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Time to care? The effects of retirement on informal care provision[☆]

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ABSTRACT

This paper analyzes the impact of women's retirement on their informal care provision. Using SOEP data, we address fundamental endogeneity problems by exploiting variation in the German pension system in two complementary ways. We find a significant effect of retirement on informal care provision, when using early retirement age thresholds as instruments. Heterogeneity analyses confirm the underlying behavioral mechanism, a time conflict between labor supply and informal care. We further exploit a sizable increase in the early retirement age for German women and find that affected women provide less non-intensive care. High intensity care is not impacted, which leads to a double burden and potentially negative health effects for caregivers. Exploiting the policy reform, we find evidence supporting the notion that formal care is no substitute for informal care. This implies that less overall care is received, which can be damaging to the health of the recipients of care.

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1. Introduction

Governments increasingly try to relieve pressures on pension systems generated by population aging through reforms that seek to prolong working lives and increase

women's labor market participation. Yet, aging also strains long-term care (LTC) systems and raises the demand for informal care from the same people who are called on to increase their (formal) labor supply. The potential conflict between the pension and LTC systems brought about by policy responses to aging has been largely neglected.

This paper is one of the first to provide causal evidence of an effect of retirement on informal care provision on the basis of convincing exogenous variation. We focus on women's retirement and care responses to the early retirement age (ERA), an age at which they are potentially primary caregivers. To identify the causal effect, we exploit variation in the German pension system with two separate, complementary identification strategies. First, we investigate whether women increase informal care provision when retiring at their ERA, employing an ERA

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discontinuity design in an instrumental variables (IV) setup (Battistini et al., 2009; Eibich, 2015). We refer to this as ERA discontinuity analysis. Second, we exploit a reform-induced increase in the ERA for German women within a regression discontinuity design (RDD) (Manoli and Weber, 2016; Mastrobuni, 2009). This will be referred to as ERA reform analysis. We use the German Socio-Economic Panel (SOEP) (Goebel et al., 2019). Its rich information allows analyzing different margins of informal care. We can also estimate effect heterogeneity in several dimensions with both identification strategies. Exploiting household-level information on different types of care received, we go one step beyond an analysis of informal care supply. Within the ERA reform analysis, we also estimate whether a reform-induced reduction in informal care is compensated by care from outside the household, which includes formal care.

Results based on both identification strategies confirm that labor supply puts restrictions on women's supply of informal care. When women postpone early retirement, they provide less informal care. We find a significant increase of previously employed women's informal care provision upon retirement at their ERA. Care hours rise on average by about 0.8 hours (h) and the caregiving probabilities by about 13 percentage points (pp) on a weekday. When women face an increase of their ERA and prolong their working lives, they provide significantly less informal care, compared to cohorts with a lower ERA. Affected women's caregiving probability is reduced by almost 6 pp, mostly driven by a decrease in low-intensity care. High intensity caregivers do not react and thus face a double burden of labor and care supply. Heterogeneous effects support this interpretation: Effects are larger when the time conflict binds, e.g. for (full-time) employed or highly educated women. These findings hold under various robustness checks and are absent in placebo regressions. A household-level ERA reform analysis on different types of care received confirms our findings. It also provides evidence that low-intensity care is not substituted by care from outside the household. Care-dependent individuals receive a different quality and lower quantity of care.

These effects are economically relevant in two dimensions: First, previous literature has shown that informal care is preferred over formal care by care recipients and their relatives. It is also prioritized in LTC systems (Blaise, 2018; Hajek et al., 2018; Lipszyc et al., 2012; Mentzakis et al., 2009) and plays an integral part in overall care: around 48% of care-dependent persons in Germany are cared for in their own homes, exclusively by family and friends (informal care) (Wetzstein et al., 2015). The highest shares of informal caregivers are found among women around the retirement age (about 12%; Geyer and Schulz, 2014; Wetzstein et al., 2015). Second, women have used ERA thresholds extensively (Geyer and Welteke, 2019; Keck and Krickl, 2013). Studying the caring behavior of women in this context shows that pension institutions are also important for the supply of informal care.

The study contributes in several ways to the literature on the behavioral relationship between informal care, labor supply, and retirement. Literature has found negative effects of caregiving behavior on labor market

participation.¹ Van Houtven et al. (2013) among others² further report that informal care providers have *ceteris paribus* a higher probability to be in retirement. First, we confirm a causal effect of retirement on informal care with two identification strategies, based on exogenous variation. In this way, we contribute to a strand of literature that investigates effects of labor market participation on informal care and finds a negative causal relationship.³ Second, we show effect heterogeneity, consistent with an underlying time conflict, which has been solved through retirement at ERA thresholds. Third, we contribute to the question of substitutability of informal and formal care by estimating ERA reform effects on different types of care received at the household level. A large body of literature discusses whether informal and formal care can be seen as substitutes, finding mixed evidence.⁴ Fourth, combining our own with previous findings from literature, we discuss further implications for the health of caregivers and care receivers. Future reforms of social security systems need to be more aware of trade-offs between pension and LTC policies.

The paper is organized as follows. We sketch relevant institutional features of the German LTC and pension systems in Section 2. The data set is introduced, and estimation samples are described in Section 3. We discuss our two identification strategies in Section 4. Empirical results from both approaches are presented and discussed in Section 5. Section 6 concludes.

2. Institutional setting

2.1. The state system of formal and informal care provision

In 2016, around 2.7 million people received benefits from the Social Care Insurance (Soziale Pflegeversicherung), the German governmental care insurer. Nearly 2 million of those were outpatients (BMG, 2017). The governmental care insurer defines a strict priority of home care.⁵ Benefit eligibility is defined only with respect to individual care needs (online Appendix A.1). Three levels of care dependency existed during the period of observation that were extended to five levels in 2017. Most recipients receive monetary benefits, in order to support relatives who take on the responsibilities, so-called *informal care*.

It is possible to combine informal with external care, bought from professional providers. Parts of the costs are covered by insurance. Care receivers are free to spend the

¹ Carmichael and Charles (1998), Carmichael and Charles (2003a,b), Heitmueller (2007), and Schmitz and Westphal (2017) are prominent examples who all confirm a negative causal impact of care activities on gainful employment.

² See Carr et al. (2018), Jacobs et al. (2017) and Niimi (2018)

³ See Berecki-Gisolf et al. (2008), Boaz (1996), Carrino et al. (2019), Carmichael et al. (2010), Doty et al. (1998), Golberstein (2008), He and McHenry (2016), Mentzakis et al. (2009), Michaud et al. (2010), Moscarola (2010), Nizalova (2012), or Stern (1995).

⁴ See Bell et al. (2007), Bonsang (2009), Hollingsworth et al. (2017), Karlsberg Schaffer (2015) and Van Houtven and Norton (2004).

⁵ Defined in A3 SGB XI (German social security law book 11); see also Hajek et al. (2018), HCHE (2017) and Geyer and Korfhage (2015).

amount and can use it to reimburse family carers. Geyer and Schulz (2014) point out that many individuals in need of care do not meet eligibility conditions. Informal care is then provided privately, without any state support and the budget constraint is not influenced by the insurance system.

Several current laws (e.g. the 'Pflegezeitgesetz' or the 'Familienpflegezeitgesetz') promote the compatibility of informal care and gainful employment. Conditions for the provision of informal care without having to quit employment have improved significantly in recent years (BMAS, 2017). However, the take-up of these rights and benefits seems to be very limited, although official statistics have not been published (online Appendix A.2). Were these laws to effectively address the time conflict between informal care and labor supply during the period under observation, we arguably would not identify significant effects of retirement on informal care. This paper therefore provides indirect evidence that these policies are not effective. Although these laws do not invalidate our identification strategies, an increased take-up over time could reduce the size of the estimated effects.

