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Working life and human capital investment

Niklas Gohl
Peter Haan
Elisabeth Kurz
Felix Weinhardt

Abstract
This paper provides a novel test of a key prediction of human capital theory that educational investment decisions depend on the length of the pay-off period. We obtain causal estimates by leveraging a unique reform of the German public pension system that, across a sharp date-of-birth cutoff, increased the early retirement age by three years. Using RDD, DiD, and IV estimation strategies on census and household-panel data, we show that this reform causally increased educational investment in the form of on-the-job training. In contrast, non-job related training before retirement was not affected. We explore heterogeneity and additional outcomes.

Key words: human capital, retirement policies, RDD
JEL codes: J24; J26; H21

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Niklas Gohl, DIW Berlin, Potsdam University. Peter Haan, DIW Berlin, FU Berlin, Netspar. Elisabeth Kurz, European University Viadrina, DIW Berlin, CESifo, and IZW. Felix Weinhardt, European University Viadrina, DIW Berlin, CESifo, IZW and Centre for Economic Performance, London School of Economics.

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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. This important prediction is the basis for explaining the joint increases in life expectancy and educational investments witnessed in most countries starting in the twentieth century; see e.g. Soares (2005); Cervellati and Sunde (2013). Yet, causal empirical evidence that individuals indeed consider the length of the payout period when making decisions about human capital investment is extremely scarce. For the developed world, the only causal study we are aware of is by Oster et al. (2013), who uses variation in life expectancy driven by Huntington disease realizations. Their key finding is that the duration of the expected payoff period significantly affects contemporaneous investment decisions. However, Huntington disease is a very serious condition and resulting variation in payoff periods can be extremely salient to affected individuals. Thus, it remains undetermined, if more representative and healthy individuals have similar foresight when making investment decisions.[1] Importantly, this generalization is required when we use human capital theory to explain the society-wide joint increases in life-expectancy and educational investments.

In this paper, we propose a novel empirical test of the key prediction of the human capital theory that the length of the payoff period causally affects human capital investment decisions. We contribute to the literature by testing this prediction in a general setting: we study the human capital investment decisions of training participation for women in their later working career (between the ages of 47 to 59).

For the identification, we exploit a pension reform in Germany that increased the early retirement age of women by three years. Since working life is largely determined by state pension rules, exogenous changes in pension rules provide quasi-random variation in the duration of the working life.[2] Thus, we exploit an exogenous increase in the working life induced by a sizable pension reform to study

[1] Learning-by-doing is an alternative explanation for educational investments over the life-course (Killingsworth, 1982; Foster and Rosenzweig, 1995).

[2] As shown in the Appendix, the theoretical prediction that links the duration to the payoff period to educational investments readily extends to the case of on-the-job training.

effects on on-the-job training.

The pension reform we study has two features that make it particularly well suited to provide causal evidence on the effect of working life on human capital investment. First, 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 or later, this pathway was closed; these women can enter retirement only at the age of 63. This means that not only does this reform provides a sharp cutoff, it also provides large variation at the cutoff. In the context of pension reforms, this is an unusual feature, as such reforms are generally phased-in on a (birth)month-to-month basis or provide only smaller variation. As a second key feature, the pension reform was already announced in 1998 and implemented in 1999. Thus, the affected women, i.e. women born in 1952 and aged 47, still had a long remaining working life to benefit from human capital investment.

We estimate the effect of the increase in the retirement age using two separate data sets. Our main analysis is based on the German Microcensus. This is a representative yearly household survey that covers 1% of all German households (about 370,000 households per year). The data includes detailed information about specific job-related training, which we use to measure post-schooling human capital investment. Importantly, the sample size of this household survey is unusually large, allowing regression discontinuity design (RDD) estimation. We complement the analysis using longitudinal data from the German Socio Economic Panel Study (SOEP). Sample size limitations do not allow estimating an RDD, therefore we use a difference-in-differences (DiD) design in which we compare outcomes of the treatment group, women born in 1952 and 1953, with those of the control group, women born in 1951 and 1950, before and after the introduction of the pension reform. In addition to the effect on training, we estimate the implications of the pension reform on labor market outcomes, i.e. employment measures, wages and job satisfaction. Moreover, we take advantage of the panel nature of the SOEP to examine the mechanism between the longer working life (after age 60) and training (before age 60). Specifically, we test if women who increased employment after the age of 60 invested more in training. We propose an Instrumental Variable (IV) strategy to account for the simultaneity between employment and training (Ben-

Porath, 1967), while using the pension reform to instrument employment after the age of 60.

Our main finding is that an increase in the working life causally increases human capital investment: based on the Microcensus, we show that that training - measured as participation in training in the last 12 months -increases depending on the specification by about 2.5-5 percentage points. Depending on the specification, the point estimates correspond to a relative increase of about 20-30%, which suggests that an increase in the working life has sizable effects on training. We investigate heterogeneity and find that the positive reform effect is driven by individuals who have higher education. The pension reform increases training for women with a college degree or more by about 13 percentage points. In contrast, the effect for women without college degree is not significant. Investigating further heterogeneity, we do not find evidence that this positive response is limited to specific firms or regions. We test the robustness of our main result using balancing checks, placebo analysis, donut-regressions, and variations to the specification of the running variable (birth month) as well as bandwidth choices. A range of specifications using parametric and non-parametric estimators all return similar results.

The DiD-analysis based on the SOEP confirms the positive effect of the pension reform on training. In contrast, we do not find a significant effect of the pension reform on the other labor market outcome variables before the age of 60. Finally, we provide evidence for the link between longer employment and training. Specifically, the IV-estimator suggests that training is higher for the treated women due to the exogenous increase in employment induced by the reform. This IV-estimate is only significant at the 15% level. This is related to the small sample size for this part of the analysis and not to a small point estimate.

Taken together, our results fail to reject that individuals do not take working life into account when making human capital investment decisions, which is in line with theory.

Our study is related to several strands of the literature. Most importantly, we contribute to empirical studies related to human capital theory, which estimates 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 that matters for investment in human capital, but the survival rates during working 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 heath 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 realizations or information (genetic testing) undertaken between the ages of 17 and 35.[3] In developing countries, Jayachandran and Lleras-Muney (2009) use a strong decline in maternal death rates in Sri Lanka and 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 complements 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. Our main specification uses 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 and training. In general, these studies document

[3]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.

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[4], or they are based on structural retirement models[5]. Crucially, these studies typically assume an exogenous process of human capital investment, thus implying 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 that is not consistent with the assumption of an exogenous human capital process and, similarly, Blundell et al. (2019) document for women that human capital accumulation through training has positive effects that partly compensate for the negative career effects of children. Several papers also show that reducing the generosity of 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.[6] Lastly, 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, and employment probability.[7]

The paper is organized as follows. Section 2 describes the German public pension system, describes the 1999 pension reform, introduces the data, and provides descriptive evidence on training. In Section 3, we describe the RDD estimation method and document causal evidence from graphical and regression analyses. In Section 4 we present the longitudinal analysis. Finally, Section 5 concludes.

[4]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).

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

[6]Investigating further effects of pension reform, there is some empirical evidence that pension reforms have small effects on life expectancy (Kuhn et al., 2010; Fitzpatrick and Moore, 2018).

[7]See, among others, Pischke (2001), Zweimueller and Winter-Ebmer (2000), Barrett and O'Connell (2001), Leuven (2005), Frazis and Loewenstein (2005), Picchio and van Ours (2011), and Ruhose and Weilage (2019).

2 Institutional Setting and Data

2.1 Pension reform

Before turning to the empirical analysis, we start by summarizing the relevant aspects about the German pension system and the 1999 pension reform[8] 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, most important civil servants, and the self-employed. It includes old-age pensions, disability pensions, and survivorâs 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*, which allows drawing benefits starting from age 60.[9]

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, thusly increasing the working life.[10] 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; 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 who are neither self-employed nor civil servants; about 89% of these women fulfill the criteria and, therefore, are eligible for this

[8]Note, the reform was already passed and announced in late 1997, hence in the empirical analysis we chose 1998 and later as the post-reform period.

