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Labour market effects of a parental health shock

Evidence from the Netherlands

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Abstract

We link Dutch administrative data on labour market outcomes, hospitalisations and family relations to evaluate the effect of an exogenous increase in informal care demand on the probability of employment and conditional earnings for the working age population. We estimate a reduced form difference-in-difference model combined with inverse probability weighting and individual fixed effects. In addition, we provide separate results for at-risk care giver subsamples. We find no effect of an unexpected parental health shock on either the probability of employment or conditional earnings for Dutch men and women. This result may potentially be explained by the comprehensive long-term care system in the Netherlands.

1 Introduction

The aging of the population raises questions on how a declining workforce can provide informal care to a growing proportion of the elderly. For the Netherlands, the Central Bureau of Statistics projects that the share of 65+ individuals in the total population will increase from 18% in 2015 to 26% in 2040 (Statistics Netherlands, 2014). Much of the responsibility for providing informal care to the elderly is likely to fall to family members such as spouses and children. From a public finance perspective, informal care may be a relatively inexpensive form of elderly care, as there are no direct expenses. However, providing care can be time-intensive and may reduce the well-being and health of care givers. Furthermore, care responsibilites may lead to a reduction in employment and earnings penalties, which in turn reduces pension and tax contributions. Assessing the effects of informal care on labour market outcomes is thus important to both understand the situation of care givers and to gain insights for long-term care and labour market policy formulation.

The relationship between informal care and labour market outcomes has been studied extensively over the past two decades. Yet, the literature has not reached a consensus on the existence and the magnitude of such an effect, potentially due to differences in sample selection criteria across studies, institutional differences, and identification issues. The later arise from the selection of the non-employed or low-wage individuals into care giving activities and unobserved heterogeneity. In addition, most studies may suffer from non-random non-response due to time constraints of care giving.

Using quarterly Dutch administrative data from 1999-2005, we contribute to this literature by evaluating the effect of an increase in informal care demand on *i)* the probability of employment and *ii)* conditional earnings over 10 quarters. To this end, we link records for working-age individuals to their parents' health information and exploit an unexpected hospitalization of a parent as a plausibly exogenous increase in the demand for informal care. Unanticipated parental health shocks have not been used as instruments in the informal care literature so far, even though they are more likely to be exogenous than, for example self-reported parental health indicators because the latter may be prone to justification bias. As informal care data is not available, we estimate a reduced form model controlling for individual fixed effects in a difference-in-difference framework combined with inverse probability weighting. In a subsample analysis, we check for heterogenous effects among at-risk care-givers based on the residence of parents, number of siblings, alone living parents, employment status and the age of parents. Besides a high incidence of parental health shocks in our sample, our data set has two other major advantages. On one hand, there is no non-response, and on the other hand, the high frequency of outcomes with up to 28 quarters per individual allows us to paint a detailed picture of the consequences of a parental health shock over ten quarters after its occurrence.

We find that in the Netherlands, an unexpected parental health shock does not have an effect on employment probabilities nor conditional earnings for both men and women. Given our large sample size, this result is precisely estimated and not due to power issues. The comprehensive Dutch long-term care system may play a large role in reducing this trade-off especially in the medium and long run. It seems that the Dutch state lives up to the expectations of its citizens, who are of the opinion that elderly care is mainly the responsibility of the state (Mot, 2010). At the societal level, there seems to be no large scale loss in forgone tax or pension income due to an informal care - labour market participation trade off, at least not for informal care induced by a unexpected parental health shock.

The rest of the thesis is organised as follows. Section (2) gives an overview of the literature about informal care and labour market outcomes, followed by a description of the Dutch institutional background (Section 3). In Section (4), I use a theoretical model to illustrate potential mechanisms of the effect of informal care provision on labour market outcomes. Section (5) and (6) describe the data and methods. The results are presented in Section (7) and discussed in (8).

2 Literature

The impact of informal care giving on labour market outcomes has received considerable attention in the last two decades, but there is no consensus on the existence and magnitude of the effect. The main challenge of these studies is to address endogeneity arising from potential omitted variable bias and reverse causality. Omitted variable bias can arise if a family-oriented individual is more likely to provide care to a sick family member and at the same time is less likely to be active in the labour market. There is a broad consensus that for this reason, controlling for individual fixed effects is important (Ciani, 2012; Heitmueller, 2007; Jacobs et al., 2016; Leigh, 2010; Moscarola, 2010). On the other hand, sorting of the non-employed or the low-paid into care giving activities may lead to reverse causality. The majority of studies addresses the causality with instrumental variables, where mostly parental health indicators are used to instrument care demand (Ciani, 2012; Crespo and Mira, 2014; Heger, 2014; Meng, 2013; Van Houtven et al., 2013). Some studies provide evidence that reverse causality is indeed a problem (Carmichael et al., 2010; Heitmueller, 2007; Michaud et al., 2010), whereas others find that care giving can be treated as exogenous once unobserved heterogeneity is controlled for with individual fixed effects or correlated random effects (Casado-Marín et al., 2011; Ciani, 2012; Jacobs et al., 2016; Meng, 2013; Van Houtven et al., 2013).

Using instrumental variables combined with individual fixed effects, the effect of informal care giving on employment ranges from zero (e.g. Meng, 2013; Van Houtven et al., 2013) to negative but relatively small (e.g. Bolin et al., 2008; Ciani, 2012). An alternative to instrumental variables are dynamic models (e.g. Casado-Marín et al., 2011; Schmitz and Westphal, 2016; Viitanen, 2010). For example, (Moscarola, 2010) finds for the Netherlands that being in employment in $t-1$ decreases the probability of care giving by 2 percentage points, and care giving in $t-1$ reduces the probability of employment by 6 percentage points. Yet, she finds no contemporaneous trade-off between care giving and employment.

Instead of giving up their jobs, informal care givers may also devote less time, energy and motivation to their jobs than non-care givers, possibly suppressing earnings. Some studies find lower earnings for care givers (Heitmueller and Inglis, 2007; Van Houtven et al., 2013), whereas others find no effect (Bolin et al., 2008). Using the German Socio-Economic Panel, Schmitz and Westphal (2016) for example find no effect on contemporaneous earnings but a negative effect in the long run.[1]

How can the variation in results of these studies be reconciled? First, the studies reviewed cover many countries and time periods with a large variety of underlying institutions, as for example norms about care giving responsibilities and the amount of publicly provided elderly care. The care burden faced by adult children may vary along these dimensions. Pan-European studies underline the importance of institutions and present evidence for a north-south/generous-less generous welfare state gradient (Bolin et al., 2008; Ciani, 2012; Heger, 2014).

Second, the aforementioned identification issues have been addressed using various empirical methods, such as a combination of individual fixed effects, (correlated) random effects, instrumental variables, or dynamic (simultaneous equation) models. Differences between methods seem likely to contribute to the dispersion in results, on the one hand because instrumental variable studies estimate local treatment effects, and sometimes are borderline weak (e.g. Bolin et al., 2008). The information available for instruments also differ considerably between surveys (parental health, death, distance to children, etc). On the other hand, a validity assessment is more difficult for dynamic simultaneous equation model than for instrumental variables, as it relies on assumptions on initial conditions that are not testable. Moreover, Viitanen (2010) and Moscarola (2010) use random effects, assuming that the individual specific error terms are uncorrelated with the other

[1]For a more extensive literature review see Bauer and Sousa-Poza (2015); Lilly et al. (2007).

explanatory variables, which may not be satisfied in practice.

Third, data issues are another important factor contributing to the divergence in results. The studies mentioned so far rely on survey data. Different surveys include different measures with respect to living arrangements (extra-residential versus residential care), relationship to the care receiver (spousal versus parental care), definition of informal care (any care giving, intensity of care giving, hours of care provided in a time interval, primary versus secondary care giver, etc). Hence, the care situation and care giver subgroup differ from study to study (Bauer and Sousa-Poza, 2015). Another limitation of the use of survey data is non-random non-response. As care giving can be a time and energy intensive task, care givers are less likely to respond to a survey than the average population. This problem is largely ignored by most studies, except for Casado-Marín et al. (2011) and Viitanen (2007), who find evidence for non-random attrition among care givers in Spain and Europe respectively. Viitanen (2007) tests this for the Netherlands, finding that attrition is random for the Dutch European Community Household Panel sample (ECHP), implying that attrition is not problematic for the Dutch part of the ECHP. Of course, this is not informative about non-random non-response in the first wave, as care givers may decline to participate in the survey already for the first wave. Assuming that the care givers who have the strongest employment response are less likely to fill out a survey due to time constraints, then the effect of care giving on employment is biased towards zero. Therefore, one would expect to find larger effects in absolute terms when using administrative data if care giving was observed.

Overall, this means that it is difficult to compare the findings of this literature. Working with administrative data covering the entire population can help overcoming some of these limitations, as it enables us to look at the whole population as well as specific subsamples while avoiding non-random non-response. Two studies using Norwegian register data have examined links between labour market outcomes and informal care provision. Fevang et al. (2012) find that employment and earnings of adult children declines prior to the death of a lone parent, especially for daughters. Not disposing of any parental health information, they have to assume that informal care giving is concentrated at the end of a parent's life. Furthermore, by limiting their sample to individuals who lost a parent in the sample period, they do not have a comparison group. The second study uses a change in the supply of substitutes in informal care as an exogenous variation (Løken et al., 2017). They find that a formal home care expansion had a positive effect on employment but not earnings of female only-children.

