Consequences of Childhood Cancer on Parental Labour Market Outcomes and Family Labour Division

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Abstract

This article focuses on estimating the effects of childhood cancer on parental labour market outcomes, especially on the earnings gap between genders. Estimating the causal connection between health and socioeconomic variables is difficult for many reasons. In this article, I use rich administrative data and event study methodology to estimate the effect of the diagnosis on family outcomes.

The results show that childhood cancer reduces parents' income significantly. In short run, the effect is around 30% of the income before the diagnosis for mothers and around 7% for fathers. The gender earnings difference increases around 20% in the short run, and increases also in families where mother is the main provider in the years before the diagnosis. My results are robust to different checks, including alternative specifications and estimators to correct for possible cohort-heterogeneous effects.

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1 Introduction

Childhood cancer is one of the leading causes of death for children in high-income countries (World Health Organization 2021) and a serious health shock with long term effects on health (Kenborg et al. 2019, Norsker et al. 2020) and other aspects of life (Carlsen et al. 2008) even for those surviving. Since the extent of the effects on children is large, it is also possible that, like the proverb suggests, the illness has so-called spillover effects on other family members. This article focuses on quantifying the effects on parental labour market outcomes, especially the earnings gap between genders.

Evidence from existing research is mixed. Both Costa-Ramon (2020) and Breivik (2020) provide causal estimates of the effects of all serious childhood illnesses on parental labour market outcomes, but not specifically the earnings gap. Both find adverse effects, somewhat stronger on mothers than on fathers. Öhman et al. (2021) finds that the impact of childhood cancers on earnings in the short run is equal for both sexes and stronger for fathers than mothers in the long run.

In literature (i.e. Schmitz & Westphal (2017), three different channels for these spillover effects can be found: 1) the changes in behavior caused by stress and worrying seeing someone close sick and 2) the effect caused by sick family member requiring more care than before 3) the effect caused by loss of income and increased medical expenses in the household. The extensive public healthcare in Finland likely reduces the effect of the increased expenses, while especially young children have trouble caring for themselves if they can not attend school and are likely to require additional care. A clear hypothesis from the simplest economic model would be that if the burden of childcare is increased so that one family member has to miss out on work, the family would choose the parent who has lower wage and lower disutility from staying at home. Using data on earnings before the health shock, I am able to some extent asses whether this really happens.

Health and economic outcomes like wage earnings and socioeconomic status have

a well-documented and complicated connection (Condliffe & Link 2008). Difficult situations in many aspects of life have negative effects on health, but health shocks also seem to have an impact on socioeconomic outcomes (Smith 2004). This two-way causality causes difficulties for estimating the magnitude of these effects.

In this article, a quasi-experimental strategy known as 'staggered differences-in-differences' is employed to identify the causal effects of a health shock. My data includes all healthcare visits and background information for the whole population for years 1998-2017. Apart from implementing the two-way fixed effects regression often used in the literature, I also implement tools that have shown to be unbiased in the presence of time-heterogeneous treatment effects.

The two-way causality is in a way also what makes this kind of problems so important and relevant for economics literature. Health shocks can have strong impacts on socioeconomic outcomes. Scientific knowledge of these effects is necessary for understanding the magnitude and quality of these effects and crafting policies that are cost-effective and support those in need. Some measures, like psychological consultations to the parents of the childhood cancer patients, are already part of the medical process. Spillover effects can affect the cost-benefit analysis of different policy measures and treatments (Fletcher & Marksteiner 2017), so research concerning the level and variability of the spillovers should be an area of interest for decision-makers too.

Motivated by this literature, my main research question is **"What kind of effect does childhood cancer have on parents' labour market outcomes, especially on the earnings gap between genders?"** This question includes many different dimensions: How large and long-lasting are the effects? Are the effects different for different groups, especially different types of cancers, different education levels and different divisions of labour before the shock?

In this article I find that the effects on earnings and earnings gap are large (around 30% of pre-shock earnings for mothers and 7% for fathers) and robust

to different robustness checks. More serious cancers and fatal shocks have larger effects, while educational attainment does not seem to be very strongly linked to the magnitude of the effects. No evidence is found that the cancer penalty is smaller in the families where mother is the main provider before the shock.

This article contributes to earlier research in many ways. The existing research on the causal childhood illness spillover effects, especially using broad administrative data with proper causal identification, is scarce. Beyond scientific advances, research like this provides valuable information for policymakers on the effects of an extreme shock that can have long-lasting effects on the whole family. As the magnitude of the effects and mechanisms at play are understood more clearly, it is also possible to better support these families through social insurance policies.

In the next section, I present the most relevant literature on the subject. Section 3 presents the data, institutional background and provides an in-depth view of the event study -method. In Section 4 I present my results in the form of figures and tables and Section 5 provides some additional checks. Finally, Section 6 finishes with conclusions and directions for future research.

2 Literature review

The empirical literature on childhood cancers is plentiful and different fields have somewhat different approaches to the topic. In this section I summarize most relevant empirical papers that have either tried to estimate the effects on parental labour market outcomes or provide important background information in some other way.

Many of these papers suffer from limitations set by small sample size and unavailability of a suitable control group or both, and the results therefore lack causal interpretation. Nonetheless, I have reviewed some of the literature, since understanding the nature of the childhood cancers is also necessary for discussion on the concerns regarding the assumptions of the identification strategy, analyzing the

strengths and weaknesses of previous literature highlights the contributions of this article, and qualitative evidence has potential to guide research focused on causal effects.

There are three recent articles with a focus on parental effects of all severe illnesses or cancers among children. Using Finnish data, Costa-Ramon (2020) considers all children with hospitalizations longer than 4 days. She finds that earnings are significantly lower for both parents in the years following the hospitalization and probability of working is reduced. The effects are more profound on mothers than on fathers and seem to be driven by severe hospitalizations or hospitalizations leading to the death of the child, and mental health is argued to be an important mediator especially for fathers. Breivik (2020) finds similar results with Norwegian administrative data. The effects are gendered and persistent on both the intensive and extensive margin. There are also short run effects on mental health.

Third, recent article by Öhman et al. (2021) also use administrative data and dynamic two-way fixed effects specification and focus only on childhood cancers, but their results stand in contrast to i.e. Costa-Ramon (2020). They find large initial effects on earnings, but in later periods, the effect of fathers is remarkably large and the effect on mothers is actually positive. These are interpreted as a result of support given to mothers during the treatment process of the cancer by the hospital and other communities. They find that employment somewhat declines in both genders and mental health effects are insignificant. A possible reason to these differences is the regression specification, which is slightly different from the one used in this article or the ones used by either of Costa-Ramon (2020) or Breivik (2020). I discuss alternative results using this specification in Section 4.1 and discuss the role of and implications of these differences as well as other potential explanations for the contrasting results in Section 6.

In contrast to these studies, Syse et al. (2011) compared affected parents from Norway to observably similar parents from the general population and did not find

significant effects of childhood cancers on employment, although central nervous system tumours, germinal cell tumors and especially some types of leukaemia were reported to have significant effects on earnings, which is in line with Breivik (2020).

The literature focusing on gender-specific effects of childhood illnesses is somewhat scarce. Both Costa-Ramon (2020) and Breivik (2020) provide estimates separately for mothers and fathers as do Öhman et al. (2021), but none of these articles assess the changes the gap in earnings or significance of the differences in more detail. A somewhat similar albeit much happier event is the birth of a child, which also affects the labour market outcomes of the family. So called "child penalty" research, using similar methods and administrative data as mine, has in recent years provided new insights to this matter.

The term "child penalty" is best known from literature following Kleven, Landais & Søgaard (2019), who estimate the extent mothers fall behind fathers due to the birth of children. Multiple studies (Kleven, Landais & Søgaard 2019, Kleven, Landais, Posch, Steinhauer & Zweimüller 2019, Sieppi & Pehkonen 2019) in different countries have found the long-run child penalty (on gross earnings) to be around 20-25% (for short run, around 60 %) of the predicted earnings and report also that this penalty occurs together with changes in labour market participation, hours worked, employment sectors and occupation Kleven, Landais & Søgaard (2019). They also find that women who grow up with the father as the main breadwinner in the family incur larger child penalties, but paternal family characteristics seem to have no impact. However, results concerning the pre-shock family labour division are scarce in this literature.

