

Bauer, Ann Barbara; Eichenberger, Reiner

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Center for Research in Economics, Management and the Arts

Endogenous aging: How statutory retirement age drives human and social capital

Working Paper No. 2017-02

CREMA Südstrasse 11 CH - 8008 Zürich www.crema-research.ch

Endogenous aging: How statutory retirement age drives human and social capital*

Ann Barbara Bauer[†]

Reiner Eichenberger[‡]

ABSTRACT: The return on investments in human and social capital increases in their economic lifetime. Thus, personal, parental, and societal investments in the capacities of individuals take place when these persons are young. Interestingly, the complementary thesis has been widely neglected; investments in the productive capacities of older workers—by the employees themselves, their employers, and their co-workers—should be expected to depend on the time left before retirement. In this paper, we analyze how an increase in the statutory retirement age affects investments in the productivity of older workers. We compare pre- and post-pension reform cohorts and estimate the treatment effect on training participation, job involvement, support from colleagues, and leisure activities. Using a Swiss natural experiment, we find strong support for higher human and social capital investments and the reallocation of time from leisure to work.

KEYWORDS: Pension reform, natural experiment, old-age productivity, human capital, social capital, allocation of time

JEL Code: H55, J14, J24, J26

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[†] University of Fribourg, Boulevard de Pérolles 90, 1700 Fribourg, Switzerland. E-mail: annbarbara.bauer@unifr.ch

[‡] University of Fribourg, Boulevard de Pérolles 90, 1700 Fribourg, Switzerland and CREMA. E-mail: reiner.eichenberger@unifr.ch

1 Introduction

The aging of the population is one of the main challenges to developed countries.¹ The old-age dependency ratio in the OECD countries (i.e., the ratio of people over 65 years old to the working population) grew from 13% in 1950 to almost 30% in 2014.² This situation causes problems in the labor market and social security systems. The most directly affected are pay-as-you-go (PAYG) pension systems with a strong redistributive character (Weil 2006). While reducing benefits or increasing contributions may ameliorate the situation, the policy strategy with the most direct impact is postponing retirement.

However, the consequences of increasing the retirement age for the productivity of the firms and the older workers themselves are not yet well understood. On one hand, the productive capacity of older workers is typically thought to decrease with age and to be lower than that of younger and middle-aged workers (Kotlikoff and Gokhale 1992). Thus, keeping older employees working for additional years brings about decreasing returns and intricate problems when their productivity levels fall below their wages. Against this pessimistic hypothesis, it could be argued that productivity before retirement is not exogenously driven by biological and medical forces but is endogenous to the retirement age. It pays less to invest in the productive capacity of older workers, as they have less time available before retirement to recoup the investment than do younger workers. Thus, an institutional change in the working horizon affects older workers more basically, since their remaining working horizon increases disproportionately compared to that of young workers (Jaag 2012). Therefore, we expect firms, co-workers, and the (older) workers themselves to invest more in the human and social capital of senior workers when the effective retirement age increases.

To test out expectations, we analyze how the 1997 reform of the retirement scheme in Switzerland, which raised the statutory retirement age (SRA) for women only, has affected the incentives to invest in their productivity. This natural experiment provides an optimal setting to apply a regression discontinuity design (RDD). Under the pension reform, women born in and before 1941 remain subject to the existing retirement age of 63. Women born in 1942 and thereafter are subject to the reform,

¹ For recent contributions regarding demographic transition, see Conesa and Krueger (1999), Börsch-Supan (2003), Bovenberg and Knaap (2005), and Rojas (2005).

² Source: OECD Statistics, <https://stats.oecd.org> on population statistics.

meaning they would work one year longer, up to the age of 64.³ Using two Swiss survey datasets on labor, the workplace environment, and leisure activities, we gain insights on how the reform modifies incentives to invest in older workers from three relevant perspectives. We find strong support for the hypothesis that productivity is not only exogenously determined by the worker's physical age but also by endogenously generated investment decisions. We provide robust evidence that the postponement of retirement leads to higher investments in human and social capital on the part of the firm and co-workers, as well as to the reallocation of time by older workers themselves.

While a small body of literature on endogenous aging already exists, it mostly focuses on the specific case of workers' training participation (Picchio and van Ours 2011, Stenberg et al. 2012, Staubli and Zweimüller 2013, Brunello and Comi 2015). An exception is the study of Bertoni et al. (2016), which investigates how an increase in the expected years before retirement due to a change in the Italian early retirement plans affects health-related activities, such as smoking, drinking alcohol, and dietary habits, as well as physical exercises.

In this paper, we take a broader perspective and contribute to the literature in three ways. First, we replicate the effect of a pension reform on training participation and consider a second measure for firms' investments in the productive capacity of older workers. Second, we adopt the perspective of the older employees' co-workers and analyze how their incentives to invest in good relations with the older workers are affected by the pension reform. Third, we investigate how a longer working horizon influences the older workers' time allocation—away from leisure and toward working time. This threefold perspective offers new insights on the endogeneity of productivity before retirement.

The remainder of this paper is organized as follows: Section 2 analyzes the theoretical framework and outlines the underlying mechanisms of the three perspectives. Section 3 provides more information on the institutional background of our experiment. Section 4 focuses on the identification of the RDD, our econometric approach, the dataset, and some descriptive statistics. Section 5 presents estimates from the three perspectives and discusses the results. Sensitivity analyses are provided in Section 6. Finally, conclusions are drawn in Section 7.

³The first step of the reform involved women born before December 31, 1938, working until their 62nd birthday, and women born between January 1, 1939 and December 31, 1941, working until their 63rd birthday. As discussed later, this first step is left out in this paper.

2 Theoretical Framework and Empirical Evidence

Generally, the return on investments depends on how long they can be recouped. This simple idea was first applied to human capital investment by Becker (1964) and Ben-Porath (1967). Thus, younger workers receive a higher present value of net benefits from training than their older counterparts. Astonishingly, the idea had been transferred to the link between the aging population and fixed retirement institutions only decades later (Eichenberger 2002). We hypothesize that older workers' productivity depends not only on physical age but also, to an important extent, on the expected time remaining until their retirement. While a small number of academic publications have tackled the issue, it is still neglected in public and political discourses.

To test the hypothesis, we study a pension reform in Switzerland. We expect to observe a change in investment incentives due to the longer working horizon T after the pension reform in Switzerland. By “*investments*”, we mean all costly actions (input and effort) taken to make older workers more productive and more involved in work. In this environment, we focus explicitly on investment in human and social capital and time reallocation to work. We therefore examine three relevant actors for whom the pension reform affects incentives: the firms, the professional networks (i.e., co-workers), and the older workers themselves. In the following paragraphs, we study several channels through which these three types of agents take action to invest in older workers.

First, firms invest in the human capital of their workers. To date, only a few papers have dealt with changes in human capital investments in older workers. All these authors observe training participation and how it varies due to policy reforms that affect the expected retirement age. To our best knowledge, we are the first to investigate the direct effects of an increase in the SRA. So far, previous research could only examine indirect effects through early retirement schemes or other policy reforms (e.g., tax reforms). Less generous early retirement schemes incentivize older workers to stay in the labor market and accept training offers from their employers (Lau and Poutvaara 2006, Fourage and Schils 2009, Brunello and Comi 2015). Other empirical studies focus on related questions. Montizaan et al. (2010) investigate how abandoning a favorable tax treatment for early retirement in the Netherlands decreases pensions, thus raising the expected retirement age and hence training participation. Messe and Rouland (2012) concentrate on how the introduction of a layoff tax in France, which should be paid by firms that lay off older workers (i.e., over 50 years old), strengthens the incentives to

keep them employed and raises the training incidence of the 45–49 age group. Leuven and Oosterbeek (2004) find that a tax deduction for firms that train workers older than 40 in the Netherlands increases this group of workers’ training participation by 15–20%. Brunello and Comi (2015) examine how Italy’s early retirement reform changes the expected working horizon and hence increases training participation. Moreover, greater human capital accumulation through training incidence has positive effects on employability (Picchio and van Ours 2011, 2013, Stenberg et al. 2012), job satisfaction, and productivity (Leppel et al. 2012).

Another strand of the literature provides evidence that increasing the SRA affects human capital through not only formal training participation but also informal on-the-job learning (Marsick and Watkins 1990, Machin and Vignoles 2004). In this context, the firm sets the environment that facilitates the conditions for informal learning (Eraut 2004, Tynjälä 2008, Kyndt et al. 2009). This informal learning can be perceived as a result of job involvement, which increases work intensity (Edwards 1998, Manuti et al. 2015) and results in higher productivity. To our best knowledge, informal learning has never been linked to the endogeneity of aging. We measure informal learning by higher work intensity and expect a longer working horizon to increase investments in human capital I_{HC} (i.e., training participation and the workplace learning process; see Figure 1).

Second, from the co-worker perspective, we expect higher investments in social capital⁴ I_{SC} . To our best knowledge, in the literature, the concept of social capital has not yet been linked to decisions of older workers and pension reforms. Following Burt’s (1992) study, we understand social capital as general contacts (e.g., friends or colleagues) and social institutions (e.g., networks and ties) that enhance individual workers’ opportunities to fruitfully interact with other employees in making outcomes more efficient. We limit our analysis to the interaction between older employees and their co-workers. As their main advantage, such social ties allow access to a knowledge pool, thus making information less costly and more easily available for individuals (Harhoff et al. 2013). Huang (2015) suggests professional networks as means to enhance performance and productivity. For the colleagues of older workers, engaging in social ties is an appealing strategy to support the creation of the latter’s social capital. Investing in an older worker makes it easier to benefit from his or her network’s

⁴Social capital has emerged as a bridge between many disciplines and has been discussed, using different meanings. For details, see Coleman (1988), Burt (1992), Putnam (1993, 2000), Narayan and Prichett (1999), Dasgupta (2002), and Dinda (2008).

information pool. In turn, this situation improves the worker's productivity and enhances the whole network's efficiency and productivity. Moreover, older workers typically have accumulated larger human and social capital endowments over their lifetimes than their younger counterparts. Strengthening ties with older employees thereby improves the network's overall level of skill and experience. Younger colleagues' incentives for doing so increases in the older employees' work horizon. Thus, we expect investments (e.g., support) in older workers to have larger returns, following the pension reform. Consequently, this result increases the social capital of both the worker and the co-worker (I_{SC} increases). The longer work horizon makes investments more beneficial without changing the costs. Such investments are mirrored in the support that older workers receive from colleagues. Other empirical studies propose that support at work has a positive effect on job performance (Abualrub 2004) and job satisfaction (Cortese et al. 2010, Hombrados-Mendieta and Cosano-Rivas 2013). Consequently, this outcome leads to higher productivity levels for the professional network pool and the older workers.

Third, a worker can increase his or her job-specific human capital and productivity by reallocating time from leisure to work. Some studies link human capital investment to the concept of time allocation (Gronau 1976, Juster and Stafford 1991, Anxo et al. 2007, Fougère et al. 2009). According to this approach, time is allocated either to market activities or non-market activities (e.g., leisure, non-paid work, and sleep). Since time is scarce, there is a tradeoff between market and non-market activities. Raising the SRA gears older workers to allocate more time to market activities, thus raising their productivity and involvement in work. At least three mechanisms are at play in this case. First, a longer working horizon raises the expected future earning capacity,⁵ which translates to greater incentives to work. Second, the increase in the SRA is equivalent to a pension cut for retirement at the former retirement age (which would then count as early retirement), which makes it more expensive to retire early. Third, the closer a worker reaches the retirement age, the stronger are the incentives to substitute market time by non-market time (Anxo et al. 2007). An increase in the working horizon therefore results in higher market time (i.e., work) of older workers. We expect to observe a decrease in leisure activities.⁶ Among the different

⁵ This case is true only if the income difference between employment and retirement is high (Jaag et al. 2010, Jaag 2012).

⁶ We expect to observe a decrease in leisure activities and hence an indirect effect of lower market time. Measuring market time is challenging for several reasons. First, contractual work hours can only be increased if the firm has enough capacity, so this measure does not reflect the worker's effective higher involvement. Second, the number of effective work hours depends on at

types of possible leisure activities, we anticipate everyday activities to be affected the most (i.e., physical and social activities, such as eating out or meeting friends, are more direct substitutes to work than weekend activities, such as sports events, visits to the cinema, or do-it-yourself projects).

<Figure 1 here>

Figure 1 illustrates the theoretical framework of the three dimensions. An increase in the SRA raises time horizon T for women born after January 1, 1942. An extreme case is most illustrative. Woman A and Woman B were born on January 1, 1942 and December 31, 1941, respectively (i.e., B is one day older than A). Before the pension reform, both had the same working horizon ($T - 1$), as they would retire on their 63rd birthday, which differed only by one day. After the reform, which affects A but not B, A's expected time in the labor market increases by one year, up to T . Even with both women at the same age (e.g., 61), the firm that they work for has stronger incentives to invest in the human capital of A than of B ($I_{HC1942} > I_{HC1941}$), their co-workers have stronger incentives to invest more in the social capital of A than of B ($I_{SC1942} > I_{SC1941}$), and A has stronger incentives than B to reallocate her time from leisure to work ($I_{TR1942} > I_{TR1941}$).

3 Institutional Background

3.1 Swiss Pension System

The Swiss pension system consists of three complementary pillars.⁷ The first pillar, known as Federal Old-age, Survivors' and Invalidity Insurance, is a public PAYG system with a redistributive character. It is meant to cover the individual's basic needs after retirement. The second pillar is an occupational pension scheme. Together with the first pillar, it usually ensures that the retiree receives at least 60% of his or her last income before retirement. The third pillar is a privately funded tax-privileged pension plan. The worker can individually choose how much to contribute to this plan, which is intended to ensure that pensioners can enjoy the same standard of living as they did before retirement.

least two other factors: the firms' frictions, that is, capacities (Feather and Shaw 2000) and early retirement schemes. Consequently, we expect work hours to have contrasting effects and to not truly reflect the employee's involvement in work. Hence, an increase in market time has an ambiguous effect. For this reason, we approximate the reallocation of time through leisure activities.

⁷ For further information, see the Federal Social Insurance Office, Switzerland, <https://www.bsv.admin.ch>.

The first pillar is mandatory for everyone,⁸ including students and the self-employed. Those who are younger than the Swiss SRA but do not work have to pay a fixed minimum contribution. The second pillar is compulsory for all salaried workers whose incomes exceed a certain threshold. The third, private funds, are optional for everyone. Since the first pillar is the only one based on a PAYG system, it is the most affected by changes in the population structure⁹ (Jaag 2012). The Swiss SRA, at which point individuals become eligible for their public and occupational pensions, was set at 65 for men and 62 for women between 1964 and 2000.