2.2. Early retirement thresholds in the state pension system

The German state pension system provides different paths into retirement that differ for men, women, and various cohorts.⁶ The six paths stated in the Statutory Pension Insurance Scheme ('Gesetzliche Rentenversicherung', GRV) differ in the defined normal retirement age (NRA), or the early retirement age (ERA).⁷ In this paper, we exploit the following ERA thresholds, which applied to women in the time period 2001–2015 for identification:

- 8 People who have acquired 35 years of contributions can retire early at the age of 63, but face benefit reductions.⁸
- 9 The pension reform of 1999 introduced a change to the ERA for German women abolishing women's pension.⁹ While women born until Dec 31st 1951 could use women's pension to retire early at age 60, if they fulfilled the contribution criteria;¹⁰ this pathway into retirement was no longer available for women born from 1952 onward. In 2012 the last cohort was eligible for early old age pensions for women at age 60.
- 10 People born before 1952 could retire from unemployment, if they had 15 years of contributions. Cohorts born until 1945 could use this path into retirement from the age of 60. Those born from 1949 onwards were eligible from an age-threshold of 63. Eligibility age was raised

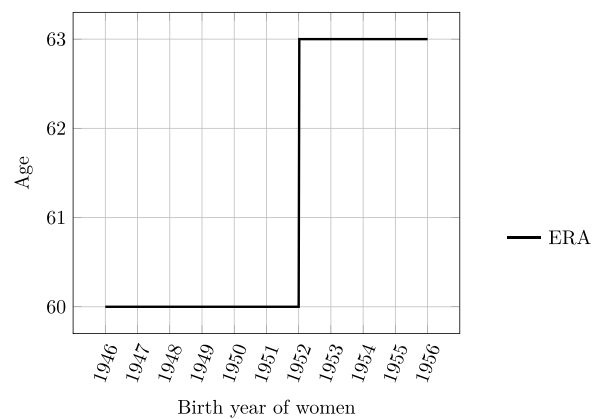


Fig. 1. Female eligibility ages along birth cohorts.

in monthly steps from 60 to 63, for those born between January 1946 to December 1948, per one month of later birth.

The ERA thresholds at ages 60 and 63, as described above, are used in our ERA discontinuity analysis as instruments. The NRA is 65 for all women in our sample. It defines the reference age for the calculation of deductions under early retirement. Our sample includes the years 2001–2015 (Section 3). All aforementioned age thresholds are relevant for early retirement of women. We pool women, born before 1952 with later-born cohorts for our main specification in the ERA discontinuity analysis. We define the ERA accordingly at age 60 years or age 63, respectively.

2.3. The pension reform of 1999

The pension reform of 1999 raised the effective ERA from 60 to 63 years for women born from Jan 1, 1952 (Fig. 1). The contribution criteria for claiming retirement benefits at age 63 (Retirement for long-term insured) did not change. Geyer and Welteke (2019) argue that those retirement paths could be claimed by very similar groups of women. They calculate that around 60% of women born in 1951 were eligible for women's pension at age 60. We make use of this policy-induced variation in retirement behavior in our ERA reform analysis.

As set out above, women, who used age 60 to retire through women's pension faced higher deductions, compared to women who retired through invalidity pension or women claiming pension for long-term insured at age 63 (online Appendix A.3). Women born from 1952 onward lose the most frequently utilized option to exit the labor market and receive benefits before the age of 63. The only pension type they can claim before age 63 is invalidity pension.¹¹ The reform, therefore, particularly affects the retirement behavior of women, aged between 60 and 62,

⁶ See Boersch-Supan and Wilke (2004) for details on the German pension system. Geyer and Welteke (2019) provide an extended overview including the 1999 pension reform and alternative paths into retirement.

⁷ An overview is given in GRV (2017a).

⁸ Since 2014 people who have been born before 1953 and have 45 years of contributions can retire without any deductions at the age of 63. Women born between 1952 and 1953 can therefore retire at age 63 without deductions.

⁹ The Law was announced on December 16, 1997.

¹⁰ Required were 15 years of pension contributions 10 of which had to be accumulated after the age 40.

¹¹ Geyer and Welteke (2019) argue that, although being preferable to women's pension at age 60 because of lower deductions, eligibility criteria for invalidity pension are hardly fulfilled. Claiming unemployment benefits, going into inactivity, or applying for disability pensions are the remaining options to leave the labor force at age 60.

as those born before 1952 could retire at age 60, yet for most women born later the earliest retirement age was 63.

3. Data, variables, and estimation samples

In this paper, we use the German Socio-Economic Panel (SOEP, version 33). Besides rich longitudinal information on individuals and households, it covers several behavioral dimensions, among them the supply of labor and LTC, as well as the retirement age (Goebel et al., 2019).¹²

3.1. Definition of variables

The SOEP questionnaire contains a question on the allocation of time on a weekday. Since 2001 individuals are asked to report their time spent on taking care of an adult person in need.¹³ Information on hours of informal care are self-reported and therefore, presumably, not perfectly accurate.¹⁴ In order to capture the extensive margin of informal care provision, we additionally collapse the hours information into a binary variable that is equal to one, when a person spends time on care provision for the elderly and zero otherwise. To avoid linearity assumptions, we also define a binary variable for intensive care. This variable is coded one, when an individual provides more than 10 h of informal care per week and zero otherwise. It is used in the heterogeneity analysis to assess, whether the demand for intensive care induces a more severe time conflict. We also use self-reported information from the SOEP to determine whether a person is retired or not (details in online Appendix B.1).

3.2. ERA discontinuity analysis: sample construction

In the ERA discontinuity analysis, we investigate, whether women can resolve a time conflict between the provision of informal care and labor through retirement at an ERA threshold. If a person is non- or unemployed prior to retirement, a time (and/or a budget) constraint cannot be removed through a transition into retirement. Therefore, we eliminate non- and unemployed women who are not yet retired from our main estimation sample (details in online Appendix B.2).

The actual size of the estimation sample in the ERA discontinuity analysis also depends on the bandwidth, chosen around the age-cutoffs that are used for identification. In the main ERA discontinuity analysis, we employ different ERA's as instrument and use a bandwidth of 5 years before

and after the cutoff, respectively (leading to a 10 year observation window).¹⁵ The resulting sample includes 10,095 person-year observations for 1,659 women (Table A1 in the online Appendix, see also details in online Appendix B.3).

3.3. ERA reform analysis: sample construction

The ERA reform analysis is based on the removal of the ERA at age 60. We investigate, whether this reform led to a time conflict between informal care and labor supply for women, who would have been eligible for women's pension at this threshold. By construction, ERA reform effects can only be estimated on a smaller sample. As a consequence, we use all women aged from 60 to 62 and born between 1949 and 1954 as the baseline sample. In the heterogeneity analysis, we focus on women, who are employed before they reach age 60. We also create a sample of women, who fulfil the eligible criteria for women's pension. The main ERA reform analysis sample contains 3,569 person year observations on 1,390 women. Table A2 in the online Appendix provides summary statistics for the outcome variables and some important covariates.

4. Identification strategies

We are interested in the impact of retirement on informal caregiving activities. Estimation approaches are confronted with endogeneity issues that result from simultaneity (reverse causality) and selection on unobservables. Both problems are inherent in the related individual decisions. The ERA discontinuity analysis deals with these issues by using ERA thresholds as instruments for retirement. The ERA reform analysis is based on reform-induced variation in one specific ERA and identifies the effect on informal care provision, locally around the cutoff date.

4.1. ERA discontinuity analysis

When women reach their ERA and fulfil the contribution criteria, they become eligible for retirement benefits. This changes their choice set and budget constraint, as retirement with pension benefits becomes available. Eligibility is determined solely through age, i.e. women's treatment status is exogenous. Around the ERA thresholds, individuals differ only in benefit eligibility and are similar in all other aspects. We exploit this setting within a fuzzy regression discontinuity design (fuzzy RDD; see Battistin et al. (2009), Eibich (2015), or Müller and Shaikh (2018)). We define the cohort-specific ERA for all women in our main sample, to estimate a Two Stage Least Squares (2SLS) model. Retirement (R_{it}) as the treatment variable is an endogenous regressor. The threshold variable I_{it} serves as instrument, with $I_{it} = 1$, if $Age_{it} > c$. The cutoff c is defined for women born before 1952 as $c = 60$ and for women born from 1952 onward as $c = 63$. The first stage captures the impact of

¹² Further details on the data and the two different estimation samples are given in online Appendix B.

¹³ The exact question reads: "What is a typical day like for you? How many hours do you spend on care and support for persons in need for care on a typical weekday?" Separate questions are asked for care provided on Saturdays and Sundays. We use this information in a robustness check (online Appendix C.3). Taking care of children is also a separate question. Thus, we do not mix those two activities.

¹⁴ 47% of informal caregivers supply 1 h, around 24% state 2 h, about 10% provide 3 h, and the remaining about 18% perform 3 or more hours. A relatively high number of people (1.5%) provide more than 20 h of care per weekday.

