Raising the Statutory Retirement Age: Self-Interest, Voter Behavior and Political Attitudes*

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Abstract

I use a referendum on a pension reform in Switzerland to learn about the drivers of political acceptance for raising the statutory retirement age. Combining rich administrative and survey data with a cohort-by-gender discontinuity, I find that individuals experiencing a one-year raise of the retirement age – a policy associated with a personal loss of about USD 26,000 – increase voter participation in the referendum by 5.8 percentage points and reform rejection by 20.2 percentage points. Moreover, personal incentives spill over within households. However, focusing on prospective political attitudes, the penalized voters do not disproportionately punish the actors supporting the reform (government, center-right parties, business associations) or reward those opposing it (left-wing parties, unions). This missing electoral effect may be related to the lump-sum character of the policy or be the filtering result of direct democracy.

Keywords: Pension Reforms, Voter Behavior, Pocketbook Voting, Direct Democracy
JEL Classification: D72, H55

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1 Introduction

Demographic pressures have placed pension reforms high on the political agenda of governments in most advanced economies. At the same time, a broad political consensus on the measures to be taken is often lacking. Among the most commonly proposed measures is the raise of the statutory retirement age, which is set to increase in 23 out of 38 OECD countries (OECD, 2024). France provides the most striking example of how controversial such policies can be, as the two-year increase in the retirement age announced in January 2023 was met with extreme political unrest and only implemented by overriding the parliament using special constitutional powers of President Macron. Despite the current relevance of the topic and its tight political support, little is known about the concrete political acceptance of this measure and about the drivers behind its consent or disagreement. Does the opposition stem from a general disagreement with the policy or from particular interests being affected? How persistent is this opposition and what is its electoral effect? For instance, survey polls data suggest that Macron lost 5 percentage points (-19%) of voters between November 2022 and March 2023 (Regan, 2023), which he could not recover in the rest of 2023 (POLITICO, 2024).

This paper aims at improving the understanding of the drivers and the persistence of political acceptance towards raises in the statutory retirement age in Western countries. I leverage a quasi-experimental setting in the Swiss direct democracy system to identify which portion of the dissent towards raises in the statutory retirement age originates from particular interests being touched, and how this part of dissent translates into persistent changes in political attitudes. In September 2022, Swiss citizens voted on *AHV-21*, a reform proposing the raise the statutory retirement age for women from 64 to 65 years (see Section 3.1 for details). In contrast to earlier reforms, this proposal was relatively simple, allowing to identify voters' material interest using basic demographic information. Exploiting a cohort-by-gender discontinuity, I can isolate the effects of a particular interests – quantified in CHF 26,000 by the referendum committee – on voter behavior.[1]

Using administrative panel data from the cantons of Geneva and St. Gallen, I find that particular interests increased the average probability of voting by 5.8 percentage points (+10.6%). This result combines the age discontinuity in a triple difference-in-differences framework using men and previous voting as control groups, thus accounting for differences in other factors influencing voter behavior. The finding holds in a variety of robustness checks. The greatest mobilization is registered among voters with medium usual participation (+9.0pp and +11.8pp between the 3rd and the 6th deciles). Moreover, I show that particular interests spill over within households. Individuals married to an affected woman increase their voter participation by 3.2 percentage points when compared to individuals

[1] On September 25, 2022, the Swiss National Bank reported that CHF 1 would trade for USD 1.03.

bound to a marginally unaffected woman. Individuals living with an affected woman increase their voter participation by 4.8 percentage points. Using post-vote survey data, I then estimate that the personal loss increased the probability of rejecting the reform conditional on having voted by 20.2 percentage points (+37.1%). This effect is driven by centrist voters (+23pp) and – in particular – right-wing voters (+50pp), which switched fields only when having to bear personally the costs of the reform. Thus, when focusing solely on the retirement age issue, self-interested motives play an important role in political positioning and can explain a big fraction of the opposition to raises in the retirement age in the center-right part of the political spectrum.

Given the important role of particular motives during the referendum, I test whether the personal loss translated into a shift in broader political preferences such as government trust or voting intentions. Interestingly, I find no evidence among the penalized cohorts of increased support for trade unions or anti-reform parties, nor of decreased trust in the government, pro-reform parties, or business associations. Thus, when considering the issue of the statutory retirement age into a broader political context, the role of pocketbook motives is downsized. I claim this result could be explained by two factors: the nature of political attitudes formation and the filter of direct democracy. On the one hand, the raise of the retirement age is a lump sum policy, and the affected women do not disproportionately expect further future losses in the aftermath of the vote. This is fundamentally different, for example, to most forms of government subsidies. If voter attitudes are formed prospectively rather than retrospectively, consistently with Elinder et al. (2015), political punishment and reward of the actors involved in the referendum will be downsized. On the other hand, the retrospective punishing mechanism may be weakened when the reform's approval is filtered with direct democracy, which work as a political relief valve (by providing a medium for expression) and as a legitimisation tool (by shifting the burden of responsibility to the citizens themselves).

This paper contributes to two main strands of literature. First, it contributes to the literature on the politics of pension reforms. Boeri et al. (2002) survey the acceptance around possible reforms of the public pay-as-you-go systems in Germany and Italy in the early 2000s, finding low approval rates and indications of self-interest driven opinions. Related literature analyses the role of information (Boeri and Tabellini, 2012) or work ability concerns (Scheubel et al., 2009). I add to this literature by exploring administrative and survey data tied to a referendum, which I interpret as a device to reveal the preferences of voters. The referendum allows to assess the political impact both on the single, unbundled policy and on broader and more complex political attitudes such as voting intentions. In this regard, the Swiss direct democracy system offers an unique opportunity to learn about voters' political opinions in a concrete situation, as direct votes reach the entire eligible voter population, provide legally binding decisions, and are embodied in a formal process

of voting infrastructure and information.

Second, it contributes to the literature on economic voting.[2] In particular, this paper joins a growing strand of literature that examines pocketbook effects using quasi-experimental methods. Among others, Manacorda et al. (2011), Pop-Eleches and Pop-Eleches (2012), Labonne (2013), De La O (2013), Zucco Jr. (2013), Elinder et al. (2015), Galiani et al. (2019), Zimmermann (2021), Kaba (2022) and Vannutelli (2023) link individual gains from government spending to increased political support in Uruguay, Romania, Philippines, Mexico, Brazil, Sweden, Honduras, India, Turkey, and Italy respectively. Closely related, Alpino (2018) and Levy (2023) connect promises of tax cuts to increased electoral support in Italy and France. Moving to single issue, referendum settings, the evidence is limited to Meya et al. (2020), who find that German university students increasingly vote for collectively funded transportation and cultural tickets when they profit individually. I add to this literature in terms of context, policy, and target population. Switzerland is an advanced economy with a long-standing tradition of direct democracy. Further, in contrast to the general focus of past literature on expansionary government spending, I investigate the effects of a policy implying a (future) lump-sum loss. Whereas there is indication of sociotropic loss aversion (Nannestad and Paldam, 1997) and of sociotropic reaction to local losses (Fetzer, 2019), the dynamics in pocketbook voting are unknown. Finally, the policy focus implies a different target population, which is not only composed by hand-to mouth voters and allows to test for effect heterogeneity and within-household spillover effects.

The remainder of this paper is organized as follows. Section 2 discusses the theoretical framework used to model voting behavior. Section 3 describes the quasi-experimental context, presents the data, and formalizes the empirical methodology. Section 4 describes the data. Section 5 presents the results, Section 6 discusses them, and Section 7 concludes. Section 8 further provides a brief description of the Swiss pension system, a discussion of the identifying assumptions and of the role of salience, and robustness checks.

[2] Lewis-Beck and Stegmaier (2013) and Lewis-Beck and Stegmaier (2019) summarize this literature, which mostly finds a limited role for personal economic conditions (*pocketbook voting*) relative to national economic conditions (*sociotropic voting*). However, this literature suffers from serious identification problems: aggregate correlational studies are subject to omitted variable bias (a multitude of unobservable factors may determine both personal economic conditions and voter behavior), reverse causality (governments may target economic policies towards groups that are expected to provide more or less support), and – when based on subjective reporting of the personal economic conditions – measurement error (voters' economic evaluations may be endogenous to partisanship, see Healy et al. (2017) and Schaffner and Roche (2017)).

2 Theory: Rational Voter Model

I borrow the notation of Riker and Ordeshook (1968)'s *calculus of voting* to formalize a decision model for the rational voter in a referendum context. I consider the case where individual i decides whether to vote or abstain on object j, which has two options (*yes* or *no*).[3] The model is represented by Equation (1):

$$R_{ij} = p_{ij}B_{ij} - C_{ij} + D_{ij}(B_{ij}). \tag{1}$$

R_{ij} is the expected reward, in utils, that individual i receives from voting for her utility-maximizing option on object j.[4] Abstention is normalized as reference option, so that the rational voter will vote in case $R_{ij} \geq 0$. The term $p_{ij}B_{ij}$ captures the *instrumental* motive for voting, i.e. the utility derived from individual i's ability to influence the outcome. $p_{ij} \in (0, 1)$ is individual i's perceived probability to be pivotal voter on object j. B_{ij} is individual i's expected differential benefit, in utils, in case the preferred option of object j is chosen. B_{ij} contemplates motives which can be self-interested or altruistic, as well as economic or non-economic. C_{ij} are individual i's voting costs for object j, covering practical voting costs (e.g. letter stamps or queuing) and information costs. Information costs are those that are additional to the decision to abstain.[5] D_{ij} represents individual i's *expressive* benefits from voting on object j in utils, i.e. the utility received regardless of the vote outcome and of voter's ability to influence it. On the one hand, D_{ij} captures object-invariant factors such as the utility of voting to preserve and support democracy (Downs, 1957), for civic duty (Riker and Ordeshook, 1968) or by party affiliation (Fiorina, 1976). On the other hand, D_{ij} takes into account individual i's utility from voting specifically on object j's. In particular, the expressive benefit is assumed to depend positively on the options' utility differential B_{ij}, i.e. $\frac{\partial D_{ij}(B_{ij})}{\partial B_{ij}} > 0$. In this sense, D_{ij} embodies the feeling of *doing the own part*, which grows with voter's persuasion for one of the two options.

Economic self-interest influences the voting decision through the differential benefit B_{ij}. Since in large groups p_{ij} is expected to be infinitesimal, B_{ij} will mostly influence R_{ij} through the expressive benefit channel. Figure 1 illustrates the voter decision model depending on $B_{ij,no}$, i.e. the differential utility for the *no*-option with respect to the *yes*-option. B_1 and B_2 are the values of differential benefit at which the individual is indifferent between voting or abstaining ($R_{ij} = 0$). Consequently, the rational voter will vote *yes* for $B_{ij,no} \leq B_1$, will abstain for $B_1 < B_{ij,no} < B_2$ and will vote *no* for $B_2 \leq B_{ij,no}$. In the case of an increased (economic) benefit for the *no*-option, three possible shifts can happen

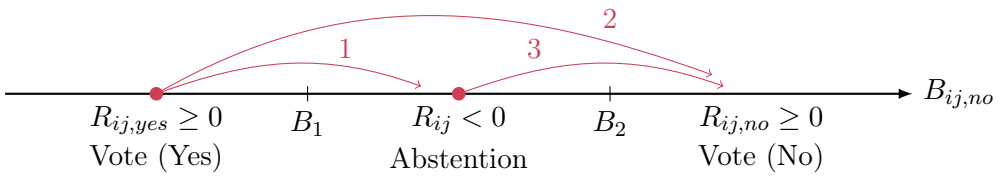
[3] The option of "voting for abstention", i.e. casting a blank vote, is ignored.

[4] In the equation, the notation about different voting options is reduced for conciseness. So $R_{ij} = \max\{R_{ij}(yes); R_{ij}(no)\}$.

[5] Abstention's information costs are assumed to be lower or equal to the voting information costs, as the voter can stop searching for further information if she expects information costs to exceed voting utility.

(marked with the red arrows): 1. Citizens who end up in the abstention zone, leaving the *yes*-voting zone (*dilemma effect*); 2. Citizens who change from the *yes*- to the *no*-voting zone (*switching fields*); 3. Citizens who leave the abstention zone towards the *no*-voting zone (*convincing effect*). Due to the counteracting effects of the first and the third shifts, the effect of an increased (economic) benefit for one option on voter turnout is theoretically unclear. Instead, all the three shifts lead to an increase in rejection rates – either through a decrease in *yes*-votes (1), through an increase in *no*-votes (3), or through both (2).

Figure 1: Voter Participation and Personal Benefits.



3 Methods

3.1 Context: Swiss Direct Democracy and *AHV-21*

Voting is a frequent activity in Switzerland. In addition to electing their representatives, Swiss citizens regularly decide directly on specific objects. Between 1990 and 2022, direct votes were called about 3 times a year with an average of 3 items per voting day. Swiss direct voting is relatively unbureaucratic: Citizens receive the voting papers at home three weeks before the polls close; they can then vote anytime by mail or by casting their vote in a letterbox in their municipality, or they can vote at the local polling station on the last voting weekend. No registration is required other than a signature on the voting material. Only Swiss adults are eligible to vote on national issues. Between 1990 and 2022, the average turnout in direct national votes was 45.4% (standard deviation of 7.5).

On September 25, 2022, Swiss citizens voted on a pension reform called *AHV-21*.[6] The reform proposal was relatively simple and contained two main points: i) The raise of the statutory retirement age for women from 64 to 65 years; ii) The increase of the sales tax (from 7.7% to 8.1% for the standard VAT, from 2.5% to 2.6% for the reduced VAT). The two objects were voted separately, but their implementation was conditional on their joint acceptance. The reform was designed to ensure the medium-term financial stability of the pay-as-you-go part of the Swiss pension system, the Old Age and Survivors Insurance (AHV, see Appendix A for more details). The reform was proposed by the Swiss government and supported by the center-right majority in parliament, with 65% approval in the

[6] On the same day, Swiss voters rejected an initiative to ban factory farming and approved an amendment to the withholding tax. As shown in Section 3.2, these objects were generally of secondary importance.

lower house (National Council) and 72% in the upper house (Council of States). Left-wing parties, trade unions, and women's organizations opposed the reform and triggered a referendum. The main arguments of the two opposing sides are summarized in Table A1. The reform passed with a narrow majority of 50.6% and an above-average turnout of 52%.

AHV-21 was a relatively simple pension reform, involving only the pay-as-you-go part of the Swiss pension system and with little differences between income groups. Table 1 summarizes the implementation details. The statutory retirement age was gradually raised to 65 years for women born since 1961, and accompanied by a compensation scheme ending with the 1969 cohort. Notice that, although these detailed plans were available in the voting material, their salience in the campaign was limited (see Appendix C for a more detailed discussion). I account for the limited salience of the implementation plans by estimating a lower-bound treatment effect ignoring compensation measures and – in some of the models – by excluding the 1960-cohort.

Table 1: Retirement Perspectives for Women after the Approval of the Reform *AHV-21*.

Cohort	Age in 2022 (y)	Ref. Retirement Age (y)*	Compensation**
≤1960	≥62	64	none
1961	61	64.25	25%
1962	60	64.5	50%
1963	59	64.75	75%
1964	58	65	100%
1965	57	65	100%
1966	56	65	81%
1967	55	65	63%
1968	54	65	44%
1969	53	65	25%
≥1970	≤52	65	none

* The official voting papers consider an entry into force of the law by 2024.

** The monthly compensation is CHF 160 for an annual income below CHF 57,360, CHF 100 between CHF 57,360 and 71,700, CHF 50 over CHF 71,700.

The raise of the statutory retirement age is equivalent to a financial loss for the affected women. In Switzerland, workers are allowed to retire before (after) the reference age, but face costs (benefits). Thus, women affected by the reform can still retire at 64 years - but at a cost; alternatively seen, women could have retired at 65 years also prior to the reform - but with better financial conditions. It is difficult to monetize these costs precisely. The committee against the reform calculated the policy impact as a lump-sum loss of CHF

26,000.[7] Given the intense advertisement campaign on this amount (see Figure A13), I use this monetization as the impact of the personal loss.

3.2 Data

Voter Participation

I use administrative panel data on individual participation in federal direct votes from the cantons of St. Gallen and Geneva (STISTAT, 2022; OCSTAT, 2022).[8] The data cover the universe of eligible voters, where participation in the voting day is automatically registered. The participation in single votes within the same voting day cannot be distinguished, but *AHV-21* was the most important object of its voting day. In the 9 weeks before the polls closed, 437 contributions on the *AHV-21* reform appeared in the main Swiss newspapers, while only 274 were counted for the factory farming initiative and 217 for the withholding tax amendment (Udris, 2022). Furthermore, respondents to the VOX post-vote survey (see description in the next section) give the *AHV-21* reform an average importance of 8.03 on a 0-10 scale, while the average importance is only 6.53 for the factory farming initiative and 5.66 for the withholding tax amendment. Finally, there is no expected discontinuity at the cut-off age for other objects.

