Bavarian Graduate Program in Economics

BGPE Discussion Paper

No. 76

New Evidence on Financial Incentives and the Timing of Retirement

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July 2009

ISSN 1863-5733

Editor: Prof. Regina T. Riphahn, Ph.D.
Friedrich-Alexander-University Erlangen-Nuremberg

BGPE Discussion Paper Series

New Evidence on Financial Incentives and the Timing of Retirement

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28 July 2009

We investigate the responsiveness of individual retirement decisions to changes in financial incentives. The causal effect is identified based on the natural experiment generated by an institutional reform. The results of a binary retirement model are robust to alternative model specifications, to a competing risks framework with endogenous panel attrition, and to alternative representations of unobserved individual-specific heterogeneity. We find strong 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 the reforms intensifies over time suggesting that retirement behavior may be affected by social norms. 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.

Keywords:	retirement insurance, incentives, social security, labor force exit, natural experiment, Switzerland
JEL Keywords:	J26, H55, J14

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We gratefully acknowledge helpful comments received at the ESPE (European Society for Population Economics) meeting 2009 in Seville and from seminar participants at the University of Hannover research seminar.

1. Introduction

The funding of retirement is high on the policy agenda of many demographically aging industrialized countries. Retirement insurance funds are affected by individual behavior and it is important to know whether and to which extent behaviors respond to changes in the institutional framework. Therefore we study the impact of financial incentives on retirement behavior.[1]

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 2000 and 2007, or Chan and Stevens 2004). Much of the literature identified behavioral responses to financial incentives based on cross-sectional comparisons of individuals with different benefit claims and focused on the appropriate representation of dynamic incentives.[2] 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 has vast effects on the estimates of responses to retirement incentives.

An empirical approach that does not rely on cross-sectional identification takes advantage of natural experiments, such as institutional reforms of retirement finances. A classic example of this approach is Krueger and Pischke (1992). They show that workers affected by reduced retirement benefits did not respond as strongly as would have been expected based on prior findings. Similarly, Mastrobuoni (2006) investigates whether the 1983 reform of the U.S. normal retirement age affected retirement behavior. Comparing the retirement behavior of birth cohorts subject to different retirement age regulations he finds substantial responses to this reform.

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1 For a general discussion pension reform options see Lindbeck and Persson (2003).

2 For cross-national comparative studies see e.g. Gruber and Wise (2004) or Duval (2004).

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, for both men and women. As these measures reflect policy options available in about every social security system it is both interesting and important to study their effects.[3]

In addition, this study contributes to the literature in three 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 any related 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 across various dimensions such as education and the unemployment situation in different regional labor markets. 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. 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. 1995, Duflo and Saez 2003), where they are discussed as a potential explanation.

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[3] For a discussion of the costs and benefits of alternative reforms see Burtless and Quinn (2002) or Lindbeck and Persson (2003).

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). Both, the first and the second pillar are obligatory.[4] The public AHV pillar sets a minimum age as an eligibility criterion for benefit receipt, but not a mandatory retirement age. For men the eligibility 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 minimum age for women's full retirement benefit eligibility was ratcheted up in two steps from 62 to 63 years in 2001, and to 64 years in 2005. The minimum retirement age for men remained at 65 years. Second, it became possible for women and men to retire prior to the minimum age. The option to draw retirement benefits one year (and later two years) prior to the minimum retirement age was connected with a permanent benefit discount, which amounted for men to a 6.8 percent benefit deduction for every year of early retirement.[5] Since women already suffered the disadvantage of postponed regular retirement, their benefit reduction was set to half that of men, i.e. 3.4 percent for retiring one year early

[4] In addition, a third voluntary pillar consists of tax deductions for individual savings accounts. However, this instrument is not relevant for our analysis.

[5] The benefit discount for men is considered to be actuarially fair.

and 6.8 percent for retiring two years prior to the regular retirement age.[6] **Table 1** reports the timing of the reform steps as well as the benefit reductions tied to early retirement.[7]

Based on these reforms we expect behavioral adjustments in the timing of retirement. In the framework of an intertemporal consumption model individuals' maximization problem at time t is given by:

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

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, and 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:

$$e^{-\delta(R-t)}[u(c_R, l_w) - u(c_R, l_r)]\pi(R) =$$

$$\lambda e^{-r(R-t)} \left[(y_R - b_R(R))\pi(R) + \int_R^T e^{-r(s-R)} \frac{\partial b_R(R)}{\partial R} \pi(s) ds \right]$$

The integral on the right hand side indicates the effect of retirement date R on pension accrual. The Swiss reform changed the benefit function for 62 years old women (and later on for 63 years old): in 2001 benefits after retirement at age 62 declined (initially) by 3.4 percent. Consequently, since the reform it is more likely that the disutility connected to working at age 62 is offset by financial incentives. We expect the probability of continued work to increase after the reform first for women at age 62 and later for those at age 63. Their retirement propensity at age 62 and later at age 63 should decline.

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6 Starting with birth cohort 1948, women will have the same benefit reduction rates as men.

7 Individuals can also postpone benefit receipt for up to five years. Since 1991 this yields a maximum increase in benefits of 31.5 percent (BSV 2006).

For men at the age of 64 (and later at age 63) the difference between labor market income and pension decreased after the reform, as a pension became newly available (see the first term in brackets on the right hand side of the first order condition). This should result in a higher probability of labor force exit at that age. However, since benefit discounts are considered to be actuarially fair on average, total retirement wealth remains unaffected for an average individual, regardless of the date of retirement entry. Depending on individual time preferences and expected survival probabilities retirement at an early age is more or less attractive than late retirement (see the second term in brackets on the right hand side of the first order condition). While individuals with a low life expectancy and an above average discount factor will prefer to retire at age 64, individuals with a high life expectancy or a below average discount factor will not respond to the reform, but continue to claim benefits at the age of 65. Therefore, we expect modest shifts to early retirement among men.

