The Effect of Pension Wealth on Employment

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Abstract

This study provides novel evidence about the pension wealth elasticity of employment. For the identification we exploit reform-induced variation of pension wealth that is related to the number of children but which does not affect the implicit tax rate of employment. We use a difference-in-differences estimator based on administrative data from the German pension insurance and find that, on average, the negative employment effect of pension wealth is significant and economically important. Heterogeneity analyses document a strong age pattern showing that the employment effects are driven by behavioral responses of women close to retirement. The age pattern is partly explained by the positive effect of pension wealth on disability pensions after the age of 60.

Keywords: pension reform, pension wealth elasticity, female labour supply, retirement, differences in differences

JEL: H55, J13, J21, J26

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1 Introduction

The pension wealth elasticity of employment is a crucial parameter for the design of the pension system and of pension reforms. This elasticity measures employment responses induced by a change in pension wealth that does not affect the implicit tax rate of earnings. While there is a large body of literature providing credible evidence about the overall employment effects with respect to changes in the retirement age or the generosity of the pension system, see, e.g., Blundell et al. (2016), there is hardly any evidence about the employment responses induced by a change in pension wealth and about the size of the pension wealth elasticity. However, a thorough understanding and a quantification of this behavioral margin is necessary for various pension policies, for example when designing policies to guarantee a standard of living for pensioners and to reduce old age poverty by introducing a minimum pension that is independent of contributions to the pension system.

In this paper, we provide novel empirical evidence about the effect of pension wealth on employment. We exploit unique variation that allows us to estimate overall employment responses and heterogeneous effects by age and by accumulated pension wealth. In the analysis, we focus on employment effects of mothers in their late career, i.e. after the age of 50. This group is of central interest as previous studies find that individuals toward the end of the working life – specifically mothers – show the largest labor supply responses, see e.g. Keane (2011) or Blundell et al. (2016). Specifically, we use the variation of a pension reform in Germany that raised pension entitlements related to children while not affecting the implicit tax rate of earnings. In 2014, the so-called "mother's pension" increased the pension entitlements granted for each child born before 1992. Importantly, the sizable increase in pension wealth is determined by the number of children born before 1992 and, by design, eligibility cannot be changed through behavioral adjustments after the introduction of the reform in 2014. For the identification, we use the sharp discontinuity induced by the reform: only mothers with children born before January 1, 1992, were affected by the pension reform (treatment group). For

mothers with children born after this cut-off date, pension wealth did not change (control group).[1] In the first part of the paper, we exploit this variation using a difference-in-differences estimator with individual fixed effects. We compare the employment behavior of treatment and control group members before and after the pension reform in 2014. In addition to the overall reform effect, we estimate heterogeneous reform effects by treatment intensity, i.e., the number of eligible children, age, and pre-reform pension wealth. Effect heterogeneity, specifically by age, is informative to understand for which groups, and at what times during the working life, employment effects are most important. Further, we analyze the effect of the pension reform on disability pensions and marginal employment to better understand behavioral responses. In the second part of the paper, we widen the perspective going beyond the analysis of this specific pension reform. We exploit the reform to estimate overall and age-specific elasticities of pension wealth in an instrumental variable setting. These elasticities are important for the general discussion about employment responses at the end of the working life and can be used to assess incentive effects of future pension reforms that affect pension wealth.

The empirical analysis is based on administrative data from the German pension insurance, which includes complete individual employment and earning histories along with detailed information about the age and number of children of 2% of all individuals covered by the German public pension system.

We find that employment effects of the pension reform are significant and economically important. Our estimates show that the pension reform, which increased pension wealth of affected women by on average 7.6%, reduces their employment rate by about one percentage point or 2%. We find a clear pattern by treatment intensity: The higher the increase in pension wealth, which is determined by the number of eligible children, the greater the reduction in employment. We find the largest effects for mothers with three or more children. Their employment rates decrease by more than 1.7 percentage points (close to 4%). These results are robust to variations in the cut-off period, hold for

1. Pension benefits for children born after 1992 have been higher since 1992.

different sample restrictions and using an event study design. Heterogeneity analyses document a strong age pattern, which shows that the employment effects are driven by behavioral responses of women close to retirement. Employment effects of women younger than 55 are not significant. For older ages, employment effects increase and are strongest for women close to the early retirement age, which is at age 63 for the cohorts considered. In contrast, we do not find a clear employment pattern by levels of pre-reform pension wealth. We further document that the reduction of employment for women older than 60 is partly explained by an increase in disability pensions but we find no measurable effect on marginal employment.

Turning to the more general picture we show that the pension wealth elasticities are sizable. Overall, we find that an increase in pension wealth by 1% significantly reduces employment by 0.13 percentage points, which translates into a pension wealth elasticity of about 0.22. The first stage results in which we use the pension reform as instrument are clear and very strong with F-statistics above 450. We document again a clear age pattern with elasticities close to zero before the age of 55 and high elasticities of 0.75 in the year before the early retirement age.

A sizable body of literature exploits variation and discontinuities in pension system design to estimate its causal effects on employment and retirement behavior; see e.g. Atalay and Barrett (2015); Cribb et al. (2016); Engels et al. (2017); Geyer and Welteke (2021); Manoli and Weber (2016b,a); Morris (2022b); Rabaté and Tréguier (2022); Seibold (2020); Staubli and Zweimüller (2013). In general, these studies provide evidence that individuals respond to financial incentives in the pension system and to changes in the retirement age. While these studies are important for assessing the overall effects of pension reforms, they cannot disentangle the role of the pension wealth effect and the substitution effect as both margins are simultaneously affected. Only a few studies[2] focus on the role of an income, aggregate wealth, or pension wealth effect for the retirement decision. Brown et al.

2. Atalay and Barrett (2015) and Morris (2022a) evaluate a reform of the Australian "Age Pension" that provides means-tested benefits regardless of the employment history. The reform gradually increased the qualifying age of the Age Pension for women. Thus, the reform lowers pension wealth of affected cohorts. Conditional on the means test, the results can be interpreted as income effects.

(2010) is a specific example in which the authors exploit variation in inheritances and document that a wealth shock reduces employment.[3] Fetter and Lockwood (2018) show sizable effects of Old Age Assistance in the US on labor supply and propose a method to bound estimates for the effects of income transfers versus the effects of marginal incentives to work. They report sizable income effects. Gelber et al. (2016) also use a bounding method and find large income effects of Social Security in the US. In contrast to these studies, which provide bounds of the income effect, we can directly exploit variation in pension rules to identify point estimates. In this sense, our study is similar to the studies by Danzer (2013), Giupponi (2019), Ye (2021), and Artmann et al. (ong). Danzer (2013) studies a massive and very particular increase in pension income in Ukraine, i.e. a threefold increase in the legal minimum pension, and shows sizable negative employment effects. Giupponi (2019) exploits a discontinuity in the generosity of survivor benefits in Italy related to the date of death which induces a sizable reduction of income for the surviving spouse. She finds a sizable income effect in the long run. Ye (2021) documents as well a significant and large pension wealth effect for low income women in Germany. For the identification she exploits a kinked relationship with the recipients' past contributions and a subsidy for pension income. Artmann et al. (ong) focus on the same pension reform as this study. In contrast to our analysis, they use social security data and focus exclusively on mothers giving birth to their first child in the months surrounding the cut-off date. For this group they find no employment effects on the extensive margin, but negative effects on the intensive margin.

We extend these studies as we can estimate the pension wealth effect for a very general group, namely all mothers – independent of the number of children. We provide novel evidence about heterogeneous employment effects by treatment intensity, age, and pension wealth. Moreover, we derive

3. A similar strand of the literature exploits lottery wins as exogenous sources to estimate income effects. For example, Cesarini et al. (2017) report a small income effect induced by lottery wins in Sweden. In contrast Golosov et al. (2021) find sizable income and wealth effects by lottery wins for the US.

age-specific elasticities of pension wealth that are important for understanding and interpreting the magnitude of the overall pension wealth effects.

The paper is structured as follows. In Section 2, we describe the German pension system and the 2014 pension reform. Then we discuss the data (Section 3), the method (Section 4), and present the results (Sections 5 and 6). Finally, Section 7 concludes.

2 Institutional Background

Public pensions are by far the largest source of income individuals have during retirement in Germany.[4] They are based on a contributory scheme that features only a small number of redistributive elements. Therefore, benefits are roughly proportional to the contributions during working life. Entitlements are calculated as pension points that are equal to the ratio of own earnings (up to a ceiling) to the average earnings during a year, see, e.g., Börsch-Supan and Wilke (2004); OECD (2021). Entitlements that are not linked to employment, are mainly related to children. One parent (usually the mother) receives pension points for having (raised) children. The overall number of child related pension points depends on the number of children and the child's year of birth. The statutory pension age (SRA) was 65 for cohorts born before 1947. It was stepwisely raised to age 67 and fully phased in for all cohorts born in 1964 or later. People qualify for this regular old-age pension after five years of pension contributions. For the cohorts we consider, retirement before the SRA (with permanent deductions) is possible at the early retirement age (ERA) of 63. The pathway to retirement before the ERA is only possible via disability pensions ("Erwerbsminderungsrente"), for people with severe health problems who are not able to work more than three hours a day.[5]

4. In 2019, about 60% of average gross income of the population 65+ came from the public pension system (Bundesregierung, 2020, p.101).

5. People who are able to work more than three hours a day but less than six are eligible for partial disability benefits.

2.1 Pension reform 2014

Child related pension points for 12 months were introduced in 1986. Since 1992, credited points differ by the child's year of birth: a pension reform in 1992 increased credited periods for children born from January 1, 1992, onwards to three years, whereas for children born before January 1, 1992, the credited period of one year remained unchanged.[6] Subsequently, the number of pension points granted for a credited period of one year was raised from 0.75 to 1.0 for all children, independent of year of birth, in three steps between 1998 and 2000. Therefore, since July 1, 2000, three pension points have been granted for each child born from January 1, 1992, onwards and one pension point for all children born before that date.[7]

The differential treatment of children was controversially discussed and it was the aim of the 2014 pension reform to level, to a certain extent, the differential treatment of children born before and after January 1, 1992. The first official reform proposal was presented on January 15, 2014. The final details of the reform were then published on June 24, 2014, and formally enacted on July 1, 2014. With the pension reform, the credited period for children born before January 1, 1992, was increased from one to two years.[8] For the majority of mothers, this implies that pension wealth increases by one pension point for each child born before 1.1.1992. The increase was lower for mothers with relatively

6. See Thiemann (2015) for a discussion and labor supply analysis of this pension reform.

7. This general rule has to be qualified. Total pension points in one year are capped at an upper ceiling that is given by the ratio of the contribution assessment ceiling ("Beitragsbemessungsgrenze") and the average wage income ("Durchschnittsentgelt"). This ratio increased from around 1.6 in the 1960s to slightly above 2 in the 2010. In the 1970s and 1980s, the two decades most relevant for this study, the ratio averaged 1.59 and 1.80, respectively. For 1990 to 1993, the average was 1.77.
Up to mid-1998, pension points for children were withdrawn on a one-to-one basis for pension points from employment. After a ruling by the constitutional court, the current rules were introduced in the 1999 Pension Reform Act (article 70) (published 22 December 1997). Those already retired were granted a supplement of one pension point per child (reduced to 0.75 (0.85, 0.9) points for pensions paid in 1998 (1999, 2000)) and, thus, "additivity" of pension points from employment and for child raising was also achieved for that group (article 307d of the 1999 Pension Reform Act). The pensions paid before mid-1998 remained unchanged.

8. The legislative process is documented on http://www.portal-sozialpolitik.de/index.php?page=rv-leistungsverbesserungsgesetz. The first draft bill, which already included the final rule of an additional credited period of one year for each child born before 1992 was published on January 15, 2014.

high labor earnings in the first two years after giving birth since the overall number of pension points per year is capped.[9]

To better understand the financial implications of the 2014 pension reform, in Table 1, we provide first descriptive evidence by comparing the accumulated number of pension points and the pension wealth in the month before (June 2014) and in the months after the introduction of the pension reform (July 2014). In the first row we present the effects for the control group, i.e. women with children born after 1992. Then, we focus on the treated women and show how the effect of the pension reform, i.e. the treatment intensity varies by number of children. On average, the number of pension points increased by about 7.6%. The striking difference in the increase of pension points by number of children shows the sizable financial impact of the pension reform. For mothers with one child, pension points increase by 4%, for women with two children by 10%, for those with three children by 14%, and for mothers with four or more children they increase by 21.0%. The increase in pension points is directly comparable to changes in pension wealth. The effect on pension wealth has the same structure but effects are slightly larger since, between these two months, not only did the child related pension change but there was also the regular increase in the nominal pensions of about 2%, which is independent of the presence and number of children. Therefore, pensions of women with no children born before 1992 (fist row) also see an increase in pension wealth.

