that pensions were not directly offset by reductions in other public transfers. According to the Social Security Administration only Norway had an unemployment insurance system in place when social pensions were introduced. Private transfers, in contrast, probably declined, however, the literature from today's developing countries shows that the reduction in private transfers is typically much smaller than the amount of public pensions paid (≈25-30% according to Aguila et al. 2015 and Jensen 2004). The substantial labor market response also suggests that social pensions have indeed affected the budget constraint of elderly people. Overall, it seems most plausible that either the income shock came to late in life when elderly people had already accumulated irreversible health deficits. Or the pension was not generous enough, although income transfers of 10% to 20% of average earnings are a substantial amount especially given the absence of a comprehensive social security system. The still relatively basic medical technology, which lacked the tools to treat many (chronic and infectious) diseases, probably also limited the ability to buy more lifetime by consuming additional healthcare products.

7 Conclusion

Do more generous social security systems improve poor people's health outcomes? I argue in this paper that this is not universally true. Using a difference-in-difference-in-difference as well as a regression discontinuity design, I find no evidence that means-tested minimum pensions, which were introduced throughout Northern and Western Europe as well as North America in the late 19th–early 20th century, have reduced elderly mortality. The result seems surprising given the scale of these programs in a time of limited public assistance and might be partly driven by a fall in elderly labor force participation. I argue based on aggregated census data that the labor supply response is too small to completely offset the income transfer to the elderly. Thus, I conclude that social pensions have not reduced elderly mortality although elderly incomes most likely increased due to the reform. The results suggest that increasing the generosity of the welfare state alone will not necessarily be sufficient to narrow the life expectancy gap between rich and poor. The limited medical technology at the time potentially restricts the external validity of this result, though.

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A Social Pensions in the sample countries

The introduction of social pensions in *Denmark* was mostly due to the pressure of farmers (Petersen, 1990). Pensions were introduced in 1891 for every citizen aged 60 or older without sufficient means. Moreover, pensions were subject to a character test (e.g. no criminal record, not "responsible" for being poor based on an extravagant life-style etc.) (Petersen, 1990). The benefit level was determined on the municipality level and intended to be sufficient to support the poor elderly person plus the family.

Social pensions in *Belgium* implemented in 1901 (legislated in 1900) have been introduced as an advertisement for the new voluntary pension scheme for workers that were to old to contribute to the new system (British Medical Journal Group, 1910). Workers over 55 in the year 1901, received a fixed amount of pension benefits once they reached the age of 65. Afterwards, Belgium followed the insurance-based pension path and introduced a mandatory public pension system in the mid 1920s.

Social pensions in *France* were first legislated in the law from 1905 that granted means-tested pensions to people from age 70 upwards. The law took effect in 1907 and benefit levels were determined at the municipality level (Guillot, 1907). Like Belgium, France implemented an insurance-based pension system later (in 1910), which however took some time until it affected the elderly.

Public pensions in the *United Kingdom* go back to the Old-Age Pension Act from 1908, which provided means-tested weekly fixed minimum pensions for all people aged 70 and older. Pensions were first paid in January 1909. The legislation was inspired by the Danish pension model, however, a lower pension age was dismissed due to financing concerns. The UK stuck to the assistance type pension until WW II.

The *Netherlands* implemented their first old-age pension in 1913 as a special form of invalidity insurance. The invalidity insurance was mainly designed as a contribution-based system (Dorrestijn and Kingma, 2008), however, a governmental change delayed the implementation until 1919. In contrast, a transitory arrangement targeting the elderly poor that were already too old to contribute to the new scheme was implemented in 1913 (Westerveld, 1994). Under the 1913 scheme, former low-paid employees that turned 70 received a fixed amount of invalidity pensions.

In the same year, *Sweden* introduced two pension schemes at once, a contribution-based as well as a means-tested social pension program. Even though both schemes have been introduced at the same time, it took many years until the contribution-based pensions reached the level of the means-tested model (Hagen, 2013). In contrast, a fixed amount of social pension was paid out almost immediately after the introduction to needy people at the age of 67 and older.

