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Time to care?

The effects of retirement on informal care provision

Björn Fischer* and Kai-Uwe Müller

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This paper analyzes the impact of a reduction in women's labor supply through retirement on their informal care provision. Using SOEP data from the years 2001-2016 the analysis addresses fundamental endogeneity problems by applying a fuzzy regression discontinuity design. We exploit early retirement thresholds for women in the German pension system as instruments for their retirement decision. We find significant positive effects on informal care provided by women retiring from employment at the intensive and extensive margin that are robust to various sensitivity checks. Women retiring from full-time employment, highly educated women and women providing care within the household react slightly stronger. Findings are consistent with previous evidence and underlying behavioral mechanisms. They point to a time-conflict between labor supply and informal care before retirement. Policy implications are far-reaching in light of population aging. Prevalent pension reforms that aim to increase life-cycle labor supply threaten to reduce informal care provision by women and to aggravate the existing excess demand for informal care.

Keywords: retirement; informal care; regression discontinuity; age threshold

JEL classification: J22; J14; J18; J26; H43; H55

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1 Introduction

Population aging creates problems for various pillars of modern welfare states. A higher life expectancy increases the age dependency ratio as well as the individual risk to become care dependent (Gusmano and Okma, 2018). The number of elderly persons in need of care grows faster than the group of potential care providers which puts pressure on the care system (Broese van Groenou and De Boer, 2016; Geerts et al., 2012). A rising age dependency ratio leads to increases in social security expenses and urges changes in pension schemes (OECD, 2017; McGrattan and Prescott, 2017). In a prevalent reform strategy policy-makers aim to boost employment rates by prolonging working lives and increasing the labor market participation of underrepresented groups, most notably those of women. This could, however, create a conflict of objectives with the rising demand for private home care, often called informal care, that has been largely neglected so far.

Our paper tackles this looming trade-off as one of the first studies on the causal effect of the retirement decision on informal care provision. Informal is usually preferred over professionalized care by care recipients and their relatives. It is also favored within institutionalized long-term care (LTC) systems for cost reasons (Lipszyc et al., 2012; Blaise, 2018; Mentzakis et al., 2009). The German LTC insurance is an example (European Commission, 2016). Around 48% of currently care-dependent persons are cared for in their own homes exclusively by family and friends (Wetzstein et al., 2015). About two thirds of the 4 to 5 million informal caregivers in Germany are women. The highest shares of care providers are found around retirement (about 12% in the age group between 55-69 years vs. at most 8% in any other age group; see Wetzstein et al., 2015 or Geyer and Schulz, 2014). Age differences within marriages and the lower life-expectancy of men are explanations for the gender gap in the prevalence of informal care (Meyer, 2006). Men also rarely take on care responsibilities for close persons other than their wife.

In order to assess whether a conflict of objectives between retirement and care policy is indeed impending, our analysis is focused on women as primary caregivers. We investigate whether women increase informal care provision when retiring from their early retirement age (ERA). Women have not only used these thresholds extensively (Geyer and Welteke, 2017; Keck and Krickl, 2013). They also exhibit the highest care rates in this age range (Wetzstein et al., 2015). We estimate a causal effect employing the ERA of women as instruments for retirement behavior (Battistin et al., 2009; Eibich, 2015). From the causal effect of retirement on care we conclude whether gainful employment may crowd out informal care before retirement becomes a viable option. This is of great importance for the sustainability of LTC insurance systems in connection with future reforms to the retirement scheme. Policy makers would face a trade-off between policy goals: The prevalent reform strategy to extend the working life and increase female labor force participation threatens to diminish the supply of informal care.

Such a crowding out effect might occur when individuals who are confronted with the demand for care face a *time conflict*. This term refers to the decision problem between the supply of labor and informal care subject to a budget constraint. People may refrain from taking on care activities because they depend on their market income (Al-Janabi et al., 2018). Ceteris paribus opportunity costs of home production (including informal care) are higher for employed people. As soon as they are eligible for retirement benefits individuals may re-evaluate their care decision. There could be a positive causal effect of reaching the ERA on informal care provision. Alternatively, the supply of informal care may be inflexible and not adjusted upon early retirement. The time conflict would then be solved by reducing labor supply or time in other activities (leisure, other home production). Care activities are in this case taken up regardless of opportunity cost arguments. It is therefore an empirical question whether the described relationship exists.

This paper circumvents the endogeneity problem inherent in simultaneous decisions on the supply of labor and informal care by exploiting women's ERA thresholds as instruments for retirement (Battistin et al., 2009). Age cutoffs for early retirement are defined with the German pension legislation as one of different paths to go on pension. Depending on the cohort the ERA in Germany is 60 or 63. In 2000 about 37% of retiring women made use of ERA rules for women (Keck and Krickl, 2013). As the crossing of an cutoff is solely determined by age and thus exogenous, we can utilize related changes in the choice set and budget constraint within a fuzzy regression discontinuity design (RDD). A threshold serves as instrument for the individual retirement decision. This approach deals with reverse causality and selection on unobservables. The necessary assumption that individuals cannot select into one of the age groups (being older or younger than the cutoff) holds by definition.

We estimate the effect of this negative labor supply shock on informal care provision using data from the German Socio-Economic Panel (SOEP). The SOEP is one of very few data sets containing comprehensive information on the labor market status, the retirement age and state as well as the provision of informal care (Goebel et al., 2018). This allows us to assess changes in informal care activity as well as effect heterogeneity in terms of working hours, location of care provision and educational attainment of the care provider. To the best of our knowledge this is the first paper on the causal effect of retirement on care provision with clean identification based on retirement thresholds. We provide evidence for this causal mechanism by showing theoretically consistent effect heterogeneity patterns. As the legal retirement age in Germany was increased recently, our results contribute to a discussion on the potential impact of such pension reforms for future supply of informal care.

We find a significant and robust increase of previously employed women's informal care provision upon retirement at their ERA. Care hours rise on average by about 0.8 hours and the caregiving probability by about 13 percentage points on a weekday. Effects are of similar magnitude, but more significant when care is provided within the own household. Women retiring from full-time employment and highly educated women react slightly stronger. The findings are consistent with underlying behavioral mechanisms and previous evidence on informal care provision in Germany. The time-conflict is larger for full-time employed women. Highly educated women have a higher labor market attachment and propensity to supply care.

The paper is organized as follows: After a brief outline of the related literature (section 2) we characterize the relevant institutional features of the system of formal and informal care as well as the state pension system in Germany (section 3). The data set is introduced and a description of the sample and variables is given in section 4. The identification strategy is presented in section 5. We sketch reasons for endogeneity and explain how retirement age cutoffs are utilized as instruments for the retirement decision in a fuzzy RDD. We discuss instrument relevance and validity as well as threats to identification. Empirical results are presented in section 6. We start with our main specification for women retiring at their respective ERA threshold. We then discuss effect heterogeneity, effects for a homogenous sample with a single cutoff at 60 years, various robustness tests, and a comparison with older women and men. Section 7 discusses these findings and concludes.

2 Related literature

This paper contributes to the empirical literature relating labor supply, retirement decisions, and the provision of informal care. Lilly et al. (2007) and Bauer and Sousa-Poza (2015) provide reviews on the impact of informal care on labor supply. Reverse causality (Ettner, 1995; Michaud et al., 2010) and selection on unobservables (He and McHenry, 2016) may arise. Thus, research designs have been developed and evidence has been provided for causal effects in both directions.

One line of research investigates whether the provision of informal care affects retirement. Dentinger and Clarkberg (2002) find retirement odds to be higher by a factor of 5 for caregiving wives, while caregiving husbands retire at a later age. Estimating the impact of caregiving on retirement in Germany, Meng (2012) shows effects for women are stronger and that men are affected only by care intensity. Schneider et al. (2013) show that the physical burden, not the time spent in care drives intentions to exit the labor market. Van Houtven et al. (2013), Jacobs et al. (2017), Carr et al. (2018) and Niimi (2017) report that informal care providers have ceteris paribus a higher probability to be in retirement in the U.S., the U.K. and Japan. Geyer and Korfhage (2018) make use of the introduction of the German LTC insurance system and point to the time conflict between informal care provision and gainful employment. A related branch of literature relying on instrumental variables estimates direct labor supply effects of informal care. Carmichael and Charles (1998), Carmichael and Charles (2003a), Carmichael and Charles (2003b), Heitmueller (2007) and Schmitz and Westphal (2017) are prominent examples who all confirm a negative causal impact of care activities on gainful employment.

Evidence for an impact of informal care on retirement and thus on labor supply does not mean that the opposite effect holds. The aforementioned studies take the decision to provide informal care as exogenous. Informal caregivers tend to retire earlier. When facing the demand for care, individuals may decide irrespective of their labor force status and trade off informal care against other time uses. Besides, a transition into retirement is not only an adaption in the time spend on the labor market, but implies a status change. So underlying mechanisms could be different.