¹⁵ The sample thus theoretically consists of women aged 55 (the ERA at age 60 minus 5 years for women born before 1952) to 68 (the ERA at age 63 plus 5 years for women born post 1952; in reality, women born in 1952 can, however, only turn 63 in 2015) who retire from employment.

the respective threshold on treatment assignment, i.e. the retirement decision:

$$R_{it} = \alpha + \beta_1 I_{it} + \beta_2 (Age_{it} - c) + \beta_3 (Age_{it} - c) I_{it} + \varepsilon_{it} \quad (1)$$

We allow the relationship between treatment variable R_{it} and the forcing variable, centered at the respective cut-off age c , $(Age_{it} - c)$, to be different on each side of the threshold.¹⁶ The parameter β_1 measures the direct effect of crossing the threshold on the retirement probability. The second stage uses the predictions of treatment assignment from the first stage and regresses it on an outcome indicator for caregiving, $Care_{it}$:

$$Care_{it} = \gamma + \delta_1 \hat{R}_{it} + \delta_2 (Age_{it} - c) + \delta_3 (Age_{it} - c) I_{it} + \mu_{it} \quad (2)$$

We analyze effects on $Care_{it}$, namely the extensive and intensive margin of informal care and a binary indicator for intensive care (Section 3.1). The parameter of interest is δ_1 . For relevant and valid instruments, the predicted retirement probability carries only exogenous variation at the cutoff and is locally independent of the error term. Therefore, we can identify the effect of exogenous variation in retirement behavior locally around the threshold. In the main specification, we estimate the effects for all women in our sample, crossing their cohort-specific ERA.¹⁷ We perform several robustness checks in terms of bandwidth choice and using non-parametric estimators for the discontinuity (Gelman and Imbens, 2018).

Identification requires, first, that individuals cannot manipulate their age to select into treatment (i.e. being eligible to retirement benefits before reaching the defined age). Second, the potential outcome needs to be smooth around the threshold, absent of treatment. There must not be any discontinuous change in the retirement probability by age in the absence of age cutoffs in the retirement rules. Given those assumptions, effects of the instrumented retirement behavior on care provision can be causally attributed to the local treatment (Lee and Lemieux, 2010; Hahn et al., 2001; Trochim, 1984). In our setting, the local average treatment effect (LATE) is specific to those women that retire at an age threshold, because of its incentive structure (and not coincidentally for other reasons). Under valid and relevant instruments, this approach deals with simultaneity and selection on unobservables.¹⁸

Even if ERA thresholds are used frequently (Geyer and Welteke, 2019; Keck and Krickl, 2013), retiring remains a choice variable. Using ERA thresholds for identification therefore allows for self-selection according to care demand and the willingness or ability to supply informal care. This does not threaten identification. However, it slightly changes the interpretation of our estimates: exter-

nal validity for later retirement ages is reduced, when the demand for informal care changes with age. It could also be that those individuals facing a demand for and are willing to provide informal care retire at the ERA because of this reason. The remaining group of women retiring at later thresholds does not face a time conflict between work and care to the same degree. This leads to an upward bias in the estimated parameters in comparison to the underlying parameters for the overall population of women.

4.2. ERA reform analysis

In the ERA discontinuity analysis, we estimate whether women increase informal care provision through retirement at an ERA threshold. An implication of this effect – which is relevant for the design of welfare state policies – is that the removal or increase in the retirement age, i.e. a delay of retirement, negatively impacts the supply of informal care. The ERA reform analysis can explicitly test, whether this interpretation holds: the reform strategy exploits that women who are born shortly before or after Jan 1, 1952, and who are between 60 and 62 years old, differ institutionally in their possibility to retire early. We compare cohorts treated by this reform with older cohorts in a similar age range. Accordingly, individual care activity is regressed on a binary treatment variable (D_i) which equals 0, if the person was born before 1952, and 1 otherwise.

$$Care_{it} = \gamma_0 + \gamma_1 D_i + \gamma_2 (z_i - c) + \gamma_3 (z_i - c) D_i + \delta X_{it} + v_{it} \quad (3)$$

Again, we allow for a trend of the outcome with respect to the running variable – now the exact individual month of birth date z_i – centered at Jan 1, 1952 (c). We also allow for a break in this trend ($z_i - c$) at the cutoff date. The parameter of interest in this specification, γ_1 , represents an intention to treat (ITT) effect, because not all women would have been eligible for women's pension at age 60. We include the year of reporting for each observation in the sample and the exact age of the women as control variables (X_{it}).¹⁹

The ERA reform analysis identifies the causal ERA reform effect on informal care provision, if individuals cannot manipulate their birth date and therefore select into or out of being eligible for retirement at age 60. In addition, we must assume that absent of the reform the cohorts born in 1951 and in 1952 do otherwise not discontinuously differ in their retirement and in their caregiving behavior. Moreover, we assume that a discontinuous change in the demand for informal care does not occur around the cut-off date. Geyer and Welteke (2019) show that other policy changes cannot explain differences in retirement behavior between these cohorts.

¹⁶ We define the binary threshold variable as 1, if the individual is older than the respective age cutoff ($Age_{it} > c$). The first stage has more predictive power in comparison to the alternative specification with $I_{it} = 1$ if $Age_{it} \geq c$.

¹⁷ In an additional estimation, we use solely women born until 1952 and define as threshold c only the ERA at 60 years that applied to this group.

¹⁸ Indirect evidence on the validity of these identification assumptions is provided in online Appendix C.1.

¹⁹ Further control variables are included in robustness checks (online Appendix C.6).

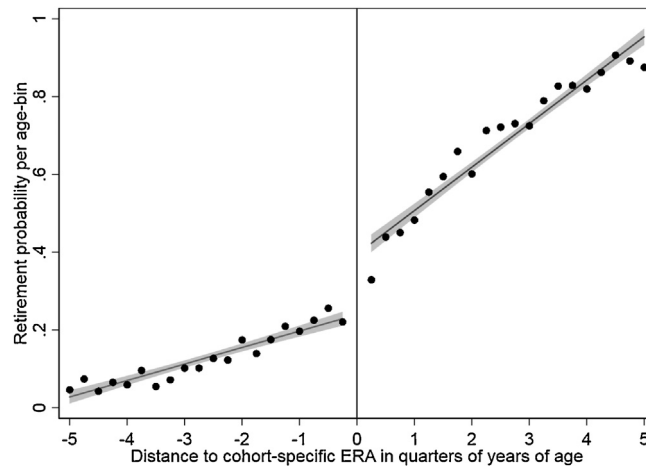


Fig. 2. Retirement behavior by distance to ERA.

Note: Each dot represents the mean of the outcome per bin (quarters of age difference to cohort-specific ERA); Linear trend and 95% confidence interval included.

5. Results

5.1. ERA discontinuity analysis

In this section, we first present evidence for the validity of instruments. Then, we analyze graphically, how women's informal care provision behaves around ERA thresholds. Third, ERA discontinuity analysis results for our main sample of women based on cohort-specific ERA's with a 5-year bandwidth (i.e. a 10-year estimation window around the thresholds) are discussed. All estimates include heteroskedasticity-consistent standard errors clustered at month of age (Lee and Card, 2008). Various heterogeneity analyses and comprehensive robustness checks complete the results of the ERA discontinuity analysis.

5.1.1. ERA discontinuity analysis: instrument validity

Instruments need to significantly affect retirement decisions to be considered valid. We depict retirement behavior of women in the estimation sample, by distance to their cohort-specific ERA in bins of quarters of age to show, whether this condition holds. A graph that also includes linear trends displays a marked jump in retirement probabilities at women's cohort-specific ERA (Fig. 2). The discontinuity is substantive and roughly amounts to a 20 pp effect.²⁰ Less than 30% of women are retired before reaching the respective ERA. After crossing the threshold, retirement probability jumps to about 50%. Women retiring from employment at their cohort-specific ERA cutoffs exhibit a jump in the retirement probability of 17.2 pp (Table A3 in the online Appendix, column (1)).²¹

²⁰ A similar discontinuity emerges in the graph that is based only on women born before 1952 with the ERA at age 60 used as an instrument (Figure A1 in the online Appendix).

²¹ A more comprehensive discussion of instrument validity including additional first stage results is provided in the online Appendix C.2.

5.1.2. ERA discontinuity analysis: graphical evidence on informal care provision

The identification strategy in our ERA discontinuity analysis is essentially a fuzzy RDD estimation. A simple graphical analysis can be informative about discontinuous changes in outcome variables at the thresholds used as instruments. The share of caregivers along with a quadratic trend and a 95%-confidence interval is plotted for our main sample by distance to the ERA for women around the age cutoffs of interest (Figure A2 in the online Appendix). The share of caregivers among all women peaks around 60 and declines with higher ages.²²

We show, how the main outcome variables behave around the ERA threshold, introducing a trend line, as well as a 95% confidence interval around that trend line (Fig. 3). We find that, while the means of respective outcomes per bin are quite dispersed, a small increase in the mean hours of daily care provision occurs at the ERA. Note that few women per bin perform informal care, and that this graph discards the dimension of retirement. We therefore turn to the results from our 2SLS (ERA discontinuity analysis) estimation to get a clearer picture of these effects.

