The Timing of Retirement - New Evidence from Swiss Female Workers

Barbara Hanel
(University of Melbourne)

and

Regina T. Riphahn(*)
(University of Erlangen-Nuremberg)

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We investigate the responsiveness of individual retirement decisions to changes in financial incentives. A reform increased women's normal retirement age (NRA) in two steps from age 62 to age 63 first and then to age 64. At the same time retirement at the previous NRA became possible at a benefit discount. Since the reform affected specific birth cohorts we can identify causal effects. We find strong and robust behavioral effects of changes in financial retirement incentives. A permanent reduction of retirement benefits by 3.4 percent induces a decline in the age-specific annual retirement probability by over 50 percent. The response to changes in financial retirement benefits varies with educational background: those with low education respond most strongly to an increase in the price of leisure.

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Correspondence to:
Regina T. Riphahn (*)
Department of Economics
University of Erlangen-Nuremberg
Lange Gasse 20
90403 Nuremberg
Germany
Phone: +49-911-5302 826
Fax: +49-911-5302 178
Email: riphahn@wiso.uni-erlangen.de

Barbara Hanel
Melbourne Institute of Applied Economic and Social Research
Alan Gilbert Building
University of Melbourne
VIC 3010, Australia
Tel.: +61 - (0)3 - 9035 4565
Fax: +61 - (0)3 - 8344 2111
Email: bhanel@unimelb.edu.au

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1. Introduction

Since an understanding of the magnitude of workers' responsiveness to institutional reforms is crucial for policy design, it is important to provide reliable empirical estimates. A large literature attempts to quantify the effect of retirement incentives, and the problems involved in identifying their causal effects are widely discussed (see, e.g., Lumsdaine and Mitchell 1999, Coile and Gruber 2007, or Chan and Stevens 2004). Much of the literature identifies behavioral responses to financial incentives based on the cross-sectional comparison of individuals with different benefit claims and focuses on the appropriate representation of dynamic incentives (for cross-national comparative studies see, e.g., Gruber and Wise 2004 or Duval 2004). This approach mostly neglects the possibility of unobserved heterogeneity in tastes for retirement which might affect both incentives and responses. In their study of retirement expectations Chan and Stevens (2004) find that such heterogeneity strongly affects the estimates of responses to retirement incentives.

Some studies rely on natural experiments to obtain estimates the effect of financial incentives that are not biased by unobserved heterogeneity: Krueger and Pischke (1992) show that workers affected by reduced retirement benefits in the U.S. in 1977 did not respond as strongly as would have been expected based on prior findings. Mastrobuoni (2009) investigates whether the 1983 reform of the U.S. normal retirement age affected retirement behavior. He finds that every 2-months-increase in the NRA (normal retirement age) at actuarially fair benefit reductions for early retirement increases the mean age of benefit claiming by one month.

Similar to these studies we take advantage of a reform in the retirement system to identify the effect of financial incentives on retirement behavior. The 1991 reform of the Swiss mandatory retirement insurance introduced two separate institutional modifications. On the one hand the normal retirement age for females was raised in two steps from 62 to 64. On the other hand the possibility of early retirement was introduced at the expense of a benefit discount. As these measures reflect policy options available in about every social security system, it is both interesting and important to study their effects. Also, since the retirement reform is tied to the year of birth as a fixed individual characteristic, the experiment is not subject to endogenous sorting into treatment.

This study contributes to the literature in various ways: first, it identifies the labor supply response to retirement incentives by comparing the behavior of birth cohorts which differ only with respect to the financial incentives of a policy regime. In contrast to studies which rely on the cross-sectional identification of incentive effects, we can take advantage of an exogenous institutional reform. We know its precise timing and can therefore avoid measurement error. In addition, we avoid the problem that individuals may not be informed about their retirement incentives (Asch et al. 2005): the reform we look at here was subject to intense public debate due to a national public referendum (Bütler 2002).

Second, we evaluate the heterogeneity of the behavioral response to the policy reform, i.e. labor force exit, across various levels of individual human capital. Song and Manchester (2007) find that there are large differences in the response to changes in the Social Security earnings test along the income distribution. Similarly, Mastrobuoni (2009) finds stronger responses to retirement incentives among men with less formal education.

Third, we investigate whether the behavioral response to the institutional change happens instantaneously or whether the adjustment process takes time. If retirement age is strongly affected by social norms the response to policy reforms might be dampened and protracted. Social norms play a key role in the debate on excess retirement at age 62 and 65 in the U.S. (cf. Lumsdaine et al. 1996, Coile and Gruber 2000, Duflo and Saez 2003), where they are discussed as a potential explanation.

Finally, while many studies in this literature focus on males we take advantage of a retirement reform specifically for female workers. For historical reasons, benefit eligibility rules for retiring females are often more lenient than those for retiring males. Therefore, adjustments specifically for the female labor force are a relevant policy issue in many countries. In addition, both health and financial restrictions may cause different responses to given policy changes for men and women.

We find clear behavioral adjustments in response to changes in retirement incentives. Labor supply elasticities differ across population groups with heterogeneous educational backgrounds. The estimation results are robust to controls for endogenous panel attrition. The evidence suggests that the adjustment of retirement behavior to changed institutional circumstances intensifies over time.

2. Institutional Background and Hypotheses

The Swiss retirement system consists of a public social security pillar (AHV), financed mainly by payroll taxes on a pay-as-you-go basis, and of heterogeneous, typically employer-based fully funded private pension systems as a second pillar (for a detailed description see, e.g. Bütler 2002 or Dorn and Sousa-Poza 2003, for a cross-national comparison see Krieger and Traub 2011). Both, the first and the second pillar are obligatory. Before the reform, the public AHV pillar set a normal retirement age as an eligibility criterion for benefit receipt, but not a mandatory retirement age. For men the normal retirement age has always been 65, while women used to be able to retire at age 62. In 1991, a reform law (the "10th revision") was enacted, which was confirmed by a referendum in 1995. This reform prompted two types of changes that we summarize in **Table 1** and use as a natural experiment: first, the normal retirement age for women was ratcheted up in two steps from 62 to 63 years in 2001, and to 64 years in 2005. Second, it became possible to choose early retirement. The option to draw retirement benefits one year (and later two years) prior to the normal retirement age was connected with a permanent benefit discount of 3.4 percent for females if they retired one year early and of 6.8 percent for retiring two years prior to the normal retirement age. **Table 1** reports the timing of the reform steps as well as the benefit reductions tied to early retirement.[1]

