The kids are alright - labour market effects of unexpected parental hospitalisations in the Netherlands

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Abstract

Unexpected negative health shocks may have serious consequences for labour force participation, not only for those who incur the shock but also for their family members. In particular, adult children may spend substantial time providing informal care and may incur stress-induced mental health problems following a parental health shock, which may in turn lead to reductions in labour supply. We link administrative data on labour market outcomes, hospitalisations and family relations for the full Dutch population for the years 1999-2008 to evaluate the effect of an unexpected parental hospitalisation on the probability of employment and on conditional earnings for the working age population. Using a difference-in-differences model combined with coarsened exact matching and individual fixed effects, we find no effect of an unexpected parental hospitalisation on either the probability of employment or conditional earnings for Dutch men and women, and neither for the full population nor for subpopulations most likely to become a caregiver. These findings suggest that the extensive public coverage of formal long-term care in the Netherlands provides sufficient opportunities to deal with adverse health events of family members without having to compromise one's labour supply.

1 Introduction

Severe adverse health events occur more frequently in old age. These health shocks do not only affect the patient herself, but also family members, such as adult children. If an elderly woman falls and breaks her hip, her son may spend time supporting her at home after she has returned from the hospital. In addition, the son probably worries about his mother and may be stressed due to the caring responsibilities. Both time spent caring and stress may affect the son's labour market activities. Against this background, this study assesses how an unexpected parental hospitalisation affects labour market outcomes of their adult children.

Labour market effects of parental health shocks are undesirable because they cause uncertainty for individuals with regard to their income that they cannot insure. Moreover, parental health shocks may have long-term financial consequences that the caregiver may not be aware of when deciding about giving up his job or reducing work time to be able to care: (i) the need for informal care often lasts a few years and re-entering the job market thereafter may be hard, especially for the stereotypical female, middle-aged caregiver and (ii) reducing labour market activity (even if temporary) or quitting one's job altogether may have negative consequences for old-age pension benefits. Finally, the reduction of tax and pension contributions due to caregiving can jeopardise public finances in a context of population aging. Assessing the effects of a parental health shock on labour market outcomes is thus important to both understand the trade-off that the family members face and to gain insights for long-term care (LTC) and labour market policy. Specifically, the Dutch government has among its policy goals to increase both labour market participation and informal caregiving, two goals which may not be easy to reconcile (Josten and De Boer, 2015). However, if labour market participation is indeed reduced following a parental health shock, then steps taken towards achieving one goal may put the other one further out of reach. Policy makers may then prefer to create an environment that facilitates combining caregiving and paid work, or lower their expectations.

Addressing this question for the Netherlands is of interest, as it is the country with the highest LTC expenditure per capita in ther OECD (OECD, 2017c). As a result, the Dutch LTC system is universal, comprehensive, and very generous (Bakx et al., 2015a): if workers are able to combine caregiving and work anywhere, it would be in the Netherlands. Insights from studies about the Netherlands should be informative for other countries considering to considering extending the coverage provided by their LTC systems.

Simply regressing children's labour market outcomes on parental health outcomes will lead to biased estimates for two reasons. First, if parental health is gradually deteriorating, e.g. because of chronic illnesses such as dementia or COPD, individuals may have anticipated the care needs of their parent(s), and have adjusted their labour market status already before the health deterioration warrants LTC. In order to avoid such anticipation bias, we exploit diagnoses from unexpected hospitalisations classified by physician expert opinion as plausibly exogenous variation in parental health. While these hospitalisations represent a subset of all health problems that the elderly experience, they represent a large and relevant subset. In addition, these parental health shocks need to be severe enough to lead to informal care demand. Indeed, our parental health shock indicator is related to increased risk of mortality, and the included diagnoses are correlated with informal and formal LTC use in the Netherlands (Bakx et al., 2015b; García-Gómez et al., 2015b; Van Exel et al., 2002; Wong et al., 2010). Second, we can rule out that the parental health shock indicator suffers from justification bias that may be common in survey data, since it is based on hospital admission diagnoses from administrative data.

Using quarterly Dutch administrative data from 1999-2008, we evaluate the effect of an unexpected parental hospitalisation on (i) the probability of employment and (ii) conditional earnings over 24 quarters. We link records for working-age individuals to their parents' health information and estimate a difference-in-differences model combined with coarsened exact matching and individual fixed effects. In subsample analyses, we check for heterogeneous effects among individuals most likely to be caregivers based on the residence of parents, number of siblings, alone living parents, alone living individuals, employment status in the quarter before the parental shock and the age of parents.

A parental health shock can negatively affect the labour market involvement of the child in two ways: through informal care provision and through stress. Giving care to a sick parent can be time intensive and energy demanding, and caregivers may quit their jobs, reduce working hours and/or suffer from earnings penalties. The relationship between informal caregiving and labour market outcomes has been studied extensively over the past two decades and either no or a negative effect of caregiving on labour market outcomes was reported.[1]

The second channel consists of the mental health effects that a parental hospitalization may inflict. Naturally, daughters and sons worry about their parents if they suffer from a severe illness or injury, which might lead to stress-induced health issues that could in turn have adverse labour market consequences. The literature reports a positive association between parental and child health, which persists when controlling for individual fixed effects and caregiving effects (Amirkhanyan and Wolf, 2006; 2003; Bobinac et al., 2010), implying that there is often a mental health effect induced by a parental health shock.[2] Moreover, Banerjee et al. (2017), among others, have documented a reduced labour market involvement caused by bad mental health. On the other hand, the absence of any of the links in the causal chain described will result in no effect of parental hospitalisation on labour market outcomes.[3]

Through one or both of these two channels, we expect either a negative or no total effect of a parental health shock on children's labour market outcomes. Empirical evidence on the subject is sparse. Using Norwegian register data, Fevang et al. (2012) find that employment and earnings of adult children decline *prior* to the death of a lone parent, especially for daughters. By limiting their sample to individuals who lost a parent in the sample period, they do not have a control group. We refine the approach of Fevang et al. (2012) in two ways. First, we exploit unexpected parental hospitalisations, which cause a shock in the demand for informal care for a larger share of the affected parents. This may be a more precise indicator of increased informal care demand than the death of a parent. Second, we compare potential caregivers with individuals not experiencing a parental health shock by introducing a control group that does not differ significantly from the treatment group prior to treatment.

Three other studies have evaluated the labour market responses of *spouses* after a health shock of their partner. First, García-Gómez et al. (2013) find that an unexpected hospitalisation of a spouse in the Netherlands reduces employment by 1 percentage point, and earnings by 2.5% two years after the spousal hospitalisation. Second, Jeon and Pohl (2017) examine labour market responses after a cancer diagnosis of spouses in Canada and find a strong earnings and employment decline. . Our study applies a similar meth-

[1]See Bolin et al. (2008); Carmichael et al. (2010); Casado-Marín et al. (2011); Ciani (2012); Crespo and Mira (2014); Ettner (1995; 1996); Geyer and Korfhage (2017); Heger (2014); Heger and Korfhage (2017); Heitmueller (2007); Heitmueller and Inglis (2007); Jacobs et al. (2016); Leigh (2010); Løken et al. (2017); Meng (2013); Michaud et al. (2010); Moscarola (2010); Schmitz and Westphal (2016); Schneider et al. (2013); Van Houtven et al. (2013); Viitanen (2010). For a more extensive literature review see Bauer and Sousa-Poza (2015); Lilly et al. (2007).

[2]This is not a problem for our identification strategy, because we are interested in the total effect of a parental health shock on labour market outcomes.

[3]Finally, a combination of the mental health and the informal caregiving channel is also possible, where caregiving stress can impact the health of the caregiver, also leading to less involvement in labour market activities. Negative health effects of informal caregiving have been documented in various studies (Bauer and Sousa-Poza, 2015; Coe and Van Houtven, 2009; Zwart et al., 2017).

odology as Jeon and Pohl (2017) to a broader population group and a wider range of adverse health events, which implies a higher incidence of health shocks. Third, Fadlon and Nielsen (2015) study the effect of health and mortality shocks on the labour market outcomes of Danish spouses. They find that a spousal death to lead to an increase in labour supply, especially for women, whereas non-fatal health shocks do not affect the labour supply of the spouse. The identification strategy of Fadlon and Nielsen (2015) relies on individuals with a future health shock as a control group. Our study uses a more general control group based on the overall population, while our results still hold when using their identification strategy as a robustness test.

Our research complements these studies because we focus on the effects on the labor market outcomes of adult children rather than spouses. As severe health shocks occur mainly among the oldest old,[4] the spouses of these patients have often retired and labour market effects are most likely to occur among their children.

In addition, we offer the following contributions to the literature to date. First, the quarterly frequency of observed outcomes in our data enables us to test underlying assumptions, while still painting a detailed picture of the consequence of a parental health shock over 24 quarters. Second, our analysis is not affected by non-response or attrition bias as we include the whole population of the Netherlands. Third, compared to the literature on labour market effects of informal caregiving, our study can be interpreted as a reduced form set up which avoids having to separate the effects of "caring for" and "caring about" (Bobinac et al., 2010), which are difficult to disentangle and challenge the validity of using a parental health shock as an instrument (Bom et al., 2018). Moreover, unexpected parental hospitalisations are a more disaggregated and precise instrument than previously used health shock proxies (e.g. Bolin et al., 2008; Jacobs et al., 2016; Van Houtven et al., 2013). Fourth, our measure does not suffer from any reporting biases compared to the common 5-point scale self-reported parental health indicator that is used in other studies (e.g. Ciani, 2012). Finally, we provide estimates for the entire population, not only a specific at-risk caregiver subsample.

We find that in the Netherlands, an unexpected parental health shock does not have any labour market effect, neither on employment probabilities nor on conditional earnings, neither for men, nor for women. Because of the large study population, our result is very precisely estimated. Subgroup analyses for at-risk caregivers and various robustness tests confirm the zero effect. Our finding suggests that the LTC and labour market policies of the Dutch government facilitate the combination of paid work and caregiving. Since the Dutch LTC system is very generous, our findings can be reconciled with studies from other countries reporting labour market effects of less generous LTC system policy reforms (e.g. Fu et al., 2017; Geyer and Korfhage, 2017; Løken et al., 2017).

[4] Fadlon and Nielsen (2015) report that less than 12% of the households experiencing a shock has two spouses younger than 60 (at which most Danes appeared to retire in that period). In the other 88% of cases, the labor responses are mostly among the children. The average age of the parent experiencing a shock in our data is 74 for mothers, and 77 for fathers.

2 Institutional Background

The Dutch formal LTC system is comprehensive and has a longstanding tradition; a public LTC insurance (ABWZ[5]) was introduced in 1968 already. In the period of study (1999-2008), it covers all LTC in institutions and at home, where care can consist of domestic help,[6] social assistance, personal care, and nursing care (De Meijer et al., 2015; Mot, 2010). Given the broad coverage of the public LTC insurance, private LTC is marginal and concentrated only among the wealthy (Maarse and Jeurissen, 2016). An independent assessment agency grants access to LTC 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 LTC 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 caregiver (Mot, 2010). Roughly 5% of the elderly eligible for LTC chose a cash benefit in 2014 (CBS, 2017). Co-payments are low (making up 8% of total revenues) and income-dependent (Bakx et al., 2016).[7]

Informal caregiving is common in the Netherlands. Around 20% of the Dutch adult population reported providing either intensive (more than 8 hours per week) and/or prolonged (more than 3 months) spells of caregiving in 2008. Around 60% of caregivers 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. Women are more likely to provide parental care, whereas men mostly provide spousal care (Oudijk et al., 2010). Focusing on parental care, we would therefore expect to find a larger effect for daughters than sons in this study.[8]

The Dutch labour market is characterised by a high participation rate, and one of the highest part-time employment rates among OECD countries (OECD, 2017a;b). Participation rates for the 35-65 age group were around 60% for both genders in 2003-2005 (Statistics Netherlands, 2017), but around 40% of it was part-time, with large gender differences (15% for men and 80% for women). For men, half of the part-time employees work 28-35 hours a week, whereas the majority of part-time working women do not work more than 20 hours. If the combination of care and paid work is problematic, employed Dutch caregivers 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).