Finally, Table 1 points to another important finding: The number of accumulated pension points is decreasing with the number of children, which is related to the lower employment rates and lower contributions to the pension system of mothers with more children.

In Figure 1, we provide more information about the distribution of the increase in pension points. Since the accumulation of the number of pension points is capped (see Footnote 7), mothers with sufficiently high pension contributions only partly benefited from the increase in child related bene-

9. See the second last footnote. About a quarter of new pension claimants in the second half of 2014, mainly from East Germany, were affected by the cap, see Keck et al. (2015).

Table 1. Change in average pension points by the number of children born before 1992

	Pension points			Pension amount in €		
No. of children before 92	June 2014	July 2014	Δ	June 2014	July 2014	Δ
0	28.19	28.23	0.00	793.30	807.59	0.02
1	26.62	27.55	0.04	749.09	788.26	0.06
2	24.45	26.22	0.09	688.08	750.10	0.11
3	21.95	24.51	0.14	617.62	701.29	0.16
4+	20.69	24.39	0.21	582.34	697.69	0.23
Total Treated	25.20	26.67	0.08	709.00	763.13	0.09

Notes: This table shows the average number of accumulated pension points of individuals in June 2014 and July 2014 and the associated pension amount for individuals separated by the number of children born before 1992. The sample consists of mothers who turn 50 between January 1st, 2010, and December 31st, 2018 and are not older than 63 years before January 1st, 2010. Additionally, the columns denoted by Δ show the average increase in Pension points/pension wealth between June and July 2014. Note that the control group only consists of individuals with 0 children born before 1992 while all individuals with at least one child born before 1992 are included in our treatment group.
Source: VSKT 2020, own calculations

fits.[10] In the left panel of Figure 1, we show that about 60% of the women received one pension point per child. About 40% of the mothers were affected by the capping but the majority of this group received close to one point per child. Average pension wealth increased by 0.703 points per child for this group. In the right panel of Figure 1, we summarize the overall increase in pension wealth for all children. Close to 40% of the mothers received one additional pension point, about 15% received two additional pension points, 5% three pension points, and only very few mothers received more than three points.

3 Data

We use high-quality administrative data from German public pension insurance accounts (Versichertenkontenstichprobe, VSKT). The VSKT is a stratified 2% random sample of all pension insur-

10. We are able to identify the exact increase in pension wealth for roughly 98.14% of the sample. For the remaining individuals we impute the amount of pension points by the total number of children born before 1992.

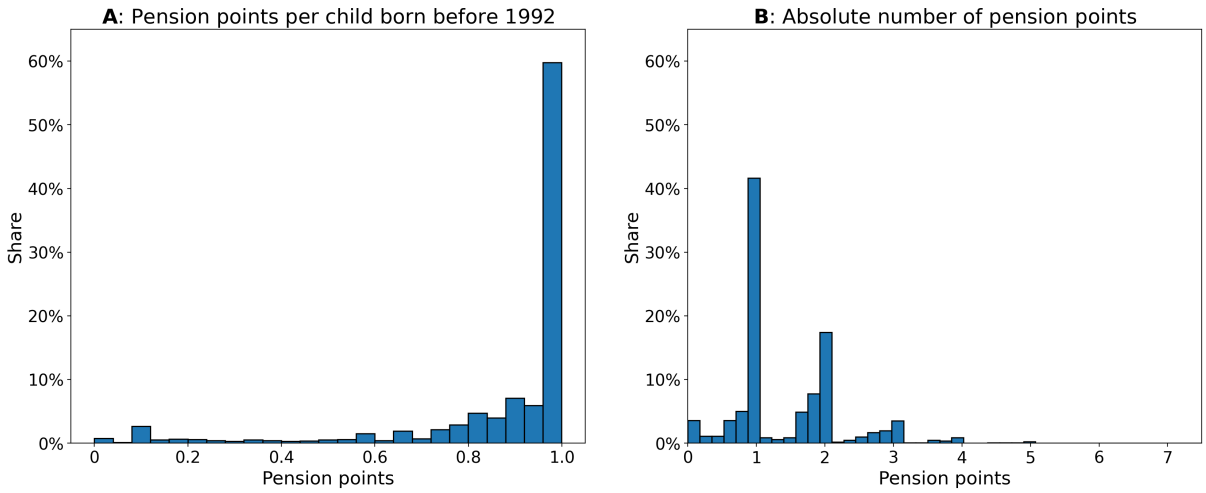


Figure 1. Reform induced increase in pension points

Notes: This figure presents the distribution of the actual increase in pension points individuals received through the reform in 2014. The figure only utilizes information regarding the share of the treated population for which we were able to determine the exact increase (98% of all treated individuals). On the left hand side, Panel **A** shows the distribution of the average pension point increase for each child born before 1992. On the right hand side, Panel **B** depicts the distribution of the total amount of additional pension points.
Source: VSKT 2020, own calculations

ance accounts of people in Germany aged 30–67.[11] The full individual employment history including retirement entry and contributions to the pension system are reported with monthly accuracy. In addition, and key for our analysis, the birth dates of all children are reported, thus allowing for the precise identification of the treatment and control groups, meaning that, together with the earnings history, we can exactly calculate the treatment intensity for each mother.

The central outcome variable in the analysis is regular employment with social security contributions. In addition, we focus on the effect of the pension reform on marginal employment, defined as jobs with low earnings[12] and on disability pensions.[13] Since we study the employment effects before the ERA of 63, retirement is only possible via disability benefits.

11. Since the data are process-produced, recall errors due to memory gaps and wrong temporal assignments are avoided and panel attrition is negligible. For more information about the data, see (Fachinger and Himmelreicher, 2006).

12. The threshold for marginal employment changed over time. During our observation period the threshold was 420 euros per months until 2012; thereafter 450.

13. The VSKT data do not include information about working hours or part-time employment.

For the main analysis, we restrict the sample to West German mothers who gave birth to at least one child between January 1, 1990, and December 31, 1993.[14] Individuals who lived in East Germany before the reunification of the country in 1990 (about 12.76% of the remaining observations) are excluded from the sample, as fertility rates in the East declined drastically in the early 1990s, which might affect the treatment and the control group differently (Chevalier and Marie, 2017). We also exclude all first generation immigrants[15] as well as individuals who do not have German citizenship (2.9% of the remaining observations). In addition, we also exclude all individuals born before 1955. We concentrate on employment effects toward the end of the working life but before the early retirement age. Therefore, we focus on women aged 50 to 63, which we observe for the 2010-2018 period. Given the age definition the panel over this time period is unbalanced. Applying all aforementioned restrictions leaves us with a total of 30,474 mothers and over 2 million monthly observations.

The identification strategy is explained in detail in Section 4. In short, it exploits variation between the treatment group and the control group before and after the introduction of the pension reform in July 2014. In the main specification we use a two year window before and after the cut-off date January 1, 1992, and assign all women who gave birth to at least one child between January 1, 1990, and December 31, 1991, to the treatment group. Women with children born between January 1, 1992, and December 31, 1993, are assigned to the control group. On average women, who gave birth after January 1, 1992, are younger; however, since the age at which a woman gives birth varies between mothers, we observe women born in the same cohorts in the treatment and the control groups (see Figure 8 in Appendix A). Thus, our identification strategy does not rely on the assumption that cohort effects are constant over time and do not change before and after the pension reform.

In total, we assign 7,692 women to the control group and 22,782 women to the treatment group. The difference in the sample size between the two groups is predominantly driven by the fact that

14. We also exclude all women who paid contributions to a special miners' pension scheme (Knappschaftliche Versicherung) for at least one month, which applies to about 0.83% of all women in the VSKT 2020.

15. First generation immigrants are defined as individuals who are not born in Germany, which corresponds to roughly 8.01% of individuals in the remaining dataset.

Table 2. Descriptive statistics of treatment and control group

Variable		Control Group	Treatment group
Birth Year	Mean	1963.70	1961.90
	Std	(3.07)	(3.34)
No. of children	Mean	1.80	2.43
	Std	(0.79)	(1.02)
No. of children bf 92	Mean	0.00	1.66
	Std	(0.00)	(0.84)
Pension points (June 2014)	Mean	28.37	25.03
	Std	(11.45)	(11.26)
Pension Wealth (June 2014, in €)	Mean	798.38	704.47
	Std	(322.11)	(316.74)
Acc. years employment (June 2014)	Mean	15.49	14.91
	Std	(8.91)	(9.63)
No. of Individuals		7,692	22,782

Notes: The table shows means and standard deviations of key variables for the 30,474 individuals in the treatment and the control group. In the main specification, we assign all women who gave birth to a child between January 1, 1990, and December 31, 1991, to the treatment group. Women with children born between January 1, 1992, and December 31, 1993, are assigned to the control group. Pension wealth is expressed in euros per month.
Source: VSKT 2020, own calculations

women are only part of the control group if they gave birth to their first child after January 1, 1992. Women in the treatment group may have children on either side of the cutoff and therefore have, by construction, more children. Importantly, only children born before the cutoff date define the treatment intensity.

In Table 2, we provide descriptive statistics for the treatment and the control groups of the main sample. As explained above, individuals in the treatment group are, on average, older than individuals in the control group (about two years). In addition, we observe compositional differences regarding the total number of children. While mothers in the control group have, on average, 1.88 children, the average for the treatment group is higher, at more than 2.43. Related to this difference, mothers in the control group have, on average, a longer labor market history, more accumulated pension points, and their pre-reform pension wealth, expressed in euros per month, is higher. Importantly, in the empirical analysis, we account for these differences by including individual specific effects and by controlling for the age of the children, which changes over time. Moreover, in a robustness check (Appendix B), we reduce the age difference of mothers in the treatment and control groups focusing

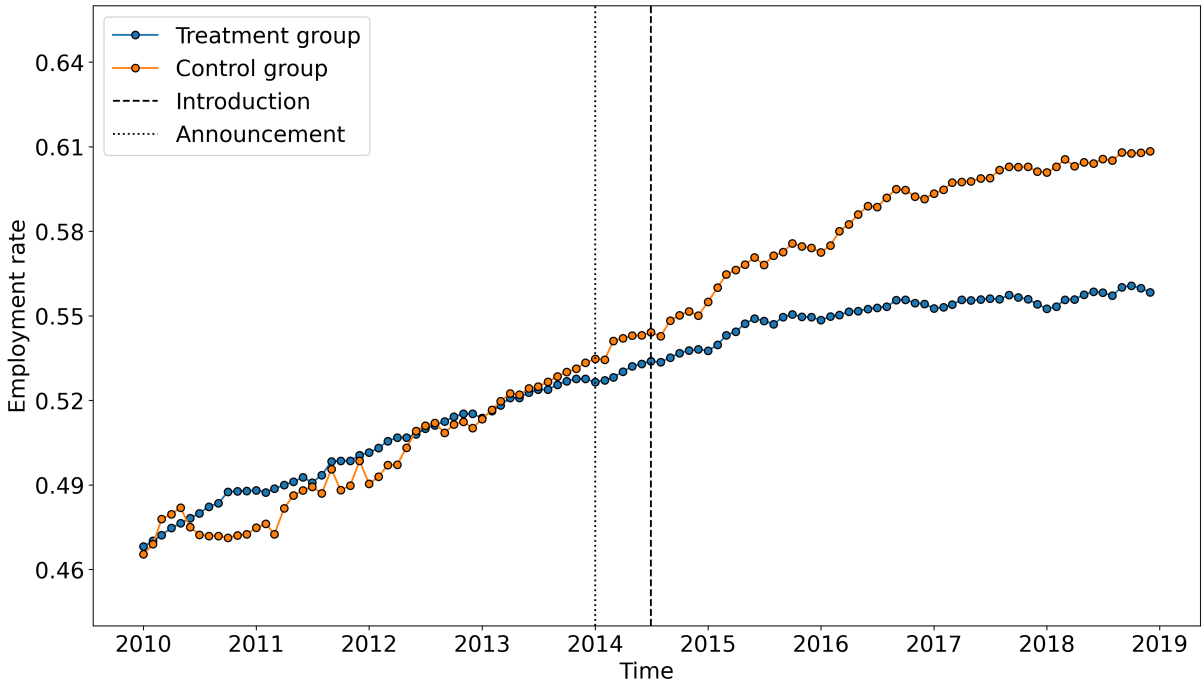


Figure 2. Monthly employment rates of treatment and control group by age (2010–2019)

Notes: This figure illustrates the employment rates of the estimation sample between January 2010 and December 2018. The blue line indicates the treatment group while the orange line denotes the employment rate in the control group. The dotted and dashed vertical lines represent the announcement and the introduction of the reform in January and July 2014.
Source: VSKT 2020, own calculations

solely on a balanced sample of women born in cohorts 1960-1964. As discussed below, the main results are very similar using either of the two different samples.

