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

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ABSTRACT

Unexpected negative health shocks of a parent may reduce adult children's labour supply via informal caregiving and stress-induced mental health problems. We link administrative data on labour market outcomes, hospitalisations and family relations for the full Dutch working age population for the years 1999-2008 to evaluate the effect of an unexpected parental hospitalisation on the probability of employment and on conditional earnings. Using an event study difference-in-differences model combined with coarsened exact matching and individual fixed effects, we find no effect of an unexpected parental hospitalisation on either employment or earnings for Dutch men and women, and neither for the full population nor for the subpopulations most likely to become caregivers. These findings suggest that the extensive public coverage of formal long-term care in the Netherlands combined with widespread acceptance of part-time work 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 frequently in old age. These health shocks do not only affect the patient, 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 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 themselves against. 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 pen-

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sion 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 aims to increase in both labour market participation and informal caregiving, two goals which may not be easy to reconcile (Josten and De Boer, 2015). Indeed, if labour market participation is lower 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 the OECD (OECD, 2017b). The Dutch LTC system is universal, comprehensive, and very generous (Bakx et al., 2015a). Combined with many opportunities to work part-time, this generosity means that 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 extend 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 chronic obstructive pulmonary disease (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. 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 not self-reported but 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 the subsequent 24 quarters. We link records for working-age individuals to their parents' health information and estimate an event study 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 children, 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. Providing 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] For example, Van Houtven et al. (2013) use the Health and Retirement Study with an instrumental variable fixed effects model, using parental health and parental death indicators as instruments. They find that there are no employment effects of informal caregiving for women, and small negative effects for men. At the intensive margin, they find a reduction of 3-10 working hours per week with a 3 percentage point wage reduction, but no effect for men. More relevant to our setting, Ciccarelli and Van Soest (2018) provide recent evidence for Europe and instrument informal caregiving with the death of a parent, poor health of a parent, and distance to the mother's residence. They find that daily caregiving significantly reduces the probability of being employed and the number of hours of paid work, especially for females. On the other hand, providing care on a weekly basis does not significantly affect paid work.

The second channel consists of the mental health effects that a parental hospitalization may inflict. Naturally, children 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 (Bobinac et al., 2010; Amirkhanyan and Wolf, 2006, 2003), 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

[1] See Ciani (2012), Meng (2013), Van Houtven et al. (2013), Jacobs et al. (2016), Casado-Marín et al. (2011), Leigh (2010), Heitmueller (2007), Moscarola (2010), Heger (2014), Bolin et al. (2008), Viitanen (2010), Schmitz and Westphal (2016), Heitmueller and Inglis (2007), Carmichael et al. (2010), Michaud et al. (2010), Ettner (1996, 1995. 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 (Coe and Van Houtven, 2009; De Zwart et al., 2017; Bauer and Sousa-Poza, 2015; Bom et al., 2019).

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 is arguably 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 choosing 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 methodology 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 (2019) study the effect of health and mortality shocks on the labour market outcomes of Danish spouses. They find that a spousal death leads 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 (2019) 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 findings barely change 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 fairly 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 entire 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 for informal caregiving (Bom et al., 2019). 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. A complementary analysis of Dutch panel survey data shows that a health shock of a relative leads to more informal care provision, but that this increase in caregiving does not lead to labour market effects. The mental health effect of a health shock of a relative seems to be less important. 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).

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 (Mot, 2010; De Meijer et al., 2015). Given the broad coverage of the public LTC insurance, private LTC is marginal and concentrated among the wealthy (Maarse and Jeurissen, 2016). Only between 0.3-1.0% of yearly household expenditure for LTC was for private LTC in 2001-2005 (Statistics Netherlands, 2017b). 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., 2015b; 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., 2015c).[7]

[4] Fadlon and Nielsen (2019) 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 76 for mothers, and 78 for fathers.

[5] *Algemene Wet Bijzondere Ziektekosten*

[6] Transferred to the Social Support Act in 2007

[7] During the study period, some changes were introduced in the AWBZ. In the 1990s, there were relatively long waiting times, and in 2001 there was a policy effort to shorten waiting times through budgetary expan-

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 (longer than 3 months) spells of caregiving in 2008 (de Boer and de Klerk, 2013). In the Study on Transitions in Employment, Ability and Motivation (STREAM) survey, 13% of Dutch caregivers report to provide more than 15 hours of care per week. On the demand side, Swinkels et al. (2015) report based on a representative survey that 25.6% of 55+ respondents received informal care in the Netherlands in 2001-2003. Around 60% of caregivers are female, and about half of them are aged 45-65. In 40 % of the cases, the care recipient 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. Caregiving tasks in the Netherlands 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).

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

A recent report suggests that 26% of the 16-69 years old who work at least 12 hours per week combine paid and care work. 80% of these caregivers provide care on at least weekly basis; 20% intensively (at least 8 hours per week) (de Boer et al., 2019), corresponding to around 400,000 individuals. These people work on average 31 hours per week, and give around 21 hours of care. Most of this care goes to parents (or parents in law). If the combination of care and paid work is problematic, 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). One reason for the limited popularity of care leave could be that it is unpaid when using it for more than two weeks per year.

sions. In an effort to curb rising LTC costs, higher co-payments and regional budgets were introduced in 2004 and 2005 (Mot, 2010). In our analysis, these changes may lead to different effects for different treatment cohorts. In a robustness check, we shift the treatment period, but we do not find a different effects across cohorts. We are therefore confident that these policy changes do not affect our results.

3. Data

The study population consists of the entire Dutch non-institutionalised population aged 35-65 between 1999 and 2008, with at least one parent still alive.[8] We use quarterly data from Statistics Netherlands on demographics linked to data on employment and earnings (1999-2011), hospitalisations (1995-2005), residence coordinates, and the cause of death registry.[9]

We use two labour market outcomes as dependent variables: the probability of employment and earnings conditional on employment. Employment is specified as being employed at least one day in a quarter. The original tax data contains yearly gross earnings after social security contributions per job contract, and the beginning and the end date of a job. To get quarterly data, we compute daily earnings with the information on yearly earnings and contract duration. We then multiply daily earnings with the number of days covered by the contract in a given quarter. Lastly, we sum quarterly earnings per job over all jobs held in a quarter. For the regression analysis, we use a logarithmic transformation of conditional earnings.[10]

The data available limits the type of work interruptions we can detect. Table (1) shows possible labour market effects of a parental health shock, their legal implications, and how we capture these with our data. Short and long-term care leave, unpaid leave and sickness leave reduce earnings within the same contract, similar to a reduction in the number of hours worked with the same employer. In this case, the effect of an earnings reduction is spread across a whole calendar year. We will find a smaller, but still detectable, effect.[11] We observe the full immediate reduction in earnings only when there is a new contract. We are not able to observe if the individual takes up holidays, neither if the employer pays full wages instead of the legal minimum required for care leave or sick leave.

The main exposure variable of interest is an unexpected parental hospitalisation related to a new health problem. We limit the health shock to ICD-9CM[12] diagnoses that are only treated in the hospital and that an expert physician considered to be not foreseeable (see also García-Gómez et al., 2015b; García-Gómez et al., 2017).[13] In addition, these hospitalisations are classified as a health shock only if the individual has not been hospitalised unexpectedly since 1995. This restriction makes parents with and without a health shock more comparable before the shock.

[8] We drop all parents if they are 105 or older, since there seem to be some death registrations missing. None of these parents have experienced a health shock in the sample period.

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

[10] 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 it is approximately normally distributed and hence a log transformation is appropriate.

[11] This can be an issue for the common trend assumption. Inspection of pre-trends show that it is no problem in our case.

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

[13] The full list of included conditions is available as an online appendix.

Table 1
Potential labour market effects and how they are measured in our data

Status	Legal situation	In the data	Event observed
Short-term care leave	2 weeks/y, paid at 70%	Earnings ↓	Spread over 1 calendar year
Long-term care leave	6 weeks/y, unpaid	Earnings ↓	Spread over 1 calendar year
Unpaid leave	Individual agreement	Earnings ↓	Spread over 1 calendar year
Sick leave	Paid at 70%([a])	Earnings ↓	Spread over 1 calendar year
Reduction in hours	Same contract	Earnings ↓	Spread over 1 calendar year
Reduction in hours	New contract	Earnings ↓	Next quarter
Change job	New contract	Earnings change	Next quarter
Holidays	20+ days per year([b])	Not observed	na
Unemployment	No work contract	Not employed	Next quarter
Disability insurance	No work contract([c])	Not employed	Next quarter

[a] Until 2003, the first year of sickness is paid at 70% (but the payment has to be at least the sector-specific minimum wage). From 2004 onward, sickness pay is extended to two years of sickness, also paid at 70%. This is the minimum; most industry-level collective labour agreements entitle workers to 100% of the wage in the first year, and 70% in the second. After two years of sick leave, one is transferred to the disability insurance.
[b] Exact rule for the minimum number: 4 times the days worked per week.
[c] DI can also manifest as a job change or a reduction in hours, depending on the degree of disability. Source: Dutch Government (2001, 1996).

Table 2
The five most frequent parental health shocks

Diagnosis	ICD9-CM	Frequency	%
Atrial fibrillation and flutter	427.3	18,273	7%
Transcervical fracture of neck of femur (closed)	820.0	11,090	4%
Angina pextoris; not elsewhere specified	413.9	10,492	4%
Intermediate coronary syndrom	411.1	10,295	4%
Cerebral artery occlusion; unspecified	434.9	9,633	3%

Sample selection: parents in the treatment group (see Section 4).

For our analysis, the parental health shock needs to be i) unexpected, ii) severe and iii) causing an increase in the need for informal care. Since we only use first hospitalisations since 1995 (no hospitalisation in at least four years), the hospitalisation can be viewed as plausibly exogenous variation in parental health. Note that unexpectedness in our framework implies that in quarter $q-1$, the hospitalisation in q is not foreseeable. It is thus not required that we only include emergency room type of conditions. Some types of cancer, for example, are also included in our list of health shocks, because they require fast action after detection, which will typically happen in the time frame of a quarter. First time heart attacks are included too because, even though a heart attack could be expected if a parent smokes and drinks a lot, the exact timing of the attack cannot be anticipated.

The unexpectedness of our health shock is tested in two ways. First, we test the common trend assumption, which shows insignificant pre-trends in all analyses. Second, we conduct a robustness test using a subset of nondeferrable conditions that occur with the same frequency on weekends as on weekdays (Card et al., 2009; Dobkin et al., 2018) (see Section 5.3 for more details). Since our list of health shocks covers a larger part of the population than the nondeferrable conditions, we use the broader definition in our main analysis.

The second condition, ii) severity, is a requirement for the health shock to have an impact on the parent and his/her family members. Related to severity, the shocks need to occur frequently enough to have an impact in a broad study population. For the 55+ population that had been hospitalised in 1999-2005, 37% was due to one of the conditions labelled as a health shock. In the first quarter of 2001 alone, around 1.4% of *all* mothers (26,180 women) and 1.5% of *all* fathers (23,161 men) were hospitalised due to such a health shock. The five most frequent conditions by health shock classification are shown in Table 2. On a more aggregate level, Table 3 shows the frequency of grouped diagnoses classified as health shocks in the treatment group.[14] The most common shocks are cancers, circulatory diseases, injuries, and strokes. Health shock admissions are different from non-shock admissions in two ways. For the 55+ hospitalised population in 1999-2005, they lead on average to a longer hospital stay: a health shock admission lasts on average for 8 nights, while a non-shock patient stays ‘only’ for 5 nights. Moreover, health

Table 3
Parental health shocks by diagnosis group

ICD9 diagnosis group	Frequency	%
Cancers	66,322	24%
Circulatory diseases	61,586	22%
Injuries	53,611	19%
Strokes	34,256	12%
Respiratory diseases	14,539	5%
Diseases of the digestive system	12,749	5%
Diseases of the genitourinary system	12,500	4%
Diseases of the nervous system	11,096	4%
Musculoskeletal diseases	5,376	2%
Infectious diseases	4,292	2%
Skin diseases	1,993	1%
Endocrine diseases	.	.

Sample selection: parents in the treatment group (see Section 4). Statistics Netherlands does not release data cells below 10 observations to protect privacy. Therefore, the numbers are missing for the diagnosis group ‘endocrine diseases’.

[14] see Section 4 for how the treatment group is defined

shocks are less likely to be day care admissions (27 vs 73%).[15] The severity of the health shocks is also reflected in the difference in subsequent mortality. After a health shock, mothers (fathers) are 7 (20) percentage points more likely to die before the second quarter of 2008 if they had a health shock around 5-6 years before (significant at 1%) when controlling for age, migration background, and living with a partner (see Table A4 for details). Taken together, we interpret these statistics as evidence that the diagnoses we use are indeed severe.