[5] *Algemene Wet Bijzondere Ziektekosten*

[6] Transferred to the Social Support Act in 2007

[7] During the period of study, some changes were introduced in the AWBZ. In the 1990s, there were relatively long waiting times, and in the beginning of the 2000s there was intensified policy effort to shorten through budgetary expansions. In an effort to curb rising LTC costs, higher co-payments and regional budgets were introduced in 2004 and 2005 (Mot, 2010). We do not expect these policy changes to interfere with our results, as they affected everyone in the same way and will thus be differenced out between treatment and control group in the difference-in-differences framework.

[8] Caregiving tasks consist most commonly of emotional support and supervision (90%), escort for errands outside the home (90%), housework (84%), help with administrative tasks (74%), followed by personal care (39%), and nursing care (37%). Extra-residential care, where the care recipient does not live in the same household, is provided for 21 hours per week on average (de Boer and de Klerk, 2013).

3 Theoretical framework

In the standard static labour supply model, an adult child maximises utility $U^k(\cdot)$ depending on consumption c and leisure time t^l subject to a time and budget constraint (Becker, 1965). In order to include caregiving and mental health in this model, we add parental utility $U^p(\cdot)$ and own health $h^k(\cdot)$ (Johnson and Lo Sasso, 2000).[9] Adding parental utility implies that adult children provide care to their parents out of altruistic motives. Parental utility is increasing in parental health h^p, care from the child t^c and others t^o such as the state or family members. Including own health in the utility function allows for a potential mental health channel of a parental health degradation. Health of the child depends on leisure time and parental health.[10]

The child's consumption is constrained by income from wages. All income is consumed, as there is no option for saving. Time available is normalised to 1 and divided into working t^w, caregiving t^c and leisure time t^l. All time variables are assumed to be non-negative.[11]

$$\begin{aligned} \max_{c,t^w,t^c,t^l} \quad & U^k(c,t^w,t^c,t^l;t^o) = u(c) + v(t^l) + U^p(h^p,t^c,t^o) + h^k(h^p,t^l) \\ \text{s.t.} \quad & wt^w \geq c \\ & t^w + t^l + t^c \leq 1 \end{aligned}$$

$$w = \frac{v_{t^w} + h^k_{t^w}}{u_c} \tag{1}$$

$$v_{t^c} = U^p_{t^c} - h^k_{t^c} \tag{2}$$

Solving the first order conditions gives rise to two equilibrium relations (Equation 1 and 2), where for example v_{t^c} denotes the first derivative of $v(\cdot)$ with respect to t^c. The wage is equal to the marginal rate of substitution between leisure and consumption plus the marginal rate of substitution between health and consumption. The first part is in line with the standard results of the labour supply model, whereas the second part derives from the positive dependence of child health on leisure time. Equation 2 shows the added dimension of optimal time allocation because of caregiving. In equilibrium, marginal utility of leisure equals marginal utility of caregiving. The latter consists of two parts: the marginal utility of improving parental utility via caregiving, and the marginal disutility of decreasing own health due to caregiving.

[9] Utility $U^k(\cdot)$ is separable in consumption $u(\cdot)$, leisure $v(\cdot)$, parental utility $U^p(\cdot)$, and own health $h^k(\cdot)$. The functions $u(\cdot)$, $v(\cdot)$, $U^p(\cdot)$, $h_k(\cdot)$ are all strictly increasing and concave.

[10] Empirically, for example Bobinac et al. (2010) and Byrne et al. (2009) find indeed that good parental health status is positively associated with reported life satisfaction for caregivers, whereas care tasks have a negative impact.

[11] In addition to the time scarcity, most static models about informal caregiving include an option for substitution between caregiving and buying care on the market (e.g. Pestieau and Sato, 2008). For the Netherlands, formal home care is mostly provided through the public LTC insurance, so the model does not contain care bought on the market.

When parental health degrades due to a health shock, the model predicts that the child provides care. If an individual provides care, leisure time is reduced. To compensate for this loss, working hours will be reduced as well due to decreasing marginal utility. The wage rate is exogenous in this model, and therefore not affected by a parental health degradation; total earnings are lower when providing care because caregivers work fewer hours. Hence, based on this model, we expect to find a negative effect of a parental health shock on both employment and total earnings if care from other sources does not crowd out own caregiving.

The model also provides a useful framework to predict which subgroups of the population are most likely to provide care. First, if the amount of care from other sources is high, the marginal benefit of own care (U^p_{tc}) is reduced. Thus, individuals who are living alone, or have alone living parents, and only children are expected to provide more informal care than other individuals. Second, when a parent is in bad health, care demand is higher. This is the case, on average, when parents are old. Third, individuals provide more care if the time costs of caregiving are lower, i.e. when parents live close and incur low travel time costs. Fourth, time constraints imposed by labour market activities constrain the time available for caregiving. Individuals who are not employed are expected to provide more care.

4 Data

The study population consists of the entire Dutch non-institutionalised population aged 35-65 between 1999 and 2011, with at least one parent still alive.[12] We use quarterly data from Statistics Netherlands on personal information on demographics linked to data on employment and earnings (1999-2011), hospitalisations (1995-2005), residence coordinates, and the cause of death registry.[13]

We use two labour market outcomes as dependent variables: the probability of employment and earnings conditional on employment. Employment is specified 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. The original data contains yearly earnings data and the beginning and the end date of a job. To get quarterly earnings, we compute daily earnings and multiply them by the number of days in a quarter.[14] We use a logarithmic transformation of conditional earnings.[15]

The main exposure variable of interest is an unexpected parental hospitalisation related

[12]We code all parents dead if they are 105 or older. None of these parents have experienced a health shock in the sample period.

[13]Table A1 in the Appendix gives an overview of the data sets used.

[14]The earnings definition we use is gross earnings after social security contributions. For individuals who are only employed during a part of a given quarter, we impute the potential quarterly earnings by adding the daily earnings times the number of days not employed such that the quarterly wage of an individual employed only one day of a quarter is comparable to the earnings of an individuals in the same job that worked during the whole quarter.

[15]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-differences setting. Inspection of the distribution of the log of earnings shows that they are approximately normally distributed and hence a log transformation is appropriate.

Table 1: Distribution of diagnoses for parents with a health shock

	Mothers		Fathers	
Shocktype	Frequency	%	Frequency	%
Infectious	919	2%	810	1%
Cancer	13’741	25%	15’429	27%
Nervous	3’061	5%	1’525	3%
Circulatory	10’839	19%	15’322	27%
Stroke	6’675	12%	8’585	15%
Respiratory	2’336	4%	4’008	7%
Digestive	2’627	5%	2’644	5%
Genitourinary	3’849	7%	1’192	2%
Skin	445	1%	411	1%
Musculosceletal	1’481	3%	704	1%
Injury	9’826	18%	5’634	10%
Total	55’799	100%	56’264	100%

Distribution of grouped hospital admission diagnoses classified as unforseeable by a medical doctor for the parents of the 35-65 year old population with at least one parent alive in 1999-2005. Note that, for example, not all infectious diseases of the ICD-9CM classification are contained in the category ’infectious’, but only selected diagnoses that have been classified as unforeseeable by a medical doctor.

to a new health problem. We limit the health shock to ICD-9CM[16] diagnoses that are only treated in the hospital that an expert physician considered to be not foreseeable.[17] In addition, these hospitalisations are classified as a health shock only if the individual has not been hospitalised for the same condition in the two previous years (see also García-Gómez et al., 2015b; García-Goméz et al., 2017). While these health shocks constitute a subset of all conditions requiring informal care, they are an important subset because they occur frequently: in 2001q1 alone, around 1% of *all* mothers (11’508 women) and 1% of *all* fathers (11’816 men) were hospitalised due to such a health shock.

Table 1 shows the frequency of grouped diagnoses classified as health shocks in the population in 1999-2005 for mothers and fathers. Mothers experiencing a shock most often suffer from cancer (25%), circulatory diseases (19%) and injuries (18%). Similarly, fathers experiencing a shock are most frequently affected by cancer and circulatory diseases (27% each), and strokes (15%).

For our analysis, the parental health shock needs to be i) exogenous and ii) severe. The definition above ensures that a health shock is an unexpected onset that is treated for the first time, and thus gives us a plausibly exogenous variation in parental health. The second condition, severity, requires that the parental health shock is correlated with increased informal care demand. We do not observe informal caregiving, but the parental health shocks increase the mortality rate,[18] and other studies have shown that these diagnoses

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

[17]The full list of included conditions is available on request.

[18]Comparing parental mortality rates by treatment status in the period of analysis (1999-2008) gives a first indication of the severity of the shock. Indeed, mothers (fathers) are 40% or 0.4% point (15% or 0.2% point) more likely to die before 2008q2 if they had a health shock around 5 to 6 years before

constituting a parental health shock led to increased informal and formal care use in the Netherlands and Spain (Bakx et al., 2015b; García-Gómez et al., 2015b; Van Exel et al., 2002; Wong et al., 2010).[19]

As time-variant control variables, we use the log of age, living with a partner, and the number of children below 13. In the earnings equation, we add the number of jobs per quarter, and the tenure in the main[20] 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 caregiving. All the analyses are done separately by gender, as women are likely to react stronger to a parental health shock than men due to gender norms.

Table 2 and 3 show summary statistics of these variables. It can be seen that our sample consists of working population individuals aged 44 years on average, whereas their parents are in their seventies. Hence, our data includes old parents who potentially need care, and working age individuals who could experience labour market effects after a parental health shock.

In addition to the main sample, we use eight subsamples for which either informal caregiving is more prevalent and/or we expect a different effect than for the overall population based on our theoretical model. First, we use a subsample of nearby living parents, with children living in a 5km radius from their father and mother, since the probability of providing informal care is decreasing in the distance to parents' place of residence. Second, we condition on being employed one year before the health shock. Having a stable job may discourage people from providing care, which would result in a weaker effect than for the overall population. Third, we look at individuals not employed one year before the parental health shock. They may be more likely to provide care since they have no time constraints from a paid job. Fourth, we restrict the sample to parents aged 80 and older, whose children are expected to face a larger care demand compared to individuals with younger parents. Fifth, we limit the sample to only children, so as to exclude situations where care may be provided by siblings. Our sixth subsample consists of alone living children, as they do not have a partner who could provide care instead. Seventh, we look at alone living parents, whose children face a higher care demand as there is no other parent who could provide care. Lastly, we combine some of the above to only-children with alone and close-living parents, which is the subgroup for which we expect the largest effect. If not indicated differently, the subsamples are chosen on characteristics at the time of the parental health shock.

