Forward-Looking Labor Supply Responses to Changes in Pension Wealth: Evidence from Germany*

Elisabeth Artmann† Nicola Fuchs-Schündeln‡ Giulia Giupponi§

March 2026

Abstract

We provide new evidence of forward-looking labor supply responses to changes in pension wealth. We exploit a 2014 German reform that increased pension wealth for mothers by an average of 4.4% per child born before January 1, 1992. Using administrative data on the universe of working histories, we implement a difference-in-differences design comparing women who had their first child before versus after January 1, 1992. We document significant reductions in labor earnings, driven by intensive margin responses. Our estimates imply that, on average, an extra euro of pension wealth reduces unconditional labor earnings by 54 cents.

Keywords: labor supply, social security, pension wealth
JEL Codes: H55, J22, J26

*We thank the editor Marika Cabral and four anonymous referees, Camille Landais, Nathaniel Hendren and Attila Lindner for helpful comments. We also thank participants in seminars at the BI Norwegian Business School, Bocconi University, Central European University, Columbia University, Erasmus University Rotterdam, Goethe University Frankfurt, Graduate Institute Geneva, Hong Kong University, KU Leuven, National University of Singapore, Pompeu Fabra, Universitat Autònoma de Barcelona, the University of Copenhagen, the University of Cologne, the University of Geneva, and the University of Oslo, conference participants at ESPE 2023, EEA 2023, EALE 2023, the 2023 meeting of the Verein für Socialpolitik, the 2023 meeting of German Economists Abroad, and the 2024 symposium of the Spanish Economic Association, and workshop participants at the University of Cambridge, the CEPR Public Economics Annual Symposium, CESifo, the European Labor Symposium for Early Career Economists, the FDZ-RV, the Forschungsnetzwerk Alterssicherung, the IAB, the Workshop on Micro and Macro Facts and Inequality at the University of Geneva, the NBER Summer Institute, and the Ausschuss für Sozialpolitik for helpful discussions and suggestions. We are grateful to the staff at the Research Data Center of the German Pension Insurance (DRV) for helpful assistance with the VSKT data. Celina Proffen and Ginevra Casini provided excellent research assistance. This paper uses administrative data sampled from the Integrated Employment Biographies (IEB) data of the Institute for Employment Research (IAB). The data are social data with administrative origin which are processed and kept by IAB according to the German Social Code, Book III. Fuchs-Schündeln and Giupponi gratefully acknowledge financial support from the Forschungsnetzwerk Alterssicherung. Fuchs-Schündeln additionally gratefully acknowledges financial support from the German Research Foundation through a Gottfried Wilhelm Leibniz-Prize, and from the ERC through Consolidator Grant No. 815378. Giupponi additionally gratefully acknowledges financial support from Bocconi University, Baffi CAREFIN and the Steven H. Sandell Grant Program sponsored by the Center for Retirement Research at Boston College and funded by the U.S. Social Security Administration.

†Institute for Employment Research (IAB). Email: elisabeth.artmann2@iab.de
‡WZB Berlin Social Science Center, Goethe University Frankfurt and CEPR. Email: nicola.fuchs@wzb.eu
§Bocconi University and CEPR. Email: giulia.giupponi@unibocconi.it

Introduction

Demographic trends have increased the potential labor supply of the elderly, but are simultaneously exerting rampant pressure on the fiscal sustainability of pay-as-you-go public pension systems. To address this challenge, governments have implemented – or are planning to implement – different types of pension reforms.[1] A large literature investigates how the design of pension systems affects the labor supply choices of individuals close to retirement. However, if individuals are forward-looking, pension reforms can affect behavior also at younger ages, with potential aggregate labor supply implications. For example, more generous systems might induce workers to reduce their lifetime labor supply by working fewer hours. In light of the demographic and economic trends common to most developed economies, understanding the size and nature of these forward-looking effects has become increasingly important.

In this paper, we study how changes in the generosity of public pension systems affect the labor supply behavior of late-career workers – i.e. individuals in their early to mid-50s – before retirement. In particular, we provide new estimates of forward-looking labor supply responses to changes in pension wealth. Knowledge of income (or wealth) effects is key for the evaluation of pension policies and other old-age income support schemes. In addition, income effects are important for welfare analyses and normative assessments of the distributional effects of retirement policies, since they are directly related to the insurance value of social security (Chetty, 2004; 2008; Kolsrud et al., 2024).

The identification of forward-looking labor supply responses to changes in pension wealth is challenging for various reasons. First, separately identifying income and substitution incentives is notoriously difficult, since most policy changes conflate the two.[2] Second, the gradual phase-in of reforms implies that they typically generate small discontinuities, which are unlikely to trigger meaningful responses. Third, individuals far from retirement are usually all identically treated, leaving little room for exogenous variation in incentives. Finally, changes in benefit generosity are generally achieved through changes in pension rules or formulae, making it hard to isolate behavioral responses that are not attenuated by cognitive or information frictions.

This paper contributes to our understanding of how changes in pension wealth affect labor supply in middle-age years by overcoming these limitations. We leverage unique variation stemming from a German pension reform to the pension scheme popularly known as 'Mütterrente' (mothers' pension). The reform exogenously changed the amount of pension contributions credited to mothers for the time spent raising their children. From July 2014, mothers of children born before January 1, 1992 had their pension contributions increased by an amount correspond-

[1] Pension reforms typically entail a combination of a tightening of the contribution-benefit link, a reduction in benefit levels, and an increase of statutory retirement ages (Giupponi and Seibold, 2024).

[2] Most social security reforms entail both changes in benefit levels and in the net marginal returns to additional earnings, making it difficult to separate income (or wealth) and substitution effects. For example, the well-known Social Security Notch, which led to large cuts in Old Age and Survivors Insurance (OASI) benefits for individuals born after 1917 in the United States, decreased both the level of pension benefits (income effect) and the returns to additional earnings (substitution effect); see Gelber, Isen and Song (2016).

ing to 4.4% of average pension wealth per child; the pension contributions of mothers of children born on or after January 1, 1992 were left unchanged. The timing and nature of the Mütterrente reform generate a pure pension wealth effect on affected mothers, which is permanent, is not confounded by changes in substitution incentives, and is sizable when compared to pension reforms in other countries. Importantly, the reform did not alter the pension benefit formula. If leisure is a normal good, the increase in pension wealth is expected to reduce the lifetime labor supply of recipients.

Using administrative data from the Integrated Employment Biographies (IEB) of the German Institute for Employment Research (IAB) covering the universe of employment histories in the private sector, we select women who gave birth to their first child in a small window around January 1, 1992. We analyze their labor supply responses to the change in pension wealth induced by the Mütterrente reform of July 2014 over the five years following its enactment. During this period, the affected women were on average aged 50 to 55 and thus 12 to 17 years away from the full retirement age and – conditional on meeting the eligibility criteria for early retirement – 8 to 13 years from the early retirement age. To identify the effect of interest, we implement a difference-in-differences design and compare the employment dynamics of women who had their first child just before (*treatment group*) and just after (*control group*) January 1, 1992, from before to after the 2014 reform.

We document significant reductions in total labor earnings unconditional on employment among treated mothers following the 2014 reform. In the 2.5 years following the policy change, unconditional labor earnings drop on average by EUR 101 per year or 0.7% of pre-policy levels. Consistent with the notion that optimization frictions may limit the ability of individuals to adjust their labor supply in the short run, we find larger medium-run responses – of EUR 196 per year or 1.3% of the pre-policy mean – in the 3-5 years after the reform. To interpret the magnitude of these reduced-form effects, we rescale the estimated medium-run earnings response by the pension wealth shock due to the 2014 reform. For an average increase in the present discounted value of pension wealth of EUR 3,830 or 4.4%, the implied pension wealth effect is EUR -5.1 per year per EUR 100 of wealth, and the implied pension wealth elasticity is -0.3.[3] The size of the pension wealth effect that we describe above can be hard to assess, since it does not account for the time horizon over which individuals plausibly allocate the increase in pension wealth. In order to better gauge the size of the wealth effect, we adopt the annuitization method described in Golosov et al. (2024) and compute a marginal propensity to earn (MPE) out of pension wealth for late-career workers. Our estimates imply a moderately large MPE of -0.54.[4] Both the magnitude of the economic incentives generated by the pension reform and the salience of the reform itself could be responsible for the sizable response that we document.

[3] As we explain in more detail in Section II, we use a second administrative data source, the Insurance Account Sample Versicherungskontenstichprobe (VSKT) from the German Pension Insurance (DRV), to calculate the change in pension wealth generated by the reform.

[4] The estimate has a bootstrapped standard error of 0.18.

We probe the anatomy of the labor supply response by decomposing the change in unconditional labor earnings into its extensive and intensive margin components. We find that the response is entirely concentrated along the intensive margin: in the medium run, labor earnings conditional on employment drop by 0.9%, and the probability of working full-time (as opposed to working part-time or in marginal employment) by 1.8%.[5] By contrast, the probability of being employed is not affected by the reform.

A large literature has investigated the effects of social security and other government old-age support programs on labor supply close to retirement (for reviews, see Gruber and Wise, 1999; Krueger and Meyer, 2002; Blundell, French and Tetlow, 2016). Instead, only a small number of papers have analyzed labor supply responses to changes in pension benefits far from retirement. Among these, existing studies examine the role of changes in the statutory retirement age (Hairault, Sopraseuth and Langot, 2010; Carta and De Philippis, 2023), the contribution-benefit link (French et al., 2022), and pension rules more generally (Bovini, 2019). Our paper contributes to this emerging strand of the literature by providing new evidence of 'forward-looking' labor supply responses to permanent changes in pension wealth.[6] This paper complements a contemporaneous study by Becker et al. (2022), which examines the same reform using a sample of administrative data. Our analysis instead draws on comprehensive administrative records covering the full population. The broader coverage of these data affords greater statistical power and enables us to examine a wider range of outcomes, allowing us to estimate effects with precision on both the intensive and extensive margins. A more detailed comparison of research designs and estimated magnitudes is provided in the discussion of our results. This paper is also related to work that attempts to separately identify income/wealth effects and substitution incentives in the context of retirement benefits (e.g., Costa, 1997; French, 2005; Liebman, Luttmer and Seif, 2009; Danzer, 2013; Gelber, Isen and Song, 2016; Fetter and Lockwood, 2018; Gelber, Jones and Sacks, 2020) and other welfare transfers (Deshpande, 2016; Jones and Marinescu, 2022; Coyne et al., 2024; Giupponi, 2024), using quasi-experimental or structural approaches. Our paper contributes to this literature in three ways. First, our setting allows us to compellingly identify the wealth effect of retirement income through quasi-experimental policy variation, rather than through the estimation or simulation of structural models that typically require strong functional-form assumptions. Second, thanks to the nature of the policy variation that we exploit, we can provide a point estimate of the wealth effect, thus complementing previous work that sets bounds on it (e.g., Deshpande, 2016; Gelber, Isen and Song, 2016; 2017; Giupponi, 2024). In this respect, our parameter estimates are similar in nature to those that can be obtained studying lottery winnings (Imbens, Rubin and Sacerdote, 2001; Cesarini et al., 2017; Picchio, Suetens and van Ours, 2018; Golosov et al., 2024), with the additional advantage that the windfall gain that we examine does not lead to contemporaneous marginal tax rate changes, since it only materializes in the future. Finally, in contrast to the

[5] Marginal employment – also called minijob – is a low-wage contract with monthly earnings below given thresholds. Employees in marginal employment are exempt from employee social security contributions.