A smaller strand of the literature focuses on the reverse effect of labor market participation on informal care finding mixed results. Various methods are applied to deal with endogenous labor supply in a model for the provision of care. Stern

(1995) uses employment histories as instruments for current labor supply without finding an effect on informal care. Golberstein (2008) exploits a policy reform with a Difference-in-Differences (DiD) estimator and finds negative effects of women's labor supply incentives on the probability of co-residing with a disabled parent. Boaz (1996) and Doty et al. (1998) use age, education and number of children as exclusion restrictions in simultaneous-equation models. Boaz (1996) finds substantial, Doty et al. (1998) only limited effects on care provision. Using regional unemployment rates and industry structure in an instrumental variables (IV) approach Nizalova (2012) finds high negative effects of wages on care provision. In a similar IV framework He and McHenry (2016) find that for women of prime caregiving age (40-64) an increase of weekly working hours by 10% is associated with a reduction in caregiving probability of 2%. Those studies are based on U.S. data.

Applying a dynamic probit model on Dutch data Moscarola (2010) finds that prior employment reduces the caregiving probability by 2.4%. Berecki-Gisolf et al. (2008) cannot show an impact of the employment status on later caregiving uptake on Australian women. However, caregiving women show a negative correlation between hours previously spent in paid employment and caring hours. Carmichael et al. (2010) analyze the effect for the UK and find that employment and earnings impact informal care provision negatively. Mentzakis et al. (2009) also report negative effects of employment on informal care, but a positive effect of income and wealth. Michaud et al. (2010) estimate both directions of the relationship between employment and care for England simultaneously uncovering a negative effect of employment on future co-residential and extra-residential caregiving.

A prominent strand of literature is concerned with the substitutability of formal and informal care (Hollingsworth et al., 2017). Findings support the interpretation that the decision to provide informal care is not only influenced by the budget constraint, but also by other factors, e.g. the necessity to provide this particular form of care. In terms of identification our work is also related to a broader research that exploits age thresholds in retirement legislation and estimates causal effects of (early) retirement. Outcomes include the individual health status (Eibich, 2015; Müller and Shaikh, 2018) or consumption decisions (Battistin et al., 2009; Moreau and Stancanelli, 2015).

3 Institutional setting

The German system of social insurances consists of five pillars: State Pension Insurance, Health Insurance, Accidence Insurance, Unemployment Insurance and since 1995 also State Long-time Care (LTC) Insurance. In the following we will sketch the features of the LTC and pension insurance systems that are most relevant for our empirical analysis.

3.1 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. Benefit eligibility is defined only with respect to individual care needs: If a person needs help with at least two activities of every day life (cooking, mobility, etc.) for not less than 45 minutes per activity a day and he or she additionally needs support in household maintenance, benefits are granted. In sum, a person has to be in demand of 90 minutes of care per day. 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. Those benefits start from 326€ in care level II and go up to 901€ in care level V.[1] Care receivers are free to spend the 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.

A number of current laws (e.g. the 'Pflegezeitgesetz' or the 'Familienpflegezeitgesetz') promote the compatibility of informal care and gainful employment.[2] Con-

[1] The exact benefits can be accessed via BMG (2017, 2018).

[2] Since 2008 the 'Pflegezeitgesetz' guarantees anyone working in a firm with 15 or more employees to be released temporarily (6 months at the maximum) on a part- or full-time basis when the demand for care arises (BMJ, 2008). Introduced in 2012 the 'Familienpflegezeitgesetz' allows to further reduce the working time to a minimum of 15 hours per week for up to 24 months when employees perform care for close relatives (BMJ, 2011). It includes a loan-like instrument to absorb the related income shock. The 'Act to Strengthen Long-Term Care' from 2015 bolstered the financial basis of the LTC system and provides carers with the opportunity to take time off

ditions 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.[3] This paper only provides indirect evidence on the effectiveness of these policies. The different laws do not affect our identification strategy. Improvements in the institutional framework over time could, however, reduce the size of the estimated effects if take-up increases.

3.2 The state pension system in Germany

The German old-age provisions system consists of three pillars: state, employer-based, and private pension insurance schemes. In spite of efforts to increase the prevalence of private schemes, the state pension system is by far the most important pillar. In 2015 the total sum of old-age provisions amounted to 278 billion €, 74% of which originate from the Statutory Pension Insurance Scheme (Gesetzliche Rentenversicherung, GRV). When private income (from interest, rentals etc.) is included, state pension plan benefits still make up 63% of overall net income in retiree households (BMAS, 2016).

Certain paths into retirement through the German state pension that differ for men, women, and different cohorts are crucial for our identification approach.[4] Eligibility for retirement benefits mainly depends on the number of years with paid contributions including periods in employment, with voluntary contributions, or recognized non-income periods. The GRV states six paths into retirement differing in the defined normal retirement age (NRA) or early retirement age (ERA).[5] For identification of our main effect we refer to different ERA thresholds for women in the time period 2001-2015:

(i) People who have acquired 35 years of contributions can retire early at the age of 63, but face benefit reductions.[6]

their care duty for holidays and in cases of illness. It secures a 10 day job leave with benefits in emergency situations to organize the caretaking arrangements.

[3] Firms are not obliged to register the take-up of these instruments which is why official statistics do not exist. According to the German government the take-up of benefits for the 'Familienpflegezeitgesetz' amounted to 219 persons between 1 January 2015 31 May 2016 (Deutscher Bundestag, 2016).

[4] See Boersch-Supan and Wilke (2004) for details on the German pension system. Geyer and Welteke (2017) provide an extended overview including the 1999 pension reform and alternative paths into retirement.

[5] An overview is given in GRV (2017a).

[6] Since 2014 people who have been born before 1953 and have 45 years of contributions can retire

(ii) Women born before 1952 could retire early at the age of 60 if they fulfilled the contribution criteria and were willing to accept benefit deductions.[7] In 2012 the last cohort was eligible for early old age pensions for women at 60.

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

Geyer and Welteke (2017) and Geyer et al. (2018) show that the abolishment of women's retirement at age 60 led to a drop in the retirement probability in the group of 60-62 year old women born in the cohorts 1951 and 1952 of around 20 percentage points (pp). Employment increased in the group affected by the reform, yet unemployment and inactivity were likewise raised.

The NRA is 65 for women in our dataset. It defines the reference age for the calculation of deductions under early retirement.[8] Our sample includes the years 2001 to 2015 (sub-section 4.1). All aforementioned age thresholds are relevant for early retirement of women. For our main specification we pool women born before 1952 with later-born cohorts. We define the ERA accordingly at 60 years or 63 years. In an alternative estimation with a smaller, but more homogenous sample, we use only women born before 1952 and the applicable threshold at age 60 as an instrument.

4 Data, sample & variables

We use the German Socio-Economic Panel (SOEP). Since 1984 households and individuals have been followed on an annual basis to collect information on household structure and socio-demographic characteristics, working biography, income, attitudes, economic behavior, health etc. resulting in about 150 questions. Since 1990

without any deductions at the age of 63. This is not relevant for our observation period.

[7] These women need an accumulated 15 years of contributions, 10 of which have to be after their 40th birthday.

[8] Before 2012 the regular old age pension threshold for men was 65. Since then the regular old age pension threshold is gradually rising from 65 to 67 years for individuals born between 1949 and 1964.The data set contains no persons born after 1949 and aged 65 or older. The relevant old age pension age is 65 throughout our observation period.

East German households were added. The result is a representative panel study on about 44,000 individuals in around 13,000 households in 2016 (Goebel et al., 2018).

4.1 Sample construction

We identify retirement effects on the provision of informal care for women. We follow these women from 2001 to 2015 using SOEP wave v33. The underlying behavioral mechanism is the dissolution of an existing time conflict between labor supply and care as soon as the choice of retirement together with some form of pension benefits become available. If a person is non- or unemployed prior to retirement, there are no time (and/or potentially budget) constraints removed through the transition into retirement. We would not expect an impact on the supply of informal care under these circumstances.

Therefore, we eliminate unemployed women who are not yet retired from our main sample. To avoid sample selection around retirement these individuals are removed completely and all of their spells in later stages of retirement are discarded as well. Disabled individuals are also discarded throughout the empirical analysis as they face different choice sets with respect to retirement and care provision. As one dimension of the heterogeneity analysis we only include women retiring from full-time employment into the estimation sample.[9] For the comparative analysis of men we apply the same sample restrictions.

