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Balancing Paid Work and Unpaid Care over the Life-Cycle

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Balancing Paid Work and Unpaid Care over the Life-Cycle

Betaald en onbetaald werk balanceren gedurende de levensloop

Thesis

to obtain the degree of Doctor from the
Erasmus University Rotterdam
by command of the
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Prof.dr. F.A. van der Duijn Schouten

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Chapter 1

Introduction

The existence of human beings or any other species relies on reproduction. Reproduction in a broad sense, however, is not completed after having given birth, but it encompasses many other activities that can be summarised with the concept of 'reproductive work'. Reproductive work consists of all the tasks that contribute to nurturing and supporting all members of society (EIGE, 2020; Charmes, 2019). It consists thus of biological reproduction, but extends to many other types of activities that have traditionally been taken care of by the (extended) family. Reproductive work is crucial for societies, as children are future workers, tax payers, contributors to pension systems, innovators and parents who may have children themselves. Once these children have grown up, they are not dependent anymore - but they still rely on reproductive work, as they need to eat or have a clean home to live in. Finally, at some point in life, due to sickness or old age, people become dependent again, and reproductive work ensures that they can age with dignity.

One distinctive feature of reproductive work is that a large part of it is unpaid, such as for example cooking dinner for your family, or raising your children. A part of reproductive work can be outsourced from the family and hence is paid, for example when bringing children to a child care facility or when an elderly care-dependent person goes to a nursing home. Historically, the separation between paid work and unpaid work became more pronounced during the industrial revolution, when families transformed from production units with no clear boundary between products for the market or for own use into households with paid workers and unpaid housework (Folbre, 1991). Due to its unpaid nature, the separation between paid and unpaid work also resulted in a monetary devaluation of unpaid work.

Today, unpaid care and home production account for a large part of our economies. For example for the Netherlands in 2006, the OECD (2011) estimates that unpaid work[1] has a value of about 25% of GDP using the replacement cost[2] approach, or close to 50% with the opportunity cost approach.[3]

Most reproductive work today is provided by women. For example in the EU in 2010, women spent on average 2 hours per day more than men on 'household and family care' (Eurostat, 2020). More recently, data on the lockdown policies in response to the corona virus suggests that women took over more of the child care and home schooling tasks than men (Biroli et al., 2020; Sevilla and Smith, 2020; Farré et al., 2020).

The division of labour between paid and unpaid work in the household is the result of bargaining between household members. Every family has to decide on how its members allocate their time between paid work, unpaid work, and leisure so that they have enough money available, they are happy with their work-life balances, and that all family members get the care they need. The negotiation on who engages in what type of work may be guided by earnings potentials on the labour market, preferences, government policies, culture and norms among others. Engaging in reproductive work has thus an opportunity cost in time and energy that one cannot devote to paid work or leisure. On the individual level, this can also have negative consequences such as missed career opportunities, financial dependency and lower earnings.

The individual negative consequences of doing reproductive work are reinforced by its low valuation by society, as the time spent for unpaid work cannot compensate for the 'time lost' on the labour market. For example, when applying for a job, having managed a household with two children for ten years will be not be favourably regarded by employers, even though it could be viewed as gaining experience in organisation and management. The low valuation of reproductive work can be explained by two features that reduce the bargaining

[1] The OECD defines unpaid work as the production of goods and services produced by family members that are not sold on the market, but that could be purchased from a third person not belonging to the family.

[2] In the replacement-cost approach, the value of unpaid work is calculated using the cost of hiring a worker to perform the task. Since paid care work is usually not well paid (England et al., 2002), this method is likely to underestimate the value of the task performed.

[3] The opportunity cost approach uses the market wage an unpaid worker could have earned in his/her job to proxy the value of unpaid work. This method may be overestimating the value of unpaid work for people with very high market wages, and for people who are not active in the labour market. For people without a paid job, it is difficult to impute a market wage - it may overestimate their market wage if they selected out of paid work because of unobserved characteristics that lower their productivity compared to similar individuals that have paid work.

power of the caregiver (Folbre, 2018). First, it is difficult to attach a monetary value to care work, as its quality depends largely on the quality of the interpersonal relationship. When the value of something is unclear, bargaining power is low. Second, care work is often intrinsically motivated. The caregiver cares about the care recipient and therefore derives utility from the process, and hence his outside options are limited. At the same time, the intrinsic motivation drives people to engage in reproductive work despite its potential economic disadvantage.

There is a tension between the personal economic costs of engaging in reproductive work and its necessity and desirability for society as a whole. One way to reduce this tension is state intervention. The welfare state may alleviate the negative individual consequences of reproductive work by making care commitments less time-intensive or energy-intensive. For example, elderly care, which was traditionally the task of the family, is now partly provided for by the state in the form of home or institutional care. Furthermore, childcare is available for parents, enabling both to take part in the labour market, and the health care system takes over part of caring for the sick. While the state can take over some of the reproductive work, it cannot take over all of it, because it relies on specific interpersonal relationships such as family ties. For example, a nurse can help with dressing and giving medication, but he cannot replace the personal connection that family members have with the care recipient. For this reason, even when the welfare state is highly developed, a considerable part of reproductive work is still delivered as unpaid work from family members.

In this thesis, I examine potential labour market effects of engaging in reproductive work in the Netherlands of the beginning twenty-first century, where a relatively generous welfare state could alleviate potential negative consequences of involving in reproductive work. Furthermore, I show to what extent women are affected disproportionately by the costs of reproductive work, since they have traditionally been responsible for it. The three chapters are dedicated to different stages in human life that involve reproductive work: before birth, childhood, and old age. For all three chapters, I link individual level demographic information to labour market and health outcomes from administrative data provided by Statistics Netherlands (CBS). Methodologically, I rely on new advances in event study methods to estimate causal effects.

Chapter 2, written together with Pieter Bakx and Pilar García-Gómez, focuses on early pregnancy, which stands in the very beginning of reproduction. However, around 10-20% of

pregnancies are not successful and end in early pregnancy loss. This chapter examines how pregnancy loss, which is a natural part of reproduction, may affect labour market outcomes and the mental health of parents-to-be. Economic consequences of reproductive work do not have to be limited to labour market outcomes. In this chapter, we also examine mental health, as mental health declines may be the main source of burden in this context. Indeed, we find that early pregnancy losses increase mental health care use for both women and their partners. Labour market outcomes are largely unaffected by an early pregnancy loss when accounting for subsequent fertility.

The second chapter contributes to the existing literature of the mental health effect of early pregnancy loss and the economics of grief. We establish whether women get mental health care after an early pregnancy loss, which is important given that previous literature has found an association with/an effect of an early pregnancy loss on mental health. Furthermore, we are the first to examine the labour market consequences of an early pregnancy loss. Therefore, we contribute to the economics of grief literature by analysing a different grief entailing event that occurs frequently.

In the third chapter, I focus on the period (right) after children are born. This is a period when children need a lot of care and attention, such that parents reduce their labour market involvement. Raising children spans over a long period of time, and hence a career break related to child rearing is likely to be long-lasting with substantial foregone earnings. In this context, I analyse the labour market effect of parenthood, and I find that labour market costs related to children are substantial for women but negligible for men. In a second step, I show that gender norms are important for explaining the difference in effect for mothers and fathers.

Chapter 3 contributes to the literature on gender wage gaps, the labour market effects of parenthood in gender, and its interaction with gender norms. While the body of studies examining gender wage gaps is large, evidence on the connection between labour market costs of children for women and gender norms is scarce. Among others, I take advantage of the difference in prevailing gender norms in the Dutch Bible Belt and other parts of the Netherlands to demonstrate the importance of gender norms for women's labour market costs associated with children.

Unpaid care work is not only necessary to bring up children, but also when older adults get sick. In many developed countries, the state is highly involved in care provision to

the elderly. Population ageing puts pressure on state budgets for elderly care, and leads governments to encourage informal elderly care, since this appears less costly for the state. However, this reasoning abstracts from potential labour market consequences for caregivers, which may be costly for caregivers and potentially reduce state revenue. Chapter 4, which is joint work with Pieter Bakx, Pilar García-Gómez, and Eddy van Doorslaer, evaluates the labour market consequences of a parental health shock for middle-aged children in the Netherlands. The Netherlands is an interesting framework to study this question, as it is the OECD country with the highest long-term care spending (3.7% of GDP; OECD, 2019b). 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.

In Chapter 4, we contribute to the literature on health shocks of family members. In contrast to other studies which evaluate the effect of a spousal health shock, we focus on the effect of a parental health shock. This is an event that entails labour market consequences for a larger group of the population, as most spouses are not in the labour force anymore when their partner incurs the health shock. Children, on the other hand, are still in the working age population when their parent's health declines. Furthermore, we contribute to the literature on labour market effects of informal elderly care. By estimating a reduced form effect, we avoid having to distinguish between 'caring for' and 'caring about'.

The results of this thesis show that the labour market consequences of reproductive work vary strongly at different stages of life. In the beginning of the reproductive cycle, an early pregnancy loss has no labour market consequences for women, but mental health for both women and their partners declines. After the first child, there are large and long-lasting labour market reductions for women, but not for men. Once an elderly parent suffers from a health shock, both men and women do not adjust their labour market involvement. This suggests that child rearing is the activity of reproductive work that entails most negative consequences on the labour market, despite the availability of child care. These consequences are not shared equally - women incur most of the labour market costs of child rearing that remain, even in a context with a generous welfare state.

Chapter 2

The Effect of an Early Pregnancy Loss on Mental Health, Labor Market, and Family Outcomes

Joint with Pieter Bakx, and Pilar García-Gómez

2.1 Introduction

A miscarriage[4] is the most common form of early pregnancy loss, and it is the most prevalent pregnancy complication: around 10-20% of detected pregnancies end in miscarriage (Michels and Tiu, 2007). Due to their high prevalence, early pregnancy losses are a part of having children (Freidenfelds, 2019). Given that an increasing number of couples delay childbirth and age of both the mother- and the father-to-be are a risk-factor for miscarriage (Andersen et al., 2000; Kleinhaus et al., 2006), early pregnancy losses may become even more frequent in the future.

An early pregnancy loss may be difficult for both the woman and her partner, and it may lead to a mental health decline for a number of reasons. First, it is easy to avoid unwanted pregnancies nowadays. This means that most pregnancies, when they occur, are wanted or at least welcomed. A pregnancy loss may put a sudden end to the anticipation of having a (new member in the) family. Furthermore, the risk of a miscarriage may be underestimated (Banno

[4] A miscarriage is defined as fetal death in the first 20 weeks of gestation (Dulay, 2019b)

et al., 2020). Moreover, many women think that the early pregnancy loss is their fault, even though the most common cause is a random non-viable combination of genes, implying that the particular genetic combination of the fetus is not able to develop into a healthy baby independently of the woman's behaviour. These misconceptions are upheld also because there is a stigma on early pregnancy loss. For this reason, pregnancies are often not talked about until after the first 12 weeks and only a minority of women share their experiences with their social environment (Bellhouse et al., 2018).[5] Furthermore, technological progress has enabled women to detect pregnancies very early and has "fed the expectation that careful planning and loving care ought to produce perfect pregnancies", leading to the illusion that everything in life is projectable (Freidenfelds, 2019). Finally, despite evolving gender roles, motherhood is perceived as the cornerstone of female identity (Wager, 2000; Gillespie, 2003; Bell, 2019). For some women, a pregnancy loss may feel like a failure to be a woman. The consequences of these issues may be aggravated by the fact mental health problems are more of a taboo than most physical problems.

Indeed, there is evidence that early pregnancy losses lead to post-traumatic stress disorder (PTSD) for some women (Farren et al., 2016) and the consequences of a traumatic event may go way beyond mental health problems. Van den Berg et al. (2017) find that losing a child unexpectedly has negative consequences on a range of labour market, health, and family outcomes. Depression resulting from an early pregnancy loss may lead to a shift in preferences and expectations; and it may increase constraints such as lowering productivity on the labour market (Baranov et al., 2020). While early pregnancy loss may to a large extent be unavoidable, the consequences may be attenuated by proper policy responses. Examining the consequences of an early pregnancy loss beyond mental health is thus important as a first step towards defining these responses.

We analyse the effect of an early pregnancy loss on mental health care use, labour market and family outcomes in the Netherlands. We are able to identify the majority of women who went to the hospital for a pregnancy loss in the Netherlands in 2011 to 2014. For these women, we link hospital admission data to mental health care expenditures, psychiatric drugs prescriptions, labour market, and demographic information. In an event study framework controlling for individual fixed effects, we establish the effect of an early pregnancy loss on

[5]This is slowly changing with social media and the internet. An increasing number of women share their loss online (Freidenfelds, 2019).

the women's and their partners' mental health care use, women's labour market outcomes and the probability of a divorce.

Miscarriages are to a large extent random: around 70-80% are caused by random non-viable chromosomal anomalities (Banno et al., 2020). If miscarriages are indeed random, a simple event study model is sufficient to determine the causal effect of a early pregnancy loss on mental health care use. However, there are also non-random behavioural miscarriage risk-factors such as smoking that could also be related to mental health (care use). We argue that these risk factors are largely time-invariant, and control for them including individual fixed effects.

This paper is related to two strands of the literature. First, in the medical and epidemiological literature, there are many studies examining mental health after a miscarriage. For example, in a case control study, Jacob et al. (2017) use the German gynaecologist data base and compare women with a miscarriage to a matched control group of women with completed pregnancies. One year later, women with a miscarriage are 3 percentage points more likely to be diagnosed with depression, anxiety or adjustment disorder. These results are confirmed by various other studies for different mental health measures and countries (Shreffler et al., 2011; deMontigny et al., 2017; Broen et al., 2005; Nynas et al., 2015; Farren et al., 2018; Brier, 2008). Mental health effects are worse for recurrent miscarriages (Chen et al., 2019; Toffol et al., 2013), and there is increased anxiety in the following pregnancy after a miscarriage (Hunter et al., 2017).[6] We contribute to this literature by studying mental health care use instead of measures for mental health. Given that in the literature, an effect on/association with mental health has been shown, it is important to establish whether women get mental health care after an early pregnancy loss. Moreover, we can follow a large part of Dutch women who had an early pregnancy loss and their partners over a long time span (i.e. two years before up to four years after the early pregnancy loss). This enables us to use an event study framework, where we implement the most recent advance in the econometrics of event studies.

[6]Related to this literature on the mental health effects of miscarriage, there is a literature on the mental health effects of voluntary abortions. Women who are pregnant but do not want a child may get an abortion in many developed countries. These women - and the situations they are in - are on average very different from women who plan to have a child but lose their pregnancy. It is therefore plausible that abortions do not have the same effect as miscarriages. Janys and Siflinger (2019) evaluate the effect of an abortion on mental health and find no effect for Swedish women. Miller et al. (2020) find that being denied an abortion leads to increased financial distress for US women.

Second, we contribute to the recent literature on the economics of grief. Van den Berg et al. (2017) examine the economic impact of losing a child by estimating the effects on parental (mental) health, labour market and family outcomes. They compare parents who lose a child in an unexpected accident with a control group of parents whose children are in non-fatal accidents. They find significant declines in health, labour market and family outcomes, and put forward grief as the main explanation for this. For Finland, Costa-Ramón (2020) reports large earning reductions of mothers after losing a child in an event study framework. Fathers do not show significant earnings declines, but are less likely to be employed after having lost a child. The death of the partner is also a distressing event. Becoming a widow is associated with a significant increase in the mental stress score from the general health questionnaire (Gardner and Oswald, 2006), and leads to long-lasting mental health declines (Siflinger, 2017). Unborn children are also family members, and their loss during early pregnancy entails grieving as well. We contribute to this literature by examining economic consequences of grief stemming from a different event that entails a loss for a large share of the adult working-age population.[7] To our knowledge, ours is the first study to examine labour market and divorce effects of an early pregnancy loss and its interaction with fertility.

We find that early pregnancy losses lead to a 17% (2 percentage point) increase in the probability of using any mental health care, a 9% (0.5 percentage point) increase in the probability of having mental health care expenditures (which are most likely used for therapy) and a 19% (1.5 percentage point) increase in the probability of using psychiatric drugs for women. The absolute effect on the mental health care use for partners is very low, but still larger than for women in relative terms given their much lower initial prevalence. An early pregnancy loss also has negative consequences on women's labour market outcomes. We find that they are 5 percentage points less likely to work after an early pregnancy loss. In addition, our results show that the probability of divorce increases by 0.25 percentage points. We also provide suggestive evidence that the decrease in employment is likely to be linked to subsequent fertility instead of the early pregnancy loss, as a large share of women do have a healthy baby four years after the pregnancy loss. However, this is not the case for the mental health costs, and childless women four years after the early pregnancy loss are more likely to get divorced.

[7] There are about 100 times more miscarriages than fatal car accidents per year in the Netherlands (CBS, 2019a).

Our findings suggest that there are women and some of their partners who have mental health problems after an early pregnancy loss. Availability of care for these people is crucial, and since midwifes or gynecologists are no mental health specialist, efficient communication and smooth referrals between providers is important to avoid discontinuities in care. Moreover, reducing the stigma on both early pregnancy loss and mental health issues may be beneficial, such that people activate their full support network, which may help them to deal with the loss.

2.2 Background

2.2.1 Miscarriages

Incidence

Miscarriages[8] occur frequently: roughly 10% of all pregnancies end in a miscarriage (Verschoor, 2017). In the Netherlands, this means that there are around 20,000 miscarriages annually.[9] However, this number is merely an estimate since not all miscarriages are recorded and very early miscarriages may not be detected. Miscarriages are usually detected in either of two ways: (i) a woman experiences abdominal pain or blood loss and sets a meeting with a midwife who may confirm the miscarriage through an ultrasound, or (ii) the midwife detects during an ultrasound that the heart of the fetus is not beating. Improved ultrasound technology and more frequent and early visits to midwifes mean that more miscarriages are detected than in the past (Freidenfelds, 2019). About 5% of couples trying to conceive experience two or more miscarriages (Rai and Regan, 2006). Women with a previous miscarriage are more likely to have another miscarriage than women with a live birth, but there is still a 90% chance to have a healthy baby after a miscarriage.

Causes and risk factors

Around 80% of miscarriages occur during the first trimester of pregnancy (ACOG, 2018). It is estimated that up to 70-80% of miscarriages are due to chromosome abnormalities or a suboptimally functioning placenta (Kajii et al., 1980; NHS, 2018; Banno et al., 2020). While

[8] Miscarriages represent the vast majority of all early pregnancy losses (84% in our data).
[9] There are around 170,000 babies born in the Netherlands annually (CBS, 2019b).

chromosome abnormalities occur at random, medical research suggests that there are several risk factors for early miscarriages, including ethnicity (Mukherjee et al., 2013), the age of the mother (Andersen et al., 2000), the age of the father (Slama et al., 2005) and a number of lifestyle-related factors (NHS, 2018): obesity, smoking, high caffeine intake, and alcohol and drugs consumption. In addition, there is evidence that some types of medication increase the likelihood of a miscarriage; for many other types of medication, there is no conclusive evidence about reproductive toxicity.[10]

Miscarriages that occur at a later stage of pregnancy are often caused by health problems of the mother including chronic diseases (poorly controlled diabetes, severe high blood pressure and chronic kidney disease), infections (HIV, malaria), and acute problems such food poisoning (listeriosis, salmonella) (NHS, 2018).

2.2.2 Health care in the Netherlands

Treatment of miscarriages in the Netherlands

Around two-thirds of all miscarriages are treated at the hospital (Verschoor, 2017); the others are not treated medically. After the miscarriage is confirmed, the patient has three options for treatment: (i) removing the fetus surgically (curettage), (ii) using medication (misoprostol) and (iii) waiting for a spontaneous abortion (Verschoor, 2017).[11]

Treatment for early pregnancy losses is covered by social health insurance in the Netherlands. Enrolment in a social health insurance plan is mandatory, so all women who experience such a loss are covered under this scheme. Treatment of an early pregnancy loss does not fall under the annual deductible of 385 € per year that all social health insurance plans have.[12]

[10] For many types of medication that are unsafe or for which the side effects in humans are unknown, (imperfect) substitutes exist. The Dutch GP guidelines (NHG, 2015) suggest recommending pregnant women not to stop or start taking medication or to switch without consulting a doctor.

[11] There are no national-level guidelines about which type of treatment is to be preferred (Verschoor, 2017). Treatment usually starts in an outpatient or day-care setting; surgery is sometimes done in an inpatient setting. Misoprostol treatment and waiting are most cost-effective than curettage and have a lower risk of complications limiting the women's fertility, but are not always successful; 30% of the misoprostol treatments is followed by curettage (Verschoor, 2017).

[12] Individuals may choose to increase to a maximum of 885 € in return for an insurance premium discount.

Treatment of mental health problems in the Netherlands

There is no uniform protocol for the mental health follow-up after an early pregnancy loss in the hospital (Verschoor, 2017); the Dutch guidelines for general practitioners (GPs) and midwifes for miscarriage treatment (NHG, 2019) recommend scheduling a follow-up meeting 4-6 weeks after the miscarriage.

The first step for getting mental health care is a meeting with the GP, who may decide to treat patients with mild mental health problems in his/her own practice or refer to a mental health care provider.[13] Generally, treatment of mild mental health problems consists of therapy, medication or a combination of both (Rijksoverheid, 2020c). While there are no waiting times for GP care, there was an 8-9 weeks average waiting time for treatment of mild mental health problems by a mental health provider in 2018 (V&Z, 2020).

GP care is exempted from the deductible, medication prescribed by the GP and mental health care providers are not exempted. Medication for mild mental health problems is generally cheap (under 1 € per daily dose).

Sickness, maternity leave and labour market protection

Women with an early pregnancy loss are not entitled to maternity leave when the loss happens before the 24th week of pregnancy (UVW, 2020c). If the woman is reporting sick due to the early pregnancy loss, she is covered by Social Security (specifically, by the *ziektewet*) and 100% of her salary is paid, capped at a maximum daily salary of 220 € in 2020 (UVW, 2020a; Rijksoverheid, 2020a). These benefits are paid until the woman has recovered, with a maximum of two years Rijksoverheid (2020b). She cannot be laid off during this period (UVW, 2020b).[14]

2.3 Data

We use hospital data from Statistics Netherlands (CBS) to identify the population of women with an early pregnancy loss treated in the hospital between 2007 to 2016. We then link their consumption of mental health care (2009-2017), family links, demographic information, and

[13] Treatment by a mental health care provider requires a referral from the GP.

[14] There are exceptional circumstances that allow employers to lay off sick employees, for example when the firm defaults or when the employee was in her probationary period.

Table 2.1: Number of pregnancy losses per year treated in a hospital

Year	EPLs	Data Set
2009	10,625	LMR *(Landelijke Medische Registratie)*
2010	11,054	LMR
2011	9,424	LMR
2012	5,810	LMR
2013	8,862	LBZ *(Landelijke Basisregistratie Ziekenhuiszorg)*
2014	8,544	LBZ
2015	8,990	LBZ
2016	8,342	LBZ

Note: Early pregnancy losses (EPL) are taken from the *LMR (Landelijke Medische Registratie)* hospital data for the years 2009-2012, and the *LBZ (Landelijke Basisregistratie Ziekenhuiszorg)* data for the years 2013-2016. The reason for a declining number of EPLs in 2011 and 2012 is hospital attrition from the LMR. The reason for the lower numbers of EPLs in 2013-2016 is that in these years diagnoses were not reported for some hospital admissions.

income (2009-2017) and construct a yearly panel. Table A.2.1 in the Appendix gives an overview of the data sets used in this study. Since we want to focus on first pregnancy losses, we drop all women with a pregnancy loss in the years 2007-2010, and only use women with a first pregnancy loss in 2011-2014. This allows us to follow women from two years before the pregnancy loss up to 4 years after.

In the hospital data, we can identify all pregnancy losses treated either as a day care admission or as inpatient care, but we do not observe outpatient visits. There are around 20,000 miscarriages per year (Verschoor, 2017), implying that our data covers more than 40% of all miscarriages in the Netherlands. The sample size drops for the years 2011 and 2012 as the number of hospitals who provided information dropped between 2006 and 2012.[15]

[15] As long as there is no relationship between the effect of an early pregnancy loss on our outcome variables and a hospital dropping out of the data, this does not affect our results. To be on the safe side, we weight our results by the number of women having an early pregnancy loss in a given year to correct for different sample inclusion probabilities across the years.

Table 2.2: Types of pregnancies losses included in the analysis

LMR (2009-2012)	ICD9-CM code	Frequency	%
Missed abortion[a]	632	37,849	58%
Spontaneous abortion[b]	634	18,112	28%
Ectopic pregnancy[c]	633	7,945	12%
Other abnormal pregnancy[d]	631	871	1%
Molar pregnancy[e]	630	267	<1%
LBZ (2013-2016)	ICD10-CM code	Frequency	%
Missed abortion[a]	O02	19,059	51%
Spontaneous abortion[b]	O03	11,320	30%
Ectopic pregnancy[c]	O00	6,778	18%
Molar pregnancy[e]	O01	529	1%

Note: [a]Missed abortion: fetal death in the first 20 weeks of gestation with embryonic tissues and placenta still in the uterus (Dulay, 2019b).
[b]Spontaneous abortion: fetal death in the first 20 weeks of gestation (Dulay, 2019b).
[c]Ectopic pregnancy: implantation of the embryo outside the uterus (Dulay, 2019a).
[d]Other abnormal pregnancy: very early pregnancy loss (Healthline, 2020).
[e]Molar pregnancy: abnormal growth of cells that would normally develop into the placenta, accompanied by a non-vital or no fetus (NHS, 2017).

Table 2.2 details the types of involuntary pregnancy losses that we include in our study, their ICD codes, and their frequencies. We use the *LMR (Landelijke Medische Registratie)* hospital data for the years 2009-2012, and the *LBZ (Landelijke Basisregistratie Ziekenhuiszorg)* data for the years 2013-2016. Since these two data sets use different diagnosis codes, we report the frequencies of the diagnoses per hospital data set. The most frequent type of pregnancy loss is a missed abortion, which indicates a fetal death that is not expelled (yet) (Dulay, 2019b). This is the typical case treated in the hospital, since fetal death has been established, but the miscarriage has still to be completed. An additional third of women go to the hospital for a spontaneous abortion, implying fetal death and (partial) fetal expulsion. Another 12-14% have an ectopic pregnancy, where the embryo attaches outside the uterus (Dulay, 2019a).[16] Other types of pregnancy losses are less common.[17]

The outcomes of interest are mental health care use, and labour market and family outcomes. For mental health care use, we use two different data sources. First, we have in-

[16]In a prospective cohort study, Farren et al. (2016) compare the mental health effects of a miscarriage with the effect of an ectopic pregnancy. They find no difference in post-traumatic stress disorder, anxiety and depression.

[17]The early pregnancy losses reported in the table represent a subset of all early pregnancy losses in the Netherlands. For example, they do not include i) most very early pregnancy losses, where the woman is not aware of her pregnancy, or ii) a complete, spontaneous abortion with a check-up by the midwife.

formation on total mental health care expenditures, which mainly include costs of psychologists or psychiatrist visits (but not for medication). We construct two variables regarding mental health care costs: i) a binary indicator of having any mental health care costs in a year; and ii) total annual mental health care expenditures (in Euros). Second, we use information on prescriptions for psychiatric medication. On the individual level, we know every prescribed medication (at ATC4 group level) covered by the basic health insurance[18] that has been dispensed by a pharmacy. We include four types of psychiatric drugs in our analysis: anti-psychotics (N05A), anxiolytics (N05B), hypnotics and sedatives (N05C), and anti-depressants (N06A).[19] These drugs are used to treat mental illnesses like schizophrenia, bipolar disorder, anxiety, insomnia, chronic pain and depression and must be prescribed by a physician.[20] For the main analysis, we combine these four types of drugs into one indicator for taking any psychiatric drugs, but investigate the sensitivity of our results to this aggregation in the Appendix. Finally, since psychiatric drug use does not have to coincide with therapy and conversely, we constuct an indicator for any mental health care use, which is equal to one if the person has mental health care expenditure *or* uses psychiatric drugs.

A pregnancy loss may not only affect the woman experiencing the loss, but her partner as well. In the literature, men also report grief reactions to miscarriages (Obst et al., 2020; Williams et al., 2019), yet there may be less space for their grief because they are seen as the main supporter for their partner rather than a patient. There is evidence that men also suffer from mental health effects after a miscarriage, but their mental health reaction is less pronounced than for women (Cumming et al., 2007; Volgsten et al., 2018). 51% of women have a male married/registered partner at the time of the early pregnancy loss.[21] We link information on the mental health care use of these partners and follow them over time as well (independently of whether they stay married to or registered with their partner).

Mental health problems are especially disabling (Layard, 2017), and can have severe labour market consequences (Biasi et al., 2019). Therefore, we also consider the probability of having paid work at some point during the calendar year and total income from work.

[18] The only main category of psychiatric medication that is usually *not* covered by basic health insurance are benzodiazepines.

[19] We can observe if this types of medications were dispensed at least once, but we do not have information about the number of doses.

[20] They may be prescribed by a GP.

[21] In the Netherlands, 57% of children were born to married mothers or mothers with registered partners in 2011-2014 (Statline, 2019b). This means that it is sensible that about half of the women are married/registered at the time of the EPL.

We hypothesise that labour market effects would mainly be driven by mental health, as there are commonly no long-lasting physical consequences of an early pregnancy loss.[22] Having paid work includes being employed or self-employed, defined as having any income from work. Total unconditional income from work includes gross earnings from employment and self-employment.

Last, we are interested in the effects on family outcomes. We focus on divorces because there may be discrepancies in coping style and time spent grieving between partners (Carter et al., 2007). Divorce is captured by a indicator of getting a divorce or a separation from a registered partnership in a given year.

We focus on all women going to the hospital due to early pregnancy loss. This group is interesting because it provides an overall picture of the effect of an early pregnancy loss on mental health care use. The disadvantage of studying this group is we ignore differences in past and future fertility. As past and future fertility has an influence on the mental health effects of the EPL, this means that we compare women for whom the consequences are likely very different. Since fertility may also interact with mental health, we explore the potential role of fertility in Section 2.7.

Table 2.3 shows descriptive statistics for the two years before their first early pregnancy loss and three years after. Mental health care use is relatively rare, about 11% of women use mental health care. About half of these women have mental health care expenditures. Accordingly, mean mental health spending is relatively low. About 7% of women use psychiatric drugs, where the most common drugs are anti-depressants. Before the early pregnancy loss, women in our sample have very similar mental health care use pattern as the overall Dutch population of women (See Table A.2.2 in the Appendix for detailed population summary statistics on women's mental health). Partners are less likely to use mental health care (3%), only 1% have mental health care expenditures and 2% use psychiatric drugs. A large majority of women is employed before the early pregnancy loss, with about 30,000 € annual income. Divorces are not common.

Comparing the situation before the early pregnancy loss to three years after, mental health care use increases after the early pregnancy loss for almost all measures, for both women and

[22] Sepsis is a possible physical consequence of an early pregnancy loss. However, sepsis after completed pregnancies is rare (Bauer et al., 2013), and hence it is likely to be rare as well after early pregnancy losses.

their partner. Income from work stagnates, and women are less likely to be employed. The probability of a divorce does not change.

2.4 Empirical strategy

An early pregnancy loss is largely random, since the most common (70-80%) cause are nonviable genetic combinations. Ideally, we would use a simple event-study model in which we regress the outcome on event-time (time away from EPL), age, year fixed effects, and individual fixed effects. Age and the individual fixed effects allow to capture risk-factors increasing the chance of an early pregnancy loss that potentially also interact with mental health and the other outcomes of interest, while calendar year fixed effects pick up trends in the treatment of EPLs and mental health problems and event-time enables us to study whether effects persist (if there are any).

Including all four sets of regressors is, however, not possible due to the multicollinearity between the year fixed effects, event-time, age, and individual fixed effects. Therefore, we implement the model in Equation 2.1, where we regress the outcome on time away from the first[23] early pregnancy loss q_{it} (event-time) and individual fixed effects. The reference period is one year before the early pregnancy loss, and we follow women from two years before up to three years after the loss ($k = -2, ..., 3$). We believe that it is most important to control for individual fixed effects, as these may filter out person-specific propensity to suffer from a mental health problem (and to get therapy for these) and to have an increased risk of an early pregnancy loss. In a robustness check, we use alternative specifications of the model, such as adding age groups as controls, controlling for age or calendar year, or using random instead of fixed effects as suggested by Borusyak and Jaravel (2017) (see Section 4.5.3).[24]

$$y_{it} = \alpha_i + \sum_{k \neq -1, k=-2}^{3} \beta^k q_{it}^k + \varepsilon_{it} \tag{2.1}$$

[23] The first early pregnancy loss is the first we observe in our data.

[24] Yet another option to circumvent the collinearity problem would be to use a control group of women who have an early pregnancy loss in the future. Since these women chose to become pregnant later than our treatment group, the two groups may not be comparable and hence future pregnancy losses may not be a good control group.

Table 2.3: Summary statistics before and after the EPL

	2 years before EPL		**3 years after EPL**	
	Mean	SE	Mean	SE
Mental health				
Any MHC	0.11	0.00	0.13	0.00
Any MHC costs	0.06	0.00	0.08	0.00
MHC costs	202	11	262	13
Cond. MHC costs	1394	76	951	70
Any sychiatric drugs	0.07	0.00	0.08	0.00
Anti-psychotics (N05A)	0.01	0.00	0.01	0.00
Anxiolytics (N05B)	0.01	0.00	0.02	0.00
Hypnotics and sedatives (N05C)	0.01	0.00	0.01	0.00
Antidepressants (N06A)	0.05	0.00	0.07	0.00
Demographics & Family				
Age	30	0.03	35	0.03
Age partner	34	0.06	39	0.05
Married	0.40	0.00	0.56	0.00
Divorce	0.01	0.00	0.01	0.00
Fertility treatment	0.003	0.00	0.002	0.00
Number of children	0.36	0.00	1.33	0.00
Labour market outcomes				
Income from work	27440	144	27786	182
Employed	0.84	0.00	0.78	0.00
Mental health partner				
Any MHC partner	0.03	0.00	0.03	0.00
Any MHC costs partner	0.01	0.00	0.02	0.00
MHC costs partner	41	8	69	8
Any psychiatric drugs partner	0.02	0.00	0.02	0.00
Anti-psychotics (N05A) partner	0.00	0.00	0.01	0.00
Anxiolytics (N05B) partner	0.00	0.00	0.00	0.00
Hypnotics and sedatives (N05C) partner	0.00	0.00	0.00	0.00
Antidepressants (N06A) partner	0.01	0.00	0.02	0.00
Early pregnancy loss cohorts				
EPL Cohort 2011	0.29	0.00	0.29	0.00
EPL Cohort 2012	0.18	0.00	0.18	0.00
EPL Cohort 2013	0.27	0.00	0.27	0.00
EPL Cohort 2014	0.26	0.00	0.26	0.00
N				*29,504*

Note: Summary statistics two years before and three years after the early pregnancy loss (EPL). MHC stands for mental health care.

The parameters β^k provides the estimate of the causal effect of an early pregnancy loss under three identifying assumptions (see Abraham and Sun (2020) for details and formal derivation)[25] The assumptions are formulated for the event-time estimates by treatment cohort, where a treatment cohort is defined as women experiencing the early pregnancy loss in the same calendar year (because our outcome data is yearly): 1) Parallel trends in baseline potential outcomes, i.e., the difference in the expected outcome between two periods t and s if never treated is the same for all treated cohorts and all periods; 2) There is no anticipation of the event;[26] 3) The treatment effect is homogeneous across cohorts. We cannot formally test assumption 1 as we do not observe the outcome of treatment women if never-treated. However, we can test if there are differences in the trends pre-treatment. Moreover, pre-trends that are zero provide supportive evidence that assumption 2 holds.

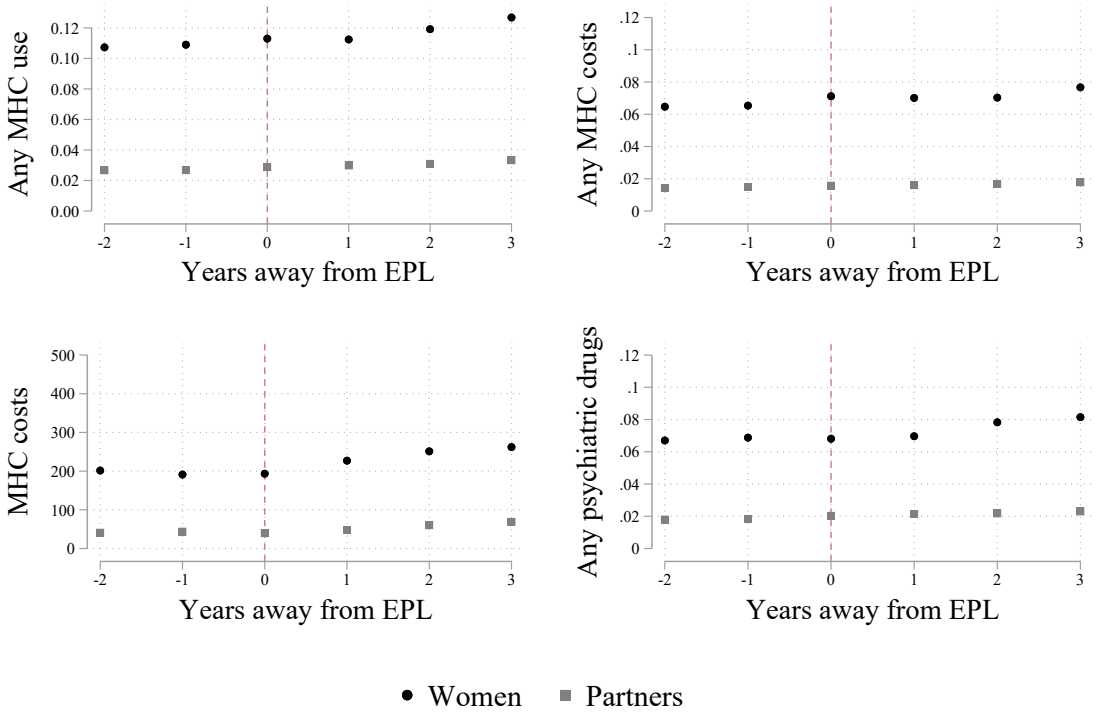
There are some differences in the type of EPL included in the data due to attrition in the hospital data. For example, women with an EPL are less likely to be included in 2011 and 2012. In addition, there may be some differences in the treatment, with a lower prevalence of more invasive procedures in the later years. To the extent that this can influence the mental and economic consequences of an EPL, assumption 3) homogeneous treatment effects may not be satisfied in our setting. However, we can still obtain valid estimates of the treatment effect by taking a weighted average of cohort effects by cohort size for every event-time (Abraham and Sun, 2020).[27] We implement this aggregation method, and bootstrap standard errors with a 1000 replications. In the main text, only cohort-aggregated effects are reported for brevity, but results by cohorts can be found in Appendix 2.A.1.

[25]There is one difference between the solution discussed by Abraham and Sun (2020) and ours: they deal with the collinearity between year fixed effects, event time and individual fixed effects by excluding two pre-trend indicators as the reference category (as proposed by Borusyak and Jaravel (2017)) while we drop the calendar year fixed effects.

[26]Since we deal with the collinearity of event-time coefficitens, time and individual fixed effects in a different way than Abraham and Sun (2020) and do not control for calendar year effects, we have to make the following addition to assumption 2: no time trends in the outcome.

[27]Callaway and Sant' Anna (2019) suggest to report the most policy-relevant aggregation of the cohort event-time effects. For example, one can aggregate the effect of all cohorts by event-time, or to one single effect of all cohorts and event-time. For our purposes an aggregated effect by the time since the EPL is most useful because we expect that the effect of an EPL on the women's mental health and other outcomes changes when the EPL occurred longer ago.

Figure 2.1: Descriptive trends in mental health care use around an early pregnancy loss



2.5 Mental health effects for women and their partners

Figure 2.1 shows mental health care use before and after an early pregnancy loss (EPL) for women and their partners. In the two years before the pregnancy loss, mental health care use is stable, and there is no indication of a trend before the event. From time zero at the early pregnancy loss up to three years after, mental health care use increases slightly for women: the proportion of women with any mental health care increases from 11 to 13%, the proportion of having any mental health care costs increases from 7 to 8%. Average mental health care costs increase from around 200 to 300€, and the percentage of women using psychiatric drugs increases from around 7% before the event to 8% three years thereafter. Descriptive trends by type of psychiatric drug for women can be found in Figure A.2.13. The largest initial level but also the largest increase is for anti-depressants.

Compared to women with an early pregnancy loss, partners are less likely to use mental health care. This is in line with the finding that men use mental health services less frequently than women, and that they are less likely to be diagnosed with anxiety or depression (Affleck

et al., 2018).[28] After the early pregnancy loss, partner's mental health care use remains stable, except for a small increase in mental health care expenditure.

Figure 2.2 shows mental health event-time coefficients aggregated over cohorts and weighted by cohort size to correct for heterogeneous treatment effects by cohorts for both women and their partners. 95% confidence intervals are obtained with a bootstrap over 1000 replications.[29]

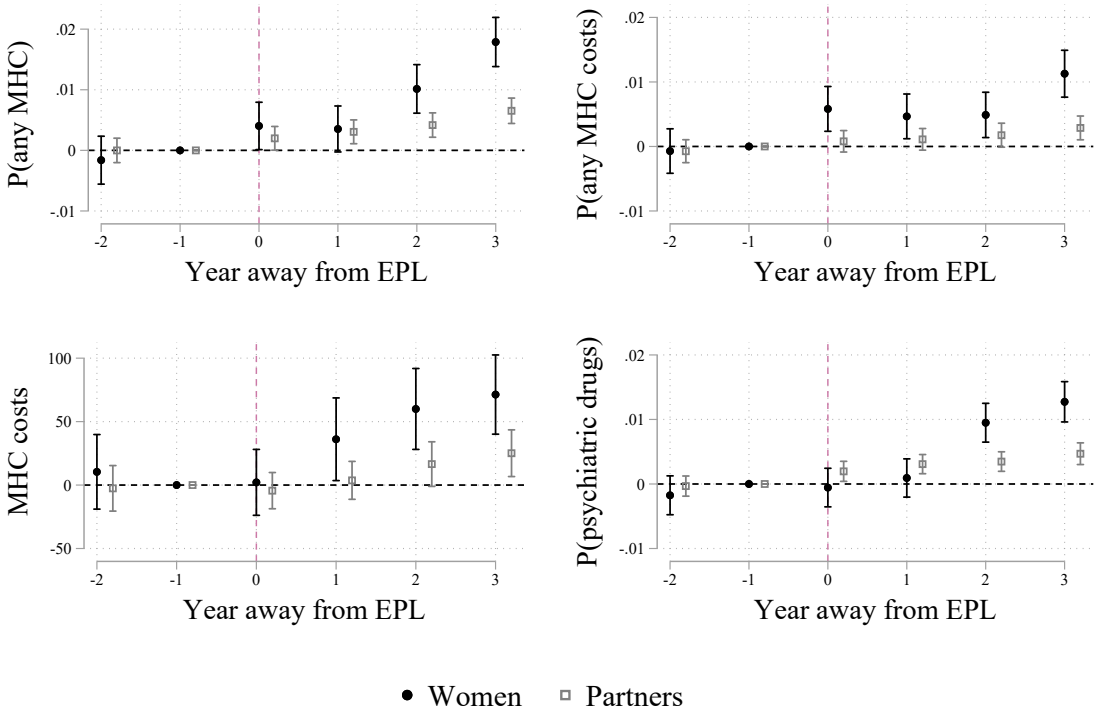
For women, an early pregnancy loss increases the probability of mental health care use by almost 2 percentage points. When comparing this estimate to the average proportion of mental health care users in the year before the EPL (11%), this represents a 17% increase of mental health care use. The increase in the probability of having any mental health care expenditure is smaller, around half a percentage point, or 9% relative to the pre-loss mean. Mental health care costs also increase, by around 75 € 3 years after the EPL. This represents approximately one-fifth of the cost of a short psychologist trajectory consisting of about 5 45-minute visits (Bakker and Jansen, 2013), and hence is a relatively small effect in absolute terms. However, relative to the pre-treatment mean (202 €), the effect is large as it translates to a 37% increase. There is also a clear increase in women's psychiatric drug use due to the early pregnancy loss. In event-time 3, the increase amounts to about 1.5 percentage point. This is a 19% increase with respect to the pre-treatment mean (7%), and hence also a large effect. The increase in the probability of using psychiatric drugs is mainly driven by an increase in antidepressants (see Figure A.2.14 in the Appendix for the full results by type of drug).