Voter Opinion

I use individual survey data on voter object's opinion collected by *gfs.bern* on behalf of the Swiss Radio and Television (gfs.bern, 2022). The VOX survey is an inquiry on federal votes conducted since 1977 in the two weeks following the voting day. The survey draws a random sample of about 3,000 respondents from a register-based sample frame of the Federal Statistical Office with stratification by language region, age, and gender. Respondents are first contacted by letter and can then complete the questionnaire on paper (1,274/3,112 respondents on September 25, 2022) or online (1,838/3,112).[9]

Funk (2016) shows a mismatch between revealed and stated voter behavior using the VOX survey between 1987 and 2007. On average, she calculates a survey bias on approval of 4.7 percentage points. She finds that some policy areas exhibit larger distortions, which suggests that respondent lying may explain part of the mismatch. Similarly to other pension

[7] This calculation is calibrated as the sum of one year of contributions and one year of pension for the median earner. By ignoring the transition phase, it likely represents a higher bound on the true cost.

[8] Data have been collected since 2010 for the city of St. Gallen, and since 2017 for other 8 municipalities (Au, Mels, Quarten, Schmerikon, Thal, Uzwil, Wildhaus-Alt St. Johann and Gossau) in the canton. Data from the canton of Geneva cover the entire region since 2010.

[9] The data provide sampling weights. In do not use them in my main analysis, and provide robustness checks indicating similar effect magnitudes when applying weights. I avoid using sampling weights because of the lower estimator efficiency, and because of doubts on the weight comparison across survey waves. See Bollen et al. (2016) for a discussion on the trade-off of using survey weights in regression analysis.

related issues, *AHV-21* has a low VOX survey bias (2.9 percentage points). Furthermore, distortions due to sampling differences or strategic response are not expected to depend on the cohort cutoff used as identifying variation.

3.3 Quasi-Experimental Design

Identification Strategy

The *AHV-21* constellation serves as a quasi-experimental setting to isolate the effect of particular interest on voter behavior in the context of raises of the statutory retirement age. Specifically, I can estimate the share of women who are marginally induced to vote by the personal economic loss, as well as the impact on the share of voters who reject the reform.

The main identification strategy combines a triple-differences design along cohort, gender and past votes. First, women's cohort is used as a running variable for the assignment of the reform's costs in a Regression Discontinuity Design (RDD). Second, the same cohort-discontinuity for men is used to control for unobservable age differences in a Difference-in-Differences (DiD) setup. Using men's cohort-discontinuity helps to account for other potential discontinuities around the costs' assignment cutoff. For example, cohorts differ in the propensity to be already retired, which itself influences voter behavior and may introduce a bias in the estimation. Third, when focusing on time-comparable dependent variables (e.g. turnout), I further use the cohort discontinuity in past votes to ensure the absence of unobservable factors differently affecting men and women. Panel data even allow to control for time-invariant individual voting behavior. This setting is formalized econometrically in Section 3.4.

The identification strategy cancels out factors other than the policy's personal loss that might influence voter behavior. I claim that around the cutoff, after introducing the triple differences design and under the identifying assumptions discussed in the following subsections, treated and control individuals are similar in: i) The perceived probability of being a pivotal voter p_{ij}; ii) Voting costs C_{ij}; iii) Object-invariant elements of the expressive utility D_{ij}, such as the taste for democratic participation or the party affiliation; iv) Object-specific elements of the expressive utility D_{ij} that shaped the political campaign, such as solidarity with women, preferences for gender equality in duties, or the expectations towards further reforms of the pension system. All these factors being considered as equal, I am arguably able to estimate the impact on voter behavior coming solely from the policy change in the individual statutory retirement age.

Discussion of the Identifying Assumptions: RDD

Melly and Lalive (2020) list two main requirements for the validity of the Regression Discontinuity Design.

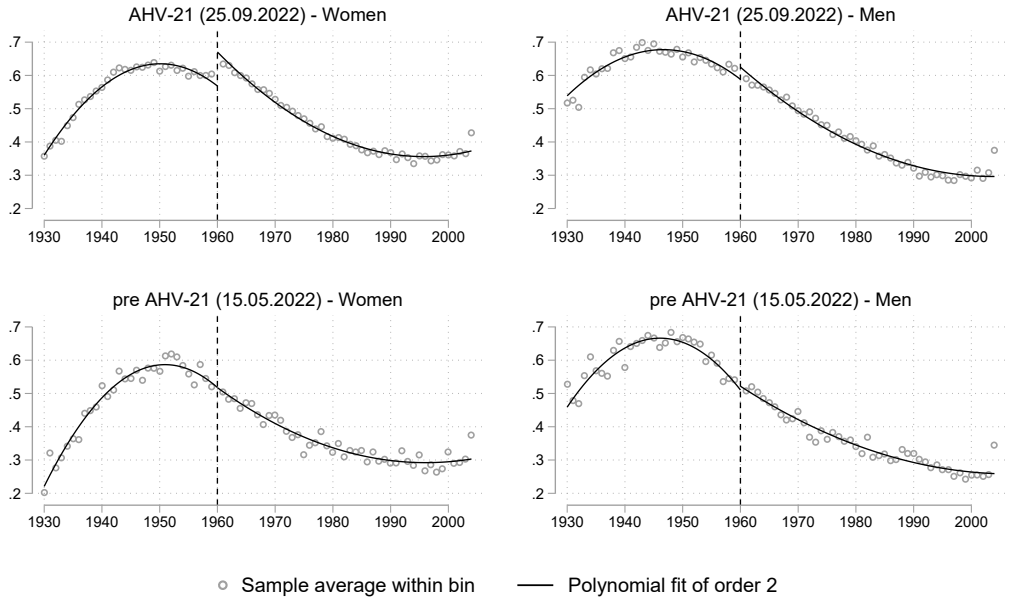
First, the treatment must be assigned by a discontinuous function of a continuous observable variable. This first criterion is met because the cost of the policy among women is assigned based on age, starting from the 1961-cohort. Although most of the data report age at cohort level, I run robustness checks on daily birth date for the Canton of St. Gallen. In Appendix C, I discuss the motives for a response of the untreated 1960 cohort. To ensure the absence of biases from a response of the 1960-cohort, I estimate three different models: using the 1960/61 cutoff (M1); using the 1959/61 cutoff, i.e. excluding the 1960 cohort (M2); using the 1959/1960 cutoff (M3). Model M2 follows a donut-hole approach as in Barreca et al. (2011) and is the reference model.

Second, there must be no discontinuity in the potential outcomes at the cutoff. A potential threat to this assumptions is that the used cutoff is close to the retirement cutoff, which could itself affect voter behavior. Reassuringly, no comparable jump in voter participation is observed at either the cutoff age for men or the cutoff age for women in past votes. Visual evidence on voter participation using the donut-hole model (M2) in the *AHV-21* voting day (September 25, 2022) and in the previous voting day (May 15, 2022) is provided in Figure 2.[10] As shown in Figure A19, this holds also when including the 1960-cohort and when de-grouping individuals from cohorts to birth date (with data of the Canton of St. Gallen). As shown in Figure 3, the same rationale holds for reform rejection. Further, Figure A1 and Figure A2 visual evidence is robust to a linear fit and to the use of the respective optimal bandwidth τ^* from Cattaneo et al. (2018).

Finally Figure A3 and Figure A4 provide the McCrary (2008)'s test for manipulation in administrative and survey data, and find smooth quantity transitions around the cutoff.

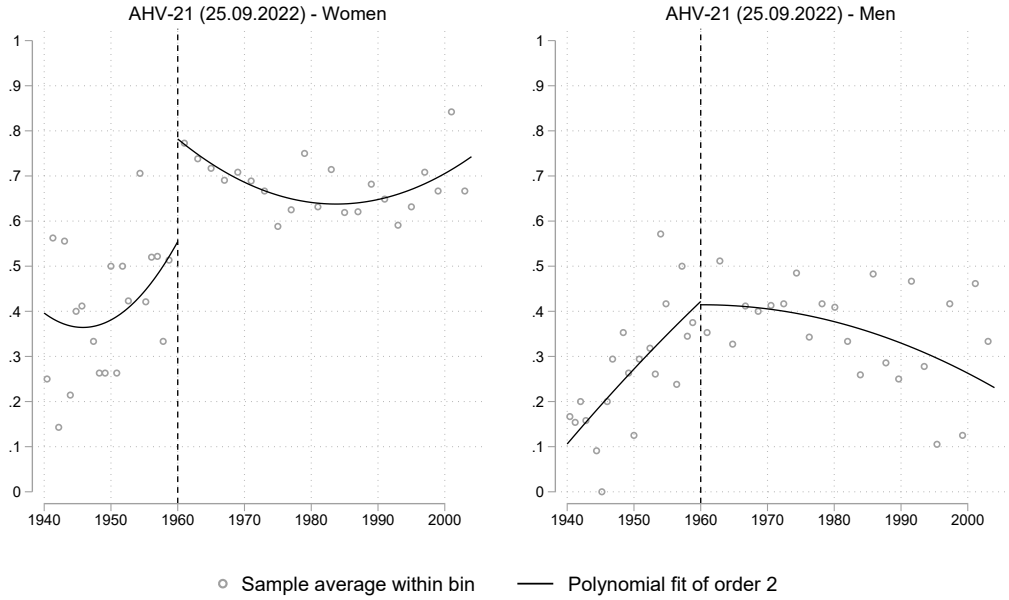
[10] On 15 May 2022, the Swiss citizens voted on three objects: the Film Act, the Transplantation Act, and the Development of the Schengen Acquis.

Figure 2: Voter Participation by Cohort, Object and Gender.



Source: Geneva and St. Gallen.
Method: Donut-hole quadratic RDD, 1959/61 (M2), whole distribution from cohort-1930.

Figure 3: Reform Rejection by Cohort and Gender.



Source: gfs.bern.
Method: Donut-hole quadratic RDD, 1959/61 (M2), whole distribution.

Discussion of the Identifying Assumptions: DiD

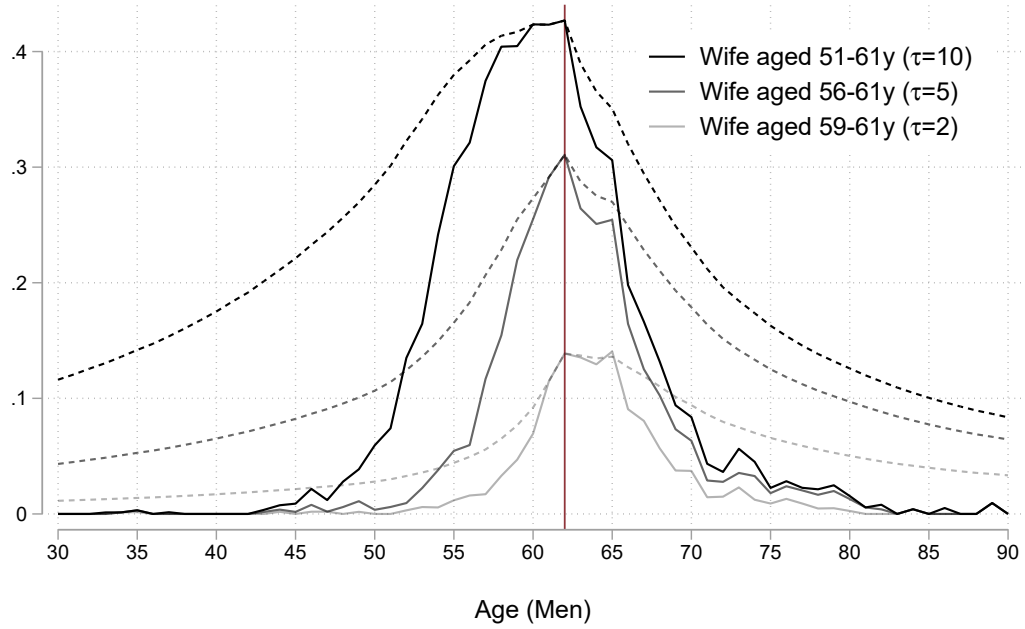
In addition to the difference between younger and older women around the treatment cutoff in the *AHV-21* vote, I use two other differences to ensure unbiasedness: i) the same age discontinuity, but for men; ii) the same age discontinuity, but in past votes. Here, I discuss the necessary assumptions of Stable Unit Treatment Value (SUTVA), No Anticipation (NA), Common Trend (CT), Exogeneity (EX), and Common Support (CS) for the voter behavior outcomes.

The Stable Unit Treatment Value (SUTVA) requires the absence of spillovers on *AHV-21* voting behavior: i) of women older than the cutoff age; ii) of men, or at least that men born before and after the cutoff age are equally (un)treated. The first part of the assumption seems to be robust, at least in the donut-hole model, where theoretically unaffected working cohorts are excluded to prevent a "salience" response. The second part of the assumption requires more reasoning. A potential source of bias could come from men in couples with treated women, either directly in case of married couples (the AHV benefit is calculated jointly) or indirectly in case of unmarried couples (by taking household income into account). The direction of the bias depends on the side of the age cutoff. A man in the younger age-group could be paired with a woman older than the cutoff age, which would generate no bias, or with a woman in the same cutoff group, which could bias the treatment effect downward. A man in the older age-group could be paired with a woman in the same cutoff group, which would generate no bias, or with a woman in the treated cutoff group, which could bias the treatment effect upward.[11] Assuming men react to intra-household spillovers, the combined bias would depend on the share of men affected by the marriage spillover to the two sides of the cutoff. Using the canton of St. Gallen, where I have information on the household structure, I can check what is the share of affected men in the two groups. Figure 4 shows, for each cohort in the *AHV-21* vote, the share of men who is married to an affected woman – varying the definition of affected woman in a range of 10, 5, and 2 years. Thick lines represent the average by men's cohort, while dotted lines represent the average of all men contained between a given cohort and age 62. Mechanically, the larger is the chosen women's treatment range (τ), the larger is the share of men that are affected by spillovers. Generally, as shown by the dashed lines, the bias is quite balanced, though the symmetry of the curves visibly varies with women's age of treatment definition. The asymmetry is driven by two factors: i) younger age-group men are closer in age to younger age-group women (decisive for high levels of τ); ii) men tend to be slightly older than their spouses (decisive for low levels of τ). When the women's range is large (51-61y, $\tau = 10$), the introduced bias works downward – as younger men (left of the cutoff bar) have larger spillover shares than older men (right). At a medium

[11] The reasoning is similar for same-sex couples. In this case the bias would work downward, as older age-group women paired with women in the younger age-group would be the only group with a spillover.

range (51-61y, $\tau = 5$), the bias is very similar – as younger men have comparable spillover shares to older men (right). At a narrow range of two years (59-61y, $\tau = 2$), the introduced bias works upward – as younger men have smaller spillover shares than older men. As the Cattaneo et al. (2018)'s optimal bandwidth is $\tau^* = 6$ of medium size, the bias to mostly cancel out.

Figure 4: Share of Men Married with a Treated Woman by Men's Cohort.



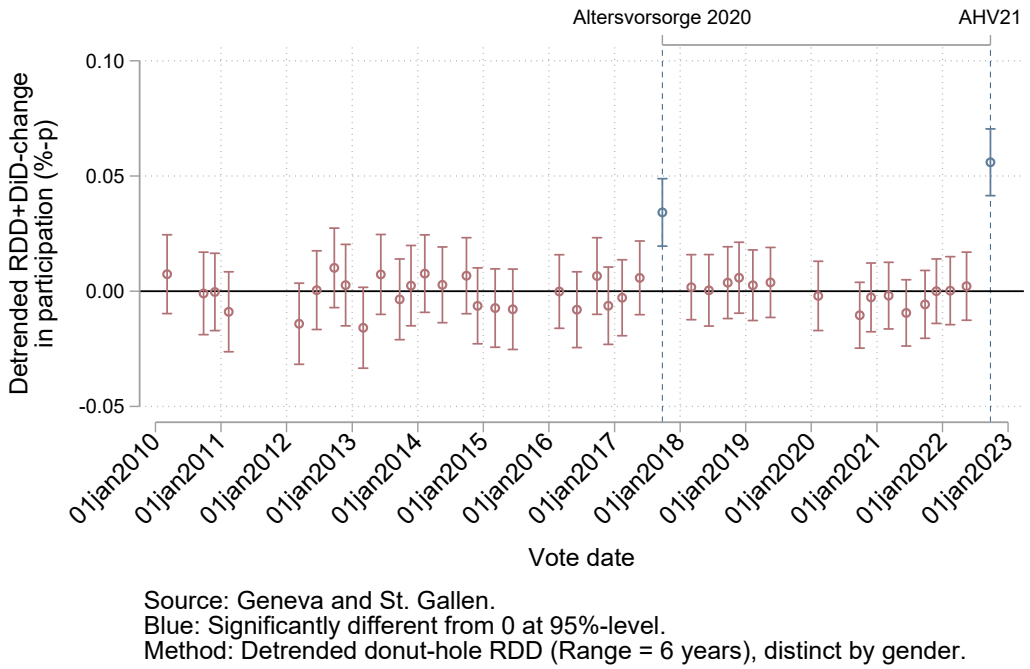
Source: St. Gallen.

