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ABSTRACT

Working Life and Human Capital Investment: Causal Evidence from Pension Reform*

This paper presents a life-cycle model with human capital investment during working life through training and provides a novel empirical test of human capital theory. We exploit a sizable pension reform across adjacent cohorts in a regression discontinuity setting and find that an increase in working life increases training. We discuss and test further predictions regarding the relation between initial schooling, training, and the reform effect, showing that only individuals with a college degree increase human capital investment. Our results speak to a large class of human capital models as well as policies extending or shortening working life.

JEL Classification: J24, J26, H21
Keywords: human capital, retirement policies, RDD

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1 Introduction

Human capital theory, starting with Ben-Porath (1967) and Becker (1962), predicts that the value of human capital investment increases with the payout period of the investment.[1] In this paper, we propose a novel empirical test of this key prediction of the human capital theory. We exploit an exogenous increase in the working life induced by a sizable pension reform that changes the payout period of the human capital investment. Our main finding, based on theory and empirical analysis using a regression discontinuity design (RDD), is that an increase in the working life causally increases human capital investment. This is novel evidence that investments, in line with the human capital theory, respond to changes of the payout period.

The policy-induced variation of the pension reform that we exploit affects the employed in the later part of their working career, i.e. after the age 50. This period is of central importance for the labor market and employment policies since employment rates start to decline for these age groups. Moreover, the reform that we study is relevant for large parts of the working population, such that our results are informative for macroeconomic analyses based on human capital theory as well as for the design of pension reforms.

We proceed in two steps. In the first step, we theoretically discuss the implications of an increase in the working life on human capital accumulation. The model is based on the standard life cycle theory of human capital by Ben-Porath (1967) but we allow for human capital accumulation during the working life through on-the-job training (in the following training). Moreover, changes in the payout period for human capital investment are not determined by variation in life expectancy, but by a change in the working life. We derive three propositions from the model. Proposition I shows that investment in training increases with working life, while, according to Proposition II, training is increasing with initial education. Finally, Proposition III determines the conditions when investments in training induced by

[1]This important prediction is the basis that explains the joint increases in life expectancy and educational investments witnessed in most countries over the past century, see e.g. Soares (2005); Cervellati and Sunde (2013).

an increase in the working life increase with initial schooling.

In the second step, we provide empirical evidence for the propositions of the life-cycle model. The empirical analysis is based on data from the German microcensus; the microcensus is a representative yearly household survey that covers 1% of all German households (about 370,000 households per year). This data includes detailed information about specific job-related training which we use to measure post-schooling human capital investment. In the analysis, we exploit a pension reform to provide causal evidence for the effect of working life on human capital investment. Specifically, the pension reform abolished an important early retirement program for women born after 1951. Women born in 1951 and before could enter retirement at the age of 60 through this pathway. In contrast, for women born in 1952 and later, this pathway was closed; these women can only enter retirement starting at the age of 63. The reform is well suited to estimate the effect of an increase in working life on training and to test human capital theory. This is because the pension reform was already implemented in 1999. Thus, the affected women, i.e. women born in 1952, still had a long remaining working life to benefit from human capital investment. Moreover, Geyer and Welteke (2019) document a sizable positive employment effect after the age of 60, so this reform causally increased in working life. As a result, we can exploit the variation at the cut-off between these adjacent cohorts to isolate the causal effect of an increase in working life on human capital investment on the job.

In the empirical analysis we first use descriptive evidence from the microcensus to provide empirical support for Propositions I and II: In line with Proposition I, we show that training positively correlates with remaining working life. Moreover, training also positively correlates with initial educational levels (Proposition II). We then present causal support for Propositions I and III. Our empirical results show that the increase in the retirement age has a sizable effect on human capital accumulation: depending on the specification, trainingincreases by about 2.5-5 percentage points, corresponding to an increase of 20% - 30% for these age groups. We carefully examine robustness. Our key finding is robust to the omission or inclusion of individual-level control variables. Moreover, results are stable to changes in the bandwidth for different specifications of the running variable and results do not change in Donut-RDD specifications that omit the first or the first two month on each side of the cutoff. We also test for balancing in individual characteristics and show balance across the cutoff. Finally, we conduct placebo tests for which we artificially alter the cut-off date and document null effects of training.

Turning to heterogeneity, we find clear evidence that the training effect increases with initial education (Proposition III). The pension reform increases training for women with a college degree or more by about 13 percentage points, which corresponds to a relative increase of about 40%. The effect for women without college degree is not significant.

Our study is related to several strands of the literature. Most importantly, we contribute to empirical studies related to the human capital theory that estimate the effect of mortality on educational outcomes and economic growth, surveyed in e.g. Bloom et al. (2019). Studies using variation in mortality face at least two challenges. First, as discussed in Hazan (2009) and Cervellati and Sunde (2013), it is not the change in the length of life *per-se* which matters for investment in human capital, but the survival rates during adult life. Second, variation in life expectancy is rarely random or unexpected, complicating causal estimation. A large part of the empirical literature uses variation in mortality rates between countries or states, e.g. Acemoglu and Johnson (2007), Lorentzen et al. (2008) or Hansen and Strulik (2017), with mixed findings. Several papers specifically address the methodological challenges, focusing on specific diseases or on changes in health services in the context of developing countries. Oster et al. (2013) use variation in life expectancy driven by Huntington disease realizations across individuals who have *ex-ante* similar risks for realizations of this neurological disorder. They find effects in line with human capital theory on college attendance and completion, health outcomes, as well as on job training for individuals with different realisations or information (genetic testing) undertaken between the ages of 17 and 35.[2] In developing

[2] In this study, job training is measured using a variable on whether individuals have ever undertaken job training for promotion or job advancement since starting their job.

countries, Jayachandran and Lleras-Muney (2009), using a strong decline in maternal death rates in Sri Lanka, find positive effects on girls' educational investments measured in years of school education and literacy rates. In another important study, Baranov and Kohler (2018) exploit variation in mortality rates related to HIV medication in Malawi to study effects on savings and on children's educational investments. They find positive effects of an increase in life expectancy on both types of outcomes.

Our study is complementary to these studies as it presents evidence that is not related to variation in life expectancy and focuses on training in the later part of the working career instead of schooling as the central human capital variable. We use a regression discontinuity design and variation in working life induced by a pension reform, a common policy parameter across the developed world. This allows us to study effects of changes in working life on educational investment for a population that is relatively older and comprises an important part of the labor force.

The paper is also related to the literature that analyzes the effect of pension reforms on employment, income, training and mortality. In general, these studies document positive employment effects and an increase in the working life of pension reforms that reduce the generosity of the pension system. These studies either exploit exogenous variation in the pension rules for the identification[3] or they are based on structural retirement models.[4] Crucially, these studies typically assume an exogenous process of human capital investment, which implies that individuals cannot adjust their human capital investment through additional training in response to a pension reform. Notable exceptions are the structural analyses by Fan et al. (2017) and Blundell et al. (2019). Fan et al. (2017) show that a reduction in the generosity of the pension system leads to an increase in human capital accumulation, which is not consistent with the assumption of an exogenous human capital process. Similarly, Blundell et al. (2019) document for women that human capital accumulation through

[3]Examples include, Duggan et al. (2007),Mastrobuoni (2009), Staubli and Zweimueller (2013),Atalay and Barrett (2015),Manoli and Weber (2016),or,Geyer and Welteke (2019).

[4]See e.g.Gustman and Steinmeier (1986), Rust and Phelan (1997), French (2005), French and Jones (2011) or Haan and Prowse (2014)

training has positive effects that partly compensate for the negative career effects of children. Several papers also show that a reduction in the generosity of the pension system leads to an increase in training, e.g. Montizaan et al. (2010), Brunello and Comi (2013), and Bauer and Eichenberger (2017). However, these papers do not link their findings to a theoretical model and are based on smaller reforms or specific settings, such as workers in large public sector firms only.[5] Finally, there is little empirical evidence that pension reforms have meaningful effects on life expectancy. Although the literature shows mixed evidence about the effect of retirement on mortality, in general the effects are relatively small, especially for women. For example, Kuhn et al. (2010) and Fitzpatrick and Moore (2018) document that retirement has a positive effect on mortality for men but no significant effect on female mortality. Hernaes and Vestad (2013) find that the retirement age has no effect on mortality. The pension reform setting that we study is therefore well suited to examine effects of working life on human capital investment of females, holding life expectancy constant.