Based on these reform steps we expect behavioral adjustments in the timing of retirement. In the framework of a standard intertemporal consumption model (compare e.g. Fields and Mitchell 1984) individuals' maximization problem at time t is given by:

$$\max_{c_s,R} \int_{s=t}^{R} e^{-\delta(s-t)} u(c_s, l_w)\pi_t(s)ds + \int_{s=R}^{T} e^{-\delta(s-t)} u(c_s, l_r)\pi_t(s)ds$$

$$\text{s.t. } \int_{s=t}^{T} e^{-r(s-t)} c_s\, \pi_t(s)ds = \int_{s=t}^{R} e^{-r(s-t)} y_s\, \pi_t(s)ds + \int_{s=R}^{T} e^{-r(s-t)} b_s(R)_s\, \pi_t(s)ds + A_t,$$

where R is the date of retirement. Utility u depends on the level of consumption, c_s, and the amount of leisure if the individual is working (l_w) or not (l_r). The survival probability until period s is denoted as $\pi(s)$, δ is the individual discount factor, and r is the interest rate. A_t is the net present value of assets held in period t. The labor market income received prior to retirement is denoted as y_s and $b_s(R)$ indicates the retirement benefits received from the date of retirement until death in period T. The stream of benefits depends on the date of retirement R. If benefits are a differentiable function with respect to R, the first order condition yields:

$$e^{-\delta(R-t)}\left[u(c_R, l_w) - u(c_R, l_r)\right]\pi(R) = \lambda e^{-r(R-t)}\left[(y_R - b_R(R))\pi(R) + \int_R^T e^{-r(s-R)} \frac{\partial b_R(R)}{\partial R}\pi(s)ds\right]$$

The integral on the right hand side indicates the effect of retirement date R on pension accrual.[2] The Swiss reform changed the individual budget constraint: starting 2001, $b_s(62 \leq R < 63)$ declined by 3.4 percent, while $b_s(R \geq 63)$ remained unchanged. These changes in the

[1] Since 1991, individuals can delay benefit receipt for up to five years after the normal retirement age. This possibility was not affected by the reform discussed here. Postponing retirement increases benefits by about 5 percent per year for the first 5 years (BSV 2006). Both, before and after the reform, less than one percent of female retirees use the delayed benefit option (Dorn and Sousa-Poza 2003, BSV 2000, BSV 2006). A possible explanation for this limited utilization is that mandatory contributions continue to be collected (currently, 5.15 percent of gross earnings) during the period of benefit delay; however, these contributions usually generate no or very low additional benefit entitlements. The additional contributions paid during the period of delay thus offset the nominal increase in benefits due to the actuarial adjustment at least partly.

[2] The model implies that retirement behavior is driven by pension wealth as well as by pension accruals. We cannot distinguish between the two effects because the Swiss reform changed both at the same time. However, previous literature finds pension accruals to be the central determinant of retirement behavior (e.g. Samwick 1998, Gruber and Wise 2004).

budget constraint imply changes in the optimal labor supply due to income and substitution effects: first, the downward shift in the budget constraint at age 62 should decrease the demand for leisure if leisure is a normal good (income effect). In addition and at the same time, the benefit adjustment upon retirement at age 62 after the reform implies that leisure becomes more expensive. Thus, also a substitution effect should thus decrease the demand for leisure and we expect an increase in labor supply. This is illustrated by the first order condition: assume that before the reform an individual's utility function, survival probabilities and discount rate result in an optimal retirement age R=62. After the reform of 2001, both, $\left(y_R - b_R\left(R = 62\right)\right)$ and $\frac{\partial b_R\left(R = 62\right)}{\partial R}$ on the right hand side of the first order condition, i.e. the revenue from an additional year of working, increase. Consequently, after this reform step it is more likely that the disutility connected to working at age 62, $\left[u\left(c_{62}, l_w\right) - u\left(c_{62}, l_r\right)\right]$ is offset by financial incentives. We thus expect that the employment probability of women aged 62 increases and consequently the probability of labor force exit decreases. Likewise, the second reform step in 2005 decreased the level of benefits available upon retirement age R=63 and R=62. This results again in a lower but steeper budget constraint at these ages. Again, we expect the income and substitution effect to decrease the probability of labor force exit prior to age 64.

Additionally, we hypothesize that a given change in pension accrual should call up different responses depending on individual wealth. The multiplier λ at the right hand side of the first order condition can be interpreted as the shadow value of wealth and links losses in wealth to utility losses. The marginal utility of additional consumption might differ across individuals depending on their utility function and depending on their wealth level. In particular, we expect larger changes in marginal utility for those with little wealth such that the effect of the new incentives to delay retirement should be highest for those who most depend on public pensions and who have little alternative income in old age.

We use individual human capital as a proxy for wealth and expect that individuals with little human capital and education are unlikely to take advantage of the early retirement options which come at the price of retirement income.[3] We test whether the response to financial incentives differs across the wealth distribution.

Finally, in addition to testing whether behaviors respond to changed incentives we investigate the time pattern of the responses, i.e. whether (a) behavioral adjustments take time to intensify after the reform, (b) retirement behavior adjusts immediately at a single point in time without a time trend, or (c) response behavior disappears over time. All three patterns are possible. Mastrobuoni (2009) discusses an intertemporal retirement model where forward-looking individuals smooth their lifetime consumption when they are given a long notice period regarding upcoming institutional changes. In this model, the labor supply response to the reform should be higher for individuals who were surprised by the reform and it should decline for later cohorts who can increasingly take advantage of long term behavioral adjustments.In contrast, option value models of retirement do not model savings explicitly and behavioral adjustments occur only with respect to labor supply, both in the short-run as well as in the long-run. Consequently, labor supply responses to a reform do not change between early and late cohorts and occur without a time-lag or trend. Finally, changes in labor market behavior could intensify over time. Possible explanations for such a pattern include social norms affecting the retirement decision (e.g. Lumsdaine et al. 1996), or that individuals do not recalculate benefits continuously. Such mechanisms could generate a delay in behavioral adjustments.

3. Data and Empirical Approach

[3] In a descriptive analysis of the correlates of early retirement based on the 2002 cross-section of our data, Dorn and Sousa-Poza (2005) find a significantly negative correlation of education with the propensity to retire early. Similarly, immigrants and those in low income professions, with unemployment experience and low incomes are least likely to leave the labor force early. Mastrobuoni (2009) shows heterogeneous responses to changes in retirement incentives across education groups for the United States, as well.

Our data are taken from the Swiss Labor Force Survey (SLFS, *Schweizerische Arbeitskräfteerhebung BFS*, 1991-2006). The SLFS is a rotating panel with up to five interviews per person covering a representative sample of the Swiss population. A disadvantage of the data is that information on spouse characteristics, occupational pensions, and work history is unavailable. However, given that the policy change considered here is orthogonal to all individual characteristics except for the birth year, this should not affect our results. In our analysis sample we follow those at risk of retirement, i.e. all females aged 60 through 65 who were members of the labor force when they were first interviewed. This provides us with 3,213 person-year observations for 1,773 different female labor force participants, for whom at least one transition can be observed.[4] We thus follow the literature (e.g. Coile and Gruber 2007, Chan and Stevens 2004, or Song and Manchester 2007) and consider transitions to retirement conditional on labor force participation at the first interview. Therefore the causal effects measured in our approach can be considered as treatment effects on the treated rather than average treatment effects.