Old-age pensions in *Canada* have been first introduced based on the Federal Old Age Pension Act from 1927. The act provided subsidies to the provinces to finance old-age pensions to people aged 70 and older. The majority of Canadians became eligible in the year 1929 when Ontario as well as 3 other western Provinces joined. Thus, I consider 1929 as the implementation date given that prior to 1929 only 6% of the population (the residents of British Columbia) were eligible. However, it took until 1936 when Quebec introduced old-age pensions, until all Canadian provinces had introduced old-age pensions.

Like Sweden, the *United States* introduced two different pension schemes at the same time. The Social Security Act from 1935 included 1) a federal contribution-based program which took until 1940 to pay regular benefits and 2) Subsidies to enable the federal states to legislate their own social pension program.

The pension age was set at 65, even though a small minority of states implemented a retirement age of 70 (Stoian and Fishback, 2010). Even before the introduction of the Social Security Act, some states had already implemented social pensions. According to Balan-Cohen (2008), the share of eligible elderly, however, was very limited before 1935 and increased considerably afterwards. Therefore, I chose to stick to the year 1935 as the implementation date of old-age pensions in the United States.

The first major public pension law in *Norway* was only legislated in 1936 and implemented in 1937, which made Norway a relative late-comer in welfare policy. The law provided minimum pensions for people over the age of 70, because a lower retirement age was considered to expensive.

Neither *Switzerland*, *Finland*, *Italy* nor *Spain* had introduced comprehensive social pension schemes before WW II. *Switzerland* set-up mandatory public pensions, including means-tested minimum pensions, right after WW II. *Finland* introduced pension legislation, as a mixture of a contribution-based and a means-tested system, in 1937, however, no benefits were paid before 1947 (Kangas, 2006). *Spain* and *Italy* followed the German tradition and both legislated contribution-based systems in 1919. Given the close link between contributions and pensions, only few elderly people received significant pensions from these schemes before WW II.

B Summary statistics

Table B.1: **Sample countries**

Country	First sample year
Belgium	1870
Canada	1921
Denmark	1870
Finland	1878
France	1870
Italy	1872
Netherlands	1870
Norway	1870
Spain	1908
Sweden	1870
Switzerland	1876
UK*	1870
USA	1933

*UK until 1922 without Northern Ireland

Table B.2: **Summary statistics**

	N	**Mean**	**sd**	**min**	**max**
Mortality rate	60434	0.06	0.06	0.0	0.4
War dummy	60434	0.05	0.22	0.0	1.0
GDP per capita	60434	3336.57	1500.98	1110	8636
Years of schooling	60434	5.36	1.90	0.5	8.7
Health Insurance dummy	60434	0.10	0.31	0.0	1.0
Accident Insurance dummy	60434	0.62	0.49	0.0	1.0
Polity IV democracy score	60188	3.80	6.58	-10.0	10.0

C Further robustness checks

In this Appendix chapter, I argue that the results are not substantially biased due to common identification problems such as a flawed control group or omitted variables. Moreover, I also report a range of robustness checks that are specific to my empirical setting such as the presence of an unbalanced panel in event-time, the choice of the age groups in treatment and control or the country composition of the sample. Results are presented in Table C.1, C.2 and C.3. Furthermore, I present the results for the RD model using different combinations of bandwidth, polynomial, weighting and mortality rate (Table C.4).

Flawed control group

Although the DDD event-time model points to common pre-treatment trends, I cannot rule out that the middle-aged in "treated" countries are also affected by the reform. In fact, social pension can influence middle-aged mortality through at least two channels. First, assuming no complete debt-financing, social pensions need to be financed by higher government revenues, lower public spending or a combination of the two. Second, social pensions could have partly replaced private (family) transfers to needy elderly people, which would result in higher incomes of middle-aged persons.

In practice both effects are likely to partly balance out, given that they imply two opposing directions of biases. If the public revenue/spending channel is more important, social pensions should rather raise middle-aged mortality rates due to declining incomes. The resulting downward bias would not endanger my overall findings. In contrast, the dominance of the private transfers channel would threaten the results, because my estimates would be upward biased, meaning that elderly mortality could in fact be negatively affected by the introduction of social pensions even though the estimated coefficients point in another direction. Unfortunately, I do not have sufficient information to judge which channel likely dominates. However, in the following, I argue that any potential upward bias is likely to be quite limited due to the small relative size of the treated elderly population. The upward bias is probably less of a concern for the RD model because it puts more weight on age groups close to the legal retirement age. In this case, the most relevant control age groups are already relatively old and are less likely to still support their parents (e.g. because they have already died). Therefore, the next paragraph focuses on the DDD model.

