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Long-term care reform and the labor supply of household members – evidence from a quasi-experiment*

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Germany introduced a new mandatory insurance for long-term care in 1995 as part of its social security system. It replaced a system based on means-tested social welfare. Benefits from the long-term care insurance are not means tested and depend on the required level of care. The insurance provides both benefits in kind and cash benefits. The new scheme improved the situation for households to organize informal care at home. This was one goal of the reform since policymakers view informal care as a cost-saving alternative to formal care. This view however neglects possible opportunity costs of reduced labor supply of carers. We exploit this reform as a quasi-experiment and examine its effect on the labor supply of caregivers who live in the same household as the care recipient. We find strong negative labor market effects for men but not for women. We conduct a series of robustness tests and find results to be stable.

Keywords: labor supply, long-term care, long-term care insurance, natural experiment, quasi-experiment
JEL Classification: J22, H31, I13

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1. Introduction

The organization and provision of long-term care (LTC) is one of the most significant challenges for aging societies. LTC systems rely to a large extent on family care and informal networks. However, for caregivers who are of working age, it is a challenge to reconcile market work and care obligations. Similar to childcare, LTC can be associated with opportunity costs resulting from reduced labor supply. In particular intensive caregiving is associated with negative labor supply effects (Lilly et al., 2007; Colombo et al., 2011). From a budgetary perspective informal care is a cost-saving alternative to formal care. However, if the labor market effects are large, the cost-advantage diminishes. The problem might be exacerbated as the number of caregivers of working age will decline in the course of demographic aging, while the demand for LTC will increase. These opposing trends may cause a growing need to balance caring responsibilities and market work.[1] Therefore, many countries implement policies that help family carers balance work and caring (Colombo et al., 2011).

Policies supporting family care include different measures such as direct cash benefits, benefits in kind and care leave. Care policies affect the carers' trade-off between informal care and labor supply. In order to understand the relation of labor supply of caregivers and informal care it is important to take into account the incentives set by care policies. In this paper, we contribute to the literature by providing causal evidence on the labor supply reactions of caregivers to the introduction of the long-term care insurance (LTCI) in Germany in 1995.

The LTCI was supposed to insure the entire population against the risk of LTC and to strengthen family care (BMG, 2007) which has always been the most important source of care for older people in Germany. Eligibility for benefits from the LTCI requires a substantial degree of limitations of activities of daily living (ADL) which have to last for at least six months. The LTCI prioritizes help for informal home care. Formal home care and institutional care should only be considered as subordinated options.[2] The most important elements of this reform are benefits for the care recipient that are not means tested. Thereby, individuals in need of LTC can choose between cash benefits which consists of direct monetary transfers, benefits in kind that provide a certain amount of professional formal help, or a combination of the two benefits.

The introduction of the LTCI can be interpreted as a quasi-experiment that changed labor market incentives for caregivers. Our focus is on male and female co-residential carers who are of working age. We compare the labor supply of co-residential caregivers (treatment group) before and after the LTCI reform applying a difference-in-differences (DiD) strategy. The majority of co-residential carers are partners or grown up children who provide care for their spouse or parent and who are normally the main caregivers. In general, they have less freedom in their care decisions as compared to extra-residential

[1] Colombo et al. (2011) report that informal caregivers face also other costs of caring such as wage penalties, higher poverty risks and detrimental health effects.

[2] See SGB XI §3 (German social security code (*Sozialgesetzbuch*))

carers, with the labor supply of partners or children to be more likely negatively affected by care obligations (Heitmueller, 2007). The control group consists of persons in the same age group who do neither live alone nor together with a care recipient. As a robustness check we also apply DiD-matching to estimate the treatment effect. The analysis is based on data from the German Socioeconomic Panel Study (SOEP). The SOEP survey includes a question asking whether a household member is receiving personal care for either old age or health reasons. The question is not directly related to eligibility for benefits from the LTCI, is asked before 1995 and does not change after the reform was implemented. Therefore, the introduction of LTCI allows us to set up treatment and control groups in which self-selection can be ruled out and to estimate causal reform effects. Furthermore, in the period under study no other major reform was implemented that could yield different macro trends for treatment and control groups.

The introduction of the LTCI changed the incentives for family carers in a nontrivial way and its effect on labor supply is theoretically ambiguous. The benefits are designed to support and not to replace informal care. That is, cash benefits do not cover all costs of informal care and benefits in kind do not suffice to meet all care needs. Therefore, people in need of care who are eligible for benefits as well as those who are not, need complementary care. On the one hand, cash benefits increase non-labor income and therefore give incentives to decrease labor supply. On the other hand, benefits in kind reduce the need of informal care and might therefore enable family members to increase labor supply. Looking at the observed choices of benefit types gives an indication of the labor supply effects of the reform. Since its introduction, the vast majority (70%) of households which receive benefits from the LTCI selects cash benefits to improve the informal home care setting. Survey data show the main reasons for this choice: cash benefits are more flexible, the money is needed for regular expenses related to care, and care recipients as well as caregivers prefer carers who are close to the care recipient (TNS Infratest Sozialforschung, 2011). Since the benefits from the LTCI are complementary to informal care and given the preference for cash benefits, we expect either no or negative labor supply effects in the treatment group.

We find a large negative effect on the labor supply of caring men. Female labor supply is not affected. This includes both employment rates and as working hours, with the effects robust across various specifications. Consistent with this finding female carers show a below average labor market participation even before the 1995 reform. Their labor supply decision is more affected by the care situation itself and they would provide care independently of the LTCI. This is, cash benefits give small incentives to further reduce labor supply for many women. On the other hand, for men who work more hours and are more frequently employed, on average it is more difficult to reconcile care provision and labor supply. For them the extra household income therefore provides the opportunity to reduce labor supply and take on more caring responsibilities.

The paper is structured as follows: The next section provides a short overview over the related literature, Section 3 gives an introduction into the institutional settings in Germany,

Section 4 describes the data used, and Section 5 illustrates our empirical approach. We discuss the DiD-assumptions in Section 6, where we also provide graphical representations and descriptive statistics. The main results are presented in Section 7. We provide a set of robustness checks of our results in Section 8. Section 9 provides a discussion of our findings and Section 10 concludes.

2. Related literature

Most of the empirical literature focuses on the general relationship between labor supply and caregiving (for an extensive literature overview, see Lilly et al., 2007). Depending on the data set or identification strategy, studies focusing on the general relationship between labor supply and caregiving find either no significant effect (e.g. Wolf and Soldo, 1994; Stern, 1995) or a negative impact of caring hours on labor supply (e.g. Ettner, 1995, 1996; Johnson and Lo Sasso, 2000). Carmichael and Charles (1998) argue that LTC is similar to childcare in that it affects the trade-off between leisure and consumption. Intensive informal care is associated with negative labor market effects. Heitmueller (2007) emphasizes that the effect depends also on other factors such as the preference relation between leisure and care (substitutes or complements) or the availability of alternative care services. While he cannot identify an effect for extra-residential carers, he finds a negative relationship for co-residential carers. Carmichael and Charles (1998, 2003) find that labor supply is negatively affected, firstly, by a direct effect of caring and, secondly, by an indirect effect that is due to wage penalties for carers. Viitanen (2005) uses European data and account for individual heterogeneity, state dependency and country specific effects. Thereby, she finds a negative impact of care-giving on labor supply for Germany but not for any of the other European countries analyzed. Schneider et al. (2001) examine data of the German Socio-economic Panel Study (SOEP) of waves 1985 through 1996. They find that the existence of a household member in long-term care increases a women's propensity to change from working to not working, but cannot find a significant relationship for the reduction of labor supply from full time towards part time. Meng (2013) uses more recent waves of the SOEP (2001–2007) and while she cannot find a significant effect of caring on labor participation, she finds a small negative effect on working hours that is slightly larger for men than for women. In another study, also using SOEP, Meng (2012) estimates the effect that caring responsibilities have on retirement decisions and finds that the propensity to turn into retirement significantly increases, if people are engaged in long-term care.

The influence of LTCI or other institutions on labor supply of carers is not extensively analyzed. For Norway, Løken et al. (2014) find that particularly daughters reacted to an increase of formal care supply by reducing their work absences. For Japan, Sugawara and Nakamura (2014) show that the negative relationship between care and female labor supply became weaker after the introduction of a LTCI in 2000. In contrast to Germany, the Japanese LTCI provides only formal services and no cash allowances.[3] A positive

[3] Campbell et al. (2010) compare the LTCI in Germany and Japan while discussing possible conclusions

labor supply effect of benefits in kind was also found in Geyer and Korfhage (2015). They analyze the LTCI in Germany using a structural labor supply model. Their extended choice model includes the decision about the type of benefits from the LTCI. They find that benefits in kind have a small positive labor supply effect and cash benefits seem to have negative labor supply effects.[4] Skira (2015) estimates a dynamic structural model for the US and finds, in addition to a negative labor supply effect of care allowances, that care leave can strengthen carer's labor market attachment. Heger (2014) compares the labor supply of caregivers across different institutional settings using data from the Survey for Health, Aging and Retirement in Europe (SHARE). She finds negative effects on labor participation in countries with few formal care options, the effect is insignificant in countries with more generous care systems.

3. Institutional setting

Germany introduced a universal-coverage social insurance program for LTC in April 1994. Before the reform, there were very few elements of the social system that directly supported people in need of LTC. In particular, support for LTC was (and still is) part of means-tested social assistance (*Hilfe zur Pflege*). Eligibility for receiving benefits requires individuals first to exhaust all private assets and income resources. Furthermore, close family members are supposed to give financial support, before the social assistance chips in. Private care insurance had been available since the mid-1980s, but it only played a minor role and failed to reach the majority of the population (Götting et al., 1994).[5]

The new insurance system provides benefits without regard to age or financial status of the person in need of LTC.[6] The amount of benefits depends on the level of impairments. If the individual in need of LTC has permanent (at least six months) impairments in at least two activities of daily living (ADL) and one instrumental activity of daily living (IADL), she is eligible for benefits from the LTCI (for more details, see Table 1). Depending on the degree of impairments, three care levels are distinguished (see Schulz, 2010, for more details). The care-levels are assessed by the Medical Service of the Health Funds (MDK) or by other independent evaluators. The eligible care recipient can choose between cash benefits, benefits in kind, or a mix of both types. In 1995, monthly cash benefits amount to 205 euro (care level I) up to 665 euro (care level III) and could be used to pay family carers. Cash benefits amount to 10% of average gross earnings in care level I up to 33% in care level III. However, they are neither earmarked nor is spending monitored. If the individual

for the US.

[4] Zuchandke et al. (2010) show that household's self-perceived financial security in case of LTC need has improved in Germany after the reform.

[5] Cuellar and Wiener (2000) provide a general discussion of experiences and lessons from the introduction of LTCI in Germany.

[6] The long-term care expenses are financed by income-related contributions that are split equally between employees and employers (in the beginning, employers were compensated by changing the Penance Day from a holiday to a regular working day). In 1995, the initial contribution rate was 1%. To finance further benefits, the contribution rate was raised to 1.7% in 1996 and to 1.95% in 1998.

chooses benefits in kind, she receives formal care services and the nursing service is directly reimbursed by the LTCI. In 1997 about 77% of benefit recipients received cash benefits, 7% relied only on benefits in kind, and 10% combined both types of benefits (BT-Drucksache 13/9528, 1997). It is important to note that coverage of LTCI comprises only a part of the care risk. Regardless of choosing benefits in kind or in cash, the care recipient always needs a certain amount additional help that is usually provided informally.[7]

In addition to the two benefit schemes, the LTCI offers a set of other benefits related to employed carers and to the relief of intensive carers. Employed carers can take an unpaid leave of up to six months and emergency leave for medical reasons up to ten days per year. The LTCI also includes different forms of relief for intensive carers. They have a right to take a leave of up to four weeks per year in which the LTCI organizes care. Furthermore, there is the possibility to apply for short-term stationary care of up to four weeks per year. All these benefits help carers to deal with their care obligations and should be positively related to the employment probability.[8]

Table 1: Benefits from the LTCI by care level in 1995 (monthly amounts)

	Care level		
	I	II	III
Cash benefits	205	410	665
Benefits in kind	384	921	1432
No. of beneficiaries	532,000	490,000	143,000
in %	45.6	42.1	12.3
benefits/earnings	10.2%	20.5%	33.2%
Necessary care:	Limitations in at least two ADL (personal hygiene, feeding, mobility; so called "basic care" (*Grundpflege*). And limitations in at least one IADL. Average care needed per day of at least 90 minutes. More than 45 minutes have to be necessary for basic care.	Average care needed per day of at least 180 minutes. More than 120 minutes have to be necessary for basic care.	Average care needed per day of at least 300 minutes. More than 240 minutes have to be necessary for basic care.