To further explore for the potential bias introduced by marriage, I estimate the triple-difference donut-hole model separately for married and unmarried individuals in the whole sample. Figure A5 plots the impact of the personal loss by range width and marital status. Figure A6 also adds the estimates for unmarried individuals living alone, a proxy for singles, but - due to information availability - only in the canton of St. Gallen. The presence of a significant impact on singles and unmarried individuals suggests the validity of the approach, beyond any possible bias introduced by spillovers tied to the civil status of men. Furthermore, the estimates in the different groups are quite similar. Finally, the swap in the size of the impact between married and unmarried/singles when opening the width range confirms the analysis proposed in Figure 4.

The discussion of the No Anticipation (NA) and Common Trend (CT) assumptions in this context is not trivial, because each period refers to different voted objects. NA requires the absence of treatment effects in pre-trend voting, while CT requires the treated and the control groups to follow the same time trends in the absence of treatment. The main issue is represented by past objects with different incentives for younger to older women than for

younger to older men. Figure A7 plots a placebo exercise where the gender differences in donut-hole RDDs are calculated around the cutoff at each past vote. A significant difference is found for some past objects. This is mainly an inflated result, because – as shown in Figure A8 – the four groups are not identical in their voting behavior, and the difference in participation between older and younger women is generally higher than between older and younger men. In Figure 5, where I subtract the trend average of these differences, the outlying result of the *AHV-21* reform becomes clearer. Only the vote of the September 24, 2017 shows a significant outline effect. On this voting day, *Altervorsorge 2020* was voted on and rejected. *Altervorsorge 2020* was itself a pension reform that, among other measures, proposed to raise the statutory retirement age for women from 64 to 65 years. Thus, this robustness check not only confirms the validity of the identification, but even strengthens it with a second example.

Figure 5: Detrended Placebo RDD.



Finally, the Exogeneity (EX) assumption is supported by the discontinuity argument discussed in the previous section and the Common Support (CS) assumption is guaranteed by the large sample size.

3.4 Voter Random Utility Model

I translate the theoretical framework discussed in Section 2 into a voter random utility model. The latent variable y_{it}^* represents the utility of individual i from voting for her preferred option in vote t, and leads to the observed turnout $y_{it} \in \{0, 1\}$ following the

decision rule:

$$y_{it} = \begin{cases} 1 & \text{if} \quad y_{it}^* \geq 0 \\ 0 & \text{if} \quad y_{it}^* < 0 \ . \end{cases} \tag{2}$$

In models investigating voting intentions through survey data, y_{it} represents the declaration of rejecting ($y_{it} = 1$) or accepting ($y_{it} = 0$) the reform, conditional on voting, and y_{it}^* represents the latent utility behind this choice. The model is estimated linearly, so that the following regression equations are also valid when the dependent variable is not binary, as in the case of political attitudes.

Single Cross-Sectional Models

For survey outcomes that are not time-comparable (e.g. reform rejection), I estimate the following difference-in-differences model:

$$y_i = \alpha + \delta \cdot T_i(\tau) \cdot woman_i + \lambda \cdot T_i(\tau) + X_i'\beta + u_i, \tag{3}$$

where individual i's outcome variable y_i (e.g. reform rejection) is regressed on the interaction between the treatment age range $T_i(\tau) = 1\{62 - \tau \leq age_{it} \leq 61\}$ and gender. $\tau \in \mathbb{N}_+$ represents the annual width of the estimation range. The model also controls for the treatment age range $T_i(\tau)$ and for individual characteristics X_i (e.g. age polynomials, gender, municipality). X_i also contains a dummy variable $out_i(\tau) = 1\{(age_i \leq 61 - \tau) \vee (62 + \tau \leq age_i)\}$ and its interaction with gender, which allows to distinguish between the control group and observations outside the range of interest. The out-of-range observations are not discarded in order to estimate the age polynomials more accurately. u_i is the idiosyncratic error term.

As a robustness check, I also provide parametric and nonparametric results from the cohort discontinuity using only women. Nonparametric estimates follow Cattaneo et al. (2019)'s local polynomial continuity-based approach and are computed by a linear polynomial approximation using triangular kernel weighting on the common MSE-optimal bandwidth. Standard errors for the nonparametric estimates are derived using Cattaneo et al. (2019)'s proposed robust bias-correction.

Panel Models

For vote participation in administrative panel data, I estimate the following triple-difference fixed effects model:

$$\begin{aligned} y_{it} = {} & \delta \cdot T_{it}(\tau) \cdot woman_i \cdot AHV\text{-}21_{it} \\ & + \lambda_1 \cdot T_{it}(\tau) \cdot woman_i + \lambda_2 \cdot T_{it}(\tau) + X_{it}'\beta + V_t'\gamma + \alpha_i + v_t + u_{it}, \end{aligned} \tag{4}$$

where individual i's participation in vote t (y_{it}) is regressed on the interaction between the treatment age range $T_{it}(\tau) = 1\{62 - \tau \leq age_{it} \leq 61\}$, gender, and the *AHV-21* vote fixed effect. $\tau \in \mathbb{N}_+$ represents the annual width of the estimation range. I also control for the treatment age-group $T_{it}(\tau)$ and its interaction with gender, as well as for individual time-varying characteristics X_{it} (e.g. age polynomials) and vote-specific time-varying characteristics V_t (e.g. year, season). X_{it} also contains a dummy variable $out_{it}(\tau) = 1\{(age_{it} \leq 61 - \tau) \vee (62 + \tau \leq age_{it})\}$ and its interaction with gender and gender by *AHV-21* vote fixed effect, which allows to distinguish between the control group and observations outside the range of interest. α_i is the individual fixed effect, v_t the vote fixed effect, and u_{it} the idiosyncratic error term.

When looking at spillover effects, I estimate a similar model adding a spillover treatment age range $T^{spil}_{it}(\tau) = 1\{62 - \tau \leq age_{jt} \leq 61\}$ measured on i's married partner j (or i's household member j):

$$\begin{aligned} y_{it} = {} & \delta^{spil} \cdot T^{spil}_{it}(\tau) \cdot woman_j \cdot AHV\text{-}21_{it} \\ & + \lambda_1^{spil} \cdot T^{spil}_{it}(\tau) \cdot woman_j + \lambda_2^{spil} \cdot T^{spil}_{it}(\tau) \\ & + \delta \cdot T_{it}(\tau) \cdot woman_i \cdot AHV\text{-}21_{it} + \lambda_1 \cdot T_{it}(\tau) \cdot woman_i + \lambda_2 \cdot T_{it}(\tau) \\ & + X'_{it}\beta + V'_t\gamma + \alpha_i + v_t + u_{it}. \end{aligned} \tag{5}$$

As a robustness check, in Appendix D I also estimate difference-in-differences panel models (TDD) on voter participation using within-gender variation in cohorts and past votes.

Repeated Cross-Sectional Models

For survey outcomes that are time-comparable (e.g. trust in government, but not reform rejection rates), I estimate the following triple-difference model on repeated cross-sectional data:

$$\begin{aligned} y_{i(t)t} = {} & \alpha + \delta \cdot T_{i(t)t}(\tau) \cdot woman_{i(t)} \cdot AHV\text{-}21_{i(t)t} \\ & + \lambda_1 \cdot T_{i(t)t}(\tau) \cdot woman_{i(t)} + \lambda_2 \cdot T_{i(t)t}(\tau) + X'_{i(t)t}\beta + V'_t\gamma + v_t + u_{i(t)t}, \end{aligned} \tag{6}$$

where individual $i(t)$'s outcome in vote t ($y_{i(t)t}$) is regressed on the interaction between the treatment age range $T_{i(t)t}(\tau) = 1\{62 - \tau \leq age_{i(t)t} \leq 61\}$, gender, and the *AHV-21* vote fixed effect. $\tau \in \mathbb{N}_+$ represents the annual width of the estimation range. I also control for the treatment age range $T_{i(t)t}(\tau)$ and its interaction with gender, as well as for individual time-varying characteristics $X_{i(t)t}$ (e.g. age polynomials) and vote-specific time-varying characteristics V_t (e.g. year, season). $X_{i(t)t}$ also contains a dummy variable $out_{i(t)t}(\tau) = 1\{(age_{i(t)t} \leq 61 - \tau) \vee (62 + \tau \leq age_{i(t)t})\}$ and its interaction with gender and gender by *AHV-21* vote fixed effect, which allows to distinguish between the control group and observations outside the range of interest. v_t is the vote fixed effect, and $u_{i(t)t}$ the idiosyncratic error term.

4 Descriptives and Balancedness in Observables

Table 2 summarizes the administrative data on voter participation in the cantons of Geneva and St. Gallen. The first column aggregates all the votes used, while the following columns focus on the *AHV-21* vote. The demographic and vote-specific characteristics are both very similar between the whole data and the *AHV-21* vote. Comparing the selected age-groups and differentiating by gender, there are meaningful differences in marital and household status. Men around the retirement age are more likely than women to be married and less likely to be single, and these differences increase with age. The divergence is due to the lower life expectancy of men, and highlights the importance of controlling for marital status beyond the use of individual fixed effects. Other characteristics related to migration and religion are balanced. Looking at usual participation (calculated on votes preceding the *AHV-21* vote), I find that around the retirement age – consistent with Mulligan and Sala-i Martin (1999) – older voters tend to participate more (+5pp). This is true both for men and women: 58% vs. 53% for men, 55% vs. 50% for women. During the *AHV-21* vote, participation is generally increased around the retirement age. However, women aged between 53 and 61 years disproportionately increase their participation, resulting in a difference-in-differences value of 5 percentage points.

Table 2: Descriptives: Administrative Data

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	All	*AHV-21*					
		All	*Women*		*Men*		*DiD*
			53-61y	62-70y	53-61y	62-70y	
Socio-Demographics:							
Women (%)	0.54	0.54	1.00	1.00	0.00	0.00	–
Age (y, 18+)	50.75	51.00	58.45	64.39	58.43	64.38	0.00
Geneva (%)	0.79	0.76	0.78	0.76	0.76	0.76	0.01**
Born Swiss (%)	0.84	0.85	0.79	0.81	0.83	0.85	-0.00
Married (%)	0.43	0.41	0.52	0.50	0.60	0.64	0.06**
Single (%, only SG)	0.22	0.23	0.24	0.29	0.22	0.19	-0.09**
Rel: Reformed (%, only SG)	0.26	0.23	0.20	0.22	0.19	0.21	0.00
Rel: Catholic (%, only SG)	0.42	0.41	0.42	0.42	0.40	0.40	-0.00
Rel: No/Other (%, only SG)	0.32	0.37	0.38	0.36	0.41	0.38	0.00
Participation:							
Participation (%)	0.48	0.48	0.61	0.61	0.57	0.62	0.05***
Usual Participation (%)	0.48	0.47	0.50	0.55	0.53	0.58	0.00
% 1st Quintile	0.21	0.23	0.20	0.17	0.19	0.17	-0.00
% 2nd Quintile	0.20	0.19	0.18	0.16	0.16	0.14	-0.00
% 3rd Quintile	0.20	0.19	0.21	0.18	0.19	0.17	0.00
% 4th Quintile	0.20	0.18	0.21	0.23	0.21	0.22	-0.01
% 5th Quintile	0.20	0.20	0.20	0.26	0.24	0.31	0.01**
Observations	12,686,644	358,470	20,693	17,015	17,946	14,741	70,395

Source: Administrative data from the cantons of Geneva and St. Gallen. Singles are imputed as unmarried individuals living in a one-adult household. Usual participation is calculated excluding the *AHV-21* vote. For some categories, the number of observations might be lower due to missing values. Heteroskedasticity-robust standard errors are estimated in the difference-in-differences. */**/*** denotes statistical significance at the 10%/5%/1%-level. Abbreviations: y = Years.

Table 3 summarizes the characteristics of the respondents in the VOX survey data. The first column aggregates all the used votes, while the subsequent columns concentrate on the *AHV-21* vote. The only variable with a difference-in-differences coefficient statistically different from zero is reform rejection.

Table 3: Descriptives: VOX Survey Data

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	All	*AHV-21*					
		All	*Women*		*Men*		*DiD*
			53-61y	62-70y	53-61y	62-70y	
Socio-Demographics:							
Women (%)	0.46	0.51	1.00	1.00	0.00	0.00	-
Age (y, 18-82)	51.14	5.94	56.41	65.95	56.27	66.30	0.49
Income (1-15)	8.08	8.25	8.87	6.40	9.92	7.55	0.10
% Low- (1 to 4)	0.30	0.29	0.26	0.50	0.14	0.37	-0.01
% Middle- (4 to 9)	0.36	0.35	0.32	0.30	0.33	0.35	0.04
% High- (10 to 15)	0.34	0.36	0.42	0.20	0.53	0.29	-0.03
Political Orientation:							
Left-Right (0-10)	5.02	5.05	4.70	4.61	5.49	5.24	-0.17
% Left (0 to 3)	0.23	0.22	0.27	0.26	0.14	0.17	0.04
% Centre (4 to 6)	0.53	0.54	0.57	0.61	0.52	0.60	0.03
% Right (7 to 10)	0.24	0.24	0.16	0.13	0.33	0.23	-0.07
Usual Participation (0-10)	7.86	7.86	8.13	8.64	8.06	8.55	-0.02
Vote-Specific:							
Rejection (%)	0.47	0.47	0.73	0.48	0.40	0.39	0.23***
Participation (%)	0.72	0.74	0.82	0.86	0.80	0.86	0.02
Importance (0-10)	5.72	6.70	6.50	6.61	7.16	7.11	-0.16
Political Attitudes:							
No-parties support (%)	0.36	0.34	0.43	0.39	0.25	0.30	0.10
Gov. Trust (0-10)	0.67	0.63	0.61	0.64	0.61	0.63	-0.01
Econ. Ass. Trust (0-10)	0.46	0.45	0.43	0.41	0.43	0.44	0.03
Trade Union Trust (0-10)	0.49	0.48	0.50	0.49	0.41	0.42	0.02
Observations	77,754	2,968	270	258	305	235	1,174

Source: VOX suvey data. For some categories, the number of observations might be lower due to missing values. Heteroskedasticity-robust standard errors are estimated in the difference-in-differences. */**/*** denotes statistical significance at the 10%/5%/1%-level. Abbreviations: y = Years.

5 Results

Voter Participation

Part i) of Table 4 presents the results of the triple-difference fixed effects model formalized in Equation (4), where the difference in the *AHV-21* vote participation around the age cutoff for women is compared with that of men and past votes. All models control for individual and vote fixed effects, as well as for third-degree age polynomials and general voting trends (quarters and years). Different columns report variation in the cutoff year (1960/61 or 1959/60) and the exclusion of the 1960 year. The three models produce qualitatively similar results. My preferred model, excluding the 1960 cohort (donut-hole), reports a highly significant increase in vote participation of 5.8 percentage points at the optimal range τ^* of 6 years. Using the baseline participation rate of 60.6% for the affected women (cohorts 1961-1966), the effect translates into a 10.6% increase in voter turnout. Following the rationale presented in Section 2, this finding implies that the sum of individuals moving from the abstention zone to the *no*-voting zone (*convincing effect*) greatly exceeds the group of individuals moving from the *yes*-voting zone to the abstention zone (*dilemma effect*).

Figure 6 shows the variation of the effects depending on the range τ. The impact of the personal loss on voter participation is highest in the ranges between $\tau = 2$ and $\tau = 5$ (+6pp), and then gradually decreases as the estimation range is widened. This development is consistent with the argument that younger generations discount the loss over a longer period and with greater uncertainty. In addition, younger generations may expect higher contribution rates in case of a rejection of the reform, which dilutes the direct effect of the change in the reference retirement age. Finally, the more the range is widened, the less the RD identification can ensure comparable groups.