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 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.[8] Similarly, those in regions with a high

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[8] 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. Similary, immigrants and those in low income professions, with unemployment experience and low incomes are least likely to leave the labor force early. Similar evidence exists for Germany (Clemens et al. 2007).

risk of unemployment may be hesitant to give up employment prematurely for an early retirement if the probability of a successful return to the labor market is low. This relates to the debate on whether it is pension wealth or pension accrual that drives retirement behavior (e.g. Samwick 1998). Controls for institutional rules and their exogenous changes over time allow us to measure the relevance of changes in pension accrual, which Samwick (1998) finds to be the central determinant of retirement behavior. We test whether the accrual effect 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 (2006) sets up an intertemporal retirement model where forward-looking individuals smooth their lifetime consumption when they are given a long notice period regarding upcoming institutional changes. He argues that only those with little time to smooth their consumption paths would be expected to abruptly adjust their labor force exit behavior when an institutional reform occurs.[9] In this model the labor supply response to the reform should decline over time as individuals increasingly can take advantage of long term behavioral adjustments.

In contrast, option value models of retirement take account of modified financial consequences of retiring at every given age. Here, rational individuals are predicted to adjust optimal behavior paths immediately after a reform and to consequently change behaviors as soon as a reform is introduced, without a time lag or trend.

Since this type of model typically underpredicts the observed retirement propensity at focal points such as age 62 and 65 in the United States, many authors discuss the relevance of social norms for retirement behavior (e.g. Lumsdaine et al. 1995): it is common to retire at 65

[9] For a recent analysis of the mechanisms behind non-smooth consumption paths see e.g. Blau (2008).

and because it is an accepted practice individuals behave this way. In this scenario behavior patterns of current and past peers affect current choices and it takes time until economically rational changes in retirement behavior are fully established. The hypothesis of consumption smoothing predicts a declining and the hypothesis of social norms an increasing responsiveness to institutional changes over time, while pure option value models lead one to expect a one-off change in behavior without adjustment paths in either direction.

In sum, we test five hypotheses: women postpone retirement when early retirement becomes costly, men retire earlier when it becomes possible, changes in retirement behavior vary with educational attainment and across regional labor markets and they intensify over time.

3. Data and Empirical Approach

Our data are taken from the Swiss Labor Force Survey (SLFS, 1991-2006[10]). The SLFS is a rotating panel with up to five interviews per person covering a representative sample of the Swiss population.[11] In our analysis sample we follow those at risk of retirement, i.e. all individuals aged 60 through 65 who were members of the labor force when they were first interviewed. This provides us with 3,213 and 4,720 person-year observations for 1,773 different female and 2,450 different male labor force participants, for whom at least one transition can be observed. 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.

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10 The German language name of the data is "*Schweizerische Arbeitskräfteerhebung, BFS*".

11 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 birth year, this should not affect our results.

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. In the weighted data we observe a transition to retirement among 31.1 percent of all females and 22.3 percent of all males. We consider retirement to be an absorbing state and censor observations thereafter. The dependent variable is described in the first row of **Table 2**, separately for males and females.

Figure 1 depicts the retirement probabilities by age over time: women's propensity to retire at age 62 clearly declined around 2001, when the first reform was implemented. Instead, the probability of retirement at age 63 went up and came down again when regular retirement age increased to age 64. The spike in the retirement propensity at age 64 in 2002 is spurious and related to an extremely small number of observations (across all birth cohorts only 30 women retired at age 64). Male retirement entry shows no clear response to the modified regulations which allowed retirement at age 64 starting in 1997 and at 63 since 2001.

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. Nevertheless, we follow the literature and refer to those who exit the labor force as retirement entrants.

In order to identify the shift in age-specific retirement propensities following institutional reforms 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 year old females after 2000 and again after 2004, similarly for 63 year old women after 2004. For men, we expect increasing inflows into retirement for the 64 (63) year olds after 1996 (2000). The 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 interaction terms (3 indicators for females, 2 for men) we control for different specifications of the covariate vector X, which consider education, marital status, industry, and regional indicators. We separately model the retirement choices of males and females as they are subject to different regulatory regimes. Descriptive statistics on the explanatory variables for both subsamples are provided in **Table 2**.

The difference-in-differences approach reliably 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 would 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 3**. Given the considered reform one can define five different treatment and control group pairs. We randomly chose to look at two examples: women aged 62 before and after the reform in 2001 and men aged 64 before and after the reform in 1997. As expected, average characteristics do not differ substantially for the treatment and control groups. Significant differences can either be explained by general demographic shifts to higher educational degrees over time or they concern only small subgroups. Overall, they do not cast doubt on our identification strategy.

Our empirical approach proceeds in four 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.[12] As we use panel data we can enhance the efficiency of the logit estimator by applying a random effects model. Assuming that individual unobserved

[12] We obtained very similar results when a least squares estimator was applied. As we intend to study predicted probabilities we prefer to consider a logit estimator to the linear model.

heterogeneity is uncorrelated with the covariates, we compare random effects estimators with normally distributed errors and with a non-parametric discrete-factor error term distribution.[13]

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

An important characteristic of our data is that the SLFS suffers from panel attrition. In step three 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 3,508 observations for men and 2,429 observations for women of which as before 1,086 and 958 are observed to transit to retirement. 29.5 and 26.4 percent of the male and female observations are censored due to attrition.[14] 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 effect 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 four of the analysis we test whether the treatment effect of the retirement reform is heterogeneous over time, across education groups, and for the German and French

13 See Heckman and Singer (1984) and for software implementation Rabe-Hesketh et al. (2004).

14 The share of transitions to retirement now amounts to 21.9 percent for women and 16.2 percent form men, somewhat below those presented in Table 2.

speaking regions of Switzerland. These regions differ in their economic and particularly in their labor market situation,[15] where we take the local labor market situation to be indicative of different levels of income and social security wealth.[16]

4. Results

4.1 Baseline Results

In the first step of our analysis we compared alternative logit estimators. **Table 4** presents estimation statistics (log likelihood values, number of parameters, and AIC statistic) of three logit estimators using the model specifications as outlined above. Based on a likelihood ratio test the explanatory power of two random effect estimators (columns 2 and 3) can be compared to that of the standard logit in column 1. The addition of random effects improves the log likelihood values significantly at the one percent level for the female sample, while the improvement in the likelihood values is not statistically significant for men. Since the random effect models in columns 2 and 3 are not nested we apply the AIC criterion to compare their fit: the discrete random effect distribution always provides a better fit than the normal random effect in the estimations for females. For men, the difference between the estimators is minor. Based on the stronger results for females we chose the random effects model with a discrete error term distribution.[17] All estimations with discretely distributed random effects use a specification with two masspoints. The hypothesis that a third masspoint improves the model fit was rejected in all cases.