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

[10]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.

pathway. 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.

2.2 Data

For the analysis, we use two different data sets, each representative of the German population. The German Microcensus, a cross sectional survey that covers one percent of the German population, and the Socio-Economic panel (SOEP), a longitudinal survey of German households carried out since 1984. The data sets are complementary and have different advantages. With the large sample size of the Microcensus, it is possible to compare the training participation of women in two adjacent cohorts around the cut-off, i.e. women born in 1952 and women born in 1951, and to estimate the causal effect of the pension reform on training using a RDD with the month of birth as the running variable. Moreover, the sample size allows for studying the effect for different subgroups and to focus on effect heterogeneity.

In the SOEP data, we can use the longitudinal dimension and have information about the working history of individuals. Thus, we can explicitly focus on women who fulfill the eligibility criteria and who were affected by the pension reform. Moreover, SOEP allows us to study not only the effect of the pension reform on training but also on other labor market outcomes and we can study the effects over a longer period, i.e. just after the announcement of the reform. Finally, based on the SOEP, we can not only study the effect of the pension reform on training and employment jointly but can also analyze the link between training participation and employment. The sample size of the SOEP is considerably smaller than in the Microcensus. To establish causality in the SOEP data, we exploit its longitudinal dimension and compare the training participation of treated women, i.e. women born in 1952 and 1953, with the training participation of a control group, i.e. women born in 1951 and 1950, before and after the pension reform, using a difference-in-differences framework.

Both data sets include information about training participation; however the definition of training and the question design differs. Therefore, the level of training rates strongly differ between the two data sets; see Eisermann et al. (2014). In the following, we describe the data sets and provide first descriptive information about training participation for women before and after the pension reform.

2.2.1 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[11] women younger than 60 years born in 1951 and 1952, who we observe from 2005 through 2012. For these years, the data include information about the month of birth and consistent information about participation in on-the-job training.[12] We observe around 1,250 individuals for each birth month in our sample. Thus, overall, the sample includes information on about 30,000 women born in the two cohorts of interest. The Microcensus includes important socio-demographic variables, such as age, education,[13] marital status, household income, and firm size. We consider college education and the geographical "West"-dummy as predetermined and include these as controls. The other variables are potentially endogenous, which we keep in mind when using these for balancing checks.

The Microcensus provides in addition information if an employed person has participated in on-the-job training during the twelve months prior to the survey. The training information includes specifically courses that are related to career development, e.g. to improve management, computer, or rhetoric skills.[14] A further

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

[12]Before 2005, the Microcensus only provides information about the birth year and the definition of training changes at several points in time. Therefore, the extension of the sample before 2005 would require additional assumptions.

[13]Education is measured with ISCED 2011 levels: based on this information with define women without college degree or with college degree or more.

[14]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 development, and general training courses in, for example, the fields computing,*

question that is included relates to training that is not job-related. Examples of such training are classes in music, sport and health, cooking, or art that many individuals take in their free time, offered through a network of "Volkshochschulen."

In Table 1, we provide descriptive information about the key variables in our sample and compare our sample, employed women born in 1951 and 1952, to all working women born between 1940 and 1997, which we observe over the 2005 to 2012 period in the Microcensus. By definition, women born in 1951 and 1952 are older with a lower level of training participation. The difference in the training participation is related to the different age composition in the estimation sample and related to cohort effects.

In Figure 1, we provide further evidence about the age and cohort pattern of training for employed women. In Figure 1, we focus on all employed women born between 1940 and 1997. We find a declining pattern that is explained by cohort and age effects. Training rates are above 25% at the age of 30 and then monotonically decline to about 10% at the age of 60.

This descriptive pattern could already be interpreted as evidence in favor of human capital theory and the theoretical prediction that training should decrease toward the end of working life, but it is important to re-iterate the following: this descriptive figure mixes up cohort and age effects and we know that job-related training is increasingly important (Köller et al., 2017). One result of this could be that younger cohorts might have higher training levels throughout their working life and, thus, the age effect could be non-existent or even upward sloping to generate this overall pattern. Thus, descriptive evidence, like that shown in Figure 1, is relevant for documenting the incidence of training for different age groups but cannot inform causal questions.

To shed some more light on this, Figure 2 shows age-specific training participation rates for the two cohorts from the Microcensus that we use in the RDD to

management, and public speaking.

estimate the training effects. Two observations are of interest: First, the younger cohort of 1952 shows a higher incidence of training for all ages 53 to 59, when compared to the older cohort of 1951. In this Figure, this difference is a combination of the reform effect, since only the younger cohort was affected by the 1999 pension reform, as well as general differences that might occur between cohorts. In our analysis, we control for the latter using the RDD design. Second, the age-trend from 53 to 59 is negative, but only marginally so. This stands in stark contrast to the age-pattern shown in Figure 1 and underlines the necessity to separate out cohort form age-effects for a causal strategy .

In Figure 3, we turn to the training pattern by initial education. We find that training increases with the level of education. Specifically, employed women born in 1951 with no college degree (ISCED $<$ 5) have training rates of about 11.7 percent. In contrast women with a college degree or higher tertiary education (ISCED $>$ 5) have training rates of 31.7 percent. Again, this figure is merely of descriptive nature, but the differences by educational level document here motivate to examine the effects of the pension reform that we study on training along this dimension.[15]

2.2.2 The German Socioeconomic Panel

The SOEP is a representative longitudinal data set with relevant socio-economic variables on the individual and household levels since 1984 (Goebel et al., 2019). The sample size of the SOEP is increasing over time. The most recent wave includes information of more than 25,000 individuals in about 15,000 households.

For the analysis, we exploit the longitudinal dimension of the SOEP data and compare the training participation of treated women, with the training participation of a control group in the pre-reform and the post reform periods. The treatment group consist of women born in 1952 and 1953 who fulfill the working

[15]In Appendix Figures A.2 and A.1, we also show figures with/without college for the cohorts 1951 and 1952. The patterns are similar to the discussion of Figure 2 vs. 1 above.

criteria for the pension for women (see above). For the control group, we select women born in 1951 and 1950 who are eligible for the pension for women.[16]

In Appendix Table A.2, we compare key variables for the treatment and the control groups. The treatment and the control groups differ in two important dimensions. First, by design, the treatment group is about two years younger than the control group. Second, women in the treatment group are better educated than in the control group, which is consistent with the general education expansion over this period in Germany, see e.g. Ammermüller and Weber (2005). In the empirical analysis, we account for theses difference by including individual fixed effects and age fixed effects. All other variables observed pre-reform are not statistically different between the treatment and the control groups. As mentioned above, the number of observations in the SOEP is considerably lower than in the Microcensus. Therefore, we pool observations of two cohorts to define the treatment and the control groups and control for cohort specific effects using a difference in difference strategy. Still, it is important to note that sample size remains relatively small with 231 women in the treatment group and 200 women in the control group. We need to keep this in mind when interpreting the estimation results that are based on the SOEP data.

As mentioned above, the definition of training differs between SOEP and the Microcensus. The SOEP provides information about the current participation in training, instead of the last year [17]Therefore, participation rates in the SOEP are considerably lower than in the Microcensus in which training in the last 12 months is reported.

3 The reform effect: RDD results

3.1 Empirical method: RDD

In the RDD-analysis, we exploit the 1999 pension reform to estimate the effect of an increase in working life on human capital investment. The reform leads to an

[16]In contrast to the Microcensus, we have information about the working history of all women in the SOEP. Thus, we can focus on women eligible for the pension.

[17]The SOEP questionnaire asks whether the individual is currently participating in a training course. See for the wording of the question in Table A.4 in the Appendix

arbitrary and distinct cutoff for women born before and after December 31, 1951, which determines assignments into the treatment and the control groups.