Most of these papers utilize an event study approach similar to this article. The methodology behind these estimates is presented in Section 3.4.3. A possible difference between the shocks is also that having children is more often a planned decision, which could cause women to invest less in their education and career in response to expected motherhood Kleven, Landais & Søgaard (2019) or work more, since

the maternity benefits are based on these earnings. The possibility of anticipation effects is then more realistic than in the case of cancer diagnosis.

Previous research has considered spillovers between other family members that might act as caregivers when other family member falls ill. Having to care for a close relative also has effects on the general well-being and labour market outcomes. Having to take care of one's relative exerts a psychological toll, but also means missing on work affecting labour market outcomes this way. This complex nature of the caregiving effect and possibility of long-lasting effects is noted for instance by Schmitz & Westphal (2017). Using administrative data, Fadlon & Nielsen (2020) find that a fatal shock facing a spouse often increases the survivors' labour supply in Denmark, while Jeon & Pohl (2017) find adverse effects on spouses' labour market outcomes caused by cancer.

3 Data and methods

In this section I outline the institutional setting and properties of the data used in the article. After that, I provide an in-depth review of the staggered differences-in-differences framework and the assumptions that need to be satisfied in order to identify causal effects using two-way fixed effects regression. The method allows for examination of the critical parallel trends assumption, estimation of dynamic effects and flexible combination of groups treated in different times.

Recently, the problems of estimating differences-in-differences setup using two-way fixed effects with differing treatment timing have gained attention. In line with this recent literature, this article examines some of the techniques presented in recent literature that allow for more robust inference from staggered event study designs, namely those introduced in Callaway & Sant'Anna (2020), Sun & Abraham (2020) and Goodman-Bacon (2018). So far, not many published applications of these methods exist. Still, the evidence (Baker et al. 2021) indicates using these

methods can alert us of these problems and fixing them can change the results of a study entirely. As Baker et al. (2021) notes, econometrics literature has yet to settle on an approach best suited to tackle these problems.

3.1 Institutional Background

Finland has a comprehensive welfare state and public healthcare. Virtually all childhood cancer cases are treated with minor direct costs to families. Families with children can receive different benefits and childcare is inexpensive and free for the poorest families.

The treatment of childhood cancers is centralized in five university hospitals (Helsinki, Tampere, Turku, Oulu, and Kuopio). The private sector plays some role in the health care system, but it is rather small. Private health care users can apply for reimbursement of the costs from the National Health Insurance, which covers about 30% of the expenses. There is also a ceiling for public medical expenses. In 2020 this was around 600 euros and there is a similar ceiling for medical drug expenses.

Parents who take time off from work can apply for the Special care allowance, paid by the National Pension Institute. Parents whose child experiences "severe" illness are eligible for the allowance which lasts for maximum of 60 days and is based on the earnings of the previous year, although the share imbursed decreases when earnings increase over thresholds in 26 700 and 41 100 euros. The benefit is only available to parents when the child is less than 16 years old. (Health Insurance Act 1224/2004 Chapter 11 §1 2004).

Parents with children of any age can apply for Support for Informal Care which is paid by municipalities with amounts that are varying. This benefit is not limited by age. To receive this benefit, a high care of burden is required, especially after the child is more than 18 years old. For families where a child suffers from disability or chronic illness, the Disability Allowance is also available.

3.2 Data

The data comprises of two major sources. The healthcare visit data are provided by Finnish Care register for Health Care (HILMO), maintained by Finnish Institute for Health and Welfare. The other main data source is the FOLK-module maintained by Statistics Finland, which includes yearly economic and labour market data and other background statistics for the entire population of Finland. These registers can be combined using pseudonymized ID codes. The storage and analysis of data was done securely in the remote environment provided by Statistics Finland.

The HILMO registry includes all outpatient (meaning patients who do not stay in the hospital overnight) visits from 1998-2017. Data from inpatient visits is available for a longer time, but I only utilize the years for which both are available, since there are some cases also included in outpatient visits. Cases from year 1998 are omitted since this year is used to check for pre-existing cancer diagnoses.

The data includes the time, length and reason of the visit and information about the background of the patient. The diagnosis is coded using ICD-10 codes. The categorization of cancers and associated ICD-10 (International Classification of Diseases) codes can be found the Appendix E. I also utilize the Cause of Death Registry, which contains all primary, secondary and tertiary causes of death for the whole research period.

The economic and labour market data are combined from a few sources. The main one is the FOLK-module, which includes yearly data for earnings and other income, education, socioeconomic status and other background variables for all individuals. In addition, I use data from the Finnish Birth Registry which includes some health information at the moment of birth.

3.2.1 Analysis sample

Overall, 3503 under 17-year olds are diagnosed with cancer during the research period. The age limit is motivated by the age limit of the Special Care Allowance,

the wish to focus on children who most probably are living with their parents as well as the differences in the distribution of the cancers that are encountered in children aged 16-20 years old.

I also drop families where information on both parents is missing. I then limit the sample to the families that are observed for all 23 years between 1994-2017. The final sample then includes 3130 families. Table 1 includes some descriptive statistics of the sample.

During the five-year follow-up period, 453 (11%) of these patients died. For most of them, the primary reason for death was cancer. As there are often effects on parents also after the death of the child, I do not limit the sample based on this, but conduct analysis separately for the families where child dies and where the child survives. It could be assumed that the mental health effects on parents would be larger when the child dies (Pohlkamp et al. 2019), but it is also assumable that in these cases the cancer is also relatively serious and has effects as such.

I define the first diagnosis as the first observation in the HILMO-data which includes a diagnosis of some cancer. In this analysis, additional diagnoses of different kind of cancers are not considered and cancer is categorized based on the ICD10-code of the first diagnosis. I do not estimate the effects of the length or numbers of visits although this would be an interesting extension.

The dataset used includes yearly data on activity, benefits, income and other economics variables. I define employment based on the variable indicating the main activity during the year. This is used as an outcome variable. I classify education using the highest degree person has achieved and is considered at 3 levels: primary education (9 years) secondary education (high school or vocational degree, 12 years) and tertiary education (university and polytechnic degrees).

I also consider the possibility of health differences between big cities and other parts of the country: the indicator for living in a big city is given value 1 if a person is living in one of Helsinki, Espoo, Vantaa, Kauniainen, Turku, Tampere or Oulu.

Table 1: Descriptive statistics of the sample (one year before diagnosis).

Income (€)	
Mother labour earnings, median	18639.04
Father labour earnings, median	34691.38
Mother income in taxation	23060.29
Father income in taxation	36078.05
Mother household disposable income, median	18519.54
Father household disposable income, median	19502.54
Education (% of sample)	
Mother primary	17
Mother secondary	62
Mother tertiary	19
Father primary	23
Father secondary	57
Father tertiary	20
Family (% of sample)	
Mother employed	73
Father employed	87
Mother lives with the child	98
Father lives with the child	83
Child dies within 5 years of diagnosis	11
N	3130

Notes: Descriptive statistics of the analysis sample. The sample consists of all child cancers diagnosed in Finland between 1999-2017 at ages 0-16.

These are the biggest cities in Finland with some margin and the population has arguably different characteristics than other parts of the country.

Other control variables that could be included in some specifications are indicators whether the parent lives with the child, whether the parents live together, whether the mother is employed in public/private sector and information regarding other children the parents might have or children that are born. However, care is needed in including too many control variables that, even if measured before the diagnosis, could be an outcome of bad health (Angrist & Pischke 2008). In this article, I run the regressions first without controls and provide some results using control variables as a robustness check.

3.2.2 Outcome variables

The main outcome variables are the gross annual labour market earnings, taxable income and social benefits. There is also data of income after benefits and transfers which I will use to evaluate the effects of the social security system, which I call the

individual income. Since many benefits are household-based, some income measures are also included at the family level. The income measures are deflated to the 2015 level using Harmonized Index of Consumer Prices.

The data includes a variable measuring the main activity of each person, which can be used to estimate effects on employment. These data are available for most persons each year. This variable could also be used to estimate effects on other activities like studying etc., but in light of theory, employment probability seems to be the most interesting, as it implies a larger setback in labour market outcomes than just decreased earnings.

3.3 Childhood cancers

The etiology of childhood cancers is not well known. To some extent, the risk is genetic and might be hereditary. If childhood cancers are caused by environmental causes, these causes could then affect also the outcome variables biasing the estimates. In adults such confounders could plausibly exist, but since there is little evidence of such effects for childhood cancers, this supports the identification.