3.2 *The Reform of Pension Insurance in Switzerland*

In the 1995 national referendum,¹⁰ the Swiss population accepted the 10th revision of the first pillar of the pension insurance scheme. This revision intended to align the SRA of women and men. The adjustment took place in two main steps. The SRA for women was increased from 62 to 63 in 2001 and from 63 to 64 in 2005. Thus, women born between 1939 and 1941 are only affected by the first increase and have to work until their 63rd birthday. In contrast, women born after January 1, 1942 retire at 64. A normal full-benefit level is only attained by an individual who retires at the SRA. Early or late retirement causes pension cuts or increases, respectively (Jaag et al. 2010). This radical change is smoothed by a transition in the early retirement scheme, which is another reform within the 10th revision. For early retirement, birth cohorts from 1939 to 1947 are subject to a pension cut of 3.4% per year, whereas men and women born in 1948 or later are subject to a cut of 6.8% per year. This amount is deducted from pensions each year (i.e., for lifetime) and not only restricted to the years of early retirement. Moreover, retiring before the SRA does not release the individual from the obligation to contribute the yearly minimal amount to the first pillar. Lifetime pension cuts, combined with the financial burden of contributing, thus make it expensive to retire early. The occupational pillar is less homogeneously organized. While pension funds offer different eligibility and payout options, the retirement age is generally the

⁸ All of the retirees are covered by the first pillar, 68% are covered by the second pillar, and only 28% are covered by the third pillar (see the Swiss Federal Office of Statistics <https://www.bfs.admin.ch>).

⁹ In other words, today's working population contributes to the pensions of today's retirees. Modifying the ratio of the working population to retirees has crucial effects on the persistence and soundness of the public pension fund.

¹⁰ The legal text can be found in 831.10 Bundesgesetz über die Alters- und Hinterlassenenversicherung (AHVG), i.e. the Federal Law on Old-age, Survivors' and Invalidity Insurance.

same as in the first pillar. Therefore, it also changes due to the reform.¹¹ However, early retirement is possible at the age of 58, with actuarially fair adjustments (Lalive and Staubli 2014). Again, it is not appealing to retire early from a financial perspective.¹²

Late retirement is generally possible in Switzerland. An individual can either postpone receiving the pensions or can continue working while being officially retired and receiving pensions. This is unappealing for tax reasons (the labor income and pension are added and subject to highly progressive income taxes) and is also problematic just because of endogenous aging. Postponing retirement has occurred both before and after the reform. A linear adjustment leads to a maximal pension of 131.5% of the full pension, corresponding to a maximal delay of five extra years after the SRA, both before and after the reform. Women born before 1939 reach the maximal pension of late retirement at the age of 67, whereas women born between 1939 and 1941 reach this threshold at 68, and women born in 1942 or later do so at 69. Since all adjustments are linear, we do not expect the reform to change the incentives to delay retirement.¹³

3.3 Effectiveness and Anticipation of the Pension Reform

Figure 2, panel (a) shows the adjustment of the SRA for women, and panel (b) illustrates the change in the mean of the *de facto* retirement age in Switzerland. Comparing both panels reveals the effectiveness of this reform. For the second reform step (the SRA from 63 to 64), the mean *de facto* retirement age adapts by one year (compared to seven months for the increase from 62 to 63). Figure 2, panel (c) shows the *de facto* delay in the retirement age by monthly activity. The probability of working decreases with every year of aging but the reform increases the time left before retirement. The example of women aged 60 shows that they are working longer. Women born in 1941 exhibit a 55% probability to work at age 60 (jan01). Women born in 1942 demonstrate a 68% probability for the same measure (jan02) and 61% at age 61

¹¹ For this reason, we expect no systematic effect of the second pillar on our results. Moreover, the first pillar is the most important one. For the median yearly Swiss income of 77,160 CHF, the first pillar replaces half of the 80% from the previous yearly working income in the five years after retirement (see the Federal Social Insurance Office, Switzerland, www.bvs.admin.ch). The rest is covered by the second and the third pillars. The lower the income is, the higher is the replacement rate of the first pillar.

¹² The reform also eliminates the supplementary pensions for women born in 1942 or later. Following the study of Lalive and Staubli (2014), we find no evidence that this change influences the analysis. Moreover, the reform changes the way that the joint pension of a married couple is calculated, but this does not affect our calculations.

¹³ However, after the reform, it is less beneficial for women to retire at the age of 67, rather than before, since these women in the treatment group have not yet reached the maximal pension.

(jan03). Thus, three years before their SRA, the probability of working lies at 55–61% in both cohorts.

<Figure 2 here>

These results are in line with those of Lalive and Staubli (2014), who find statistically significant effects of the female labor supply after the Swiss pension reform. When comparing women born in 1941 and 1942 and the increase in the SRA from 63 to 64, their findings suggest a 10–10.9 (depending on the bandwidths away from the cutoff) percentage point higher labor supply at age 62, a 31–32 percentage point higher labor supply at age 63, and a 4.5–5.4 percentage point at age 64. Figure 2, panel (d) presents an overview on (i) the ages of the two cohorts in the years used in our sample, (ii) the two cohorts' retirement ages and years, and (iii) the probability of full-time or part-time work at different ages, using the monthly activity calendar.¹⁴ Similar to the results of Lalive and Staubli (2014), we find the younger cohort's higher probability to work at every age. However, the effect does not seem to be highest at exactly the former SRA (63) but rather in the years before.

It is interesting to consider the full information and anticipation effects. The reform was enacted in 1997; the second increase took place in 2005. Thus, it can be assumed that women were fully informed, at least during the second step. Three observations support this assumption. First, the enactment of the reform in 1997 was the result of an intense public debate, starting in the late 1980s, and a national referendum in 1995. This case implies at least an eight-year time span. Second, the first reform step raised the SRA in 2001, when older women and firms had already adapted their behavior (e.g., as observed in the labor supply). Finally, the election results¹⁵ suggest women's awareness of the consequences. While at the national level, 60.7% of the voters favored the reform, only 52.9% (50.6%) of the treated women supported the vote if affected by the first (second) increase. Contrasting this vote to that of older women born before 1939 (i.e., remaining at the former SRA), the yes share of treated women was 20.6 (22.9) percentage points lower for the first increase (the second increase, respectively). Thus, the reform seemed to have been anticipated. Consequently, we have to expect anticipation effects also in investment behavior. Figure 3 shows the adjustment of the investment with a higher retirement age. A steady decrease leads to

¹⁴ Source: Swiss Household Panel, which starts at the end of 1999; therefore, we start the comparison at age 59.

¹⁵ Source: Own calculations based on VOX Analysis, a survey conducted after each federal referendum to know more about the outcomes of the votes.

lowest investments at the retirement age. The pension reform shifts the investment curve to the right, which results in higher investment at all ages (e.g., age 61). Hence, we expect an investment decision not only at a single point in time but in a continuous way. In the following section, we deal with anticipation in two ways: First, we estimate the overall effect at each specific age in the dataset (see further details in Section 5). Second, we estimate the effect of each year of aging to obtain knowledge about where the impact is highest. Since we cannot cover all the anticipation going back to the early 1990s when the discussions started, our effect can be perceived as a lower bound.

<Figure 3 here>

4 Empirical Strategy

4.1 Identification: A Sharp Regression Discontinuity Design (RDD)

Now we econometrically examine how the increase in women’s retirement age changes incentives to invest in human and social capital and to reallocate time from leisure to work. As outlined in the previous section, the 1997 reform induces women to work longer. The structure of the retirement age-dependent investments is discontinuous for the birth years 1938 and 1939, as well as for 1941 and 1942. The fact that the new law creates a discontinuity at two ages allows for an RDD (Campbell 1969, Lee and Lemieux 2009). Simply being born in 1939 (or 1942) rather than 1938 (or 1941) completely determines whether a worker is qualified to be included in the treatment group (i.e., being subject to a longer time before retirement). In turn, this inclusion affects the probability of a worker being subject to higher investments by her firm and co-workers. In this sharp RDD, assignment to the treatment group depends, in a deterministic way, on variable d_i (date of birth), with the known discontinuity cutoff point \bar{d} . Since the SRA has increased in two consecutive steps, we can define two cutoff points: the first corresponding to having been born after January 1, 1939 and the second to having been born after January 1, 1942. In this paper, we consider only the second point for two reasons: 1) a better database is available, and 2) after the first increase, women were fully aware of the reform. Following the study of Hahn et al. (2001), we use D_1 as an indicator for the assignment to the treatment, where \bar{d} is the cutoff. The assignment rule is such that:

$$D_1 = \begin{cases} 1 & \text{if } d_i \geq \bar{d} = \text{Jan 1, 1942} \\ 0 & \text{otherwise.} \end{cases}$$

In the following sections, we estimate a nonparametric approach of a local linear regression, as well as a parametric approach of a linear ordinary least squared (OLS) regression. Comparing these approaches allows us to combine the advantages of both methods and provides us with a more complete picture of the setting. While the parametric approach searches for the optimal functional form for a given dataset, the local linear regression focuses on the optimal data range within which a linear regression is estimated consistently (Jacob and Zhu 2012). Both methods limit the analysis to observations that lie in the neighborhood of the cutoff (Lee and Lemieux 2009). Moreover, both approaches allow us to vary the bandwidth and use the polynomial order for robustness checks. The model is estimated by using the following equation:

$$y_i = \alpha + \tau D_i + \beta_0 f(\text{Age}_i - \bar{d}) + \beta_i X_i + \varepsilon_i, \quad (1)$$

where $f(\text{Age}_i - \bar{d})$ is a function of the centered age in months from the cutoff, and τ is the treatment effect of interest, which measures the change in the outcome variable y_i due to a higher retirement age. Table A-1 provides an overview of the following six outcome variables: training participation, intensity of work, emotional support from co-workers, practical support from co-workers, amount of physical activity, and amount of activity related to going out and meeting friends. In Equation 1, X_i is a set of control variables, including age, health status, education, marital status, part-time work, firm affiliation measured as job and firm changes, job position, log income, and early retirement.

Figure 4 shows the distance from the cutoff in months on the horizontal axis and the six outcome variables used in the RDD on the vertical axis. All figures provide the fitted values reported from the linear regression model on both sides of the cutoff¹⁶. The visual analysis suggests a discontinuity at the cutoff point. In Figure 4, panels (a)–(d), the jump is positive. For panels (e) and (f), the jump is negative. Both observations fit our hypothesis developed in Section 2. For the treated women, we expect higher investments in human and social capital and a reallocation of time toward work. Near the cutoff, there seems to be a marked jump, allowing us to conclude that the effect is most prominent just around the cutoff point. The further we move away from the cutoff, the more forceful the confounding factors become and the less comparable the

¹⁶ The local polynomial fit is provided in Figure A-1.

individuals are. The visual overview also permits us to identify that for some variables (e.g., physical activity), the discontinuity is small at the cutoff level. However, generally, the graphical representation supports our hypothesis for all three dimensions. It could be argued that the variance is quite high and the only reason for the effects observed. However, the covariates in Figures A-2 and A-3 also demonstrate high variance and spreading but no discontinuity. These findings support the validity of our hypothesis.

<Figure 4 here>

4.2 *Data and Sample*

To answer our research question, we need detailed information about individuals and their workplace environments. Therefore, we rely on the analysis of two different, pooled cross-section datasets: the Swiss Labor Force Survey (SLFS) and the survey of the Swiss Household Panel (SHP). Both provide detailed information about individuals, information on their work environments, and some background characteristics, which we use for further analysis. In our sample, we assess women born in 1941 and 1942 who are still working (at least 10%), which involves observing them at age 56 (or 55) up to their SRA at 63 (or 64).

The SLFS is based on an individual questionnaire targeting permanent residents of Switzerland of all working or non-working statuses. Starting in 1991, the survey covered about 0.3% of the respective Swiss populations (about 16,000 interviews) up to 2001 and about 0.8% since 2002 (about 35,000 interviews). The sample is randomly generated, using information from public registers. We restrict the analysis to the time period between the reform and the SRA of the youngest cohort (i.e., 1997 to 2006¹⁷). Moreover, we focus on employees who work at least 10% of full-time work hours. These employed persons are asked detailed questions concerning their work status, specific aspects of their work contracts, work schedule arrangements, educational backgrounds, and training participation. The sample consists of 1,467 and 2,379 individuals born in 1941 and 1942, respectively¹⁸.

The SHP complements the SLFS and provides more information regarding leisure activities, work intensity and job involvement, professional relationships, and support at

¹⁷ Workers born in 1942 retired in 2006 at age 64. However, we alter the bandwidth around the cutoff by up to 18 months, thereby including some months of the birth year 1943. We thus also include some observations from 2007.

¹⁸ The exact loss of observations due to restrictions in the sample and missing variables is shown in Table A-2.

work. The SHP data collection began in 1999 with 13,000 interviews in approximately 5,000 households. We again use pooled cross-section data from 1999 to 2006, when the younger cohort reached the SRA. By using the proper sample weights, the data are representative of the whole Swiss population. Over the years, the sample size changes for each outcome variable, since we have missing values in some variables or some years of the cross-section data. This SHP dataset is also smaller than that of the SLFS for three reasons. The former consists of fewer interviews per year, has fewer years of observation since its data collection started later, and covers not only the labor force but also women who are not in paid employment.

4.3 *Validity of RDD*

The validity of the RDD relies on four conditions (Lee and Lemieux 2009). First, subjects must not be able to manipulate the assignment variable. In our context, the treatment status is randomly assigned to individuals, depending on their dates of birth.¹⁹ Hence, we can safely use the variability in the exposure to treatment across cohorts to identify causal effects. Second, randomization around the cutoff is necessary. To avoid seasonality, we have chosen different bandwidths, according to which our main estimate covers one year on each side.²⁰ McCrary (2008) offers a formal test to identify manipulations at the cutoff point. For the RDD to be valid, we should not observe any discontinuity in the density of the forcing variable at the cutoff. Figure 5 shows the densities for the two datasets 24 months from the cutoff on each side as well as the McCrary test, both suggesting validity.

<Figure 5 here>

Third, there must not be any other discontinuity around the cutoff. To our best knowledge, this requirement is also fulfilled. Fourth, individuals on both sides of the cutoff should be similar and should possess similar characteristics.²¹ Thus, it is important that women in the control and the treatment groups have similar background

¹⁹ However, the two cohorts could differ systematically (e.g., they had different schooling or education on any underlying reform), just because they were born in different years. However, two arguments can be presented against this statement. First, a possible school reform would concern all children, one cohort just one year longer than the other. Second, the Swiss constitution gives the sub-governments (i.e., cantons) full authority over the school organization. Only a school reform that was enacted differently for the two cohorts could cause another discontinuity at the cutoff. However, this case is highly improbable. Thus, we can exclude a systematic difference due to different school entries.

²⁰ This means 1941 and 1942 as the respective years of birth. We do not observe a higher probability of being born in one of the years.