We follow the literature and refer to those who exit the labor force as retirement entrants. This neglects the possibility to exit the labor force without benefit receipt and to receive benefits while working. Our dichotomous dependent variable describes whether a member of the labor force in year t indicates to have left the labor force in year t+1, i.e. the hazard of retirement. In the weighted data we observe a transition to retirement among 31.1 percent of our observations. We consider retirement to be an absorbing state and censor observations thereafter. **Figure 1** depicts age-specific labor force exit probabilities over time that match expectations: in 2001, when the NRA shifted to age 63 and retirement at age 62 started to generate a 3.4 percent benefit cut, women's propensity to retire at age 62 dropped. It immediately dropped from about 50 percent in 2000 to 40 percent in 2001. This short-term behavioral change appears to intensify in subsequent years. In 2002 the probability of a

[4] Our results are robust to adding younger workers (e.g., age 55-60) to the sample.

transition to retirement further declines to about 20 percent in subsequent years, where it remains unchanged for the following years. This 30 percentage point decline constitutes a substantial response. Surprisingly, the retirement probability at age 62 did not continue to decline after 2005, when benefit discounts doubled. In 2001, the probability of retirement at age 63 increased from about 20 to 40 percent. While the change is substantialoverlapping confidence intervals suggest that it is not statistically significant. Age 63 had become the normal retirement age and female workers now retired later. After 2005, the retirement propensity at age 63 dropped clearly from more than 50 to about 37 percent, i.e. by about 15 percentage points: the normal retirement age had increased to age 64 and benefits were cut when retiring at age 63. Due to a small number of observations our depiction of the development of the age 64 retirement probability is difficult to interpret. In 2001 we see no significant change in the age 64 retirement propensity. Since the 2005 reform rendered 64 the NRA we expect an increase in the retirement propensity at this age. Overall, **Figure 1** yields no clear patterns for the retirement propensity at age 64. The spike in 2002 appears to be spurious and related to an extremely small number of observations (across all birth cohorts only 30 women retired at age 64).[5]

In order to identify the shift in age-specific retirement propensities following the reform steps, we apply a difference-in-differences-type approach: we control for age (A), calendar year (Y), relevant interaction terms (I = A*Y), as well as a vector of control variables (X). If β represents a vector of parameters we can write:

$$\Pr\,(\text{retirement}) = F\,(\beta_0 + \beta_1\, A + \beta_2\, Y + \beta_3\, I + \beta_4\, X).$$

Our interaction terms (I) indicate the groups whose behavior should be affected by the modified retirement incentives: the propensity to retire should decline for 62 years old females after 2000 and again after 2004, similarly for 63 years old women after 2004. The

5 Unfortunately, the SLFS data do not inform on retirees' income sources, a problem frequently encountered in retirement analyses (e.g. Asch et al. 2005, Blundell et al. 2002). Therefore some of the individuals who exit the labor force may not be receiving benefits from the first pillar of the retirement insurance.

reform effects are identified both by a comparison of given age groups over time as well as by year effects across different age groups. Besides age, calendar year, and three interaction terms we consider education, marital status, industry, and regional indicators in the covariate vector X. Macro-economic developments over time, such as possible shifts in unemployment rates are accounted for by the flexibly specified set of calendar year indicators (Y). Descriptive statistics of the explanatory variables are provided in Appendix **Table A.1**.

The difference-in-differences approach estimates the causal effect of the institutional change if no contemporaneous shock other than the reform affects retirement behavior of the treatment group relative to the control group. Thus, in the absence of a reform any change in retirement behavior should be identical for treatment and control group. We assume that this condition holds. As a first approach to corroborate this assumption we compare the characteristics of treatment and control groups in **Table 2**. Given the considered reform we obtain three treatment and control group pairs: (i) women age 62 before vs. after (2001-2004) the reform of 2001, (ii) women age 62 before (2001-2004) vs. after the reform of 2004, (iii) women age 63 before vs. after the reform of 2004. Generally, the characteristics do not differ substantially for treatment and control groups. We address the difference in educational attainment among the 62 years olds in a robustness test.

Our empirical approach proceeds in three steps. First, we apply a dichotomous logit estimator to estimate the parameter vector β and to determine the impact of the retirement reform on retirement behavior.[6] Unobserved heterogeneity, even if uncorrelated with the covariates, may bias the parameter estimates in the duration model if it is not accounted for appropriately. We apply random effects models and compare estimators using normally distributed errors and with those using a non-parametric discrete-factor error term distribution (see Heckman and Singer (1984) and for an implementation Rabe-Hesketh et al. (2004)).

[6] We obtain very similar results when a least squares estimator is applied. Since we intend to study predicted probabilities we prefer to consider a logit estimator.

Next, we gauge the robustness of our results and compare three alternative specifications: specification 1 controls for age, calendar year, and the interaction effects discussed above. Specification 2 adds controls for educational attainment and marital status, and specification 3 considers industry of last employment and region of residence, as well.

An important characteristic of our data is that the SLFS suffers from panel attrition. In step two of our analysis we investigate whether non-random panel attrition affects our results: if the unobserved determinants of panel attrition also affect transitions to retirement or to continued employment, this neglected heterogeneity will generate inconsistent estimates. To test the hypothesis that no such heterogeneity exists, we replace the binomial dependent variable with a multinomial outcome measure, considering panel exit as a competing risk. With the new dependent variable we can use a large sample of 2,429 observations of which as before 958 are observed to transit to retirement. 26.4 percent of the observations are censored due to attrition. The share of transitions to retirement now amounts to 21.9 percent, somewhat below those presented in **Table A.1**. We reestimate our models within the framework of a multinomial logit estimator. To relax the restriction of the independence of irrelevant alternatives (IIA) assumption, we allow for error term correlation in the form of random effects specifications and evaluate the reform effects in this framework. In addition, we apply a Hausman test to determine whether panel attrition is an independent outcome. If its unobserved determinants are not correlated with those of transitions to retirement, we can rely on the binomial logit estimator. - In step three of the analysis we test whether the treatment effect of the retirement reform is heterogeneous over time and across education groups.