4.2 Definition of variables

Outcomes

The SOEP questionnaire contains a question on the allocation of time on a weekday. Since 2001 individuals can report the time spent on taking care of an adult person[10] in need.[11] As the hours-variable is self-reported it is likely that the information is not perfectly accurate.[12] In order to capture the extensive margin of informal care 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

[9] We define this as having worked on average 35 hours per week in the 3 years prior to retirement.

[10] Taking care of children is a separate question, so we can differentiate the two activities.

[11] The exact question is: "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?"

[12] 47% of informal caregivers supply 1 hour, around 24% state 2 hours, about 10% provide 3 hours, and the remaining about 18% perform 3 or more hours. A relatively high number of people (1.5%) provide more than 20 hours of care.

avoid linearity assumptions we also define a binary variable for intensive care. This variable is coded one when an individual provides more than 10 hours 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.

Treatment

We use self-reported data to determine whether a person is retired or not. Individuals can state in which months of the previous year they received an old-age pension. This data is matched to the respective year and compared to the exact month of the interview. There are several definitions used in the literature to define retirement (see, e.g., Coe and Zamarro, 2011; Insler, 2014). A RDD becomes more adequate when the retirement information is precise. Given the type of information available in the SEOP, this is the optimal definition to realize a precise age measurement at retirement. Doing so we can use the retirement information until 2015 as it is reported retrospectively until 2016. We can likewise use information on informal caregiving until 2015.

4.3 Sample description

Different samples in our empirical analysis are a function of the bandwidth choices around the age-cutoffs for estimation. For our main analysis we employ the ERA as instrument and use a bandwidth of five years before and after the cutoff, respectively. The sample thus consists of women aged 55 (60 years minus 5) to 68 (63 years plus 5) who retire from employment. The resulting sample includes 16,908 person-year observations for 2,624 women (Table 1). Around 50.4% of these women are in retirement. The share of retired is higher among caregivers (54.1%) than non-caregivers (50.0%). 20.4% of women in the sample live in single-person households with a share of 7.9% providing informal care. In multi-person households the share of caregivers amounts to 10.8%. More than 84% of all female caregivers live in multi-person households. About 79.1% of women who do not provide informal care live in multi-person households.

The mean age in our main sample is about 61.45 years (Table 1). Women provide on average about 0.24 hours of informal care per normal weekday. Around 10% of women in the sample supply positive care hours. About 47% of those informal

Table 1: Summary statistics, main sample: women aged 55 to 68, 2001-2015.

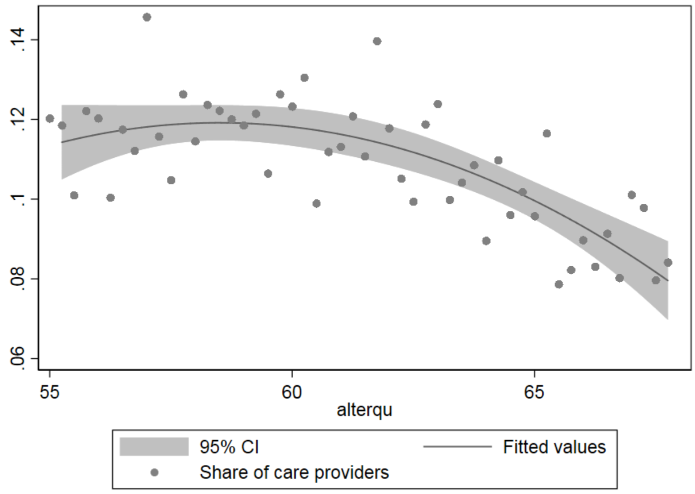
	Mean	S.D.	Minimum	Maximum
Outcomes				
Hours of Care	0.24	1.12	0	24
Caring Probability	0.10	0.30	0	1
Intensive Care	0.05	0.23	0	1
Covariates				
Retired	0.50	0.49	0	1
Age	61.45	3.89	55.08	68
Kids in HH.	0.13	0.43	0	5
Married	0.69	0.46	0	1
Education	12.16	2.82	7	18
Work. Hours	16.87	18.85	0	98
Health	2.71	0.84	1	5
Observations	16,908			

Notes: S.D.: Standard deviation; HH: Household; Work.: Working.
Source: SOEP v33, own calculations.

caregivers supply 1 hour, around 25% state 2 hours, about 10% provide 3 hours, and the remaining about 16.5% perform more than 3 hours of care. A relatively high number of people (0.8%) provide more than 20 hours of care. The mean retirement age is 62.0 for women in the sample. A large standard deviation in the number of working hours shows that we observe individuals at a point of their employment biography when they experience substantial changes in their labor supply. We find that of those women that provide care 80% are married. However, only a significant 5 pp higher probability to be married exists for carers than for non-carers. The probability to provide care is around 2 pp higher for married individuals. The probability to still have a parent that is alive is not significantly different among care providers (49% vs. 50%, respectively). The probability to provide care is slightly lower if both parents are dead (1.7 pp).

The share of caregivers along with a quadratic trendline and a 95%-confidence interval is plotted for our main sample by age for women around the age cutoffs of interest (Figure 1). The percentages and relations are consistent with the findings of other descriptive studies on the provision of informal care that are based on alternative data sources (Wetzstein et al., 2015). The share of caregivers peaks between 60 and 65 and declines with higher ages. The graph confirms the considerable variance in the dependent variable. Besides the aforementioned changes in the labor supply status occurring at this point of employment biographies this may also point to some measurement error in the care variable. Figure A1 in the Appendix gives the mean of provided hours of care by quarters of age. The pattern is similar.

Figure 1: Share of female care providers by age (in bins of quarters of years of age)



Source: SOEP v33, own calculations.

5 Identification strategy

In this paper, we estimate the impact of retirement on informal caregiving activities. We conjecture that eligibility for pension benefits allows individuals to resolve the time conflict between employment and care. Labor supply and the provision of care are decided upon simultaneously. Endogeneity arises irrespective of incentives generated by pension and LTC insurance benefits. One underlying mechanism is selection on unobservables. Individuals' characteristics and preferences determine their behavior in terms of labor supply and retirement decisions as well as the provision of informal care. Another mechanism is reverse causality. Individuals retire (early) because demand for (informal) care arises when family members or close friends become care-dependent. In this case labor supply is adjusted as a consequence of care demand. We are interested in the causal effect in the opposite direction. Does (the dependence on) labor supply crowd out informal care? The identification strategy addresses both issues. We exploit retirement age thresholds in the German pension system that generate exogenous variation in labor supply.

5.1 Fuzzy regression discontinuity design

When women reach their ERA and fulfill 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 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, e.g. Battistin et al., 2009; Eibich, 2015, or Müller and Shaikh, 2018). The exogenous variation in retirement behavior created by these instruments is used to estimate local average treatment effects on the provision of informal care for compliers (Trochim, 1984; Lee and Lemieux, 2010; Hahn et al., 2001). We can thus identify the effect of retirement (the related reduction in labor supply) on the care provision for those individuals that react to changed incentives at a threshold.

Identification requires, first, that individuals cannot manipulate their age to select into treatment (i.e. being eligible to retirement benefits before actually 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. Under those assumptions effects of the instrumented retirement behavior on care provision can be causally attributed to the local treatment. In our setting the local average treatment effect (LATE) is specific to those women retiring at an age threshold. Under valid and relevant instruments this approach deals with simultaneity and selection on unobservables.

The institutional setting of the German state pension system strongly incentivizes individuals to retire at sharp cutoff ages (sub-section 3.2). ERA thresholds in Germany, set at age 60 for women born before 1952 and at age 63 for cohorts born later, are shown to be used frequently (see Geyer and Welteke, 2017; Eibich, 2015 and sub-section 5.2 below). We define 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 onwards as $c = 63$. The first stage captures the impact of the respective threshold on treatment assignment, i.e. the retirement decision:

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

We allow the relationship between the treatment variable R_{it} and the forcing variable centered at the respective cutoff age c, $(Age_{it} - c)$ to be different on each side of the threshold.[13] 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 caretaking $Care_{it}$:

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

We analyze different measures of caregiving $Care_{it}$, namely the extensive and intensive margin of informal care for the main analysis and a binary indicator for intensive care in the heterogeneity analysis (sub-section 4.2). The effect of interest is δ_1. For relevant and valid instruments the predicted retirement probability carries only exogenous variation and is independent of the error term. There is, thus, no endogeneity bias. In the main specification we estimate the effects for all women in our sample crossing their cohort-specific ERA. 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. We perform a number of robustness checks in terms of bandwidth choice and using non-parametric estimators for the discontinuity (Gelman and Imbens, 2018).