Third, the parental health shock has to be correlated with an increase in informal care demand. We use survey data for later years in the Netherlands that contain both information about informal caregiving and an indicator that 'a close family member (except for spouses) has a serious disease' to support this assumption. In this analysis (see Section 5.4), we find clear evidence that a health shock of a close family member is correlated with informal caregiving. This is backed up by two other types of evidence. First, other studies have shown that diagnoses constituting a parental health shock are associated with increased informal care use in the Netherlands (Van Exel et al., 2002) and Spain (García-Gómez et al., 2015). Second, when combining the health shock definition with information on health determinants of formal LTC use,[16] we see that at least one third of patients aged 65+ hospitalised for the 23 most prevalent admission diagnoses received formal home care after their hospitalisation (based on Wong et al., 2010, see Table A5 in the Appendix for details). Furthermore, combining diagnosis group-specifc information from Bakx et al. (2015) 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.

To sum up, we feel confident that the parental health shock measure we use indeed is unexpected, and has severe consequences that lead to LTC demand.

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[17] 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 4 and 5 show summary statistics of these variables.[18] Our sample consists of working individuals aged 47 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. 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 greater care demands 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 partner 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 prevailing at the time of the parental health shock.

4. 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 event study 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 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. Schmitz and Westphal, 2016; Skira, 2015; Michaud et al., 2010; Fevang et al., 2012; Viitanen, 2010; Casado-Marín et al., 2011; Moscarola, 2010). We therefore follow labour market outcomes for 8 quarters before until 24 quarters after a health shock.

4.1. 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. This avoids that relapses of pre-existing conditions play a role and thus reinforces the unexpectedness of the parental health shock. Figure 1 depicts how the sample is selected and how individuals are attributed to either the treatment (T) or the control (C) group. The treatment group consists of individuals experiencing a parental health shock between 2001q1

[15] Tables (A2) and (A3) provide more information on the type of hospital diagnoses not labelled as a health shock.

[16] Note that formal LTC use does not rule out the provision of informal caregiving. More than half of informal caregivers in the Netherlands report to provide care in collaboration with formal care services (De Klerk et al., 2017).

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

[18] Table A6 and A7 in the Appendix show the same summary statistics for the working sample.

Table 4
Women - summary statistics treatment and control group

	Control		Treatment			
Variable	Unweighted Mean	Weighted Mean	Unweighted Mean	Weighted Mean	Unweighted StdDiff	Weighted StdDiff
Employed	0.55	0.57	0.57	0.57	−0.02	0.00
Employed$_{q-4}$	0.55	0.56	0.56	0.56	−0.02	0.00
Employed$_{q+24}$	0.57	0.57	0.59	0.59	−0.02	−0.02
Earnings	4,661	4,750	4,672	4,660	0.00	0.02
Earnings$_{q-4}$	4,403	4,463	4,401	4,395	0.00	0.02
Earnings$_{q+24}$	5,956	6,366	5993	6350	−0.01	0.00
Age	46.7	46.6	46.6	46.6	0.01	−0.01
Age mother	74.5	74.9	75.1	75.1	−0.05	−0.02
Age father	77.4	77.6	77.7	77.7	−0.03	−0.01
Living with a partner	0.10	0.10	0.10	0.10	0.00	0.00
Dutch	0.92	0.93	0.92	0.93	−0.01	0.00
1st generation migrant	0.03	0.02	0.03	0.03	0.01	0.00
2nd generation migrant	0.06	0.05	0.05	0.05	0.01	0.00
Number of siblings	2.1	1.6	1.6	1.6	0.16	0.00
Number of kids < 13	0.5	0.5	0.5	0.5	0.02	0.00
Father has partner	0.4	0.5	0.5	0.5	−0.10	0.00
Mother has partner	0.4	0.5	0.5	0.5	−0.10	0.00
Distance residence mother in km	25.9	26.4	28.1	27.9	−0.04	−0.02
Distance residence father in km	27.0	27.7	42.3	42.0	−0.22	−0.21
Number of jobs	1.1	1.1	1.1	1.1	0.00	0.00
Quarters employed in the main job	29.7	29.8	29.5	29.7	0.01	0.00
Distance to closest parent	24.3	24.5	23.4	23.4	0.02	0.02
One parent dead	0.32	0.14	0.14	0.14	0.31*	0.00
Age oldest parent	77.7	77.8	77.9	78.0	−0.02	−0.01
N	258,128	236,988	136,595	134,281		

* 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 5
Men - summary statistics treatment and control group

	Control		Treatment			
Variable	Unweighted Mean	Weighted Mean	Unweighted Mean	Weighted Mean	Unweighted StdDiff	Weighted StdDiff
Employed	0.76	0.78	0.77	0.77	−0.02	0.00
Employed$_{q-4}$	0.77	0.78	0.78	0.78	−0.02	0.00
Employed$_{q+24}$	0.71	0.71	0.73	0.73	−0.03	−0.03
Earnings	9,720	9,869	9,825	9,774	−0.01	0.01
Earnings$_{q-4}$	9,212	9,334	9,293	9,253	−0.01	0.01
Earnings$_{q+24}$	12,171	12,466	12,453	12,539	−0.02	−0.00
Age	46.7	46.4	46.6	46.6	0.01	−0.02
Age mother	74.5	74.8	75.1	75.1	−0.05	−0.03
Age father	77.3	77.5	77.6	77.7	−0.03	−0.02
Living with a partner	0.13	0.12	0.13	0.12	0.00	0.00
Dutch	0.91	0.92	0.92	0.92	−0.02	0.00
1st generation migrant	0.04	0.03	0.03	0.03	0.03	0.00
2nd generation migrant	0.06	0.05	0.05	0.05	0.01	0.00
Number of siblings	2.1	1.6	1.6	1.6	0.16	0.00
Number of kids < 13	0.7	0.7	0.7	0.7	0.00	0.00
Father has partner	0.4	0.5	0.5	0.5	−0.11	0.00
Mother has partner	0.4	0.5	0.5	0.5	−0.11	0.00
Distance residence mother in km	24.5	25.2	26.9	26.6	−0.04	−0.02
Distance residence father in km	25.5	26.9	40.9	40.7	−0.22	−0.20
Number of jobs	1.1	1.1	1.1	1.1	0.00	0.01
Quarters employed in the main job	43.0	43.0	42.9	43.1	0.00	0.00
Distance to closest parent	22.8	23.4	22.2	22.1	0.01	0.02
One parent dead	0.32	0.14	0.14	0.14	0.31*	0.00
Age oldest parent	77.6	77.7	77.9	77.9	−0.02	−0.02
N	269,635	246,117	141,727	139,289		

* 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.

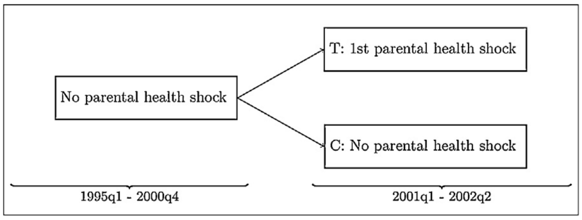

Fig. 1. Timing of the parental health shock and treatment (T) and control group (C) assignment.

and 2002q2.[19] This selection allows to test at least 8 quarters of pre-treatment trends in labour market outcomes (employment and earnings are 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 a 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.[20] 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.[21] Therefore, each cohort of treatment and control group is an unbalanced panel.

4.2. Coarsened exact matching (CEM)

It is possible that individuals with a parental health shock are different from the ones without a parental health shock. We therefore make the treatment and control groups more comparable on observables using coarsened exact matching (CEM). 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.[22] Individuals who cannot be matched receive weight zero.

We use CEM instead of propensity score matching since for a 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 origin, 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[23]) 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 16’000 possible bins for each gender and lose 1-2% of our treated individuals for whom no match could be found.[24] Given that the matched and unmatched results are fairly similar, we are confident that this 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 Tables 4 for women and 5 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 one parent has died, which is 0.31 for both men and women. This is addressed in the weighted sample, where the standardised difference for this variable is close to 0 for both genders. The similarity between the weighted and unweighted sample gives additional support for the exogeneity of our parental health shock.

4.3. 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 (Jeon and Pohl, 2017; Hijzen et al., 2010). We define an indicator of how many quarters an individual is away from a health shock q_{it}^k with

[19] In a robustness check, we shift the treatment period to 2004q3-2005q4. The results remain stable (Figure A13 in the Appendix).

[20] 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 (5.3) we provide evidence that our results are not driven by this particular random sample of controls.

[21] 82% of the sample is observed for the full 33 quarters.

[22] 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.

[23] 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

[24] For women, 2589 bins contain at least one observation, out of which 846 bins containing treated women that could not be matched. These unmachted treated bins contain around 2.7 women on average (as opposed to 51.9 treated women per matched bin on average).

$k \in [-8, 24]$ 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.

$$y_{it} = \alpha_i + \alpha_t + \sum_{k=-7}^{24} \gamma^k q_{it}^k + \sum_{k=-7}^{24} \beta^k D_i q_{it}^k + \delta x_{it} + \varepsilon_{it} \qquad (1)$$

Equation (1) is estimated using the within transformation plus CEM weighted least squares for the probability of employment and log conditional earnings. The first sum in Equation (1) captures the common time trends of treatment and control before and after the health shock. The second sum is the difference-in-differences term, with coefficients of interest $\beta^0, \ldots \beta^{24}$. 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).[25]

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, 2019), but the inspection of raw employment and earnings trends by group before the health shock is informative. Figure 2 depicts the CEM-weighted employment proportions and conditional earnings median trends in the 8 quarters before and 24 quarters after the parental health shock. The main conclusion is that the pre-trends are similar between treatment and control group. Weighted on pre-treatment characteristics but not controlling for covariates, the treated are more likely to work after the parental hospitalisation; and this difference is statistically significant at 1% after 24 quarters. This is somewhat surprising, as we would have expected that the treated are less likely to work after a parental health shock. Yet, when looking at standardised differences (see Table 4 and 5, line 3), the treatment and the control group seem to be balanced in employment (and earnings) 24 quarters after the parental health shock. In earnings, there does not seem to be a difference in the treatment and the control group after the parental health shock.

More formally, potential pre-treatment differences in trends can be detected through t-tests for significance of $\beta^{-7}, \ldots, \beta^{-1}$. 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. Furthermore, we conduct a robustness test where we restrict the population to parents without any hospitalisation, thereby forcing common parental health trends to the extent possible with our data.

5. Results

5.1. CEM weighted Difference-in-Differences

In Figure 3, we plot the CEM weighted coefficients of the difference-in-differences term β^k and their 95% Bonferroni adjusted[26] confidence interval for the probability of employment and conditional log earnings by gender. The leads of the parental health shock are 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. Given the confidence intervals, we can rule out with 95% confidence a negative employment effect outside the range of [-1.0,0.6] percentage point for women, and [-0.6;1.4] percentage point for men. For earnings, the corresponding intervals are [-1.8;1.1] percentage point for women, and [-1.0;1.0] percentage point for men. This means that, even if the estimated effect was significant, it would be extremely small and thus it would not be regarded as economically significant. This also holds for male employment. It seems that towards the end, the estimated effect becomes positive and nearly significant - but the estimated effect is only 0.8 percentage point. The no-effect finding is consistent over multiple at-risk caregiver subsamples (as explained in the next subsection) and other robustness checks.

The Bonferroni correction does not come at a price in terms of power. For an F-test that all difference-in-differences terms are jointly equal to zero with a Bonferroni adjusted significance level at 5% and given our sample size, the power of the F-test is at least 83% for both genders and labour market outcomes (Cohen, 1988). Hence, our results are indeed a precisely estimated zero effect and not due to a lack of power.[27]

[25] Our conclusions are robust to clustering the standard errors at individual level.

[26] 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.

[27] Given these high level for power, we are well protected against type II error. Leamer (1978) argues that type I error should be minimised as well by setting the significance level as a decreasing function of sample size. We have considered applying this principle with guidance from Kim (2015). Since the Leamer adjustment would result in a very low (practically zero) level of the significance threshold for some specifications, we do not use it for our results. If we implemented it, this would result in even stronger evidence for no effect.