4. Results

4.1 Baseline Results

We present our estimation results in **Table 3** and in **Table 4** the retirement probabilities which are predicted for the entire sample with and without treatment based on the estimation results in **Table 3**. The coefficients of the incentive effects in the first rows of **Table 3** are highly statistically significant and confirm the expected decline in the probability of retirement at ages 62 and 63 when benefit cuts were introduced. The effect is quantified in **Table 4**: we predicted each individual retirement probability and integrated it out over the estimated distribution of the random effects. Then we calculated the average retirement probability across all individual predictions. Based on specification 1 the predicted retirement propensity at age 62 differs significantly before and after the reform. The annual retirement probabilities change substantially by about 25 percentage points or 53 percent: it amounted to 46.4 percent before the reform and dropped to 21.9 percent after the benefit reduction of 3.4 percent was mandated in 2001 as reform step 1. The retirement probability drops slightly further to 21.0 percent after the benefit reduction of 6.8 percent was introduced in 2005 as reform step 2. At age 63, the responsiveness of Swiss women is smaller. Here, the drop in retirement probabilities amounts to about ten percentage points or 24 percent (from 39.5 to 29.9 percent) following the introduction of the 3.4 percent benefit discount in 2005 as reform step 3. The results with additional control variables are presented in subsequent columns and do not differ substantially: the coefficients of the incentive indicators remain statistically significant and the predicted changes in retirement probabilities are of similar magnitude. Overall these predicted effects match the developments indicated by **Figure 1**. We bootstrapped the standard errors of the difference in predicted retirement probabilities before and after the reform steps. The decline in retirement probabilities is highly significant for women aged 62 and significant at least at the 10 percent level for the 63 years olds. Thus the reform had significant effects on behavior and the older female labor force responded strongly to shifts in retirement incentives.[7]

[7] The estimation approach in **Table 3** was chosen after comparing alternative estimators. **Table A.2**

At first glance, these results are somewhat smaller than those found by Mastrobuoni (2009): when we calculate the expected retirement age before and after step 1 of the reform we find that the delay in NRA by one year (at a benefit discount of 3.4 percent) generates a delay in expected retirement entry by 2.3 months; after the reform steps in 2005 the total effect of shifting the NRA by two years (at a benefit discount of 6.8 percent) amounts to a delay of retirement entry by 7.7 months.[8] Mastrobuoni (2009) obtains a drop in average retirement age by about one month for every two months delay in the U.S. normal retirement age (NRA).[9] Börsch-Supan et al. (2004), using a structural model, simulate a reduction in the retirement propensity of German women at age 60 by between 50 and 70 percent when NRA is raised from 60 to 65 at a benefit discount of 6 percent per year of early retirement. Hanel (2010) models the effect of a similar reform in the German institutional framework: shifting the NRA by 5 years from 60 to 65 generates a reduction in the propensity to retire at age 60 (at a 3.6 percent benefit discount per year) by 90 percent. This translates to a 10 months delay in expected retirement entry when NRA is increased by 5 years. This effect is comparable in magnitude to the one we find for Swiss retirees, who delay retirement by 2.3 months after a shift in NRA by 1 year and at an annual benefit discount of 3.4 percent. Thus, the responsiveness of Swiss females to financial incentives is within the range of estimates obtained e.g. by Mastrobuoni (2009) and by Coile and Gruber (2000) for the U.S., and it is comparable in magnitude to results from a neighboring country.

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presents estimation statistics (log likelihood value, number of parameters, and AIC statistic) of three logit estimators for different model specifications. We find that the addition of random effects improves the log likelihood values significantly. Based on the AIC criterion the discrete random effect distribution provides a better fit than normally distributed random effects. The estimation results are not sensitive to the choice of the estimator. All estimations with discretely distributed random effects use a specification with two mass points. The hypothesis that a third mass point improves the model fit was rejected in all cases.

8 To calculate expected retirement age we considered the behavior of those aged 62 and 63 before and after the different reform steps, assuming that younger and older females remained unaffected by the reform.

9 In contrast, Coile and Gruber (2000) obtain very small effects of at most an 11 percent reduction in the retirement propensity at age 65 when NRA is simulated to increase from 65 to 67. They find a similar effect when the delayed retirement credit is raised from about 5 to 8 percent. Also, Samwick (1998) predicts the effect of changes in NRA and jointly considers several reforms. He concludes that a shift of the NRA from 65 to 67 reduces the probability of retirement between age 50 and 70 by one percentage point, which is difficult to interpret.

To test the plausibility of our results and interpretations we perform a "placebo analysis", in the spirit of Angrist and Krueger (1999, section 2.4). We test (a) whether the probability of female retirement at age 62 also changed significantly in non-reform periods ("wrong year"). If the probability of retirement at age 62 declined already prior to the reform we cannot be certain that our estimates indeed identify the reform effect. Additionally, we test (b) whether the probability of retirement in the period 2001-2004 changed significantly for other, i.e. non-affected age groups ("wrong age"). If non-affected age groups modified their behavior, we may have identified period-specific effects rather than causal reform effects.

The estimation results are presented in **Table 5**. Specification 4 (see panel a of **Table 5**) adds "wrong year" effects to specification 3 of **Table 3**. Not surprisingly, even with these detailed controls the estimates of the "correct" incentive effects remain large and statistically significant. The estimated coefficients suggest that throughout the 1990s and prior to the reform the probability of retirement at age 62 increased (see also the probability of labor force exit for females aged 62 over time in **Figure 1**).[10] This positive and statistically significant trend was reversed by the reform: we tested whether a decline in retirement probabilities for 62 years old females occurred in non-reform periods. Column four of **Table 5(a)** provides the p-values of one-sided tests of the hypothesis that the retirement probability in year t is below that of year t-1. The first row compares the joint retirement probability for the years 2001-2004 to that of 2000, row two compares the probability of 2005 to that of 2001-2004 and the rows below the "wrong incentives" title compare annual retirement propensities. The only significant decline in retirement probabilities occurred with step 1 of the reform, which supports our previous conclusions: compared to the preceding years the

[10] There were no changes to the eligibility rules in the first and second pillar of the retirement system at that time. Since the specification controls for calendar year fixed effects, general labor market trends cannot explain the observed patterns. Bütler et al. (2005) present evidence for a secular shift to earlier retirement ages for men and women over the 1990s.

probability of retirement at age 62 started to decline significantly only after 2000, exactly when the benefit cuts were enacted.

In specification 5 (see panel b of **Table 5**) we test whether the retirement probability in 2001-2004, i.e. after the first shift in retirement incentives for 62 years old women, changed significantly for other age groups as well. The results show that only the retirement probability for the 63 years old women increased and there were no significant changes in the behavior of other age groups. The significant increase for the 63 years old women is the immediate consequence of the reform which caused 62 years old women to postpone retirement by one year. Thus, the placebo analysis corroborates our evidence in favor of causal reform effects.

The comparison of control and treatment group characteristics in **Table 2** yielded substantial improvements in educational attainment over time. In order to test whether such shifts in characteristics affect our results we repeated our analyses as presented in **Tables 3** and **4** after adding time trend interactions of the education variables to the model. The coefficient estimate for the time trend interaction of secondary education indeed is statistically significant. However, the predicted transition probabilities are robust and unaffected by the change in specification (results available upon request).