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

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

For identification of a causal effect, it is important that no manipulation of the month of birth for women born in 1951 and 1952 and 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:

$$y_i = \alpha + \beta D_i + \gamma_0(M_i - c) + \gamma_1 D_i(M_i - c) + X_i\delta + \varepsilon_{it} \tag{2}$$

D_i is a dummy specifying treatment, that is equal to 1 if a woman is born 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). The difference between a woman's birth date and the beginning of the ERA increase, $M_i - c$, gives the running variable. The running variable is interacted with the treatment variable D_i to allow for different slopes before and after the cutoff. γ_0 is the coefficient of the running variable and γ_1 is the coefficient of the interaction term. In addition we account for further explanatory variables (X), including age, predetermined education and regional information.

First, we estimate this linear specification using OLS and further include polynomials up to the third degree in the running variable and its interaction with the treatment indicator. Second, we estimate local regressions (linear and quadratic) and test for robustness 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 women has participated in training in the last twelve months and 0 if she has not.[19]

3.2 Graphical analysis

Figure 4 shows participation rates in training by month of birth, 12 months before and after the cut-off birth date, 1.1.1952 for on all employed women in their later working life, i.e., when they are aged between 53 and 60.[20] The share of employed women participating in training is higher after the cut-off. Specifically, the average rate of participation in the 12 months before the cut-off date is approximately 15.4%. After the cut-off date, the graphs show a jump in the average rate of training participation for employed women under 60 to more than 16.5%.

Importantly, and in contrast to the descriptive evidence discussed in Section 2.2.1, women close to the cut-off are of almost identical age, thus cohort-effects are an unlikely explanation for this jump. While graphical RDD-evidence can be informative, eye-balling alone can be misleading. Thus, in the next section, we examine the robustness and significance of the graphical evidence using various choices in the RDD framework.

3.3 RDD results - overall effects

To quantify the effect of an increase in the working life on the investment into human capital, we use the RDD described in Section 3.1. 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 estimated using OLS with polynomials with linear, quadratic, and cubic specifications of the running variable,

[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.

[20]Training participation is only measured in a consistent way from the age of 53, a data limitation from the Microcensus that we relax using SOEP data.

as well as local estimation that allow for local linear and quadratic. Moreover, the table includes these regressions without and with additional control variables. Standard errors are reported in brackets and clustered at the birth month level. Our inference is robust to a specification without clustered standard errors as suggested by Kolesar and Rothe (2018).

The results of these different specifications all point in the same direction despite some 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.

Although positive, only the linear OLS-specification in the top panel (without covariates) in Column 1 is not statistically significant. 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 Table 2 consistently show positive and significant estimates in similar magnitude across all specifications. Most point estimates show that the participation in training increases between 2.5-5 percentage points. This is a sizable effect that translates into a relative increase of about 20-30% given the pre-reform share in training of 15.4%. We return to the quantification and discussion of the effect size in Section 3.6 and the Conclusions.

3.4 RDD results - robustness

Balance checks: The assumption underlying the RD design 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. The Microcensus does not offer many variables that safely can be considered as pre-determined with respect to the 1999-policy change. Therefore, we restrict this analysis to whether the individual has a college education and an indicator for "West" (Panel A). Since we only focus on women close to the cut-off, to alleviate issues of sample size, we aggregate the ISCED educational levels into two education groups of women with "college" and "non-college." The latter “West” indicator is a dummy variable that equals 1 if the individual was in West Germany in 1989. For both balancing variables and across all specifications, this analysis reveals no jump at the threshold. This supports the underlying RDD assumption of no other changes across the threshold. In addition, we present the balancing of further variables that potentially might be affected by the pension reform. In Panel B and in Table 3, we show that the reform had no effect on household income before the age of 60, sorting into big companies, or marital status [21].

In addition, 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. The results of this additional balancing check are presented in Appendix Table A.4. Here, we show estimates similar to specification 2 but for the population of all women in these age groups that responded to the Microcensus survey. 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 polynomial and the linear local specification, which show significant estimates at the 10 and 5 percent level, respectively, when looking at employment outcome. Conditioning on covariates, however, these estimates become insignificant. Moreover, in Appendix Figures A.3 and A.4, we show the corresponding RDD graphs for employment/unemployment balancing. It is also visible that there are no effects on unemployment or employment before the age of 60.[22] 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.

[21]The indicator variable *Single* is significant at the 10 per cent level in the local linear regression, however, insignificant across all other specifications.

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

Placebo Analysis - different cohorts: In addition, we conduct placebo analyses, as presented in Table 4. In the first placebo analysis, we artificially shift the cut-off date by one year to 1.1.1950 or to 1.1.1952. Importantly, the pension rules are identical before and after the chosen placebo cut-offs. The shift of one entire year (in either direction) 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 supports 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 effects are precisely estimated and clearly differ from our main findings in Table 2.

Placebo Analysis - private training: Next, we exploit the additional information on "private training" that is recorded by the Microcensus. In contrast to job-related training, such "private training" has private value beyond the working life and, thus, we do not expect to find differential take-up depending on the pension rule.[23]

These results are presented in Table 5. As expected, none of the estimates are statistically significant. Moreover, all estimates are very close to zero and negative, clearly different to the main results found for job-related training.

Bandwidth choices: 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.5, we replicate our main results from Table 2 but using a bandwidth of six months, rather than a full year on both sides of the cutoff, for all specifications

[23] In the Microcensus, private training is classified as general training measures with a predominant private focus to advance one's own skills and knowledge. Examples given for private training in the Microcensus questionnaire are training in the fields of music, sport and health, cooking, or art.

of the running variable. Second, in Appendix Table A.6, 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.5 that shows the results of the analysis with a quadratic polynomial, remain statistically significant at the ten percent level or higher.

Donut RDD: As additional robustness check, we examine if observations close to the cutoff drive our effects by estimating effects from donut-RD regressions. We estimate different specification of Equation 2 without the one or two birth month closest to the cutoff on both sides. Appendix Table A.7 shows the resulting estimates for the various functional form choices as well as with and without individual control variables. Some of the specifications without covariates loose 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.

3.5 Heterogeneity

Effects by initial education: We now extend the empirical analysis by focusing on effect heterogeneity along prior educational levels. We already show descriptively that training participation positively correlates with prior educational levels in Section 2.2.1 and Figure 3. In this analysis, we test if the reform effect also varies by prior educational level. Since we only focus on women close to the cut-off, to alleviate issues of sample size, we aggregate the ISCED educational levels into two education groups of women classified as "college" and "non-college."

Before turning to the results, note that (Geyer et al., 2018) show that employment effects of the same pension reform for women aged 60-62 are of similar size for highly educated women (9.5%) and for women without higher education (8.2%). Thus, any differences in the reform effect along the education dimension are not related to differences in retirement decisions.

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, corresponding to a relative increase of about 35%.[24] The effect for women without college education is estimated to be close to zero and not significant at conventional levels.

Effects by company size: Next, we examine if the training effect differs by the size of the company. For this, we estimate the RDD separately for women working in large vs small/medium-sized companies. We use the classification from the Microcensus, where companies with 50 or more employees are classified as large. Before turning to the interpretation of the results, we note again that company size is potentially an outcome in its own right, and can thus be considered endogenous. But as documented in Table 3, the pension reform did not have an affect on sorting into bigger companies.

Appendix Table A.8 splits the sample by company size. For large companies, the effects vary between 0.87 and 3.85 percentage points without, and between 1.02 and 4 percentage points when including controls. For small-medium sized companies the effects always fall in the range of 2.1 and 6.56 percentage points. Overall, out of the twenty estimates provided, thirteen are significant at least at the 10 percent level, of which eleven are significant at 5 percent or higher. The estimates thus suggest slightly larger and more significant effects in small and medium sized companies, but the confidence intervals of the estimates are overlapping. We therefore conclude that the main dimension of heterogeneity is along levels of initial education.

3.6 Quantification

As discussed in Section 2.1, the pension reform only affected the working life of women who fulfill the eligibility criteria for the so-called *pension for women*. The

[24]We discuss the magnitude of this effect in detail in Section 3.6.