5 Empirical strategy

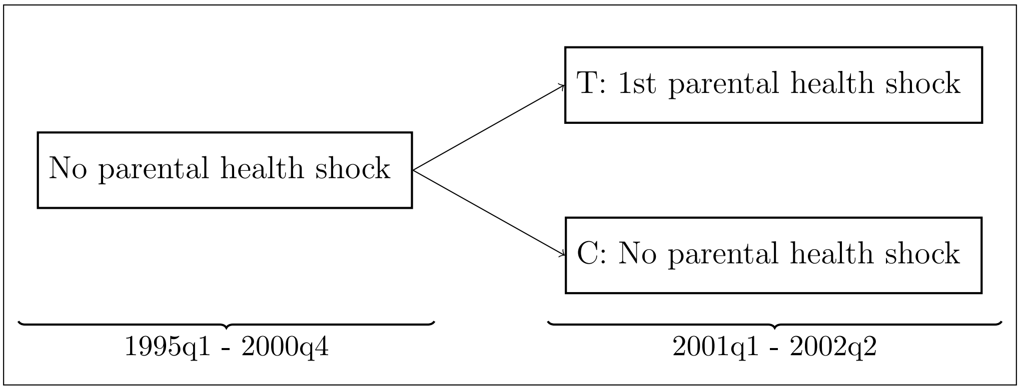
In order to evaluate the effect of a parental health shock on the probability of employment and conditional earnings, we rely on a difference-in-differences model over multiple treatment periods combined with coarsened exact matching (CEM) (Jeon and Pohl, 2017). Many studies about the labour market effects of informal care provision thus far have

(significant at 1%). The mortality difference also stays significant when controlling for observables such as age, migration background, living with a partner, and time fixed effects.

[19] For a more detailed discussion, see Appendix (8.2).

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

Figure 1: Timing of the parental health shock and treatment and control group allocation



concentrated on the immediate effect of caregiving. However, prior research taking a 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). We therefore follow labour market outcomes from 8 quarters before until 24 quarters after a health shock.

5.1 Selection of the treatment and control group

We start by excluding observations with an unexpected parental hospitalisation between 1995q1 and 2001q2 to make the sample more homogeneous. Moreover, this avoids relapses of pre-existing conditions and thus reinforces unexpectedness of the parental health shock. Figure 1 depicts how individuals experiencing a first parental health shock are selected in the sample and attributed to the treatment (T) and control (C) group. The treatment group consists of individuals experiencing a parental health shock between 2001q1 and 2002q2.[21] This selection allows to test at least 8 quarters of pre-treatment trends in labour market outcomes (employment and earnings only available since 1999). 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 2002q2.

In order to link control individuals to the treated individual for each of six treatment cohorts, every observation in the control group is duplicated six times (Jeon and Pohl, 2017). For computational reasons, we then draw a random subsample of controls.[22] Individuals exit the sample at different points in time if both parents die, upon reaching retirement age, or the death of the parent experiencing the health shock.[23] Therefore, each cohort of treatment and control group is an unbalanced panel.

[21] In a robustness check, we shift the treatment period to three other instances. The results are stable across different treatment periods (Figure A12, A13 and A14 in the Appendix).

[22] The study sample contains all treated and a clustered random sample of twice as many control individuals. The unit of the clustering is the family, so that siblings are not separated. In Section (6.3) we provide evidence that our results are not driven by this particular random sample of controls.

[23] 82% of the sample is observed for the full 33 quarters.

Table 2: Women - summary statistics treatment and control group

	Control		Treatment			
Variable	Unweighted Mean	Weighted Mean	Unweighted Mean	Weighted Mean	Unweighted StdDiff	Weighted StdDiff
Employed	0.6	0.6	0.6	0.6	-0.039	0.004
Employed$_{q-4}$	0.6	0.6	0.6	0.6	-0.040	0.001
Earnings	5,579	5,573	5,568	5,5456	0.001	0.003
Earnings$_{q-4}$	5,233	5,239	5,455	5,282	-0.010	-0.005
Age	44.3	43.8	44.1	44.1	0.026	-0.040
Age mother	73.0	72.6	72.8	72.8	0.019	-0.024
Age father	75.7	75.3	75.4	75.4	0.028	-0.011
Living with a partner	0.8	0.8	0.8	0.8	-0.027	-0.000
Dutch	0.9	0.9	0.9	0.9	-0.039	-0.000
1st generation migrant	0.04	0.02	0.02	0.02	0.047	0.005
2nd generation migrant	0.06	0.05	0.06	0.05	0.013	-0.004
Number of siblings	1.3	1.3	1.3	1.3	-0.025	-0.001
Number of kids <13	0.5	0.5	0.5	0.5	-0.018	0.003
Father has partner	0.5	0.7	0.7	0.7	-0.208	-0.000
Mother has partner	0.5	0.7	0.7	0.7	-0.199	-0.000
Distance residence mother in km	25.3	26.0	25.7	25.7	-0.006	0.004
Distance residence father in km	27.1	27.3	26.9	26.7	0.004	0.010
Number of jobs	1.1	1.1	1.1	1.1	-0.008	-0.004
Quarters employed in the main job	33.6	33.2	33.1	33.3	0.010	-0.004
Distance to closest parent	24.9	25.7	25.3	25.3	-0.007	0.006
One parent dead	0.01	0.01	0.01	0.01	0.047	0.000
Age oldest parent	76.0	75.6	75.7	75.7	0.028	-0.012
N	99,234	90,479	50,124	49,007		

* StdDiff > 0.25 (Imbens and Wooldridge, 2009). Standardised difference one quarter before the parental health shock StdDiff=$\frac{\bar{X}_{C,-1}-\bar{X}_{T,-1}}{(\hat{\sigma}^2_{C,-1}+\hat{\sigma}^2_{T,-1})^{0.5}}$ where $\bar{X}_{C,-1}$ corresponds to the mean of variable X of the control group in the quarter before the shock, and $\hat{\sigma}^2$ to the estimated variance. Earnings, the number of jobs and the tenure in the main job are only considered for the employed.

Table 3: Men - summary statistics treatment and control group

	Control		Treatment			
Variable	Unweighted Mean	Weighted Mean	Unweighted Mean	Weighted Mean	Unweighted StdDiff	Weighted StdDiff
Employed	0.8	0.8	0.8	0.8	-0.026	0.005
Employed$_{q-4}$	0.8	0.8	0.8	0.8	-0.026	-0.002
Earnings	10,348	10,172	10,560	10,333	-0.004	-0.002
Earnings$_{q-4}$	9,636	9,552	9,950	9'768	-0.007	-0.015
Age	44.4	44.0	44.3	44.3	0.018	-0.044
Age mother	73.0	72.6	72.8	72.8	0.014	-0.028
Age father	75.7	75.3	75.4	75.4	0.028	-0.010
Living with a partner	0.7	0.8	0.8	0.8	-0.026	-0.000
Dutch	0.9	0.9	0.9	0.9	-0.057	-0.000
1st generation migrant	0.04	0.03	0.03	0.02	0.069	0.020
2nd generation migrant	0.06	0.05	0.06	0.05	0.017	-0.014
Number of siblings	1.3	1.3	1.3	1.3	-0.023	-0.001
Number of kids <13	0.7	0.8	0.7	0.7	-0.016	0.005
Father has partner	0.5	0.7	0.7	0.7	-0.208	-0.000
Mother has partner	0.5	0.7	0.7	0.7	-0.197	-0.000
Distance residence mother in km	23.0	23.6	23.9	23.7	-0.014	-0.002
Distance residence father in km	24.4	24.8	25.1	24.9	-0.012	-0.002
Number of jobs	1.1	1.1	1.1	1.1	0.011	0.007
Quarters employed in the main job	39.4	39.1	39.1	39.3	0.007	-0.004
Distance to closest parent	22.6	23.3	23.5	23.4	-0.016	-0.001
One parent dead	0.01	0.01	0.01	0.01	0.047	0.000
Age oldest parent	76.0	75.6	75.7	75.8	0.027	-0.011
N	121,270	111,587	61,334	60,203		

* StdDiff > 0.25 (Imbens and Wooldridge, 2009). Standardised difference one quarter before the parental health shock StdDiff=$\frac{\bar{X}_{C,-1}-\bar{X}_{T,-1}}{(\hat{\sigma}^2_{C,-1}+\hat{\sigma}^2_{T,-1})^{0.5}}$ where $\bar{X}_{C,-1}$ corresponds to the mean of variable X of the control group in the quarter before the shock, and $\hat{\sigma}^2$ to the estimated variance. Earnings, the number of jobs and the tenure in the main job are only considered for the employed.

5.2 Coarsened exact matching (CEM)

It is possible that individuals with a parental health shock are very different from the ones without a parental health shock. We therefore make treatment and control group more comparable on observables using coarsened exact matching. CEM is an exact matching algorithm that splits the data into strata according to all possible combinations of pre-imposed bins of observables. For every stratum l, weights w_l are calculated that balance the empirical distribution of the matching variables between the treated and the controls.[24] Individuals who cannot be matched receive weight zero.

We use CEM instead of propensity score matching since for our large data set, the curse of dimensionality is less of a problem than for smaller survey data sets while CEM has two main advantages over propensity score matching. First, there is no need for ex-post balance checking as the maximal acceptable imbalance is decided beforehand by imposing the bins in which the observations are matched. Moreover, the validity of CEM does not rely on a correct functional form specification of the propensity score and never increases the imbalance (King and Nielsen, 2016).

The main trade-off of CEM is between internal and external validity. On the one hand, the more bins, the more accurate the match will be and the higher the internal validity. On the other hand, a greater number of bins decreases the probability of finding a match for the treated, thus lowering external validity. Our compromise to this trade-off is as follows. We use coarsening bins based on the age of the oldest parent (cut-offs at 65,73,80,90), the number of siblings (cut-offs at 0,1,2, and 3), the number of kids below 13 (cut-off at 0), Dutch citizenship, an indicator if one parent has passed away, and the minimum distance to mother and father (cut-off at 5 and 50 km and missing[25]) one quarter before treatment. Moreover, we add the pre-treatment mean over two years of employment (cut-off at 0.2, 0.8, 1) and wage quintiles to match also on pre-treatment labour market attachment. We have 3800 bins for each gender and lose 1-2% of our treated individuals since no match could be found for them.[26] Given that the matched and unmatched results are fairly similar, we are confident that small loss of treated individuals does not affect the external validity of our results.

The effect of the CEM weighting on the pre-treatment summary statistics can be seen in Table 2 for women and 3 for men. The weighting does not affect the difference between the means one period before the shock for the control group (column 1 and 2) and the treatment group (column 3 and 4) very much. Nonetheless, the weighting does bring treatment and control groups closer to one another. This is illustrated by column 5 and 6, where the standardised differences in the means between treatment and control group are shown. Imbens and Wooldridge (2009) suggests the rule of thumb that a standardised difference should be below 0.25 to ensure that the linear regression methods are not sensitive to the model specification. In our unweighted sample, the standardised differences in means are all well below 0.25, except for the indicator whether the father

[24] All treated individuals received $w_l = 1$. Control individuals receive $w_l = \frac{N_{C,tot} N_{T,l}}{N_{T,tot} N_{C,l}}$ where $N_{C,tot}$ is the total number of control individuals and $N_{T,l}$ the number of treated individuals in strata l.

[25] The address data is missing for certain individuals for unknown reasons. In order not to lose the observations with missing distance measure, 'missing' is added as a coarsened category to this variable

[26] For women, 3065 bins contain at least one observation, out of which 648 bins containing treated women that could not be matched. These unmachted treated bins contain around 1.7 women on average (as opposed to 20.3 treated women per matched bin on average).

or mother has a partner, which comes close to 0.25 (-0.21/-0.2 for both women and men). This is addressed in the weighted sample, where the standardised differences for these two variables approach 0 for both genders. The similarity between the weighted and unweighted sample gives additional support to the exogeneity of our parental health shock.