In **Table 5** we present our estimation and prediction results. Panels A and C show the estimated coefficients with standard errors for the female and male subsamples. Panels B and

[15] Unemployment rates are generally higher in the French speaking regions. In 2007 and 2008 they reached 4.2 and 3.9 percent there compared to 2.2 and 2.1 percent in the German language region. Also, average earnings differ with higher levels in German speaking cantons (BFS 2008). For a general discussion of the phenomenon see Brügger et al. (2009).

[16] Income and social security wealth are not considered directly in the specifications, first, because they should be endogenous to retirement behavior, and second, because social security wealth as a function of past earnings and labor force participation is unavailable in our data.

[17] The results are not sensitive to the choice of the estimator.

D display the retirement probabilities which were predicted for the entire sample with and without treatment based on the estimation results presented just above.[18]

For the female sample the coefficients of the incentive effects in the first rows of panel A are highly statistically significant and confirm the expected decline in the probability of retirement at age 62 and 63 when benefit cuts were introduced. The effect is quantified in Panel B: based on specification 1 the predicted retirement propensities at age 62 differ significantly for women with and without the reform. The age-specific annual retirement probabilities changed substantially by 53 percent from 46.4 percent before the reform to 21.9 percent after a benefit reduction of 3.4 percent was mandated in 2001. The retirement probability dropped to 21.0 percent after the benefit reduction of 6.8 percent was introduced in 2005. At age 63 the responsiveness of Swiss women is smaller. Here, the drop in retirement probabilities amounted to about 24 percent (from 39.5 to 29.9 percent) following the introduction of the 3.4 percent benefit discount in 2005. 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. We bootstrapped the standard errors of the difference in predicted retirement probabilities before and after the reform. The decline in retirement probabilities is highly significant for the 62 years old women and significant at least at the 10 percent level for the 63 year olds. Thus the reform had significant effects on behavior and the older female labor force responded strongly to shifts in retirement incentives.

At first glance, these results are remarkably close to those found by Mastrobuoni (2006): he obtained a drop in retirement probabilities by fifty percent for every year the U.S. normal retirement age was postponed, which matches our result for the 62 year olds.

[18] We predicted each individual retirement probability and integrated it out over the estimated distribution of the random effect. Then we calculated the average retirement probability across all individual predictions.

However, since Mastrobuoni (2006) investigated a scenario with twice the benefit reduction compared to the Swiss case and one where workers are substantially older, Swiss women at age 62 appear to be more responsive. Samwick (1998) also predicted the effect of changes in the normal retirement age (NRA) on social security receipt. He jointly considers several reforms and concludes that a shift of the NRA from 65 to 67 reduces the cumulative probability of retirement by age 70 by one percentage point. Hanel (2009) models the effect of a reform similar to the Swiss one in the institutional framework of Germany: shifting NRA by 5 years from age 60 to 65 combined with a benefit discount of 3.6 percent for every year of retirement prior to age 65 generates a reduction in the propensity to retire at age 60 by 90 percent. The effect is comparable in magnitude to that found for Swiss retirees, who have to sacrifice 6.8 percent of their benefit for two years of early retirement at age 63. In the Swiss case we find the retirement propensity to decline by about 56 percent. Thus, while our results for females suggest a larger responsiveness than comparable studies based on U.S. data they are comparable in magnitude to results from a neighboring country.[19]

To test the plausibility of our results and interpretations we performed a "placebo analysis" for the female sample, 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 other periods and (b) whether the probability of retirement in the period 2001-2004 changed significantly for other age groups, as well. If these effects are statistically significant we cannot be certain that the measured response is in fact caused by the reform. The estimation results are presented in **Table 6**. Column (1) presents the results as in **Table 5**(3). In column (2) we control for the calendar year specific retirement probability of 62 years old women. Not surprisingly, even with these detailed controls our main incentive effects remain large and statistically significant. The estimated coefficients suggest that over the course of the

[19] Börsch-Supan et al. (2004) find 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 and the benefit discount amounts to 6 percent per year of early retirement.

1990s the probability of retirement at age 62 increased. While we have no explanation for the significantly lower levels in the early 1990s the evidence supports our previous conclusions: it is only after 2000 that the probability of retirement at age 62 started to decline significantly, exactly when the benefit cuts were enacted.[20] In column (3) 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 retirement probability for the 63 year old women increased and there were no significant changes in the behaviors of other age groups. In column (4) we confirm this result by modeling the joint effect all age groups older than 62. The increase for the 63 years old is the immediate consequence of the reform which caused 62 year old women to postpone retirement by one year. Out of the group of the 61, and 64-69 years old women we find a significant change at the 10 percent level only for the small group of 66 year olds. When the different age groups are considered jointly all effects are separately and jointly insignificant, thus corroborating our evidence in favor of causal reform effects.

The estimated response of men to changes in retirement regulations are much more modest. The newly introduced possibility of early retirement has no significant effect on retirement behavior (see **Table 5**, Panel C). When early retirement was first allowed at age 64 the overall probability of labor force exit at that age increased slightly, yet no specification yields significant changes in predicted retirement propensities (see Panel D). After the second reform in 2001, which allowed retirement at age 63, we again find a small increase in the propensity to retire early. The results are robust across specifications but yield no significant

[20] There were no changes to the eligibility rules in the first and second pillar of the retirement system at the time. Since the specification controls for calendar year fixed effects general labor market trends also cannot explain the observed patterns. However, Bütler et al. (2004) present evidence for a secular shift to earlier retirement ages for men and women over the 1990s. Mean female retirement age in their firm-specific data dropped from 61.5 to 60.2 years between the periods of 1990-1994 and 2000-2003. The authors consider early retirement options to explain this development. This suggests that the increasing retirement probability at age 62 in the early 1990s may correspond to increasing labor force exit at earlier ages. This is the opposite of the developments observed starting 2000, when retirement shifted to higher ages. Therefore the significant effects found for the first years in our data does not seem to pose a problem for our analysis.

response of male retirement behavior to the reform.[21] Possible explanations of the difference in responses observed for females and males may relate to a variety of issues: (i) differences in the actuarial fairness of the reform, (ii) differences due to longer life-expectancies for women or similarly higher time discount rates for men, and (iii) higher risk aversion among females who do not want to run the risk a long life with low benefits. In addition, the results match existing evidence, which shows that the labor supply elasticity of older women with respect to retirement benefits is higher than that of older men (Blau and Riphahn, 1999).

