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Employment Responses to Income Effect - Evidence from Pension Reform^{*}

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Abstract

For the design of the pension system, it is crucial to disentangle the employment responses related to the substitution effect and the income effect. In this paper, we provide causal evidence regarding the importance of the income effect, which is generally assumed to be small or non-existent. We exploit a pension reform in Germany that raised pension benefits related to children. For the identification, we exploit the discontinuity induced by the reform: only mothers with children born before 1.1.1992 were affected by the pension reform. Children born after this cut-off date did not change pension income. We use a difference-in-differences estimator based on administrative data from the German pension insurance that includes complete individual employment histories. We find that income effects are significant and economically important. We show that the policy led to a reduction in the employment of affected females. Further, we are able to show effect heterogeneity on different dimensions: by treatment intensity, age of the mother, and pre-reform pension wealth.

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1 Introduction

The substitution effect and the income effect are central parameters when designing public policies.¹ For the design of income taxation or the pension system, it is crucial to separate and quantify the role of the two effects since it allows for understanding potential distortions and to compare the employment effects of different reforms (see, e.g. [Boskin \(1977\)](#)). For example, a universal basic pension that is unconditional of the working history mechanically induces no substitution effect² and would only lead to employment responses when an income effect is present.³ In general it is assumed that income effects play no or only a minor role relative to the substitution effect (see e.g. [Blundell and Macurdy \(1999\)](#)). This assumption is used in theoretical welfare analyses ([Chetty, 2006](#)) and it is the basis for influential calculations of optimal taxation, see e.g. [Saez \(2002\)](#). However, empirical support for this key assumption is scarce and inconclusive, which is mainly related to identification challenges as most policy reforms involve simultaneous changes in income and work incentives, see e.g. [Giupponi \(2019\)](#).

In this paper, we exploit unique variation of expected pension income that does not affect marginal working incentives. Therefore, we provide direct evidence about the income effect and can quantify its impact on employment. We use the variation of a pension reform in Germany that raised pension benefits related to children. In 2014, the so called “mothers pension” increased the pension benefits granted for each child born before 1992. Importantly, the sizable increase in permanent pension income is determined by the number of children born before 1992 and cannot be changed through behavioral adjustments. For the identification, we use the discontinuity induced by the reform: only mothers with children born before January 1, 1992, were affected by the pension reform. For mothers with children born after this cut-off date, pension income did not change. We exploit this discontinuity in a difference-in-differences estimator and compare employment outcomes of mothers aged 50 and older who gave birth before and after 1.1.1992, before and after the introduction of the pension reform in 2014.

The empirical analysis is based on rich administrative data from the German pension insurance, which includes complete individual employment histories. Thus, we estimate the causal effect of the pension reform conditional on individual specific fixed effects and time effects. We focus on several margins of heterogeneity that are important for understanding the mechanism of the income effect. First, we separate the effects by number of eligible

1. The substitution effect measures how individuals adjust employment when prices (net wages) change while income remains constant, i.e. it refers to changes in employment due to changes in the marginal incentives to work. The income effect captures employment changes induced by changes in income that are independent of working incentives.

2. The financing of the universal basic pension for example via a progressive income tax could induce substitution effects.

3. The quantification of the income effect is also important as it directly affects the marginal utility of consumption ([Chetty, 2008](#)). For a more detailed discussion, see [Giupponi \(2019\)](#). Moreover the size of the income effect, specifically the relation between the income effect and substitution effect is crucial for interpreting implications of welfare analysis based on sufficient statistics, see [Chetty \(2006\)](#).

children (children born before 1992), which induces variation in the size of income effect. Second, we study the heterogeneity by the age of the mother, which allows for analyzing when employment effects are most important. Finally, we analyze the role of the income effect by pre-reform pension wealth, which allows us to test for the role of credit constraints.

We find that income effects are significant and economically important. Our estimates show that the permanent income effect, which increases pensions on average by 7.4%, reduces the employment rate of affected women by about two percentage points. We find a clear pattern by treatment intensity. The higher the income effect, i.e., the more eligible children that a mother gave birth to, the greater the reduction in employment. Moreover, the effect size increases with maternal age. Since the income effect only materializes when women enter retirement, age effects reflect the different horizons of women. Moreover, the effect is consistent with lower labor attachment of women close to retirement. Finally, we show that the negative employment effects are larger for women with low pre-reform pension wealth, which suggests that credit constraints play an important role. Our results are robust to various changes in the specification. They do not change when we alter the distance to the cut off date to define the treatment group and they are robust to changes in the observation period. Moreover, we show in placebo regressions that our identification assumptions hold.