[6] Other work has looked at savings responses to pension wealth changes. See, for instance, Attanasio and Brugiavini (2003); Attanasio and Rohwedder (2003); Bottazzi, Jappelli and Padula (2006); Lachowska and Myck (2018).

papers cited above, we identify a forward-looking response, as opposed to a response to contemporaneous (or a mix of contemporaneous and future) wealth changes.

Finally, this paper complements a literature that has predominantly examined men's labor supply, by placing its contextual focus on female labor supply in the early twenty-first century. As recent demographic trends have given greater significance to the aggregate potential of female labor force at middle ages, it is ever more important to understand how the design of pension systems affects the labor supply decisions of this group.

The paper proceeds as follows. Section I outlines the institutional details of the German pension system and the 2014 Mütterrente reform. Section II describes the data used in the analysis. Section III discusses the expected effects of the reform on individual labor supply and illustrates the empirical strategy. Estimates of the pension wealth effect on labor supply are presented in Section IV. Section V discusses welfare implications, and Section VI concludes.

I. Institutional Context

A. The German Pension System

The German statutory public pension system is an earnings-related Pay-As-You-Go (PAYG) system.[7] Pension payments are calculated using a formula based on cumulated pension points, the monetary value assigned to each pension point, and an adjustment factor depending on age at retirement. One pension point represents annual contributions from a reference contributor earning the average income in a given year. The monthly pension is determined by multiplying total pension points by the monthly point value, which is set annually by the government based on gross wage growth and is thus independent of individual conditions (e.g. individual earnings or contributions).

Eligibility for retirement pension is conditional on a minimum qualifying period of 5 years of contributions. Childcare periods count as qualifying periods. For the majority of individuals in our sample, the full retirement age is 67.[8] Individuals can retire at most four years earlier under the early retirement regulations, which require 35 years of contributions and imply a 3.6% reduction in pension entitlements for each year of retirement before the full retirement age.[9]

[7]Pensions from the statutory public pension system are the main form of retirement income in Germany. In 2019, about 91% of West German women aged 65 or older received payments from the statutory pension system. The average gross pension entitlement per recipient amounted to EUR 1,037. Only around 28% of women received retirement income from company-provided pension plans, averaging EUR 357 per recipient (Deutsche Rentenversicherung Bund, 2022).

[8]More precisely, the full retirement age was 65 years and 6 months for women born in 1952, the oldest cohort in our sample. It progressively increased for subsequent cohorts, reaching 67 for women born in or after 1964. The average woman in our sample was born in 1964.

[9]An additional pathway into retirement is the seniority pension. Eligibility for seniority pension requires 45 years of contributions and 63 years of age for individuals born before 1953. The seniority pension age threshold increases

In Germany, individuals insured with the public pension system are regularly informed about their pension entitlements. Since 2002, the German Pension Insurance automatically sends information about pension entitlements on an annual basis to all insured persons aged 27 and over who have acquired at least five years of contribution periods. This information includes the amount of cumulated pension contributions and the projected level of old-age pension one would obtain if the individual kept contributing as in the previous five years until the full retirement age.[10]

B. The Childcare Contribution Benefit 'Mütterrente'

In Germany, parents are credited pension contributions for the time spent raising their children under the Mütterrente scheme, which was first introduced in 1986. Since then, parents have been credited pension points for certain periods after childbirth, the generosity and length of benefits having changed over time. Childcare pension points are by default attributed to mothers and only assigned to fathers on the mother's request. Around 98% of the pension benefits go to mothers (Deutsche Rentenversicherung Bund, 2014).

The childcare benefit scheme has been subject to various reforms, which are visually summarized in Appendix Figure A1 and described below.[11] We note here that, for our analysis, we exploit a 2014 reform that increased the number of pension points credited for children born before 1992 from one to two, leaving unchanged to three the number of points credited for children born in or after 1992.

From 1986 to 1991, mothers were credited a maximum of 0.75 pension points per child for the first year after childbirth; this also applied to childbirths before 1986. The full 0.75 pension point benefit was granted to mothers who were not employed in this first year after childbirth. For employed mothers, childcare pension points were fully withdrawn against pension points from regular employment. Hence, if mothers earned half of a pension point through work during the first year after childbirth, they would be credited only 0.25 pension points as childcare benefit.

The 1992 Pension Reform Act, passed in December 1989, increased benefit duration from one to three years, but only for mothers of children born on or after January 1, 1992. These mothers were thus entitled to a maximum of 2.25 pension points (three years $\times$ 0.75 pension points) instead of a maximum of 0.75 pension points. Mothers of children born before January 1, 1992 continued to receive a maximum of 0.75 pension points per child. Childcare pension points were still withdrawn against employment contributions at a 100% rate.

A reform in 1999 introduced two main changes: (i) it increased the generosity of the benefit from 0.75 to one pension point per year; (ii) it repealed the earnings test, i.e. the withdrawal of

step-wise to 65 for subsequent cohorts. Only a small fraction of individuals in our sample are likely to satisfy the seniority pension requirements (Börsch-Supan, Coppola and Rausch, 2015).

[10]See Figure A7 in Seibold (2021) for an illustration of the information letter (*Renteninformation)*.

[11]See also Thiemann (2015).

childcare points against employment points.[12] The 1999 pension reform was retroactive, hence both current retirees and individuals entitled to future pension payments benefited from it.

We exploit in this paper the reform – popularly known as 'Mütterrente' reform – implemented in 2014. Starting July 2014, mothers whose children were born before January 1, 1992 got one extra pension point per child and, as a result, reached a total of two pension points per child born before 1992. In 2014, the accreditation of one additional pension point was equivalent to a EUR 336/312 higher pension per year in the West/East, respectively (Deutsche Rentenversicherung Bund, 2020). As we describe in Section B, for the average mother in our sample, who was close to 50 at the time of the reform, one additional pension point implied an increase in real pension wealth of approximately EUR 3,830 at the time of the reform. This increase corresponds to 4.4% of pension wealth, a sizable effect compared to other changes analyzed in the literature. By comparison, the US Social Security 'Notch' – the largest discontinuous change in Old Age and Survivors Insurance benefits in the United States – generated a reduction in average lifetime discounted benefits of 2% for women and of 5% for men (Gelber, Isen and Song, 2016; 2017).[13] At the time of reform, individualized letters were sent to affected mothers informing them of the accreditation of additional childcare periods for each of their eligible children (the letter was sent separately from the regular yearly information letter we mentioned before). Moreover, the German Pension Insurance (DRV) provided plenty of easily accessible information on the reform and its implications on its website.

The 2014 reform was an item of the Christian Democratic Union (CDU) throughout the electoral campaign for the 2013 elections, aimed at attenuating the unequal treatment of mothers with children born before or after January 1, 1992. After the CDU won the election and formed a coalition with the Social Democratic Party (SPD), the reform became a central part of the coalition agreement signed in December 2013. The agreement already stated that the reform would be implementated on July 1, 2014. The reform was, indeed, legislated on May 23, 2014 and implemented six weeks later on July 1, 2014. The reform was highly debated during the electoral campaign and received ample news coverage around the time of its announcement and implementation. Appendix Figure A2 reports the number of online and newspaper articles in German including the word 'Mütterrente' over time, highlighting an abrupt increase in newspaper coverage of Mütterrente in 2013, followed by even greater coverage in 2014 (Factiva, 2022). The salience of the policy is also confirmed by the substantial increase in the intensity of Google searches for the term 'Mütterrente' in the German territory over the months preceding the reform implementation, as shown in Appendix Figure A3. The chart compares the evolution of the index of Google search intensity for Mütterrente with those for other salient gender- and pension-related policies. In light of this evidence, we consider December 2013, the signature

[12] This is true as long as the sum of childcare and employment contributions does not exceed the 'contribution ceiling', which is roughly equivalent to twice the average income in the country and is defined separately for East and West Germany each year. In our sample, only 0.7% of women ever hit the contributory ceiling in the three years after childbirth.

[13] Ye (2022) studies the effects of a German pension subsidy program targeting the most disadvantaged retirees, which increased pension benefits by 15% on average.

date of the coalition agreement, as the time in which the enactment of the reform became almost certain and use it as the relevant baseline period in our empirical strategy (see Section III for more details). From now on, we will refer to the period until December 2013 as pre-reform, and the period from January 2014 onward as post-reform.

In order to progressively equalize the treatment of mothers of children born before and after January 1, 1992, additional 0.5 pension points were credited to mothers of children born before January 1, 1992 starting from January 1, 2019, under the Mütterrente scheme. Appendix Figures A2 and A3 show substantial coverage and salience of this additional reform too. Due to the smaller wealth shock generated by the 2019 reform and the short post-reform time window, we do not analyze this reform here and restrict our analysis to the years up to 2018 only. We would argue that it is highly unlikely that individuals had anticipated the 2019 reform and expected a full equalization of childcare pension points already in 2014, or at any point before 2018. We discuss why we believe this to be the case in Appendix B.1.

To provide a complete picture of the institutional context, Appendix B.2 describes reforms of maternity leave legislation in Germany that took place during the same time period as the reforms to the Mütterrente scheme. Specifically, a 1992 maternity leave reform took place concurrently with the Mütterrente reform, and increased the duration of job-protected leave from 18 to 36 months. As we argue in Section III, this concurrent maternity leave reform does not pose a problem for identifying the effect of the July 2014 pension reform on labor supply behavior in our empirical design.