The paper is organized as follows. In Section 2 we develop a model of human capital accumulation during the working life. Section 3 describes the German public pension system and the 1999 pension reform, introduces the data and provides descriptive support for the predictions of the human capital model. In Section 4, we turn to the causal analysis. We describe the method and document causal evidence from graphical and regression analyses. Finally, Section 5 concludes.

2 Theoretical model

In this section, we derive a theoretical human capital model and show that individuals, ceteris paribus, have an incentive to increase training when working life increases.[6] The central

[5] Our paper is also related to studies focusing on other dimensions of human capital investment and training. Several studies discuss the theory of human capital investment through training and provide empirical evidence about the effect on labor market outcomes in form of wages, job security, or employment probability (see for example Pischke (2001), Zweimueller and Winter-Ebmer (2000), Barrett and O'Connell (2001), Leuven (2005), Frazis and Loewenstein (2005), Picchio and van Ours (2011), or Ruhose and Weilage (2019)).

[6] There exist several reasons why individuals have a general motivation to invest in training. Most importantly, empirical evidence shows that training has a positive effect on wages and on employment, see e.g.

mechanism for this human capital effect is that the returns to training increase with the remaining working life of an individual i, denoted by R_i.[7] Further, we analyze how the human capital effect induced by increasing working life differs by initial schooling, S_i. To derive testable predictions by initial schooling, it is necessary to establish first how training, ceteris paribus, varies with initial schooling endowment. With the assumption that initial schooling and further human capital accumulation are dynamic complements, see e.g. Cunha and Heckmann (2007) and Jacobs (2009), we can show that training increases with initial education. In the Appendix, we replace the assumption of dynamic complementary by instead introducing utility costs of training as in Blundell et al. (2019) and derive the same proposition. Finally, we determine the conditions when investments in training induced by an increase in the working life increase with initial schooling.

The theoretical model presented below illustrates the mechanism in a simplified and intuitive setting trough a discrete time model consisting of three stylized periods. Note that Y_{ti} denotes an individual i's income in period t and C_{ti} denotes the level of consumption in period t.

1. Period

Each individual derives utility through consumption, $U(C_{1i})$, with the standard assumption of $U'(C_{1i}) > 0$, $U''(C_{1i}) < 0$.

Income in period one is given by:

$$Y_{1i} = w_1(S_i)(1 - I_i) \tag{1}$$

Frazis and Loewenstein (2005) and Blundell et al. (2019). Moreover, training can improve the quality of work and can have positive effects on non-pecuniary outcomes Ruhose and Weilage (2019).

[7] An analogous human capital effect can be generated in a model of a firm's investment decision when the working life of workers increases. When workers are not perfectly mobile (Acemoglu and Pischke, 1998, 1999), the intuition is straight forward in our model: The longer the payout period for the investment of the firm, i.e. the longer the worker stays in the firm, the higher the investment in human capital. In the data we cannot observe if the training is initiated and financed by the worker or the firm. However, for the empirical test of the human capital effect, this information is not required as we focus on the effect of working life on training investment and not on the incidence of the human capital investment.

Each individual earns wage $w_1(S_i)$.[8] The wage in period one depends on the initial level of schooling, which is determined exogenously prior to period one. The wage is increasing with education, specifically we assume $w_1'(S_i) > 0$, $w_1''(S_i) < 0$. In this period, the individual decides on his or her time investment in human capital, I_i, through participation in on-the-job training measures. Time investment in on-the-job training implies opportunity costs, which are characterized by a fraction of foregone labour income, $w_1(S_i)I_i$, in the model. Hence, individuals with high education face higher opportunity costs.

2. Period

Income in period two is given by:

$$Y_{2i} = w_2(S_i, I_i)R_i \tag{2}$$

R_i is the duration of the remaining working life and $w_2(S_i, I_i)$ is the wage earned in period two. Individuals in period two collect the returns from their human capital investment made in period one through their wage, as the wage in period two is a function of their training and their initial schooling, i.e. $w_2(I_i, S_i)$. Note that by assumption, $w_{2I} > 0$ and $w_{2II} < 0$. Further, in order to capture the dynamic complementarity between initial schooling, S_i, and human capital investment, I_i, we assume $w_{2IS} > 0$.

3. Period

Period three is the period of retirement. The duration of period three is $T_i - R_i$, where T_i is the individual life expectancy. We assume that retirement is a discrete decision to exit the labor market completely. Income in the retirement period is covered by the state pension, which is a fraction α, with $\alpha < 1$, of labor income in period two.

[8] We assume that individuals are either full time employed or unemployed. Wages of the unemployed are zero. Thus an increase in the wage can result from entering employment or an increase in earnings in full time employment.

$$Y_3 = \alpha w_2 (S_i, I_i) (T_i - R_i) \tag{3}$$

Utility over all three periods is given by:

$$U_G = U(C_{1i}) + \beta U(C_{2i}) R_i + \beta^2 U(C_{3i})(T_i - R_i)$$

where β is the discount factors, with $\beta < 1$. The intertemporal budget constraint is:

$$y_{1i} + \beta y_{2i} + \beta^2 y_{3i} \geq C_{1i} + \beta C_{2i} R_i + \beta^2 C_{3i}(T_i - R_i)$$

Hence, the maximization problem can be depicted by the Lagrangian:

$$L = U_G + \lambda \left[y_{1i} + \beta y_{2i} + \beta^2 y_{3i} - \left(C_{1i} + \beta C_{2i} R_i + \beta^2 C_{3i} (T_i - R_i) \right) \right] \tag{4}$$

The details of the solutions and the set of First Order Conditions are presented in the Appendix.

Using comparative statics, we derive three main propositions from the human capital model. Firstly, in Equation 5, we show how the effect of an exogenous increase in the remaining working life duration (R_i), affects the investment in training (I_i).

$$\frac{\partial I_i}{\partial R_i} = \frac{[\alpha\beta - 1] \, w_{2I} (S_i, I_i)}{[R_i + (T_i - R_i)\alpha\beta] \, w_{2II}(S_i, I_i)} \tag{5}$$

By assumption, $w_{2I} > 0$ and $w_{2II} < 0$. Further, $\alpha\beta - 1$ is negative [9]. Therefore the expression above is positive and we can derive the following proposition.

Proposition I (Working life effect)

The effect of an increase in the working life on training is positive.

Further, we discuss how initial schooling affects the amount of time allocated towards on-

[9] This follows from $\alpha < 1$ and $\beta < 1$.

the-job training. The relation between initial schooling and training is captured by Equation 6.[10]

$$\frac{\partial I_i}{\partial S_i} \gtreqless 0 \Longleftrightarrow w_{2IS}(S_i, I_i)\frac{S_i}{w_{2I}(S_i, I_i)} \gtreqless w_{1S}\frac{S_i}{w_1(S_i)} \tag{6}$$

The above shows that as long as the relative return to human capital investment collected in period two, $w_{2IS}(S_i, I_i)$, proportionally out-weighs the opportunity costs, i.e. the return of schooling received in period one, w_{1S}, human capital investment will increase in schooling, i.e. $\frac{\partial I_i}{\partial S_i} > 0$.

Proposition II (Initial education effect)

The level of time investment in training rises with initial schooling as long as the additional returns to human capital investment related to the initial level of schooling are larger than the opportunity cost of the investment.