This study contributes to the existing literature by extending on Fevang et al. (2012). In contrast to their identification strategy, we are able to identify a point in time where informal care demand effectively increases by focusing on unexpected parental hospitalizations while keeping the aforementioned advantages of administrative data. Identification problems are tackled by using unexpected parental hospitalisations, which is a more disaggregated and precise instrument than previously used in the literature. This strategy has a major advantage: it does not suffer from any reporting bias compared to the common 5-point scale self-reported parental health indicator that is used in other studies and has not been applied in this context. Moreover, our estimation strategy allows us to introduce a control group that is the same as the treatment group before the treatment, comparing potential care giver with individuals not experiencing a parental health shock. Furthermore, our quarterly panel allows following individuals before and after the parental health shock and permits inference over 18 quarters. This enables us to check the underlying assumptions on the one hand and to follow the outcomes in detail over a long period of time on the other hand.

3 Institutional Background

Long-term care consists of medical and non-medical services provided to a chronically ill or disabled person. It is mainly delivered in nursing homes and at home by a professional (formal home care) or informally by relatives, friends, or neighbours (Norton, 2000).

Informal care is a wide-spread phenomenon in the Netherlands. Based on a representative survey, it is estimated that around 20% of the Dutch population were involved in either intensive and/or prolonged care giving in 2008. Around 60% of care givers are female, and about half of them are aged 45-65. In 40 % of the cases, the care receiver was a parent or a parent in-law in 2008. Women are more likely to provide parental care, whereas men commonly provide spousal care (Oudijk et al., 2010). Focusing on parental care, we would therefore expect to find a larger effect for women than for men in this study.

Descriptive evidence about care giving and employment shows that there has been an increasing number of people who are combining employment and care work in the Netherlands. For men, employment does not seem to be an obstacle to providing care, whereas for women there seems to be a concurrent relationship between paid work and care giving (de Boer and de Klerk, 2013). If the combination of care and paid work is problematic, Dutch care givers are entitled to care leave. Yet, in 2009 this was not very popular: only 1% of employees took care leave in order to care for a partner, child or parent (de Boer and de Klerk, 2013).

The formal Dutch long-term care system is relatively comprehensive and has a longstanding tradition with the introduction of a public long-term care insurance (ABWZ[2]) in 1968. In the period of study (1999-2005), this insurance covers all long-term care in institutions and at home, where care can consist of domestic help, social assistance, personal care, and nursing care (De Meijer et al., 2015; Mot, 2010). An independent assessment agency grants access to long-term care usually in six weeks time depending on the physical and mental health status of the applicant, living conditions, social environment, and informal care availability in the household (Bakx et al., 2016; CIZ, 2016). Other household members are expected to provide a 'reasonable' amount of informal care (Mot, 2010). Instead of using the publicly provided long-term care in kind, users can opt for a personal budget instead paying out 75% of the public care costs in cash to either purchase their care on the market or pay their informal care giver (Mot, 2010).

During the period of study, some changes were introduced in the AWBZ. In the 1990s, there were relatively high waiting times, and in the beginning of the 2000s there was a policy effort to reduce these by increasing the budget. In 2003, reforms of the AWBZ system sought to make the system more responsive by detaching services from specific providers. While leading to difficulties in controlling the overall long-term care budget, these changes improved the position of users. To counteract the budget developments, higher co-payments and regional budgets were introduced in 2004 and 2005 (Mot, 2010). I do not expect these policy changes to interfere with my results, as they affected everyone

[2] *Algemene Wet Bijzondere Ziektekosten*

in the same way and will thus be differenced out between treatment and control group in the difference-in-difference framework.

4 Theoretical model informal care provision and labour market outcomes

The relationship between informal care provision and labour market decisions can be modeled with a time allocation framework, where agents choose the amount of time they want to dedicate to each activity while maximising their utility. Most static models in the literature include two main mechanisms (Ciani, 2012; Crespo and Mira, 2014; Stabile et al., 2006; Wolf and Soldo, 1994). First, time scarcity leads to a trade-off between working and providing care. Working increases money available for consumption, which increases utility. Providing care on the other hand increases utility because agents care about their parents, but leaves less time for working which decreases consumption and therefore utility.

The second mechanism is about the substitutability of informal care with formal care. Instead of providing care themselves, children can also work and buy care on the market. For the Netherlands, however, formal home care is mostly provided through the public long term care insurance and does not have to be purchased on the market. For this reasons, I focus on the time scarcity trade-off only and closely follow Johnson and Lo Sasso (2000).

In the simple time allocation model based on Johnson and Lo Sasso (2000), an adult child maximises its utility subject to a time and budget constraint. The child's utility depends positively on consumption c, hours of leisure h_l, and parental health $p(.)$, which can be increased by hours of care provided by the child h_c and others h_o (exogenous). Hours spent caring and working (h_w and h_c) reduce children's utility, as these activities leave less room for leisure. Empirically, the assumption how care giving and parental health enter the utility function are confirmed by for example Bobinac et al. (2010) or Byrne et al. (2009). They find that good parental health status is positively associated with reported life satisfaction for care givers, whereas care tasks has a negative impact. Utility $U(.)$ is separable in consumption $u(.)$, leisure $v(.)$, and a parental well-being component $x(.)$. The functions $u(.)$, $v(.)$ and $x(.)$ are all concave with strictly positive first derivatives and negative second derivatives. The child's consumption is constrained by income from wages. All income is consumed, as there is no option for savings in a one-period model. Time available is normalised to 1 and divided into working, care giving and leisure time.

$$
\begin{aligned}
\max_{c,h_w,h_c,h_l} \quad & U(c,h_w,h_c,h_l;h_o) = u(c) + v(1-h_w-h_c) + x(p(h_c,h_o)) \\
\text{s.t.} \quad & h_w + h_c + h_l = 1 \\
& wh_w = c
\end{aligned}
$$

$$
w = \frac{\partial v(h_l)}{\partial h_w} \bigg/ \frac{\partial U(.)}{\partial c} \tag{1}
$$

$$\frac{\partial v(h_l)}{\partial h_c} = -\frac{\partial x(p(h_c, h_o))}{\partial p(h_c, h_o)} \frac{\partial p(h_c, h_o)}{\partial h_c} \tag{2}$$

Solving the first order conditions gives rise to two equilibrium relations (Equation 1 and 2). On the one hand, wage is equal to the marginal rate of substitution between leisure and consumption. This is in line with the standard results of the labour supply model. Additionally to the standard labour supply model, Equation 2 adds a care giving component to optimal time allocation. In equilibrium, the marginal utility of leisure equals the marginal utility of care giving. The later consists of two parts: the marginal utility of parental well-being is multiplied by the marginal productivity of care giving on parental health.[3] This implies that depending on the ability of the son to increase parental health by caring for his father, the more likely he is to provide care and accord less importance to leisure after a parental health shock. Due to diminishing returns to scale, also working hours will be reduced if care work is taken up. Therefore, the model predicts lower employment for care givers. If all care is provided by others (h_o large), the productivity of care from the child is close to zero. Therefore, the utility gain from providing care is so low that the child does not provide any care. In that case, the model reduces to a standard labour supply model where time allocation decision of the child depends on the marginal rate of substitution between leisure and consumption only. Wage rate is exogenous in the model, so it is not affected by a parental health shock. Yet, total earnings are reduced when providing care because care givers work fewer hours. Hence, based on this model, I expect to find a negative effect of a parental health shock on both employment and total earnings if care from other sources is not too large.

5 Data

The sample consists of the Dutch resident population 35-65 in the municipality registers between 1999 and 2005, with at least one parent is still alive.[4] The parents are allowed to have one health shock only. In case of two or more parental health shocks, the observation is dropped when the second health shock occurs, as the effect of a second health shock close to the first may be different than the first one. I transform the data into quarterly observations per person. 1999 is the first year the employment data is available, whereas the hospital data covers 1995-2005. The lower age limit of the sample avoids memory problems.

I link administrative basic personal information with data on employment, earnings, hospitalisations, residence coordinates, and the cause of death registry with family relations (children-parents and partners) from Statistics Netherlands. Table (4) in the Appendix lists details of the data sets used. Data on formal long-term care use is not available for this research project. I do not consider the lack of this data to be an issue because formal home care entitlement is the same for everyone conditional on health status, living conditions, social environment and informal care availability.

Apart from the main sample, I use six subsamples for which either informal care giving is more prevalent and/or I expect a large effect. First, I look at alone living parents, whose

[3]In Equation (2), the minus on the right hand side cancels because $\frac{\partial v(1-h_w-h_c)}{\partial h_c} = (-1) * v'(.)$.

[4]There are some implausibly old parents (e.g. 132 years old). I code all parents dead if the are 105 or older. None of these parents have experienced a health shock in the sample period.

children face a higher care demand as there is no other parent who may provide care. Second, I condition on being employed one year before the health shock. For this sample, a larger effect may be expected on the one hand, as they are all employed before the shock. On the other hand, being in a stable job may also prevent them from providing care, which would result in a weaker effect than for the overall population. Third, I restrict the sample to parents aged 80 and older, whose children are also expected to face increased care demand compared to individuals with younger parents. Fourth, I limit the sample to only children, so as to exclude situations where care may be provided by siblings.[5] Fifth, I use a subsample with children living up to 5km away from their father and mother in 'as the crow flies' distance. Last, I combine the above to only-children with alone but close-living parents, which is the subgroup for which I expect the largest effect. The downside of the subsamples using the address data is that there is a considerable amount of missing values in the address data. I therefore provide intermediary results in the appendix where the baseline results are recalculated without the missing address individuals to ensure comparability of results.

I analyse the effect of a parental health shock on two labour market outcomes: the probability of employment and earnings conditional on employment. Employment is defined as having a job based on an employment contract between a firm and a person. Earnings are defined as the sum of earnings before taxes over all jobs in a given quarter. As the original data contains yearly earnings data and the beginning and the end date of a job, I impute the potential quarterly earnings for the individuals that are only employed during a part of a given quarter. I use a logarithmic transformation of conditional earnings. Lechner (2011) shows that if the outcome variable is log-normally distributed (and thus the log of the outcome follows a normal distribution), the common trend assumption is violated when using levels instead of logs in a difference-in-difference setting. Inspection of the distribution of the log of earnings shows that they are approximately normally distributed (see Table 6 in the Appendix) and hence a log transformation is appropriate.