5.3 Difference in differences

We use a difference-in-differences model to follow every cohort of treated and controls over time and average this effect over the six cohorts (Hijzen et al., 2010; Jeon and Pohl, 2017). We define an indicator of how many quarters an individual is away from a health shock q_{it}^k with $k \in [-8, 24]$ with zero indicating the quarter in which the shock occurs. For the control group, this variable is coded according to the corresponding treated individuals in the attached treatment cohort. The treatment group is designated by D_i.

$$y_{it} = \alpha_i + \alpha_t + \sum_{k=-7}^{24} \gamma^k q_{it}^k + \sum_{k=-7}^{24} \beta^k D_i q_{it}^k + \delta x_{it} + \varepsilon_{it} \tag{3}$$

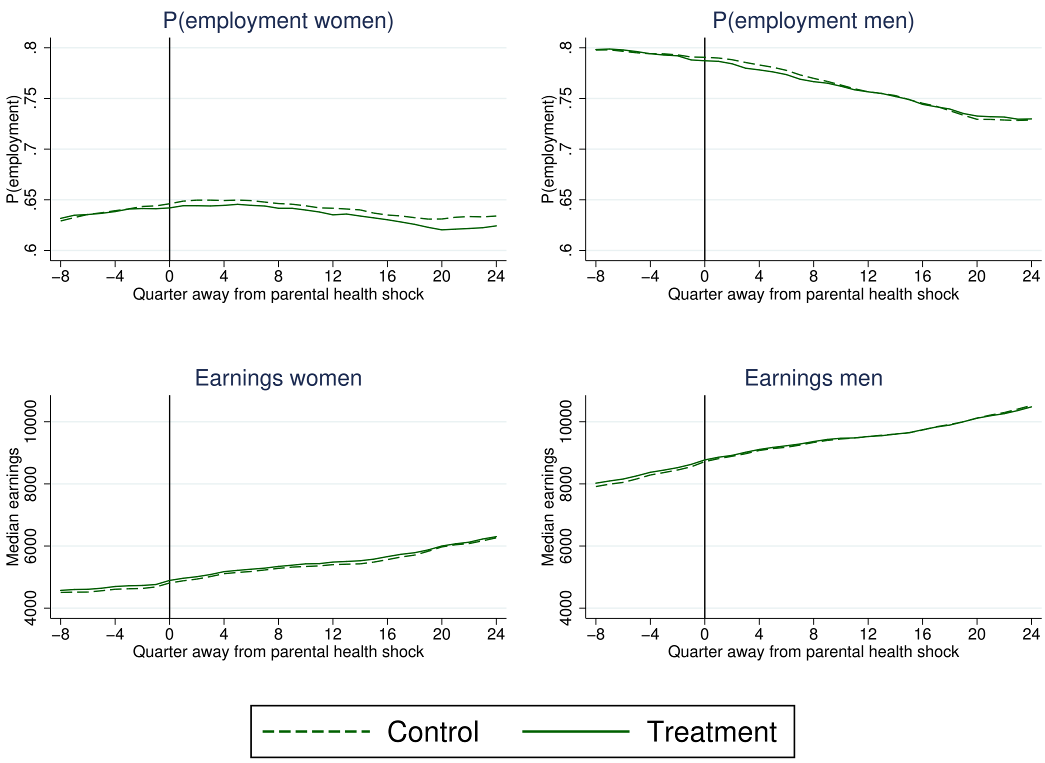
Equation (3) is estimated using the within transformation plus CEM weighted least squares for the probability of employment and log conditional earnings. The first sum of Equation (3) captures the common time trends of treatment and control before and after the health shock. The second sum is the difference in difference term, with coefficients of interest $\beta^0, ...\beta^{24}$. 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. We cluster the error term on sibling level because they are affected by the same parental health shock (Abadie et al., 2017).[27]

The identifying assumption of a difference-in-differences approach is the common trend assumption, implying that the treatment and control group would have had the same trend had the treatment not occurred. A violation of the assumption could occur if a parent suffering from a chronic illness in t is more likely to experience a health shock in the future $t + m$. Therefore, if the health shock is a symptom for overall health deterioration, the underlying parental health distributions may not be the same for the treatment and the control group. This could imply that the informal care demand – and thus labour supply – evolves differently for the treatment and the control group over time.

Directly testing for the evolution of parental health is not possible (cf. Fadlon and Nielsen, 2015; García-Gómez et al., 2013), but the inspection of raw employment and earnings trends by group before the health shock shows if this is indeed a problem. Figure 2 shows the CEM weighted employment proportions and earnings median trends in the 8 quarters before and 24 quarters after the parental health shock. Employment proportions are declining over time, whereas nominal earnings are increasing for both genders as the study sample becomes older. Men earn more than women, and they are also more likely to be employed. The pre-trends seem to be fairly similar between treatment and control group, except for female employment two quarters before the shock. Treated

[27] Our conclusions are robust to clustering the standard errors at individual level. Results are available on request.

Figure 2: CEM weighted employment and earnings trends



women are less likely to work after the health shock has occurred; conditional median female earnings are virtually identical between treatment and control group. For men, there does not seem to be a difference, neither in employment nor in conditional earnings trends, between treatment and control group.

More formally, potential pre-treatment differences in trends can be detected through t-tests for significance of $\beta^{-7}, ...\beta^{-1}$. If pre-treatment 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.

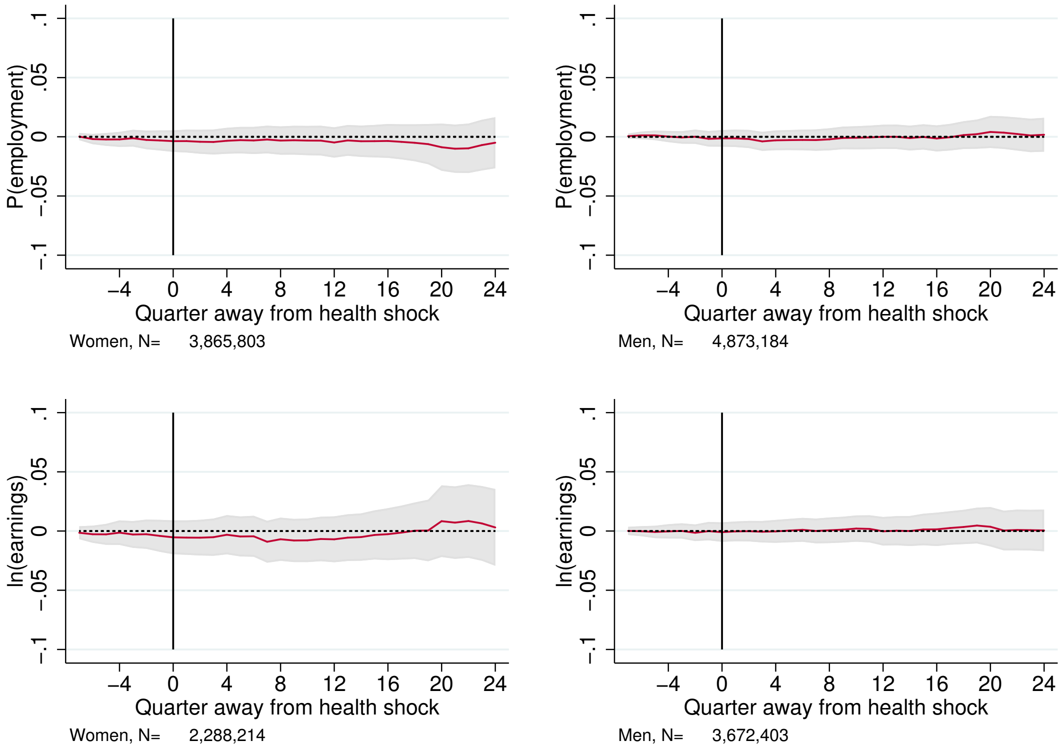
6 Results

6.1 CEM weighted Difference-in-Difference

In Figure 3, we plot the CEM weighted coefficients of the difference-in-differences term β^k and their 95% Bonferroni adjusted[28] confidence interval for the probability of employment and conditional log earnings by gender. The leads of the parental health shock are

[28] We always report Bonferroni adjusted statistical significance, since we conduct simultaneous t-tests (Armstrong, 2014) and would therefore expect some significant results due to chance. The Bonferroni correction adjusts our significance levels as following: Significance at 10% needs a p-value below 0.0031, 5% 0.0016 and for 1% 0.0003 respectively. Our conclusion does not change if we do not use the Bonferroni adjustment.

Figure 3: Earnings and employment effects of a parental health shock



The grey shaded areas correspond to the Bonferroni adjusted 95% confidence intervales.

not significant in any of the specifications. The common trend assumption thus seems reasonable.

The main result from the difference-in-differences analyses is that a parental hospitalisation does not have any effect on short run or long-run labour market outcomes for men and women. This finding is consistent over multiple at-risk caregiver subsamples (as explained in the next subsection) and other robustness checks. Moreover, the estimated coefficients are very close to zero, so even if they had been statistically significant at some p level, they would not be regarded as economically significant.

The Bonferroni correction does not come at a large price in terms of power. We find from the F-test that all difference-in-differences terms are jointly equal to zero with a Bonferroni adjusted significance level at 10% and given our sample size, the power of our test lies between 90 and 99% for both genders and labour market outcomes (Cohen, 1988). Even at a corrected significance level of 1%, the lowest power we get is 77%. Hence, our results are indeed a zero effect and not due to a lack of power.

Table 4: Subsamples

	CEM	Close parents	Employed	Not employed	Old parents	Only children	Single children	Single parent	Combination
k	Women employment								
-4	-0.002	0.002	-0.001	-0.006	-0.001	-0.004	-0.005	-0.004	-0.022
	(0.002)	(0.004)	(0.002)	(0.003)	(0.005)	(0.004)	(0.004)	(0.004)	(0.019)
8	-0.003	0.001	0.002	-0.001	-0.007	0.001	-0.003	-0.011	0.016
	(0.004)	(0.007)	(0.003)	(0.006)	(0.010)	(0.008)	(0.009)	(0.007)	(0.045)
N	3,865,803	1,207,521	1,849,663	1,428,720	536,253	892,084	829,778	1,388,389	34,499
k	Women earnings								
-4	-0.001	-0.002	-0.001	n.a.	0.002	-0.000	0.007	0.001	0.061
	(0.004)	(0.006)	(0.004)		(0.009)	(0.007)	(0.007)	(0.006)	(0.038)
8	-0.007	-0.004	-0.004	-0.066	0.004	-0.011	-0.002	-0.013	0.012
	(0.006)	(0.011)	(0.006)	(0.090)	(0.017)	(0.013)	(0.012)	(0.011)	(0.068)
N	2,288,214	720,483	1,830,930	137,080	258,251	507,365	487,097	768,818	17,732
k	Men employment								
-4	0.000	0.001	-0.000	-0.004	0.005	-0.001	-0.000	-0.000	-0.012
	(0.002)	(0.002)	(0.001)	(0.005)	(0.005)	(0.003)	(0.004)	(0.003)	(0.015)
8	-0.002	0.001	-0.000	0.000	-0.002	-0.008	-0.006	-0.007	0.004
	(0.003)	(0.005)	(0.002)	(0.007)	(0.010)	(0.006)	(0.007)	(0.005)	(0.026)
N	4,873,184	1,669,092	3,050,672	1,011,100	685,231	1,166,456	1,054,058	1,785,781	54,384
k	Men earnings								
-4	-0.000	0.002	-0.000	n.a.	0.002	-0.002	-0.004	-0.003	0.018
	(0.002)	(0.003)	(0.002)		(0.005)	(0.004)	(0.006)	(0.003)	(0.014)
8	0.001	0.003	0.001	0.188	-0.008	0.003	-0.006	-0.003	-0.013
	(0.004)	(0.006)	(0.003)	(0.094)	(0.012)	(0.008)	(0.010)	(0.006)	(0.034)
N	3,672,403	1,272,607	3,022,481	74,780	451,300	859,251	628,341	1,277,225	37,463

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$ with Bonferroni adjustment for multiple testing. Difference-in-difference coefficients for k quarter away from the shock and their standard error in parenthesis. For the not employed, $k = -4$ is not applicable, as nobody has a wage 4 quarters before the health shock in this subsample. A more detailed definition of the subsamples can be found in Section (4).