Note: The person in need of care can choose between both types of benefits or combine them. Cash benefits are directly paid to the individual while benefits in kind reimburse formal care services. The amounts remained stable until 2008. Relative benefits are shown in relation to the average gross monthly earnings (national accounts). The number of beneficiaries refers to the number of people in ambulatory care.

[7] LTCI also includes benefits for nursing home care. For two reasons this is of minor importance for our analysis. First, home care is generally preferred over stationary care. People prefer to stay in familiar surroundings and stationary care is chosen only if it is unavoidable. The share of home care of all individuals who received benefits from the LTCI amounted to 75% at the end of 1996. Second, we only analyze multi-person households. Klein (1998) shows that the number of household members plays a crucial role for nursing home entry. He finds that an additional household member reduces the chance of entering a nursing home by 38%. If the person in need for LTC is married to the additional household member, the effect is even larger at about 80%. Therefore, we assume that the introduction of nursing home care support only has a minor effect on the sample we are analyzing.

[8] Moreover, intensive carers receive a small amount of additional pension entitlements. Since 2008, workers in firms with more than 50 employees can request a reduction in working hours (unpaid) for a period of up to six months (renewable once) but that is outside of our observation period.

4. Data and estimation sample

We use data from the SOEP, a representative panel study of households and individuals. Started in 1984, in 2011 the SOEP annual survey included about 20,000 individuals living in almost 13,000 households.[9] Using data from SOEP has the advantage that it contains questions to identify individuals in need for LTC prior to the introduction of the LTCI in 1995. Additionally, it comprises a large set of socio-economic variables that allow us to control for changing group compositions over time, i.e. treated and non-treated individuals.

Sample

While benefits for home care paid in cash were already available in January 1995, benefits in kind could not be obtained until July 1995. To be able to compare decisions before and after the reform was completely implemented, we omit all observations gathered in 1995. Moreover, we excluded a subsample of SOEP (Sample D, a special migration sample) from the estimation because it was first surveyed in 1995 and could, therefore, include biased post-reform effects. We concentrate on only West-Germany because during the 1990s the East-German labor market was characterized by the transition process from state planned socialism to a market economy.

We focus on people living in multi-person households and limit the sample to individuals aged between 45 and 65 who are fully able to participate on the labor market. In general we drop pensioners but keep retired individuals in the sample, if they retired only one year before $(t-1)$. The reason is that for elder carers who decide to reduce labor supply, it could be more convenient to exit the labor marked permanently and turn to early retirement instead of relying on unemployment insurance (Meng, 2012).[10] Omitting all pensioners from the sample could, therefore, lead to an underestimated labor market effect. Retirement decisions are permanent and working after retirement (at least in the period we are interested in) is very uncommon in Germany. Keeping all pensioners in the sample could, thus, overestimate the true labor market effect of the reform.

SOEP does not contain information about the receipt of LTCI benefits before 2001. We identify the treatment group by a more general LTC indicator. The indicator is constructed from a question to the household head. Individuals are asked, if there is anyone in their household who is permanently receiving care for reasons of old age or health. We assume that people who have a household member in need for care were affected by the implementation of the LTC reform. If individuals in the sample report to have a household member in need for care, they are assigned to the treatment group. If no household member in need for care exist, they are assigned to the control group. As the question is unrelated to the benefit receipt from the LTCI, there is no incentive for survey participants to self-select themselves into a certain group or change answering behavior after treatment. As described above, eligibility – even for care level I – requires a substantial amount of time

[9] To obtain detailed information about SOEP, see Wagner et al. (2007).
[10] At the time the LTCI was introduced, early retirement was possible at the age of 60.

spent on care (90 minutes per day).

We pool observations two years before and after the treatment. Overall, we observe 2,437 males (2,231 females) before 1995 and 2,287 (2,205) after. Thereby, 89 males (88 females) report to have a household member in need of care before treatment and are part of the pre-treatment treatment group and 71 (92) belong to the post-treatment treatment group.

Variables

We are interested in the effect of introducing LTCI on the labor supply of co-residential carers. The labor market status is defined by a binary employment indicator and by reported hours of work (including overtime). Covariates include variables that might affect the individual's labor supply decision. They are included into the econometric models to avoid biases from changing group composition and comprise socio-economic variables such as age,[11] migration background, working experience, education, non-labor income and self-reported health status of the potential carer.[12] If households live in smaller communities they could be forced to rely on informal care more often than they would in larger communities where infrastructure for formal care could be better developed. Therefore, we include community size.

Household size is also an important control variable. Additional household members might have diverse effects on labor supply. Firstly, the presence of children in the household might affect the total time available to the caring household member. Secondly, additional adults might provide further financial or time resources that could be seen as other non-labor income or other informal help that is provided to the person in need for care. Hence, different household compositions could either lead to an increase or a decrease of the carer's labor supply. To capture possible effects of additional household members, we use a dummy to indicate if more than two persons live in the household.

Labor supply might also depend on the actual need of the disabled household member. Therefore, dummies are used to capture the type of support that is needed to help the dependent household member. SOEP includes a question that asks in four categories for impairments in activities of daily living. The four answer categories are arranged hierarchically in ascending order. If a person needs major care, it is assumed that he or she relies on minor care categories as well, meaning that each household can only be associated with one of the four categories (or non) and that shares always add up to 100%.[13]

[11] Age enters the models in a flexible way. Additionally to a linear and a squared term, a dummy is included that indicates whether a person is aged 60 or older. This is to avoid that estimated effects are simply driven by changing portions of individuals who have access to an early retirement program.

[12] To determine the health status, individuals are asked the following question: *"How would you describe your health at present? Very good, good, satisfactory, poor, very poor."* Unfortunately, the question is not asked in 1993. If models include the year 1993, its values are taken from answers given in 1992 or 1994.

[13] The exact answer categories are as follows: *"Needs assistance with – 1. errands outside of the house; 2. running the household, preparing meals and drinks; 3. minor care, such as help with dressing himself, washing up, combing hair, shaving; 4. major care, such as getting in and out of bed".*

5. Estimation Strategy

We treat the introduction of the LTCI as a quasi-experiment and exploit the exogenous variation induced by this reform using a DiD estimation. In order to construct a counterfactual, the sample is divided into different groups: Firstly, a control group that is not influenced by the reform is split into observations made before and after treatment (pre- and post-treatment control-group). Secondly, a treatment group that is affected by the reform is split into observations made before and after treatment as well (pre- and post-treatment treatment-group). Only the post-treatment treatment group is actually affected by the exogenous policy change.

In order to estimate causal effects, it is crucial to ensure that a number of identifying assumptions hold. Most notable are the assumptions of a *stable unit treatment value* and a *common trend*.[14]

The assumption of a *stable unit treatment value* states that treatment must only affect the post-treatment treatment group. Neither should the treatment group be affected before the policy reform was introduced nor should the control group be affected in any period through interactions between the members of the population. We innocuously assume that the need of care is exogenous. This means for co-residential carers (our treatment group), individuals cannot select themselves in the treatment group unless people move between households because of the reform. Therefore, as a robustness check we estimate our model including only households whose composition did not change during the observation period. For the same reason, a small part of the control group might be affected by the reform. Since we cannot identify extra-residential carers, we have some in the control group. For two reasons we think that this is not a severe problem for our estimation. First, the group is small: only about 3% of all working age individuals provided extra-residential care on a regular basis (data from 2001, see Geyer and Schulz (2014)). Second, on average they provide less intensive care than co-residential carers and previous studies showed that their labor market behavior is not affected by the provision of LTC (Heitmueller, 2007). The control group is also affected by the compulsory contribution to finance the insurance. As the contribution rate of 1% at the beginning is rather low, we assume that its effect on labor supply is negligible. It is also unlikely that labor market decisions made by the treatment group are large enough to affect equilibrium wages and, therefore, the labor supply of individuals in the control group. The number of people treated seems too small to affect the entire labor market.

The *common trend* assumption implies that the potential non-treatment outcomes follow the same trend independently of group membership. That is, both the treatment- and control-groups ought to be influenced by the same macro-trends. This assumption can be relaxed, if the group compositions of treatment and control group differ and if covariates can be found that capture all variables that would otherwise lead to different time trends.

[14] More background in the identifying assumptions of the DiD approach is available in Lechner (2011) and Blundell and Dias (2009).

Then, the common-trend assumption must hold conditional on the covariates.

If the identifying assumptions hold, the treatment effect can be estimated in a regression framework. Thereby, biases resulting from permanent differences between treatment and control group as well as biases resulting from macro trends that are unrelated to the change in policy regulation are removed (Imbens and Wooldridge, 2009). To do so, dummy variables are constructed to indicate group membership. $Tr_i \in \{0,1\}$ indicates whether individual i belongs to the treatment group ($Tr = 1$) and $Post_t \in \{0,1\}$ indicates whether observations are made after treatment has occurred ($Post_t = 1$). Our estimation strategy can be summarized in the following equation:

$$y_{it} = \alpha + \beta(Tr_i \times Post_t) + \lambda Tr_i + \delta Post_t + \mathbf{X_{it}'}\gamma + e_{it}, \tag{1}$$

where y_{it} measures the labor supply of individual i at time t, α is a constant, $\mathbf{X_{it}}$ is a vector of covariates, and e_{it} represents the error-term. The coefficients β, λ, δ, and γ are to be estimated. The coefficient of interest, β, captures the causal labor supply effect of the LTCI.

When estimating binary or censored dependent variables, in our case employment status and working hours, generally, non-linear models such as *probit* or *tobit* come to mind. However, in a DiD framework these models cannot be applied without further assumptions. As Lechner (2011) shows, the common-trend assumption only holds in a non-linear specification, if there is no group specific difference in the dependent variable. This means that treatment and control group ought to start at the same average initial levels of labor market participation (or working hours) before treatment. As this assumption is most likely not valid in our case, we continue with a linear specification. We analyze the labor supply behavior separately for men and women.

6. Descriptives and discussion of identifying assumptions

In this section, we provide descriptives for all groups that are constructed for the econometric model. We show how LTCI and carer's labor supply might interact and discuss whether all necessary identifying assumptions are met.

Graphical illustration

To give initial insights into the development of employment over time and to provide a visual impression of trend components in each group, in Figures 1, 2, 3, and 4 we plot the employment rates as well as hours worked for men and for women from 1985 through 2010. The dashed line represents the control group while the continuous line shows values of the treatment group. The gray background indicates the time period that is used to calculate the econometric models presented in Section 7. Note that the SOEP questionnaire

asking for the need of care changed after 1990, using a broader definition of care.[15] Hence, comparisons of pre-1990 results with those from 1990 or later can be misleading. The graphs therefore only display developments on the basis of the current questionnaire.

Male overall labor participation – represented by the control group in Figure 1 – lies between about 75% and 85% over the whole observation. As the number of observations in the treatment group is much smaller, the volatility over time is higher. While their employment rates always fall below the values of the control group, the magnitude of the gap changes over time. In general, the gap is smaller before the LTCI was introduced. After 1997 it stays roughly constant.

The graphs for male working hours in Figure 2 show almost the same picture. Note that working hours are unconditional on employment status, they are set to *zero* if an individual is not working.