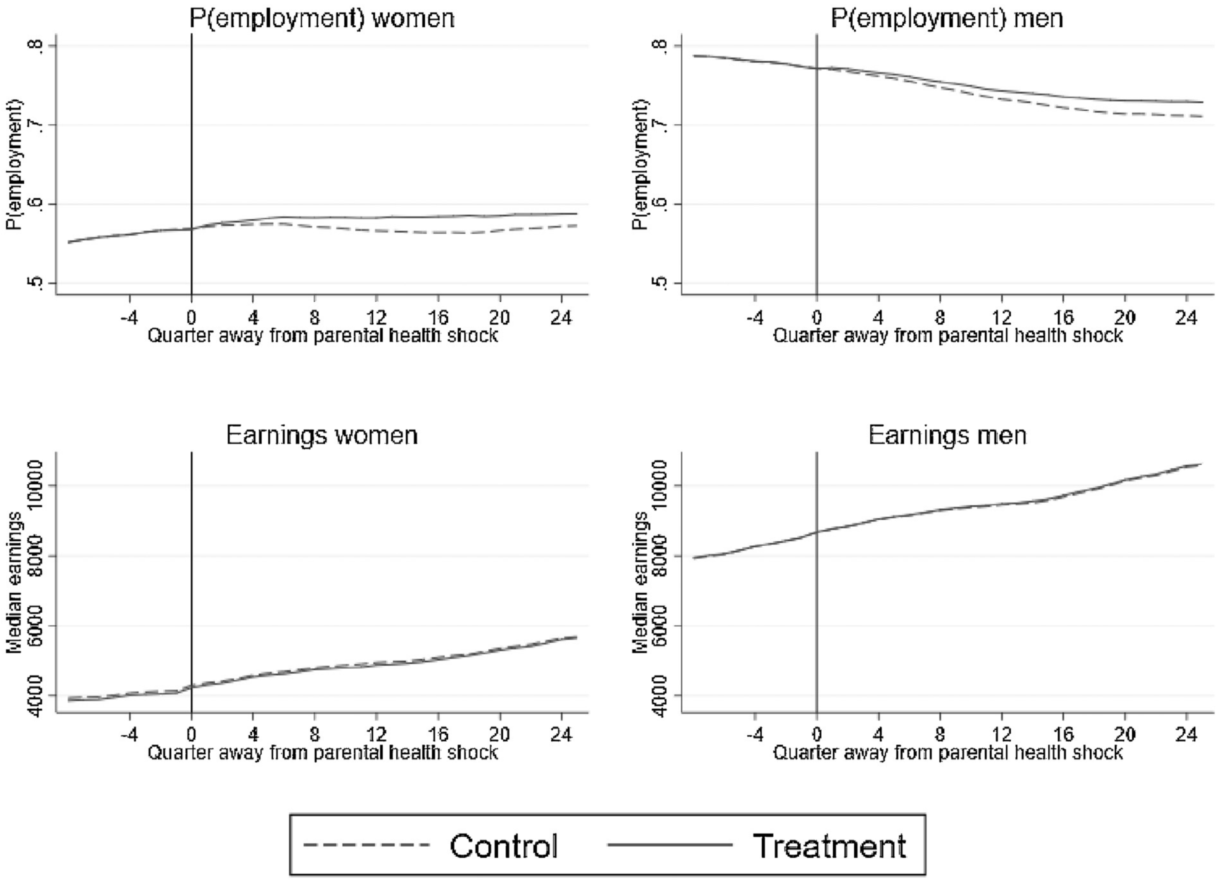

Fig. 2. CEM weighted employment and earnings trends.

5.2. *Subgroups with the highest caregiving probability*

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 with individuals who are most likely to become caregivers or for whom we expect a larger effect. First, we look at parents living close by. The closer the parents live, the more likely caregiving becomes. Distance to parents has also been used as an instrument for informal caregiving (e.g. Jacobs et al., 2016). Second, we analyse children who are employed one year before the parental health shock. In this group, we would expect a larger effect since they are more time-constrained than children who were initially not working.[28] On the other hand, we would expect children who are not employed to be more likely to take on a caregiving task. Therefore, the third group consists of children not employed one year before the shock. Fourth, it may be that the parents we are looking at are not frail enough so that their health shock does not have labour market consequences for the children. We therefore look at parents aged 80 and above. Fifth, caregiving tasks could also be taken over by siblings or spouse of the parent. For this reason, we look at the subgroup of only-children, and children of alone-living parents. Finally, we construct a combination of the above with only-children with alone but close-living parents. If there is an effect, it would be in this group, since there are no siblings nor a partner who can take over the caregiving task, and since the parent lives close caregiving is even more likely.

Table 6 gives an overview of these results by showing 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 and these subsamples. A graphical representation of the full results is displayed in Figures A1-A8 in the Appendix. We do not find a significant effect for any of these at-risk caregiving subgroups, not even for the only children with alone but close living parents. Even though we lose some precision in smaller subsamples, the power of the smallest subsample, the only children with a single parent who lives close-by, is still 99% thanks to our large administrative data set. Hence, these null-results are not due to a lack of power either. Given these subsample results, we are confident that the zero effect we found in the main analysis is not due to the broad sample.

[28] Ideally, we would want to have in this group only people who are full-time employed, but unfortunately this information is not available in our data.

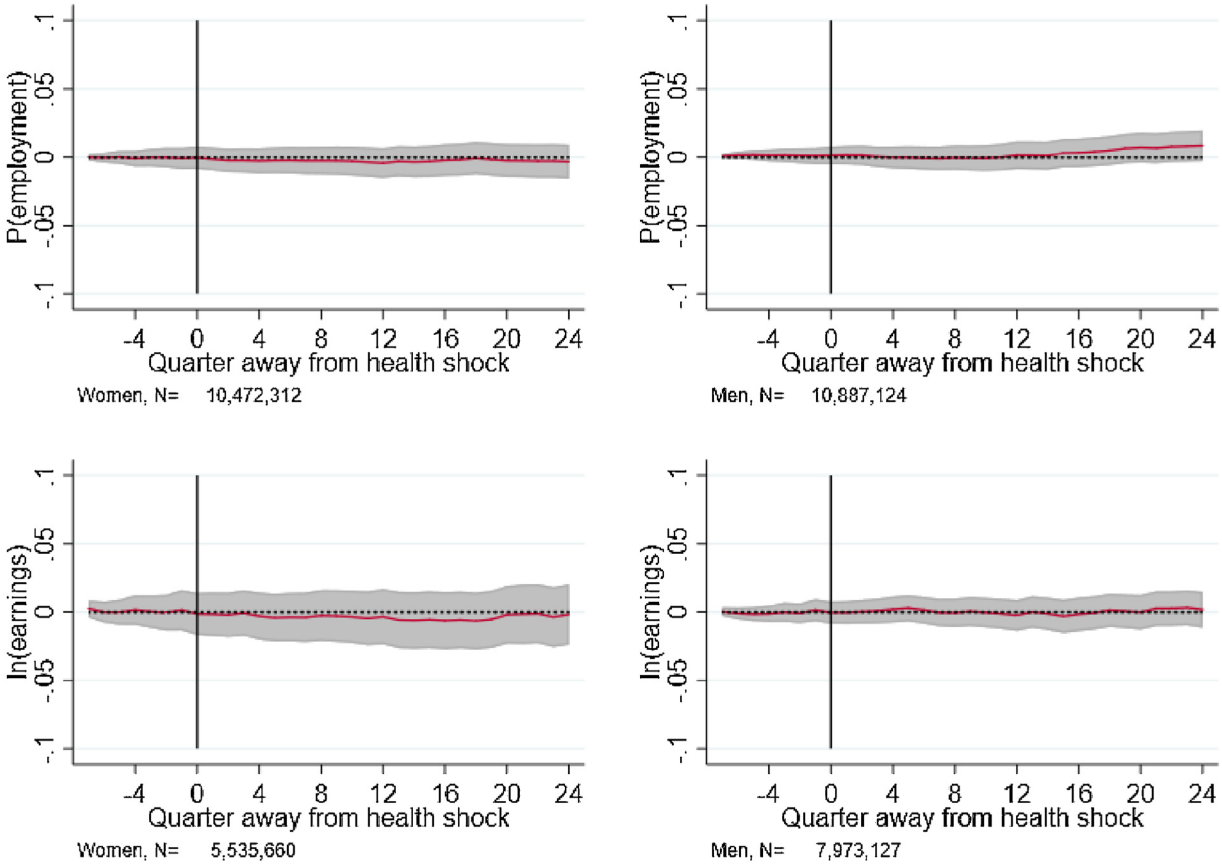

Fig. 3. Earnings and employment effects of a parental health shock. The grey shaded areas correspond to the Bonferroni adjusted 95% confidence intervales.

5.3. Robustness checks

We check the robustness of our main findings in Table 7. 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 A9-A14). The first column shows the main results for ease of comparison. The first robustness check shows that the CEM weighting (column 'No CEM') does not drive our results.

In the column 'future health shock', 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 a control group only the individuals who experienced a parental health shock in 2005, in the spirit of Fadlon and Nielsen (2019).[29] This should make the control group more comparable to the treated and thus increase the 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. We find a borderline significant, very small employment effect for women, which is never larger than 0.76 percentage points, and the confidence interval never includes an effect larger than -1.1 percentage points. These are extremely small effects, which we do not consider economically significant. In terms of the effect size, the findings are comparable to the main specification, but there is more precision since we are looking at a more homogeneous group. For men in general, and for female earnings, the null results of the main specification are confirmed.

Furthermore, we check if our selection of the treatment period affects our results by redefining the treatment group as individuals with a parental health shock in 2004q3-2005q4 ('Shift treatment'). There is no effect of a parental hospitalisation on labour market outcomes in this different treatment group.[30]

In two further checks, we use a stricter the definition of a parental health shock. In the column 'severe health shock', we only include individuals with parents who stay in the hospital longer than 6 nights, which is the median length of stay. Length-of-stay might be a proxy for very severe cases, which in turn require a lot of informal care.

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

[30] This also shows that the minor LTC policy changes in the study period are not influencing our results.

Table 6
Subsamples with the highest caregiving probability

	Main results	Parents living close	Employed at t-1	Not employed at t-1	Parents aged 80 and older	Only children	Single children	Single parent	Only-child with single parent living close-by
k	Women employment								
−4	−0.001	0.004	−0.001	−0.004	0.001	−0.002	−0.001	−0.003	0.002
	(0.002)	(0.002)	(0.002)	(0.003)	(0.004)	(0.005)	(0.002)	(0.003)	(0.011)
8	−0.002	−0.001	−0.003	−0.003	−0.010	−0.009	−0.004	−0.004	−0.030
	(0.003)	(0.005)	(0.004)	(0.005)	(0.008)	(0.009)	(0.004)	(0.006)	(0.025)
N	10,472,312	3,785,132	5,664,304	4,074,596	2,358,443	1,332,005	9,421,949	4,761,327	155,356
k	Women earnings								
−4	0.001	0.003	0.002	n.a.	−0.004	−0.001	0.000	−0.007	−0.021
	(0.004)	(0.005)	(0.004)		(0.009)	(0.010)	(0.004)	(0.007)	(0.028)
8	−0.003	−0.004	−0.003	0.100	−0.011	−0.014	−0.002	−0.011	−0.042
	(0.006)	(0.008)	(0.006)	(0.122)	(0.019)	(0.017)	(0.007)	(0.012)	(0.041)
N	5,535,660	2,068,478	5,266,047	20,059	893,359	687,325	4,933,449	2,247,920	76,536
k	Men employment								
−4	0.001	0.001	−0.001	0.001	0.004	−0.005	0.000	0.001	−0.008
	(0.001)	(0.002)	(0.001)	(0.005)	(0.004)	(0.004)	(0.002)	(0.003)	(0.010)
8	−0.000	0.002	0.002	−0.002	0.002	−0.012	−0.002	−0.005	−0.008
	(0.003)	(0.003)	(0.003)	(0.008)	(0.008)	(0.008)	(0.003)	(0.006)	(0.019)
N	10,887,124	4,280,767	8,346,671	2,068,191	2,432,290	1,399,697	9,531,344	4,956,231	163,135
k	Men earnings								
−4	−0.001	−0.002	−0.002	n.a.	0.000	0.002	−0.001	0.000	0.013
	(0.002)	(0.003)	(0.002)		(0.006)	(0.006)	(0.002)	(0.004)	(0.025)
8	−0.001	−0.007	−0.000	0.157	−0.004	0.005	0.001	0.003	−0.007
	(0.004)	(0.004)	(0.003)	(0.118)	(0.010)	(0.010)	(0.004)	(0.007)	(0.041)
N	7,973,127	3,191,206	7,840,758	18,250	1,535,933	990,626	6,973,002	3,431,813	116,391

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$ with Bonferroni adjustment for multiple testing. Difference-in-differences coefficients for k quarters away from the shock and their standard error in parenthesis. For the subgroup who are not employed, $k = -4$ is not applicable, as nobody has earnings 4 quarters before the health shock in this subsample. A more detailed definition of the subsamples can be found in Section (3).