Using the advertised loss of CHF 26,000, we obtain an increase in voter participation by 0.22 percentage points (+0.48%) for a loss of CHF 1,000.[12] This effect is modest when compared with Andersen et al. (2014), who – using exogenous variation in Norwegian local revenues from hydropower taxes – find a USD 1,000 increase in per capita revenues to increase turnout in local elections by about 0.7 percentage points. However, Andersen et al. (2014) observe a variation in tax revenues between USD 0 and USD 9,000, resulting in a maximum turnout increase by about 6 percentage points. This total shift is comparable to the estimated response of 5.8 percentage points observed in Switzerland.

[12] An alternative standardization can be done using lost (pension) time, instead of lost money. Data from the Federal Statistical Office (2022) indicate an average life expectancy of 86.6 years for Swiss women in the 1961-1966 cohorts. Assuming retirement at age 64 and combining the expected retirement time with a salient reduction by one year, I calculate that a 1% reduction in retirement time implies on average 2.39% higher voter participation.

Voter Opinion

Part ii) of Table 4 presents the results from the cross-sectional difference-in-differences model, where the difference in the probability of rejecting the reform (conditional on having voted) around the cutoff for women is compared with those of men. All models control for third-degree age polynomials, interacted with gender. Again, different columns report variation in the cutoff year (1960/61 or 1959/60) and the exclusion of the 1960 cohort. Despite the limited sample size, the effect of the instrumental loss on the probability of rejecting the reform (conditional on voting) is generally statistically significant and large. The donut-hole model reports a highly significant 20.2 percentage point increase in the rejection rate at the optimal range τ^* of 7 years. Using the baseline rejection rate of 74.7% for affected women (cohorts 1960-1966), the effect translates into a 37.1% increase in reform rejection conditional on having voted. This substantial impact is consistent with the rationale presented in Section 2, since here all the three possible flows (abstention to *no*-zone, *yes*-zone to abstention, and *yes*-zone to *no*-zone) are in favor of higher rejection rates.

Figure 7 shows the variation of the effects depending on the range τ: as for voter participation, the impact on the reform rejection is stronger near the cutoff. The same arguments as above apply here. In addition, note that age is capped at 83 years in the survey data, so the older side of the cutoff runs out of observations after about 20 years.

A standardization using the advertised loss of CHF 26,000, implies that a loss of CHF 1,000 increases reform rejection conditional on voting by 0.78 percentage points (+1.43%).[13]

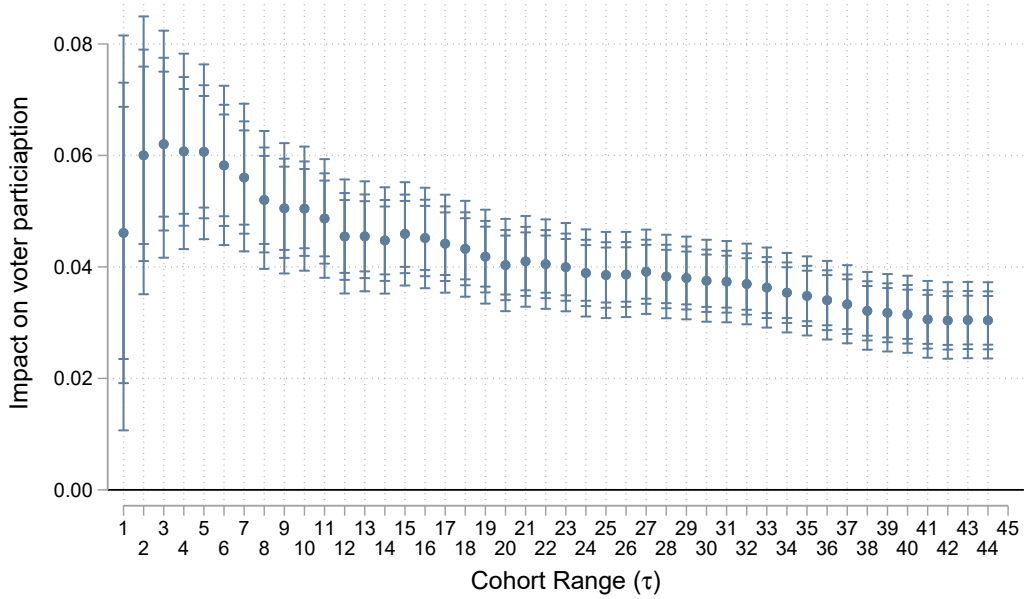
[13] Using lost (pension) time, I obtain that a 1% reduction in retirement time implies on average 8.38% higher rejection rates.

Table 4: Impact of the Personal Loss on Voter Behavior.

	i) Vote Participation			ii) Reform Rejection		
	(M1)	(M2)	(M3)	(M1)	(M2)	(M3)
δ at τ^*_{RDD}	.052***	.058***	.057***	.240***	.202***	.153*
	(.006)	(.006)	(.006)	(.090)	(.077)	(.078)
δ at $\tau = 2$	.043***	.060***	.042***	.374***	.298**	-.008
	(.010)	(.010)	(.010)	(.142)	(.138)	(.146)
δ at $\tau = 5$	.054***	.061***	.056***	.240***	.223**	.092
	(.006)	(.006)	(.006)	(.090)	(.090)	(.093)
δ at $\tau = 10$	.047***	.050***	.050***	.215***	.170**	.125*
	(.004)	(.004)	(.004)	(.068)	(.069)	(.069)
Cutoff year	1960/61	1959/61	1959/60	1960/61	1959/61	1959/60
τ^*_{RDD}	6	6	5	5	7	7
Vote fixed-effects	yes	yes	yes	no	no	no
Individual fixed-effects	yes	yes	yes	no	no	no
No. obs.	12,688,030	12,497,455	12,688,030	2,085	2,039	2,085
No. ind.	534,432	368,714	368,858	2,085	2,039	2,085
No. treated wom. at τ^*_{RDD}	20,693	20,693	16,785	113	158	169
No. treated wom. at $\tau = 2$	6,575	6,575	6,336	44	44	51
No. treated wom. at $\tau = 5$	17,211	17,211	16,785	113	113	116
No. treated wom. at $\tau = 10$	33,695	33,695	33,645	229	229	235
Adjusted R^2 at τ^*_{RDD}	.03	.04	.03	.10	.10	.10

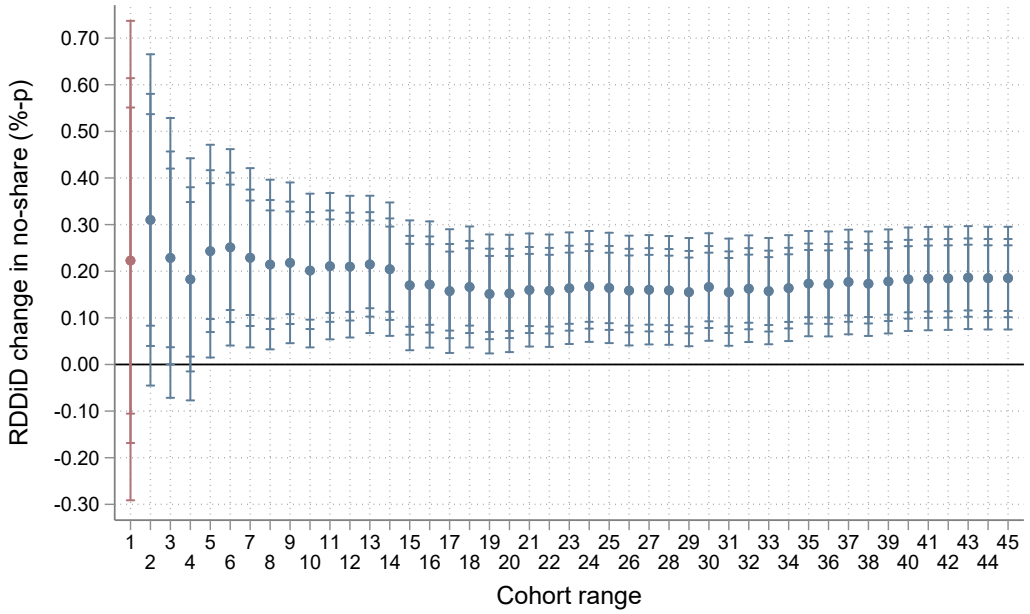
Source: i) Geneva and St. Gallen; ii) VOX Survey data. Method: i) Triple-Difference Estimation; ii) Difference-in-Differences Estimation. Further controls: i) Age (pol. 3) by gender, vote quarter, vote year; ii) Age (pol. 3) by gender. Standard errors in parentheses: i) Clustered at individual-level; ii) Heteroskedasticity-robust. *,**,***: Significant at the 10%-, 5%-, 1%-level.

Figure 6: Impact of the Personal Loss on Voter Participation by Range Width τ.



Source: Geneva and St. Gallen. Method: Donut-hole DDD, 1959/1961 (M2).
Blue: Significantly different from 0 at the 90%-level. 90%, 95% and 99% CI reported.

Figure 7: Impact of the Personal Loss on Reform Rejection by Range Width τ.



Source: gfs.bern. Method: Donut-hole DiD, cutoff 1959/61 excluding 1960.
Blue: Significantly different from 0 at 90%-level. 90%, 95%, 99% CI reported.

Voter Political Attitudes

Table 5 presents the results from the cross-sectional difference-in-differences donut-hole model (M3) on four outcomes related to political attitudes. "No-Parties" is a binary variable indicating whether the respondent would vote for a party that supported the referendum if a national election was held the following weekend.[14] The other variables indicate the trust in trade unions, government and business associations on a discrete 0/1 scale. The results do not indicate significant shifts in political preferences caused by the instrumental loss. The lack of statistical significance may be due to the small sample size, although the results on reform rejection are based on an even smaller sample. These results are robust to restricting the sample to respondents who reported voting in *AHV-21*.

Table 5: Impact of the Personal Loss on Voter Attitudes.

	i) Expected Positive		ii) Expected Negative	
	NO-Parties	Lab. Unions	Government	Business Assoc.
δ at τ^*_{RDD}	-.001	-.013	-.000	.035
	(.078)	(.036)	(.031)	(.034)
δ at $\tau = 2$	.207	.053	.061	-.019
	(.128)	(.064)	(.060)	(.058)
δ at $\tau = 5$	-.001	.006	.037	.041
	(.084)	(.041)	(.036)	(.037)
δ at $\tau = 10$	.034	-.013	-.018	.032
	(.065)	(.033)	(.029)	(.029)
Cutoff year	1959/61	1959/61	1959/61	1959/61
τ^*_{RDD}	6	7	8	6
No. obs.	2,370	2,702	2,799	2,611
No. treated wom. at τ^*_{RDD}	141	190	236	157
No. treated wom. at $\tau = 2$	49	52	54	50
No. treated wom. at $\tau = 5$	117	131	141	126
No. treated wom. at $\tau = 10$	236	276	294	267
Adjusted R^2 at τ^*_{RDD}	.01	.04	.00	.01

Source: VOX Survey data. Method: Difference-in-Differences Estimation, (M2). Further controls: Age (pol. 3) by gender. Heteroskedasticity-robust standard errors in parentheses. *,**,***: Significant at the 10%-, 5%-, 1%-level.

[14] Parties are assigned according to their vote indication, which were collected by Swiss Votes for large parties and supplemented for small parties. No statistical difference in the intention to abstain is found at the age-cutoff for women. Accordingly, result do not change when excluding individuals willing to abstain.

Intra-Household Spillovers

The *AHV-21* constellation can be used to measure the spillover transmission of pocketbook effects within households.[15] In particular, individuals close to treated women are expected to have increased economic incentives, either directly in case of married couples (the AHV benefit is calculated jointly) or indirectly in case of unmarried couples and of individuals living in the same household (by taking household income into account).[16] Individuals in couple with women just too old to be treated provide a counterfactual, whereas – similarly to the main identification, but in reversed roles – the same difference on individuals in couple with men further improves the identification.

Table 6 presents the results of the impact of intra-household spillover of personal loss on voter participation in the Canton of St. Gallen. Part i) focuses on married couples, whereas part ii) focuses on individuals living in the same household. I show results for the triple-differences fixed effects models (DDD) formalized in Equation (5) both with cutoff at 1959/1960 and with the exclusion of the 1960 cohort, as well as for males difference-in-differences panel models (TDD) with the exclusion of the 1960 cohort.[17] The three models produce qualitatively similar results. My preferred model, the triple-differences fixed effects model excluding the 1960 cohort (donut-hole), reports a significant increase in vote participation of 3.2 percentage points at the optimal range τ^* of 6 years among married individuals and of 4.8 percentage points within household. These estimates account for about half the magnitude of the direct women reaction of 6.9 percentage points found in the canton of St. Gallen (see next section).

Figure 8 shows the variation of the effects depending on the range τ: the marriage spillover impact of the personal loss on voter participation is highest when the treated wife is close to the retirement age, almost reaching an increased voter participation of 10 percentage points in the first 2 years. Then, it declines as the estimation range is widened and becomes statistically insignificant at a range of 8 years. The decrease seems to be faster through spillover effects than through direct effects (Figure 6). The pattern is very similar when considering within-household spillovers.

[15] As explained in the discussion of the DiD identifying assumptions in Section 3.3, I do not expect these spillovers to bias the main estimation because the incentives are balanced around the cutoff age.

[16] In the canton of St. Gallen, on the *AHV-21* vote, 99.5% of the individuals married to women aged between 50 and 70 years old were men, and 0.5% were women in same-sex marriages.

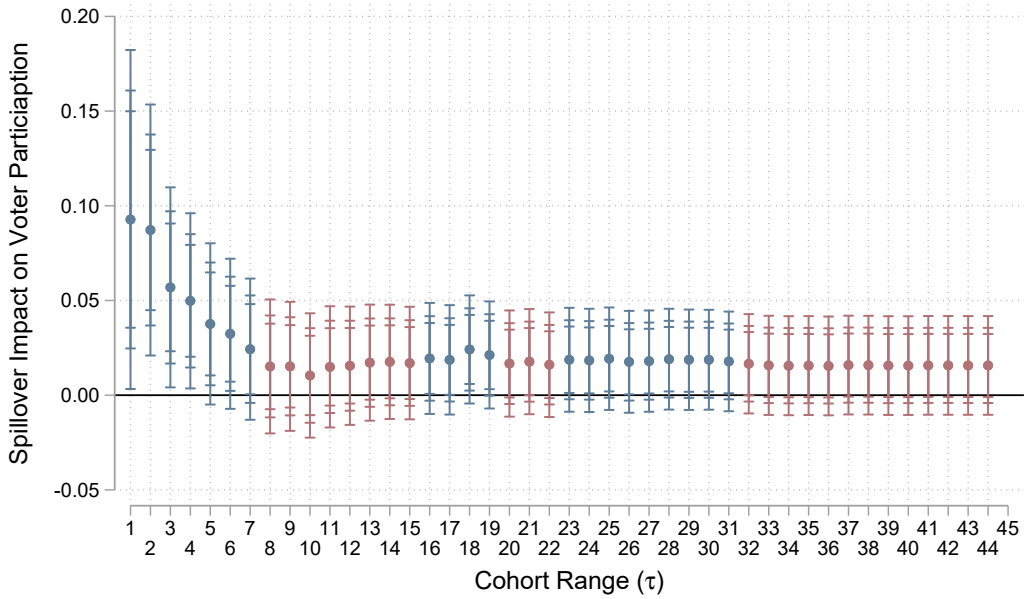
[17] As expected, the placebo difference-in-differences panel model (TDD) on women – that is, using males' cohort as treatment variable – produces insignificant results.

Table 6: Intra-Household Spillover Impact of the Personal Loss on Voter Participation.

	i) Marriage			ii) Same Household		
	DDD		*TDD*	*DDD*		*TDD*
	(M1)	(M2)	(M2)	(M1)	(M2)	(M2)
δ at τ^*_{RDD}	.031**	.032**	.040***	.040**	.048***	.022**
	(.015)	(.015)	(.010)	(.016)	(.017)	(.009)
δ at $\tau = 2$	.065**	.087***	.053***	.039	.084**	.049***
	(.026)	(.026)	(.017)	(.028)	(.036)	(.018)
δ at $\tau = 5$	.040**	.038**	.043***	.050***	.061***	.025**
	(.016)	(.017)	(.011)	(.017)	(.019)	(.010)
δ at $\tau = 10$	.013	.010	.031***	.015	.018	.017**
	(.013)	(.013)	(.009)	(.013)	(.014)	(.008)
Cutoff year	1960/61	1959/61	1959/61	1960/61	1959/61	1959/61
τ^*_{RDD}	6	6	6	6	6	6
Vote fixed-effects	yes	yes	yes	yes	yes	yes
Individual fixed-effects	yes	yes	yes	yes	yes	yes
Age (by Gender) fixed-effects	yes	yes	yes	yes	yes	yes
No. obs.	2,601,539	2,581,335	1,212,012	2,601,539	2,565,152	1,207,366
No. ind.	165,475	165,304	78,664	165,475	165,419	78,722
No. treated wom. at τ^*_{RDD}	4,633	4,633	2,354	9,819	9,819	4,097
No. treated wom. at $\tau = 2$	1,543	1,543	791	3,034	3,034	1,232
No. treated wom. at $\tau = 5$	3,898	3,898	1,972	8,254	8,254	3,441
No. treated wom. at $\tau = 10$	7,492	7,492	3,800	16,136	16,136	6,817
Adjusted R^2 at τ^*_{RDD}	.05	.05	.03	.05	.05	.05

Source: St. Gallen. Method: DDD: Triple-Difference Estimation: TDD: Within gender, difference-in-differences estimatin. Further controls: married, vote quarter, year trend. Individual-level clustered standard errors in parentheses. *,**,***: Significant at the 10%-, 5%-, 1%-level.