II. Data and Sample Definition

Our empirical analysis is based on two administrative data sources. The first and main source are the Integrated Employment Biographies (IEB) of the Institute for Employment Research (IAB). The second source is the Insurance Account Sample Versicherungskontenstichprobe (VSKT) from the German Pension Insurance (DRV), which we use exclusively to calculate the change in pension wealth caused by the 2014 reform, and to check for differential fertility around the January 1, 1992 cut-off date. Pension points and information on all births are included in the latter data set, but not in the former. Yet, the sample size of the latter data set is two orders of magnitude smaller.

A. Integrated Employment Biographies (IEB)

The IEB provide administrative data on *working histories* for the *entire* population of individuals who have ever worked in private sector employment subject to social security contributions or in marginal employment (Institut für Arbeitsmarkt- und Berufsforschung, 2021*b*).[14] It thus covers approximately 80% of the German working population starting from 1975 (1992) for

[14]Marginal employment is covered by the IEB since April 1999.

West (East) Germany, excluding individuals who never worked, or always worked as either self-employed or civil servants. The data include earnings and spell data on regular employment, marginal employment, receipt of unemployment insurance (unemployment benefit I) or of unemployment assistance (unemployment benefit II), registered job search, and participation in active labor market policies. Earnings are given as gross daily wages. Some spells indicate bonus payments which we include in our measure of total earnings. With this information, it is possible to construct the entire working and earnings history of individuals at a daily frequency.

For our analysis, we determine individuals' main labor market status in a month selecting the status with the longest monthly duration.[15] Indicators for the different labor market statuses are coded as 1 if it is an individual's monthly main status and 0 otherwise. The monthly labor market indicators are collapsed at the half-year (six months) level to obtain the average participation rate per status and half-year period. Monthly earnings from employment in the main status are determined by multiplying the gross daily wage and the monthly spell duration. Earnings per half-year period are obtained by adding up all earnings in individuals' main status in a half-year period.

Rich information on occupation is also included in the data. Occupations are classified according to the 5-digit German classification of occupations (Statistik der Bundesagentur für Arbeit, 2011; Eurostat, 2024). The first four digits indicate an increasingly fine-grained categorization of occupations, while the fifth digit indicates the qualification level of the occupation (helper, professional, specialist, expert). In our analysis, an occupation is defined as the interaction between (i) the first two digits and (ii) the fifth digit, for a total of 127 categories.[16]

As commonly the case in administrative data, hours worked are not recorded in the IEB data. We can, however, differentiate between part-time and full-time employment, and we will use that as our main indicator of intensive margin responses. The decision of marking a spell as part- vs. full-time is up to the reporting firm.

The IEB data do not provide direct information on childbirths, which we impute following the methodology developed by Müller and Strauch (2017) and widely adopted in studies of parental leave policies and maternal employment (see, for instance, Schönberg and Ludsteck, 2014; Collischon, Kuehnle and Oberfichtner, 2024; Illing, Schmieder and Trenkle, 2024). A woman aged 38 or younger is identified as giving birth if she features an employment interruption of at least 14 weeks (which is the mandatory maternity leave period) or an unemployment insurance de-registration, both due to entitlements from the statutory health insurance.[17] Since maternity leave is mandated to start six weeks before the estimated date of childbirth, the

[15] We break ties according to the following ranking: unemployment benefit I receipt > regular employment > unemployment benefit II receipt > marginal employment > registered job search > ALMP participation > not in IEB data. In a second step, if regular employment was determined to be the monthly main status in the first step, but spells of equal length in full-time and part-time employment exist, full-time employment is prioritized over part-time employment.

[16] A few very small categories have been combined with similar larger ones to obtain 127 groupings.

[17] Employment interruptions due to compensation by the statutory health insurance provider could be due to maternity leave or long-term sickness, which we cannot distinguish in the administrative data. The imputation procedure rests on the assumption that women of childbearing age are unlikely to experience employment interruptions

expected date of delivery is identified by adding 42 days to the last date of employment or the date of unemployment de-registration.[18] With this procedure, we can identify (expected) childbirths only for women with a record in the administrative data source immediately preceding childbirth, since we do not observe maternity leave spells for the self-employed, civil servants, or women out of the labor market.[19] This makes it likely that we miss second or higher order births, since many mothers do not return to the labor market between subsequent childbirths. We therefore focus in our analysis on first births only.

Using a 1% sample of the IEB data that supplements the social security records with direct information on childbirth from the German Pension Insurance, Schönberg (2009) shows that approximately 54% of women in the IEB sample who gave birth in 1991 or 1992 took maternity leave and can thus be identified as mothers with the imputation procedure. Considering that Schönberg (2009) estimates that women are 37 percentage points less likely to take maternity leave for their second than for their first (recorded) child, 46 for their third and 49 for their fourth or more child, and taking the birth order percentages from 1991 and 1992 into account, we can conclude that the share of first-time mothers who take maternity leave and can therefore be identified with our imputation strategy is around 75% of all first-time mothers.[20]

In the IEB data, individuals can be matched to their spouses using a statistical matching procedure based on surnames and exact geo-referenced addresses (Bächmann et al., 2021; Institut für Arbeitsmarkt- und Berufsforschung, 2021*c*). We describe the matching procedure in detail in Appendix C.3 and exploit this feature of the data to investigate intra-household spillover effects in Appendix F.

B. Insurance Account Sample (VSKT)

The second data source used in the analysis is the Insurance Account Sample Versicherungskontenstichprobe (VSKT) from the German Pension Insurance (DRV) (Forschungsdatenzentrum der Rentenversicherung, 2022). VSKT is a stratified random sample of 2% of individuals living in Germany and registered with the statutory pension system. The data set provides the entire *contributory histories* of individuals aged between 30 and 67 in the reference year of the data. We use the 2019 wave of VSKT, which includes individuals born between 1952 and 1989.

The data set contains the full contributory histories of the sampled individuals up to the reference year, starting from age 14 and ending at age 67. For each individual, a monthly history of employment, unemployment, sickness, childcare and other contributions to the pension

due to long-term sickness. For women who move out of unemployment insurance receipt, we can distinguish de-registrations due to maternity from those due to sickness.

[18]We are unable to observe if a woman goes on maternity leave earlier than six weeks before the estimated due date, e.g. due to pregnancy issues. In those cases, we would underestimate the date of childbirth, potentially misclassifying as treated individuals in the control group.

[19]In addition, we cannot distinguish between single and multiple births, nor between live and still births.

[20]The birth order percentages are only available for births to married couples, which make up 88% of all births (Statistisches Bundesamt, 1991; 1992).

system is recorded. With this information, we can compute the exact number of pension points (both relating to the specific time period or cumulated) at each point in time, both in total and from different contributory events. We use these data to calculate the pension wealth effect of the 2014 reform.[21] The VSKT data also include detailed information on the number of children and the exact month of birth of each child, irrespective of the previous labor market status of the mother. When computing statistics based on the VSKT, we always use sample weights, because the VSKT oversamples certain groups.

C. Sample Selection

IEB Sample We restrict the main sample of analysis in the IEB data to women born between 1952 and 1974, who gave birth to their first child in the last quarter of 1991 (treatment group) or the first quarter of 1992 (control group). We exclude mothers who gave birth in the two weeks right before and after January 1, 1992 to deal with mismeasurement in childbirth imputation, which could lead us to misassign treatment status. Any remaining misassignment of treatment status would bias our estimates towards zero. Since East Germans were only systematically included in the IEB data starting in 1992, we cannot identify births in the treatment group for East Germans, and therefore exclude them from the analysis. To do that, we exclude all women who had their first spell recorded after 1989, or at least one spell recorded in East Germany, or missing district of employment. We also exclude women who ever worked in the mining sector, which is subject to different pension contribution rules. Last, we exclude women who died before July 2014. Our final sample comprises 99,104 women.

Appendix Table A1 reports summary statistics for our analysis sample as of December 2013 (with earnings relating to the half-year period from July to December 2013), distinguishing individuals in the treatment and control groups. The average woman in our sample was 27 at the time of her first childbirth and 49 at the end of 2013. In December 2013, 72% of women are employed, 21% full-time and 38% part-time, with the remainder in marginal employment. About 23% of women have no employment or unemployment spell in the data in December 2013, meaning they are either out of the labor force, in civil service, or self-employed. Earnings when in work average EUR 10,500 for the second half of 2013. Differences in means between the treatment and control groups are very small in magnitude. Out of those that are statistically significant at 5% or less, most differences are below 2% of the mean in the control group.[22]

[21] For our heterogeneity analysis, we also impute pension points in the IEB data based on individual employment histories as described in more detail in Appendix C.2. Therein we also validate the quality of our imputation by comparing it with administrative records from the Insurance Account Sample of the German Pension Insurance.

[22] Mean unconditional earnings are 1.4% lower in the treatment group than in the control group. While the higher pension wealth starting in 1992 should have induced mothers in the control group to reduce their earnings, their slightly higher level of education is in line with higher earnings. In fact, we find a similar earnings difference in favor of the control group also in placebo treatment and control groups giving birth around January 1, 1994 or 1995, in line with increasing education levels among women in the relevant birth cohorts.

VSKT Sample We follow the same sample selection criteria in the VSKT data as in the IEB data, with a few exceptions. Due to sample size being two orders of magnitude smaller, we restrict the sample in the VSKT to women who gave birth to their first child in the twelve months, rather than three months, before or after January 1, 1992.[23] To increase consistency with the IEB sample of mothers, we further restrict the sample to mothers who have at least one employment spell in the nine months prior to giving birth. Our final VSKT sample comprises 2,787 women.

The summary statistics reported in Appendix Table A2 show that the average woman in the VSKT sample was approximately 49 in December 2013 with total fertility of two children and a total of about 24 pension points.

Representativeness of the sample To assess the representativeness of our main sample, we use data from the German Socio-Economic Panel (SOEP) – a longitudinal household study representative of the German population – to compare the education, fertility, and labor market characteristics of our sample to those of two reference groups (DIW Berlin, 2023). Our main sample consists of mothers born in 1952–1974 who had their first child in 1991 or 1992. We compare them to (i) all mothers born in 1952–1974 ("mothers in general") and (ii) mothers born in 1952–1974 who gave birth before 1992 ("mothers affected by the reform"). As shown in Appendix Table A3, our sample closely resembles mothers in general in terms of age at first birth, total fertility, education, and labor market experience. Compared to mothers affected by the reform, women in our sample are younger, slightly more educated, and display stronger early labor force attachment, differences that follow from the sample definitions. Overall, the evidence suggests that our main sample is broadly representative of mothers in the same cohorts.