Table 7
Robustness checks

	Main results	No CEM	Future health shock	Shift treatment	Severe health shock	Nondeferrable health shock	No hospitalisations
k	Women employment						
−4	−0.001	−0.000	−0.003	0.001	0.000	−0.006	−0.002
	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)	(0.012)	(0.002)
8	−0.002	−0.001	−0.007*	0.007	−0.002	−0.007	−0.003
	(0.003)	(0.003)	(0.002)	(0.004)	(0.004)	(0.019)	(0.004)
N	10,472,312	11,163,541	10,562,227	7,967,087	7,718,097	5,167,069	7,989,373
k	Women earnings						
−4	0.001	0.006	0.004	0.002	−0.001	0.040	−0.000
	(0.004)	(0.003)	(0.003)	(0.002)	(0.005)	(0.022)	(0.004)
8	−0.003	0.006	−0.002	0.009	−0.008	0.022	0.002
	(0.006)	(0.005)	(0.004)	(0.006)	(0.008)	(0.030)	(0.007)
N	5,535,660	6,328,643	5,969,159	4,652,946	3,996,809	2,979,330	4,183,222
k	Men employment						
−4	0.001	0.001	0.001	0.000	0.003	0.018	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.010)	(0.002)
8	−0.000	0.004	−0.000	−0.002	0.001	−0.000	0.001
	(0.003)	(0.002)	(0.002)	(0.003)	(0.004)	(0.015)	(0.004)
N	10,887,124	11,644,517	11,020,382	9,126,660	8,065,470	5,298,997	8,303,397
k	Men earnings						
−4	−0.001	0.001	−0.001	−0.000	−0.002	−0.007	−0.002
	(0.002)	(0.002)	(0.001)	(0.002)	(0.003)	(0.012)	(0.002)
8	−0.001	0.003	−0.002	−0.003	−0.001	−0.021	−0.000
	(0.004)	(0.003)	(0.002)	(0.004)	(0.005)	(0.021)	(0.004)
N	7,973,127	8,667,909	8,420,805	6,770,736	5,876,284	4,391,123	6,054,516

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$ with Bonferroni adjustment for multiple testing. Difference-in-differences coefficients for k quarters away from the shock and their standard errors in parenthesis are displayed. (1) Main results: baseline results using CEM weighting for comparison. (2) No CEM: baseline results not using weights. (3) Future health shock: Control group only includes individuals with a future health shock. Based on the population and not on a random sample. (4) Shift treatment: Treatment period shifted to 2004q3-2005q4. (5) Severe health shock: Subset of health shocks with more than 6 hospital nights. (6) Nondeferrable health shock: Subset of health shocks that happen as frequently on weekends as on weekdays. (7) No hospitalisations: No parental hospitalisation from 1995q1-2001q1.

The results show that this subset of hospitalisations do not have labour market effects for their children either. In the column 'nondeferrable health shock', we restrict the parental health shocks to a narrower set of diagnoses for which the patients are hospitalised as frequently during the weekend as during the weekdays (see Card et al., 2009; Dobkin et al., 2018).[31] This implies that these conditions are nondeferrable. While this definition ensures unexpectedness, we do not use it in our main specification because it excludes many diagnoses that can be considered a health shock in the sense that they cannot be foreseen in $q-1$. For the subset of nondeferrable parental health shocks, we do not find different results than with the full set of parental health shock.

In the column 'No hospitalisations', 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 trends with the data available. With this stricter selection criterion, the sample is considerably reduced, since parental hospitalisations are a frequent phenomenon. The results are again very similar to our main results, providing further 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 of controls that we draw leads to similar result as with other random samples. We have conducted the main analysis for women's employment also on 99 other clustered random subsamples of controls. The treatment effects are never jointly significant, whereas the pre-treatment effects are jointly significant 16[32] times out of a 100. All pre-treatment and post-treatment coefficients contain zero between the 2.5th and 97.5th percentile of their distribution as illustrated by Figure A15 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.

5.4. The role of informal care and mental health

A parental health shock can negatively affect the labour market outcomes of the child in through informal care provision and through stress.[33] We explore whether these two are affected by a health shock to explore what might explain our results and to increase the external and internal validity of our findings. To this end, we use the Study on Transitions in Employment, Ability and Motivation (STREAM), a Dutch yearly panel data set covering the years 2010-2013 (Ybema et al., 2014). While it does not cover the whole Dutch population (there are 40,063 individual - year observations), this data set is useful since it contains information about employment, a serious disease of a close family member (excluding spouses) or friend in the past 12 months,[34] informal caregiving, and mental health. Therefore, we can reproduce our analysis using a similar set up, and additionally we can shed light on the channels - i.e. does a parental health shock not lead to informal caregiving/mental health decline, or does the take up of informal care/mental health decline just not translate into a labour market effect?

As an indicator for mental illness, we use a depression score (CES-D-10, range [0;30] where a higher score indicates more depressive symptoms) and a mental component summary scale (MCS12, range [0;100], where a higher score indicates better mental health). More information about these two measures can be found in (Bom et al., 2019).

Descriptive statistics show that 38% of the people experiencing a serious health event in the family are informal caregivers. Among all informal caregivers, 62% are employed. In terms of mental health, people with a serious health event in the family have on average a 0.6 point higher depression score, and report a 1.2 points worse overall mental health. Given the range and the mean of these two measures, these differences are very small. The employed are on average in better mental health.

We conduct two type of analyses for both the informal care and the mental health channel. First, we regress the variables for informal caregiving and the mental health measure on the onset of a serious health event of a close family member, individual fixed effects and a set of control variables: log of age, living with a partner, the financial situation of the household in five categories, and year. Others might consider using - or might have used - such a set up as the first stage regression in an instrumental variable analysis, but we refrain from this because we are not convinced that a serious illness of a parent is a valid instrument for informal caregiving: both the exclusion restriction and the monotonicity assumption might not be met. The mental health channel, and the fact that one cares *about* the care recipient mean that the exclusion restriction (Bom et al., 2019) is likely violated (and the other way around for mental health). Moreover, showing whether there is a strong relationship between a parental health shock and informal caregiving and a such a health shock and mental health problems is the most important to understand the main results of our study and this regression suffices for that.

The results (colum 1-6 of Table 8) show that the illness of a close family member is a strong predictor of informal

[31] By ICD9 diagnosis, we test if the proportion of weekend admissions is equal to $\frac{2}{7}=0.29$. If we do not reject H0, the diagnosis is defined as nondeferrable.

[32] We would expect significant results by chance only 5 times out of 100 random samples. However, when looking at effect size, the coefficients are on average -0.0005, and the largest coefficient is 0.006 in absolute value. This means that even if pre-trend effects are jointly significant, they are extremely small. Moreover, none of the coefficients are individually significant at 10%. We are therefore not concerned about the too high occurrence of joint significance of pre-trends in our random samples.

[33] These two might be interrelated as informal care may have a negative effect on the caregiver's mental health (Bom et al., 2019)

[34] This does not exactly coincide with the definition of a parental health shock used in the rest of the paper. However, the basic ingredients are there nevertheless. Parents are close family members, and the onset of a serious disease carries the notion of unexpectedness.

Table 8
The effect of a health shock on informal caregiving and mental health

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sample	Women	Men	Women	Men	Women	Men	Women	Men
Dep. variable	IC	IC	CES-D-10	CES-D-10	MCS12	MCS12	LMO	LMO
Employment (All)								
Serious health event	0.0740***	0.0663***	0.104	0.218***	−0.454***	−0.311**	−0.00616	−0.00283
	(0.008)	(0.007)	(0.081)	(0.075)	(0.158)	(0.146)	(0.008)	(0.007)
Observations	18,648	21,415	18,648	21,415	18,648	21,415	18,648	21,415
Working hours (Working population)								
Serious health event	0.0881***	0.0646***	0.0740	0.237***	−0.552***	−0.586***	−0.0848	0.0951
	(0.011)	(0.008)	(0.107)	(0.087)	(0.210)	(0.171)	(0.138)	(0.121)
Observations	10,469	15,460	10,469	15,460	10,469	15,460	10,469	15,460

Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Dependent variables: IC = provides any informal care; CES-D-10 = depression score with range [0;30] where zero indicates no depressive symptoms; MCS12 = mental component summary scale with range [0;100] where 100 indicates good mental health; LMO = labour market outcomes employment (top) or working hours (bottom). Control variables (ln age, living with a partner, the financial situation of the household in five categories, year and individual fixed effects) are not shown but included in the regressions.

caregiving for both the overall and the working sample, and the effects are large. A serious health event in the family seems to slightly increase depressive symptoms (if at all), and seems to reduce overall mental health. The coefficients are (mostly) statistically significant, but the effect size is very small given the range of the indicators and not economically significant. This suggests that informal caregiving may be the most affected (if we ignore interaction effects between informal caregiving and mental health).

In a second step, we reproduce a reduced-form model similar to the one estimated in the rest of the paper, where an indicator for illness of a close family member is regressed on employment and working hours while controlling for the log of age, living with a partner, the financial situation of the household in five categories, year and individual fixed effects. The results are displayed in column (7) and (8). As in the main analysis, we find that an illness of a close family member does not affect employment nor working hours for both genders.

These supplementary analyses are informative in three ways. First, the first analysis shows that a serious health event is related to informal caregiving in the Netherlands, while the second shows that such an event does not have labour market consequences. It is thus likely, that our no-effect finding in the main analysis is due to a no-effect of informal caregiving on labour market outcomes, but *not* because a parental health shock is unrelated to informal caregiving. Second, it reproduces the main analysis of this study for a different time period and a different sample, thus increasing the external validity of our results. Third, it provides suggestive evidence that informal caregiving may be a more common response to a health shock of a relative than a mental health decline.

6. 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 unexpected parental hospitalisations 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 frequent and correlated with formal and informal LTC use and thus relevant. We estimate an event study 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 such a health shock on the probability of employment and conditional earnings. The analysis of subsamples such as for example only children with an alone and close living parent, for whom we expected the effects to be larger, do not show any effects either. In some specifications, we find borderline significant effects, but the point estimates are too small to be economically significant. 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. Exploring potential explanations, we find that unexpected parental health shocks lead to informal caregiving, but do not affect mental health. Therefore, our zero result is likely to be due to the absence of an effect of informal care on labour market outcomes, and *not* due to lack of correlation of a parental health shock and informal caregiving.

This interpretation of our results is also in line with a recent report on caregiving and working in the Netherlands. de Boer et al. (2019) study working caregivers, among which 73% indicate that paid and care work can be combined. 25% of caregivers report that they accomodate their tasks by taking holidays. Only 10 % takes paid leave, and still fewer take unpaid leave (6%) or report sick (4%). Around one-third of the caregivers provide care on working days, whereas the two thirds do this on off-days/weekends. Hence, most caregivers seem to find ways to combine their paid job with caregiving tasks. Other 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 either (Meng, 2013; Viitanen, 2010; Moscarola, 2010; Josten and De Boer, 2015). The results then suggest that Dutch caregivers 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., 2015), which means that the demand for intensive informal care is short-lived or low and thus may be met by the child while having a paid job.[35]

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, 2017b). 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.

In addition to the generous LTC system, 40 percent of the 35-65 years old work part-time in the Netherlands (Statistics Netherlands, 2017a). Part-time workers have more time available outside their paid job that can be dedicated to caregiving tasks. This has two consequences. On the one hand, this may reduce the effect of caregiving on labour market outcomes. On the other hand, this may lead to sorting of part-time workers into informal caregiving. Our results show that these part-time workers do not adapt their degree of part-time work after a parental health shock, since we do not find earning effects. Descriptive statistics show patterns that are consistent with sorting behaviour (de Boer et al., 2019): non-caregivers work on average 35 hours per week, caregivers 33, and intensive caregivers 31.

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 and labour market institutions. In the Netherlands, where the LTC system is generous and comprehensive and part-time work widespread, 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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Appendix A

Tables A1–A7.
Figs. A1–A15

[35] The onset of formal care does not imply that informal caregiving stops, however. More than half of the Dutch informal caregivers who provide care to someone outside of their own household does so together with at least one formal caregiver (De Klerk et al., 2017).

Table A1
Data sets

Data set	Version	Content
PARTNERBUS	V1 2015	Partner identification
GBAPERSOONTAB	V1 2015	Basic personal data
Do	V1 1995-2005 &2009-2011, V2 2006-2008	Death register
GBAADRESOBJECTBUS	V1 2017	Address register
VSLGWBTAB	V1 2018	Address municipality codes
KINDEROUDERTAB	V2 2015	Children parent linkages
LMR_Basis	V2 1999-2004, V3 2005	Hospital admissions
BAANKENMERKENBUS	V3 1999-2006 &2008-2011, V2 2007	Employment
BAANSOMMENTAB	V3 1999-2005, V2 2006-2011	Earnings
STREAM	n.a.	Survey data from TNO

Information about the data sets can be found at https://www.cbs.nl/nl-nl/onze-diensten/maatwerk-en-microdata/microdata-zelf-onderzoek-doen/catalogus-microdatahttps://www.cbs.nl/nl-nl/onze-diensten/maatwerk-en-microdata/microdata-zelf-onderzoek-doen/catalogus-microdata (available in Dutch only). The STREAM data are described in Ybema et al. (2014).