5.1.3. ERA discontinuity analysis: main specification

2SLS estimates form the ERA discontinuity analysis for women retiring from employment at the cohort-specific ERA reveal positive and significant effects of retirement on overall informal care hours. Daily care hours increase on average by 0.8 hours (almost 5 times the pre-treatment mean) upon retirement (Table 1, upper panel). The coefficient is significant at the 1% level. Employed women aged between 55 and 60, i.e. before crossing an ERA threshold, provide on average about 0.2 h of informal care per week. Thus, the effect is substantial and driven by women who take up informal care or increase hours of care. We find comparable positive effects on the hours of daily care provi-

²² Figure A3 in the online Appendix provides mean informal care hours by quarters of age.

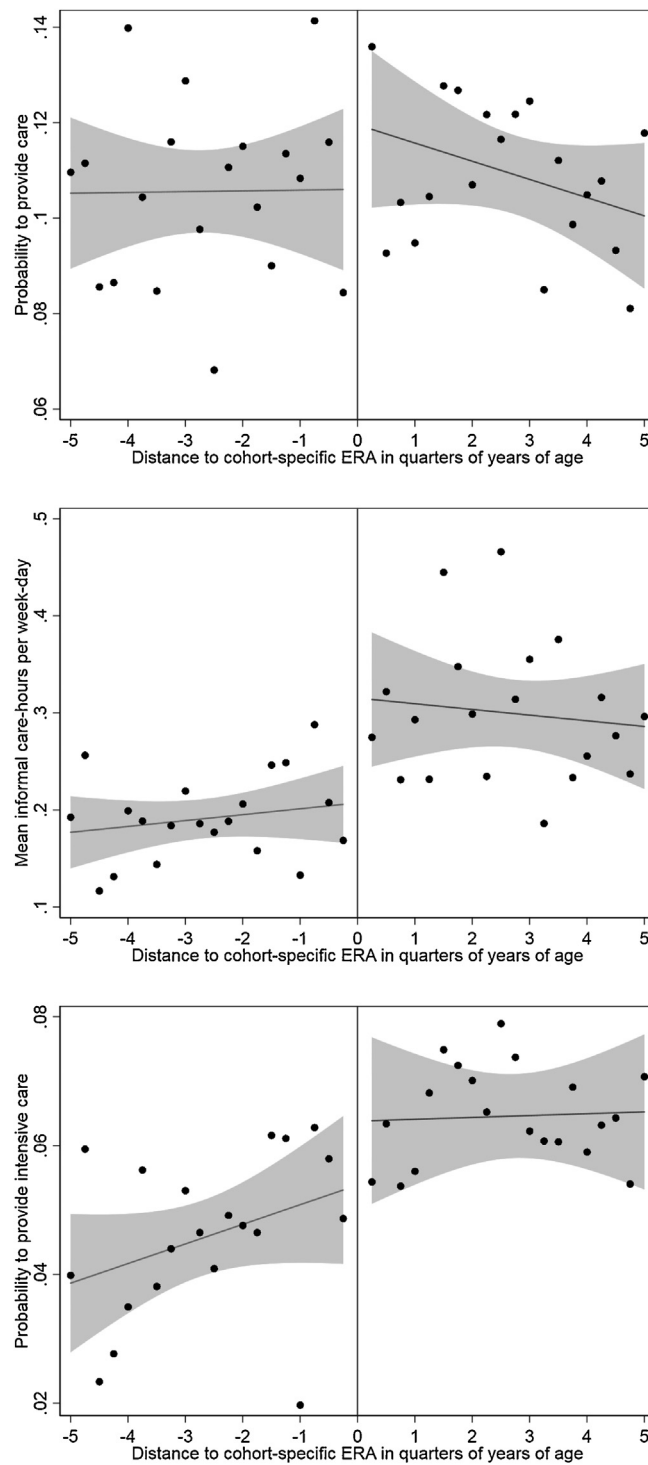


Fig. 3. Care provision around the ERA.

Note: Each dot represents the mean of the outcome per bin (quarters of age difference to cohort-specific ERA); Linear trend and 95% confidence interval included.

Table 1

ERA discontinuity analysis: effects of retirement on informal care provision.

| Instrument | (1) ERA | (2) ERA | (3) ERA | (4) Age 60 |
|-----------------------------|---------------------|-------------------|---------------------|---------------------|
| Hours of care provision | | | | |
| | 0.772*** (0.252) | 0.898* (0.460) | 0.813*** (0.264) | 0.695*** (0.248) |
| Observations | 10095 | 6189 | 10095 | 8379 |
| Bandwidth years | 5 | 3.282 | 5 | 5 |
| Pre-Treatment mean | 0.159 | 0.159 | 0.159 | 0.151 |
| Probability to provide care | | | | |
| | 0.131* (0.075) | 0.163 (0.165) | 0.146* (0.078) | 0.118 (0.076) |
| Observations | 10095 | 5484 | 10095 | 8379 |
| Bandwidth years | 5 | 2.935 | 5 | 5 |
| Pre-Treatment mean | 0.091 | 0.087 | 0.091 | 0.085 |
| Intensive care | | | | |
| | 0.096* (0.050) | 0.092 (0.083) | 0.102* (0.053) | 0.075 (0.052) |
| Observations | 10095 | 6705 | 10095 | 8379 |
| Bandwidth years | 5 | 3.562 | 5 | 5 |
| Pre-Treatment mean | 0.038 | 0.038 | 0.038 | 0.035 |
| Controls | - | - | YES | - |
| KL.Paap | 58.75 | - | 46.73 | 63.22 |

Notes: Main effects and robustness checks, women retiring from employment, women observed in 2001–2015; ERA: cohort-specific early retirement age (all women), Age 60: only age 60 as instrument (women born before 1952); (2): optimally selected bandwidth; Cluster robust (clustered on the month of age level) standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; YES: controls for year of observation, number of children in the household, and marital status; KL.Paap: Kleibergen–Paap statistic.

sion in several robustness checks, with only some variation in effect size.²³ The effect size increases slightly, with an optimal bandwidth, but is less precisely estimated (column (2)). Using only a single ERA threshold yields a virtually identical estimate (column (4)).

The hours effect is substantially higher for the group of women, who already provide some care, before they reach their ERA: the provision of informal care increases on average by 5.4 hours per day (Table A4 in the online Appendix). Before crossing their ERA thresholds, caregiving women in our main sample provide about 1.7 hours of informal care. This number almost triples indicating a resolution of a time conflict through retirement. Effects in this small group of women are, however, threatened by weak instrument problems.

According to our estimates, the probability to be a caregiver increases through retirement by 13 pp (equaling a 140% increase) (Table 1, middle panel).²⁴ The baseline probability to be a caregiver for employed women in the age-range 55–60 is around 9%. The group of caregivers more than doubles through early retirement. Note that this increase is estimated for a specific group of women retiring at their ERA. Robustness checks yield similar patterns as for hours of informal care. The effect increases slightly with an

optimal bandwidth, but is no longer statistically significant (Table 1, column (2)). Including control variables and using a single ERA cutoff does not alter effect sizes (columns (3) and (4)).

The probability to be an intensive caregiver increases by 9.6 pp (equaling a 255% increase, Table 1, lower panel). With a baseline probability of 3.7%, the effect size for intensive care is comparable to overall care in relative terms. Parameters are only slightly altered in the robustness checks (columns (2)–(4)). Selecting the optimal bandwidth, again yields a coefficient of comparable magnitude that is not statistically significant (column (2)). Extensive margin parameters are in general less precisely estimated with our data than the hours coefficients.

To gauge the quantitative relevance of these estimates, we compare the effect size to other activities. Women retiring from their ERA decrease their time in employment by about 6.3 h per normal weekday. This change in their time budget constraint leads to increased time investments in several activities besides caregiving (Table A5 in the online Appendix).²⁵ The comparison of parameters shows that the increase in informal care provision is large, relative to other activities given that only a relatively small portion of women face demand for informal care.

Finally, we compare IV coefficients from the ERA discontinuity analysis with plain OLS estimates measuring the correlation between retirement and informal care provision. We find that retired women have on average a 6

²³ The other columns refer to similar estimates based on models with (2) an optimally chosen bandwidth (Calonico et al., 2014), (3) additional control variables (year of observation, number of children in the household, years of education, marital status), and (4) using only age 60 as instrument for retirement behavior in a group of women born before 1952, respectively.

²⁴ Parameter estimates hold at the mean of the distribution in linear probability models (LPM). Predicted probabilities based on LPM estimates are not bound between 0 and 1.