The main independent variable of interest is a parental hospitalisation, which is assumed to represent an exogenous variation in informal care demand. As I do not observe informal care, I estimate a reduced form model instead, where the instrument (the parental health shock) is directly regressed on the outcome. A solid instrument needs to be strong and valid. Validity could be threatened if the parental health shock is anticipated and the labour market behaviour adjusted beforehand. Therefore, I limit the health shock to ICD-9CM[6] diagnoses treated in the hospital that are not forseeable based on expert opinion (see also Garcìa-Gòmez et al., 2015; García-Goméz et al., 2017). These diagnoses include, for example, different forms of cancer, fractures, stroke and heart failure, infectious diseases, or injuries.

Even though this health shock definition does not capture all informal care demand, an unexpected hospitalisation is still likely to induce care demand and should therefore be a strong instrument. Usually, the strength of an instrument is tested in the first stage regression, but this is not possible here. Instead, I discuss studies qualifying health determinants of informal care demand on the one hand, and determinants of formal home care use on the other hand in Appendix (9.2), which show that indeed, part of informal and formal long-term care demand are induced by a health shock as defined above.

[5] Half-siblings are not considered.

[6] International Statistical Classification of Diseases and Related Health Problems

As control variables, I use age, living with a partner, and the number of children below 13. In the earnings equation, I add the number of jobs per quarter, and the tenure in the main[7] job to proxy experience. These covariates are used because they are likely to capture relevant time-variant variation in employment and/or earnings and may be correlated with care-giving. In order to avoid omitted variable bias, it would be desirable to additionally control for education, personality traits, and formal home care eligibility. Personality traits and education are likely to be time invariant and thus captured by the individual fixed effects.[8] Formal home care access depends on living conditions, informal care availability in the household, social environment, and the physical and mental health status of the applicant. Informal care availability in the household and living conditions are dealt with in the subsamples analysis on alone-living parents. The social environment of a person is unlikely to change substantially over 18 quarters, so it may be picked up by the fixed effects.[9] The physical and mental health status is partly included in the model through the parental health shock, but chronic diseases and slowly deteriorating parental health may lead to an omitted variable bias. In a difference-in-difference approach, this problem is linked to the common trends assumption and is discussed in Section (6.2). All the analyses are done separately by gender, as women are likely to react differently to a parental health shock than men due to gender norms and differences in labour market attachment.

6 Methodology

In order to evaluate the effect of a parental health shock on the probability of employment and conditional earning, I rely on a difference-in-difference model over multiple treatment periods combined with inverse probability weighting. I allow for the health shock to persist up to 10 quarters in my model specification.[10] Many studies thus far have concentrated on the immediate effect of informal care provision on labour market outcomes. However, taking a more long-run perspective has shown that cumulative effects over time are important (e.g. Casado-Marín et al., 2011; Fevang et al., 2012; Michaud et al., 2010; Moscarola, 2010; Schmitz and Westphal, 2016; Skira, 2015; Viitanen, 2010). In order to check the common trend assumption, I check for differences in treatment and control group up to 8 quarters (7 excluding the base case) before the shock.

6.1 Selection of the treatment and control group

The treatment group consists of individuals experiencing a parental health shock between 2001q1 and 2002q2. This selection allows to test 8 quarters of pre-treatment trends for the first cohort of treated. Observations with a health shock between 1995q1 and 2001q2

[7]The main job is defined as the job with the highest earnings if a person has more than one jobs.

[8]Given that our sample starts at age 35 and education is typically completed before, it is likely to be time-invariant for this age group.

[9]Of course, one can think of situations where this argument does not apply, such as neighbours moving away. However, these cases are not more likely to occur for the treatment group than the control group, so that the difference-in-difference approach will average out this potential source of bias.

[10]I choose to limit the health shock effect to 10 quarters as a compromise between *i)* being able to allow for leads, and *ii)* to include people with a health shock at the end of the sample period in the control group.

are excluded from the sample, as the effect of a second shock may be different from the first and therefore not be averaged out between treatment and control group.

Table 1: Treatment (T) and control (C) group selection

Timing shock	Allocation	Reason
1999q1-2000q4	dropped	8 leads not possible
2001q1-2002q3	T	8 leads possible
2002q4-2004q4	dropped	Would interfere with the treatment effect as it is less than 10 quarters away from 2002q3
2005q1-2005q4	C	Health shock is more than 10 quarters after 2002q3 and thus does not interfere with the treatment effect (assuming that the leads are not significant)
No shock 1999-2005	C	

In the case of a second parental health shock, individuals have been dropped from the sample from the second shock onwards.

The treatment group is separated in six cohorts according to the quarter of the shock. For each cohort, a corresponding control group is selected, consisting of people who did not experience a parental health shock between 1995q1 and 2004q4. In contrast, the control group is allowed to experience a parental health shock from 2005q1 onwards, to make it more comparable to the treatment group. The comparability is increased because the control group then consists of people with approximately the same probability of a health shock as the control group, only that the shock happens 4 years later.[11] Table (1) shows an overview of how individuals are selected into treatment and control group. This set up implies that every individual of the control group is duplicated six times so that there is one observation that can be attached to every treatment cohort (Jeon and Pohl, 2017). Individuals can exit the sample at different points in time due to a second parental health shock, death of both parents, or reaching retirement age, and therefore each cohort of treatment and control group is an unbalanced panel.

6.2 Difference in difference

I use a difference-in-difference model to follow every cohort of treated and control over time and average this effect over the six cohorts (Hijzen et al., 2010; Jeon and Pohl, 2017). I define an indicator of how many quarters an individuals is away from a health shock q_{it}^k with $k \in [-8, 10]$. For the control group, this variable is coded according to the corresponding treated individuals in the same cohort. The treatment group is designated with D_i.

$$y_{it} = \alpha_i + \alpha_t + \sum_{k=-7}^{10} \gamma^k q_{it}^k + \sum_{k=-7}^{10} \beta^k D_i q_{it}^k + \delta x_{it} + \varepsilon_{it} \tag{3}$$

Equation (3) is estimated using the within transformation plus inverse probability weighted OLS for the probability of employment and log conditional earnings.[12] Section (6.3)

[11]See for example Fadlon and Nielsen (2015).

[12]Earnings are not corrected for selection bias using the Inverse Mills ratio.

provides details about the inverse probability weights calculation. The first sum of Equation (3) captures the common time trends of treatment and control before and after the health shock. The second sum is the difference in difference term, with coefficients of interest $\beta^0, ...\beta^{10}$. In order to test the common trend assumption, I also add $\beta^{-7}, ...\beta^{-1}$. The reference period is eight quarters before the shock ($q = -8$). In addition, quarterly time fixed effects α_t, individual fixed effects α_i, time-varying controls x_{it} and the error term ε_{it} are included in the model.

The identifying assumption of a difference-in-difference approach is the common trend assumption, implying that the treatment and control group would have had the same trend had the treatment not happened. A violation of the common trend assumption could occur if suffering from a chronic illness in t is correlated with experiencing a health shock in the future $t + m$. Therefore, if the health shock is a symptom for overall health degradation, the underlying parental health distributions are not the same for the treatment and the control group. This could imply that the treatment and the control group face different evolutions of informal care demands over time.[13] Testing for the evolution of parental health before the health shock is not possible, but the inspection of raw employment and earnings trends by group can give a first hint if this is indeed a problem. More formally, potential pre-treatment differences in trends can be detected through t-tests for significance of $\beta^{-7}, ...\beta^{-1}$. If pretreatment indicators are not significant, underlying differences in parental health between the groups are unlikely, and hence the parental health shock is indeed unexpected and exogenous.

6.3 Inverse probability weighting (IPW)

In order to make treatment and control group more balanced in observed characteristics, I combine the above difference-in-difference model with a matching approach to increase balance in the covariates between treatment and control group. For this study, Coarsened Exact Matching (CEM)[14] would be the preferred matching method (Iacus et al., 2012). Due to memory intensive weight calculation and time constraints, I use inverse probability weights[15] based on estimated propensity scores (PS) for now (Hirano et al., 2003), which will be replaced by CEM weights later. The main advantage of CEM compared to propensity scores is that there is no need for ex-post balance checking as the maximal acceptable imbalance is decided on beforehand. Moreover, the validity of CEM matching does not rely on a correct functional form specification of the propensity score.

[13] Differences in levels are not important as they are differenced out.

[14] CEM is an exact matching algorithm that splits the data into strata according to all possible combinations of pre-imposed bins. For every stratum l, weights w_l are calculated balancing the empirical distribution of the matching variables, which can be used in the further analysis.

[15] IPW is preferable to matching based on the distance between propensity scores that drops observations in addition to common support pruning. This would be the case with for example caliper matching, where only control observations are kept for which the absolute distance of the estimated propensity score of treatment unit i and control unit j is smaller than a certain caliper. The reason for this is that propensity scores approximate a randomised experiment, and pruning based on random treatment allocation effectively results in random deletion of data points, which in turn leads to increased imbalance (King and Nielsen, 2016).