A sizable body of literature exploits variation and discontinuities in pension design to estimate the causal effect on employment and retirement behavior; see e.g. [Geyer and Welteke \(2021\)](#); [Atalay and Barrett \(2015\)](#); [Staubli and Zweimüller \(2013\)](#); [Engels et al. \(2017\)](#); [Cribb et al. \(2016\)](#); [Manoli and Weber \(2016\)](#); [Seibold \(2020\)](#); [Morris \(2019a\)](#). In general, these studies provide credible 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 income effect and the substitution effect as the considered reforms involve simultaneous changes in income and work incentives. A few studies⁴ focus on the role of the income effect or wealth effect for the retirement decision.⁵ [Brown et al. \(2010\)](#) is a specific example in which the authors exploit variation in inheritances and document that a wealth shock reduces employment.⁶ [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

4. [Atalay and Barrett \(2015\)](#) and [Morris \(2019b\)](#) 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, and the results can be interpreted as income effects. [Morris \(2019b\)](#) provides a critical assessment of [Atalay and Barrett \(2015\)](#) showing that they probably overestimate employment effects.

5. See as well the literature review in [Giupponi \(2019\)](#).

6. 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.

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 \(2013\)](#) and by [Giupponi \(2019\)](#). [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 more common 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. Survivors fully offset the benefit loss with increases in earnings. In contrast to this study, which focuses on the employment behavior of surviving spouses, we can estimate the income effect for a very general group, namely mothers.

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

2 Institutional Background

Public pensions are by far the largest source of income individuals have during retirement in Germany. Public pensions 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 earnings (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 \(2004\)](#). Entitlements that are not linked to employment, are mainly related to children. One parent (usually the mother) receives earnings points for having (raised) children. The overall number of child related pension points depends on the number of children and the birth year of the child. In this paper, we exploit the variation related to the birth year of children as induced by the 2014 pension reform. On average, female pensioners who had children during their working life had about 22 earnings points in 2013.⁷

2.1 Pension reform 2014

Historically, child related pension points differ by the birth date of the child: a pension reform in 1992 increased credited periods for children born from 1.1.1992 onwards to three years, whereas for children born before 1.1.1992 the credited period of one year remained unchanged.⁸ Subsequently, the number of earnings points granted for the credited period was raised from 0.75 to 1.0 for all children, independent of birth year, in three steps between 1998 and 2000. Therefore, since July 1, 2000, three earnings points have been

7. SUFRITBN13XVSB – Source: www.fdz-rv.de.

8. See [Thiemann \(2015\)](#) for a discussion and labor supply analysis of this pension reform.

granted for each child born from 1.1.1992 onwards and one earnings point for all children born before that date.⁹

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 1.1.1992. The final details of the reform were published on June 24, 2014, and formally enacted on July 1, 2014. With the pension reform, the credited period for children born before 1.1.1992 was increased from one to two years.¹⁰ For the majority of mothers, this implies that pension income increases by one pension point for each child born before 1.1.1992. The increase was lower for mothers with relatively high labor earnings in the first two years after giving birth since the overall number of earnings points per year is capped.¹¹ To better understand the financial implications of the 2014 pension reform, we provide first evidence by comparing the number of pension points and the pension amounts in the year before and after the pension reform as well as how the increase in pensions varies by number of children. In Table 1 we compare pension entitlements and the pension amount of women who entered retirement in 2013 and 2014.¹² Between these two years, the average number of pension points across all women increased by 8%. The striking difference in the increase by number of children shows the sizable impact of the pension reform. For women without children, who are not affected by the pension reform, there is no change in the average number of pension points. Yet, pension points increase by 4% for women with one child, 9% for those with two children, 12% for those with three children, and 24% for women with four or more children. This increase in pension points directly translates into changes in pension amounts. This increase has the same structure but is slightly larger since, between these two years, not only did the child related pension change but there was also the regular increase in the nominal pensions. Therefore, the overall pension amounts increased by about 2%, which is also true for women without children.

Table 1 refers only to women who entered retirement in the year before or after the pension reform. To estimate the employment effect of this pension reform and to quantify

9. This general rule has to be qualified. Total earnings 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. Up to mid-1998, earnings points for children were withdrawn on a one-to-one basis for earnings points from employment. After a ruling by the constitutional court, the current rules were introduced in the pension reform act 1999 (article 70) (published 22.12.1997). Those already retired were granted a supplement of one earnings point per child (reduced to 0.75 (0.85, 0.9) points for pensions paid in 1998 (1999, 2000)) and, thus, “additivity” of earnings points from employment and for child raising was also achieved for that group (article 307d of Pension Reform Act 1999). The pensions paid before mid-1998 remained unchanged.

10. The legislative process is documented on <http://www.portal-sozialpolitik.de/index.php?page=rv-leistungsverbesserungsgesetz>.

11. See previous 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. \(2015\)](#).

12. This table includes only mothers with children born before 1.1.1992. Mothers with children born after this date were too young to enter retirement during these years.

Table 1. Change in average pension points by number of children between 2013 and 2014

No. of children	Pension points			Pension amount		
	2013	2014	Δ	2013	2014	Δ
0	25.8	25.8	0.00	655	668	0.02
1	24.6	25.6	0.04	611	647	0.06
2	21.9	23.8	0.09	541	603	0.11
3	19.7	22.2	0.12	488	563	0.15
4+	18.7	23.1	0.24	463	590	0.27
Total	22.5	24.3	0.08	561	617	0.10

This table shows the average number of accumulated pension points and the resulting pension benefits for women who entered retirement in 2013 or 2014, respectively.