Before turning to the econometric analysis, we provide first graphical evidence about the effect of the pension reform on employment. Figure 2 shows the monthly average employment rates of individuals in the treatment (blue) and the control (orange) groups between January 2010 and December 2018. The dashed vertical line represents the introduction date of the pension reform. The graph gives a clear picture: In line with the overall employment trend for women in Germany during that time period (see Appendix, Figure 9), employment rates increase for women in the treatment and the control groups between 2010-2014. In contrast, in the post-reform period, employment rates of the control group continue to increase while this was not the case for the employment rates of the treated women. After the introduction of the pension reform employment rates remain stable for

this group. The trends of the treatment and the control start to diverge several months before the introduction of the reform. This might be explained by anticipation effects related to the first official publication of the reform proposal in January 2014 (see above). In the empirical analysis we return to the discussion of potential anticipation effects.

4 Methodology

In the empirical analysis, we use two different approaches. First, we directly estimate the effect of the pension reform on labor market outcomes using a difference in differences (DID) approach. Second, we leverage the variation induced by the pension reform to estimate the elasticity of pension wealth in an instrumental variable (IV) setting.

Effect of the pension reform

To identify the effect of the pension reform, we exploit the discontinuity in the reform design by the date of birth of children. This allows us to define a treatment group, mothers with children born before January 1, 1992, and a control group, mothers with children only born after this date.[16] We then compare labor market outcomes of the treatment and the control groups before and after the introduction of the pension reform. In the main specification, we assign all women who gave birth to a child between January 1, 1990, and December 31, 1991, to the treatment group and all women with children born between January 1, 1992, and December 31, 1993, to the control group. As documented above, the number of children and the birth cohorts of mothers differ between the treatment and control groups. In the empirical analysis, we account for this variation by including monthly

16. As discussed in Section 3, women who gave birth before and after the cut-off date belong to the treatment group. However, only children born before 1992 count for the treatment intensity.

time-specific fixed effects, individual fixed effects, and other time invariant differences between the treatment and the control groups. The fixed effects account as well for education, overall number of children,[17] birth date of children, and further unobserved effects that are constant over time. Finally, we include a linear age trend for the first child born. Importantly, as discussed above, we observe women born in the same cohorts in both the treatment and control groups (See Figure 8). Thus, the identification does not rely on the assumption that cohort effects of the mother are constant over time.

More formally, to estimate the labor market effects of the pension reform, we specify the following DID regression:

$$y_{it} = \alpha_i + \lambda_t + \beta Post_t \times Treatment_i + \gamma X_{it} + \varepsilon_{it} \quad (1)$$

where y_{it} is the outcome variable for individual i at time t; α_i, and λ_t are individual and monthly time fixed effects, respectively. β is the reform coefficient of main interest which measures the effect of the interaction term of indicator variables for the treatment group, i.e. women with children born before January 1, 1992, and the post reform period, i.e. after July 1, 2014. Variables that vary between individuals and over time are captured by X_{it} and ε_{it} denotes the idiosyncratic error term.

Effect of pension wealth on employment

In the second analysis we turn to the IV framework to estimate the effect of pension wealth on employment. In the first stage, we estimate the effect of the pension reform on log pension wealth

17. We only focus on women older than 50 years, therefore the number of children does not change over time.

(PW).

$$logPW_{it} = \theta_i + \delta_t + \gamma Post_t \times Treatment_i + \kappa X_{it} + \varepsilon_{it} \tag{2}$$

θ_i and δ_t denote again individual and monthly time fixed effects. $Post_t$ is a dummy variable that is equal to one if period t occurs after the introduction of the reform and $Treatment_i$ is a binary variable that takes value one if individual i is part of the treatment group. In the second stage we then formulate the relation between log pension wealth and employment conditional on individual fixed effects (α_i) and monthly time fixed effects (λ_t). Variables that vary between individuals and over time are included in X_{it}:

$$y_{it} = \alpha_i + \lambda_t + \beta \widehat{logPW}_{it} + \xi X_{it} + \varepsilon_{it} \tag{3}$$

where $\widehat{logPW}_{it}$ are the predicted values of pension wealth obtained in the first stage of the regression.

5 Effect of pension reform

In this section, we present the estimation results. We first focus on the average employment effects, provide various robustness checks and discuss the heterogeneous effects by age and pre-reform pension wealth. Then we turn to the results for the effects on retirement and marginal employment.

5.1 Average employment effects

In Table 3, we present the main estimation results. We consider three different specifications. In Column 1, we focus on the overall reform effect as described in Equation 1 (Dummy specification).

As documented in Table 1, the treatment intensity increases with the number of eligible children. Therefore, in Column 2, we interact the reform indicator with the number of children a mother gave birth to before 1992, i.e. the number of eligible children (linear specification). This specification assumes that the treatment intensity has a linear effect. In the next specification (Column 3), we split the treatment group into three categories based on whether the mother gave birth to one, two, or more than two children before 1992 (nonlinear specification). We assign an indicator variable to each of these subgroups and include the interaction of the respective dummy with the post reform indicator variable in our regression. In addition to the point estimates that measure changes in percentage points we calculate the relative effects (in percent) with respect to the group specific pre-reform employment level of the treatment group. These effects are presented in square brackets.

All specifications point in the same direction and show a clear picture: The increase in child related pensions significantly reduces employment. Specifically, the results in Column 1 indicate that mothers affected by the pension reform reduce labor supply by an average of 1.08 percentage points, which is a reduction of about 2% relative to the pre-reform employment rate of 53%. The employment effects increase with treatment intensity (Columns 2 and 3): While the estimated effect for mothers with only one child is small (0.062) and not statistically significantly different from zero, the effect is larger and significant for mothers with two eligible children (1.3 percentage points or 2.5%) and for mothers with more than two eligible children (1.69 percentage points or 3.8%).

Robustness

The validity of the DiD approach relies crucially on the parallel trend assumption of the treatment and the control group. We documented in Figure 2 that the data for the pre-reform period are compatible with this assumption. To further corroborate this assumption and to ensure the robustness of the regression results, we complement the DID analysis with an event study analysis for the overall reform

Table 3. Results Baseline

Dependent Variable: Employment			
	(1)	(2)	(3)
Post × Mother pension	−0.0108**		
	(0.0044)		
	[−0.0198]		
Post × no. of children before 92		−0.0049***	
		(0.0016)	
		[−0.0089]	
Post× 1 child before 92			−0.0062
			(0.0049)
			[−0.0108]
Post × 2 children before 92			−0.0130**
			(0.0051)
			[−0.0245]
Post × 3 or more children before 92			−0.0169***
			(0.0059)
			[−0.0387]
Observations		2,097,523	
Individuals		30,474	
Treated Individuals		22,782	
Untreated Individuals		7,692	
Time & Individual Fixed Effects		✓	

Notes: This table displays the reform's effect in 2014 on employment between January 2010 and December 2018. The dataset is limited to women who were born after 1954, are between 50 and 63 years between January 2010 and December 2018, and have given birth to at least one child between January 1990 and December 1993. Individuals are assigned to the treatment group if they had at least one child before January 1st, 1992. The control group consists of individuals whose children were all born after this cutoff date. Standard errors are reported in parentheses and are clustered on the individual level. Relative effects are shown in square brackets.

*** p<0.01, ** p<0.05, * p<0.1

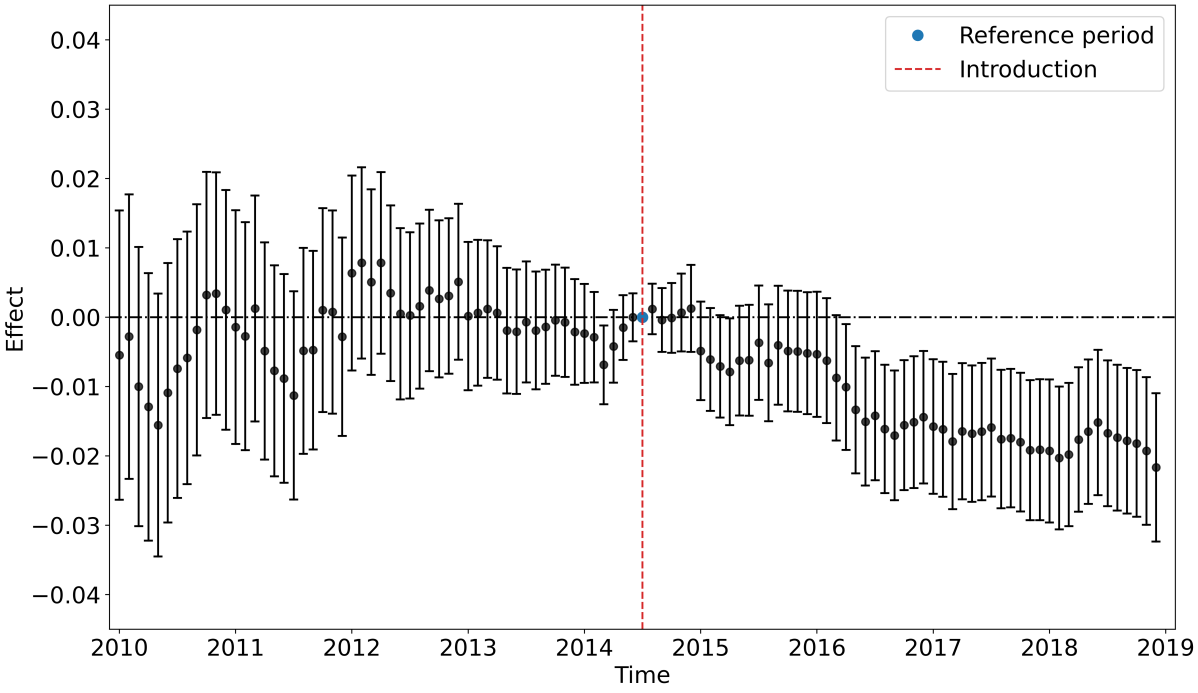


Figure 3. Event study results employment

Notes: This figure displays the estimates of a regression of the employment status on a set of interaction terms of the treatment group identifier and monthly time dummies from January 2010 to December 2018. Time and individual fixed effects are included in the regressions. The black dots denote the point estimates and the vertical black lines represent the associated 95% confidence intervals. The vertical dashed red line indicates the introduction of the reform in July 2014.

effect. Specifically, in Figure 3, we split the time period from January 2010 to December 2018 in monthly intervals and estimate for each month the treatment effect relative to the reform effect in July 2014 conditional on individual fixed effects and time fixed effects.

We present point estimates of the effect on employment and the associated 95% confidence intervals. The dashed red line represents the introduction of the reform in July 2014 and the blue point denotes the reference period. The results show a clear common pre-reform trend for the treatment group and the control group: in general, the differences before the introduction of the reform are small in magnitude and never significantly different from zero. This holds as well for the months before the introduction of the pension reform. Thus, there is no empirical evidence for anticipation effects. Further, the graph shows that the reform effect increases during the post reform period. While we observe no significant and only small effects in the first year after the introduction of the reform, the estimates become statistically significant and larger (close to -2 percentage points) 18 months after the introduction of the reform. This time pattern can be explained by adjustment costs in the short run. In addition, the pattern might be related to age effects since the women in our sample get older over time. We return to the age effects in Figure 4.

Employment effects in all specifications are robust to variations in the length of the cutoff period (Table 4). Results in Column 1 include all mothers who had at least one child born between 1986 and 1997. In the following columns we reduce the cutoff period by one year on both sides until only women with children born in 1991 and 1992 are considered (Column 6). The sample in Column 5 replicates the results in Table 3. By extending the cutoff period the sample significantly increases.