²¹ In the optimal case, we observe women born on December 31, 1941 and women born on January 1, 1942. There is no reason to believe that they are different in characteristics other than the discontinuity.

characteristics and that jumps occur only in the outcome variables. Table A-3 shows the descriptive statistics. Indeed, differences between the control and the treatment groups are relatively small and for most control variables, statistically insignificant.²² The outcome variables show statistically significant differences between the two groups. Section 6.1 provides further tests of the validity of the design regarding the covariates. Having confirmed these conditions, we propose the validity of the RDD.²³

5 Estimation Results

5.1 Non-parametric Regression Results

Table 1 shows an overview of the local polynomial regressions where the coefficients reflect the main effects of the increase in women’s SRA on the incentives to invest in human capital, social capital, and the allocation of time. Columns (1) and (2) show the results from the firm dimension, columns (3) and (4) from the co-worker side, and columns (5) and (6) from the worker dimension. In each row, different bandwidths are selected. The second row shows the narrowest distance around the cutoff. When choosing the optimal bandwidth, there is a tradeoff between bias and precision. The farther away from the cutoff, the more influential are the confounding factors and the less we can be sure that the individuals on both sides of the cutoff are comparable. Hence, estimates with large bandwidths can be biased. Nevertheless, a smaller bandwidth around the cutoff would be more imprecise, since we have fewer observations (Imbens and Lemieux 2008), which are moreover susceptible to the seasonality of birth cohorts (Banks and Mozzonna 2012). For this reason, we prefer the estimates with 12 months on each side of the cutoff (i.e., each birth year in its entirety, 1941 and 1942) but vary the bandwidths as robustness checks. The first row shows the optimal estimated bandwidth by Calonico, Cattaneo and Titiunik (CCT),²⁴ which changes for each outcome variable. It is often selected at around nine months. Table A-4 shows the higher polynomial orders (2nd, 3rd, and 4th) for the main 12-month bandwidth. This allows for more flexibility of the underlying functional form. The effects are robust in terms of signs.

<Table 1 here>

²² To overcome the problems of significant mean differences, we perform a further validity test of the covariates, as discussed in Section 6.1.

²³ Lalive and Staubli (2014) also suggest the validity of this RDD.

²⁴ Calonico et al. (2014) discuss the exact derivation of the bias-corrected CCT estimator.

Column (1) of Table 1 shows the effect of the increase in the SRA on women’s training participation. The effect size is between 5% and 14%, consistent with the results found by Brunello and Comi (2015) but slightly higher than those of other studies. Brunello and Comi (2015) estimate that an increase in the expected retirement age by one year increases training participation by 7–11%. Montizaan et al. (2010) find an effect of 1.3% when estimating the effect of the remaining time before the expected retirement on training incidence. We find significant evidence for the other variable from the firm’s perspective (i.e., the intensity at work), which would support the hypothesis regarding higher involvement in work (column [2]) though only in the nine-month bandwidth. Columns (3) and (4) examine the effects of the reform on investments in social capital. Indeed, our results suggest that the women in the treatment group receive greater emotional and practical support, implying that their colleagues interact with them more intensely. These effects are important in terms of size. Emotional support and practical support grow by 0.6–2.1 points on a 10-point scale compared to the control group. These findings suggest that the Swiss pension reform leads to greater social capital investments. The worker perspective shows some evidence for time reallocation. We find negative signs for the two leisure variables in columns (5) and (6), representing a decrease in physical activities and going-out activities. Physical activities decline by one per week for the 12-month bandwidth.

It could be argued that the probability of receiving any investments from the firm or co-workers and of reallocating time to work is a function of age. To approach the question of whether the investments in the two birth cohorts are different for each age group, we estimate fixed age coefficients. Figure 6 shows the age profile of the coefficients originating from the local linear regression models. A positive coefficient means higher investments in the treated cohort, whereas a negative coefficient implies higher investments in the control group. Training participation is higher for women born in 1942 for all age profiles except 56, for which we do not observe any difference. Intensity at work is higher for the treatment group at age 63.²⁵ The women in the treatment group benefit from the advantage of higher investments in social capital. It could be argued that in the year just before retirement, investments decline significantly. Interestingly, the difference seems to be highest at age 62, the penultimate year before retirement. Thus, we can exclude the conclusion that our results are driven by a last-

²⁵ Note that in this case, our comparison uses only the age of 63 because the data collection for this variable started in 2004.

term effect, that is, women are treated differently, not because they have different time horizons with respect to work, but just because they are in their last working year as the 1941 cohort is at age 63. Moreover, emotional support and practical support are greater in the treatment group for all ages. The workers seem to reallocate their time to work but only at age 60 and older.²⁶

<Figure 6 here>

5.2 Parametric Regression Results

Adding personal and job characteristics to the estimation model helps us gain a better understanding of the mechanisms at work and analyze whether the expected time before retirement, rather than physical age, drives the effects. In approaching this issue, we include the age measured in months and other background characteristics, in addition to the simple treatment dummy. In detail, we estimate linear models for all continuous outcome variables (columns 2 to 6) and a probit model for the dummy variable (column 1). Table 2 shows the results for a 12-month bandwidth on each side of the cutoff. The standard errors are clustered at the individual level to account for unobserved heterogeneity, and year dummies are included to correct for year-specific effects. Overall, estimations are robust across the different outcome variables. Belonging to the treatment group increases the investment incentives for the firm, co-workers, and the worker herself. Even after inclusion of the age control variable, the treatment dummy is still significant.

<Table 2 here>

From the firm's side, both training incidence (column 1) and job intensity (column 2) are higher for the subjects in the treatment group. The size of the training incidence coefficient cannot be directly interpreted due to the probit specification. Job involvement as measured by intensity²⁷ is 2.7 points on a 10-point scale, higher subjects in the treatment group. Emotional support and practical support, shown in columns (3) and (4), also increase for the women in the treatment group. These results cohere with those from the local linear regressions. The size of the treatment effect indicates a 1.9-point and a 1.8-point increase for emotional and practical support, respectively on a 10-

²⁶ This result is in line with Bertoni et al.'s (2016) finding about the positive effects of the exercise behavior of younger workers (aged 40–49), being 8–20 years away from retirement age. The effect turns negative only a few years before the SRA.

²⁷ However, this intensity could also be measured as stress. It is unclear whether or not stress is a contradictory measure for job involvement. On one hand, stress is linked to more work and higher job connection and participation. On the other hand, stress is mitigated by greater support at work (Abualrub 2004). Moreover, the stress level at work might be expected to be reduced as an employee approaches retirement. We thereby conclude that the effect on stress is ambiguous.

point scale—somewhat higher for the 12-month bandwidth than in the non-parametric results. Regarding the worker side, the results are comparable with the non-parametric results. In column (5), the number of physical activities decreases by 1.6 times a week for the treatment group, holding all other factors constant. In column (6), other leisure activities, such as going out and dining in restaurants, decrease by 0.65 points on a 5-point scale compared to those of women born in 1941. Women with a longer working horizon are pulled away from leisure and pushed into work. Overall, these results are in line with the non-parametric estimations.

6 Robustness Checks

6.1 *Validity Tests of Covariates*

For further testing of RDD validity, we examine the covariates at the cutoff point. We use three different methods. First, we examine the covariates at the cutoff point graphically. Figure A-2 (Figure A-3, respectively) provides a graphical representation of the covariates in Panel A (Panel B, respectively) around the cutoff. These graphs show no discernible difference in most covariates at the cutoff. This conclusion holds true for both datasets. Second, as suggested by Lee and Lemieux (2009), we test the null hypothesis of discontinuities in all covariates simultaneously, estimated by a seemingly unrelated regression (SUR). Each column contains a different baseline covariate, shown in Table 3. The joint significance test is not statistically significant in either dataset, testing for the 1st and the 2nd polynomial order. Hence, we cannot reject the null hypothesis. This result suggests zero discontinuity in the covariates. The Appendix Table A-5–Table A-7) provides the higher polynomial orders and age-dependent SUR models, leading to the same result.

<Table 3 here>

Third, we test whether a systematic manipulation occurs at the cutoff.²⁸ We thus estimate the predicted log wage of women who are still working (and hence before retirement), according to monthly age cells, in a linear wage regression on a number of control variables. Figure 7 plots the results for Panels A and B. Both datasets suggest no manipulation around the cutoff. Upon finding no important differences at the cutoff in any of the three tests, we can conclude that the RDD is valid.

²⁸ This test is similar to that of Schmieider and Trenkle (2016).

<Figure 7 here>

6.2 Further Robustness Checks

Table A-8 shows the higher polynomial order of the estimations in Table 2, allowing for more flexibility of the underlying functional form. For simplicity, only the treatment effects are reported. The underlying model is the same as in Equation 1, but it is enriched by adding the 2nd, 3rd, and 4th higher orders. The effects are robust and of similar size. However, adding higher polynomials can lead to insignificant effects, especially including the 4th order. Table A-9 shows different bandwidths for the same estimates as those in Table 2. The third row has the 12-month bandwidth, which has already been estimated in Table 2 (without including higher polynomials). Generally, the results are comparable. Some effects become insignificant through a change in the bandwidth, but overall, the signs support our hypothesis of higher human capital, higher social capital investments, and a systematic reallocation of time. The more we approach the cutoff, the more pronounced are the effects. The subjects near the cutoff are most comparable, because being born at the end of 1941 or in the beginning of 1942 is random. However, closer to the cutoff, the number of observations declines, which could lead to imprecise estimates. Expanding the bandwidth could invoke two effects. First, the individuals on both sides of the cutoff are heterogeneous and differ in various covariates. This situation causes a bias in the estimates (i.e., smaller or insignificant effects). Second, the impact of the reform could be heterogeneous over the local linear effect (i.e., different sign of the effect) (van der Klaauw 2008). We expect the effect to be similar for all women in the treatment group. Thus, we interpret the smaller and sometimes insignificant effects when expanding the bandwidth as a result of heterogeneity among individuals. Accordingly, we assume the 12-month cutoff to be the most preferred. Moreover, with a whole year period, we can exclude seasonality. We conclude that including background control variables generates robust estimates. Regarding the size of the estimates, we suggest that the non-parametric estimator somewhat underestimates the effects.

As shown in Table A-10, we conduct placebo regression tests. Placebo I encompasses the men who are not affected by the pension reform. The comparison of men born in 1941 and 1942 shows a significant difference in the physical activities variable. However, physical activities are higher for men born in 1942, indicating an effect opposite to that in our main treatment group. This finding could be perceived as

an intra-household substitution effect, where men are also indirectly affected by the pension reform. Placebo II includes women born in 1943 and 1944, who are affected by the pension reform to the same extent. We find no significant effects in any variable. Both placebo regressions are consistent with our hypothesis and demonstrate the robustness of the results.

As shown in Table 4, we conduct some further specifications as robustness checks. Row (a) shows the main estimates from Table 2 for comparison. Furthermore, we provide estimates, without any control variables (b), of individual longitudinal weights (which ensure representativeness for the Swiss population) and (c) using household income instead of individual work income (g). Rows (d), (e), and (f) show subsamples of the preferred estimates. From the descriptive statistics in Table A-3, we see that technicians, as well as Italian-speaking individuals, are each underrepresented in one of the two cohorts. Therefore, we provide estimates with subsamples by leaving out Italian speakers (d) and technicians (e). The donut estimation examines the model, excluding December 1941 and January 1942. Controlling for household income seems to account for some parts of the treatment dummy for the firm dimension (g), where the treatment effect becomes insignificant. Taken together, all robustness checks support our hypothesis and leave the results qualitatively unchanged.

<Table 4 here>

6.3 *Are Early Retirements the Driving Force?*

Since the sample includes individuals retiring at their SRA and individuals retiring early, we might compare these two groups. The reform changes the incentives regarding early retirement in two ways. First, the pension reform offers the legal possibility of early retirement, but this remains costly since it is accompanied by a cut in the pension amount. Second, women born in 1942 might feel unfairly treated due to their birth year; thus, the higher work load is in some way random. It is unclear whether this situation provokes adverse effects.²⁹ Thus, it could be argued that the effects found so far do not reflect an incentive effect of an increase in the SRA but a selection effect, since those who work until the age of 64 exhibit higher work motivation. To address the potential threat of comparing two different groups, we conduct another robustness check

²⁹ Retiring early is accompanied by a pension cut. This means that retiring at the age of 62 incurs a 3.4% pension cut for women born in 1941 and 6.8% (2 x 3.4%) for women born in 1942. Hence, it is more expensive for the younger cohort to retire early. Retiring at the age of 63 incurs no pension cut for women born in 1941 and a pension cut of 3.4% for women born in 1942. Again, retiring early is thus more expensive for those born in 1942.

by computing an interaction effect of the treatment dummy and a dummy in terms of whether an individual retires early or at the SRA (*Early*). Specifically, we estimate the following model:

$$y_i = \alpha + \tau D_i + \beta_0 f(\text{Age}_i - \bar{d}) + \beta_1 \text{Early}_i + \beta_2 \text{Early}_i D_i + \beta_i X_i + \varepsilon_i. (2)$$

Table 5 shows the predictive marginal effect for both the control and the treatment groups (i.e., workers retiring early and workers retiring at the SRA). The statistically significant effects can be interpreted directly, holding the other covariates at means. We find strong evidence that the treatment group enjoys greater investments in terms of both human and social capital and a reallocation of time for both types of retirement. Our specific observations are as follows: The likelihood of participating in training is 8% (or 13%, respectively) higher for women born in 1942 than for the control group if they retire early (or at the SRA, respectively). Intensity seems to be higher in the treatment group if they retire early or at the SRA. Moreover, emotional support and practical support are both between one and two points higher for women in the treatment group. Physical activity decreases from 2.0 to 1.9 (in the treatment group) for those who retire early and decreases from almost 3.8 to 2.1 for those working up to the SRA. Hence, we observe a decrease in physical activities, again for early and statutorily retired women. The other leisure variable (i.e., going out) increases from 3.2 to 3.3 for women in the treatment group taking early retirement and decreases from 3.4 to 2.8 for those taking normal retirement. The results from the predictive margins suggest that the early retired and normally retired women experience higher investments in human and social capital and a reallocation of time away from leisure in the treatment group. However, effects seem to be more pronounced for women who retire at the SRA. Table A-11 shows similar results for the predictive marginal effect of the underlying interaction of women in blue-collar or white-collar jobs, as well as in management positions.³⁰ The effect seems to occur at all hierarchical levels. Table A-12 confirms the results for samples split into blue-collar versus white-collar workers. However, in the sample split, the effects seem to be more pronounced for those in blue-collar and management positions.

<Table 5 here>

³⁰ We cannot show estimates for all the jobs, because we lack enough observations. Therefore, we approximate this by estimating effects for women in blue-collar jobs, white-collar jobs, and management positions/academics.

6.4 Are Career Women the Driving Force?

Next, we turn our focus to another factor that may affect investments, that is, whether the worker generally focuses on her career rather than on family life, which again complicates the interpretation of our results. Therefore, we split our sample into married women (approximation for family life) and unmarried women (approximation for career focus). We suppose that unmarried women enjoyed higher investments than married women before the reform, as well as a greater increase in investments after the reform. Several reasons can be offered for this expectation. First, married women experience higher opportunity costs in terms of work and investments due to rearing children or taking care of grandchildren. Second, married women are often less appealing subjects for investments by firms and co-workers for the same reason. Similar to Table 5, Table 6 shows the predictive marginal effect of the underlying interaction between the dummy for marital status (mar) and the treatment dummy, as shown in the following equation:

$$y_i = \alpha + \tau D_i + \beta_0 f(Age_i - \bar{d}) + \beta_1 mar_i + \beta_2 mar_i D_i + \beta_i X_i + \varepsilon_i. \quad (3)$$

Again, the effects can be interpreted directly as predicted means for each category. Note that investments in the women belonging to the treatment group are higher for married and unmarried women. However, married women experience lower investments, both before and after the pension reform. This finding is in line with our predictions. The fact that being included in the treatment group increases the investments for both married and unmarried women strengthens our hypothesis. Moreover, it disproves the supposition that only career women constitute the driving force. Again, the sample split in Table A-12 confirms the results.