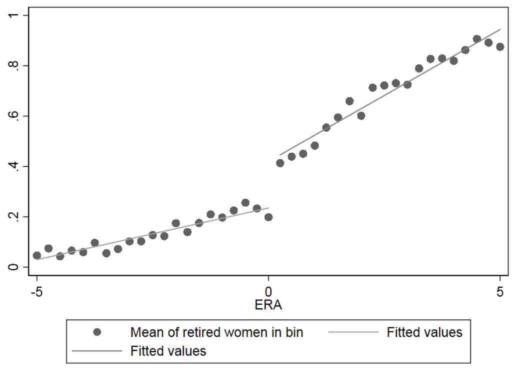
5.2 Discontinuities in retirement behavior

To serve as valid instruments age thresholds need to significantly affect retirement decisions. We therefore depict individual retirement behavior by age to check for jumps in the treatment variable, i.e. the retirement probability. We consider women at their cohort-specific ERA in our main sample. The retirement probability is calculated only on the basis of individuals that are in the labor force or that are retired. Retirement from unemployment is not included as there is no time conflict with informal care. Probabilities are depicted in bins of quarters of age (Figure 2).

[13] 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 $D_{it} = 1$ if $Age_{it} \geq c$.

The graph depicts the jump in retirement probability at women's cohort-specific ERA including linear trends. The discontinuity is substantive and roughly amounts to 20 pp.

Figure 2: Retirement behavior by age, 5 year bandwidth, 2001-2015



Source: SOEP v33, own calculations.

A similar discontinuity emerges in the graph that is based only on women born before 1952 with the ERA at 60 used as an instrument (Figure A2 in the Appendix). Less than 30% of women are retired before reaching the respective ERA. After crossing the threshold the retirement probability jumps to about 50%.

A look at first stage estimates largely confirms the graphical evidence. For cohort-specific ERA cutoffs with our main sample of all women retiring from employment a jump in the retirement probability of 16.6 pp results (Table 2, column 1). Women born before 1952 that retire from employment also exhibit a highly significant 18.9 pp jump in their retirement probability at the age-cutoff of 60 (Table 2, column 4). We find reduced effects of crossing the cohort-specific ERA with a 3-year bandwidth of 11.8 pp and a 16.5 pp jump in the retirement probability when introducing several control variables with a 5-year bandwidth (Table 2, column 3).

Table 2: First stages estimates, 2001-2015

	(1)	(2)	(3)	(4)
Instrument	Cohort specific ERA	Cohort specific ERA	Cohort specific ERA	Age 60 (Birthyear$\leq$ 1952)
ERA	0.172***	0.121***	0.165***	0.187***
	(0.022)	(0.030)	(0.024)	(0.024)
Age 65				0.187***
Observations	10,095	5,573	10,095	8,379
Controls	-	-	YES	-
Bandwidth—years	5	3	5	5

Notes: Standard errors in parentheses; YES: controls for year of observation, number of children in the household, and marital status; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: SOEP v33, own calculations.

Our main analysis is based on a 5 year bandwidth around the ERA thresholds. Reducing the bandwidth for the main sample with cohort-specific ERA decreases these jumps in the retirement probability. A bandwidth of 2 years results in a discontinuity estimate of 9.7 pp, a further reduction to a 1 year bandwidth leads to an increase of 13.6 pp at the threshold (Table A1 in the Appendix). This pattern is confirmed in the same exercise with the reduced sample of women born before 1952 with ERA at age 60 as single instrument (Table A3 in the Appendix). Statistical significance is reduced only for 1 and 2 year bandwidth estimates due to the smaller sample size. The graphical evidence and the strong and robust coefficients from the first stage estimates confirm that ERA thresholds used here are indeed relevant instruments for the retirement decision.

5.3 Validity of identification

One identification condition for the effect of interest is that individuals cannot manipulate the forcing variable that selects them into treatment. Age is an exogenous factor, therefore this assumption holds by construction. Histograms do not exhibit discontinuities in the number of women by age (Figure A3 in the Appendix, upper left picture). A related identification condition is that no apparent discontinuities exist in the number of individuals in the sample and for important covariates that drive the outcome at a threshold. In the age range of interest no jumps in any of the population variables (share of married women, mean of years of education, and children in the household, plotted by age respectively) become apparent for women (Figure A3 in the Appendix).

When women reach their cohort-specific ERA they face the choice to retire, reduce labor supply and claim benefits or keep on working. As stated, many individuals actually use their earliest possible pathway into retirement (Geyer and Welteke, 2017; Keck and Krickl, 2013). Still, retiring is a choice variable opening up the possibility that people select into retirement according to care demand and their willingness or ability to supply informal care takes. This does not threaten identification, changes, however, the interpretation of our estimates slightly: We would still identify the effect of retirement on informal care provision for women retiring at their ERA. However, external validity for later retirement ages is reduced, if the demand for informal care changes at these other cut-off ages. It could also be that those individuals facing a demand for and are willing to provide informal care go into retirement 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.

Last, we need to presume that independent of treatment (if there were no age cutoffs and no transition to retirement observed for a given individual) there would be no discontinuity in the outcome variable. We therefore need to assume a smooth function of care demand for the individual in the absence of retirement. If there are natural jumps in care demand at official retirement ages, we would simply identify this discontinuity, instead of changes in the provision of informal care that are driven by an exogenous shock in labor supply under a smooth function of care demand.

6 Results

First, we discuss 2SLS results for our main sample of women using the cohort-specific ERA as instrument (sub-section 6.1). We analyze effect heterogeneity in terms of type of previous employment (full-time vs. part-time) and type of care (within vs. outside the household; see sub-section 6.2). After a number of robustness checks (sub-section 6.3), we conclude this section with a comparison to low-intensity carers (sub-section 6.4). In the main specification and the heterogeneity analysis we use a 5 year bandwidth (i.e. a 10 year estimation window around the thresholds). All results include heteroskedasticity-consistent standard errors clustered at month of age level (Lee and Card, 2008).

6.1 Main specification

Results 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 upon retirement (Table 3). The coefficient is significant at the 1% level. Employed women aged 55-60, i.e. before crossing an ERA threshold, provide on average about 0.2 hours of informal care per week. Thus, the effect is substantial and and driven by women who take up informal care or increase hours of care. 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. We find comparable positive effects on the hours of daily care provision in all of those robustness checks with only some variation in effect size. The effect size increases slightly with an optimal bandwidth, but is less precisely estimated (column (2)). Using only a single ERA threshold yields virtually an identical estimate (column (4)).

Table 3: 2SLS estimates on the hours of care provision, 2001-2015

	(1)	(2)	(3)	(4)
Instrument	Cohort-specific ERA	Cohort-specific ERA	Cohort-specific ERA	Age 60 (Birthyear≤ 1952)
Retired	0.772***	0.898*	0.813***	0.695***
	(0.252)	(0.460)	(0.264)	(0.248)
Observations	10095	6189	10095	8379
Controls	-	-	YES	-
Bandwidth—years	5	3.282	5	5
Pre-Treatment mean	0.159	0.159	0.159	0.151
KL.Paap	58.75	-	46.73	63.22

Notes: Standard errors in parentheses; (2): optimally selected bandwidth; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$; YES: controls for year of observation, number of children in the household, and marital status; Kl.Paap: Kleibergen-Paap statistic.
Source: SOEP v33, own calculations.

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 4). Before crossing their ERA thresholds caregiving women in our main sample provide about 1.7 hours of informal care.

Table 4: 2SLS estimates on the hours of care provision, only care providers before retirement, 2001-2015

	(1)	(2)	(3)	(4)
Instrument	Cohort-specific ERA	Cohort-specific ERA	Cohort-specific ERA	Age 60 (Birthyear≤ 1952)
Retired	5.434*	8.133	5.592**	4.555*
	(2.785)	(8.474)	(2.817)	(2.429)
Observations	1.082	735	1.082	887
Controls	-	-	YES	-
Bandwidth—years	5	3.568	5	5
Pre-Treatment mean	1.743	1.759	1.743	1.779
KL.Paap	6.118	-	6.181	6.722

Notes: Standard errors in parentheses; (2): optimally selected bandwidth; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$; YES: controls for year of observation, number of children in the household, and marital status; Kl.Paap: Kleibergen-Paap statistic.
Source: SOEP v33, own calculations.

This number almost triples indicating a resolution of a time conflict through retirement. Due to the smaller sample size we are not able to identify a significant effect with an optimal bandwidth (column (2)). Similar to the estimates for the whole sample, we find a slightly larger effect size. Adding controls and using a single cut-off as instrument yields robust results (columns (3) and (4)).

According to our estimates for the extensive margin of care the probability to be a caregiver increases through retirement by 13 pp (Table 5).[14] The baseline

[14]The parameter estimate holds at the mean of the distribution in linear probability models (LPM).