The results depict a clear pattern: For all cutoff lengths and all presented specifications, the estimated parameter values point in the same direction and are highly statistically significant at the 1% or the 5% level throughout all three different specifications, except for the last specification, which only includes observations for one year before and after the cut-off. For this specification, the effects only become significant when we consider the treatment intensity by the number of children born

Table 4. Results for different cutoff lengths - employment

	(1)	(2)	(3)	(4)	(5)	(6)
Part A: Dummy specification for different lengths of the cutoff period						
	1986-1997	1987-1996	1988-1995	1989-1994	1990-1993	1991-1992
Post × Mother pension	-0.0281***	-0.0252***	-0.0211***	-0.0164***	-0.0108**	-0.0080
	(0.0031)	(0.0033)	(0.0035)	(0.0038)	(0.0044)	(0.0057)
	[-0.0523]	[-0.0468]	[-0.0393]	[-0.0306]	[-0.0202]	[-0.0151]
Part B: Linear specification for different lengths of the cutoff period						
	1986-1997	1987-1996	1988-1995	1989-1994	1990-1993	1991-1992
Post × no. of children before 92	-0.0087***	-0.0080***	-0.0069***	-0.0064***	-0.0049***	-0.0052**
	(0.0011)	(0.0012)	(0.0013)	(0.0014)	(0.0016)	(0.0021)
	[-0.0161]	[-0.0148]	[-0.0129]	[-0.0119]	[-0.0091]	[-0.0098]
Part C: Nonlinear specification for different lengths of the cut off period						
	1986-1997	1987-1996	1988-1995	1989-1994	1990-1993	1991-1992
Post × 1 child before 92	-0.0216***	-0.0195***	-0.0160***	-0.0107**	-0.0062	-0.0026
	(0.0035)	(0.0037)	(0.0039)	(0.0043)	(0.0049)	(0.0062)
	[-0.0382]	[-0.0344]	[-0.0281]	[-0.0188]	[-0.0108]	[-0.0046]
Post × 2 children before 92	-0.0322***	-0.0282***	-0.0238***	-0.0193***	-0.0130**	-0.0111*
	(0.0035)	(0.0037)	(0.0039)	(0.0043)	(0.0051)	(0.0067)
	[-0.0591]	[-0.0518]	[-0.0436]	[-0.0357]	[-0.0245]	[-0.0216]
Post × 3 or more children before 92	-0.0310***	-0.0293***	-0.0256***	-0.0223***	-0.0169***	-0.0156**
	(0.0042)	(0.0044)	(0.0047)	(0.0051)	(0.0059)	(0.0079)
	[-0.0687]	[-0.0643]	[-0.0569]	[-0.0507]	[-0.0387]	[-0.0368]
Observations	4,265,016	3,832,166	3,348,183	2,777,947	2,097,523	1,168,539
Individuals	60,734	54,785	48,077	40,098	30,474	17,149
Treated Individuals	41,305	37,854	33,918	28,960	22,782	13,153
Untreated Individuals	19,429	16,931	14,159	11,138	7,692	3,996
Time & Individual Fixed Effects	✓	✓	✓	✓	✓	✓

Notes: This table displays the effect of the reform in 2014 on employment between January 2010 and December 2018 for varying lengths of the cutoff period. The dataset is limited to women who are born after 1954, are between 50 and 63 years between January 2010 and December 2018 and have given birth to at least one child in the respective time period. Individuals are assigned to the treatment group if they had at least one child before January 1st, 1992. The control group consists of individuals whose children were all born after this cutoff date. All regressions include time and individual fixed effects. Standard errors are reported in parentheses and are clustered on the individual level. Relative effects are presented in square brackets.
*** p<0.01, ** p<0.05, * p<0.1

before 1992. The point estimates are larger for the specifications with a broad cutoff window, which is related to the larger variation in the age composition in the treatment and the control groups.

As shown in Table 2, women in the treatment group are older than women in the control group in the main sample. Moreover, the composition of cohorts changes over time (see Figure 8). In a final robustness check, we restrict the sample to a balanced panel of women born in cohorts 1960-1964 to make the age and cohort composition more homogeneous. In Appendix B, we present, in addition to the main estimation table, descriptive statistics, the common trend graph, and the results of the event study for this restricted sample. Despite the differences in the cohorts and the different age distributions, results based on the restricted sample are very similar to the results in the main specification that includes all cohorts.

5.2 Effect heterogeneity

So far, we focus on the average effect of the pension reform on employment. In the following, we examine effect heterogeneity along two important dimensions: by age and by accumulated pension wealth.

Age: The reform effects can vary by age for two main reasons. First, the pension reform does not affect income in the current period but pension wealth, i.e. expected income during retirement. Under the assumption that individuals are forward-looking and discount future income, the intensity of treatment is higher for individuals closer to the statutory retirement age. Second, labor market attachment is declining at the end of the working career, which is related to preferences and labor market constraints; see e.g. Blundell et al. (2016). Both mechanisms suggest that the employment responses should be increasing with age.

In our sample, we include women born in cohorts 1955-1968. Thus, we only observe women younger than 59 in the pre-reform period (before the year 2014). In a first approach, we use a non-

parametric age specification to identify age-specific reform effects for women younger than 59. To also identify age-specific effects for women 60 or older, we use, in a second approach, a parametric age specification and interact the reform effect using a linear and quadratic functional form for age. The detailed estimation results for the reform effect are presented in Table 11 in Appendix C while Figure 4 summarizes the results of the age patterns. Specifically, we present point estimates and confidence intervals of the non-parametric specification and the parametric specification with linear and quadratic age effects. The age specific point estimates of the two specifications are very similar and not statistically different. This provides evidence that the age pattern in the parametric specification is accurately described by the imposed functional form.

The results show a very clear age pattern and document that the relatively large employment effects for the full sample (Table 3) are mainly driven by older women close to the early retirement age. Employment effects are small and not significant for women younger than 56. Then the effects increase with age. For women aged 62, we find the largest effects which suggest that they reduced employment by about 6 percentage points.

Pre-reform Pension wealth: In Table 5 we show how employment effects vary with pension wealth accumulated before the pension reform. We split the sample by cohort-specific median pre-reform pension wealth. Pension wealth is higher for individuals with a long working history and stronger attachment to the labor market. At the same time, individuals with low accumulated pension wealth are more likely to be credit constrained. The increase in pension wealth due to the pension reform relaxes this constraint. Both channels suggest that women with low pension wealth should respond stronger to the reform.

Overall, we do not find strong differences in employment behavior between the two groups. While we find a stronger absolute reduction in employment for women with above median pension wealth (about 2 percentage points), the relative effect shown in square brackets is larger for women

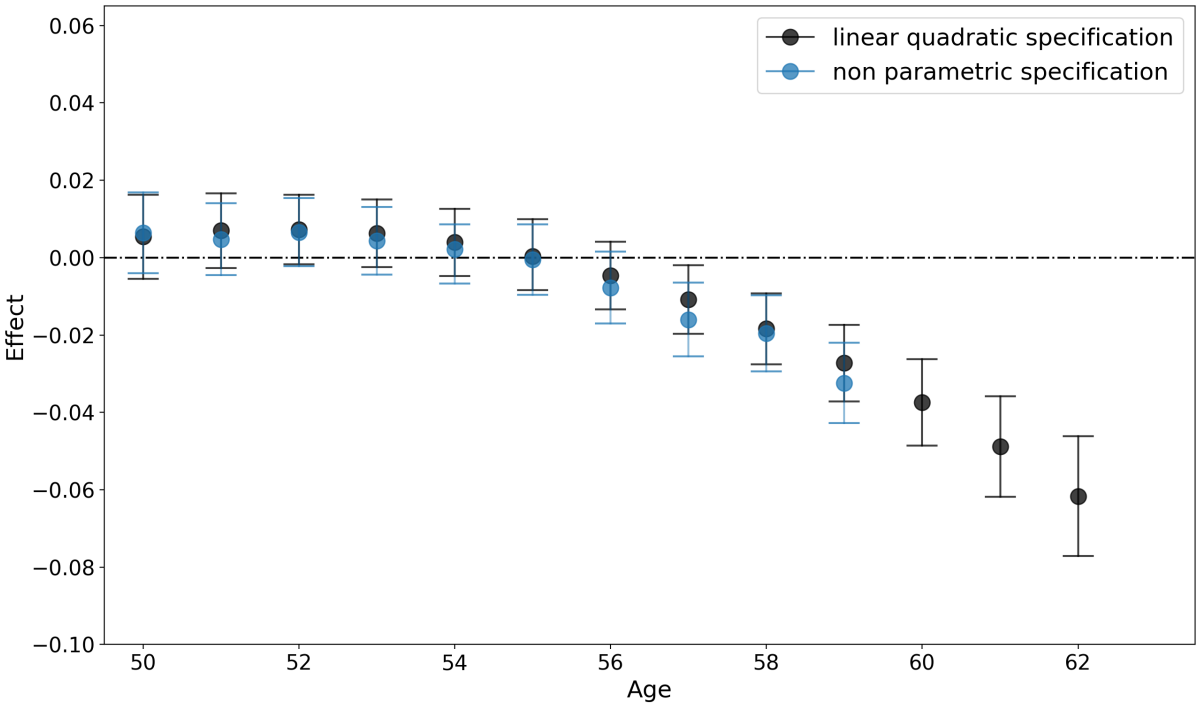


Figure 4. Reform effect on employment by age

Notes: This graph illustrates the reform effect on employment depending on age for the linear quadratic (black) as well as the non parametric specification (blue). The dots indicate the point estimates whereas the vertical lines show the associated 95% confidence intervals.

Table 5. Effect heterogeneity - Pre-reform pension wealth

Dependent Variable: Employment						
	Below Median			Above Median		
	(1)	(2)	(3)	(4)	(5)	(6)
Post × Mother pension	-0.0111*			-0.0201***		
	(0.0063)			(0.0061)		
	[-0.0376]			[-0.0253]		
Post × no. of children bf 92		-0.0066***			-0.0105***	
		(0.0021)			(0.0025)	
		[-0.0222]			[-0.0132]	
Post × 1 child bf 92			-0.0050			-0.0109*
			(0.0071)			(0.0066)
			[-0.0174]			[-0.0133]
Post × 2 children bf 92			-0.0100			-0.0272***
			(0.0072)			(0.0070)
			[–0.0326]			[-0.0346]
Post × 3 or more children bf 92			-0.0234***			-0.0322***
			(0.0079)			(0.0092)
			[-0.0807]			[-0.0464]
Observations	1,049,045	1,049,045	1,049,045	1,048,478	1,048,478	1,048,478
Individuals	15,242	15,242	15,242	15,232	15,232	15,232
Treated Individuals	12,004	12,004	12,004	10,778	10,778	10,778
Untreated Individuals	3,238	3,238	3,238	4,454	4,454	4,454
Time & Individual Fixed Effects	✓	✓	✓	✓	✓	✓

Notes: The table reports the effect of the 2014 pension reform on employment between January 2010 and December 2018 differentiated by pension wealth. The baseline sample is split according to the cohort specific median of accumulated pension points individuals had accumulated in June 2014. The datasets are limited to women who are born after 1954, are between 50 and 63 years and have given birth to a child between 1990 and 1993. The treatment group consists of women who have given birth to at least one child before January 1st, 1992. Individuals are assigned to the control group if all of their children were born after this cutoff date. All regressions include time and individual fixed effects. Standard errors are reported in parentheses and are clustered on the individual level. Relative effects are presented in square brackets.
*** p<0.01, ** p<0.05, * p<0.1

with below median pension wealth. The picture is again mixed when we account for the number of treated children.

5.3 Reform effect on disability pensions and marginal employment

To better understand the negative employment effects of the pension reform, we extend the analysis of the pension reform on labor market outcomes and present results on how the increase in the pension wealth affects disability pensions and employment in marginal jobs. A decline in regular employment might come along with an increase in marginal employment, the receipt of disability

Table 6. Results baseline - disability & marginal employment

	Panel A: disability pension			**Panel B**: marginal employment		
	(1)	(2)	(3)	(4)	(5)	(6)
Post × Mother pension	0.0044**			−0.0080		
	(0.0022)			(0.0019)		
Post × no. of children bf 92		0.0050***			−0.0042**	
		(0.0010)			(0.0019)	
Post× 1 child bf 92			0.0006			−0.0030
			(0.0024)			(0.0054)
Post × 2 children bf 92			0.0045*			−0.0124**
			(0.0026)			(0.0056)
Post × 3 or more children bf 92			0.0131***			−0.0104
			(0.0034)			(0.0068)
Observations			2,097,523			
Individuals			30,474			
Treated Individuals			22,782			
Untreated Individuals			7,692			
Time & Individual Fixed Effects			✓			

Notes: This table displays the effect of the reform in 2014 on disability and marginal employment between January 2010 and December 2018. The dataset is limited to women The dataset is limited to women who are born after 1954, are between 50 and 63 years between January 2010 and December 2018 and have given birth to at least one child between January 1990 and December 1993. Individuals are assigned to the treatment group if they had at least one child before January 1st, 1992. The control group consists of individuals whose children were all born after this cutoff date. Standard errors are reported in parentheses and are clustered on the individual level. Relative effects are shown in square brackets.
*** p<0.01, ** p<0.05, * p<0.1

pensions, unemployment or non-employment without receiving unemployment or pension benefits. We look at two of these states that can be observed in our data set, namely the receipt of disability pensions and marginal employment.[18]

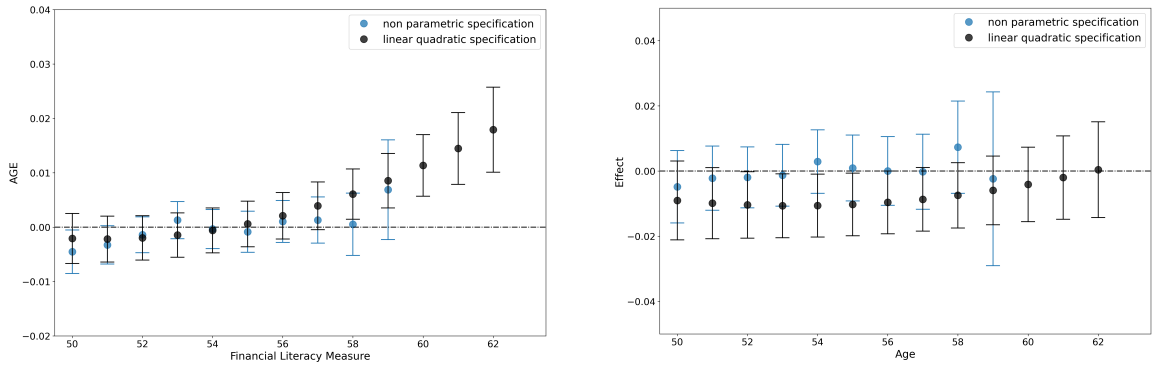
For the analysis, we focus on the main sample and use the empirical specification defined in Equation 1. The increase in pension wealth makes disability pensions financially more attractive. Therefore, we expect a positive effect on disability. However, given that eligibility for disability pensions depends on a rigorous medical assessment, the responses to the financial incentives can be expected to be limited. The effect of the pension reform on marginal employment are ex-ante not clear. On the one hand, women might reduce marginal employment and turn to disability pensions,

18. In the data we can not clearly distinguish unemployment or non-employment.

unemployment, or non-employment. On the other hand, women with regular employment might reduce working hours and switch from regular employment to marginal employment.