Source: SUFRTBN13XVSBB, SUFRTBN14XVSBB

the income effect, we focus on all women older than 50 who are not yet retired in the empirical analysis.

3 Data

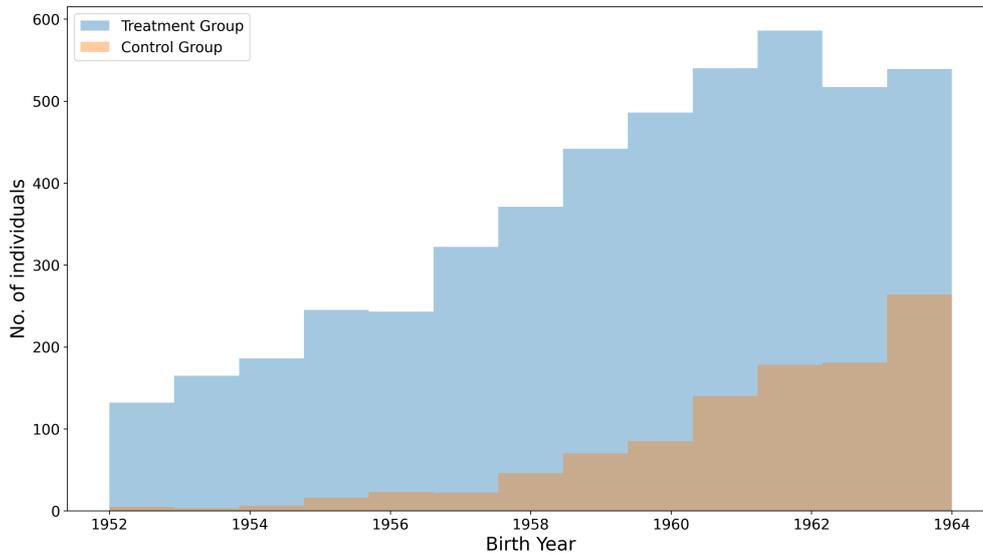
We use high-quality administrative data from German public pension insurance accounts (Versichertenkontenstichprobe, VSKT). The VSKT is a stratified random sample of all pension insurance accounts of people in Germany aged 30–67. We utilize a subset of organization-provided scientific use files. Each file contains a random selection of 25% of the full VSKT.¹³ Since the data are process-produced, recall errors due to memory gaps and wrong temporal assignments are avoided and panel mortality is negligible (Fachinger and Himmelreicher, 2006). Furthermore, both individual employment behavior as well as retirement entry are reported with monthly accuracy.

To increase the number of observations, we use different waves of the SUFVSKT. We identify and drop duplicates by using information unique to the individual, such as full employment history, the total amount of monthly pension points and a broad variety of other characteristics.¹⁴

Relevant for our analysis are the data for 2015-2017. We restrict the sample to mothers who gave birth to at least one child between 1986 and 1997, who are older than 50 at the time of reform implementation, and who had not entered retirement before January 2013. We exclude individuals born prior to 1952 since they had the option to retire at 60 instead of 63 (Geyer and Welteke, 2021). Furthermore, we only consider individuals

13. Using appropriate weights, the scientific use file of the VSKT (SUFVSKT) is representative of the German population of public pension insurance accounts.

14. Based on this information we are able to identify 2,888 (23.02%) duplicates in our remaining sample. We only keep information from the most recent dataset for these individuals and drop the remaining 1,483 observations from earlier waves.



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

Figure 1. Age distribution

with verified accounts and exclude all women who paid contributions to a special miners' pension scheme (Knappschaftliche Versicherung) for at least one month, which applies to about 9.27% of all women in the sample. Individuals who lived in East Germany before the reunification of the country in 1990 (about 23% of the remaining observations) are also excluded from the sample, as fertility rates in the East declined drastically in the 1990s (Chevalier and Marie, 2017).

One last important aspect in the construction of the final dataset is that information within the VSKT does not allow us to distinguish between civil servants and individuals who left the work force but did not notify the German pension insurance agency. While civil servants are usually excluded from the German pension system, they could either have earned pension entitlements through previous employment in the private sector or through child related pension entitlements. Consequently, we would expect monthly employment status information to be missing starting from the point in time when they joined the public sector. The same applies to individuals who left the work force. To account for this, we make use of the existing age limits for the appointment of civil servants by excluding all individuals with no data on their monthly status starting from age 49.¹⁵ Applying all aforementioned restrictions leaves us with a total of 5,813 observations.