This first graphical analysis supports the assumption that changing regulations relating to LTC affected male carers labor supply. The almost parallel proceeding trends in the years from 1991 to 1995 support the idea of a *common trend* in treatment and control group that is only disturbed by reforms relating to long term care. The figure also suggests that the reform had an immediate impact on labor supply and lasted for several years. The small drop at the end of the observation period in 2008 could be related to the first *extensive* reform of the LTCI after 1995, increasing not just monetary support but also provided for professional help to informal carers.[16] Because the treatment group in our analysis consists of a small number of observations, we cannot rule out that similar trends in the treatment and control group might be caused by random sample composition.[17]

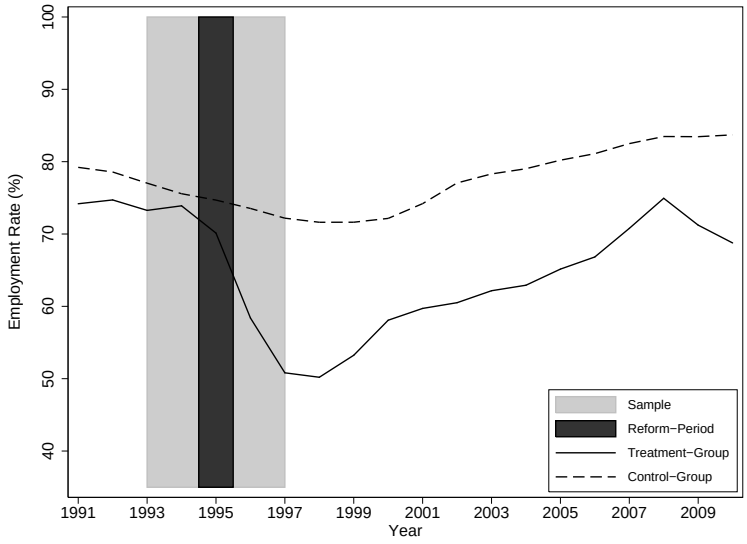
Figure 3 shows female employment rates. From 1991 until 2010 employment rates of the control group increase steadily from about 50% to 70%. Unlike men, women's labor supply in the treatment group exhibits less variation. Even though its level is about 20 percentage points lower in the treatment group its graphical representation almost always moves parallel to the line representing the control group. A similar picture can be seen in Figure 4, which shows female working hours. However, after 2003 working hours start stagnating and even declining after 2007. In general we do not find visual indication of a link between care reform and labor supply of women.

[15] While the general question is unchanged (*"Is there anyone in your household who is receiving care because of old age or health reasons?"*), the context with in it is asked in changes after 1990. Prior to 1990 LTC is classified into *"bed-ridden"* or *"not bed-ridden, but in need of help with daily domestic tasks in the household"*. After 1991 it is contextualized into care categories that comprise a broader definition of care. It is possible to distinguish between help with *"running errands outside the house, running the household, meals and drinks, simple tasks, e.g. help with dressing, washing, etc., and complex tasks, e.g. moving from the bed, bowel movements, etc."*

[16] The so called *Pflegeweiterentwicklungsgesetz* came into force in July 2008. For details see e.g. Kostorz et al. (2010).

[17] Unfortunately we do not have other data sources from before 1995. In Appendix A we use data from the German microcensus to compare if trend lines behave any different in a sample that includes a larger number of observations for the years after 1995. Unlike SOEP, the microcensus does not allow us to identify the treatment group in the pre-treatment period and cannot be used for the DiD estimation, but can be utilized in order to compare trends in the post-treatment period. Trend lines generated from SOEP and microcensus show similar trends after LTCI was introduced. In particular we can observe a decreasing employment rate of the treatment group after 1995.

Figure 1: Employment Rates (Male)



Note: The dashed line and the continuous line represent the control and the treatment group, respectively. The dark-gray background indicates the time period that is used to estimate the DiD models in Section 7. The light-gray background indicates the time span with different questionnaire. Moving averages are used that are calculated as follows: $y = 0.5(x_t + 0.5x_{t-1} + 0.5x_{t+1})$.
Source: SOEP v30, own calculation.

The graphical analysis gives a fist hint about the average effect of the reform on the labor supply of the treatment group. It appears that while men reacted with a decreased labor supply, the reform did not have any visible effects on the labor supply of women. We observe more volatility of male carer's labor supply compared to female carers. Given the small sample size a possible explanation could be that the group composition of men simply changed over the observation period. The averages are unconditional on covariates and not weighted. Hence, the drop in male labor supply might vanish in the econometric model once covariates are included.

Summary statistics

To have a more detailed view of group compositions and to discuss the model assumptions, in Tables 2 and 3, we present the main summary statistics of treatment and control groups before and after treatment for men and women.

Table 2 provides summary statistics for men. The years before treatment include 1993 and 1994, while years after treatment comprise of 1996 and 1997. As already noted in the graphical analysis above, employment stays almost constant in the control group, decreasing slightly from a 75% employment rate to 71%. The same is true for working hours, which change from about 32.1 to 30.6 hours. However, in the treatment group

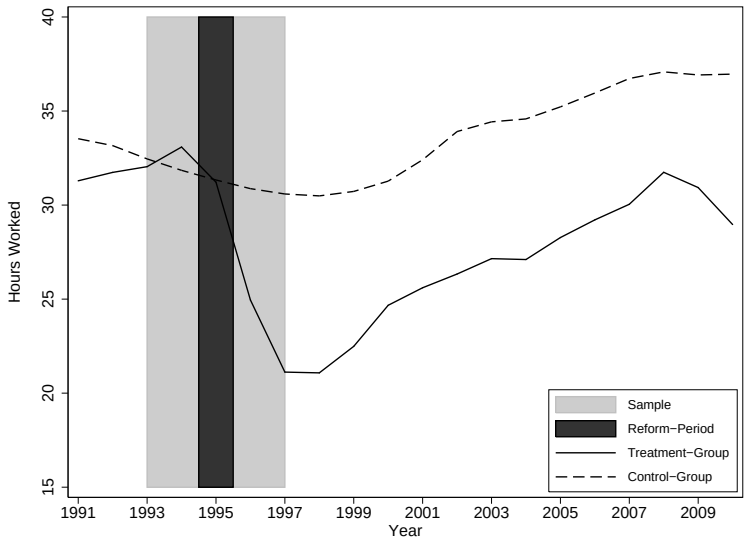
Table 2: Descriptive Statistics for Treatment and Control Group (Male)

	tr before 95	tr after 95	control before 95	control after 95
Employed	0.72	0.49*	0.75	0.71*
Working hours	32.54	21.48*	32.07	30.64*
Retired	0.15	0.28*	0.16	0.16
Age	55.28	56.46	53.91	54.11
Age≥60	0.24	0.34	0.18	0.20
Migration background	0.25	0.30	0.35	0.32*
Working experience in years	32.24	33.42	31.66	31.72
Years of education	11.07	10.68	10.94	11.17*
Health status:				
good – very good	0.36	0.35	0.43	0.41
satisfying	0.39	0.39	0.35	0.35
poor – very poor	0.25	0.25	0.22	0.24
Married	0.92	0.93	0.94	0.92*
Other household income /1000	15.67	17.92	24.13	22.01*
Household size	3.88	3.94	3.20	3.11*
Community size:				
<20,000	0.28	0.14*	0.13	0.13
20,000–100,000	0.46	0.59	0.55	0.57
>100,000	0.26	0.27	0.33	0.30*
HH-member needs help with:				
no help or not known	0.01	0.03		
getting around outside the house	0.11	0.10		
household chores, preparing meals	0.16	0.13		
washing, dressing, etc.	0.37	0.30		
getting into and out-of-bed, etc.	0.35	0.45		
HH-member in need for care is:				
spouse/partner	0.22	0.24		
child	0.33	0.38		
parent	0.36	0.27		
other	0.04	0.07		
more than two or unknown	0.04	0.04		
Observations	89	71	2348	2216

Note: We performed t-tests to check whether means a significantly different before and after treatment (within treatment or control group). * indicates statistical significance on the 5% level. All means are calculated without the use of individual weights. Working hours are not restricted to working, it is zero if a person is not employed. The health status is self-reported. Other household income is the sum of yearly income (before tax) other than the persons own labor income, own retirement benefits and the households benefits from the long-term care insurance. It is reported in 1000Euro and is inflation adjusted (base year = 2006).

Source: SOEP v30, own calculation

Figure 2: Working Hours (Male)

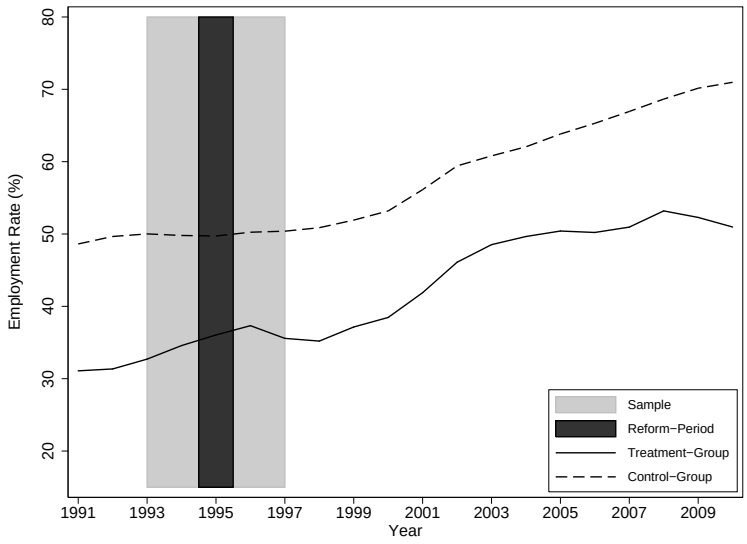


Note: The dashed line and the continuous line represent the control and the treatment group, respectively. The dark-gray background indicates the time period that is used to estimate the DiD models in Section 7. The light-gray background indicates the time span with different questionnaire. Moving averages are used that are calculated as follows: $y = 0.5(x_t + 0.5x_{t-1} + 0.5x_{t+1})$.
Source: SOEP v30, own calculation.

both variables measuring labor supply decrease. While averages are similar to the control group before treatment at 72% employment participation and 32.5 weekly hours worked, they drop to 49% and 21.5 hours in the post treatment period. Both of those changes are significant at 5%. As previously discussed, a part of that reduction seems to be driven by individuals who choose to leave the labor market and turn to early retirement. Remember that we only keep those pensioners in the sample, who have retired the year prior to the observation. The fraction of new retirees increases from 15% to 28% in the treatment group, while it stays constant at 16% in the control group. When we try to estimate causal effect of the reform, it is important to avoid biases through changing group composition. Thus, in regard to pensioners, we check for changing age structure in the sample. In Table 2, it can be seen that the share of men aged 60 or older increases from 24% in the *pre-treatment treatment group* to 34% after treatment. Even though this change does not prove to be statistically significant at the 5% level, one should not rule out that a part of the increase of pensioners might relate to a higher share of individuals who are eligible for early retirement.

The availability of formal care services might depend on the size of the community the household lives in. Table 2 reveals that while the fraction of households living in communities larger than 100,000 people stays constant at around 27% in the treatment

Figure 3: Employment Rates (Female)



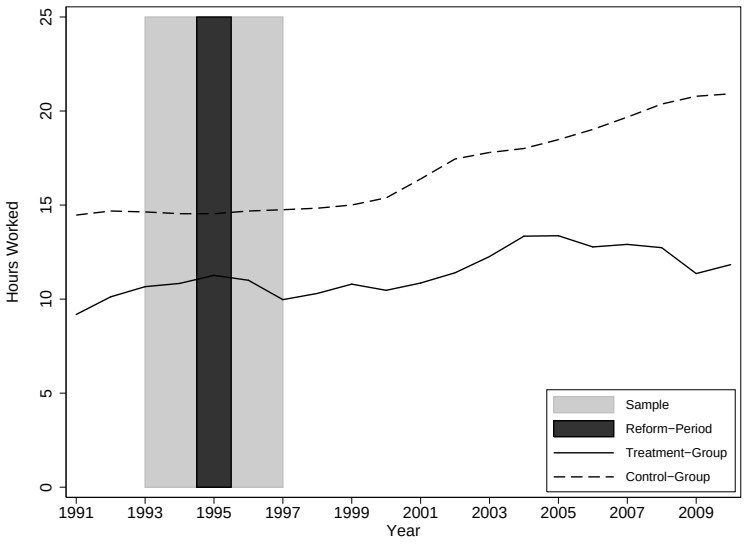
Note: The dashed line and the continuous line represent the control and the treatment group, respectively. The dark-gray background indicates the time period that is used to estimate the DiD models in Section 7. The light-gray background indicates the time span with different questionnaire. Moving averages are used that are calculated as follows: $y = 0.5(x_t + 0.5x_{t-1} + 0.5x_{t+1})$.
Source: SOEP, own calculation.

group before and after treatment, a shift from small to medium sized communities can be observed. While 28% of households live in communities smaller than 20,000 before treatment, after treatment this is true for only 14%. If the argument about different access to formal care is correct, this could result in a downward bias of the estimated reform effect, if community size is not controlled for.