<Table 6 here>

7 Conclusion

This study sheds light on how an increase in the SRA changes the incentives to invest in older workers. General human capital theory predicts higher net returns on investments if the working horizon is longer. In this context, we suggest that older workers are not unproductive because of their physical age but due to a lack of investments resulting from a fixed and short working horizon. The latter is rooted in a fixed SRA. The 1997 pension reform in Switzerland constituted an exogenous shock,

increasing the retirement age for women born in 1942 from 63 to 64. Although this reform affects all women born after 1942, the effect is greater for older women because their working horizon increases disproportionately. We use this natural experiment to examine how investments in women affected by the increase (i.e., the 1942 cohort) differ from those in the control group (i.e., the 1941 cohort). In this context, we bridge the literature gap between the human capital theory and two other important strands of literature. The second factor (besides firms' investments in human capital) involves the social capital investments of the employees' co-workers. The policy reform implies the professional network's greater investments in older employees. The third factor is that rational older workers reallocate time from leisure to work. The combination of these three perspectives fosters our understanding of how an increase in the SRA contributes to the mitigation of the problem of demographical change.

Using two Swiss survey datasets, we show all three relevant agents' higher investments in the women belonging to the treatment group. Training participation increases by 5–14%, consistent with the findings of Montizaan et al. (2010) and Brunello and Comi (2015) when estimating the effects of the expected retirement age. Additionally, from a professional network perspective, greater investments are observed in social interactions and relationships with older workers, measured as support at work. Emotional support and practical support at work increases by one to two points on a 10-point scale. Finally, we observe the workers' reallocation of time away from leisure toward working time when affected by the pension reform. We show that these findings are true for both parametric and non-parametric approaches and that our results withstand various robustness checks.

For policymakers who envisage pension reforms, our results call for dedicating attention, not only to the firms' incentives to invest in the human capital of their employees, leading to human capital formation, but also to the professional network's incentives to invest in the social capital of older workers and the workers' incentives to reallocate their time. Moreover, our results pinpoint the second major advantage of an increase in the SRA. Besides its obvious and well-known effect of decreasing the ratio of retirees to the working population, it makes older workers more productive. This situation calls for a reassessment of the common belief that older workers are less productive due to their age. In the context of an aging population, an increase in the SRA is thus all the more desirable and required.

We identify at least three possible policy suggestions from this research. The first is an increase in the SRA. As in the case of Switzerland, this increase will likely lead to higher investments in older workers. However, since the working horizon is again fixed after the reform, a new albeit higher barrier is created, and productivity will decrease near the end of the working horizon, especially in the last few years before retirement. The second recommendation is a policy that increases the retirement age, combined with a flexible retirement age. This combination causes uncertainty in the exact timing of retirement; thus, older workers might be more productive in the period leading up to their (unspecified) retirement. However, this approach presumes credibility. Third, politics might foster the lucrativeness of voluntarily prolonging individual careers. Most notably, to make people voluntarily work longer, there is no need for subsidies and expensive incentive programs. It is simply required that today's hindrances to working beyond the SRA be abolished or at least decreased. Thus, we propose that income taxes and social security contributions from the labor income of pensioners should be cut in half. This benefit would incentivize them to work beyond retirement, thus making them richer, making society wealthier as it would gain additional tax income, and gearing all actors involved to invest in the human and social capital of older workers.

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Figure and Tables

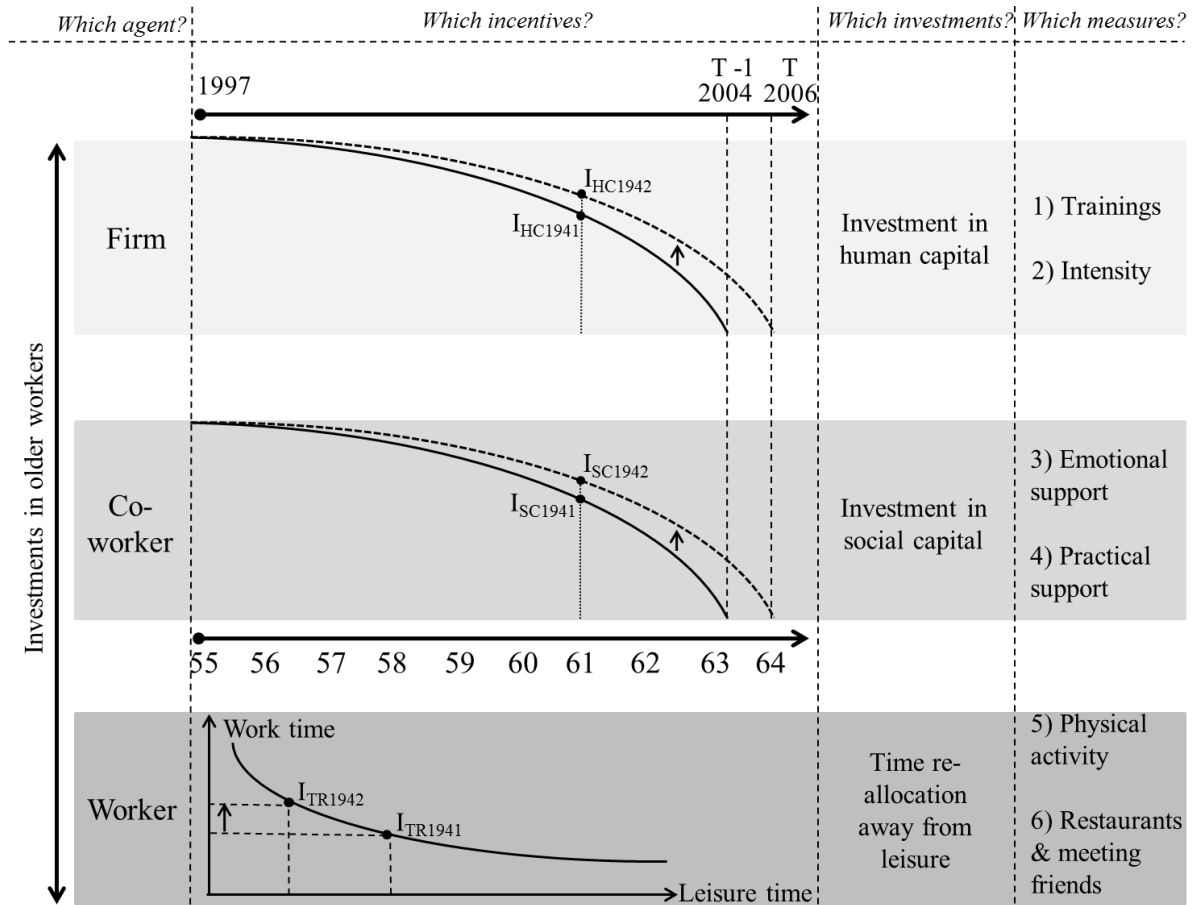


Figure 1: Theoretical framework of investments in older workers from the firm, the co-workers and the older worker

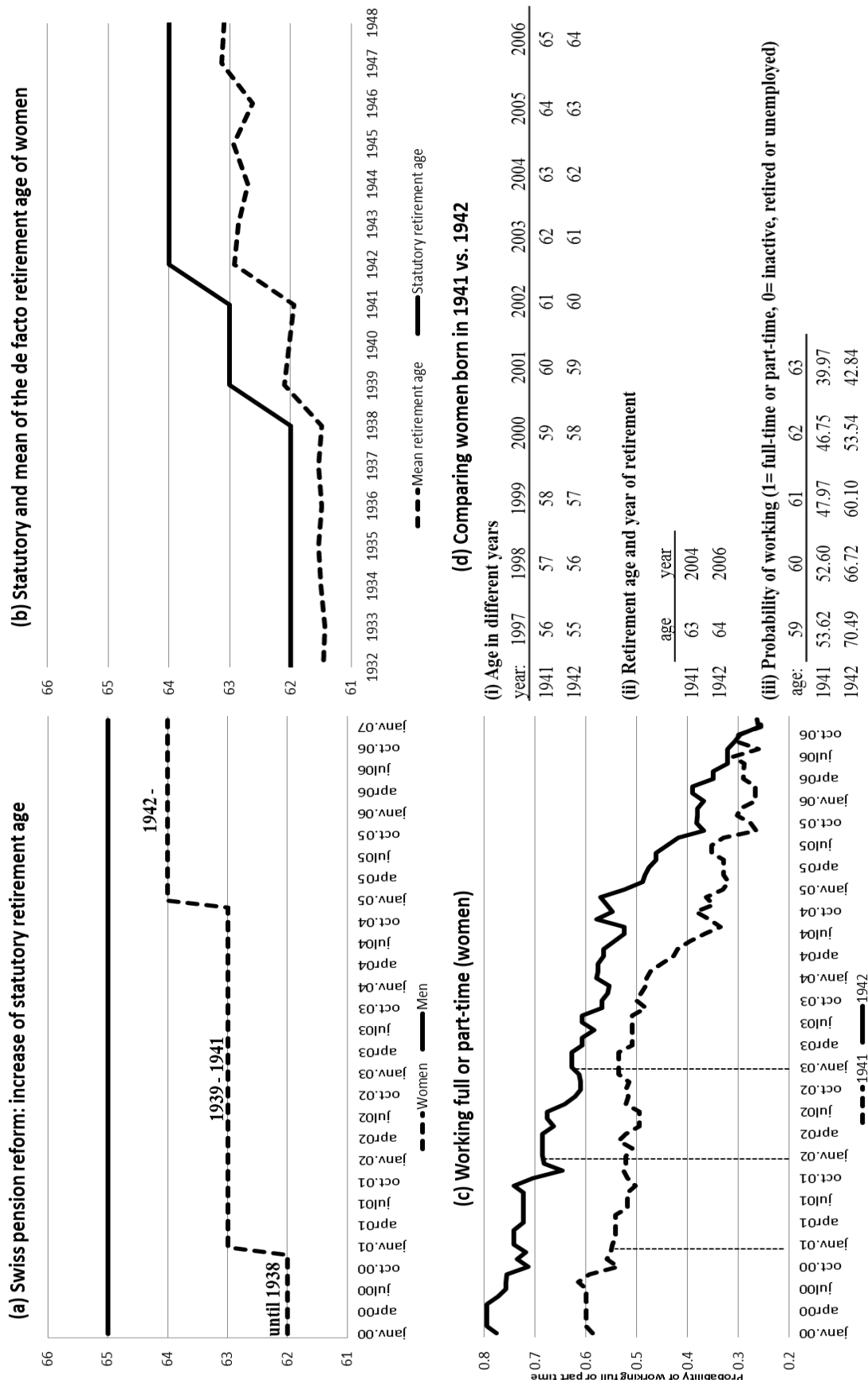


Figure 2 : Swiss pension reform (a), statutory and mean of the de facto retirement age (b), monthly activity calendar: probability of working full or part-time (c) and comparing women born in 1941 vs. 1942 (d).