4.2 Effects of Panel Attrition

As discussed above, our data raise the concern of endogenous panel attrition.[22] 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 reformulated our dependent variable and reestimated the determinants of the transition to retirement while at the same time controlling for possible endogenous panel attrition. We applied a multinomial logit model with and without random effects. In a first step we evaluated - as before - the fit of alternative estimators to the data (see **Table 7**). The results are very similar to those presented in **Table 4**: 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 incentive indicator for the probability of retirement relative to the probability of employment are highly significant and negative for females and insignificant for males.

[21] We additionally performed all estimations and predictions applying both a linear probability model and alternative logit estimators and always obtained very similar results.

[22] Only about 30 percent of all interviewees reach the fifth interview in our rotating panel survey. All others leave the survey before that.

Table 8 summarizes the predicted retirement probabilities obtained based on the binomial and multinomial logit estimations for the two gender subsamples using three model specifications.[23] 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. Men increased their transition rates to retirement at age 64 (63) by about 7 (28) 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 both subsamples and all three model specifications.[24] The results (see **Table 9**) 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 fourth step of our empirical analysis we investigate the heterogeneity of the reforms' treatment effects. We are interested in changes over time, in differences between individuals in the German and French language regions of Switzerland, and between groups

[23] 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.

[24] While the comparison presented in **Table 8** 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.

with different levels of human capital. **Table 10** presents the estimation results of specifications which test for significant differences in the reform effect over time (columns 1 and 2) and regional labor markets (columns 3 and 4) based on model specification (3), as presented before. Given that our data describe only one year after the introduction of the second reform step for females, the effect of a time trend for women can only be studied for the first reform. The coefficient of the interaction term 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. For the male sample both reform steps can be interacted with a time trend. Here, both coefficients are positive indicating rising retirement probabilities over time. However, the coefficients are neither individually nor jointly significant. Overall we thus find weak evidence for a protracted effect of the reform. Ceteris paribus, this can be interpreted as evidence in favor of a social norm which may inhibit an immediate behavioral adjustment to changed incentives.[25]

We evaluate the heterogeneity of the treatment effects across regional labor markets and education groups, to gauge 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 wages, low wealth, and in weaker labor markets.

The last columns of **Table 10** yield that there are no significant differences in responses to retirement reforms between individuals in the German and French regional labor markets of Switzerland. Only on average women in western Switzerland appear to have a higher retirement propensity than those in the German-speaking region. This confirms results of additional estimations (not presented) where we failed to find any heterogeneity in responses to the retirement reform based on regional unemployment, on actual or predicted

[25] 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).

individual unemployment. These results match the findings of Samwick (1998): once controls for pension accrual are considered, we find no behavioral effects of pension wealth.

Finally, we added education group interaction terms to our model in specification (3). **Figures 2 (a)** and **(b)** depict the predicted age-specific retirement probabilities for females and males, by education group and at different points in time: the initial pre-reform retirement probability at age 62 (or 63) was 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 reforms. The drops after 2001 and 2005 are significant at the 1 percent level for the two lower educational (standard errors were obtained by bootstrap). 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 comparatively 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.

An inspection of retirement probabilities among men confirms that individuals with lower levels of human capital appear to be more likely to retire prior to age 65. The introduction of early retirement (labeled incentive) is expected to generate an increase in retirement probabilities. However, the reform in 1997, which allowed for early retirement at age 64, yielded an increase only for the two higher education subsamples. In 2001, we observe particularly men at middle education levels to respond to the new opportunity and retire significantly earlier. In general, male responses to the new retirement options remain weak and do not seem to reflect a large demand for more flexible early exit opportunities.

5. Conclusions

This study has identified the effect of financial incentives on retirement behavior taking advantage of the natural experiment of exogenous institutional reforms in Switzerland. This source of identification helps avoid the substantial biases of up to 50 percent that e.g. Chan and Stevens (2004) found when they compared the results of cross-sectional estimations with those obtained when controlling for individual unobserved heterogeneity.

The reform of the Swiss retirement insurance increased 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.[26] Additionally, the reform introduced the option of early retirement at an actuarially fair benefit discount for men.

We apply a difference-in-differences type procedure and confirm the robustness of our results with respect to alternative model specifications and estimators. We find substantial responses to the shift of the regular retirement age in connection with benefit reductions for females: a reduction in benefits by 3.4 (6.8) percent, caused a decline in the retirement probability at age 62 from 46 to 22 (21) percent, i.e. by over 50 percent. 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. Permitting early retirement for men at a benefit discount has only small and insignificant effects on labor force exit behavior. Since generally behavioral adjustments intensify over time a social norm may affect behavioral choices of Swiss workers.[27] 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.

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26 For OECD recommendations on retirement policy reforms see Casey et al. (2003); also Lindbeck and Persson (2003) have a comprehensive discussion.

27 However, we cannot separately identify the effect of a social norm and of birth cohort effects.

Overall, retirement behavior shows a substantial response to changes in incentives. At the same time one can argue that the effect for Switzerland may indicate a lower bound on the effect possible in other countries, because the reform here affects only the first pillar of the retirement insurance system while the other pillars remained unchanged.[28] We expect stronger effects if a reform comprehensively addresses all funding sources for retirement. Our findings confirm prior studies (e.g. Asch et al. 2005) and suggest that financial retirement incentives may still be able to substantially affect the retirement plans of the generations to come and therefore can contribute to solve the funding problems of retirement insurance funds.

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[28] Coile and Gruber (2007) find that individuals are equally responsive to social security and pension plan incentives.