Microcensus is a cross sectional data set without information about the respondentâs employment history. Therefore, we can not directly determine the 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 SOEP longitudinal data, according to which about 76% of all women employed before entering retirement were indeed eligible for this pathway into retirement. This rate increases to 89.14% when excluding self employed and civil servants, who are not, by definition, eligible. Further, SOEP data show that about 86% of employment women without a college degree and 94% 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) overall and 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. The point estimates suggest that overall training increases by roughly 4 percentage points (column 1), for women with a college degree the increase is over 13 percentage points (column 2), and the effect for women without college is close to zero (column 3). These estimates imply a relative increase in training of 30% of all women and 40% of women with college degree. Although these are sizable estimates, so far this discussion does not account for the fact that, while being distinct from zero at conventional levels of statistical significance, these coefficients are of course estimated with uncertainty. When we instead focus on the lower bounds of the corresponding 95%-confidence intervals, we obtain a value of 1.59 percentage points for the ITT using the quadratic specification for the whole sample. This corresponds to an ATT of approximately 1.78 percentage points and a relative size of 11.6% overall. For college graduates the lower bound ITT estimate is 5.28 percentage points resulting in an ATT estimate of 5.92 percentage points, and a relative size of 18.7%, again using the quadratic specification.

4 Longitudinal Analysis

4.1 Reform effects before the age of 60: Difference-in-difference

We now use the longitudinal dimension of SOEP to analyze the effect of the same pension reform in a difference in difference setting (DiD). The sample size of SOEP does not allow for estimating an RDD with a monthly running variable. Instead, we compare the evolution of training across four birth cohorts: the treatment group comprising women born in 1952 and 1953 who full fill the working criteria for the pension for women. For the control group we select women born in 1951 and 1950 who are eligible for the pension for women. More formally, we estimate the following model:

$$y_{it} = \beta_1 Post_t * Treat_i + \beta_2 Post_t + \sum_i \alpha_i D_i + \sum_a \kappa_i A_{it} + X'_{it}\beta_3 + u_{it}, \quad (3)$$

where y_{it} is the outcome variable. $Post_t$ is an indicator for the post reform period.[25] Further, the model accounts for individual specific fixed effects using dummy variables $\sum_i D_i$, which also absorb $Treat_i$, dummy variables for age fixed effects $\sum_a A_{it}$,[26] time-varying variables X'_{it}, and an error term u_{it}, which we cluster at the individual level. The coefficient β_1 is the coefficient of interest, capturing the effect of the pension reform. In the first set of analyses, we again focus on training participation before the age of 60 and confirm the findings we obtained using the Mircocensus. We then extend the analysis and turn to effects of the pension reform on different labor market outcomes before the age of 60, which we can measure with the SOEP.

Training by cohort and reform eligibility: Table 8 presents descriptive statistics of the estimation sample, including training participation rates for the control group and the treatment group before and after the pension reform. Distinguishing between different time periods, we show in Panels A and C the par-

[25]As mentioned in Section 2.1, the reform was already passed in late 1997, hence we chose 1998 and later as the cut-off. Results do not significantly change if we use 1999 to define the pre- and post-reform periods

[26]Note that a is the number of different ages that are observed in our sample.

ticipation rates of all employed women in addition to the participation rates of women in the estimation sample, i.e., employed women who full fill the criteria for the pension for women. As mentioned above, the training incidence in SOEP is markedly lower than in the Microcensus, which is related to the different design of the questionnaire. In the pre-reform period, we find similar participation rates for women in the treatment and in the control groups. For all women training rate are lower in the post-reform period which is related to the higher age of the women in the post period. However, training rates are always higher for women in the treatment group after the pension reform. Therefore, the difference in training participation, i.e. the reduction in training before and after the pension reform is more pronounced in the control group. As expected, the difference becomes clearer when focusing only on eligible women (Panels B and D). In these samples, we find a significant reduction in the participation rates for women in the control group but no significant difference for the treated women.

DiD results: The regression results of Equation 3 confirm the descriptive evidence. Table 9 shows the estimates of the reform effect (β_1) in specifications with and without fixed effects and for eligible women in different time periods.[27] In the preferred specification with fixed effects (Column 3), we find that training incidence increases by about two percentage points. This effect is statistically significant at the 10% level. The point estimates are remarkably similar in the DID specifications without fixed effects and without controls (Column 1), as well as with additional controls (Column 2) or with fixed effects but over a shorter time period (Column 4). In the final two columns of Table 9, we split the sample and focus on different age groups. We find that the reform has a positive effect on training for women aged 45-55 in Column (5) and women aged 55-59 in Column (6). The effect for the younger women amounts to about 2.5 percentage points and is significant at the 10% level while the point estimate for women aged 55-59 is lower and not significant at this level. This is consistent with the theory that the

[27] All standard errors are clustered at the individual level to account for heterogeneity; accounting for robust standard error estimation yields the same standard error values.

length of the payout period matters; i.e., a finding that the overall effect is driven only by older women would have been problematic. In terms of effect sizes, given the low pre-reform training incidence of about 2% observed in the SOEP data, the point estimates imply that training incidence – on average – doubles. This effect should be interpreted with some caution. First, as discussed, the SOEP sample size is relatively small and, therefore, the standard errors are large. Second the low participation rates might mechanically lead to large relative effects, even for small uptake in additional training sessions.

Overall, the analysis based on the SOEP confirms the positive findings based on the Microcensus. Given the different design of the questionnaire, and the different sample selection, the magnitude of the point estimate is difficult to compare. However, both analyses suggest that the pension reform had a sizable effect on training participation.

Effects on further outcomes before retirement: In Table 10, we extend the analysis and use Equation 3 to test if the pension reform had effects on labor market outcomes before the age of 60. We focus on employment, as in Section 3.4, but include other (labor market) outcomes measured by the SOEP. More specifically, we study if the increase in the retirement age affected employment, part time work, working hours, wages, work satisfaction, and sorting into big companies. We only present the results based on the preferred specification with individual fixed effects and for the full sample period. The regression results show a clear picture. The pension reform does not significantly affect the other outcome variables. This finding is important for the interpretation of the training effects and the mechanism of how an increase in the working life affects training. We turn to this question in the next section.

4.2 Effects beyond the age of 60: Instrumental Variable estimation

We now take advantage of the panel nature of the SOEP data to examine the mechanism between the longer working life (after age 60) and training (before age 60). In the analysis we address the following question: Did those women whose retirement was postponed, thus increasing their working life, also invest more in training before the age of 60?

Descriptive results: In Table 11, we provide descriptive evidence about the joint effect of training and employment of the pension reform. More precisely, we show how often women participated in training between the age 46 and 59, on average, documenting this training count for employed and eligible women in the treatment and the control groups by employment status after the age of 60. For all women not employed after the age of 60 and for women in the control group but employed after 60, average training counts are at similarly low levels. However, for women affected by the pension reform who are still employed after the age of 60, we find counts that are clearly higher (nearly three times as large). Although the number of observations is relatively small, meaning the standard errors are large, we can still see a clear difference between the treatment and control groups for the group of women employed after age 60. To test if these differences are really driven by the reform effect, we use IV estimation.

Since employment and education decisions are taken jointly (Ben-Porath, 1967), it is important to use an instrumental variable strategy to identify the effect of an increase in the employment on training. As an instrument, we use the pension reform, which increased the retirement age form age 60 to 63.

Formally we estimate the following equation:

$$Training_i = X_i'\beta_1 + \beta_2 Employment_i + \epsilon_i \tag{4}$$

where $Training_i$ is a count of how many times between the introduction of the reform to the point of observation the individual participated in training and

Employment is a dummy equaling one if the individual is not retired and employed after the age of 60. In addition, we account for further observed variables X_i and an error term u_i.