6.2 Subsamples

Our main study population (i.e. the population of the Netherlands) might contain too many individuals who would never provide care (or too many parents who do not need it) to detect an effect. Therefore, we conduct the same analysis for subsamples of at-risk caregivers: parents living close by, children employed one year before the parental health shock, children not employed one year before the parental health shock, parents aged 80 and older, only children, alone living children, children of alone living parents, and only-children with alone but close-living parents. Table 4 shows the coefficient of the difference-in-differences term one year before the parental hospitalisation (as an indication for common trends, $k = -4$) and the coefficient of two years after the parental hospitalisation ($k = 8$) for both the main results (column 'CEM') and these subsamples. Appendix Figures A2-A9 contain a graphical representation of all coefficients. Overall, the at-risk caregiver subsamples confirm the picture painted by the main specification. There is no effect of an unexpected parental health shock on the probability of employment and conditional earnings.

6.3 Robustness checks

We check the robustness of our main findings in Table 5. Again, the coefficient of the difference-in-differences term one year before the parental hospitalisation (as an indication for common trends, $k = -4$) and the coefficient of two years after the parental hospitalisation ($k = 8$) are reported in the Table, whereas complete graphical evidence can be found in the Appendix (Figure A10-A15). The first column shows the main results (labeled 'CEM') for ease of comparison. The first robustness check shows that if we do not use the CEM weighting (column 2 'No CEM'), the results are similar to the weighted results.

In column 3, we limit the potential effect of a parental health shock on labour market outcomes to 10-15 quarters depending on the cohort of the shock. This enables us to choose as control group only the individuals who experienced a parental health shock in 2005, in the spirit of Fadlon and Nielsen (2015).[29] This should make control individuals more comparable to the treated and thus increase internal validity. The downside of this approach is a decrease in external validity, since we are not looking at the population as a whole anymore. Also in this specification, the null results are confirmed.

Furthermore, we check if our selection of treatment period affects our results by redefining the treatment group as individuals with a parental health shock in 2003q1 - 2004q2, 2004q1-2005q2, and 2004q3-2005q4 ('Shift T I-III'). There is also no effect of a parental hospitalisation on labour market outcomes in these three different treatment groups.

In column (7) 'No hosp.', we limit our sample to individuals with no parental hospitalisation in the period 1995q1-2000q4, be it unexpected or any other potentially foreseeable hospitalisation. This is the furthest we can go in order to force common parental health

[29]Concentrating only on individuals with a future parental health shock as controls reduces the sample considerably. This enables us to conduct the analysis on the whole population instead of all treated individuals and a random subsample of controls, resulting in a higher number of observations than in the main specification.

Table 5: Robustness checks

	(1) CEM	(2) No CEM	(3) Control gr.	(4) Shift T I	(5) Shift T II	(6) Shift T III	(7) No hosp.
k	Women employment						
-4	-0.002	-0.001	-0.003	0.001	-0.000	-0.001	-0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
8	-0.003	-0.002	-0.005	0.002	-0.000	0.002	0.002
	(0.004)	(0.004)	(0.003)	(0.003)	(0.004)	(0.004)	(0.005)
N	3,865,803	4,129,528	5,092,595	5,215,318	5,789,632	5,202,417	2,453,594
k	Women earnings						
-4	-0.001	-0.000	-0.002	0.001	0.001	0.001	0.003
	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)	(0.002)	(0.004)
8	-0.007	-0.009	-0.003	0.003	-0.004	-0.008	0.006
	(0.006)	(0.006)	(0.005)	(0.005)	(0.005)	(0.006)	(0.008)
N	2,288,214	2,571,776	3,107,501	3,158,760	3,533,378	3,181,145	1,451,998
k	Men employment						
-4	0.000	0.001	0.000	-0.000	0.001	-0.001	0.000
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
8	-0.002	0.004	-0.004	0.001	0.001	-0.001	0.001
	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)	(0.004)
N	4,873,184	5,173,397	6,247,276	6,472,741	7,112,205	6,391,470	3,086,735
k	Men earnings						
-4	-0.000	0.001	-0.001	-0.002	-0.002	-0.001	0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
8	0.001	0.004	-0.005	-0.002	-0.001	-0.003	0.002
	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)	(0.005)
N	3,672,403	3,941,241	4,786,944	4,895,123	5,364,877	4,807,022	2,333,659

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$ with Bonferroni adjustment for multiple testing. Difference-in-difference coefficients for k quarter away from the shock and their standard errors in parenthesis are displayed. (1) CEM: baseline results using CEM weighting for comparison. (2) No CEM: baseline results not using weights. (3) Control gr.: Control group only includes individuals with a future health shock. Based on the population and not on a random sample. (4) Shift T I: Treatment period shifted to 2003q1 - 2004q2. (5) Shift T II: Treatment period shifted to 2004q1-2005q2. (6) Shift T III: Treatment period shifted to 2004q3-2005q4. (7) No hosp: No parental hospitalisation from 1995q1-2001q1.

trends with the data available. This reduces the sample considerably, since parental hospitalisations are a frequent phenomenon. The results are again very similar to our main results, providing evidence that potential remaining differences in underlying parental health between treatment and control group do not influence our results.

Finally, we verify whether the random sample that we draw leads to similar result as with other random samples. We have conducted the main analysis also on 99 other clustered random subsamples. The pre-treatment effects are jointly significant 4 times out of a 100, whereas the treatment effects are never significant. All coefficients contain zero between the 5th and 95th percentile of their distribution as illustrated by Figure A1 in the Appendix. We are therefore confident that our results are not sensitive to the random sample we have selected.

In sum, these robustness tests confirm that our main finding of no effect of a parental health shock on the labour market outcomes of their children is robust to a series of additional tests.

7 Conclusion and discussion

Health shocks occur frequently and may not only have a severe and lasting effect on the labour market status of the patients, but also on the labour supply decisions of their working-age family members because they may care for - and care about - the patient. As these health shocks are most frequent in old age, labor supply effects may be the most frequent for their middle-aged children, who are an important source of informal caregiving. These labour market effects are undesirable if they cause unavoidable financial uncertainty for the caregivers.

Our study exploits parental hospitalisations classified by physician expert opinion as unexpected to evaluate their effect on the probability of employment and conditional earnings of adult children. While these health shocks cannot capture all care needs, especially not those related to slowly deteriorating chronic conditions like e.g. dementia, they are still correlated with formal and informal LTC use and thus relevant. We estimate a difference-in-differences model over multiple treatment cohorts and combine it with coarsened exact matching. The main findings show that there is no effect of an unexpected parental health shock on the probability of employment and conditional earnings. The analysis of subsamples, for whom we expected the effects to be particularly large, do not show any effects either. Given the large sample size, these results are very precisely estimated and are not due to lack of power. Various robustness tests confirm our findings.

The results indicate that there is no effect of a parental health shock on informal care provision and/or no labour market effect of informal care giving. We cannot test the first element of the causal chain - the effect of parental hospitalisation on care giving - directly because informal care giving is not observed. However, it is clear from other sources that informal care giving is a common phenomenon in the Netherlands. A large part of the Dutch population indicates to be informal care givers, and 20% of the population reports to provide intensive and/or prolonged care (Oudijk et al., 2010). Moreover, other studies show that the parental hospitalisation as defined for this analysis leads to more formal care demand, and is positively related to informal care demand in Spain and the Netherlands (Bakx et al., 2015b; García-Gómez et al., 2015a; Van Exel et al., 2002; Wong et al., 2010). Therefore, it does not seem plausible that there is no effect of a parental health shock on informal care giving.

It is more plausible, however, that the labour market effect of informal care giving is absent. Prior studies combining data from the Netherlands with data from other European countries indeed do not find earnings (Bolin et al., 2008) or employment effects (Josten and De Boer, 2015; Meng, 2013; Moscarola, 2010; Van Houtven et al., 2013; Viitanen, 2010). The results then suggest that Dutch care givers do not face a trade-off between paid work and care responsibilities. One explanation for this finding may be that the Dutch formal long-term care system largely meets care needs and is readily accessed thanks to low co-payments and low waiting times (Bakx et al., 2016), which means that the demand for informal care is short-lived or low and thus may be met by the child while having a paid job. Brandt et al. (2009) shows that Europeans do not provide less informal care as the state takes over more responsibility in elderly care, but that the care giving tasks change to less intensive, less constraining and less burdensome tasks. This is also the prediction of our theoretical model, where there is no effect of a parental health shock on labour market outcomes of the children if care from other sources is high. A developed formal system implies that the marginal benefit of informal care to improve parent's health is low, leading to a low marginal utility of care giving and hence no care is provided.

What do our findings mean in a broader context? The Dutch are able to continue working even if their elderly parents need care after a hospitalisation. We interpret this as a sign that the comprehensive-yet-expensive public LTC insurance scheme in the Netherlands protects children against the risk of having to give up one's job to care for a sick parent. This interpretation is in line with a study for Norway, where the LTC system is also generous, and expansion of formal home care in 1998 had no effect on long-run employment or earnings for only-child daughters (Løken et al., 2017).

Other recent studies do underscore the fact that the labour market - caregiving trade-off does arise in systems that are not as generous as the Dutch. This can be illustrated for example by a comparison to Japan, which only spends 2.2% of its GDP on LTC, versus 3.7% in the Netherlands (OECD, 2017c). Fu et al. (2017) find that the introduction of LTC insurance in Japan in 2000 did have positive spill-over effects on labour market outcomes of informal caregivers, whereas a reduction of generosity of the insurance in 2006 had a negative effect.

Geyer and Korfhage (2017) show that the institutional background seems to play an important role in determining the existence of a labour market - caregiving trade-off. They report that the introduction of LTC insurance in Germany in 1995, which entitled the elderly to formal home care or cash benefits, had negative effects on male labour market participation, but not on female labour market participation. This somewhat counter-intuitive finding can be related to the peculiar design of the LTC system in Germany, with cash benefits at the free disposal of recipients (unlike in the Netherlands, where cash benefits are earmarked to be spent on formal or informal care only (Bakx et al., 2015a)). Geyer and Korfhage (2017) therefore hypothesize that only German men use the cash benefit as an income replacement source enabling them to retire early. Since this result seems to be linked to the specific features of the German LTC system, it does not contradict our findings.

Overall, our findings strongly indicate that in general, a trade-off between paid and care work may exist but that it may be weakened substantially by the design of the LTC system. In the Netherlands, where the LTC system is generous and comprehensive, the trade-off appears to have vanished at least for care induced by parental health shocks, and the duties of caregiving and paid work can be reconciled, leading us to conclude that Dutch adult kids are alright.