²⁵ Time spend on hobbies increases by 1.9 h, housework by 1.2 h, running errands by 0.4 h, repairs by 0.5 h and informal care provision by about 0.8 hours per weekday. Time spend on childcare does not increase significantly.

Table 2

ERA discontinuity analysis: heterogeneous effects of retirement on informal care provision.

| | (1) | (2) | (3) | (4) | (5) | (6) |
|--------------------|---|---------------------|---------------------|---------------------|---------------------|---------------------|
| | Hours of care provision | | | | | |
| | 0.772*** (0.252) | 0.636*** (0.233) | 0.984*** (0.319) | 1.657*** (0.484) | 0.832*** (0.289) | 0.998** (0.471) |
| Pre-treatment mean | 0.159 | 0.042 | 0.137 | 0.180 | 0.038 | 0.056 |
| | Probability of care provision | | | | | |
| | 0.131* (0.075) | 0.101** (0.047) | 0.141 (0.087) | 0.403*** (0.140) | 0.132** (0.060) | 0.155* (0.082) |
| Pre-treatment mean | 0.091 | 0.019 | 0.084 | 0.105 | 0.018 | 0.026 |
| | Probability of intensive care provision | | | | | |
| | 0.096* (0.050) | 0.118*** (0.037) | 0.144** (0.065) | 0.279*** (0.088) | 0.153*** (0.049) | 0.165*** (0.062) |
| Pre-treatment mean | 0.038 | 0.012 | 0.031 | 0.042 | 0.009 | 0.014 |
| Observations | 10095 | 9303 | 6057 | 6127 | 5593 | 5555 |
| KL.Paap | 58.75 | 63.11 | 52.19 | 23.54 | 55.09 | 22.48 |

Notes: Women retiring from employment, cohort-specific ERA, women observed 2001–2015, 5-year bandwidth; (1): main effect – cohort-specific ERA, (2): care within the own household, (3): retiring from full-time employment, (4): highly educated women, (5): care within the own household & retiring from full-time employment, (6): care within the own household & highly educated women; Cluster robust (clustered on the month of age level) standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; KL.Paap: Kleibergen-Paap statistic.

pp higher probability to be caregivers and to provide 0.25 additional hours of informal care per weekday (Table A6 in the online Appendix). The OLS coefficients cannot be interpreted causally, since they do not represent the average treatment effect (ATE). Simultaneity and selection on unobservables lead to an upward bias (Section 4). In addition, the ATE and the LATE estimated in the ERA discontinuity analysis do not coincide. The LATE represents the causal effect on informal care provision for those women who utilize the ERA for retirement (compliers). Women who face demand for informal care and a time conflict with labor supply will seize the opportunity of an ERA. Thus, the LATE of retirement on informal care provision is larger than the ATE, because women who retire past their ERA are on average less frequently confronted with demand for informal care (Section 4.1). This difference is obviously more important than the bias in OLS estimates.

5.1.4. ERA discontinuity analysis: heterogeneity

When a caregiver lives in the same household as the recipient, care decisions could potentially be different. People might also have a more precise conception of their own care activities, when these take place within their household.²⁶ Point estimates are slightly smaller for average care hours and for the extensive margin, compared to the main effects. They are also slightly smaller for the probability to be an intensive caregiver. Yet, estimates turn out to be more precisely estimated at the intensive and extensive margins (Table 2, column (2)).

When estimating effects for women who were full-time employed before retirement, we check, whether the time conflict between employment and care is more binding. This should yield larger point estimates. Women retiring from full-time employment on average increase their

care-provision by about one hour. Their probability to be a caregiver increases by 14.1 pp and their probability to provide intensive care by 14.4 pp upon retirement through the ERA (Table 2, column (3)). Compared to the main effect, the estimated increases are larger for all margins.²⁷

We also break down the main effect by level of education. Descriptive studies have shown that informal care varies substantially by education (Wetzstein et al., 2015). In addition, highly educated women exhibit significantly greater employment rates and thus a higher probability to be eligible for retirement at their ERA. Higher educated women show markedly larger and also more significant effects for all margins of care (Table 2, column (4)).²⁸

The final heterogeneity exercise looks separately at married and unmarried women. Effects are more precisely estimated for married women (Table A7 in the online Appendix). Validity of the instrument does no longer hold in the smaller sub-sample of unmarried women (Table A8 in the online Appendix).

5.1.5. ERA discontinuity analysis: robustness

First, we conduct a placebo test. We analyze a sample of women who are unemployed in the relevant age window. This group also uses ERA thresholds for retiring but does not face a time conflict between care provision and labor supply. The instrument still works fine in this sample, without yielding significant effects on informal care provision (Table A9 in the online Appendix).

A related robustness check also revolves around the theoretical concept of a time conflict: we use the measure of informal care provided on weekends. We find that hours of care are impacted positively when women retire

²⁶ We discard observations, when care provision is reported in some period, but this person lives in a household which at no point in the observed time span is inhabited by a person in need of care.

²⁷ The same pattern holds when we look specifically at women providing informal care for people living in their household and retiring from full-time work (Table 2, column (5)).

²⁸ Highly educated women are those with at least 11 years of schooling. This heterogeneity pattern can be replicated for women that care only within their own household (Table 2, column (6)).

at their ERA. However, the point estimate is less than half the size compared to weekday estimates and not statistically significant (Table A10 in the online Appendix). Neither the probability of care provision, nor intensive care provision are impacted significantly. These results support the interpretation that the main effects are indeed driven by a time conflict between labor supply and informal care provision.

All the aforementioned results are based on a bandwidth choice of five years. We check whether narrowing the bandwidth to 4, 3, 2 and 1 years produces different results. This is also interesting for substantive reasons: the 5-year bandwidth for the specification using cohort-specific age thresholds includes besides the ERA at 60 also the ERA at 63 for women born before 1952 as well as the NRA at 65 years which applies to all women in the main sample. Estimating similar models with narrower bandwidths rules out that the paths into early retirement are influenced by other thresholds at higher ages. The trade-off is that identification is based on less observations, which produces noisier estimates.

Narrowing the bandwidth from 5 to 2 years does hardly alter point estimates on the extensive and intensive margin (Figures A4 and A5 in the online Appendix). However, confidence intervals increase, and coefficients become partly insignificant, when the sample size is reduced. Point estimates for small bandwidths of 1 and 2 years are an exception, as they become substantially larger. The sample is too small to identify meaningful effects there.

The exclusion restriction in our IV setting holds, if only those women who utilize institutionalized early retirement opportunities and who retire at their ERA exhibit a discontinuous jump in the respective outcome variables. Women who choose not to retire at, but at some time before (never-takers) or after (always-takers) an ERA threshold, must not show any discontinuity in the respective outcome variable when crossing their ERA. We do not find any discontinuities in the caregiving behavior of never-takers (Figure A6 in the online Appendix). Never-takers are women retiring from their next available retirement thresholds onwards. When a woman is eligible for early retirement at age 60, she needs to retire after the next available cut-off at age 63. A woman who would be eligible at an ERA of 63, retires at the earliest at age 65 which is her NRA. Likewise, no visible jumps emerge in either of the three outcomes for always-takers (Figure A7 in the online Appendix). All these women retire before reaching their respective ERA. In contrast, we find discontinuities at the ERA cutoff, when plotting informal care outcomes by distance to the ERA for those women who use their ERA threshold for retirement (Figure A8 in the online Appendix). These patterns are confirmed by reduced-form effects of crossing the age threshold (ERA threshold) on caregiving behavior in the respective groups (Table A11 for never-takers, Table A12 for always-takers, Table A13 for the whole group of women retiring from employment).²⁹

5.2. ERA reform analysis

This sub-section comprises findings from the ERA reform analysis. We identify the effect of the removal of one ERA cutoff on informal caregiving. After providing graphical evidence, we discuss regression results from our main specification. This is complemented by a heterogeneity analysis and a set of robustness checks. Finally, we estimate the ERA reform effect on different types of care at the household level and address substitution between informal and formal care.

5.2.1. ERA reform analysis: graphical evidence on informal care provision

The 1999 pension reform reduced the probability of women, aged between 60 and 62, to be retired, for those women born from 1952 onward. Following Geyer et al. (2020) and Geyer and Welteke (2019) we expect the probability to be employed to increase in this group. Plotting linear trends for the probability to be retired by month of birth around the cutoff date, we find a substantial drop for women aged between 60 and 62, born after the cutoff (Fig. 4, upper left graph). Moreover, women show an increase in their employment probability as well as their probability to be unemployed (Figure A9 in the online Appendix).