Partners are about 0.7 percentage points (or 26% compared to the pre-pregnancy loss mean of 3%) more likely to use mental health care three years after the EPL. The probability of having any mental health care costs increases by 0.3 percentage points (or 20%). Their mental health care expenditures also increase by 25 € (or 58%), and the probability of taking psychiatric drugs increases by about 0.4 percentage points (or 26%). Given that partner's mental health take-up is very low, small increases in absolute terms translate into large relative increases.

[28] This does not necessarily suggest that men are in better mental health - they are more likely than women to commit suicide, to have a substance use disorder, or to be diagnosed with ADHD.

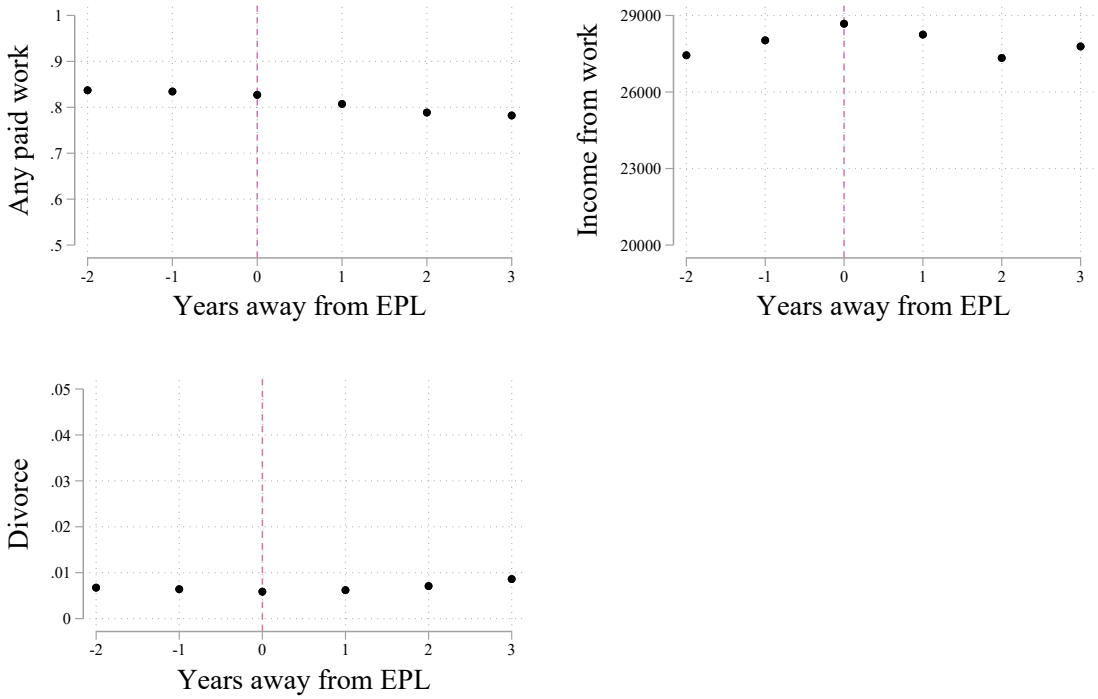
[29] These results also include the cohorts for which we reject the null hypothesis of $\beta^{-2} = 0$ (assumption 2) no pre-trends). The results are robust to excluding these cohorts from the analysis (see Figures A.2.15 - A.2.17 in the appendix).

Figure 2.2: Mental health care (MHC) use effects of an early pregnancy loss (EPL cohorts aggregated)



Note: Displays the mean estimates of Equation 2.1 by cohort weighted by cohort size. An early pregnancy loss (EPL) cohort: a women having an early pregnancy loss in 2011 belongs to the 2011 cohort, and similar for the other years. 95% confidence intervals are obtained with a bootstrap using 1000 replications. Event-time -1 is the reference period.

Figure 2.3: Descriptive trends in labour market and family outcomes around an early pregnancy loss



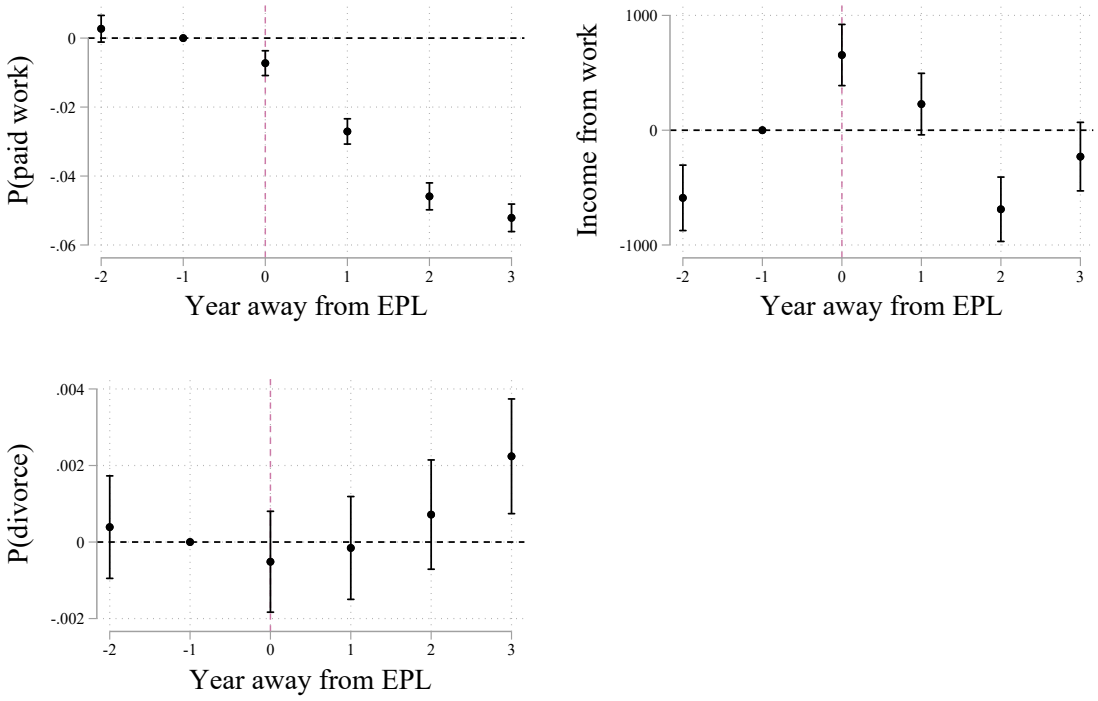
To sum up, both women and their partner's mental health care use increases after an early pregnancy loss and these increases are large when compared to the pre-event averages.

2.6 Labour market outcomes

Figure 2.3 plots the descriptive trends for labour market outcomes. Before an early pregnancy loss, trends in employment are stable, but income is growing. After an early pregnancy loss, women are less likely to have paid work, and their income from work drops. Given that income drops after the event and that the pre-event trend is growing, our event-study estimate underestimates the true effect of the event.

Figure 2.4 shows the aggregated event study coefficients weighted by cohort size and their 95% bootstrapped confidence interval. Since for some cohorts, the zero pre-trends condition was not fulfilled, Figure A.2.18 in the appendix shows the same results only for cohorts that satisfy assumption 2, the pattern stays overall similar. Three years after the early pregnancy loss, women are 5 percentage points less likely to have paid work. Their income increase (with a temporary dip in event-time 1 and 2).

Figure 2.4: Labour market and divorce effects of an early pregnancy loss



Note: Displays the mean estimates of Equation 2.1 by cohort weighted by cohort size. An early pregnancy loss (EPL) cohort: a women having an early pregnancy loss in 2011 belongs to the 2011 cohort, and similar for the other years. 95% confidence intervals are obtained with a bootstrap using 1000 replications. Event-time -1 is the reference period.

2.7 Family outcomes: divorce

Figure 2.3 bottom left shows how frequent divorces occur around an early pregnancy loss. There seems to be no effect on the likelihood of a divorce up to event-time two, but there is an increase by 0.25 percentage points 3 years after the early pregnancy loss. The aggregated event study coefficients weighted by cohort size and their 95% bootstrapped confidence interval are depicted in Figure 2.4. Dropping the cohorts that do not satisfy the no pre-trends assumption yields very similar results in terms of effect size, but the loss of power makes the effect at event-time 3 insignificant (Figures A.2.18).

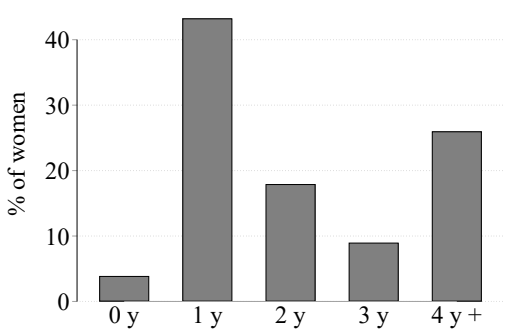
2.8 The role of fertility

Many women have a successful pregnancy relatively soon after their early pregnancy loss. Figure 2.5 panel a) shows the time it takes to complete a successful pregnancy for our sample. Four years after the EPL, around 75% of women have had a baby. Panel b) plots the average total number of children by event time. These fertility dynamics after an early pregnancy loss are relevant to interpret our previous results for at least two reasons. First, some of our estimated effects could be potentially driven by future fertility, either because of postpartum depression or decreased labour market participation after a child birth (see Chapter 3). Second, early pregnancy loss may be perceived as a signal for fertility problems, and influence mental health and tension within the family through this channel. We therefore split our sample in five different groups: 1) women who have their first child in the same year as the EPL, 2) women who have their first child one year after the EPL, 3) women who have their first child two years after the EPL, 4) women who have their first child three years after the EPL, and 5) women who remain childless up to four years after the EPL.

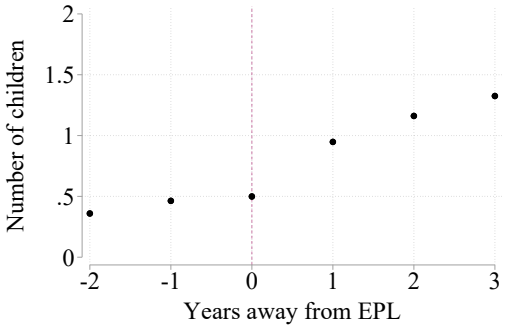
This is an endogenous sample selection, since women with worse mental health, labour market outcomes or a divorce after the EPL may be less likely to become pregnant again. However, we still find these samples informative to illustrate the potential importance of the fertility channel. Figures A.2.19 - A.2.22 in the Appendix show the results by subgroups.

For mental health care use, the concern may be that women who have a child may develop postpartum depression, and the effects we are observing are stemming mainly from these women. However, there are no clear patterns of sudden consistent increase in mental health

Figure 2.5: Fertility around an EPL



(a) Time to the first child after an EPL



(b) Total number of children

care use when a child is born; there are no big increases in mental health care use in the calendar year in which a woman gives birth, nor in the next calendar year.

In contrast, the decrease in paid work and the dip in income seem to be related to the arrival of a child rather than the EPL. Furthermore, the results show that the increase in divorces seem to be driven by couples who remain childless, as all the other groups seem to have no change or a decrease in the likelihood of a divorce. A second way of studying the impact that (signals about) fertility may have on the other outcomes is to zoom in on the subgroup of women undergoing fertility treatment in the calendar year prior to or of the EPL. By focusing on this subgroup of women, we narrow the sample down to women who already know that they have a higher likelihood of staying childless, but at the same time we do not condition on an post-treatment outcome. It is hence more exogenous than conditioning on post-EPL fertility outcomes. Indeed, women who had received fertility treatment in the year of the EPL or the year before are twice as likely to not have a child up to four years after the EPL compared to all women with an EPL.[30] The drawback of this approach is that the group of women with fertility treatment in the year of the EPL or the year before is small - only 457 women qualify. This decreases the power of the analysis, and hence makes statistical inference difficult.

Figures A.2.23 - A.2.26 in the Appendix show the results for women with fertility treatment. For both women and their partners, the point estimate for mental health care use increases more for women with fertility treatment. Due to small sample size, the estimate

[30] 30% of women with fertility treatment have no child up to four years after the EPL, compared to 15% of all women who have an EPL.

is never statistically significant. Pattern are also similar for the labour market and family outcomes.

2.9 Robustness checks

As robustness check, we use other specifications of the event-study model. In our baseline model (equation 2.1), we control for individual fixed effects, but we cannot control for age or calendar year effects due to multicollinearity. With this specification, there may be worries that we pick up general mental health declines in our event study estimates that are related to age or calendar year instead of the early pregnancy loss.

Table 4.7 shows the results for the estimate in the years of the EPL for different model specifications. We chose to present the results for event-time zero in the table since these are the least likely to be influenced by subsequent fertility. Results for the other event-times are plotted in Figures A.2.5 - A.2.8 in the Appendix. In the column 'Main', we display the results of the main specification based equation 2.1 for comparison. In column 'Age', we control for age and omit the individual fixed effects, and in column 'Age + RE' we add random effects as suggested by Borusyak and Jaravel (2017). Column 'Year' and 'Year + FE' show similar results controlling for calendar year instead of age. Finally, in column 'FE + age groups', we add four-year age categories (starting at 16 up to 48) as controls to Equation 2.1. In general, using these alternative models yields very similar results to our main specification. Hence, it is unlikely that our event-study estimates are driven by age-related changes in mental health care use. One exception is income from work. For this outcome, controlling for age instead of calendar year or individual fixed effects makes a large difference for the results for income, potentially because income is on a different trajectory with different ages.

In our last robustness test, we use two pre-trend event-time indicators as a baseline instead of one in order to be able to control for calendar year (Borusyak and Jaravel, 2017). To still be able to check pre-trends, we only include the EPL cohorts 2013 and 2014 for which we have 4 or more pre-event observations. This gives us two pre-trend indicators as the base line (-4 and -1), and two estimated pre-trends (-3 and -2) to check whether pre-trends are zero. These results are not presented in Table 4.7, as it is more instructive to look at the estimates for all event-times including pre-trends (see Figures A.2.9-A.2.12 in the Appendix). Borusyak and Jaravel (2017) note that the results may be different according to

which two pre-trend event-time indicators are chosen as the baseline. The authors argue that using the pre-trend indicators that are furthest away from each other are the most reliable as the baseline, which are -4 and -1 in our application. However, in our setting, there are large differences in size and significance depending on which pre-event indicator we use as the baseline. For this reason, this strategy seems unreliable in our setting.

2.10 Discussion

Early pregnancy losses are the most frequent complication in pregnancy. In this study, we estimate the effect of an early pregnancy loss on mental health care use of both the woman and her partner, divorce, and women's labour market outcomes. In a second step, we explore the role of fertility and how it interacts with our findings.

We find that early pregnancy losses lead to a 2 percentage points (or 17%) increase in the probability of using any mental health care for women, and a 0.7 percentage point (26%) increase for men. Women's (partner's) probability of having any mental health care expenditure increases 0.5 percentage points or 9% (0.3 percentage points or 20%). Furthermore, an early pregnancy loss leads to a 1.5 percentage point or 19% increase in the probability of using psychiatric drugs for women, and 0.4 percentage points or 26% for their partners. Baseline mental health care use for partners is very low, so relative to the pre-early pregnancy loss mean their increase in mental health care use is even larger than women's, but on absolute terms their increases are smaller. Women are also 5 percentage points less likely to work after an early pregnancy loss. The decrease in employment is, however, likely to be linked to subsequent fertility instead of the early pregnancy loss. Divorces increase by 0.25 percentage points (or 40%) after an early pregnancy loss. This effect is driven by women who remain childless up to four years after the pregnancy loss.

In line with other studies, our findings confirm that there are mental health effects of an early pregnancy loss for both women and their partners. This shows that at least part of mental health issues after an early pregnancy loss do not go untreated. Our absolute estimates are small, implying that only a small group of women or partners get treated for mental health issues. Since we do not observe mental health directly, we cannot establish whether only a small group of people suffers from a mental health decline after an early pregnancy loss, or whether there is untreated need of mental health care after an early pregnancy loss.

Table 2.4: Robustness checks for the year of the early pregnancy loss (q=0)

	Main	Age	Age + RE	FE + agegroups	Year	Year + RE
Mental health women						
Any MHC	0.004**	0.004**	0.004**	0.004*	0.004	0.004**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)
Any MHC costs	0.006***	0.007***	0.007***	0.005***	0.006***	0.006***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
MHC costs	2.117	11.407	10.293	4.767	2.034	2.489
	(13.234)	(10.841)	(11.830)	(14.575)	(12.904)	(11.648)
Psychiatric drugs	-0.001	-0.002	-0.002	-0.001	-0.001	-0.001
	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)
Mental health partner						
Any MHC	0.002**	0.001	0.001	0.002*	0.002	0.002**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Any MHC costs	0.001	0.000	0.000	0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
MHC costs	-4.409	-5.189	-5.111	-1.756	-4.252	-4.246
	(7.278)	(5.642)	(6.255)	(7.355)	(6.261)	(5.861)
Psychiatric drugs	0.002**	0.001	0.001	0.002**	0.002*	0.002**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Labour market & family outcomes						
Paid work	-0.007***	-0.009***	-0.007***	-0.007***	-0.007***	-0.007***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)
Income from work	655.103***	-611.266***	-503.198***	655.909***	642.100***	653.845***
	(135.815)	(120.682)	(133.066)	(125.215)	(240.755)	(133.029)
Divorce	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)

Note: Estimates for event-time zero (q=0), the year of the early pregnancy loss for all outcomes. Standard deviations of the bootstrapped coefficients are in parenthesis. Statistical significance is given at 10% *, 5% **, and 1% ***. Column 'Main' displays the results from Equation 2.1. The 'Age' coefficients come from an OLS regression of the outcome on event-time indicators and age fixed effects. The 'Age + RE' coefficients come from panel random effects regression of the outcome on event-time indicators and age fixed effects. The 'individual FE + age groups' model displays the results from Equation 2.1 with four year age groups as additional controls. The 'year' coefficients come from an OLS regression of the outcome on event-time indicators and calendar time fixed effects. The 'year + RE' coefficients come from panel random effects regression of the outcome on event-time indicators and calendar time fixed effects. The results for other event-times can be found in Figures A.2.5-A.2.8.

There seems to be no labour market effects of an early pregnancy loss once subsequent fertility is taken into account. Since many women have children relatively fast after the early pregnancy loss and having a child leads to large declines in labour market outcomes for women (see Chapter 3), it is unlikely that the labour market declines we observe in the full sample (which occur only in the year the child is born) are due to the early pregnancy loss. Hence, labour market effects of losing an unborn child are clearly smaller than losing an older child as documented for example by Costa-Ramón (2020).

The implications of our findings are twofold. First, we find that some women and their partner use mental health care after an early pregnancy loss, whereas others do not. Ex-ante, it is unknown to health care professionals who will face mental health care issues. For this reason, it is important that mental health care is readily available and provided by the most suited mental health care expert. Since health care is highly specialised, and midwifes or gynecologists are no experts on mental health, this requires efficient communication between the hospital, the midwife and the GP. Sometimes, referrals from specialists back to the GP do not go smoothly, which may cause discontinuity in care (NHG, 2017). While efficient communication between providers is important for any type of health problem, patients especially rely on the effective functioning of the health care system concerning mental health issues after an early pregnancy loss, because there is a stigma on both.

Second, we showed in this paper that early pregnancy losses happen frequently and that there is a group of people for which this has mental health consequences. Knowing about the high frequency of early pregnancy losses and the fact that they may entail mental health consequences could help to reduce the stigma around the two issues, and make it easier to talk about and deal with them. If the taboo around mental health problems and early pregnancy loss prevents people from activating their support network, dealing with the loss may be more difficult. This is also argued by Freidenfelds (2019), who notes the importance of creating a narrative of pregnancies that includes miscarriage.

Acknowledgements

We are grateful for the access to linked non-public microdata provided by Statistics Netherlands (CBS). Under certain conditions, these microdata are accessible for statistical and scientific research. For further information: microdata@cbs.nl.

2.A Appendix

Table A.2.1: Data sets overview

Data	Name data set
Hospital	LMR 2007-2012
	LBZ 2013-2016
Health insurance expenditure	Zvwzorgkostentab 2009-2017
Psychiatric prescription drugs	Medicijntab 2009-2017
Demographic info	Gbapersoontab
Parents-children linkages	Kindoudertab
Income	Inpatab
Partners	Huishoudenbus

Note: 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-microdata` (available in Dutch only).

Table A.2.2: Population statistcs mental health care use women aged 25-39 in the Netherlands in 2006-2017

	Mean	SE
Any mental health care use	11%	0.000
Starting mental health care use per year	5%	0.000
Duration mental health care use in years	1.3	0.002
Any mental health care costs	6%	0.000
Duration mental health care costs in years	1.8	0.002
Starting mental health care costs per year	3%	0.000
Mental health care costs	282	0.836
Conditional mental health care costs	4346	12.149
Psychiatric drugs	8%	0.000
Starting psychiatric drugs per year	2%	0.000
Duration psychiatric drugs use in years	2.2	0.003
Antidepressants (N06A)	6%	0.000
Starting Antidepressants (N06A) per year	1%	0.000
Duration Antidepressants in years (N06A)	2.2	0.003
N		*15,094,770*

Note: Women aged 25-39 fall into the interquartile age range of women with an EPL.

2.A.1 Event study results by treatment cohort

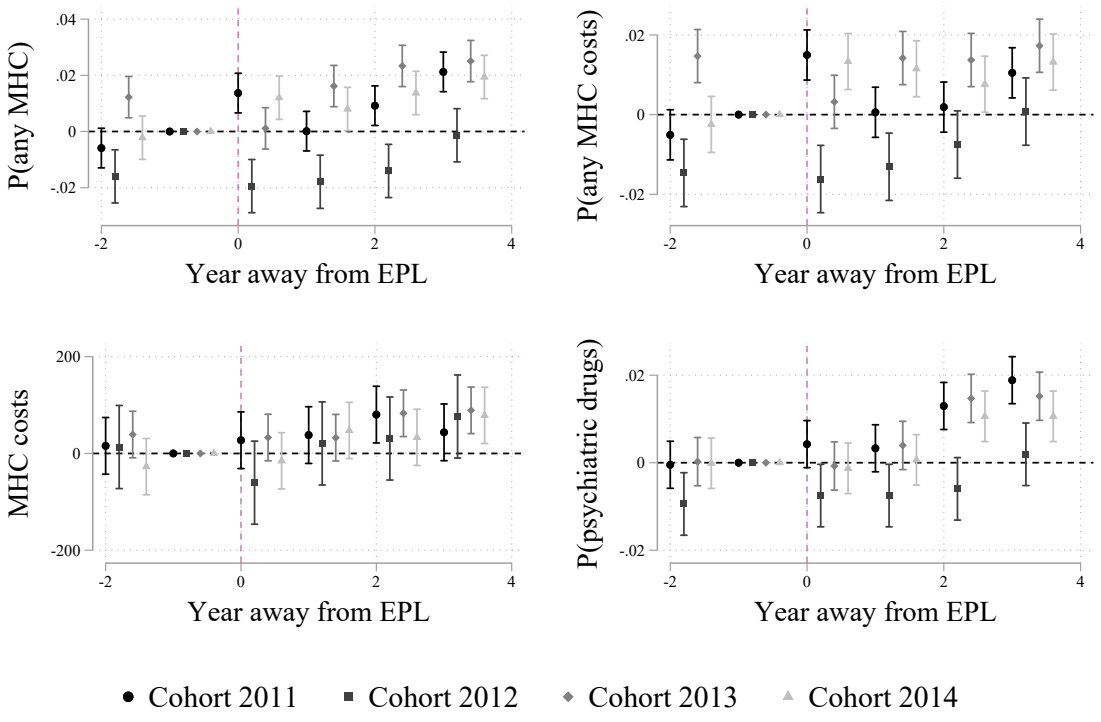
Section 2.4 illustrates that the identifying assumptions have to be checked by cohort of early pregnancy loss. Figure A.2.2 - A.2.4 show the event-study coefficients by EPL cohort. As-

sumption 2[31] states that there should not be pre-trends different from zero. For the probability of using any mental health care and the probability of having mental health care costs, this is only fulfilled for cohort 2014. In contrast, for mental health care costs, this assumption is fulfilled for all cohorts. For the probability of taking any psychiatric drugs, all cohorts except for 2012 fulfil this condition. For partner's mental health, assumption 2 is satisfied for all cohorts and outcomes except for cohort 2014 for the probability of using any mental health care. As for labour market outcomes, it seems to be fulfilled for all cohorts for paid work. For income from work, only cohort 2014 fulfils the requirement. For divorce, cohort 2012 and 2014 satisfy assumption 2.

Assumption 3 states that there should be homogeneous treatment effects across cohorts. In general, we believe that it is important to correct for heterogeneous effects in our setting, since the hospital attrition in the data in 2011 and 2012 alters the inclusion probability to be in the data depending on the year a women incurs a pregnancy loss. Moreover, some point estimates are clearly different by cohort (see, for example, partner's probability of using mental health at event-time zero or the probability of having paid work at event-time 3). Therefore, we correct our overall results for heterogeneous effects by the method proposed by Abraham and Sun (2020).

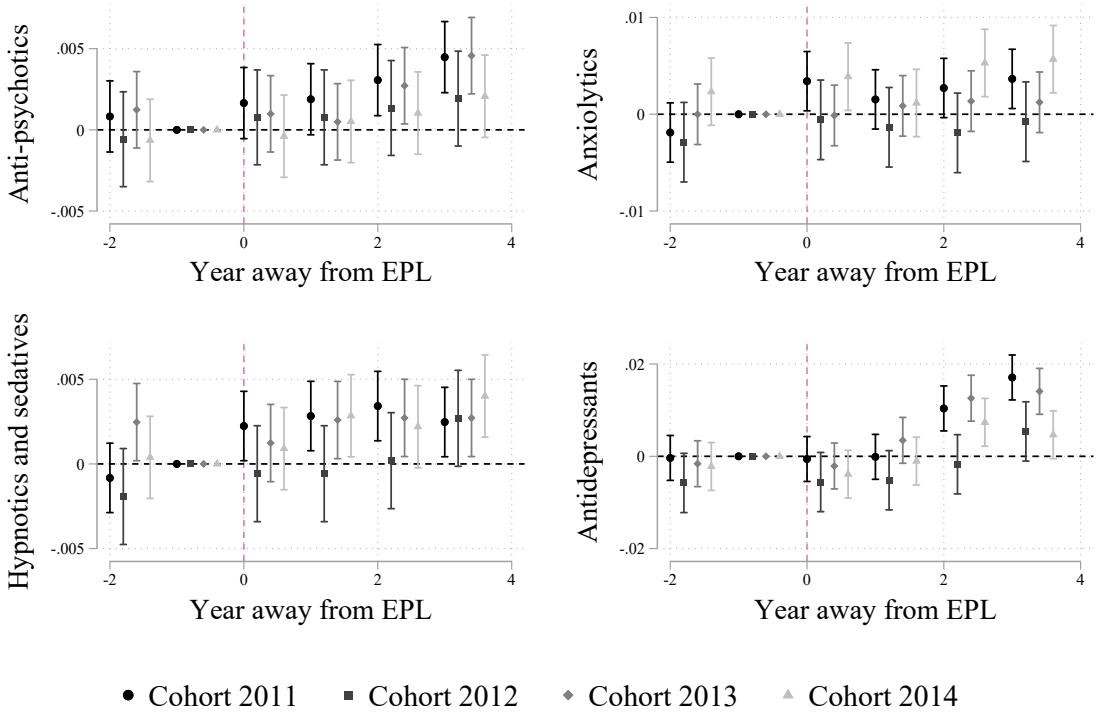
[31] Satisfying assumption 2 implies that assumption 1 is also satisfied. Therefore, we do not comment on assumption 1.

Figure A.2.1: Women's mental health: event-study results by EPL cohort



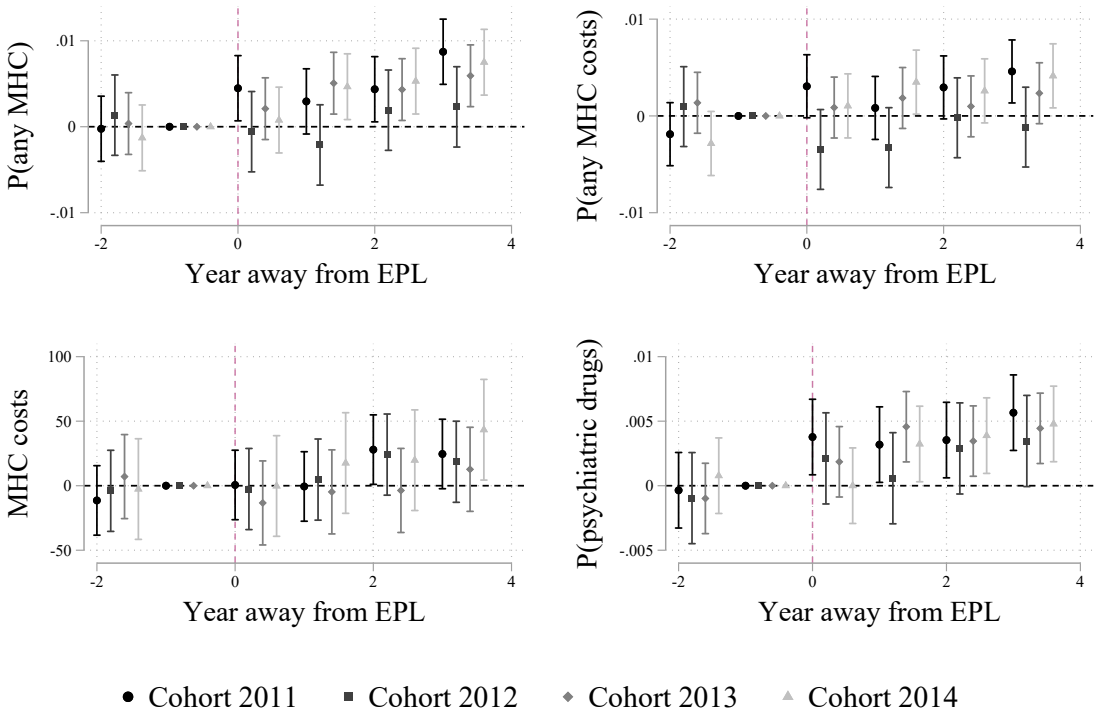
Note: Displays estimates of Equation 2.1 by cohort. Early pregnancy loss (EPL) cohort: a women having an early pregnancy loss in 2011 belongs to the 2011 cohort, and similar for the other years.

Figure A.2.2: Psychiatric drug use: event-study results by EPL cohort



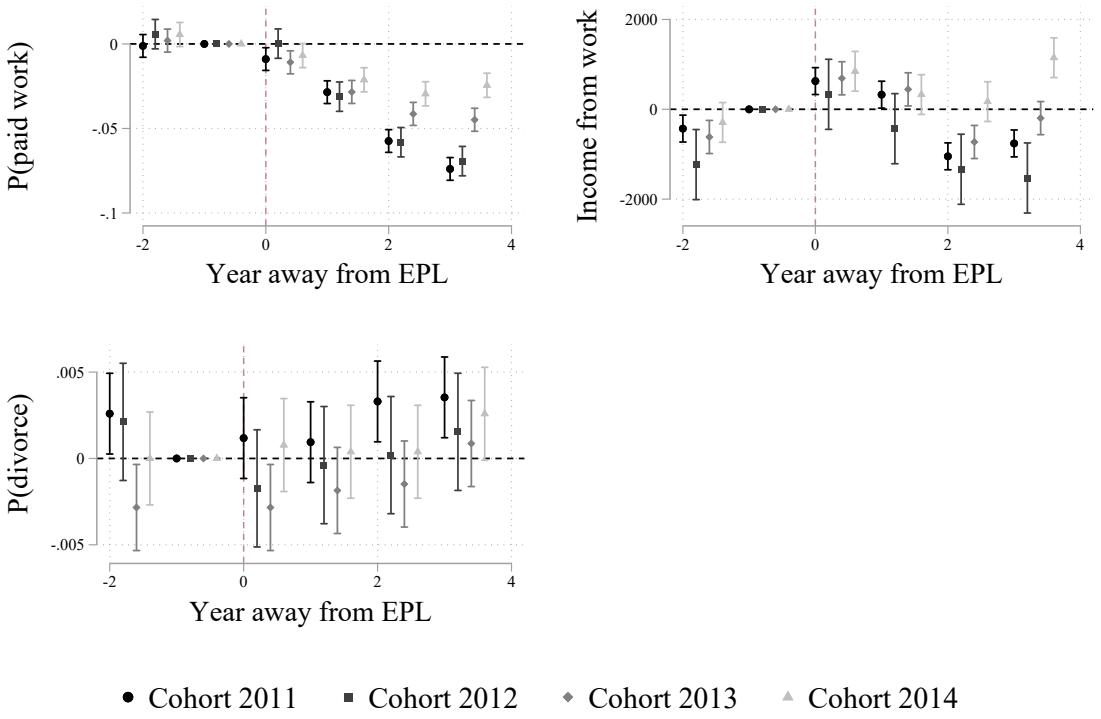
Note: Displays estimates of Equation 2.1 by cohort. Early pregnancy loss (EPL) cohort: a women having an early pregnancy loss in 2011 belongs to the 2011 cohort, and similar for the other years.

Figure A.2.3: Partner's mental health: event-study results by EPL cohort



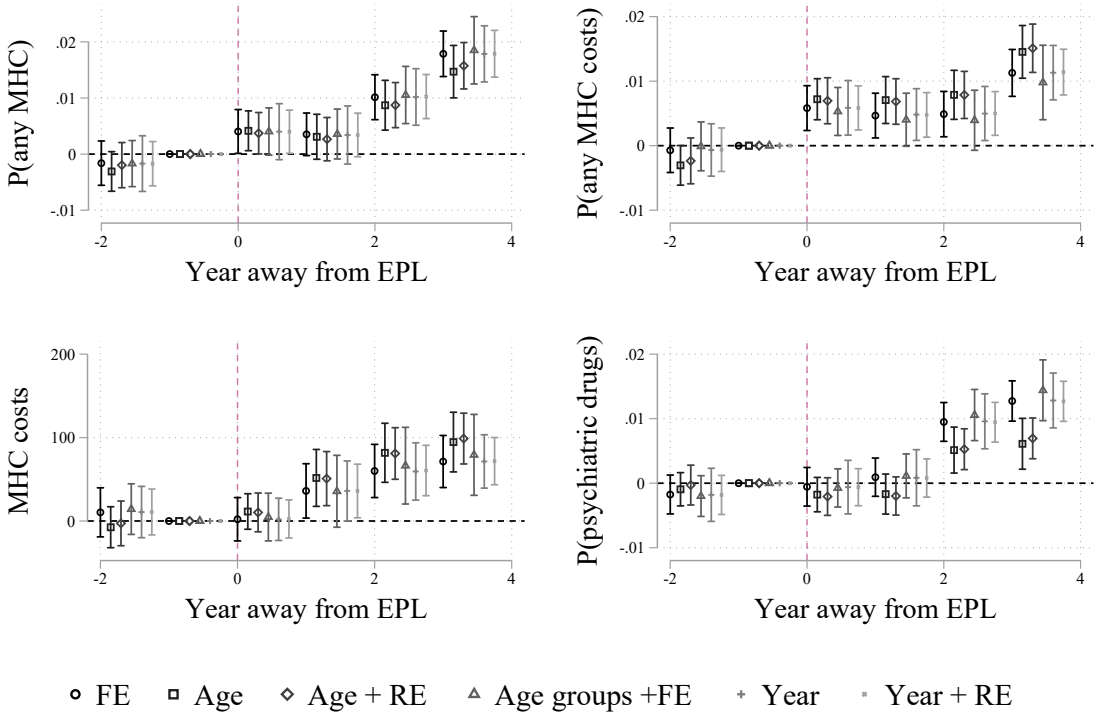
Note: Displays estimates of Equation 2.1 by cohort. Early pregnancy loss (EPL) cohort: a women having an early pregnancy loss in 2011 belongs to the 2011 cohort, and similar for the other years.

Figure A.2.4: Labour market and divorce: event-study results by EPL cohort



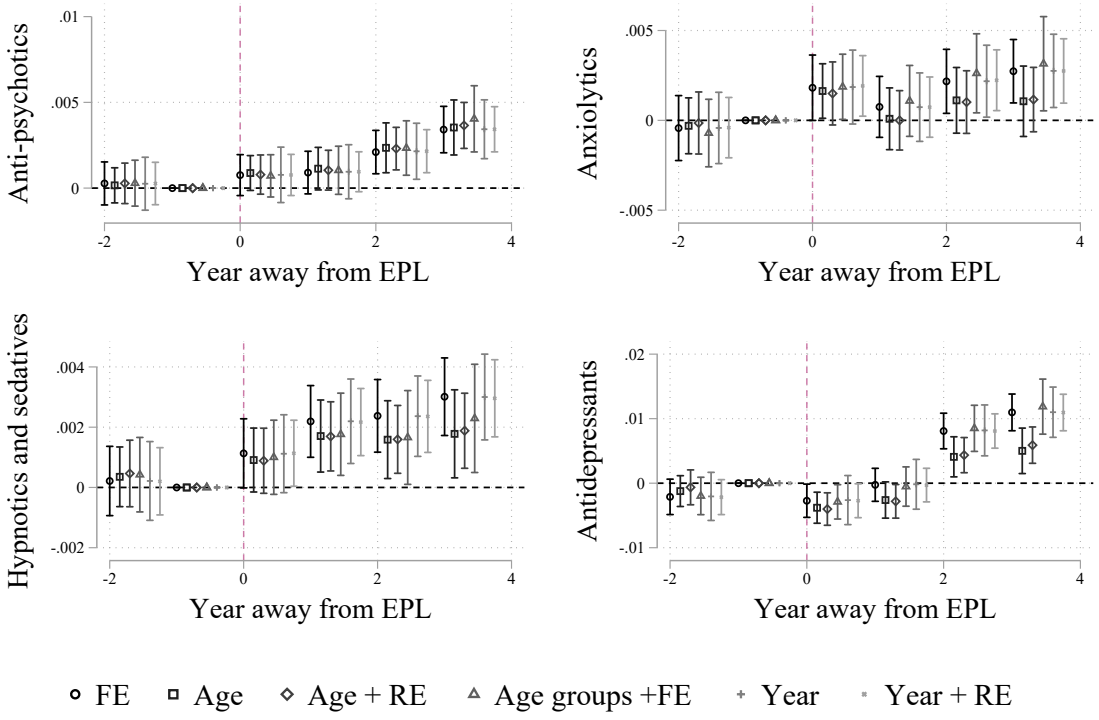
Note: Displays estimates of Equation 2.1 by cohort. Early pregnancy loss (EPL) cohort: a women having an early pregnancy loss in 2011 belongs to the 2011 cohort, and similar for the other years.

Figure A.2.5: Mental health care: aggregated event-study results comparison of models



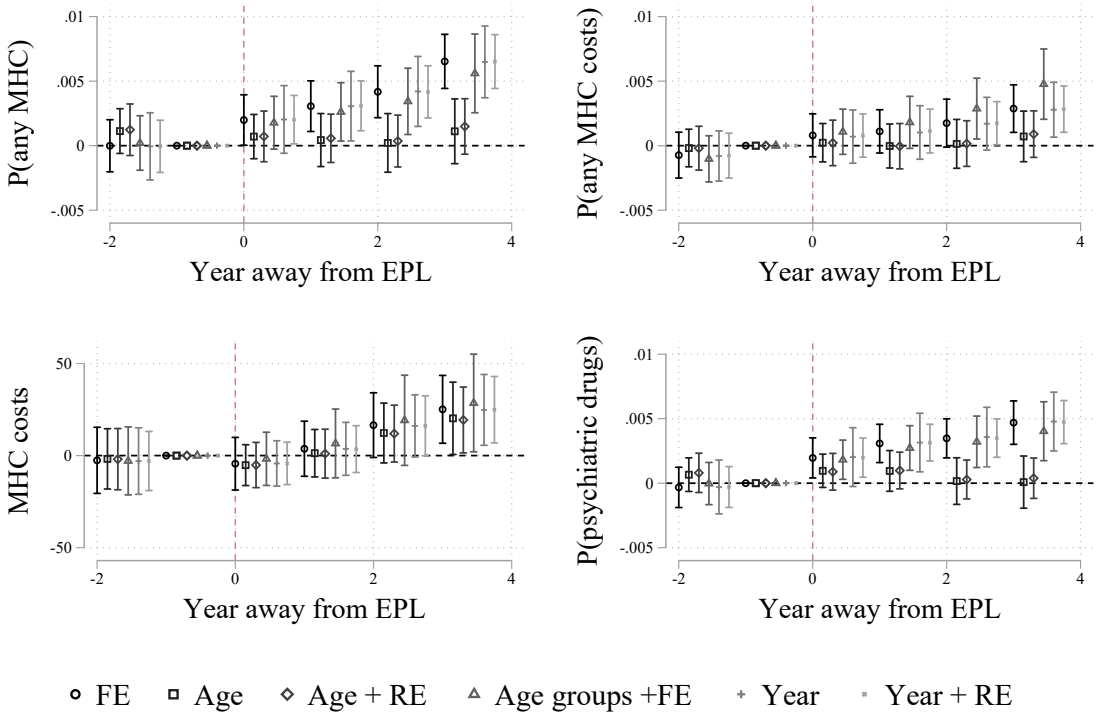
Note: Displays the mean estimates of six different models by cohort weighted by cohort size. An early pregnancy loss (EPL) cohort: a women having an early pregnancy loss in 2011 belongs to the 2011 cohort, and similar for the other years. 95% confidence intervals are obtained with a bootstrap using 1000 replications. Event-time -1 is the reference period. The 'FE' model displays the results from Equation 2.1. The 'Age' coefficients come from an OLS regression of the outcome on event-time indicators and age fixed effects. The 'Age + RE' coefficients come from panel random effects regression of the outcome on event-time indicators and age fixed effects. The 'Age groups+FE' model displays the results from Equation 2.1 with four year age groups as additional controls. The 'year' coefficients come from an OLS regression of the outcome on event-time indicators and calendar time fixed effects. The 'year + RE' coefficients come from panel random effects regression of the outcome on event-time indicators and calendar time fixed effects.