Within the sample, the 65 to 85 age group is around one third the size of the age group 45 to 64. Based on an average recipiency rate of 35%, the ratio of treated elderly people relative to the total 45-64 age group is hence around one ninth. Thus, even if for every treated elderly individuals, one person in the middle-aged group was better off due to the reform (because of lower family transfers), only a fraction (one ninth) would have benefited from the reform. Hence, for the estimated 3% elderly mortality increase (DDD benchmark specification) to be solely driven by the response of the control group, mortality of hypothetically affected middle-aged persons had to decline by a huge number.[15] A substantial short-run mortality decline simply driven by lower private transfers, however, is implausible, given the relatively modest pension replacement rates and the fact that overall middle-aged mortality declined only by around 40% over the entire sample period, a time of significant medical intervention and a doubling of GDP per capita. Based on these considerations, I do not expect that my overall results are driven by the private transfer channel.

As an additional robustness check, I have also run a simple difference-in-difference (DiD) model where I omitted the middle-aged as a control group. The results are presented in Table C.2, and show that elderly

[15] A simplifying back-of-the-envelope calculations suggest a necessary decline of 27% (9 * 3%).

mortality rates in "treated" countries also increased relative to mortality rates of elderly people in "non-treated" countries. However, the coefficients in the event-time model turn mostly slightly negative. The simple DiD model does not account for general health-shocks in "treated" countries and thus potentially wrongly attributes medical progress or other health related factors that in reality affect the whole population to the introduction of social pensions. Therefore, I consider the DiD results less reliable. The finding that the results do not differ by gender (see Section 5), suggests that cohabitation of one eligible and one non-eligible partner is not a big problem for the analysis considering that it was usually men who were older.

Omitted variables

The implementation of social pensions might have coincided with other welfare reforms, political changes or country-specific health shocks (e.g. diseases, medical breakthroughs). To reduce a potentially arising omitted variable bias, I augmented the DDD specification by a range of additional control variables. The results presented in Table C.2, show that the findings obtained from the benchmark specification are robust to the inclusion of several different dummy variable sets as well as other controls such as the existence of health and accident insurances, measures of democracy and the interactions of these control variables with the age dummies.

First, I allow age-specific mortality trends to differ between treated and non-treated countries by including a dummy for the treated countries which I interact with a time trend and the age group fixed effects. Second, I add two welfare reform dummies, a public health insurance and a public accident insurance dummy, which equals one if the respective system existed in the given year. Moreover, I add the "Polity score" from the Polity IV-project which ranges from -10 (hereditary monarchy) to +10 (consolidated democracy) to account for political changes. In addition, I allow all control variables to have age-specific effects by interacting them with the age dummies.

Further robustness checks

My results are robust to a range of additional robustness checks (see Table C.2) that I briefly describe in the following. To investigate whether my findings are affected by the existence of an unbalanced panel in event-time, because detailed mortality data for the US and Norway is lacking for some event-time years, I have estimated the event-time model without the US and Norway. I also re-estimated the model twice with different treatment and control age group definitions. Therefore, I raised the middle-aged entry age from 45 to 50 (55) and reduced the maximum age of senior citizens from 85 to 80 (75).

The role of individual countries

Neither the DDD nor the RD results are driven by the mortality behavior in specific countries, since the results are robust to excluding one country at a time. The only exception is the exclusion of Finland in the DDD model, in this case the DDD estimate becomes slightly negative (-0.3%) as presented in Figure C.1.