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. The substantial effect sizes for care hours and the care probability could be partially due to selection into early retirement: The group of women using the ERA could be selective with respect to the demand for and, or their willingness to supply informal care (section 5.3). 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 5, column (2)). Including control variables and using a single ERA cut-off does not alter effect sizes or significance (columns (3) and (4)).

Table 5: 2SLS estimates probability of care provision, 2001-2015

	(1)	(2)	(3)	(4)
Instrument	Cohort-specific ERA	Cohort-specific ERA	Cohort-specific ERA	Age 60 (Birthyear≤ 1952)
Retired	0.131*	0.163	0.146*	0.118
	(0.075)	(0.165)	(0.078)	(0.076)
Observations	10095	5484	10095	8379
Controls			YES	
Bandwidth—years	5	2.935	5	5
Pre-Treatment mean	0.0910	0.0874	0.0910	0.0850
KL.Paap	58.75		46.73	63.22

Notes: Standard errors in parentheses; (2): optimally selected bandwidth; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$; YES: controls for year of observation, number of children in the household, and marital status; Kl.Paap: Kleibergen-Paap statistic.
Source: SOEP v33, own calculations.

The probability to be an intensive caregiver increases by 9.6 pp (Table 6). With a baseline probability of 4% the effect size of intensive is comparable to overall care. Parameters are only slightly altered in the robustness checks (columns (2)-(4)).

Table 6: 2SLS estimates probability of intensive care provision, 2001-2015

	(1)	(2)	(3)	(4)
Instrument	Cohort-specific ERA	Cohort-specific ERA	Cohort-specific ERA	Age 60 (Birthyear≤ 1952)
Retired	0.096*	0.092	0.102*	0.075
	(0.050)	(0.083)	(0.053)	(0.052)
Observations	10095	6705	10095	8379
Controls	-	-	YES	-
Bandwidth—years	5	3.562	5	5
Pre-Treatment mean	0.0377	0.0376	0.0377	0.0354
KL.Paap	58.75	-	46.73	63.22

Notes: Standard errors in parentheses; (2): optimally selected bandwidth; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$; YES: controls for year of observation, number of children in the household, and marital status; Kl.Paap: Kleibergen-Paap statistic.
Source: SOEP v33, own calculations.

Selecting the optimal bandwidth again yields a coefficient a 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.

Predicted probabilities based on LPM estimates are not bound between 0 and 1.

6.2 Effect heterogeneity

We analyze effect heterogeneity for our main sample along the following dimensions: informal care provided to people living within the same household, women who were full-time employed before retirement, for highly educated women, and the combination of care within the own households with the latter two dimensions (Table 7). We discuss effects for overall care hours, the probability to be a caregiver, and the probability to provide intensive care. The sample size for women who provided informal care before retirement is too small for the heterogeneity analysis.

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 it takes place within their household.[15] 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 statistically more significant at the intensive and extensive margin (Table 7, 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 7, column (3)). Compared to the main effect estimated increases are larger for all margins. The same pattern holds when we look specifically at women providing care for people living in their household and retiring from full-time work (Table 7, column (5)). Consistent with our expectations women retiring from full-time employment show more substantive increases in the provision of informal care and coefficients become more significant. The time conflict between labor supply and informal care indeed seems to be more binding for full-time employed women.

The final heterogeneity exercise breaks 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

[15] We discard observations, if care provision is reported in a period, but this person lives in a household which at no point in the observed time span is inhabited by a person in need for care.

Table 7: 2SLS estimates – effect heterogeneity, cohort-specific ERA, 2001-2015

		(1)	(2)	(3)	(4)	(5)	(6)
Hours of care provision	Retired	0.772***	0.636***	0.984***	1.657***	0.832***	0.998**
		(0.252)	(0.233)	(0.319)	(0.484)	(0.289)	(0.471)
	Pre-treatment mean	0.159	0.0423	0.137	0.180	0.0379	0.0556
Prob. of care provision	Retired	0.131*	0.101**	0.141	0.403***	0.132**	0.155*
		(0.075)	(0.047)	(0.087)	(0.140)	(0.060)	(0.082)
	Pre-treatment mean	0.0910	0.0199	0.0844	0.105	0.0189	0.0260
Prob. of intensive care provision	Retired	0.096*	0.118***	0.144**	0.279***	0.153***	0.165***
		(0.050)	(0.037)	(0.065)	(0.088)	(0.049)	(0.062)
	Pre-treatment mean	0.0377	0.0115	0.0306	0.0418	0.00908	0.0144
	Observations	10095	9303	6057	6127	5593	5555
	Bandwidth—years	5	5	5	5	5	5
	KL.Paap	58.75	63.11	52.19	23.54	55.09	22.48

Notes: Standard errors in parentheses; Prob.: Probability; (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; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$; Kl.Paap: Kleibergen-Paap statistic.

Source: SOEP v33, own calculations.

greater employment rates and thus a higher probability to be eligible for retirement at their ERA. All early retirement paths condition on a certain number of contribution years. We use years of schooling and separately estimate the 2SLS model for those who have at least 11 years of schooling. Higher educated women show markedly larger and also more significant effects for all margins of care (Table 7, column (4)). This heterogeneity pattern can be replicated for women that care only within their own household (Table 7, column (6)).

6.3 Robustness tests

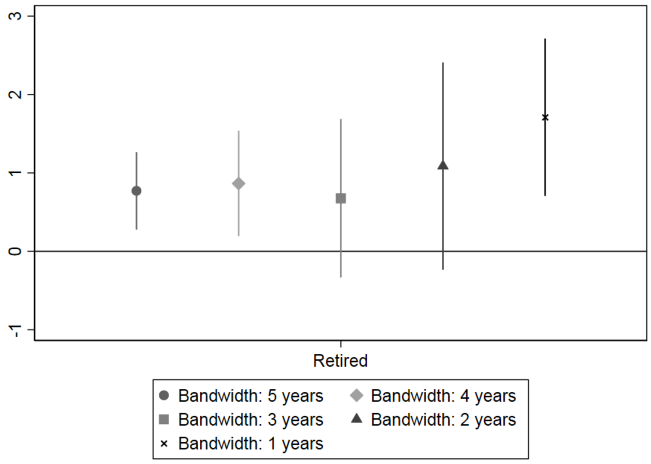
We follow the common practice in RDD analysis and test whether the choice of bandwidth around the age cutoff drives our results. We also check the robustness of findings by including several covariates in the estimation procedure. Finally, we show results based on local linear and local polynomial estimators choosing rectangular, triangular and Epanechnikov kernels.

All of the aforementioned results are based on a bandwidth choice of five years. Individuals in the age range of five years around the cutoff age are used for estimation. 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.

Figure 3 graphs point estimates and 95% confidence intervals for women's hours of care provision upon retirement at an ERA for 5 different bandwidth choices. Narrowing the bandwidth from 5 to 2 years does hardly alter point estimates. Confidence intervals are increasing with lower sample sizes. A zero effect is within the boundaries of the confidence interval for the 2 and 3 year bandwidths. A one-year bandwidth, however, not only widens the 95% confidence interval. It also increases the point estimate markedly leading to a statistically significant effect of almost 2 hours of informal care per week.

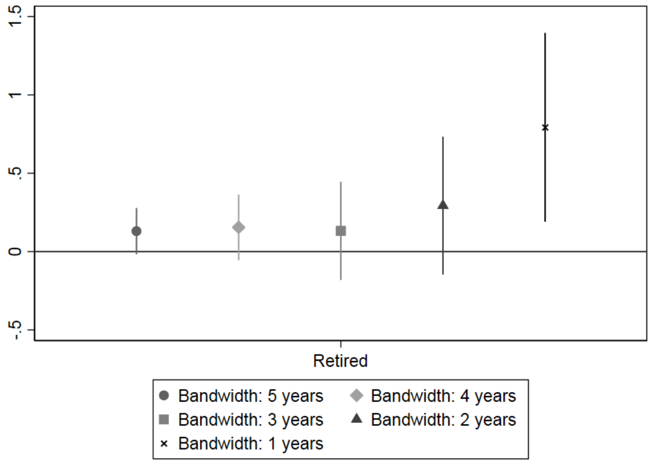
Robustness tests for the binary outcome, i.e. the extensive margin of informal care, reveal that for all bandwidths between five and two years the 95% confidence intervals include a zero effect (Figure 4). For bandwidths between five and 3 years the point estimate virtually does not change. A bandwidth of two years and particularly a bandwidth of one year lead to substantially larger point estimates. The coefficient for the one year bandwidth seems to be upward biased. An increase in the care probability of about 80 pp in this rather small sample does not seem plausible.

Figure 3: 2SLS estimates – robustness for bandwidth choice, daily hours of informal care, cohort-specific ERA, 2001-2015



Notes: ERA: early retirement age.
Source: SOEP v33, own calculations.