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

	Men			Women		
Retirement Regime as of	Regular Retirement Age	Early Retirement Age (% Benefit Reduction)		Regular Retirement Age	Early Retirement Age (% Benefit Reduction)	
1996	65	-	-	62	-	-
1997 - 2000	65	64 (6.8 %)	-	62	-	-
2001 - 2004	65	64 (6.8 %)	63 (13.6 %)	63	62 (3.4 %)	-
starting 2005	65	64 (6.8 %)	63 (13.6 %)	64	63 (3.4 %)	62 (6.8 %)

Table 2 Descriptive Statistics

	Women	Men
Variable	Mean (Std.Dev.)	
Dependent Variable:		
labor force exit	0.311	0.223
Incentive:		
age 62 after 2000	0.051	--
age 62 after 2004	0.022	--
age 63 after 2004	0.037	--
age 64 after 1996	--	0.127
age 63 after 2000	--	0.069
Age:		
age = 60	0.162	0.128
age = 61	0.227	0.182
age = 62	0.184	0.205
age = 63	0.145	0.196
age = 64	0.117	0.164
age = 65	0.101	0.080
age = 66	0.039	0.029
age = 67	0.019	0.012
age >= 68	0.007	0.003
Marital status:		
married	0.548	0.828
single	0.098	0.059
widowed/ divorced	0.354	0.113
Calendar Year	1998.1 (4.61)	1997.7 (4.47)
Education:		
higher education	0.119	0.307
secondary education	0.499	0.491
lower education	0.382	0.201
Industry:		
agriculture and mining	0.063	0.104
utility (electric power, water) and construction	0.136	0.347
trade, transport and communication	0.221	0.182
hotel and catering trades	0.100	0.019
credit and insurance, real estate investment	0.113	0.150
public administration	0.038	0.055
education and health sector	0.215	0.086
other	0.114	0.057
Region:		
Lake Geneva Region	0.150	0.165
Swiss Mittelland	0.240	0.228
North-Western Switzerland	0.132	0.135
Zurich	0.198	0.179
Eastern Switzerland	0.164	0.148
Central Switzerland	0.085	0.105
Ticino	0.030	0.039
Person-Year Observations (unweighted)	3,213	4,720

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

Table 3 Comparison of Control and Treatment Group Characteristics

	Women at age 62					Men at age 64				
Variable	Mean before 2001	Mean after 2000	Diff.	Standard Error of Diff.		Mean before 1997	Mean after 1996	Diff.	Standard Error of Diff.	
Marital status:										
married	53.2%	57.0%	-3.8%	4.4%		84.0%	82.3%	1.7%	3.5%	
single	13.8%	8.3%	5.5%	2.7%	*	7.7%	5.1%	2.6%	2.6%	
widowed/ divorced	33.0%	34.7%	-1.7%	4.0%		8.3%	12.6%	-4.2%	2.5%	
Education										
higher education	8.6%	10.3%	-1.8%	2.4%		35.1%	27.9%	7.2%	4.4%	
secondary education	48.6%	56.8%	-8.2%	4.4%		41.1%	53.1%	-12.0%	4.6%	*
lower education	42.9%	32.8%	10.0%	4.4%	*	23.8%	19.1%	4.8%	4.2%	
Industry:										
agriculture and mining	7.9%	3.4%	4.5%	2.5%		9.3%	8.6%	0.7%	2.9%	
utility and construction	16.6%	13.1%	3.5%	3.2%		36.5%	37.3%	-0.8%	4.6%	
trade, transport communic.	21.7%	22.8%	-1.2%	3.7%		21.0%	17.5%	3.4%	3.9%	
hotel and catering trades	7.4%	7.8%	-0.3%	2.3%		0.2%	2.5%	-2.3%	0.8%	**
credit, insurance, real estate	10.9%	10.8%	0.1%	2.6%		11.3%	15.1%	-3.8%	3.0%	
public administration	3.7%	6.0%	-2.2%	1.8%		6.4%	3.4%	3.0%	2.1%	
education and health	22.8%	26.3%	-3.5%	3.8%		8.9%	9.7%	-0.7%	2.5%	
other	8.9%	9.8%	-0.8%	2.6%		6.4%	5.9%	0.5%	2.2%	
Region:										
Lake Geneva Region	17.8%	14.5%	3.2%	3.1%		14.7%	18.3%	-3.6%	3.0%	
Swiss Mittelland	22.2%	26.9%	-4.7%	3.9%		24.0%	19.3%	4.7%	4.2%	
North-Western Switzerland	13.7%	12.9%	0.9%	2.9%		13.9%	12.7%	1.2%	3.1%	
Zurich	17.6%	16.2%	1.4%	3.3%		18.3%	17.4%	0.9%	3.5%	
Eastern Switzerland	18.4%	14.3%	4.0%	3.4%		14.2%	16.1%	-1.9%	3.5%	
Central Switzerland	7.1%	10.7%	-3.5%	2.4%		11.5%	12.5%	-1.0%	3.4%	
Ticino	3.1%	4.5%	-1.4%	1.9%		3.4%	3.6%	-0.3%	1.6%	
Observations (unweighted)	353	389				209	709			

Note: ** and * indicate statistically significant differences between treatment and control group at the 1 and 5 percent level.

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

Table 4 Alternative Logit Estimators

		Logit (1)	Logit with random effects (normally distributed) (2)	Logit with random effects (discrete distribution) (3)
A - Women				
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
B - Men				
Specification (1):	Log Likelihood	-2332.6	-2331.4	-2331.3
age, year	# of parameters	25	26	27
	AIC	4715.2	4714.9	4716.7
Specification (2):	Log Likelihood	-2316.0	-2315.1	-2315.0
age, year, education, marital status	# of parameters	29	30	31
	AIC	4690.1	4690.3	4692.0
Specification (3):	Log Likelihood	-2290.7	-2289.6	-2288.6
age, year, education, marital status,	# of parameters	42	43	44
industry, region	AIC	4665.4	4665.2	4665.1

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 5 Estimation and Prediction Results – Random Effects Logit (Discrete Distribution)