III. Expected Effects of the 2014 Reform and Identification Strategy

Our objective is to identify the effect of pension wealth on the labor supply behavior of late-career workers. Before delving into the identification strategy and its empirical implementation, it is useful to consider the potential impact of the 2014 childcare pension benefit reform on mothers' labor supply decisions.

Consider two identical women giving birth to a child right before and right after January 1, 1992. As a consequence of the 1992 pension reform and the subsequent 1999 reform, the situation until July 2014 is the following: the mother who gave birth before January 1, 1992 receives one pension point for this birth, while the mother who gave birth on or after January 1, 1992 receives three points. The 2014 reform attributes one extra pension point to the first mother who gave birth before January 1, 1992, raising her points to two. In other words, the 2014 reform increased

[23] We do not exclude mothers who gave birth in the two weeks around the January 1, 1992 cutoff, since we observe the exact month of birth of children in the VSKT, thus not running the risk of mismeasurement.

the number of cumulated pension points of mothers of children born before the cutoff date by one (per child), while leaving the number of cumulated pension points of mothers of children born after that date unchanged. We thus assign mothers of children born before January 1, 1992 to the treatment group, and mothers of children born on January 1, 1992 or later to the control group.

Since the increase in pension points mechanically increases pension wealth, the reform generates a positive and permanent wealth effect for mothers of children born before January 1, 1992. This is a pure wealth effect, not confounded by changes in substitution incentives. The reason why the reform creates a pure wealth effect is that the accreditation of pension points is independent of current and future labor supply choices. It is also fully exogenous to fertility choices, since eligibility is uniquely based on the birth date of existing children, which obviously cannot be manipulated ex post. In addition, since future pension wealth does not count towards taxable income, the windfall gain generated by the reform does not alter the marginal tax rate faced by affected mothers during their working life. The accreditation of the additional pension point does not affect the qualifying period for pension eligibility under either the early retirement or the seniority pension scheme.[24] Importantly, the pension wealth effect stems from a change in pension points for a constant pension benefit formula. The stability of pension rules makes it likely that information or cognitive frictions are minimal. As a result, the reform provides useful variation to neatly identify the effect of pension wealth on labor supply decisions.

If leisure is a normal good, the increase in pension wealth is expected to induce individuals to reduce labor supply throughout their remaining lifetime, relative to a counterfactual scenario in which the 2014 reform did not take place. Labor supply reductions can occur at the intensive and/or the extensive margin, including via an anticipation of retirement. Clearly, we do not expect any labor supply response from individuals who had already retired by 2014, though the fraction of retired individuals among those who gave birth in 1992 is negligible.[25] Even though the increase in pension wealth could in principle induce individuals to anticipate retirement benefit claiming, this is a margin that we are unlikely to uncover since less than 5% of our sample is old enough to qualify for retirement benefits over the time period of analysis.[26]

Our objective is to identify the effect of pension wealth on labor supply behavior prior to retirement. The regression equation that describes the causal relationship of interest is:

$$Y_{it} = \alpha_0 + \alpha_1 W_{it} + X_{it}'\alpha_2 + \varepsilon_{it} \tag{1}$$

[24]As we explain in Appendix C.2, mothers are credited *consideration* periods for child care until the child turns 10. Such consideration periods do not increase pension entitlements (i.e. pension wealth), but count towards the qualifying period for eligibility for early retirement or seniority pension. On the other hand, *contribution* periods raise pension entitlements and count towards pension eligibility. The Mütterrente reform transformed the second year after childbirth for mothers of children born before 1992 from a consideration period to a contribution period, thus increasing pension wealth without affecting the overall qualifying period for retirement eligibility.

[25]In our VSKT sample, we see that around 2.7% of women who gave birth to their first child around January 1, 1992 had retired by December 31, 2013, primarily receiving pensions for (partial) disability.

[26]Appendix Figure A4 reports the age distribution of mothers in our main sample as of December 2013. Only mothers aged 57 and over in 2013 are potentially eligible for retirement benefits by the end of 2018 (subject to meeting other eligibility requirements).

where Y_{it} is the labor supply outcome of interest Y for individual i at time t, W_{it} is the present discounted value of pension wealth cumulated by i at time t, X_{it} represents a vector of individual controls, and ε_{it} is an error term. For $Y = z$, where z is labor earnings, the parameter α_1 identifies the wealth effect of retirement benefits. Given the potential endogeneity of W, we exploit exogenous variation in cumulated pension wealth due to the 2014 pension reform, and use the timing and eligibility requirements of said reform as an instrumental variable (IV) for pension wealth. In practice, we estimate the coefficient α_1 using a two-sample instrumental variables estimator, where, due to data availability, the first stage is estimated in the VSKT data and the reduced form in the IEB data.

To identify the reduced-form effect of the reform on labor supply, we implement a difference-in-differences design. We compare labor supply outcomes between mothers who had their first child before versus after January 1, 1992, and trace out the dynamics of this difference from before to after the 2014 reform. More formally, for each half-year period t running from the first six months of 2010 to the last six months of 2018, our reduced form is estimated using the following specification:

$$Y_{it} = \sum_{s \neq Jul-Dec\ 2013} \gamma_s \cdot D_i \cdot \mathbb{I}[t = s] + \delta_t + \delta_i + \nu_{it} \tag{2}$$

where Y_{it} is defined as above, D_i is an indicator taking value 1 if individual i's first child was born before January 1, 1992, and zero otherwise, δ_t and δ_i are calendar time and individual fixed effects, $\mathbb{I}[\cdot]$ is an indicator function and ν_{it} an error term. Standard errors are clustered at the individual level. The set of coefficients γ_s identifies the difference in outcomes between the treatment ($D_i = 1$) and control ($D_i = 0$) group in each half-year period relative to the July-December 2013 one, which we take as baseline as explained in Section B. The magnitude of the γ_s coefficients quantifies the reduced-form effect of the reform on outcome Y. When presenting the empirical estimates, we plot the set of estimated γ_s coefficients from January-June 2010 to July-December 2018, which allows to visually assess pre-policy trends and the dynamics of post-policy responses. We also report the estimated coefficients of a version of equation (2) in which we replace the first term with a set of interactions between D_i and indicators for 1-2.5 years after the baseline period and 3-5 years after the baseline period. We label the former as short-run effect and the latter as medium-run effect. Our two-sample instrumental variables estimates will be based on the medium-run reduced-form effect.

Our first stage effect is the change in the present discounted value of pension wealth generated by the 2014 reform. As we will explain in more detail below, we simulate the absolute and proportional increase in pension wealth using detailed data on pension contributions from the VSKT.

The key assumptions for the γ_s coefficients in equation (2) to be identified are a standard common trends assumption and the lack of contamination effects. The former requires that the evolution of labor supply outcomes of women with children born after January 1, 1992 (control group) offers a good counterfactual for that of mothers of children born before that date (treatment group). This assumption can be corroborated by the lack of pre-policy differential trends between the treatment and control groups. To strengthen the case for a compelling counterfactual, we focus on women giving birth to their first child in a three-month bandwidth around the January 1, 1992 cutoff. Concerning contamination effects, the imputation procedure to impute childbirth is prone to measurement error around the January 1, 1992 cutoff, implying that the treatment/control group classification may be incorrect in a narrow window around the threshold. As described above, to overcome this issue, we implement a doughnut difference-in-differences estimation in which we exclude women with imputed childbirths in a two-week window around the cutoff. One may also worry about general equilibrium effects: if treated individuals reduce their labor supply in response to the reform, this may affect equilibrium wages and tax rates. However, it seems reasonable to believe that these effects will be small and, in any case, identical for individuals in both treatment and control groups, who should be perfect substitutes in the labor market.

For our two-sample instrumental variables estimate to have a causal interpretation, we also require the instrument to be relevant and to satisfy the exclusion restriction. We document below that the reform generated a meaningful increase in the present discounted value of pension wealth of EUR 3,830 or 4.4% of total pension wealth as of December 2013. As for the exclusion restriction, we are not aware of other reforms taking place around the same time and with the same eligibility criteria. Moreover, we believe it is unlikely that the timing and eligibility requirements of the reform affect employment outcomes through channels other than pension wealth. To the extent that they may do so, for example through general equilibrium effects, we expect those to be identical across the treatment and control groups.

The main advantage of this empirical strategy is that it neatly identifies a pure wealth effect, which is typically hard to do using policy variation. Moreover, our difference-in-differences design allows us to account for the potential confounding effect on labor supply of (i) the 1992 Mütterrente reform, which gave more generous pension contributions to mothers of children born after January 1, 1992, and (ii) a 1992 maternity leave reform, which increased the duration of job-protected leave for those same mothers. The 1992 pension reform was legislated in 1989 and, as such, was anticipated by mothers giving birth in a window around the January 1, 1992 cutoff. Knowledge of future benefits might have induced mothers to time the birth date of their children in order to take advantage of the more generous regime. The 1992 maternity leave reform is, instead, likely not anticipated, since the draft bill for the reform was proposed after children born within three months of the policy reform were conceived. Panels A and B of Appendix Figure A5 show the density of first births and total births based on VSKT data. The graphs report parametric regression-discontinuity-design (RDD) estimates of the effect of the 1992 reforms on those outcomes, conditional on month of birth fixed effects. We do not detect any discontinuity in the density of first births and total births around the January 1, 1992 cutoff. This suggests that fertility choices did not respond to the 1992 reforms, neither among first-time mothers nor among mothers in general. Evidence from Schönberg and Ludsteck (2014) aligns with our findings in ruling out strategic fertility behavior. Nonetheless, by affecting the likelihood and timing of mothers returning to work after childbirth, both reforms are likely to have had an impact on the working history and, hence, on cumulated contributions for women with children born after January 1, 1992. The time difference of our design accounts for these potential effects.