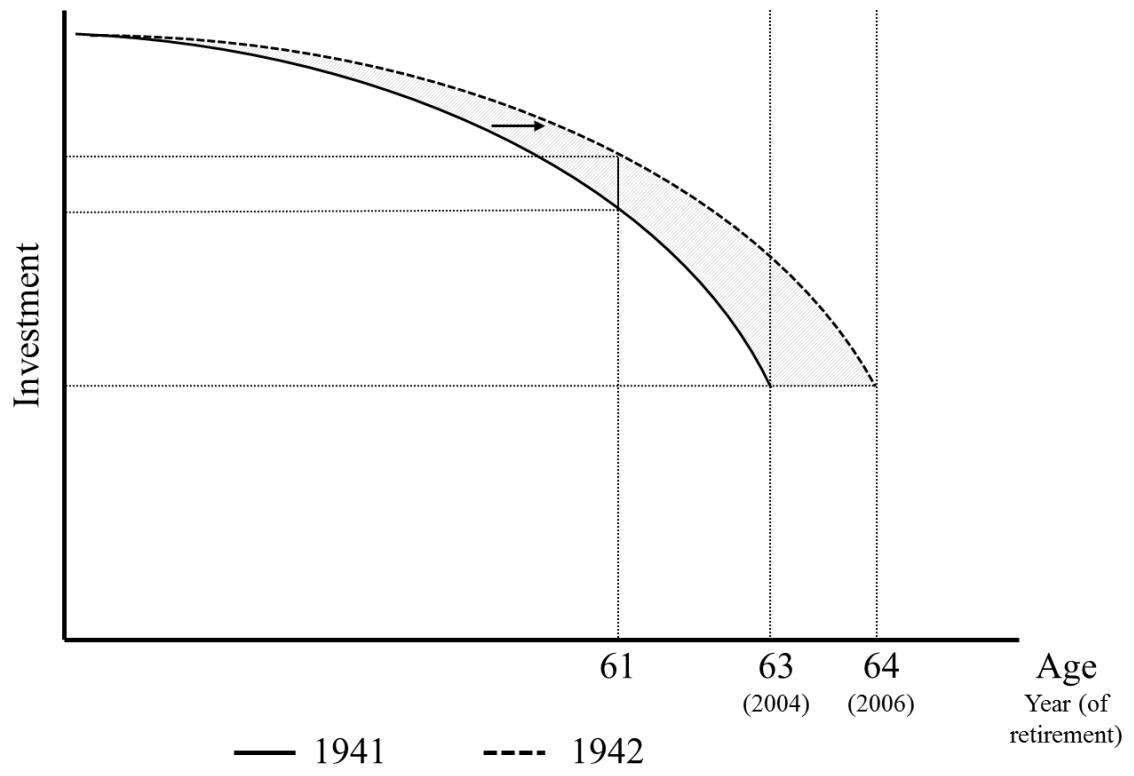


Figure 3: Anticipation effects and (expected) investment behavior

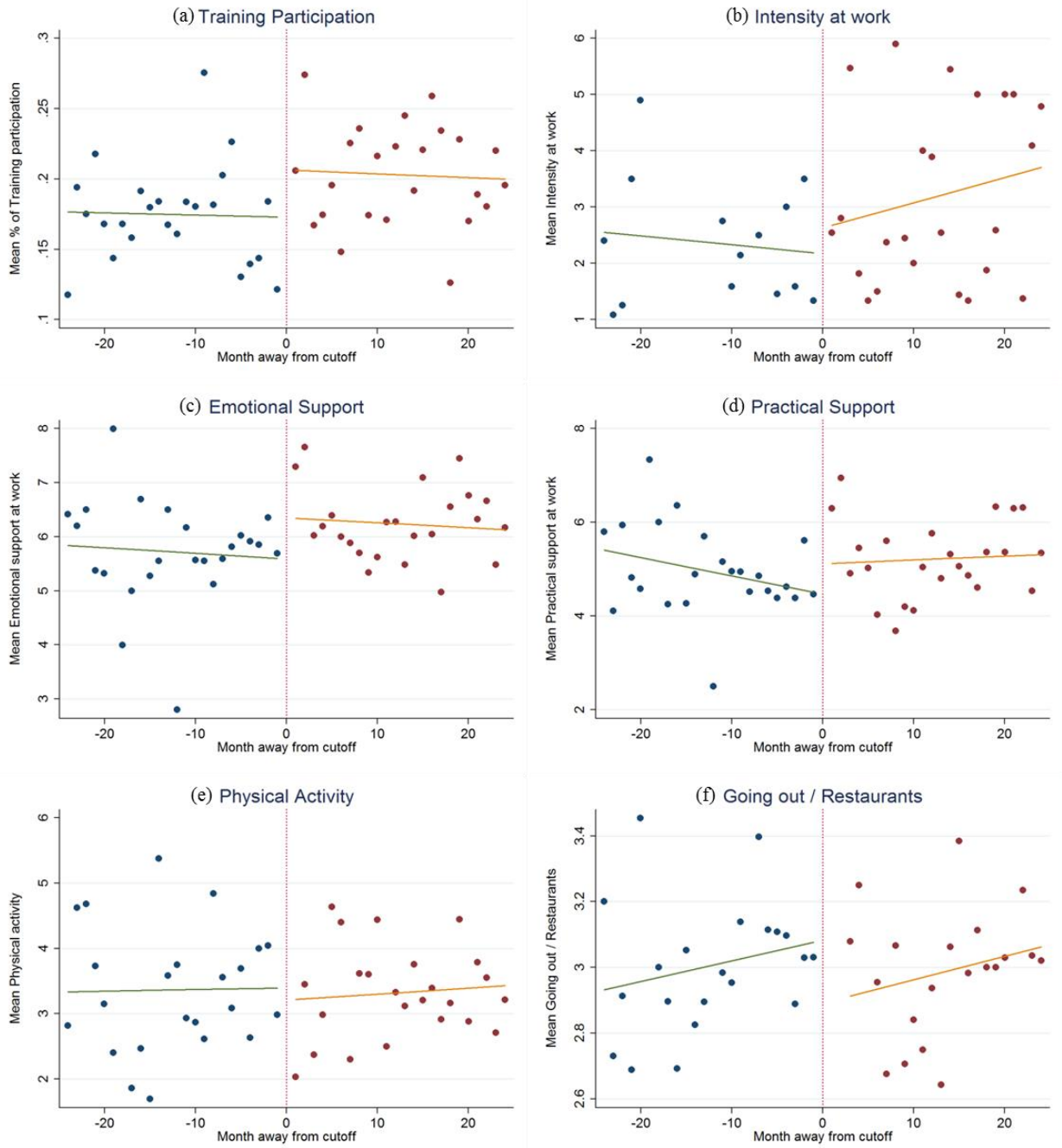


Figure 4 : Level of outcome variables. Bandwidth one month (48 bins). Linear fit.

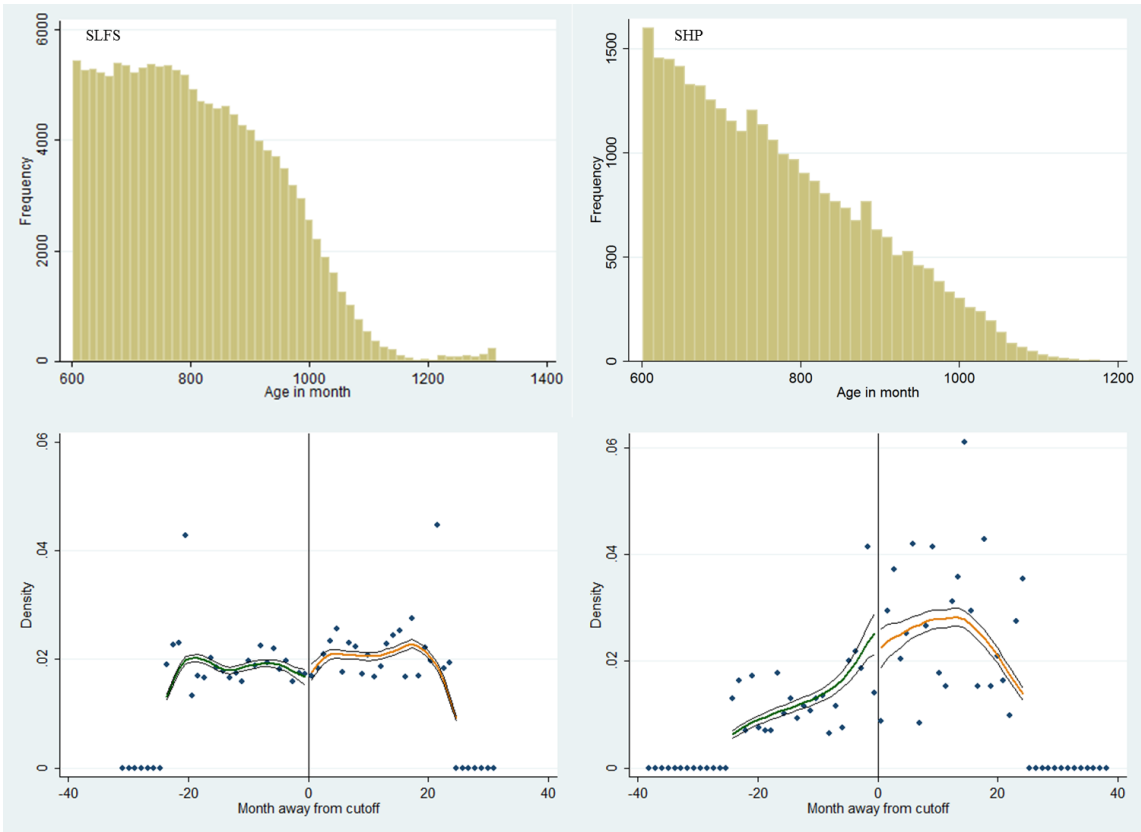


Figure 5 : Density of the forcing variable (months from cutoff): Histogram of age and McCrary Test

Table 1: Regression discontinuity design (RDD): Overview of the main non-parametric effects

Dependent Variable	Firm dimension		Co-worker dimension		Worker dimension	
	(1)	(2)	(3)	(4)	(5)	(6)
	Training in the last 12 month	Intensity of job	Emotional support in work	Practical support in work	Number of physical activities	Going out, Restaurant
Optimal bandwidth	0.135*** <i>0.047</i> N=2067 BW=7.67	1.252 <i>1.246</i> N=143 BW=15.87	1.824** <i>0.721</i> N=247 BW=7.48	1.757** <i>0.863</i> N=266 BW=8.94	-1.810** <i>0.710</i> N=251 BW=7.39	-0.165 <i>0.256</i> N=472 BW=8.45
6 months	0.118** <i>0.056</i> N=1412	3.489 <i>2.268</i> N=45	2.094** <i>0.833</i> N=188	1.803 <i>1.105</i> N=184	-1.704** <i>0.783</i> N=202	-0.579* <i>0.346</i> N=329
9 months	0.121*** <i>0.042</i> N=2403	2.892* <i>1.712</i> N=71	1.490** <i>0.663</i> N=274	1.757** <i>0.862</i> N=266	-1.356** <i>0.634</i> N=291	-0.111 <i>0.244</i> N=472
12 months	0.103*** <i>0.036</i> N=3220	1.918 <i>1.429</i> N=114	1.128** <i>0.556</i> N=372	1.423** <i>0.685</i> N=363	-0.956* <i>0.539</i> N=398	0.121 <i>0.191</i> N=639
15 months	0.078** <i>0.031</i> N=4104	1.358 <i>1.277</i> N=138	0.800 <i>0.498</i> N=455	0.952 <i>0.600</i> N=446	-0.493 <i>0.485</i> N=500	0.0933 <i>0.165</i> N=799
18 months	0.052* <i>0.028</i> N=5046	1.103 <i>1.189</i> N=152	0.585 <i>0.457</i> N=523	0.753 <i>0.545</i> N=513	-0.269 <i>0.451</i> N=586	0.0361 <i>0.149</i> N=938

Note: Estimations (1) are drawn from Panel SLFS, estimations (2) – (6) from Panel SHP. We estimate sharp regression discontinuity using local polynomial regressions (Lee and Lemieux 2009) with 1st order local polynomial (p) and 2nd order bias (q), following Calonico et al. (2014). The kernel type is triangular and the optimal bandwidth type (shown in the first row) is estimated CCT. Number of observations in a column varies because of changes in the bandwidth. Number of observations in a row (e.g. over the different dependent variables) varies because of missing values in the dependent variables. Robust standard errors are shown in *italics*. *p < 0.1, **p<0.05, ***p<0.01

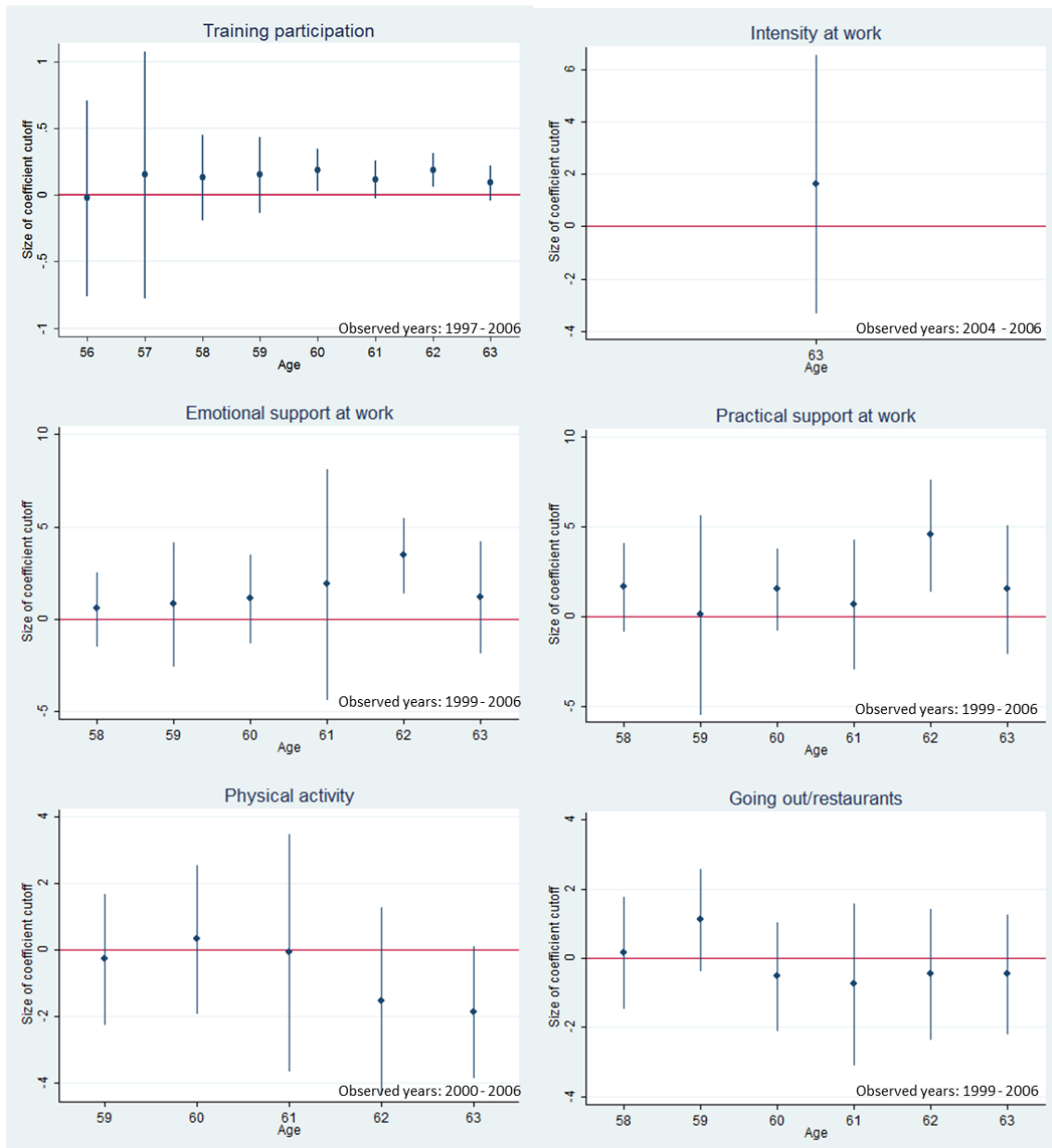


Figure 6: Age profiles coefficients of local linear regression

Note: This graph shows the age profile coefficients of the local linear regression with 1st order local polynomial (p) and 2nd order bias (q), following Calonico et al. (2014). The kernel type is triangular and the bandwidth type is CCT. We show only estimates for the 12 months bandwidth. Age 59 corresponds to observation of the individuals in the year they turn 59 (i.e., 2000 for birth year 1941 and 2001 for birth year 1942). We cannot observe all ages for all outcome variables since some workers were only questioned in later years. Not all coefficients are statistically significant different from zero for every age. However, pooled together they are, as seen in Table 1.