Figure A.2.6: Psychiatric drugs: aggregated event-study results comparison of models



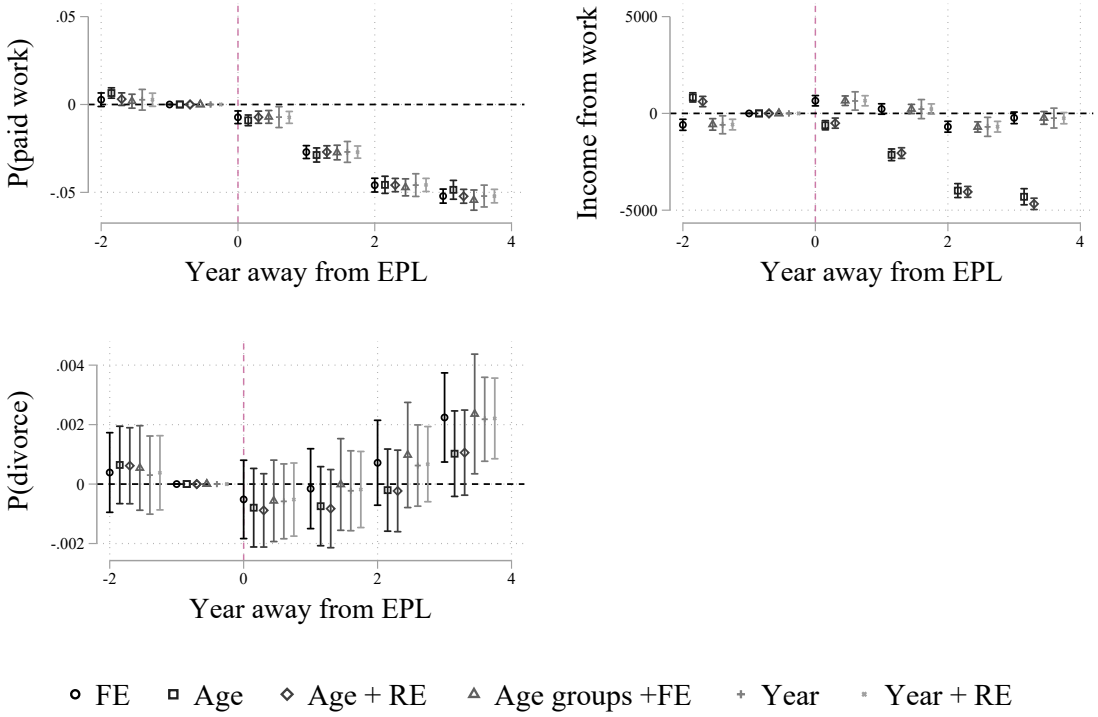
Note: Displays the mean estimates of six different models by cohort weighted by cohort size. An early pregnancy loss (EPL) cohort: a women having an early pregnancy loss in 2011 belongs to the 2011 cohort, and similar for the other years. 95% confidence intervals are obtained with a bootstrap using 1000 replications. Event-time -1 is the reference period. The 'FE' model displays the results from Equation 2.1. The 'Age' coefficients come from an OLS regression of the outcome on event-time indicators and age fixed effects. The 'Age + RE' coefficients come from panel random effects regression of the outcome on event-time indicators and age fixed effects. The 'Age groups+FE' model displays the results from Equation 2.1 with four year age groups as additional controls. The 'year' coefficients come from an OLS regression of the outcome on event-time indicators and calendar time fixed effects. The 'year + RE' coefficients come from panel random effects regression of the outcome on event-time indicators and calendar time fixed effects.

Figure A.2.7: Partner's mental health care use: aggregated event-study results comparison of models



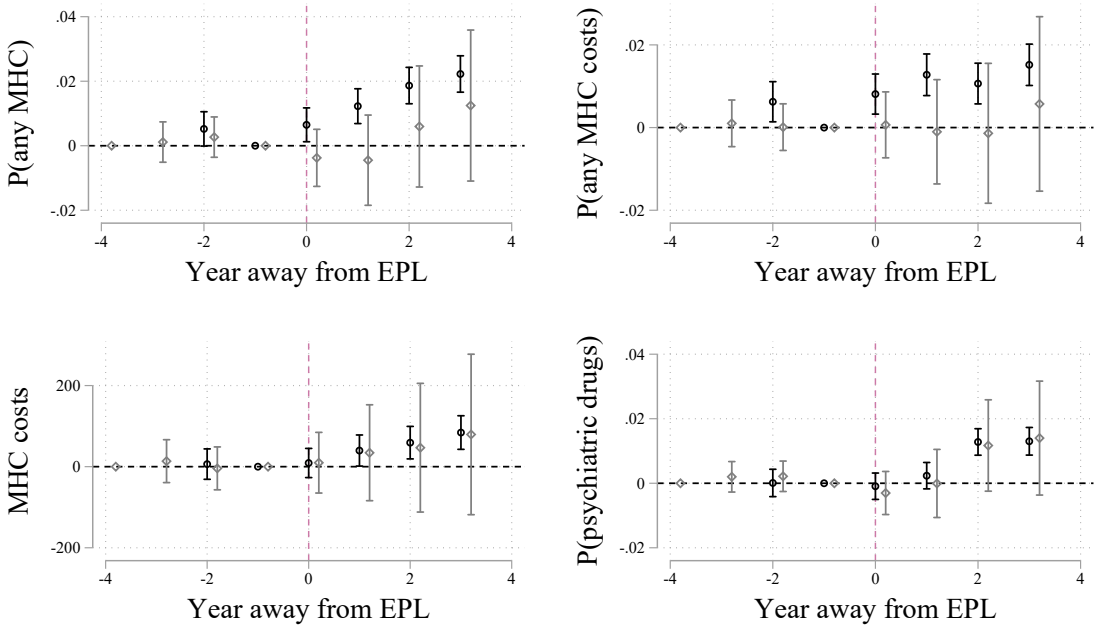
Note: Displays the mean estimates of six different models by cohort weighted by cohort size. An early pregnancy loss (EPL) cohort: a women having an early pregnancy loss in 2011 belongs to the 2011 cohort, and similar for the other years. 95% confidence intervals are obtained with a bootstrap using 1000 replications. Event-time -1 is the reference period. The 'FE' model displays the results from Equation 2.1. The 'Age' coefficients come from an OLS regression of the outcome on event-time indicators and age fixed effects. The 'Age + RE' coefficients come from panel random effects regression of the outcome on event-time indicators and age fixed effects. The 'Age groups+FE' model displays the results from Equation 2.1 with four year age groups as additional controls. The 'year' coefficients come from an OLS regression of the outcome on event-time indicators and calendar time fixed effects. The 'year + RE' coefficients come from panel random effects regression of the outcome on event-time indicators and calendar time fixed effects.

Figure A.2.8: Labour market and divorce: aggregated event-study results comparison of models



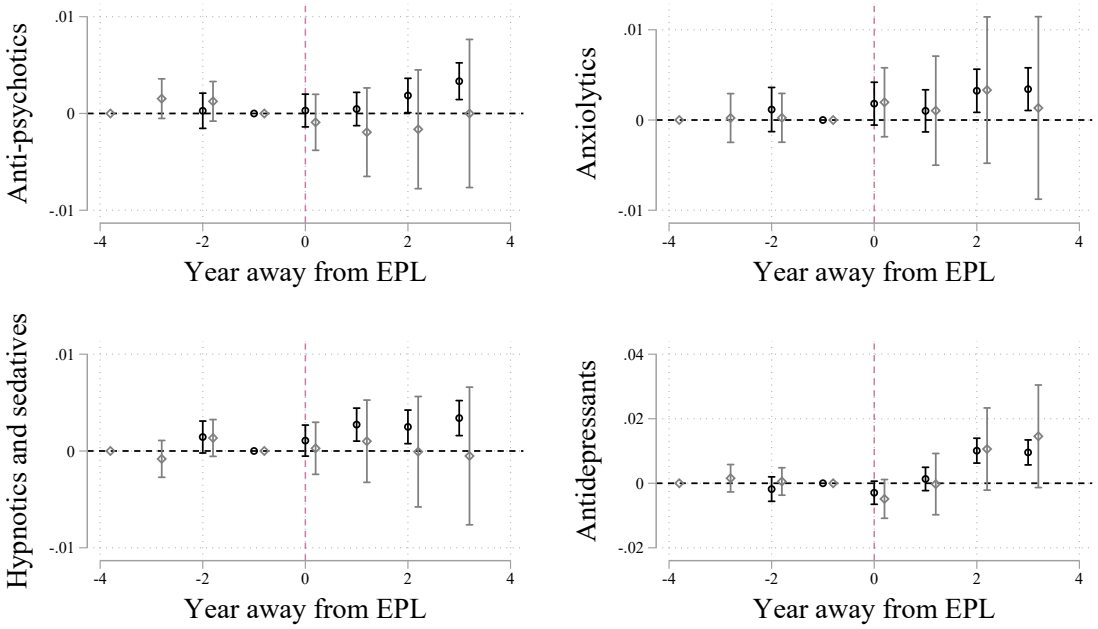
Note: Displays the mean estimates of six different models by cohort weighted by cohort size. An early pregnancy loss (EPL) cohort: a women having an early pregnancy loss in 2011 belongs to the 2011 cohort, and similar for the other years. 95% confidence intervals are obtained with a bootstrap using 1000 replications. Event-time -1 is the reference period. The 'FE' model displays the results from Equation 2.1. The 'Age' coefficients come from an OLS regression of the outcome on event-time indicators and age fixed effects. The 'Age + RE' coefficients come from panel random effects regression of the outcome on event-time indicators and age fixed effects. The 'Age groups+FE' model displays the results from Equation 2.1 with four year age groups as additional controls. The 'year' coefficients come from an OLS regression of the outcome on event-time indicators and calendar time fixed effects. The 'year + RE' coefficients come from panel random effects regression of the outcome on event-time indicators and calendar time fixed effects.

Figure A.2.9: Mental health care: aggregated event-study results omitting more than one pre-trend indicator



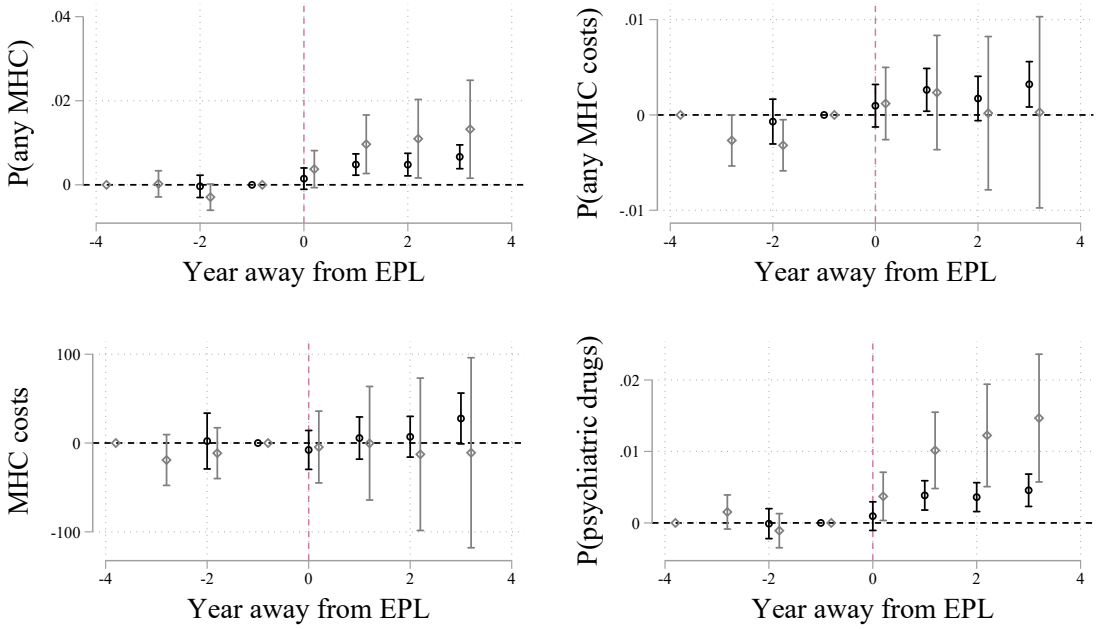
Note: Borusyak and Jaravel (2017) proposes to exclude the two furthest away pre-event indicators in order to be able to control for time and individual fixed effects. We use the 2013 and 2014 early pregnancy loss (EPL) cohorts, and we designate -1 and -4 as the reference periods. We estimate this model aggregated for the 2013 and 2014 EPL cohort and label it '2 pre-event periods as baseline'. We compare these results to the baseline estimates from Equation 2.1 (only including cohort 2013 and 2014 to ensure comparability) labeled 'Main'. 95% confidence intervals are obtained from the regression when we drop two pre-event indicators, and for the baseline model they stem from a bootstrap using 1000 replications.

Figure A.2.10: Psychiatric drugs: aggregated event-study results comparison of models



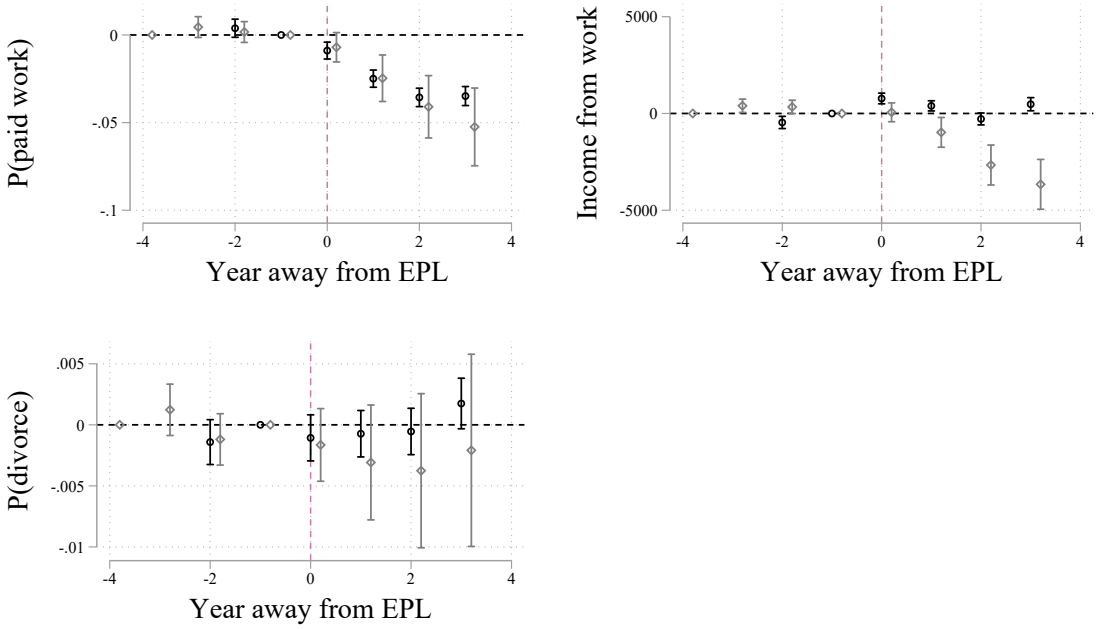
Note: Borusyak and Jaravel (2017) proposes to exclude the two furthest away pre-event indicators in order to be able to control for time and individual fixed effects. We use the 2013 and 2014 early pregnancy loss (EPL) cohorts, and we designate -1 and -4 as the reference periods. We estimate this model aggregated for the 2013 and 2014 EPL cohort and label it '2 pre-event periods as baseline'. We compare these results to the baseline estimates from Equation 2.1 (only including cohort 2013 and 2014 to ensure comparability) labeled 'Main'. 95% confidence intervals are obtained from the regression when we drop two pre-event indicators, and for the baseline model they stem from a bootstrap using 1000 replications.

Figure A.2.11: Partner's mental health care use: aggregated event-study results comparison of models



Note: Borusyak and Jaravel (2017) proposes to exclude the two furthest away pre-event indicators in order to be able to control for time and individual fixed effects. We use the 2013 and 2014 early pregnancy loss (EPL) cohorts, and we designate -1 and -4 as the reference periods. We estimate this model aggregated for the 2013 and 2014 EPL cohort and label it '2 pre-event periods as baseline'. We compare these results to the baseline estimates from Equation 2.1 (only including cohort 2013 and 2014 to ensure comparability) labeled 'Main'. 95% confidence intervals are obtained from the regression when we drop two pre-event indicators, and for the baseline model they stem from a bootstrap using 1000 replications.

Figure A.2.12: Labour market and divorce: aggregated event-study results comparison of models



Note: Borusyak and Jaravel (2017) proposes to exclude the two furthest away pre-event indicators in order to be able to control for time and individual fixed effects. We use the 2013 and 2014 early pregnancy loss (EPL) cohorts, and we designate -1 and -4 as the reference periods. We estimate this model aggregated for the 2013 and 2014 EPL cohort and label it '2 pre-event periods as baseline'. We compare these results to the baseline estimates from Equation 2.1 (only including cohort 2013 and 2014 to ensure comparability) labeled 'Main'. 95% confidence intervals are obtained from the regression when we drop two pre-event indicators, and for the baseline model they stem from a bootstrap using 1000 replications.

Figure A.2.13: Descriptive trends psychiatric drugs by type of drug

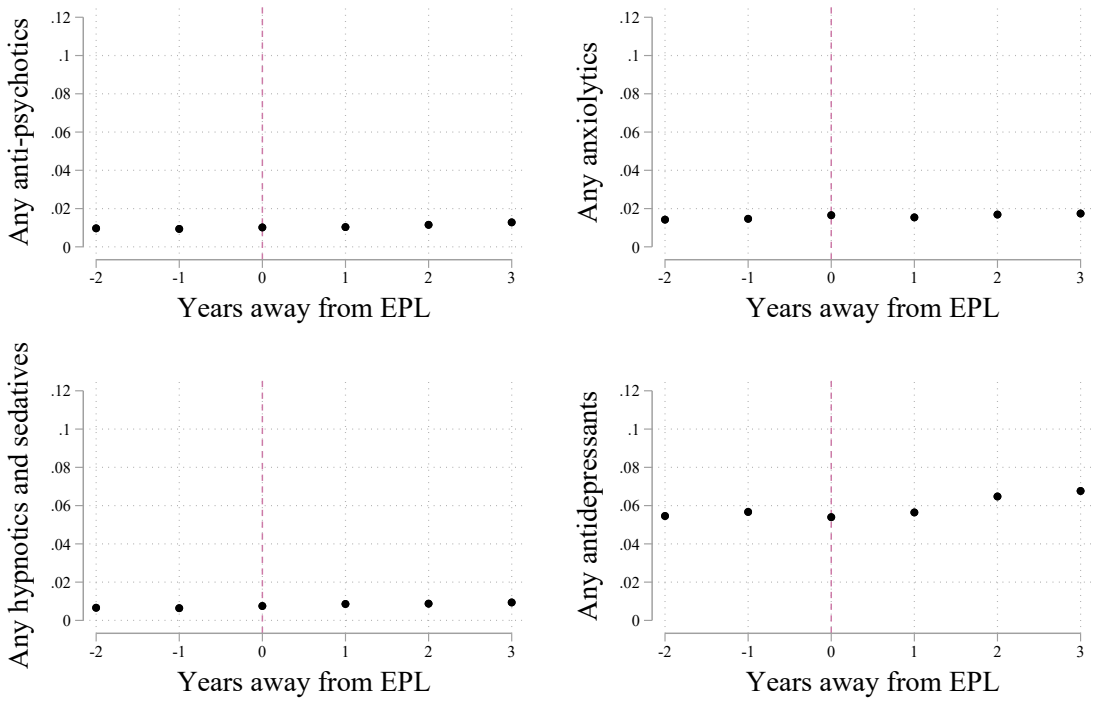
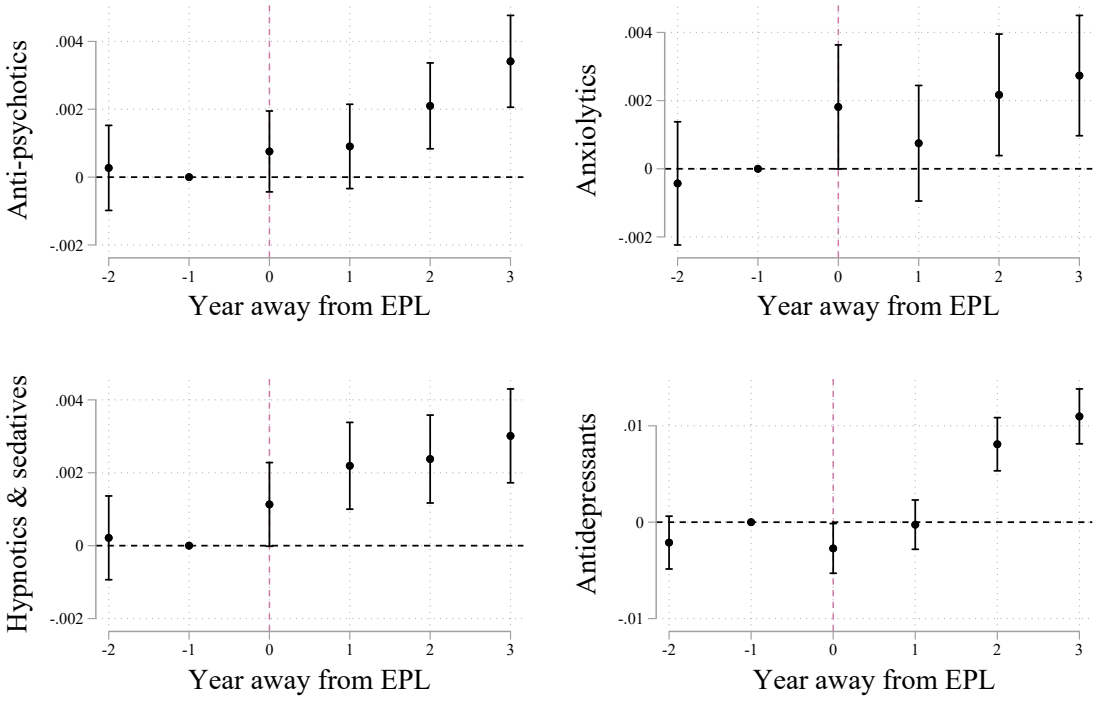
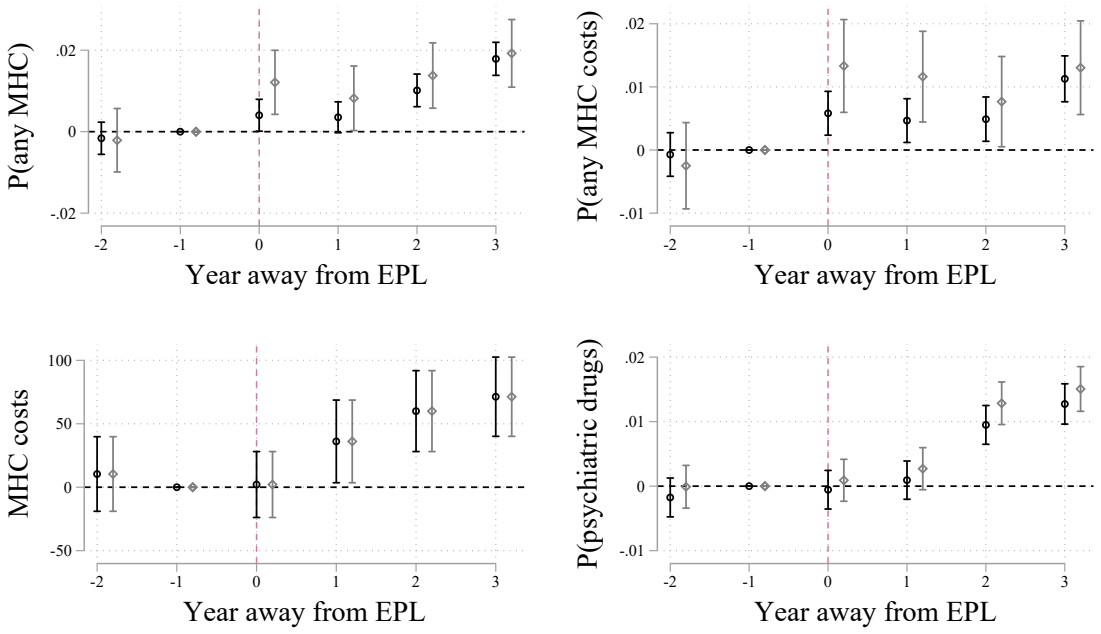


Figure A.2.14: Psychiatric drugs: event-study results aggregated



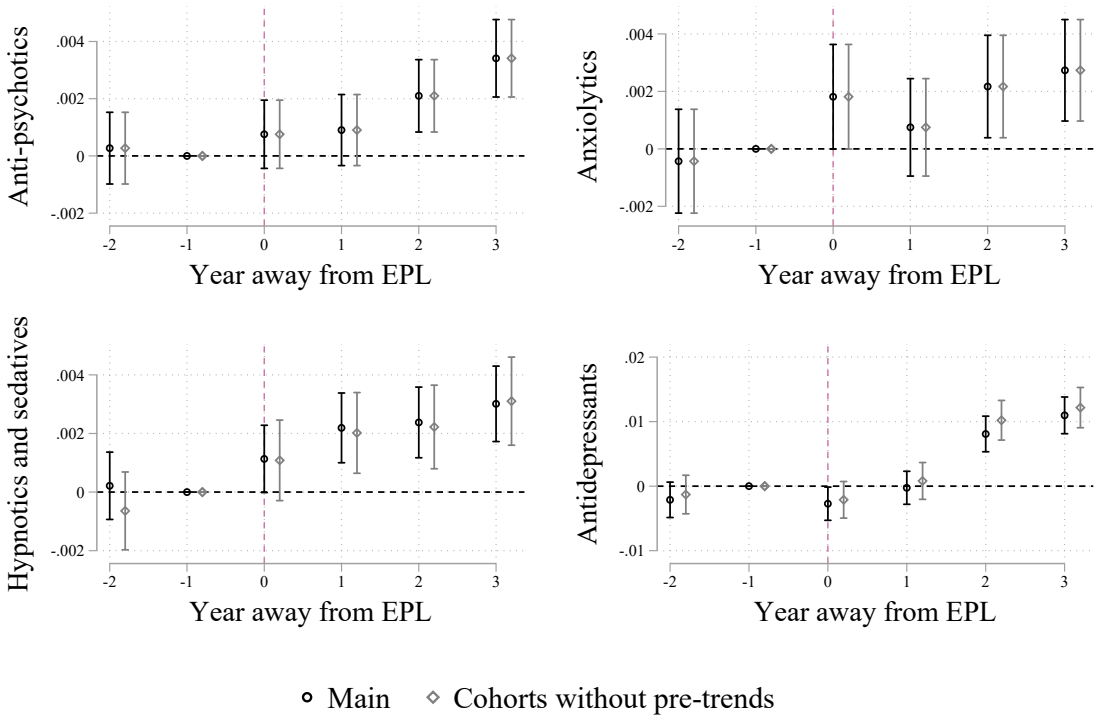
Note: Displays the mean estimates of Equation 2.1 by cohort weighted by cohort size. An early pregnancy loss (EPL) cohort: a women having an early pregnancy loss in 2011 belongs to the 2011 cohort, and similar for the other years. 95% confidence intervals are obtained with a bootstrap using 1000 replications. Event-time -1 is the reference period.

Figure A.2.15: Mental health care: aggregated event-study results only based on cohorts with no pre-trends



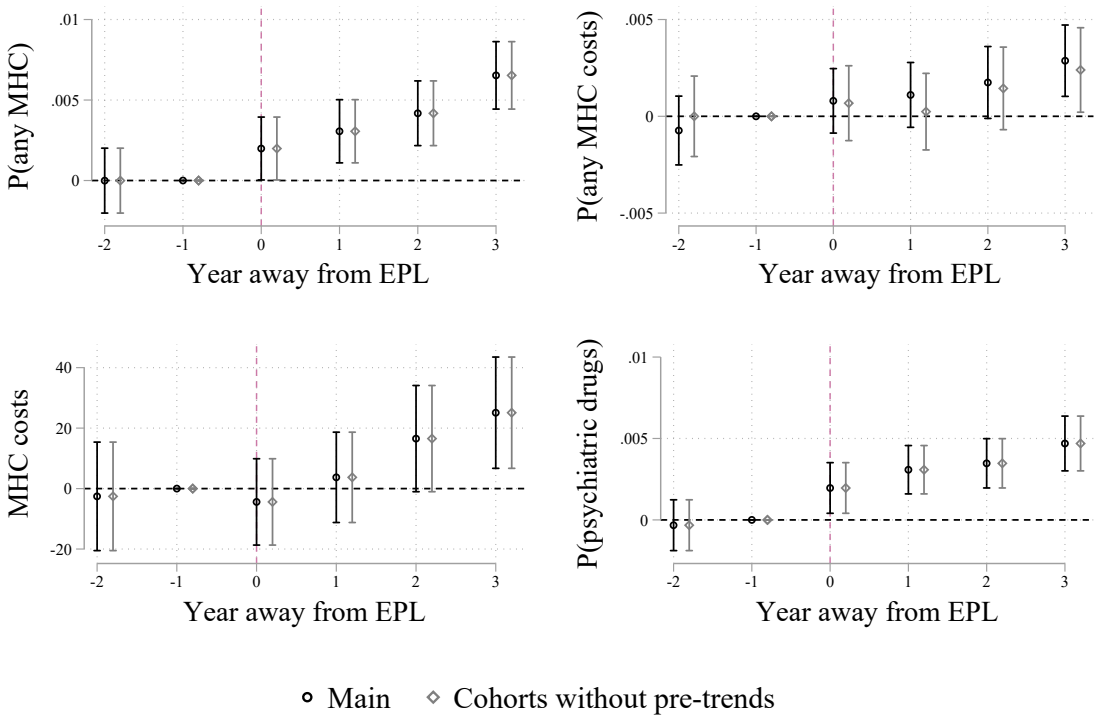
Note: Displays the mean estimates of three different models by cohort weighted by cohort size. An early pregnancy loss (EPL) cohort: a women having an early pregnancy loss in 2011 belongs to the 2011 cohort, and similar for the other years. 95% confidence intervals are obtained with a bootstrap using 1000 replications. Event-time -1 is the reference period. The 'Main' model displays the results from Equation 2.1. The 'Cohorts without pre-trends' coefficients come from an aggregation of cohorts who satisfy the no-pretrends assumption only.

Figure A.2.16: Psychiatric drugs: aggregated event-study results only based on cohorts with no pre-trends



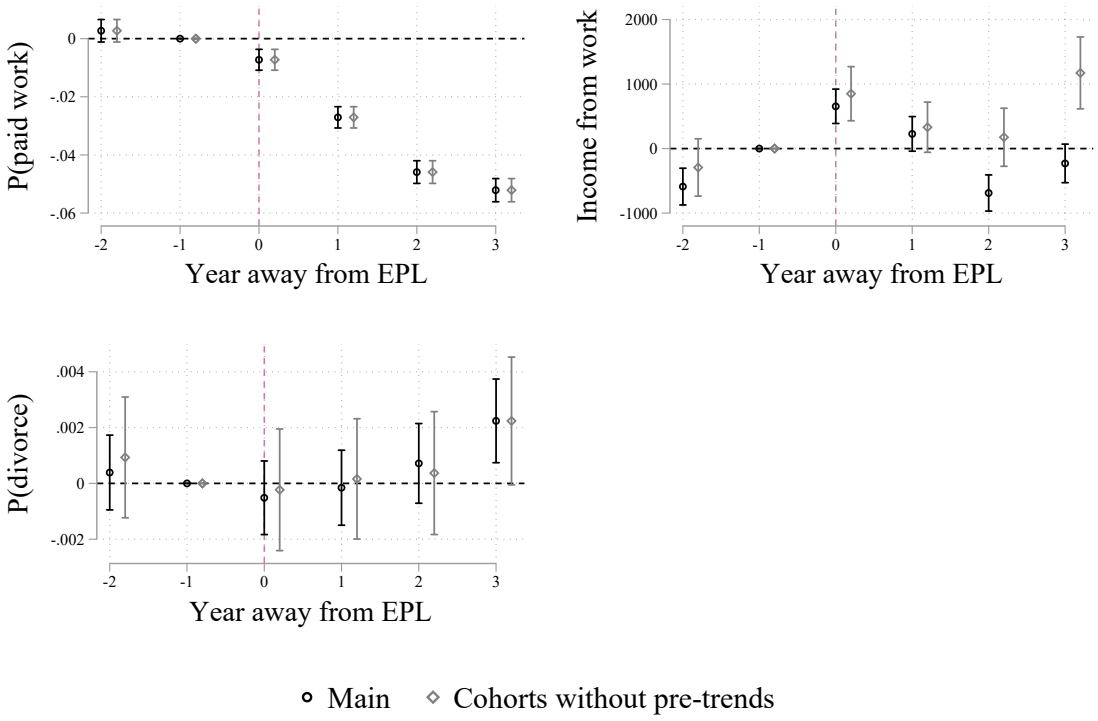
Note: Displays the mean estimates of three different models by cohort weighted by cohort size. An early pregnancy loss (EPL) cohort: a women having an early pregnancy loss in 2011 belongs to the 2011 cohort, and similar for the other years. 95% confidence intervals are obtained with a bootstrap using 1000 replications. Event-time -1 is the reference period. The 'Main' model displays the results from Equation 2.1. The 'Cohorts without pre-trends' coefficients come from an aggregation of cohorts who satisfy the no-pretrends assumption only.

Figure A.2.17: Partner's mental health care use: aggregated event-study results only based on cohorts with no pre-trends



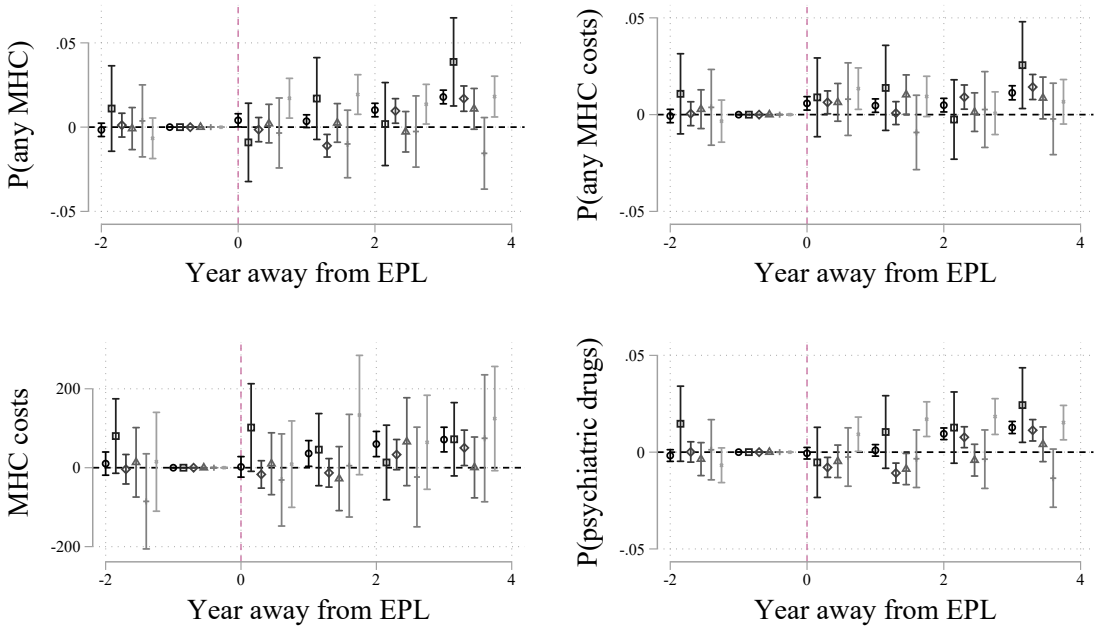
Note: Displays the mean estimates of three different models by cohort weighted by cohort size. An early pregnancy loss (EPL) cohort: a women having an early pregnancy loss in 2011 belongs to the 2011 cohort, and similar for the other years. 95% confidence intervals are obtained with a bootstrap using 1000 replications. Event-time -1 is the reference period. The 'Main' model displays the results from Equation 2.1. The 'Cohorts without pre-trends' coefficients come from an aggregation of cohorts who satisfy the no-pretrends assumption only.

Figure A.2.18: Labour market and divorce: aggregated event-study results only based on cohorts with no pre-trends



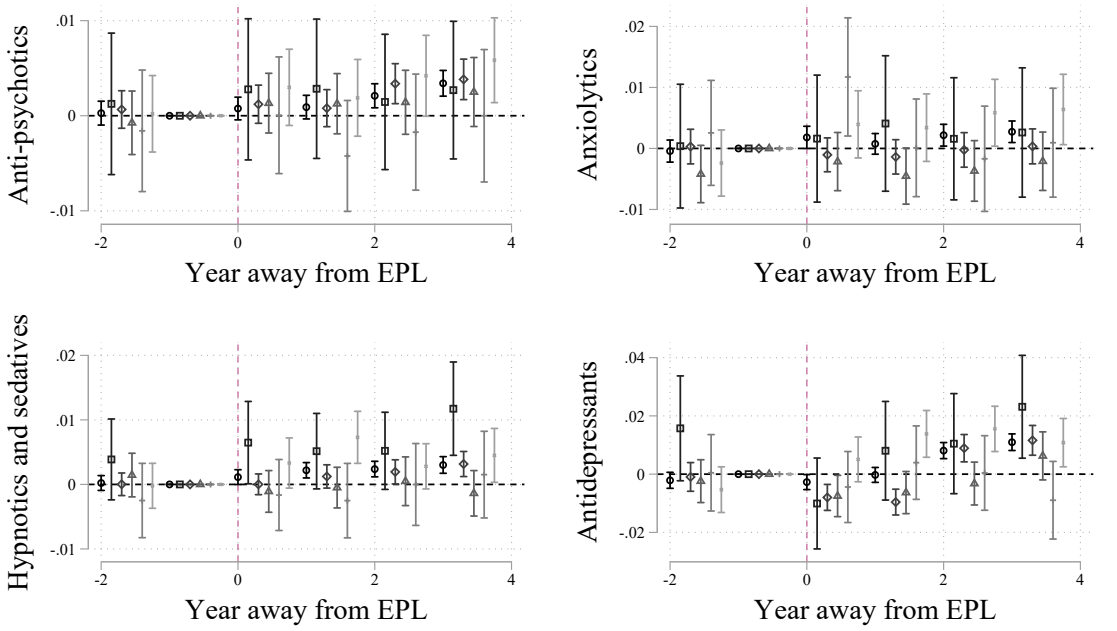
Note: Displays the mean estimates of three different models by cohort weighted by cohort size. An early pregnancy loss (EPL) cohort: a women having an early pregnancy loss in 2011 belongs to the 2011 cohort, and similar for the other years. 95% confidence intervals are obtained with a bootstrap using 1000 replications. Event-time -1 is the reference period. The 'Main' model displays the results from Equation 2.1. The 'Cohorts without pre-trends' coefficients come from an aggregation of cohorts who satisfy the no-pretrends assumption only.

Figure A.2.19: Mental health care: aggregated event-study results by arrival of first child



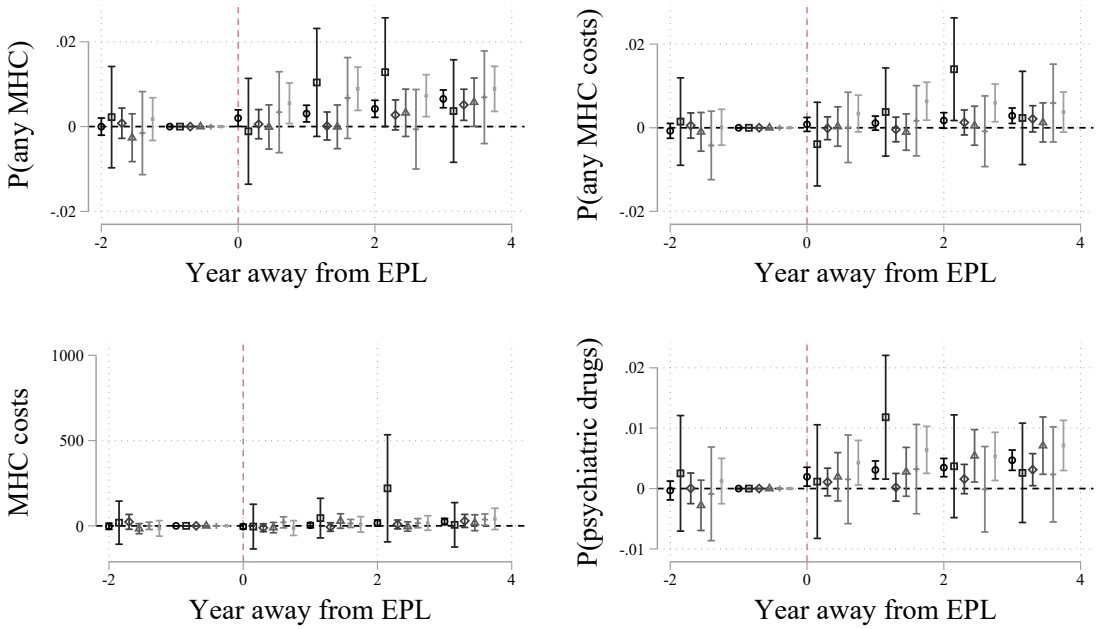
Note: Displays the mean estimates of Equation 2.1 by cohort weighted by cohort size for different samples. An early pregnancy loss (EPL) cohort: a women having an early pregnancy loss in 2011 belongs to the 2011 cohort, and similar for the other years. 95% confidence intervals are obtained with a bootstrap using 1000 replications. Event-time -1 is the reference period. The 'Main' include all women with an EPL. The 'Child at q=0' sample includes only the women who have their first child in the year of the early pregnancy loss. The 'Child at q=1' sample includes only the women who have their first child in the year after the early pregnancy loss. The 'Child at q=2' sample includes only the women who have their first child two years after the early pregnancy loss. The 'Child at q=3' sample includes only the women who have their first child 3 years after the early pregnancy loss. The 'No child up to q=3' sample includes only the women who have not had a child up to four years after the early pregnancy loss.