IV. Effect of Pension Wealth on Labor Supply

A. Reduced-Form Effects

Figure 1 reports estimates of equation (2) for our main outcome of interest: total unconditional earnings. These are defined as the sum of labor income from dependent employment (full-time, part-time and marginal employment), and take value zero when individuals are not recorded as employed (e.g. because they are registered as job-seekers or because they are out of the labor force and hence not covered by the data) or receive unemployment benefits. As such, they provide an all-encompassing measure of employment at the extensive and intensive margin. Total unconditional earnings drop significantly for the treatment group after the reform. Our estimate of the short-run effect over the 2.5 years following the policy change is of a reduction of EUR 51 per half-year period, or 0.7% of the pre-policy mean in the treatment group (see the first row of Panel A of Table 1). Consistent with the notion that optimization frictions may limit the ability of individuals to adjust their labor supply in the short run, we estimate a larger medium-run effect in the 3-5 years after the reform, when the magnitude of the treatment effect appears to stabilize at around EUR 98 per half-year period, or 1.3% of the pre-policy mean.[27] The treatment leads are statistically insignificant, indicating that there are no differential pre-trends. Thus, we conclude that the positive pension wealth effect of the childcare pension benefit reform significantly reduces total unconditional earnings of the affected mothers.[28]

Appendix Table A4 reports estimates of the effect of the reform on total unconditional earnings for the sample of individuals employed in the baseline period in Panel A and the sample of the non-employed in Panel B. The table shows that the effect we detect on unconditional earnings in Figure 1 is entirely driven by individuals who are working before the reform and reduce their labor supply afterwards, rather than by fewer entries into the labor market following the reform.

Panels A to C of Figure 2 and Panels A and B of Table 1 uncover the anatomy of the labor supply response. As illustrated in Panel A of Figure 2, the reform has essentially no effect on the

[27] According to VSKT data – where we observe all children born to a woman – the youngest child of the average woman in our sample is 18.3 years old in December 2013. This suggests that childcare constraints should not play a role in labor supply adjustment.

[28] In principle, the reduced-form effect that we estimate for 2018 could encompass both the effect of the 2014 reform and some anticipated responses to the 2019 one. In practice, though, the stability of the estimated effects in 2017 and 2018, coupled with the lack of evidence of anticipated effects of the 2014 reform, leads us to conclude that such anticipated responses are likely minimal.

extensive margin, as measured by the probability of being employed. There is also no change in the probability of receiving unemployment benefits, nor of not appearing in the IEB records (because of non-employment, self-employment or employment in the civil service), as reported in Panel A of Table 1. Rather, individuals seem to respond at the intensive margin: Panel B of Figure 2 shows that labor earnings conditional on employment drop by 0.4% in the short run and 0.9% in the medium run. Whilst the data do not allow us to measure hours worked – and thus disentangle whether the drop in conditional earnings is due to a change in hours or wages – the estimates in Panel C document a drop in the likelihood of working full-time (as opposed to working part-time or in marginal employment) conditional on employment in the aftermath of the reform. Although less precisely estimated, the coefficients reported in Panel C indicate a drop in full-time employment of 1.1% in the short run and of 1.8% in the medium run. In Panel B of Table 1, we can see positive, but insignificant increases in the probability of working part-time and, to a smaller extent, of being in marginal employment in the medium run – effects that are also visible in Panels A and B of Appendix Figure A6. These results suggest that affected mothers reduce their hours of work in response to the positive pension wealth shock.

Finally, we investigate whether individuals adjust their labor supply by switching employer or type of job. As reported in Panel C of Table 1, we do not find evidence of an increase in the probability of switching establishment, nor in the likelihood of being employed in a part-time intensive occupation, that is an occupation with above-median share of employees working part-time in the pre-reform period.[29] In practice, though, the probability of working in a part-time intensive occupation is close to 90% at baseline, indicating that the vast majority of mothers already work in occupations that are suitable for part-time arrangements.

By reducing labor supply in response to the reform, mothers are implicitly reducing the amount of contributions paid into the system. We quantify the pension point loss associated with the unconditional earnings response, assuming that the unconditional earnings reduction remains stable at EUR 196 per year for the 12 years that separate the average individual from the full retirement age after our period of analysis. The pension point loss amounts to a total of 0.09 points over the entire post-reform period, which corresponds to approximately 1/11 of the childcare pension point increase due to the reform.[30]

Comparison of extensive-margin employment effects with estimates in Becker et al. (2022)
In contemporaneous work, Becker et al. (2022) use VSKT data to analyze the effect of the 2014

[29]To determine the pre-reform share of part-time employees in an occupation, we use a representative 2% sample of the IEB and only consider employment spells that include December 31, 2013 (Institut für Arbeitsmarkt- und Berufsforschung, 2021*a*). In case a worker has more than one relevant spell in an occupation, we keep the one with the highest working time. For each of our 127 occupations, we then calculate the fraction of part-time workers as the share of part-time and marginal employees among all employees.

[30]This ratio does not correspond to the marginal propensity to earn that we calculate below, since the latter scales earnings changes by the discounted present value of the pension wealth shock. While pension points accrue approximately proportionally with contemporaneous earnings below the contribution ceiling, pension wealth reflects the annuitized and discounted value of those points. Hence, the implied reduction in pension wealth from lower earnings is not a simple multiple of the pension point loss.

Mütterrente reform on the probability of employment. In their preferred specification, they compare mothers who had *any* child in 1990 or 1991, to mothers who had their *first* child in 1992 or 1993. This specification leads to a reduced form effect of -0.011 with a standard error of 0.004, i.e. points to a statistically significant reduction of 1.1 percentage points in the likelihood of employment. Our estimate of the effect of the reform on the probability of employment is of -0.002 with a standard error of 0.002. Two elements may be responsible for the differing headline estimates in the two papers: (i) the definition of treatment (any versus first child before 1992), (ii) the childbirth bandwidth around the January 1, 1992 cutoff (two years versus three months). In addition, (iii) IEB data having a larger sample size (100% of the population versus the 2% in VSKT data) is likely responsible for the improved precision of our estimates. To understand what the main drivers of the differing findings are, we compile a set of estimates of the reduced form effect of the policy on the probability of employment, starting from Becker et al. (2022)'s preferred specification and altering one specification or sample parameter at a time, until we get to our main specification. We provide a detailed illustration of this exercise in Appendix D and summarize here the main conclusions. Comparing estimates across specifications reveals that (i) the definition of treatment (any versus first child before 1992) and (ii) the childbirth bandwidth are both significant drivers of the difference between the two estimates. In fact, even when looking at Becker et al. (2022)'s results only, moving from their preferred specification (coefficient of -0.011 with a standard error of 0.004) to a specification comparing women who had their *first* child in a two year bandwidth around the cutoff halves the point estimate, which becomes insignificant. Reducing the bandwidth around the January 1, 1992 cutoff to twelve months further halves the point estimate (coefficient of -0.003 with a standard error of 0.006), making it very close to ours. Having access to the full population allows us to achieve greater precision. If we compare the latter estimate from Becker et al. (2022) to our headline one, ours is substantially more accurate: we can rule out extensive margin effects more negative than -0.006, while Becker et al. (2022) can only rule out effects more negative than -0.015.

B. Implied Marginal Propensity to Earn out of Pension Wealth

To interpret the magnitude of the reduced-form estimates illustrated in the previous subsection, we rescale the estimated earnings response by the simulated pension wealth shock, in the spirit of two-sample instrumental variables. To simulate the pension wealth shock, we resort to the VSKT data. We describe our simulation in detail in Appendix C.1. Based on a monthly discount factor of 0.9983, assumed retirement at age 67 and an expected retirement period of 20 years (240 months), as well as an estimated monthly growth rate of the real value of a pension point of -0.0001619, we calculate that the present discounted value of pension wealth for mothers in the treatment group increases by EUR 3,830 as of December 2013, corresponding to a 4.4% increase in the present discounted value of their pension wealth.

To obtain measures of the wealth effect and the wealth elasticity that are plausibly not attenuated by optimization frictions, we consider the medium-run estimates of the unconditional earnings response reported in the first row of Panel A of Table 1. Total unconditional earnings dropped by EUR 196 per year or 1.3% of their pre-policy level. For an increase in the present discounted value of pension wealth of EUR 3,830, or 4.4%, the implied wealth effect is of EUR -5.1 per year per EUR 100 of wealth, and the implied wealth elasticity is of -0.3.

The size of the pension wealth effect that we estimate above can be hard to assess, since it does not account for the time horizon over which individuals plausibly allocate the increase in pension wealth. In order to obtain an estimate of the marginal propensity to earn (MPE) out of pension wealth, we first calculate the allocation of the pension wealth increase over time, following the annuitization method described in Golosov et al. (2024). We assume that individuals perfectly smooth the pension wealth increase over the years that separate them from the full retirement age, which correspond to 17 years on average in our sample. Under this assumption, individuals allocate 29% (5/17) or EUR 1,126 of the windfall to the 5 years covered by our analysis. Our estimates of the unconditional earnings response from the first row of Panel A of Table 1 imply that, over the 5 years after the reform, earned income drops cumulatively by EUR 612 on average, discounting the per-period changes to December 2013.[31] Based on these computations, we estimate an MPE of -0.54 ($= 612/1,126$): an extra euro of pension wealth leads to a 54 cent reduction in labor earnings in middle-age years. The size and salience of the economic incentives generated by the pension reform could both be important for the magnitude of the response that we document (Seibold, 2021). For statistical inference, we use a bootstrap procedure with 1,000 replications, where we allow for clustering at the individual level. We obtain a bootstrapped standard error of 0.18.[32,33]

We can compare the magnitude of our MPE to other recent estimates of labor supply responses to wealth shocks within the domain of retirement benefits, in an attempt to reconcile the results in this paper with prior literature. Compared to our estimates, existing work identifies responses to contemporaneous rather than future wealth changes. Ceteris paribus, we would expect responses to contemporaneous benefit changes to exceed those to future changes if individuals are myopic or face liquidity constraints. Consistent with this reasoning, in Appendix E, we show that mothers who are likely to face liquidity constraints respond less strongly to the reform. In the context of retirement benefits, Gelber, Isen and Song (2016) and Gelber, Isen and Song (2017) find MPEs of -0.6 for men and -0.9 for women in response to changes in Old Age

[31] For discounting, we use a monthly discount factor of 0.9983. This is the same discount factor used to simulate the pension wealth change in Appendix C.1.

[32] We rely on the IEB data to obtain 1,000 bootstrapped samples, based on random draws with replacement of individuals from our main sample. We bootstrap all steps in our estimation of the MPE, from the reduced-form estimate of the total unconditional earnings response (the numerator of the MPE) to the simulation of the present discounted value of the pension wealth change (the denominator of the MPE). Only two factors entering the simulation of the pension wealth change may vary across bootstrapped samples: (i) the average age of mothers and (ii) the mechanical change in pension points due to the reform, which may vary depending on the occurrence of multiple births. We account for (i) directly, using the average age of mothers in each bootstrapped sample. Since we do not observe multiple births in the IEB data, we assume that the change in pension points is fixed at 1.03, as calculated in VSKT.