Figure C.1: **Sensitivity of the DDD coefficient**

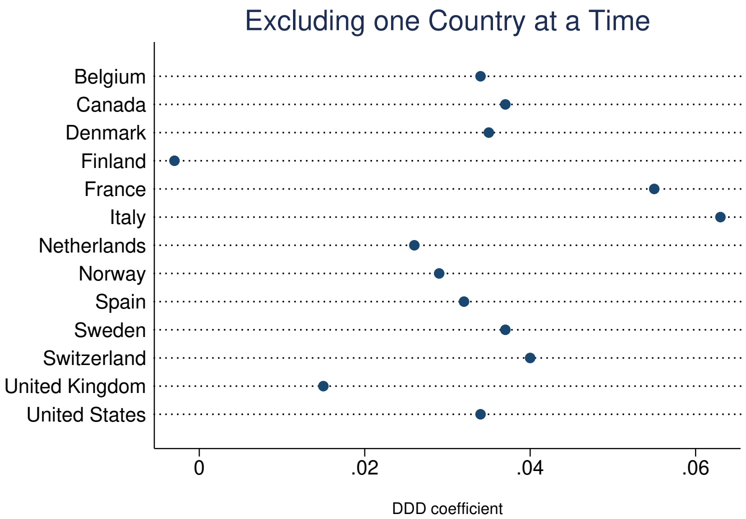


Table C.1: **Robustness checks DDD results**

	DDD-estimate	N
Hypothetical retirement age 70		
Allowing for different age-specific mortality trends in treated countries	3.2% (0.021)	60,434
Further controls + All Controls interacted with age f.e.	3.6% (0.041)	60,188
Only elderly control group	1.0% (0.015)	23,584
Narrower age groups (50-80)	2.8% (0.044)	45,694
Narrower age groups (55-75)	1.7% (0.041)	30,954
Only include treated countries	2.6% (0.02)	41,902
Only reforms before 1913	1.4% (0.033)	35,260
Without countries heavily affected by WW I	7.1% (0.047)	38,048
Control for Spanish flu years	3.2% (0.048)	60434
Hypothetical retirement age 65		
Allowing for country-specific annual shocks (country-year f.e.)	3.9% (0.042)	60,434
Including gender*age mortality shocks/trends (gender-age-year f.e.)	4.5% (0.043)	60,434
Allowing for different age-specific mortality trends in treated countries	3.0% (0.021)	60,434
Further controls + All Controls interacted with age f.e.	4.4% (0.038)	60,188
Only elderly control group	1.0% (0.015)	23,584
Narrower age groups (50-80)	4.1% (0.036)	45,694
Narrower age groups (55-75)	3.6% (0.029)	30,954
Only include treated countries	2.6% (0.02)	41,902
Only reforms before 1913	1.9% (0.028)	35,260
Without countries heavily affected by WW I	7.4% (0.045)	38,048
Control for Spanish flu years	4.4% (0.042)	60434

Standard errors in parentheses and adjusted for clustering at the country level. To account for the low number of clusters, critical values are calculated using the number of clusters minus 2. The coefficients are multiplied by 100 to arrive at the %-interpretation. Given the small size of the coefficients, I consider this approximation appropriate.

Table C.2: **Robustness checks DDD event-time results, Hypothetical retirement age 70**

Time window around the introduction (in years)	**5**	**4**	**3**	**2**	**1**
Allowing for country-specific annual shocks (country-year f.e.)	0.7% (0.013)	0.6% (0.010)	0.8% (0.009)	1.8% (0.009)	1.5% (0.013)
Including gender*age mortality shocks/trends (gender-age-year f.e.)	1.0% (0.015)	0.7% (0.012)	1.0% (0.011)	2.3% (0.010)	2.1% (0.014)
Allowing for different age-specific mortality trends in treated countries	1.1% (0.011)	0.9% (0.009)	1.2% (0.008)	2.4% (0.008)	2.2% (0.012)
Further controls + All Controls interacted with age f.e.	0.9% (0.015)	0.5% (0.012)	0.7% (0.010)	2.2% (0.008)	2.3% (0.013)
Only elderly control group	-0.4% (0.011)	-0.4% (0.008)	-0.8% (0.008)	0.2% (0.011)	-0.5% (0.018)
Excluding Norway and USA	1.2% (0.018)	1.0% (0.015)	1.2% (0.013)	2.5% (0.012)	2.7% (0.017)
Narrower age groups (50-80)	0.7% (0.015)	0.6% (0.013)	1.0% (0.011)	2.2% (0.010)	2.1% (0.013)
Narrower age groups (55-75)	0.9% (0.015)	0.8% (0.012)	1.1% (0.010)	2.4% (0.009)	2.7% (0.013)
Only include treated countries	1.0% (0.012)	0.9% (0.010)	1.2% (0.009)	2.3% (0.009)	2.0% (0.014)
Only reforms before 1913	1.7% (0.021)	1.6% (0.018)	1.5% (0.019)	2.4% (0.019)	2.7% (0.023)
Without countries heavily affected by WW I	0.4% (0.013)	0.1% (0.011)	0.5% (0.011)	2.0% (0.012)	1.9% (0.017)
Control for Spanish flu years	0.9% (0.016)	0.7% (0.012)	1.0% (0.010)	2.3% (0.009)	2.1% (0.013)