Figure A.2.20: Psychiatric drugs: aggregated event-study results by arrival of first child



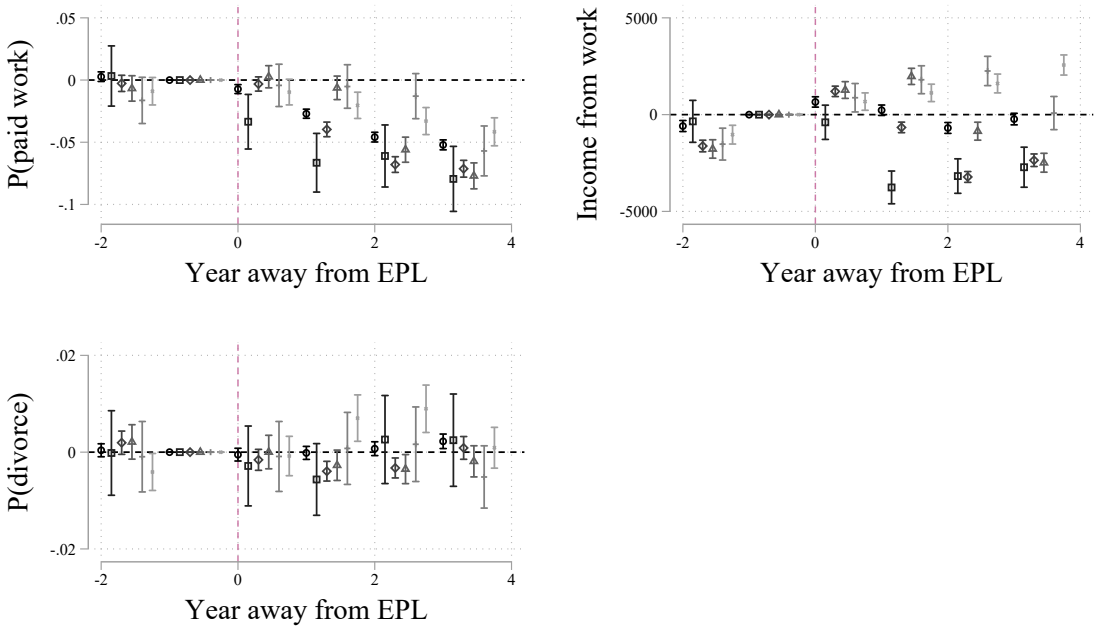
Note: Displays the mean estimates of Equation 2.1 by cohort weighted by cohort size for different samples. An early pregnancy loss (EPL) cohort: a women having an early pregnancy loss in 2011 belongs to the 2011 cohort, and similar for the other years. 95% confidence intervals are obtained with a bootstrap using 1000 replications. Event-time -1 is the reference period. The 'Main' include all women with an EPL. The 'Child at q=0' sample includes only the women who have their first child in the year of the early pregnancy loss. The 'Child at q=1' sample includes only the women who have their first child in the year after the early pregnancy loss. The 'Child at q=2' sample includes only the women who have their first child two years after the early pregnancy loss. The 'Child at q=3' sample includes only the women who have their first child 3 years after the early pregnancy loss. The 'No child up to q=3' sample includes only the women who have not had a child up to four years after the early pregnancy loss.

Figure A.2.21: Partner's mental health care use: aggregated event-study results by arrival of first child



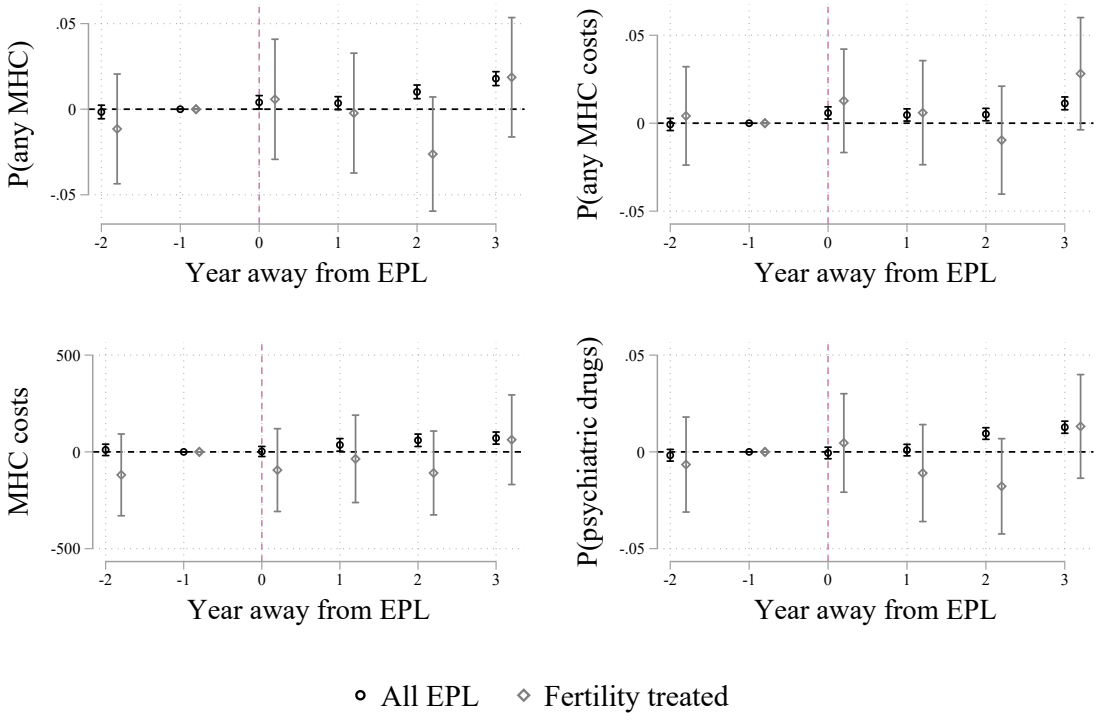
Note: Displays the mean estimates of Equation 2.1 by cohort weighted by cohort size for different samples. An early pregnancy loss (EPL) cohort: a women having an early pregnancy loss in 2011 belongs to the 2011 cohort, and similar for the other years. 95% confidence intervals are obtained with a bootstrap using 1000 replications. Event-time -1 is the reference period. The 'Main' include all women with an EPL. The 'Child at q=0' sample includes only the women who have their first child in the year of the early pregnancy loss. The 'Child at q=1' sample includes only the women who have their first child in the year after the early pregnancy loss. The 'Child at q=2' sample includes only the women who have their first child two years after the early pregnancy loss. The 'Child at q=3' sample includes only the women who have their first child 3 years after the early pregnancy loss. The 'No child up to q=3' sample includes only the women who have not had a child up to four years after the early pregnancy loss.

Figure A.2.22: Labour market and divorce: aggregated event-study results by arrival of first child



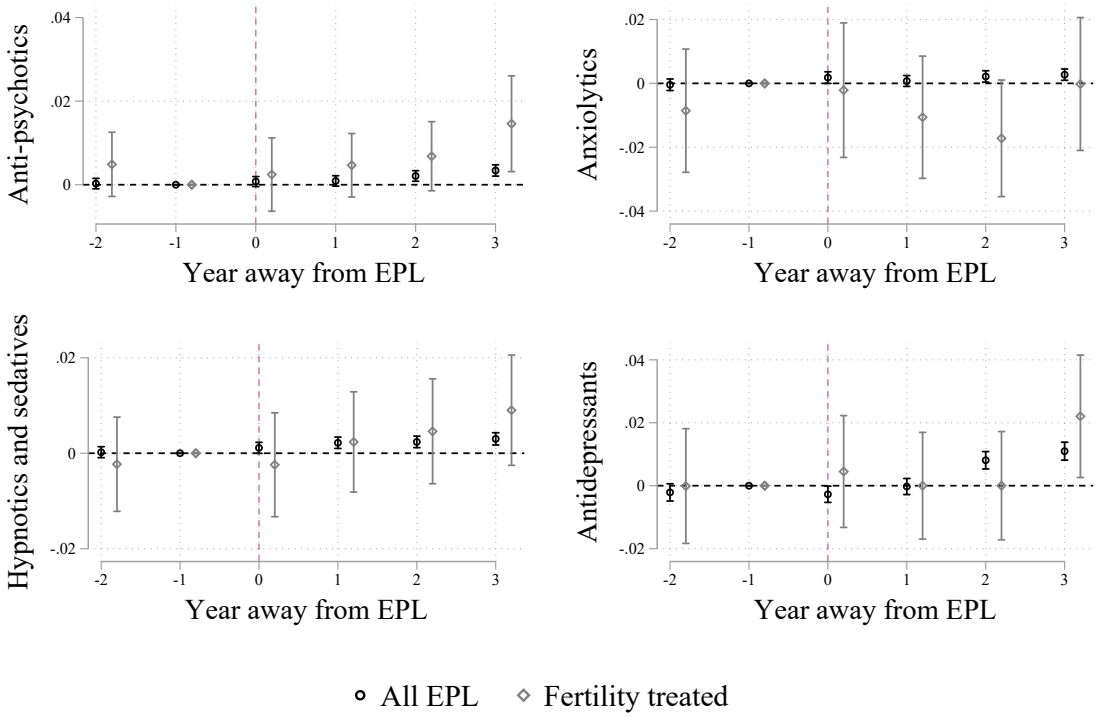
Note: Displays the mean estimates of Equation 2.1 by cohort weighted by cohort size for different samples. An early pregnancy loss (EPL) cohort: a women having an early pregnancy loss in 2011 belongs to the 2011 cohort, and similar for the other years. 95% confidence intervals are obtained with a bootstrap using 1000 replications. Event-time -1 is the reference period. The 'Main' include all women with an EPL. The 'Child at q=0' sample includes only the women who have their first child in the year of the early pregnancy loss. The 'Child at q=1' sample includes only the women who have their first child in the year after the early pregnancy loss. The 'Child at q=2' sample includes only the women who have their first child two years after the early pregnancy loss. The 'Child at q=3' sample includes only the women who have their first child 3 years after the early pregnancy loss. The 'No child up to q=3' sample includes only the women who have not had a child up to four years after the early pregnancy loss.

Figure A.2.23: Mental health care: aggregated event-study results for women with fertility treatment



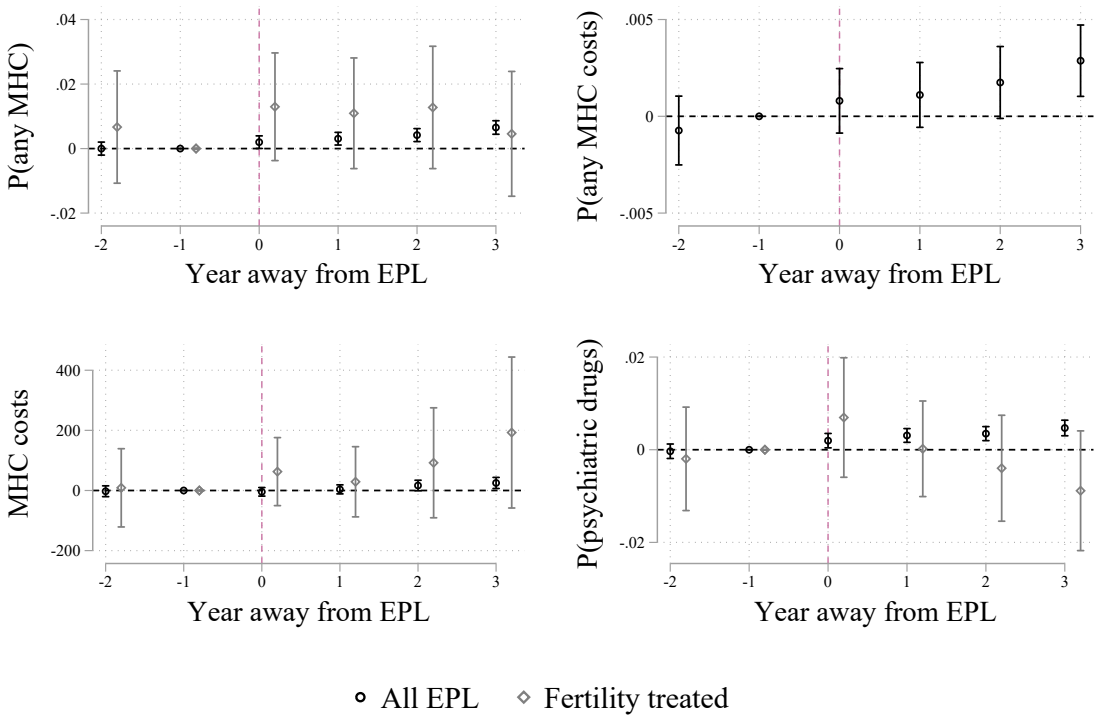
Note: Displays the mean estimates of Equation 2.1 by cohort weighted by cohort size for different samples. An early pregnancy loss (EPL) cohort: a women having an early pregnancy loss in 2011 belongs to the 2011 cohort, and similar for the other years. 95% confidence intervals are obtained with a bootstrap using 1000 replications. Event-time -1 is the reference period. The 'All EPL' include all women with an EPL. The 'Fertility treated' sample includes only the women who have undergone fertility treatment in the year before or in the year of the pregnancy loss.

Figure A.2.24: Psychiatric drugs: aggregated event-study results for women with fertility treatment



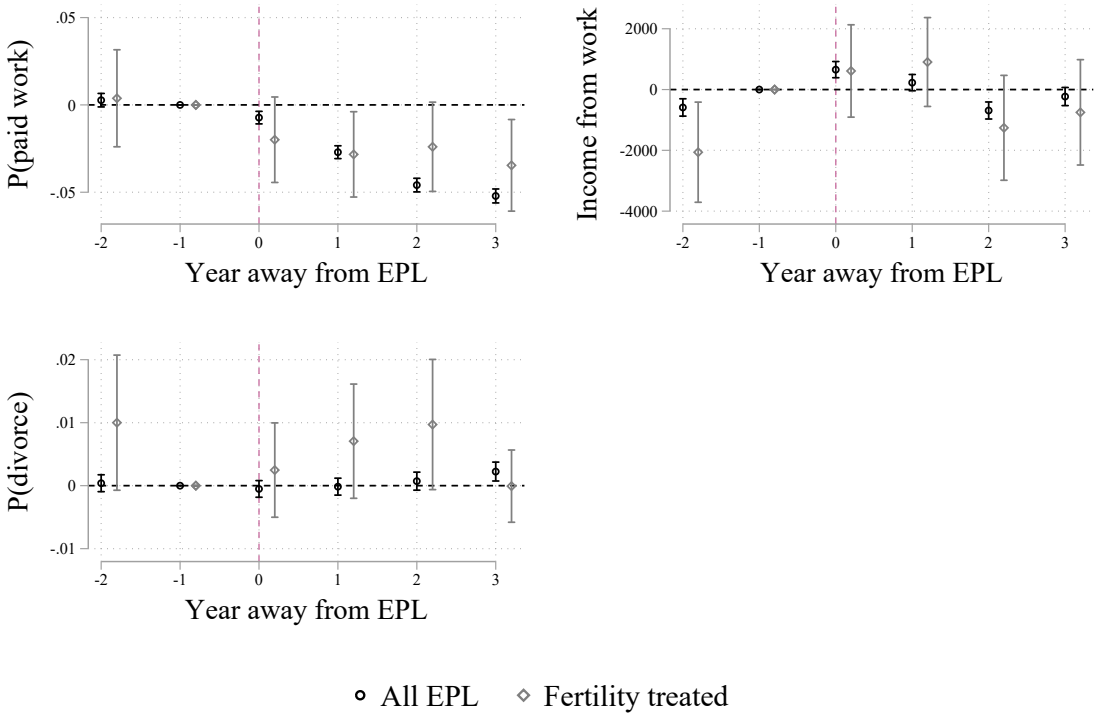
Note: Displays the mean estimates of Equation 2.1 by cohort weighted by cohort size for different samples. An early pregnancy loss (EPL) cohort: a women having an early pregnancy loss in 2011 belongs to the 2011 cohort, and similar for the other years. 95% confidence intervals are obtained with a bootstrap using 1000 replications. Event-time -1 is the reference period. The 'All EPL' include all women with an EPL. The 'Fertility treated' sample includes only the women who have undergone fertility treatment in the year before or in the year of the pregnancy loss.

Figure A.2.25: Partner's mental health care use: aggregated event-study results for women with fertility treatment



Note: Displays the mean estimates of Equation 2.1 by cohort weighted by cohort size for different samples. An early pregnancy loss (EPL) cohort: a women having an early pregnancy loss in 2011 belongs to the 2011 cohort, and similar for the other years. 95% confidence intervals are obtained with a bootstrap using 1000 replications. Event-time -1 is the reference period. The 'All EPL' include all women with an EPL. The 'Fertility treated' sample includes only the women who have undergone fertility treatment in the year before or in the year of the pregnancy loss.

Figure A.2.26: Labour market and divorce: aggregated event-study results for women with fertility treatment



Note: Displays the mean estimates of Equation 2.1 by cohort weighted by cohort size for different samples. An early pregnancy loss (EPL) cohort: a women having an early pregnancy loss in 2011 belongs to the 2011 cohort, and similar for the other years. 95% confidence intervals are obtained with a bootstrap using 1000 replications. Event-time -1 is the reference period. The 'All EPL' include all women with an EPL. The 'Fertility treated' sample includes only the women who have undergone fertility treatment in the year before or in the year of the pregnancy loss.

Chapter 3

Can Gender Norms Explain the Child Penalty?

3.1 Introduction

From political rights to education and equality before the law, there has been an effort to anchor gender equality in developed societies in the last century. Women have now the same rights as men; and they have similar or even higher education levels on average. Despite equal rights, there are still substantial gender differences on the labour market, especially once children are born. The so-called 'child penalty'[32] measures the relative change in female labour market outcomes compared to men after birth of the first child. The persistence of this child penalty for women is remarkable for two reasons. First, the parity in education levels between men and women, and hence their similar earnings potential give little monetary predisposition of which person should sacrifice his/her career for taking care of the children. Second, the increasing availability of child care would give room for both parents to keep pursuing their careers with modest child penalties. In this paper, I examine whether bringing gender norms into the picture can provide a partial explanation for the child penalty.

The Netherlands is an interesting setting to study the child penalty, because of particular gender norms with regard to labor supply decisions. Part-time work is very widespread even among men, they are twice as likely to work part-time as the OECD average (OECD, 2019c). Furthermore, female labour market participation is high (OECD, 2019a). At the same time,

[32]The term 'child penalty' is used in the literature, and I follow this terminology. It designates the negative labour market effect of having children.

the one-and-a-half earners household model is very prevalent, meaning that men often work (close to) full-time, and women part-time. This implies that it is not clear a priori whether the child penalty in the Netherlands can be expected to be large or small compared to other countries. On the one hand, the part-time work of men may attenuate the child penalty for women and distribute the labour market costs of having children more evenly in couples. On the other hand, extensive part-time work of women may lead to an even larger child penalty, if only women reduce their hours of work after child birth. This scenario may be plausible especially since the difference in hours worked is one of the most important determinants of the gender wage gap today (Blau and Kahn, 2017), and because women work more often part-time than men, even when they do not have children (Merens and Bucx, 2018).

In this study, I estimate the child penalty for Dutch first-time parents in 2002-2008 for unconditional earnings, participation, and fulltime-equivalent (FTE) based on the method proposed by Kleven et al. (2019b). Given the substantial child penalty for all three labour market outcomes for women, I explore to what extent gender norms are a plausible explanation for the child penalty in the Netherlands by comparing child penalties of the following informative sub-groups. First, I compare the child penalty of the Dutch Bible Belt, where more non-egalitarian gender norms prevail, to the rest of the country. Second, I examine the child penalty by closeness to grandparents, hypothesising that individuals with less egalitarian gender attitudes are more likely to live close to their parents. Third, I verify whether a part-time culture for men at the firm level[33] can alleviate the child penalty of their female partner. Fourth, I examine female same-sex parents, whose negotiation about the division of labour in the household cannot be guided by gender norms. Finally, I rule out that the child penalty arises due to income maximisation only by focusing on couples where she earns more than him before giving birth.

In the literature, the gender wage gap and its determinants have been examined extensively. Blau and Kahn (2017) review the determinants of the gender wage gap. They report that in general the gender wage gap has narrowed over time, but that the narrowing tendencies have stalled since the 1990ies. Not only the size of the gap, but also its determinants have changed over time. While human capital differences at the beginning of the career used to be important in the past, nowadays work interruptions and shorter hours of women take a

[33] A part-time culture at firm level may include firm institutions that allow fathers to reduce their hours, and/or male colleagues who set examples for their peers to reduce their hours when becoming a parent.

more prominent place in explaining the gender wage gap, especially at the higher end of the income distribution.

In line with this development, many studies focus on the impact of children on labour market outcomes of their parents (for example Kleven et al., 2019a,b; Angelov et al., 2016; Andresen and Nix, 2019; Sieppi and Pehkonen, 2019; Moberg, 2016; Rosenbaum, 2019; Sigle-Rushton and Waldfogel, 2007; Adema et al., 2019).[34] All these studies find a large and persistent decline in labour market outcomes of heterosexual mothers compared to their partners (or heterosexual fathers in general) at birth of the first child. There is variation in the magnitude of the decline, and it seems that mothers from countries with more egalitarian gender norms and/or historically more generous family policies are likely to experience smaller declines than others (Kleven et al., 2019a).

Linking these child penalties to family policies, there is mixed evidence that the introduction of these policies is followed by a reduction in the child penalty (Andresen and Nix, 2019; Patnaik, 2014; Mari et al., 2018; Olivetti and Petrongolo, 2017). While studies evaluating the introduction of family policies find a small or negligible effect of these policies on the child penalty, countries with a long tradition of generous family policies, like the Nordic countries, have relatively low child penalties compared to countries with less generous or more recent family policies (Kleven et al., 2019a; Olivetti and Petrongolo, 2017). This seems contradictory, but may be reconciled if such policies mostly have long-run effects by changing norms.

The limited short-term potential of family policies to decrease the child penalty calls for additional explanations. Gender norms may be a good candidate for explaining the persistence in child penalties for three reasons. First, various studies have shown that norms can alter (labour market) outcomes (Vella, 1994; Fortin, 2005, 2015; Fernandez and Fogli, 2009; Fernández, 2007; Bertrand, 2011; Bursztyn et al., 2017; Akerlof and Kranton, 2000; Steinhauer, 2018). Second, norms are persistent (Alesina et al., 2013; Tur-Prats, 2019) and change slowly. Third, they are passed on generation by generation through the family or other social environment (Farré and Vella, 2013; Fernández et al., 2004; McGinn et al., 2019; Olivetti

[34] Sigle-Rushton and Waldfogel (2007) also reports results for the Netherlands. Using cross-sectional data from 2000, they compare cumulative forgone lifetime earnings of mothers to non-mothers and men for anglo-saxon, nordic and central european countries (Germany and the Netherlands). They find that an average women with medium education and two children at age 45 has earned 46% of a non-mother, or 39% of a man in the Netherlands. This is mainly due to a large gap in hours and participation compared to non-mothers. Compared to other countries, Central European mothers experience the largest earnings gaps.

et al., 2020; Fogli and Veldkamp, 2011). So far, the role played by culture and norms for the child penalty has only sparsely been examined. On the one hand, Kleven et al. (2019b) show that the daughter of a mother with a low labour market involvement when raising her children has a larger child penalty. Second, same-sex parents have a lower child penalty than different-sex parents (Andresen and Nix, 2019; Moberg, 2016; Rosenbaum, 2019).

Adema et al. (2019) have documented the child penalty for the Netherlands in a Dutch policy report using a representative survey sample combined with administrative data. I extend their analysis to the universe of Dutch first-time parents in 2002-2008. Exploiting the linked administrative data, I show that the pervasiveness of the one-and-a-half-job family model and the underlying gender norms attached to it increase the Dutch child penalty importantly. I contribute to the economics of gender norms literature by adding analyses comparing parts of the population with more (or less) egalitarian gender norms.

My results show that eight years after child birth, mothers have a child penalty of 44 percent of their gross earnings relative to fathers, which corresponds to about 16,000 € on average in a year. There is also a relative child penalty in participation (-22%) and the rate of part-time work (35% fewer hours). There are considerable differences by socio-economic groups. Pre-birth high income mothers have lower child penalties than lower income mothers. When examining the role of gender norms in five separate analyses, I find that women living in the Dutch Bible Belt, where less egalitarian gender attitudes prevail, incur a higher child penalty than women in the rest of the Netherlands. Second, women who live close to their parents, and therefore may have arguably less egalitarian gender attitudes, have a higher child penalty than women who moved further away from their origin - despite the higher availability of informal child care because of the proximity of their parents. Third, I show that a part-time work culture for fathers in the father's firm does not lead to a male child penalty, and that the partners of men working in part-time friendly firms have the same child penalties as other women with a male partner working in a part-time unfriendly firm. Fourth, same-sex couples, whose division of labour in the household cannot be inspired by gender norms, have a considerably lower child penalty than heterosexual women. Finally, I exclude that the child penalty results from an income maximisation strategy alone, where the lower income spouse, which is more often the woman, specialises in house and care work. In couples where the woman earns more than the man before children, the relative child penalty for women is extremely large (54%).

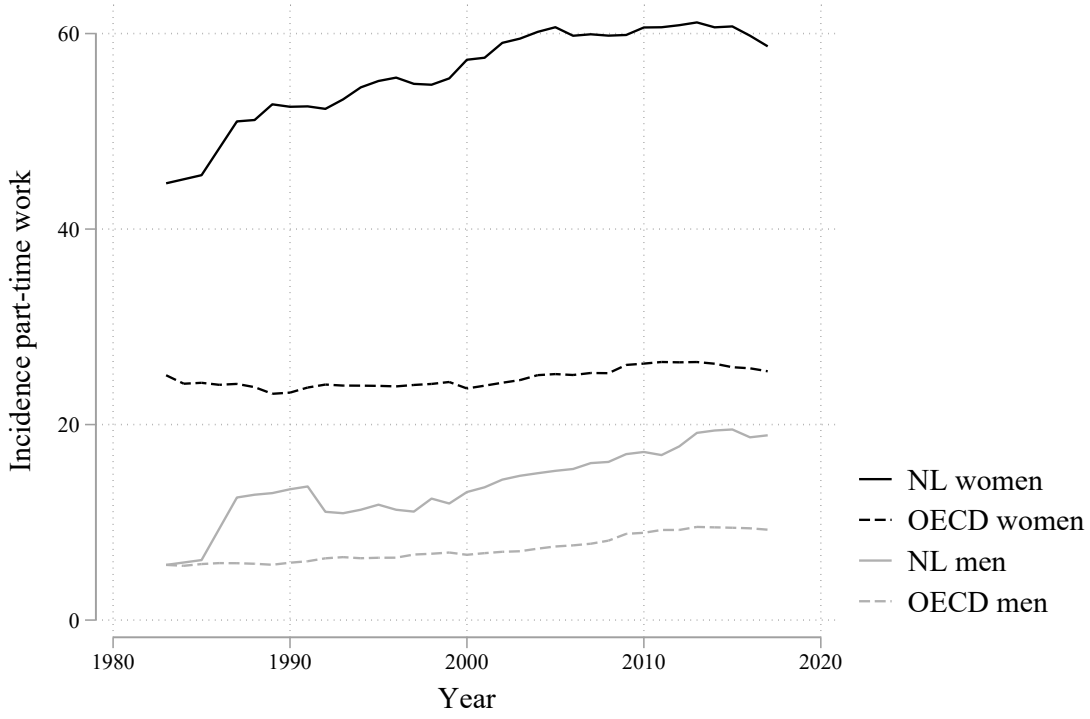
These findings suggest that the gendered division of labour is common in Dutch households. It is more pronounced in contexts with less egalitarian gender attitudes, but there are few different-sex couples who do not adhere to it, since there is no group of women that has a smaller child penalty than her partner. Only same-sex couples, not subject to intra-couple gender norms, seem to spread caregiving tasks more equally between partners. These results stand in contrast to results of the Dutch sample of the European Value Survey, where 80% of respondents state that they are of the opinion that men and women should have equal responsibilities for home and children. Gender norms of current generations are difficult to alter, since they are for a large part formed early in life (Bian et al., 2017). This implies that there is no policy that will immediately address the child penalty, because the evolution of norms takes time. However, such an evolution may be encouraged by increasing paid paternity leave (which is implemented in 2020 in the Netherlands), and by introducing paid parental leave for all families (planned for 2022). This may be one way to increase the currently low take-up of parental leave by fathers. In addition, any other policy increasing father's leave take-up may contribute to a faster change in norms and may thus reduce the child penalty.

3.2 Background

3.2.1 Part-time work in the Netherlands

The Dutch labour market is characterised by high participation and high part-time occurrence (OECD, 2013). Figure 3.1 shows the incidence of part-time work among the working population in the Netherlands and on average in OECD countries. Part-time work is almost three times as prevalent among Dutch women as among average OECD women, with around 60% of working women working part-time. Also Dutch men are about twice as likely to work part-time as their OECD counterparts - one fifth of Dutch working men works part-time. Yet, working part-time implies a very different reality for men and women. Whereas employed men work on average around 37 hours a week in 2003 to 2010, employed women work on average around 25 hours a week, even though they are on average better educated than men for the generations below 45 (Merens and Bucx, 2018; CBS, 2019d).

Figure 3.1: Part-time work in the Netherlands by gender



Note: Percent of the work-force that works part-time in the Netherlands and the OECD average by gender. Source: OECD (2019c).

The Dutch part-time culture has its origins in the second half of the twentieth century. Dutch married women had their entry into the labour market late compared to other countries. When there was a labour shortage in the 1960s, the first part-time jobs were created to enable women to participate in the labour force and meet this shortage, while still being able to do unpaid work at home. This tendency was further institutionalised in the 1980s, when the government and social partners actively promoted the creation of part-time jobs to increase labour market flexibility and participation in times of recession. In a relatively short time, the Dutch economy developed from the male breadwinner model with low female labour market participation into a gendered one-and-a-half-job-households where the man was the breadwinner and the women works part-time and takes care of children and household (Tijdens, 2006). This model was kept and further institutionalised in the past thirty years (Bosch et al., 2010).

The institutionalisation of the gendered one-and-a-half-jobs household is reflected in many aspects (Bosch et al., 2010). For example, there are many part-time jobs with good working conditions (Merens and Bucx, 2018). Offering part-time jobs has even been turned

into a legal obligation for employers (Wielers and Raven, 2013).[35] Moreover, in typical female-dominated sectors such as health care and teaching, part-time work is very common. In typically male-dominated professions, the options are more limited. Since women are more likely to choose typical female professions, they will also have more opportunities to work part-time (Merens and Bucx, 2018).

Today, labour market patterns still largely confirm the gendered one-and-a-half-job household model. When entering the labour market, women are as likely to work as men, and they work at better wage rates than men thanks to their higher education levels. But already at entry stage, they are more likely to work part-time. Both genders increase their hours worked until they are around thirty years old, but from then onwards trends diverge. Women start to become less likely to work, and if they do they work fewer hours, since they are the main responsibles for child rearing (CBS, 2011).

Yet, Dutch women are on average not unhappy with their part-time work. Only a minority of part-time working women reports that they would want to increase working hours, probably also because of the good working conditions in part-time jobs. Moreover, apart from caring for children and running the household, women also indicate to work part-time to have time for themselves and hobbies. There is a clear gender difference in priorities. Whereas men report wanting to focus on their career and that part-time work would hinder their career, women report wanting to combine family care and paid work (Merens and Bucx, 2018).

3.2.2 Family policies in the Netherlands

In order to facilitate and incentivise the combination of having a family and paid work, the Netherlands has introduced a set of family and tax policies in the past fifteen years. These include universal child care, parental leave, a short paternity leave, maternity leave, and tax incentives for second earners.

Since 1990, the use of formal child care has been growing continuously (with a decrease from 2011-2014, which is more than compensated by 2018) (Jongen, 2008; Statline, 2019a). Before 2005, child care in the Netherlands was partly subsidized by municipalities and em-

[35] In practice, this law is considered to be a confirmation of existing practices and mostly of symbolic nature (The Economist, 2015).

ployers (76%) or provided by commercial, unsubsidized providers (24%). The latter was more expensive than the former, but deductible from taxes (Bettendorf et al., 2015).

In 2005, the child care law unified subsidies for child care and made them available for all families with two working parents (or one working single parent) (Bettendorf et al., 2015). In the following years, the budget for child care subsidies was expanded, and public expenses for formal child care went from 0,5 € billion in 2005 to 2,9 billion € in 2009 (Statline, 2019c). Due to the large increase in public expenses and the economic crisis in 2008, the subsidies were cut again until 2015, and are increasing again since (see Figure A.3.1 for details). There are three types of child care in the Netherlands: children day care for 0-3 year old children, out of school care (4-12 years old), and host parenting[36] for both age groups of children (Jongen, 2008). Parents get a subsidy per hour of child care, and the amount of the subsidy depends on household income, price of child care (capped at a maximum by type of child care), the rank of the child in the family and the hours worked by the less working partner since 2012. Bettendorf et al. (2015) evaluated the impact of the introduction of child care on female labour supply. They find a modest increase of participation and hours worked due to the reform, but there was no effect for the low educated (Jongen et al., 2011).

Second earner tax credits are another instrument aimed at encouraging female labour supply. De Boer et al. (2018) provide an overview of the changes in the tax and family subsidy system in the Netherlands. Between 2005 and 2017, there are changes in the tax system mostly benefiting dual earner couples and thus incentivising both partners to work.

There are three types of leave related to child birth in the Netherlands. First, maternity leave consists of 16 weeks, of which at least 4 weeks have to be taken before birth, and at least 6 after birth. It is paid at 100% up to a ceiling equivalent to the maximum daily payment for sickness benefit. Since it is mandatory, take up of maternity leave is near 100%. Second, there are two days of paternity leave after birth, paid at 100% without a maximum. Some collective agreements are more generous, others more restrictive. In 2013, 83% of fathers took leave, and 60% of these prolonged their leave by taking annual leave den Dulk (2017). Take up of paternity leaves stayed constant between 2011 and 2017 (Korvorst, 2019).

[36]Host parenting is a flexible form of child care provided to up to 6 children by anyone who registers as a host parent, either at the children's or the host parent's home. In 2010, criteria to be able to register as a host parent were made stricter. Until then, almost half of host-parent child care was provided by grandparents (Intomart Gfk, 2011).

Third, both parents are entitled to parental leave for 13 working weeks in the period 2002 to 2008 (CBS, 2011). It is not paid, but some collective agreements offer partial payment of the leave. This leave can be taken until a child is 8 years old, and parents are flexible in how they want to use the leave. Before 2015, parents were eligible if they were employed one year before birth at the current employer (den Dulk, 2017). Statistics Netherlands calculates that in 2013, 57% of women who were entitled to parental leave took some leave, on average for 10 hours per week during a year. Only 23% of men took parental leave, on average for 8 hours per week during 16 months. For both genders, take up of parental leave has increased since 2003, from 15% for men and 42% for women (den Dulk, 2017). Take-up of parental leave is higher for higher educated parents[37] and varies per sector. Public employees and health care workers more often have partially paid leave, and are also more likely to take up parental leave. For example, in the public sector (including teachers), most of the parental leave is paid. There, 55% of fathers and 74% of mothers take parental leave, compared to 13 and 32% in other sectors (CBS, 2011). Half of parent who take leave do so before their kid turns two (CBS, 2011).

3.2.3 Gender norms in the Netherlands

Gender norms may be an important driver of the child penalty. They are defined as a set of prescriptions that are attached to the social categories 'men' and 'women'. They tell people how they should be and how to behave (UN, 2020). These norms are for example about the role of women and men at home or in the public domain. By consequence, they may influence decisions about the division of labour in a household, as they can be used as a guidance to answer questions like: who earns money, and who takes care of the children?

In this study, I talk about gender norms on a scale between egalitarian and non-egalitarian. Egalitarian attitudes towards gender are the idea that all people are equal regardless of their gender, and that they hence should have the same rights and the same opportunities (Cambridge Dictionary, 2020). Conversely, non-egalitarian attitudes reflect the idea that women and men are not equal, and hence they should have different roles.

[37] Low-educated individuals are more likely to work in a sector where parental leave is unpaid, and at the same time they are less likely to be able to afford taking unpaid leave.

Measures of gender norms can be constructed using elicited attitudes towards statements about the role of women and men.[38] For example in the European Value Study EVS, there is information on prevailing gender attitudes (EVS, 2015). Appendix 3.A.1 gives an overview of the gender-related EVS questions for the Netherlands and compares the Netherlands to other countries. The EVS contains information about Dutch people's attitude towards working mothers, but there is no information on working fathers. SCP and CBS (2016) provide more relevant insights into expectations related to parenthood. Their results show that the Dutch used to think that the ideal working time for fathers is full-time. However, this has shifted towards an ideal of fours days per week in the past years. In contrast, mothers are expected to work two to three days a week, and there is very little support for full-time working mothers. These different expectations by gender show that gender norms are not fully egalitarian in the Netherlands, but that there is a shift towards more egalitarian gender norms in the past years.

3.3 Data

I use administrative data from Statistics Netherlands (CBS) containing information about parent-children linkages, basic demographic information, employment (1999-2016), earnings (1999-2016), self-employment (1999-2016), the death registry, address information, and partners, and transform it into yearly observations.

The analysis includes all first-born children born in the Netherlands between 2002 and 2008, whose legal[39] parents are between 20 and 45 years old at birth. Moreover, I exclude children with two legal parents of the same sex in order to focus on different-sex partnerships with children. I then follow the labour market path for these parents before and after the birth of their first child. The panel is balanced in event-time, so I only include parents that are observed from three years before birth until 8 years after.[40]

I analyse three labour market outcomes: unconditional earnings, the probability of employment, and conditional full-time equivalent (FTE). Employment is specified as having a

[38] Such measures have been used for example in Farré and Vella (2013); Tur-Prats (2019).

[39] Legal parents may be adoptive or biological parents, and I cannot distinguish between the two.

[40] I do not condition on the parents remaining a couple, nor on having two legal parents in the main analysis. In Appendix Figure A.3.5 and A.3.6, results of parents being together for the whole observation period and single legal parents with no partner are presented.

job based on an employment contract between a firm and a person.[41] Second, the earnings data consists of yearly gross earnings after social security contributions, but before taxes and health insurance contribution from official tax data. Third, the conditional full-time equivalent (FTE) is a measure of part-time work, and represents working hours as a ratio of reference full-time hours. For example, a person working 20 hours when the full-time reference is 40 hours, the % of FTE worked is equal to $\frac{20}{40} = 0.5$. The full-time reference hours are defined by sector or firm by Statistics Netherlands. The FTE is only available from 2001 onwards.[42]

In the data, maternity leave is not detectable for most of women, as earnings are paid at 100%. Only women with high daily earnings above the cap of 216.90 € may experience a decrease in earnings due to maternity leave, depending on whether their employer tops up the benefit to the usual salary (UWV, 2019). Paternity leave is negligible and does not manifest in the data. In contrast, parental leave may manifest in the data to some extent. Most employers do not pay parental leave, and some pay it partly. By consequence, the part of the parental leave that is *not* paid is visible in the data, since it will lead to a drop in earnings and FTE.

3.4 Empirical strategy

I estimate the child penalty as suggested by Kleven et al. (2019b). By gender, I estimate Equation (3.1), where y_{it} is the labour market outcome. I control for a set of event time indicators ranging from 3 years before first child birth[43] up to 8 years after. The baseline is chosen at $q = -1$, one year before child birth. I control for age and year fixed effects.

[41] Self-employment is not included because of inconsistencies in the data and a change in recording of income from self-employment. Results including self-employment can be found in the Appendix (A.3.7)

[42] This implies that the pre-trends are missing for year -3 and -2 for the 2002 cohort of parents, and for the year -3 for the 2003 parent cohort. Moreover, 35% of the data is imputed between 2001 and 2005. Comparing the years 2006 and 2007, where there are no imputations or missing data, to 2002-2007, the child penalty results do not change. Furthermore, measurement error in the dependent variable does not bias estimates (Wooldridge, 2002). Therefore, it seems unproblematic to use the imputed FTEs for my analysis.

[43] This implies that the child penalty measure represents the total impact of all children independent of the total number of children. Kleven et al. (2019b); Sieppi and Pehkonen (2019) show that the penalty tends to be larger the more children a woman has, but that there is no difference by the number of children for men. Result by the number of children by year 8 after the first child are available on request.

$$y_{it} = \sum_{q=-3}^{8} \alpha_q \mathbb{1}[eventtime_{it} = q] + \sum_{k=17}^{53} \beta_k \mathbb{1}[age_{it} = k] + \sum_{t=1999}^{2016} \gamma_t \mathbb{1}[time_{it} = t] + \nu_{it} \tag{3.1}$$

In order to estimate the child penalty P_q, I compute, by gender, the relative change in the outcome due to the child compared to the pre-birth trend. This is expressed as the event time coefficient relative to the counterfactual outcome $\tilde{y}_{it}^q = \sum \hat{\beta}_k \mathbb{1}[age_{it} = k] + \sum \hat{\gamma}_t \mathbb{1}[time_{it} = t]$, which is the predicted labour market outcome at event time q in absence of a child. The counterfactual is thus calculated based on parents to be, and not with a formal control group.[44] The child penalty represents the percentage change in the outcome due to the child.

$$P_q = \frac{\hat{\alpha}_q}{\tilde{y}_{it}^q} \tag{3.2}$$

The effect of children on earnings could also be obtained by estimating Equation 3.1 in log-level form. However, I am interested in the total effect of children on unconditional earnings, and in order to also consider parents without any earnings, I am using the level-level model.[45]

The child penalty P_q specifies the percentage gain/loss in the labour market outcome due to the child compared to people of the same gender and cohort without children yet. To compare mothers with fathers, the relative child penalty P_q^r (the percentage change of women w compared to men m) is calculated as follows:

[44] I follow seven cohorts of parents over eight years. That means that the calendar year estimate of the year 2009 onwards, or the estimates of the age dummies of older parents, (and more generally part of the counterfactual estimates) are based solely on people that are already parents. This may influence the quality of the estimated counterfactual. When comparing child penalty estimates when I follow the same cohorts over a shorter period of time, and hence the problem is smaller, I find very comparable estimates of the child penalty (see Figure A.3.8 in the Appendix). This is evidence that the bias introduced by counterfactual estimates based on parents only is likely to be small.

[45] Not using the log-level specification means that outliers could distort the results. Kleven et al. (2019b) have showed using quantile regression that in their setting, using a level-level model is unproblematic. Another advantage of using logs is the relative interpretation of the results. Since I am converting the estimates to a relative percentage in a second step, it is unproblematic to use levels.

$$P_q^r = \frac{\hat{\alpha}_q^m - \hat{\alpha}_q^w}{\tilde{y}_{it}^{q,w}} \tag{3.3}$$

Having a child is potentially endogenous to income and its determinants. However, a causal effect can be identified if two assumptions are satisfied. First, there needs to be a sharp discontinuity at the arrival of the child. Second, there is no sharp discontinuities in other determinants of income at child birth that are unrelated to the child. Kleven et al. (2019b) provide evidence that the simple event study framework controlling for age and time fixed effects gives similar results as methods where the arrival of a child is instrumented (using the sex-composition of the first two children as an instrument for the third, and a difference-in-differences model comparing people with and without children).