We instrument employment after age 60 using the pension reform in the first stage which is given by:

$$Employment_i = X_i'\delta_1 + \delta_2 Treat_i + u_i \tag{5}$$

where $Treat_i$ is an indicator for whether the individual was affected by the reform.

β_2 from Equation 4 is the coefficient of our main interest. For the estimation we restrict our sample to cohorts 1950-1953. Moreover, we only include eligible (employed and unemployed) women we observe before and after age 60.[28] We estimate β_2 using TSLS and cluster the heteroskedasticity-robust error term at the individual level.

IV results: In Table 12 we present the results of the first and the second stage of the instrumental variable estimation with and without further control variables. Turning to the first stage, we find a sizable and significant effect of the pension reform on employment after the age of 60. According to the estimations, the pension reform increased the probability of employment after the age of 60 by about 15 percentage points. This is in line with previous studies of employment effects, e.g. Geyer and Welteke (2019), who use administrative data of the public pension insurance accounts. Accordingly the second stage without (Column 1) and with controls (Column 2) shows positive effects on training before the age of 60: The interpretation of this finding is that women whom the reform induced to postpone retirement invest more in training. Specifically, the second stage suggests that the average training count in the post treatment period increased by 0.05 for members of the treatment group due to the exogenous variation induced by the reform.

The effect is only significant around the 15% level, which is related to the small sample size. Thus, these findings do not allow us to draw a clear conclusion,

[28]We exclude women for whom we only have observations for ages 58 and 59 due to the extension of the SOEP in the year 2011 and 2012.

but together with the evidence presented in Table 11 and the causal effects on training obtained from the cross-sectional RDD estimates and the Microcensus in Section 3, they provide evidence for the mechanism that an increase in the working life positively affects training.

5 Conclusion

In this paper, we provide novel causal evidence for the theory of human capital accumulation, which has the key prediction that education investments depend on the length of the payout period. Providing causal estimates is difficult because of the long-run nature and joint determination of these variables.

We address these problems and note that an exogenous change in the working life increases the payout period for the human capital investment. Specifically, we exploit a sizable pension reform that sharply increased the early retirement age for women between two adjacent cohorts from 60 to 63 years. The analysis is based on the German Microcensus using RDD, and the SOEP household panel data using DiD and IV estimation approaches.

The empirical analysis offers support for the key prediction of human capital theory, that the duration of the payoff matters for educational investment decisions. We 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. In more detail, based on the Microcensus, our empirical results show that the increase in the retirement age has a sizable effect on the human capital accumulation of employed women: 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, and at least 11.6% considering lower bound of the 95% confidence interval. This finding is robust to changes in the bandwidth and for different specifications of the running variable in the RDD and is supported by placebo tests. Investigating heterogeneity, 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%, with a lower bound of 18.5%. The effect for women without college degree is not significant. These findings are supported by the analysis using the SOEP data. Using a DiD strategy, we confirm that the pension reform does not significantly affect (labor market) outcomes before the age of 60, but that it positively affects training before retirement. Our analysis based on the SOEP provides additional support, also showing that a longer working life increases investment in training. Using an IV approach, we estimate that an increase in the working life, i.e., employment effects after the age of 60, induced by the pension reform, positively affects training before the age of 60.

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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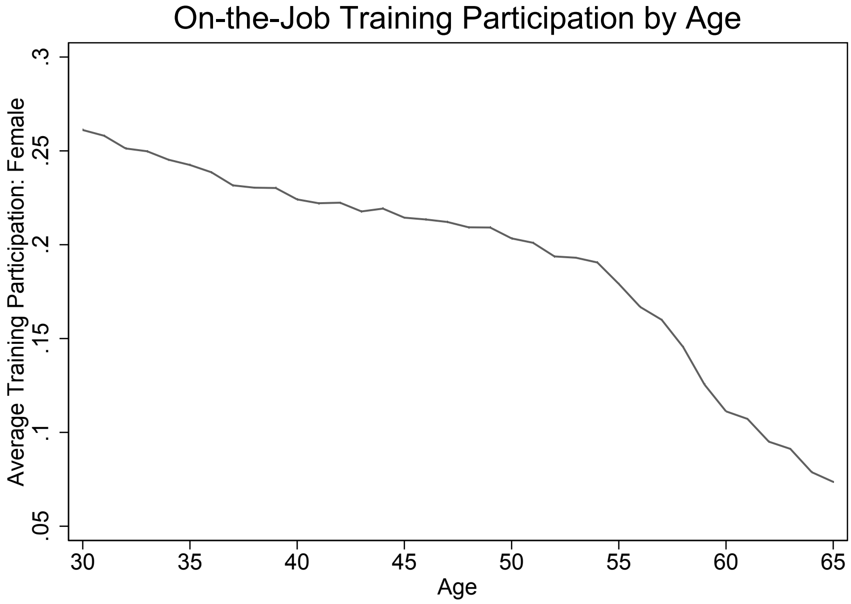
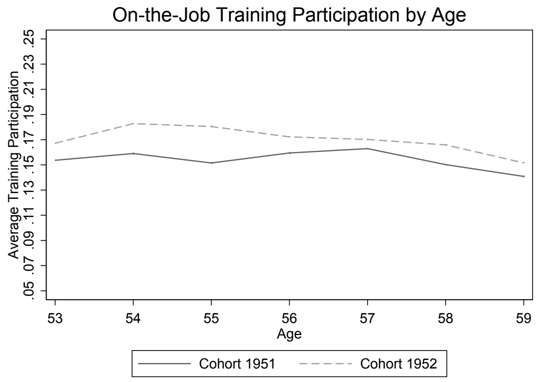
Figures and Tables

Figure 1: Average On-the-job Training Participation by Age, Cohorts: 1940-1997



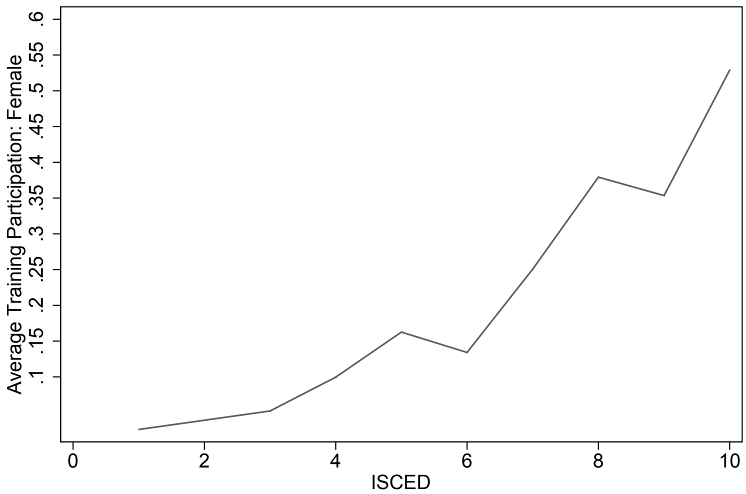
Notes: The figure plots the average on-the-job training participation for all employed women of the cohorts 1940-1997 from 2005 onward.

Figure 2: Average On-the-job Training Participation for Sample Group



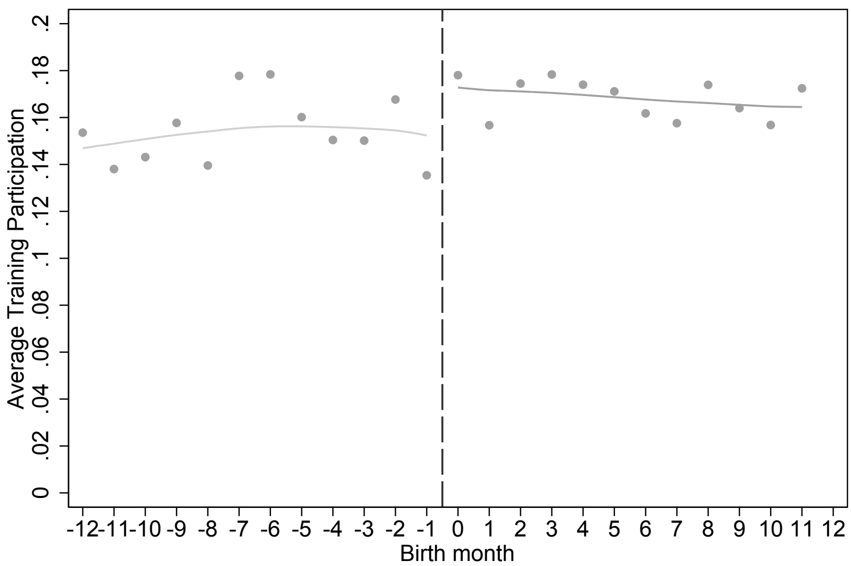
Notes: The figure plots the average on-the-job training participation for all employed women of the cohorts 1951 and 1951 from 2005 onward.