Table 2: Parametric estimation results of women born in 1941 versus 1942 (treated) and control variables

Dependent Variable	Firm dimension		Co-worker dimension		Worker dimension	
	(1)	(2)	(3)	(4)	(5)	(6)
	Training in the last 12 months	Intensity of job	Emotional support in work	Practical support in work	Number of physical activities	Going out, Restaurant
Treated: born in 1942	0.431* (0.236)	2.743* (1.391)	1.851** (0.730)	1.824* (1.004)	-1.609* (0.856)	-0.653* (0.379)
Age in months away from cutoff	-0.0342** (0.0173)	-0.242 (0.321)	-0.174 (0.170)	-0.118* (0.0670)	-0.0436 (0.116)	0.118** (0.0549)
Age in months	-0.0666 (0.0734)	-0.159 (0.311)	-0.0592 (0.160)	0.538*** (0.162)	-0.176* (0.105)	0.0909* (0.0462)
Legal retirement=1	0.0737 (0.231)	-0.236 (1.542)	0.627 (0.601)	0.215 (0.577)	0.763 (0.495)	-0.337 (0.250)
Bad health status		-0.377 (0.585)	0.145 (0.239)	0.000540 (0.281)	0.00129 (0.249)	-0.0400 (0.101)
Level of education	0.0371*** (0.0124)	0.236 (0.159)	0.119* (0.0621)	0.146* (0.0834)	-0.0127 (0.0709)	0.0215 (0.0373)
Married=1	-0.133 (0.107)	-0.152 (0.727)	-0.670* (0.345)	-0.189 (0.430)	-0.548 (0.442)	-0.374** (0.153)
% working	-0.0414 (0.0504)	-0.0132 (0.0128)	-0.00154 (0.00675)	-0.00413 (0.00728)	-0.0144** (0.00608)	-0.00110 (0.00245)
Firm affiliation	-0.00166 (0.0505)	-0.922 (1.287)	2.235** (0.890)	1.931* (1.079)	-0.221 (0.909)	0.0701 (0.288)
Job position	-0.121*** (0.0322)	0.218 (0.150)	-0.0919 (0.0949)	0.00691 (0.112)	-0.0325 (0.0835)	-0.0147 (0.0412)
Log income	0.118 (0.0993)	1.004*** (0.367)	-0.0321 (0.267)	0.282 (0.294)	0.155 (0.243)	-0.0699 (0.0928)
Constant	2.499 (4.368)	112.2 (235.5)	46.11 (113.1)	-31.39*** (10.55)	126.4* (74.40)	-59.41* (32.47)
Year dummies	yes	yes	yes	yes	yes	yes
R2		0.208	0.136	0.130	0.178	0.097
Pseudo R2	0.075					
Observations	1096	105	201	193	297	365

Note: Estimations (1) are drawn from Panel SLFS, estimations (2) – (6) from Panel SHP. Estimates are based on OLS or probit regressions using equation (1), including only women working until their retirement age (statutory or early). We include waves 1 – 8, hence years 1999 – 2006 (1997-2006 for the SLFS). Age is normalized around the cutoff, January 1, 1942. Number of observations varies due to missing values in both, left and right hand side variables. Robust standard errors are adjusted for clustered ID of persons and shown in parentheses. *p < 0.1, **p<0.05, ***p<0.01

Table 3: Covariates no-discontinuity test (SUR model)

Polynomial order	(1) Health status	(2) Married	(3) Job position	(4) Firm affiliation	(5) Income	(6) Education	(7) % -working
Panel A: SAKE							
1st		0.0064 <i>0.0047</i>	0.0048 <i>0.0197</i>	-0.0094 <i>0.0089</i>	0.0008 <i>0.0084</i>	-0.042 <i>0.0399</i>	-0.016 <i>0.0136</i>
Joint significance test (Prob> Chi2)	0.2350						
2nd		0.0063 <i>0.0047</i>	0.0043 <i>0.0197</i>	-0.0093 <i>0.0089</i>	0.001 <i>0.0084</i>	-0.044 <i>0.0399</i>	-0.0152 <i>0.0135</i>
Joint significance test (Prob> Chi2)	0.2702						
Panel B: SHP							
1st	0.003 <i>0.002</i>	0.002 <i>0.002</i>	-0.011 <i>0.0114</i>	0.005 <i>0.004</i>	-0.001 <i>0.006</i>	0.015 <i>0.012</i>	-0.028 <i>0.149</i>
Joint significance test (Prob> Chi2)	0.449						
2nd	0.002 <i>0.002</i>	0.001 <i>0.002</i>	-0.008 <i>0.012</i>	0.005 <i>0.005</i>	-0.001 <i>0.006</i>	0.012 <i>0.012</i>	0.020 <i>0.153</i>
Joint significance test (Prob> Chi2)	0.826						

Note: The table shows the joint significance test (Prob > Chi2) of the underlying seemingly unrelated regression (SUR) models. Standard errors are clustered at the individual level. *p < 0.1, **p<0.05, ***p<0.01

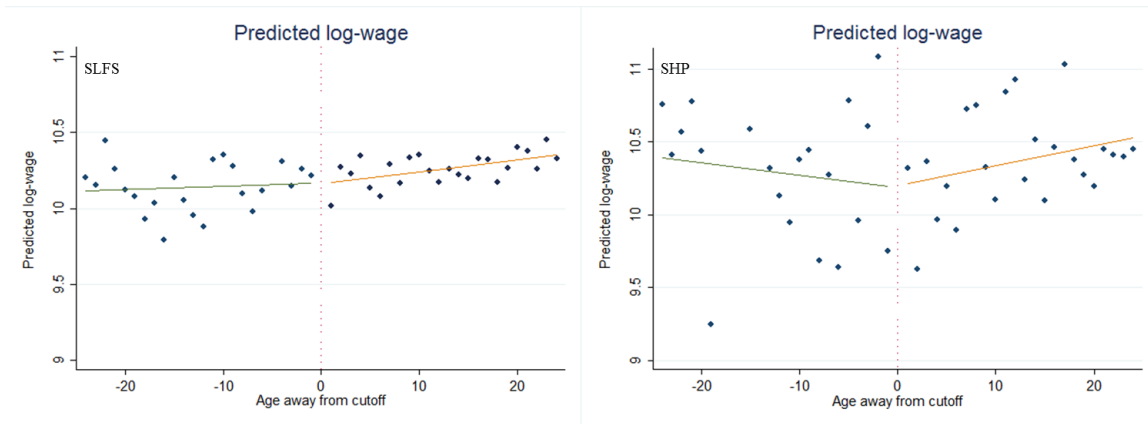


Figure 7 : Predicted log-wage

Note: This graph shows women's predicted log-wage per month per cohort from an underlying OLS regression on predetermined covariates. Control variables included are education level, age, marital status, firm affiliation, job position, working percent and yearly controls.

Table 4: Further robustness checks

Dependent Variable		Firm dimension		Co-worker dimension		Worker dimension	
		(1)	(2)	(3)	(4)	(5)	(6)
		Training in the last 12 months	Intensity of job	Emotional support in work	Practical support in work	Number of physical activities	Going out, Restaurant
(a)	Main	0.431* (0.236) N=1096	2.743* (1.391) N=105	1.851** (0.730) N=201	1.824* (1.004) N=193	-1.609* (0.856) N=297	-0.653* (0.379) N=365
(b)	Without covariates	0.430* (0.225) N=1096	1.537 (1.658) N=105	1.474* (0.766) N=201	1.315 (0.995) N=193	-1.631* (0.915) N=297	-0.498 (0.349) N=365
(c)	Individual weight to size of CH		2.609* (1.449) N=105	1.842** (0.723) N=201	1.817* (1.020) N=193	-1.603* (0.864) N=297	-0.649* (0.375) N=365
(d)	Without Italians	0.480* (0.250) N=980	2.796* (1.396) N=104	1.791** (0.722) N=196	1.742* (1.001) N=188	-1.640* (0.857) N=293	-0.653* (0.378) N=360
(e)	Without technicians	0.422* (0.240) N=1081	2.589* (1.477) N=89	1.962** (0.758) N=179	1.795* (1.031) N=172	-1.485* (0.856) N=266	-0.730* (0.390) N=328
(f)	Donut estimation	0.474* (0.262) N=1017	2.321 (1.749) N=100	1.832** (0.876) N=191	2.755** (1.230) N=164	-1.756* (1.025) N=283	-0.165 (0.323) N=347
(g)	Household income	0.371* (0.222) N=1148	1.970 (1.805) N=105	2.078*** (0.733) N=211	1.720* (0.976) N=203	-1.838** (0.838) N=308	-0.603* (0.340) N=385

Note: Estimations (1) are drawn from Panel SLFS, estimations (2) – (6) from Panel SHP. Estimates are based on OLS or probit regressions using equation (1), including only women working until their retirement age (statutory or early). We include waves 1 – 8, hence years 1999 – 2006 (1997-2006 for the SLFS). Age is normalized around the cutoff, January 1, 1942. Number of observations varies due to missing values in both, left and right hand side variables. (a) shows the main estimates as a comparison. (b) shows the parametric approach without any control variables. (c) takes individual weights into account, which accounts for representativeness relative to the Swiss population (only for SHP). (d) and (e) splits the sample due to under- or over-representativeness in those groups. (f) shows the Donut estimation, which excludes December 1941 and January 1942. Finally, (g) controls for the household income rather than the individuals work income. Robust standard errors are adjusted for clustered ID of persons and shown in parentheses. *p < 0.1, **p < 0.05, ***p < 0.01

Table 5: Marginal effects of early retirement vs. statutory retirement

	Firm dimension		Co-worker dimension		Worker dimension	
	(1)	(2)	(3)	(4)	(5)	(6)
	Training in the last 12 months	Intensity of job	Emotional support in work	Practical support in work	Number of physical activities	Going out, Restaurant
Early retirement						
Control (born 1941)	0.206 (0.152)	-1.078 (1.367)	0.833 (1.074)	1.402 (1.196)	2.033*** (0.668)	3.180*** (0.510)
Treated (born 1942)	0.285*** (0.0787)	5.137*** (1.576)	5.938*** (0.490)	5.008*** (0.463)	1.948*** (0.533)	3.316*** (0.216)
Statutory retirement						
Control (born 1941)	0.211*** (0.035)	2.035** (0.912)	4.892*** (0.449)	4.219*** (0.661)	3.800*** (0.514)	3.423*** (0.206)
Treated (born 1942)	0.342*** (0.039)	4.697*** (0.648)	6.804*** (0.370)	5.453*** (0.423)	2.181*** (0.452)	2.768*** (0.204)
N	1096	105	201	193	297	365

Note: Estimations (1) are drawn from Panel SLFS, estimations (2) – (6) from Panel SHP. Estimates are based on OLS or probit regressions using equation (1), including only women working until their retirement age (statutory or early). We include waves 1 – 8, hence years 1999 – 2006 (1997-2006 for the SLFS). Background characteristics controlled for: normalized age, age in month, early retirement, marital status, %-working, firm affiliation, job position, log income, year dummies. Age is normalized around the cutoff, January 1, 1942. Number of observations varies due to missing values in both, left and right hand side variables. The table shows the marginal effects $\partial \Pr[\cdot] / \partial X$ of the interaction at mean (holding all other factors at their means). Effects can be interpreted directly as the predicted mean for each category. These effects confirm the results from above: that there is a difference between workers born in 1941 vs. those born in 1942. Please note that the significance level does not imply anything regarding the significance difference between the categories. However, this is not our interest here. We want to compare different investment decisions (in terms of sign) for those who retire early vs. those who retire at the SRA.

Table 6: Marginal effects of married vs. unmarried women

	Firm dimension		Co-worker dimension		Worker dimension	
	(1)	(2)	(3)	(4)	(5)	(6)
	Training in the last 12 months	Intensity of job	Emotional support in work	Practical support in work	Number of physical activities	Going out, Restaurant
Not married						
Control (born 1941)	0.225*** (0.039)	1.931* (1.031)	5.130*** (0.487)	4.267*** (0.777)	4.367*** (0.675)	3.731*** (0.293)
Treated (born 1942)	0.371*** (0.043)	5.191*** (0.743)	7.119*** (0.361)	5.454*** (0.458)	2.306*** (0.510)	2.994*** (0.219)
Married						
Control (born 1941)	0.190*** (0.045)	2.022* (1.064)	4.579*** (0.572)	4.075*** (0.793)	3.029*** (0.497)	3.222*** (0.190)
Treated (born 1942)	0.298*** (0.0426)	4.132*** (0.903)	6.252*** (0.451)	5.282*** (0.474)	2.172*** (0.456)	2.709*** (0.212)
N	1096	105	201	193	297	365

Note: Estimations (1) are drawn from Panel SLFS, estimations (2) – (6) from Panel SHP. Estimates are based on OLS or probit regressions using equation (1), including only women working until their retirement age (statutory or early). We include waves 1 – 8, hence years 1999 – 2006 (1997-2006 for the SLFS). Background characteristics controlled for: normalized age, age in month, early retirement, marital status, %-working, firm affiliation, job position, log income, year dummies. Age is normalized around the cutoff, January 1, 1942. Number of observations varies due to missing values in both, left and right hand side variables. The table shows the marginal effects $\partial \Pr[\cdot] / \partial X$ of the interaction at mean (holding all other factors at their means). Effects can be interpreted directly as the predicted mean for each category. These effects confirm the results from above: that there is a difference between workers born in 1941 vs. those born in 1942. Please note that the significance level does not imply anything regarding the significance difference between the categories. However, this is not our interest here. We want to compare different investment decisions (in terms of sign) for those who retire early vs. those who retire at the SRA.

9 Appendix

Table A- 1 : Variable Description

	Dimension	Description
Panel A: SLFS 1997-2006		
Trainings	Firm	Did you have training in the last 12 month? 0=«no», 1=«yes»
Panel B: SHP 1999-2006		
Intensity	Firm	Rhythm of work: intense, scaled 1-10
Emotional support at work	Co-worker	Emotional support at work, scaled 1-10
Practical support at work	Co-worker	Practical support at work, scaled 1-10
Physical activity	Worker	Number of physical activities per week? Min=0, Max=7
Restaurants / Meeting friends	Worker	Frequency, scaled 1= «never» - 5 = «more than once a week»

Table A- 2 : Loss of observations

	Swiss Labor Force Survey		Swiss household panel											
	1941	1942	1941		1942		1941		1942		1941		1942	
Individuals in whole sample 1997 -2006	339'910		89'915											
Female	184'475		45'992											
Born in 1941 or 1942	2450	2666	486		537									
Retirement in year	2004	2006	2004		2006									
Still working and before retirement	1467	2380	298		484									
	1941	1942	1941	1942	1941	1942	1941	1942	1941	1942	1941	1942	1941	1942
	Training participation		Intensity		Emotional support		Practical support		Physical Activity		Going out/restaurant			
Losses due to missings in outcome variables and observations in non-parametric approach	1236	1984	51	63	141	231	137	226	171	227	266	373		
	3220		114		372		363		398		639			
Losses due to missings in control variables and observations in parametric approach	443	653	39	66	67	134	61	132	126	171	150	215		
	1096		105		201		193		297		365			

Note: This table shows the number of observations for each group (treatment and control) and the different estimations.

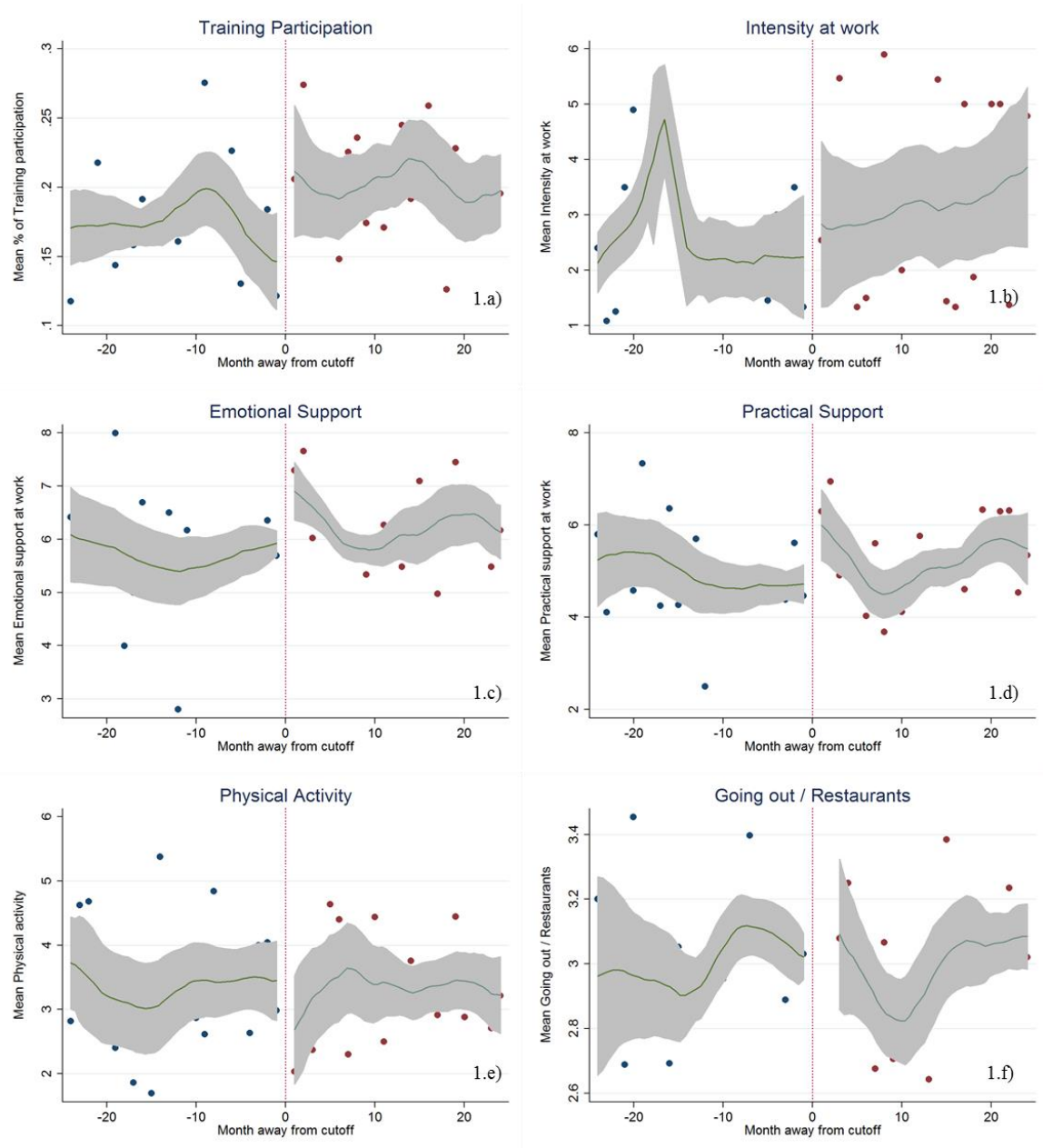


Figure A-1 : Level of outcome variables. Bandwidth one month (48 bins). Local polynomial fit.