The formal identifying assumptions for an event-study of this form are i) parallel pre-trends between cohorts of first-time parents, ii) zero pre-trends for all first-time parents cohorts, and iii) homogeneous treatment effects (Abraham and Sun, 2020). Appendix A.3.9 and A.3.10 shows the estimates for α_k by parent cohorts. Statistically speaking, neither of the identifying assumptions are fulfilled. While the other papers using the same method have not conducted this check of identifying assumptions, it is likely that they do not satisfy the no-pretrends by cohort of parents assumption either, since there are pre-trends on the aggregate for Kleven et al. (2019b) and Andresen and Nix (2019). Still, since the effect of having a child is very large for women, it is unlikely that the comparatively small pre-trends drive the post-effects. A similar argument is advanced by Bilinski and Hatfield (2019) who also state that with high power, one is likely to detect pre-trends when they are not meaningful. This argumentation can also be used for the homogeneity of effects. It seems that for participation and % of FTE worked reduction, the decrease after having the child for later cohorts is slightly less pronounced (and there is a less clear pattern for unconditional earnings), but compared to the effect sizes the differences between cohorts are small.

Figure 3.3, and also the estimates by parent cohort show a decline in growth of labour market outcomes before the child arrives. If this slow down of growth is related to having children, the true child penalty is underestimated. For example, if women sort into more flexible jobs in anticipation of parenthood, the child penalty would even be larger had they

not sorted into these jobs. In contrast, if this trend is unrelated to having children, the true child penalty would be overestimated.

In the subsequent analyses, I am reporting the child penalties for the whole Netherlands, and by subgroups. By construction of the child penalty measure, the mean of child penalties over subgroups is not necessarily equal to the child penalty of the whole Netherlands, because the child penalty is a relative measure. It should be kept in mind when comparing the mean child penalty of subgroups with the overall child penalty for the Netherlands.

3.5 The child penalty in the Netherlands

3.5.1 Descriptives

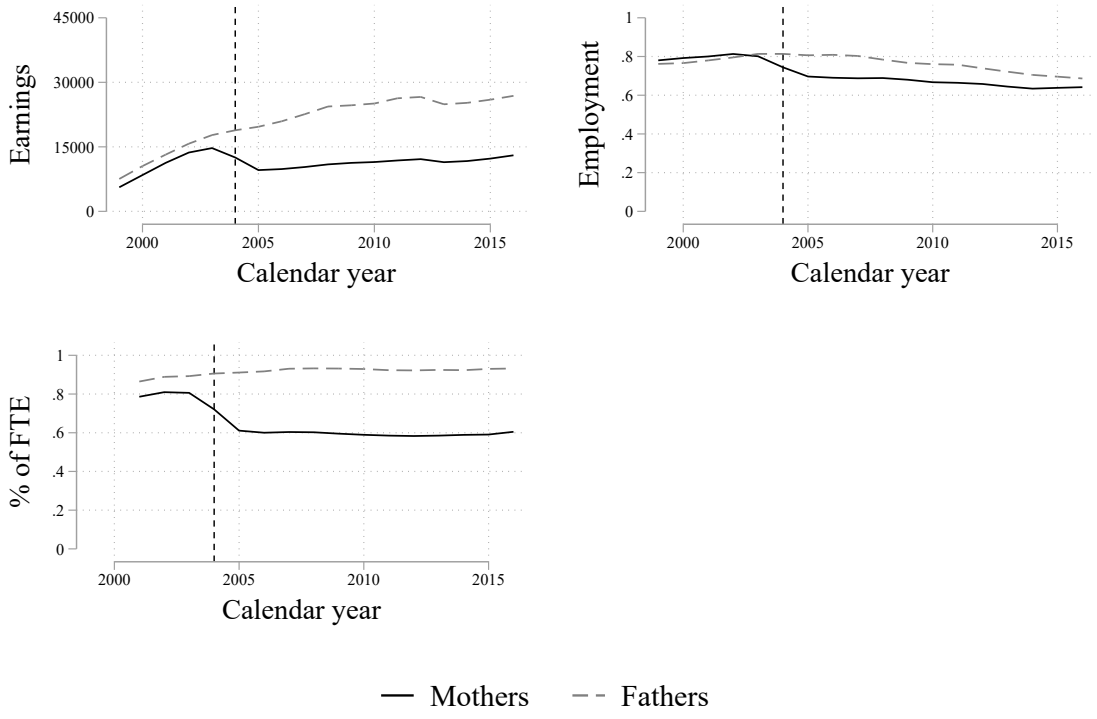
Figure 3.2 shows the average earnings, participation, and % of FTE worked of the 1979 birth cohort who have their first child in 2004 by gender. This cohort is 20 years old and at the beginning of their labour market career in 1999, when the data is available. They have their first child in 2004, at the age of 25. Women start with lower earnings on average - around 2000 € lower per year (or 175 € per month). They are 2 percentage points less likely to be employed, and their % of FTE worked is around 8 percentage points lower than for men. Differences in labour market outcomes before children may be explained by education choices, lower likelihood of having a permanent contract, or discrimination (Merens and Bucx, 2018). Before the first child, labour market outcomes of both genders evolve similarly. After the arrival of the first child, men's labour market trajectory continues on the pre-child trend. Women, in contrast, experience a sharp drop in the year of birth. From then onwards, labour market outcomes diverge by gender, resulting in a large gap in male/female earnings. Twelve years after, the average difference in yearly earnings is 14,000 €, or 1200 € per month. When selecting people of the 1979 cohort who have a child at 32, the pattern is similar (see Figure A.3.2).

The analysis of the 1979 cohort shows two important things. First, there is already a wage gap before birth, but giving birth increases the gap considerably. Second, there does not seem to be a recovery for women to pre-child trends.

Figure 3.3 generalises this pattern for all different-sex first-time fathers and mothers in 2002-2008. It shows the descriptives of the data not balanced in event-time, in order to show

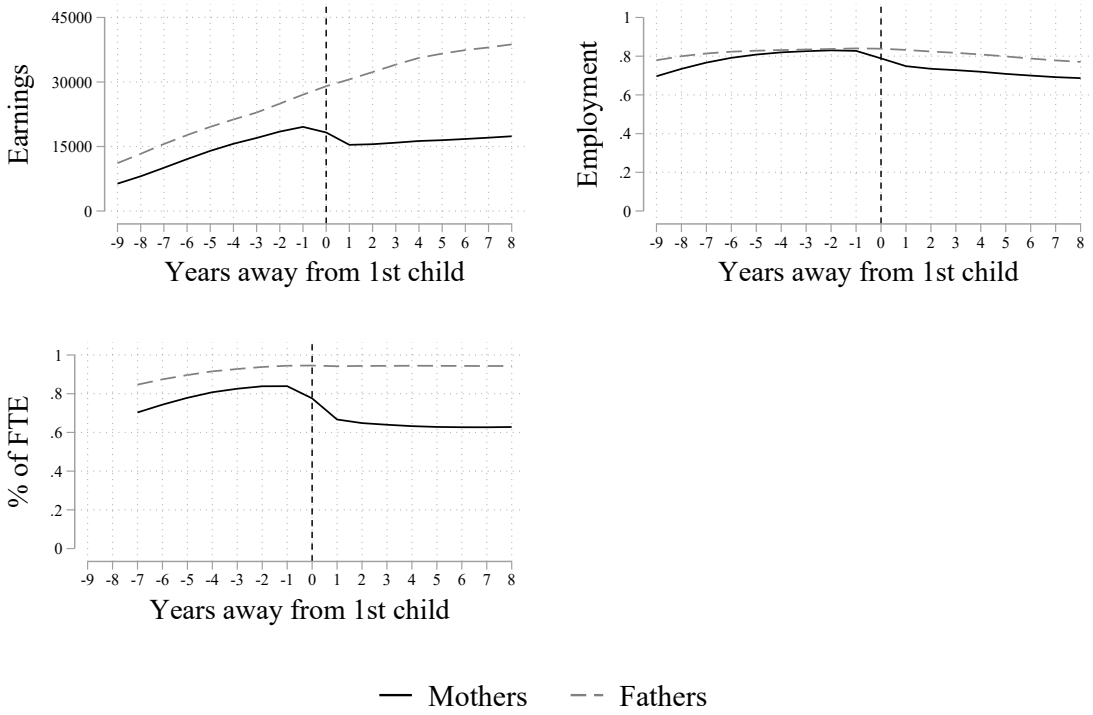
longer pre-trends. The average pre-birth earnings gap is around 6000 € per year or 500 € per month three years before birth. 8 years after birth, the average difference is about 22,000 €, or around 1800 € per month. Fathers' and mothers' participation is around 83% three years before the arrival of the first child. One year after birth, 8 (1) percentage points fewer women (men) are working compared to 1 year before birth. The bottom left panel shows that fathers work around 94% of a full-time job on average, before and after childbirth. Women, on the other hand, start off by working around 83%, and reduce their working hours to around 63% of a full-time job on average after birth.

Figure 3.2: Average labour market outcomes of the 1979 cohort who have their first child in 2004, by gender



Note: Yearly average labour market outcomes of the cohort born in 1979 with a child in 2004 (at age 25) by gender. $N_{women} = 4,728$, $N_{men} = 2,391$. FTE stands for full-time equivalent.

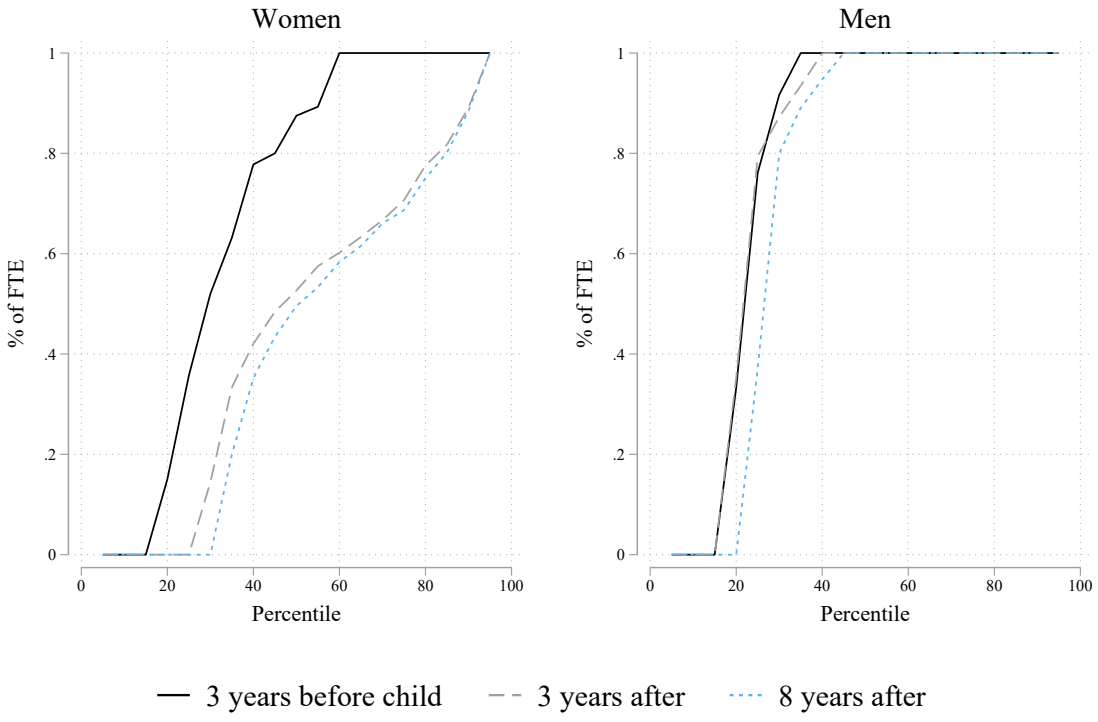
Figure 3.3: Labour market outcomes before and after having a child



Note: Yearly average unconditional earnings, participation, and full-time equivalent (FTE) by gender for first-time parents 2002-2008. The panel is balanced in calendar years and not event-time.

Since average numbers of the % of FTE hide interesting heterogeneity, Figure 3.4 shows cumulative distribution functions for both genders before and after childbirth. For this purpose, the variable includes the individuals not working with the % of FTE worked equal to zero. Already before birth, fewer women work full-time, and the fraction of part-time working women is larger than the fraction of men (black solid lines). After the birth of the first child, some women stop working (lines shift to the right), and part-time work becomes more common (slope is less steep), especially in the 0.5 to 0.8 FTE jobs. Full-time work is very rare for mothers. Men are less likely to work part-time before and after child birth. However, there is a small increase in men who work 0.8 to 0.9 FTE once their child is born. This may be the so-called 'papadag' (daddy day), during which some Dutch fathers take care of their children. The figure also shows a decrease in participation for fathers by the time the oldest child is 8.

Figure 3.4: Percentage of FTE worked cumulative distribution before and after having the first child

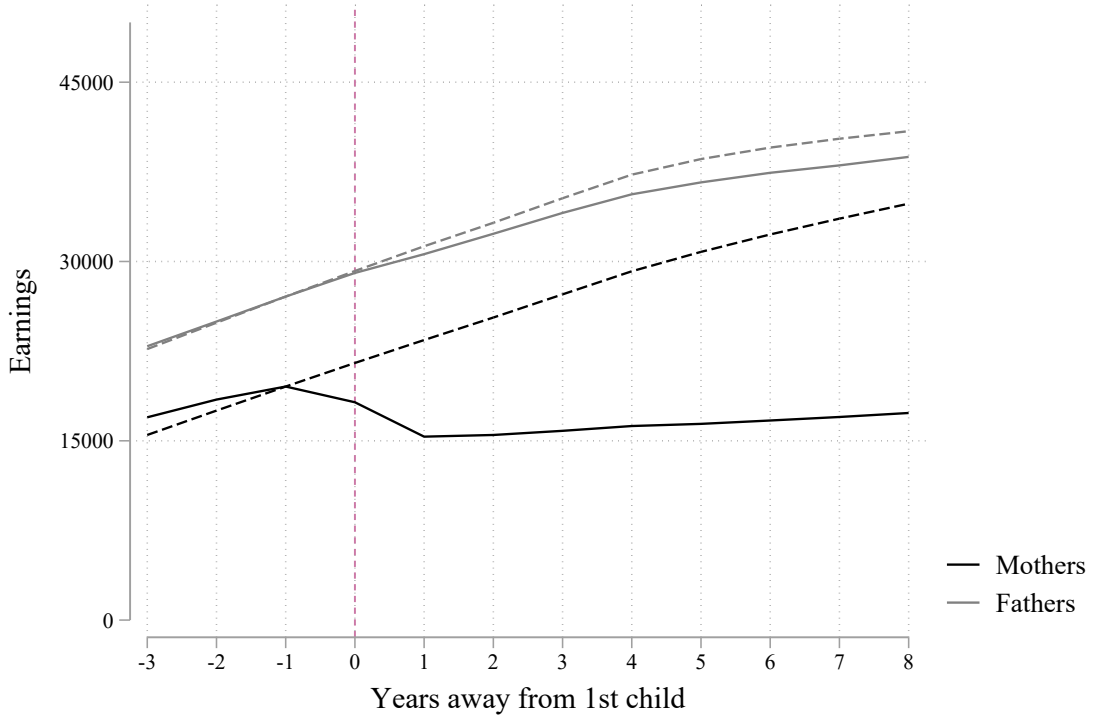


Note: Percentage of full-time equivalent (FTE) cumulative distribution 3 year before, 3 years after, and 8 years after having the first child by gender. FTE exceptionally includes the non-working as zeros in this figure.

3.5.2 Child penalties

Before presenting the child penalty results from Equation 3.2, I graphically show how the estimation strategy works. In Figure 3.5, I plot the predicted values of Equation 3.1 with the solid lines. This reproduces the pattern of average earnings shown in the top left panel in Figure 3.3, but includes controlling for age cohort and time effects. The dashed lines show the counterfactual $\tilde{y}_{it}^{q}$, the trend in absence of children - i.e. the prediction of Equation 3.1 not taking into account the estimates for the time away from birth indicators. Figure 3.6 then plots the child penalty P_q as the percentage of the difference between the counterfactual and the predicted earnings (or other labour market outcomes).

Figure 3.5: Predicted earnings vs counterfactual earnings



The solid lines show the predicted unconditional earnings from Equation 3.1. The dashed lines plot the counterfactuals showing the situation had there not been a child, calculated as the predicted values from Equation 3.1 omitting the contribution of the event-time dummies.

Fathers' earnings grow slightly less with a child than compared to when they had not had one (Figure 3.6). The reduction in earnings eight years after the first child is around 3 percent, which is, even though statistically significant, negligible compared to mothers. Mothers, on the other hand, earn around 50 percent less due to the child eight years after the birth of the first child. Compared to men, they earn 44% less than fathers when their first child is 8 years old.[46]

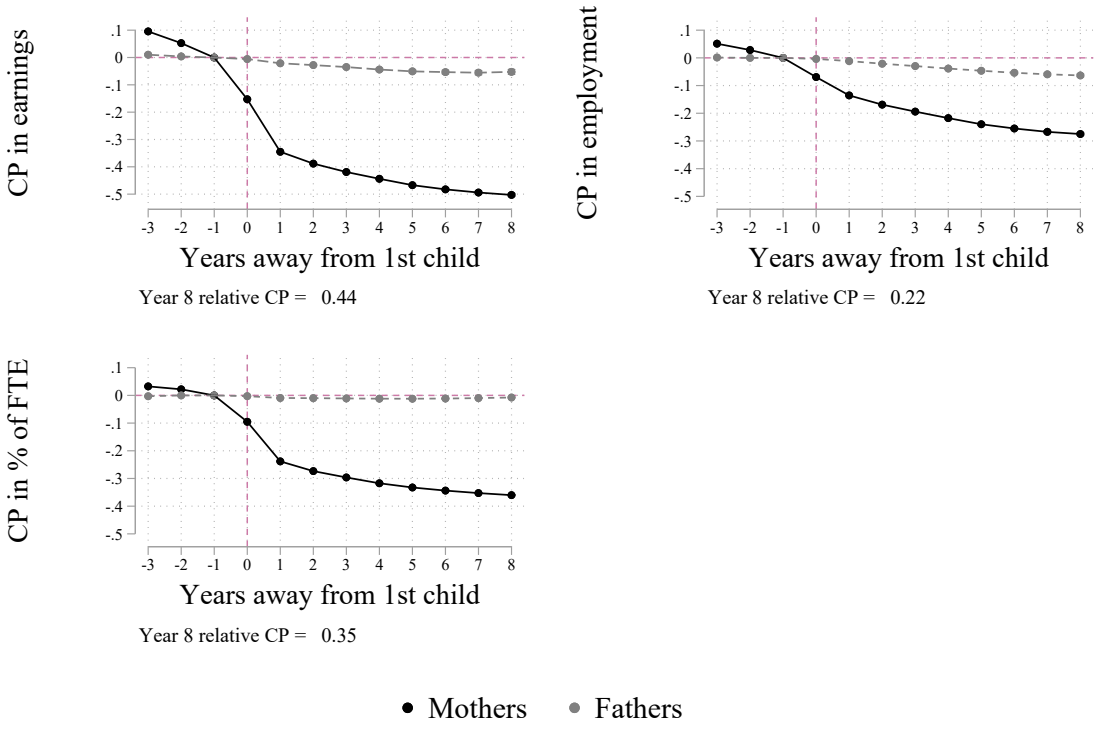
The reduction in unconditional earnings of mothers is partly due to relatively higher exit rates from the labour market (top right panel in Figure 3.6). Eight years after the first child

[46]These results are for all different-sex parents. When only including parents in the analysis that live together for the whole 12 years of the panel, the results are very similar (see Figure A.3.6).

is born, mothers are 22% less likely to work than fathers, and around 27% less likely than mothers to be. Fathers are also less likely to work than fathers to be, by around 6%.[47]

There is almost no change in the % of FTE worked of fathers compared to fathers to be(see bottom left panel): around 0.8% less. Mothers, on the other hand, are a lot more likely to decrease their FTE. Eight years after their first child, they have a decrease of % of FTE worked by around 35% compared to mothers to be and fathers.

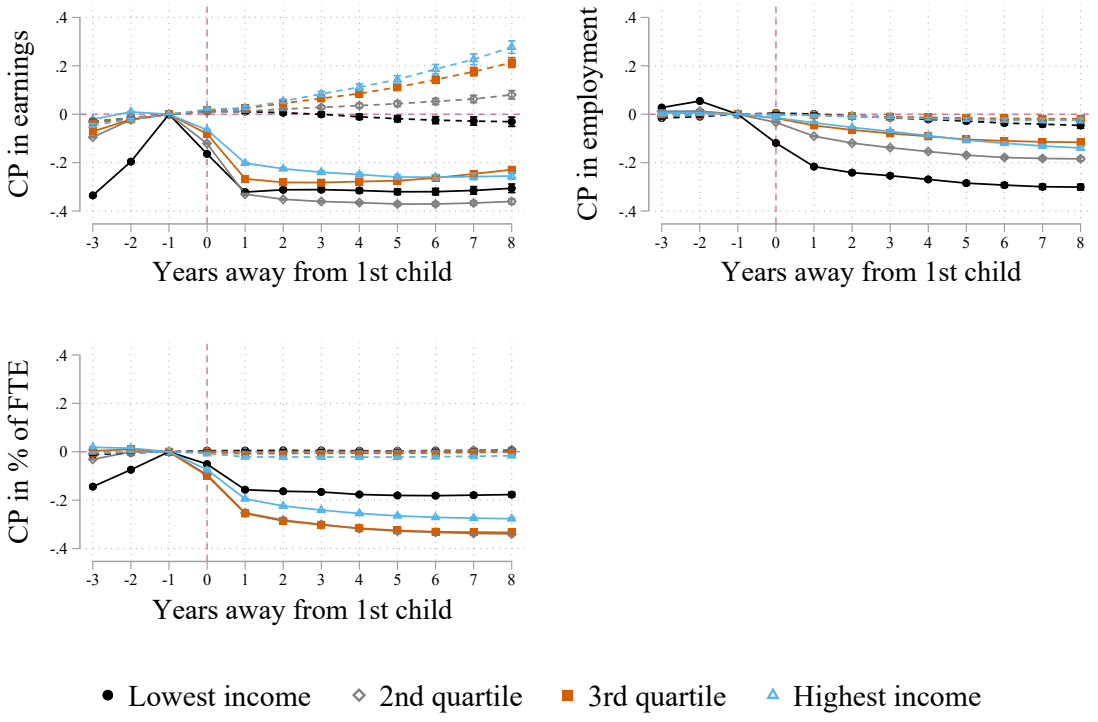
Figure 3.6: Child penalties (CP) for unconditional earnings, particpation, and FTE



Child penalties (CP) based on Equation 3.2 for unconditional earnings, participation, and full-time equivalent (FTE). The year 8 relative CP (from Equation 3.3) compares women to men eight years after birth of the first child.

[47]The identification strategy for the child penalty requires a sudden change in labour market outcomes once the child is born. This slow decline in participation of men does not fulfil this criteria, and hence one cannot attribute this decline to the arrival of the child with the same level of confidence as for women. To know more about these men, I compare men who were employed at child birth, but not eight years after birth with men who stay employed. Men who stop working are likely to be in shorter relationships, they are less likely to still be together with the mother of their child, and only half of their partners work eight years after child birth. Furthermore, at birth, they have a median income of about 23,000€ (compared to 31,000€ for the rest of fathers). The decline does not seem to be related to additional children, since it is also present for men who only have one child. This suggests that men who become fathers in less favourable conditions may drive the decline in father's employment (Figure 3.6). It is less clear however whether this decline is caused by child birth.

Figure 3.7: Child penalties (CP) for women and men by the women's pre-birth income quartile



Note: Child penalties (CP) based on Equation 3.2 for unconditional earnings, participation, and full-time equivalent (FTE) by pre-birth income quartile of women. Solid lines refer to women, their partners are represented with dashed lines.

3.5.3 Distributional differences in the child penalty by socio-economic status of women

Adema et al. (2019) show that in the Netherlands, the child penalty is higher for low educated women, who adjust both the intensive and extensive margin, whereas high educated women only adjust the intensive margin. Angelov et al. (2016) examines Swedish couples where she is better educated than him, and finds that the intra-couple wage difference decreases in this subgroup. Both findings suggest that there may be heterogeneity in the child penalty by socio-economic status. I examine this heterogeneity in the child penalty by pre-birth income quartiles of women. I condition on working women two and three years before the child is born, and base the quartiles on the mean income of the women 2 and 3 years before child birth.

Figure 3.7 presents the child penalty results for women (solid lines) and their male partners[48] (dashed lines) by pre-birth income quartile of the woman. The top left panel shows

[48]Partners are the other legal parents, independently of whether the parents are still a couple.

that there are considerable differences by income quartile in the child penalty in unconditional earnings. For women, the child penalty is smallest for high income quartiles. This might be explained by a higher labour market attachment of high earning mothers. Middle income women incur the largest child penalties, low income mothers[49] lie in between. There is also variation in the child penalty of partners. Whereas for partners of low income women, having children does not change unconditional earnings, there seems to be a 'child premium' for partners of high income mothers of up to 28%. Since both participation and hours decrease slightly for this group of men, the wage rate of these fathers must grow more than the pre-birth trend. This is unexpected, as for example Mari (2019) does not find a 'fatherhood premium' for Germany or the UK. However, this result should be interpreted with caution, as a continuous increase in earnings does not satisfy the identifying assumption of a sudden change after birth.

The results for participation are less surprising: In line with the results from Adema et al. (2019), I find that the child penalty in participation is largest for low income women, and smallest for high income women. For high income women, the steady decline in participation calls into question whether the identifying assumptions are satisfied for participation for this group. Partners of high and low income women are slightly more likely to stop working after having a child.

The % of FTE worked child penalty is lowest for low income women. These women work on average 65% of full-time hours, so the small penalty is not due to no a lack of room for adjustment downwards. The largest cut-backs in % of FTE worked happen in the middle income classes of women, high income women are in the middle. Partners are also interesting: it seems that mainly partners of high income women reduce their % of FTE worked somewhat (and possibly take a papa day). However, it stands in no relation to the cut-backs of their partners.

The difference in child penalties by income quartiles may be driven by fertility differences between the groups. However, up to one year after birth, fertility differences are minor between income groups (Figure A.3.11). Between year 2 and 6 after the first birth, high

[49]The increasing trend in income and % of FTE before child birth for low income women may have two explanations. On the one hand, about half of these women are below 25 three years before child birth. They may thus be working in student jobs or just started working and therefore experience high growth of earnings and hours. On the other hand, this may also be regression to the mean, that may be particularly important for this group.

income women are more likely to have a consecutive children earlier, but by year seven low income women catch up, and total fertility is on average similar again between groups. Hence, it is unlikely that differences in fertility drive differences in child penalty between groups just after and from seven years after the first birth.

Another confounder of the results by pre-income quintile may be age at birth. Women falling into the lowest income category are on average younger than the other women, and women in the highest income category are on average older. This may have two reasons. On the one hand, younger people earn less, so women having a child at a relatively young age earn less by construction. On the other hand, high earning potential women may sort into having children later. It is unlikely, however, that these factors drive the results entirely. When fixing the age at birth of the mother to 30,[50] the pattern of the results remains similar.

These results suggest considerable heterogeneity in child penalties across pre-birth income groups of women. Low-income women are in the middle grounds for earnings child penalties (30%). They are more likely to quit their job than any other woman, but if they do not, they reduce their hours by only 18%. Third quartile and high income women have relatively low child penalties (22 and 25% respectively), since they are likely to keep working, and they do not cut their hours as much as middle income women. When comparing them with their partners, however, high income women have a very large relative child penalty (51%), since their partner incur a considerable child premium (28%). Second quartile income women have the highest child penalties (36%).

3.6 The role of gender norms

So far, my results confirm what has been shown in other studies: the child penalty for women is substantial and almost nonexistent for men. If policy makers want to prevent women's disproportionate labour market drop-outs and reduction in hours, it is important to know more about the drivers of the child penalty.

The child penalty may arise for various reasons. First, biological explanations for the child penalty are plausible to a certain extent because there are tasks related to child rearing that only women can do, such as giving birth and breast-feeding. This means that especially

[50] I choose age 30 because at this age, I still have enough low income mothers while not losing too many high income mothers.

in the first year after birth, a child penalty for women is difficult to avoid. Kleven and Landais (2020) and Rosenbaum (2019) assess the role of biology in the child penalty by comparing adoptive and biological mothers. Rosenbaum (2019) finds that there is also a child penalty for adopitve mothers, and Kleven and Landais (2020) shows that adoptive mothers have a similarly high child penalty than biological mothers in Denmark. This suggests that the act of giving birth and breastfeeding is limited for the child penalty, and that there have to be different forces at play.

Giving birth also changes the structure of the brain and hormone levels. These changes facilitate the adaption to parenting tasks and bonding with the baby, and thus lead to a change in preferences (Kim, 2016; Feldman and Bakermans-Kranenburg, 2017). However, neuroscience studies show that also adoptive mothers and caregiving fathers can experience a change in brain structure and hormonal levels when becoming parents (Abraham et al., 2014; Bick et al., 2013; Feldman and Bakermans-Kranenburg, 2017). Hence, becoming a parent will lead to a change in preferences through brain and hormonal changes, but these changes in preferences are not limited to biological mothers.

Given that non-biological mothers also experience a child penalty, and that fathers can also experience changes in hormonal levels and brain structure, it is unlikely that the child penalty comes from biological factors only. Societal factors are likely to play a role as well. Gender norms shape our understanding of ourselves and the world around us from the moment we are born, and they may contribute to the formation of our preferences and expectations. In this section, I provide evidence that in social contexts where gender norms are more egalitarian, the child penalty is smaller with four different analyses. Then, I rule out that the child penalty is due to income maximisation only.

3.6.1 The child penalty in the Dutch 'Bible Belt'

Gender norms may vary regionally. In the Netherlands, non-egalitarian gender norms are especially strong in the 'Bible Belt' (Gielen and Zwiers, 2018). The Bible Belt is defined as municipalities with a high share of voters for the conservative christian party *Staatkundig Gereformeerde Partij (SGP)* in parliamentary elections. For example, the SGP advocates against abortion, and insists on the "God given difference between men and women" (SGP, 2020).

Figure 3.8: Child penalties for men and women in the Bible Belt

Note: Child penalties (CP) based on Equation 3.2 for unconditional earnings, participation, and full-time equivalent (FTE) for Bible Belt and non-Bible Belt municipalities. Bible Belt municipalities are defined as having a voter share for the SGP of at least 5 percent in the 2017 elections. The non-Bible Belt municipalities are defined as having an SGP voter share of less than 0.5 percent. Solid lines refer to women, their partners are represented with dashed lines.

I compare child penalties in the Bible Belt, in municipalities where at least 5%[51] of voters voted for the conservative christian party in the parliamentary elections in 2017 (Kiesraad, 2019).[52] I disregard municipalities with a voter share between 0.5 and 5%, and compare the Bible Belt families to families living in a place where less than 0.5% of votes went to the SGP, and can thus be safely classified as non-Bible Belt. According to this definition, 10% of the sample is classified as living in the Bible Belt, and 48% is non-Bible Belt, and 42% is not considered.[53]

Figure 3.8 shows that for all three labour market outcomes, the child penalty is larger in the Bible Belt. The divergence between groups starts in year one after child-birth, and

[51] 5% is the 90th percentile of SGP voter shares in 2017. In the median Dutch municipality in 2017, the voter share for the SGP was 0.5%. In the municipality with the highest voter share for the SGP, 56% voted for this party.

[52] The share of voters for the conservative christians remains fairly stable over time. Therefore, the 2017 results are also good indicators for early calendar years.

[53] Including the excluded in the non-Bible Belt group does not change the results.

persists over time. For example in unconditional earnings, the child penalty for women is 9 percentage points larger than in non-Bible Belt regions. Families in the Bible Belt have more children on average than non-Bible Belt families (Figure A.3.12). However, the difference in total fertility only manifests from three years after the first birth onwards, whereas the difference in child penalties arises already in the first three years. Hence, the difference in child penalties cannot only stem from fertility differences between groups. These results show that in regions with less egalitarian gender norms, the child penalty is larger. Yet, it is also striking how large the child penalty is outside the Bible Belt.

3.6.2 Closeness to grandparents

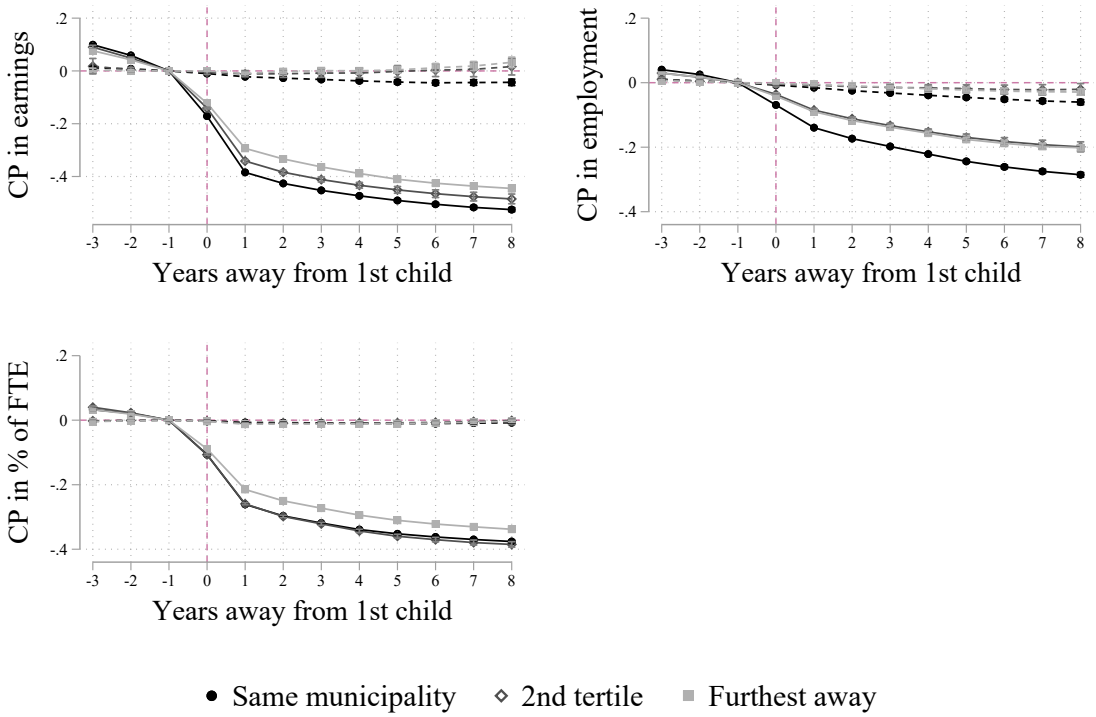
Second, I examine individuals by the geographical distance of their place of living to their own parents. Individuals with more egalitarian attitudes towards gender roles may be more likely to move further away from their parents. When starting a family, they may conform less to a traditional division of labour in the household, and hence the child penalty of these women may be lower. On the other hand, far away living grandparents decrease informal child care availability, which may increase the child penalty.

For this purpose, I link grandparents and the distance between the municipality of the parents and the closest living grandparent.[54] I estimate the child penalty by tertile of distance to the closest grand parent three years before birth of the first child. I choose the distance three years before birth to avoid measuring grandparents moving closer to the parents because of the arrival of the grand-child, or the other way around. The first tertile comprises all individuals who live in the same municipality as their parents. In the second tertile, people live on average 6 km away from their parents, and in the third tertile they live 55 km away.

Figure 3.9 shows that the child penalty is about 8 percentage points larger when the grand parents live close than if they live far away. Most of this is due to more women exiting employment if grand parents live close, where the child penalty is one third larger than for women that live further away. Moreover, women with far away living grand parents cut their hours less than the others. Differences in total fertility by pre-birth distance to grandparents

[54] 15% of sample parents cannot be matched to any grand parent. One reason for the high rate of non-matches may be that the parents-children linkages are only fully trustworthy for parents born from 1966 onwards. However, only 7% of the parents in my sample were born before. Other reasons for non-matches may be immigration and mortality: neither grandparents of immigrants nor grandparents who died before 1995 are in the Dutch registries.

Figure 3.9: Child penalties for men and women by distance to the closest grand parent



Note: Child penalties (CP) based on Equation 3.2 for unconditional earnings, participation, and full-time equivalent (FTE) by tertile of pre-birth distance of residence to the closest grandparent. Solid lines refer to women, their partners are represented with dashed lines.

are small (Figure A.3.13), therefore it is unlikely that the differences between groups are driven by differences in total fertility. For men, there is less variation in the child penalty by distance to grand parents. Women living closer to their parents and their partner's parents are thus more likely to allocate tasks in the household according to a non-egalitarian division of labour - despite the higher availability of informal child care thanks to close-by grandparents.

3.6.3 Part-time culture for fathers at the workplace

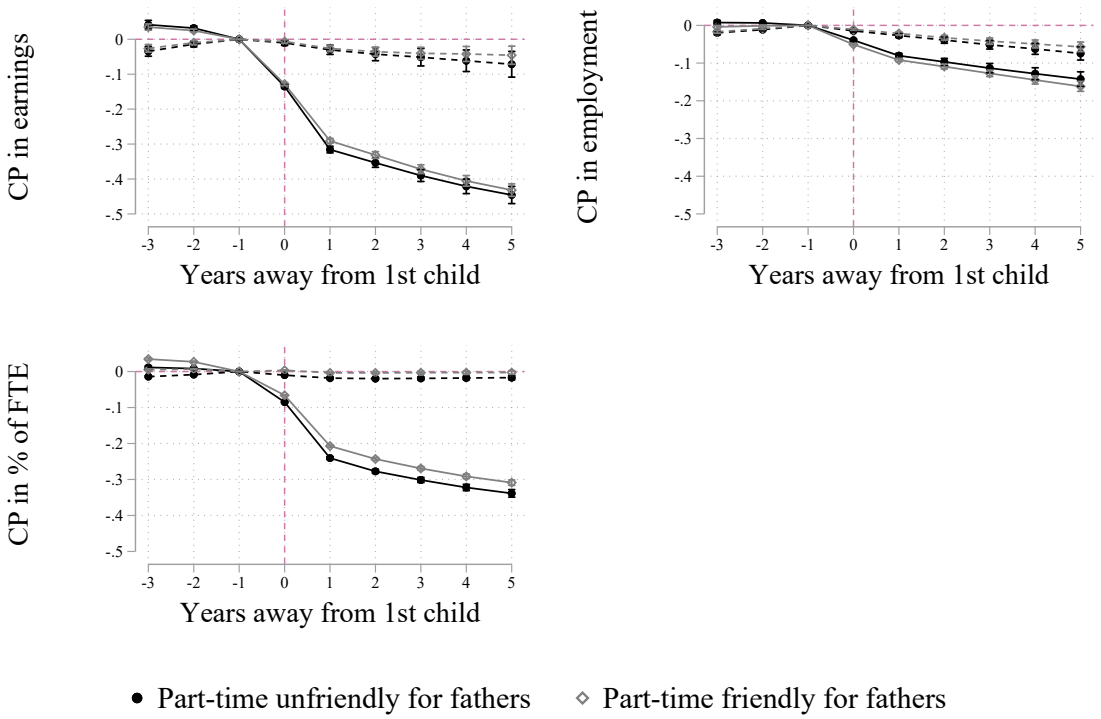
Gender norms may not only be important when deciding about the division of labour in the household. Firm culture may also play a role in the perceived possibilities to work part-time for men. For example, Dahl et al. (2014) show that peer effects are important in the take-up of paternity leave. Male colleagues who start working part-time when they have a child may encourage other fathers-to-be to do the same. Furthermore, for men with male colleagues who reduced their hours after having a child, it may be more accepted to reduce working hours as well. These men may then be more involved in child rearing tasks, and their partners may thus have a smaller child penalty.

I limit my analysis to couples where the father works in a firm where in the year before birth there was at least one other father with a child of age zero or one. For every firm, I calculate the fraction of new first-time fathers who reduced their hours in year zero or one of the child, and I match this to new first-time fathers a year later.[55] About 7% of fathers work in firms where another man became a father in the past two years. Given that the FTE information without relying on imputations is only available from 2005 onwards,[56], I use births in 2008-2010, and follow these for five years to keep the panel balanced in event-time. I distinguish between firms where no father reduced his hours, and firms where at least one father reduced hours after birth of his child. This implies that I select fathers working in firms where there are other men in child bearing age, so men working in small firms or in female dominated professions are less likely to be selected. Among new fathers, two thirds reduced their hours at least temporarily in the first two years after child birth.

[55] I use fathers of children with age zero and one, because it may be that the father only reduces his hours once maternity leave is over. Depending on the date of birth of the child, this may happen in event-time one only.

[56] Using the imputed data as an outcome is not problematic, as the overall results do not change when excluding the imputation years. However, in order to determine whether a father has decreased his hours, I need precise indications of the FTE, especially also because it is likely that most fathers decrease their % of FTE worked only modestly.

Figure 3.10: Child penalty for women by father's workplace part-time culture



Note: Child penalties (CP) based on Equation 3.2 for unconditional earnings, participation, and full-time equivalent (FTE) by part-time culture of father's firm. Solid lines refer to women, their partners are represented with dashed lines.

In contrast to expectations, the part-time friendliness for fathers in the firm does not lead to a higher reduction in hours for men (see Figure 3.10, bottom left panel). Consequently, there is no difference in child penalties between women who have a partner working in a part-time friendly firm compared to a partner working in a part-time unfriendly firm. Men in both types of firms experience a steady decline in employment. This may suggest that many of the firms I select as being part-time friendly for fathers are firms with many temporary employees, and that fathers reducing their hours is a coincidence and not a consequence of child birth. Alternatively, new father colleagues working part-time may discourage other fathers to do the same, because they do not advance in their careers at the same speed anymore, or they have lost reputation in the firm. These results suggest that workplace part-time culture for new fathers does not attenuate the child penalty of mothers, and that male colleagues working part-time after having a child do not encourage other new fathers to do the same.

3.6.4 Same-sex couples

The behaviour of same-sex parents[57] gives insights on how people may divide household and care tasks in couples where gender roles are not informative for the negotiation about the division of tasks. Research on the division of labour in same-sex households shows indeed that homosexual couples share housework and paid work more equally than heterosexual couples, and that there is less specialisation after child birth (Cudeville et al., 2020; Jaspers and Verbakel, 2013).