Table in Appendix F shows the amounts of different diagnoses in the sample. The types of cancers that are most common in young children are somewhat different than the ones most often found in older children. As Figure 10 shows, the most common cancers in young children are types of leukaemia, central nervous system cancers and bone cancers, while older children more commonly experience lymphomas, melanomas, sarcomas and cancers in the genital area.

The cancers also differ in intensity of symptoms and side effects of treatment. For central nervous system cancers, the treatments can have significant effects on the central nervous system (Tolkkinen et al. 2018). For leukaemia, like acute lymphoblastic leukaemia (ALL) or acute myeloic leukaemia (AML), the treatments are long and demanding (Tolkkinen et al. 2018). In contrast, lymphomas are mostly associated with lower mortality and also with less demanding treatments.

3.4 Econometric models

A fundamental problem in estimating causal effects in social sciences is the problem of confounding or selection bias. As the causes of childhood cancer are not entirely known, there exists potential for confounding factors affecting both the outcome and the selection to treatment. This then causes bias in the estimates known as selection bias. This concern is based on research showing associations between child health and family socioeconomic status. As often is the case in health-related questions, it is also possible that bad health itself also affects other outcomes, even before it is observed.

A common method to circumvent this problem in public health research and other fields is matching the treatment group with a control group from the population in hopes of capturing all the confounding factors that could bias the estimates. To the extent that the factors are observable, strategies like this work well, but if the confounding factors are not observable, the selection bias persists.

To solve this "fundamental problem of causal inference", as coined by Rubin (1974) and Holland (1986), I utilize a quasi-experimental method called staggered differences-in-differences with event study specification.

As in previous research like Druedahl & Martinello (2017), Costa-Ramon (2020) and Breivik (2020), I use the dynamic specification, also called event study specification. In practice, this can be applied by using regression with relative time dummies. Taking dynamic effects into account provides additional information on the length of the effect, since it can be assumed that the effect is largestjust after the health shock and then fades after some years. It also has additional benefits in the staggered treatment design.

In my specifications, I mainly use 5 years after the shock and 4 years before the shock. This allows for a long enough period to observe the pre-trends and provides information on longer term effects. This also seems like a standard choice in the literature (i.e. Costa-Ramon (2020), while some papers use more pre-treatment

periods or only 3 post-treatment periods.

Here I outline the basic event study design estimated in this article and the assumptions required for causal interpretation of the estimates.

In the event study design N units are observed over $T+1$ periods. To apply the design, it is necessary to observe Y_{it} i.e. the outcome variable, and treatment variable $D_{it} \in \{0,1\}$ for each period t. I denote unit being treated in period t by $D_{it} = 1$. Additionally, I wish to observe some control variables $\mathbf{X}_{it}$. Also I assume that the observation pairs $\{Y_{i,t}, D_{i,t}\}_{t=0}^{T}$ are i.i.d, independent and identically distributed.

In the dynamic specification, instead of simple treated -dummy we use the indicator showing the relative time to the treatment. In my notation, periods $l > 0$ are the periods after the treatment and periods $l < 0$ are periods before the treatment. In the simplest case the dynamic effects in event study design can be estimated using single regression which resembles the differences-in-differences regression:

$$Y_{i,t} = \alpha_i + \lambda_t + \phi_t + \psi_t + \sum_l \mu_l \mathbf{1}\{t - E_i = l\} + v_{i,t} \tag{1}$$

where the equation includes individual fixed effects α_i and time fixed effects $\lambda_t, \psi_t, \phi_t$ (child age, parent age, calendar year). This is the main regression I use in this article. The coefficients of interest are μ_l, coefficients for relative time indicators $\mathbf{1}\{t - E_i = l\}$. As observations from same persons are related to each other, the standard errors have to be clustered at the level of randomization, in this case the individual level Abadie et al. (2017).

As Borusyak & Jaravel (2017) show, if there are no never-treated units, it is necessary to bin the periods beyond certain points both before and after the treatment. Not doing this results in bias from these periods affecting the coefficients of interest. In addition, one period needs to be chosen as a reference period and other periods are compared to this. In line with literature, I choose the period $t-1$, i.e. one period before the diagnosis.

3.4.1 Identifying assumptions

In order to identify the causal effect $CATT$ (and ATT), three assumptions are needed (Sun & Abraham 2020):

1) Parallel trends. Intuitively, this means that all units in a cohort e and all cohorts used as a control, e_2, would have experienced the same outcome had there been no treatment, i.e. no cancer diagnosed on a child. Mathematically, $E[(Y_{i,t}^{\infty} - Y_{i,s}^{\infty}) \mid E_i = e] = E[(Y_{i,t}^{\infty} - Y_{i,s}^{\infty}) \mid E_i = e_2] = E[(Y_{i,t}^{\infty} - Y_{i,s}^{\infty})]$.

The parallel trends assumption is critical for the identification strategy Angrist & Pischke (2008). The whole causal interpretation is based on this assumption that the treated and control units would behave similarly in the absence of treatment. Problems can emerge for instance if researchers choose the sample in a way that the trends are parallel but do not adjust for this selection process (Rambachan & Roth 2019).

In simplest form, the plausibility of this assumption for the periods preceding the treatment can be assessed by graphically examining the trends, similarly as in traditional differences-in-differences design. Since in the event study a coefficient is estimated for each relative period, checking if the coefficients are statistically significant from zero provides information on whether there seem to be different trends in treatment- and control groups.

It is important to note that this assumption does not mean that the outcomes need to be at similar level before the treatment. The setting compares people that experience the shock at different years and ages, so the levels are likely to differ. However, as long as the units develop in parallel, this assumption is satisfied and causal conclusions can be drawn. This can be further ensured by using time-fixed effects.

2) No anticipation. This assumption requires that no one adjusts their behaviour to the treatment before it is observed, i.e. $E[(Y_{i,t}^{e} - Y_{i,t}^{\infty}) \mid E_i = e] = 0 \ \forall t < e$.

The no anticipation -assumption is somewhat straightforward in this setting, since children's cancers are very unpredictable. It is notable that the treatment is only observed when the disease is diagnosed at a hospital. However, children's cancers show relatively little symptoms that could have sizable effects, and are usually always treated. It seems unlikely that this kind of serious disease would affect a family for long before it was diagnosed. Since the outcome variables are included yearly, I probably see effects in different relative periods depending on the time of the year in which the diagnosis was made. The data includes the specific date of the first visit, which means that whether the diagnosis was made early or in the beginning of a year can be controlled for. Also it is important to note that if this assumption fails, it makes it impossible to use the standard checks to see if the parallel trends assumption holds.

3) Homogeneous treatment effects: for all relative periods l, $CATT_{e,l}$ is independent of cohort e.

This assumption is rather demanding. It means that for all cohorts, the treatment is of similar intensity and length, i.e. the profile of effects is similar. There should not be different intensity of treatment at any cohort, and problems with a single cohort can endanger the whole identification scheme. This is a potential problem as there are assumably advances in cancer treatments all the time, and also during the years considered in the analysis.

The assumption does not however consider heterogeneity within cohorts, as this does not cause similar bias to the estimates. This assumption and the consequences of dropping it is considered at length in section 3.4.2.

$$\mu_g = \sum_{l \in g} w_l^g ATT_l + \sum_{g' \neq g} \sum_{l' \in g} w_{l'}^g ATT_{l'} \sum_{l' \in g^{excl}} w_{l'}^g ATT_{l'}. \tag{2}$$

3.4.2 Heterogeneous treatment effects

Recent literature, i.e. Sun & Abraham (2020), Callaway & Sant'Anna (2020) has shed light on the detrimental impact of heterogeneity in treatment effects for event study estimates. In this sense, most important cases of heterogeneity are 1) the case where the treatment effects are different for different lags 2) the case where the treatment effects are different for different cohorts. The first case can be solved by allowing dynamic effects, as in equation (1), but bias from the second case can still persist and requires different methods.

Breivik (2020) mentions three clear possibilities for this kind of heterogeneity in a similar setting. One is selection to treatment at a particular time and differing treatment effects due to effects varying in calendar time. Another possible problem are calendar time-varying effects, since there can be differing macroeconomic conditions affecting families during different years. Also the childhood cancer mortality is constantly falling, driven by advances in treatment, which could set the cohorts into different position. Breivik (2020) suspects that child age undoubtedly affects both the labour market situation of the parents and the severity of the cancer (see Figure 10), and if the age composition of the cohorts is different, this is a cause for concern. Additionally, especially when considering the earnings gap, it should be noted that there has been significant increases in many countries in gender equality (Blau & Kahn 2017). The rising trend in the outcome variable is not a problem in itself, but if the profile of the treatment changes, Assumption 3 is violated. It can also be argued that the long time frame of the data makes this setting particularly prone to this kind of bias.