Further changes in group composition of the treatment group can be observed in the amount of care needed. While 35% of all household members rely on major help (the highest category: getting into and out-of-bed, etc.) before treatment, after treatment this is true for 45% of all households. It is obvious that these changes can potentially drive the demand for informal as well as formal care. Even though t-tests for the differences do not prove to be significant at the 5% level, controls will be added to capture these changes. Other variables that might influence the estimates, are migration background, that increases from 25% in the pre-treatment treatment period to 30% afterwards and other household income that increases from 15,670 euro to 17,920 euro.

Table 3 provides summary statistics for women and reveals a different picture compared to the male sample. Both variables to measure labor supply stay about constant over time in both groups. However, both participation rates and working hours are considerably larger in the control group than the treatment group. While the employment rate of the

Figure 4: Working Hours (Female)



Note: The dashed line and the continuous line represent the control and the treatment group, respectively. The dark-gray background indicates the time period that is used to estimate the DiD models in Section 7. The light-gray background indicates the time span with different questionnaire. Moving averages are used that are calculated as follows: $y = 0.5(x_t + 0.5x_{t-1} + 0.5x_{t+1})$.
Source: SOEP, own calculation.

treatment group is about 33%, it amounts to 48% in the control group. Working hours are at about 10.6 compared to 14.6. Interestingly, the fraction of retirees increases in the treatment group after the reform. But different from the male sample, this happens even though the share of women aged 60 years or older decreases from 34% to 30%. Overall, the descriptives for women reveal less variation in characteristics between the different groups, compared to the differences that are found for men. Yet, women in the treatment group are older on average than women in the control group – in particular the fraction of individuals above 60 years being larger. Also, working experience is about three years longer in the *post-treatment treatment group* than it is before treatment and the portion of household members needing major care (getting into and out-of-bed, etc.) increases from 27% to 38%.

In summary, given the small sample size of the treatment group, it is difficult to provide unambiguous evidence that supports the *common trend*. It seems to be a reasonable assumption for female carers. For men it seems to be different. The unconditional trend of employment rates and working hours shows volatility that we cannot explain in some periods. One concern is that the volatility is related to changing group composition. However, most differences between treatment and control group turn out to be insignificant. Furthermore, it cannot be related to other policy reforms, since in the years of our analysis

Table 3: Descriptive Statistics for Treatment and Control Group (Female)

	tr before 95	tr after 95	control before 95	control after 95
Employed	0.33	0.35	0.48	0.49
Working hours	10.57	10.45	14.56	14.68
Retired	0.11	0.20	0.11	0.12
Age	56.30	55.47	53.61	53.48
Age≥60	0.34	0.30	0.18	0.18
Migration background	0.28	0.28	0.31	0.29
Working experience in years	16.22	19.55	18.68	19.53*
Years of education	9.72	9.76	10.09	10.34*
Health status:				
good – very good	0.26	0.32	0.36	0.33*
satisfying	0.48	0.38	0.39	0.42*
poor – very poor	0.26	0.30	0.25	0.25
Married	0.86	0.86	0.90	0.88*
Other household income /1000	34.06	29.27	38.96	36.74*
Household size	3.58	3.59	2.94	2.88
Community size:				
<20,000	0.22	0.23	0.12	0.13
20,000–100,000	0.51	0.50	0.54	0.55
>100,000	0.27	0.27	0.34	0.31*
HH-member needs help with:				
no help or not known	0.02	0.01		
getting around outside the house	0.11	0.09		
household chores, preparing meals	0.19	0.13		
washing, dressing, etc.	0.40	0.39		
getting into and out-of-bed, etc.	0.27	0.38		
HH-member in need for care is:				
spouse/partner	0.25	0.30		
child	0.33	0.32		
parent	0.31	0.30		
other	0.07	0.02		
more than two or unknown	0.04	0.05		
Observations	88	92	2143	2113

Note: We performed t-tests to check whether means a significantly different before and after treatment (within treatment or control group). * indicates statistical significance on the 5% level. All means are calculated without the use of individual weights. Working hours are not restricted to working, it is zero if a person is not employed. The health status is self-reported. Other household income is the sum of yearly income (before tax) other than the persons own labor income, own retirement benefits and the households benefits from the long-term care insurance. It is reported in 1000Euro and is inflation adjusted (base year = 2006).
Source: SOEP v30, own calculation

no major additional reform was implemented that could yield different macro trends for treatment and control group. Therefore in the following section we test the treatment effect in a regression framework that takes group characteristics into account.

7. Results

Regression results are presented in Tables 4, 6, 5, and 7. All models are estimated using ordinary least squares (OLS) and standard errors are clustered on household level. We estimate seven models for each group and dependent variable using different sets of control variables.

Table 4 presents the results for regressions on male employment status. The variable of

particular interest is the interaction term $Post95 \times Tr$ that measures the treatment effect. For all models, its coefficient is negative and statistically significant at least at 5% or 10%. Without covariates (Model 1), the estimate suggests that the introduction of the LTCI led to an average reduction of male labor supply of 19.3 percentage points in the treatment group. The effect is the same as depicted in Figure 3. The regression framework shows that the large estimate – in relative terms it means a reduction in employment by about 30% – is very imprecise with a standard error of 0.082. Nonetheless, the point estimate remains remarkably stable when introducing more controls. It is slightly reduced to 14.8 percentage points but still significant when we introduce a dummy for all carers above the age of 59 (Model 2). Introducing more controls does not change the estimated coefficient much. In the full specification (Model 7), which also controls for community size and level of impairments, the point estimate of the interaction is at 14.6 percentage points with a standard error of 6.8 percentage points.

The coefficient on Tr indicates that initial differences between treatment and control group are not significant in any of the models. No matter what, $Post95$ is always negative and significant at 1% or 5%, respectively. Most of the common covariates to explain labor supply have the expected signs and most are significant.

Results for male working hours, presented in Table 5, reveal an equivalent picture. The effect of the reform is significant at the 1% or 5% level for all models. Estimated effects vary between -6.5 and -9.6 hours with a standard error of about three hours. Patterns and signs of covariates are equivalent to those found in the models of employment rates.

While we find a stable negative effect for men, we do not find any significant effect for women. Table 6 presents the results for regressions on female employment rates. The coefficient of $Post95 \times Tr$ is never statistically significant. Model (1) again resembles the graphical analysis, which showed no indication of an effect of the LTC reform. The descriptives suggested that women in the treatment group worked less than women in the control group throughout the observation period. The coefficient of Tr is significantly negative only in model (1). The effect disappears as soon as $Age \geq 60$ is included into the model. This indicates that the unconditional differences are mainly driven by different age structures – at least in the time period that is analyzed within the model. Most covariates have expected signs and are significant.

Table 7 shows results for working hours and reveals no treatment effects on women. Results are almost equivalent to the model on employment rates.

Our main concern from the descriptive analysis was that the drop in male employment rates was driven by small sample size. Yet, even with a full set of covariates we find a significantly negative effect on both employment and working hours. Women seem not to react to the reform. In order to test whether this effect is robust we conducted a series of tests of our model which are documented in the next section.

Table 4: Regression on Male Employment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post95	−0.033**	−0.026*	−0.023*	−0.031**	−0.026*	−0.023*	−0.025*
	(0.013)	(0.011)	(0.011)	(0.010)	(0.010)	(0.010)	(0.010)
Tr	−0.029	−0.002	0.012	0.017	0.021	0.020	−0.087
	(0.060)	(0.055)	(0.056)	(0.053)	(0.054)	(0.054)	(0.104)
Post95× Tr	−0.193*	−0.148*	−0.144*	−0.127†	−0.134†	−0.141*	−0.146*
	(0.082)	(0.074)	(0.073)	(0.070)	(0.071)	(0.070)	(0.068)
Age≥60		−0.506**	−0.092*	−0.075*	−0.086*	−0.085*	−0.084*
		(0.022)	(0.036)	(0.034)	(0.034)	(0.034)	(0.034)
Age			0.234**	0.204**	0.196**	0.188**	0.188**
			(0.030)	(0.030)	(0.029)	(0.030)	(0.030)
Age^2/100			−0.247**	−0.234**	−0.223**	−0.215**	−0.215**
			(0.029)	(0.028)	(0.028)	(0.028)	(0.028)
Migration background				−0.011	−0.009	−0.014	−0.008
				(0.020)	(0.019)	(0.019)	(0.019)
Working experience in years				0.019**	0.018**	0.018**	0.018**
				(0.002)	(0.002)	(0.002)	(0.002)
Years of education				0.039**	0.035**	0.034**	0.034**
				(0.003)	(0.003)	(0.003)	(0.003)
Health status:							
good - very good (base)							
satisfying					−0.037**	−0.036**	−0.036**
					(0.013)	(0.013)	(0.013)
poor – very poor					−0.163**	−0.158**	−0.157**
					(0.020)	(0.020)	(0.020)
Married					0.016	0.011	0.009
					(0.032)	(0.033)	(0.032)
Other household income /1000						0.001**	0.001**
						(0.000)	(0.000)
Household size≥3						0.035*	0.028†
						(0.017)	(0.017)
Community size:							
<20,000 (base)							
20,000–100,000							0.020
							(0.025)
>100,000							−0.035
							(0.027)
HH-member needs help with:							
getting around outside the house (base)							
household chores, preparing meals							−0.017
							(0.155)
washing, dressing, etc.							0.155
							(0.115)
getting into and out-of-bed, etc.							0.153
							(0.107)
R^2	0.01	0.21	0.26	0.34	0.36	0.36	0.37
Obs.	4724	4724	4724	4724	4724	4724	4724

Note: Values denote estimated coefficients. Standard errors are clustered on household level and reported in parentheses. Constants are calculated in each model but not reported here.

Significance levels: † p <0.10, ∗ p <0.05, ∗∗ p <0.01

Source: SOEP v30, own calculation.

Table 5: Regression on Male Hours Worked

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post95	−1.432*	−1.141*	−1.012*	−1.438**	−1.225*	−1.073*	−1.132*
	(0.578)	(0.518)	(0.493)	(0.482)	(0.483)	(0.482)	(0.483)
Tr	0.467	1.604	2.281	2.233	2.436	2.383	−1.469
	(3.016)	(2.836)	(2.834)	(2.634)	(2.649)	(2.666)	(4.725)
Post95× Tr	−9.625**	−7.735*	−7.560*	−6.533*	−6.855*	−7.245*	−7.410**
	(3.399)	(3.076)	(3.024)	(2.896)	(2.918)	(2.874)	(2.811)
Age≥60		−21.356**	−2.966†	−2.283	−2.788†	−2.735†	−2.662†
		(1.026)	(1.652)	(1.568)	(1.547)	(1.543)	(1.548)
Age			9.649**	8.566**	8.218**	7.758**	7.768**
			(1.430)	(1.429)	(1.413)	(1.417)	(1.417)
$Age^2/100$			−10.291**	−9.874**	−9.412**	−8.931**	−8.943**
			(1.370)	(1.348)	(1.335)	(1.340)	(1.341)
Migration background				−1.930*	−1.833*	−2.151*	−1.893*
				(0.921)	(0.905)	(0.905)	(0.907)
Working experience in years				0.765**	0.704**	0.709**	0.704**
				(0.103)	(0.101)	(0.100)	(0.100)
Years of education				1.923**	1.759**	1.722**	1.735**
				(0.172)	(0.169)	(0.166)	(0.167)
Health status:							
good - very good (base)							
satisfying					−1.453*	−1.406*	−1.387*
					(0.664)	(0.660)	(0.660)
poor – very poor					−7.086**	−6.815**	−6.780**
					(0.931)	(0.925)	(0.919)
Married					1.319	1.044	1.007
					(1.434)	(1.419)	(1.411)
Other household income /1000						0.051**	0.051**
						(0.014)	(0.014)
Household size≥3						2.062*	1.792*
						(0.821)	(0.825)
Community size:							
<20,000 (base)							
20,000–100,000							0.446
							(1.240)
>100,000							−1.579
							(1.343)
HH-member needs help with:							
getting around outside the house (base)							
household chores, preparing meals							−3.783
							(6.887)
washing, dressing, etc.							6.761
							(5.063)
getting into and out-of-bed, etc.							5.579
							(5.198)
R^2	0.00	0.17	0.22	0.31	0.32	0.33	0.33
Obs.	4724	4724	4724	4724	4724	4724	4724

Note: Working hours are not conditional on working. If a person is not working, hours are equal to *zero*. Values denote estimated coefficients. Standard errors are clustered on household level and reported in parentheses. Constants are calculated in each model but not reported here.