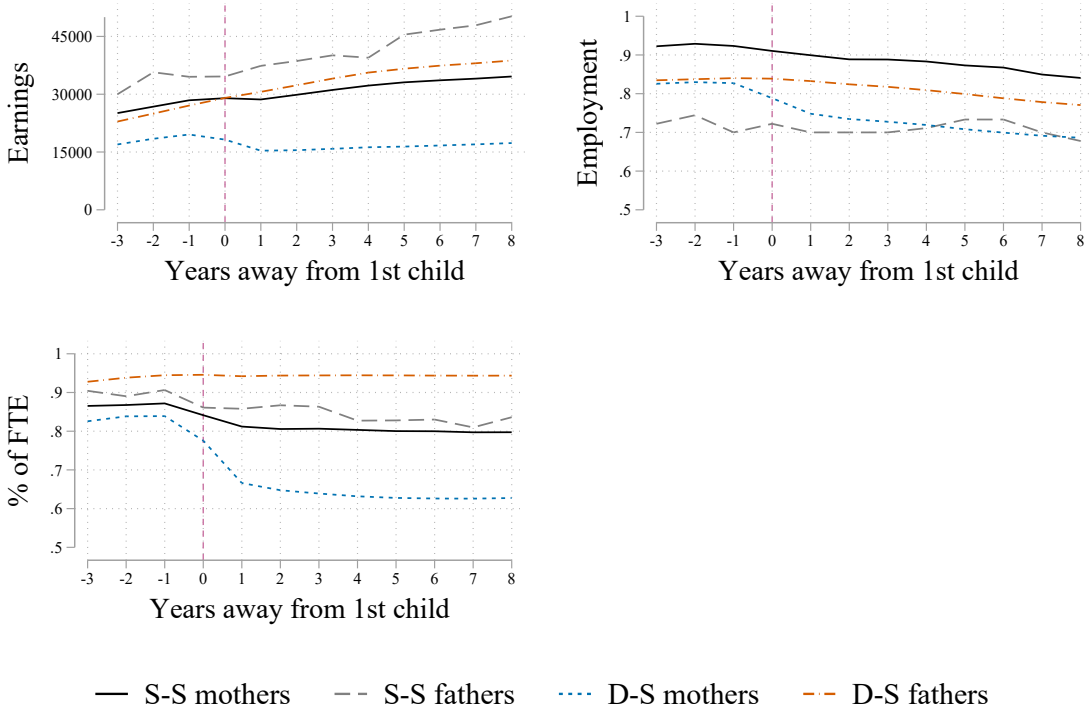
I compare the child penalties of same-sex and different-sex parents to determine the influence of gender norms on the child penalty (Andresen and Nix, 2019; Rosenbaum, 2019; Moberg, 2016). In 2002-2008, 1746 women and 68 men have had a first child registered with another legal parent of the same sex.

Adjusted for cohort and time effects, different-sex fathers are the only group where the arrival of the child does not seem to affect unconditional earnings (Figure 3.11). The trends of same-sex fathers are less stable, since the number of observation is very small.[58] In the two years before child birth, their labour market outcomes seem to decline - this may be related

[57] In the data, same-sex parents consist of parents who have a child with the help of a sperm donor, a surrogate mother, or adoption. There are also other family forms, for example co-parenting by a lesbian and a gay couple. Since legally, parenthood of more than two individuals is not foreseen in the Netherlands, these cases will be classified as heterosexual couples, as the biological mother and the biological father most probably are registered as the legal parents.

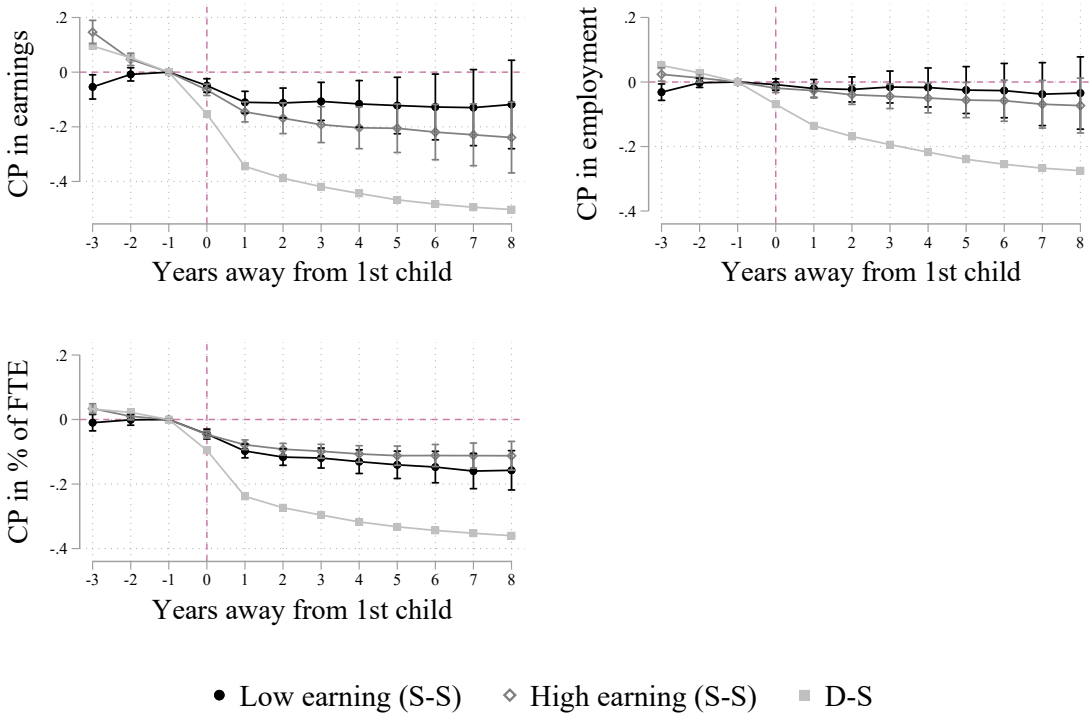
[58] For this reason, I present their results in the Appendix only (Figure A.3.15).

Figure 3.11: Labour market outcomes adjusted for cohort and time effects of same-sex (S-S) and different-sex (D-S) parents



Note: Predictions from Equation 3.2 for unconditional earnings, participation, and full-time equivalent (FTE) for same-sex (S-S) and different-sex (D-S) parents.

Figure 3.12: Child penalties for same-sex and different-sex women



Note: Child penalties (CP) based on Equation 3.2 for unconditional earnings, participation, and full-time equivalent (FTE) for same-sex (S-S) and different-sex (D-S) women. Same-sex women are divided into lower and higher earning partner pre-birth.

to the substantial time and/or monetary cost to find a surrogate mother or to be granted an adoption. The largest drop after the birth of the first child is incurred by different-sex mothers. Same-sex mother's decline in labour market outcomes is less pronounced.

However, the trends for same-sex mothers are an average of the birth-giving and the co-mother, and it may be that their average hides a similar pattern as for different-sex parents.[59] In order to exclude that, I divide the female same-sex mothers into higher and lower earning partner three years before the arrival of the child. If the couple maximises income and wants to follow a similar division of labour as different-sex parents, this classification may uncover patterns similar to different-sex parents if there are any.

Figure 3.12 shows that there is no clear pattern according to high/low pre-birth earning partner for same-sex mothers. In contrast, it emerges clearly from the figure that the child penalties of these two groups of same-sex mothers are very different from different-sex

[59]It may be that the co-mother does not adjust labour market involvement, and the birth-giving mother has a similar decline as different-sex mother. Since both co-mother and biological mother are pooled, their average child penalty may be lower while the child penalty of the birth-giving mother may be similar to the child penalty of different-sex mothers.

mothers. On average, same-sex mothers have 29 percentage point lower child penalty than different-sex mothers for unconditional earnings, a 21 percentage point lower child penalty in participation, and a 22 percentage point lower child penalty in % of FTE. Same-sex mothers also have fewer children than different-sex parents (see A.3.14). This difference starts to manifest three years after birth of the first child. Up to that time, the difference in child penalties between different-sex and same-sex mothers cannot be due to difference in the number of children.

It is interesting to compare my findings to Andresen and Nix (2019) who use the same method for Norwegian data. They are able to distinguish between birth-giving and co-mother, and find that after one year, the birth-giving mother (co-mother) incurs a child penalty of 13% (5%). In the Netherlands, these child penalties are larger: the lower earning (higher earning) partner has a child penalty of 11% (15%) after one year. In Norway, after four years same-sex mothers do not have child penalty anymore, whereas in the Netherlands it stays relatively constant at 11-13% for the lower earning partner, and increases to 24% for the higher earning partner. As the child penalty of subgroups is not equal to the overall child penalty, it may be that these differences in results stem from differences in group classification. Alternatively, different institutional settings may also contribute to the disparity in results.

3.6.5 Specialisation of the lower earnings-potential spouse in household and care tasks

The four previous sections have shown evidence that the child penalty is lower in environments with more egalitarian gender norms. However, an alternative explanation for the child penalty may be that couples maximise income while not outsourcing child care completely (which is not common in the Netherlands). In this scenario, the higher earnings-potential spouse becomes the main bread-winner once the first child is born. This is the man in the majority of couples in the US (Bertrand et al., 2013). In my data as well, three years before the first child, in 70% of parent-couples the man out-earns his partner. I test if income maximisation can explain the child penalty in the Netherlands by investigating the child penalty for couples where the woman out-earns her partner before the first child. If the child penalty

is due to income maximisation behaviour, there would be no child penalty in couples where the woman out-earns the man pre-child.

To test if the child penalty is present in couples where it would make more sense financially for the man to specialise in house and care work, I select couples where the women earn $\geq$1200 € per year more on average than their partners in year three and two before the child. These leaves me with 135,000 (or 22% of) women and their partners. At the median, these women earn around 9000 € per year (or 750 per month) more than their partners.

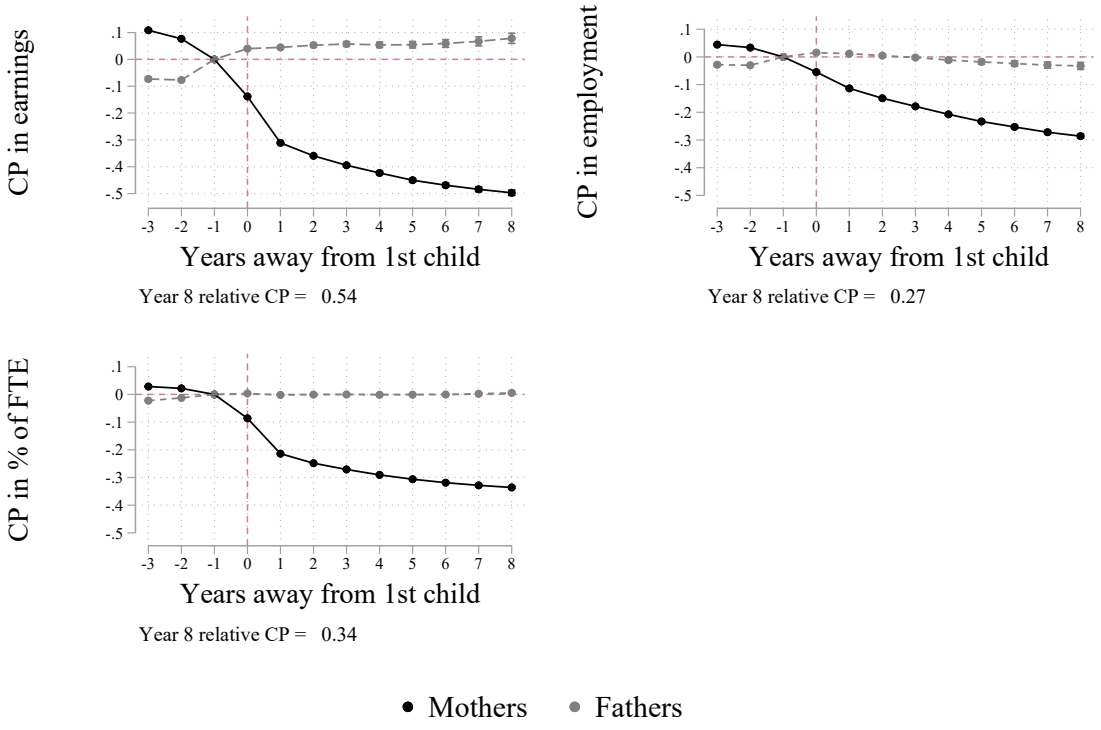
The relative child penalty in unconditional earnings for this group (Figure 3.13) is particularly large and amounts to 54%.[60] The relative child penalties in participation (27%) and % of FTE worked (34%) are very similar to the overall sample. This implies that the large child penalty in earnings does not seem to be driven by a more than average response in participation or % of FTE worked reduction, but mostly in a more than average reduction in the wage rate. This may suggest that these women are more likely to change into lower paid jobs, or that they are more likely to be demoted.

To better understand the large relative child penalty of this group, I plot predicted earnings after controlling for cohort and time effects, and the counterfactual in Figure 3.14. The Figure shows that the drop in earnings of women is large (blue solid line), and that after child birth men overtake women (grey solid line) and become the main bread-winners on average. The figure also shows that these are high-earning potential women (blue dashed line), who would have had a high paying job in the absence of a child. A large part of the child penalty is due to their inability to realise their earning potential once their first child is born. These findings match with the stylised fact that there are only very few women in top positions, and suggest that having children may play a role in this. They are also in line with Chung et al. (2017), who investigate the intra-couple earning differences after the birth of the first child.

The findings suggest that income maximisation behaviour in the household alone cannot explain the child penalty, since women who out-earn their partners before birth experience a very large child penalty.

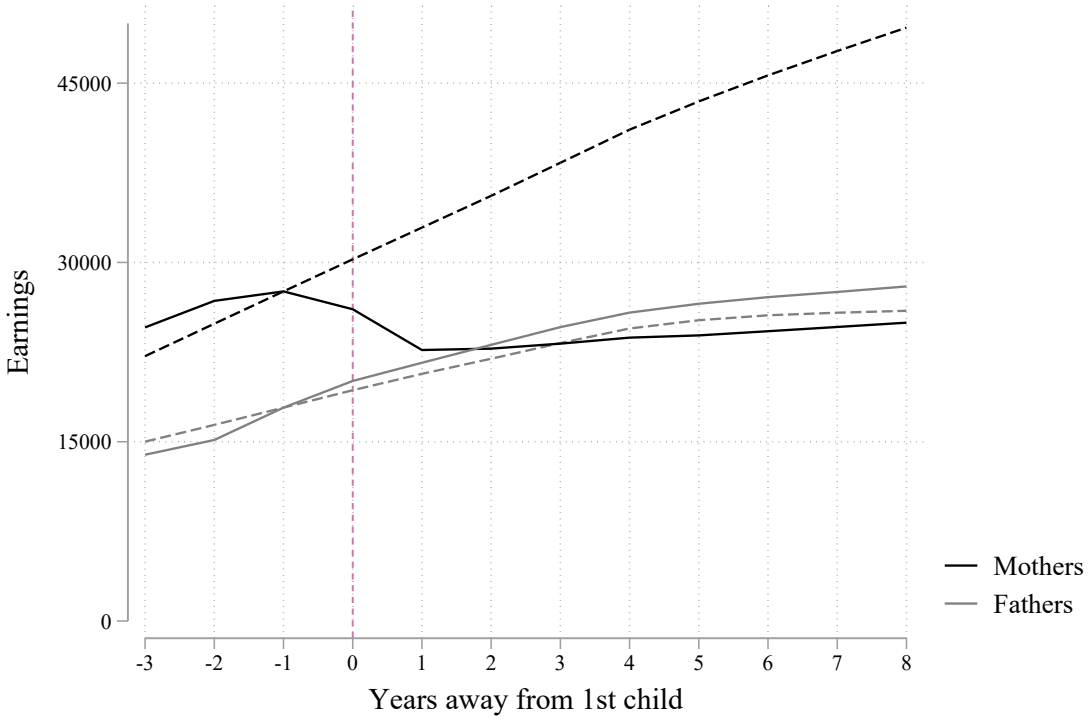
[60] These findings do not contradict the finding in Section 3.5.3, which shows that the highest income women incur the lowest child penalties. Indeed, women who out-earn their partner occur across the income distribution: 14% are in the first pre-birth income quartile, 18% in the 2nd, 25% in the third, and 42% in the highest quartile. In the highest income quartile of women, 52% are not classified as out-earning their partners. Moreover, couples in which women out-earn their partner are mainly determined by men who have low earnings, and not women who belong to the highest earnings category: Partners of women in the highest income quartile earn on average 32,000 € per year, whereas partners of women out-earning their partners earn 13,000 € per year.

Figure 3.13: Child penalties (CP) for women out-earning their partners pre-birth



Note: Child penalties (CP) based on Equation 3.2 for unconditional earnings, participation, and full-time equivalent (FTE) for couples where she earns more than him pre-birth. Solid lines refer to women, their partners are represented with dashed lines. The year 8 relative CP (from Equation 3.3) compares women to men eight years after birth of the first child.

Figure 3.14: Prediction and counterfactual for women out-earning their partners pre-birth



Note: For couples where she out-earns him before birth of the first child, the solid lines show the predicted unconditional earnings from Equation 3.1. The dashed lines plot the counterfactuals showing the situation had there not been a child, calculated as the predicted values from Equation 3.1 omitting the contribution of the event-time dummies.

3.7 Discussion

Despite a convergence towards gender equality in the last century, there are still gender differences in labour market outcomes. Many gender differences in the labour market are linked to having children. I estimate child penalties as in Kleven et al. (2019b) for unconditional earnings, participation, and FTE. In a second step, I document the importance of gender norms for the child penalty.

Eight years after birth of the first child, I find that mothers have 44% lower unconditional earnings than fathers. This is can be due to three margins: a reduction in hours worked, quitting paid work, a reduction in wage rate. I am able to analyse the first two, and find that both quitting paid work and reducing hours contribute to the child penalty, but that the reduction in hours is quantitatively more important. Moreover, there are large differences between socio-economic groups. The child penalty is lowest for high earning women, and highest for middle income women. High earning women may be more attached to the labour market, and thus reduce their labour market involvement less. Low earning women, on the other hand, may keep their labour market involvement out of necessity to complement the family income and outsource child care; or because their child care subsidies are higher.

All women, independent of where they are on the income distribution, experience a persistent decline of labour market outcomes once their first child is born, despite the availability of child care. Biological reasons are unlikely to fully explain the child penalty, because adoptive mothers who do not give birth nor breastfeed experience similar child penalties as biological mothers (Kleven and Landais, 2020). Moreover, not only biological mothers, but also caregiving fathers experience brain and hormonal changes (Abraham et al., 2014; Feldman and Bakermans-Kranenburg, 2017). Furthermore, I exclude that the child penalty is explained by income maximisation in the couple alone, as women out-earning their partners pre-birth have a particularly large child penalty. This implies that it is likely that there is a social norm component to the child penalty.

I indeed find that women in a social context with less egalitarian gender norms such as the Bible Belt have larger child penalties. In contrast, a history of new fathers decreasing their working hours in the firm does not result in more part-time work among new fathers. Dutch men report that their career has priority for them (Merens and Bucx, 2018). If reducing

working hours has negative consequences for their career, they may not be willing to reduce hours.

Yet, in the more egalitarian social environments such as the non-Bible Belt area or families living far away from their grandparents, the child penalty is relatively large. Only for women in same-sex parenthood, where division of labour in the household cannot be guided by gender roles at all, the child penalty halves compared to the different-sex child penalty. Same-sex couples illustrate that it is possible to distribute paid work and care work more equally in a couple. However, the results for same-sex parents also reveal that there is no couple without some form of child penalty. This is not surprising, as child rearing takes time, and this time has to be freed up by at least one of the legal parents if sending the child to full-time child care is not an option.

Is it necessarily a bad thing if women take over the large part of child rearing activities? One may argue that given women's overall happiness with part-time work, the child penalty is not a problem. However, in the European Value Study, 80% of the Dutch is of the opinion that men should have as much responsibility for home and children as women do. This indicates that a large majority of the population feels that there should be an equal distribution of non-remunerated work at home between men and women. My results show that the Netherlands are still far away from what a large part of the population finds desirable. Apart form equality concerns, the current division of labour has various negative consequences. First, women cannot always live up to their potential on the labour market (Kuziemko et al., 2018), and/or they are not economically independent (Merens and Bucx, 2018). With high divorce rates, this is a non-negligible financial risk. Indeed, about 16% of divorced women are dependent on aliments, which amount to 1000 € per month on average (Statline, 2014). For this reason, economic independence of women is a policy issue in the Netherlands that is monitored every year. Even though the share of women who are financially independent has risen in the past 10 years, in 2019 still roughly 40% of women are considered financially dependent (CBS, 2019c). Second, men get to spend less time with their children than their partners.[61] Third, there is evidence that the double burden for paid work and child rearing experienced by women leads to health problems (Fontenay and Tojerow, 2020; Persson and Rossin-Slater, 2019). Fourth, diversity brings economic gains (Ostry et al., 2018; Hoogen-

[61] An Australian palliative care nurse has gathered the top five regrets of her dying patients. Many men regretted to have worked too much, and they wish they had spent more time with their family (Steiner, 2012).

doorn et al., 2012; Green and Homroy, 2018; Joecks et al., 2013; Khamis et al., 2019; Dang et al., 2020), so the reduced involvement of women in the labour market may have negative impacts for firms and the economy as a whole.

Can policy do something about the child penalty? My results show that the child penalty depends on gender norms. It is difficult to implement policies that target norms directly, because they are largely influenced by the examples parents set for their children (Farré and Vella, 2013; Olivetti et al., 2020; Fernández et al., 2004; McGinn et al., 2019); and because they are learned at an early age (Bian et al., 2017). While schools and child care could be used to reduce gender stereotypes learned in childhood[62] such a policy may not be feasible in the Netherlands. In the Dutch constitutions, school's freedom to teach religion is protected, and religious schools may be attached to abiding by gender stereotypes.

Instead of targeting educational institutions, making different care and work arrangements possible could be a way of targeting gender norms indirectly, because it gives the freedom to choose the model that works best for a family in the short run; and it may alleviate the child penalty in the long run by broadening existing gender roles.[63] The Netherlands already has relatively generous family policies, but two aspects could be improved. First, paternity leave was very short in the period of study. In 2019, paternity leave has been extended to one working week, and it will be extended to five more weeks in 2020 (Rijksoverheid, 2019). The extended paternity leave may lead to more equal division of household work (Patnaik, 2014) and eventually decrease the child penalty. Second, parental leave is unpaid for some firms, whereas other firms pay part of the leave, mainly in high-skilled jobs. This raises a socioeconomic equity concern, as only well-off families can afford parental leave in this system. This short-coming is going to be addressed in 2022 in order to comply with European law (Koolmees, 2019). Finally, take-up of parental leave of men in non-teaching occupations is relatively low. Any policy encouraging a higher take-up of this leave, especially once that it is paid and thus affordable for all parents, may contribute to reducing the child penalty.

[62] For example, gender stereotypes of teachers have been found to influence girls' math performance negatively (Carlana, 2018). Reducing unconscious bias of teachers could be a starting point for policy makers. Alternatively, same-sex schools may be an interesting policy to consider. Giardili (2019) finds that children attending single-sex schools make less gendered subject choices, and Booth and Nolen (2012a,b) find that gender differences in making risky choices and competitiveness disappears for girls having attended girls-only schools.

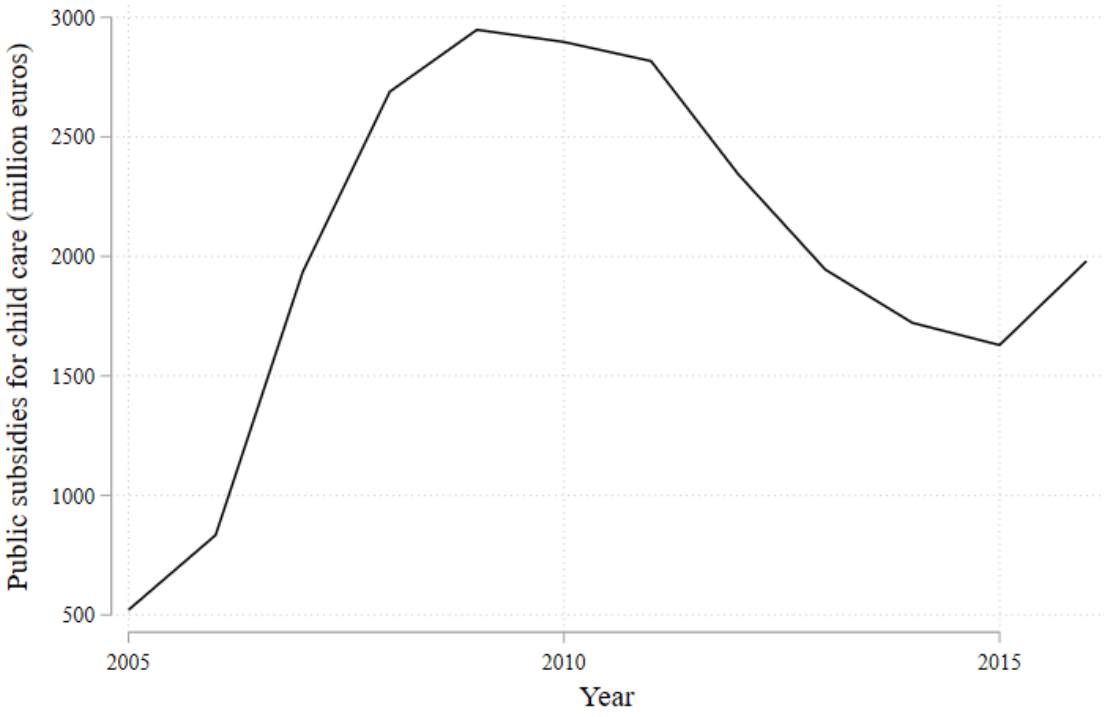
[63] An example of a policy influencing norm is provided by Aksoy et al. (2018), who show that same-sex marriage legalisation policies shaped societal attitudes towards sexual minorities in Europe.

Acknowledgements

I am grateful for the access to linked non-public microdata provided by Statistics Netherlands (CBS). Under certain conditions, these microdata are accessible for statistical and scientific research. For further information: microdata@cbs.nl.

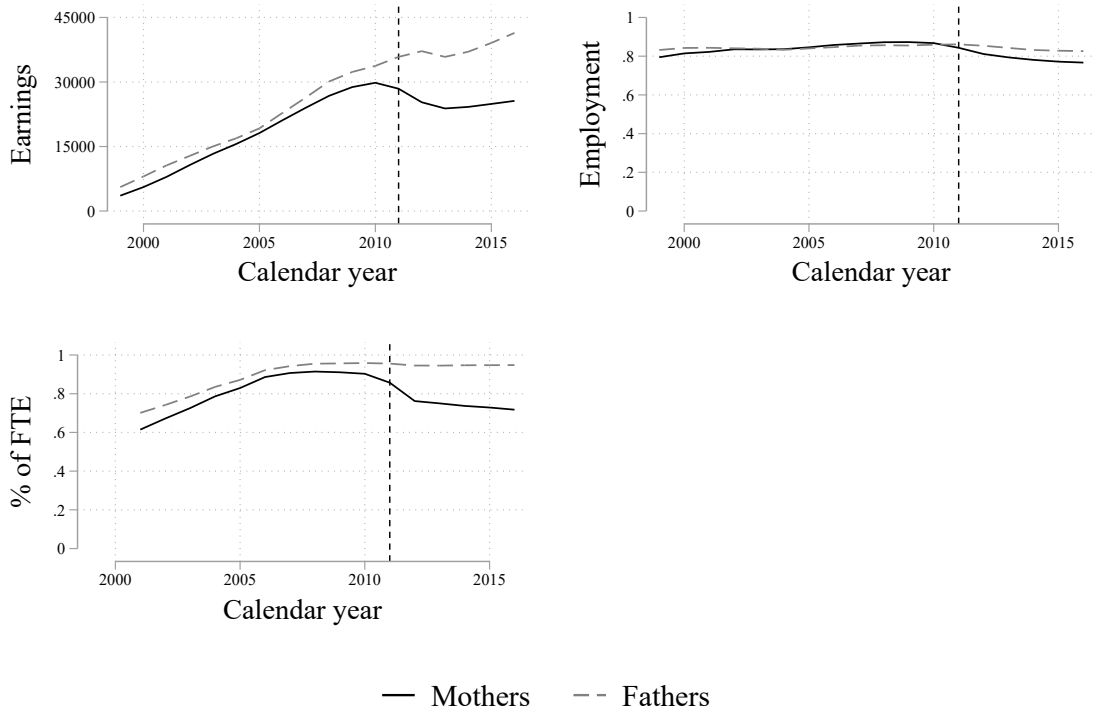
3.A Appendix

Figure A.3.1: Public expenditures for child care subsidies



Note: Public subsidies for child care in million euros. Source: Statline (2019c)

Figure A.3.2: Average earnings of the 1979 cohort who has their first child in 2011 by gender



Yearly average labour market outcomes of the cohort born in 1979 with a child in 2011 (at age 32) by gender. $N_{women} = 6,048$, $N_{men} = 6,507$. FTE stands for full-time equivalent.

3.A.1 Gender norms in the European Value Study

In order to give an overview of prevailing gender norms in the Dutch population, I use the European Value Study EVS (EVS, 2015). Table A.3.1 shows the statements related to gender norms in the EVS 2008, and the corresponding percentage of women and men who agree with the statement. The questions relate to the role of women as mothers and housewives, the mother's/wife's roles on the labour market, the interaction between these two roles, and men's role as fathers.

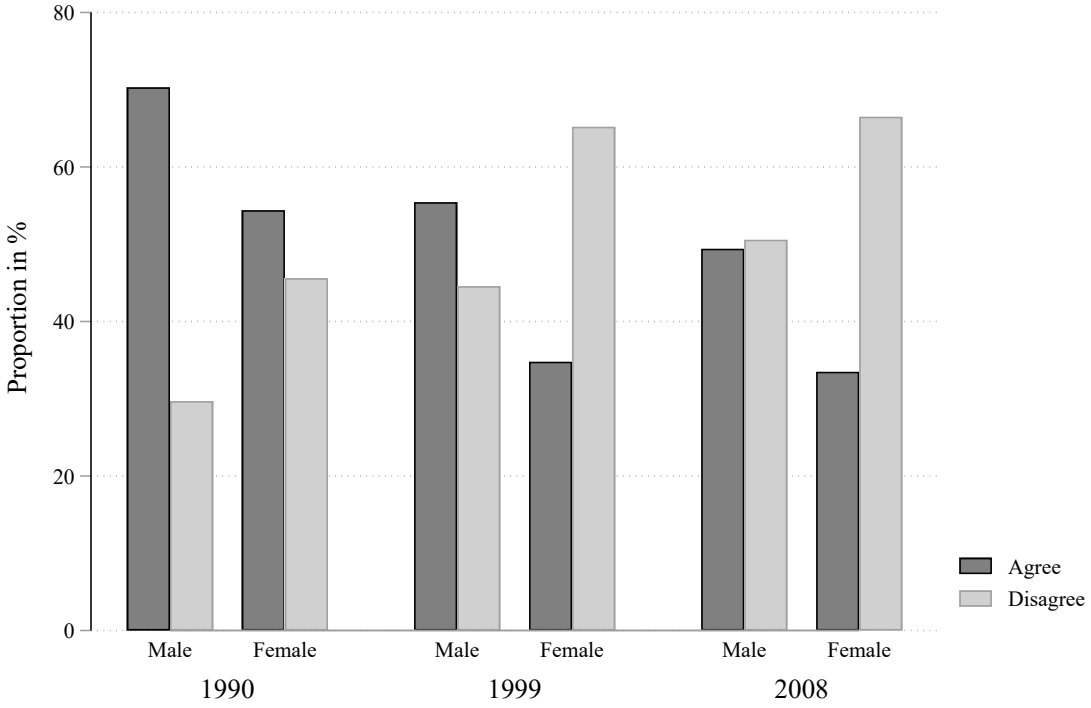
Table A.3.1: Attitudes related to gender roles in the Netherlands

	Women % Agreeing	**Men** % Agreeing
1. "A woman has to have children to be fulfilled"	7%	10%
2. "A job is alright but what most women really want is a home and children"	40%	45%
3. "A pre-school child suffers with a working mother"	34%	49%
4. "A working mother can establish just as warm and secure a relationship with her children as a mother who does not work"	85%	78%
5. "Being a housewife is just as fulfilling as working for pay"	55%	64%
6. "If jobs are scarce, men should have more right to a job than women"	13%	11%
7. "Husband and wife should both contribute to income"	52%	39%
8. "A man has to have children to be fulfilled"	5%	7%
9. "Fathers are as well suited to look after their children as mothers"	84%	71%
10. "Men should take as much responsibility as women for home and children"	84%	86%

Source: European Value Study (2008), Dutch sample. N_{men} = 701, N_{women} = 853.

There seems to be a consensus that both women and men do not need to have children to be fulfilled (statement 1 and 8). Almost half of the Dutch believe that for women, family is more important than work (statement 2). Many think that mothers of small children should not work (statement 3), but the reason for this does not seem to be that these children have a lower quality relationship to their mothers (statement 4). More than half of respondents think that being a housewife is as fulfilling as working for pay (statement 5). Only a small minority thinks that men should be prioritised on the labour market in general (statement 6). Less than half think that both women and men should contribute to the household income (statement 7). There is a consensus that men are suited to look after children, and that men should take responsibility at home (statement 9 and 10).

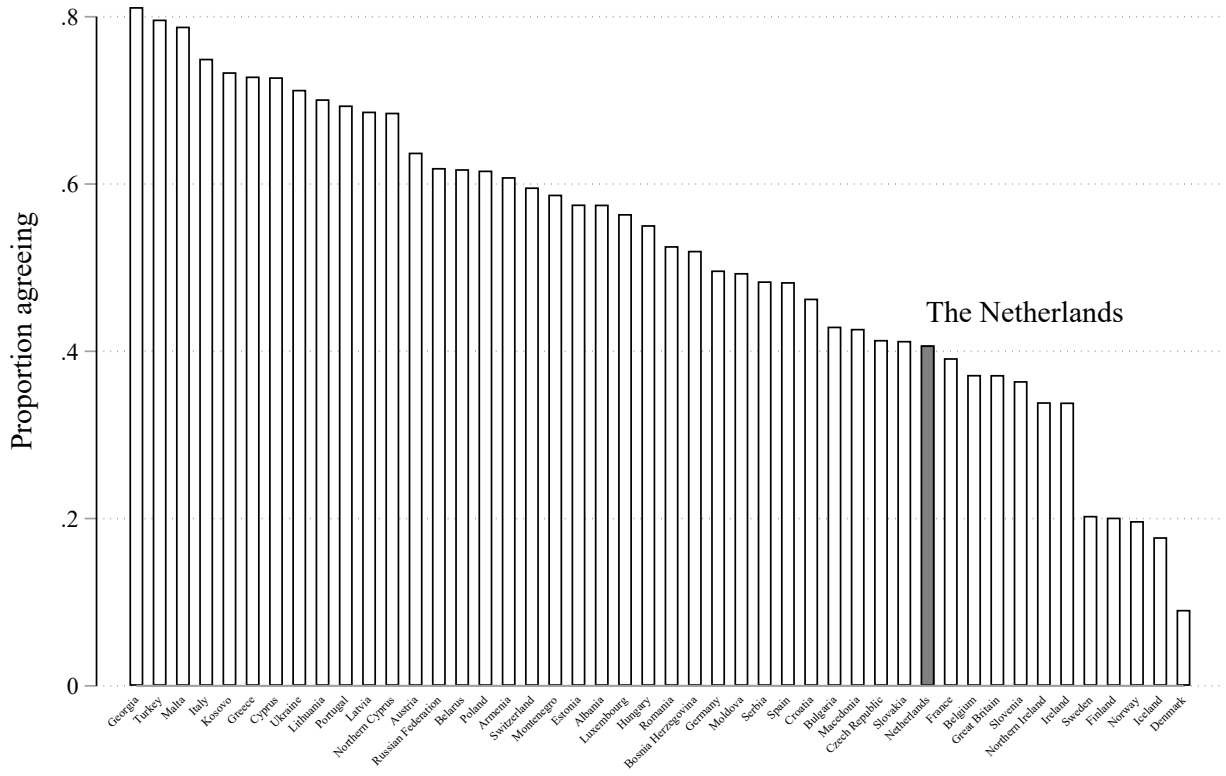
Figure A.3.3: Attitudes towards 'a pre-school child suffers with a working mother' in the Netherlands over time by gender



Source: EVS (2015), Dutch sample.

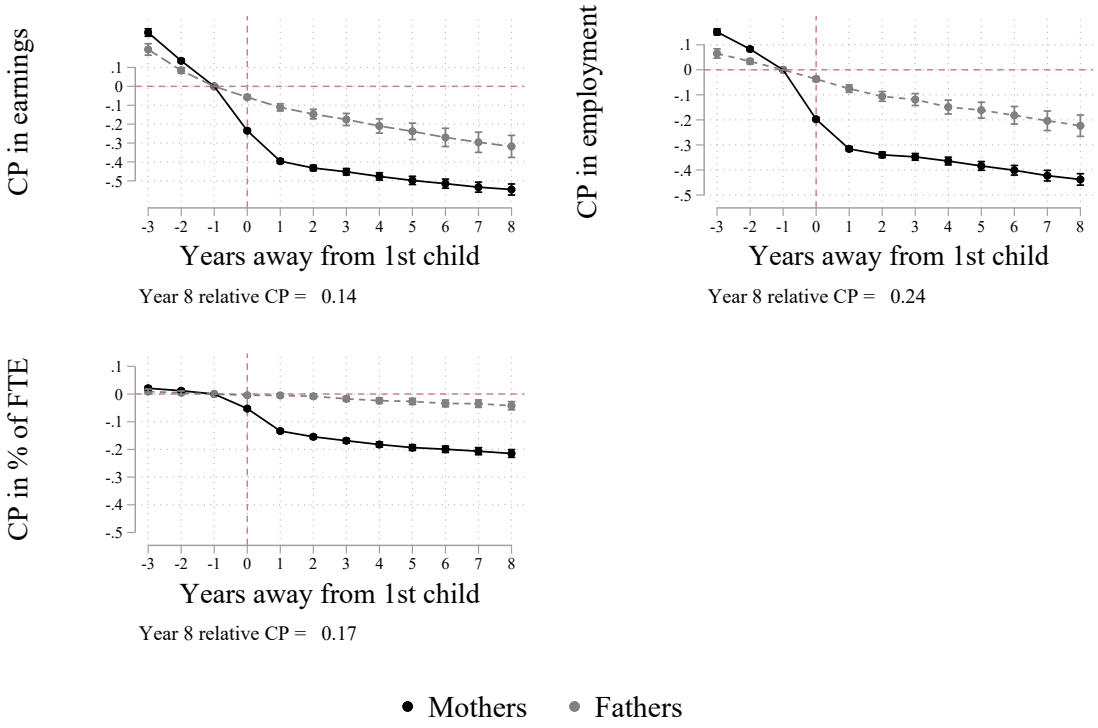
These norms have evolved importantly since 1990. For example, a majority of both men and women agreed with the statement 'pre-school children suffer with a working mother'. Twenty years later, the large majority of women disagrees, as well as about half of men (see Figure A.3.3 for details). There is also considerable variation by country. Figure A.3.4 shows that the Netherlands has a relatively low proportion of people agreeing to the statement, but it is still four times as high as the proportion in Denmark.

Figure A.3.4: Proportion agreeing to 'a pre-school child suffers with a working mother' by country



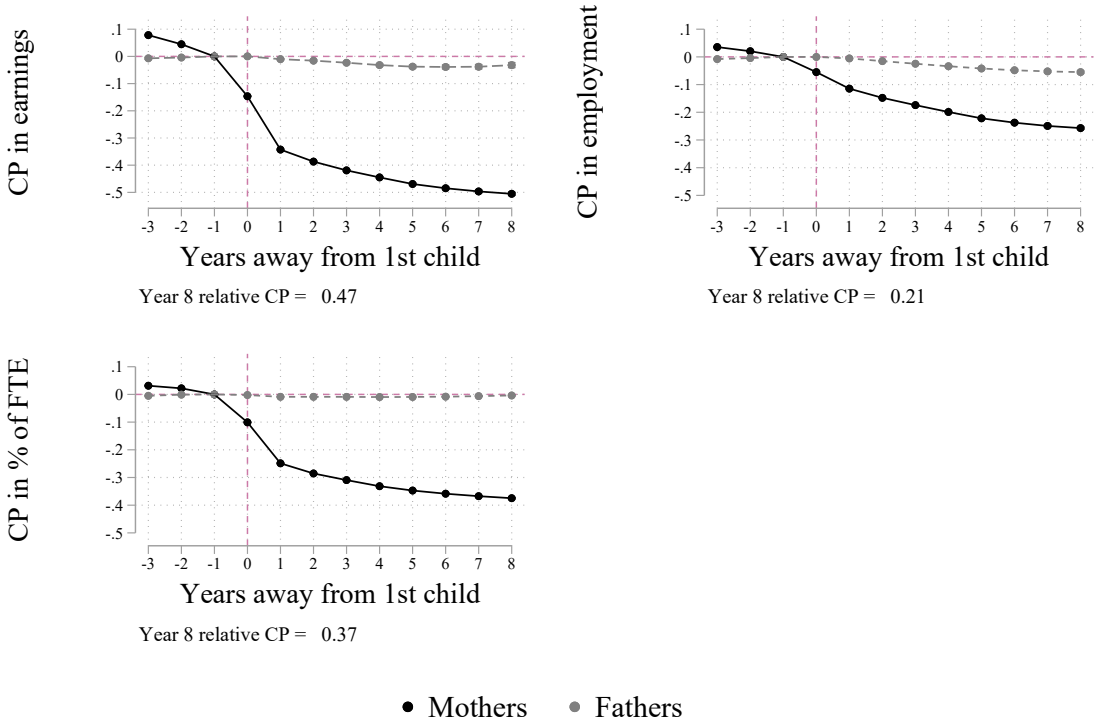
Source: European Value Survey (EVS, 2015), waves 2008-2009.

Figure A.3.5: Child penalties (CP) for non-traditional families



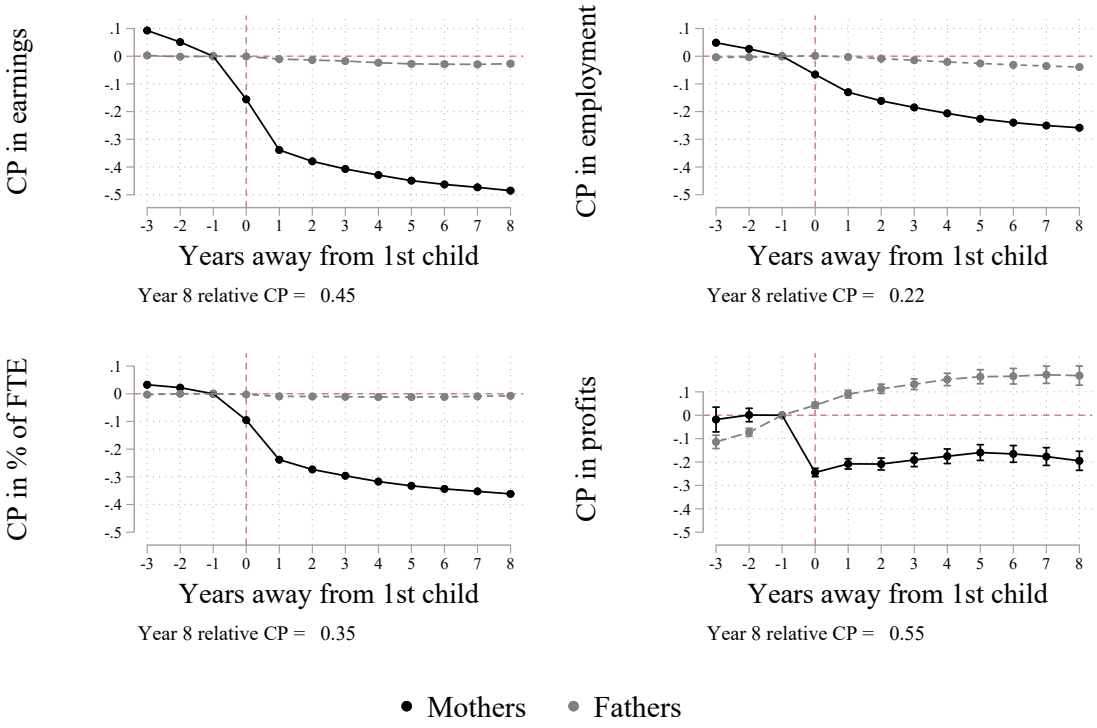
Note: Child penalties (CP) based on Equation 3.2 for unconditional earnings, participation, and full-time equivalent (FTE) for single legal parents who are not living with a partner in the first two years of life of the child. Solid lines refer to women, their partners are represented with dashed lines. The year 8 relative CP (from Equation 3.3) compares women to men eight years after birth of the first child. $N_{women} = 32,000$, $N_{men} = 13,000$.