	Specif. 1	Specif. 2	Specif. 3
A - Women: Estimation results	Coeff. *Std. Err.*	Coeff. *Std. Err.*	Coeff. *Std. Err.*
Incentive 2001: age 62 after 2000	-1.546 **	-1.548 **	-1.540 **
	0.274	*0.270*	*0.273*
Incentive 2005a: age 62 after 2004	-1.678 **	-1.674 **	-1.691 **
	0.318	*0.312*	*0.315*
Incentive 2005b: age 63 after 2004	-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
B - Women: Predicted retirement probability			
Age 62, without incentive	0.464	0.465	0.456
Age 62, with incentive 2001	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, without incentive	0.464	0.465	0.456
Age 62, with incentive 2005a	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, without incentive	0.395	0.400	0.401
Age 63, with incentive 2005b	0.299	0.295	0.296
Difference	-0.096	-0.105 °	-0.105 °
Standard Error of Difference	*0.069*	*0.057*	*0.060*
C - Men: Estimation results	Coeff. *Std. Err.*	Coeff. *Std. Err.*	Coeff. *Std. Err.*
Incentive 1997: age 64 after 1996	0.134	0.102	0.098
	0.244	*0.244*	*0.247*
Incentive 2001: age 63 after 2000	0.327	0.327	0.311
	0.224	*0.224*	*0.227*
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	-2331.34	-2314.98	-2288.56
# parameters estimated	27	31	44
D - Men: Predicted retirement probability			
Age 64, without incentive	0.177	0.179	0.179
Age 64, with incentive 1997	0.193	0.192	0.192
Difference	0.017	0.013	0.013
Standard Error of Difference	*0.038*	*0.048*	*0.036*
Age 63, without incentive	0.131	0.130	0.129
Age 63, with incentive 2001	0.167	0.167	0.164
Difference	0.036	0.037	0.035
Standard Error of Difference	*0.025*	*0.024*	*0.026*

Note: **, * and ^{o} indicate statistical significance at the 1, 5, and 10 percent level. In panels B and D the asterisks indicate the statistical significance of the difference between the predicted probabilities under old and new regulations. Standard errors were bootstrapped with 100 draws from the original sample. Weighted data are applied. The numbers in parentheses indicate the number of categories including the reference.

Table 6 Placebo-Analysis: Estimation and Prediction Results – Random Effects Logit (Discrete Distribution) with contrafactual incentive effects

	(1)			(2)			(3)			(4)		
	Coeff.	*Std. Err.*		Coeff.	*Std. Err.*		Coeff.	*Std. Err.*		Coeff.	*Std. Err.*	
Incentive 2001: age 62 2001-2004	-1.540	*0.273*	**	-2.627	*0.592*	**	-1.105	*0.471*	*	-1.121	*0.511*	*
Incentive 2005a: age 62 since 2005	-1.691	*0.315*	**	-2.774	*0.611*	**	-1.571	*0.318*	**	-1.678	*0.316*	**
Incentive 2005b: age 63 since 2005	-0.587	*0.242*	*	-0.574	*0.238*	*	-0.117	*0.290*		-0.559	*0.242*	*
A) Placebo-Incentives:												
Age 62 in 1992	no			-2.188	*0.736*	**	no			no		
Age 62 in 1993	no			-1.793	*0.698*	*	no			no		
Age 62 in 1994	no			-1.504	*0.731*	*	no			no		
Age 62 in 1995	no			-1.615	*0.743*	*	no			no		
Age 62 in 1996	no			-0.566	*0.804*		no			no		
Age 62 in 1997	no			-0.528	*0.776*		no			no		
Age 62 in 1998	no			-0.973	*0.756*		no			no		
Age 62 in 1999	no			0.291	*0.801*		no			no		
Age 62 in 2000	no			(reference)			no			no		
Wald-Test: all placebos												
χ^2(dF) *z-value*	no			21.77 (8)	*0.005*	**	no			no		
Wald-Test: placebos 1996-1999												
χ^2(dF) *z-value*	no			3.46 (4)	*0.484*		no			no		
B) Placebo-Incentives:												
Age 61 in 2001-2004	no			no			(reference)			no		
Age 63 in 2001-2004	no			no			1.017	*0.463*	*	no		
Age 64 in 2001-2004	no			no			0.359	*0.472*		no		
Age 65 in 2001-2004	no			no			0.141	*0.487*		no		
Age 66 in 2001-2004	no			no			-0.197	*0.508*		no		
Age 67 in 2001-2004	no			no			0.559	*0.589*		no		
Age 68 in 2001-2004	no			no			0.223	*0.774*		no		
>= Age 69 in 2001-2004	no			no			1.330	*1.338*		no		
Wald-Test: all placebos												
χ^2(dF) *z-value*	no			no			13.23 (7)	*0.067*		no		
Wald-Test: placebos 64-69												
χ^2(dF) *z-value*	no			no			3.96 (6)	*0.682*		no		
>= Age 63 in 2001 -2004	no			no			no			0.450	*0.456*	
Age (9)	yes			yes			yes			yes		
Year (15)	yes			yes			yes			yes		
Education (3)	yes			yes			yes			yes		
Marital status (3)	yes			yes			yes			yes		
Industry (8)	yes			yes			yes			yes		
Region (7)	yes			yes			yes			yes		
Log Likelihood	-1844.44			-1833.26			-1837.56			-1843.89		
# parameters estimated	45			53			53			46		

Notes: See Table 5

Table 7 Alternative Multinomial Logit Estimators

		Multinomial Logit (1)	Multinomial Logit with random effects (normally distributed) (2)	Multinomial Logit with random effects (discrete distribution) (3)
A - Women				
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
B - Men				
Specification (1):	Log Likelihood	-6180.04	-6173.52	-6171.56
age, year	# of parameters	50	53	53
	AIC	12460.1	12453.0	12449.1
Specification (2):	Log Likelihood	-6159.90	-6153.79	-6151.55
age, year, education, marital	# of parameters	58	61	61
status	AIC	12435.8	12429.6	12425.1
Specification (3):	Log Likelihood	-6132.07	-6125.39	-6122.57
age, year, education, marital	# of parameters	84	87	87
status, industry, region	AIC	12432.1	12424.8	12419.1

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 8 Prediction Results Based on Multinomial Logit Estimation with Random Effects (Discrete Distribution)