[33] The precision of our estimate is comparable to that in Gelber, Isen and Song (2016), who estimate an MPE of -0.6 with a standard error of 0.17. This is the most precisely estimated of the effects that we report in the next paragraph.

and Survivors Insurance benefits in the United States.[34] Those estimates are based on responses to benefit changes occurring when affected individuals are on the cusp of retirement, and thus reflect responses to contemporaneous wealth changes. Large contemporaneous wealth effects have similarly been documented in the context of survivor insurance. Giupponi (2024) finds an MPE of -0.8 among surviving spouses in Italy, while Coyne et al. (2024) of -0.3 in the United States. The latter, though, captures the short-run response to an anticipated income change, which could explain the smaller effect in absolute levels. Overall, the existing evidence points to larger responses to contemporaneous rather than future wealth changes. This finding is in line with evidence in French et al. (2022) that individuals are more responsive to changes in contemporaneous substitution incentives than to changes in the link between current social security contributions and future pension benefits.

The labor supply responses that we document are concentrated along the intensive margin. Studies that examine pension wealth changes closer to the retirement age (and disentangle margins of adjustment) tend to find, instead, responses along the extensive margin (Gelber, Isen and Song, 2016; Becker et al., 2022).[35] Our evidence of intensive margin responses is in line with the findings in Deshpande (2016), who documents large intensive margin responses by parents to the income effect induced by the loss of Supplemental Security Income payments for their children. Taken together, this evidence points towards age patterns in the use of different margins of adjustment, which could be explained by varying (utility) costs of adjusting labor supply over the lifecycle.

C. Identification Tests and Robustness Checks

Our difference-in-differences strategy rests on the 'common trends' assumption that the labor supply outcomes of treated and control individuals would have evolved in parallel absent the 2014 reform. As we noted in Section A, not finding differential earnings and employment changes between the treatment and control groups in the pre-policy period is reassuring in this respect. As a further check, we run a battery of placebo tests in Panels A and B of Appendix Table A5, where we use – respectively – January 1, 1994, and January 1, 1995, as placebo thresholds to define (placebo) treatment and control groups.[36] The tables report estimates of equation (2) for our main outcomes of interest. Our placebo tests show no significant differences between treatment and control groups, suggesting that our main estimates are picking up a genuine causal effect of the reform.

As discussed in Section I, the January 1, 1992 threshold is not only relevant for the 2014 Mütterrente reform, but also for (i) the 1992 Mütterrente reform, which gave additional pension

[34]It is worth noting that the estimate in Gelber, Isen and Song (2017) is based on a selected sample of women born in 1916-1917 who had particularly high lifetime earnings relative to their husbands.

[35]Gelber, Isen and Song (2016) find that the probability of being employed decreases by -0.67 (standard error 0.18) percentage points per 10,000 dollars of additional pension wealth. For an equivalent pension wealth shock, our estimates imply an employment effect of -0.005 (standard error 0.005), and thus allow us to rule out effects lower than -0.015.

[36]We omit January 1, 1993, since a maternity leave reform was implemented on that date.

points per child born on or after January 1, 1992, and (ii) a maternity leave reform, which doubled the duration of job-protected leave for mothers of children born on or after January 1, 1992. One may worry that differential fertility responses to those 1992 reforms might make our treatment and control groups different with respect to their childcare constraints as of 2014 and, more generally, their life-cycle labor supply trajectories. In Panel C of Appendix Figure A5, we use data from the VSKT to document that the combination of the 1992 reforms did not affect the total fertility of women in our sample. The gray dots plot the average number of children born to mothers who had their first child in a given month around the January 1, 1992 cutoff, conditional on mother's cohort fixed effects and child's month of birth fixed effects. The number of children is computed as of December 2019, that is approximately 28 years after the first childbirth, and can therefore be considered a close proxy for total fertility. We estimate a very small and statistically insignificant discontinuity at the threshold.

To account for measurement error in childbirth imputation, our main doughnut difference-in-differences specification excludes women with imputed childbirths in a two-week window around the cutoff. We assess the robustness of our estimates to different doughnut-hole widths (zero, four and six weeks) in Appendix Table A6. Point estimates are stable across specifications.

As a final robustness check, we consider the soundness of conditioning some of our main outcomes – earnings and the probability of working full-time – on employment. In general, conditioning on an outcome may raise concerns of endogenous selection due to compositional changes. In our specific case, the lack of employment effects of the reform suggests that endogenous selection should not be a concern. To further probe this point, we turn to Panel A of Appendix Table A4, which replicates our main analysis on the sample of mothers who were already in employment in the second half of 2013. Albeit imperfectly, conditioning on pre-reform employment implicitly allows us to isolate the effect of the reform on earnings (and the likelihood of working full-time) from that of potential compositional changes due to extensive-margin responses to the reform itself. Results are very similar to the ones for the baseline sample in the paper, suggesting that endogenous sample selection is not confounding our estimates.

D. Evidence on Heterogeneous Effects

The observed response to a change in pension wealth may vary across individuals depending on a range of factors, such as their idiosyncratic returns to tenure and costs of working, their liquidity constraints, or their ability to self-insure against the longevity risk through their savings. We study heterogeneous responses to the pension wealth effect along these and other dimensions. We summarize our main findings here and refer the reader to Appendix E for more details. We acknowledge that our heterogeneity analyses often lead to imprecise estimates and should, therefore, be interpreted with caution.

First, we consider whether individuals with different labor market returns to tenure and costs of working respond differently to the wealth shock. Consistent with forward-looking behavior, individuals with higher predicted returns to tenure are less likely to leave full-time employment. On the other hand, work disutility – measured by how physically demanding an individual's occupation is – does not seem to matter substantially for the size and nature of the response. Second, we show that the intensive margin response is stronger among those who, within their birth cohort, have higher pre-reform pension wealth, which can be viewed as a proxy for the income an individual expects to rely upon once retired. Third, we show that labor supply responses are muted among those who are likely to face stronger liquidity constraints, as proxied by their partner's pre-reform labor earnings. Finally, we do not find evidence of differential effects by age at the time of the reform.

E. Evidence on Intra-Household Spillover Effects

For approximately 40% of our sample, we can match mothers to their spouses. Thanks to the possibility of jointly observing their working histories, we can explore the presence of behavioral spillovers across spouses. We detail our analysis of intra-household spillovers in Appendix F and summarize our findings here. We find some evidence that male spouses also reduce their labor supply along the intensive margin, with effects that become quantitatively more pronounced in the medium run, are approximately half in magnitude compared to those of women, but are not always precisely estimated due to the small sample size. We also find that, qualitatively, the labor supply responses of women are amplified when their partner is older and thus closer to the full retirement age. These results are large in magnitude but not significant. Both these pieces of evidence are consistent with some degree of within-household interaction in labor supply choices (Goux, Maurin and Petrongolo, 2014; Carta and De Philippis, 2023).

V. Welfare Implications

The results show that the Mütterrente reform caused a substantial drop in labor income among mothers and, albeit smaller and less significant, their partners. We now turn to consider the implications of these results for social welfare using Hendren (2016)'s policy elasticity approach. This approach uses the causal impact of the behavioral response to a given policy change on net government spending as a sufficient statistics for the welfare impact of that policy change.

An important implication of our results is that the true government cost of the reform is higher than its mechanical cost. Let us first focus on mothers' labor supply responses. Our estimates indicate that increasing pension wealth by EUR 3,830 decreases labor earnings by EUR 101 per year in the short run and EUR 196 per year in the medium run. If we are willing to assume that our estimates of the medium-run effect of the policy on earnings hold for all years until full retirement age, our estimates imply that increasing pension wealth by EUR 3,830 decreases labor earnings by EUR 2,660 in present discounted value. Assuming that labor is the only source of taxable income, tax simulations indicate that the average mother in our sample faces a 42% marginal income tax and social security contribution rate when filing taxes individually and of 47.5% when filing taxes jointly with her spouse. In the 2014 German Microcensus, approximately 21.5% of mothers are not married and thus file individually. Virtually all married couples file jointly. For an average 46.3% marginal income tax and social security contribution rate, the EUR 3,830 increase in pension wealth has a net impact on government expenditures of EUR 5,062 (= EUR 3,830 + EUR 2,660 $\times$ 0.463 = EUR 3,830 + EUR 1,232). Equivalently, the policy generates a fiscal externality (FE) of 32 cents per euro of transfer (= EUR 1,232/EUR 3,830).

Even though we acknowledge that spousal earnings responses are rather imprecisely estimated (see Panel B of Appendix Table F1), we provide here calculations of the fiscal externality accounting for their impact on the government budget. If we consider spousal earnings responses, the total net impact of the reform on government expenditures amounts to EUR 5,541. This is equal to the sum of the mechanical increase in pension wealth due to the policy change (EUR 3,830), the reduction in income tax and social security contribution paid by mothers (EUR 1,232 = EUR 2,660 $\times$ 0.463) and the reduction in income tax and social security contribution paid by their married partners on the estimated present discounted value change in their labor earnings (EUR 372 = EUR 997 $\times$ 0.475 $\times$ 0.785).[37] As before, the effect of the reform on the present discounted value of spousal labor earnings is computed assuming that our estimates of the partners' medium-run earnings response to the policy holds for all years until full retirement age. The above calculations imply that, accounting for spousal labor supply responses, the fiscal externality of the policy amounts to 42 cents per euro of transfer (= EUR 1,604/EUR 3,830).

We can use our estimate of the fiscal externality to assess the welfare implications of the reform using the marginal value of public funds (MVPF). The MVPF is the ratio of the beneficiaries' willingness to pay for the policy (WTP) to its net government cost (1+FE), and quantifies the amount of welfare delivered to policy recipients per euro of government spending on the policy. Inferring the willingness to pay for future benefits would require knowledge of how individuals discount future well-being and – for household-level valuations – knowledge of how resources are allocated across spouses. We take a conservative stance and assume that the willingness to pay for the increase in pension wealth is determined by the mechanical cost of that increase for the government. In other words, we assume that individuals have a willingness to pay of EUR 1 for a EUR 1 increase in the net present value of pension wealth. The MVPF of the Mütterrente reform is thus $\frac{1}{1+\text{FE}} = \frac{1}{1+0.32} = 0.76$ when considering mothers' labor supply responses only, and of $\frac{1}{1+0.42} = 0.70$ when also accounting for spousal responses.