4.2 Effects of Panel Attrition

As discussed above, our data raise the concern of endogenous panel attrition. Only about 30 percent of all interviewees reach the fifth interview in our rotating panel survey, all others leave the survey before. If the propensity to leave the survey is correlated with the individual response to the retirement reform our estimators generate biased coefficients and predictions. To test the robustness of our outcomes to this concern we reformulate our dependent variable and reestimate the determinants of the transition to retirement while at the same time controlling for possible endogenous panel attrition. We apply a multinomial logit model with and without random effects. In a first step we evaluate - as before - the fit of alternative estimators to the data (see Appendix **Table A.3**). The results are very similar to those presented in **Table A.2**: both, the random effects estimators with normally and discretely distributed random effects significantly improve on the cross-sectional approach. Since - based on the AIC criterion - the discrete random effects specification provides the best fit, we use this estimator for our robustness test. We do not present the results of the multinomial logit estimations to save space (the results are available upon request). The estimated coefficients of the reform step indicators for the probability of retirement relative to the probability of employment are highly significant and negative.

Table 6 summarizes the predicted retirement probabilities obtained based on the binomial and multinomial logit estimations using three model specifications. We present the predicted probability of retirement relative to the joint probability of either retiring or staying in the labor force. The binomial results are identical to those presented in **Table 5** above. The impact of the retirement reform is quantified by a comparison of retirement probabilities predicted for the situation with and without the reform. The direction of the predicted effect agrees for the two considered estimators and its magnitude is generally quite similar: women aged 62 reduced their retirement probability by about 50 percent and those at age 63 by about 25 percent. The similarity of the results across estimators informally supports the hypothesis that panel attrition does not bias the binomial logit estimator.

A Hausman test of the independence of irrelevant alternatives property of the multinomial logit estimator provides a more formal test of the hypothesis that panel attrition is independent of the other considered outcomes. The test compares the coefficient vector of one outcome alternative (e.g. transition to retirement) relative to a given baseline outcome (e.g. transition to continued labor force participation) using both, the logit and the multinomial logit estimators (Hausman and McFadden 1984). We performed the test for all three model specifications.[11] The results (see **Table A.4**) indicate that the hypothesis of identical coefficient vectors for the two estimators cannot be rejected. Therefore attrition is an independent event and we can rely on the binomial logit estimator.

4.3 Heterogeneity of Treatment Effects

In the third step of our empirical analysis we investigate the heterogeneity of the reform's treatment effects. We are interested in changes over time, and in differences between groups with different levels of human capital. **Table 7** presents in panel (a) the estimation results of specifications which test for significant differences in the reform effect over time based on model specification (3), as presented in **Table 3** above. Given that our data describe only one year after the introduction of the second reform step, the effect of a time trend can only be studied for the first reform step. The coefficient of the interaction term (-0.401) is negative and statistically significant at the five percent level. This suggests that female retirement probabilities continued to decline in the years after the first reform. The magnitude of the effect is predicted in panel (b) of **Table 7**. While the retirement probability of women aged 62 drops immediately and significantly by 13.7 percentage points from 45.7 to 32.0 percent in the year of the reform, retirement probabilities continue to decline further in subsequent years. The additional annual drops are of substantial magnitude and statistically significant. Overall, there is evidence for a protracted effect of the reform. While we cannot separately identify the effect of a social norm and of birth cohort effects, the evidence matches the pattern that we would expect if a social norm inhibited immediate behavioral adjustments to changed incentives.[12]

[11] While the comparison presented in **Table 7** requires that we relax the IIA assumption – otherwise predicted probability ratios are necessarily identical – the Hausman test requires that we do not relax IIA. Considering random effects and thus allowing for correlated error terms across alternatives would eliminate the IIA property of the estimator. Therefore the random effects specification was not considered in the framework of the Hausman test.

[12] The information on changes in retirement incentives was available since 1991 and had been broadly publicized through a public referendum on the issue in 1995 (Bütler 2002).

We evaluate the heterogeneity of the treatment effects across education groups, to study whether differences in social security wealth might affect the response to the new retirement incentives. Our expectation was to find more elastic labor supply responses among those with low human capital and thus likely low wages and low social security wealth. We add education group interaction terms to our model in specification (3). While the additionally estimated coefficients of the interaction terms are not statistically significant the resulting patterns are interesting. **Figure 2** depicts the predicted age-specific retirement probabilities, by education group and at different points in time: the initial pre-reform retirement probability at age 62 (or 63) is highest among the least educated women. For all education groups we observe a clear drop in the probability that females retire at age 62 after the reform. The drops after 2001 and 2005 are significant at the 1 percent level for the two lower educational groups (standard errors were obtained by bootstrap; estimation and simulation results not presented to save space). The absolute size of the decline is largest for females with low levels of education (28.7 and 24.0 percentage points in 2001 for those with lower and upper secondary education). The response of the tertiary education group is much smaller. For them retirement probabilities declined by 11.5 percentage points in 2001 and fell only slightly more in 2005. The new retirement incentives at age 63 again yield larger drops in retirement probabilities among those with less human capital. Thus, women with lower human capital and possibly lower wages, earnings, and wealth respond most strongly to the increased price of leisure at old age.

5. Conclusions

This study identifies the effect of financial incentives on retirement behavior taking advantage of the natural experiment of an exogenous institutional reform in Switzerland. This source of identification helps to avoid the substantial biases of up to 50 percent that e.g. Chan and Stevens (2004) found when they added controls for individual unobserved heterogeneity:

the marginal effect of pension incentives on subjective retirement expectations robustly dropped by at least half after the authors added individual fixed effects to their model.

The reform of the Swiss retirement insurance increased the normal retirement age for females born after 1940 in two steps from 62 to 64 years. After the reform, female retirees at age 62 incurred a benefit reduction of initially 3.4 and later 6.8 percent. The modification of the normal retirement age is a potent policy instrument as it affects both, the length of the contribution period as well as the duration of benefit payments.

We apply a difference-in-differences type procedure and confirm the robustness of our results with respect to alternative model specifications and estimators. We observe a strong response to the shift of the normal retirement age in connection with benefit reductions: a reduction in benefits by 3.4 percent caused a decline in the retirement probability at age 62 from 46 to 22 percent, i.e. half of those who would have left the labor force at age 62 prior to the introduction of benefit discounts now remain in the labor force. The probability of retirement at age 63 drops from 40 to 30 percent, i.e. by 25 percent, after benefit discounts of 3.4 percent were implemented. Behavioral adjustments intensify significantly over time. This finding is in line with social norms affecting behavioral choices of Swiss workers, or with a time-lag in recalculating expected benefits.. Females with low education show the strongest response to changes in retirement incentives and appear to be most reluctant to incur a decline in benefit payments.

Overall, retirement behavior responds strongly to changes in incentives. However, the effect for Switzerland may indicate a lower bound on the effect that is possible in other countries, because the Swiss reform affects only the first pillar of the retirement insurance system leaving the other pillars unchanged. We expect stronger effects if a reform comprehensively addresses all funding sources for retirement. On the other hand our analysis is limited to the extensive margin of labor force participation. De Grip et al. (2011) show that reducing benefits and delaying retirement may adversely affect mental health of older workers. Such effects, which are beyond our analysis, may limit the effectiveness of retirement benefit reforms. Our findings confirm prior studies (e.g. Asch et al. 2005) and suggest that financial retirement incentives can substantially affect the retirement plans of the generations to come. This may contribute to solve the funding problems of retirement insurance funds.