We draw similar graphs for informal caregiving behavior of women in this group along their birth date, in bins of birth-months (Fig. 4, upper right, and both lower graphs).³⁰ There is a small drop in the probability to be a caregiver that seems borderline significant. No significant changes appear for the two other outcomes. This does not come as a surprise, given the visible noise in the outcome variables. Only a relatively small share of women performs informal care. These women are dispersed in bins which are based on a limited sample size. Each point represents a small number of individuals, which leads to statistical uncertainty. To reach a more precise understanding of these effects, we turn to a multivariate ERA reform analysis procedure.

5.2.2. ERA reform analysis: main specification

The key parameter of interest in the ERA reform analysis that is shown in subsequent tables measures the effect of being born from Jan 1, 1952 onward.³¹ All estimations are performed employing robust standard errors that are clus-

³⁰ We include linear trends for the respective outcome separated by birth dates before and after Jan 1, 1952 on a 24-month BW and with a 90% confidence interval in these figures.

³¹ We use Stata's *rdrobust* package to estimate the non-parametric local polynomial procedure (Calonico et al., 2017). We include the age of the individual and year of reporting as covariates in all specifications and control for marital status, cohabitation with a child and educational attainment. In the baseline specification, we use a 24-month BW, which accumulates to a 48-month window of birth-months around the cutoff date (women born from Jan 1, 1950 to Dec 31, 1953). In the main tables, we present parameter estimates obtained from OLS (upper panel) and a local polynomial estimator (lower panel). In columns (1) and (3) we include additional covariates and in columns (3) and (4) we include quadratic global (OLS) or local (local polynomial) trends. In the baseline local polynomial estimation, we employ a triangular kernel. In addition, further robustness checks employ an Epanechnikov kernel and different BW's.

²⁹ We provide a number of additional robustness checks and a comprehensive discussion of the bandwidth tests in online Appendix C.3.

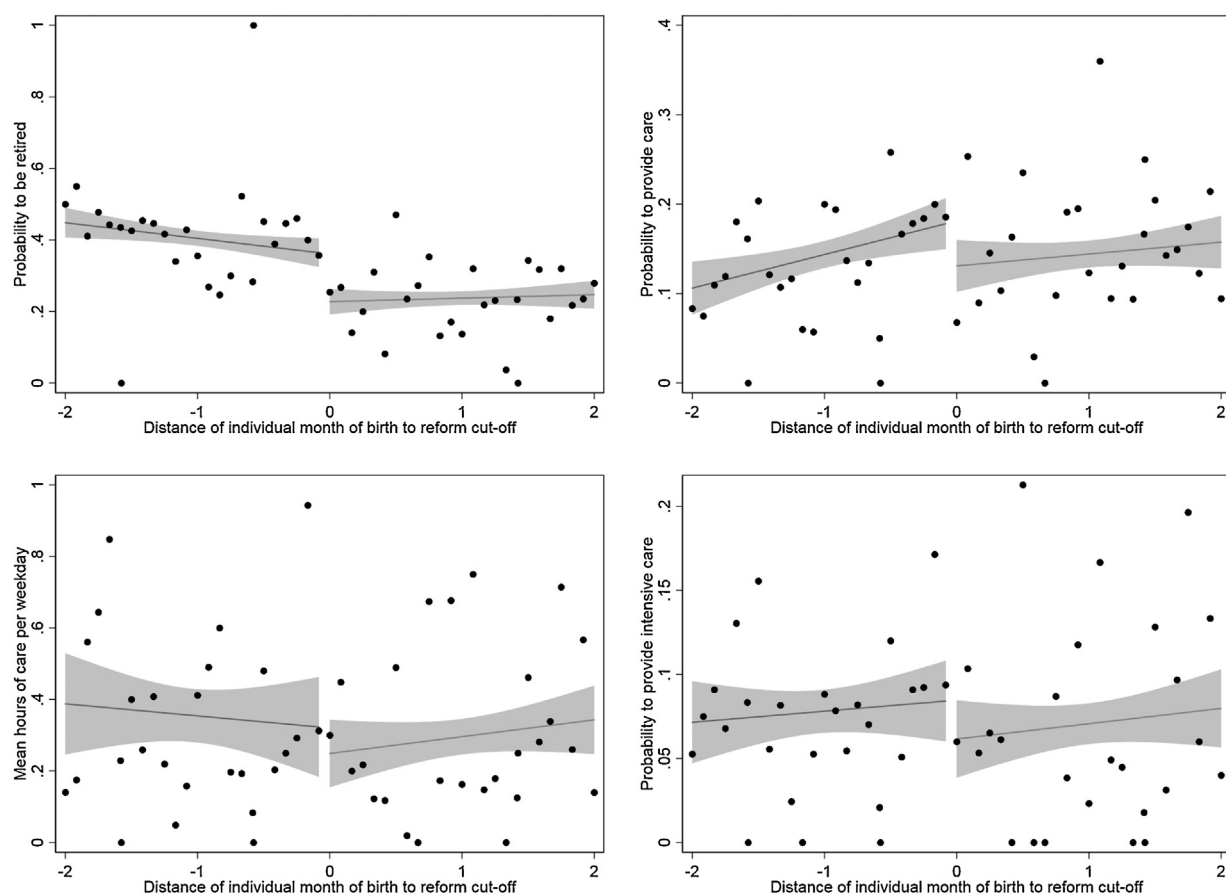


Fig. 4. Retirement and caring behavior around the reform cut-off (Jan 1, 1952).

Note: Each dot represents the mean of the outcome per bin (difference of individual birth-month to Jan 1, 1952); women aged 60–62; Linear trend and 90% confidence interval included.

tered on a quarter of year of birth level to take into account the panel dimension of the data.³²

Using a 24-month BW, we find a significant (5% level) 5.8 pp decrease in the probability to be a caregiver in the group of women born from Jan 1, 1952 onward in comparison to women born before the cutoff (Table 3, column (1), upper panel). This parameter is not altered significantly, when the non-parametric procedure is employed, or further covariates and a quadratic trend are included (Table 3, columns (2)–(4)). Effect sizes are slightly bigger in these robustness tests. Hours of informal care are reduced due to the reform by about 0.07 hours per normal weekday (Table 3, columns (5)–(8)). The probability to be an intensive caregiver decreases in this group by 2.3 pp (Table 3, columns (9)–(12)). Those two parameters, however, are not significantly different from zero. The magnitude of effects, however, is largely insensitive to these checks. The estimated effect sizes make up less than one third, or one fourth of a standard deviation at the extensive margin. They amount to a 34% decrease in the probability to pro-

vide informal care compared to the expected probability of informal care provision in the absence of the reform.³³

Based on a 12-month BW, the probability to be a caregiver is also reduced significantly (10%-level), while effect sizes and significance are more sensitive with the smaller sample size (Table A14 in the online Appendix). Using a 36-month BW, effects on the main outcome are very similar to the 24-month bandwidth (Table A15 in the online Appendix). We cannot identify significant effects on the hours of daily care provision or the provision of intensive care. Sign and magnitude of the estimates are, however, not altered much.

The next specification is designed to decompose the extensive margin effect without relying on the implicit linearity assumption in the estimates for hours of informal care. We construct a categorical variable, stating whether no care, non-intensive care (1 hour per normal weekday), or intensive care (at least 2 hours per normal weekday, at least 10 hours per week) is provided. A multinomial logit model is estimated, employing the reform strategy as out-

³² We follow Lee and Card (2008). Cluster robust standard errors are calculated according to Calonico et al. (2014) in the non-parametric estimations.

³³ We estimate the model on a sample of women not affected by the reform (women born until December 1951) and predict the probability to be a caregiver for women born at the cutoff.

Table 3

ERA reform analysis: effects on informal care provision.

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
|------------------|-----------------------------|---------------------|----------------------|---------------------|-------------------|---------------------|-------------------|-------------------|---------------------------------------|-------------------|---------------------|--------------------|
| | Probability to provide care | | | | | Daily hours of care | | | Probability to provide intensive care | | | |
| OLS | −0.058** (0.022) | −0.055** (0.023) | −0.069** (0.028) | −0.067** (0.031) | −0.073 (0.101) | −0.068 (0.103) | −0.107 (0.151) | −0.105 (0.147) | −0.023 (0.017) | −0.021 (0.020) | −0.035 (0.023) | −0.032 (0.026) |
| Local polynomial | −0.062** (0.026) | −0.061** (0.027) | −0.083*** (0.031) | −0.083** (0.032) | −0.086 (0.101) | −0.091 (0.107) | −0.175 (0.150) | −0.175 (0.157) | −0.027 (0.018) | −0.025 (0.020) | −0.049** (0.023) | −0.047* (0.026) |
| Observations | 2412 | 2397 | 2412 | 2397 | 2412 | 2397 | 2412 | 2397 | 2412 | 2397 | 2412 | 2397 |
| Polynomial | 1 | 1 | 2 | 2 | 1 | 1 | 2 | 2 | 1 | 1 | 2 | 2 |
| Controls | YES | YES+ | YES | YES+ | YES | YES+ | YES | YES+ | YES | YES+ | YES | YES+ |
| Pre treat. pred. | 0.170 | 0.170 | 0.184 | 0.184 | 0.310 | 0.310 | 0.385 | 0.385 | 0.082 | 0.082 | 0.112 | 0.112 |

Notes: Main effects and robustness checks, 24-month BW, OLS and local polynomial estimator using a triangular kernel.