Figure 3: Average On-the-job Training Participation by ISCED Groups



Notes: The figure plots the average on-the-job training participation by ISCED group for all employed women from 2005 onward.

Figure 4: On-the-job Training around the Cutoff 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	Sample Cohorts 1951/1952
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)

Source: Microcensus 2005-2012, own calculations. Average values of outcome variables and covariates. Standard deviations in parentheses.

Table 2: Regression Discontinuity: Main Results

	OLS			Local Regression	
	(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	0.0159*	0.0359***	0.0435***	0.0239***	0.0393***
	(0.0077)	(0.0096)	(0.0108)	(0.0080)	(0.0088)
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. Significance levels: $*$ p 0.10, $**$ p 0.05, $* * *$ p 0.01; Pre-Policy Mean: 15.41 percent; Number of observations without covariates: 28,531; Number of observations with covariates: 28,519.
Source: Microcensus 2005-2012, own calculations.

Table 3: Balancing

	OLS			Local Regression	
	(1)	(2)	(3)	(4)	(5)
Panel A: Pre-determined covariates					
College Education					
Treatment Variable	-0.0010	0.0087	0.0324	0.0029	0.0191
	(0.0175)	(0.0300)	(0.0322)	(0.0230)	(0.0307)
West					
Treatment Variable	0.0156	0.0056	-0.0097	0.01162	-0.0010
	(0.0192)	(0.0297)	(0.0415)	(0.0233)	(0.0326)
Panel B: Further Variables					
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)
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; clustered at the birth month level. Sample includes twelve month before and after reform cutoff. Significance levels: ∗ p 0.10, ∗∗ p 0.05, ∗ ∗ ∗ p 0.01; Number of observations in descending order of rows: 28,519, 28,531, 28,531, 28,175, 28,531. Source: Microcensus 2005-2012, own calculations.

Table 4: RDD with Placebo Cohorts

	OLS			Local Regression	
	(1)	(2)	(3)	(4)	(5)
Panel A: 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	0.0097	0.0132	-0.0003	0.0044	0.0035
	(0.0156)	(0.0167)	(0.0251)	(0.0207)	(0.0325)
Running Variable	Linear	Quadratic	Cubic	Linear	Quadratic
Panel B: 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	-0.0042	-0.0047	-0.0074	-0.0009	-0.0029
	(0.0079)	(0.0076)	(0.0102)	(0.0086)	(0.0109)
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. Significance levels: $*$ p 0.10, $**$ p 0.05, $***$ p 0.01; Number of observations Panel A: 24,641 without covariates and 24,623 with covariates; Number of observations Panel B: 34,162 without covariates and 34,150 with covariates.
Source: Microcensus 2005-2012, own calculations.

Table 5: RDD on Private Training

	OLS			Local Regression	
	(1)	(2)	(3)	(4)	(5)
Without Covariates					
Treatment Variable	-0.0032	-0.00002	-0.0017	-0.0020	-0.0007
	(0.0027)	(0.0037)	(0.0046)	(0.0030)	(0.0038)
With Covariates					
Treatment Variable	-0.0035	-0.00003	-0.0022	-0.0021	-0.0009
	(0.0027)	(0.0038)	(0.0045)	(0.0030)	(0.0038)
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. Significance levels: $*$ p 0.10, $**$ p 0.05, $***$ p 0.01; Number of observations without covariates: 28,531; Number of observations with covariates: 28,519.
Source: Microcensus 2005-2012, own calculations. Private training in the microcensus is defined as an activity that mostly serves a private purpose such as acquiring private skills and benefits. The question gives examples of music, sport, health, art, education, technology and cooking.

Table 6: Heterogeneity by Educational Level

	OLS			Local Regression	
	(1)	(2)	(3)	(4)	(5)
Panel A: 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	0.0062	0.0116	0.0205	0.0084	0.0155
	(0.0090)	(0.0131)	(0.0171)	(0.0095)	(0.0133)
Running Variable	Linear	Quadratic	Cubic	Linear	Quadratic
Observations	22,984	22,984	22,984	22,984	22,984
Panel B: 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	0.0545	0.1290***	0.1513***	0.0835***	0.1390***
	(0.0344)	(0.0368)	(0.0477)	(0.0355)	(0.0394)
Running Variable	Linear	Quadratic	Cubic	Linear	Quadratic
Observations	5,535	5,535	5,535	5,535	5,535

Notes: Standard Errors in parentheses; clustered at birth month level. Sample includes twelve month before and after reform cutoff. Significance levels: $*$ p 0.10, $**$ p 0.05, $***$ p 0.01; Participation rates in training for cohort 1951 are 0.154, 0.317, 0.117 for all employed women, employed women with college and employed women without college, respectively.
Source: Microcensus 2005-2012, own calculations.

Table 7: Average Treatment Effect on the Treated (ATT)

	All (1)	College (2)	Non-College (3)
Panel A: OLS			
Linear			
ATT	0.0178	0.0579	0.0712
Relative Size ATT (in%)	11.56	18.27	6.09
Quadratic			
ATT	0.0403	0.1370	0.0134
Relative Size ATT (in%)	26.17	43.24	11.45
Cubic			
ATT	0.0488	0.1608	0.0237
Relative Size ATT (in%)	31.70	50.73	20.24
Panel B: Local Regression			
Linear			
ATT	0.0268	0.0887	0.0097
Relative Size ATT (in%)	17.40	27.99	8.29
Quadratic			
ATT	0.0441	0.1477	0.0179
Relative Size ATT (in%)	28.64	46.59	15.30
Observations	28,531	5,469	22,694
Eligibility (in%)	89.14	94.12	86.58

Notes: The ATT is derived by weighting the ITT effects presented in Tables 2 and 6. The eligibility rates are calculated from the SOEP data. For the ITT estimates we choose the OLS specification equivalent to column 2 in both tables, with a quadratic running variable and with control variables. The sample includes twelve month pre and post reform. Participation rates in training for cohort 1951 are 0.154, 0.317, 0.117 for all employed women, employed women with college and employed women without college, respectively.

Table 8: SOEP: Summary Statistics: Training Particpation

	Control Group			Treatment Group		
	Pre	Post	Pre-Post	Pre	Post	Pre-Post
Panel A: Employed women 1984-2012						
	0.0226	0.0041	0.0184	0.0206	0.0075	0.0132
	(0.003)	(0.0041)	(0.0035)	(0.0038)	(0.0075)	(0.0034)
N	1,817	2,191		1,405	2,945	
Panel B: Eligible women 1984-2012						
	0.0271	0.0048	0.0223	0.0199	0.0105	0.0094
	(0.0061)	(0.00181)	(0.0023)	(0.0062)	(0.0023)	(0.0056)
N	702	1,459		502	1,809	
Panel C: Employed women 1990-2012						
	0.0223	0.0041	0.0182	0.0206	0.0074	0.0130
	(0.0042)	(0.0014)	(0.0036)	(0.0042)	(0.0016)	(0.0036)
N	1,254	2,191		1,119	2,945	
Panel D: Eligible women 1990-2012						
	0.0227	0.0048	0.0179	0.0192	0.0105	0.0087
	(0.0065)	(0.0018)	(0.0049)	(0.0067)	(0.0024)	(0.0059)
N	527	1,459		416	1,809	

Source: SOEP, own calculations. Average values of outcome variables: On-the-job training. Standard errors in parentheses. Control group includes cohorts 1950-51; treatment group includes cohorts 1952-1953.