Finally, in Equation 7, we discuss how the effect of the increase in the working life on training varies by the initial level of schooling. This effect can be decomposed in two effects.

$$\frac{\partial}{\partial S_i}\left[\frac{\partial I_i}{\partial R_i}\right] = \frac{\partial I_i^2}{\partial R_i \partial S_i} + \frac{\partial I_i^2}{\partial R_i \partial I_i}\frac{\partial I_i}{\partial S_i} \tag{7}$$

The first effect captures the direct effect of how the working life effect, $\frac{\partial I_i}{\partial R_i}$ (Proposition I), changes with initial schooling and the second effect captures the indirect effect of how the working life effect is influenced by the existing level of training given the initial schooling effect $\frac{\partial I_i}{\partial S_i}$ (Proposition II). Under the assumption of complementarity between schooling and training (or, alternatively, about the utility cost of training, see the Appendix), the direct effect is positive. Therefore, individuals with a higher initial level of schooling have a stronger incentive to invest in training in response to an increase in their working life. However, the indirect effect is negative as the returns to training are positive, albeit decreasing. Given an increase in the working life, individuals with higher levels of training, therefore, have

[10] For a detailed derivation of Equation 6 see Appendix

less incentive to further increase their training investments. The overall effect ultimately depends on the two opposing sub-effects, as summarized in Proposition III. In the Appendix we present a detailed formal derivation of the two opposing effects and their respective signs.

Proposition III (Working life effect by education)

The reform effect rises with the initial level of schooling, as long as the positive direct effect outweighs the negative indirect effect.

The remainder of this paper empirically assesses the predictions derived from the theoretical model depicted above. First we will present descriptive evidence to provide empirical support for Propositions I and II. Then, we exploit the variation in the working life induced by the pension reform to causally test Propositions I and III.

3 Institutional Setting and Data

3.1 Pension reform

Before we turn to the empirical analysis, we summarize the relevant aspects of the German pension system and the 1999 pension reform that induced exogenous variation in the working life.

The statutory public pension system is the central part of the pension system in Germany. It covers more than 80% of the workforce with the exceptions of groups that are not subject to compulsory pension insurance, comprising, most importantly, civil servants, and the self-employed. It includes old-age pensions, disability pensions, and survivors' benefits. The system is financed by a pay-as-you-go (PAYG) scheme and has a strong contributory link: pension benefits depend on the entire working history. The pension system provides several pathways into early retirement, i.e. claiming retirement benefits before reaching the normal retirement age. In this analysis, we focus on the *pension for women* that allows for drawing benefits starting from age 60.[11]

[11] In addition early retirement is possible via : (1) the *invalidity pension*; (2) the *pension after unemployment*

The 1999 reform abolished the *pension for women* for cohorts born after 1951. Effectively, the reform raised the early retirement age (ERA) for most women from age 60 to age 63, therefore increasing their working life.[12] Women born before 1952 could claim the *pension for women* if they fulfilled certain qualifying conditions. The eligibility criteria were: (i) at least 15 years of pension insurance contributions overall; and (ii) at least 10 years of pension insurance contributions after the age of 40. According to Geyer and Welteke (2019), about 60% of all women born in 1951 were eligible for the old-age pension for women. In our empirical analysis, we focus only on employed women, about 75% of these women fulfill the criteria and, therefore are eligibility for this pathway (see Table 6 below). The pension reform was implemented when affected women born in 1952 were aged 47. Thus, these women had still a long horizon to benefit from human capital investments.

Geyer and Welteke (2019) and Geyer et al. (2018) evaluate the labor market effects of the pension reform based on administrative data of the public pension insurance accounts and the microcensus, respectively. Several findings of these studies are relevant for the following empirical analysis. Most importantly, the increase in the ERA has a sizable positive effect on the working life on individuals, which is the necessary condition for an increase in human capital investment. In more detail, employment rates for eligible women aged between 60 and 62 increase by about 15 percentage points, the combined effect on inactivity and unemployment is, at about 12 percentage points, a similar size. Second, Geyer and Welteke (2019) document that the pension reform has no significant effect on employment, unemployment, disability or inactivity before the age of 60 (we replicate these results based on the microcensus in Section 4.4. This implies that estimation results on the effect on human capital accumulation for employed women before the age of 60 are not affected by selection effects into employment induced by the pension reform. Third, there exists effect

or after old-age part-time work; and (3) the *pension for the long-term insured*, for more details see Geyer et al. (2018). For a more general description on the German pension system, see Boersch-Supan and Wilke (2004).

[12]The *pension after unemployment or after old-age part-time work* was abolished at the same time as the *pension for women*. However, this does not affect our analysis, as the ERA for this pension type was already 63.

heterogeneity by initial education (Geyer et al., 2018). Specifically, while employment effects for women aged 60-62 are larger for higher educated women (9.5%), the effects are still sizable and significant for women without higher education (8.2%). We return to this finding when we discuss heterogeneity in section 4.7.

3.2 The German Microcensus

For the analysis, we use the German microcensus. The microcensus is an annual, household based survey with representative information about the population and the labor market in Germany. Participation in the survey is mandatory. It has a sampling fraction of one percent of the German population (about 370,000 households) and constitutes the largest annual household survey in Europe (RDC of the Federal Statistical Office and Statistical Offices of the Laender, 2015).

In the main analysis, we concentrate on employed[13] women younger than 60 years born in 1951 and 1952, who we observe through the 2005 to 2012 period. For these years, the data include information about the month of birth and consistent information about participation in on-the-job training.[14] We observe around 1,250 individuals for each birth month in our sample. Thus, overall, the sample includes information for about 30,000 women born in the two cohorts of interest. The microcensus includes important socio-demographic variables, including age, education,[15] marital status and firm size. These are used as control variables.

Table 1 shows descriptive statistics for the female working population in column 1 as well as for our sample cohorts in column 2. The key difference between the two samples is age, shown in the last row. While females in our estimation sample average 55.9 years of age, the average age of the female working population is 41.1 years. Many of the other (small) differences likely reflect cohort or age trends. The first row shows the incidence of on-the-job

[13]Women working in "mini-jobs" are not counted as employed.

[14]Before 2005, the microcensus only provides information about the birth year and the definition of training changes at several points in time. Therefore, extending the sample to include years before 2005 would require additional assumptions.

[15]Education is measured using ISCED 2011 levels: based on this information, we define women without a college degree or with a college degree or more.

training participation. Our (older) estimation sample participates less often in training (16.3 percent) than the (younger) female working population (20.5 percent). Next, we examine the incidence of training more systematically.

3.3 On the job training: descriptive evidence

The microcensus provides information showing if an employed person has participated in on-the-job training during the last twelve months. The training information specifically includes courses that are related to career development, e.g. to improve management, computer, or rhetoric skills.[16]

In the following, we describe how training participation varies by age and education thus allowing us to provide descriptive evidence for Propositions I and II.[17] Figure 1 shows the age pattern of training for all employed women born between 1940 and 1997. As expected, we find a hump shaped age profile. While training rates are low at very young ages, they rapidly increase during the first years of the working career. Toward the end of the working life, i.e., after the age of 50, training strongly decline. Specifically, Figure 1 suggests that training rates decline between age 53 and 58 by about 10 percentage points. Hence, the period after the age of 50, which we study in the causal analysis, is particularly sensitive to changes in the incentives for training. This implies even small changes in the incentives to invest in training can lead to large responses. In summary, the pattern of Figure 1 provides first support for Proposition I. Training is reduced when the working horizon, i.e. the payout period, decreases.

[Figure 1 about here]

In Figure 2 we turn to the training pattern by initial education. In line with Proposition II we find that training increases with the level of education. Specifically, employed women

[16]The exact wording of the question reads: *Did you, in the last 12 months take part in any form of vocational training? Examples of vocational training are: occupational re-training, courses for career developmen,t and general training courses, for example, in the fields computing, management, public speaking.*

[17]For more information on training participation and its measurement in Germany see, among others, Eisermann et al. (2014).

born in 1951 with no college degree (ISCED < 5) have training rates of about 15.4 percent. In contrast women with a college degree or higher tertiary education (ISCED > 5) have training rates of 31.7 percent.

[Figure 2 about here]

4 Causal Analysis: RDD

4.1 Empirical method

In the empirical analysis we exploit the 1999 pension reform to estimate the effect of an increase in working life on human capital investment and to provide a causal test of Propositions I and III using a RDD. The reform leads to an arbitrary and distinct cutoff for women born before and after December 31, 1951, which determines the assignment into the treatment and control groups.