Table 2: Women summary statistics treatment and control group raw and inverse probability weighted

	(1)	(2)	(3)	(4)	(5)	(6)
		Employment				**Earning**
	Raw			**IPW**	**Raw**	**IPW**
Variable	**Mean Control**	**Mean Treatment**	**Diff**	**Diff**	**Diff**	**Diff**
Employed	0.607	0.638	-0.030***	-0.001	0.000	-0.000***
Earnings	5799.335	5522.185	277.150***	-350.948***	277.150***	-349.771***
Age	45.096	43.740	1.356***	-0.081***	0.777***	-0.076**
Age mother	73.866	72.373	1.493***	-0.059	0.874***	-0.048
Age father	76.513	74.960	1.553***	-0.223***	0.966***	-0.208***
Partnered	0.757	0.777	-0.020***	-0.001	-0.011***	0.000
Dutch	0.896	0.919	-0.023***	-0.001	-0.017***	-0.001
1st generation migrant	0.040	0.024	0.016***	0.000	0.011***	0.000
2nd generation migrant	0.064	0.057	0.007***	0.001	0.006***	0.002
No. of siblings	1.251	1.313	-0.062***	-0.011*	-0.047***	-0.008
No. of kids <13	0.477	0.500	-0.023***	0.001	-0.006	0.002
Father partnered	0.474	0.715	-0.241***	-0.009***	-0.221***	-0.009***
Mother partnered	0.463	0.698	-0.234***	-0.008***	-0.215***	-0.008**
Distance to mother	25.574	25.487	0.087	0.098	0.032	0.016
Distance to father	27.311	26.691	0.620***	0.150	0.673**	0.020
No. of jobs	1.076	1.077	-0.001		-0.001	-0.001
Tenure (main job)	33.781	32.639	1.142***		1.142***	-0.130
Distance to closest parent				-0.147		-0.162
One parent dead				0.000		0.000
Age oldest parent				-0.215***		-0.199***
Number of obs. treatment group			874,700	866,573	556,905	527,368
Number of obs. control group			31,535,422	25,782,112	19,101,857	15,618,484

The significance of raw difference in means is tested with a t-test of equality of pre-treatment means with unequal variance in the period before the shock. For the IPW version, significance test from a weighted regression of the treatment indicator on the pre-treatment values of the respective variable is used. The number of jobs and tenure are not tested for the employment IPW version, as these are not included in the propensity score model. The distance to the closest parent, one parent dead, and the age of the oldest parent are not tested in the raw version because they are only used in the propensity score model.

Table 3: Men summary statistics treatment and control group raw and inverse probability weighted

	(1)	(2)	(3)	(4)	(5)	(6)
	Employment				**Earning**	
	Raw			**IPW**	**Raw**	**IPW**
Variable	**Mean Control**	**Mean Treatment**	**Diff**	**Diff**	**Diff**	**Diff**
Employed	0.756	0.785	-0.030***	-0.002	0.000	0.000
Earnings	10333.928	10339.340	-5.412	-270.234**	-5.412	-277.589**
age	45.135	43.850	1.285***	-0.069***	0.896***	-0.068***
Age mother	73.802	72.401	1.401***	0.002	0.992***	-0.005
Age father	76.489	74.962	1.527***	-0.218***	1.128***	-0.209***
Partnered	0.745	0.764	-0.019***	0.000	-0.014***	0.001
Dutch	0.888	0.917	-0.029***	-0.002	-0.024***	-0.002
1st generation migrant	0.051	0.026	0.025***	0.001	0.019***	0.002
2nd generation migrant	0.061	0.057	0.004***	0.001	0.005***	0.001
No. of siblings	1.264	1.308	-0.045***	-0.022***	-0.035***	-0.018***
No. of kids <13	0.640	0.674	-0.033***	0.003	-0.023***	0.007
Father partnered	0.466	0.712	-0.246***	-0.010***	-0.234***	-0.010***
Mother partnered	0.455	0.692	-0.237***	-0.010***	-0.226***	-0.010***
Distance to mother	23.280	23.633	-0.352**	0.263	-0.243	0.182
Distance to father	25.140	24.816	0.324*	0.038	0.447**	-0.052
No. of jobs	1.065	1.064	0.001		0.001	-0.002*
Tenure (main job)	39.977	38.332	1.645***		1.645***	-0.290*
Distance to closest parent				0.057		-0.001
One parent death				-0.001		0.000
Age oldest parent				-0.179***		-0.180***
Number of obs. treatment group			1,071,257	1,056,404	837,722	812,846
Number of obs. control group			38,977,833	31,459,816	29,193,471	23,917,209

The significance of raw difference in means is tested with a t-test of equality of pre-treatment means with unequal variance in the period before the shock. For the IPW version, significance test from a weighted regression of the treatment indicator on the pre-treatment values of the respective variable is used. The number of jobs and tenure are not tested for the employment IPW version, as these are not included in the propensity score model. The distance to the closest parent, one parent dead, and the age of the oldest parent are not tested in the raw version because they are only used in the propensity score model.

A necessary condition for an increase in the balance of covariates is a well-specified propensity score model. In the case of misspecification, the treatment effect can suffer from large biases (Austin and Stuart, 2015; Imai and Ratkovic, 2014). Whereas the model choice of probit versus logit does not matter, variable selection and function form specification should be validated by checking for imbalance.

I specify a probit model for the propensity score including the 8 quarters pre-treatment means of covariates likely to be correlated with post-shock labour market outcomes on the one hand (employment, living with a partner, and tenure and number of jobs for the earnings equation) and variables that may be correlated with the probability of a parental health shock and post-treatment labour market outcomes on the other hand (the number of children below 13, age, age of the oldest parent, first and second generation migrant indicators, living situation of both parents, the distance between residence of the individuals and her father and mother, and an indicator of parental death) as recommended by Austin and Stuart (2015), Brookhart et al. (2006), or King and Nielsen (2016).[16]

To assess the quality of the propensity score model specification, I use a formal test of balance by Imai and Ratkovic (2014), which conducts a GMM overidentifying restriction test with H0 correct specification of the propensity score. If H0 is rejected, there is misspecification and thus there must be imbalance for at least one covariate. At covariate level, a t-test of difference in means between the control and treatment group can be conducted, but as the scale of every variable is different, this is not very informative on which variables are the most problematic (Caliendo and Kopeinig, 2005). Standardised differences in means and variances allows to compare imbalance of specific covariates on the same scale in the first and second moment. This test is informal as there are no standard errors available, but it can still be useful to locate the origin of balance problems. If imbalance is detected, the inclusion of higher order terms in the propensity score model are recommended (Caliendo and Kopeinig, 2005).[17]

The balance checking shows that for most variables, imbalance has been reduced with IPW (see Table 9). Also, in Table (2) and (3), a comparison of column (3) and (4) for the employment equation (and (5) and (6) for the earnings equation) show that the difference in mean between treatment and control group is not significant anymore for a majority of the covariates. However, for selected variables, imbalance in the weighted sample has increased compared to the raw data. The overidentification test (Table 8, last row) confirms this. I therefore reestimate the propensity score model including quadratic and or interaction terms for selected variables.[18] Including these higher order terms does not change the overall balance results, and also the difference-in-difference results with the weights for the higher order propensity score model are pratically identical to the

[16] There is an on-going debate on which variables should be included in the propensity score model. Imbens (2015) does not agree with the approach I chose, suggesting to include covariates explaining only the probability of treatment. However, he does not provide any formal proof or simulation results to support his claim, whereas the conclusions of Austin and Stuart (2015); Brookhart et al. (2006) are based on simulation studies.

[17] The aim of the propensity score model is to produce balance in the covariates. For this reason, it is recommended not to base variable selection of the propensity score model on the significance of covariates in the propensity score model but to use balance checking instead (Austin and Stuart, 2015; Brookhart et al., 2006). Again, Imbens (2015) advices to do the opposite.

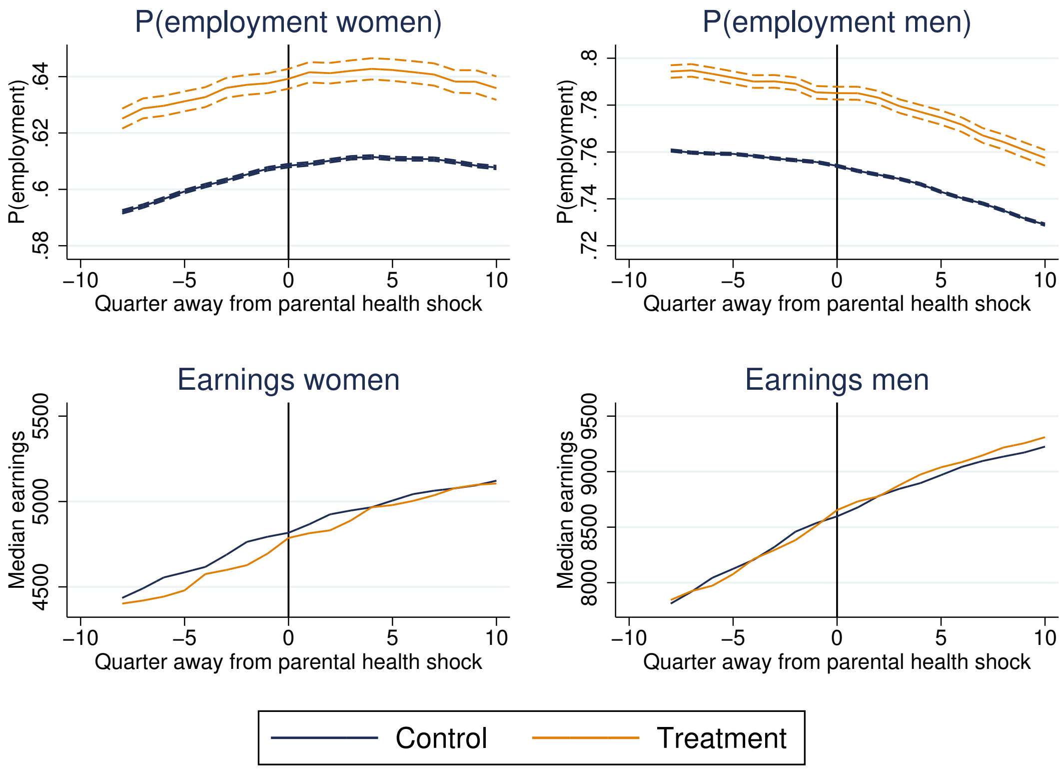
[18] I included a squared term of age, oldest parent age, distance to closest parent, tenure, the number of jobs, and an interaction between mother and father partnered (because there is a significant difference in means) in every equation.

one with the simpler propensity score model. For this reason, I continue working with the linear propensity score model. The propensity scores enter the calculation of inverse probability weights, which are used in the difference-in-difference model of the previous Section 6.2.