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Table 1 Normal Retirement Age and Early Retirement Options after the 1991 Reform

(a) Timing of Changes

Retirement Regime as of	Normal Retirement Age	Early Retirement Age (% Benefit Reduction)	
before 2000	62	-	-
2001 - 2004	63	62 (3.4 %)	-
starting 2005	64	63 (3.4 %)	62 (6.8 %)

(b) Summary of Reform Steps

Reform Steps	
1	starting 2001 retirement for 62 years olds at 3.4 % benefit discount
2	starting 2005 retirement for 62 years olds at 6.8 % benefit discount
3	starting 2005 retirement for 63 years olds at 3.4 % benefit discount

Table 2 Comparison of Control and Treatment Group Characteristics

2.1 Women, Age 62

	(1)	(2)	(3)	(1)-(2)			(2)-(3)		
	<=2000	2001-2004	>=2005	Difference	Std. Err. of		Difference	Std. Err. of	
	Mean			of Means	Difference		of Means	Difference	
Marital status:									
married	0.538	0.555	0.524	-0.017	0.052		-0.031	0.055	
single	0.131	0.086	0.106	0.045	0.029		0.020	0.032	
widowed/ divorced	0.331	0.359	0.370	-0.028	0.049		0.012	0.052	
Education									
higher education	0.066	0.094	0.158	-0.028	0.026		0.064	0.036	°
secondary education	0.471	0.589	0.528	-0.118	0.053	*	-0.060	0.055	
lower education	0.463	0.317	0.313	0.146	0.052	**	-0.004	0.053	
Industry:									
agriculture and mining	0.088	0.033	0.019	0.055	0.028		-0.014	0.016	
utilities and construction	0.177	0.135	0.104	0.043	0.039		-0.031	0.037	
trade, transport, communic.	0.218	0.256	0.190	-0.038	0.046		-0.066	0.047	
hotel and catering trades	0.072	0.061	0.085	0.011	0.024		0.024	0.028	
credit, insurance, real estate	0.104	0.108	0.141	-0.003	0.032		0.033	0.035	
public administration	0.031	0.068	0.063	-0.037	0.024		-0.005	0.028	
education and health sector	0.216	0.241	0.306	-0.025	0.043		0.065	0.048	
other	0.094	0.099	0.093	-0.004	0.030		-0.006	0.032	
Region:									
Lake Geneva Region	0.185	0.134	0.158	0.051	0.038		0.024	0.038	
Swiss Mittelland	0.217	0.266	0.265	-0.049	0.047		-0.001	0.050	
North-Western Switzerland	0.137	0.139	0.103	-0.002	0.034		-0.036	0.034	
Zurich	0.179	0.166	0.205	0.013	0.040		0.038	0.043	
Eastern Switzerland	0.181	0.136	0.148	0.045	0.039		0.012	0.040	
Central Switzerland	0.071	0.107	0.088	-0.035	0.029		-0.018	0.030	
Ticino	0.030	0.052	0.032	-0.022	0.022		-0.020	0.022	

2.2 Women Age 63

	(1)	(2)	(1)-(2)	
	<=2004	>=2005	Difference	Std. Err. of
	Mean		of Means	Difference
Marital status:				
married	0.530	0.527	0.002	0.049
single	0.110	0.065	0.045	0.028
widowed/ divorced	0.360	0.408	-0.047	0.046
Education				
higher education	0.104	0.121	-0.017	0.032
secondary education	0.541	0.521	0.020	0.049
lower education	0.355	0.358	-0.003	0.047
Industry:				
agriculture and mining	0.060	0.047	0.013	0.025
utilities and construction	0.105	0.146	-0.041	0.032
trade, transport, communic.	0.253	0.191	0.062	0.039
hotel and catering trades	0.085	0.066	0.019	0.025
credit, insurance, real estate	0.109	0.123	-0.014	0.031
public administration	0.050	0.031	0.019	0.017
education and health sector	0.233	0.288	-0.054	0.044
other	0.104	0.107	-0.003	0.031
Region:				
Lake Geneva Region	0.161	0.165	-0.004	0.034
Swiss Mittelland	0.243	0.207	0.036	0.042
North-Western Switzerland	0.127	0.117	0.010	0.029
Zurich	0.169	0.220	-0.050	0.039
Eastern Switzerland	0.152	0.163	-0.011	0.039
Central Switzerland	0.100	0.086	0.014	0.027
Ticino	0.048	0.042	0.006	0.017

Note: **, *, and ° indicate statistically significant differences between treatment and control group at the 1, 5, and 10 percent level. We observe 258, 282, and 202 women in the first three columns of **Table 2.1** and 434 and 216 women in the first two columns of **Table 2.2**.
Source: Own calculations using weighted data from the Swiss Labor Force Survey (1991-2006).

Table 3 Estimation Results – Random Effects Logit of Labor Force Exit (Discrete Distribution)

	Specif. 1	Specif. 2	Specif. 3
	Coeff.	Coeff.	Coeff.
	Std. Err.	*Std. Err.*	*Std. Err.*
Reform Step 1: Age 62 x 2001 and later	-1.546 **	-1.548 **	-1.540 **
	0.274	*0.270*	*0.273*
Reform Step 2: Age 62 x 2005 and later	-1.678 **	-1.674 **	-1.691 **
	0.318	*0.312*	*0.315*
Reform Step 3: Age 63 x 2005 and later	-0.531 *	-0.581 *	-0.587 *
	0.241	*0.240*	*0.242*
Age (9)	yes **	yes **	yes **
Year (15)	yes	yes *	yes °
Education (3)	--	yes **	yes **
Marital status (3)	--	yes	yes
Industry (8)	--	--	yes
Region (7)	--	--	yes
Log Likelihood	-1871.76	-1853.32	-1844.44
# parameters estimated	28	32	45

Note: **, *, and ° indicate statistical significance at the 1, 5, and 10 percent level. The numbers in parentheses indicate the number of categories including the reference.

Table 4 Predicted Probability of Retirement – Random Effects Logit of Labor Force Exit (Discrete Distribution)

	Specif. 1	Specif. 2	Specif. 3
Age 62, before step 1	0.464	0.465	0.456
Age 62, after step 1	0.219	0.216	0.217
Difference	-0.245 **	-0.249 **	-0.239 **
Standard Error of Difference	*0.041*	*0.067*	*0.065*
Age 62, before step 1	0.464	0.465	0.456
Age 62, after step 2	0.210	0.207	0.206
Difference	-0.254 **	-0.258 **	-0.250 **
Standard Error of Difference	*0.046*	*0.066*	*0.070*
Age 63, before step 3	0.395	0.400	0.401
Age 63, after step 3	0.299	0.295	0.296
Difference	-0.096	-0.105 °	-0.105 °
Standard Error of Difference	*0.069*	*0.057*	*0.060*

Note: **, *, and ° indicate statistical significance at the 1, 5, and 10 percent of the difference between the predicted probabilities of labor force exit under old and new regulations. Standard errors are bootstrapped with 100 draws from the original sample. Weighted data are applied.