In Table 6, we present the average effect of the reform on disability pension and marginal employment and how the effects vary with the treatment intensity. In Figure 5, we additionally show the age-specific effects of the pension reform, as we documented the strong age gradient for employment. In Appendix D, we present results of the event studies (Figure 12 and 13), showing that findings do not change with varying cut-off periods (Table 16 and 17). Overall, we find a small positive effect on retirement of 0.44 percentage points, which is significant at the 5% level. The effect is increasing with the number of eligible children (Columns 2 and 3 in Table 6) and only significant for women with two and three or more children. In fact, for the latter group the effect is sizable. Retirement increases by 1.31 percentage points. Still, for all groups the employment effect is clearly larger than the retirement effect, thus indicating that the majority of women make a transition into non-employment. The positive retirement effects have again a clear age pattern (Figure 5a), which is consistent with higher disability rates for older individuals. According to the non-parametric age specification, the retirement effects are close to zero and not significant for women younger than 60. They increase after the age of 60 and are close to 2 percentage points for women aged 62.

Panel B of Table 6 and Figure 5b show the overall and age-specific results for marginal employment. The reform effect for all mothers is not significant (Column 1). This result holds over the full age distribution. We only find significant and negative effects when accounting for the different treatment intensity. Specifically, women with two children reduce marginal employment by about 1 percentage points.



(a) Disability pension **(b)** Marginal employment

Figure 5. Reform effect on selected employment states by age

Notes: This graph illustrates the reform effect on disability pension (part a) and marginal employment (part b) depending on age for the linear quadratic (black) as well as the non parametric specification (blue). The dots indicate the point estimates whereas the vertical lines show the associated 95% confidence intervals.

6 Pension Wealth Elasticity of Employment

In the final section, we discuss the role of pension wealth on employment more generally, using the pension reform to identify and quantify pension wealth elasticities. First, we focus on the overall elasticity and then we use the age-specific reform effects to derive wealth elasticities over the age distribution.

The bottom of Table 7 presents the results of different specifications of the first stage regression in which we use the pension reform as instrument. In Column 1, we use the specification defined in Equation 2 with the overall pension reform as instrument. In Columns 2 and 3 we account for different treatment intensity by number of eligible children. We find positive and highly significant effects of the pension reform on log pension wealth in all specifications: On average the pension reform increases pension wealth by 8% (Column 1). The effect is increasing with the number of eligible children. For mothers with one eligible child pension wealth increases by close to 5%, for mothers with two children by about 9%, and for women with 3 or more children by more than 14%. The F-statistics which are above 450 in all specifications, show the high relevance of the instruments.

Table 7. Results baseline regressions IV

Second Stage	Employment		
	(1)	(2)	(3)
Log Pension Wealth in €	-0.1257**	-0.1187***	-0.1257***
	(0.0516)	(0.0388)	(0.0385)
Elasticity	-0.2346	-0.2216	-0.2345
	[-.423 ; -.046]	[-.364 ; -.080]	[-.375 ; -.094]
First Stage	Log Pension Wealth in €		
	(1)	(2)	(2)
Post × Mother Pension	0.0856***		
	(0.0014)		
Post × no. of children bf 92		0.0411***	
		(0.0007)	
' Post × 1 child bf 92			0.0543***
			(0.0017)
Post × 2 children bf 92			0.0923***
			(0.0018)
Post × 3 or more children bf 92			0.1442***
			(0.0025)
F-value	459.6	503.2	492.0
Observations		2,091,146	
Individuals		30,422	
Treated Individuals		22,730	
Untreated Individuals		7,692	
PW Treated Pre		700.53	
Time and Individual Fixed Effects		✓	

Notes: The Table presents the results of a IV regression of employment between 2010 and 2018 on log pension wealth in € which are instrumented by our post × treatment indicator (difference in differences approach). The respective dataset only includes women who are between 50 and 63 years old and had at least one child between 1990 and 1993. The treatment group consists of women who have given birth to at least one child before January 1st, 1992. Individuals are assigned to the control group if all of their children were born after this cutoff date. All regressions include individual and time fixed effects. Standard errors are reported in parentheses and are clustered on the individual level.
*** p<0.01, ** p<0.05, * p<0.1

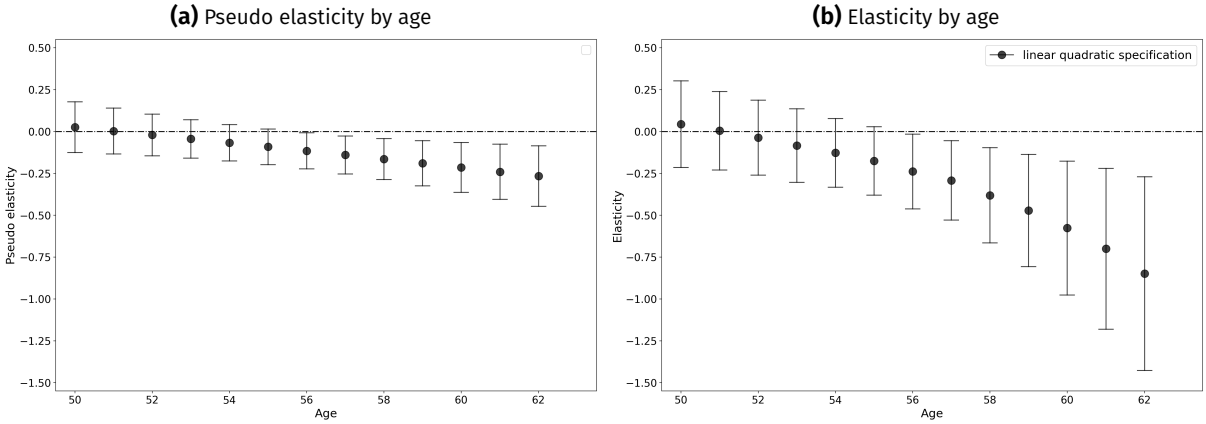


Figure 6. Elasticities and pseudo elasticities for different samples

Notes: The graphs shows the pseudo elasticities (panel **(a)** and the resulting elasticities (panel **(b)**) at different ages based on the linear quadratic interaction between log pension wealth and age presented in table 12. The marker symbols denote the point estimates and the vertical lines the 95% confidence intervals. The elasticities are constructed by dividing the age dependent pseudo elasticities by the pre reform employment rates for the given age group. Since there are no individuals aged 60 to 62 in the pre reform period, we use extrapolated employment values to compute the elasticities for this age groups.

The instrumented log wealth has a strong and significant effect on employment (Top panel of Table 7). The results of the three specifications are very similar. According to the most general specification (Column 3), we find that an increase of pension wealth by 1% significantly reduces employment by about 0.13 percentage points. This translates to age pension wealth elasticity of about 0.23. This elasticity is sizable and implies that pension reforms that change pension wealth without affecting the implicit tax rate of earnings lead to behavioral responses.

To better understand why the average pension wealth effect is large, we focus on the age distribution of the elasticities. Specifically, we use a linear quadratic specification of age effects to estimate age specific responses to an increase of pension wealth by 1%. Estimation results are presented in Table 12 in the Appendix. In Figure 6 we show the distribution of pseudo elasticities measured in percentage points and pension wealth elasticities defined as relative changes.

In line with the age pattern discussed above, we find that elasticies of pension wealth strongly increase with age and are largest close to retirement. Elasticities are not significant until the age of

55 and only increase for older women. We find the highest wealth elasticity - about 0.75 - for women close to retirement, at the age of 62.

7 Conclusion

In this paper, we exploit unique variation to identify the employment effect induced by an exogenous change in pension wealth. Specifically, we use the variation induced by a pension reform in Germany that raised pension benefits related to children but that did not affect the marginal tax rate of employment. In 2014, the so-called "mothers pension" increased the pension benefits granted for each child born before 1992. The empirical analysis is based on rich administrative data from the German pension insurance, which include complete individual employment histories.

Based on a difference in differences estimation we find that employment effects of the pension reform are significant and economically important. Our estimates show that the pension reform, which increases pension wealth on average by 7.6%, reduces the employment rate of affected women by about one percentage point. We find a clear pattern by treatment intensity. The higher the wealth effect, i.e., the more eligible children, the greater the reduction in employment. These results are robust to variations in the cut-off period, hold for different sample restrictions, and for using an event study design. Heterogeneity analyses document a strong age pattern, thus showing that the employment effects are mainly related to behavioral responses of women close to retirement. In contrast, we do not find a clear employment pattern by pre-reform pension wealth. We further document that the employment effects for women older than 60 are partly explained by an increase in disability pensions. The effects on marginal employment are negligible.

Turning to the more general picture, we show that pension wealth elasticities are sizable. Overall, we find that an increase in pension wealth by 1% significantly reduces employment by 0.13 percent-

age points, which translates into a pension wealth elasticity of about 0.22. This elasticity has again a clear age pattern with elasticities close to zero before the age of 55 and high elasticities of 0.75 close to the retirement age.

Our results have important policy implications. We show that pension wealth effects are significant and meaningful, but we also document that they are only present later in the working life at ages close to retirement. This implies that the introduction of a minimum pension unconditional on life time earnings should not lead to meaningful employment responses before the age of 55. However, for women closer to the retirement age, such a reform would reduce employment with negative consequences for the labor market and for the overall fiscal costs of a minimum pension.

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A Additional Sample Statistics

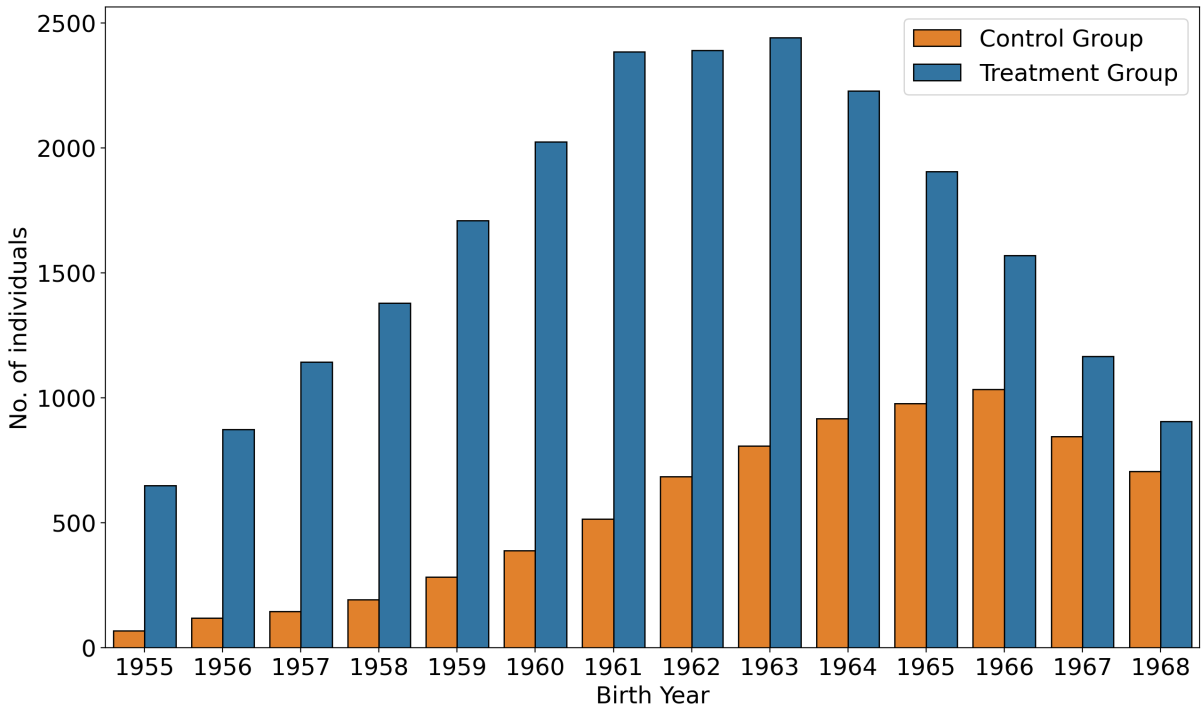


Figure 8. Age distribution

Notes: This figure shows the number of treated and untreated individuals in each birth cohort.