As these concerns seem like a realistic worry in this setting as well as other similar questions, I think it is valuable to carefully examine the assumptions made in the event study settings. In addition to providing robustness checks to this particular setting, the examination has potential to guide implementation of event study approach in other similar research questions in the future.

Solutions to problems related to heterogenous treatment effects have been proposed by i.e. Sun & Abraham (2020) and Callaway & Sant'Anna (2020).These articles include practical tools that can be used for robust estimation (i.e. Stata package `eventstudyinteract` (Sun 2021)). The results can then be compared the standard event study estimates to gain understanding of the effect of heterogeneity on estimates. I provide results using these tools as robustness checks in Appendix X.X.

3.4.3 Cancer penalty

I estimate the child penalty coefficient proposed by Kleven, Landais & Søgaard (2019) but in the case of cancer diagnosis instead of child birth. This can then be implemented as the causal effect of cancer diagnosis on gender earnings gap (if assumptions similar to Assumptions 1-3 hold). I use the term "cancer penalty" to denote this coefficient. The benefits of this approach are the clear assessment of the significance and size of the changes in the earnings gap as well as comparison to the child penalty literature. The coefficient is then calculated as follows:

$$P_l \equiv \frac{\hat{\mu}_l^M - \hat{\mu}_l^F}{E[\tilde{Y}_i^M \mid l]}.$$

As in Kleven, Landais & Søgaard (2019), in this formula μ_l^M and μ_l^F refer to coefficients in regression 1 for mothers' and fathers' respectively. The term $E[\tilde{Y}_i^M \mid l]$ denotes the earnings of the mother predicted by the child age, mother age and calendar year dummies. The interpretation of the coefficient is then as follows: coefficient of -0.2 means that the gap in mothers' and fathers' earnings increased by 20% of the predicted earnings of mothers, predicted using child age, mother age and calendar year.

While the coefficient is used in the literature in the context of a child being born, the method extends naturally to other changes in the family, like health shock. In addition, the assumption of no anticipation effects and parallel trends is more

plausibly satisfied in this case, since having a child rarely is a complete surprise and rather results of some decisions individual make in the years before.

Kleven, Landais & Søgaard (2019) interpret the estimates as causal effect of children on earnings. For the case of cancer penalty, this is a bit more unclear. In order for the child to get cancer, the child has to first be born, so the cancer penalty for these families is experienced on top of the child penalty. Using a regression like this one and balancing sample so that years before children are born are included, it is inevitable that some control families do not yet have children and some have. This makes the interpretation a little more unclear, as it is possible that some of the difference between the treatment and controls in some comparisons (see **??** for example of the decomposition), is due to the children born and some due to cancer.

4 Results

In this section I present my most important results. Section 4.1 provides a look to the estimates of the main effects using equation 1. The following sections explore the heterogeneity of the effects in more detail and provide my estimates for effect on the wage earnings gap.

4.1 Labour market outcomes

As noted in section **??**, the parallel trends assumption is vital for the identification strategy and can not directly be tested. However, if we observe no statistically significant pre-trends, this strengthens the causal interpretion of the estimates. In Figures 1, 2 and 3 I present the main results of the estimation. From these figures and the associated Tables 2 and 3 the lack of the pretrends can be noted, which means that no evidence on violations of this assumption is observed.

The significant gendered nature can be noted as women experience far stronger declines in earnings (Figure 1). As the earnings for mothers are lower even before

the diagnosis, the difference is even more substantial if it is compared to the earnings before the shock: the impact one year after the diagnosis equals 30% of the pre-shock mean earnings for mothers and 7 % for men. The point estimates then gradually approach zero but are still negative after 5 years for both genders. For mothers, even at the the end of the observation window the effect on annual wage earnings was around 1800 euros.

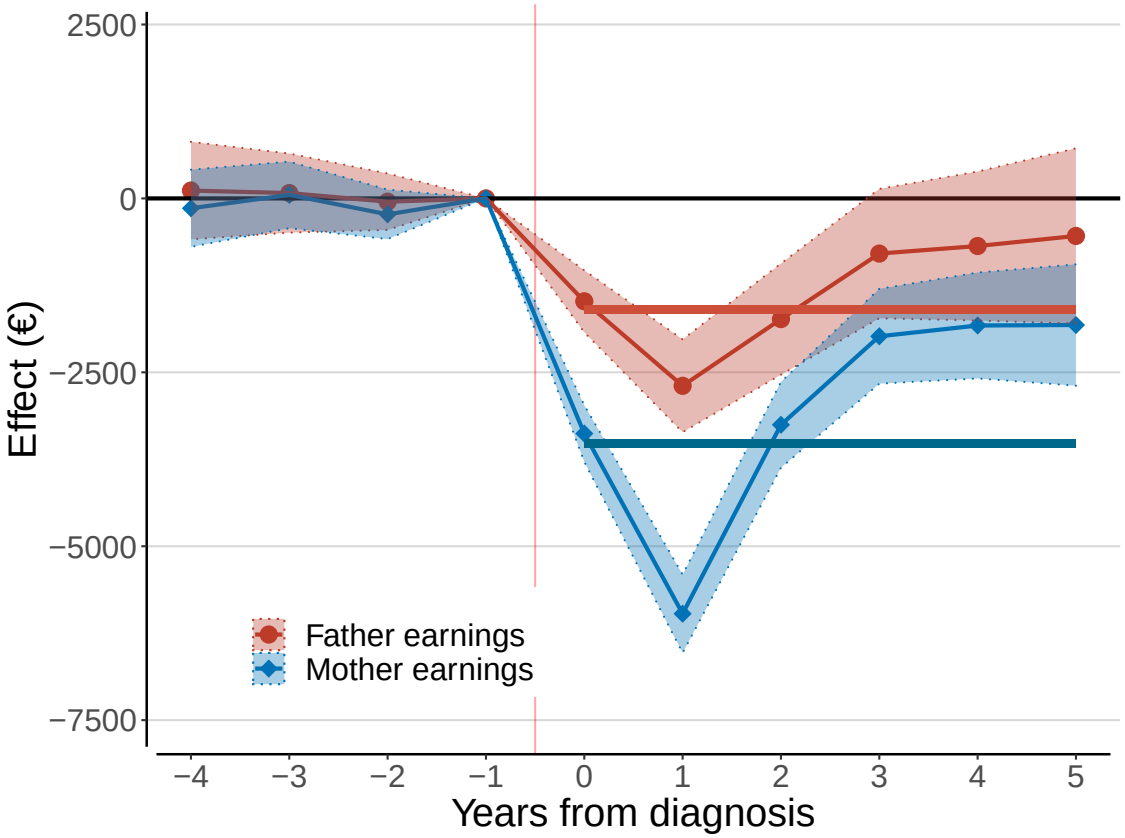
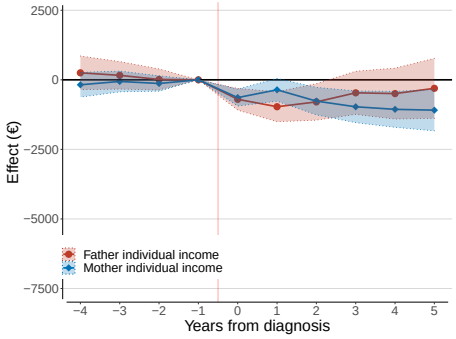
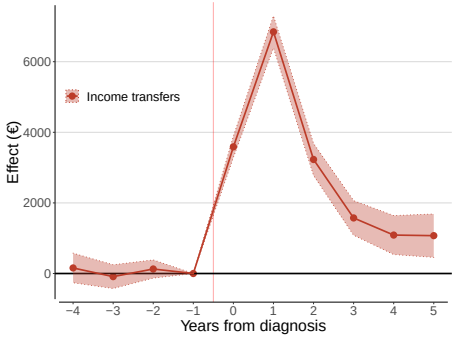


Figure 1: Effect on mother and father wage earnings. Period $l = -1$ excluded, treatment happens at $l = 0$. 95% confidence intervals included. Solid lines denote estimates with DID instead of event study specification.



(a) Effect on mothers' and father individual income. Period $l = -1$ excluded, treatment happens at $l = 0$. 95% confidence intervals included.



(b) Effect on household income transfers. Period $l = -1$ excluded, treatment happens at $l = 0$. 95% confidence intervals included.