The identification strategy is explained in detail in Section 4. In short, it exploits variation in employment outcomes of the treatment group and the control group before and

15. The specific age limits differ by federal state as well as type of position and range from 42 to 50.

Table 2. Descriptive statistics

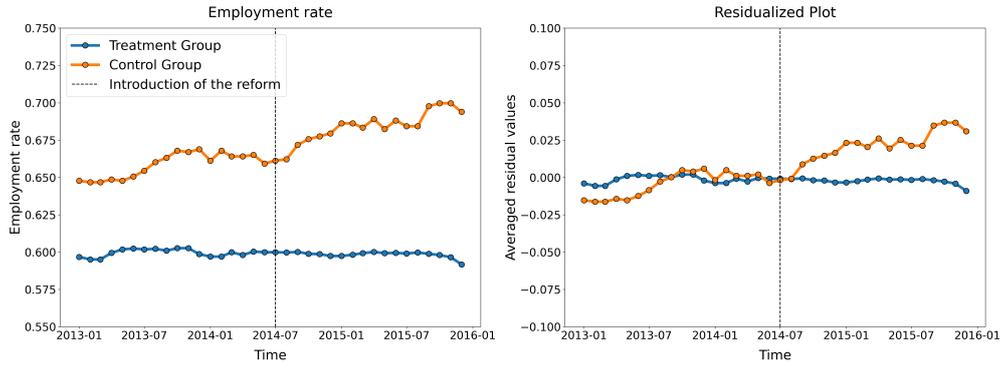
Variable		Control group	Treatment group
Birth Year	Mean	1961.5380	1959.5280
	Std	(2.4340)	(3.3370)
No. of children	Mean	1.6920	2.3700
	Std	(0.7480)	(1.0840)
Prior years employment	Mean	17.8890	16.8170
	Std	(9.0700)	(9.0700)
Pre avg. emp. rate	Mean	0.6580	0.6000
	Std	(0.4500)	(0.4690)
Post avg. emp. rate	Mean	0.6840	0.5980
	Std	(0.4410)	(0.4680)
No. Individuals		1039	4774

Note: The table shows means and standard deviations of key variables for the 5813 individuals included in the main estimation sample.

after the introduction of the pension reform in 2014. In the main specification, we assign all women who gave birth to a child between January 1, 1986, and December 31, 1991, to the treatment group. Women with children born between January 1, 1992, and December 31, 1997, 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 group (see Figure 1).

In total we assign 1,039 individuals to the control group and 4,774 individuals to the treatment group. The difference in sample size between the two groups is predominantly driven by the fact that individuals are only part of the control group if they gave birth to their first child after January 1, 1992. Individuals in the treatment group are allowed to have children on either side of the cutoff. Importantly, for these women only children born before the cutoff date define the treatment intensity.

In Table 2 we provide descriptive statistics for the two groups. Individuals in the treatment group are approximately 2 years older than individuals in the control group. In addition, we observe large compositional differences regarding the total number of children. While mothers in the control group have an average of about 1.7 children, the average for the treatment group is higher, at more than 2.3. Accordingly, mothers in the control group were, on average, employed for 17.9 years before the introduction of the reform in 2014 compared to the 16.8 years worked by the treatment group. Finally, the average rate of employment in the 18 month period preceding the reform (pre period) is 5.8 percentage points higher in the control group than in the treatment group. 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. Further, we show that our results are robust to changes in the definition of the treatment and control group. Specifically, in the



Note: This figure illustrates the employment status of mothers within our sample over time. The graph on the right shows the employment rate for the treatment (blue) and control (orange) groups between January 2013 and December 2015. The graph on the left-hand side presents the average monthly residuals of a regression of employment on cohort dummies, total number of children, and the birth year of the mother’s youngest child.

Figure 2. Employment rate from 2013 to 2016

robustness checks, we reduce the distance to the cutoff date, which makes the treatment and control groups more similar.

Before turning to the econometric analysis, we provide graphical evidence regarding the employment outcomes for the treatment and the control groups. As outlined in Table 2, employment rates between mothers in the treatment and control groups strongly differ. This is further illustrated by the graph on the left in Figure 2, which shows the monthly average employment rates of individuals in the treatment (blue) and the control (orange) groups between January 2013 and December 2015. The dashed vertical line represents the introduction of the reform. The figure highlights that the employment level of mothers in the control group is clearly higher relative to the treatment group before and after the introduction of the reform. The lower employment rates in the treatment group are consistent with compositional differences regarding age and the total number of children between the treatment and the control groups.

We account for these differences and present an additional graph (right panel of Figure 2) that illustrates the monthly average of the residuals of a regression of employment on cohort dummies, the total number of children, and the birth year of the mother’s youngest child. The graph clearly indicates that the relatively large level differences in employment rates between the two groups in the 18 months leading up to the reform vanish as soon as we control for the aforementioned characteristics. Strikingly, we observe that the remaining variation in the employment rates of both groups clearly diverges after the introduction of the reform in 2014. Thus, graphically the income shock induced by the reform appears to have affected the employment rates among individuals in our treatment group.

4 Methodology

To identify the income effect, we exploit the variation in child related pension income induced by the 2014 pension reform. The set up is straight forward: we use the discontinuity in the reform design by the birth date of children, thus allowing us to define a treatment group, mothers with children born before 1.1.1992, 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 reform. In the main specification, we assign all women who gave birth to a child between January 1, 1986, and December 31, 1991, to the treatment group and all women with children born between January 1, 1992, and December 31, 1997, to the control group. The treatment intensity is defined by the number of children born before 1.1.1992 (eligible children).