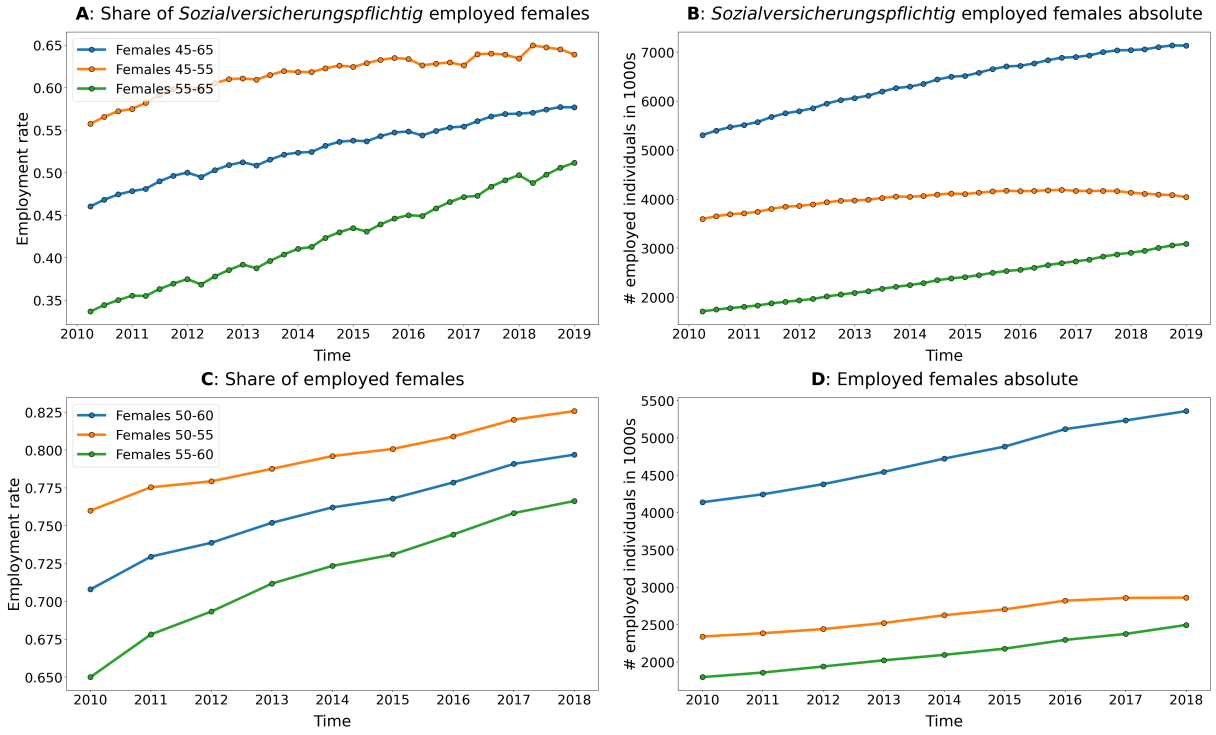


Figure 9. General employment trend

Notes: This figure presents the development of employment rates of females between the age of 45 and 65 in Germany from 2010 to 2018. Panel **A** illustrates the quarterly share of *sozialversicherungspflichtig* employed females who are between 45 and 55 (orange), 55 and 65 (green), and 45 and 65 (blue) years old from the first quarter of 2010 to the last quarter of 2018. **B** shows the corresponding absolute numbers of all groups for the same time period, respectively. Panel **C** depict the yearly share of minor and *sozialversicherungspflichtig* employed females between the age of 50 and 55, 55 and 60, as well as 50 and 60. Panel **D** presents the corresponding absolute numbers.
Source: Statistik d. sozialversicherungspfl. Beschäftigten, Tabelle 13111-0001 Sozialversicherungspflichtig Beschäftigte am Arbeitsort: Deutschland, Stichtag, Geschlecht, Altersgruppen; own calculations

B Robustness analyses with balanced panel (cohorts 1960-64)

In Appendix B, we present employment effects of the pension reform based the main specification (Equation 1) for a restricted sample of women born in cohorts 1960-1964. In this sample, the age and cohort composition for the treatment group and control group are more comparable. Moreover, by construction in this sample, we use a balanced panel in which we observe all women over the full time period. We first present descriptive statistics and the common trend graph. Then, we turn to the main estimation table, present results of the event study and robustness checks with different cut-off periods. Overall, results based on the restricted sample are very similar to the results in the main specification, which includes all cohorts.

Table 8. Descriptive statistics - cohort restriction

Variable		Control Group	Treatment group
Birth Year	Mean	1962.41	1962.04
	Std	(1.35)	(1.38)
No. of children	Mean	1.83	2.38
	Std	(0.78)	(0.96)
No. of children bf 92	Mean	0.00	1.62
	Std	(0.00)	(0.79)
Pension points June 2014	Mean	27.80	25.29
	Std	(12.02)	(11.23)
Pension Wealth June 2014 in €	Mean	781.15	710.76
	Std	(337.76)	(315.42)
Prior years employment in June 2014	Mean	16.25	15.25
	Std	(9.20)	(9.30)
No. Individuals		3,316	11,474

Notes: The table shows the means and standard deviations of key variables for the 14,790 individuals in the treatment and the control group. We assign all women who gave birth to a child between January 1, 1990, and December 31, 1991, to the treatment group. Women with children born between January 1, 1992, and December 31, 1993, are assigned to the control group. Pension wealth is expressed in Euros per month.

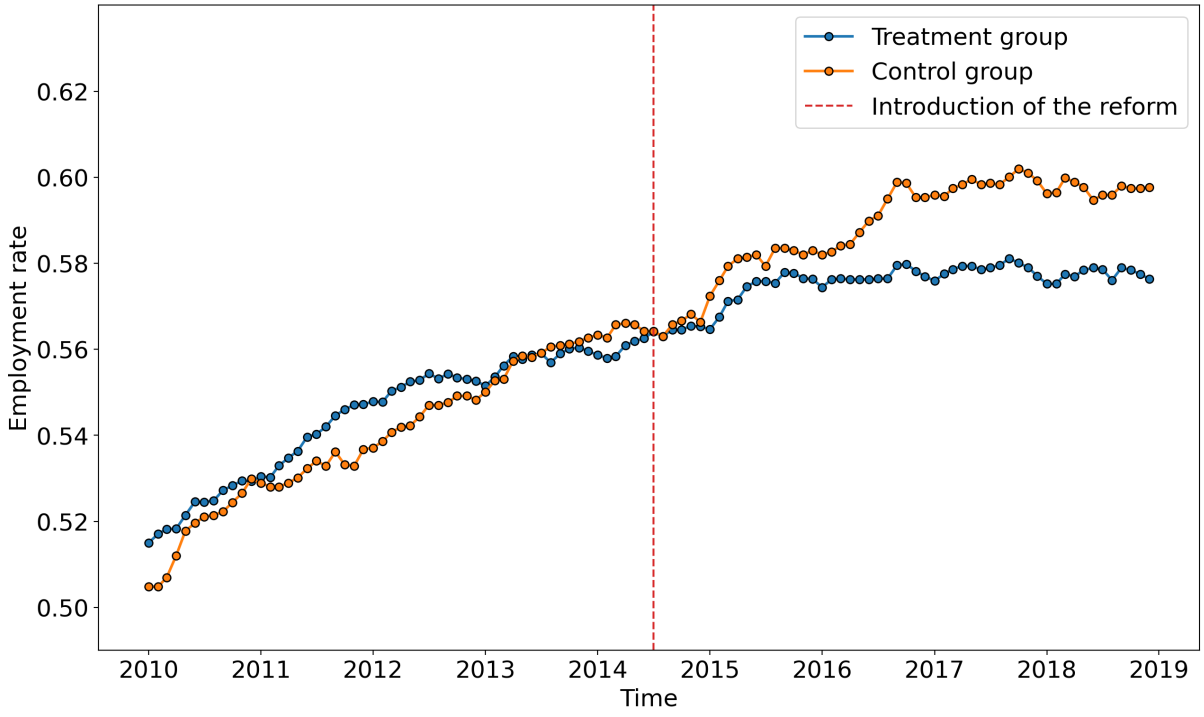


Figure 10. General employment trend - cohort restriction

Notes: This figure illustrates the employment rates of the balanced sample between January 2010 and December 2018. The blue line indicates the treatment group while the orange line denotes the employment rate in the control group. The dashed vertical line represents the introduction of the reform in July 2014.

Table 9. Results Baseline - cohort restriction

	Dependent Variable: Employment		
	(1)	(2)	(3)
Post × Mother pension	−0.0174***		
	(0.0048)		
Post × no. of children before 92		−0.0088***	
		(0.0021)	
Post× 1 child before 92			−0.0119**
			(0.0052)
Post × 2 children before 92			−0.0236***
			(0.0057)
Post × 3 or more children before 92			−0.0235***
			(0.0083)
Individuals		14,790	
Treated Individuals		11,474	
Untreated Individuals		3,316	
Pre avg emp rate treated		.564	
Time & Individual Fixed Effects		✓	

Notes: This table displays the effect of the reform in 2014 on employment between January 2010 and December 2018. The dataset is limited to women who are between 50 and 55 years old in 2014 and have given birth to a child between January 1990 and December 1993. Individuals are assigned to the treatment group if they had at least one child before January 1st, 1992. The control group consists of individuals whose children were all born after this cutoff date. All regressions include time and individual fixed effects. Standard errors are reported in parentheses and are clustered on the individual level. *** p<0.01, ** p<0.05, * p<0.1

Table 10. Results for different cutoff lengths - cohort restriction

Part A: Dummy specification for different lengths of the cutoff period

	1986-1997	1987-1996	1988-1995	1989-1994	1990-1993	1991-1992
Post × Mother pension	−0.0308***	−0.0297***	−0.0278***	−0.0211***	−0.0174***	−0.0152**
	(0.0033)	(0.0035)	(0.0037)	(0.0041)	(0.0048)	(0.0062)

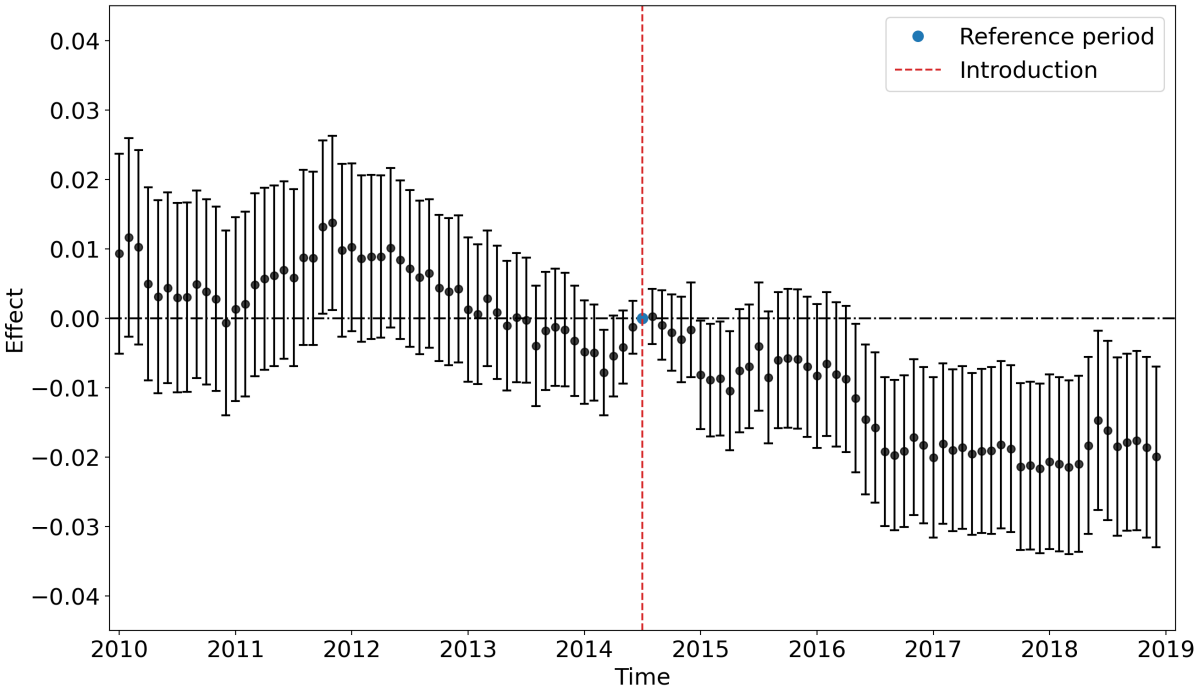
Part B: Linear specification for different lengths of the cutoff period