7 Results

7.1 Summary statistics and trend graphs

Figure 1: Employment and earnings trends



The dashed lines correspond to the 95% confidence intervals.

The first three columns of Table (2) and (3) show the unweighted mean of covariates and their differences by treatment group for the employment sample one period before the health shock. The earnings, the number of jobs and the tenure in the main job are only considered for the employed. The difference in mean in the raw data is significant for almost all variables for both men and women (Column 3 and 5).

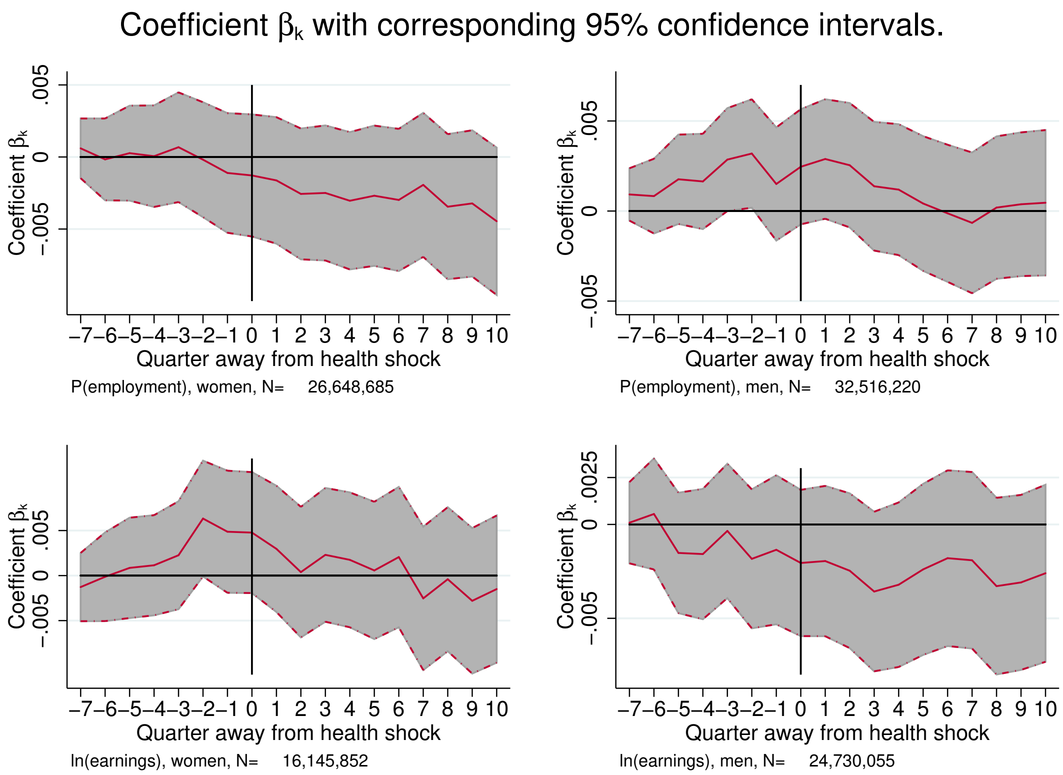
Figure (1) shows the unweighted employment proportions and earnings median trends in the 8 quarters before and 10 quarters after the parental health shock for the baseline sample.[19] By graphical inspection, there does not seem to be a visible difference in trends between the treatment and the control group in the 8 quarters before the treatment. This is a first indication that the common trends assumption may be reasonable. Moreover,

[19]The trends are calculated as follows for the employment proportions (and similarly for the median

the parental health shock does not seem to change the trends, which seems to suggest that the employment and earnings effects of an unexpected parental health shock are limited. The figures show that for both genders, the treatment group is more likely to be employed on average than the control group, but there seems to be no large difference in median earnings across the groups.

7.2 Difference-in-Difference combined with IPW

Figure 2: Earnings and employment effects of a parental health shock



In Figure (2), I plot the inverse probability weighted coefficients of the difference-in-difference term β^k and their 95% confidence interval for the probability of employment and conditional log earnings by gender. The leads of the parental health shock are not significant, which confirms that the common trend assumption is likely to hold. The plots show that compared to 8 quarters before the health shock, a parental health shock does not have an effect on either the probability of employment or conditional wage for women and men.

of earning).

$$y_{k..} = \frac{1}{6}\sum_{c=1}^{6} y_{kc.} = \frac{1}{6}\sum_{c=1}^{6}\left[\frac{1}{N_{kc}}\sum_{i=1}^{N_{kc}} y_{kci}\right] \tag{4}$$

First, the proportion of employment y is calculated over all individuals i in cohort c in quarter k away from the parental health shock. Second, I average these over the six cohort and plot $y_{k..}$ against k for both treatment and control group.

Inference based on propensity scores requires an overlap of the common support of the propensity score between treatment and control group (Rosenbaum and Rubin, 1983). This condition eliminates 26 (40) women (men) for the employment equation, and 126 (19) women (men) for the wage equation (see Table 8). [20]

Figure 3: Comparison results with and without IPW

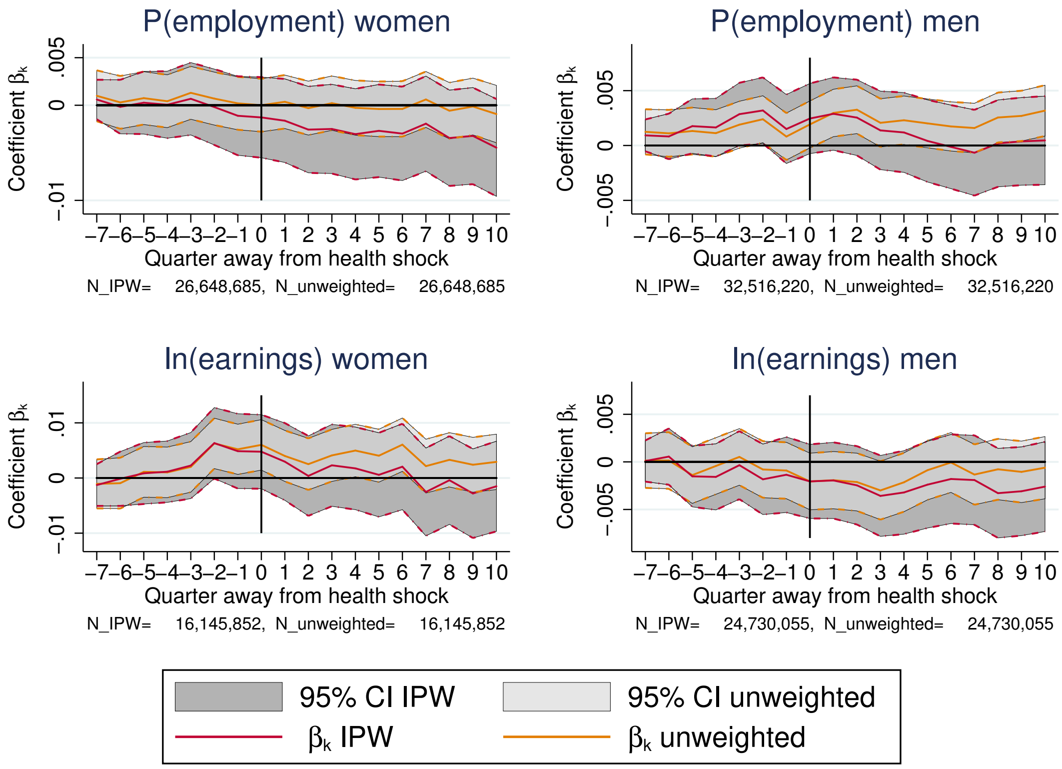


Figure (3) shows a comparison of the IPW and the unweighted results for the difference-in-difference term β_k and their corresponding confidence interval. It illustrates that the inverse probability weighting is necessary to satisfy the common trend assumption, as the health shock leads are borderline significant for female earnings in the unweighted specification. In addition, the effects found are slightly more negative for the IPW than for the unweighted results, and the IPW confidence intervals mostly contain the confidence interval of the unweighted results. This implies that the weighting does not change results importantly, but it helps to satisfy the common trend assumption.