More formally, in the empirical analysis, the woman's month of birth is the running variable M, which determines treatment D as one if she was born after December 31, 1951, and zero otherwise:

$$D_i = \begin{cases} 1, & if\ M_i \geq c \\ 0, & if\ M_i < c \end{cases} \tag{8}$$

For the identification of a causal effect, it is important that no manipulation of the month of birth for women born in 1951 and 1952 as well as no selection into or out-of treatment is possible. As a result, the treatment and control groups should be otherwise comparable around the cut-off. We provide supporting evidence based on balancing tests of important pre-policy covariates of the 1951 and 1952 birth cohorts, as well as by moving the cutoff to hypothetical placebo dates. Moreover, as discussed e.g. in Geyer and Welteke (2019), no other relevant policy reform differently affected women born in 1951 and 1952.

In the main specification, we implement the RDD in the following regression model:

$$y_i = \alpha + \beta D_i + \gamma_0 f(M_i - c) + \gamma_1 D_i f(M_i - c) + X_i \delta + \varepsilon_{it} \qquad (9)$$

D_i is a dummy specifying treatment, that is equal to 1 if a woman is born on 1.1.1952 or later, and 0 otherwise. A woman's month of birth is described by M_i and c is the cut-off date for the increase in early retirement age, ERA (January 1, 1952). Therefore, f is a function of the difference between a woman's birth date and the beginning of the ERA increase $M_i - c$, i.e. the running variable. This function is interacted with the treatment variable D_i to allow for different slopes before and after the cutoff. In addition, we account for further explanatory variables (X), including age, education, marital status, firm size and regional information. We implement this specification using either global polynomials for f up to the third degree, as well as local polynomials. Further, we allow for various bandwidth choices.[18]

The outcome variable Y in our analysis is on-the-job training, which is dichotomous i.e. taking on the value 1 if a woman has participated in training in the last twelve months and 0 if she has not. In the main specification, we implement the RDD in a linear probability model.[19]

4.2 Graphical analysis

Before turning to the estimation result, we provide graphical evidence how an increase in the working life affects training. In Figure 3, we present the pattern of training for the relevant cohorts, i.e. women born in 1951 and 1952, for the ages that we can observe in the data. The figure clearly shows that women with a longer working horizon, i.e. women of cohort 1952, have higher training rates than women born in 1951. As expected, the difference gets smaller close to age 60, i.e. when both groups have only a short working horizon.

[Figure 3 about here]

[18]Local polynomials are estimated using the Stata package "rdrobust" (Calonico et al., 2018).
[19]Estimation results based on a Probit model (not reported) show very similar results.

In the next figure, we present how training participation varies around the cut-off date. Specifically, Figure 4 shows participation rates in training by month of birth 12 months before and after the cut-off birth date, 1.1.1952. The share of employed women participating in training is clearly higher after the cut-off. The average rate of participation in the 12 month before the cut-off date is approximately 15.4 %. After the cut-off date, the graphs shows a jump in the average rate of training participation for employed women under 60 to more than 16.5 percent. In the next section, we examine the robustness and significance of the graphical evidence using various choices in the RDD framework.

[Figure 4 about here]

4.3 Regression results - overall effects

To quantify the effect of an increase in the working life on the investment into human capital, we use the RDD as described in Section 4.1. In the analysis, we focus on all employed women in their later working life, i.e. when they are aged between 53 and 60.

In Table 2, we present the estimation results for different specifications with observations 12 months before and after the cut-off date. We consider regressions with global polynomials, with linear, quadratic, and cubic specifications, as well as with local linear and local quadratic specifications. Moreover, the table includes these regressions without and with additional control variables. Standard errors are reported in brackets and are clustered at the birth month level. Our inference is robust to a specification without clustered standard errors, as suggested by Kolesar and Rothe (2018).

[Table 2 about here]

The results of these different specifications all point in the same direction despite some expected differences in the magnitude of the point estimates: the increase in the early retirement age has a positive and significant effect on the investment in training. Most point estimates show that participation in training increases between 2.5-5 percentage points, which translates into a relative increase of about 20-30% given the pre-reform share in training of 15.4%.

Only the global linear specification in the top panel (without covariates) in column 1 is not statistically significant, although it is positive. However, this is the least flexible version of conditioning on the running variable and, therefore, -*a priori*- not the preferred specification. In contrast, the local linear regressions in Columns 4 to 5 of Table 2 consistently show positive and significant estimates in similar magnitude across all specifications.

4.4 Balancing tests

[Table 3 about here]

The assumption underlying the RDD is that other factors vary smoothly across the cutoff. We provide support for this assumption by using individual control variables as outcomes using the same specification as in Table 2. The resulting estimates are presented in Table 3. All control variables, except the share of women with high income, are insignificant at conventional levels of statistical significance. Yet, the income variable is significant only at the 10% level of statistical significance. The balancing in these variables is also reflected in the fact that adding the controls to our main specification hardly affects the estimates for the training outcome, as already seen in Table 2.

[Table 4 about here]

As additional balancing check, we test for direct effects on employment of the reform for pre-treatment ages. This is of particular relevance because the microcensus data set is a repeated cross-section and we base our analysis sample on women in employment before the age of 60. Thus, any effects of the reform on employment could induce sample selection and bias our estimates. Table 4 shows corresponding estimates for a sample of all women aged 53-60, independent of working status. Here, we show results of an RD specification similar to Equation 9 but for the population of all women. Unemployment and employment levels before the age of 60 are used as outcome variables. We show that there are no significant effects on unemployment or employment before the age of 60, except for the quadratic global polynomial and the linear local polynomial specifications, both show significant estimates at the 10 and 5 percent levels, respectively, when looking at the employment outcome. However, when conditioning on covariates these estimates become insignificant. Moreover, in Appendix Figures A.1 and A.2, we show the corresponding RDD graphs for the employment/unemployment balancing. It is clearly visible that there are no effects on unemployment or employment before the age of 60.[20] Taken together, it is clear that there are no direct effects on employment or unemployment before the age of sixty. As a result, changes in the incidence of training before the age of 60 are not caused by differential selection into the sample for the affected age groups.

4.5 Placebo analysis

[Table 5 about here]

In addition, we conduct two placebo analyses, which are presented in Table 5. In the first placebo analysis, we artificially shift the cut-off date by one year to 1.1.1950 and, in the second placebo analysis, to 1.1.1952. Importantly, the pension rules are identical before and after the chosen placebo cut-offs. The shift of one entire year is of particular relevance as this could capture potential seasonal effects related to the December to January timing of the reform introduction.

The result from this additional analysis support our identification strategy: the treatment effect is very close to zero and not significant in both placebo specifications, with and without additional explanatory variables. Moreover, these estimated effects clearly differ from our main findings in Table 2.

[20]This result replicates findings from earlier studies on labor supply effects before the age of sixty of this particular reform (Geyer and Welteke, 2017).

4.6 Further robustness checks: bandwidth and donut regressions

Bandwidth choices can affect RD estimates, so we carefully examine if and how our main results are sensitive to these. First, in Appendix Table A.2, we replicate our main results from Table 2 but use a bandwidth of six months rather than a full year on both sides of the cutoff, for all specifications of the running variable. Second, in Appendix Table A.3, we show that our results also hold for additional bandwidth choices for local polynomials, where we present result for bandwidth choices of 6, 9, and 12 months. The bandwidth of 6 months is the chosen bandwidth of the endogenous bandwidth selection routine "rdbwselect", using the mean squared error criterion and a triangular kernel (Calonico et al., 2014). In all cases, our estimates remain in the same ballpark. All estimates except the second column in Appendix Table A.2, which shows results of the global analysis with a quadratic polynomial, remain statistically significant at the ten percent level or higher.

As additional robustness check, we also ensure that observations close to the cutoff do not cause our results. Specifically, we examine if observations close to the cutoff drive our effects by estimating effects from donut-RD regressions. We estimate different specification of Equation 9 without the one or two birth months closest to the cutoff on both sides. Appendix Table A.4 shows the resulting estimates for the various functional form choices, both without and with individual control variables. Some of the specifications without covariates lose statistical significance in the one-month donut, presented in the upper panel. However, overall this additional analysis confirms the main findings, with estimates of similar magnitudes throughout. Our results are not driven by observations close to the cutoff.