Figure 8: Marriage Spillover Impact of the Personal Loss on Voter Participation by Range Width τ.

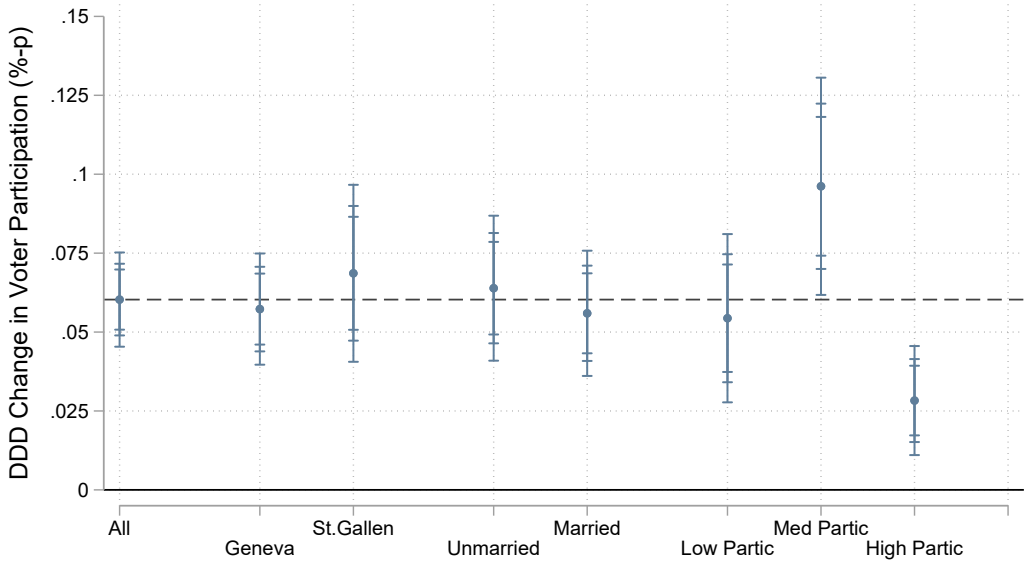


Source: St. Gallen. Method: Donut-hole DDD, 1959/1961 (M2).
Blue: Significantly different from 0 at the 90%-level. 90%, 95% and 99% CI reported.

Effect Heterogeneity

The *AHV-21* constellation can be used to measure the effect heterogeneity of pocketbook effects. Figure 9 shows how the impact of personal loss on voter participation varies by canton, civil status and usual participation for the triple-differences fixed effects models (DDD) with the exclusion of the 1960 cohort (see Figure A5 and Figure A6 for further variation depending on the treatment range τ). The local treatment effects are slightly larger in the canton of St. Gallen than in Geneva, and among unmarried than married individuals. The biggest difference is recognizable by tertile of usual participation (calculated prior to *AHV-21*): the economic incentive brings individuals with medium usual participation to vote (2nd tertile: participating in 22.5% to 72.5% of the votes) rather than those who rarely participate (1st tertile); the most inelastic group is, mechanically, the group of frequent voters. Figure A15 further disentangles the effect heterogeneity by decile of usual participation, confirming the concave path with the greatest mobilization effects occurring between the 3rd and the 6th deciles.

Figure 9: Heterogeneous Impact of the Personal Loss on Voter Participation.

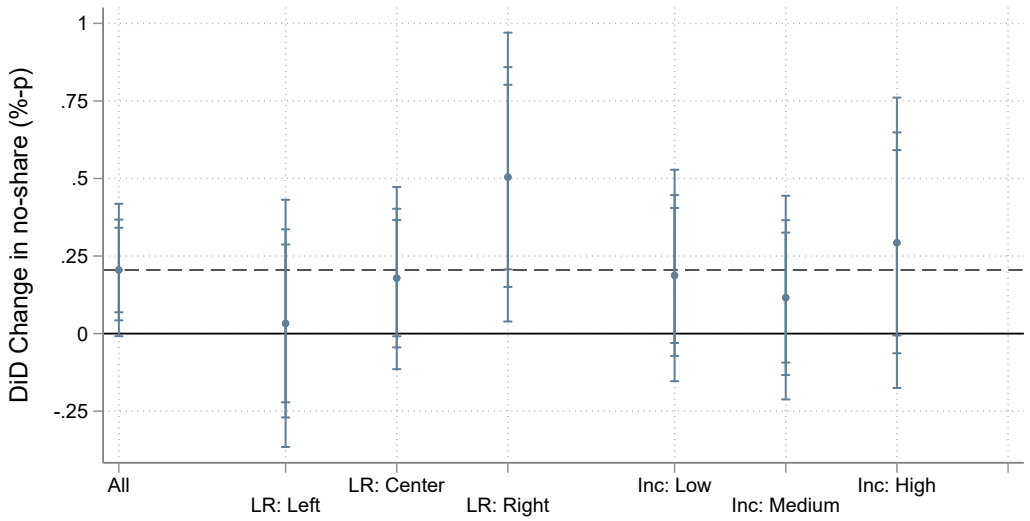


Source: Geneva and St.Gallen. Method: Donut-hole DDD, 1959/61 (M2), τ = 6, with controls. 90%, 95%, and 99% CI reported.

Figure 10 shows how the impact of personal loss on rejection rates varies by self-declared political orientation and income (see Figure A16 and Figure A17 for further variation depending on the treatment range τ). Whereas the effect of political loss clearly varies by political orientation, no pattern can be recognized by income level.[18] Using a 7-years range width around the cutoff, I find almost zero effects of the self-interest factor on the rejection rates of left-wing voters. Instead, centrist and – in particular – right-wing voters increase their rejection rates by 25- and 50 percentage points, respectively. This heterogeneity is confirmed descriptively in Figure 11: Centre and right-wing voters generally approve the reform, with rejection rates between 20 and 40%; however, when having to pay personally, a substantial increase in rejection rates is observed. On average, treated right-wing women have similar rejection rates as treated left-wing women. This result suggests that right-wing voters may be particularly sensitive to economic self-interest. Beware that, while the comparison with centrist voters is fair, left-wing voters already consistently oppose the reform, and their absent response to self-interest incentives may simply reflect the lack of space to measure a shift in preferences at the margin.

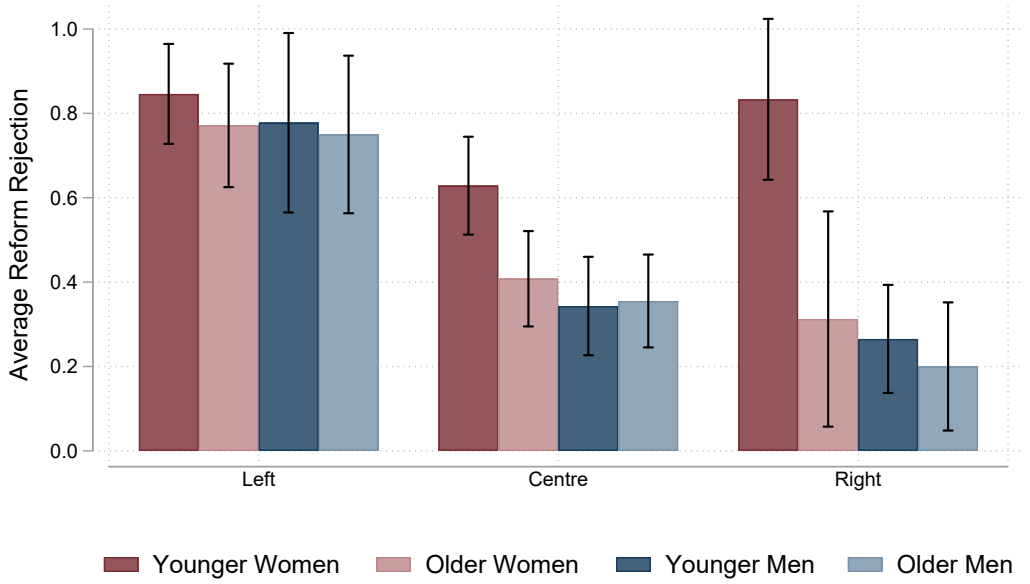
[18] Notice that the coefficients in Figure A16 are estimated controlling for self-declared income, which is therefore not a confounding factor.

Figure 10: Heterogeneous Impact of the Personal Loss on Reform Rejection.



Source: gfs.bern. Method: Donut-hole DiD, 1959/61 (M2), $\tau = 7$, with controls.
90%, 95%, and 99% robust CI reported.
Self-reported political orientation: Left (0-3), Centre (4-6), Right (7-10).
Self-reported income: Low (1-4), Medium (5-9), High (10-15).

Figure 11: *AHV-21* Rejection Shares by Gender, Cohort and Political Orientation.



Source: gfs.bern.
Younger: Cohorts 1961-67. Older: Cohorts 1953-59.
Self-reported political orientation: Left (0-3), Centre (4-6), Right (7-10).

6 Discussion

The results presented in Section 5 point to a clear impact of personal loss on voter behavior during the referendum. In my preferred models, voting participation and reform rejection among the affected women increase by 5.8 and 20.2 percentage points (+10.6% and +37.1%), respectively. Both findings are consistent with the theoretical model proposed in Section 2 and hold to a variety of robustness checks. However, while individuals react strongly to instrumental losses during the referendum, there is no evidence of significant changes in political attitudes toward the government, political parties, trade unions or business associations. All of these actors had a strong position in the vote and campaigned heavily for or against the reform. One might therefore expect that the personal loss would fuel their political reward or punishment.

The causal literature examining pocketbook effects on political attitudes is quite robust. Among others, Manacorda et al. (2011), Pop-Eleches and Pop-Eleches (2012), Labonne (2013), De La O (2013), Zucco Jr. (2013), Elinder et al. (2015), Galiani et al. (2019), Alpino (2018), Zimmermann (2021), Levy (2023), and Kaba (2022) link individual gains from government spending or tax cuts to increased political support in Uruguay, Romania, Philippines, Mexico, Brazil, Sweden, Honduras, Italy, India, France, and Turkey, respectively.[19] Why should this case be any different? And more importantly, can this result on the pocketbook effects of retirement age increases be generalized outside Switzerland? In general, the existing causal literature on personal benefits and political attitudes has approached the question from slightly different angles. In the following paragraphs, I discuss whether and how these differences may be relevant.

First, the policy. Past literature has focused on policies formulated as expansionary (gains) rather than restrictive (losses). If anything, however, theories of loss aversion in voter behavior would imply larger political responses (Alesina and Passarelli, 2019; Lockwood and Rockey, 2020). Moreover, socio-tropic responses to losses have been found in the context of austerity (Fetzer, 2019). Besides the positive or negative framing, timing may also be important. Political reactions to already enforced policies may differ in terms of awareness and dependency from reactions to policies with effects materializing in the future. Literature on the substantial political effects of electoral promises suggests that this may not be the case (Elinder et al., 2015; Alpino, 2018; Levy, 2023). Finally, for both arguments, the large response found in the referendum suggests that the policy effects were visible enough.

Second, the target population. Due to the focus on government spending, the existing lit-

[19] Among others, from the socio-tropic side, Levitt and Snyder (1997), Huet-Vaughn (2019) and Fetzer (2019) find the provision of regional public goods to positively influence political support.

erature mostly based on voters in the lower part of the income distribution. If pocketbook effects depend on budget constraints, then the lack of changes in political attitudes may simply reflect a different local estimate. Enke et al. (2023) formulate a similar argument, predicting material interests to have more influence among poorer voters. I estimate treatment effects for all the women aged around 63, not necessarily just poor women. Again, the strong response observed in the referendum for the whole group and the absence of income-effect heterogeneity suggest that a pocketbook reasoning is present beyond hand-to-mouth voters.

Third, the issue relevance. The increase of the retirement age may not be important enough to shape political attitudes. This would explain why I find a strong reaction during the referendum, but not a shift in political attitudes. For example, political attitudes in Switzerland might rather be driven by topics such as migration, climate change or the labor market situation. Qualitative evidence suggests that this is not the case. *AHV-21* was the second most important of 26 objects evaluated in the VOX survey by gfs.bern since November 2020, with an average importance of 8.03 points on a 0-10 scale.[20] Further, the Credit Suisse Barometer found that 37% of the respondents ranked pensions among their top-5 concerns (Golder et al., 2022).[21]

Fourth, the political perspective. Consistent with Elinder et al. (2015), I find that pocketbook reasoning operates prospectively (during the referendum) rather than retrospectively (after the referendum) – even when assessed on the same object. Differently from other government spending policies, the raise of the retirement age is a lump sum policy. Therefore, the affected women do not disproportionately expect further future losses in the aftermath of the vote, and thus do not update their broad political attitudes in a prospective way. At the same time, they do not blame political actors for their personal loss in the aftermath of the public decision, and thus do not update their attitudes in a retrospective way. This argument can be linked to similar causal literature on the updating of political attitudes through economic incentives, such as the decreased preferences for redistribution following a lottery win (Doherty et al., 2006) or following information about the own rank in the income distribution (Karadja et al., 2017).

Fifth, the political process. The absence of broad pocketbook effects may be linked to the presence of direct democracy. On the one hand, direct democracy acts as a relief valve by providing a medium for expressing opinions. The very fact of being asked for a (binding) opinion may itself reduce political punishment. On the other hand, direct democracy has a legitimizing effect on political action and can shift the burden of responsibility from

[20] The average importance measured over this period was 6.68 (standard deviation of 0.95), with a minimum of 4.15 (change in film law) and a maximum of 8.75 (revision of the Covid-19 law).

[21] The survey covers 1'774 randomly sampled respondents. Only climate change worries the Swiss more than pensions (39%), followed by energy provision (25%), relations with Europe (25%), and inflation (24%).

political actors to the citizens. It is more difficult to blame the government for something that has been decided by the majority of the citizens. These arguments could partly explain why Switzerland steadily had the highest level of the government trust among OECD countries.[22]

7 Conclusion

This paper provides empirical evidence on the effects of particular interests on voter behavior in the context of raises of the statutory retirement age. Exploiting a cohort-by-gender discontinuity in a Swiss reform, I estimate that an advertised loss of CHF 26,000 increases vote participation by 5.8 percentage points (+10.6%) and reform rejection by 20.2 percentage points (+37.1%) during the referendum. Moreover, individual personal loss spills over to married individuals (+3.2pp) and within households (+4.8p). These results are consistent with a model in which voters behave by balancing individual and general interests, while enjoying expressive utility when voting for their preferred option and doing so more when their persuasion for an option increases. Thus, individual benefits can play a considerable role on turnout and reform outcomes even in large groups, when the probability of being pivotal is small. Heterogeneity analyses show that the increased participation is driven mainly by individuals with medium usual participation, and that the increased rejection is driven mainly by right-wing women. I then test whether this voter response translates into a change in the support for the government, political parties, trade unions, or business associations. Interestingly, I find no evidence of such a preference shift. This result suggests that the long-term opposition to raising the retirement age may rather be based on general disagreement with the policy than on particular interests. I discuss the external validity of this finding. On the one hand, the lump-sum nature of the policy may be responsible for lacking shift of pocketbook incentives into broader political attitudes, which would argue for the generalization of the result beyond the Swiss case. On the other hand, direct democracy may play a special role in this context, acting as a relief valve (by providing a medium for expression) and as a legitimisation tool (by shifting the burden of responsibility to the citizens themselves). Further research across different political system could disentangle the role of these mechanisms.

[22] In 2021, 83.8% of the Swiss population had confidence in the government; in the same year, the average in OECD countries was 46.70% (standard deviation of 15.36), with a minimum of 21.58% (Slovakia) and Norway following Switzerland at the top with 77.35% (OECD, 2021). The evolution of government trust is shown in Figure A18.