Significance levels: † p <0.10, ∗ p <0.05, ∗∗ p <0.01

Source: SOEP v30, own calculation.

Table 6: Regression on Female Employment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post95	0.011	0.013	0.009	−0.013	−0.011	−0.011	−0.010
	(0.013)	(0.012)	(0.012)	(0.011)	(0.011)	(0.011)	(0.011)
Tr	−0.149*	−0.079	−0.057	0.005	0.004	0.009	0.013
	(0.060)	(0.057)	(0.057)	(0.053)	(0.053)	(0.053)	(0.096)
Post95× Tr	0.008	−0.010	−0.015	−0.058	−0.059	−0.060	−0.075
	(0.071)	(0.065)	(0.062)	(0.061)	(0.061)	(0.062)	(0.062)
Age≥60		−0.423**	−0.092*	−0.082*	−0.091*	−0.091*	−0.089*
		(0.020)	(0.039)	(0.036)	(0.036)	(0.036)	(0.036)
Age			0.082*	0.057	0.065†	0.065†	0.066†
			(0.041)	(0.036)	(0.036)	(0.036)	(0.036)
Age^2/100			−0.102**	−0.084*	−0.090**	−0.089**	−0.091**
			(0.038)	(0.034)	(0.034)	(0.034)	(0.034)
Migration background				−0.028	−0.021	−0.019	−0.027
				(0.023)	(0.023)	(0.023)	(0.023)
Working experience in years				0.018**	0.018**	0.018**	0.018**
				(0.001)	(0.001)	(0.001)	(0.001)
Years of education				0.019**	0.016**	0.016**	0.015**
				(0.005)	(0.005)	(0.005)	(0.005)
Health status:							
good - very good (base)							
satisfying					−0.064**	−0.064**	−0.063**
					(0.017)	(0.017)	(0.017)
poor – very poor					−0.125**	−0.126**	−0.126**
					(0.022)	(0.022)	(0.022)
Married					−0.095**	−0.093**	−0.092**
					(0.027)	(0.027)	(0.027)
Other household income /1000						0.000	0.000
						(0.000)	(0.000)
Household size≥3						−0.017	−0.012
						(0.020)	(0.020)
Community size:							
<20,000 (base)							
20,000–100,000							0.024
							(0.030)
>100,000							0.054†
							(0.032)
HH-member needs help with:							
getting around outside the house (base)							
household chores, preparing meals							−0.087
							(0.124)
washing, dressing, etc.							−0.016
							(0.096)
getting into and out-of-bed, etc.							0.080
							(0.110)
R^2	0.00	0.11	0.15	0.34	0.35	0.35	0.35
Obs.	4436	4436	4436	4436	4436	4436	4436

Note: Values denote estimated coefficients. Standard errors are clustered on household level and reported in parentheses. Constants are calculated in each model but not reported here.

Significance levels: † p <0.10, ∗ p <0.05, ∗∗ p <0.01

Source: SOEP v30, own calculation.

Table 7: Regression on Female Hours Worked

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post95	0.127	0.194	0.052	−0.680†	−0.686†	−0.751†	−0.723†
	(0.467)	(0.440)	(0.426)	(0.407)	(0.404)	(0.404)	(0.404)
Tr	−3.990†	−1.897	−1.158	1.104	0.925	1.646	0.444
	(2.095)	(2.036)	(2.011)	(1.859)	(1.826)	(1.835)	(2.716)
Post95× Tr	−0.244	−0.774	−0.950	−2.547	−2.494	−2.584	−3.334
	(2.478)	(2.324)	(2.249)	(2.146)	(2.151)	(2.167)	(2.192)
Age≥60		−12.647**	−2.057	−1.824	−2.045	−2.009	−1.939
		(0.739)	(1.362)	(1.274)	(1.268)	(1.268)	(1.268)
Age			2.198	1.213	1.706	1.804	1.869
			(1.460)	(1.290)	(1.273)	(1.272)	(1.275)
$Age^2/100$			−2.898*	−2.129†	−2.535*	−2.704*	−2.770*
			(1.382)	(1.225)	(1.209)	(1.209)	(1.212)
Migration background				1.171	1.428†	1.613†	1.474†
				(0.827)	(0.822)	(0.823)	(0.833)
Working experience in years				0.651**	0.639**	0.617**	0.619**
				(0.028)	(0.028)	(0.028)	(0.028)
Years of education				0.666**	0.577**	0.586**	0.571**
				(0.176)	(0.174)	(0.177)	(0.177)
Health status:							
good - very good (base)							
satisfying					−1.546**	−1.633**	−1.643**
					(0.593)	(0.591)	(0.591)
poor – very poor					−3.170**	−3.308**	−3.328**
					(0.763)	(0.760)	(0.758)
Married					−5.825**	−5.047**	−5.037**
					(1.075)	(1.092)	(1.089)
Other household income /1000						−0.012	−0.012
						(0.011)	(0.011)
Household size≥3						−2.365**	−2.292**
						(0.739)	(0.740)
Community size:							
<20,000 (base)							
20,000–100,000							0.110
							(1.107)
>100,000							0.920
							(1.166)
HH-member needs help with:							
getting around outside the house (base)							
household chores, preparing meals							−2.325
							(3.502)
washing, dressing, etc.							0.597
							(2.916)
getting into and out-of-bed, etc.							5.369
							(3.404)
R^2	0.00	0.08	0.12	0.31	0.32	0.33	0.33
Obs.	4436	4436	4436	4436	4436	4436	4436

Note: Working hours are not conditional on working. If a person is not working, hours are equal to *zero*. Values denote estimated coefficients. Standard errors are clustered on household level and reported in parentheses. Constants are calculated in each model but not reported here.

Significance levels: † p <0.10, ∗ p <0.05, ∗∗ p <0.01

Source: SOEP v30, own calculation.

8. Robustness Checks

The robustness checks include subsample analysis in which we re-estimate the baseline models on smaller samples, changing time spans of pre- and post-treatment periods that result in larger sample sizes, and placebo regressions.

Subsample analysis

We argue above that including pensioners might yield biased results, if their retirement decision is irreversible. Once individuals are retired they do not return to the labor marked. However, we kept those pensioners in the sample who retired in the period prior to the observation to account for the retirement decision. We re-estimated the baseline models using a sample without pensioners. Results are presented in Table 8. For both genders results turn out to be almost unchanged. Treatment effects for men are smaller but stay significant, for women treatment effects are insignificant.

Table 8: Sample without pensioners

	Male-Employment (1)	Male-Employment (7)	Male-Hours worked (1)	Male-Hours worked (7)	Female-Employment (1)	Female-Employment (7)	Female-Hours worked (1)	Female-Hours worked (7)
Post95	−0.037**	−0.034**	−1.582**	−1.577**	0.021	−0.005	0.413	−0.596
	(0.010)	(0.010)	(0.499)	(0.486)	(0.014)	(0.012)	(0.481)	(0.417)
Tr	−0.050	−0.155	−0.160	−4.157	−0.166*	−0.018	−4.445†	−0.497
	(0.051)	(0.102)	(2.805)	(4.908)	(0.065)	(0.102)	(2.309)	(2.923)
Post95× Tr	−0.119†	−0.118†	−6.615*	−6.540*	0.040	−0.044	0.657	−2.726
	(0.071)	(0.068)	(3.047)	(2.889)	(0.078)	(0.064)	(2.772)	(2.279)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Obs.	3946	3946	3946	3946	3907	3907	3907	3907
Obs. in Tr	127	127	127	127	152	152	152	152

Note: The year 1995 is omitted in all models. Values denote estimated coefficients. Standard errors are clustered on household level. Controls include a dummy for age≥60, age^2, age, migration background, working experience in years, years of education, self reported health status, marital status, household size, community size and the amount of help needed by household-member.

Significance levels: † p <0.10, ∗ p <0.05, ∗∗ p <0.01

Source: SOEP v30 v30, own calculations

We also previously noted that estimated effects could be biased if the LTCI had an influence on the decision to move between households (or nursing homes and households) in order to provide family care. To test this concern we re-estimate the model on a reduced sample in which all households that change household composition during the observation period are omitted. We only keep households that keep the same composition throughout the period under study. Results can be found in Table 9. For men estimated effects are larger compared to the baseline model and are significant in all specifications. For women point estimates remain insignificant in regressions on employment; for working hours we find a significant negative effect if the full set of covariates is included into the model.

The common trend assumption is not testable. However, it is more reasonable the more similar treatment and control group are to each other in regard to observables. We use propensity score matching in order to improve the balance between treatment and control group. We perform five-to-one nearest neighbor matching on the probability of belonging to the treatment group before the reform came into force in 1995. Matching covariates

Table 9: Sample with constant household composition

	Male-Employment (1)	Male-Employment (7)	Male-Hours worked (1)	Male-Hours worked (7)	Female-Employment (1)	Female-Employment (7)	Female-Hours worked (1)	Female-Hours worked (7)
Post95	−0.044**	−0.038**	−1.940**	−1.706**	0.007	−0.009	−0.072	−0.756†
	(0.014)	(0.012)	(0.653)	(0.535)	(0.015)	(0.012)	(0.514)	(0.445)
Tr	−0.036	−0.092	0.234	−1.945	−0.152*	0.065	−3.937†	1.414
	(0.071)	(0.110)	(3.606)	(5.097)	(0.067)	(0.108)	(2.349)	(2.866)
Post95× Tr	−0.221*	−0.171*	−10.449**	−7.937**	0.016	−0.090	−0.335	−4.068†
	(0.091)	(0.073)	(3.780)	(2.991)	(0.074)	(0.070)	(2.569)	(2.468)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Obs.	3962	3962	3962	3962	3796	3796	3796	3796
Obs. in Tr	122	122	122	122	143	143	143	143

Note: The year 1995 is omitted in all models. Values denote estimated coefficients. Standard errors are clustered on household level. Controls include a dummy for age≥60, age^2, age, migration background, working experience in years, years of education, self reported health status, marital status, household size, community size and the amount of help needed by household-member.
Significance levels: † p <0.10, ∗ p <0.05, ∗∗ p <0.01
Source: SOEP v30 v30, own calculation.

include the majority of control variables used in the OLS estimation and are listed in the table descriptions. It also includes the outcome variable that measures employment decision. By doing so we create a comparison group that is very similar in all observed characteristics before treatment – including the labor supply decision. We balance the panel using only individuals in the post treatment period who also belong to the matched pre-treatment sample. Note that the number of observations is considerably reduced compared to the original model. Because we also lose some observations in the treatment group due to balancing the panel the matching comes at the cost of losing some of the available information. Results can be found in Table 10. Estimated effects for men are smaller compared to the original model but stay significant in specifications with and without control variables. Point estimates for women remain insignificant.