Figure 2

As could be expected, the effects on individual income are not as profound as the effects on labour earnings. This is an expected result when we consider the support provided by the welfare state. There are however significant drops in both outcomes immediately after the diagnosis. For mothers the decline is more persistent than for fathers: the decline is still significant 5 years after the shock. This could be taken as evidence that the welfare state provides good initial support to parents, but the long-term effects caused by i.e. missing out on human capital accumulation are harder to alleviate.

Many of the transfers provided by the welfare state are household-based, so observing transfers for mothers' and fathers' separately is not possible. However, we can observe the income transfers for the whole household, shown in Figure 2 .

As is the case with the earnings data, the data on employment is also reported yearly. If employment data was available more precisely, i.e. monthly, the change could perhaps be observed better: yearly data does not allow to see the outcomes as precisely. That being said, there are still clear effects on mother's employment that are visible even after 5 years. In contrast, for men the effect on employment is not significant. These results are in line with the child penalty literature (Kleven, Landais & Søgaard 2019).

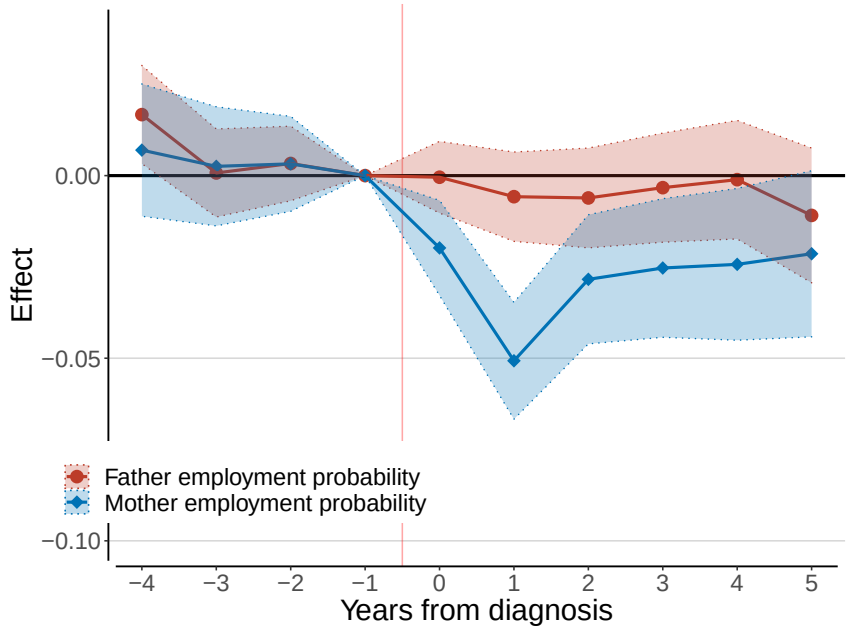


Figure 3: Effect on mother and father employment probability. Linear probability model. Period $l = -1$ excluded, treatment happens at $l = 0$. 95% confidence intervals included.

	Mother earnings	Mother log earnings	Mother individual income	Mother employment probability
	(1)	(2)	(3)	(4)
-4	-141.9	-0.00907	-178.3	0.00698
	(283.2)	(0.0775)	(226.9)	(0.00924)
-3	50.57	-0.00671	-58.97	0.00254
	(244.3)	(0.0671)	(190.6)	(0.00832)
-2	-229.2	-0.0170	-131.6	0.00322
	(181.3)	(0.0522)	(141.1)	(0.00663)
0	-3380***	-0.389***	-639.2***	-0.0198***
	(207.6)	(0.0493)	(155.5)	(0.00664)
1	-5970***	-1.082***	-359.2*	-0.0507***
	(287.3)	(0.0680)	(208.4)	(0.00816)
2	-3254***	-0.558***	-765.2***	-0.0284***
	(314.7)	(0.0730)	(252.0)	(0.00902)
3	-1982***	-0.352***	-969.4***	-0.0253***
	(347.6)	(0.0802)	(291.0)	(0.00968)
4	-1827***	-0.311***	-1061***	-0.0243**
	(388.3)	(0.0887)	(329.7)	(0.0106)
5	-1820***	-0.314***	-1091***	-0.0214*
	(445.1)	(0.0987)	(379.0)	(0.0116)
Observations	71990	71990	71990	71990
R-squared	0.281	0.174	0.367	0.173
Number of id	3130	3130	3130	3130
Calendar Year FE	YES	YES	YES	YES
Child-Age FE	YES	YES	YES	YES
Parent-Age FE	YES	YES	YES	YES
Mean at l = -1	19942	-	24979	0.73

Table 2: Main event study results for mothers. Period -1 excluded. Effect on employment estimated using a Linear Probability Model. All estimates include individual fixed effects. Clustered (family level) standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

	Father earnings	Father log earnings	Father individual income	Father employment probability
	(1)	(2)	(3)	(4)
-4	112.7	0.0637	250.8	0.0167**
	(357.8)	(0.0511)	(308.4)	(0.00686)
-3	76.77	0.0240	160.9	0.000718
	(290.0)	(0.0446)	(252.1)	(0.00615)
-2	-47.40	-0.0240	12.99	0.00332
	(206.9)	(0.0359)	(192.6)	(0.00521)
0	-1479***	-0.0886**	-700.0***	-0.000444
	(227.2)	(0.0373)	(198.8)	(0.00501)
1	-2694***	-0.207***	-967.3***	-0.00577
	(340.4)	(0.0491)	(274.9)	(0.00624)
2	-1735***	-0.112**	-794.3**	-0.00612
	(407.8)	(0.0565)	(334.4)	(0.00697)
3	-793.0*	-0.0422	-467.4	-0.00330
	(474.5)	(0.0595)	(395.7)	(0.00762)
4	-684.5	-0.00714	-495.6	-0.00111
	(546.4)	(0.0674)	(466.7)	(0.00828)
5	-539.6	-0.0394	-305.8	-0.0109
	(642.8)	(0.0778)	(549.2)	(0.00941)
Observations	69644	69644	69644	69644
R-squared	0.278	0.242	0.333	0.232
Number of id	3084	3084	3084	3084
Calendar Year FE	YES	YES	YES	YES
Child-Age FE	YES	YES	YES	YES
Parent-Age FE	YES	YES	YES	YES
Mean at l = -1	36276	-	39227	0.87

Table 3: Main event study results for fathers. Period -1 excluded. Effect on employment estimated using a Linear Probability Model. All estimates include individual fixed effects. Clustered (family level) standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

4.2 Educational attainment

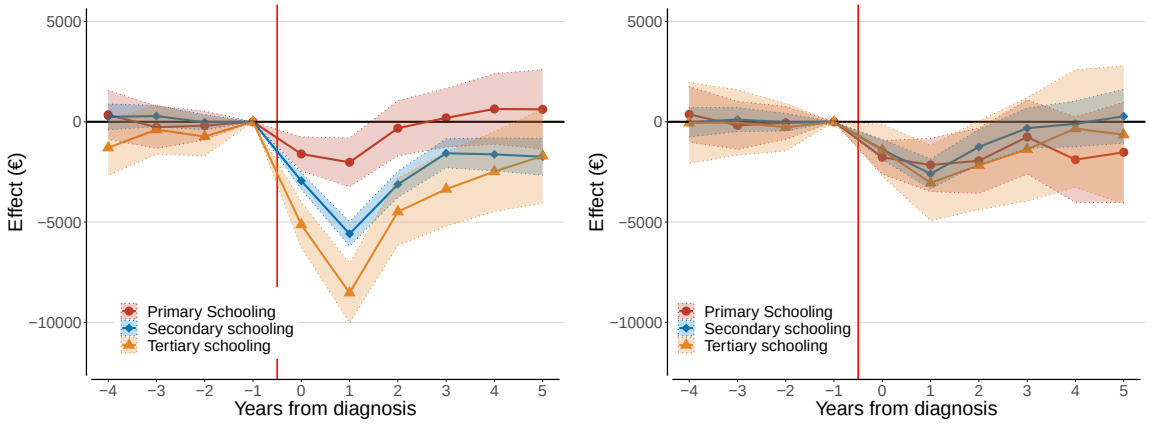
As educational attainment often results in different vocational choices, it is also plausible that child's cancer diagnosis could have a different effect depending on the level of education the parents have. If for example those with higher paying jobs

have more flexible choices regarding leave, they could suffer less; on the other hand those working jobs with lots of human capital could more quickly fall behind if the accumulation stops.