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8 Appendix

8.1 Data

Table A1: Data sets

Data set	**Version**	**Content**
PARTNERBUS	V1 2015	Partner identification
GBAPERSOONTAB	V1 2015	Basic personal data
Do	V1 1995-2011	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 1999-2011	Employment
BAANSOMMENTAB	V3 1999-2011	Earnings
MEDICIJTAB	V1 2006-2011	Prescribed drugs

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

We discuss two studies that provide insights in health determinants of informal care use in the Netherlands. First, García-Gómez et al. (2015a) 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, we compare the health shock definition with health determinants of formal LTC use provided by two studies from the Netherlands. First, Wong et al. (2010) presents the formal LTC 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 (see Table A2 in the Appendix for details). Second, Bakx et al. (2015b) examine on LTC expenditures up to three periods after a hospitalisation for different diagnosis groups. The comparison with the health shock definition shows that 32% of total LTC expenditures 3 years after a hospitalisation are caused by diagnoses we classify as health shocks.[30] High LTC expenditure could mean a nursing home admission, which does not require informal 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 almost certainly lead to a nursing home admissions.

[30] As the grouping in Bakx et al. (2015b) 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.

Table A2: 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

Table A3: Women wage summary statistics

	Control		Treatment		Control	Treatment
	Unweighted	Weighted	Unweighted	Weighted	Unweighted	Weighted
Variable	Mean	Mean	Mean	Mean	StdDiff	Diff
Age	43.3	43.1	43.5	43.5	-0.029	-0.058
Age mother	72.2	72.1	72.3	72.3	-0.016	-0.025
Age father	74.9	74.8	74.9	74.9	-0.005	-0.011
Living with a partner	0.8	0.8	0.8	0.8	-0.018	-0.001
Dutch	0.9	0.9	0.9	0.9	-0.026	0.000
1st generation migrant	0.03	0.02	0.02	0.02	0.029	-0.005
2nd generation migrant	0.06	0.06	0.06	0.06	0.011	0.003
Number of siblings	1.3	1.3	1.3	1.3	-0.022	-0.002
Number of kids <13	0.5	0.5	0.5	0.5	-0.018	-0.002
Father has partner	0.6	0.7	0.7	0.7	-0.188	-0.002
Mother has partner	0.6	0.7	0.7	0.7	-0.182	-0.002
Distance residence mother in km	26.1	26.9	26.1	26.1	0.000	0.012
Distance residence father in km	28.0	28.0	27.2	27.1	0.013	0.017
Distance to closest parent	25.6	26.5	25.8	25.8	-0.003	0.011
One parent dead	0.01	0.00	0.01	0.00	0.030	-0.001
Age oldest parent	75.2	75.1	75.2	75.2	-0.006	-0.012
N	60,601	31,960	57,259	31,602		

* StdDiff > 0.25 (Imbens and Wooldridge, 2009). Standardised difference one quarter before the parental health shock StdDiff=$\frac{\bar{X}_{C,-1}-\bar{X}_{T,-1}}{(\hat{\sigma}^2_{C,-1}+\hat{\sigma}^2_{T,-1})^{0.5}}$ where $\bar{X}_{C,-1}$ corresponds to the mean of variable X of the control group in the quarter before the shock, and $\hat{\sigma}^2$ to the estimated variance.

Table A4: Men wage summary statistics

	Control		Treatment		Control	Treatment
Variable	Unweighted Mean	Weighted Mean	Unweighted Mean	Weighted Mean	Unweighted StdDiff	Weighted StdDiff
Age	43.8	43.5	43.9	43.9	-0.014	-0.047
Age mother	72.4	72.2	72.5	72.5	-0.011	-0.028
Age father	75.1	74.9	75.1	75.1	0.000	-0.014
Living with a partner	0.8	0.8	0.8	0.8	-0.024	-0.003
Dutch	0.9	0.9	0.9	0.9	-0.053	0.002
1st generation migrant	0.04	0.03	0.02	0.02	0.061	0.018
2nd generation migrant	0.06	0.05	0.06	0.05	0.020	-0.015
Number of siblings	1.3	1.3	1.3	1.3	-0.020	-0.003
Number of kids <13	0.7	0.7	0.7	0.7	-0.016	0.002
Father has partner	0.6	0.7	0.7	0.7	-0.195	-0.001
Mother has partner	0.5	0.7	0.7	0.7	-0.186	-0.001
Distance residence mother in km	23.5	24.0	24.0	23.9	-0.008	0.001
Distance residence father in km	24.9	25.1	25.1	25.0	-0.003	0.003
Distance to closest parent	23.0	23.7	23.6	23.6	-0.010	0.002
One parent dead	0.01	0.00	0.01	0.00	0.043	-0.001
Age oldest parent	75.4	75.3	75.4	75.417	-0.000	-0.013
N	92,948	47,946	87,808	47,596		

* StdDiff > 0.25 (Imbens and Wooldridge, 2009). Standardised difference one period before the parental health shock StdDiff=$\frac{\bar{X}_{C,-1}-\bar{X}_{T,-1}}{(\hat{\sigma}^2_{C,-1}+\hat{\sigma}^2_{T,-1})^{0.5}}$ where $\bar{X}_{C,-1}$ corresponds to the mean of variable X of the control group in the shock before the shock, and $\hat{\sigma}^2$ to the estimated variance.

Figure A1: Distribution of β_{-k} from 100 different random samples

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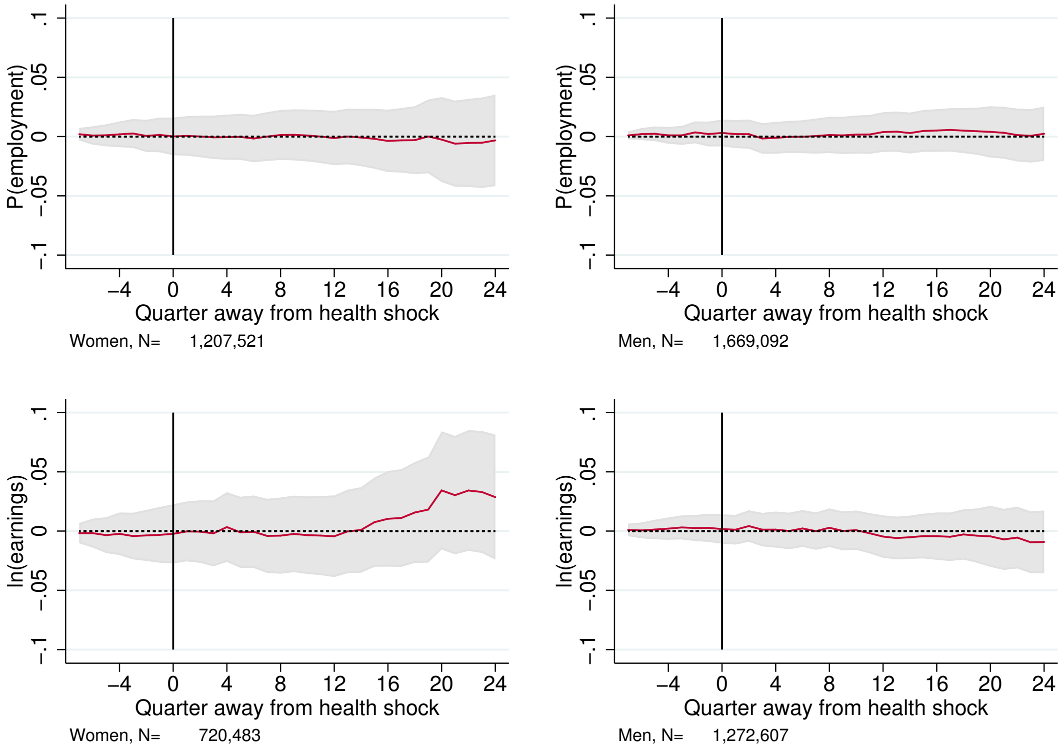
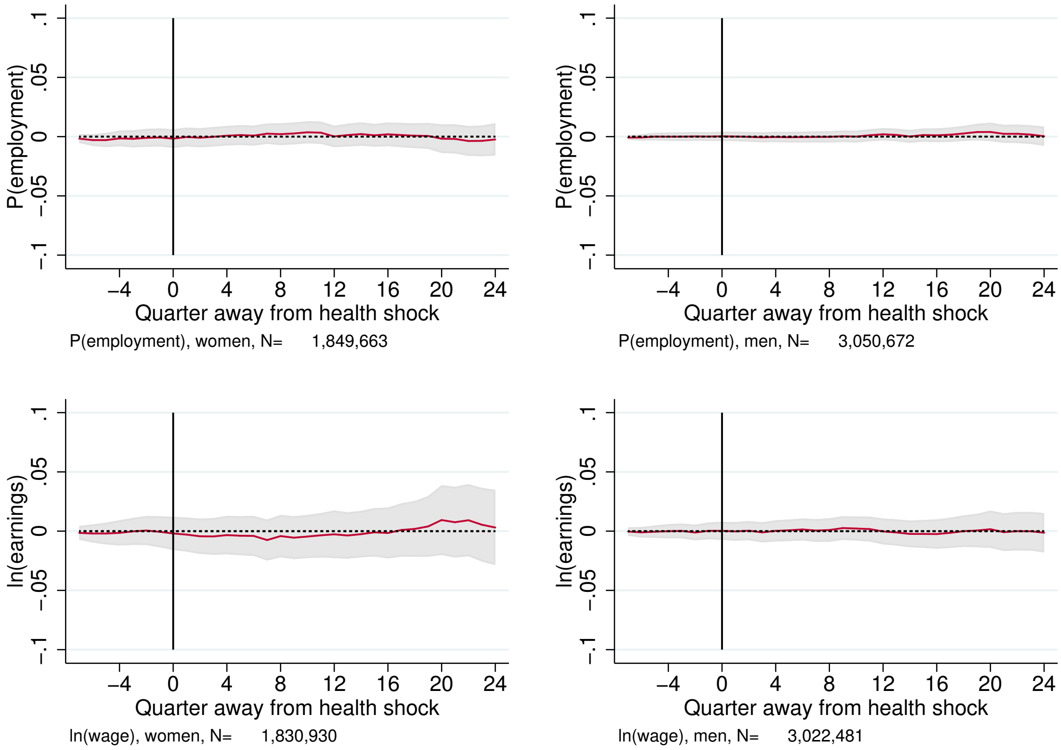
Figure A2: Close parents



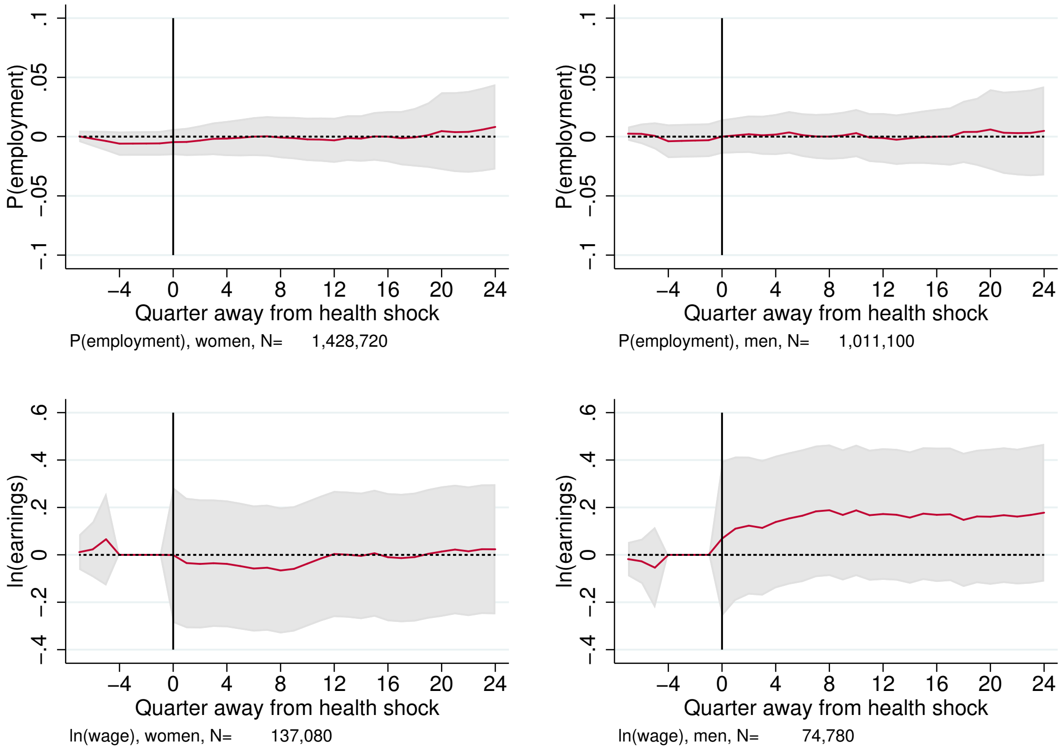
The grey shaded areas correspond to the Bonferroni adjusted 95% confidence intervales.