Figure 4: 2SLS estimates – robustness for bandwidth choice, probability of informal care, cohort-specific ERA, 2001-2015



Notes: ERA: early retirement age.
Source: SOEP v33, own calculations.

We repeat the robustness tests and limit the analysis to informal care within the household. Point estimates and the 95% confidence intervals for extensive and intensive margin estimates vary less when the bandwidth is reduced (Figures A4 and A5 in the Appendix). Results are significantly different from zero for a 4 year bandwidth. Confidence intervals include a zero effect for all narrower estimation windows. The graphs look very similar when the sample includes only women born before 1952 for whom the ERA at 60 applied (Figures A6 and A7 in the Appendix). Confidence intervals are slightly wider due to the decreased sample size.

In another robustness test a local linear estimator is used and a triangular kernel is chosen for our main sample and bandwidth of 5 years. Results are not sensitive to choosing a different estimator (Tables A5, A6 and A7 in the Appendix, column (1)). We also include local quadratic and local cubic polynomials in the specification. Results are sensitive to this specification test (Tables A5, A6 and A7 in the Appendix, column (2) and (3)). The magnitude of parameters increases in most cases. However, these increases are not statistically significant. Standard errors also increase which leads to only few statistically significant point estimates. Using an Epanechnikov kernel for our standard bandwidth of 5 years produces results for women in our main sample that are comparable in size and significant for overall informal care and informal care within the household (Tables A5, A6 and A7 in the Appendix, column (4)). The same holds when these robustness checks are done for care provided within the household (Tables A8, A9 and A10 in the Appendix).

6.4 Comparison with low intensity caregivers

We argue that the mechanism behind the effect of early retirement on informal care is an underlying time conflict between employment and care activities before eligibility for pension benefits is reached. The main analysis is focused on employed women as main caregivers in Germany and ERA thresholds are used for identification. According to previous evidence women provide informal care most frequently in the age range around early retirement (Wetzstein et al., 2015; Geyer and Schulz, 2014). Evidently the effect should be weaker for groups with a lower care propensity and completely absent for individuals with an inelastic supply of informal care. We therefore compare our findings with similar estimates for older women at their second possible ERA of 63 years[16] and at their NRA of 65 years. In addition, we analyze men at their ERA of 63 and at their NRA of 65 years.

[16]Note that we do not use women born from 1952 onwards for this comparison as the ERA at 63 is their only early retirement path. We only us women born earlier for whom the ERA at 63 is the second option to retire early. Most of those women were already eligible for early retirement at the ERA 60, albeit with larger deductions.

The ERA at 63 is a valid instrument for women, although the 4.0 pp increase in their early retirement probability is clearly smaller compared to the first early retirement options. The NRA at 65 is a weak instrument for women as shown by the insignificant 1.3 pp jump in their retirement probability (Table A11, columns (1) and (2) in the Appendix). We do not find significant effects for any of the outcomes, neither care hours nor the probability of care overall or intensive care at the ERA 63. The same holds for different margins of informal care at the NRA 65, although results need to be interpreted carefully because of the weak first stage (Table A12 in the Appendix). Most of the point estimates exhibit a negative sign. It seems that we pick up the negative trend in the caregiving probability along the forcing variable (see Figure 1 above).

For men, the ERA cutoff at 63 (11.5 pp increase) and the NRA at 65 (16.5 pp increase) turn out to be valid instruments for the retirement decision (Table A11, columns (3)-(5) in the Appendix). We do not find significant effects at any margin of informal care for men, either at the ERA 63, nor at the NRA 65 (Table A13 in the Appendix). Contrary to older women, point estimates are close to zero throughout. To sum up, the comparison with groups of individuals that exhibit a lower care intensity did not yield significant retirement effects on informal caregiving. There is no evidence for a time conflict between employment and care activities for these groups.

7 Discussion and conclusion

Causal effects between informal care provision and labor supply have been investigated in both directions of influence. We argue that implications from an increase in life-cycle labor supply in connection with population aging and structural changes on labor markets are potentially important as they may threaten future supply of informal care. Women provide the majority of informal care in Germany as in most other countries. This paper thus focuses on the effect of women's early retirement on their informal care provision. As the share of care providers in the population is highest around their ERA, the question arises whether a transition from employment to retirement induces an increase in the provision of informal care to close relatives and friends. The mechanism behind this potential retirement effect on informal care is argued to be a time conflict between the supply of labor and care as long as eligibility for retirement benefits is not reached.

The endogeneity problem inherent in these simultaneous labor supply and care decisions is addressed within a fuzzy RDD. We exploit a quasi-experimental set-up generated by German retirement legislation. Women are incentivized to retire early at ages 60 and 63. These ERA thresholds serve as instruments for retirement in a 2SLS framework. We estimate the local effect of retirement on individuals complying to these rules (women retiring at an ERA threshold) for informal care hours per weekday, the probability of caregiving and the probability to provide intensive care. We document instrument relevance and discuss identifying assumptions for this framework. Although applied in other contexts this approach has not been used in the literature on the effects of employment on care provision.

We find positive effects for informal care of women retiring from employment at their ERA. Increases in the provision of informal care are estimated at the intensive and extensive margin and robust to various sensitivity checks. The overall hours effect of about 0.8 hours per normal weekday and increases of about 13 pp in the probability of caregiving overall and 10 pp for intensive care are of plausible magnitude given care statistics for women in that age range. These effects turn out to be robust to a number of robustness checks varying the estimation window, specification and type of estimator as well as a more homogenous sample with a single ERA threshold.

Based on our rich panel data we are able to analyze effect heterogeneity along several dimensions. Women who supply informal care at home exhibit effects of similar magnitude that are estimated more precisely. This confirms that heterogeneity and measurement error in the care variable is smaller for this sub-sample. Women retiring from full-time employment and highly educated women react markedly stronger. The hours effect of women that already provided some care before retirement is also substantially larger. These findings are consistent with the assumed behavioral mechanisms and confirm previous descriptive evidence on the structure of informal care in Germany. The time-conflict is larger for full-time employed women. Highly educated women have a higher labor market attachment and propensity to supply care. The lack of an effect for groups with low care intensity – older women at their second possible ERA or NRA and men at both age thresholds – fits this behavioral interpretation. We conclude that labor supply indeed puts time restrictions on caregiving activities for women.

What are implications for pension and care policies? Induced by societal change and promoted through equal opportunities policies female labor market participation is on the rise. Women's early retirement threshold at age 60 has been abolished to cope with demographic ageing. Problems for informal care provision seem unavoidable. As shown women increase caregiving significantly through retirement at their ERA. With early retirement options no longer available, it is not clear how this additional care gap will be filled. Coming retirement reforms need to take this into account. Future research needs to assess whether particular care receivers profit from the group of informal caregivers analyzed in this paper. When early retirement is not an option, our findings could also be valid for older age groups. Prevalent increases of the pensionable age would then have similar implications for care supply.

On the other hand, policy-makers started to react to increased employment-related barriers to informal care provision by introducing care-times for employees. Parts of these reforms were not yet enacted in the observation period of this paper. Further research should also focus on the question whether such new LTC policies can effectively diminish the negative relationship between care provision and labor supply around retirement by dissolving the time conflict. Another margin for policy action could be to make the care supply of men more elastic.

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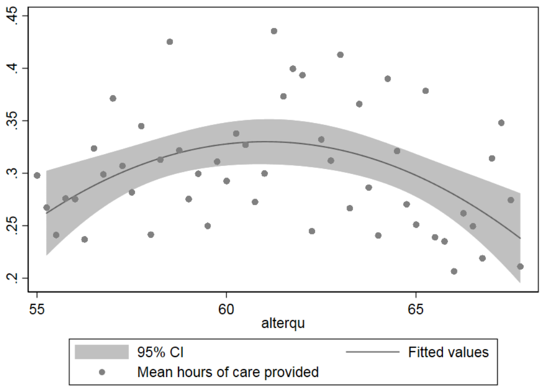
Appendix

Additional figures

Figure A1: Mean hours of informal care by age (in bins of quarters of years of age)



Source: SOEP v33, own calculations.