Table 10: Regressions on matched sample

	Male-Employment (1)	Male-Employment (7)	Male-Hours worked (1)	Male-Hours worked (7)	Female-Employment (1)	Female-Employment (7)	Female-Hours worked (1)	Female-Hours worked (7)
Post95	−0.084**	−0.025	−3.872**	−1.355	−0.033	−0.017	−1.381	−0.899
	(0.027)	(0.025)	(1.187)	(1.164)	(0.026)	(0.025)	(0.891)	(0.851)
Tr	−0.025	−0.011	0.225	0.978	−0.003	0.075	0.736	2.174
	(0.064)	(0.110)	(3.210)	(5.126)	(0.063)	(0.104)	(2.219)	(3.101)
Post95× Tr	−0.172†	−0.159*	−7.064†	−6.261†	−0.083	−0.082	−3.102	−3.138
	(0.096)	(0.080)	(4.077)	(3.510)	(0.075)	(0.065)	(2.617)	(2.185)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Obs.	1171	1171	1171	1171	1150	1150	1150	1150
Obs. in Tr	134	134	134	134	132	132	132	132

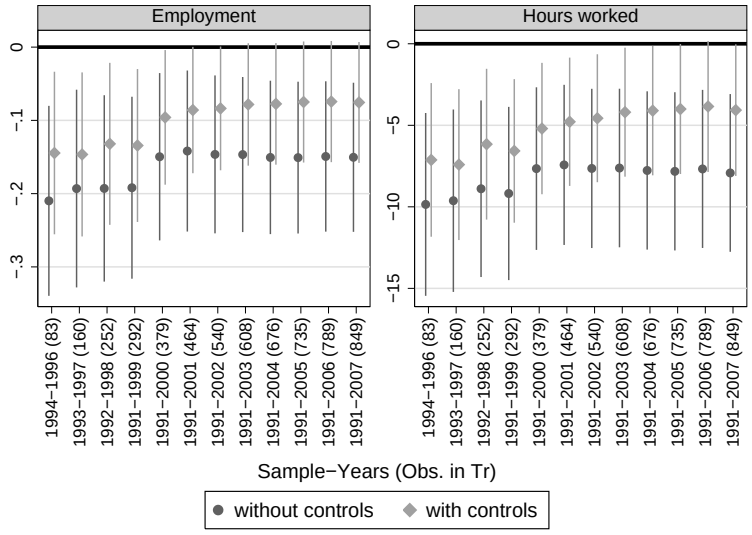
Note: The year 1995 is omitted in all models. Values denote estimated coefficients. Standard errors are clustered on household level. The matched sample is generated by five-to-one nearest neighbour matching based on propensity scores. The propensity score is the probability of belonging to the treatment group and is estimated by a probit model that includes the following covariates: Employment decision, a dummy for age≥60, age^2, age, migration background, working experience in years, years of education, self reported health status, marital status, household size and community size. Matching is performed in the pre-treatment period. In the post-treatment period only those individuals are used who can also be observed in the matched sample before treatment. Covariates in the difference-in-differences model are the same as in the probit model except employment which is not included and a measure of help needed by household-member which is added.
Significance levels: † p <0.10, ∗ p <0.05, ∗∗ p <0.01
Source: SOEP v30, own calculation.

In summary, the subsample analyses, which are estimated on smaller samples and try to address potential concerns regarding our identifying assumptions, support our results as they prove to be stable in all specifications.

Time span

We tried various sample specifications by including observations of different time spans before and after treatment. While the time span before treatment could only be increased up to four years, because of the unavailability of covariates, we increased the time span after treatment up to 12 years.

Figure 5: Different Time Spans (male)



Note: Dark-gray dots plot point estimates without control-variables; the light-gray dots represent point estimates controlling for the full vector of covariates. Lines around the dots represent the 90% confidence interval. The year 1995 is omitted in all models. Standard errors are clustered on household level.
Source: SOEP v30, own calculation.

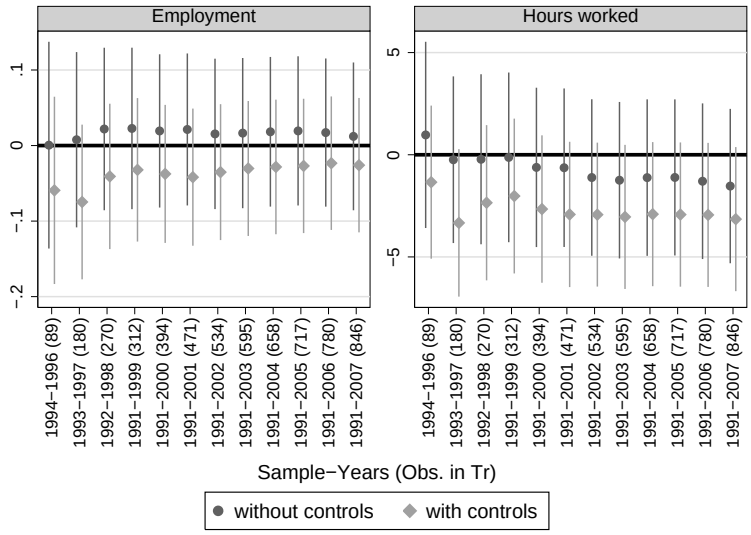
Tables 11 and 12 report results on male employment with and without covariates.

Figure 5 reports results for the male sample.[18] Dark-gray dots plot point estimates without control-variables; the light-gray dots represent point estimates controlling for the full vector of covariates. Lines around the dots represent the 90% confidence interval. The treatment effect on employment is stable with time-spans of one to up to four years before and after treatment. However, the longer the time span is extended in the post treatment period, the smaller is the size of the estimated effect. While all estimates are statistically significant without covariates, regressions including the full set of covariates yield insignificant point estimates in large samples that include more than five observation periods after treatment. Similar results can be found for male working hours. Again, the size of the estimated effect decreases if more time periods are included after treatment. Yet, except the specification with an 11 year post-reform time-span, all point estimates are significant with and without covariates. One factor which might explain the smaller

[18]Detailed estimation tables can be found in Appendix B.

estimated effects with longer post-treatment observation periods is individuals' adjustment to inflation. After the LTCI was introduced in 1995 benefits remained unchanged until they were raised for the first time in 2008. Consequently, the real value of the benefits continuously decreased over the years. For instance, monthly benefits in cash of 205 euro in care level I decreased by approximately 35 euro in purchasing power from 1995 through 2007. Hence, the income effect of the LTCI decreased as well. The longer the post-reform time span, the more carers are included into the sample who face reduced "real" benefits and the average effect of the reform therefore decreases. Another factor is the growth of the formal ambulatory care sector which increased the availability of care services. Moreover, Germany introduced deductions for early retirement starting with cohort 1937 which made it less attractive to leave the labor market.

Figure 6: Different Time Spans (female)



Note: Dark-gray dots plot point estimates without control-variables; the light-gray dots represent point estimates controlling for the full vector of covariates. Lines around the dots represent the 90% confidence interval. The year 1995 is omitted in all models. Standard errors are clustered on household level.
Source: SOEP v30, own calculation.

Results for women are reported in Figure 6. We do not find any significant effects neither in regressions on labor participation nor on working hours.

In summary, the analysis with different time spans before and after treatment supports the estimated treatment effect for men. Additionally, it indicates that effects were larger shortly after the reform came into force, but seem to have diminished in later post reform years.

Placebo regressions

Due to the available data we can only perform one placebo regression in the pre-treatment period. All other placebo regressions are on post-treatment samples. All regressions are performed with assumed pre- and post-treatment periods covering a time-span of two years. We stop the placebo regressions after 2006, because the first fundamental reform of the LTCI (*Pflegeweiterentwicklungsgesetz*) was introduced in 2008 (Kostorz et al., 2010) and, hence, results are less credible for placebo regressions. Furthermore, we do not report regressions that include the LTCI reform in 1995 as it would influence the results. Consequently, the years 1994–1997 are not reported.

Figure 7: Placebo Regressions (male)

Note: Dark-gray dots plot point estimates without control-variables; the light-gray dots represent point estimates controlling for the full vector of covariates. Lines around the dots represent the 90% confidence interval. Each model uses observations of two years before and after the assumed year of treatment. The year reported on the x-axis is always the first year of treatment. Standard errors are clustered on household level.
Source: SOEP v30, own calculation.

Figure 7 summarizes placebo regressions on male employment and working hours. Results for women can be found in Figure 8.[19] We do not find any significant effects in any of the placebo specifications for men. For women we find one statistically significant point estimate if the year 1993 is the assumed year of treatment and if the full set of control variables is used.

[19]Detailed estimation tables can be found in Appendix B.

Figure 8: Placebo Regressions (female)

Note: Dark-gray dots plot point estimates without control-variables; the light-gray dots represent point estimates controlling for the full vector of covariates. Lines around the dots represent the 90% confidence interval. Each model uses observations of two years before and after the assumed year of treatment. The year reported on the x-axis is always the first year of treatment. Standard errors are clustered on household level.

Source: SOEP v30, own calculation.

9. Discussion of results

As discussed above, the direction of the effect of the introduction of the LTCI on labor supply is difficult to anticipate *ex ante*. Because it offers a choice between benefits in cash that increase household's non working income and benefits in kind that provide a substitute for informal care, the insurance provides incentives for both the reduction and the extension of labor supply. In that sense the insurance scheme is flexible and leaves the choice to the household. Note however, that many family carers would provide care even in the absence of cash benefits. For them the LTCI increases household income but might not change behavior (a point, also raised by Campbell et al., 2010).

The insurance does not cover all care needs; a certain amount of additional informal care and/or co-payment is always needed. We cannot observe the actual choice of benefits by the household but we focus only on multi-person households for which it is reasonable to assume that most of them choose benefits in cash – in particular after the introduction of the LTCI. Surveys show that people prefer care by family members such as their spouses or children over formal care services (Schupp and Künemund, 2004). Studies show that marital status is a strong predictor of having a family carer. Himes et al. (2001, 2000), e.g., find that in Germany, being married is associated with less reliance on formal services

and a greater likelihood of receiving care from family members.

Our results suggest that male and female co-residential carers reacted differently to the introduction of the LTCI. The estimates show a negative effect on labor supply for male but not for female carers. It is important to note that we do not analyze the effect of LTC on labor supply but the effect of the LTCI. Female carers already had a very low employment rate before the LTCI came into effect, and average female employment rates were also low. When the LTCI was introduced there was not much scope for them to react. The situation was different for men: the employment rate of male carers was not so different from average employment rates. As we described above the cash benefit – at the time of the introduction of the LTCI – was relatively high (see Table 1) and replaced up to 33% of average gross earnings. Thus men had strong additional incentives to reduce labor supply. It seems that one important channel – at least for men aged 60 and older – was retirement. Unfortunately we do not have data on provided care and it would be very interesting to see if men also extended their care provision in response to the reform.

10. Conclusion

The introduction of the LTCI in 1995 in Germany had a large impact on informal caregivers. It replaced the former means-tested welfare system by an insurance that provides cash transfers or benefits in kind. In this paper we analyze the effect of this reform on the labor supply of co-residential caregivers.

The reform can be interpreted as a quasi-experiment that affected households with individuals in need of LTC. We use a DiD approach to compare labor supply before and after treatment had occurred. We find that while the insurance did not have a significant effect on the labor supply of women, a negative effect can be found on the labor supply of men. This includes employment rates as well as working hours. The effect turns out to be robust in various specifications. The point estimate seems to be rather large but very imprecisely estimated due to the small sample size and our estimation approach.

One of the goals of the LTCI was to improve the availability of home based care and allow family members to care for their relatives (BMG, 2007). In any case LTC households have more resources available to organize care at home. Given data limitations, we can only provide indirect evidence for the effect of the LTCI on the provision of informal care. At least for men, our results suggest that this goal seems to be met, as the insurance has large effect on their labor supply. We do not however have data on care provision so it remains an assumption that men increase time devoted to care when they reduce labor supply. The results reveal a trade off for policy makers that could be important for future reforms. An increasing number of male carers in the future could raise the question if they are able to combine care obligations and market work. And the same might be true for women as they continue to increases their labor market participation. In order to better understand future challenges and to get a more comprehensive view research should also consider extra-residential care settings because they represent a large share of informal

carers.