Figure A.3.6: Child penalties (CP) for parents who live together



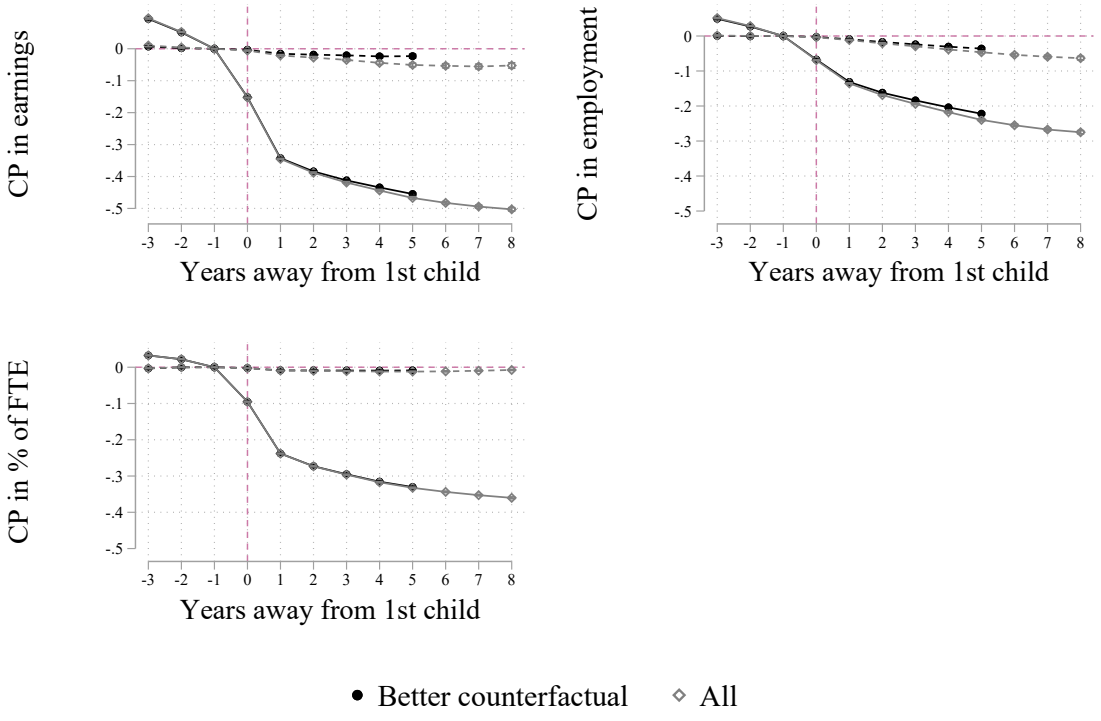
Note: Child penalties (CP) based on Equation 3.2 for unconditional earnings, participation, and full-time equivalent (FTE) for parents who live together during the whole observation period. Solid lines refer to women, their partners are represented with dashed lines. The year 8 relative CP (from Equation 3.3) compares women to men eight years after birth of the first child.

Figure A.3.7: Child penalties including self-employment



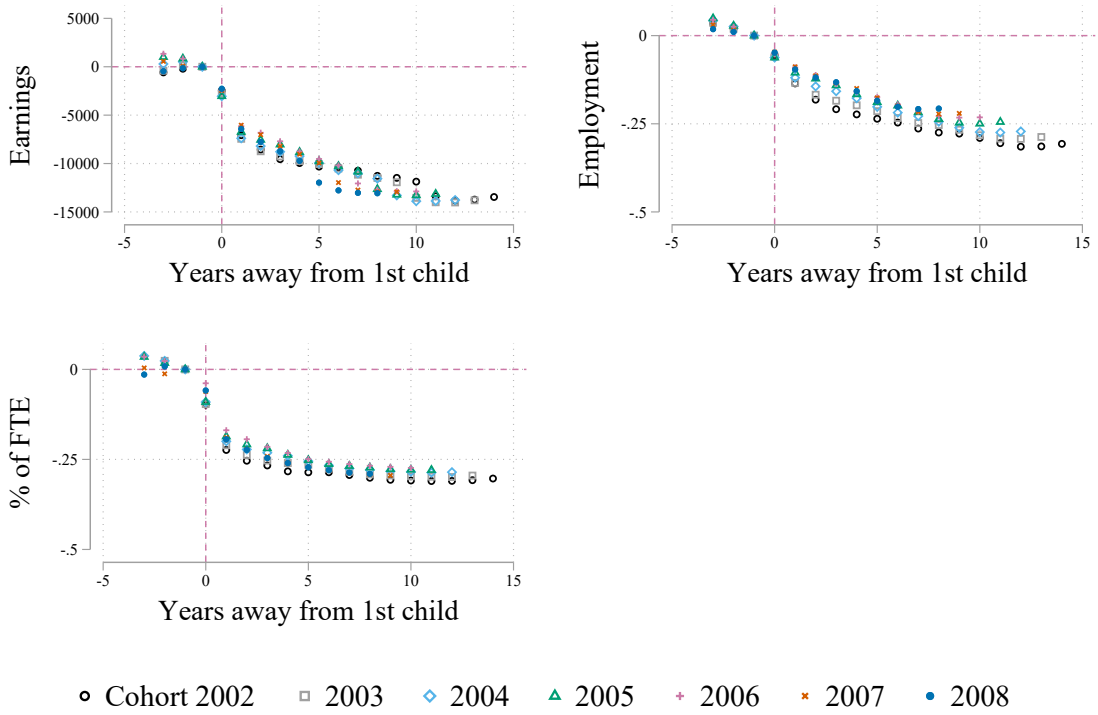
Note: Child penalties (CP) based on Equation 3.2 for unconditional earnings including profits from own firm, participation including self-employment, full-time equivalent (FTE) (only available for employees, and profits from own firm. Solid lines refer to women, their partners are represented with dashed lines. The year 8 relative CP (from Equation 3.3) compares women to men eight years after birth of the first child. Data from 2016 are omitted because of a reporting change in self-employment.

Figure A.3.8: The role of missing counterfactual for estimating child penalties



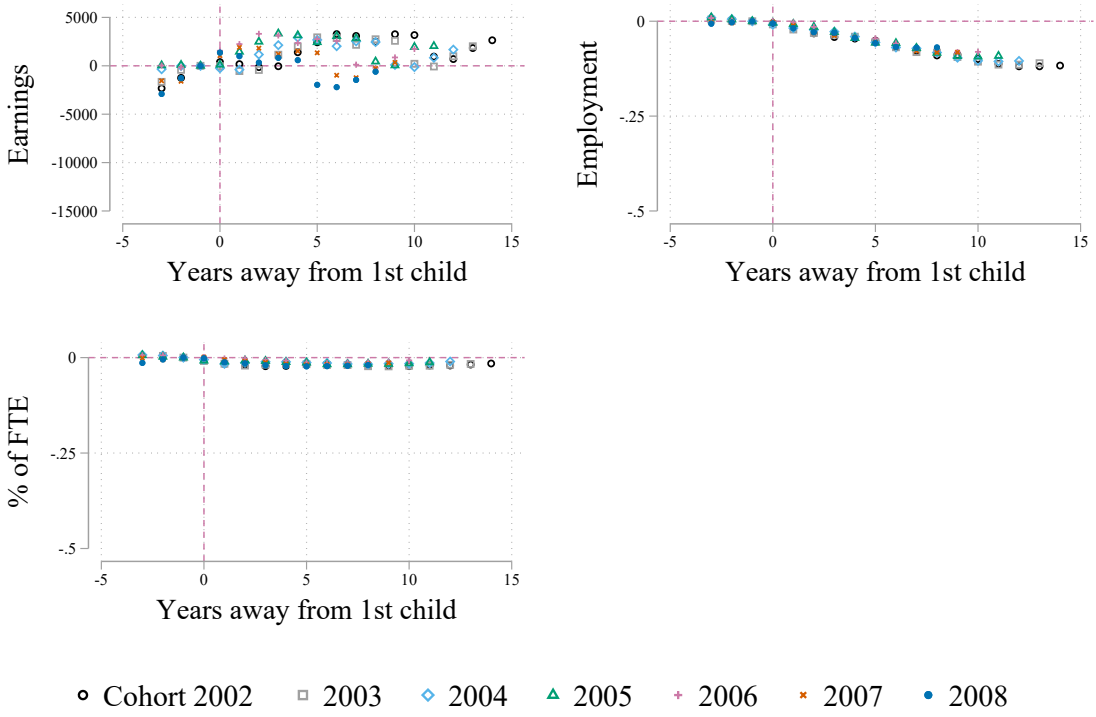
Note: Child penalties (CP) based on Equation 3.2 for unconditional earnings, employment, and full-time equivalent (FTE). Solid lines refer to women, their partners are represented with dashed lines. The 'Better counterfactual' specification follows parents for fewer years, in order to reduce the number of year and age coefficients that are solely based on individuals that are parents already, resulting in a more valid counterfactual.

Figure A.3.9: Event-study coefficients for unconditional earnings, participation, and % of FTE worked (women)



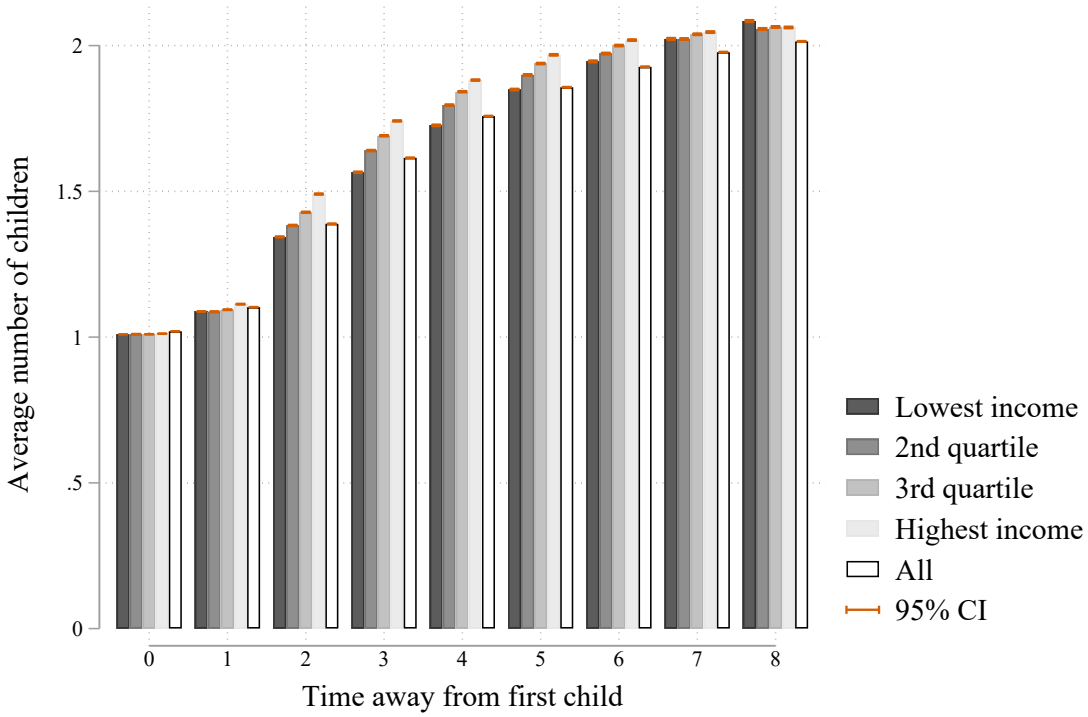
Note: Event-time coefficients and their 95% confidence intervals as estimated by Equation 3.2 for unconditional earnings, participation, and full-time equivalent (FTE) by cohort of women who have their first child in a certain year. Each cohort can be followed a different number of years.

Figure A.3.10: Event-study coefficients for unconditional earnings, participation, and % of FTE worked (men)



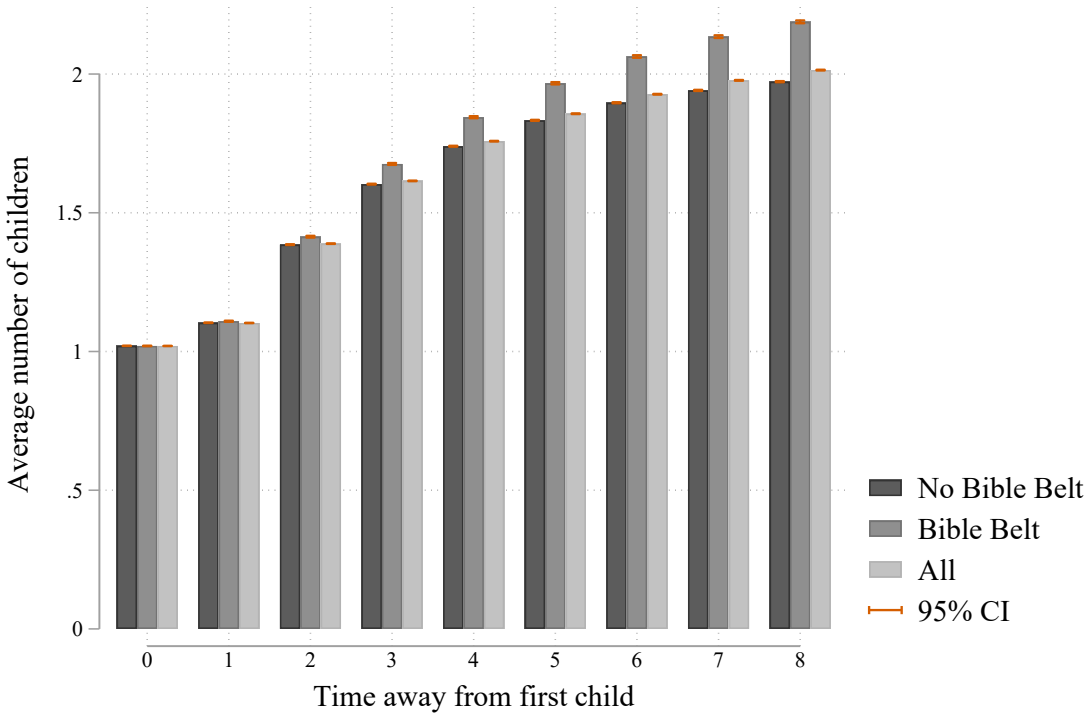
Note: Event-time coefficients and their 95% confidence intervals as estimated by Equation 3.2 for unconditional earnings, participation, and full-time equivalent (FTE) by cohort of women who have their first child in a certain year. Each cohort can be followed a different number of years.

Figure A.3.11: Average total fertility by the women's pre-birth income quartile



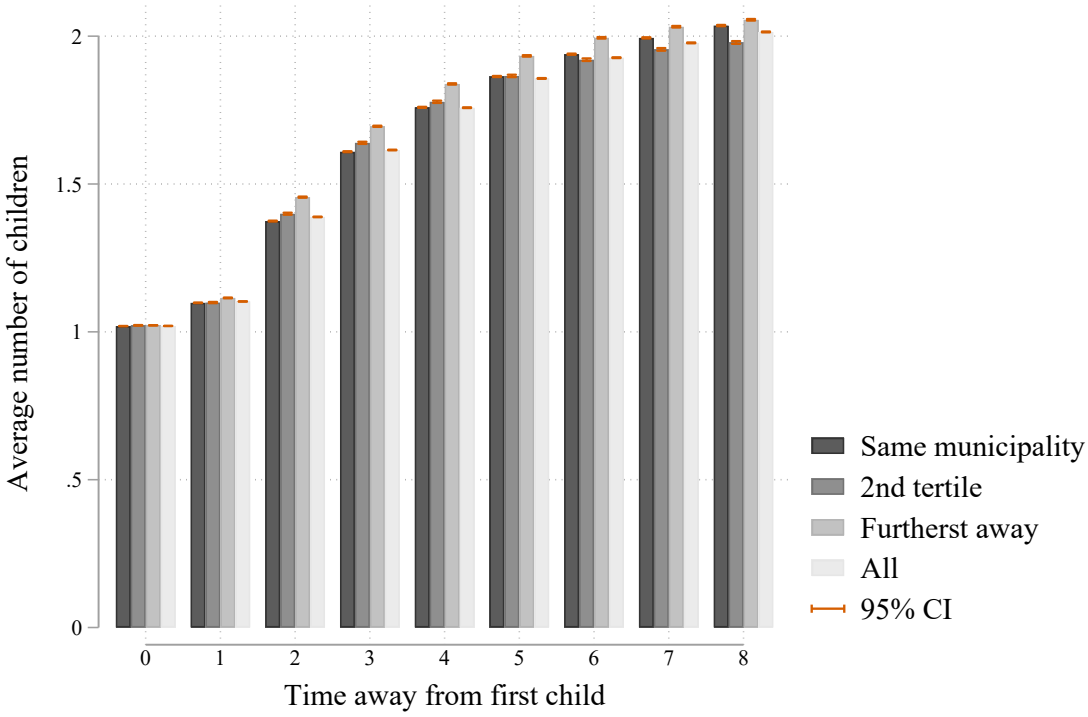
Note: Average total fertility with 95% confidence interval (CI) by the woman's pre-birth income quartile and all parents with a first child in 2002-2008.

Figure A.3.12: Average total fertility in the Bible Belt and elsewhere



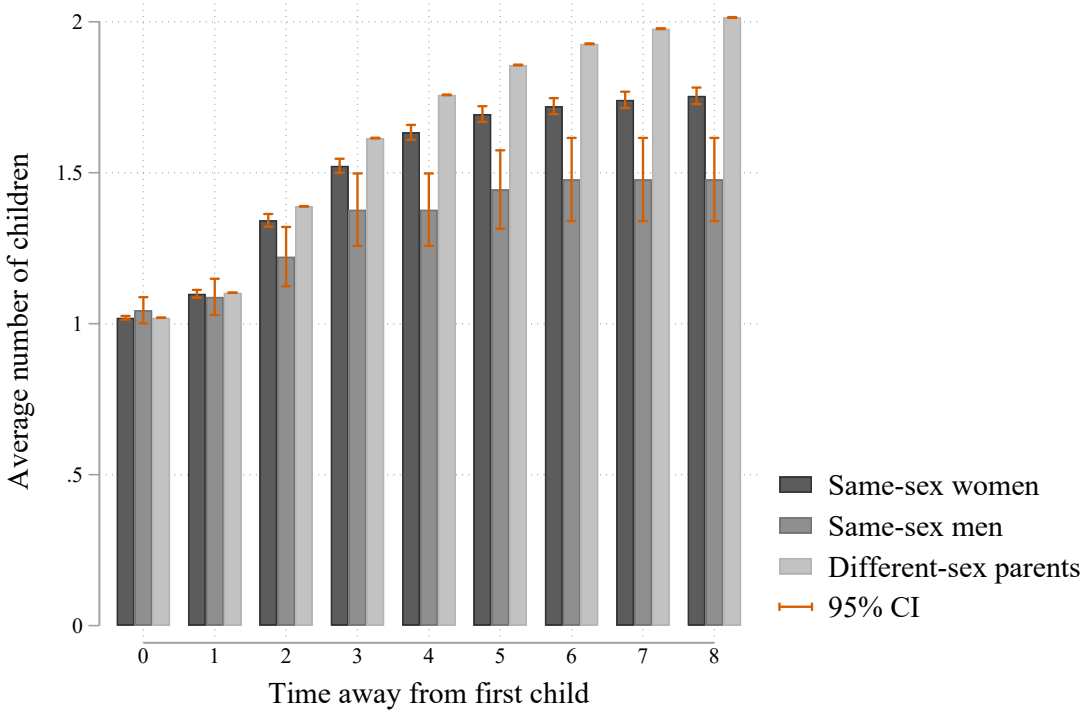
Note: Average total fertility with 95% confidence interval (CI) for families living in the Bible Belt, outside the Bible Belt, and all parents with a first child in 2002-2008.

Figure A.3.13: Average total fertility by pre-birth distance to grandparents tertile



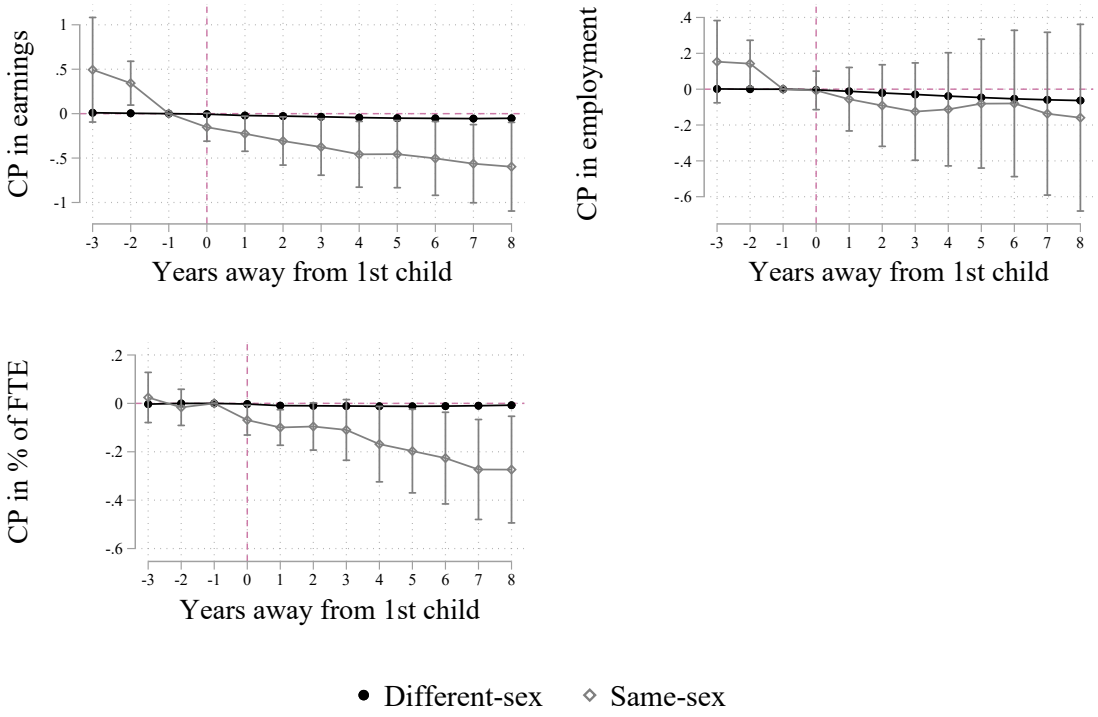
Note: Average total fertility with 95% confidence interval (CI) by tertile of the pre-birth distance of residence to the closest grandparent and all parents with a first child in 2002-2008.

Figure A.3.14: Average total fertility for same-sex and different-sex couples



Note: Average total fertility with 95% confidence interval (CI) for same-sex mothers, same-sex fathers, and different-sex couples with a first child in 2002-2008.

Figure A.3.15: Child penalties for same-sex and different-sex men



Note: Child penalties (CP) based on Equation 3.2 for unconditional earnings, participation, and full-time equivalent (FTE) for same-sex (S-S) and different-sex (D-S) fathers.

Chapter 4

The Kids Are Alright - Labour Market Effects of Unexpected Parental Hospitalisations in the Netherlands

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4.1 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 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 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., 2015b). 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.[64] 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

[64] 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); Schneider et al. (2013); Heger and Korfhage (2017); Geyer and Korfhage (2017); Løken et al. (2017); Crespo and Mira (2014); Ciccarelli and Van Soest (2018). For a more extensive literature review see Bauer and Sousa-Poza (2015); Lilly et al. (2007).

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.[65] 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.[66]

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 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 (2020) 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

[65] 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.

[66] 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).

non-fatal health shocks do not affect the labour supply of the spouse. The identification strategy of Fadlon and Nielsen (2020) 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,[67] 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

[67] 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 76 for mothers, and 78 for fathers.

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).

4.2 Institutional Background

The Dutch formal LTC system is comprehensive and has a longstanding tradition; a public LTC insurance (ABWZ[68]) 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,[69] 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 only 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., 2015a; 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., 2015a).[70]

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

[68] *Algemene Wet Bijzondere Ziektekosten*

[69] Transferred to the Social Support Act in 2007

[70] 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 expansions. 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.

(more 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 used 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.

4.3 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.[71] 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.[72]

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.[73]

The data available limits the type of work interruptions we can detect. Table (4.1) 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.[74] 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[75] diagnoses that are only

[71] 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.

[72] Table A.4.1 in the Appendix gives an overview of the data sets used.

[73] 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.

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

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

Table 4.1: 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

Note: [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)

treated in the hospital and that an expert physician considered to be not foreseeable (see also García-Gómez et al., 2015b, 2017).[76] 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.

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

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

Table 4.2: 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%

Note: Sample selection: parents in the treatment group (see Section 4.4).

on weekends as on weekdays (Card et al., 2009; Dobkin et al., 2018) (see Section 4.5.3 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 4.2. On a more aggregate level, Table 4.3 shows the frequency of grouped diagnoses classified as health shocks in the treatment group.[77] 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 shocks are less likely to be day care admissions (27 vs 73%).[78] 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 A.4.4 for details). Taken together, we interpret these statistics as evidence that the diagnoses we use are indeed severe.

[77] see Section 4.4 for how the treatment group is defined

[78] Tables (A.4.2) and (A.4.3) provide more information on the type of hospital diagnoses not labelled as a health shock.

Table 4.3: 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	.	.

Note: Sample selection: parents in the treatment group (see Section 4.4). Statistics Netherlands does not release data cells below 10 observations to protect privacy. Therefore, the numbers are missing for the diagnosis group 'endocrine diseases'.

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 4.5.4), 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., 2015a). Second, when combining the health shock definition with information on health determinants of formal LTC use,[79] 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 A.4.5 in the Appendix for details). Furthermore, combining diagnosis group-specifc information from Bakx et al. (2015c) 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.

[79]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).

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[80] 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 4.4 and 4.5 show summary statistics of these variables.[81] 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.

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

[81] Table A.4.6 and A.4.7 in the Appendix show the same summary statistics for the working sample.

4.4 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.

4.4.1 Selection of the treatment and control group

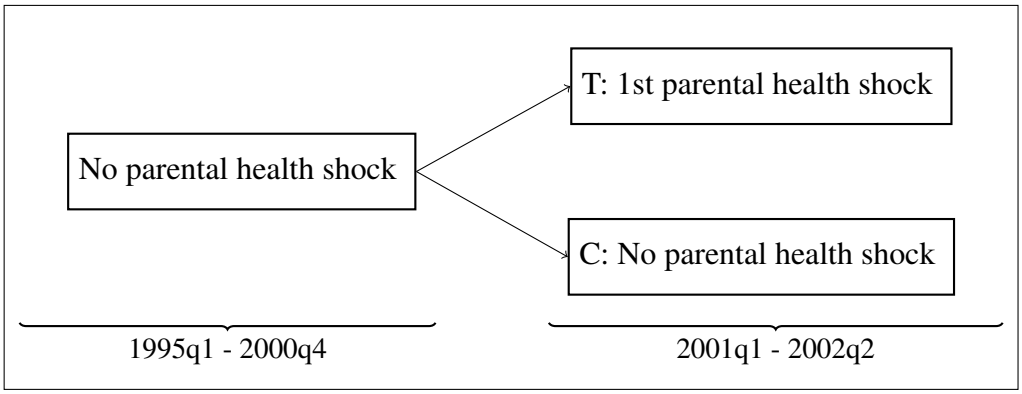
We start by excluding observations with an unexpected parental hospitalisation between 1995q1 and 2001q2 to make the sample more homogeneous. This avoids that relapses of pre-existing conditions play a role and thus reinforces the unexpectedness of the parental health shock. Figure 4.1 depicts how the 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 and 2002q2.[82] 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.[83] Individuals exit the sample at different points in time if both parents die, upon reaching retirement age, or

[82] In a robustness check, we shift the treatment period to 2004q3-2005q4. The results remain stable (Figure A.4.13 in the Appendix).

[83] 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 (4.5.3) we provide evidence that our results are not driven by this particular random sample of controls.

Figure 4.1: Timing of the parental health shock and treatment (T) and control group (C) assignment



the death of the parent experiencing the health shock.[84] Therefore, each cohort of treatment and control group is an unbalanced panel.

[84] 82% of the sample is observed for the full 33 quarters.

Table 4.4: 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		

Note: * 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 4.5: 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		

Note: * 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.

4.4.2 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.[85] 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[86]) 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

[85] 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.

[86] 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

found.[87] 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 4.4 for women and 4.5 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.

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

$$y_{it} = \alpha_i^I + \alpha_t^T + \sum_{k=-7}^{24} \gamma^k q_{it}^k + \sum_{k=-7}^{24} \beta^k D_i q_{it}^k + \delta x_{it} + \varepsilon_{it} \tag{4.1}$$

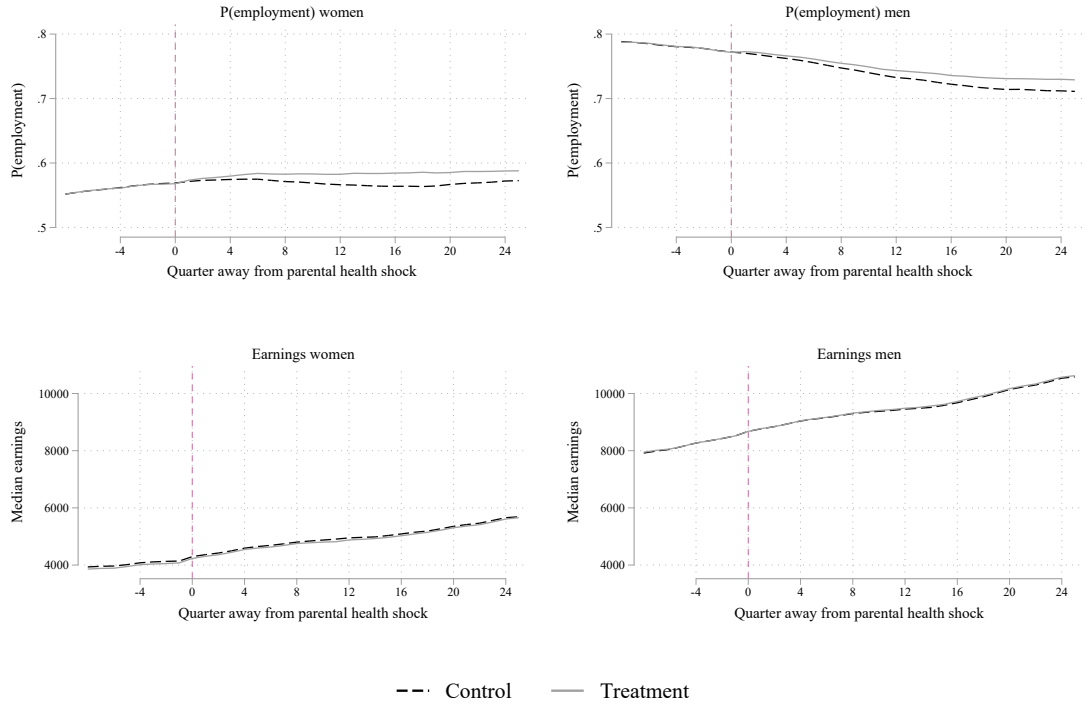
Equation (4.1) is estimated using the within transformation plus CEM weighted least squares for the probability of employment and log conditional earnings. The first sum in Equation

[87] 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).

(4.1) 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).[88]

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.

Figure 4.2: CEM weighted employment and earnings trends



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

Directly testing for the evolution of parental health is not possible (cf. García-Gómez et al., 2013; Fadlon and Nielsen, 2020), but the inspection of raw employment and earnings trends by group before the health shock is informative. Figure 4.2 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 4.4 and 4.5, 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 $\beta^{-7}, ...\beta^{-1}$. 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.

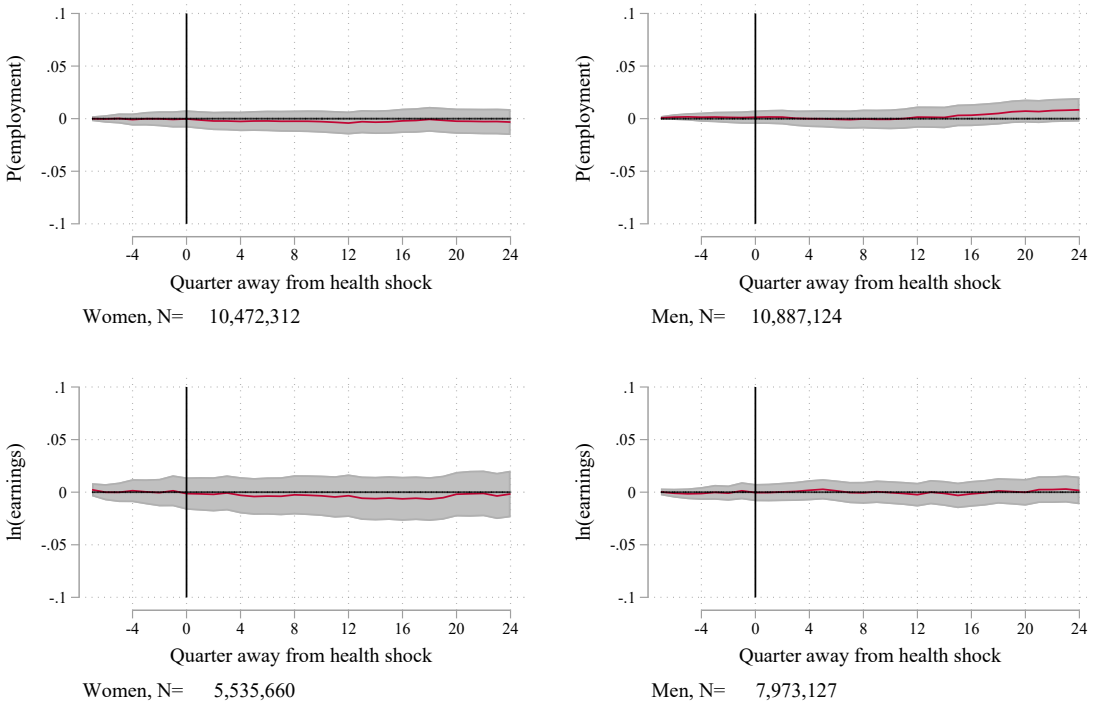
4.5 Results

4.5.1 CEM weighted Difference-in-Difference

In Figure 4.3, we plot the CEM weighted coefficients of the difference-in-differences term β^k and their 95% Bonferroni adjusted[89] 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.

[89]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.

Figure 4.3: Earnings and employment effects of a parental health shock



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

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.[90]

[90]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.

Table 4.6: 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

Note: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$ 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 a wage 4 quarters before the health shock in this subsample. A more detailed definition of the subsamples can be found in Section (4.3).

4.5.2 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.[91] 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 4.6 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 A.4.1-A.4.8 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.

[91] 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.

Table 4.7: 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

Note: $*p < 0.1$, $**p < 0.05$, $***p < 0.01$ 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.

4.5.3 Robustness checks

We check the robustness of our main findings in Table 4.7. 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 A.4.9-A.4.14). 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 (2020).[92] 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.[93]

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

[92] 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.

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

proxy for very severe cases, which in turn require a lot of informal care. 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).[94] 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[95] 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 A.4.15 in the Appendix. We are therefore confident that our results are not sensitive to the random sample we have selected.

[94] 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.

[95] 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.

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.

4.5.4 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.[96] 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,[97] 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.

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

[97] 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.

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 4.8) show that the illness of a close family member is a strong predictor of informal 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).

Table 4.8: 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
	(0.00793)	(0.00679)	(0.0810)	(0.0747)	(0.158)	(0.146)	(0.00802)	(0.00665)
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
	(0.0108)	(0.00814)	(0.107)	(0.0865)	(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

Note: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. 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.

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.

4.6 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 em-

ployment 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., 2015a), 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.[98]

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

[98]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).

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.

Acknowledgements

We are grateful for the access to linked non-public microdata provided by Statistics Netherlands (CBS). Under certain conditions, these microdata are accessible for statistical and scientific research. For further information: microdata@cbs.nl. We thank TNO for the access to the STREAM survey data.

4.A Appendix

4.A.1 Data

Table A.4.1: 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

Note: 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-microdata` (available in Dutch only). The STREAM data are described in Ybema et al. (2014).

Table A.4.2: 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 A.4.4: 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***
	(0.000486)	(0.000685)
First generation migrant	0.0785***	0.0770**
	(0.000653)	(0.000778)
Second generation migrant	0.00808***	-0.00362***
	(0.000611)	(0.000691)
Birth year	-0.0223***	-0.0233***
	(1.53e-05)	(1.72e-05)
Partnered	0.0862**	0.0804***
	(0.000236)	(0.000304)
Constant	2.524***	2.559***
	(0.00171	(0.00186)
Observations	2,828,507	2,738,722
R-squared	0.517	0.466

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

Table A.4.3: 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 A.4.5: 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	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
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 A.4.6: 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		

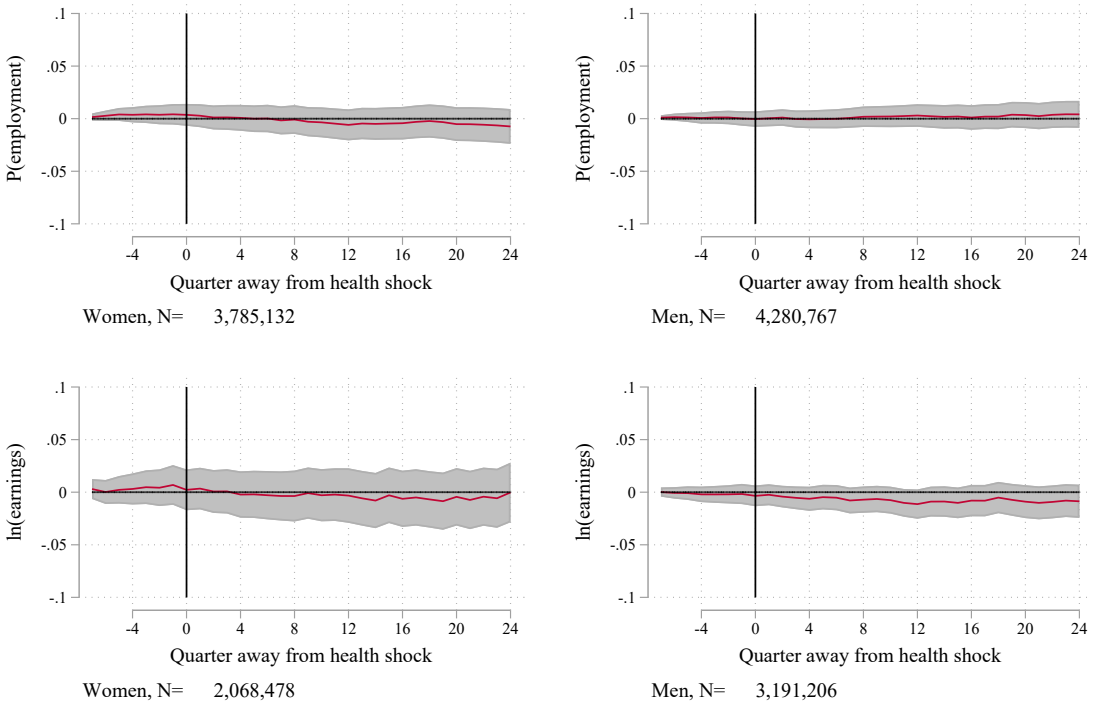
Note: * StdDiff > 0.25 (Imbens and Wooldridge, 2009). Standardised difference one quarter before the parental health shock StdDiff=$\frac{\bar{X}_{C,-1}-\bar{X}_{T,-1}}{(\hat{\sigma}^2_{C,-1}+\hat{\sigma}^2_{T,-1})^{0.5}}$ where $\bar{X}_{C,-1}$ corresponds to the mean of variable X of the control group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

Table A.4.7: 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
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		

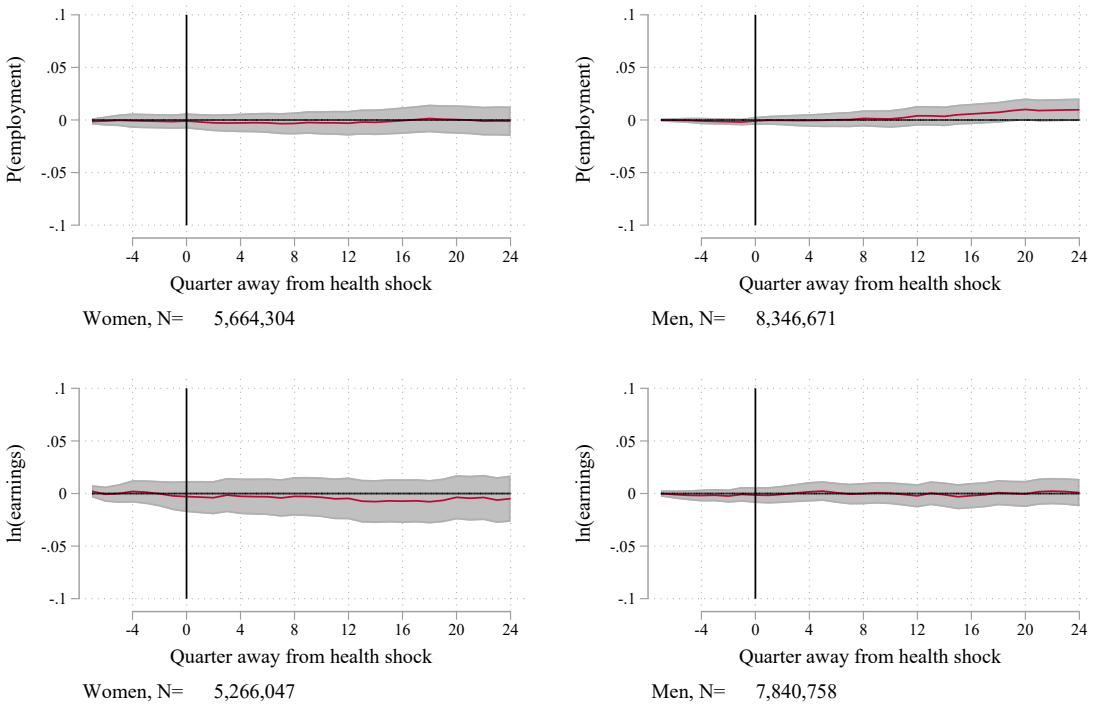
Note: * StdDiff $>$ 0.25 (Imbens and Wooldridge, 2009). Standardised difference one period before the parental health shock StdDiff=$\frac{\bar{X}_{C,-1}-\bar{X}_{T,-1}}{(\hat{\sigma}^2_{C,-1}+\hat{\sigma}^2_{T,-1})^{0.5}}$ where $\bar{X}_{C,-1}$ corresponds to the mean of variable X of the control group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

Figure A.4.1: Parents living in a 5km radius



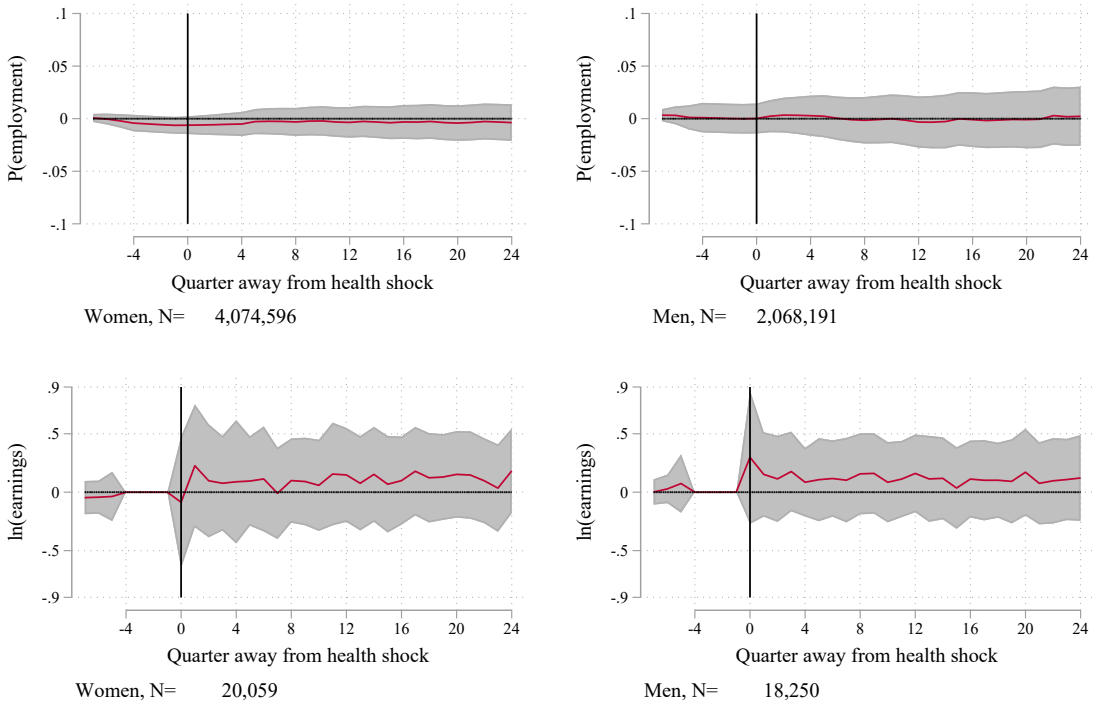
Note: The grey shaded areas correspond to the Bonferroni adjusted 95% confidence intervales.