To gauge the magnitude of these values, we compare them to the MVPFs for analogous policies reported in Hendren and Sprung-Keyser (2020) and Policy Impacts (n.d.), which provide a

[37] Note that here we account for the fact that only 78.5% of mothers are married.

living collection of MVPFs from a broad range of government programs. Therein, the average MVPF for retirement benefits is 0.8 and for disability insurance for adults 0.86. Since those MVPFs do not account for spousal responses, to ensure comparability, we consider the MVPF of the Mütterrente reform as implied by the mothers' response only. In addition, the MVPFs for retirement and disability insurance benefits in the Policy Impacts library are all based on a willingness to pay of 1, which further guarantees comparability with our estimate. The value of 0.76 that we find is in line with those found for similar programs. Taken together, these programs have MVPFs below 1 due to the earnings crowd-out that they entail. Welfare weights on pension recipients would have to be substantial to justify the Mütterrente benefit increase, compared to alternative policies with larger MVPFs.

VI. Conclusion

In this paper, we study how changes in the generosity of public pension systems affect the labor supply behavior of late-career workers. We exploit the 2014 reform of the Mütterrente, which increased the pension wealth of mothers of children born before January 1, 1992 by 4.4% per child on average. We document significant reductions in labor earnings when affected women are on average 50-55 years old, driven by shifts out of full-time employment. Our estimates imply a marginal propensity to earn out of pension wealth of -0.54, which can be considered a moderately large effect. Our findings contribute to a growing strand of the literature that analyzes how the design of pension benefits affects labor supply far from retirement. In particular, our setting allows us to overcome several empirical challenges in the identification of forward-looking pension wealth effects.

Our results have important policy implications. Our evidence of forward-looking behavior implies that pension reforms can have aggregate labor supply effects well beyond the direct impact on individuals on the verge of retirement. An interesting avenue for future research lies in integrating this and other recent empirical evidence on forward-looking responses to pension design changes in a cohesive, structural framework. This could potentially shed light on the relative effectiveness of different policy levers – a tightening of the contribution-benefit link, a reduction in the overall generosity of the system, and an increase of statutory retirement ages – in encouraging labor supply far from retirement and, thus, enhancing the fiscal sustainability of social security systems.

References

Attanasio, Orazio P., and Agar Brugiavini. 2003. "Social Security and Households' Saving." *The Quarterly Journal of Economics*, 118(3): 1075–1119.

Attanasio, Orazio P., and Susann Rohwedder. 2003. "Pension Wealth and Household Saving: Evidence from Pension Reforms in the United Kingdom." *American Economic Review*, 93(5): 1499–1521.

Becker, Sebastian, Hermann Buslei, Johannes Geyer, and Peter Haan. 2022. "The Effect of Pension Wealth on Employment." Institute of Labor Economics (IZA) IZA Discussion Papers 15836.

Blundell, R., E. French, and G. Tetlow. 2016. "Retirement Incentives and Labor Supply." In *Handbook of the Economics of Population Aging*. Vol. 1 of *Handbook of the Economics of Population Aging*, , ed. John Piggott and Alan Woodland, Chapter 0, 457–566. Elsevier.

Bottazzi, Renata, Tullio Jappelli, and Mario Padula. 2006. "Retirement expectations, pension reforms, and their impact on private wealth accumulation." *Journal of Public Economics*, 90(12): 2187–2212.

Bovini, Giulia. 2019. "Life-Cycle Labor Supply and Social Security Wealth: Evidence from the 1995 Italian Pension Reform." London School of Economics and Political Science Working Paper.

Bächmann, Ann-Christin, Corinna Frodermann, Benjamin Lochner, Michael Oberfichtner, and Simon Trenkle. 2021. "Identifying Couples in Administrative Data for the Years 2001–2014." FDZ FDZ-Methodenreport 03/2021 (en).

Börsch-Supan, Axel, Michela Coppola, and Johannes Rausch. 2015. "Die „Rente mit 63": Wer sind die Begünstigten?" *Perspektiven der Wirtschaftspolitik*, 16(3): 264–288.

Carta, Francesca, and Marta De Philippis. 2023. "The Forward-Looking Effect of Increasing the Full Retirement Age." *The Economic Journal*, 134(657): 165–192.

Cesarini, David, Erik Lindqvist, Matthew J. Notowidigdo, and Robert Östling. 2017. "The Effect of Wealth on Individual and Household Labor Supply: Evidence from Swedish Lotteries." *American Economic Review*, 107(12): 3917–3946.

Chetty, Raj. 2004. "Optimal Unemployment Insurance When Income Effects are Large." National Bureau of Economic Research Working Paper 10500.

Chetty, Raj. 2008. "Moral Hazard versus Liquidity and Optimal Unemployment Insurance." *Journal of Political Economy*, 116(2): 173–234.

Collischon, Matthias, Daniel Kuehnle, and Michael Oberfichtner. 2024. "Who Benefits from Cash-for-Care?" *Journal of Human Resources*, 59(4): 1011–1051.

Costa, Dora L. 1997. "Displacing the Family: Union Army Pensions and Elderly Living Arrangements." *Journal of Political Economy*, 105(6): 1269–1292.

Coyne, David, Itzik Fadlon, Shanthi P. Ramnath, and Patricia K. Tong. 2024. "Household Labor Supply and the Value of Social Security Survivors Benefits." *American Economic Review*, 114(1): 1248–1280.

Danzer, Alexander M. 2013. "Benefit Generosity and the Income Effect on Labour Supply:

Quasi-Experimental Evidence." *The Economic Journal*, 123(571): 1059–1084.

Deshpande, Manasi. 2016. "The Effect of Disability Payments on Household Earnings and Income: Evidence from the SSI Children's Program." *The Review of Economics and Statistics*, 98(4): 638–654.

Deutsche Rentenversicherung Bund. 2014. "Rentenbestand am 31.12.2014." Deutsche Rentenversicherung Bund, Berlin.

Deutsche Rentenversicherung Bund. 2020. "Rentenversicherung in Zeitreihen." Deutsche Rentenversicherung Bund, Berlin.

Deutsche Rentenversicherung Bund. 2022. "Rentenversicherung in Zahlen 2022." Deutsche Rentenversicherung Bund, Berlin.

DIW Berlin. 2023. "Socio-Economic Panel (SOEP), data for years 1984–2021, SOEP-Core v38, EU Edition." *https://doi.org/10.5684/soep.core.v38eu*.

Eurostat. 2024. "ISCO-08 Classification Data, Version 2.3." *TSV file downloaded from the Eurostat Data Browser https://ec.europa.eu/eurostat/databrowser/*, File name: ESTAT ISCO08 2.3.tsv.

Factiva. 2022. "Factiva Database." *https://www.dowjones.com/professional/factiva/*.

Fetter, Daniel K., and Lee M. Lockwood. 2018. "Government Old-Age Support and Labor Supply: Evidence from the Old Age Assistance Program." *American Economic Review*, 108(8): 2174–2211.

Forschungsdatenzentrum der Rentenversicherung. 2022. "Versicherungskontenstichprobe 2019 (Version 1.0)."

French, Eric. 2005. "The Effects of Health, Wealth, and Wages on Labour Supply and Retirement Behaviour." *Review of Economic Studies*, 72(2): 395–427.

French, Eric, Attila S Lindner, Cormac O'Dea, and Tom A Zawisza. 2022. "Labor Supply and the Pension-Contribution Link." National Bureau of Economic Research Working Paper 30184.

Gelber, Alexander, Adam Isen, and Jae Song. 2016. "The Effect of Pension Income on Elderly Earnings: Evidence from Social Security and Full Population Data." Goldman School of Public Policy Working Paper.

Gelber, Alexander, Adam Isen, and Jae Song. 2017. "The Role of Social Security Benefits in the Initial Increase of Older Women's Employment: Evidence from the Social Security Notch." *Women Working Longer: Increased Employment at Older Ages*, 239–268. University of Chicago Press.

Gelber, Alexander M., Damon Jones, and Daniel W. Sacks. 2020. "Estimating Adjustment Frictions Using Nonlinear Budget Sets: Method and Evidence from the Earnings Test." *American Economic Journal: Applied Economics*, 12(1): 1–31.

Giupponi, Giulia. 2024. "When Income Effects are Large: Labor Supply Responses and the Value of Welfare Transfers." Bocconi University Working Paper.

Giupponi, Giulia, and Arthur Seibold. 2024. *Rethinking Pension Reform.* CEPR Press.

Golosov, Mikhail, Michael Graber, Magne Mogstad, and David Novgorodsky. 2024. "How Americans Respond to Idiosyncratic and Exogenous Changes in Household Wealth and

Unearned Income." *The Quarterly Journal of Economics*, 139(2): 1321–1395.

Goux, Dominique, Eric Maurin, and Barbara Petrongolo. 2014. "Worktime Regulations and Spousal Labor Supply." *American Economic Review*, 104(1): 252–76.

Gruber, Jonathan, and David Wise. 1999. *Social Security and Retirement around the World.* National Bureau of Economic Research, Inc.

Hairault, Jean-Olivier, Thepthida Sopraseuth, and François Langot. 2010. "Distance to Retirement and Older Workers' Employment: The Case for Delaying the Retirement Age." *Journal of the European Economic Association*, 8(5): 1034–1076.

Hendren, Nathaniel. 2016. "The Policy Elasticity." *Tax Policy and the Economy*, 30(1): 51–89.

Hendren, Nathaniel, and Ben Sprung-Keyser. 2020. "A Unified Welfare Analysis of Government Policies." *Quarterly Journal of Economics*, 135(3): 1209–1318.

Illing, Hannah, Johannes Schmieder, and Simon Trenkle. 2024. "The Gender Gap in Earnings Losses After Job Displacement." *Journal of the European Economic Association*, jvae019.

Imbens, Guido W., Donald B. Rubin, and Bruce I. Sacerdote. 2001. "Estimating the Effect of Unearned Income on Labor Earnings, Savings, and Consumption: Evidence from a Survey of Lottery Players." *American Economic Review*, 91(4): 778–794.

Institut für Arbeitsmarkt- und Berufsforschung. 2021*a*. "2%-Stichprobe der Integrierten Erwerbsbiografien (IEB) stp2pct_V15_00_00_201912."