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8 Appendix

A The Swiss Pension System

The Swiss pension system is organized into three pillars: i) Mandatory national insurance; ii) Occupational insurance; iii) Private provision.

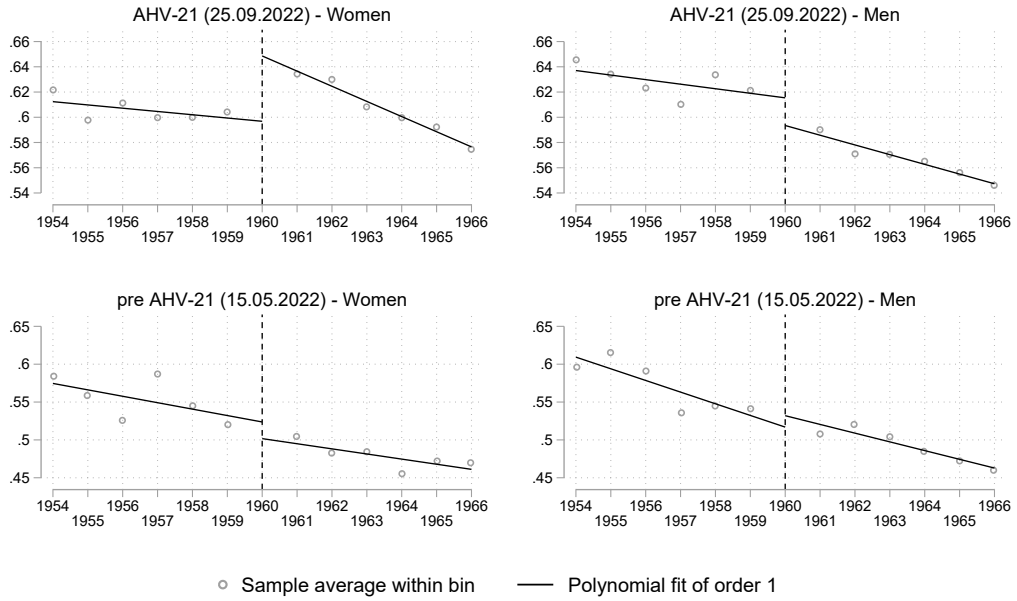
The first pillar is the Old Age and Survivors Insurance (OASI, *Alters- und Hinterlassenenversicherung AHV*), which is a state pension designed to cover subsistence needs in old age. Its financing is based on a generational pact, as the working generation covers the expenses of the retired generation. The balance of this pay-as-you-go system is under pressure as the Swiss population ages and the baby boom generation retires. The entitlement to AHV benefits is reached at the reference age (65 years for men; 64 years for women before the reform, and 65 years after the reform). Early retirement is possible up to 2 years before the statutory age, with a 6.8% reduction in the pension for each year of early claiming. Later retirement is also possible, with a pension increase of 5 to 6.9% for each year up to 5 years after the statutory age. The AHV structure is highly redistributive. In 2022, employed dependants contributed to the financing of the AHV through a flat rate of 8.7% on their total salary. AHV benefits depend on past contributions but are plafonated. In 2022, the minimum AHV benefit for individuals was CHF 14,340 per year and the maximum was CHF 28,680 per year. For married couples, the maximum joint benefit is further plafonated at 1.5 times the individual benefit. In 2021, the AHV contributed to 46% and 70% of average income from the first two pillars of male and female retirees, respectively (Federal Statistical Office, 2021).

The second pillar is the occupational pension scheme, which is designed to supplement the first pillar and guarantee approximately 60% of the last salary. It is financed by compulsory contributions paid by employers and employees during the working life. At retirement age, the accumulated funds are accessible in the form of a lump sum or as monthly pensions. The increase in the statutory retirement age has an indirect impact on the second pillar, as it delays the right to access the funds and may increase the contribution period.

The third pillar is a voluntary individual savings scheme to cover other personal needs. It is financed by tax-deductible contributions. The accumulated savings can only be accessed when the reference retirement age is approached, except in a few special cases. The impact of raising the retirement age on the third pillar is limited.

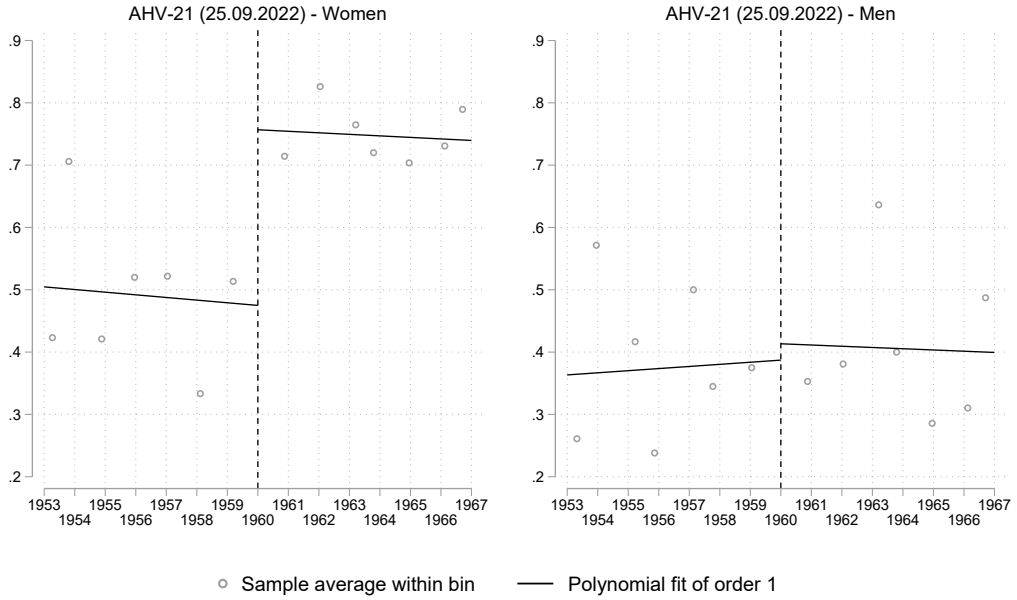
B Discussion of the Identifying Assumptions: Additional Figures

Figure A1: Voter Participation by Cohort, Object and Gender ($\tau^* = 6$).



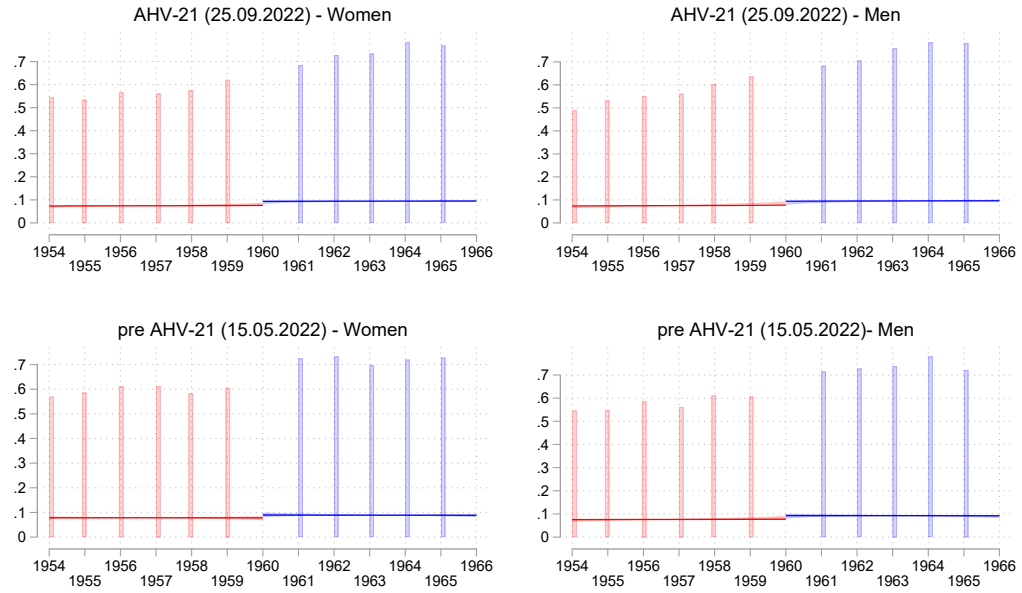
Source: Geneva and St. Gallen.
Method: Donut-hole linear RDD, 1959/61 (M2), τ = 6.

Figure A2: Reform Rejection by Cohort and Gender ($\tau^* = 7$).



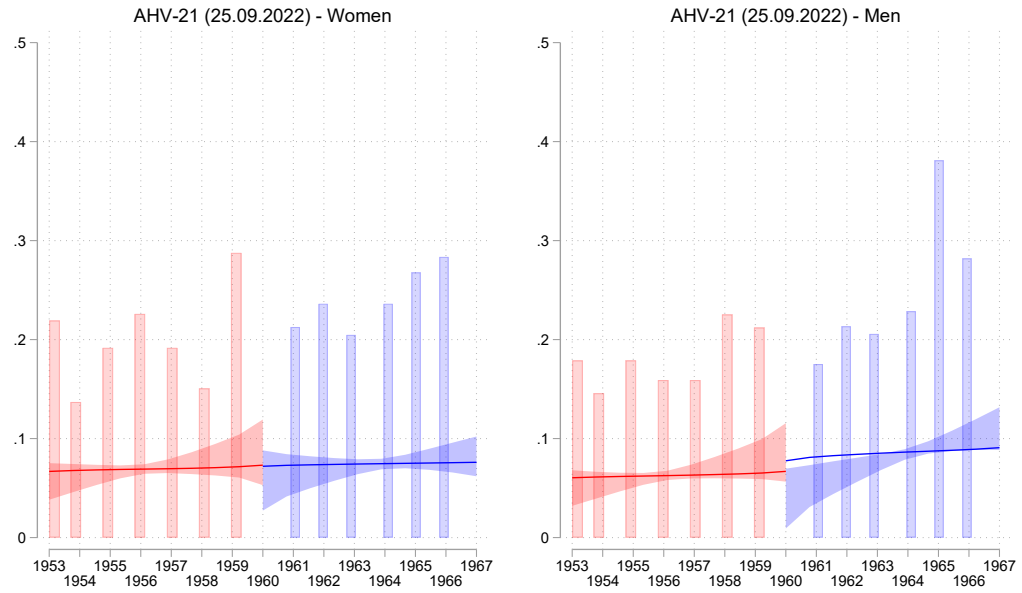
Source: gfs.bern.
Method: Donut-hole linear RDD, 1959/61 (M2), τ = 7.

Figure A3: McCrary (2008)'s Test for Manipulation, Administrative Data ($\tau^* = 6$).



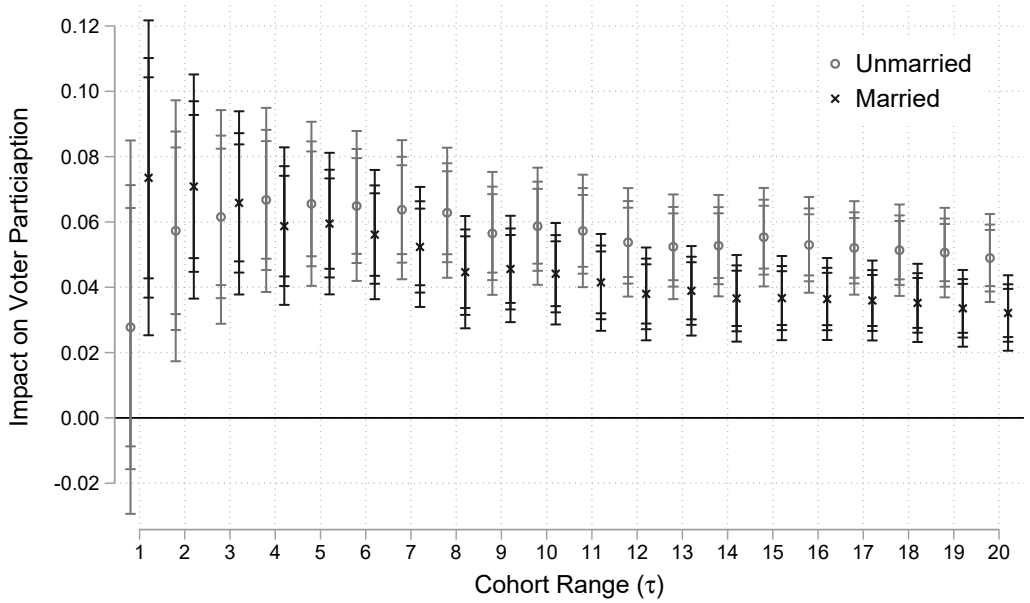
Source: Geneva and St. Gallen.
Method: McCrary (2008) test for the donut-hole linear RDD, 1959/61 (M2), τ = 6.

Figure A4: McCrary (2008)'s Test for Manipulation, gfs.bern ($\tau^* = 7$).



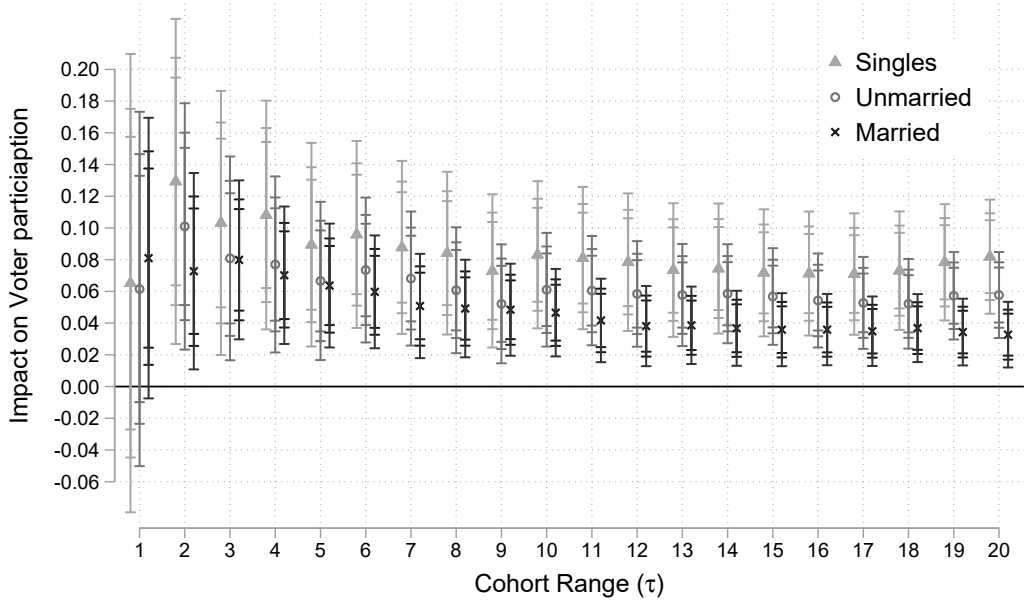
Source: gfs.bern.
Method: McCrary (2008) test for the donut-hole linear RDD, 1959/61 (M2), τ = 7.

Figure A5: Impact of the Personal Loss on Voter Participation by Range Width τ and Marriage Status.



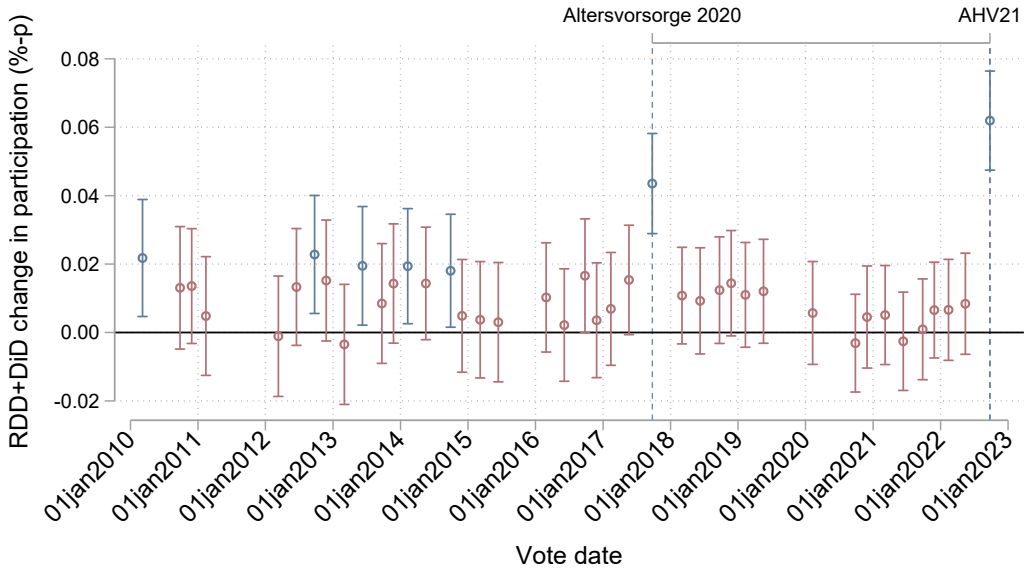
Source: Geneva and St. Gallen. Method: Donut-hole DDD, 1959/1961 (M2).
90%, 95% and 99% CI reported.