Table 5 Placebo-Analysis: Random Effects Logit of Labor Force Exit (Discrete Distribution) with Contrafactual Incentive Effects for the Female Sample

(a) Placebo-Incentives: Wrong Year					**(b) Placebo-Incentives: Wrong Age**			
Spec. 4					Spec. 5			
	Coeff.	*Std.Err.*		One-sided test p-value		Coeff.	*Std.Err.*	
Real Incentives:					*Real Incentives:*			
Age 62 in 2001-2004 (step 1)	-2.627	*0.592*	**	0.000 **	Age 62 in 2001-2004 (step 1)	-1.105	*0.471*	*
Age 62 after 2004 (step 2)	-2.774	*0.611*	**	0.317	Age 62 after 2004 (step 2)	-1.571	*0.318*	**
Age 63 after 2004 (step 3)	-0.574	*0.238*	*	n.a.	Age 63 after 2004 (step 3)	-0.117	*0.290*	
Wrong Incentives:					*Wrong Incentives:*			
Age 62 in 1992	-2.188	*0.736*	**	n.a.	< Age 62 in 2001-2004	ref.		
Age 62 in 1993	-1.793	*0.698*	*	0.736	Age 63 in 2001-2004	1.017	*0.463*	*
Age 62 in 1994	-1.504	*0.731*	*	0.677	Age 64 in 2001-2004	0.359	*0.472*	
Age 62 in 1995	-1.615	*0.743*	*	0.435	Age 65 in 2001-2004	0.141	*0.487*	
Age 62 in 1996	-0.566	*0.804*		0.916	Age 66 in 2001-2004	-0.197	*0.508*	
Age 62 in 1997	-0.528	*0.776*		0.519	Age 67 in 2001-2004	0.559	*0.589*	
Age 62 in 1998	-0.973	*0.756*		0.272	Age 68 in 2001-2004	0.223	*0.774*	
Age 62 in 1999	0.291	*0.801*		0.953	>= Age 69 in 2001-2004	1.330	*1.338*	
Age 62 in 2000	ref.			0.358				
Log Likelihood		-1833.26			Log Likelihood		-1837.56	
# parameters estimated		53			# parameters estimated		52	

Note: All estimations are based on Specification 3 in Tables A.2 and 3 (see notes below Table 3). The columns entitled "One-sided test" in Panel 6(a) present the p-value of the one-sided test that the retirement probability at age 62 in year t (see leftmost column) is below that of year t-1: row one compares the joint retirement probability 2001-2004 to that in 2000, row two compares the probability of 2005 to that of 2001-2004. The entries in the rows below "Wrong Incentives" compare single-year retirement probabilities.

Table 6 Predicted Probability of Labor Force Exit Based on Multinomial Logit Estimation with Random Effects (Discrete Distribution)

		Specif. 1		Specif. 2		Specif. 3	
		Predicted prob. of labor force exit (conditional on non-censoring)	% change relative to "before reform"	Predicted prob. of labor force exit (conditional on non-censoring)	% change relative to "before reform"	Predicted prob. of labor force exit (conditional on non-censoring)	% change relative to "before reform"
RE Logit (discrete distribution)	Age 62 , before reform	0.464		0.465		0.456	
	Age 62 , after step 1	0.219	-53%	0.216	-53%	0.217	-52%
	Age 62 , after step 2	0.210	-55%	0.207	-56%	0.206	-55%
	Age 63 , before reform	0.395		0.400		0.401	
	Age 63 , after step 3	0.299	-24%	0.295	-26%	0.296	-26%
RE Multinomial Logit (discrete distribution)	Age 62 , before reform	0.399		0.403		0.405	
	Age 62 , after step 1	0.222	-44%	0.220	-45%	0.219	-46%
	Age 62 , after step 2	0.201	-49%	0.197	-51%	0.198	-51%
	Age 63 , before reform	0.371		0.376		0.378	
	Age 63 , after step 3	0.304	-18%	0.299	-21%	0.301	-20%

Note: The specifications indicated in row 1 refer to the three specifications as presented in Tables A.2 and 3. RE stands for random effects. The predicted values are calculated using weighted data.

Table 7 Random Effects Logit Estimation of Labor Force Exit: Allowing for Time Trend Interactions of Reform Steps

(a) Estimation Results

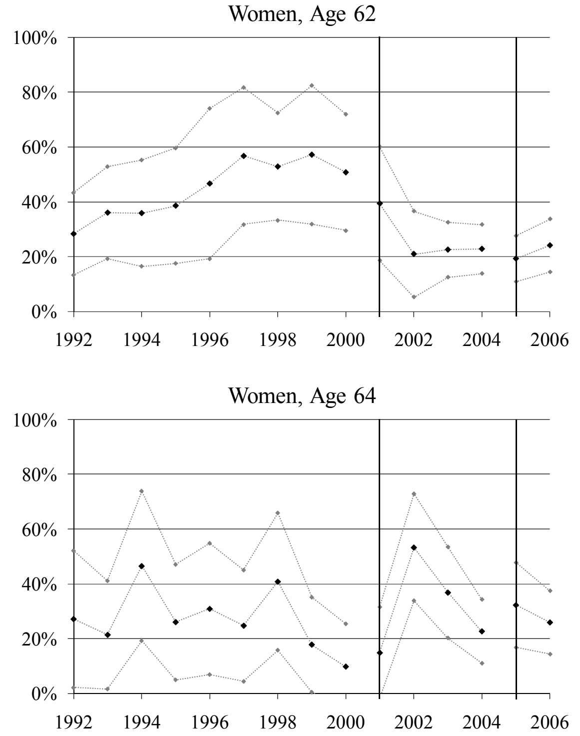
	Coeff.	*Std. Err.*	
Reform Step 1	-0.674	*0.470*	
Reform Step 2	-1.670	*0.310*	**
Reform Step 3	-0.582	*0.240*	*
Reform Step 1 * Years Since Reform	-0.401	*0.194*	*
Age (9)	yes		**
Year (15)	yes		*
Education (3)	yes		**
Marital status (3)	yes		
Industry (8)	yes		
Regions (6)	yes		
Individuals	1773		
Observations	3213		
Log Likelihood	-1842.46		
location random effect 1	-0.475		
probability random effect 1	0.908		

(b) Predicted Effects

	Prediction	Δ Prediction	Std. Err. of Δ	
Age 62, before refrom	0.457			
Age 62, immediately after step 1	0.320	-0.137	0.079	°
Age 62, 1 year after step 1	0.256	-0.064	0.023	*
Age 62, 2 years after step 1	0.209	-0.047	0.025	°
Age 62, 3 years after step 1	0.178	-0.031	0.026	

Note: **, *, and ° indicate statistical significance at the 1, 5, and 10 percent level. In panel (a) the numbers in parentheses indicate the number of categories including the reference. The predictions in panel (b) are generated using weighted data.