Table A2
Frequencies of main diagnosis groups for hospitalisations not classified as a health shock among the 55+ hospitalised population in 1999-2005

ICD9 diagnosis group	Frequency	%
Diseases of the nervous system	1,178,845	16%
Musculoskeletal diseases	1,045,818	15%
Circulatory diseases	1,001,110	14%
External causes of injury and supplemental classification	874,510	12%
Symptoms, signs, and ill-defined conditions	793,792	11%
Diseases of the digestive system	680,621	10%
Diseases of the genitourinary system	429,479	6%
Respiratory diseases	314,601	4%
Cancers	249,338	3%
Endocrine diseases	205,375	3%
Blood diseases	187,252	3%
Mental disorders	87,165	1%
Skin diseases	86,307	1%
Infectious diseases	17,465	0%
Congenital diseases	8,910	0%
Pregnancy related	18	0%
Conditions originating in the perinatal period	13	0%

Table A3
5 most frequent diagnoses not defined as health shocks among the 55+ hospitalised population in 1999-2005

Diagnosis	ICD9-CM code	Frequency	%
Senile cataract	366.1	537,414	8%
Unspecified cataract	366.9	319,584	4%
Osteoarthrosis, localized	715.3	252,466	4%
Coronary atherosclerosis	414.0	234,215	3%
Chest pain	786.5	177,516	2%

Table A4
Linear probability model for parent's mortality in 2008q2 after a health shock

	(1) Mortality mother	(2) Mortality father
Unexpected hospitalisation	0.0676***	0.199***
	(0.000)	(0.001)
First generation migrant	0.0785***	0.0770**
	(0.001)	(0.001)
Second generation migrant	0.00808***	−0.00362***
	(0.001)	(0.001)
Birth year	−0.0223***	−0.0233***
	(0.000)	(0.000)
Partnered	0.0862**	0.0804***
	(0.000)	(0.000)
Constant	2.524***	2.559***
	(0.002)	(0.002)
Observations	2,828,507	2,738,722
R-squared	0.517	0.466

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

Table A5
LTC use after hospitalisation for health shocks patients among the 23 most common diagnoses of Dutch Hospital Patients aged 65+ in 2004

Condition	% of sample	Formal care %	Home care %	Home for the elderly %	Nursing home %
Lung cancer	1.1	54.2	50.1	1.3	2.9
Ovary cancer	0.2	51.9	47.3	1.9	2.7
Intestinal, stomach and rectum cancer	2.2	50.2	46.1	1.6	2.6
Uterus cancer	0.3	34.9	32.0	1.7	1.2
Fracture of femur	1.7	53.8	29.9	5.5	18.4
Fracture of ankle of lower leg	0.4	42.4	26.7	4.8	10.9
Fracture of elbow and forearm	0.5	32.1	24.4	2.5	5.1
Bladder cancer	1	25.8	23.9	0.6	1.3
Prostate cancer	1.3	22.9	20.2	0.8	2.0
Cerebrovascular disease	3.6	38.5	17.9	1.4	19.2
Intracranial injury	0.6	27.1	17.4	2.2	7.5

Source: Wong et al. (2010).

Table A6
Women wage summary statistics

	Control		Treatment		Control	Treatment
Variable	Unweighted Mean	Weighted Mean	Unweighted Mean	Weighted Mean	Unweighted StdDiff	Weighted Diff
Age	45.1	45.2	45.2	45.2	−0.02	−0.01
Age mother	73.2	73.7	74.0	74.0	−0.07	−0.03
Age father	75.8	76.3	76.5	76.6	−0.06	−0.02
Living with a partner	0.11	0.10	0.11	0.10	0.00	0.00
Dutch	0.92	0.93	0.92	0.93	−0.01	0.00
1st generation migrant	0.03	0.02	0.03	0.02	0.00	−0.01
2nd generation migrant	0.06	0.05	0.05	0.05	0.01	0.01
Number of siblings	2.0	1.6	1.6	1.7	0.14	0.00
Number of kids < 13	0.5	0.5	0.5	0.5	0.02	0.00
Father has partner	0.5	0.6	0.6	0.6	−0.09	0.00
Mother has partner	0.5	0.5	0.5	0.5	−0.08	0.00
Distance residence mother in km	26.2	26.7	28.3	28.1	−0.03	−0.02
Distance residence father in km	27.0	27.8	39.7	39.4	−0.19	−0.17
Number of jobs	1.1	1.1	1.1	1.1	0.00	0.00
Quarters employed in the main job	29.7	29.8	29.5	29.7	0.01	0.00
Distance to closest parent	24.9	25.0	24.0	24.0	0.02	0.02
One parent dead	0.29	0.13	0.13	0.13	0.28*	0.00
Age oldest parent	76.2	76.6	76.8	76.8	−0.06	−0.02
N	142,970	132,927	77,366	76,164		

* 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 A7
Men wage summary statistics

	Control		Treatment		Control	Treatment
Variable	Unweighted Mean	Weighted Mean	Unweighted Mean	Weighted Mean	Unweighted StdDiff	Weighted StdDiff
Age	45.8	45.8	45.9	46.0	−0.01	−0.02
Age mother	73.9	74.3	74.6	74.6	−0.06	−0.03
Age father	76.6	76.9	77.1	77.1	−0.05	−0.02
Living with a partner	0.13	0.12	0.13	0.12	0.00	0.00
Dutch	0.92	0.93	0.92	0.93	−0.02	0.00
1st generation migrant	0.03	0.02	0.03	0.02	0.02	0.00
2nd generation migrant	0.05	0.05	0.05	0.05	0.01	0.00
Number of siblings	2.0	1.6	1.6	1.7	0.15	0.00
Number of kids < 13	0.7	0.7	0.7	0.7	0.01	0.00
Father has partner	0.5	0.5	0.5	0.5	−0.10	0.00
Mother has partner	0.5	0.5	0.5	0.5	−0.10	0.00
Distance residence mother in km	24.4	25.1	26.7	26.5	−0.04	−0.02
Distance residence father in km	25.3	26.3	39.6	39.4	−0.21	−0.19

Table A7 (*Continued*)

Variable	Control Unweighted Mean	Control Weighted Mean	Treatment Unweighted Mean	Treatment Weighted Mean	Control Unweighted StdDiff	Treatment Weighted StdDiff
Number of jobs	1.1	1.1	1.1	1.1	0.00	0.01
Quarters employed in the main job	43.0	43.0	42.9	43.1	0.00	0.00
Distance to closest parent	22.9	23.4	22.3	22.3	0.01	0.02
One parent dead	0.30	0.13	0.14	0.13	0.30*	0.00
Age oldest parent	76.9	77.2	77.4	77.4	−0.04	−0.02
N	204,680	189,933	109,130	107,848		

* 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.

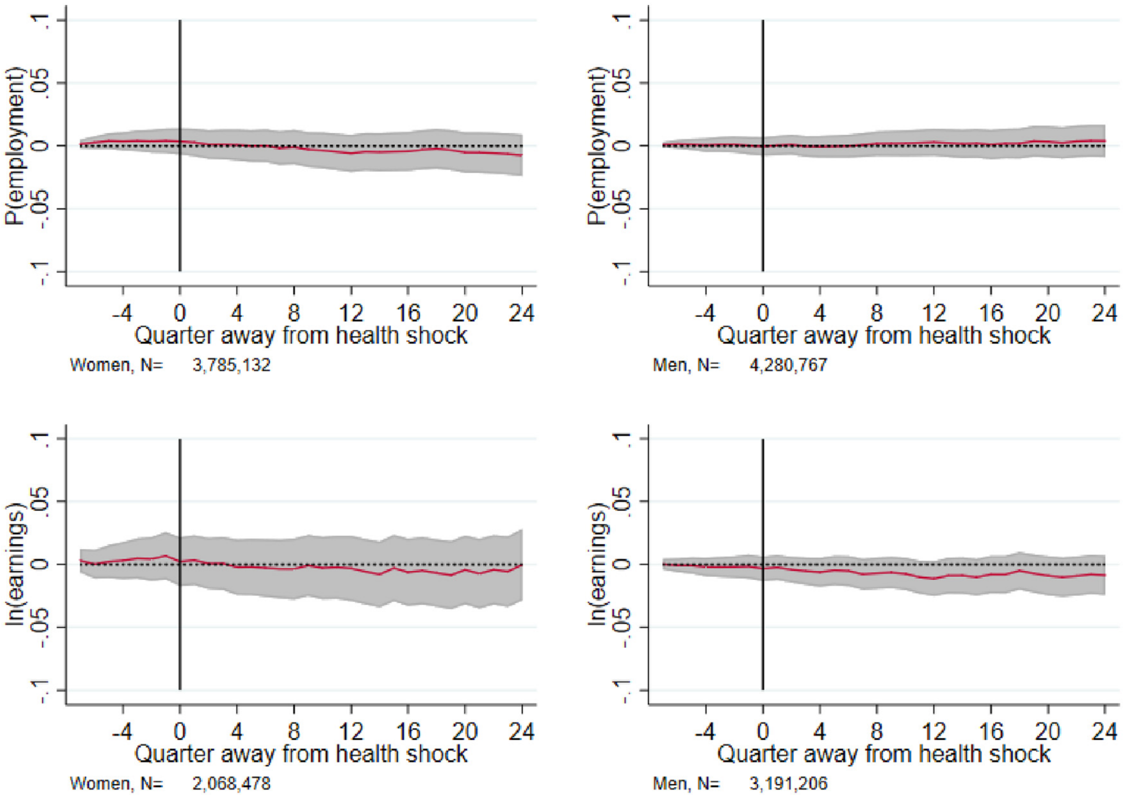

Fig. A1. Parents living in a 5km radius. The grey shaded areas correspond to the Bonferroni adjusted 95% confidence intervales.

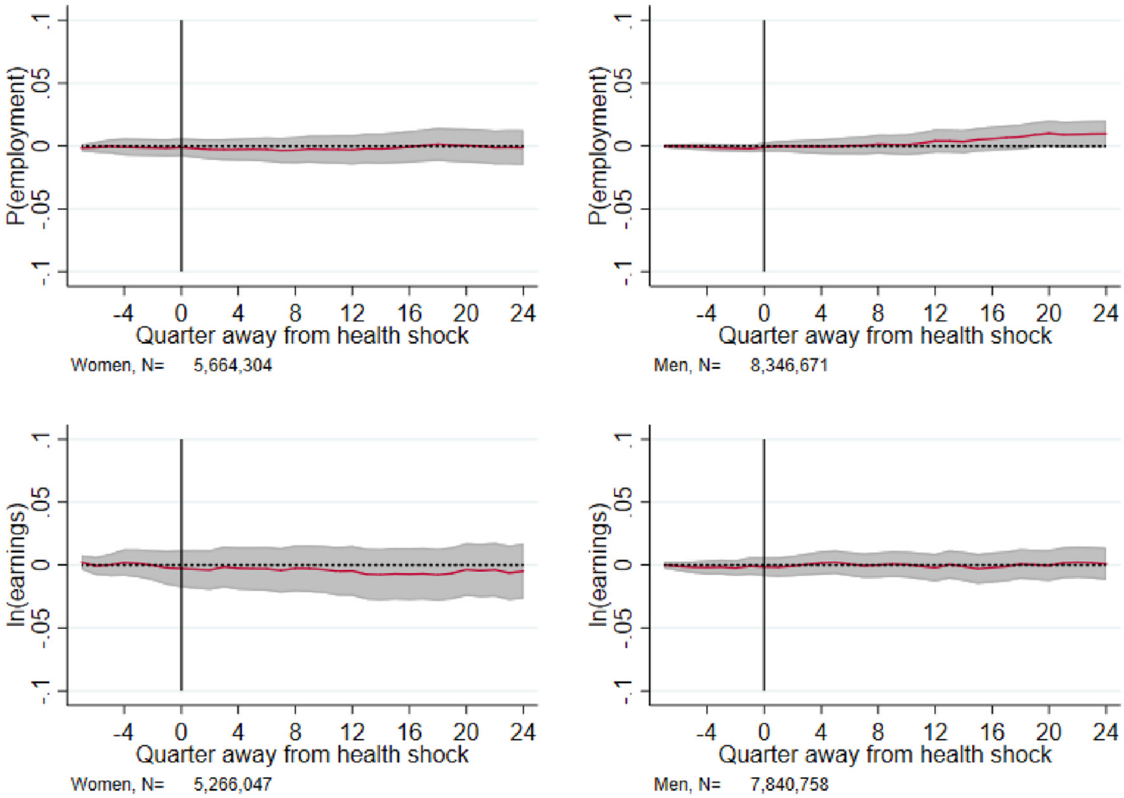

Fig. A2. Employed 1 year before the shock. The grey shaded areas correspond to the Bonferroni adjusted 95% confidence intervales.