As documented above, the age of children and the birth cohort of mothers differ between the treatment and control groups. In the empirical analysis, we control for these differences by including monthly time specific effects and individual fixed effects that account for birth cohort effects of the mother and other time invariant differences between the treatment and the control groups, such as education, overall number of children, birth date of children, and further unobserved effects that are constant over time. Moreover, we include a linear age trend of the first child born between 1986 and 1997 that varies over time. Importantly, we observe women born in the different cohorts both in the treatment and in the control group. Thus, our identification does not rely on the assumption that cohort effects of the mother are constant over time.

More formally, for the average effect, we specify the following difference-in-differences regression:

$$y_{it} = \alpha_i + \lambda_t + \beta D_{it} + \varepsilon_{it} \quad (1)$$

where y_{it} is the an indicator for employment for individual i at time t ; α_i and λ_t are individual and time fixed effects, respectively. Variable D_{it} is the interaction term of indicator variables for the treatment group, i.e. women with children born before 1.1.1992 and the post reform period, i.e. after 1.1.2014 and β is the reform coefficient of main interest. Finally, ε_{it} denotes the idiosyncratic error term.

5 Results

In Table 3, we present the results of the baseline regression framework. We consider four different specifications. All specifications except for the last (Column 4) include both time

16. Women who gave birth before and after the cut off date belong to the treatment group however only children born before count for the treatment intensity.

Table 3. Results Baseline Regressions

	Employment			
	(1)	(2)	(3)	(4)
Mother pension * post	-0.02615*** (0.0072)			-0.02615*** (0.0072)
No. of children before 92 * post		-0.01177*** (0.0024)		
1 child before 92 * post			-0.01512* (0.0082)	
2 children before 92 * post			-0.02989*** (0.0079)	
> 2 children before 92 * post			-0.03827*** (0.0093)	
Pre avg. emp. rate treated	0.5995	0.5995	0.5995	0.5995
Observations	209,268	209,268	209,268	209,268
Individuals	5,813	5,813	5,813	5,813
Treated Individuals	4,774	4,774	4,774	4,774
Untreated Individuals	1,039	1,039	1,039	1,039
Time Dummies	YES	YES	YES	YES
Individual Fixed Effects	YES	YES	YES	NO

Note: The table reports difference-in-differences estimates of the effect of the income shock induced by the 2014 reform on employment for the time period January 2013 to December 2015. The dataset is limited to women who are 50 or older in 2014 and had a child between 1986 and 1997. 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 except the one presented in Column 4 include individual fixed effects. Time fixed effects as well as a linear time trend variable that denotes the age in months of the first child born between January 1986 and December 1997 are included in all regressions. Additionally we include controls for prior years of employment, the sum of accumulated pension points and total number of children in Column 4. Standard errors are reported in parentheses and are clustered on the individual level.

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

and individual fixed effects. In Column 1, we regress employment on the reform effect, i.e. an interaction term consisting of a dummy variable denoting if the individual was affected by the reform and the post reform variable. As documented in Table 1 the reform effect 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 (eligible children). This specification assumes that the number of children has a linear effect. In the next specification (Column 3) we separate the treatment group into three categories based on whether the mother has given birth to one, two, or more than two children before the cutoff date. 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. Finally, Column 4 shows the results of a regression similar to Column 1 but using cohort fixed effects instead of individual fixed effects. In addition, we control for prior years of employment, total number of children, as well as the sum of accumulated pension points in this specification. Throughout, standard errors are clustered on the individual level and are reported in brackets.

All specifications show a clear picture: The permanent increase in pension income significantly reduces employment. All parameter coefficients are significant at the 1% level except the interaction effect with one child before 1992 in the third specification (Column 3). In more detail, the results in Column 1 indicate that individuals affected by the reform in 2014 reduce labor supply by 2.6 percentage points on average, which is a reduction of nearly 4% relative to the pre-reform employment rate of 60%. The results in Columns 2 and 3 illustrate that a greater treatment intensity leads to a stronger labor supply response: Mothers with two eligible children reduce labor supply by about 3 percentage points while the effect for mothers with one eligible child is 1.5 percentage points. Finally, the results obtained from the regression in Column 4 show that our results are robust and do not depend on the inclusion of individual fixed effects.