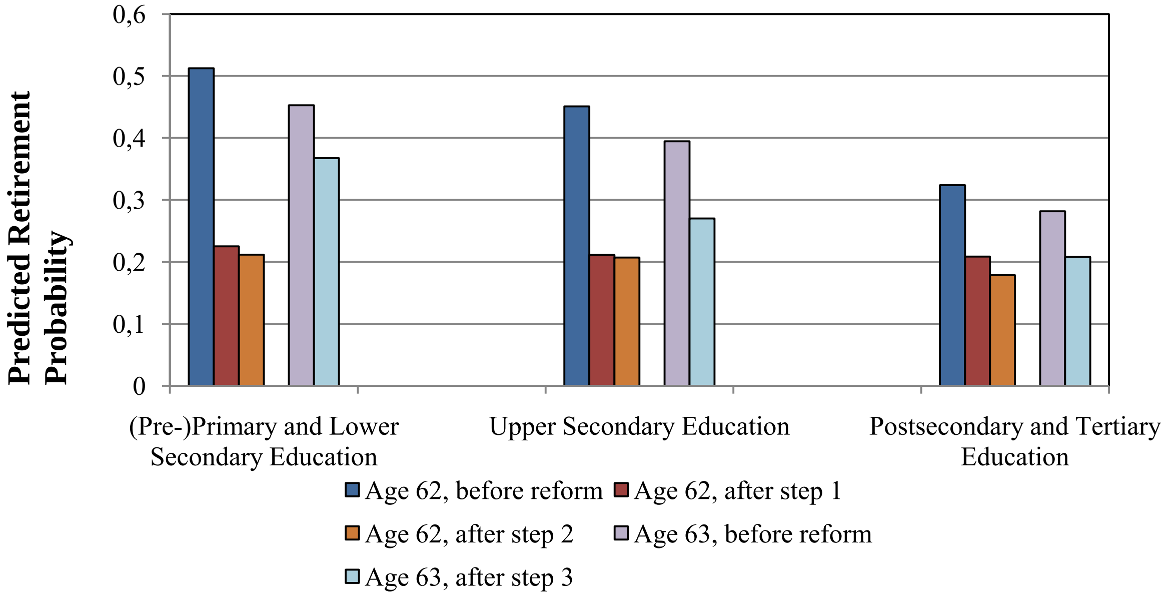
Figure 1 Probability of Labor Force Exit by Age over Time



Note: Most points in the graph are based on fewer than 30 observations. The dark lines indicate the annual subgroup specific probability of labor force exit. They are surrounded by confidence bands at the 95 percent level. Vertical lines indicate the years when reform steps were implemented. Age is measured as actual age at the interview.

Source: Own calculations based on weighted data from the Swiss Labor Force Survey (1991-2006).

Figure 2 Predicted Probability of Labor Force Exit Before and After Reform



Note: The differences in predicted probabilities for women at age 62 before and after reforms are statistically significantly different from zero for all but the highest education group. The difference at age 63 is significant for the middle education group. Standard errors of the predicted probability differences are bootstrapped with 100 draws. The predictions are based on specification (3) of the binomial logit with a discrete distribution of the random effects using weighted data.

Appendix

Table A.1 Descriptive Statistics

Variable	Mean (Std.Dev.)
Dependent Variable:	
labor force exit	0.311
Sample Share Affected by	
reform step 1 for women	0.051
reform step 2 for women	0.022
reform step 3 for women	0.037
reform step 1 for men	--
reform step 2 for men	--
Age:	
age = 60	0.162
age = 61	0.227
age = 62	0.184
age = 63	0.145
age = 64	0.117
age = 65	0.101
age = 66	0.039
age = 67	0.019
age >= 68	0.007
Marital Status:	
married	0.548
single	0.098
widowed/ divorced	0.354
Calendar Year	1998.1 (4.61)
Education:	
higher education	0.119
secondary education	0.499
lower education	0.382
Industry:	
agriculture and mining	0.063
utility (electric power, water) and construction	0.136
trade, transport and communication	0.221
hotel and catering trades	0.100
credit and insurance, real estate investment	0.113
public administration	0.038
education and health sector	0.215
other	0.114
Region:	
Lake Geneva Region	0.150
Swiss Mittelland	0.240
North-Western Switzerland	0.132
Zurich	0.198
Eastern Switzerland	0.164
Central Switzerland	0.085
Ticino	0.030
Person-Year Observations (unweighted)	3,213

Source: Own calculations using weighted data from the Swiss Labor Force Survey (1991-2006).

Table A.2 Alternative Logit Estimators of Labor Force Exit

		Logit (1)	Logit with random effects (normally distributed) (2)	Logit with random effects (discrete distribution) (3)
Specification (1):	Log Likelihood	-1879.5	-1873.8	-1871.8
age, year	# of parameters	26	27	28
	AIC	3811.1	3801.5	3799.5
Specification (2):	Log Likelihood	-1860.6	-1855.7	-1853.3
age, year, education, marital status	# of parameters	30	31	32
	AIC	3781.2	3773.3	3770.6
Specification (3):	Log Likelihood	-1851.0	-1846.4	-1844.4
age, year, education, marital status,	# of parameters	43	44	45
industry, region	AIC	3788.0	3780.9	3778.9

Note: Critical values of the chi^2 distribution are as follows: 1 percent level: 6.63 (1 df) and 9.21 (2 df); 5 percent level: 3.84 (1 df) and 5.99 (2 df).

Table A.3 Alternative Multinomial Logit Estimators of Labor Force Exit and Attrition

		Multinomial Logit (1)	Multinomial Logit with random effects (normally distributed) (2)	Multinomial Logit with random effects (discrete distribution) (3)
Specification (1):	Log Likelihood	-4237.09	-4224.91	-4223.06
age, year	# of parameters	52	55	55
	AIC	8578.2	8559.8	8556.1
Specification (2):	Log Likelihood	-4212.43	-4202.51	-4200.85
age, year, education, marital	# of parameters	60	63	63
status	AIC	8544.9	8531.0	8527.7
Specification (3):	Log Likelihood	-4200.71	-4190.83	-4189.28
age, year, education, marital	# of parameters	86	89	89
status, industry, region	AIC	8573.4	8559.7	8556.6

Note: Critical values of the chi^2 distribution are as follows: 1 percent level: 11.34 (3 df) and 5 percent level: 7.81 (3 df).

Table A.4 Results of the Hausman Test of the IIA Property in the Multinomial Logit Estimations presented in Table 6

	χ^2 Test statistic	Degrees of freedom	p-value
Specification (1): age, year	27.08	26	0.405
Specification (2): age, year, education, marital status	24.03	30	0.771
Specification (3): age, year, education, marital status, industry, region	32.64	43	0.875

Note: Estimations were executed without random effects controls.