Figure A.4.2: Employed 1 year before the shock



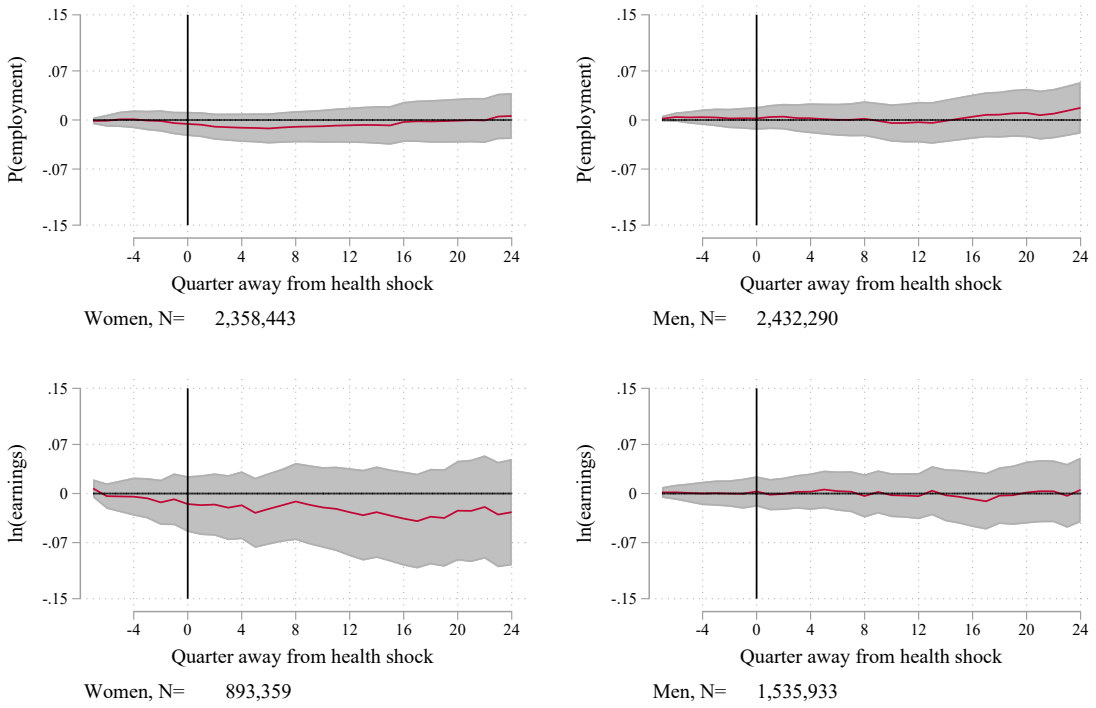
Note: The grey shaded areas correspond to the Bonferroni adjusted 95% confidence intervales.

Figure A.4.3: Not employed 1 year before the shock



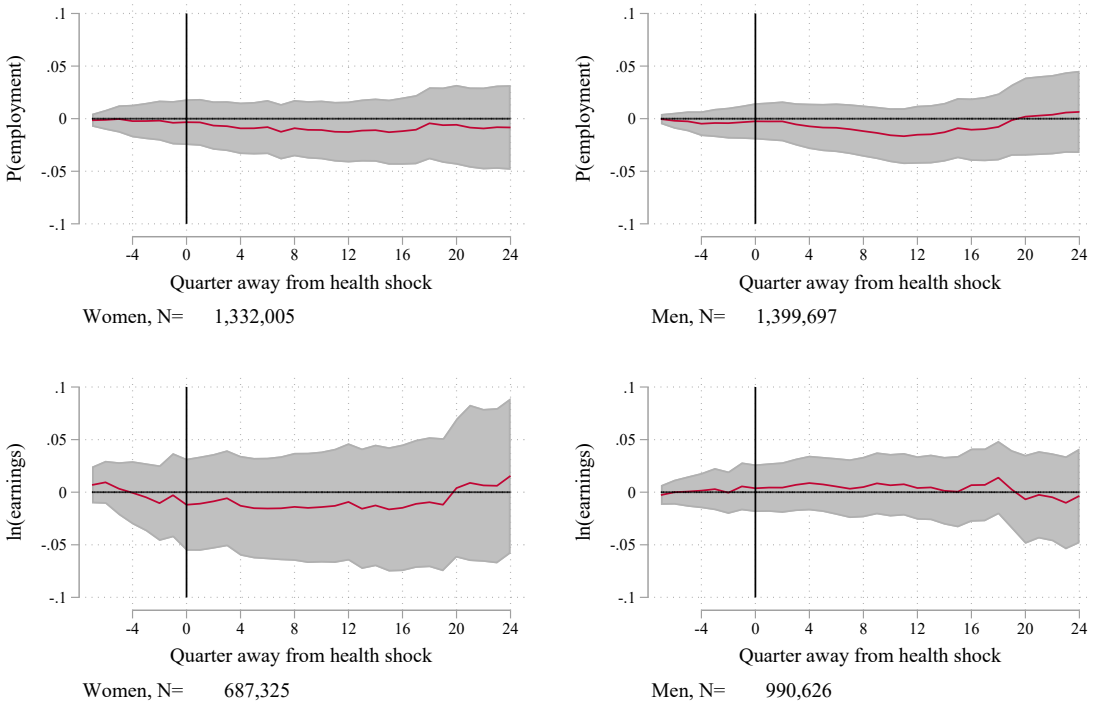
Note: The grey shaded areas correspond to the Bonferroni adjusted 95% confidence intervales.

Figure A.4.4: Parents aged 80+



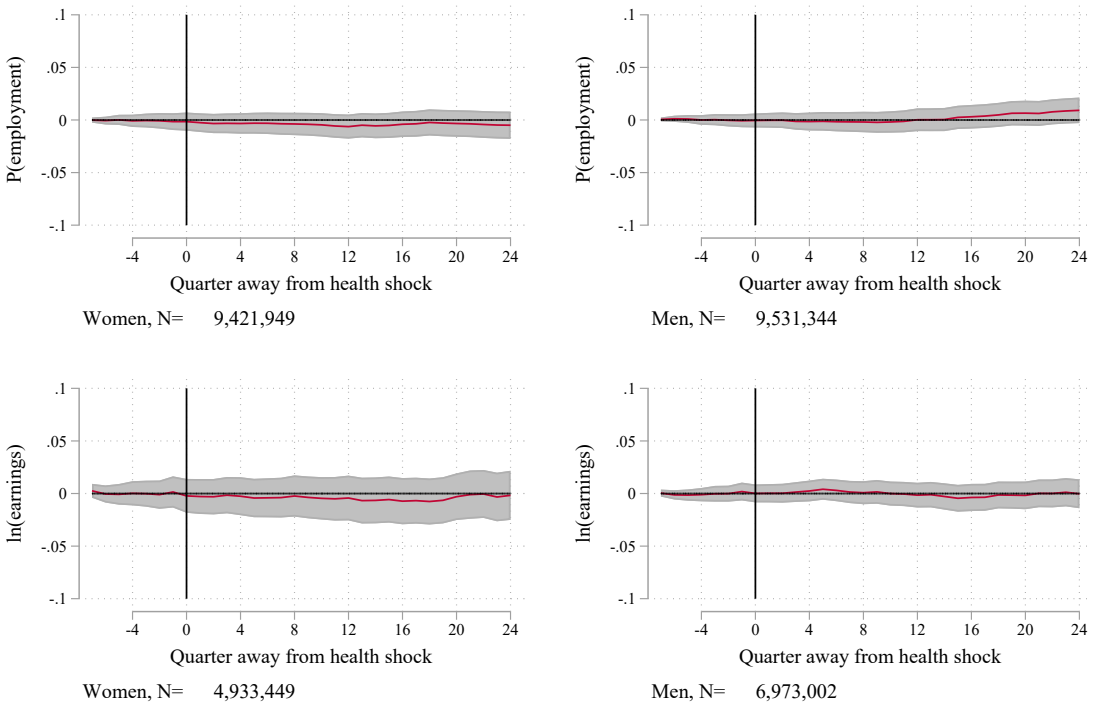
Note: The grey shaded areas correspond to the Bonferroni adjusted 95% confidence intervales.

Figure A.4.5: Only children



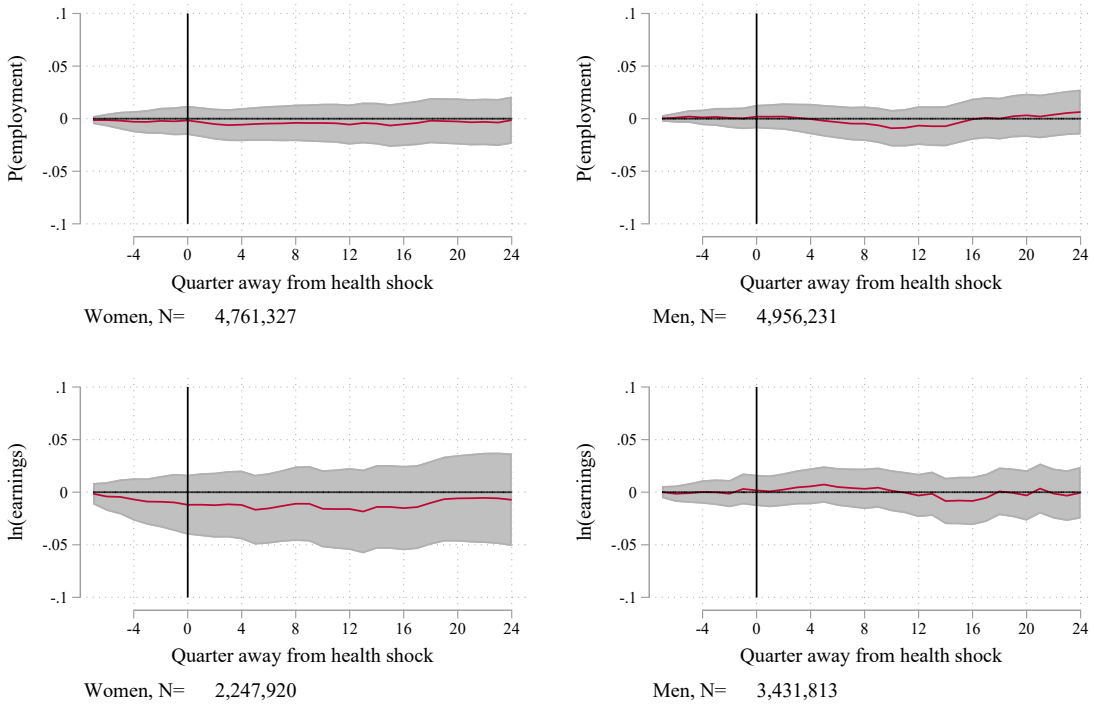
Note: The grey shaded areas correspond to the Bonferroni adjusted 95% confidence intervales.

Figure A.4.6: Alone living children



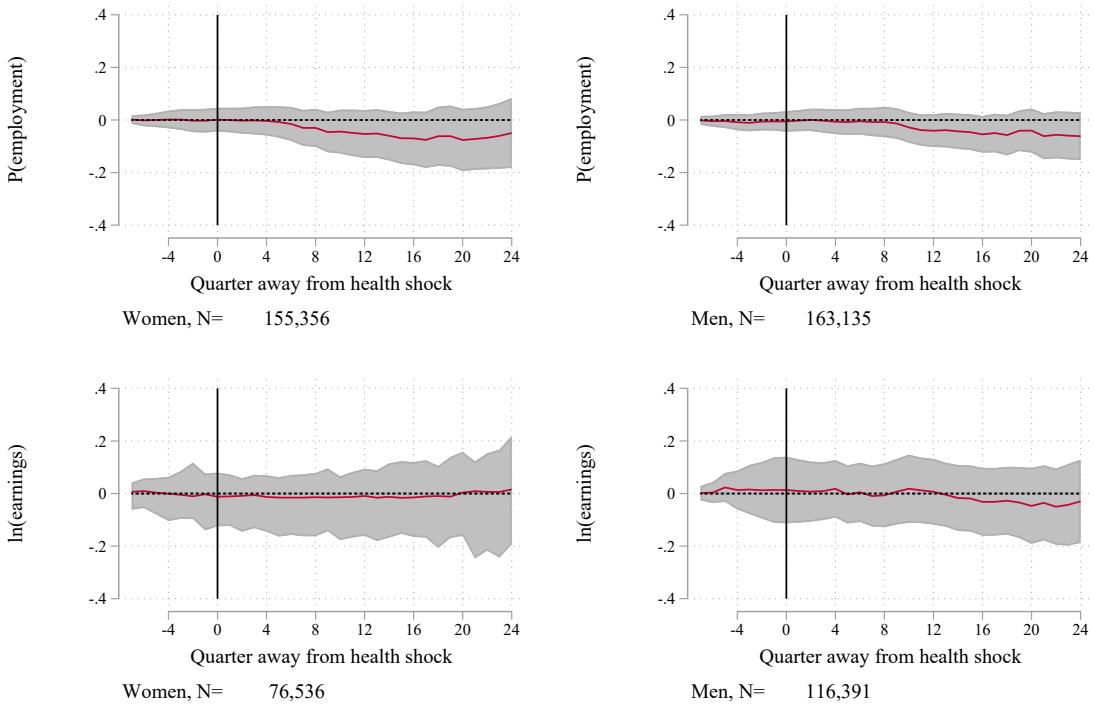
Note: The grey shaded areas correspond to the Bonferroni adjusted 95% confidence intervales.

Figure A.4.7: Alone living parents



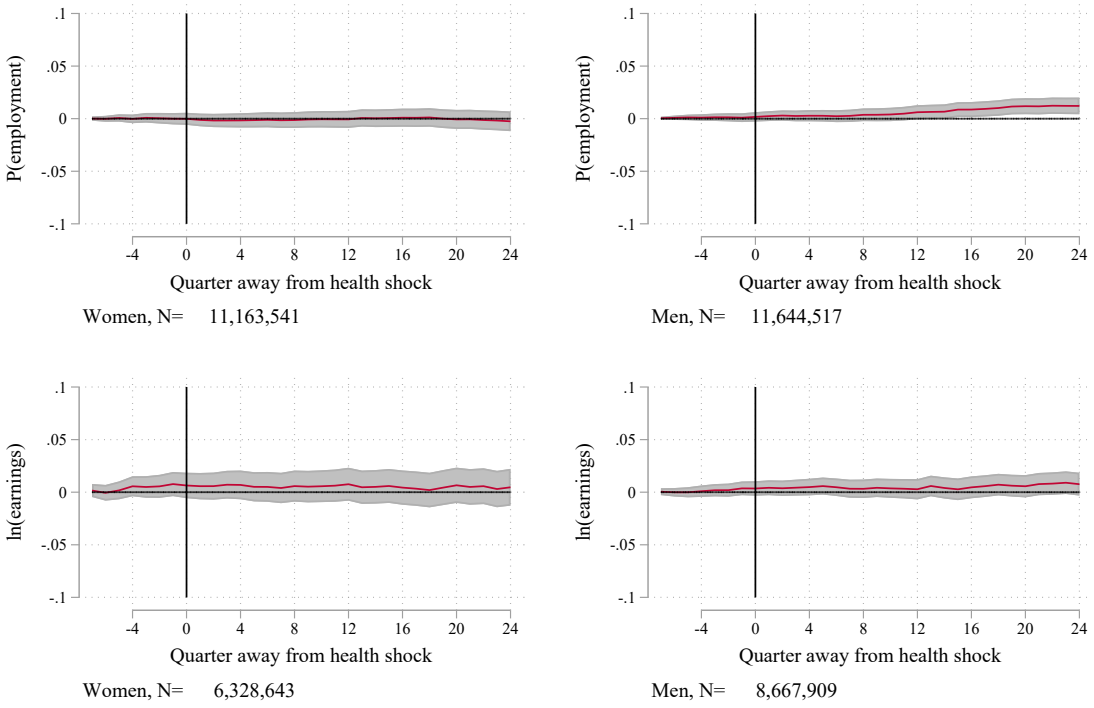
Note: The grey shaded areas correspond to the Bonferroni adjusted 95% confidence intervales.

Figure A.4.8: Only children with single close living parent



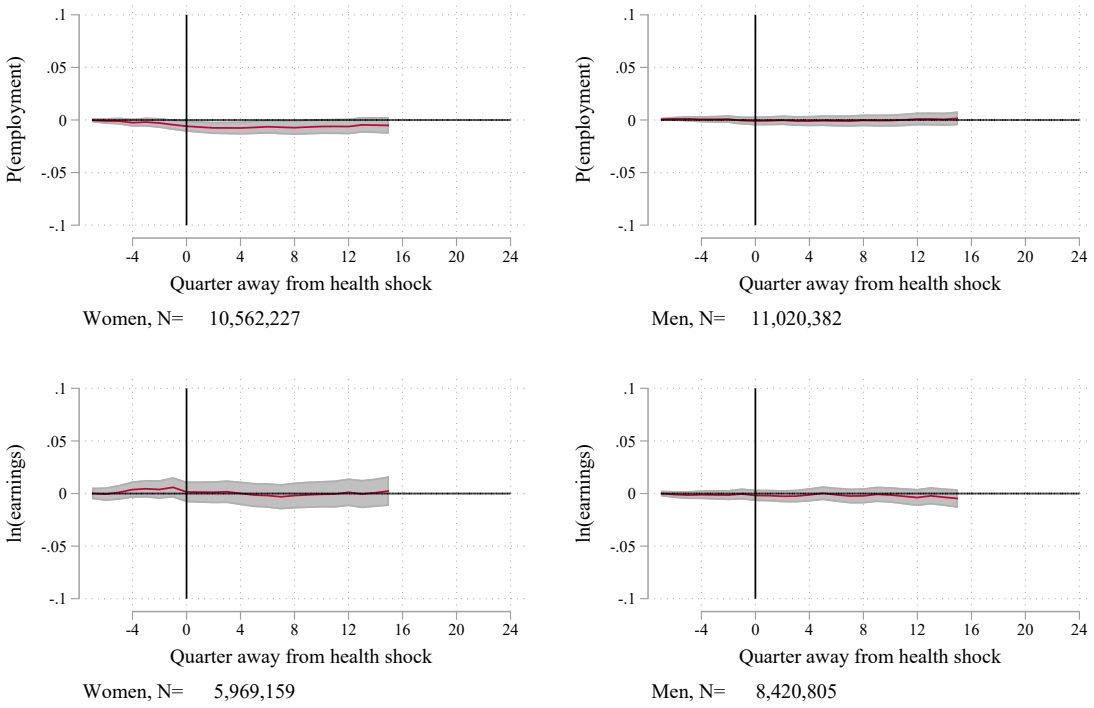
Note: The grey shaded areas correspond to the Bonferroni adjusted 95% confidence intervales.

Figure A.4.9: Unweighted



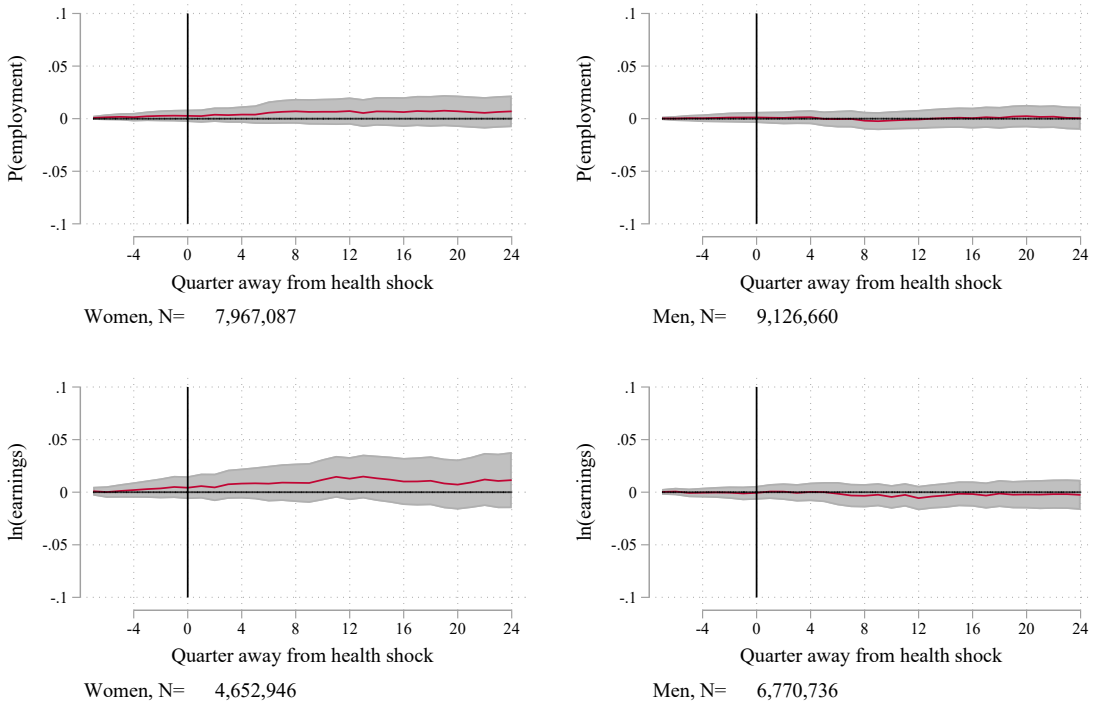
Note: The grey shaded areas correspond to the Bonferroni adjusted 95% confidence intervales.

Figure A.4.10: Control group with future health shock



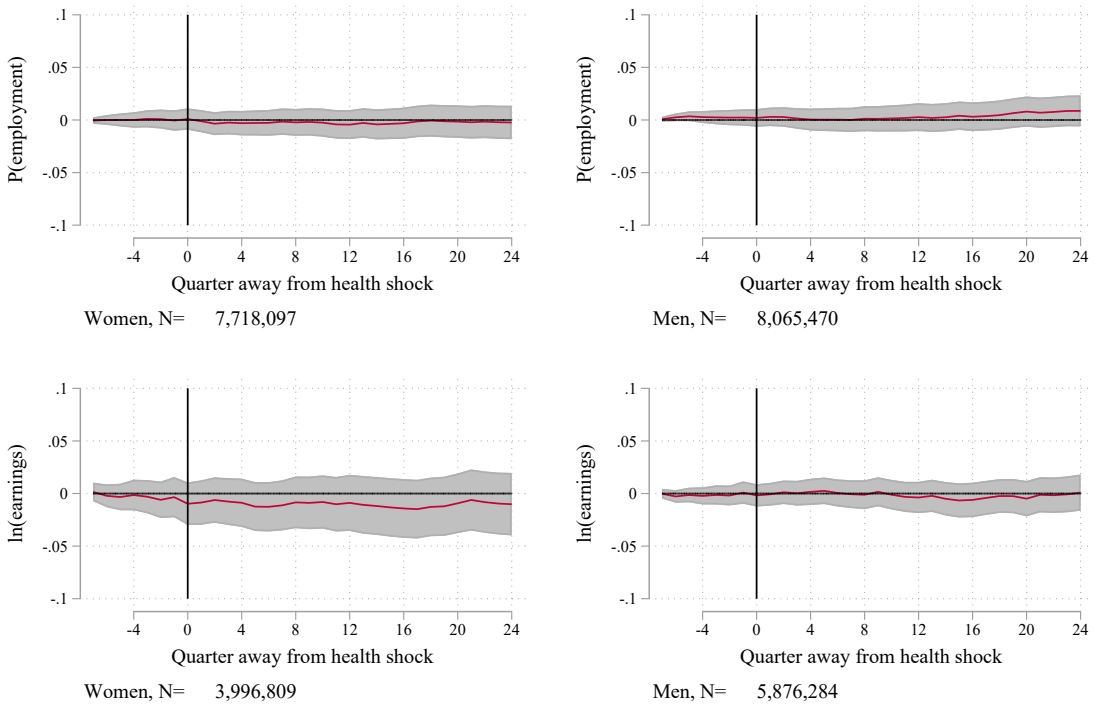
Note: The grey shaded areas correspond to the Bonferroni adjusted 95% confidence intervales.

Figure A.4.11: Shift treatment period to 2004q3 - 2004q4



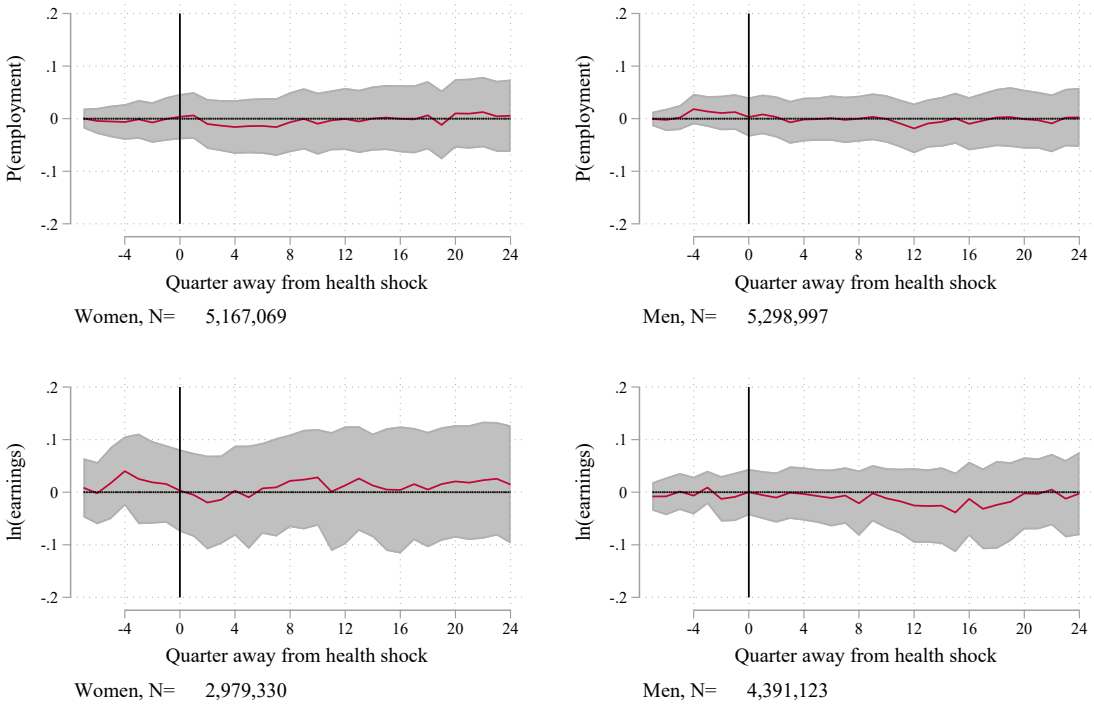
Note: The grey shaded areas correspond to the Bonferroni adjusted 95% confidence intervales.

Figure A.4.12: Severe health shocks



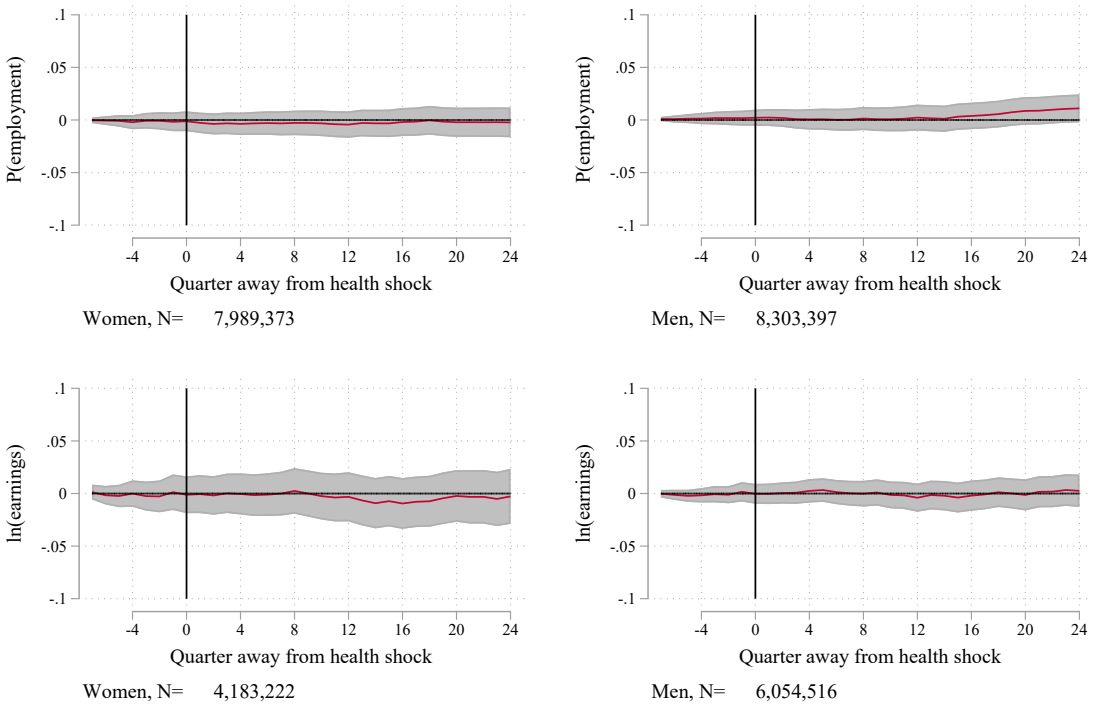
Note: The grey shaded areas correspond to the Bonferroni adjusted 95% confidence intervales.

Figure A.4.13: Nondeferrable health shocks



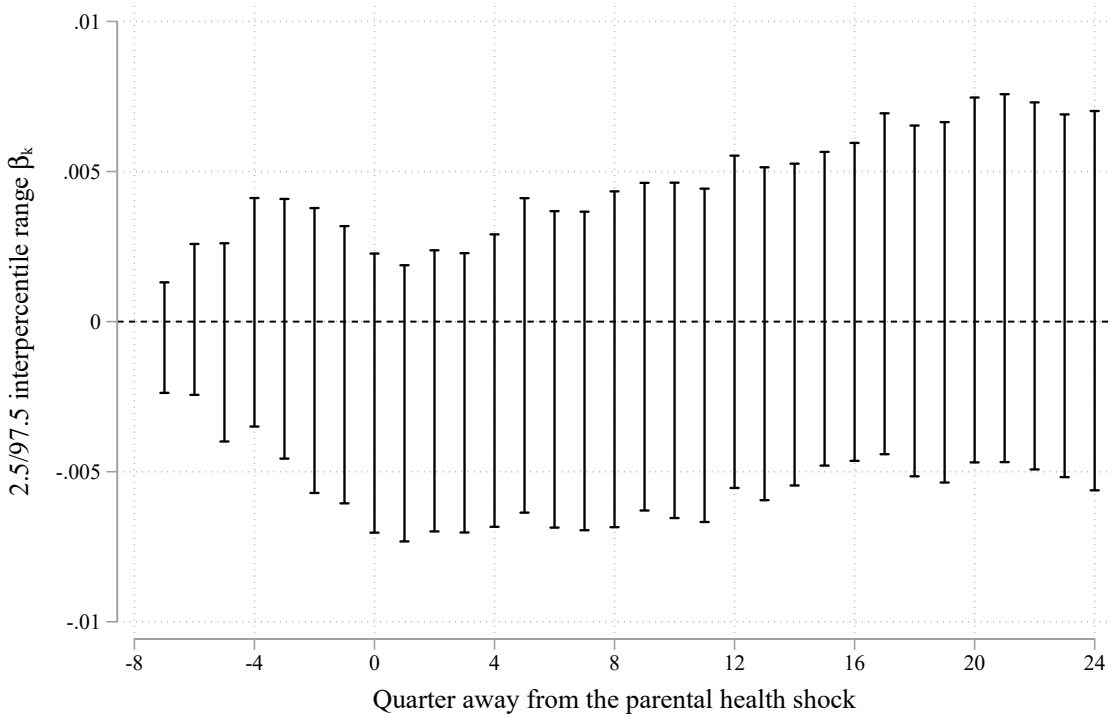
Note: The grey shaded areas correspond to the Bonferroni adjusted 95% confidence intervales.

Figure A.4.14: Drop *all* parental hospitalisations between 1995q1-2000q4



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

Figure A.4.15: 2.5 to 97.5 interpercentile range of β_k from 100 different random control group samples (Women employment)



Chapter 5

Conclusion

In this thesis, I examine potential labour market effects of engaging in reproductive work necessary at different stages in life. Chapter 2 focuses on the period before birth and analyse labour market and mental health consequences of an early pregnancy loss. In Chapter 3, dedicated to the period after birth, I show labour market consequences of child rearing. Finally, Chapter 4 examines labour market effects of a parental health shock in the context of informal elderly care. Since reproductive work has traditionally been a women's task, I assess whether women's labour market outcomes are disproportionately affected by reproductive work in the Netherlands of the twenty-first century, when the welfare state is well developed. The results of the three chapters show that labour market costs vary considerably depending on the type of reproductive work that is necessary at different stages in life. When child rearing is concerned, reproductive work is not shared equally.

In the second chapter, where we examine the beginning in the reproductive cycle, we show that an early pregnancy loss leads to an increase in mental health care use for both women and their partners. In contrast, labour market outcomes are not affected once subsequent fertility is accounted for. This illustrates that before children are born, labour market costs of reproductive work are small. However, dealing with a loss of an unborn child leads to emotional strain for both partners.

Chapter 3 discusses the period after a child is born, when a lot of care and hence reproductive work is needed. This care need competes directly with the labour market, and women consequently reduce their labour market involvement considerably for at least 8 years after the first child is born, despite the availability of child care that could potentially offset part of

the negative labour market consequences. Men only incur small labour market declines after child birth that cannot be attributed to the child with certainty. This means that for the period when children grow up, the burden of reproductive work is not shared equally by gender. Other literature has ruled out that this finding has biological origins (see Section 3.6 for a detailed discussion), and the thesis rules out that different earnings potentials of men and women explain this finding. The most convincing explanation of this finding are therefore gender norms. Consequently, the second part of Chapter 3 is dedicated to show how gender norms can contribute to the unequal division of labour market consequences by gender when it comes to reproductive work related to child rearing.

Once children have grown up, the dependency relationship between parents and children is weakened, and finally when parents get older and become sick, it may be completely reversed, such that parents depend on reproductive work of their children in the form of informal elderly care. In Chapter 4, we examine whether a health shock of an elderly parent leads to a labour market reaction of the middle aged child in order to provide informal elderly care. We do not find such a labour market adjustments of middle aged children, neither in the full population nor in subgroups with a high likelihood to become caregivers. We hypothesise that this result is linked to the comprehensive long-term care system in the Netherlands on the one hand, and the high prevalence of part-time work on the other hand. These explanations have two implications. First, the welfare state can have a protective effect for negative labour market consequences from reproductive work. Second, the high prevalence of part-time work is also linked to the results of the previous chapter. Women are more likely to work part-time, and they are more likely to work less hours than men partly due to child rearing tasks. At the same time, descriptive statistics show that women are more likely than men to care for parents (in law). So even though we do not find different labour market effects for middle-aged men and women after a parental health shock, the different labour market trajectories of men and women when they become parents may result in women's higher probability to provide informal care for parents, without adjusting their current (already lower) labour market involvement.

The findings of this thesis suggest thus that reproductive work can have important labour market consequences and that these effects depend on the type of reproductive work. While labour market consequences are negligible after an early pregnancy loss or a parental health shock, they are substantial after child birth. The welfare state is thus protecting workers well

in the case of short-term sickness after an early pregnancy loss, or by providing elderly care permitting the middle-aged children to keep their labour market involvement constant. In contrast, women incur large labour market costs once a child is born, even though child care is accessible for everyone and women are on average better educated than men. Households decide to divide their time between paid work, child care and home production in a way that cannot be explained by economic motives or government intervention alone, but is at least in part motivated by deeply rooted cultural norms. Research on how cultural norms are transmitted between generations suggests that family policies can only change gendered norms related to parenthood in the long-run.[99] Before these norms have changed, it is likely that parental leave uptake by fathers remains low. Another potential policy to influence norms related to parenthood may target schools or child care, since (gender) norms are learned at an early age (Bian et al., 2017). While this may be a realistic policy for other countries, it is not politically feasible in the Netherlands because all type of schools are guaranteed funding by the Dutch constitution, which is the result of decade long political debates between religious and other societal groups.

The current division of labour may also make having children unattractive mainly for women (but potentially also for men if they cannot spend as much time with their children as they would like to), and contribute to declining fertility rates, which are well below the replacement rate of 2.1 children per woman in Europe. This may be a concern for the sustainability of the welfare state, as for example pay-as-you go pensions systems use the contributions of the current workforce to finance the pensions of the current elderly population. While this concern is valid, it does not take into account that resources are limited. If fertility rates in the developed world increased to 2.1 children per woman, the global population would increase faster, and hence resources would become scarce at a faster pace. From that perspective, declining fertility rates in the developed world may thus slow down the speed at which resources become scarce.

In the middle of the corona crisis, the topics of this thesis have gained relevance. The outbreak of Covid19 and the associated containment policies have highlighted the central importance of reproductive work. With closure of child care facilities and schools in consequence of lockdown policies, child care work for parents increased substantially. Moreover,

[99] See Chapter 3 or Farré and Vella (2013); Olivetti et al. (2020); Fernández et al. (2004); McGinn et al. (2019)

a lower supply of formal home care in the Netherlands resulted in more work for informal care givers. This brought attention to the importance of schools, child care facilities and home care on the one hand, and the opportunity costs of engaging in care activities on the other hand, since working in the home office while home schooling/taking care of your parents is not possible. Consequently, the corona crisis entailed a discussion on the importance and costs of care work. In a broad sense, this thesis contributes to this discussion by analysing the labour market costs of engaging in reproductive work.

Summary

In this thesis, I examine potential labour market effects of creating and maintaining a family at different stages in life. Chapter 2 focuses on the period before birth and analyses labour market and mental health consequences of an early pregnancy loss. In Chapter 3, dedicated to the period after birth, I show labour market consequences of child rearing. Finally, Chapter 4 examines labour market effects of a parental health shock in the context of informal elderly care by adult children. Since maintaining a family has traditionally been a women's task, I assess whether women's labour market outcomes are disproportionately affected by work related to the family in the Netherlands of the twenty-first century, when the welfare state is well developed. The results of the three chapters show that labour market costs vary considerably depending on the type of work that is necessary at different stages in life. Women are more involved in child rearing than men, whereas women and men are equally affected by an early pregnancy loss or an increase in demand for informal elderly care.

Nederlandse Samenvatting

(Summary in Dutch)

In dit proefschrift onderzoek ik potentiele arbeidsmarkt effecten van het stichten en onderhouden van een gezin in verschillende levensfasen. Hoofdstuk 2 focust op de periode voor de geboorte van het eerste kind en analyseert de gevolgen van een vroeg zwangerschapsverlies op arbeidsmarktparticipatie en mentale gezondheid. In hoofdstuk 3, gewijd aan de tijd na de bevalling, laat ik zien wat de arbeidsmarkt consequenties zijn van het opvoeden van kinderen. Hoofdstuk 4 onderzoekt de arbeidsmarkteffecten van een gezondheidsschok van de ouder voor volwassen kinderen die mogelijk informele zorg moeten verlenen. Omdat een familie onderhouden traditioneel de rol van de vrouw was, onderzoek ik of de arbeidsmarkteffecten van het onderhouden van een familie voor vrouwen groter zijn dan voor mannen in de genereuze welvaartsstaat die Nederland in de 21e eeuw is. De resultaten van de drie hoofdstukken laten zien dat arbeidsmarkteffecten van het stichten en onderhouden van een gezin verschillen afhankelijk van het type werk dat noodzakelijk is per levensfase. Vrouwen zijn meer betrokken bij het opvoeden van kinderen dan mannen, terwijl mannen en vrouwen in gelijke maten getroffen worden door een vroeg zwangerschapsverlies of een verhoogde vraag naar informele ouderen zorg.

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