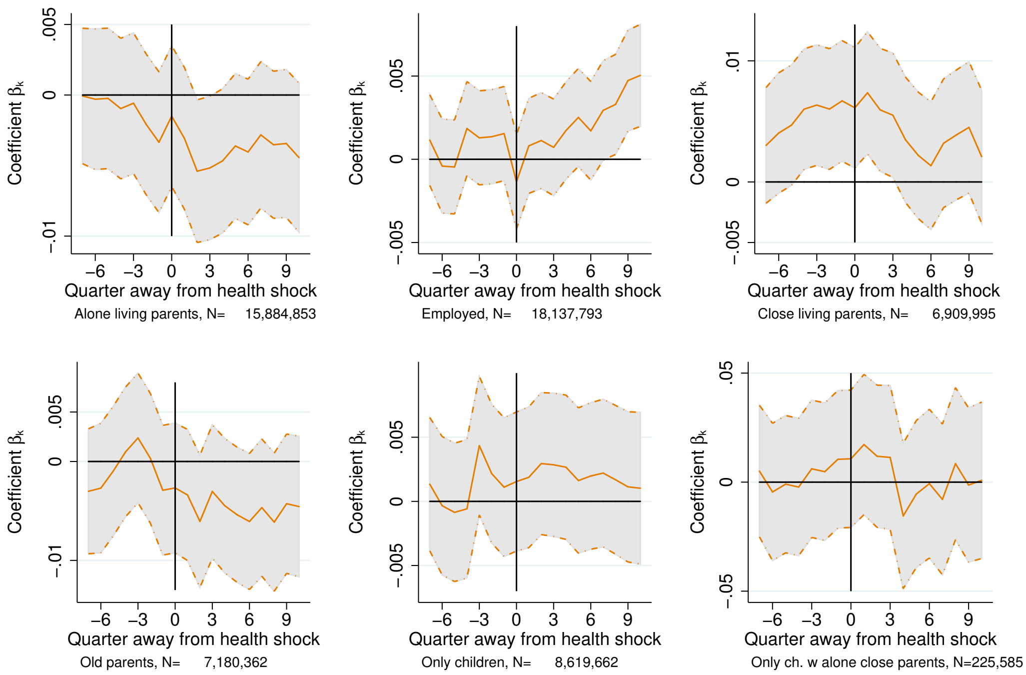
7.3 Subsample analyses

The subsample analysis is presented in Figure (4) for female employment probabilities, (5) male employment probabilities, (6) female conditional earnings, and (7) male conditional

[20]In addition, I use the distance between the residence of the closest parent and the individual in the propensity score estimation. As there are missings in this variable (and due to the elimination of the individuals outside the commons support), the sample of the IPW is smaller than the the one in the first three columns of Table (2) and (3). Table (7) in the Appendix gives an overview of the reduction in observation induced by the weighting.

Figure 4: Subsample analysis for female employment probabilities

Coefficient β_k with corresponding 95% confidence intervals.



earnings. For now, they are not inverse probability weighted due to time constraints, but weighted results will be added in a later version.

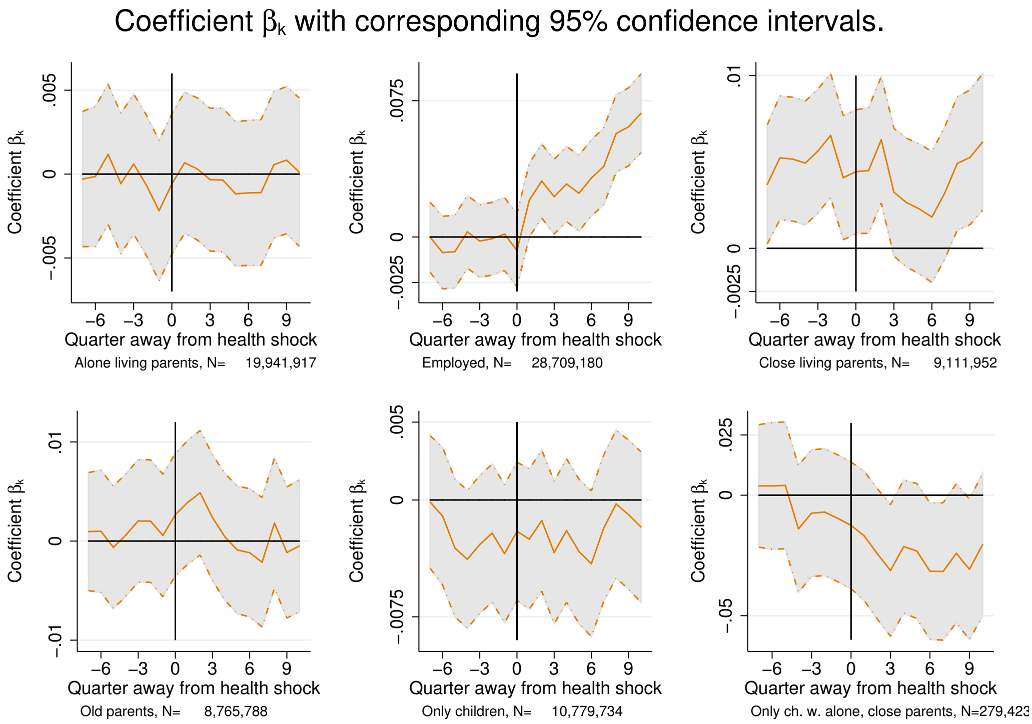
The common trend assumption is violated in the close living parents sample for female and male employment probabilities. Individuals living in a 5 km radius from their parents seem to be able to anticipate the health shock of their parents and consequently work more. Due to the violation of this assumption, I will not further comment on these results, but adding the inverse probability weights may help to address this issue.

A parental health shock for alone living parents reduces employment probability of daughters, the coefficients are borderline significantly different from zero around 2 to 3 quarters after the health shock. Moreover, for the subgroup analysis including the women employed during a year before the shock, the women who had a parental health shock are more likely to stay employed than the control group around two years after the health shock. The subgroup analyses for women with parents aged over 80, only-children, and only children with close, alone-living parents reconfirm the finding from the main analysis that there is no employment effect.

Similar to women, the subgroup analyses on men having been employed at least a year before the health shock shows that the treated are more likely to stay employed than the control group. Only-children men with close but alone-living parents reduce their employment after a parental health shock around quarter 3 and 6.

Female conditional earnings are somewhat higher two years after a parental health shock for women with parents aged at least 80. For men, conditional earnings do not seem to depend on a parental health shock, with the exception of one borderline significant spike at quarter 8 for the close-living parents sample.

Figure 5: Subsample analysis for male employment probabilities



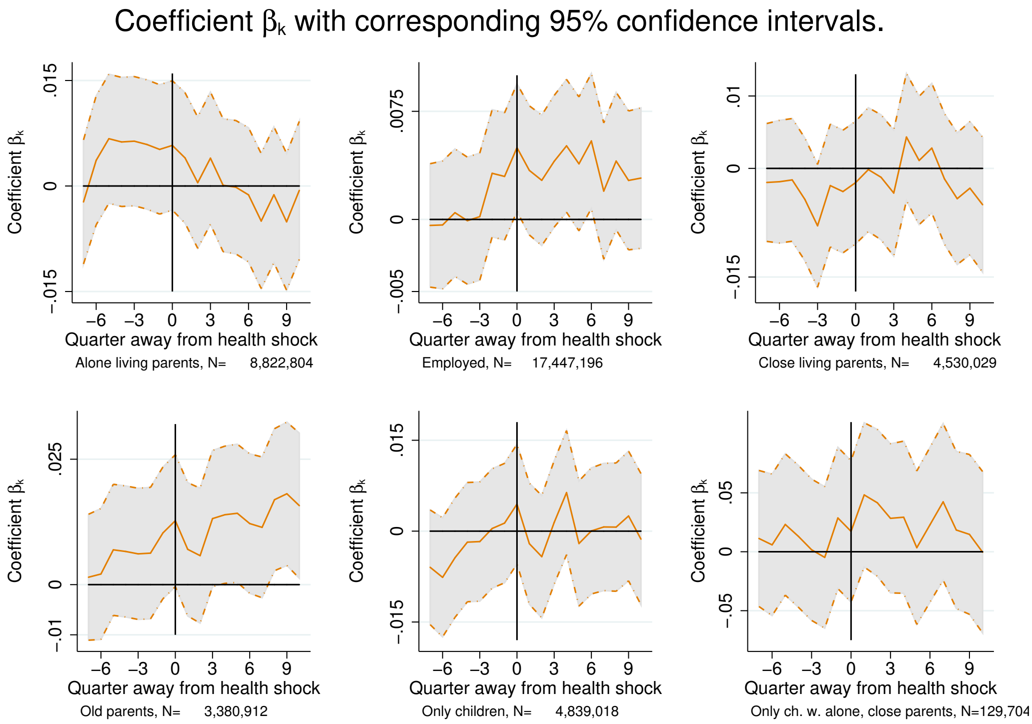
Despite consisting of at-risk care givers, where I expected a larger effect, the subsample analyses show that overall, there is no effect of an unexpected parental health shock on the probability of employment and conditional earnings.

8 Discussion

This study uses unexpected parental health shocks as exogenous variation in informal care demand to evaluate the effect of a parental health shock on the probability of employment and conditional earnings. I estimate a reduced-form difference-in-difference model over multiple treatment cohorts and combine it with inverse probability weighting. The main findings show that there is no effect of a unexpected parental health shock on the probability of employment and conditional earnings. The analysis of subsamples with at-risk care givers, for whom I expected the effects to be particularly large, also do not show pronounced effects. Given the large sample size, these results are very precisely estimated and are not due to lack of power.