Figure A3: Employed 1 year before the shock



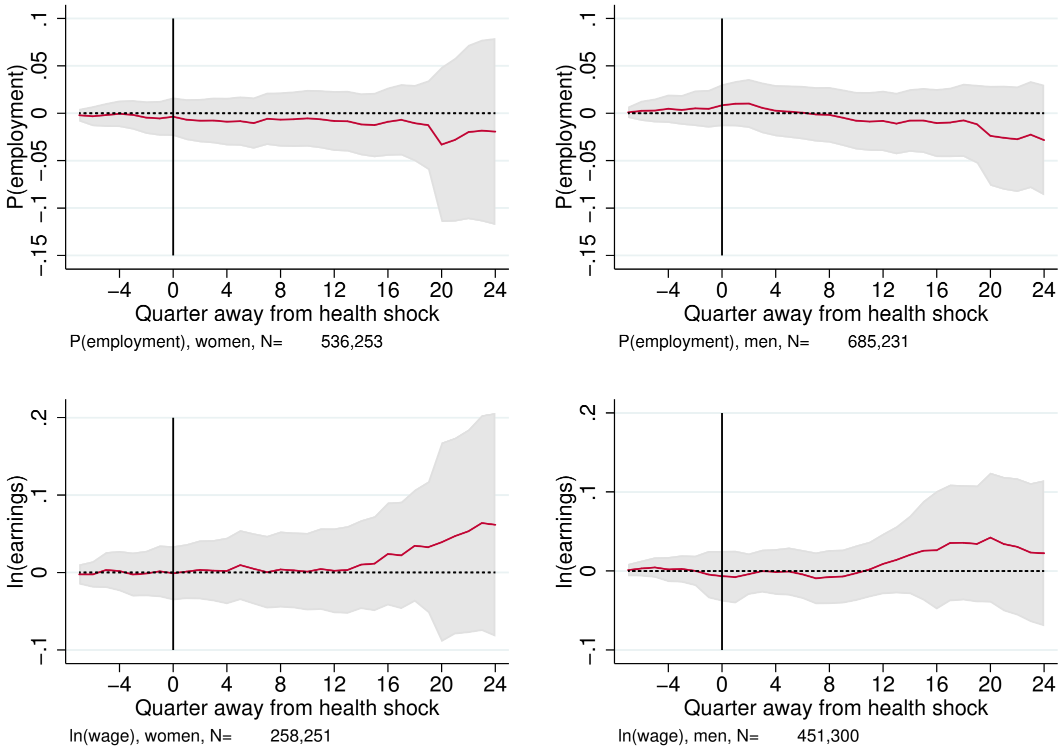
The grey shaded areas correspond to the Bonferroni adjusted 95% confidence intervales.

Figure A4: Not employed 1 year before the shock



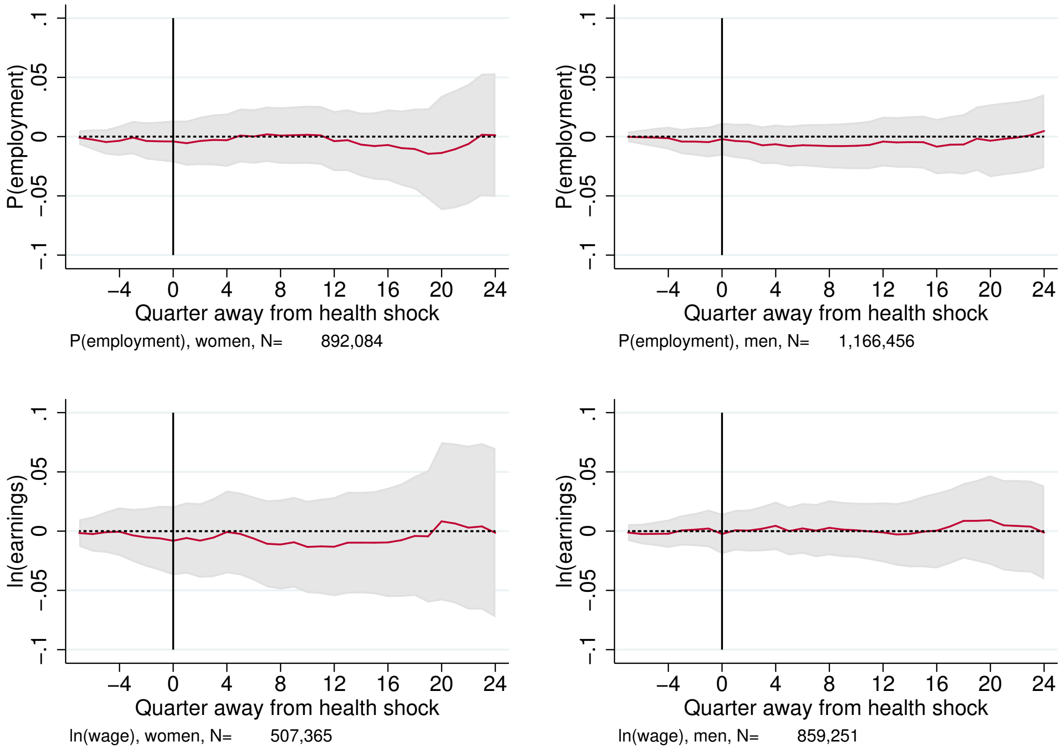
The grey shaded areas correspond to the Bonferroni adjusted 95% confidence intervales.

Figure A5: 80+ parents



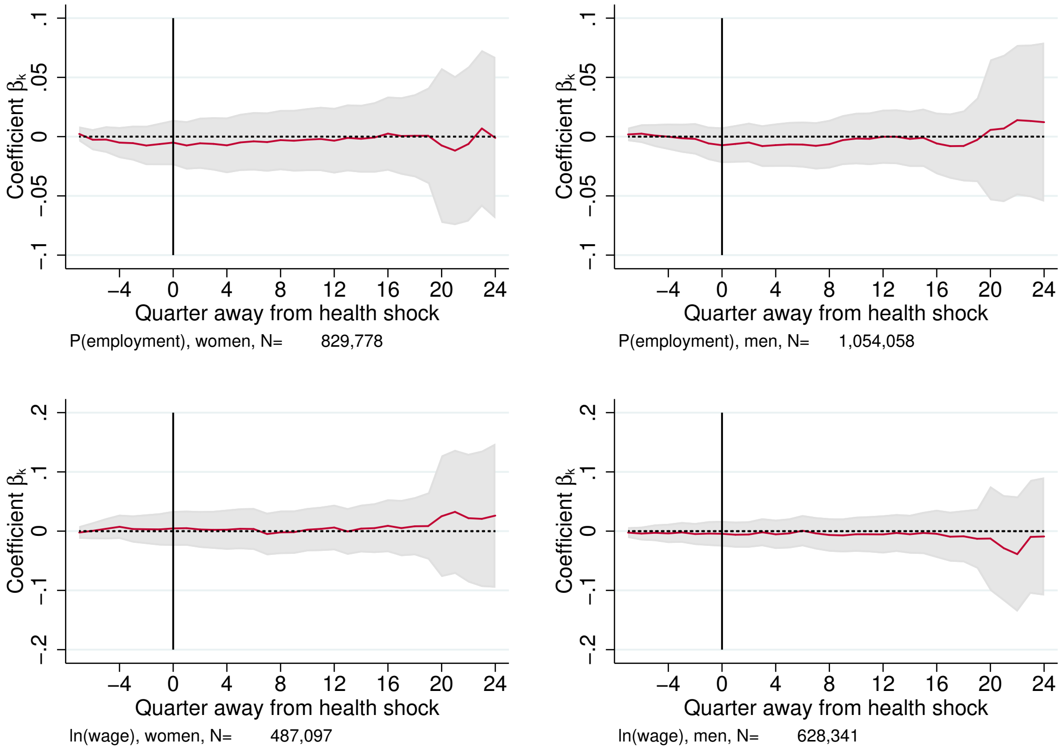
The grey shaded areas correspond to the Bonferroni adjusted 95% confidence intervales.

Figure A6: Only children



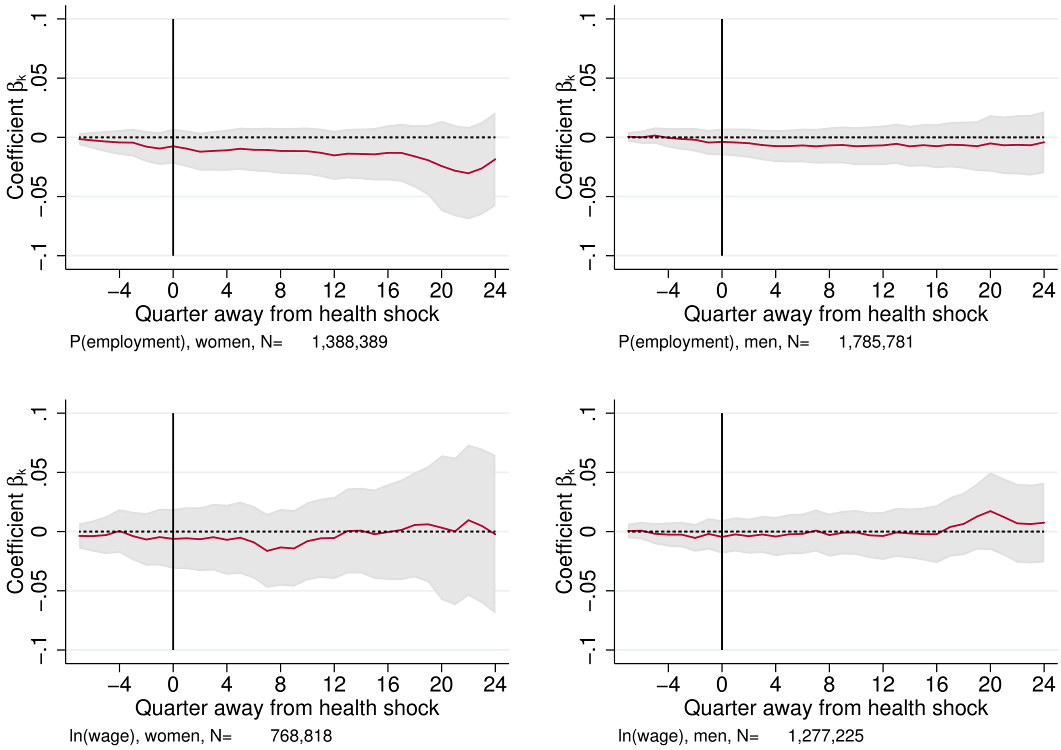
The grey shaded areas correspond to the Bonferroni adjusted 95% confidence intervales.