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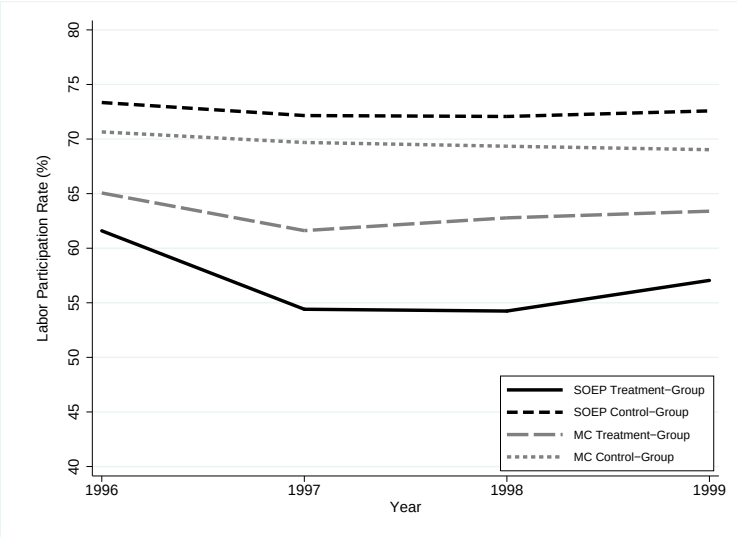
A. Comparison of SOEP and Micro-Census Data

Because our analysis relies on a small number of observations in the treatment group, in this section we use census data to compare outcomes of the post treatment period. With a sample size of about 0.7% of all German households, the microcensus is the largest survey available covering Germany.[20] In 1995 it included about 512,000 individuals in almost 224,000 households. Unfortunately, the need of LTC was not asked before LTCI was introduced and, thus, the dataset cannot be used for the estimation. Instead, we use four post treatment periods to compare the employment rates reported in the SOEP with rates calculated on the basis of census-data. Thereby, group composition slightly differs. Because the census-data is not longitudinal it is not feasible to include pensioners who participated on the labor marked a year prior to the observation period. Consequently, unlike in the specification used in our model, all pensioners are excluded from the sample. Apart from pensioners, the groups are constructed as before. All observed individuals are aged between 45 and 65, able to participate on the labor market, and live in west Germany. Individuals in the treatment group have a household member in need of long-term care. In order to account for overrepresented groups in the SOEP sample, unlike in the graphical representation above, we use individual level weights for SOEP.

Figure 9 plots male employment rates. SOEP data show higher employment rates but the pattern is quite similar. In particular we can observe a decline in employment rates of the treatment group after 1995. Figure 10 plots female labor participation rates. Rates in the census data are very similar to the rates in the SOEP-data. Because trends are similar and the deviations are reasonable, we conclude that the SOEP-sample captures differences between treatment and control group quite well. Thus, the comparison supports our main results.

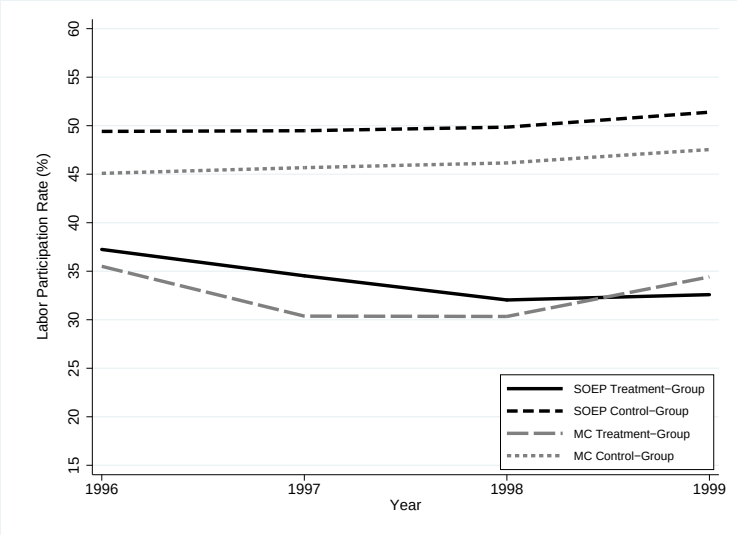
[20] For a detailed description of the data, see Lüttinger and Riede (1997)

Figure 9: Male Employment Rates in SOEP and Micro-Census



Note: Moving averages are used for the representation of the SOEP-data. It is calculated as follows: $y = 0.5(x_t + 0.5x_{t-1} + 0.5x_{t+1})$.
Source: SOEP v30, Micro-Census, own calculation.

Figure 10: Female Labor Participation Rates in SOEP and Micro-Census



Note: Moving averages are used for the representation of the SOEP-data. It is calculated as follows: $y = 0.5(x_t + 0.5x_{t-1} + 0.5x_{t+1})$.
Source: SOEP v30, Micro-Census, own calculation.

B. Tables

Table 11: Regressions on male employment with different time-spans before and after treatment (without covariates)

	1994 - 1996	1993 - 1997	1992 - 1998	1991 - 1999	1991 - 2000	1991 - 2001	1991 - 2002	1991 - 2003	1991 - 2004	1991 - 2005	1991 - 2006	1991 - 2007
Post95	-0.016	−0.033**	-0.044**	−0.046**	-0.046**	−0.042**	-0.031*	−0.023†	-0.017	−0.010	-0.004	0.002
Tr	-0.000	−0.029	-0.016	−0.016	-0.016	−0.016	-0.016	−0.016	-0.016	−0.016	-0.016	−0.016
Post95× Tr	-0.210**	−0.193*	-0.193*	−0.192*	-0.150*	−0.142*	-0.146*	−0.147*	-0.151*	−0.151*	-0.149*	−0.150*
Controls	No	No	No	No	No	No	No	No	No	No	No	No
Obs.	2365	4724	7253	8516	10721	12864	15314	17646	19890	21943	24128	26187
Obs. in Tr	83	160	252	292	379	464	540	608	676	735	789	849

Note: The year 1995 is omitted in all models. Values denote estimated coefficients. Standard errors are clustered on household level.
Significance levels: † p <0.10, ∗ p <0.05, ∗∗ p <0.01
Source: SOEP v30, own calculation.

Table 12: Regressions on male employment with different time-spans before and after treatment (with covariates)

	1994 - 1996	1993 - 1997	1992 - 1998	1991 - 1999	1991 - 2000	1991 - 2001	1991 - 2002	1991 - 2003	1991 - 2004	1991 - 2005	1991 - 2006	1991 - 2007
Post95	-0.011	−0.025*	-0.029**	−0.027**	-0.027**	−0.026**	-0.024**	−0.023*	-0.022*	−0.019*	-0.017†	−0.014
Tr	-0.004	−0.087	-0.015	0.002	0.001	−0.004	-0.003	−0.017	-0.014	−0.042	-0.044	−0.018
Post95× Tr	-0.145*	−0.146*	-0.132*	−0.134*	-0.096†	−0.086	-0.084	−0.078	-0.077	−0.075	-0.074	−0.076
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	2365	4724	7253	8516	10721	12864	15314	17646	19890	21943	24128	26187
Obs. in Tr	83	160	252	292	379	464	540	608	676	735	789	849

Note: The year 1995 is omitted in all models. Values denote estimated coefficients. Standard errors are clustered on household level. Controls include a dummy for age≥60, age^2, age, migration background, working experience in years, years of education, self reported health status, marital status, household size, community size and the amount of help needed by household-member.

Significance levels: † p <0.10, ∗ p <0.05, ∗∗ p <0.01

Source: SOEP v30, own calculation.

Table 13: Regressions on male hours worked with different time-spans before and after treatment (without covariates)

	1994 - 1996	1993 - 1997	1992 - 1998	1991 - 1999	1991 - 2000	1991 - 2001	1991 - 2002	1991 - 2003	1991 - 2004	1991 - 2005	1991 - 2006	1991 - 2007
Post95	-1.067†	−1.432*	-1.853**	−1.883**	-1.602**	−1.390*	-0.687	−0.257	0.048	0.398	0.735	1.096*
Tr	2.183	0.467	0.299	0.299	0.299	0.299	0.299	0.299	0.299	0.299	0.299	0.299
Post95× Tr	-9.851**	−9.625**	-8.890**	−9.181**	-7.655*	−7.428*	-7.645*	−7.619**	-7.769**	−7.823**	-7.675**	−7.921**
Controls	No	No	No	No	No	No	No	No	No	No	No	No
Obs.	2365	4724	7253	8516	10721	12864	15314	17646	19890	21943	24128	26187
Obs. in Tr	83	160	252	292	379	464	540	608	676	735	789	849

Note: The year 1995 is omitted in all models. Values denote estimated coefficients. Standard errors are clustered on household level.

Significance levels: † p <0.10, ∗ p <0.05, ∗∗ p <0.01

Source: SOEP v30, own calculation.

Table 14: Regressions on male hours worked with different time-spans before and after treatment (with covariates)

	1994 - 1996	1993 - 1997	1992 - 1998	1991 - 1999	1991 - 2000	1991 - 2001	1991 - 2002	1991 - 2003	1991 - 2004	1991 - 2005	1991 - 2006	1991 - 2007
Post95	-0.900†	−1.132*	-1.307**	−1.163*	-0.978*	−0.862*	-0.698	−0.656	-0.623	−0.490	-0.353	−0.152
Tr	2.594	−1.469	1.271	1.822	1.140	0.528	0.650	−0.080	-0.242	−1.456	-1.546	−0.735
Post95× Tr	-7.122*	−7.410**	-6.158*	−6.572*	-5.199*	−4.783*	-4.567†	−4.195†	-4.101†	−4.001†	-3.845	−4.069†
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	2365	4724	7253	8516	10721	12864	15314	17646	19890	21943	24128	26187
Obs. in Tr	83	160	252	292	379	464	540	608	676	735	789	849

Note: The year 1995 is omitted in all models. Values denote estimated coefficients. Standard errors are clustered on household level. Controls include a dummy for age$\geq$60, age^2, age, migration background, working experience in years, years of education, self reported health status, marital status, household size, community size and the amount of help needed by household-member.

Significance levels: † p <0.10, ∗ p <0.05, ∗∗ p <0.01

Source: SOEP v30, own calculation.

Table 15: Regressions on female employment with different time-spans before and after treatment (without covariates)

	1994 - 1996	1993 - 1997	1992 - 1998	1991 - 1999	1991 - 2000	1991 - 2001	1991 - 2002	1991 - 2003	1991 - 2004	1991 - 2005	1991 - 2006	1991 - 2007
Post95	0.019	0.011	0.013	0.019	0.025†	0.036**	0.053**	0.064**	0.074**	0.083**	0.092**	0.101**
Tr	-0.099	−0.149*	-0.160**	−0.160**	-0.160**	−0.160**	-0.160**	−0.160**	-0.160**	−0.160**	-0.160**	−0.160**
Post95× Tr	0.001	0.008	0.022	0.023	0.019	0.021	0.015	0.016	0.018	0.019	0.017	0.012
Controls	No	No	No	No	No	No	No	No	No	No	No	No
Obs.	2195	4436	6814	8049	10227	12397	14871	17278	19604	21800	24153	26423
Obs. in Tr	89	180	270	312	394	471	534	595	658	717	780	846

Note: The year 1995 is omitted in all models. Values denote estimated coefficients. Standard errors are clustered on household level.

Significance levels: † p <0.10, ∗ p <0.05, ∗∗ p <0.01

Source: SOEP v30, own calculation.