	1986-1997	1987-1996	1988-1995	1989-1994	1990-1993	1991-1992
Post × no. of children bf 92	−0.0122***	−0.0115***	−0.0110***	−0.0093***	−0.0088***	−0.0077***
	(0.0015)	(0.0016)	(0.0017)	(0.0018)	(0.0021)	(0.0027)

Part C: Nonlinear specification for different lengths of the cut off period

	1986-1997	1987-1996	1988-1995	1989-1994	1990-1993	1991-1992
Post × 1 child bf 92	−0.0261***	−0.0255***	−0.0228***	−0.0163***	−0.0119**	−0.0094
	(0.0037)	(0.0039)	(0.0042)	(0.0045)	(0.0052)	(0.0067)
Post × 2 children bf 92	−0.0361***	−0.0346***	−0.0338***	−0.0260***	−0.0236***	−0.0236***
	(0.0039)	(0.0041)	(0.0044)	(0.0048)	(0.0057)	(0.0075)
Post × 3 or more children bf 92	−0.0317***	−0.0305***	−0.0285***	−0.0258***	−0.0235***	−0.0186*
	(0.0061)	(0.0063)	(0.0066)	(0.0072)	(0.0083)	(0.0107)
Observations	2,896,560	2,679,048	2,405,916	2,060,424	1,597,320	912,276
Individuals	26,820	24,806	22,277	19,078	14,790	8,447
Treated Individuals	19,437	18,193	16,599	14,417	11,474	6,628
Untreated Individuals	7,383	6,613	5,678	4,661	3,316	1,819
Pre avg emp rate treated	0.5691	0.5681	0.5684	0.5672	0.5641	0.5578
Time Dummies	YES	YES	YES	YES	YES	YES
Individual Fixed Effects	YES	YES	YES	YES	YES	YES

Notes: This table displays the effect of the reform in 2014 on employment between January 2010 and December 2018 for varying lengths of the cutoff period. The dataset is limited to women who are between 50 and 55 years old in 2014 and have given birth to a child in the respective time period. Individuals are assigned to the treatment group if they had at least one child before January 1st, 1992. The control group consists of individuals whose children were all born after this cutoff date. All regressions include time and individual fixed effects. Standard errors are reported in parentheses and are clustered on the individual level.
*** p<0.01, ** p<0.05, * p<0.1



Note: This figure displays the estimates of a regression of the employment status on a set of interaction terms of the treatment group identifier and monthly time dummies from January 2010 to December 2018. Time and individual fixed effects are included in the regressions. The black dots denote the point estimates and the vertical black lines represent the associated 95% confidence intervals. The vertical dashed red line indicates the introduction of the reform in July 2014.

Figure 11. Event study results - cohort restriction

C Regressions with age effects

Table 11. Results age regressions - employment

	Employment		
	(1)	(2)	(3)
Post × Mother pension	0.3556***	−1.9588***	0.0066
	(0.0372)	(0.3095)	(0.0050)
Post × Mother pension × Age	−0.0065***	0.0753***	
	(0.0007)	(0.0112)	
Post × Mother pension × Age^2		−0.0007***	
		(0.0001)	
Post × Mother pension × 51			−0.0038
			(0.0029)
Post × Mother pension × 52			−0.0065*
			(0.0029)
Post × Mother pension × 53			−0.0091**
			(0.0044)
Post × Mother pension × 54			−0.0113**
,			(0.0045)
Post × Mother pension × 55			−0.0111**
			(0.0047)
Post × Mother pension × 56			−0.0123**
			(0.0051)
Post × Mother pension × 57			−0.0171***
			(0.0055)
Post × Mother pension × 58			−0.0131*
			(0.0067)
Post × Mother pension × 59			−0.0323***
			(0.0115)
Observations		2,007,624	
Individuals		30,474	
Treated Individuals		22,728	
Untreated Individuals		7692	
Time & Individual Fixed Effects		✓	

Notes: The table reports estimates of the effect of the pension wealth shock induced by the 2014 reform on employment for the period January 2010 to December 2018 depending on age. The dataset is limited to women who are between 50 and 59 years old in 2014 and have given birth to a child between 1990 and 1993. The treatment group consists of women who have given birth to at least one child before January 1st, 1992. Individuals are assigned to the control group if all of their children were born after this cutoff date. All regressions include individual and time-fixed effects. Standard errors are reported in parentheses and are clustered on the individual level.
*** p<0.01, ** p<0.05, * p<0.1

Table 12. Results age IV regressions

Dependent Variable	Employment					
	Reform Dummy		No. children bf 92		No. children dummies	
	(1)	(2)	(3)	(4)	(5)	(6)
Log Pension Wealth in €	8.0092***	0.8408	7.2121***	0.7638	4.2969***	0.5085
	(1.5279)	(0.6084)	(1.3723)	(0.6708)	(0.9307)	(0.6120)
Log Pension Wealth in € × Age	−0.1488***	−0.0099	−0.1379***	−0.0091	−0.0833***	−0.0039
	(0.0276)	(0.0112)	(0.0261)	(0.0130)	(0.0175)	(0.0117)
Log Pension Wealth in € × Age^2		−0.0001***		−0.0001***		−0.0001***
		(0.0000)		(0.0000)		(0.0000)
Observations	2,091,146					
Individuals	30,422					
Time and Individual Fixed Effects	✓					

Notes: The Table presents the results of an IV regression of employment between 2010 and 2018 on log pension wealth in €, log pension wealth in € interacted with age and age squared respectively. The specification presented in columns 1 and 2 uses the post × treatment indicator variable and its interactions with age (column 1) as well as age squared as instruments (column 2). Similarly, in columns 3 and 4, the first stage uses the total number of children that an individual had before 1992 as well as the interaction with age (column 3) and age squared (column 4). Columns 5 and 6 use a set of three dummies that take value 1 if the individual had one child, two children, or three or more children before 1992 and interacts with those with age (column 5) and age and age squared (column 6). The respective dataset only includes women who are between 50 and 59 years old in 2014 and had at least one child between 1990 and 1993. The treatment group consists of women who have given birth to at least one child before January 1st, 1992. Individuals are assigned to the control group if all of their children were born after this cutoff date. All regressions include individual and time-fixed effects. Standard errors are reported in parentheses and are clustered on the individual level.
*** p<0.01, ** p<0.05, * p<0.1

Table 13. First stage results age IV regressions - Reform dummy specification

	(1)		(2)		
	Log PW	Log PW × Age	Log PW	Log PW × Age	Log PW × Age^2
Post × Mother pension	−0.1655***	−11.2944***	−0.0979	−18.5574***	15969.5100***
	(0.0195)	(1.0046)	(0.0876)	(5.4860)	(411.1070)
Post × Mother pension × Age	0.0044***	0.2817***	0.0020	0.5382***	−611.6707***
	(0.0004)	(0.0183)	(0.0033)	(0.2029)	(15.1259)
Post × Mother pension × Age^2			0.0000	−0.0023	5.8966***
			(0.0000)	(0.0019)	(0.1387)
F-value	18.87		101.45		
Observations	2,091,041				
Individuals	30,317				
Time and Individual Fixed effects	✓				

Notes: This table shows the first stage regression outcomes for the reform dummy specifications of the age IV regressions previously presented in column 1 and 2 of Table 12. Standard errors are reported in parentheses and are clustered on the individual level. F values correspond to the Kleibergen-Paap Wald rk F statistic.
*** p<0.01, ** p<0.05, * p<0.1

Table 14. First stage results age IV regressions - Number of children linear

	(3)		(4)		
	Log PW	Log PW × Age	Log PW	Log PW × Age	Log PW × Age^2
Post × no. of children bf 92	0.0113	−0.3961	0.0004	−6.4097**	6620.8340***
	(0.0083)	(0.4241)	(0.0446)	(2.7402)	(226.1882)
Post × no. of children bf 92 × Age	0.0005***	0.0448***	0.0009	0.2559***	−248.1214***
	(0.0001)	(0.0076)	(0.0016)	(0.0996)	(8.2394)
Post × no. of children bf 92 × Age^2			0.0000	−0.0018**	2.3479***
			(0.0000)	(0.0009)	(0.0749)
F-value	22.14			79.15	
Observations			2,091,041		
Individuals			30,317		
Time and Individual Fixed effects			✓		

Notes: This table shows the first stage regression outcomes for the linear numbers of children born before 1992 specifications of the age IV regressions previously presented in columns 3 and 4 of Table 12. Standard errors are reported in parentheses and are clustered on the individual level. F values correspond to the Kleibergen-Paap Wald RK F statistic.
*** p<0.01, ** p<0.05, * p<0.1

Table 15. First stage results age IV regressions - Number of children linear

	(5)		(6)		
	Log PW	Log PW × Age	Log PW	Log PW × Age	Log PW × Age^2
Post× 1 child bf 92	−0.0895***	−7.7443***	−0.2454	−26.5356***	16 103.9900***
	(0.0226)	(1.2191)	(0.1565)	(9.7630)	(740.5007)
Post× 1 child bf 92 × Age	0.0026***	0.1919***	0.0081	0.8627**	−615.0287***
	(0.0004)	(0.0224)	(0.0059)	(0.3638)	(27.4984)
Post× 1 child bf 92 × Age^2			−0.0000	−0.0060*	5.8893***
			(0.0001)	(0.0034)	(0.2548)
Post× 2 children bf 92	−0.0084	−3.1199***	−0.1868	−23.2750**	15 573.3500***
	(0.0231)	(1.1745)	(0.1580)	(9.2382)	(671.6001)
Post× 2 children bf 92 × Age	0.0018***	0.1435***	0.0081	0.8558**	−589.7920***
	(0.0004)	(0.0214)	(0.0058)	(0.3373)	(24.4917)
Post× 2 children bf 92 × Age^2			−0.0001	−0.0063**	5.6401***
			(0.0001)	(0.0031)	(0.2229)
Post× 3 or more children bf 92	0.0073	−2.1703	0.8143***	31.6401**	17 719.5500***
	(0.0332)	(1.7253)	(0.2319)	(13.6765)	(957.2720)
Post× 3 or more children bf 92 × Age	0.0024***	0.1718***	−0.0260***	−1.0212**	−661.5662***
	(0.0006)	(0.0311)	(0.0085)	(0.4957)	(34.5808)
Post× 3 or more children bf 92 × Age^2			0.0002	0.0105**	6.2776***
			(0.0001)	(0.0045)	(0.3119)
F-value	9.53			37.44	
Observations			2,091,041		
Individuals			30,317		
Time and Individual Fixed effects			✓		

Notes: This table shows the first stage regression outcomes for the linear numbers of children born before 1992 specifications of the age IV regressions previously presented in columns 3 and 4 of Table 12. Standard errors are reported in parentheses and are clustered on the individual level. The F values correspond to the Kleibergen-Paap Wald RK F statistic.
*** p<0.01, ** p<0.05, * p<0.1

D Results retirement and marginal employment

In Appendix D, we present the effects of the pension reform based the main specification (Equation 1) on marginal employment and retirement. Note that individuals in our sample can only enter retirement via claiming disability pensions. For the analysis, we use the same sample as for the main specification presented in section 5. The baseline results are presented in Table 6.