Table A- 3 : Descriptive Statistics, means and differences in means between treated and non-treated (whole sample 1997-2006)

VARIABLE	Women born in 1941 (Control group)		Women born in 1942 (Treatment group)		Mean difference	p- value
	Mean	St.dev.	Mean	St.dev.		
Panel A: SLFS						
Training participation	0.199	0.399	0.226	0.418	0.026	0.027
<u>Background characteristics</u>						
Age in month	742.552	27.040	730.910	26.914	-11.646	0.000
Log income	10.295	0.974	10.392	0.867	0.097	0.024
Level of education	4.202	3.850	4.212	3.931	0.009	0.933
Firm affiliation	5.625	0.917	5.573	0.949	-0.052	0.180
Working % in categories	3.276	1.455	3.328	1.421	0.052	0.380
Married	0.575	0.494	0.580	0.494	0.006	0.687
Size of household	1.738	0.688	1.760	0.688	0.023	0.237
German	0.613	0.487	0.614	0.487	0.002	0.898
French	0.228	0.419	0.250	0.433	0.022	0.066
Italian	0.148	0.355	0.131	0.337	-0.017	0.075
Professionals	0.152	0.359	0.161	0.368	0.009	0.457
Technicians	0.004	0.062	0.017	0.128	0.013	0.000
Service / Sale	0.088	0.283	0.073	0.261	-0.014	0.115
Panel B: SHP						
Job intensity	2.823	2.703	4.026	3.221	1.203	0.030
Emotional support	6.045	2.198	7.163	2.596	1.118	0.005
Practical support	4.752	2.409	5.983	3.116	1.230	0.008
Physical activity	3.297	2.535	2.035	2.182	-1.262	0.002
Going out / restaurants	3.023	0.998	2.988	1.061	-0.034	0.540
<u>Background characteristics</u>						
Age in month	740.827	28.149	728.502	27.752	-12.325	0.000
Log income	9.899	1.330	9.854	1.199	-0.045	0.706
Level of education	3.866	2.374	4.076	2.525	0.209	0.177
Public (government)	0.400	0.492	0.419	0.495	0.019	0.768
Fulltime	0.242	0.429	0.259	0.439	0.017	0.676
Married	0.686	0.464	0.577	0.494	-0.108	0.000
German	0.735	0.441	0.678	0.467	-0.056	0.052
French	0.257	0.437	0.278	0.448	0.020	0.476
Italian	0.006	0.079	0.042	0.202	0.036	0.000
Firm affiliation	0.014	0.119	0.013	0.114	-0.001	0.878
Professionals	0.201	0.402	0.140	0.348	-0.061	0.086
Technicians	0.037	0.189	0.151	0.359	0.114	0.000
Service / Sale	0.158	0.366	0.189	0.392	0.031	0.400

Note: Panel A is the Swiss Labor Force Survey and the sample drawn is 1997-2006. Panel B is the Swiss Household Panel and covers years 1999-2006. The mean difference test is computed by Bonferroni comparison. For both Panels, dependent variables as well as background characteristics are shown.

Table A- 4: Regression discontinuity design: Different polynomial order 12 month

Dependent Variable	Firm dimension		Co-worker dimension		Worker dimension	
	(1)	(2)	(3)	(4)	(5)	(6)
	Training in the last 12 months	Intensity of job	Emotional support in work	Practical support in work	Number of physical activities	Going out, Restaurant
1st	0.103*** <i>0.036</i> N=3220	1.918 <i>1.429</i> N=114	1.128** <i>0.556</i> N=372	1.423** <i>0.685</i> N=363	-0.956* <i>0.539</i> N=398	0.121 <i>0.191</i> N=639
2nd	0.153*** <i>0.060</i> N=3220	4.422* <i>2.414</i> N=114	2.143** <i>0.923</i> N=372	2.041* <i>1.204</i> N=363	-2.018** <i>0.880</i> N=398	-0.534 <i>0.369</i> N=639
3rd	0.094 <i>0.093</i> N=3220	4.810 <i>4.055</i> N=114	2.934* <i>1.511</i> N=372	1.279 <i>1.900</i> N=363	-0.844 <i>1.368</i> N=398	-1.514** <i>0.640</i> N=639
4th	-0.017 <i>0.154</i> N=3220	1.913 <i>6.594</i> N=114	1.833 <i>2.445</i> N=372	1.053 <i>3.004</i> N=363	2.328 <i>2.296</i> N=398	-1.981* <i>1.044</i> N=639

Note: Estimations (1) are drawn from Panel SLFS, estimations (2) – (6) from Panel SHP. We estimate sharp regression discontinuity using local polynomial regressions (Lee and Lemieux 2009) with 1st, 2nd, 3rd and 4th order of local polynomial (p) and 2nd, 3rd, 4th and 5th order bias (q) following Calonico et al. (2014). The kernel type is triangular. Bandwidth is 12 month. Number of observations in a column varies because of changes in the bandwidth. Number of observations in a row (e.g. over the different dependent variables) varies because of missing values in the dependent variables. Robust standard errors are shown in *italics*. *p < 0.1, **p<0.05, ***p<0.01

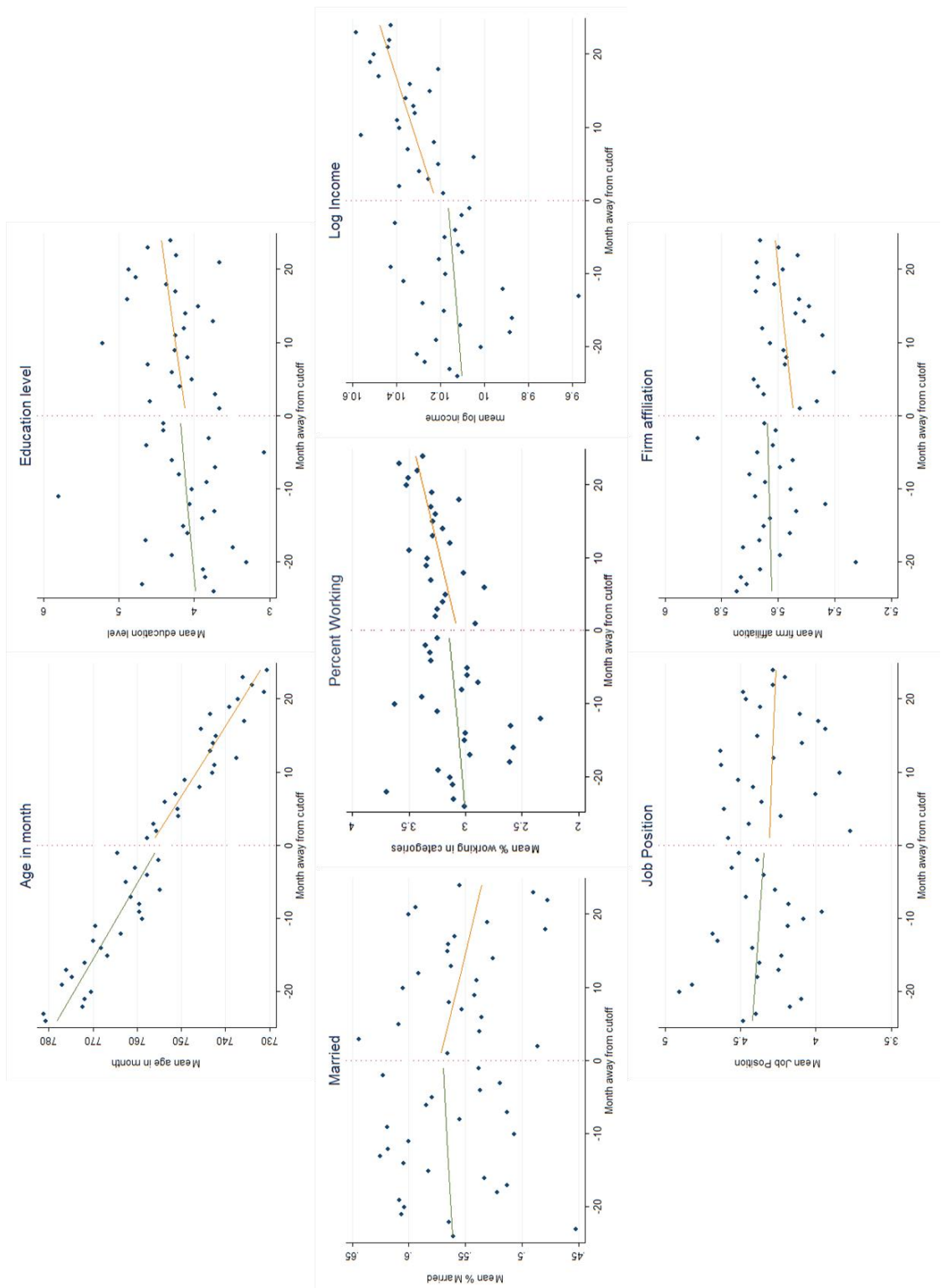


Figure A- 2: Discontinuity of covariates by month away from cutoff (48 bins), linear fit. Panel A: SAKE

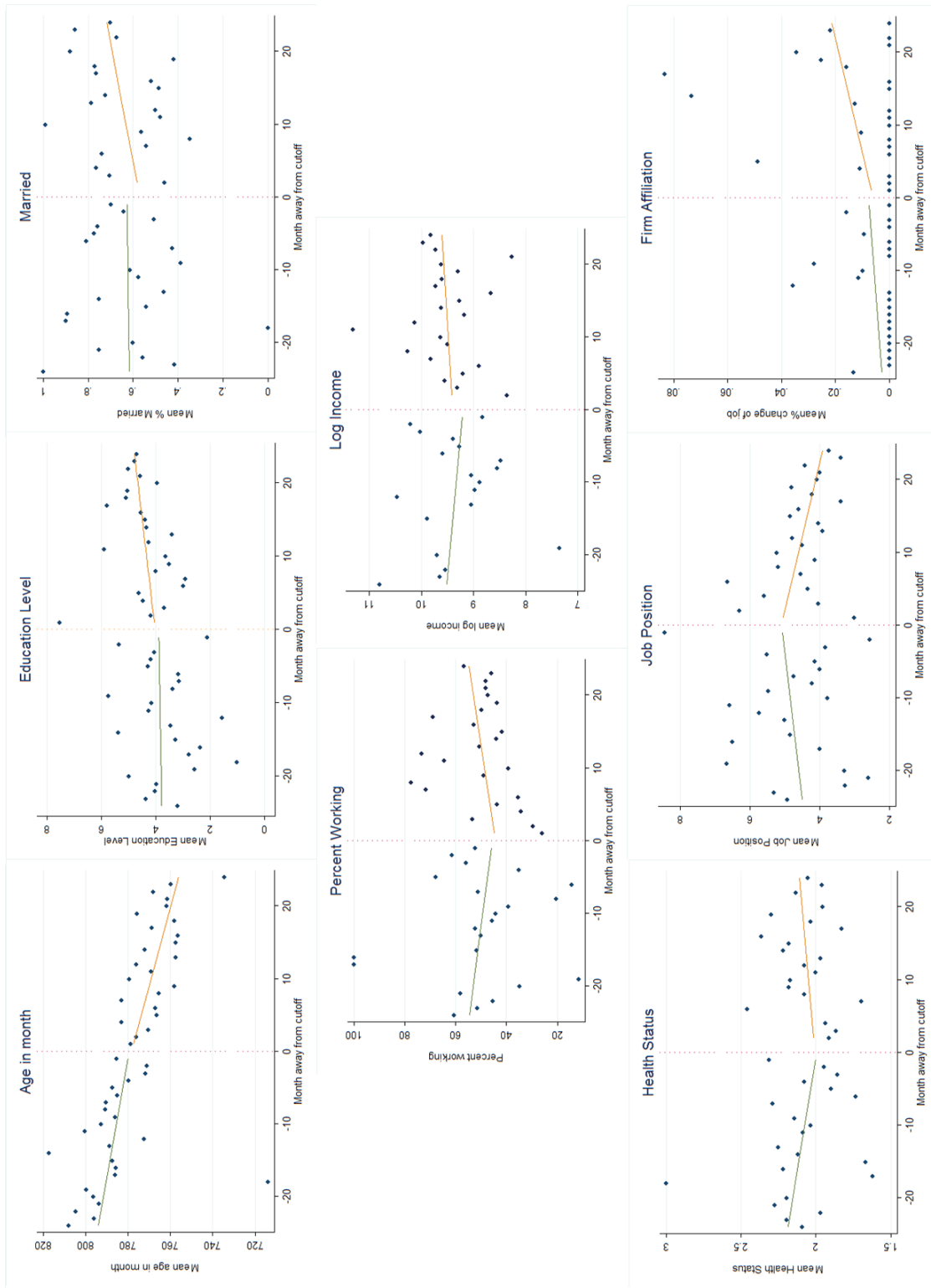


Figure A- 3: Discontinuity of covariates by month away from cutoff (48 bins), linear fit. Panel B: SHP