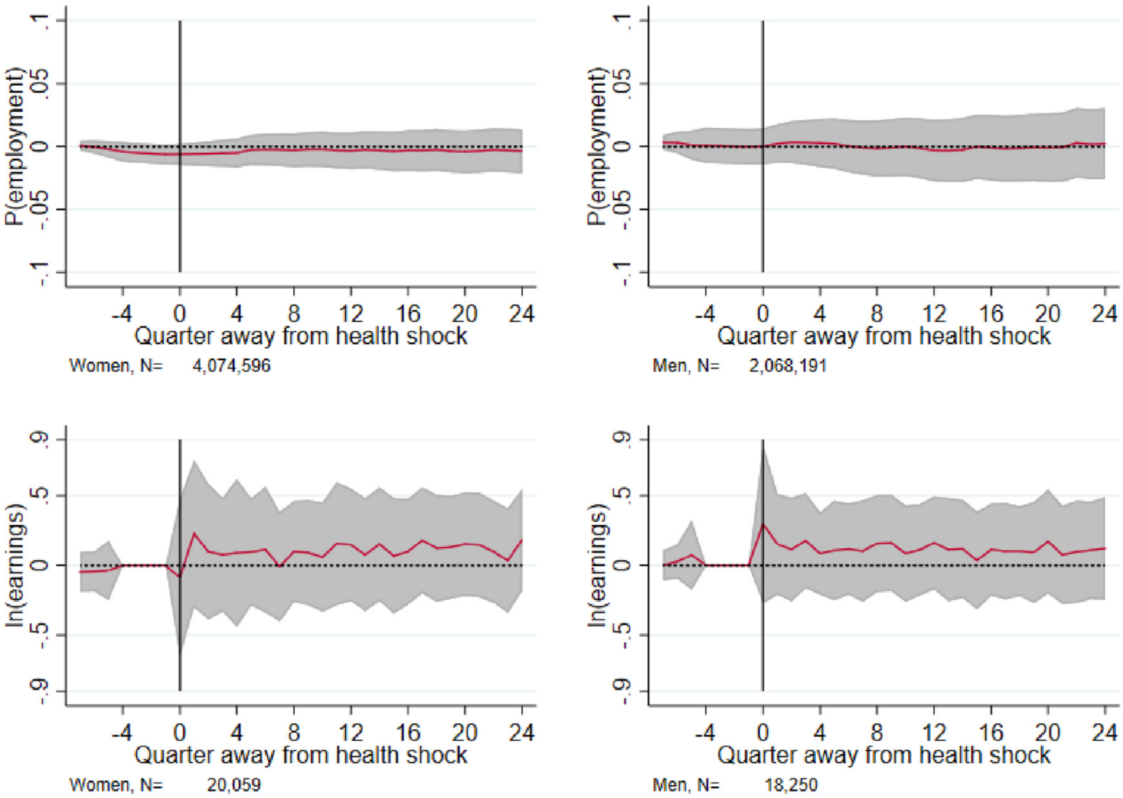

Fig. A3. Not employed 1 year before the shock. The grey shaded areas correspond to the Bonferroni adjusted 95% confidence intervales.

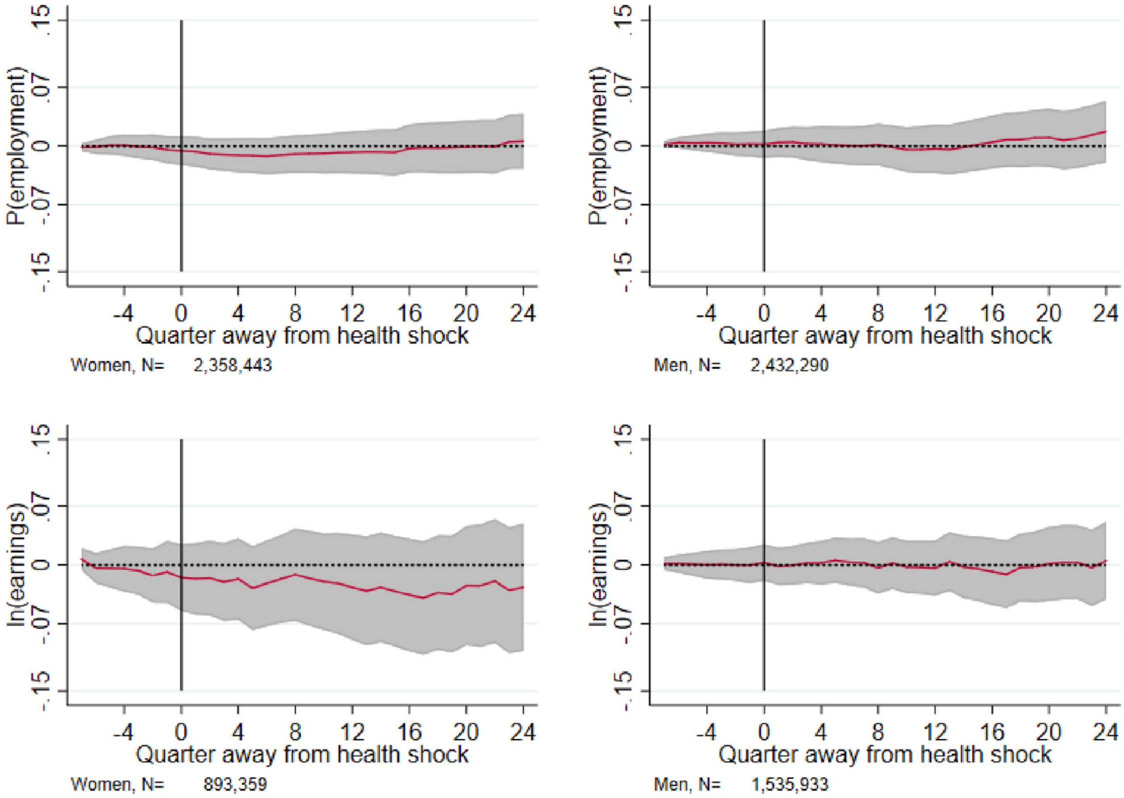

Fig. A4. Parents aged 80+. The grey shaded areas correspond to the Bonferroni adjusted 95% confidence intervales.

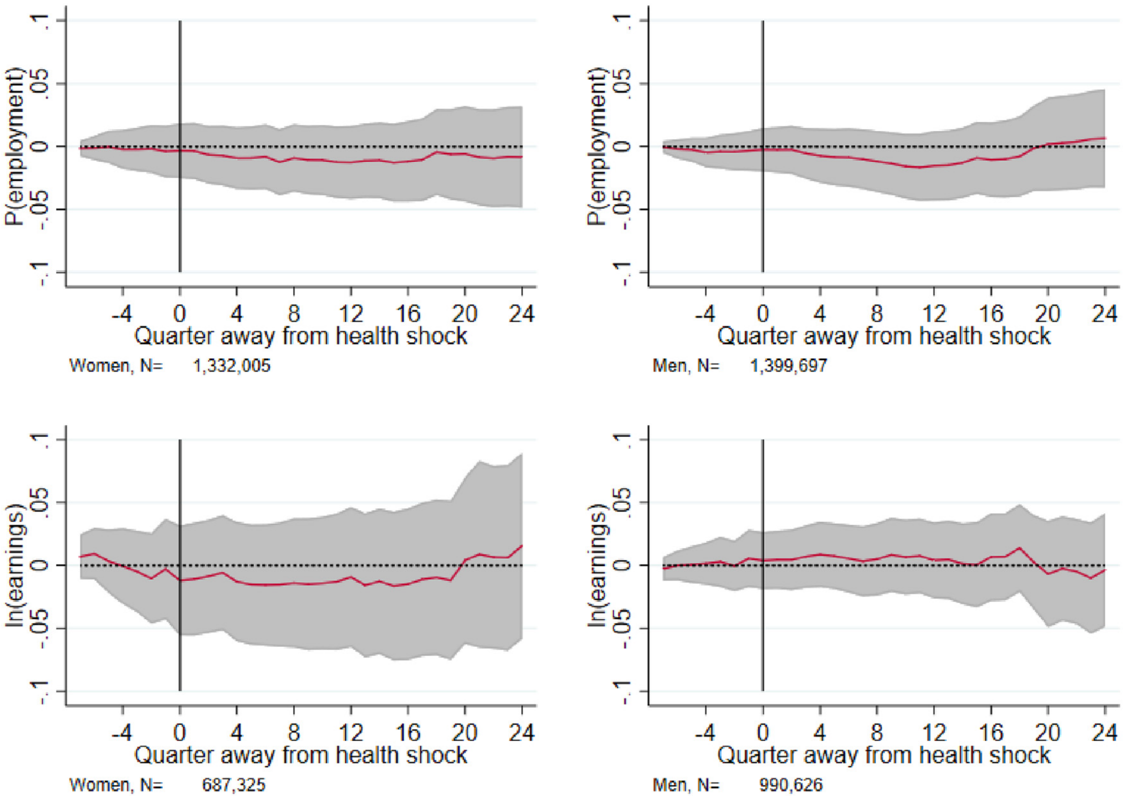

Fig. A5. Only children. The grey shaded areas correspond to the Bonferroni adjusted 95% confidence intervales.

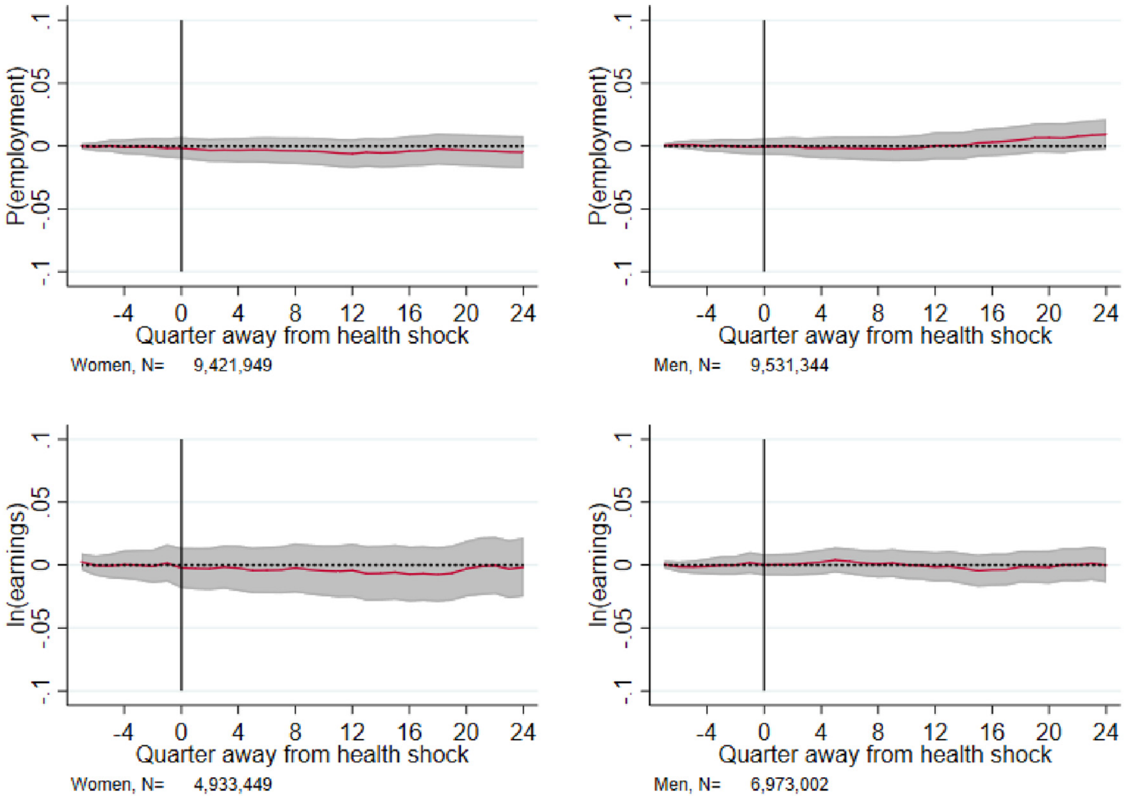

Fig. A6. Alone living children. The grey shaded areas correspond to the Bonferroni adjusted 95% confidence intervales.