I estimate effects for mothers' and fathers' separately for 3 different groups of educational attainment: primary, secondary and tertiary education. These results can be found in the Figures 6 A-B (and in Table format: Appendix A). For both mothers' and fathers', the income level before the diagnosis is strongly correlated with the educational attainment, so to make comparisons, reporting effects relative on initial earnings is necessary.

The absolute amounts are largest for those with tertiary education, but the compared to the earnings level before the diagnosis, the drop is largest for the group of mothers with only secondary education (32% compared to 21% and 28% for those with primary and tertiary education, respectively).

For men the relative drop is largest in the group with primary education (8.5 %), but differences are somewhat smaller (7.7 % vs 5.5 % for secondary and tertiary education). Interestingly, even the absolute drop is bigger for fathers with secondary education compared to those with only primary schooling, although the difference is not significant.



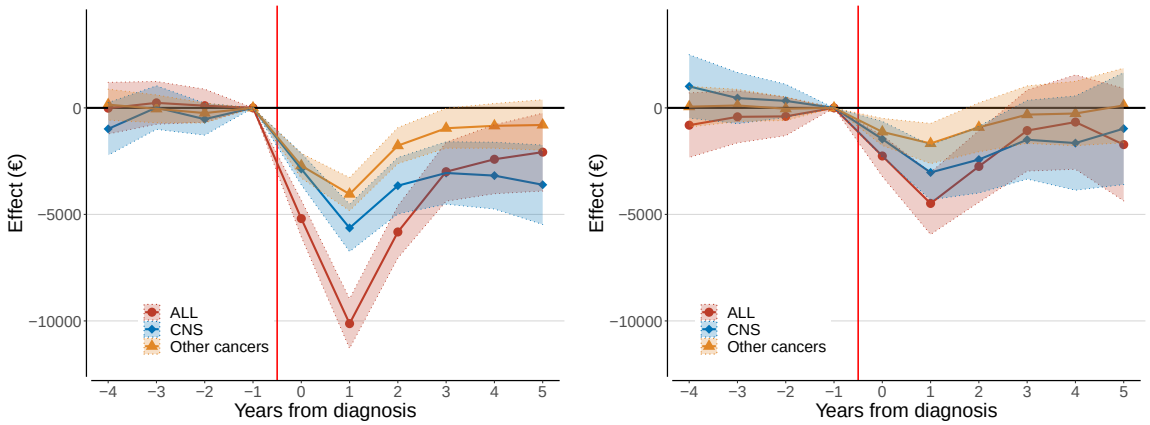
(A) Mother earnings

(B) Father earnings

Figure 4: Effect on parental earnings by the educational attainment. Period $l = -1$ excluded, treatment happens at $l = 0$. 95% confidence intervals included.

4.3 Severity of cancer

As the different cancers differ in length and severity of the treatment and survival probability, I estimate results separately for different cancers in Figures 5 A and B. As we hypothesize, the effects are larger for more severe cancers, i.e. ALL and LBL which require long treatments and for Central Nervous System cancers where treatments are short and intense. Interestingly, the effects of CNS seem to be more persistent than the effects of ALL, even if the treatment is described as longer for ALL, while patients that survive CNS can exhibit side effects on the central nervous system (Tolkkinen et al. 2018). These differences are visible in both mothers and fathers. Still, even the effects for cancers categorized as "Other cancers" are significantly different from zero for wage earnings in for both men and women.



(A) Mother earnings

(B) Father earnings

Figure 5: Effect on parental earnings by the type of cancer. Categories are: Acute lymphoblastic leukemia or Lymphoblastic Lymphoma, Central Nervous System Cancers and Other cancers. Period $l = -1$ excluded, treatment happens at $l = 0$. 95% confidence intervals included.

4.4 Family labour supply

From previous research and the results shown above, it is clear that the effects are largely different for mothers than fathers. It is also clear that the aggregate estimates hide different heterogeneous effects. It is therefore interesting to see how the effects depend on family share of labour before the diagnosis and also to observe

how the illness affects the share of income each parent provides to the family. These estimates relate to the theoretical considerations of the model in Section **??**. For example, the model predicts that in the absence of hugely different preferences and gendered norms regarding caring for the child and differing effects of illness on own productivity, the parent with lowest utility of working would reduce his/her labour supply. As can be derived from the first order conditions, the earnings gap should be decreasing in maternal pre-cancer wage. As the data do not contain information on wages, only earnings, this complicates the problem even more. Still, the it is of interest to see if we find difference effects depending on the pre-cancer household labour division. To empirically (under reasonable assumptions) prove that the differences in earnings have no effect one would need different data, i.e. data on same-sex couples Andresen & Nix (2019).

To examine this I present results using earnings as an outcome and running separate regressions for families where the share of mother earnings in years before the cancer was on average 25-50 %, 50-75 % and over 75 % in Figure 6. While this is an imperfect measure for the comparative advantage, the data does not include information on variables like hourly wages. It can be noted that the changes in mother earnings as percentage of the mean earnings for each group are relatively similar regardless of the pre-cancer earnings share. This does not support the conclusion that the earnings before the diagnosis are a deciding factor in the family decisionmaking.

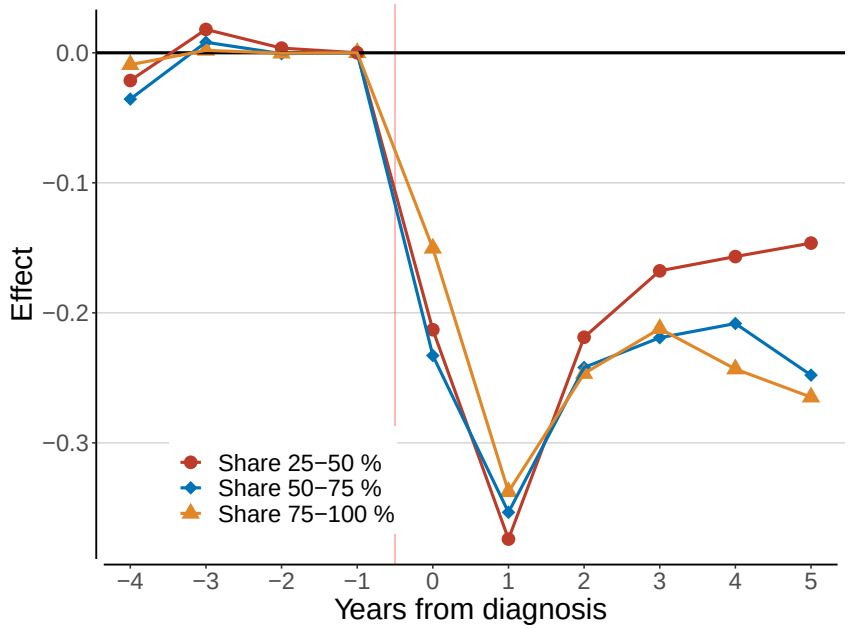


Figure 6: Effect on mother earnings, transformed into the percentage of the earnings before the cancer diagnosis, by percentage share of mothers earnings. Period $l = -1$ excluded, treatment happens at $l = 0$.

The results regarding the earnings gap and cancer penalty relate closely to the previous results. The results are only calculated for the sample with no single mother households (N=3034), although this does not considerably impact the estimates. The results quantify the causal effect of the shock (in this case cancer) on the difference in earnings between mothers and fathers. Figure 7 illustrates the effect on the earnings gap from the cancer diagnosis.

It seems that the larger penalty is correlated with maternal education, as shown in Figure 8. In a fully interacted regression, the effects on secondary or tertiary educated are significantly different (in a 95 % level) from the baseline of those with primary education, although only in periods 0 and 1. In contrast, paternal education does not seem to exhibit similar correlation.