4.7 Effects by initial education

We now extend the empirical analysis and focus on effect heterogeneity along prior educational levels. We show descriptively that training participation positively correlates with prior educational levels in Section 3.3 (Proposition II). We now test if the reform effect also varies by prior educational level (Proposition III). Since we only focus on women close to the cut-off, to alleviate sample size issues, we aggregate the ISCED educational levels into two education groups of women with "college" degree and "non-college."

[Table 6 about here]

We find very strong differences by education for the different specifications presented in Table 6. Women with college education or more increase training by nearly 12 percentage points, which corresponds to a relative increase of about 35%. The effect for women without college education is estimated to be close to zero and not significant at conventional levels. Therefore, we conclude that the direct training effects outweigh the negative indirect effect (Proposition III), which explains why the human capital induced by an increase in working life increases with education.

4.8 Quantification of effects

As discussed in Section 3.1, the 1999 pension reform only affected the working life of women who fulfill the eligibility criteria for the so-called pension *for women*. The microcensus, which we base our analysis on, is a cross sectional data set without information about employment history. Therefore, we cannot directly determine eligibility within this data. As a result, our estimates should be interpreted as "intention-to-treat" (ITT) effects, giving a lower bound of the true effect.

To gauge information about actual eligibility, we use information from the longitudinal data of the Socio-economics panel (SOEP), the largest household panel data set available in Germany. In the representative SOEP data, about 76% of women employed before entering retirement were indeed eligible for this pathway into retirement. Further, the SOEP data show that about 72% of employment women without a college degree and 85% of women with a college degree fulfill the eligibility criteria.

With this information and the estimated effects (ITT) presented in Tables 2 and 6, we can derive the average treatment effect on the treated (ATT) for the different educational groups. These are presented in Table 7. Overall, the pattern of the ATT effects is similar to the ITT effects, but the effects are slightly larger. Importantly, the ATT effects also provide empirical support for Propositions I and III derived from the human capital model. The point estimates suggest that overall women training increases by 4.6 percentage points (column 1), for women with college the increase is over 13 percentage points (column 2), and the effect for women without college is close to zero (column 3). The point estimates imply a relative increase in training of 30% for all women and of 40% for women with college degree.

[Table 7 about here]

Overall, the magnitude of the estimated ITT and ATT effects are consistent with the strongly declining pattern of training after the age of 50, presented in Figure 1. As discussed above, training participation is particularly sensitive to changes in ages close to retirement that determine the remaining working life. Therefore, it is not surprising that an increase in the working life induced by the increase in retirement age from 60 to 63 has sizable effects on training.

5 Conclusion

In this paper, we provide causal evidence for the theory of human capital accumulation. In the empirical analysis, we use an exogenous change in the working life that increases the payout period for the human capital investment. Specifically, we exploit a sizable pension reform that increased the early retirement age for women between two adjacent cohorts from 60 to 63 years. The analysis is based on the German microcensus and exploits, using RDD, the cohort specific variation of the pension reform.

The empirical analysis offers support for three key propositions of the human capital model derived in the first part of the paper. First we provide descriptive evidence that training positively correlates with remaining working life and with initial educational levels. We then present causal evidence that an increase in working life induced by the pension reform has a positive effect on human capital investment and that this human capital effect increases with initial schooling. Specifically, our empirical results show that the increase in the retirement age has a sizable effect on human capital accumulation of employed women aged 63-60: depending on the specification, training increases by about 2.5-5 percentage points, which corresponds to an increase of 20-30% for these age groups. This finding is robust to changes in the bandwidth and for different specifications of the running variable in the RDD and is supported by a placebo test. Moreover, we show that the pension reform increases training for women with a college degree or more by 11 percentage points, which corresponds to a relative increase of about 35%. The effect for women without college degree is not significant.

Besides testing a key prediction of human capital theory for a large and relevant part of the working population, our results have important implications for the policy debate about pension reforms. This debate usually abstracts from the dynamic human capital investment that we document. Future work should examine the role of individual workers and firms in initiating the positive training effects that we document, adding to the still relatively underdeveloped literature on educational investments beyond initial schooling.