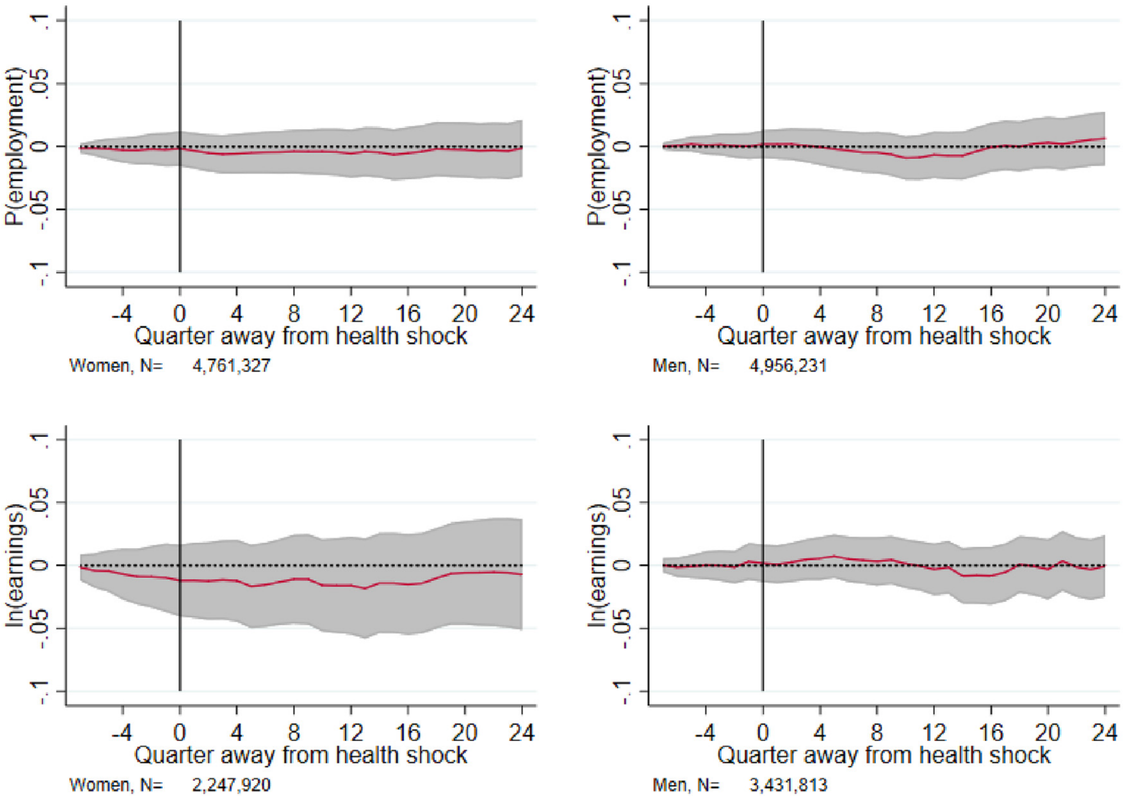

Fig. A7. Alone living parents. The grey shaded areas correspond to the Bonferroni adjusted 95% confidence intervales.

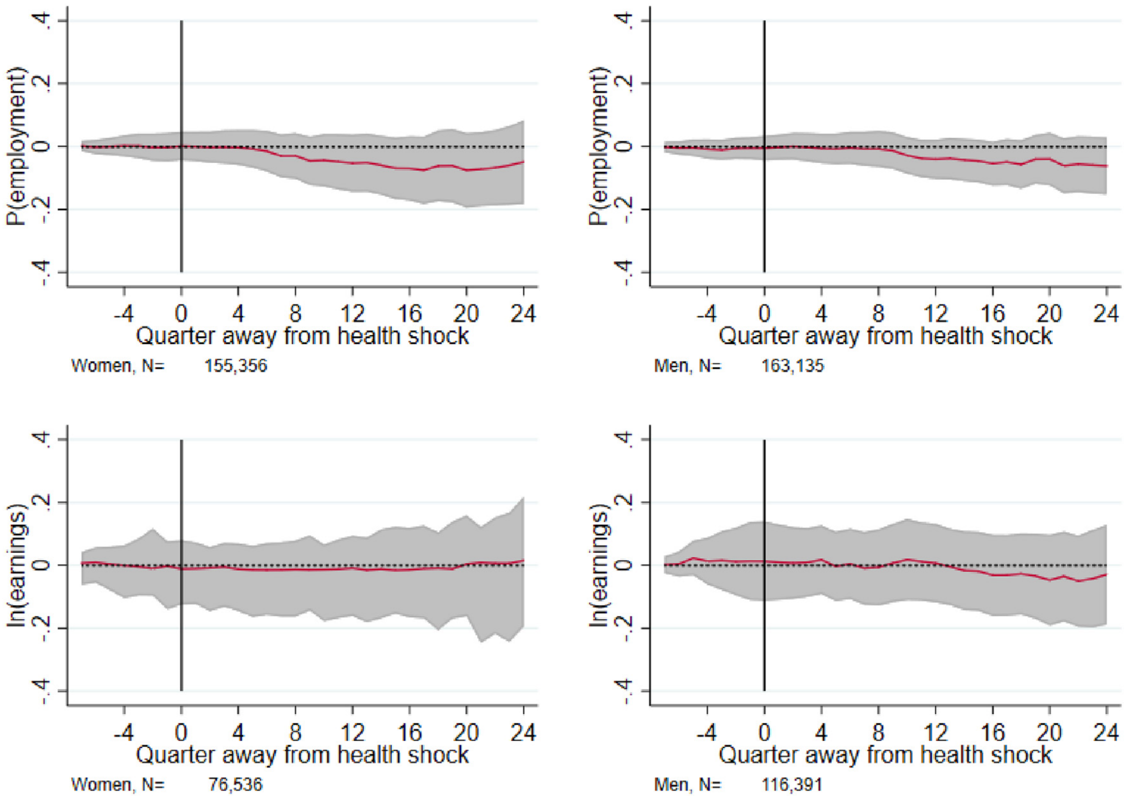

Fig. A8. Only children with single close living parent.

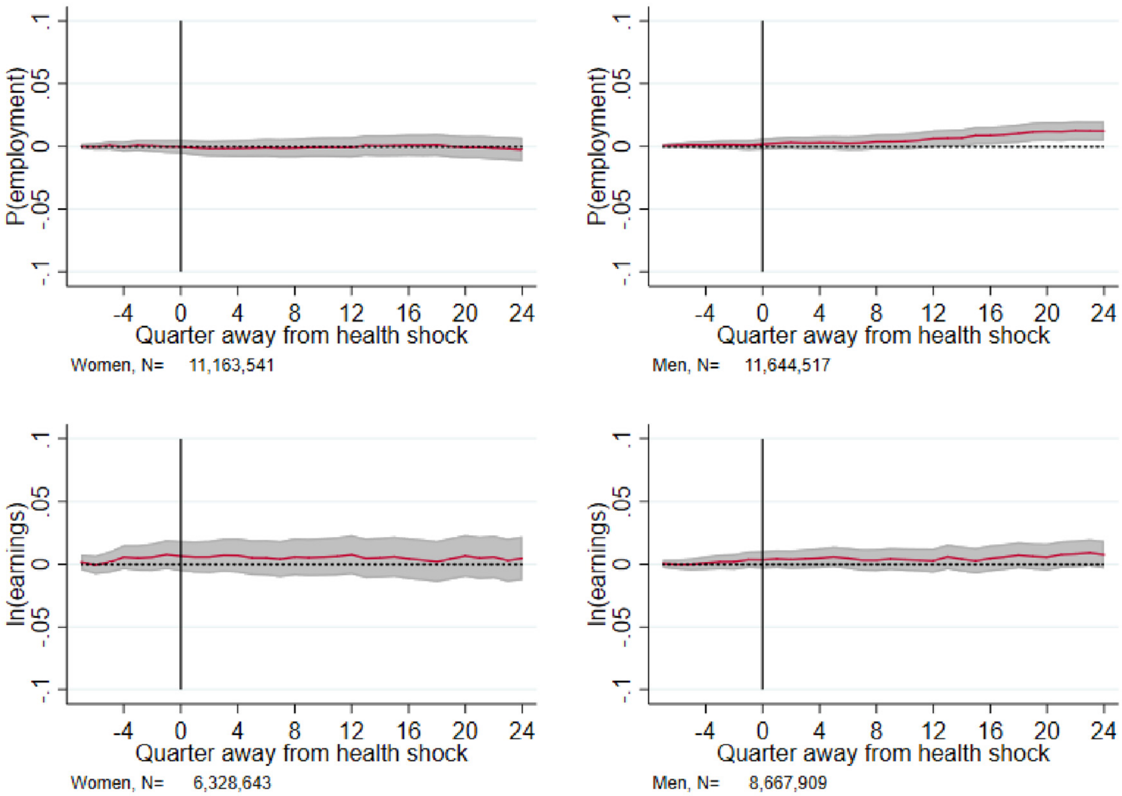

Fig. A9. Unweighted. The grey shaded areas correspond to the Bonferroni adjusted 95% confidence intervales.

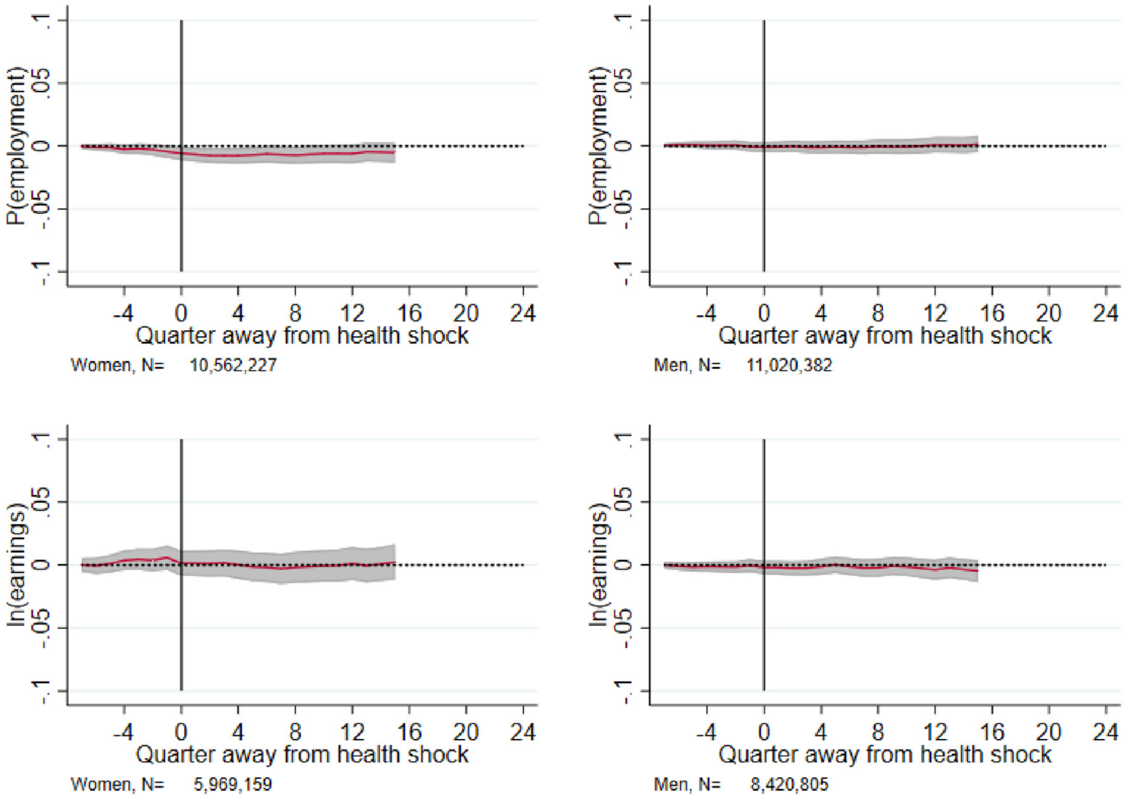

Fig. A10. Control group with future health shock. The grey shaded areas correspond to the Bonferroni adjusted 95% confidence intervales.