		Model (1) - controls for: age, year		Model (2) - controls for: age, year, education, marital status		Model (3) - controls for: age, year, education, marital status, industry, region	
		Predicted prob. of retirement (conditional on non-censoring)	% change relative to "no incentive"	Predicted prob. of retirement (conditional on non-censoring)	% change relative to "no incentive"	Predicted prob. of retirement (conditional on non-censoring)	% change relative to "no incentive"
A - Women							
Logit with random effects (discrete distribution)	Age 62 , no incentive	0.464		0.465		0.456	
	Age 62 , incentive 2001	0.219	-53%	0.216	-53%	0.217	-52%
	Age 62 , incentive 2005	0.210	-55%	0.207	-56%	0.206	-55%
	Age 63 , no incentive	0.395		0.400		0.401	
	Age 63 , incentive 2005	0.299	-24%	0.295	-26%	0.296	-26%
Multinomial Logit with random effects (discrete distribution)	Age 62 , no incentive	0.399		0.403		0.405	
	Age 62 , incentive 2001	0.222	-44%	0.220	-45%	0.219	-46%
	Age 62 , incentive 2005	0.201	-49%	0.197	-51%	0.198	-51%
	Age 63 , no incentive	0.371		0.376		0.378	
	Age 63 , incentive 2005	0.304	-18%	0.299	-21%	0.301	-20%
B - Men							
Logit with random effects (discrete distribution)	Age 64 , no incentive	0.177		0.179		0.179	
	Age 64 , incentive 1997	0.193	10%	0.192	7%	0.192	7%
	Age 63 , no incentive	0.131		0.130		0.129	
	Age 63 , incentive 2001	0.167	28%	0.167	28%	0.164	27%
Multinomial Logit with random effects (discrete distribution)	Age 64 , no incentive	0.187		0.188		0.188	
	Age 64 , incentive 1997	0.192	3%	0.192	2%	0.191	2%
	Age 63 , no incentive	0.136		0.137		0.136	
	Age 63 , incentive 2001	0.168	23%	0.168	23%	0.164	21%

Table 9 Results of the Hausman Test of the IIA Property

	χ^2 Test statistic	Degrees of freedom	p-value
A - Women			
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
B - Men			
Specification (1): age, year	24.81	25	0.473
Specification (2): age, year, education, marital status	31.49	29	0.343
Specification (3): age, year, education, marital status, industry, region	40.20	42	0.550

Note: Estimations were executed without random effects controls.

Table 10 Random Effects Logit Estimation: Heterogeneity of Incentive Effects

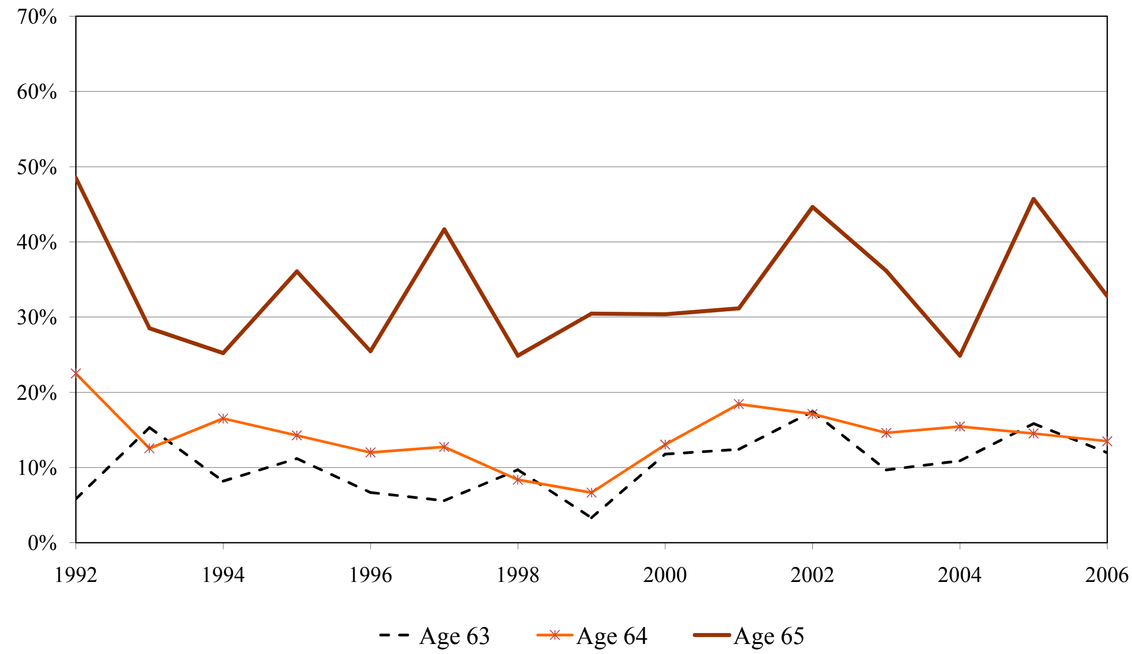
	Time Trend Interactions						Regional Labor Market Differences					
	Women			Men			Women			Men		
	Coeff.	(Std. Err.)		Coeff.	(Std. Err.)		Coeff.	(Std. Err.)		Coeff.	(Std. Err.)	
Incentive 2001: age 62 after 2001	-0.674	(0.470)		-	-		-1.389	(0.284)	**	-	-	
Incentive 2005a: age 62 after 2004	-1.670	(0.310)	**	-	-		-1.629	(0.346)	**	-	-	
Incentive 2005b: age 63 after 2004	-0.582	(0.240)	*	-	-		-0.534	(0.261)	*	-	-	
Inc. 2001 * Years Since Reform	-0.401	(0.194)	*	-	-		-	-		-	-	
Inc. 2001 * Region: French	-	-		-	-		-0.765	(0.531)		-	-	
Inc. 2005a * Region: French	-	-		-	-		-0.244	(0.508)		-	-	
Inc. 2005b * Region: French	-	-		-	-		-0.228	(0.382)		-	-	
Incentive 1997: age 64 after 1996	-	-		-0.110	(0.361)		-	-		0.134	(0.253)	
Incentive 2001: age 63 after 2000	-	-		0.305	(0.359)		-	-		0.344	(0.237)	
Inc. 1997 * Years Since Reform	-	-		0.035	(0.044)		-	-		-	-	
Inc. 2001 * Years Since Reform	-	-		0.011	(0.089)		-	-		-	-	
Inc. 1997 * Region: French	-	-		-	-		-	-		-0.128	(0.269)	
Inc. 2001 * Region: French	-	-		-	-		-	-		-0.085	(0.294)	
Region: French	-	-		-	-		0.282	(0.124)	*	-0.052	(0.114)	
Age (9)	yes		**	yes		**	yes		**	yes		**
Year (15)	yes		*	yes			yes		o	yes		
Education (3)	yes		**	yes		**	yes		**	yes		**
Marital status (3)	yes			yes			yes			yes		
Industry (8)	yes			yes		**	yes			yes		**
Regions (6)	yes			yes			no			no		
Individuals	1773			4720			1773			4720		
Observations	3213			2450			3213			2450		
Log Likelihood	-1842.5			-2288.2			-1845.6			-2290.5		