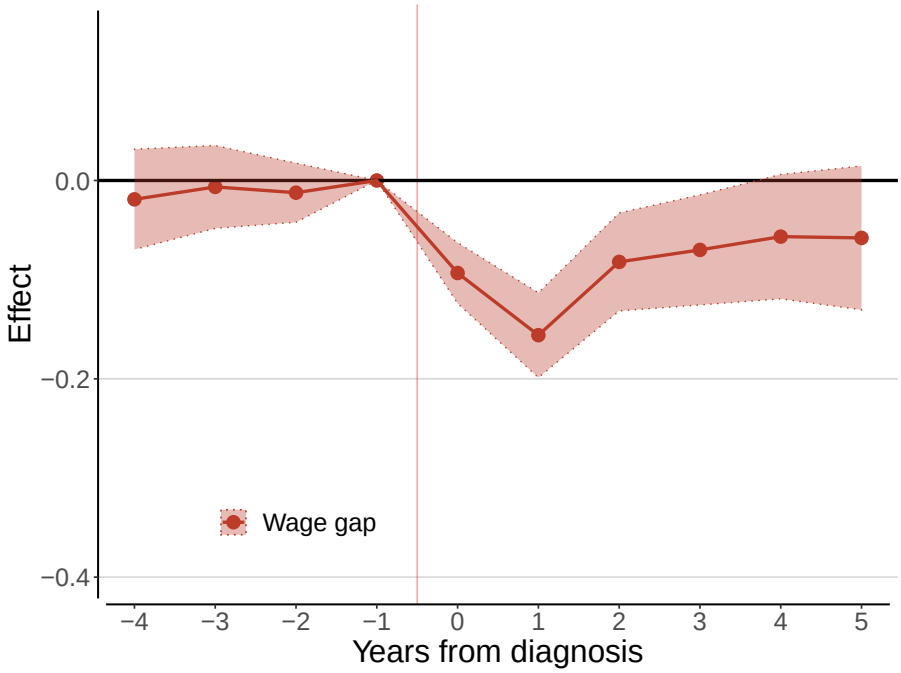


Figure 7: Effect on Earnings gap (cancer penalty) relative to year before the diagnosis for mothers in the sample. Period $l = -1$ excluded, treatment happens at $l = 0$. 95 % confidence interval included.

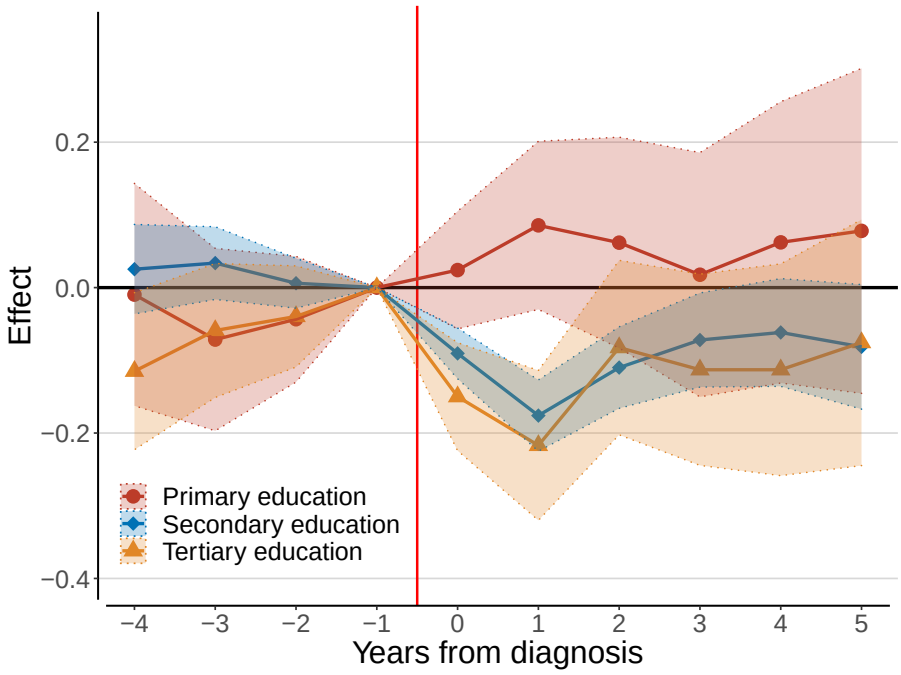


Figure 8: Earnings gap relative to earnings year before the diagnosis for mothers in the sample, by mother educational attainment. Period $l = -1$ excluded, treatment happens at $l = 0$. 95 % confidence interval included.

The most interesting part, in the light of the results of the model in Section 2,

concerns the connection of the pre-cancer earnings to the decline in maternal earnings and the family earnings gap. I run the analysis separately for the families where the share of mother earnings is 25-50 %, 50-75 % and over 75 %. The results can be found in Figure 9. It seems that even in families where mother is the main breadwinner, the impact on earnings is large and the earnings gap increases. In the group where mother's earnings are 50-75 % and more than 75 % of the family earnings, the cancer penalty estimates in relative time $l = 1$ are -0.538 and -0.661, respectively. Care is needed when interpreting these differences between the groups: I do not claim that the difference is causal, as it could for example be that socioeconomic background affects the type or fatality of the cancers found.

It should be noted that when limiting the sample by pre-event outcome variables, the parallel trends assumption is not as strongly supported by testing pre-trends as in other instances. It seems that limiting the sample using pre-trend characteristics is not a good practice when using two-way fixed effects, as the control group is different in each comparison and the limit can then have an uneven impact on the different comparisons the regression makes. However, the literature does not provide any alternative methods to assess this question, so I provide my results here with a caveat regarding the assumptions.

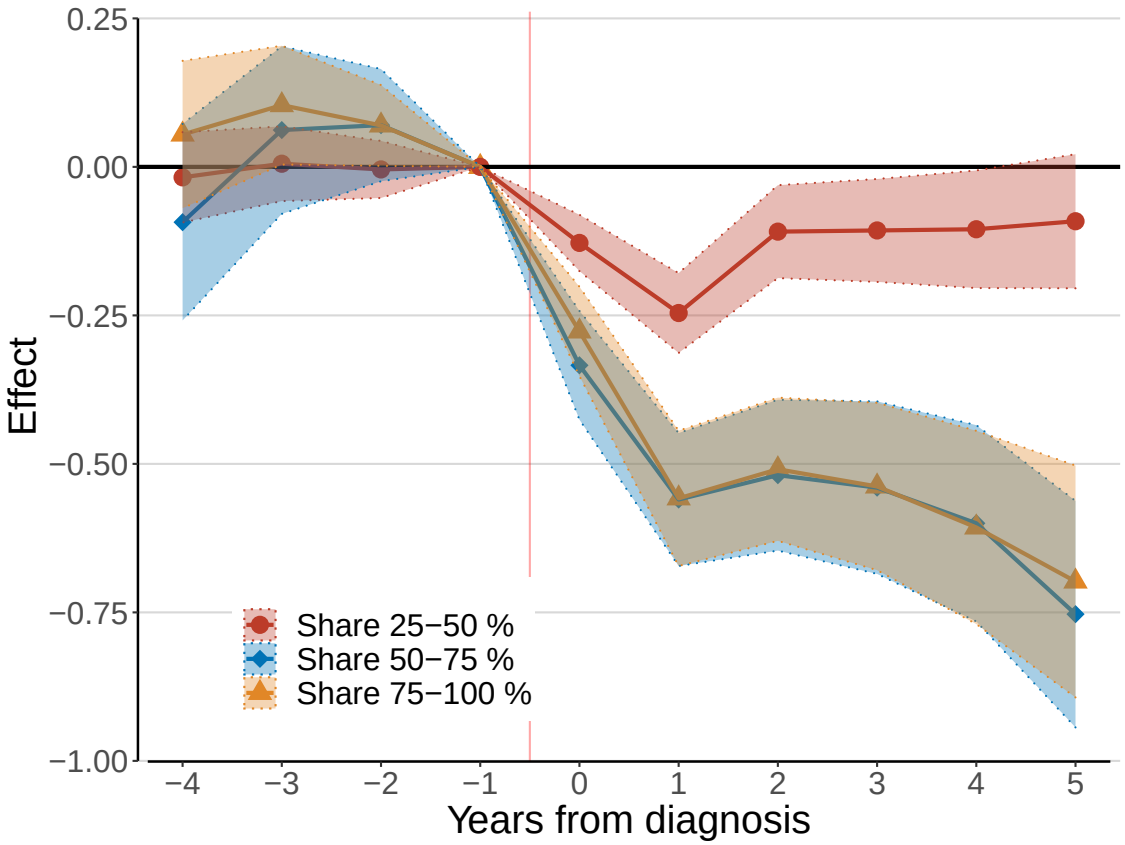


Figure 9: Effect on earnings gap as a percentage of the earnings before the cancer diagnosis, by percentage share of mothers earnings. Period $l = -1$ excluded, treatment happens at $l = 0$. 95 % confidence interval included.

5 Robustness checks

From results it can be noted that there are often surprisingly small effects on year 0. This is because most of the outcome variables are measured yearly, and therefore a family which experiences shock in December will have very different outcomes in years 0 and 1 than family that experiences a shock in January. To further examine this attenuation bias, I present results for only some months in Appendix B. The results are in line with the main findings, with the bulk of the effect distributed to year $l = 0$ if the child is diagnosed in January-March and for year $l = 1$ if diagnosed in October-December.