Coefficient depending on time window x: $\frac{1}{x}\sum_{e=1}^{+x}\hat{\delta}_e - \frac{1}{x}\sum_{e=-x}^{-1}\hat{\delta}_e$. Standard errors in parentheses and adjusted for clustering at the country level. To account for the low number of clusters, critical values are calculated using the number of clusters minus 2. The coefficients are multiplied by 100 to arrive at the %-interpretation. Given the small size of the coefficients, I consider this approximation appropriate.

Table C.3: **Robustness checks DDD event-time results, Hypothetical retirement age 65**

Time window around the introduction (in years)	**5**	**4**	**3**	**2**	**1**
Allowing for country-specific annual shocks (country-year f.e.)	0.8% (0.013)	0.7% (0.01)	0.9% (0.009)	1.8% (0.008)	1.4% (0.012)
Including gender*age mortality shocks/trends (gender-age-year f.e.)	0.8% (0.014)	0.7% (0.012)	1.0% (0.01)	2.1% (0.009)	2.0% (0.013)
Allowing for different age-specific mortality trends in treated countries	0.7% (0.011)	0.6% (0.009)	0.9% (0.007)	2.1% (0.007)	2.0% (0.012)
Further controls + All Controls interacted with age f.e.	0.7% (0.014)	0.4% (0.012)	0.7% (0.01) 2.0%	2.0% (0.008)	2.1% (0.013)
Only elderly control group	-0.4% (0.011)	-0.4% (0.008)	-0.8% (0.008)	0.2% (0.011)	-0.5% (0.018)
Excluding Norway and USA	1.0% (0.017)	0.9% (0.013)	1.1% (0.011)	2.3% (0.011)	2.5% (0.017)
Narrower age groups (50-80)	0.4% (0.014)	0.4% (0.012)	0.9% (0.01)	2.0% (0.009)	2.0% (0.014)
Narrower age groups (55-75)	0.4% (0.014)	0.4% (0.011)	0.9% (0.009)	1.9% (0.009)	2.5% (0.014)
Only include treated countries	1.0% (0.012)	0.9% (0.01)	1.2% (0.009)	2.3% (0.009)	2.0% (0.014)
Only reforms before 1913	1.5% (0.021)	1.4% (0.017)	1.2% (0.018)	1.9% (0.019)	2.7% (0.024)
Without countries heavily affected by WW I	0.1% (0.014)	0.1% (0.011)	0.6% (0.010)	2.1% (0.011)	2.1% (0.018)
Control for Spanish flu years	0.8% (0.015)	0.6% (0.011)	0.9% (0.009)	2.1% (0.008)	2.0% (0.013)

Coefficient depending on time window x: $\frac{1}{x}\sum_{e=1}^{+x}\hat{\delta}_e - \frac{1}{x}\sum_{e=-x}^{-1}\hat{\delta}_e$. Standard errors in parentheses and adjusted for clustering at the country level. To account for the low number of clusters, critical values are calculated using the number of clusters minus 2. The coefficients are multiplied by 100 to arrive at the %-interpretation. Given the small size of the coefficients, I consider this approximation appropriate.

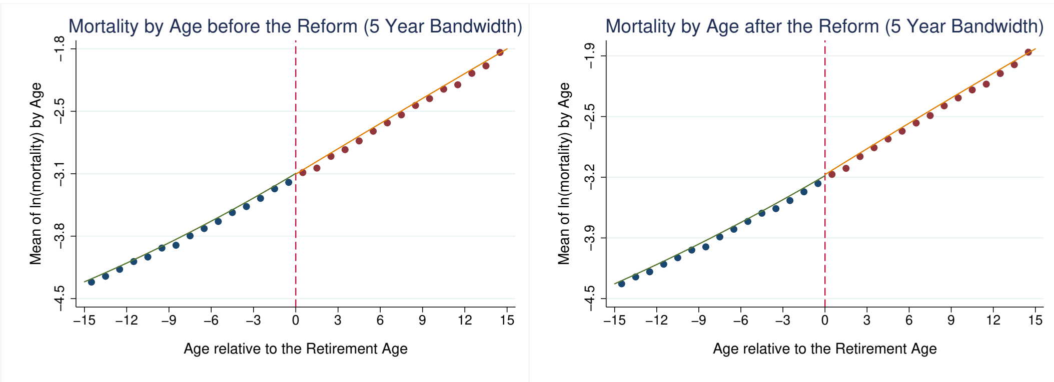
Table C.4: **Robustness checks regression discontinuity results**