Institut für Arbeitsmarkt- und Berufsforschung. 2021*b*. "Integrierte Erwerbsbiografien (IEB) V15_00_00_201912."

Institut für Arbeitsmarkt- und Berufsforschung. 2021*c*. "Paardatensatz."

Jones, Damon, and Ioana Marinescu. 2022. "The Labor Market Impacts of Universal and Permanent Cash Transfers: Evidence from the Alaska Permanent Fund." *American Economic Journal: Economic Policy*, 14(2): 315–40.

Kolsrud, Jonas, Camille Landais, Daniel Reck, and Johannes Spinnewijn. 2024. "Retirement Consumption and Pension Design." *American Economic Review*, 114(1): 89–133.

Krueger, Alan B., and Bruce D. Meyer. 2002. "Chapter 33 Labor supply effects of social insurance." In . Vol. 4 of *Handbook of Public Economics*, 2327–2392. Elsevier.

Lachowska, Marta, and Michał Myck. 2018. "The Effect of Public Pension Wealth on Saving and Expenditure." *American Economic Journal: Economic Policy*, 10(3): 284–308.

Liebman, Jeffrey B., Erzo F.P. Luttmer, and David G. Seif. 2009. "Labor supply responses to marginal Social Security benefits: Evidence from discontinuities." *Journal of Public Economics*, 93(11): 1208–1223.

Müller, Dana, and Katharina Strauch. 2017. "Identifying Mothers in Administrative Data." FDZ-Methodenreport 13/2017 (en).

Picchio, Matteo, Sigrid Suetens, and Jan C. van Ours. 2018. "Labour Supply Effects of Winning a Lottery." *The Economic Journal*, 128(611): 1700–1729.

Policy Impacts. n.d.. "The Policy Impacts Library." https://policyimpacts.org/, Accessed April 6 2023.

Schönberg, Uta. 2009. "Does the IAB Employment Sample Reliably Identify Maternity Leave Taking? A Data Report." *Journal for Labour Market Research*, 42: 49–70.

Schönberg, Uta, and Johannes Ludsteck. 2014. "Expansions in Maternity Leave Coverage and Mothers' Labor Market Outcomes after Childbirth." *Journal of Labor Economics*, 32(3): 469–505.

Seibold, Arthur. 2021. "Reference Points for Retirement Behavior: Evidence from German Pension Discontinuities." *American Economic Review*, 111(4): 1126–65.

Statistik der Bundesagentur für Arbeit. 2011. "Umsteigeschlüssel von der Klassifikation der Berufe 2010 (5-Steller) zur ISCO-08 (4-Steller)." Nürnberg.

Statistisches Bundesamt. 1991. "Statistisches Jahrbuch 1991." Statistisches Bundesamt, Wiesbaden.

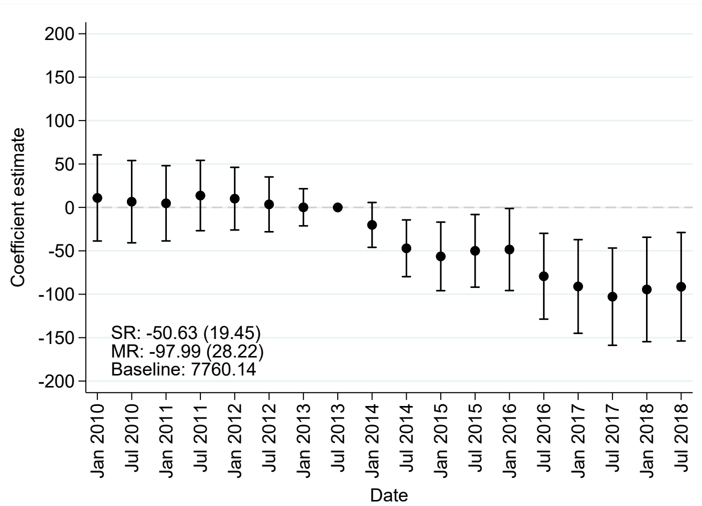
Statistisches Bundesamt. 1992. "Statistisches Jahrbuch 1992." Statistisches Bundesamt, Wiesbaden.

Thiemann, Andreas. 2015. "Pension Wealth and Maternal Employment: Evidence from a Reform of the German Child Care Pension Benefit." DIW Berlin, German Institute for Economic Research Discussion Papers of DIW Berlin 1499.

Ye, Han. 2022. "The Effect of Pension Subsidies on the Retirement Timing of Older Women." *Journal of the European Economic Association*, 20(3): 1048–1094.

Figures

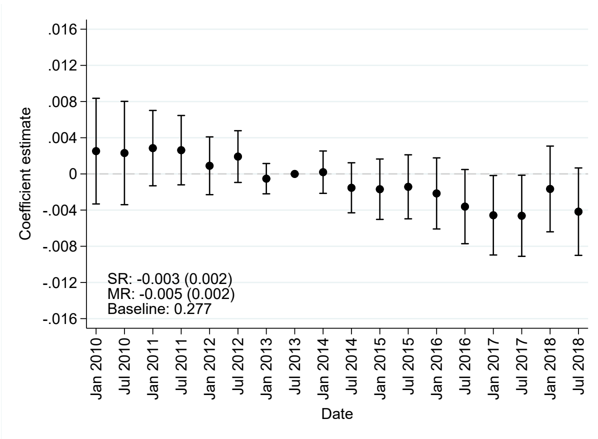
Figure 1. EFFECT OF 2014 MÜTTERRENTE REFORM ON TOTAL EARNINGS UNCONDITIONAL ON EMPLOYMENT



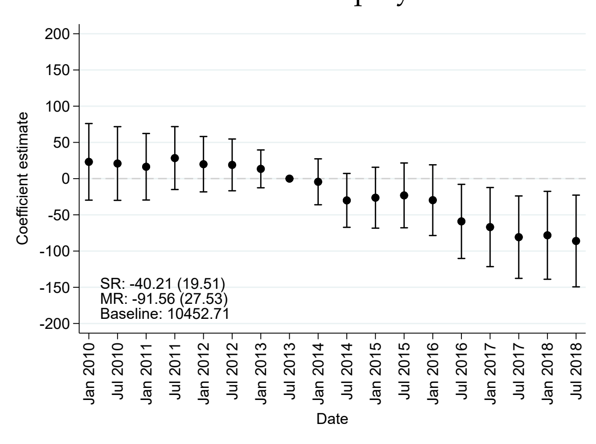
Notes: The graph reports estimates of the coefficients γ_s from equation (2). The dots represent the point estimates and the capped vertical bars 95% confidence intervals based on robust standard errors clustered at the individual level. The graph also reports estimates of the short-run effect over the 1-2.5 years after the 2014 reform (SR) and of the medium-run effect over the 3-5 years after the reform (MR), with robust standard errors clustered at the individual level in parentheses. The baseline statistic is the average value of the outcome in the treatment group in July-December 2013.

Figure 2. EFFECT OF 2014 MÜTTERRENTE REFORM ON LABOR SUPPLY OUTCOMES

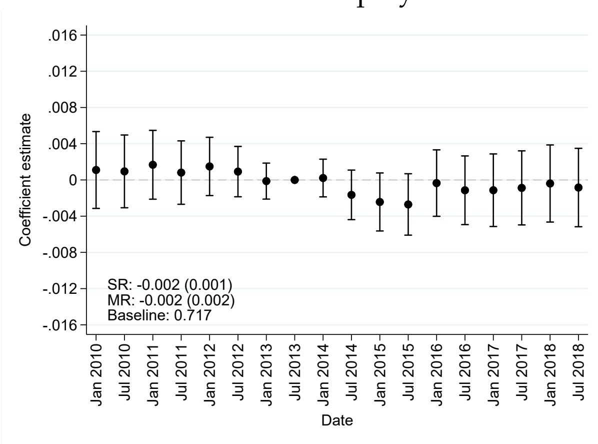
A. Probability of employment



B. Total earnings conditional on employment



C. Probability of working full-time conditional on employment



Notes: The graphs report estimates of the coefficients γ_s from equation (2). The dots represent the point estimates and the capped vertical bars 95% confidence intervals based on robust standard errors clustered at the individual level. Each graph also reports estimates of the short-run effect over the 1-2.5 years after the 2014 reform (SR) and of the medium-run effect over the 3-5 years after the reform (MR), with robust standard errors clustered at the individual level in parentheses. The baseline statistic is the average value of the outcome in the treatment group in July-December 2013.

Tables

Table 1. MAIN RESULTS

	T × Post (yr 1-2.5) (1)	T × Post (yr 3-5) (2)	Mean of dependent variable (3)	Number of individuals (4)
Panel A. Unconditional earnings and labor market status				
Unconditional earnings	-50.63	-97.99	7,760.14	99,104
	(19.45)	(28.22)		
Prob. of employment	-0.002	-0.002	0.717	99,104
	(0.001)	(0.002)		
Prob. of UI receipt/job-seeker status	-0.000	-0.001	0.053	99,104
	(0.001)	(0.001)		
Prob. of not being in IEB data	0.002	0.002	0.230	99,104
	(0.001)	(0.002)		
Panel B. Earnings and working time conditional on employment				
Conditional earnings	-40.21	-91.56	10,452.71	82,876
	(19.51)	(27.53)		
Cond. prob. of working full-time	-0.003	-0.005	0.277	82,876
	(0.002)	(0.002)		
Cond. prob. of working part-time	0.002	0.004	0.506	82,876
	(0.002)	(0.003)		
Cond. prob. of marginal employment	-0.001	0.002	0.182	82,876
	(0.001)	(0.002)		
Panel C. Other outcomes				
Prob. of switching establishment	0.000	0.001	0.052	82,250
	(0.001)	(0.001)		
Cond. prob. of being employed in part-time intensive occupation	-0.000	0.001	0.898	82,986
	(0.001)	(0.001)		

Notes: The table reports estimates of the coefficients γ_s from equation (2) for post-reform years and for a set of different outcomes. The coefficients γ_s are estimated pooling post-reform years together. Column 1 reports estimates of the short-run effect over the 1-2.5 years after the 2014 reform (SR) and column 2 of the medium-run effect over the 3-5 years after the reform (MR), with robust standard errors clustered at the individual level in parentheses. Column 3 reports the average value of the outcome in the treatment group in July-December 2013. Part-time intensive occupations are defined as occupations with above-median share of part-timers in the German economy.