Table A- 5 : Covariates no-discontinuity test (SUR model) Panel A: Sake

Polynomial order	Panel A: SAKE					
	(1) Married	(2) Job position	(3) Firm affiliation	(4) Income	(5) Education	(6) % -working
1st	0.0064	0.0044	-0.0093	0.0014	-0.0376	-0.0174
	0.0047	0.0197	0.0091	0.0084	0.0400	0.0136
Joint significance test (Prob> Chi2)	0.2329					
2nd	0.0061	0.0037	-0.0092	0.0016	-0.038	-0.0157
	0.0047	0.0197	0.0091	0.0085	0.0400	0.0135
Joint significance test (Prob> Chi2)	0.2752					
3rd	0.0122*	-0.0153	-0.0134	0.0026	-0.0254	-0.0263
	0.0068	0.0293	0.0125	0.0125	0.0586	0.0195
Joint significance test (Prob> Chi2)	0.1241					
4th	0.0116*	-0.0211	-0.0146	0.0044	-0.0199	-0.0212
	0.0068	0.0294	0.0126	0.0126	0.0588	0.0195
Joint significance test (Prob> Chi2)	0.1517					
5th	0.0052	0.0100	-0.012	0.0045	-0.0228	-0.0204
	0.0081	0.0343	0.0148	0.0149	0.0708	0.0231
Joint significance test (Prob> Chi2)	0.6185					
6th	0.0031	0.0068	-0.0142	0.0068	-0.0184	-0.0118
	0.008	0.0345	0.0149	0.0149	0.0712	0.0231
Joint significance test (Prob> Chi2)	0.7428					

Note: Estimates show the joint significance test (Prob > Chi2) of the underlying seemingly unrelated regression models (SUR). Standard errors are clustered on individual level. *p < 0.1, **p<0.05, ***p<0.01

Table A- 6 : Covariates no-discontinuity test (SUR model) Panel B: SHP

Polynomial order	Panel B: SHP						
	(1) Health status	(2) Married	(3) Job position	(4) Firm affiliation	(5) Income	(6) Education	(7) % -working
1st	0.003 0.002	0.002 0.002	-0.011 0.0114	0.005 0.004	-0.001 0.006	0.015 0.012	-0.028 0.149
Joint significance test (Prob> Chi2)	0.449						
2nd	0.002 0.002	0.001 0.002	-0.008 0.012	0.005 0.005	-0.001 0.006	0.012 0.012	0.020 0.153
Joint significance test (Prob> Chi2)	0.826						
3rd	0.018 0.007	-0.009 0.006	0.017 0.027	0.007 0.011	0.008 0.014	0.037 0.029	0.578 0.356
Joint significance test (Prob> Chi2)	0.031						
4th	0.017 0.007	-0.008 0.006	0.013 0.029	0.002 0.012	0.006 0.015	0.015 0.032	0.338 0.389
Joint significance test (Prob> Chi2)	0.289						
5th	0.015 0.009	-0.002 0.008	-0.02 0.037	-0.009 0.014	0.019 0.019	-0.061 0.036	0.403 0.476
Joint significance test (Prob> Chi2)	0.145						
6th	0.018 0.009	-0.002 0.007	-0.021 0.038	-0.012 0.014	0.011 0.019	-0.056 0.038	0.214 0.491
Joint significance test (Prob> Chi2)	0.191						

Note: Estimates show the joint significance test (Prob > Chi2) of the underlying seemingly unrelated regression models (SUR). Standard errors are clustered on individual level. *p < 0.1, **p<0.05, ***p<0.01

Table A- 7 : By age covariates no-discontinuity test (SUR model)

Polynomial order	Panel A: SAKE		Panel B: SHP	
	(1) 1st	(2) 2nd	(3) 1st	(4) 2nd
Age 57	0.6383	0.7191		
Age 58	0.9642	0.3219		
Age 59	0.0375	0.0096	0.5415	0.0186
Age 60	0.9469	0.8093	0.0411	0.4748
Age 61	0.2975	0.2421	0.6821	0.3793
Age 62	0.0353	0.0498	0.9657	0.9522
Age 63	0.0011	0.0013	0.8708	0.6255
Years observed	1997-2006		1999-2006	

Note: Estimates show the joint significance test (Prob > Chi2) of the underlying seemingly unrelated regression models (SUR) at different ages. Standard errors are clustered on individual level. *p < 0.1, **p<0.05, ***p<0.01

Table A- 8 : Sensitivity Analysis: Higher Polynomial order

	Firm dimension		Co-worker dimension		Worker dimension	
	(1)	(2)	(3)	(4)	(5)	(6)
Higher poly-nominal order	Training in the last 12 months	Intensity of job	Emotional support in work	Practical support in work	Number of physical activities	Going out, Restaurant
1st	0.431* (0.236)	2.743* (1.391)	1.851** (0.730)	1.824* (1.004)	-1.609* (0.856)	-0.653* (0.379)
2nd	0.514** (0.256)	2.731* (1.386)	1.780** (0.712)	1.819* (1.003)	-1.609* (0.838)	-0.665* (0.377)
3rd	0.633* (0.363)	5.063** (2.003)	2.965** (1.140)	3.029* (1.633)	-2.376** (1.065)	-0.883 (0.595)
4th	0.606* (0.369)	5.155*** (1.798)	2.528** (1.105)	2.175 (1.636)	-2.429** (1.087)	-0.809 (0.570)
Observations	1096	105	201	193	297	365

Note: Estimations (1) are drawn from Panel SLFS, estimations (2) – (6) from Panel SHP. Estimates are based on OLS or probit regressions using equation (1), including only women working until their retirement age (statutory or early). We include waves 1 – 8, hence years 1999 – 2006 (1997-2006 for the SLFS). Background characteristics controlled for: normalized age, age in month, early retirement, marital status, %-working, firm affiliation, job position, log income, year dummies. Age is normalized around the cutoff, January 1, 1942. Number of observations varies due to missing values in both, left and right hand side variables. Robust standard errors are adjusted for clustered ID of persons and shown in parentheses. *p < 0.1, **p<0.05, ***p<0.01

Table A- 9: Sensitivity Analysis: Different bandwidths

Dependent Variable	Firm dimension		Co-worker dimension		Worker dimension	
	(1)	(2)	(3)	(4)	(5)	(6)
	Training in the last 12 months	Intensity of job	Emotional support in work	Practical support in work	Number of physical activities	Going out, Restaurant
6 months	0.459 (0.386) N=523	5.920*** (1.688) N=43	2.793** (1.084) N=113	2.489 (1.672) N=109	-2.856** (1.272) N=145	-1.402** (0.529) N=183
9 months	0.427 (0.284) N=842	3.352** (1.583) N=73	2.430** (0.950) N=155	2.277* (1.277) N=150	-1.540 (0.948) N=218	-0.743* (0.394) N=276
12 months	0.431* (0.236) N=1096	2.743* (1.391) N=105	1.851** (0.730) N=201	1.824* (1.004) N=193	-1.609* (0.856) N=297	-0.653* (0.379) N=365
15 months	0.155 (0.198) N=1376	2.383 (1.467) N=118	1.017 (0.692) N=233	0.167 (0.907) N=226	-1.339 (0.830) N=346	-0.600* (0.351) N=420
18 months	0.094 (0.174) N=1664	3.262** (1.443) N=131	1.062 (0.688) N=264	0.458 (0.835) N=256	-1.075 (0.789) N=376	-0.387 (0.347) N=462

Note: Estimations (1) are drawn from Panel SLFS, estimations (2) – (6) from Panel SHP. Estimates are based on OLS or probit regressions using equation (1), including only women working until their retirement age (statutory or early). We include waves 1 – 8, hence years 1999 – 2006 (1997-2006 for the SLFS). Background characteristics controlled for: normalized age, age in month, early retirement, marital status, %-working, firm affiliation, job position, log income, year dummies. Age is normalized around the cutoff, January 1, 1942. Number of observations varies due to missing values in both, left and right hand side variables. Bandwidth changes in 3 month intervals. Robust standard errors are adjusted for clustered ID of individuals and shown in parentheses. *p < 0.1, **p<0.05, ***p<0.01

Table A- 10: Sensitivity Analysis: Placebo groups

Dependent Variable	Firm dimension		Co-worker dimension		Worker dimension	
	(1)	(2)	(3)	(4)	(5)	(6)
	Training in the last 12 months	Intensity of job	Emotional support in work	Practical support in work	Number of physical activities	Going out, Restaurant
Treated: born in 1942	0.431* (0.236) N=1096	2.743* (1.391) N=105	1.851** (0.730) N=201	1.824* (1.004) N=193	-1.609* (0.856) N=297	-0.653* (0.379) N=365
Placebo I (men)	0.018 (0.183) N=1642	1.168 (1.310) N=103	-0.495 (0.796) N=226	-0.779 (1.062) N=224	1.542** (0.701) N=284	-0.425 (0.341) N=355
<i>Test mean difference</i>						
χ^2	2.71*	0.79	5.19**	3.48*	8.69***	0.21
(p-value)	0.099	0.373	0.023	0.062	0.003	0.646
Treated: born in 1942	0.431* (0.236) N=1096	2.743* (1.391) N=105	1.851** (0.730) N=201	1.824* (1.004) N=193	-1.609* (0.856) N=297	-0.653* (0.379) N=365
Placebo II (Year 1943 vs 1944)	0.252 (0.178) N=1594	0.974 (1.131) N=164	0.412 (0.713) N=261	-0.542 (0.856) N=264	0.658 (0.727) N=338	-0.0479 (0.265) N=353
<i>Test mean difference</i>						
χ^2	0.40	1.12	2.18	3.53*	4.38**	1.18
(p-value)	0.526	0.291	0.139	0.061	0.036	0.178

Note: Estimations (1) are drawn from Panel SLFS, estimations (2) – (6) from Panel SHP. Estimates are based on OLS or probit regressions using equation (1), including only women working until their retirement age (statutory or early). We include waves 1 – 8, hence years 1999 – 2006 (1997-2006 for the SLFS). Background characteristics controlled for: normalized age, age in month, early retirement, marital status, %-working, firm affiliation, job position, log income, year dummies. Age is normalized around the cutoff, January 1, 1942. Number of observations varies due to missing values in both, left and right hand side variables. Placebo I is the sample of men born in 1941 vs. 1942. Placebo II is the sample of women born in 1944 vs. 1945. Robust standard errors are adjusted for clustered ID of individuals and shown in parentheses. *p < 0.1, **p<0.05, ***p<0.01

Table A-11: Marginal effects of blue collar, white collar and management

	Firm dimension		Co-worker dimension		Worker dimension	
	(1)	(2)	(3)	(4)	(5)	(6)
	Training in the last 12 months	Intensity of job	Emotional support in work	Practical support in work	Number of physical activities	Going out, Restaurant
Blue collar						
Control (born 1941)	0.0916** (0.0441)	0.348 (1.119)	4.578*** (0.719)	3.397*** (0.999)	3.510*** (0.650)	3.345*** (0.294)
Treated (born 1942)	0.130*** (0.0452)	4.723*** (1.057)	5.535*** (0.529)	4.822*** (0.537)	2.178*** (0.354)	2.919*** (0.284)
White collar						
Control (born 1941)	0.191*** (0.0369)	2.023 (1.316)	5.046*** (0.698)	4.810*** (0.809)	3.180*** (0.700)	3.190*** (0.202)
Treated (born 1942)	0.323*** (0.0452)	4.444*** (0.684)	6.439*** (0.399)	5.001*** (0.412)	2.383*** (0.612)	2.864*** (0.168)
Management						
Control (born 1941)	0.350*** (0.0694)	3.269*** (0.559)	5.218*** (0.406)	4.471*** (0.705)	3.959*** (0.635)	3.748*** (0.240)
Treated (born 1942)	0.552*** (0.0494)	4.337*** (1.045)	7.800*** (0.552)	6.056*** (0.750)	2.026*** (0.648)	2.816*** (0.318)
N	1096	105	201	193	297	365

Note: Estimations (1) are drawn from Panel SLFS, estimations (2) – (6) from Panel SHP. Estimates are based on OLS or probit regressions using equation (1), including only women working until their retirement age (statutory or early). We include waves 1 – 8, hence years 1999 – 2006 (1997-2006 for the SLFS). Background characteristics controlled for: normalized age, age in month, early retirement, marital status, %-working, firm affiliation, job position, log income, year dummies. Age is normalized around the cutoff, January 1, 1942. Number of observations varies due to missing values in both, left and right hand side variables. The table shows the marginal effects $\partial \Pr[\cdot] / \partial X$ of the interaction at mean (holding all other factors at their means). Effects can be interpreted directly as the predicted mean for each category. These effects confirm the results from above: that there is a difference between workers born in 1941 vs. those born in 1942. Please note that the significance level does not imply anything regarding the significance difference between the categories. However, this is not our interest here. We want to compare different investment decisions (in terms of sign) for those who retire early vs. those who retire at the SRA.

Table A- 12 : Sample split of early / non-early retired, married / unmarried and blue collar / white collar / management

	Firm dimension		Co-worker dimension		Worker dimension	
	(1)	(2)	(3)	(4)	(5)	(6)
	Training in the last 12 months	Intensity of job	Emotional support in work	Practical support in work	Number of physical activities	Going out, Restaurant
<i>Early retired</i>	0.511 (0.913) N=95	n.a. N=12	18.56 (80.01) N=29	7.424* (3.867) N=28	-3.350 (2.817) N=34	-0.176 (0.833) N=45
<i>Not early retired</i>	0.397 (0.243) N=990	2.297 (1.449) N=93	1.790** (0.753) N=172	1.528 (1.043) N=165	-1.530* (0.894) N=263	-0.724* (0.386) N=320
<i>Not married</i>	0.570** (0.282) N=611	4.279** (1.877) N=48	2.903** (1.112) N=82	1.561 (1.361) N=80	-0.871 (1.428) N=134	-0.682 (0.589) N=167
<i>Married</i>	0.237 (0.409) N=485	1.242 (2.525) N=57	1.069 (1.330) N=119	1.634 (1.547) N=113	-1.374 (0.941) N=163	-0.374 (0.340) N=198
<i>Blue collar</i>	-0.5485 (0.927) N=183	3.635 (4.766) N=23	5.186** (2.114) N=46	7.071*** (1.572) N=45	-0.867 (1.709) N=62	0.296 (0.823) N=77
<i>White collar</i>	0.498 (0.322) N=666	2.952 (3.588) N=58	-0.543 (1.399) N=102	-0.214 (1.196) N=97	0.469 (1.456) N=157	-0.139 (0.437) N=192
<i>Management</i>	0.788* (0.419) N=237	4.691*** (1.504) N=24	2.440** (1.122) N=53	-0.0968 (2.175) N=51	-2.191* (1.092) N=78	-1.764** (0.688) N=96
N	1096	105	201	193	297	365

Note: Estimations (1) are drawn from Panel SLFS, estimations (2) – (6) from Panel SHP. Estimates are based on OLS or probit regressions using equation (1), including only women working until their retirement age (statutory or early). We include waves 1 – 8, hence years 1999 – 2006 (1997-2006 for the SLFS). Background characteristics controlled for: normalized age, age in month, early retirement, marital status, %-working, firm affiliation, job position, log income, year dummies. Age is normalized around the cutoff, January 1, 1942. Number of observations varies due to missing values in both, left and right hand side variables. The table shows a sample split of early vs. normal retired and married vs. unmarried.