Figure A6: Impact of the Personal Loss on Voter Participation by Range Width τ and Marriage Status (with Singles).



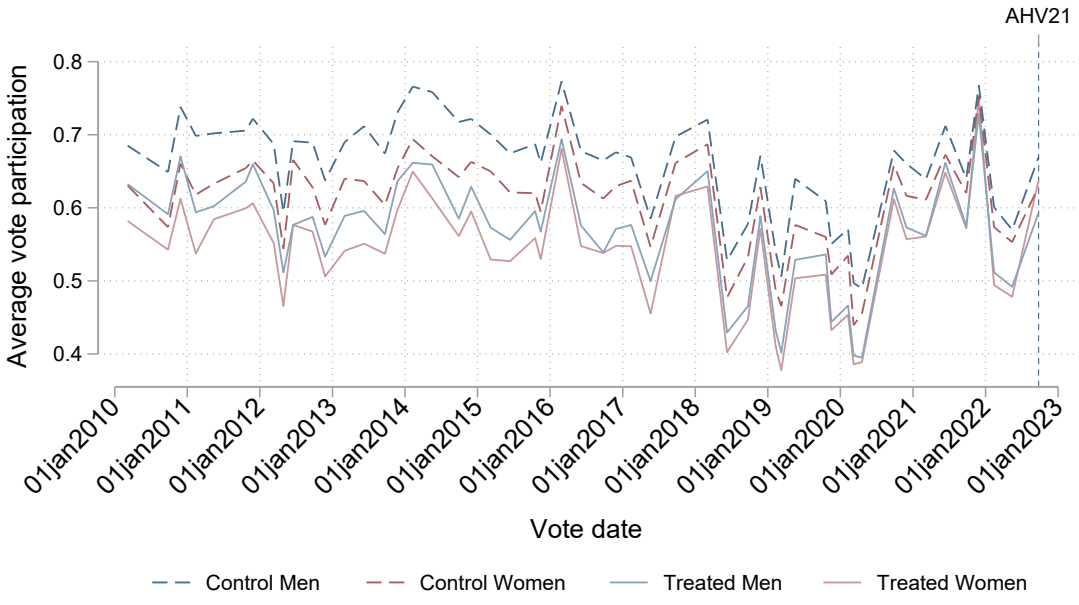
Source: St. Gallen. Method: Donut-hole DDD, 1959/1961 (M2).
90%, 95% and 99% CI reported.

Figure A7: Placebo RDD.



Source: Geneva and St. Gallen.
Blue: Significantly different from 0 at 95%-level.
Method: Donut-hole RDD (Range = 6 years), distinct by gender.

Figure A8: Common Trends.



Source: Geneva and St. Gallen.
Treated: Aged 56-61; Control: Aged 63-68.

C Salience

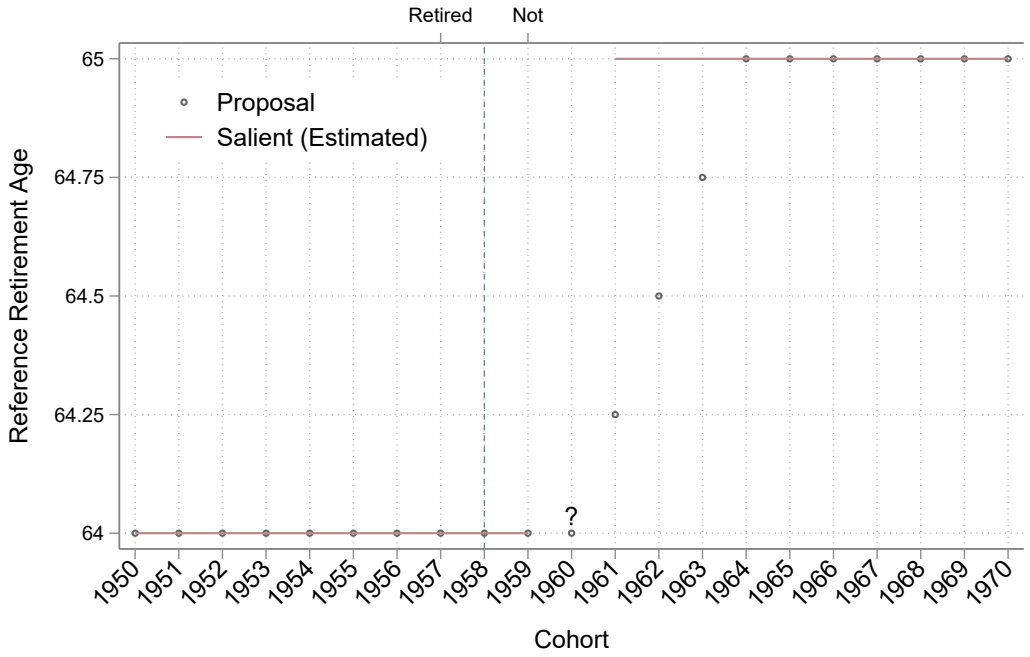
I claim that most (marginal) voters react to political campaign communication rather than to concrete policy plans. As campaign communication often trades complexity for clarity, some voters may not be totally aware of the exact consequences of the object in vote.

The first argument in this direction is the response of the 1960-cohort. Women in this cohort are supposed to work in 2023 and retire in 2024 at 64 years, both according to the old pension scheme and the reform plans for the implementation of *AHV-21*. This cohort should thus theoretically belong to the control group. The timing of governmental plans is realistic, as it takes time for the parliament to formulate an application law. Differently from the 1959 cohort, which achieves the right to pension benefits in 2023, the 1960 cohort partly reacts to the reform as if it was facing costs. Specifically, using a one-year τ parametric RDD on women in *AHV-21*, I find a difference of 2.9 percentage points in voter participation between the 1960 and the 1959 cohort (statistically significant at the 5-% level). On the contrary, using the same setup, I find no statistically significant difference in voter participation between cohorts 1960 and 1961. I interpret this finding as lack of information on the precise governmental plans. A concurrent explanation could be mistrust or uncertainty with regard to the timing of the reform's implementation. Consequently, I implement a donut-hole approach to exclude measuring errors.

The second argument pointing towards the salience of political advertisement relates to the heterogeneous cost treatment in the transition phase in cohorts 1961-1969 presented in Table 1. To smooth the reform's entry into force, the government planned to gradually increase the reference retirement age and partly compensate it with a financial transfer. For some income brackets and cohorts, the compensation was calculated to neutralize completely the reforms' cost. I test whether the end of the compensation measures between the cohorts 1969 and 1970 has an impact on voter participation and reform rejection. I do not find any evidence of marginally younger cohorts responding stronger because of the lack of compensations.[23] Again, I interpret this finding through the fact that (marginal) voters may be mostly unaware of the reform details and that the treating information would be the one communicated by the political campaigns.

[23] The triple difference coefficient on voter participation is -0.003 for a one year range (1969 cohort treated, 1970 cohort control), 0.008 for a three years range, 0.010 for a five years range. None of the estimates is statistically significant at the 10% level, with individual-level clustered standard errors being 0.014, 0.009 and 0.007 respectively.

Figure A9: *AHV-21*: Estimated Model (M2) and Transition Phase.



D Robustness Checks

As a robustness check, I use the panel structure of the administrative data to look at the effect of *AHV-21* by gender and age dummies. I estimate the following Equation (7):

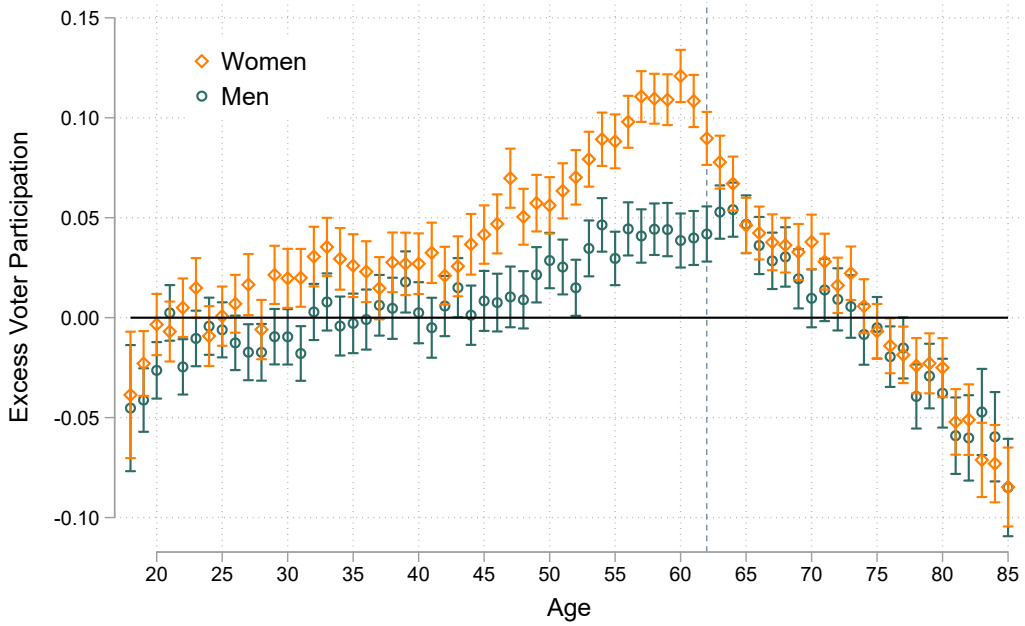
$$p_{it} = \sum_{y=18}^{85} \delta_y \cdot age_{yit} \cdot AHV\text{-}21_{it} + \sum_{y=18}^{85} \gamma_y \cdot age_{yit} \cdot AHV\text{-}21_{it} \cdot woman_i + \alpha_i + v_t + u_{it}, \tag{7}$$

where individual i's participation in vote t (p_{it}) is regressed on the *AHV-21* vote fixed effect, moderated in its heterogeneity by yearly age dummies and gender. I also use individual fixed effects (α_i), and fixed effects for votes other than *AHV-21* (v_t). u_{it} is the idiosyncratic error term.

For years between 18 and 85, Figure A10 shows the coefficients δ_y for men and the combination of coefficients δ_y and γ_y for women estimated through Equation (7). For both genders, the deviation from the average turnout varies with age: Excess participation is lower for young citizens, gradually grows with age and peaks around retirement age, decreasing again thereafter. Before retirement age, women have a higher excess participation than men. The gender excess participation gap is visible in the difference between the two series: it increases and becomes statistically significant as retirement age (and the cost of

the policy) approaches. After retirement, neither men nor women vote more than usual and no gap is recognisable since the age of 63.

Figure A10: Excess Voter Participation in *AHV-21* by Gender and Age.



Source: Geneva and St. Gallen. Method: AHV-21 fixed-effect deviation. 95%-CI reported.

Similarly, to disentangle intra-marriage spillover effects, I estimate the following Equation (8):

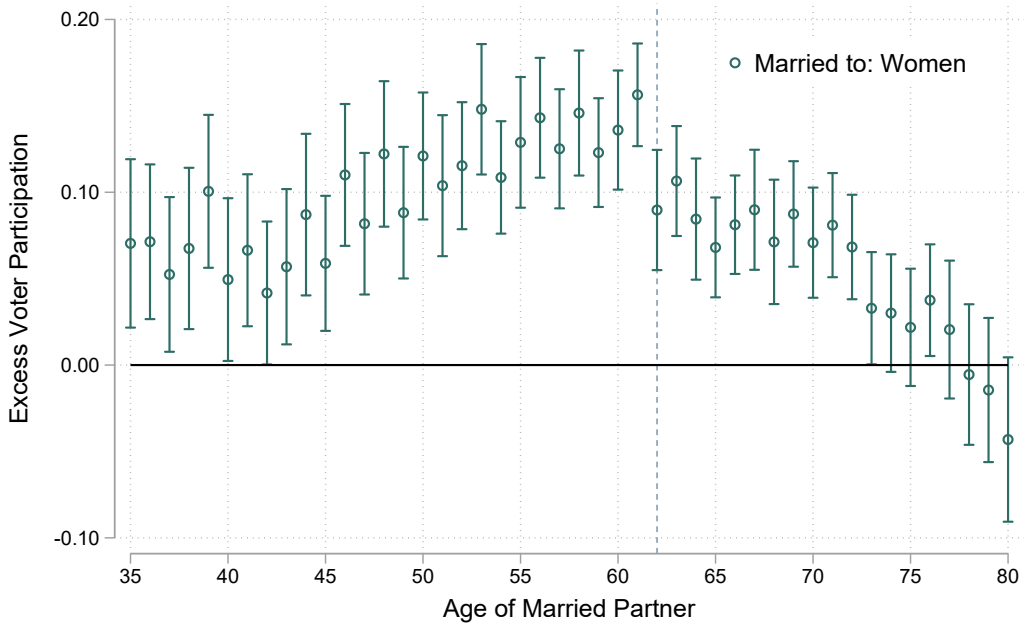
$$p_{it} = \sum_{y=18}^{85} \delta_y^{spil} \cdot age_{yit}^{spil} \cdot AHV\text{-}21_{it} + \sum_{y=18}^{85} \gamma_y^{spil} \cdot age_{yit}^{spil} \cdot AHV\text{-}21_{it} \cdot man_i^{spil} + \alpha_i + v_t + u_{it}, \tag{8}$$

where individual i's participation in vote t (p_{it}) is regressed on the *AHV-21* vote fixed effect, moderated in its heterogeneity by year dummies indicating the age of the married partner and the gender of the married partner. I also use individual fixed effects (α_i), and fixed effects for votes other than *AHV-21* (v_t). u_{it} is the idiosyncratic error term. Singles are kept in the regression receiving a married age dummy of 999, in order to better estimate of the vote fixed effects.

For values of y between 35 and 80 years, Figure A11 shows the coefficient δ_y^{spil} from Equation (8) for individuals married to a woman. Individuals married to a woman generally vote more than usual in *AHV-21*. As discussed in Section 3.3, this heterogeneity is partly explained by the own age. However, the drop in excess participation at wife's age of 62 years suggests that married individuals internalize the spillover interest. As a counterfactual,

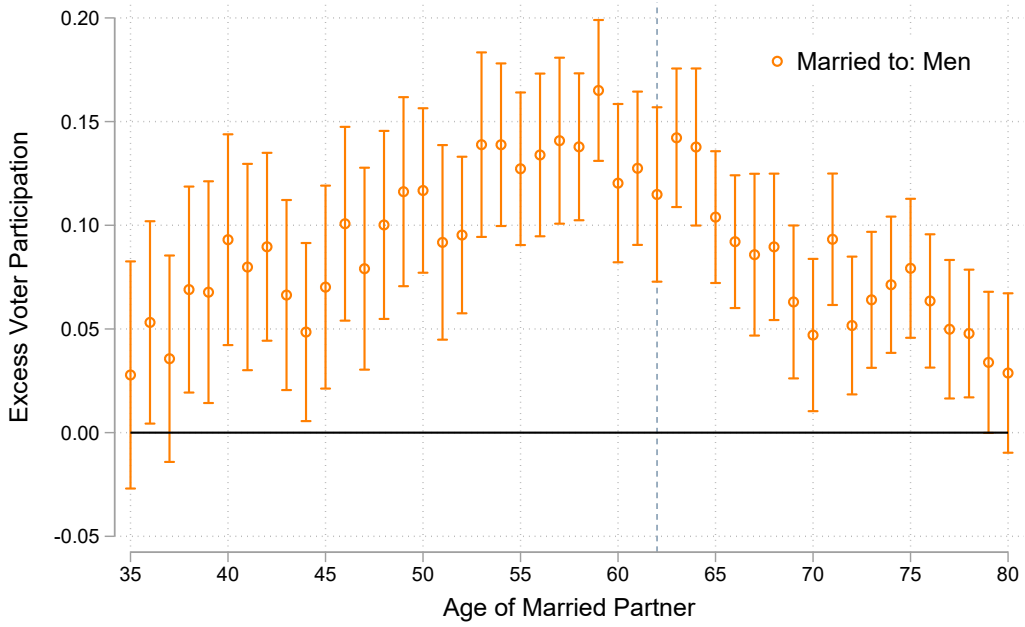
Figure A12 shows no discontinuity in the combination of coefficients δ_y^{spil} and γ_y^{spil} from Equation (8) for individuals married to a woman.

Figure A11: Excess Voter Participation in *AHV-21* by Wife's Age.