5.1 Robustness checks - different cut off period lengths

As noted in the Data section, we observe compositional differences between the treatment and control groups in terms of birth year of the mother as well as total number of children. In the main specification, we account for these differences by including individual and cohort specific effects and by controlling for the age of the children. In the following, we show that the results do not change when we vary the distance to the cutoff period which reduces the differences between the treatment and control groups. Specifically, we reduce the length of the sample cutoff period by one year on each side of the cutoff until only women with children born in 1991 and 1992 are considered. The results are shown in Table 4 and depict a clear pattern: While the parameter estimates are relatively stable in terms of magnitude, the significance level decays as the period to the cutoff date becomes smaller. This is driven by the mechanical reduction in sample size. Nevertheless the parameter estimates of Specification 1, shown in Part A of Table 4, are significantly different from

Table 4. Results for different cut off lengths

Part A: Dummy specification for different lengths of the cut off period						
	1986-1997	1987-1996	1988-1995	1989-1994	1990-1993	1991-1992
Mother pension * post	-0.0262*** (0.0072)	-0.0247*** (0.0075)	-0.0185** (0.0080)	-0.0169** (0.0085)	-0.0174* (0.0101)	-0.0197 (0.0136)
Observations	209,268	187,992	162,576	132,048	97,200	55,044
Individuals	5,813	5,222	4,516	3,668	2,700	1,529
Treated Ind.	4,774	4,290	3,712	3,017	2,222	1,244
Untreated Ind.	1,039	932	804	651	478	285
Time Dummies	YES	YES	YES	YES	YES	YES
Ind. Fixed Effects	YES	YES	YES	YES	YES	YES
Part B: Separation by number of children born before 92 for different lengths of the cut off period						
	1986-1997	1987-1996	1988-1995	1989-1994	1990-1993	1991-1992
1 child	-0.0151* (0.0082)	-0.0136 (0.0085)	-0.0047 (0.0091)	-0.0026 (0.0097)	-0.0040 (0.0112)	-0.0095 (0.0153)
bf 92 * post	-0.0299*** (0.0079)	-0.0270*** (0.0082)	-0.0240*** (0.0088)	-0.0245*** (0.0095)	-0.0256** (0.0114)	-0.0249* (0.0151)
2 children	-0.0383*** (0.0093)	-0.0407*** (0.0099)	-0.0343*** (0.0105)	-0.0317*** (0.0114)	-0.0333** (0.0138)	-0.0339* (0.0190)
3 or more children						
bf 92 * post						
Observations	209,268	187,992	162,576	132,048	97,200	55,044
Individuals	5,813	5,222	4,516	3,668	2,700	1,529
Treated Ind.	4,774	4,290	3,712	3,017	2,222	1,244
Untreated Ind.	1,039	932	804	651	478	285
Time Dummies	YES	YES	YES	YES	YES	YES
Ind. Fixed Effects	YES	YES	YES	YES	YES	YES

Note: This table displays the effect of the reform in 2014 on employment between January 2013 and December 2015 for varying lengths of the cutoff period. The dataset is limited to women who are 50 or older 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 as well as a linear time trend variable that denotes the age in months of the first child born in the cutoff period. Standard errors are reported in parentheses and are clustered on the individual level.

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

zero at least on the 10% level until we restrict the sample to only 1 year before and after the cutoff date.

In the nonlinear specification (Part B of Table 4), we find that the effect for two or three or more children stays highly significant throughout all samples. The point estimates for the coefficients are highly similar in terms of magnitude for all bandwidth definitions.

5.2 Robustness checks - Placebo specification

The validity of the difference-in-differences approach relies crucially on the parallel trend assumption. To validate this assumption and to ensure the robustness of the regression results, we conduct two different placebo tests during the pre-reform period. In particular, we first consider the time period between January 2011 and December 2013 before moving on to the period between January 2010 and December 2012. For both setups, we create a placebo post indicator variable that splits the considered time frame into 18 hypothetical pre-placebo and 18 post-placebo policy months. We then replicate our baseline specifications for both placebo setups. Table 5 shows the results for both placebo tests. For the first placebo test (Part A), for the baseline, we find significant effects only at the 10% significance level and the point estimates are markedly smaller than in the main specification. The nonlinear specification (Column 3) shows that the significant effect is driven by mothers with two or more children before 1992. This effect is again clearly smaller than in the main specification. The results of the second Placebo test (Part B) are even stronger. All effects are insignificant and the point estimates are close to zero.

5.3 Effect heterogeneity

Thus far, we focus on the average effect of the pension reform on employment. In the following, we examine effect heterogeneity along three important dimensions: by earnings potential, accumulated pension wealth (observed pre-reform), and by the age of the mother.

Earnings potential: Individuals differ in terms of their potential/ability to accumulate pension points through employment, which, in turn, affects opportunity costs of employment and might lead to different employment responses induced by an income effect. To investigate how outcomes differ based on earning potentials, we split the sample according to the cohort medians of the monthly mean value of pension points an individual earned while being employed before the reform was announced.¹⁷ We then estimate different specifications for the two sub-samples. The results are shown in part A of Table 6. With the exception of the estimate for individuals with only one child born before 1992, we find highly significant effects for mother with low earning potential (below median earnings). In contrast, our findings for both specifications show insignificant effects for mothers above median sample except for those individuals with more than two children. Moreover,

17. We focus on the period between 2000 and 2014. Since we do not observe employment during this time period for all individuals, our overall sample size decreases from 5,813 to 4,506.