Cluster robust (clustered on the quarter of year of birth level) standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

BW: bandwidth; Pre treat. pred.: pre treatment prediction; Control variables: YES (Age of individuals and year of questionnaire), YES+ (Age of individuals and year of questionnaire, marital status, children in the household, high education dummy).

lined above on a 24-month BW. We find that women born from 1952 onward show a 5.6 pp higher probability to provide no care and a 3.2 pp lower probability to provide 1 hour of informal care per weekday.³⁴ The probability to provide intensive care is hardly reduced and the parameter estimate is not statistically significant.

In the last step we compute Wald-estimators that provide retirement effects on informal care provision using the ERA reform.³⁵ This allows for a more direct comparison between results from the ERA reform analysis to the ERA discontinuity analysis (Section 4.1 above). Wald estimates provide the effect for women aged 60–62 who exercised an early retirement option, compared to women in the same age-range who did no longer have the right to retire before the age of 63 because of the pension reform. We find that retirement increases the probability to provide care by 47 pp (hours are increased by 0.6, intensive care provision by 19.3 pp; see Table A17 in the online Appendix). One reason why effects are larger compared to the ERA discontinuity analysis is the smaller sample size. The interpretation of IV estimates in the ERA reform analysis is impaired by weak instrument problems. In addition, treatment and control groups are different for both identification approaches. We compare women along the age distribution in the ERA discontinuity analysis, whereas treated and non-treated women are in the same age-range in the ERA reform analysis.

5.2.3. ERA reform analysis: heterogeneity

First, we split the sample along the following characteristics: high versus low education and women with high vs. women with low labor market attachment (Table 4, columns (2)–(6)).³⁶ We find that highly educated women

react stronger (11 pp decrease) to the reform at the extensive margin. The parameter is vaguely statistically significant (10%-level), as the sample size is strongly reduced (Table 4, column (2)). Lower educated women do not react to the reform. Geyer et al. (2020) find that highly educated women have a higher probability to stay in the labor market due to the reform. This is consistent with our results, where highly educated women experience larger effects on their informal care provision.

Women that are more attached to the labor market (those who are employed before age 60) exhibit slightly larger effects with comparable standard errors (column (4)). The reference group with low labor market attachment does not react (column (5)). We also estimate the ERA reform effect on a group of women that are proxied to be eligible for women's pension at age 60, regardless of their birth cohort. We find a 7 pp reduction for this group, which is slightly bigger than our main result. This estimate is closest to an average treatment effect on the treated (ATT).

Results for hours of informal care and intensive care provision are mixed (Table 4). We find highly educated women to react significantly and stronger to the reform in their probability to provide intensive care. Labor market attachment seems to be an important driver in the ERA reform effect on the care taking decision.³⁷

5.2.4. ERA reform analysis: robustness

The ERA reform analysis is prone to uncertainty due to the small sample applied. We also find some differences in covariates (e.g. marital status and educational attainment) between the cohorts. None of the important covariates, however, react as robustly as the main outcome variables (caring behavior and retirement). We discuss this in the online Appendix³⁸ and provide several robustness checks.

In the first set of robustness checks that can only be carried out for the ERA reform analysis, we conduct three different 'placebo tests' (Table A18 in the online Appendix). We apply the reform approach to the following groups: women aged from 57 to 59, i.e. women not impacted by

³⁴ Table A16 in the online Appendix provides estimation results from the multinomial logit regression in rows 3 and 4. Parameters can only be interpreted qualitatively. The base category is 'no care provision'. Table A16 in the online Appendix also includes the marginal effects of switching from being born before 1952 to being born from 1952 onward for the 3 categories.

³⁵ We calculate the quotient of the effect of the ERA reform on care provision and the effect of the ERA reform on retirement.

³⁶ Women aged from 55 to 60 with high labor market attachment do not report unemployment spells. The residual category consists of women with low labor market attachment.

³⁷ A more comprehensive discussion and additional heterogeneity analyses are provided in online Appendix C.5.

³⁸ A comprehensive discussion of the validity of RDD assumptions is provided in online Appendix C.4.

Table 4

ERA reform analysis: heterogeneous effects on informal care provision.

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---------------------------------------|---------------------|----------------------|-------------------|----------------------|-------------------|----------------------|
| Probability to provide care | | | | | | |
| OLS | −0.058** (0.022) | −0.110 (0.054) | −0.033 (0.028) | −0.069** (0.027) | −0.012 (0.096) | −0.070** (0.028) |
| Local polynomial | −0.062** (0.026) | −0.133** (0.058) | −0.024 (0.032) | −0.086*** (0.023) | 0.045 (0.119) | −0.080*** (0.029) |
| Pre treat. pred. | 0.170 | 0.183 | 0.166 | 0.179 | 0.146 | 0.201 |
| Daily hours of care | | | | | | |
| OLS | −0.073 (0.101) | 0.083 (0.276) | −0.115 (0.130) | −0.166 (0.095) | 0.304 (0.332) | −0.084 (0.128) |
| Local polynomial | −0.086 (0.101) | −0.112 (0.205) | −0.032 (0.120) | −0.204** (0.102) | 0.440 (0.375) | −0.136 (0.141) |
| Pre treat. pred. | 0.310 | 0.065 | 0.401 | 0.374 | 0.088 | 0.371 |
| Probability to provide intensive care | | | | | | |
| OLS | −0.023 (0.017) | −0.082** (0.039) | 0.006 (0.028) | −0.034 (0.024) | 0.023 (0.067) | −0.031 (0.024) |
| Local polynomial | −0.027 (0.018) | −0.118*** (0.034) | 0.021 (0.024) | −0.048** (0.023) | 0.066 (0.078) | −0.044* (0.026) |
| Pre treat. pred. | 0.082 | 0.099 | 0.076 | 0.090 | 0.049 | 0.097 |
| Observations | 2412 | 735 | 1677 | 1873 | 539 | 1878 |

Notes: 24-month BW, OLS and local polynomial estimator using a triangular kernel and linear trend.

Column (1): All women aged 60–62 in the BW; Column (2): Highly educated women; Column (3): Lower educated women; Column (4): Women retiring from employment; Column (5): Women with unemployment spells; Column (6): Women eligible for women's pension (in absence of the reform; proxy); BW: bandwidth; Pre treat. pred.: pre treatment prediction; Control variables: Age of individuals and year of questionnaire.

Cluster robust (clustered on the quarter of year of birth level) standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

the reform (column (1)), an alternative cutoff date set at Jan 1, 1951 (column (2)), and another cutoff date set at Jan 1, 1953 (column (3)). No other reforms apply to these two virtual birth date cutoffs. We find neither for the placebo age group, nor for the two placebo cutoffs that the probability to provide informal care changes significantly. The same holds for the probability to provide intensive care.

In another robustness check, we analyze the reduced form-effect of crossing the age threshold 60 on informal care behavior, differentially for women born before and after Jan 1, 1952. The expectation would be that, while women born before 1952 increase caregiving when crossing the age threshold (due to retirement behavior at the threshold, see ERA discontinuity analysis), women born later do not show this behavior. Tables A19 and A20 in the online Appendix depict that exactly this pattern emerges in the data. This is further confirmation that we pick up substantive effects, arising due to the reform.³⁹

5.3. Substitutability and demand for formal care

Several studies have looked into the question whether informal and formal care can be viewed as substitutes, finding mixed results. Van Houtven and Norton (2004) find that informal care reduces the utilization of formal home health care and potentially delays the entry into nursing homes for frail elderly individuals. Bonsang (2009) shows that informal care may substitute formal paid domestic help.

However, the relationship gets weaker as the health status of the frail person deteriorates. Informal care is a substitute for formal care only when tasks require low skills. According to Bell et al. (2007) and Karlsberg Schaffer (2015) who exploit a reform in Scotland that introduced free formal care, informal and formal care are no substitutes.