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Figures and Tables

Figure 1: Average on-the-job training participation by age, Cohorts: 1940-1997

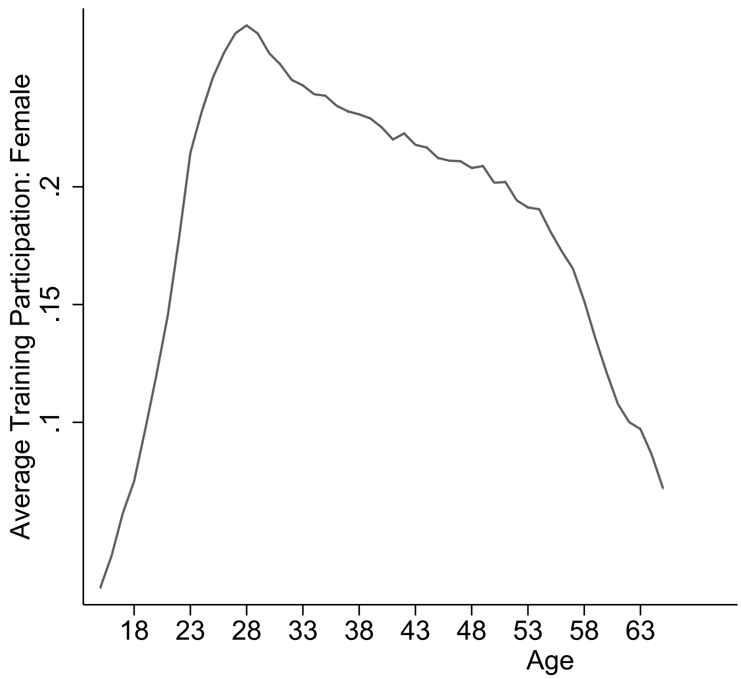


Figure 2: Average on-the-job training participation by ISCED groups

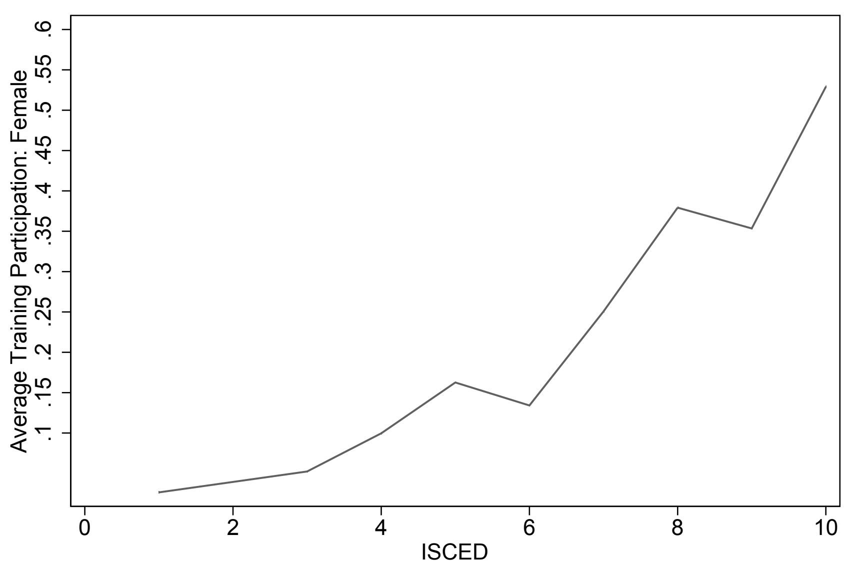


Figure 3: Average on-the-job training participation for sample group

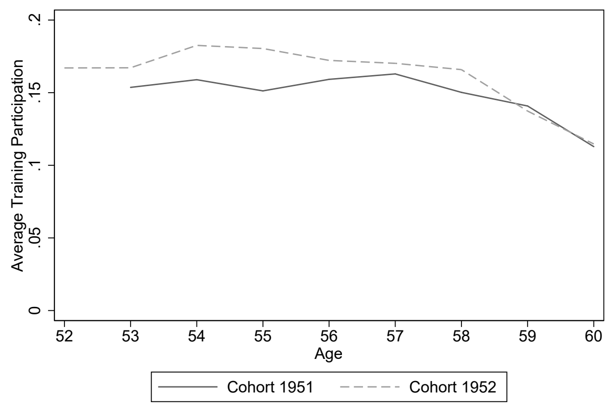
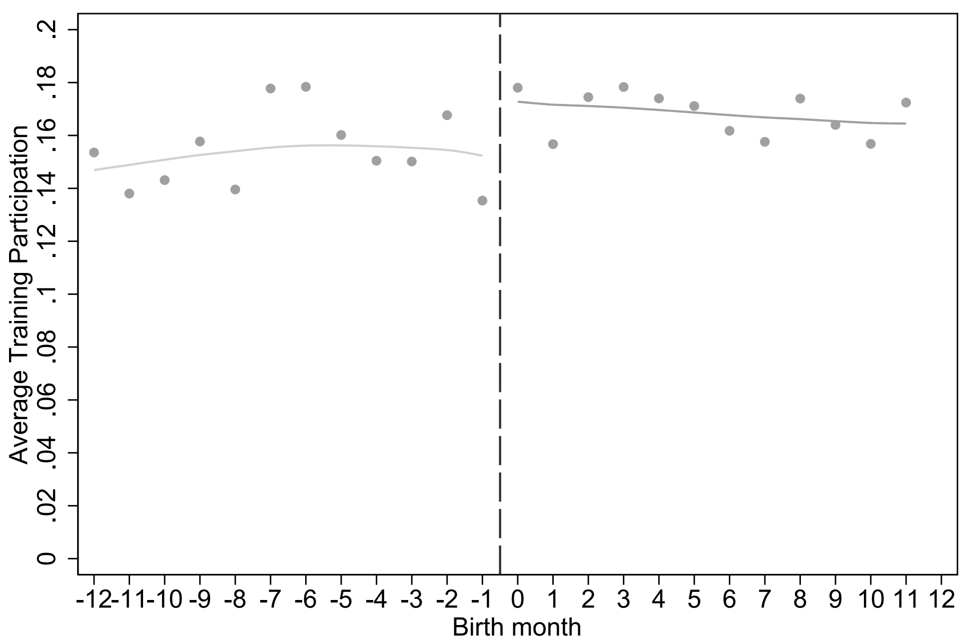


Figure 4: On the job training around the cut-off date



Notes: The fitted lines are local linear regressions using a first degree polynomial, a triangular kernel. In total, information for 13,658 individuals below the threshold and 14,873 individuals above the threshold are used.

Table 1: Summary Statistics

	Female Working Population (1)	Sample Cohorts 1951/1952 (2)
On-the-Job training participation	0.205	0.163
	(0.403)	(0.379)
Large Corporation	0.504	0.523
	(0.500)	(0.499)
High level education	0.225	0.194
	(0.417)	(0.395)
Medium level education	0.651	0.654
	(0.477)	(0.476)
High HH income	0.372	0.334
	(0.483)	(0.472)
Single	0.124	0.047
	(0.329)	(2.131)
West-Germany	0.810	0.763
	(0.0391)	(0.425)
Age	41.136	55.951
	(12.008)	(1.862)
Number of Observations	1,126,316	28,531

Source: Microcensus, own calculations. Average values of outcome variables and covariates. Standard deviations in parentheses.

Table 2: Regression Discontinuity: Main Results

	Global Polynomial			Local Polynomial	
	(1)	(2)	(3)	(4)	(5)
Without Covariates					
Treatment Variable	0.0157	0.0352	0.0502	0.0235	0.0418
	(0.0147)	(0.0142)	(0.0105)	(0.012)	(0.0131)
With Covariates					
Treatment Variable	0.0150	0.0358	0.0439	0.0252	0.0413
	(0.0082)	(0.0104)	(0.0110)	(0.0087)	(0.0097)
Running Variable	Linear	Quadratic	Cubic	Linear	Quadratic

Notes: Standard Errors in parentheses; clustered at birth month level. Sample includes twelve month before and after reform cutoff; Pre-Policy Mean: 15.41 percent.
Source: Microcensus 2005-2012, own calculations.

Table 3: Balancing

	Global Polynomial			Local Polynomial	
	(1)	(2)	(3)	(4)	(5)
College Education					
Treatment Variable	-0.0010	0.0087	0.0324	0.0029	0.0191
	(0.0175)	(0.0300)	(0.0322)	(0.0230)	(0.0307)
High Household Income					
Treatment Variable	-0.0168	-0.0295	0.0040	-0.0220	-0.0149
	(0.0108)	(0.0142)	(0.0168)	(0.0122)	(0.0186)
West					
Treatment Variable	0.0156	0.0056	-0.0097	0.01162	-0.0010
	(0.0192)	(0.0297)	(0.0415)	(0.0233)	(0.0326)
Big Company					
Treatment Variable	-0.0061	-0.0129	0.0221	-0.00900	0.0023
	(0.0096)	(0.0121)	(0.0213)	(0.0080)	(0.0104)
Single					
Treatment Variable	-0.0093	-0.0083	-0.0006	-0.0089*	-0.0050
	(0.0058)	(0.0065)	(0.0089)	(0.0051)	(0.0063)
Running Variable	Linear	Quadratic	Cubic	Linear	Quadratic

Notes: Standard Errors in parentheses. Sample includes twelve month before and after reform cutoff.
Source: Microcensus 2005-2012, own calculations.

Table 4: Employment Balancing

	Global Polynomial			Local Polynomial	
	(1)	(2)	(3)	(4)	(5)
Employment					
Without Covariates					
Treatment Variable	0.0150	0.0300	0.0019	0.0210	0.0178
	(0.0118)	(0.0126)	(0.020)	(0.0116)	(0.0142)
With Covariates					
Treatment Variable	0.0027	0.0002	-0.0009	0.0017	-0.0002
	(0.0028)	(0.0043)	(0.0063)	(0.0032)	(0.0050)
Running Variable	Linear	Quadratic	Cubic	Linear	Quadratic
Unemployment					
Without Covariates					
Treatment Variable	-0.0126	-0.167	0.02232	-0.0142	0.0004
	(0.0102)	(0.0124)	(0.0071)	(0.0106)	(0.0111)
With Covariates					
Treatment Variable	-0.0005	0.0005	0.0012	-0.0001	0.0008
	(0.0007)	(0.0006)	(0.0011)	(0.0072)	(0.0061)
Running Variable	Linear	Quadratic	Cubic	Linear	Quadratic

Notes: Standard Errors in parentheses. Sample includes twelve month before and after reform cutoff. Number of observations: 54,126 (without covariates) and 32,810 (with covariates).
Source: Microcensus 2005-2012, own calculations.

Table 5: Placebo Regression Discontinuity Designs

	Global Polynomial			Local Polynomial	
	(1)	(2)	(3)	(4)	(5)
Placebo 1950/51					
Without Covariates					
Treatment Variable	0.0056	0.0098	-0.0031	0.0017	0.0055
	(0.0149)	(0.0158)	(0.0235)	(0.0102)	(0.0153)
With Covariates					
Treatment Variable	0.0102	0.0138	0.0003	0.0063	0.0069
	(0.0155)	(0.0166)	(0.0249)	(0.0100)	(0.0151)
Running Variable	Linear	Quadratic	Cubic	Linear	Quadratic
Placebo 1952/53					
Without Covariates					
Treatment Variable	-0.0060	-0.0067	-0.0072	0.0001	0.0004
	(0.0088)	(0.0081)	(0.0119)	(0.0101)	(0.0128)
With Covariates					
Treatment Variable	-0.0041	-0.0048	-0.0084	-0.0001	0.0039
	(0.0083)	(0.0082)	(0.0109)	(0.0088)	(0.0112)
Running Variable	Linear	Quadratic	Cubic	Linear	Quadratic

Notes: Standard Errors in parentheses. Sample includes twelve month before and after reform cutoff; Pre-Policy Mean: 15.41 percent.
Source: Microcensus 2005-2012, own calculations.