Figure A2: Retirement behavior by age, 5 year bandwidth, 2001-2015

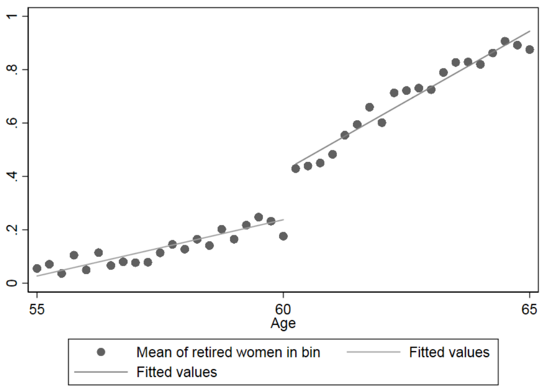
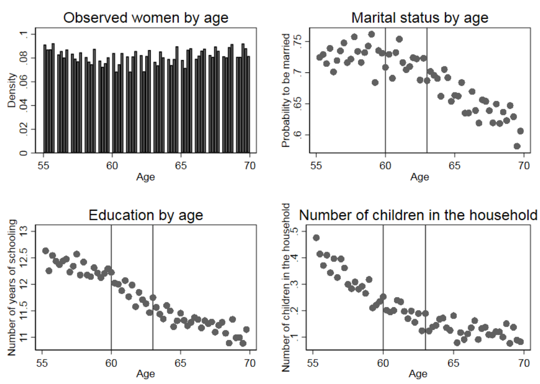
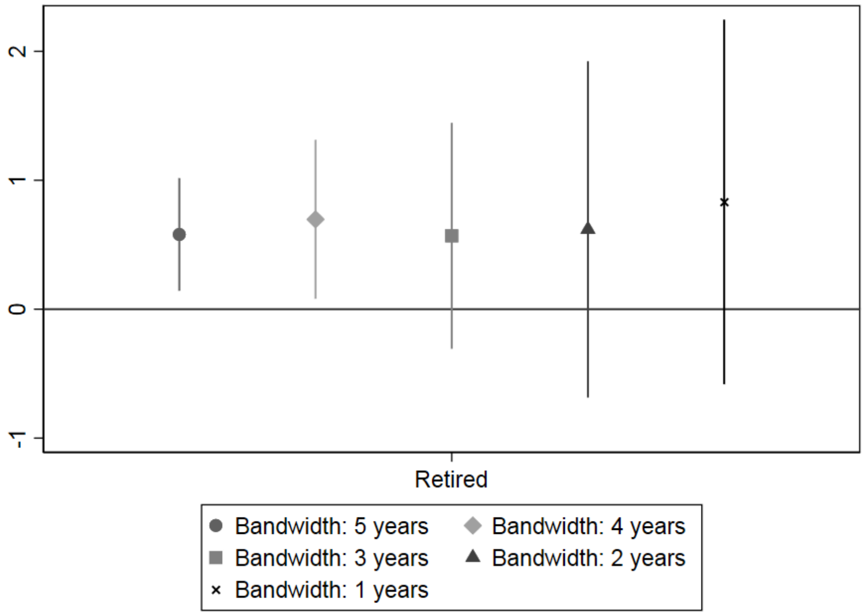


Figure A3: Distribution of covariates by age around ERA thresholds



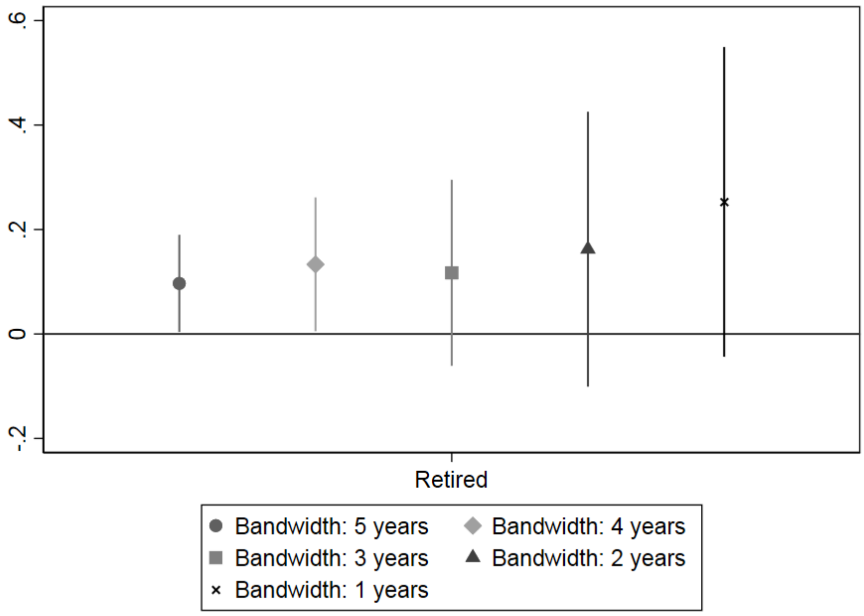
Notes: ERA: early retirement age, depicted by vertical lines in graph.
Source: SOEP v33, own calculations.

Figure A4: 2SLS estimates – robustness for bandwidth choice, daily hours of informal care within the household, cohort-specific ERA, 2001-2015



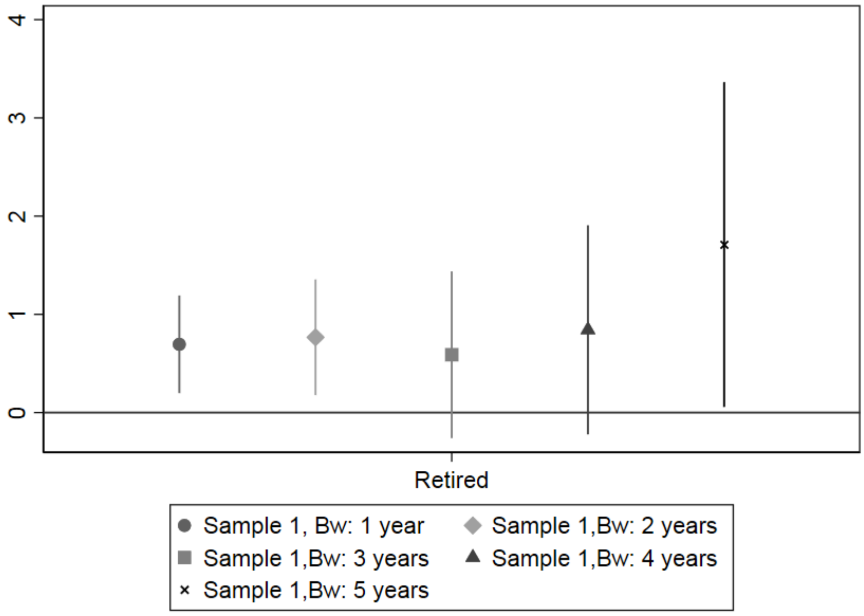
Notes: ERA: early retirement age.
Source: SOEP v33, own calculations.

Figure A5: 2SLS estimates – robustness for bandwidth choice, probability of informal care within the household, cohort-specific ERA, 2001-2015



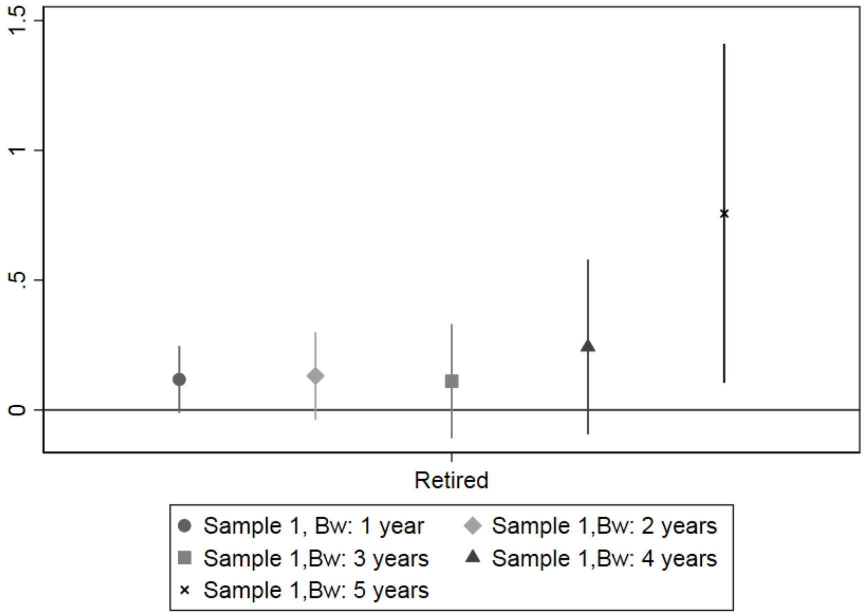
Notes: ERA: early retirement age.
Source: SOEP v33, own calculations.

Figure A6: 2SLS estimates – robustness for bandwidth choice, daily hours of informal care, women with ERA at 60, 2001-2015



Notes: ERA: early retirement age.
Source: SOEP v33, own calculations.

Figure A7: 2SLS estimates – robustness for bandwidth choice, probability of informal care, women with ERA at 60, 2001-2015



Notes: ERA: early retirement age.
Source: SOEP v33, own calculations.