Other studies also do not find contemporaneous earnings (Bolin et al., 2008) or employment effects (Meng, 2013; Van Houtven et al., 2013, e.g.), also for the Netherlands (Moscarola, 2010; Viitanen, 2010). Nevertheless, the question arises how these results are reconcilable with the fact that a large part of the Dutch population indicates to be informal care givers, and that 20% of the population reports to provide intensive and/or prolonged care. In addition, our findings seem to be at odds with another study conducted in the Netherlands based on a small-scale non-representative survey, indicating that around 15% of a sample of care givers had given up their job in order to care for relatives

Figure 6: Subsample analysis for female conditional earnings

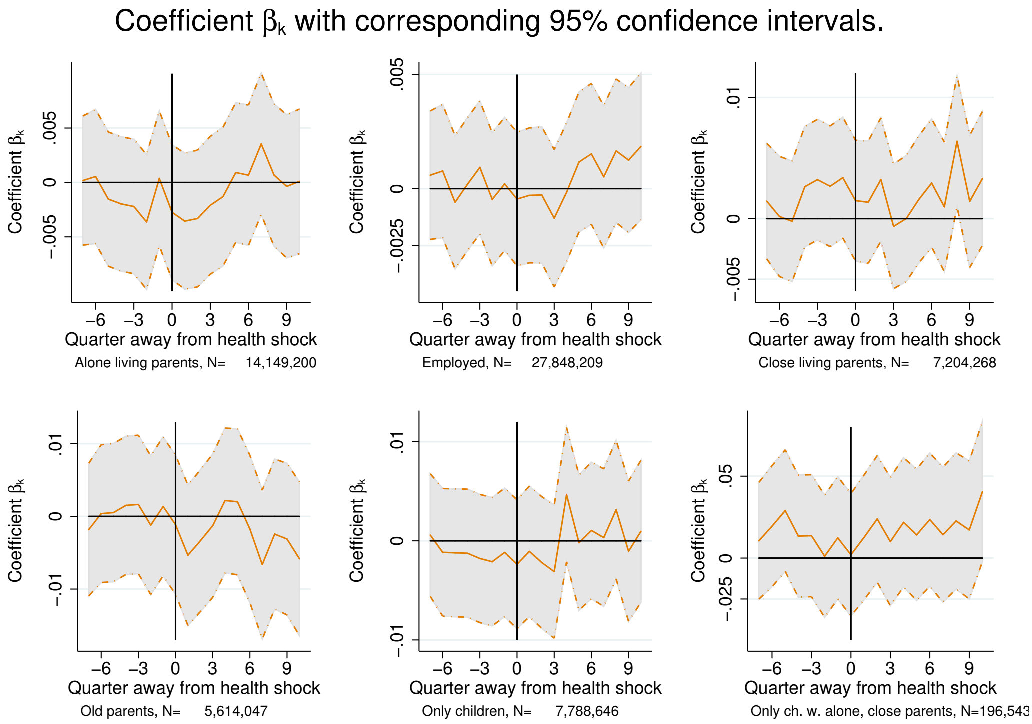


(Van Exel et al., 2002). Based on this study, there seems to be a perceived labour market - care giving trade-off seems for at least some individuals. However, the results are not comparable to our set up due to potential reporting bias and representativity issues.

The explanation of our results may lie within three connected factors. First, it is possible that Dutch care givers do not face a trade-off between paid work and care responsibilities in the medium run and manage to combine both tasks reasonably well. This may in part be because the Dutch formal long-term care system largely meets care needs and is readily accessed (low co-payments and low waiting times) (Bakx et al., 2016). Thus, in the event of sudden parental care need, adult children may face increased informal care demand for a relatively short period of time until the formal system takes over, which may not have an effect on the long-run labour market outcomes we consider in this study - at least for the majority of the population. In addition, a characteristic of the health shock measure used is that it is correlated with formal long-term care use as shown in the Appendix (9.2). So even though the health shock acts as an instrument for the demand for care, this demand may in practice be met by the formal system. This explanation is also in line with the results of the theoretical model. A developed formal system implies that the productivity of a child's care to improve his parent's health is low, leading to a low marginal utility of care giving and hence no care is provided. Of course, this is not entirely reconcilable with reality, as the model in this case predicts no care provision at all, whereas in reality people do provide care, but it illustrates the role of the formal care system well.

A second explanation may be that most care givers do not work in the first place, and that we therefore do not find employment or earnings effects. However, different data sources do not support this explanation. Informal care givers are indeed less likely to be

Figure 7: Subsample for male conditional earnings



employed, but the difference between care and non-care givers is relatively small. In 2008 for example, 71% of informal care givers between 18 and 65 had a job for at least one hour per week, opposed to 77% of non-care givers (Oudijk et al., 2010). Also Van Exel et al. (2002) find that around one quarter of informal care givers have a paid job in their sample, a tendency that is confirmed by Dautzenberg et al. (2000). Hence, there seems to be a certain selection of the non-employed into care giving activities, but it does not explain the results of our study.

The third explanation for our findings lies in our methodological approach. Given that we limit our analysis to unexpected health shocks for identification reasons, it does not capture informal care for the chronically nor the mentally ill. We can therefore not exclude that there is a trade-off between paid work and informal care for some care givers looking after a chronically ill parent with for example dementia.

Further research on the Netherlands should therefore try to include care demand by the chronically ill elderly in the picture. In the framework of this thesis, the generalisability could be improved if there was a way to include informal care demand stemming from chronic illnesses without comprising identification, which may be difficult in practice. Possible extensions on a methodological level include adding CEM weighting and including multiple parental health shocks in the spirit of Boden and Galizzi (2003). Moreover, in order to extend the interpretation of results to overall earnings (and not only conditional on employment), one could add a Heckman correction for the fact that the employed are the people with a potentially higher pay. In addition, other studies investigating the relationship between informal care and labour market participation emphasize the importance of dynamics, which could be added to the model.

In sum, we find no effect of an unexpected parental health shock on labour market outcomes. It seems therefore that the Dutch state is complying with the expectations of its citizens that it should take the main responsibility in elderly care (Mot, 2010), as adult children do not seem to be forced to stop working or suffer from earnings penalties when they have to care for a parent that has been hospitalised unexpectedly. From a public finance perspective, this means that there seems to be no loss in forgone tax or pension income due to a trade-off between informal care giving and labour market participation - at least for informal care demanded after an unexpected parental health shock. For this type of informal care, it may therefore not be financially worthwhile to replace informal care by formal home care services if we assume limited effects on health care use of care givers.

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9 Appendix

9.1 Data sets

Table 4: Data sets

Data set	Version	Content
PARTNERBUS	V1 2015	Partner identification
GBAPERSOONTAB	V1 2015	Basic personal data
Do	V1 1995-2005	Death register
GBAADRESOBJECTBUS	V1b 2015	Address register
VSLGWBTAB	V2 2015	Address municipality codes
KINDEROUDERTAB	V2 2015	Children parent linkages
LMR_Basis	V2 1999-2004, V3 2005	Hospital admissions
BAANKENMERKENBUS	V3 1999-2005	Employment
BAANSOMMENTAB	V3 1999-2005	Earnings

9.2 Diagnoses classified as parental health shocks and associated informal and formal home care use

I discuss two studies that provide insights in health determinants of informal care. First, García-Gómez et al. (2015) find that having a mental illness, cancer, respiratory, circulatory, and a congenital disease is associated with the probability of any and intensive informal care use among the disabled in Spain. Cancers, respiratory, and circulatory diseases are at least partly defined as health shocks. As the health shock diagnoses are less aggregated, it is not possible to draw further conclusions. Second, Van Exel et al. (2002) find in a non-representative Dutch survey that the most common reasons for needing informal care are hip or knee arthrosis; consequences of a stroke; dizziness and falling; depression; chronic disorder of neck/shoulder, elbow, wrist, hand; back injury or hernia; and dementia or alzheimers. Strokes, falls, and certain back injuries are indeed classified as health shocks in this study.

In a second step, I compare the health shock definition with health determinants of formal long-term care use provided by two studies from the Netherlands. First, Wong et al. (2010) presents the formal long-term care use of Dutch hospital patients aged 65+ for the 23 most prevalent hospital admission diagnoses. A comparison with our health shock diagnoses shows that among the 65+ who were hospitalised due to a health shock, 50% received formal home care after their hospitalisation (for details see Table (5) in the Appendix for details). Second, Bakx et al. (2015) use data of the long-term care expenditure up to three periods after a hospitalisation for a given diagnosis group. The comparison with the health shock definition shows that 32% of the total LTC expenditure 3 years after a hospitalisation are caused by diagnoses classified as health shocks.[21] High LTC expenditure could mean a nursing home admission, which does not require informal

[21] As the grouping in Bakx et al. (2015) is on a more aggregate level than the health shock definition, this is a conservative estimate. Using a less strict comparison criterion that potentially overestimates the coverage of the health shock, 41% of the post-hospitalisation LTC costs would be captured by the health shock.

care anymore. However, Table 1 from Wong et al. (2010) shows that for all diagnoses, it is more likely to receive formal home care after a hospitalisation than a nursing home admission. This implies that there are no specific diagnoses which should be excluded from the parental health shock because they lead almost certainly lead to a nursing home admissions.

Table 5: Health shocks and the 23 most common diagnoses of Dutch Hospital Patients aged 65+ using LTC after hospitalisation (2004)

Health shock	Condition	% of sample	Formal care %	Home care %	home for the elderly %	nursing home %
1	Lung cancer	1,1	54,2	50,1	1,3	2,9
1	ovary cancer	0,2	51,9	47,3	1,9	2,7
1	Intestinal, stomach and rectum cancer	2,2	50,2	46,1	1,6	2,6
1	Uterus cancer	0,3	34,9	32	1,7	1,2
1	fracture of femur	1,7	53,8	29,9	5,5	18,4
1	fracture of ankle of lower leg	0,4	42,4	26,7	4,8	10,9
1	fracture of elbow and forearm	0,5	32,1	24,4	2,5	5,1
1	bladder cancer	1	25,8	23,9	0,6	1,3
1	prostate cancer	1,3	22,9	20,2	0,8	2
1	cerebrovascular disease	3,6	38,5	17,9	1,4	19,2
1	intracranial injury	0,6	27,1	17,4	2,2	7,5
0	Alcoholic liver disease	0,1	45,7	34,6	2,6	8,5
0	Coxarthrosis	3,5	37,7	29,6	3,4	4,7
0	Heart failure	3,3	35	29,4	2,3	3,3
0	Glomerular disorders	0,5	31,1	29	1	1,1
0	Infections of skin	0,4	32,8	28,4	1,2	3,2
0	schizophrenia	0,2	47,8	28,1	3,8	15,9
0	chronic obstructive pulmonary disease (COPD)	3,5	31,7	27,6	1,5	2,6
0	dementia	0,4	51,1	26,5	4,6	20
0	diabetes mellitus	4,1	31,9	26,3	1,5	4,1
0	alzheimer's disease	0,1	42,9	23,8	2,9	16,3
0	Gonarthrosis	2,6	29,7	23,1	2,1	4,5
0	Epilepsy	0,4	32,7	22,6	2,2	7,8
0	Other	72,4	13,4	11,8	0,7	0,9

9.3 Earnings distribution

Table (6) shows summary statistics characterising the distribution of log earnings conditional on employment and compares them with a normal distribution at the same mean and variance. Density estimates, histograms or qq plots are not released by Statistics Netherlands due to privacy concerns, therefore these summary statistics are presented instead. It can be seen that the 25, 50, and 75 quintile correspond to the ones of a normal distribution reasonably well. There are small differences in the third and and a bit larger in the fourth moment, but the overall it seems that the normality assumption is plausible.