Bandwidth	Polynomial	Kernel	Mortality	γ -before	γ -after	After−Before
15	linear	Uniform	Regular	3.5% (0.025)	5.1% (0.026)	1.6%
15	quadratic	Uniform	Regular	-0.8% (0.039)	1.2% (0.04)	2.0%
15	linear	Triangular	Regular	1.8% (0.029)	3.5% (0.030)	1.7%
15	quadratic	Triangular	Regular	-1.0% (0.043)	0.9% (0.046)	1.9%
15	linear	Uniform	Detrended	-1.6% (0.015)	0.0% (0.014)	1.6%
15	quadratic	Uniform	Detrended	-1.6% (0.022)	0.4% (0.022)	2.0%
15	linear	Triangular	Detrended	-1.6% (0.016)	0.2% (0.016)	1.8%
15	quadratic	Triangular	Detrended	-2.9% (0.024)	-1.0% (0.024)	1.9%
10	linear	Uniform	Regular	0.9% (0.032)	2.9% (0.033)	2.0%
10	quadratic	Uniform	Regular	-1.1% (0.050)	1.0% (0.052)	2.1%
10	linear	Triangular	Regular	0.1% (0.036)	2.0% (0.038)	1.9%
10	quadratic	Triangular	Regular	0.3% (0.056)	0.8% (0.059)	0.5%
10	linear	Uniform	Detrended	-1.8% (0.018)	0.2% (0.017)	2.0%
10	quadratic	Uniform	Detrended	-2.9% (0.028)	-0.8% (0.027)	2.1%
10	linear	Triangular	Detrended	-2.3% (0.020)	-0.4% (0.020)	1.9%
10	quadratic	Triangular	Detrended	-3.1% (0.031)	-2.7% (0.031)	0.4%
5	linear	Uniform	Regular	0.1% (0.046)	1.6% (0.048)	1.5%
5	quadratic	Uniform	Regular	-0.2% (0.084)	-2.9% (0.087)	-2.7%
5	linear	Triangular	Regular	0.2% (0.054)	-0.3% (0.057)	-0.5%
5	quadratic	Triangular	Regular	3.5% (0.106)	0.2% (0.113)	-3.3%
5	linear	Uniform	Detrended	-2.6% (0.025)	-1.2% (0.025)	1.4%
5	quadratic	Uniform	Detrended	-2.7% (0.046)	-5.2% (0.045)	-2.5%
5	linear	Triangular	Detrended	-2.8% (0.030)	-3.3% (0.030)	-0.5%
5	quadratic	Triangular	Detrended	-2.5% (0.059)	-6.3% (0.059)	-3.8%

$log(mortality)_{ierg} = f(r) + \gamma * R_{ir} + \varepsilon_{ierg}$ estimated for the 5 years before and 5 years after the introduction of social pensions. Detrended mortality rates = residuals e_{itjg} from the regression: $ln(mortality)_{itjg} = age_i + time\ trend_t + age_i * time\ trend_t + e_{itjg}$. Regular mortality refers to $ln(mortality)$ not detrended. All regressions are weighted based on a triangular kernel using the rdrobust command (Calonico et al., 2014a,b). Standard errors in parentheses. The coefficients are multiplied by 100 to arrive at the %-interpretation. Given the small size of the coefficients, I consider this approximation appropriate.

D RD graphs

Figure D.1: **RD results (before & after separated)**



Each dot represents the average age-specific mortality rate for the 9 “treated ” countries (both genders). Before includes 5 years before the introduction of social pensions (Event-time: -5 to -1). After the 5 years after (Event-time: 1 to 5). RD specification using quadratic polynomial.