Table 9: SOEP Diff in Diff, Effects on Training

	(1)	(2)	(3)	(4)	(5)	(6)
Reform Effect	0.0173	0.0201	0.0217*	0.0222*	0.0256*	0.0172
	(0.0123)	(0.0124)	(0.0125)	(0.0135)	(0.0133)	(0.0139)
Covariates	No	Yes	No	No	No	No
Fixed Effects	No	No	Yes	Yes	Yes	Yes
Time	1984-2012	1984-2012	1984-2012	1990-2012	1984-2005	1984-97/2005-12
Age after reform	46-59	46-59	46-59	46-59	46-55	55-59
Individuals			431	429	322	319
Observations	4,530	4,530	4,530	4,211	2,994	2,675

Notes: Standard Errors in parentheses; clustered at individual level. Sample includes employed women under the age of 60 in birth cohorts 1950-1953 that have 15 years of work experience; 10 of which after the age of 40. Significance levels: $*$ p 0.10, $**$ p 0.05, $* * *$ p 0.01; all specifications include age fixed effects.

Table 10: Other Outcomes

	Empl.	Part Time	Working Hours	Wages	Work Satis.	Big Company
	0.0315	-0.0186	0.8874	134.3502	0.0314	0.0425
	(0.0500)	(0.0582)	(1.1545)	(96.2504)	(0.0391)	(0.0540)
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Individuals	484	484	431	431	412	431
Observations	6,003	6,003	4,530	4,530	4,449	4,530

Notes: Standard Errors in parentheses; clustered at individual level. Sample includes employed women or all women, depending on the specification, under the age of 60 in birth cohorts 1950-1953 that have 15 years of work experience; 10 of which after the age of 40. Significance levels: $*$ p 0.10, $**$ p 0.05, $***$ p 0.01; all specifications control for age fixed effects. Working hours here are defined as the reported actual working hours per week.

Table 11: Average Count of Training Participation of eligible and employed Women from Age 46-59

	Control Group	Treatment Group	Difference
Employed after 60	0.0045	0.0128	0.0083
	(0.0026)	(0.0053)	(0.0064)
Not employed after 60	0.0049	0.0032.	-0.0017
	(0.0035)	(0.0032)	(0.0054)
Pooled	0.0046	0.0112	0.0066
	(0.0021)	(0.0045)	(0.0050)
Observations	131	135	

Source: SOEP, own calculations. Average values of training count variable. Standard errors in parentheses.

Table 12: IV Approach

	(1)	(2)
Employment (First Stage)		
	0.1615***	0.14846***
	(0.0449)	(0.0454)
Training		
	0.0561	0.0541
	(0.0373)	(0.0398)
P-Value	0.133	0.174
Covariates	No	Yes
F-Statistic	79.24	44.44
N	347	347

Notes: Standard Errors in parentheses; clustered at individual level. Sample includes all eligible women observed before the age of 58 and controls for employment in all specifications. Significance levels: ∗ p 0.10, ∗∗ p 0.05, ∗ ∗ ∗ p 0.01.
Source: SOEP , own calculations.

Appendix A: Theoretical Model with Utility Cost

In this Appendix, we derive a stylized theoretical human capital model and show that individuals, ceteris paribus, have an incentive to increase training when working life increases.[29] The central 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.[30] The theoretical model presented below illustrates the mechanism in a simplified and intuitive setting through 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. Each individual derives utility through consumption, $U(C_{ti})$, with the standard assumption of $U'(C_{ti}) > 0$, $U''(C_{ti}) < 0$. Further, each individual earns wage w_t.[31] The wage in period one depends on the initial level of schooling, i.e. $w_1(S_i)$, 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 the first period, the individual decides on his or her time investment in human capital, I_i, through participation in on-the-job training measures. The wage in period two depends on the chosen level of training, specifically we assume $w_2'(I_i) > 0$, $w_2''(I_i) < 0$.

1. Period

Income in the first period consists of labor in income which varies with the level of initial schooling, S_i:

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

[29] 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. 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).

[30] An analogous human capital effect can be generated in a model of 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.

[31] 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 increase in earnings in full time employment.

2. Period
Income in period two is given by:

$$Y_{2i} = w_2(I_i)R_i \tag{6}$$

where R_i is the duration of the remaining working life and I_i denotes the level of human capital investment.

3. Period
Income in period three is given by:

$$Y_{3i} = \alpha w_2(I_i)(T_i - R_i) \tag{7}$$

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.
Utility over all three periods then is given by:

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

Where:

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

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

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

Following e.g. 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 is given by:

$$\frac{\partial L}{\partial C_{1i}} = 0 \Rightarrow U'(C_{1i} - a(S_i) I_i) - \lambda = 0 \tag{8}$$

$$\frac{\partial L}{\partial C_{2i}} = 0 \Rightarrow \beta R(U_i'(C_{2i}) - \lambda) = 0 \tag{9}$$

$$\frac{\partial L}{\partial C_{3i}} = 0 \Rightarrow \beta(T - R)\beta^2[\lambda - U(C_{3i})] = 0 \tag{10}$$

$$\frac{\partial L}{\partial I_i} = 0 \Rightarrow \lambda[-a_1(S_i) + (R_i\beta + (R_i - T_i)\alpha\beta^2) w_2'(I_i)] = 0. \tag{11}$$

Based on the set of First Order Conditions and applying the implicit function theorem, we derive our results. More precisely, we can take the derivative of 11 with respect to R_i and I_i respectively. Dividing both terms with each other and multiplying the result by negative one, then according to the implicit function theorem gives $\frac{\partial I_i}{\partial R_i}$ denoted below in 12.

$$\frac{\partial I_i}{\partial R_i} = -\frac{\frac{\partial L}{\partial I_i \partial R_i}}{\frac{\partial^2 L}{\partial I_i^2}} = \frac{[\alpha\beta - 1]\, w_2'\,(I_i)}{[R_i + (T_i - R_i)\alpha\beta]\, w_2''(I_i)} \tag{12}$$

An increase in the remaining working life, R_i implies a positive impact on training as long as $w_2' > 0$ and $w_2'' < 0$. Note that $\alpha\beta - 1$ is negative since $\beta < 1$ and $\alpha < 1$. Hence, we can derive the main proposition that motivates the empirical analysis of our paper.

Proposition I (Working life effect)
The effect of an increase in the working life on training is positive.

Secondly, the effect of training participation with respect to schooling is captured by Equation 13:

$$\frac{\partial I_i}{\partial S_i} = \frac{a'(S_i)}{\beta[R_i + (T_i - R_i)\alpha\beta]\, w_2''(I_i)} \tag{13}$$

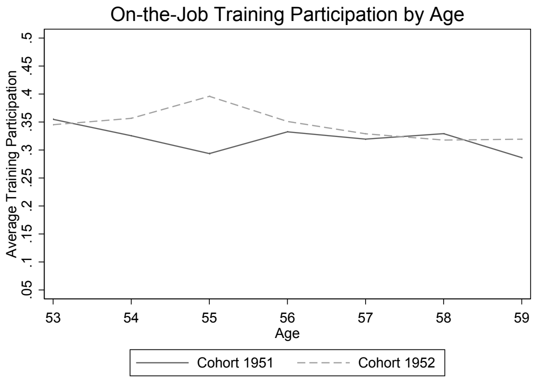
As long as the utility cost of training falls with the initial level of schooling, i.e. $a'(S_i) < 0$, the effect will be positive, which implies Proposition II.

Proposition II (Initial education effect)
The level of time investment in training rises with initial schooling as long as utility cost of training are falling with the initial level of schooling.