Our empirical analysis of substitutability is limited by data restrictions on the demand for formal care. However, based on the ERA reform analysis, we can assess the ERA reform effect on different types of care received from within and outside the household. The SOEP provides information at the household level, whether a given household is inhabited by a care-dependent person. It is also asked, how this care is provided, namely from within or from outside the household. This information on care activities is asked in the household questionnaire. It is different from the individual information, we utilized for our analysis of informal care supply. We use this additional household-level information to tease out, whether care from outside the household substitutes care provided from within the household, studying the care mix in households with a care-dependent person that are also inhabited by a woman that is affected by the reform. Since we need to match households with affected women and a care-dependent person, this exercise is based on an even smaller sample than our main ERA reform analysis.⁴⁰

First, we confirm our main result based on the alternative household information: due to the reform, the

³⁹ Additional robustness checks are reported and discussed in online Appendix C.6.

⁴⁰ More details on the composition of the estimation sample and on the estimation procedure can be found in online Appendix C.7.

Table 5

ERA reform analysis: effects on caregiving in care households by household size.

| | (1) | (2) | (3) | (4) | (5) | (6) |
|-----------------|---------------------|---------------------|-------------------|-------------------|-------------------|------------------|
| Outcome | Inside care | | | Outside care | | |
| Household size | All | >2 Persons | 2 Persons | All | >2 Persons | 2 Persons |
| Treated | −0.093** (0.046) | −0.132** (0.057) | −0.022 (0.081) | −0.018 (0.076) | −0.144 (0.099) | 0.097 (0.135) |
| Observations | 404 | 215 | 189 | 404 | 215 | 189 |
| Mean of outcome | 0.903 | 0.898 | 0.910 | 0.250 | 0.247 | 0.254 |

Notes: Cluster robust (clustered on the household level) standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

60 month Bandwidth; All: individuals from all households as described; >2 Persons: only households as described with more than 2 members; 2 Persons: Only households as described with less or equal than 2 members; Outside care: outside (formal or informal) care provided within the household; Inside care: inside (informal) care provided within the household; Data: SOEP data; Further covariates: distance in birth months to cutoff (treated person in the household).

probability that care from within the household is provided sinks by nearly 10 pp starting from a mean level of 90% in these households (Table 5, column (1)). The effect is driven by households inhabited by more than 2 persons (column (2)). No significant effect is found in households inhabited by only 2 persons (column (3)). This result is consistent with the average reduction by 6 pp we found for informal care supply. Second, we do not find a significant effect of the reform on the probability that care is provided from outside the household (Table 5, column (4)). Moreover, there are no significant differences by household composition. Estimates are not different for households inhabited by more than 2 people (column (5)) and households inhabited by 2 people (column (6)).

These results are consistent with our findings in the two estimation procedures that form our hypothesis: According to our main ERA reform analysis finding, the removal of the ERA at age 60 significantly reduces the supply of informal care for women affected by the reform (Table 3). ERA discontinuity analysis results also show that women retiring at their ERA provide significantly more informal care within their household (Table 2). As women affected by the reform should prolong their working life and reduce care provision, we expect a reduction of care from within the household. Provided that informal and formal care are substitutes, an increase in care from outside the household can be presumed. Not all, but a sizable share of care received from within the household should come from affected women. Likewise, not all but a significant amount of informal care by affected women should be provided within their household. A significant negative effect on care received within the household validates our previous findings based on individual-level information on informal care supply. Moreover, we do not find evidence that outside care is substituted, when informal care is reduced from within the household. One potential interpretation which would be in line with Bell et al. (2007), Karlsberg Schaffer (2015) is that less overall care is provided which might have consequences for care recipients (see Section 5.4). An alternative explanation could be that the necessary amount of care is provided more efficiently (Bonsang, 2009).

Why do we find a differential reaction by household size? In two-person households, care-dependent individuals are rather in an age range between 60 and 80 years. These care recipients are mostly spouses of the main care provider. In bigger households, care receivers are more

often parents of caregivers and sometimes children. They are either younger (in case they are children; 30–50 years of age), or older (80+ years of age) persons. The health status of care-dependent individuals in two-person households is also slightly worse than in bigger households. Care provided in smaller households might, therefore, be less flexible. Additional explanations might contribute to this finding but cannot be substantiated with our data.

5.4. Implications for caregivers and care receivers

Our main empirical results have further implications for caregivers and care receivers. First, we established that employed women indeed face a time conflict, when they are (suddenly) confronted with demand for informal care. ERA reform analysis results revealed that when women are forced to work longer, they provide mostly less low-intensive care. Women providing high-intensity care do either not want to, or cannot reduce care hours (Tables 3 and A16). Consequently, more, high-intensity caregivers face a double burden of labor supply and informal care provision. Demographic trends, structural change on the labor market, and pension reforms, aggravate this problem. Previous literature has shown that this enlarged double burden has negative health consequences for caregivers. Schmitz and Stroka (2013) investigate the consequences that the provision of informal care in combination with labor supply has on caregivers' health. They find that the double burden of informal care provision and full-time work increases the intake of drugs. They conclude that informal care may have deteriorating health effects for caregivers, if they need to work and generate labor earnings at the same time. Bremer et al. (2015) and Hiel et al. (2015) come to similar conclusions. Unfortunately, the identification of health effects that result from a double burden is beyond the scope of this paper. Our research design does not allow to also identify health effects. We cannot disentangle the impact of the double burden from the direct effect of retirement on health (Eibich, 2015).

Findings from the ERA discontinuity analysis imply and the ERA reform analysis results confirm that women who are not allowed to retire early and must work until later ages provide less informal care. We also find that low-intensive informal care is not compensated by care from outside the household. This means that care-dependent persons receive less overall informal care. Increased labor

market participation and prolonged working lives of women in combination with less early retirement options reinforce this effect. Several studies point out the importance of (low-intensity) informal care for health outcomes of care recipients. Hu and Li (2020) document that the supply of informal care may reduce the progression of functional limitations among elder people. Especially low intensity care is found to have these effects. Wu and Lu (2017) show furthermore that informal care can also improve the health behavior of the elderly, especially among those with chronic diseases. This in turn has important positive health effects, especially for chronically ill persons. Chon et al. (2018) find that social interaction is beneficial for the health outcomes and the progress of frailty among elders. Similarly, the well-being of care receivers may suffer, when low intensity informal care is not available. Social interactions with known persons seem to play a particularly important role.

6. Conclusion

Consistent findings based on two identification approaches suggest that the time conflict between labor and informal care provision is an economically relevant constraint for women before they reach eligibility for pension benefits. When women retire at ERA thresholds, they increase their supply of informal care significantly and substantially. Individuals with a more binding time conflict (full-time employed, highly educated) react stronger. The pension reform that increased women's ERA significantly reduced the supply of informal care by the affected group. Women are forced to prolong their working lives or become unemployed. Those, who would have provided low intensity care in early retirement, react strongest to the reform. Women, who decide to provide high-intensity care themselves, do this irrespective of their labor market situation. Formal care from outside the household does not substitute low intensity informal care. Thus, care-dependent persons receive less care overall and high-intensive caregivers face a double burden of market and care work.

What are the broader implications of our findings for LTC, healthcare, and pension policy? Induced by societal change and promoted through equal opportunities policies, female labor market participation is on the rise. Problems for informal care provision, especially for women as main caregivers, are foreseeable. When early retirement is not an option, our findings could also be valid for other age groups retiring at the designated normal retirement age. Prevalent increases of the pensionable age would then have similar implications for care supply. Future reforms of pension systems need to consider that this reduces women's informal care provision. Care-dependent persons receive less low-intensity care, e.g. in form of social interactions with relatives. Given distinct preferences for informal (vs. formal) care, it is likely that this impairs their well-being, or worsen their frailty and health. Future research needs to assess who would be primarily affected and to what extent. These trends also increase the double burden for caregivers which has detrimental effects on their health.

On the other hand, by introducing care-times for employees, policy-makers have reacted to rising employment-related barriers to informal care provision. Parts of these reforms were not yet enacted, or hardly taken-up by care receivers during the period analyzed in this paper. Further research is required to investigate whether such new LTC policies can effectively diminish the negative relationship between labor supply and care provision around retirement by dissolving the underlying time conflict. Additional research is also warranted that goes deeper into the question whether different types of formal and informal care are substitutable. Collecting more and better data on these topics would be a good starting point.

Supplementary Material. Supplementary Data

Supplementary material associated with this article can be found, in the online version, at <https://doi.org/10.1016/j.jhealeco.2020.102350>.

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