Table 6: Heterogeneity by educational level

	Global Polynomial			Local Polynomial	
	(1)	(2)	(3)	(4)	(5)
Non-College					
Without Covariates					
Treatment Variable	0.0064	0.0117	0.0213	0.0085	0.0158
	(0.0091)	(0.0131)	(0.0174)	(0.0095)	(0.0133)
With Covariates					
Treatment Variable	0.0065	0.0133	0.0227	0.0093	0.0175
	(0.0096)	(0.0138)	(0.0175)	(0.0103)	(0.0139)
Running Variable	Linear	Quadratic	Cubic	Linear	Quadratic
College					
Without Covariates					
Treatment Variable	0.0557	0.1285	0.1505	0.0844	0.1383
	(0.0346)	(0.0373)	(0.0486)	(0.0354)	(0.0392)
With Covariates					
Treatment Variable	0.0573	0.1291	0.1409	0.0851	0.1325
	(0.0328)	(0.0350)	(0.0458)	(0.0330)	(0.0375)
Running Variable	Linear	Quadratic	Cubic	Linear	Quadratic

Notes: Standard Errors in parentheses. Sample includes twelve month before and after reform cutoff; Pre-Policy Mean: 15.41 percent.
Source: Microcensus 2005-2012, own calculations.

Table 7: Average Treatment effect on the treated (ATT)

	All (1)	College (2)	Non-College (3)
Global Polynomial			
Linear			
ATT	0.0197	0.0682	0.090
Relative Size ATT (in%)	12.82	21.52	7.16
Quadratic			
ATT	0.047	0.1536	0.0185
Relative Size ATT (in%)	30.59	48.48	15.79
Cubic			
ATT	0.0578	0.1677	0.0315
Relative Size ATT (in%)	37.50	52.91	26.95
Local Polynomial			
Linear			
ATT	0.0332	0.1013	0.0129
Relative Size ATT (in%)	21.53	31.19	11.03
Quadratic			
ATT	0.0543	0.1577	0.0242
Relative Size ATT (in%)	35.30	49.76	20.66
Observations	28,531	5,469	22,694
Eligibility (in%)	76	84	72

Notes: The ATT is derived by weighting the ITT effects presented in Tables 2 and 6 using the specification including covariates with the share of employed women eligible for the pension or women. The eligibility rates are calculate from the SOEP data. For the ITT estimates we choose the specification equivalent to column 2 in Table 2, with a global quadratic running variable and with control variables. The sample includes twelve month pre and post reform. Participation rates in training before the reform are 0.154, 0.317, 0.117 for all employed women, employed women with college and employed women without college, respectively.

Appendix Figures and Tables

Table A.1: Regression Discontinuity: Bandwith 6 months

	Global Polynomial			Local Polynomial	
	(1)	(2)	(3)	(4)	(5)
Without Covariates					
Treatment Variable	0.0329	0.0273	0.0945	0.0307	0.0559
	(0.0131)	(0.0142)	(0.0204)	(0.0130)	(0.0168)
With Covariates					
Treatment Variable	0.0349	0.0243	0.0679	0.0305	0.0428
	(0.0108)	(0.0137)	(0.0134)	(0.0134)	(0.0118)
Running Variable	Linear	Quadratic	Cubic	Linear	Quadratic

Notes: Standard Errors in parentheses; clustered at birth month level. Sample includes six month before and after reform cutoff; Pre-Policy Mean: 15.41 percent.
Source: Microcensus 2005-2012, own calculations.

Table A.2: Regression Discontinuity: Bandwith 6 months

	Global Polynomial			Local Polynomial	
	(1)	(2)	(3)	(4)	(5)
Without Covariates					
Treatment Variable	0.0329	0.0273	0.0945	0.0307	0.0559
	(0.0131)	(0.0142)	(0.0204)	(0.0130)	(0.0168)
With Covariates					
Treatment Variable	0.0349	0.0243	0.0679	0.0305	0.0428
	(0.0108)	(0.0137)	(0.0134)	(0.0134)	(0.0118)
Running Variable	Linear	Quadratic	Cubic	Linear	Quadratic

Notes: Standard Errors in parentheses; clustered at birth month level. Sample includes six month before and after reform cutoff; Pre-Policy Mean: 15.41 percent.
Source: Microcensus 2005-2012, own calculations.

Table A.3: Local Polynomial Regression for different Bandwidths

	6m	9m	12m	6m	9m	12m
Without Covariates						
Treatment Variable	0.0307	0.0307	0.0235	0.0559	0.0403	0.0418
	(0.0152)	(0.0130)	(0.0120)	(0.0168)	(0.0145)	(0.0131)
With Covariates						
Treatment Variable	0.0305	0.0321	0.0252	0.0428	0.0367	0.0413
	(0.0108)	(0.0094)	(0.0087)	(0.0118)	(0.0106)	(0.0100)
Running Variable	Linear	Linear	Linear	Quadratic	Quadratic	Quadratic

Notes: Standard Errors in parentheses;
Source: Microcensus 2005-2012, own calculations.

Table A.4: Donut Regression: Leaving out one or two month on each side of cutoff

	Global Polynomial			Local Polynomial	
	(1)	(2)	(3)	(4)	(5)
One month donut					
Without Covariates					
Treatment Variable	0.0126	0.0231	0.0214	0.0149	0.0241
	(0.0067)	(0.0102)	(0.0137)	(0.0064)	(0.0130)
With Covariates					
Treatment Variable	0.0142	0.0307	0.0319	0.0200	0.0352
	(0.0061)	(0.0076)	(0.0109)	(0.0051)	(0.0103)
Running Variable	Linear	Quadratic	Cubic	Linear	Quadratic
Two month donut					
Without Covariates					
Treatment Variable	0.0172	0.0307	0.0257	0.0197	0.0407
	(0.0080)	(0.0052)	(0.0071)	(0.0071)	(0.0089)
With Covariates					
Treatment Variable	0.0147	0.0311	0.0277	0.0191	0.0424
	(0.0078)	(0.0044)	(0.0044)	(0.0072)	(0.00652)
Running Variable	Linear	Quadratic	Cubic	Linear	Quadratic

Notes: Standard Errors in parentheses. Sample includes twelve month before and after reform cutoff. Source: Microcensus 2005-2012, own calculations.

Figure A.1: Employment effect before 60

Figure A.2: Unemployment effect before 60

Appendix: Theoretical Model

Based on the theoretical model outlined in section 2, we obtain the following maximization problem:

$$U_G = U\left(C_{1i}\right) + \beta U(C_{2i})R_i + \beta^2 U(C_{3i})(T_i - R_i)$$

max U_g subject to the intertemporal budget constraint:

$$y_{1i} + \beta y_{2i}R_i + \beta^2 y_{3i}(T_i - R_i) \geq \mathrm{C}_{1i} + \beta C_{2i}R_i + \beta^2 C_{3i}(T_i - R_i)$$

The Lagrangian is given by:

$$L = U_G + \lambda\left[y_{1i} + \beta y_{2i}R_i + \beta^2 y_{3i}(T_i - R_i) - (C_{1i} + \beta C_{2i}R_i + \beta^2 C_{3i}(T_i - R_i))\right]$$

The set of First Order Conditions i :

$$\frac{\partial L}{\partial C_{1i}} = 0 \Rightarrow U'(C_{1i}) - \lambda = 0 \tag{A.1}$$

$$\frac{\partial L}{\partial C_{2i}} = 0 \Rightarrow \beta R(U_i'(C_{2i}) - \lambda) = 0 \tag{A.2}$$

$$\frac{\partial L}{\partial C_{3i}} = 0 \Rightarrow \beta(T - R)\beta^2[\lambda - U(C_{3i})] = 0 \tag{A.3}$$

$$\frac{\partial L}{\partial I_i} = 0 \Rightarrow \lambda[-w_1(S_i) + (R_i\beta + (R_i - T_i)\alpha\beta^2)w_{2I}(S_i, I_i)] = 0 \tag{A.4}$$

Based on the set of First Order Conditions, we derive our results. Firstly, comparative statics yield Equation 5 in the main paper, i.e.

$$\frac{\partial I_i}{\partial R_i} = \frac{\left[\alpha\beta - 1\right] w_{2I}\left(S_i, I_i\right)}{\left[R_i + (T_i - R_i)\alpha\beta\right] w_{2II}(S_i, I_i)}$$

The reform effect is positive as long as $w_{2I} > 0$ and $w_{2II} < 0$. $\alpha\beta - 1$ is negative since $\beta < 1$ and $\alpha < 1$.