Table 16. Results for different cutoff lengths - retirement

Part A: Dummy specification for different lengths of the cutoff period

	1986-1997	1987-1996	1988-1995	1989-1994	1990-1993	1991-1992
Post × Mother pension	0.0113***	0.0105***	0.0085***	0.0067***	0.0044**	0.0032
	(0.0015)	(0.0016)	(0.0017)	(0.0018)	(0.0022)	(0.0031)

Part B: Linear specification for different lengths of the cutoff period

	1986-1997	1987-1996	1988-1995	1989-1994	1990-1993	1991-1992
Post × no. of children before 92	0.0060***	0.0056***	0.0052***	0.0051***	0.0050***	0.0056***
	(0.0007)	(0.0007)	(0.0008)	(0.0009)	(0.0010)	(0.0015)

Part C: Nonlinear specification for different lengths of the cut off period

	1986-1997	1987-1996	1988-1995	1989-1994	1990-1993	1991-1992
Post × 1 child before 92	0.0069***	0.0067***	0.0054***	0.0032	0.0006	-0.0012
	(0.0017)	(0.0018)	(0.0019)	(0.0021)	(0.0024)	(0.0033)
Post × 2 children before 92	0.0123***	0.0112***	0.0084***	0.0065***	0.0045*	0.0033
	(0.0017)	(0.0018)	(0.0020)	(0.0021)	(0.0025)	(0.0035)
Post × 3 or more children before 92	0.0179***	0.0165***	0.0149***	0.0147***	0.0131***	0.0145***
	(0.0023)	(0.0024)	(0.0026)	(0.0029)	(0.0034)	(0.0049)
Observations	4,265,016	3,832,166	3,348,183	2,777,947	2,097,523	1,168,539
Individuals	60,734	54,785	48,077	40,098	30,474	17,149
Treated Individuals	41,305	37,854	33,918	28,960	22,782	13,153
Untreated Individuals	19,429	16,931	14,159	11,138	7,692	3,996
Time & Individual Fixed Effects	✓	✓	✓	✓	✓	✓

Notes: *Note:* This table displays the effect of the reform in 2014 on retirement between January 2010 and December 2018 for varying lengths of the cutoff period. The dataset is limited to women who are between 50 and 63 years old and have given birth to a child in the respective time period. Individuals are assigned to the treatment group if they had at least one child before January 1st, 1992. The control group consists of individuals whose children were all born after this cutoff date. All regressions include time and individual fixed effects. Standard errors are reported in parentheses and are clustered on the individual level. Relative effects are presented in square brackets.
*** p<0.01, ** p<0.05, * p<0.1

Table 17. Results for different cutoff lengths - marginal employment

Part A: Dummy specification for different lengths of the cutoff period

	1986-1997	1987-1996	1988-1995	1989-1994	1990-1993	1991-1992
Post × Mother pension	−0.0072**	−0.0074**	−0.0067*	−0.0069*	−0.0080*	−0.0052
	(0.0033)	(0.0035)	(0.0037)	(0.0041)	(0.0049)	(0.0065)

Part B: Linear specification for different lengths of the cutoff period

	1986-1997	1987-1996	1988-1995	1989-1994	1990-1993	1991-1992
Post × no. of children before 92	−0.0039***	−0.0039***	−0.0030**	−0.0031**	−0.0042**	−0.0028
	(0.0013)	(0.0013)	(0.0014)	(0.0016)	(0.0019)	(0.0024)

Part C: Nonlinear specification for different lengths of the cut off period

	1986-1997	1987-1996	1988-1995	1989-1994	1990-1993	1991-1992
Post × 1 child before 92	−0.0030	−0.0032	−0.0032	−0.0033	−0.0030	−0.0009
	(0.0037)	(0.0039)	(0.0042)	(0.0046)	(0.0053)	(0.0071)
Post × 2 children before 92	−0.0088**	−0.0094**	−0.0094**	−0.0095**	−0.0124**	−0.0107
	(0.0037)	(0.0039)	(0.0042)	(0.0047)	(0.0055)	(0.0076)
Post × 3 or more children before 92	−0.0112**	−0.0111**	−0.0077	−0.0088	−0.0104	−0.0053
	(0.0046)	(0.0048)	(0.0052)	(0.0058)	(0.0068)	(0.0093)
Observations	4,265,016	3,832,166	3,348,183	2,777,947	2,097,523	1,168,539
Individuals	60,734	54,785	48,077	40,098	30,474	17,149
Treated Individuals	41,305	37,854	33,918	28,960	22,782	13,153
Untreated Individuals	19,429	16,931	14,159	11,138	7,692	3,996
Time & Individual Fixed Effects	✓	✓	✓	✓	✓	✓

Notes: Note: This table displays the effect of the reform in 2014 on minor employment between January 2010 and December 2018 for varying lengths of the cutoff period. The dataset is limited to women who are between 50 and 63 years old and have given birth to a child in the respective time period. Individuals are assigned to the treatment group if they had at least one child before January 1st, 1992. The control group consists of individuals whose children were all born after this cutoff date. All regressions include time and individual fixed effects. Standard errors are reported in parentheses and are clustered on the individual level. Relative effects are presented in square brackets.

*** p<0.01, ** p<0.05, * p<0.1

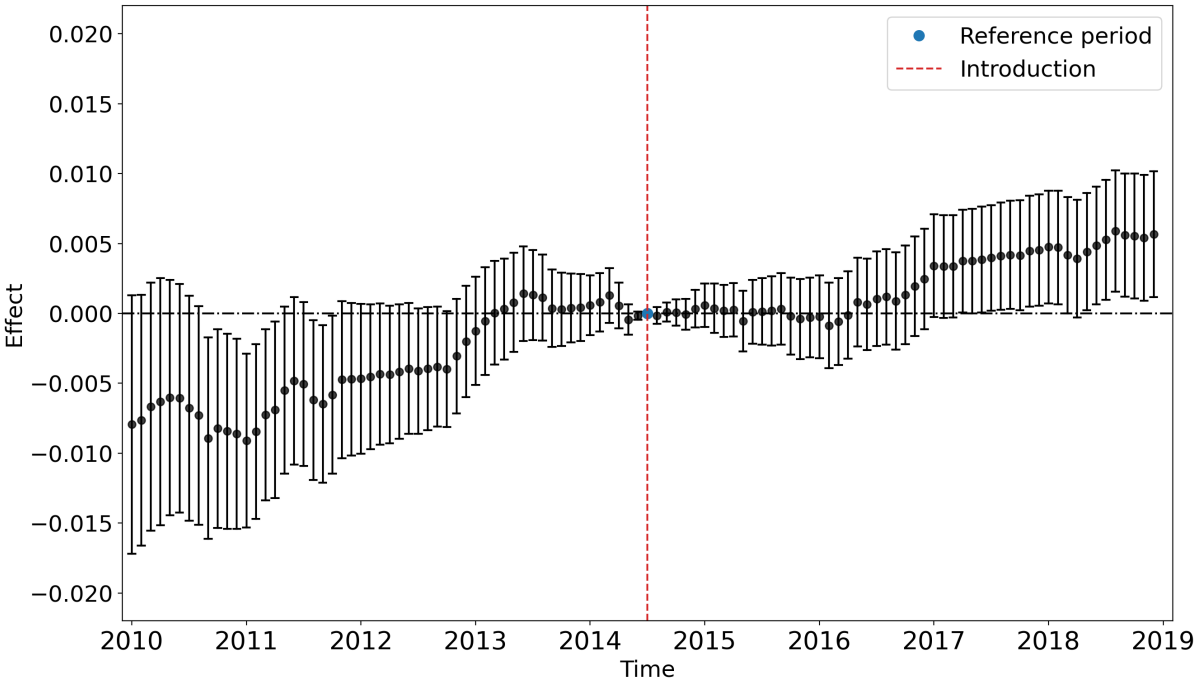


Figure 12. Event study - retirement

Notes: This figure displays the estimates of a regression of the retirement status on a set of interaction terms of the treatment group identifier and monthly time dummies from January 2010 to December 2018. Time and individual fixed effects are included in the regressions. The black dots denote the point estimates and the vertical black lines represent the associated 95% confidence intervals. The vertical dashed red line indicates the introduction of the reform in July 2014.

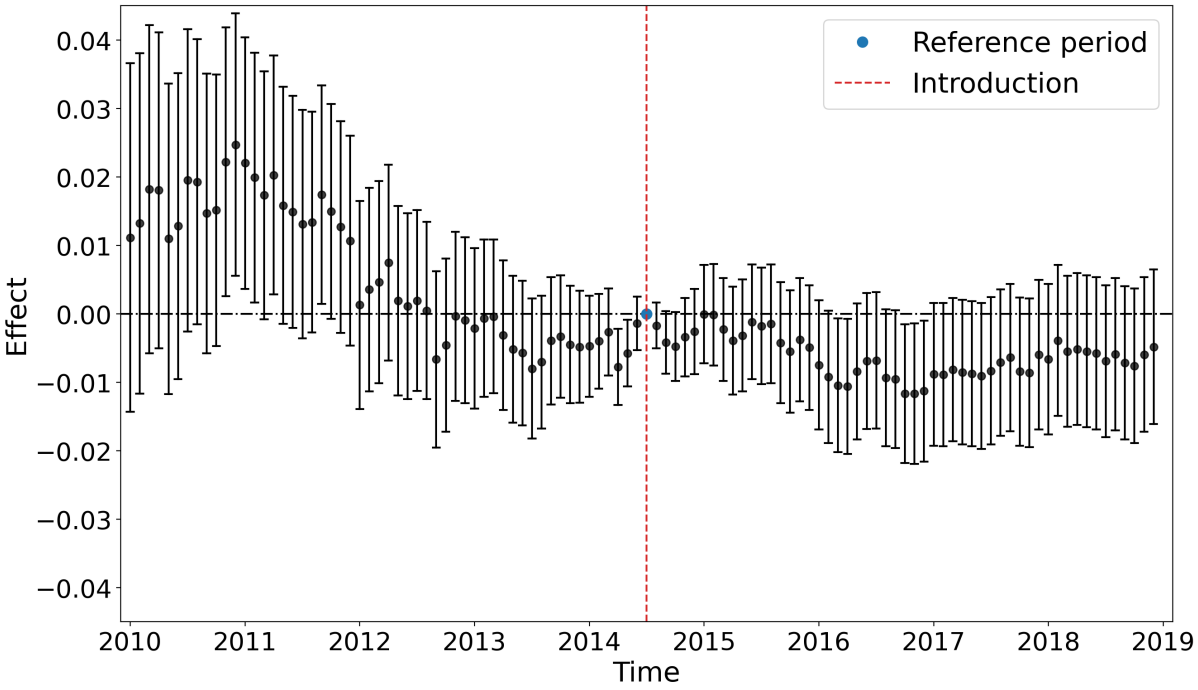


Figure 13. Event study - marginal employment

Notes: This figure displays the estimates of a regression of the marginal employment status on a set of interaction terms of the treatment group identifier and monthly time dummies from January 2010 to December 2018. Time and individual fixed effects are included in the regressions. The black dots denote the point estimates and the vertical black lines represent the associated 95% confidence intervals. The vertical dashed red line indicates the introduction of the reform in July 2014.

Table 18. Results age regressions - marginal employment & retirement

	Retirement			Marginal employment		
	(1)	(2)	(3)	(4)	(5)	(6)
Post × Mother pension	−0.1036***	0.4159**	−0.0045**	−0.0650	0.4096	−0.0048
	(0.0203)	(0.1736)	(0.0020)	(0.0413)	(0.3398)	(0.0057)
Post × Mother pension × Age	0.0019***	−0.0165***		0.0010	−0.0158	
	(0.0004)	(0.0064)		(0.0007)	(0.0123)	
Post × Mother pension × Age^2		0.0013		0.0002		
		(0.0001)			(0.0001)	
Post × Mother pension × 51			0.0013			0.0026
			(0.0011)			(0.0032)
Post × Mother pension × 52			0.0031*			0.0029
			(0.0016)			(0.0045)
Post × Mother pension × 53			0.0058***			0.0035
			(0.0020)			(0.0050)
Post × Mother pension × 54			0.0042**			0.0077
			(0.0021)			(0.0052)
Post × Mother pension × 55			0.0037*			0.0058
			(0.0021)			(0.0054)
Post × Mother pension × 56			0.0056***			0.0049
			(0.0022)			(0.0058)
Post × Mother pension × 57			0.0058**			0.0046
			(0.0023)			(0.0063)
Post × Mother pension × 58			0.0051			0.0122
			(0.0031)			(0.0077)
Post × Mother pension × 59			0.0114**			0.0025
			(0.0048)			(0.0139)
Observations			2,007,624			
Individuals			30,474			
Treated Individuals			22,728			
Untreated Individuals			7692			
Time & Individual Fixed Effects			✓			

Notes: Note: The table reports estimates of the effect of the pension wealth shock induced by the 2014 reform on retirement and marginal employment for the time period January 2010 to December 2018 depending on age. The dataset is limited to women who are between 50 and 63 years old and had a child between 1990 and 1993. The treatment group consists of women who have given birth to at least one child before January 1st, 1992. Individuals are assigned to the control group if all of their children were born after this cutoff date. All regressions include individual and time-fixed effects. Standard errors are reported in parentheses and are clustered on the individual level.
*** p<0.01, ** p<0.05, * p<0.1