Source: St. Gallen. Method: AHV-21 fixed-effect deviation. 95%-CI reported.

Figure A12: Excess Voter Participation in *AHV-21* by Husband's Age.



Source: St. Gallen. Method: AHV-21 fixed-effect deviation. 95%-CI reported.

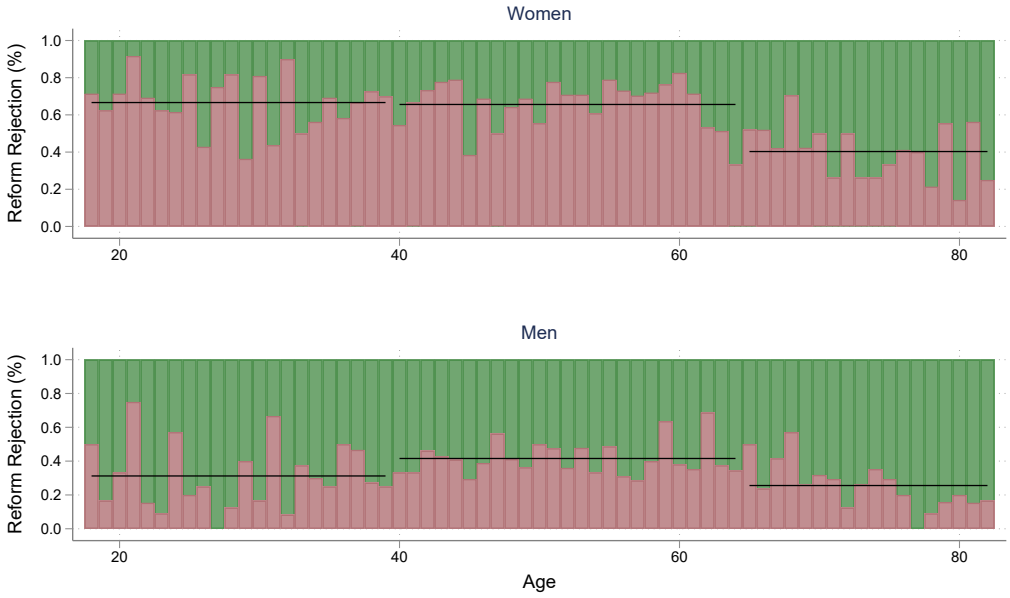
E Additional Figures

Figure A13: Campaign poster for the yes (left) and the no (right).

Image source: Swiss Radio and Television.

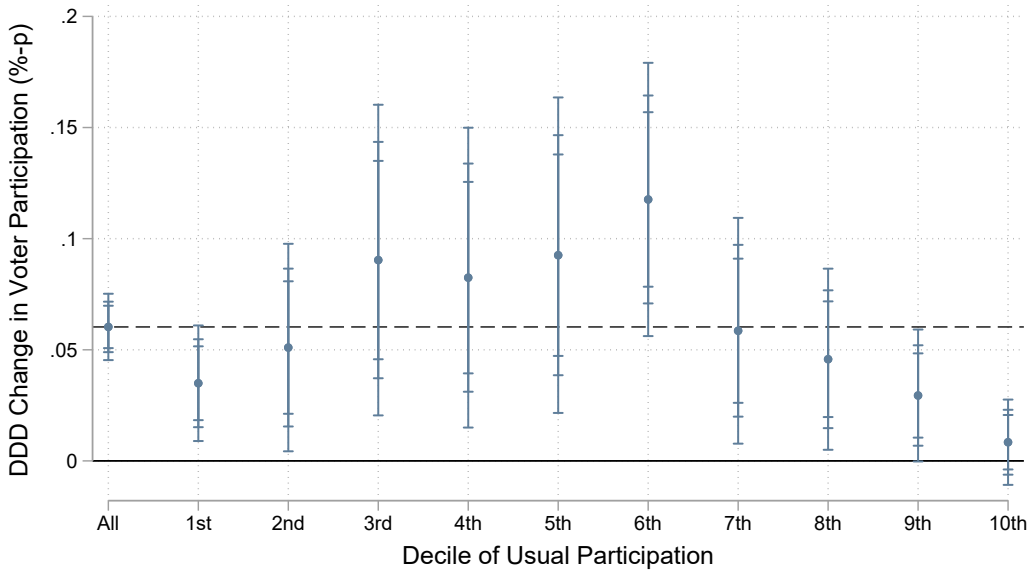
Translation: "Together we secure our AHV: 2x YES to AHV-21" (left); "CHF 26,000 less pensions? AHV-Dismantling: 2x NO" (right)

Figure A14: *AHV-21* Rejection Shares by Gender and Cohort.



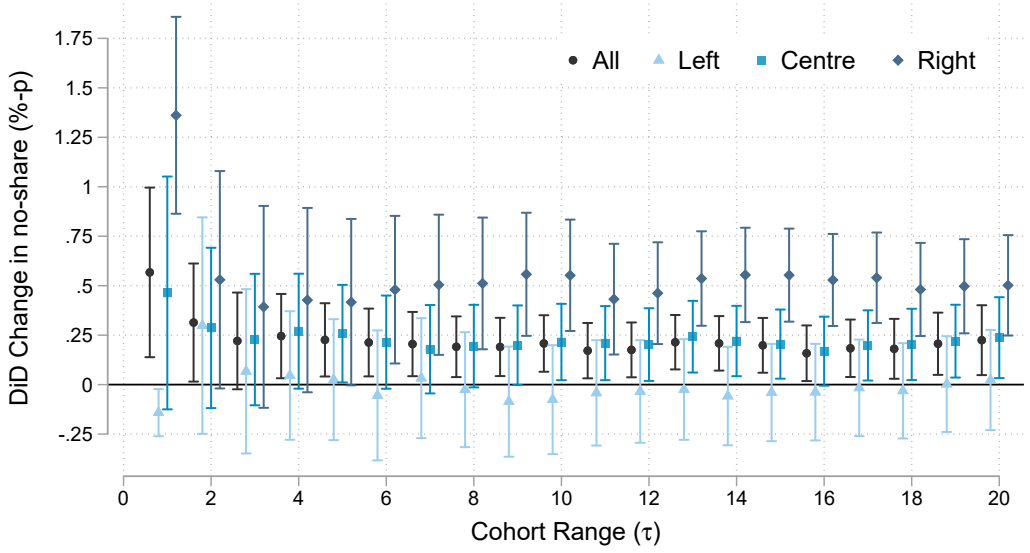
Source: gfs.bern.
Black lines: average rejection rates for age ranges: 18-39, 40-64, 65-82.

Figure A15: Heterogeneous Impact of the Personal Loss on Voter Participation by Usual Participation.



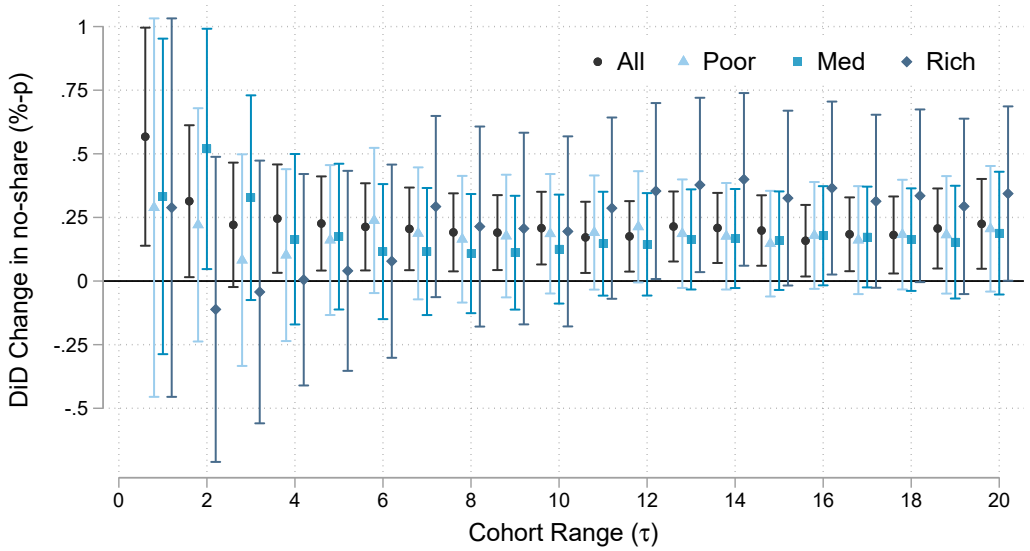
Source: Geneva and St.Gallen. Method: Donut-hole DDD, 1959/61 (M2), τ = 6, with controls.
90%, 95%, and 99% CI reported.
Decile values (%): 0 2.5 7.5 17.6 32.5 47.5 62.5 77.1 88.0 97.2 100.

Figure A16: Impact of the Personal Loss on Reform Rejection by Range Width (τ) and Political Preferences.



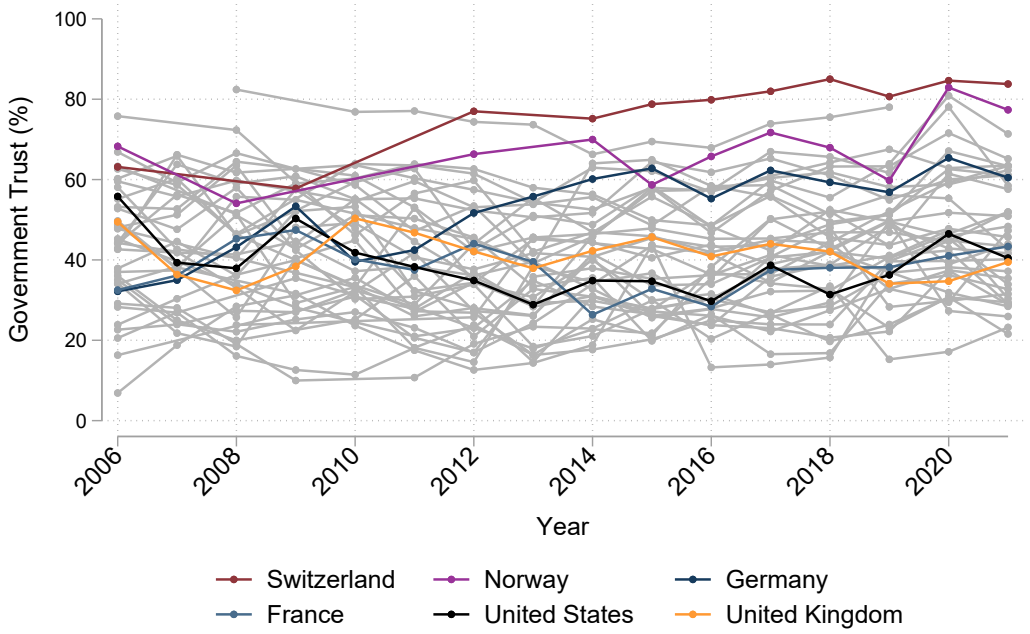
Source: gfs.bern.
Method: M1 (cutoff 1960/61), controls: age (pol. 3) by gender, usual participation, income.
95%-CI reported (heteroskedasticity-robust standard errors).
Self-reported political orientation on 0-10 scale: Left (0-3), Centre (4-6), Right (7-10)

Figure A17: Impact of the Personal Loss on Reform Rejection by Range Width (τ) and Income.



Source: gfs.bern.
Method: M1 (cutoff 1960/61), controls: age (pol. 3) by gender, usual participation, income.
95%-CI reported (heteroskedasticity-robust standard errors).
Self-reported income on 0-15 scale: Poor (1-4), Centre (5-9), Rich (10-15)

Figure A18: Government Trust by Country, 2006-2021.



Source: OECD Data. Selected: OECD Countries, Brazil, Russia, South Africa.

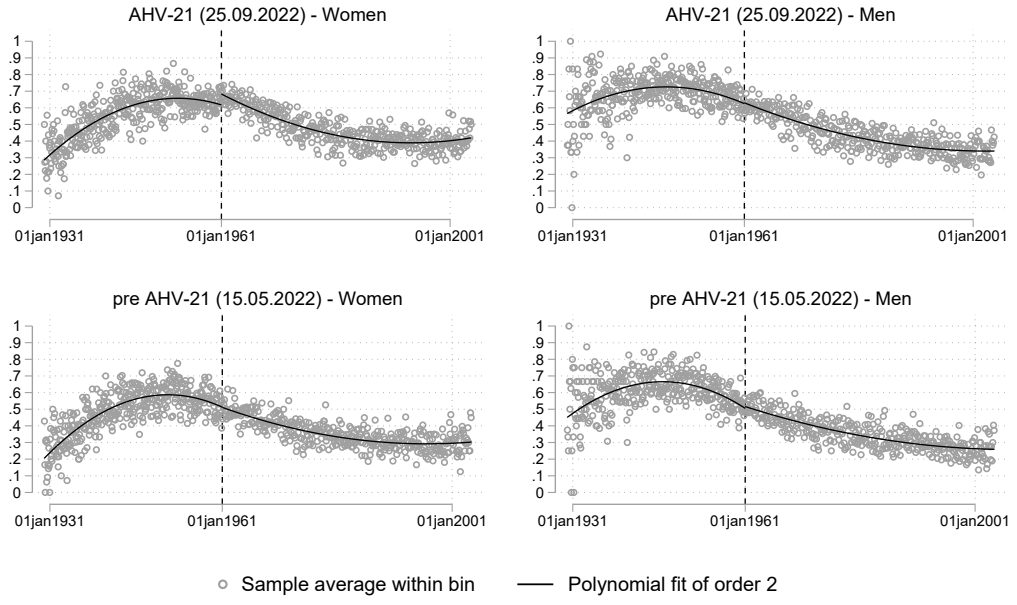
F Additional Tables

Table A1: Arguments.

Favorable	Contrary
AHV reform is urgently needed. Planned measures create financial security over the next ten years.	The increase in retirement age affects only women. However, their pensions are already one-third lower than those of men.
The two proposed items are a necessary compromise between higher revenues and lower expenditures.	The measures are just the beginning. Soon people will have to work until age 67.
Women are more educated than in the past, most are working and living longer than men. For this reason, the retirement age needs to be equalized.	Older people have difficulties in the labor market. They are often unemployed and financially dependent on the state. The measures exacerbate this problem.

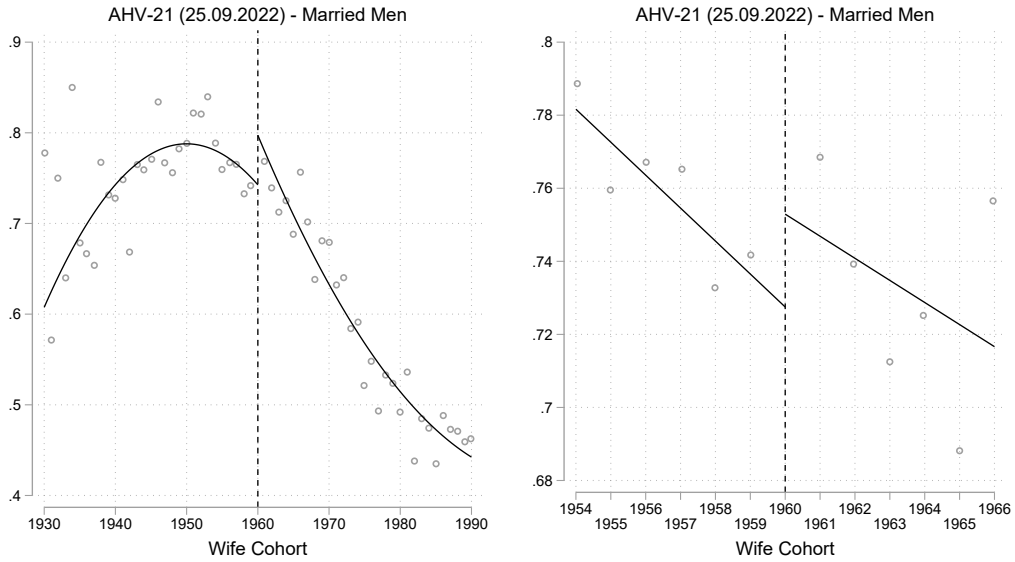
Source: Easyvote

Figure A19: Voter Participation by Cohort, Object and Gender (SG, daily).



Source: St. Gallen.
Method: Quadratic RDD, 1960/61 (M1), whole distribution from cohort-1930 on daily birth date.

Figure A20: Married Men's Voter Participation by Wife's Cohort.



Source: Geneva and St. Gallen.
Method, left: Quadratic Donut-hole RDD, 1959/61 (M2), cohorts 1930-1990.
Method, right: Linear Donut-hole RDD, 1959/61 (M2), $\tau = 6$.