Secondly, Equation 6 in the main paper can be derived as illustrated below. Solving Equation A.10, i.e. L_I; for β, gives

$$\beta = \frac{1}{R}\left(\frac{w_1(S_i)}{w_{2I}(S_i, I_i)} + \alpha\beta^2(R - T)\right)$$

Taking L_{IS} and substituting in L_{IS}:

$$sign\ L_{IS} = sign\left[\frac{w_{2SI}(S_i, I_i)}{w_{2I}(S_i, I_i)} - \frac{w_{1I}(S_i)}{w_1(S_i)}\right]$$

The above can then be simplified, written in elasticity form and solved for $\frac{\partial I_i}{\partial S_i}$. This then ultimately results in Equation 6 in the main paper:

$$\frac{\partial I_i}{\partial S_i} \gtreqless 0 \iff w_{2IS}(S_i, I_i)\frac{S_i}{w_{2I}(S_i, I_i)} =: \eta_{w_{2S},S} \gtreqless \eta_{w_1,S} := w_{1S}\frac{S_i}{w_1(S_i)}$$

Lastly, we apply the multivariate chain rule, given that the reform effect is a function of R_i, S_i and I_i, which in itself is a function of s_i and R_i. Consequently, the size of the reform effect with respect to schooling can be derived by splitting up the effect into two components:

$$\frac{\partial}{\partial S_i}\left[\frac{\partial I_i}{\partial R_i}\right] = \frac{\partial I^2}{\partial R_i \partial S_i} + \frac{\partial I^2}{\partial R_i \partial I_i}\frac{\partial I}{\partial S_i}$$

Using comparative statics, it is possible to firstly derive:

$$\frac{\partial I^2}{\partial R_i \partial S_i} = \frac{\partial I_i}{\partial R_i}\left[\frac{w_{2SI}(S_i, I_i)}{w_{2I}(S_i, I_i)} - \frac{w_{2SII}(S_i, I_i)}{w_{2II}(S_i, I_i)}\right]$$

The above expression is positive as long as $w_{2SII}(S_i, I_i)$ is positive, meaning that the larger S, the slower is the decline in $w_{2I}(S_i, I_i)$ with I.

Secondly, it is possible to derive:

$$\frac{\partial I^2}{\partial R_i \partial I_i} = \frac{\partial I_i}{\partial R_i}\left[\frac{w_{2II}(S_i, I_i)}{w_{2I}(S_i, I_i)} - \frac{w_{2III}(S_i, I_i)}{w_{2II}(S_i, I_i)}\right]$$

With $w_{2III}(S_i, I_i) \leq 0$ or sufficiently small, the above expression will be negative. Considering, the expression derived for $\frac{\partial I_i}{\partial S_i}$, we can write:

$$\frac{\partial I^2}{\partial R_i \partial I_i}\frac{\partial I_i}{\partial S_i} \gtreqless 0 \Longleftrightarrow \left[\frac{w_{1I}(S_i)}{w_1(S_i)} - \frac{w_{2SI}(S_i, I_i)}{w_{2I}(S_i, I_i)}\right]$$

Thus, assuming that $\frac{\partial I_i}{\partial S_i}$ is positive, the reform effect consists of a positive direct and negative indirect effect and the overall effect depends on the relative size of the respective effects, as formulated in Proposition III in the main paper.

Appendix: Theoretical Model with Utility Cost

In the following we relax the key assumptions of the model presented in the main text (Section 2) and show that the propositions hold without the assumption of dynamic complementarities between initial schooling and further human capital accumulation when we introduce utility costs of training as in Blundell et al. (2019).

1. Period
Income in the first period is given by:

$$Y_{1i} = w_1(S_i)$$

2. Period
Income in period two is given by:

$$Y_{2i} = w_2(I_i)R_i \tag{A.5}$$

3. Period
Income in period three is given by:

$$Y_3 = \alpha w_2(I_i)(T_i - R_i) \tag{A.6}$$

Utility over all three periods is given by:

$$U_G = U_{1i} + \beta U(C_{2i})R_i + \beta^2 U_{3i}(T_i - R_i)$$

Where:

$$U_{1i} = U(C_{1i})$$

$$U_{2i} = U(C_{2i}) - a(S_i)I_i$$

$$U_{3i} = U(C_{3i})$$

Based on the empirical estimates of the structural model in Blundell et al. (2019), we assume that individuals face utility cost of training in period 2, $a(S_i)$, which fall with schooling S_i, i.e. $a(S_i)<0$.

Individuals maximize U_g subject to the intertemporal budget constraint which is given by:

$$y_{1i} + \beta y_{2i}R_i + \beta^2 y_{3i}(T_i - R_i) \geq \mathrm{C}_{1i} + \beta C_{2i}R_i + \beta^2 C_{3i}(T_i - R_i)$$

Hence, the lagrangian is:

$$L = U_G + \lambda \left[y_{1i} + \beta y_{2i}R_i + \beta^2 y_{3i}(T_i - R_i) - (C_{1i} + \beta C_{2i}R_i + \beta^2 C_{3i}(T_i - R_i))\right]$$

and the set of First Order Conditions are:

$$\frac{\partial L}{\partial C_{1i}} = 0 \Rightarrow U'(C_{1i} - a(S_i)I_i) - \lambda = 0 \tag{A.7}$$

$$\frac{\partial L}{\partial C_{2i}} = 0 \Rightarrow \beta R(U_i'(C_{2i}) - \lambda) = 0 \tag{A.8}$$

$$\frac{\partial L}{\partial C_{3i}} = 0 \Rightarrow \beta(T - R)\beta^2[\lambda - U(C_{3i})] = 0 \tag{A.9}$$

$$\frac{\partial L}{\partial I_i} = 0 \Rightarrow \lambda[-a_1(S_i) + (R_i\beta + (R_i - T_i)\alpha\beta^2)w_{2I}(I_i)] = 0. \tag{A.10}$$

Based on the set of First Order Conditions, we derive our results. Firstly, comparative statics yield Equation A.11, which is equal to Proposition I in the main paper.

$$\frac{\partial I_i}{\partial R_i} = \frac{[\alpha\beta - 1]\, w_2'\,(I_i)}{[R_i + (T_i - R_i)\alpha\beta]\, w_2''(I_i)} \tag{A.11}$$

As before, it holds that the reform effect is positive as long as $w_2' > 0$ and $w_2'' < 0$. $\alpha\beta - 1$ is negative since $\beta < 1$ and $\alpha < 1$.

Secondly, the effect of training participation with respect to schooling is captured by Equation A.12:

$$\frac{\partial I_i}{\partial S_i} = \frac{a'(S_i)}{\beta[R_i + (T_i - R_i)\alpha\beta]\, w_2''(I_i)} \tag{A.12}$$

As long as $a'(S_i) < 0$, the effect will be positive.

Lastly, we can derive the effect of schooling on the reform effect. Equation A.13 shows that the effect is positive as long as $w_2'''(I_i)$ is positive and $w_2'(I_i)w_2'''(I_i) > w_2''(I_i)^2$.

$$\frac{\partial}{\partial S_i}\left[\frac{\partial I_i}{\partial R_i}\right] = \frac{[\alpha\beta - 1]\, a'(S_i)\, [w_2''(I_i)^2 - w_2'(I_i)w_2'''(I_i)]}{\beta[R_i + (T_i - R_i)\alpha\beta]^2\, w_2''(I_i)^2 (\text{A.13})}$$