Thus, the model captures the empirical finding that higher educated individuals participate in training more often, which is described as a dynamic complementary between initial education and later training, see e.g. Cunha and Heckmann (2007) and Jacobs (2009).

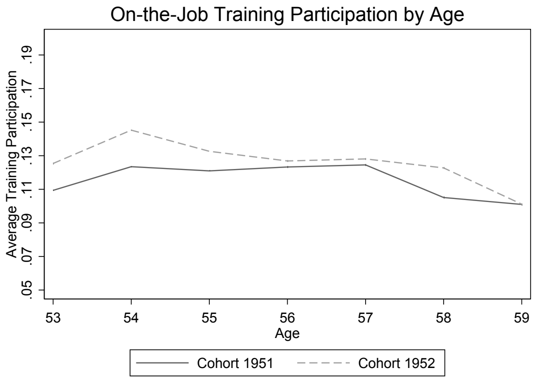
Appendix Figures and Tables

Figure A.1: Average On-the-job Training Participation for Sub-Sample with College Degree



Notes: The figure plots the average on-the-job training participation for all employed women with no college degree of the cohorts 1951 and 1951 from 2005 onward.

Figure A.2: Average On-the-job Training Participation for Sub-Sample with No-College Degree



Notes: The figure plots the average on-the-job training participation for all employed women with a college degree of the cohorts 1951 and 1951 from 2005 onward.

Figure A.3: Employment Effect before 60

Figure A.4: Unemployment Effect before 60

Table A.1: SOEP Summary Statistics

	Mean
On-the-Job training participation	0.012
Large Corporation	0.419
High Level Education	0.386
Gross Wage Income	1787.899
Single	0.067
West-Germany	0.688
Age	49.820

Source: SOEP, own calculations. Average values of range of characteristics for sample of eligible, employed women in the cohorts 1950-1953 across the years 1984-2012, age range 34-59.

Table A.2: Descriptive Statistics, by Treatment Status (SOEP)

	Control Group	Treatment Group	Difference
OJT pre reform	0.0271	0.0199	0.0071
	(0.0061)	(0.0062)	(0.0090)
Single pre reform	0.0643	0.0791	-0.0148
	(0.0079)	(0.0098)	(0.0125)
ISCED Classifcation	3.5822	4.1492	-0.5669***
	(0.0291)	(0.0288)	(0.0410)
Age pre reform	41.4382	39.6309	-1.8073***
	(0.1413)	(01585)	(0.2127)
Grown up in East Germany	0.3333	0.3306	0.0026
	(0.0293)	(0.0299)	(0.0423)
Work experience at age 59	23.6374	23.8766	-0.2391
	(1.1511)	(0.9720)	(1.5194)
Part time work experience at age 59	10.6861	10.1783	0.5078
	(1.9590)	(0.8352)	(1.298)
Individuals	200	231	

Source: SOEP, own calculations. Average values of variables by treatment status; Standard errors in parentheses. Sample includes employed women under the age of 60 in birth cohorts 1950-1953 that have 15 years of work experience; 10 of which after the age of 40.

Table A.3: Training Definition across Surveys

	On-the-job	Private
Panel A: Microcensus		
Definition	Exclusively job related	Predominantly private
Examples	Technology, management, public speaking	Cookery, sport, health, art
Timing	last 12 months	last 12 months
Panel B: SOEP		
Definition	Exclusively job related	-
Examples	No examples	-
Timing	currently	-

Source: Microcensus and SOEP questionnaires. Examples refers to examples listed in respective questionnaire.

Table A.4: Employment Balancing

	OLS			Local Regression	
	(1)	(2)	(3)	(4)	(5)
Panel A: 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	0.0156	0.0302**	-0.0030	0.0211*	0.0155
	(0.0118)	(0.0130)	(0.0194)	(0.0119)	(0.0139)
Running Variable	Linear	Quadratic	Cubic	Linear	Quadratic
Panel B: 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	-0.0136	-0.0170	0.0265*	-0.0150	0.0016
	(0.0107)	(0.0137)	(0.0152)	(0.0117)	(0.0114)
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. Significance levels: $*$ p 0.10, $**$ p 0.05, $***$ p 0.01; Number of observations without covariates: 54,126; Number of observation with covariates: 54,087 .
Source: Microcensus 2005-2012, own calculations.

Table A.5: Regression Discontinuity: Bandwith 6 Months

	OLS			Local Regression	
	(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. Significance levels: $*$ p 0.10, $**$ p 0.05, $***$ p 0.01; Number of observations: 13,949 without covariates, 13,941 with covariates; Pre-Policy Mean: 15.41 percent.
Source: Microcensus 2005-2012, own calculations.

Table A.6: Local 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	0.0289***	0.0304***	0.0239***	0.0423***	0.0354***	0.0393***
	(0.0099)	(0.0094)	(0.0080)	(0.0102)	(0.0098)	(0.0089)
Running Variable	Linear	Linear	Linear	Quadratic	Quadratic	Quadratic

Notes: Standard Errors in parentheses; clustered at birth month level . Significance levels: $*$ p 0.10, $**$ p 0.05, $***$ p 0.01;
Source: Microcensus 2005-2012, own calculations. Number of observations 6m bandwiths: 13,949 without covariates, 13,941 with covariates; Number of observations 9m bandwiths: 20,964 without covariates, 20,938 with covariates; Number of observations 12m bandwiths: 28,531 without covariates, 28,519 with covariates.

Table A.7: Donut Regression: Leaving out one or two Month on each Side of Cutoff

	OLS			Local Regression	
	(1)	(2)	(3)	(4)	(5)
Panel A: 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	0.0143**	0.0297***	0.0311***	0.0187***	0.0328***
	(0.0059)	(0.0073)	(0.010)	(0 .0045)	(0.0095)
Panel B: 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	0.0148*	0.0270***	0.0277***	0.0178***	0.0391***
	(0.0075)	(0.0042)	(0.0051)	(0.0072)	(0.0069)
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. Significance levels: $*$ p 0.10, $**$ p 0.05, $***$ p 0.01. Number of observations Panel A: 26,002 without covariates and 25,991 with covariates; Number of observations Panel B: 23,385 without covariates and 23,375 with covariates;
Source: Microcensus 2005-2012, own calculations.

Table A.8: Heterogeneity by Company Size

	OLS			Local Regression	
	(1)	(2)	(3)	(4)	(5)
Panel A: Big Company					
Without Covariates					
Treatment Variable	0.0087	0.0217	0.0385*	0.0140	0.0292*
	(0.0143)	(0.0169	(0.0224	(0.0146)	(0.0159)
With Covariates					
Treatment Variable	0.0102	0.0288**	0.0400**	0.0178	0.0337***
	(0.0115)	(0.0116	(0.0194)	(0.0109)	(0.0123)
Running Variable	Linear	Quadratic	Cubic	Linear	Quadratic
Observations	14,750	14,750	14,750	14,750	14,750
Panel B: Small-Medium Sized Company					
Without Covariates					
Treatment Variable	0.0211	0.0496**	0.0656	0.0324**	0.0566***
	(0.0139)	(0.0166)	(0.0071)	(0.0146)	(0.0143)
With Covariates					
Treatment Variable	0.0238**	0.0467***	0.0506***	0.0330***	0.0484***
	(0.0114)	(0.0135)	(0.0148)	(0.0111)	(0.0115)
Running Variable	Linear	Quadratic	Cubic	Linear	Quadratic
Observations	13,425	13,425	13,425	13,425	13,425

Notes: Standard Errors in parentheses; clustered at birth month level. Sample includes twelve month before and after reform cutoff. Significance levels: $*$ p 0.10, $**$ p 0.05, $***$ p 0.01. Big companies in the Microcensus are classified as companies with 50 or more employees.
Source: Microcensus 2005-2012, own calculations.

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The Centre for Economic Performance Publications Unit
Tel: +44 (0)20 7955 7673 Email info@cep.lse.ac.uk
Website: http://cep.lse.ac.uk Twitter: @CEP_LSE