Table 6: Verification ln(earning) approximatively Normally distributed

	Women		**Men**	
	ln(earning)	$Y \sim$ **N(8.4,0.55)**	**ln(earning)**	$Y \sim$**N(9.1,0.32)**
mean	8,394975	8,399731	9,075875	9,096766
variance	0,553095	0,556337	0,318014	0,319397
p25	8,028538	7,897064	8,830327	8,713988
p50 (median)	8,484403	8,400166	9,06093	9,098362
p75	8,881151	8,902586	9,347593	9,47784
skewness	-0,81882	0,005172	-0,8134	-0,00014
kurtosis	5,79459	2,999172	12,36652	2,98175
N	19658762	100000	30031193	100000

The seed for the normal simulations is set at 239487.

9.4 Sample comparisons

In Figure (8), the estimated effect of a parental health shock in the overall sample is always in the 95% confidence interval of the estimated effect of the missing address sample and conversely. Therefore, any difference in results found between the close-living parents subsample and the baseline specification can be attributed to differences in residence, and not sample selection.

9.5 Additional output IPW

Table (9) shows an informal balance test for all covariates. For standardised means, the number displays the proportion of the standardised means before and after weighting. A number larger than 1 implies that imbalance in the covariate increased in the weighed version compared to the raw data. For example, for female employment, the imbalance of the weighted standardised average of living with a partner decreased by 60% (1-0.4=0.6) compared to the unweighted data. Similarly, for the variance, I first calculate the absolute deviation of the standardised variance to one, and then look at the proportion of this distance between weighted and raw data. Again, a proportion larger than 1 implies increased variance imbalance with respect to the raw data. For example, for female employment, the imbalance in the variance for age is 40% larger than the initial imbalance. Increased imbalance is highlighted with italics.

Figure 8: Comparison unweighted sample with non-missing address sample

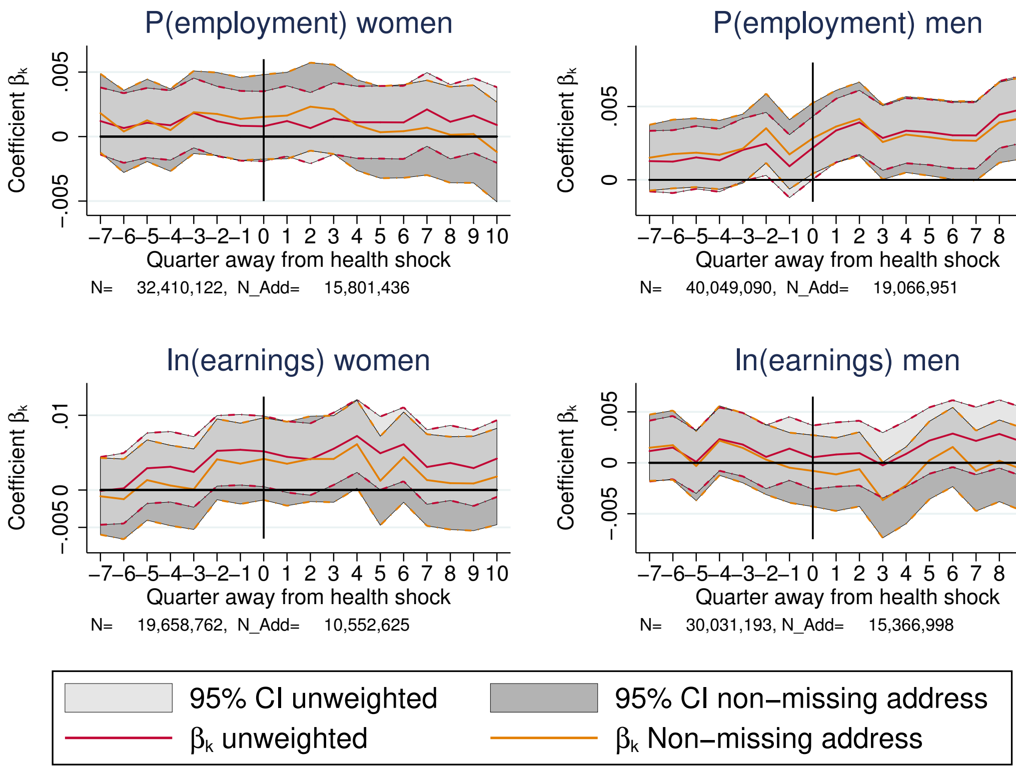

Table 7: Reduction in observations due to IPW by treatment indicator

		Treatment group		Control group	
		No. obs	**%**	**No. obs.**	**%**
Women	**Employment**	8,127	0.9%	5,753,310	18.2%
	Earnings	29,537	5.3%	3,483,373	18.2%
Men	**Employment**	14,853	1.4%	7,518,017	19.3%
	Earnings	24,876	3.0%	5,276,262	18.1%

Note that observations means quarter-person observations in for the treatment group, and quarter-person observations multiplied by six in the control group.

Table 8: Propensity score estimation (probit coefficients)

	(1)	(2)	(3)	(4)
	Women		Men	
VARIABLES	Employment	Earnings	Employment	Earnings
Number of kids below 13	-0.110***	-0.0991***	-0.0836***	-0.0805***
	(0.00655)	(0.00828)	(0.00518)	(0.00578)
Living with a partner	0.0252**	0.0175	0.0555***	0.0678***
	(0.0118)	(0.0147)	(0.0110)	(0.0129)
Age oldest parent	0.0340***	0.0349***	0.0338***	0.0358***
	(0.000865)	(0.00108)	(0.000791)	(0.000893)
First Generation Migrant	-0.413***	-0.266***	-0.547***	-0.456***
	(0.0304)	(0.0386)	(0.0265)	(0.0309)
Second Generation Migrant	-0.106***	-0.0918***	-0.0700***	-0.0868***
	(0.0198)	(0.0244)	(0.0179)	(0.0204)
Age	-0.0190***	-0.0189***	-0.0161***	-0.0173***
	(0.00130)	(0.00181)	(0.00118)	(0.00143)
Father partnered	0.719***	0.716***	0.759***	0.745***
	(0.0253)	(0.0319)	(0.0230)	(0.0263)
Mother partnered	0.224***	0.228***	0.182***	0.211***
	(0.0242)	(0.0304)	(0.0219)	(0.0251)
Dist. closest living parent in km	-0.000517***	-0.000476***	-0.000370***	-0.000406***
	(0.000112)	(0.000139)	(0.000103)	(0.000116)
One parent dead	-0.231***	-0.283***	-0.312***	-0.299***
	(0.0650)	(0.0889)	(0.0614)	(0.0722)
Employed	-0.0513***	-0.0944**	-0.0111	-0.000858
	(0.0102)	(0.0372)	(0.0111)	(0.0472)
Tenure in the main job		-0.000148		-0.000406**
		(0.000206)		(0.000163)
Number of jobs		0.0334		-0.00142
		(0.0260)		(0.0207)
Constant	-5.522***	-5.596***	-5.674***	-5.791***
	(0.0632)	(0.0936)	(0.0566)	(0.0826)
Observations	1,413,192	903,959	1,724,645	1,357,613
Number of obs. outside common support	26	126	40	19
P-value imbalance test	0.000	0.000	0.000	0.000

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table 9: Percentage decrease from raw to weighted data in standardised mean/variance of the difference between treatment and control group

	Employment				**Earning**			
	Women		**Men**		**Women**		**Men**	
	% of standadised difference weighted vs raw in							
	mean	**variance**	**mean**	**variance**	**mean**	**variance**	**mean**	**variance**
Number of kids below 13	0.0	0.6	0.1	0.4	0.1	0.6	0.2	0.8
Living with a partner	0.4	0.3	0.7	0.4	0.4	0.9	0.3	0.1
Age oldest parent	*1.4*	0.6	0.8	0.6	0.8	0.6	0.6	0.5
First Generation Migrant	0.0	0.0	0.0	0.1	0.0	0.0	0.1	0.1
Second Generation Migrant	0.2	0.2	0.3	0.3	0.3	0.3	0.2	0.2
Age	0.4	*1.4*	0.4	*1.2*	0.6	*3.8*	0.5	*26.1*
Father partnered	0.1	0.1	0.1	0.1	0.1	0.1	0.1	0.1
Mother partnered	0.1	0.1	0.1	0.1	0.1	0.1	0.1	0.1
Dist. closest living parent in km	0.8	0.1	0.4	*1.9*	*1.4*	0.5	0.4	*2.3*
One parent dead	0.1	0.2	0.0	0.1	0.1	0.1	0.0	0.1
Employed	0.3	0.9	0.1	0.0	0.3	0.3	0.6	0.6
Tenure in the main job					0.6	0.5	0.8	*4.1*
Number of jobs					1.0	0.8	*1.1*	0.7
Number of obs.	1413192		1724645		903959		1357613	

Increase in imbalance from raw to weighted is indicated in italics.