Table 16: Regressions on female employment with different time-spans before and after treatment (with covariates)

	1994 - 1996	1993 - 1997	1992 - 1998	1991 - 1999	1991 - 2000	1991 - 2001	1991 - 2002	1991 - 2003	1991 - 2004	1991 - 2005	1991 - 2006	1991 - 2007
Post95	0.002	−0.010	-0.013	−0.009	-0.005	0.002	0.008	0.010	0.012	0.014	0.016	0.019†
Tr	0.027	0.013	-0.046	−0.035	-0.059	−0.054	-0.037	−0.057	-0.051	−0.047	-0.061	−0.051
Post95× Tr	-0.059	−0.075	-0.041	−0.032	-0.038	−0.042	-0.035	−0.030	-0.028	−0.027	-0.023	−0.026
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	2195	4436	6814	8049	10227	12397	14871	17278	19604	21800	24153	26423
Obs. in Tr	89	180	270	312	394	471	534	595	658	717	780	846

Note: The year 1995 is omitted in all models. Values denote estimated coefficients. Standard errors are clustered on household level. Controls include a dummy for age≥60, age^2, age, migration background, working experience in years, years of education, self reported health status, marital status, household size, community size and the amount of help needed by household-member.

Significance levels: † p <0.10, ∗ p <0.05, ∗∗ p <0.01

Source: SOEP v30, own calculation.

Table 17: Regressions on female hours worked with different time-spans before and after treatment (without covariates)

	1994 - 1996	1993 - 1997	1992 - 1998	1991 - 1999	1991 - 2000	1991 - 2001	1991 - 2002	1991 - 2003	1991 - 2004	1991 - 2005	1991 - 2006	1991 - 2007
Post95	0.185	0.127	0.139	0.224	0.339	0.638	1.153*	1.481**	1.751**	2.009**	2.293**	2.573**
Tr	-3.225	−3.990†	-3.729†	−3.729†	-3.729†	−3.729†	-3.729†	−3.729†	-3.729†	−3.729†	-3.729†	−3.729†
Post95× Tr	0.972	−0.244	-0.221	−0.128	-0.618	−0.634	-1.114	−1.245	-1.118	−1.110	-1.295	−1.532
Controls	No	No	No	No	No	No	No	No	No	No	No	No
Obs.	2195	4436	6814	8049	10227	12397	14871	17278	19604	21800	24153	26423
Obs. in Tr	89	180	270	312	394	471	534	595	658	717	780	846

Note: The year 1995 is omitted in all models. Values denote estimated coefficients. Standard errors are clustered on household level.

Significance levels: † p <0.10, ∗ p <0.05, ∗∗ p <0.01

Source: SOEP v30, own calculation.

Table 18: Regressions on female hours worked with different time-spans before and after treatment (with covariates)

	1994 - 1996	1993 - 1997	1992 - 1998	1991 - 1999	1991 - 2000	1991 - 2001	1991 - 2002	1991 - 2003	1991 - 2004	1991 - 2005	1991 - 2006	1991 - 2007
Post95	-0.483	−0.723†	-0.904*	−0.899*	-0.840*	−0.685†	-0.569	−0.583	-0.615	−0.642	-0.668†	−0.641
Tr	-1.472	0.444	0.607	1.158	0.833	0.957	1.516	0.898	1.183	1.461	1.433	1.765
Post95× Tr	-1.342	−3.334	-2.349	−2.020	-2.657	−2.920	-2.930	−3.042	-2.906	−2.927	-2.943	−3.150
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	2195	4436	6814	8049	10227	12397	14871	17278	19604	21800	24153	26423
Obs. in Tr	89	180	270	312	394	471	534	595	658	717	780	846

Note: The year 1995 is omitted in all models. Values denote estimated coefficients. Standard errors are clustered on household level. Controls include a dummy for age≥60, age^2, age, migration background, working experience in years, years of education, self reported health status, marital status, household size, community size and the amount of help needed by household-member.

Significance levels: † p <0.10, ∗ p <0.05, ∗∗ p <0.01

Source: SOEP v30, own calculation.

Table 19: Placebo Regressions on Male Employment (without covariates)

	1993	1998	1999	2000	2001	2002	2003	2004	2005	2006
Post	-0.035**	−0.006	0.001	0.010	0.039**	0.052**	0.027**	0.019**	0.026**	0.020**
Tr	-0.036	−0.222**	-0.209**	−0.196**	-0.127*	−0.114*	-0.159**	−0.167**	-0.173**	−0.176**
Post × Tr	-0.002	0.025	0.082	0.082	-0.031	−0.053	-0.015	−0.009	0.026	0.023
Controls	No	No	No	No	No	No	No	No	No	No
Obs.	5020	4816	5884	6877	8061	9130	9169	9079	8814	8541
Obs. in Tr	193	153	203	254	288	316	297	271	249	241

Note: Each model uses observations two years before after assumed treatment. The year reported at the top of the table is always the first treatment year. Values denote estimated coefficients. Standard errors are clustered on household level.

Significance levels: † p <0.10, ∗ p <0.05, ∗∗ p <0.01

Source: SOEP v30, own calculation.

Table 20: Placebo Regressions on Male Employment (with covariates)

	1993	1998	1999	2000	2001	2002	2003	2004	2005	2006
Post	-0.031**	0.007	0.003	−0.003	0.012†	0.018**	0.008	0.013*	0.021**	0.020**
Tr	0.023	−0.162	-0.134	−0.080	-0.036	−0.063	-0.094	−0.176†	-0.188	−0.049
Post × Tr	-0.017	0.020	0.067	0.083	-0.016	−0.027	0.018	0.010	0.002	−0.020
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	5020	4816	5884	6877	8061	9130	9169	9079	8814	8541
Obs. in Tr	193	153	203	254	288	316	297	271	249	241

Note: Each model uses observations two years before after assumed treatment. The year reported at the top of the table is always the first treatment year. Values denote estimated coefficients. Standard errors are clustered on household level. Control-variables include: a dummy for age≥60, age^2, age, migration background, working experience in years, years of education, self reported health status, marital status, household size, community size and the amount of help needed by household-member.

Significance levels: † p <0.10, ∗ p <0.05, ∗∗ p <0.01

Source: SOEP v30, own calculation.

Table 21: Placebo Regressions on Male Working Hours (without covariates)

	1993	1998	1999	2000	2001	2002	2003	2004	2005	2006
Post	-1.494**	−0.122	0.560	1.102*	2.059**	2.741**	1.190**	0.610†	1.436**	1.566**
Tr	-1.784	−9.158**	-9.043**	−8.641**	-6.291**	−5.681**	-7.367**	−7.356**	-7.526**	−7.976**
Post × Tr	1.861	0.517	2.752	2.960	-1.076	−1.675	-0.159	−0.620	1.257	0.508
Controls	No	No	No	No	No	No	No	No	No	No
Obs.	5020	4816	5884	6877	8061	9130	9169	9079	8814	8541
Obs. in Tr	193	153	203	254	288	316	297	271	249	241

Note: Each model uses observations two years before after assumed treatment. The year reported at the top of the table is always the first treatment year. Values denote estimated coefficients. Standard errors are clustered on household level.

Significance levels: † p <0.10, ∗ p <0.05, ∗∗ p <0.01

Source: SOEP v30, own calculation.

Table 22: Placebo Regressions on Male Working Hours (with covariates)

	1993	1998	1999	2000	2001	2002	2003	2004	2005	2006
Post	-1.345**	0.373	0.486	0.372	0.585†	0.765*	0.103	0.294	1.173**	1.531**
Tr	0.454	−4.235	-5.657	−3.891	-2.962	−3.988	-4.925	−8.063*	-8.769	−4.234
Post × Tr	1.175	0.632	2.021	2.933	-0.140	−0.252	1.437	0.430	0.249	−1.616
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	5020	4816	5884	6877	8061	9130	9169	9079	8814	8541
Obs. in Tr	193	153	203	254	288	316	297	271	249	241

Note: Each model uses observations two years before after assumed treatment. The year reported at the top of the table is always the first treatment year. Values denote estimated coefficients. Standard errors are clustered on household level. Control-variables include: a dummy for age≥60, age^2, age, migration background, working experience in years, years of education, self reported health status, marital status, household size, community size and the amount of help needed by household-member.

Significance levels: † p <0.10, ∗ p <0.05, ∗∗ p <0.01

Source: SOEP v30, own calculation.

Table 23: Placebo Regressions on Female Employment (without covariates)

	1993	1998	1999	2000	2001	2002	2003	2004	2005	2006
Post	0.004	0.011	0.026*	0.029*	0.056**	0.061**	0.030**	0.025**	0.034**	0.035**
Tr	-0.189**	−0.141*	-0.158*	−0.133*	-0.142**	−0.139**	-0.140**	−0.131*	-0.115*	−0.115*
Post × Tr	0.045	0.008	0.016	−0.006	0.002	0.008	0.025	0.016	-0.018	−0.055
Controls	No	No	No	No	No	No	No	No	No	No
Obs.	4575	4663	5750	6806	8057	9229	9377	9403	9282	9145
Obs. in Tr	178	182	218	249	264	283	264	246	246	251

Note: Each model uses observations two years before after assumed treatment. The year reported at the top of the table is always the first treatment year. Values denote estimated coefficients. Standard errors are clustered on household level.

Significance levels: † p <0.10, ∗ p <0.05, ∗∗ p <0.01

Source: SOEP v30, own calculation.

Table 24: Placebo Regressions on Female Employment (with covariates)

	1993	1998	1999	2000	2001	2002	2003	2004	2005	2006
Post	-0.011	0.004	0.016†	0.020*	0.029**	0.017*	-0.004	0.002	0.012†	0.015*
Tr	-0.118*	−0.051	-0.131	−0.102	-0.080	−0.140	-0.069	−0.014	-0.129	0.017
Post × Tr	0.121*	−0.022	0.019	−0.026	0.023	0.069	0.033	−0.012	-0.021	−0.041
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	4575	4663	5750	6806	8057	9229	9377	9403	9282	9145
Obs. in Tr	178	182	218	249	264	283	264	246	246	251

Note: Each model uses observations two years before after assumed treatment. The year reported at the top of the table is always the first treatment year. Values denote estimated coefficients. Standard errors are clustered on household level. Control-variables include: a dummy for age≥60, age^2, age, migration background, working experience in years, years of education, self reported health status, marital status, household size, community size and the amount of help needed by household-member.

Significance levels: † p <0.10, ∗ p <0.05, ∗∗ p <0.01

Source: SOEP v30, own calculation.

Table 25: Placebo Regressions on Female Working Hours (without covariates)

	1993	1998	1999	2000	2001	2002	2003	2004	2005	2006
Post	-0.224	0.203	0.381	0.762†	1.803**	1.935**	0.845**	0.607*	1.025**	1.185**
Tr	-5.172*	−4.233*	-4.779*	−3.459	-4.771**	−4.912**	-5.431**	−5.874**	-4.352*	−3.920†
Post × Tr	1.316	0.774	0.009	−1.452	-0.661	−0.962	1.079	1.954	-1.014	−2.936
Controls	No	No	No	No	No	No	No	No	No	No
Obs.	4575	4663	5750	6806	8057	9229	9377	9403	9282	9145
Obs. in Tr	178	182	218	249	264	283	264	246	246	251

Note: Each model uses observations two years before after assumed treatment. The year reported at the top of the table is always the first treatment year. Values denote estimated coefficients. Standard errors are clustered on household level.

Significance levels: † p <0.10, ∗ p <0.05, ∗∗ p <0.01

Source: SOEP v30, own calculation.

Table 26: Placebo Regressions on Female Working Hours (with covariates)

	1993	1998	1999	2000	2001	2002	2003	2004	2005	2006
Post	-0.762*	−0.054	0.091	0.433	0.821**	0.377	-0.352	−0.197	0.160	0.365
Tr	-2.510	−1.615	-1.924	−0.316	-1.144	−3.816	-2.158	−0.508	-1.629	3.411
Post × Tr	3.947*	−0.312	0.007	−2.157	0.169	1.201	0.997	0.109	-1.828	−2.421
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	4575	4663	5750	6806	8057	9229	9377	9403	9282	9145
Obs. in Tr	178	182	218	249	264	283	264	246	246	251

Note: Each model uses observations two years before after assumed treatment. The year reported at the top of the table is always the first treatment year. Values denote estimated coefficients. Standard errors are clustered on household level. Control-variables include: a dummy for age≥60, age^2, age, migration background, working experience in years, years of education, self reported health status, marital status, household size, community size and the amount of help needed by household-member.

Significance levels: † p <0.10, ∗ p <0.05, ∗∗ p <0.01

Source: SOEP v30, own calculation.