Figure A7: Alone living children



The grey shaded areas correspond to the Bonferroni adjusted 95% confidence intervales.

Figure A8: Alone living parents



The grey shaded areas correspond to the Bonferroni adjusted 95% confidence intervales.

Figure A9: Only children with single 80+ parent

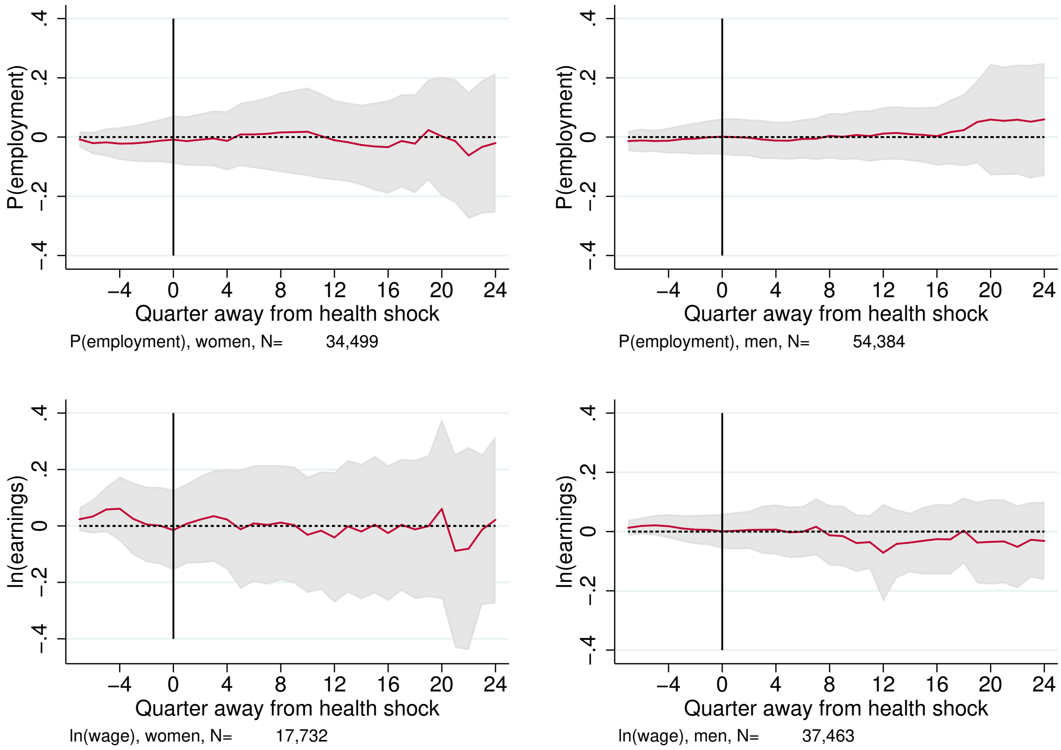
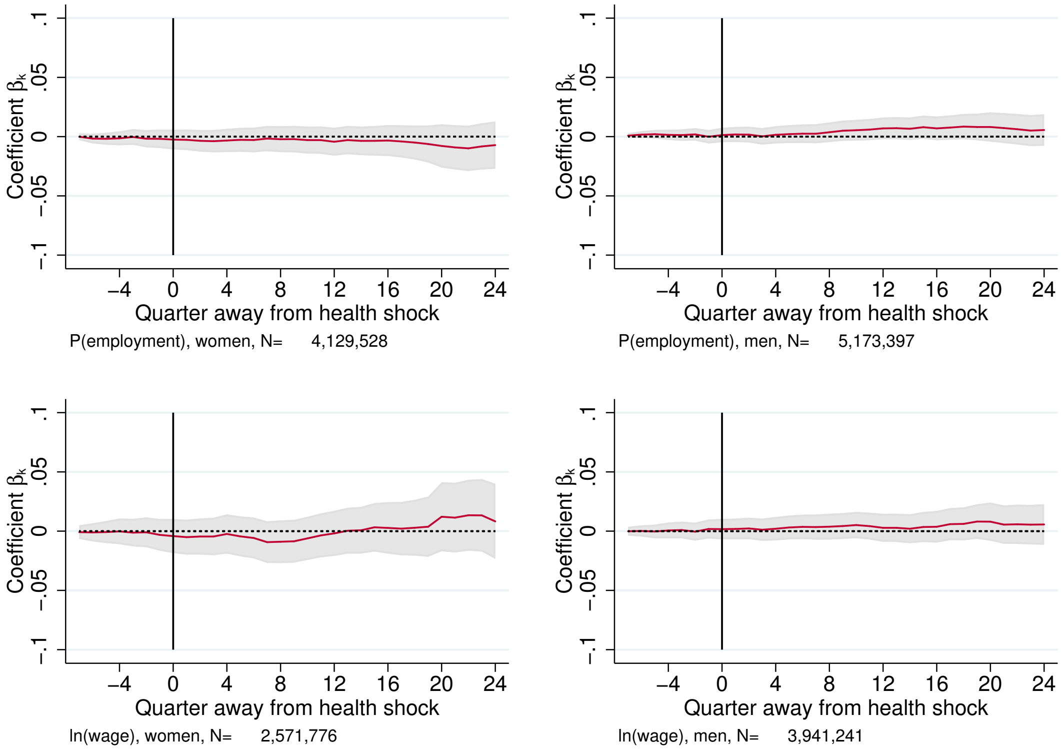
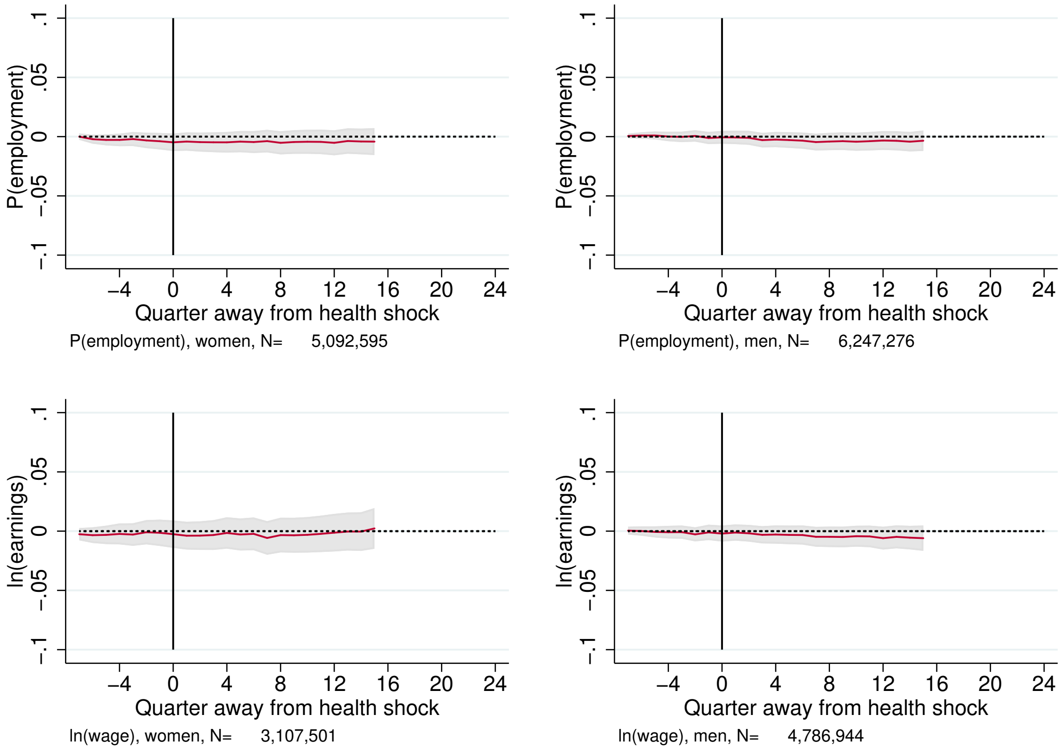


Figure A10: Unweighted



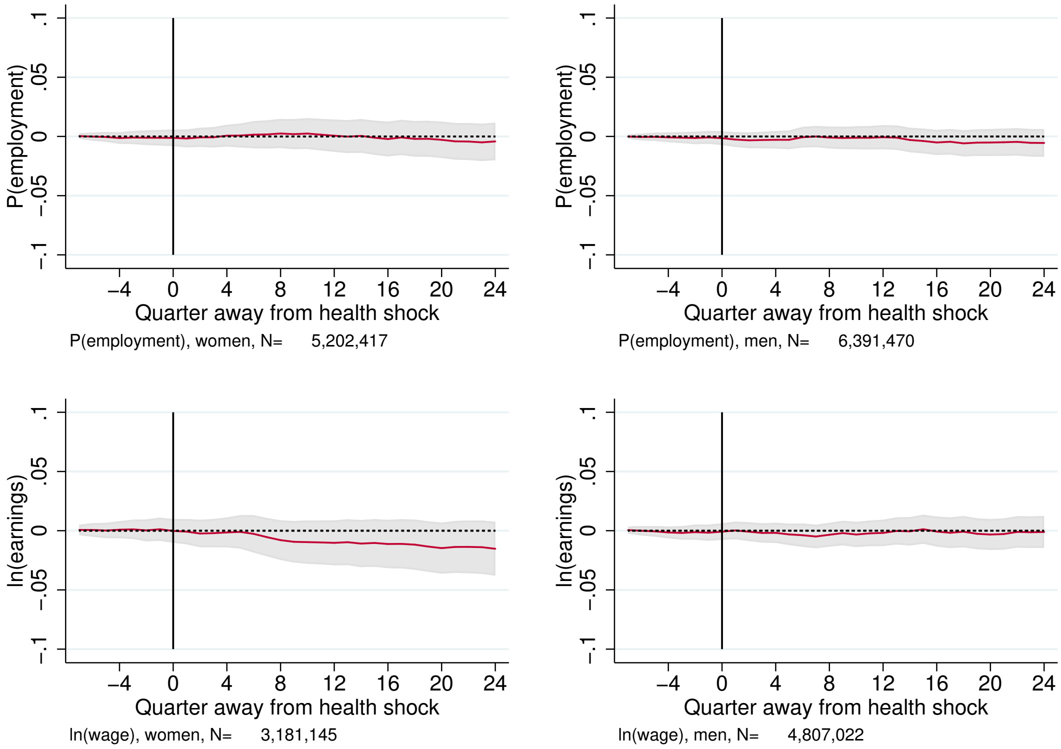
The grey shaded areas correspond to the Bonferroni adjusted 95% confidence intervales.

Figure A11: Control group with future health shock



The grey shaded areas correspond to the Bonferroni adjusted 95% confidence intervales.

Figure A12: Shift treatment period to 2004q3-2005q4



The grey shaded areas correspond to the Bonferroni adjusted 95% confidence intervales.

Figure A13: Shift treatment period to 2003q1 - 2004q2

Figure to be exported

The grey shaded areas correspond to the Bonferroni adjusted 95% confidence intervales.

Figure A14: Shift treatment period to 2004q1-2005q2

Figure to be exported

The grey shaded areas correspond to the Bonferroni adjusted 95% confidence intervales.

Figure A15: Drop *all* parental hospitalisations between 1995q1-2000q4

Figure to be exported

The grey shaded areas correspond to the Bonferroni adjusted 95% confidence intervales.