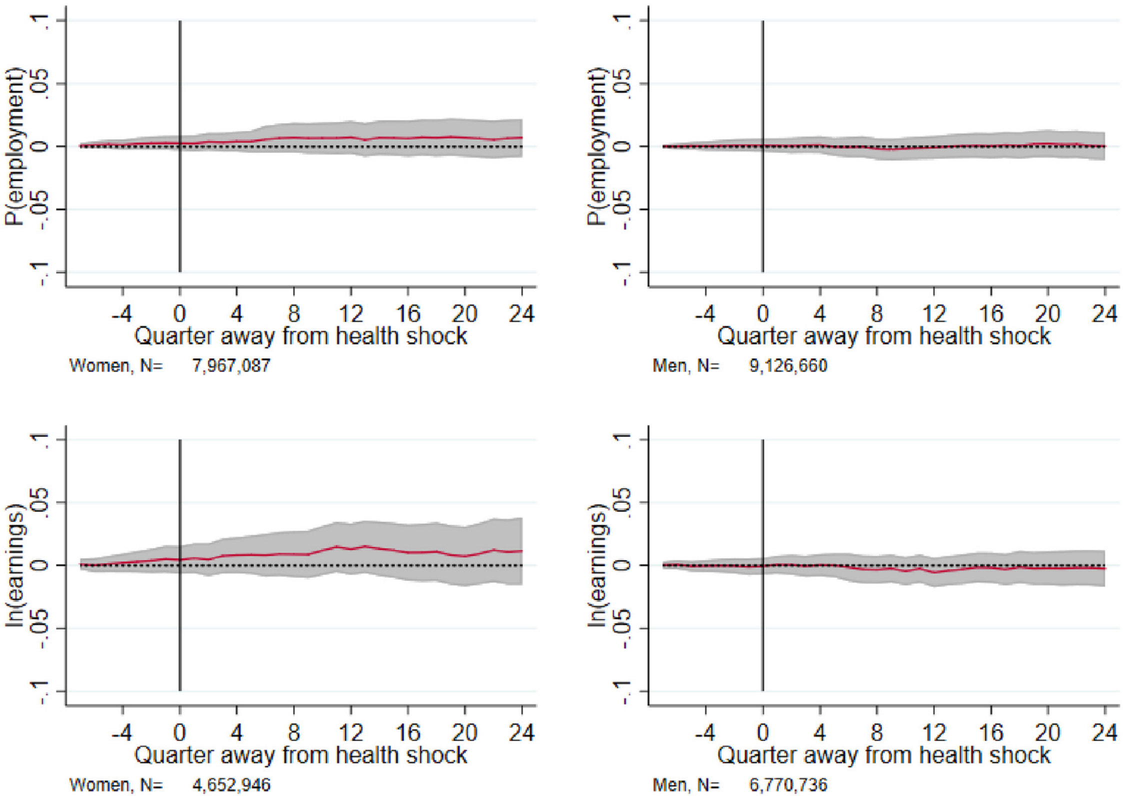

Fig. A11. Shift treatment period to 2004q3 - 2004q4. The grey shaded areas correspond to the Bonferroni adjusted 95% confidence intervales.

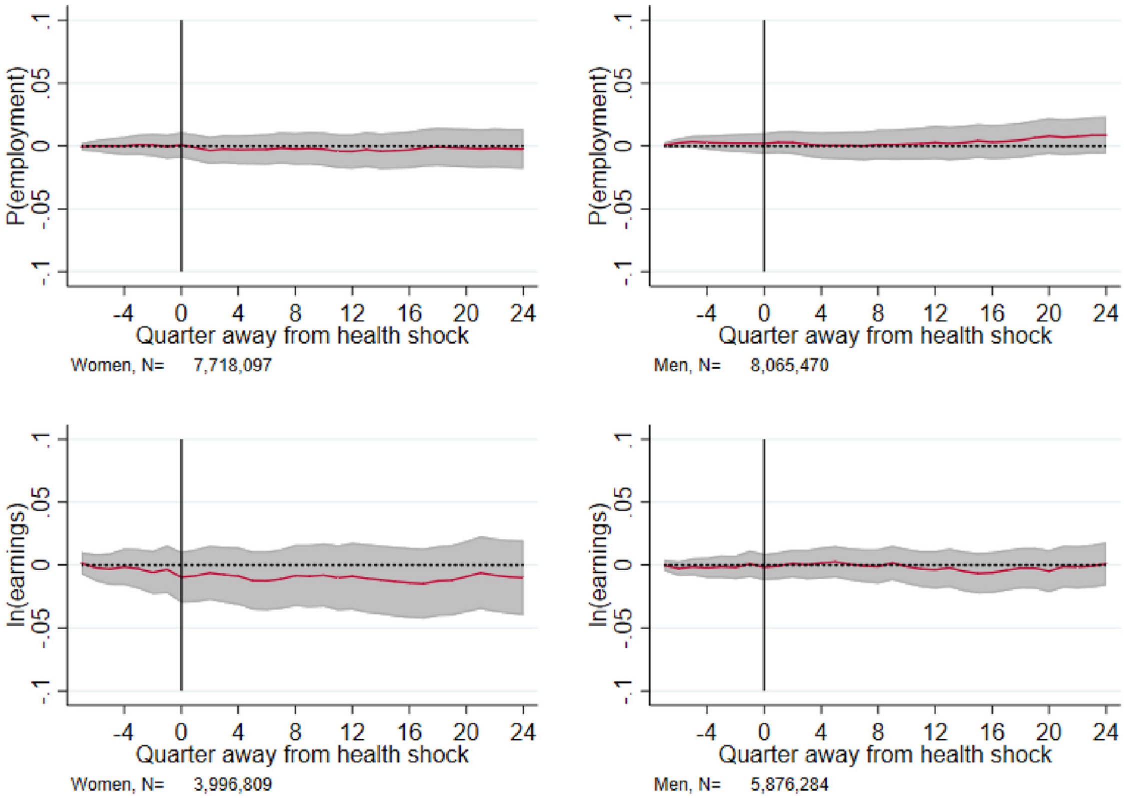

Fig. A12. Severe health shocks. The grey shaded areas correspond to the Bonferroni adjusted 95% confidence intervales.

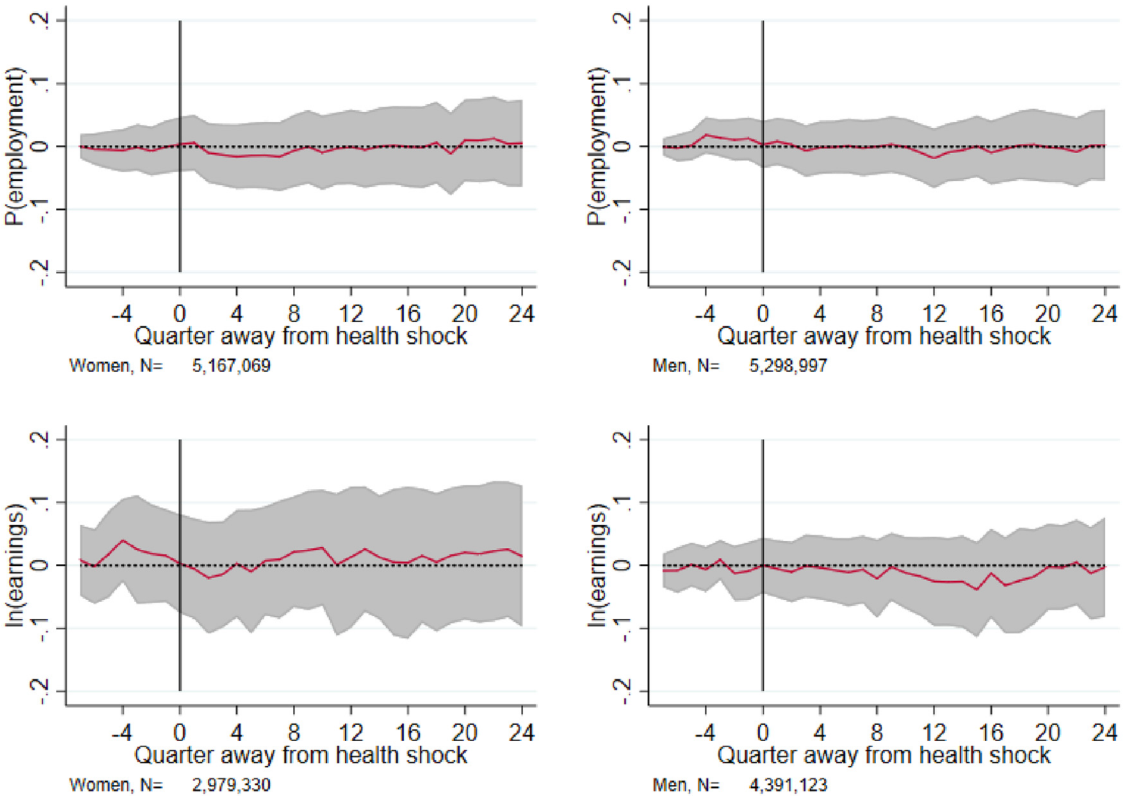

Fig. A13. Nondeferrable health shocks. The grey shaded areas correspond to the Bonferroni adjusted 95% confidence intervales.

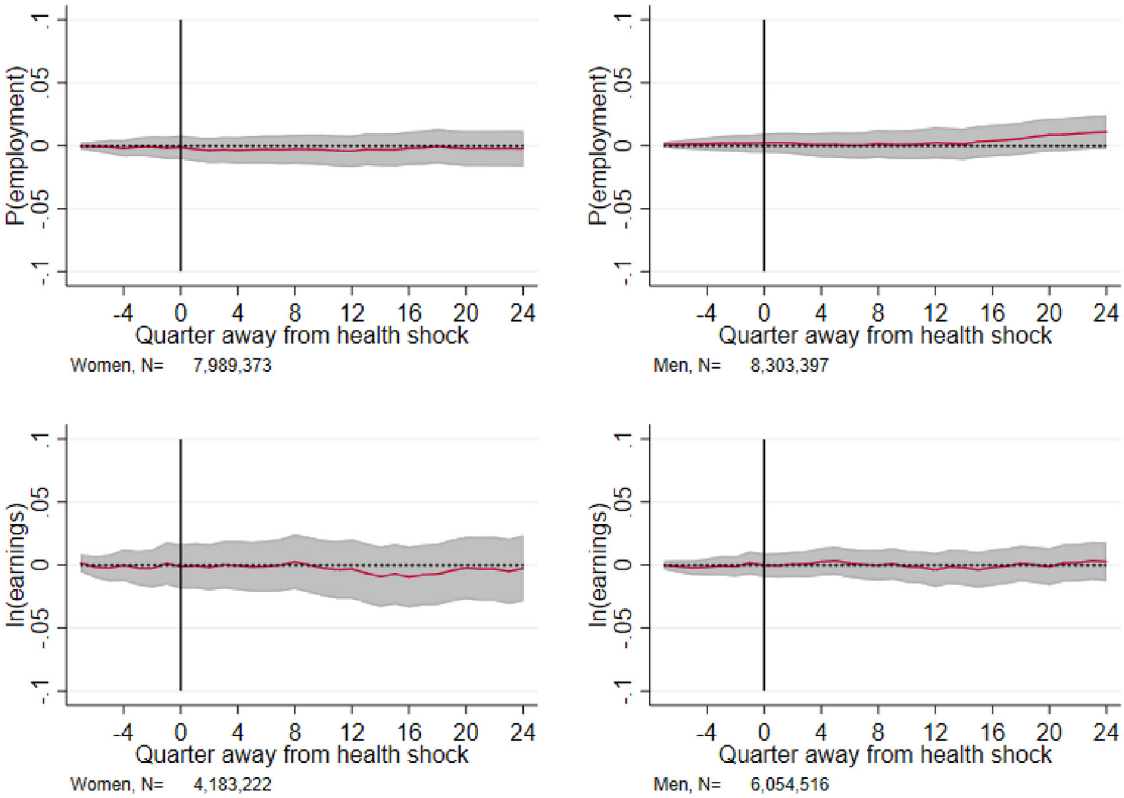

Fig. A14. Drop *all* parental hospitalisations between 1995q1-2000q4. The grey shaded areas correspond to the Bonferroni adjusted 95% confidence intervales.

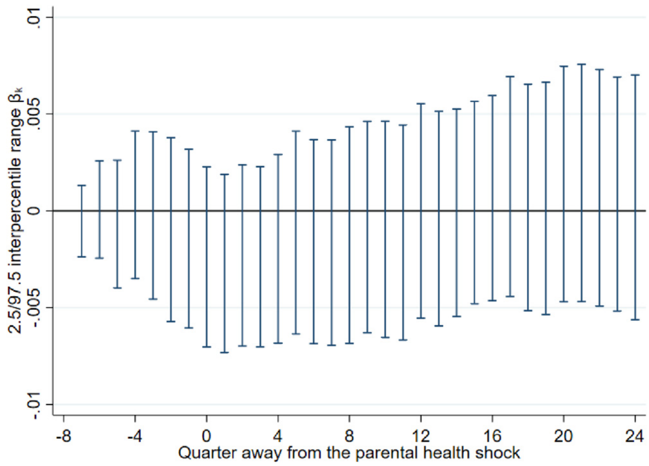

Fig. A15. 2.5 to 97.5 interpercentile range of β_k from 100 different random control group samples (Women employment).

Appendix B. Supplementary Data

Supplementary data associated with this article can be found, in the online version, at https://doi.org/10.1016/j.jhealeco.2019.102275.

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