Table 5. Results Placebo Regressions

Part A: Placebo 1 - 2011-2013				
	(1)	(2)	(3)	(4)
Mother pension * placebo post	-0.0128*			-0.0128*
	(0.0073)			(0.0073)
No. of children before 92 * placebo post		-0.0043*		
		(0.0022)		
1 child bf 92 * placebo post			-0.0097	
			(0.0083)	
2 children bf 92 * placebo post			-0.0108	
			(0.0080)	
More than 2 children bf 92 * placebo post			-0.0217**	
			(0.0089)	
Observations	217,980	217,980	217,980	217,980
Individuals	6,055	6,055	6,055	6,055
Treated Individuals	4,973	4,973	4,973	4,973
Untreated Individuals	1,082	1,082	1,082	1,082
Time Dummies	YES	YES	YES	YES
Individual Fixed Effects	YES	YES	YES	YES
Part B: Placebo 2 - 2010-2012				
	(1)	(2)	(3)	(4)
Mother pension * placebo post	-0.0066			-0.0066
	(0.0070)			(0.0070)
No. of children bf 92 * placebo post		-0.0009		
		(0.0025)		
1 child bf 92 * placebo post			-0.0073	
			(0.0080)	
2 children bf 92 * placebo post			-0.0046	
			(0.0079)	
More than 2 children bf 92 * placebo post			-0.0092	
			(0.0090)	
Observations	218,016	218,016	218,016	218,016
Individuals	6,056	6,056	6,056	6,056
Treated Individuals	4,974	4,974	4,974	4,974
Untreated Individuals	1,082	1,082	1,082	1,082
Time Dummies	YES	YES	YES	YES
Individual Fixed Effects	YES	YES	YES	NO

Note: The table presents the results of a difference-in-differences regression of employment on a placebo policy for the years 2011 to 2013 (Part A) as well as the period from 2010 to 2012 (Part B). The respective dataset only includes women who are 50 or older in 2014 and had a child between 1986 and 1997. 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 except the one presented in Column 4 include individual fixed effects. Time fixed effects as well as a linear time trend variable that denotes the age in months of the first child born between January 1986 and December 1997 are included in all regressions. Additionally we include controls for prior years of employment, the sum of accumulated pension points and total number of children in Column 4. Standard errors are reported in parentheses and are clustered on the individual level.

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

Table 6. Effect Heterogeneity - Pension Wealth

Part A: Split by the median of the mean of monthly pension points earned during employment by cohort				
	Below median	Above median	Below median	Above median
Mother pension * post	-0.0361*** (0.0130)	-0.0028 (0.0088)		
1 child bf 92 * post			-0.0229 (0.0147)	0.0104 (0.0103)
2 children bf 92 * post			-0.0452*** (0.0138)	-0.0028 (0.0101)
More than 2 children bf 92 * post			-0.0396** (0.0173)	-0.0374** (0.0149)
Observations	81,252	80,964	81,252	80,964
Individuals	2,257	2,249	2,257	2,249
Treated Individuals	1,854	1,854	1,854	1,854
Untreated Individuals	403	419	403	419
Time Dummies	YES	YES	YES	YES
Individual Fixed Effects	YES	YES	YES	YES
Part B: Split according to the median of the sum of accumulated pension points in January 2013				
	Below median	Above median	Below median	Above median
Mother pension * post	-0.0420*** (0.0130)	-0.0202** (0.0084)		
1 child bf 92 * post			-0.0333** (0.0145)	-0.0048 (0.0097)
2 children bf 92 * post			-0.0428*** (0.0138)	-0.0277*** (0.0094)
More than 2 children bf 92 * post			-0.0521*** (0.0147)	-0.0405*** (0.0136)
Observations	104,652	104,616	104,652	104,616
Individuals	2,907	2,906	2,907	2,906
Treated Individuals	2,496	2,278	2,496	2,278
Untreated Individuals	411	628	411	628
Time Dummies	YES	YES	YES	YES
Individual Fixed Effects	YES	YES	YES	YES

The table reports the effect of the 2014 pension reform on employment between January 2013 and December 2015 differentiated by measures related to pension wealth. In part A the baseline sample is split according to the cohort median of the average monthly pension points individuals earned through employment between 2000 and 2012. Part B shows the results for a split by the median of accumulated pension points in January 2013. The datasets are limited to women who are 50 or older in 2014 and had a child between 1986 and 1997. 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 as well as a linear time trend variable that denotes the age in months of the first child born in the cutoff period. Standard errors are reported in parentheses and are clustered on the individual level.

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

the point estimates of the mother with low earnings potential are clearly higher in both specifications.

Pension Wealth Accumulated pension wealth determines the relative size of the income effect related to the increase in child-related pension points. This is of particular importance since it allows us to evaluate distributional aspects of the income shock. Moreover, we can assess the importance of credit constraints for employment decisions and test if mothers with low pension wealth are more likely to reduce their employment.

We split our baseline sample by the median of accumulated pension points an individual held before the introduction of the reform¹⁸ and define a sample with low and high pension wealth. Part B of Table 6 presents the results for both sub-samples. We find that the effect is larger for individuals with pension wealth below the median in both specifications. More specifically, our results indicate that individuals with a pension wealth level below the median reduce employment by 4.2 percentage points, whereas the reduction for individuals above the median is, at about 2 percentage points, clearly lower. Further we find that individuals who were treated with a higher intensity show stronger labor supply responses in both groups. Contrary to part A, the results of both specifications are significantly different from zero except for females who had only one child before 1992 and who had an amount of pension points above the median value.