Additional tables

Table A1: First stages estimates on women, several bandwidths, 2001-2015

	(1)	(2)	(3)	(4)	(5)
ERA	0.133**	0.101**	0.121***	0.137***	0.172***
	(0.060)	(0.038)	(0.030)	(0.025)	(0.022)
Observations	1,705	3,540	5,573	7,764	10,095
Controls	-	-	-	-	-
Bandwidth—years	1	2	3	4	5

Notes: Standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: SOEP v33, own calculations.

Table A2: First stages estimates on women, care only within the household, several bandwidths, 2001-2015

	(1)	(2)	(3)	(4)	(5)
ERA	0.109*	0.098**	0.127***	0.145***	0.178***
	(0.059)	(0.038)	(0.030)	(0.024)	(0.022)
Observations	1,570	3,265	5,122	7,139	9,303
Controls	-	-	-	-	-
Bandwidth—years	1	2	3	4	5

Notes: Standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: SOEP v33, own calculations.

Table A3: First stages estimates on women, age 60 (women born before 1952), several bandwidths, 2001-2015

	(1)	(2)	(3)	(4)	(5)
Age 60	0.138**	0.115***	0.134***	0.154***	0.187***
	(0.063)	(0.039)	(0.031)	(0.026)	(0.024)
Observations	1,592	3,236	4,923	6,649	8,379
Controls	-	-	-	-	-
Bandwidth—years	1	2	3	4	5

Notes: Standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: SOEP v33, own calculations.

Table A4: First stages estimates on women, age 60 (women born before 1952), care only within the household, several bandwidths, 2001-2015

	(1)	(2)	(3)	(4)	(5)
Age 60	0.117*	0.109***	0.136***	0.158***	0.191***
	(0.062)	(0.040)	(0.031)	(0.025)	(0.024)
Observations	1,467	2,988	4,534	6,131	7,750
Controls	-	-	-	-	-
Bandwidth—years	1	2	3	4	5

Notes: Standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: SOEP v33, own calculations.

Table A5: 2SLS estimates – robustness tests on the hours of care provided, cohort-specific ERA, local linear estimator, 2001-2015

	(1)	(2)	(3)	(4)
RD-Estimate	0.816**	1.155	1.485**	0.782**
	(0.356)	(0.733)	(0.715)	(0.336)
Observation	10095	10095	10095	10095
Kernel	Tri.	Tri.	Tri.	Epa.
Bandwidth—years	5	5	5	5
Local polynomial	1	2	3	1
Pre-Treatment mean	0.159	0.159	0.159	0.159

Notes: Standard errors in parentheses; Tri.: triangular, Epa.: Epanechnikov; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Source: SOEP v33, own calculations.

Table A6: 2SLS estimates – robustness tests on the probability to provide care, cohort-specific ERA, local linear estimator, 2001-2015

	(1)	(2)	(3)	(4)
RD-Estimate	0.159	0.364	0.680*	0.139
	(0.114)	(0.254)	(0.356)	(0.105)
Observation	10095	10095	10095	10095
Kernel	Tri.	Tri.	Tri.	Epa.
Bandwidth—years	5	5	5	5
Local polynomial	1	2	3	1
Pre-Treatment mean	0.0910	0.0910	0.0910	0.0910

Notes: Standard errors in parentheses; Tri.: triangular, Epa.: Epanechnikov; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Source: SOEP v33, own calculations.

Table A7: 2SLS estimates – robustness tests on the probability to provide intensive care, cohort-specific ERA, local linear estimator, 2001-2015

	(1)	(2)	(3)	(4)
RD-Estimate	0.101	0.156	0.207	0.095
	(0.070)	(0.142)	(0.141)	(0.065)
Observation	10095	10095	10095	10095
Kernel	Tri.	Tri.	Tri.	Epa.
Bandwidth—years	5	5	5	5
Local polynomial	1	2	3	1
Pre-Treatment mean	0.0377	0.0377	0.0377	0.0377

Notes: Standard errors in parentheses; Tri.: triangular, Epa.: Epanechnikov; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Source: SOEP v33, own calculations.

Table A8: 2SLS estimates – robustness tests on the hours of care provided, cohort-specific ERA, local linear estimator, care within the own household, 2001-2015

	(1)	(2)	(3)	(4)
RD-Estimate	0.702**	0.846	0.829	0.688**
	(0.343)	(0.832)	(1.055)	(0.314)
Observation	9303	9303	9303	9303
Kernel	Tri.	Tri.	Tri.	Epa.
Bandwidth—years	5	5	5	5
Local polynomial	1	2	3	1
Pre-Treatment mean	0.0423	0.0423	0.0423	0.0423

Notes: Standard errors in parentheses; Tri.: triangular, Epa.: Epanechnikov; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Source: SOEP v33, own calculations.

Table A9: 2SLS estimates – robustness tests on the probability to provide care, cohort-specific ERA, local linear estimator, care within the own household, 2001-2015

	(1)	(2)	(3)	(4)
RD-Estimate	0.142**	0.232	0.266	0.134**
	(0.068)	(0.165)	(0.227)	(0.062)
Observations	34.253	34.253	34.253	34.253
Kernel	Tri.	Tri.	Tri.	Epa.
Observation	9303	9303	9303	9303
Bandwidth—years	5	5	5	5
Local polynomial	1	2	3	1
Pre-Treatment mean	0.0199	0.0199	0.0199	0.0199

Notes: Standard errors in parentheses; Tri.: triangular, Epa.: Epanechnikov; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Source: SOEP v33, own calculations.

Table A10: 2SLS estimates – robustness tests on the probability of intensive care provision, cohort-specific ERA, local linear estimator, care within the own household, 2001-2015

	(1)	(2)	(3)	(4)
RD-Estimate	0.138**	0.184	0.181	0.134***
	(0.056)	(0.143)	(0.198)	(0.051)
Observation	9303	9303	9303	9303
Kernel	Tri.	Tri.	Tri.	Epa.
Bandwidth—years	5	5	5	5
Local polynomial	1	2	3	1
Pre-Treatment mean	0.0115	0.0115	0.0115	0.0115

Notes: Standard errors in parentheses; Tri.: triangular, Epa.: Epanechnikov; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Source: SOEP v33, own calculations.

Table A11: First stage estimates on men and women, all cut-offs, 2001-2015

	(1)	(2)	(3)	(4)	(5)	(6)
Gender		Men			Women	
Age 60	0.016			0.187***		
	(0.013)			(0.024)		
Age 63		0.115***			0.040**	
		(0.017)			(0.018)	
Age 65			0.165***			0.033**
			(0.012)			(0.015)
Observations	15,347	14,805	14,736	8,379	10,829	12,262
Controls	-	-	-	-	-	-
Bandwidth—years	5	5	5	5	5	5

Notes: Standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Source: SOEP v33, own calculations.

Table A12: 2SLS estimates for women: ERA 63 and NRA 65, 5 year bandwidth, 2001-2015

	(1)	(2)	(3)	(4)	(5)	(6)
Instrument		ERA 63			NRA 65	
	Hours of care	Caring probability	Intensive care	Hours of care	Caring probability	Intensive care
Retired	−2.887	−0.450	−0.197	−2.817	0.004	−0.631
	(2.040)	(0.327)	(0.259)	(5.303)	(0.767)	(1.044)
Observations	10829	10829	10829	12912	12912	12912
Bandwidth-years	5	5	5	5	5	5
Kl.Paap	5.073	5.073	5.073	0.588	0.588	0.588

Notes: ERA: early retirement age; NRA: normal retirement age; standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$; Columns (1)-(3): women born before 1952, retiring from employment, columns (4)-(6): all women retiring from employment; Kl.Paap: Kleibergen-Paap statistic.

Source: SOEP v33, own calculations.

Table A13: 2SLS estimates for men: ERA 60, 63 and NRA 65, 5 year bandwidth, 2001-2015

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Instrument		Age 60			Age 63			Age 65	
	Hours of care	Caring probability	Intensive care	Hours of care	Caring probability	Intensive care	Hours of care	Caring probability	Intensive care
Retired	0.935	0.201	−0.317	−0.184	−0.021	−0.033	−0.112	0.019	0.007
	(1.655)	(0.507)	(0.379)	(0.226)	(0.063)	(0.044)	(0.161)	(0.048)	(0.033)
Observations	15347	15347	15347	14805	14805	14805	14736	14736	14736
Bandwidth-years	5	5	5	5	5	5	5	5	5
Kl.Paap	1.709	1.709	1.709	46.104	46.104	46.104	198.454	198.454	198.454

Notes: ERA: early retirement age; NRA: normal retirement age; standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$; Kl.Paap: Kleibergen-Paap statistic.

Source: SOEP v33, own calculations.