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

Figure 1 Probability of a Transition to Retirement by Age over Time

(a) Females

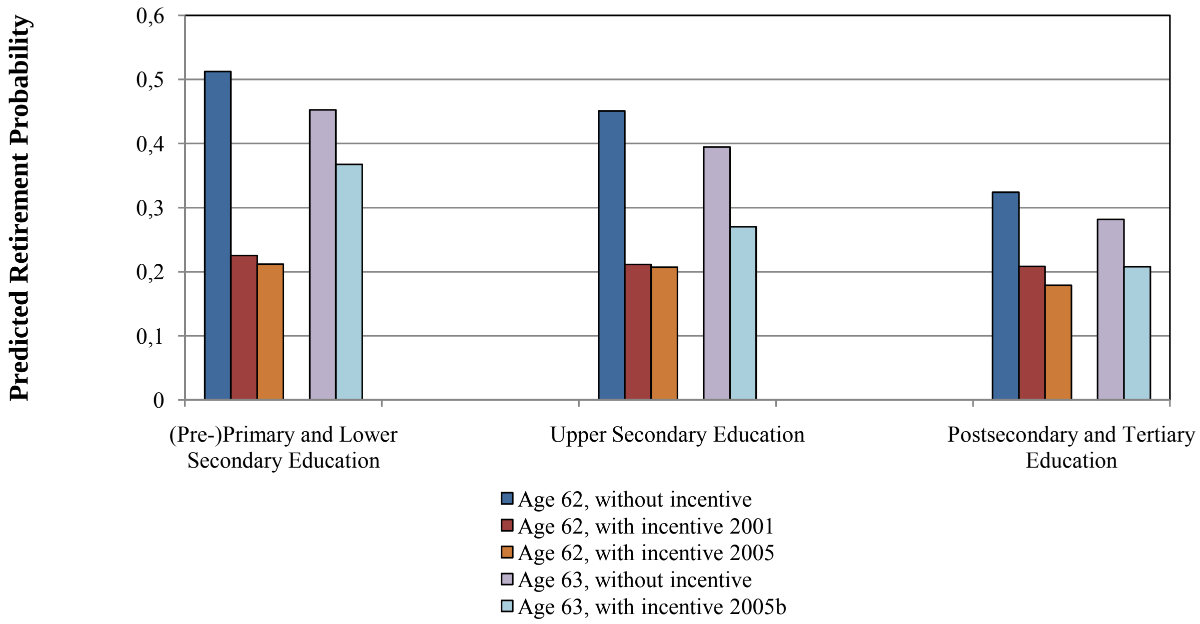
(b) Males



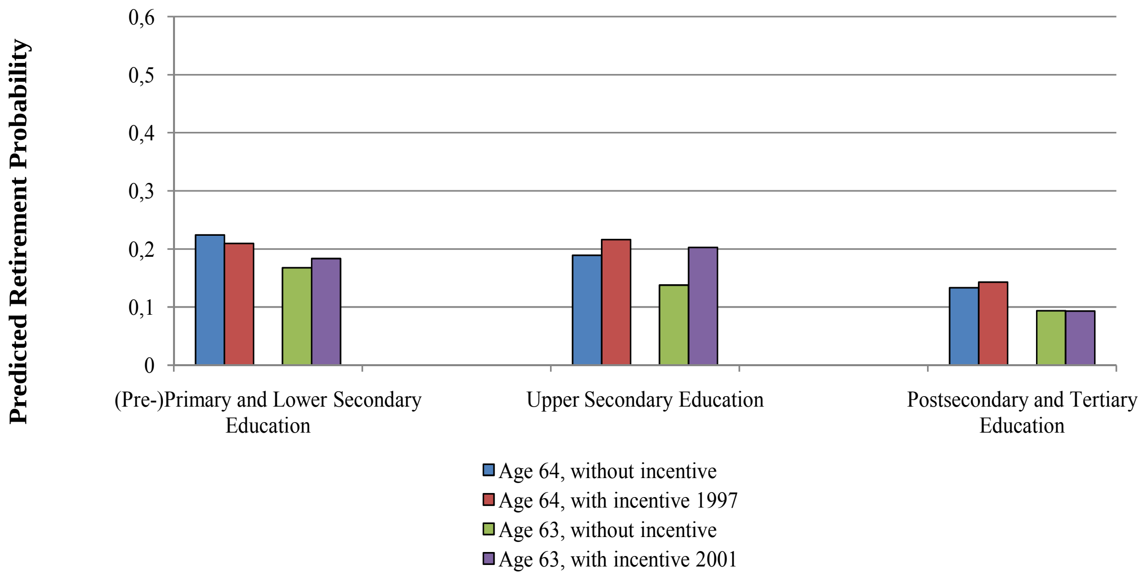
Note: Most points in the graph are based on fewer than 30 observations.
Source: Own calculations based on weighted data from the Swiss Labor Force Survey (1991-2006).

Figure 2 Predicted Retirement Probability Before and After Reform

(a) Females



(b) Males



Note: The differences in predicted retirement probabilities for women at age 62 with and without incentive mechanisms 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. For men we find a significant difference in the middle education group for the retirement probability at age 63 with and without incentives. Standard errors of the predicted probability differences were 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.