Age: Finally, we turn to the findings regarding different age groups. Age plays an important role in our design since the reform does not affect income immediately but rather expected retirement income in the future. Under the assumption that individuals are forward-looking and discount future income, the intensity of treatment might differ depending on the distance to statutory retirement age. Moreover, effects might differ since labor market attachment related to preferences or labor market constraints vary between age groups. To examine age related heterogeneity, we divide the treatment group into three different sub-groups: individuals who are between 50 and 55, 56 and 60, and above 60 years old when the reform was introduced. Subsequently we replicate all regression specifications of our baseline framework, where all respective variables are interacted with the age group indicators. The corresponding results are displayed in Table 7. Our findings indicate that the response magnitude increases with age. Individuals older than 60 decrease their labor supply, on average, up to four percentage points (Column 1) while individuals between 56 and 60 as well as individuals between 50 and 55 only decrease by 3.1 and 2.2 percentage points, respectively. This pattern is less clear for the specification in which we interact our post indicator, the age group indicator, and the number of children born before 1992 (Column 2) or child specific variables (Column 3). In these specifications the effects for the different age groups do not significantly differ.

18. We focus on accumulated pension wealth observed in January 2013

Table 7. Effect Heterogeneity - Age

	Employment			
	(1)	(2)	(3)	(4)
d6165 * post * Mother Pension	-0.0400*** (0.0146)			-0.0400*** (0.0146)
d5660 * post * Mother Pension	-0.0305*** (0.0080)			-0.0305*** (0.0080)
d5055 * post * Mother Pension	-0.0216*** (0.0077)			-0.0216*** (0.0077)
d6165 * post * No. of children bf 92		-0.0119*** (0.0046)		
d5660 * post * No. of children bf 92		-0.0130*** (0.0028)		
d5055 * post * No. of children bf 92		-0.0099*** (0.0029)		
d6165 * post * 1 child bf 92			-0.0277 (0.0297)	
d6165 * post * 2 children bf 92			-0.0540** (0.0232)	
d6165 * post * more than 2 children bf 92			-0.0357* (0.0204)	
d5660 * post * 1 child bf 92			-0.0148 (0.0118)	
d5660 * 2 children bf 92			-0.0334*** (0.0088)	
d5660 * post * more than 2 children bf 92			-0.0423*** (0.0114)	
d5055 * post * 1 child bf 92			-0.0146* (0.0087)	
d5055 * 2 children bf 92			-0.0251*** (0.0090)	
d5055 * post * more than 2 children bf 92			-0.0339*** (0.0124)	
Observations	209,268	209,268	209,268	209,268
Individuals	5,813	5,813	5,813	5,813
Treated Individuals	4,774	4,774	4,774	4,774
Untreated Individuals	1,039	1,039	1,039	1,039
Time Dummies	YES	YES	YES	YES
Individual Fixed Effects	YES	YES	YES	YES

Note: The table reports difference-in-differences estimates of the effect of the income shock induced by the 2014 reform on employment differentiated by the age of the individual for the time period January 2013 to December 2015. The dataset is limited to women who are 50 or older in 2014 and had a child between 1986 and 1997. The treatment group consists of women who had 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. The individuals in the treatment group are categorized into three groups based on their age when the reform was implemented (50 to 55, 56 to 60, and over 60 years old). All regressions except the one presented in Column 4 include individual fixed effects. Time fixed effects as well as a linear time trend variable that denotes the age in months of the first child born between January 1986 and December 1997 are included in all regressions. Additionally we include controls for prior years of employment, the sum of accumulated pension points and total number of children in Column 4. Standard errors are reported in parentheses and are clustered on the individual level.

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

6 Conclusion

In this paper, we exploit unique variation of expected pension income to identify the employment effect induced by a permanent income effect. Specifically, we use the variation induced by a pension reform in Germany that raised pension benefits related to children but that does not affect working incentives. 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 that includes complete individual employment histories. We find that income effects are significant and economically important. Our estimates show that the permanent income effect, which increases pensions on average by 7.4%, reduces employment of affected women by about 2 percentage points. We find a clear pattern by treatment intensity. The higher the income effect, i.e., the more children eligible for the reform, the larger the reduction in employment. Moreover, the effect size increases with the age of the mother. Since the income effect only materializes when women enter retirement, this age effect reflects the different time horizon of women at different ages prior to retirement. Moreover, it is consistent with lower labor attachment close to retirement. Finally, we show that the negative employment effects are larger for women with low pre-reform pension wealth, thus suggesting that credit constraints play an important role. Our results do not change when we vary the distance to the cut-off date to define the treatment group and they are robust to changes in the observation period. Moreover, we show in placebo regressions that our identification assumptions hold.

Our results have important policy implications. We document that an increase in child related pension entitlements that are motivated by distributional concerns have important labor market effects with financial consequences for the tax and the pension systems.

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