In Appendix C results using some background variables as controls are shown.

The control variables chosen are birth of a child (yearly dummy), indicator for family living in a big city and indicator whether a parent lives with the child that falls ill. These are largely similar to the main results, which provides additional robustness to the estimates.

The estimates presented here show some kind of average of effects and it is possible that the effects observed would be driven entirely by some units experiencing very strong effects. To alleviate this concern, Appendix D presents "individual effects", i.e. change in earnings from year $l = -1$ to year $l = 1$ year after the diagnosis, divided by the "predicted earnings" in year $l = 1$, i.e. the labour earnings of a mother/father with predicted by age, child age and calendar year. The effects seem to distribute somewhat normally, with no large tails present.

Since parent and child age has a clear connection to labour market outcomes and is also correlated with relative time l, including these trends in the specification seems like an intuitive choice. Regardless, I also estimate results using the specification of Öhman et al. (2021), who do not control for child and parent age. Graphs comparing my results to those can be found from Appendix F. The results without age fixed effects exhibit similar trends the ones presented in Öhman et al. (2021). The positive effect on mother earnings is not statistically significant, but the negative effect on fathers is significant. Using a sample which includes all children aged 0-18 when diagnosed, following Öhman et al. (2021), does not change the results.

Öhman et al. (2021) do not report the results using any birth year of age fixed effects. Because of this, it is hard to asses whether this is the cause for the differences in results. Another possible methodological and sample explanations are different cohorts considered and binning of endpoints. However, the effects by cohort are quite homogeneous in my data. While the binning of endpoints is somewhat ambiguous in Öhman et al. (2021) article, I have in my results in appendix I binned the endpoints outside the observation window, at -5 and +10, which is the most standard choice in the literature.

6 Conclusions

Providing causal insights to the connection between health and socioeconomic outcomes is difficult. Using variation in the age at which cancer is diagnosed in children, I am able to provide causal estimates on the effect of childhood cancers on parental labour market outcomes.

The aggregate results on the labour market outcomes seem large and robust to different types of controls. The assumptions of event study method, particularly the parallel trends assumption, seem to be satisfied. Additional robustness check for exploring the problematic time and cohort heterogeneity show that the results are robust to the violation of the cohort-homogeneous treatment effects. Compared to the previous literature the magnitude of the effect is larger than the effect of all serious hospitalizations estimated by Costa-Ramon (2020) and Breivik (2020) but similar to the results of Costa-Ramon (2020) concerning only cancers.

The profile of the effects is similar to that estimated by Costa-Ramon (2020) and Breivik (2020) in context of all illnesses, but the magnitudes are different. In our results, the estimate at time $l = 1$ for mothers' corresponds to massive 29% of their wage earnings at time $l = -1$. It is hard to assess precisely what exactly causes the effects in long and short term. In the short term, significant time is probably spent taking care of the sick child. In the long term, missing out on experience and reduced human capital accumulation are possible factors.

In contrast, the results differ from Öhman et al.'s (2021) in many ways. As shown in Appendix I, whether the age fixed effects are included or not does indeed a difference to results using my data. I would not interpret my estimates from regressions where the age fixed effects are excluded as the causal effect. Based on this comparison we can not deduct that the estimates of Öhman et al. (2021) would be different from the ones they report if the specification was changed: the only way to check for this would require access to their data.

The differences that can be answered using my data are discussed in section 4,

but the last possibility, differences in countries, is hardest to analyze. The treatment can be assumed to be relatively similar in both countries. Both Finland and Sweden are rich countries with a broad public healthcare and welfare state mechanisms to support those who face adverse situations. Sweden is often considered a model example in gender equality. However, evidence on similar situations from other similar countries, like Breivik (2020) from Norway, Kleven, Landais & Søgaard (2019) from Denmark and Angelov et al. (2016), Kleven, Landais, Posch, Steinhauer & Zweimüller (2019) from Sweden all provide support to the the hypothesis that even in relatively equal countries, when changes (illness or childbirth) affect the family, mothers still take the bulk of the hit in the labour market.

The authors theorize that "One potential explanation for gender differences in earnings is that assuming the role of primary caregiver of a child with cancer may also result in increased support from family, friends and health-care staff which may function as a protective factor in the long-term" (Öhman et al. 2021). While it is hard to assess the possible magnitude of this kind of effect, it is even harder to use this to explain the significant negative effect on fathers.

The main question in this article concerns the effect of cancer on earnings differences between genders. The method used to answer this question is similar to i.e. Kleven, Landais & Søgaard (2019). My results show that the earnings gap increases and the cancer penalty coefficient is around 0.2, which can be considered high but is still lower than the penalty caused by childbirth, estimated by i.e. Sieppi & Pehkonen (2019). A possible difference between the shocks is also that having children is often a more planned decision, which could cause women to invest less in their education and career in response to expected motherhood Kleven, Landais & Søgaard (2019). The possibility of anticipation effects is then more realistic than in the case of cancer diagnosis.

The results also show that mothers' earnings significantly decline among families where the mother earns more than the father during years before the shock. Among

these families, the cancer penalty is also significant. When compared to predictions from the family labour supply model, it seems that differences in wage earnings and opportunity costs are not the only thing driving the labour supply decisions of families. For groups where mothers earn on average 50-75 % and 75 - 100 % of the family earnings in the years before the cancer, the cancer penalty is larger than for the group where mothers earn only 25-50 %. The impact on the high-share groups gets even larger in the long-term.

While these estimates seem high, it should be noted that, as can be seen in Section 3.4.3, if fathers earnings increase, the penalty term can even be less than -1. If the families where the mother share is high are families where the father is i.e. student or taking part in military service, we are likely to expect some sort of "regression to the mean" when these fathers join the labour market. It is also possible that to some extent the effect is, even after controlling for child age, caused by child penalty, especially in the long run. A use of additional, different control group could help alleviate these concerns. Overall, while earnings are not a perfect measure of the opportunity costs, the evidence does not seem support the hypothesis that mothers suffer less when they are the main provider of the family.

My results for different cancer categories show that more serious cancers also have the greatest impact on earnings. The death of a child has the largest impact. This is in line with the previous literature, for example Syse et al. (2011).

Based on earlier literature, is was not clear whether the shocks differ in magnitude by education or other socioeconomic background variables. My results show that the absolute effects on earnings are largest for those with tertiary education, but when taking into account the higher initial level of the outcome variable, the differences are not large.

Recent literature has shed light on the shortcomings of the two-way fixed effects estimators even using event study specification. As the bias in the effects can sometimes be severe (Baker et al. 2021), I survey this literature and apply the most

relevant alternative estimators. The comparisons between alternatives, namely CS-estimator by Callaway & Sant'Anna (2020) and IW-estimator by Sun & Abraham (2020), and my preferred TWFE-estimators show the results to be robust to these controls.

The results of this article outline some directions for future research. It seems that the welfare state alleviates the effects of a child falling ill relatively well, at least when the individual income is considered. Using more precise (i.e. monthly) data on working hours and different benefits, role of the income transfers could be probed further. Using these results alone, it is hard to make policy recommendations. What can be noted is that the current benefits are shorter than the effects on labour market earnings and employment, and also seem to support one person taking the entire leave, which might be a factor increasing the differences between genders. Using more outcome variables could help decompose the effect on earnings to changes in earnings, hours worked and possible changes in employment sector or profession.

Previous research (Kleven, Landais & Søgaard 2019) has found that the gender roles carry over generations, as the families where the responsibility was shared more equally during mothers childhood have smaller child penalties. While my data does not allow to assess this, it would be interesting to know if this is true also in the case the cancer penalty. Also, extending the analysis to more common illnesses would provide more relevant information, since childhood cancers are rare occurrences, and these results can not be generalized to other illnesses or other shocks facing a family. With larger data, the aspects regarding mental health could perhaps be probed further.

Overall, the results show that childhood cancers cause a large and persistent effects on mothers' earnings and increase the earnings gap between genders. Policy steps have been taken towards more equally distributed parental leave. If the society places value on equal distribution of childcare, the results suggest that perhaps this is worth considering also in the context of benefits received when the child suffers

of serious illness. It is possible that the adversity parents face also echoes through generations, in addition to other adverse effects of cancer on the children.

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A Age distribution of cancers

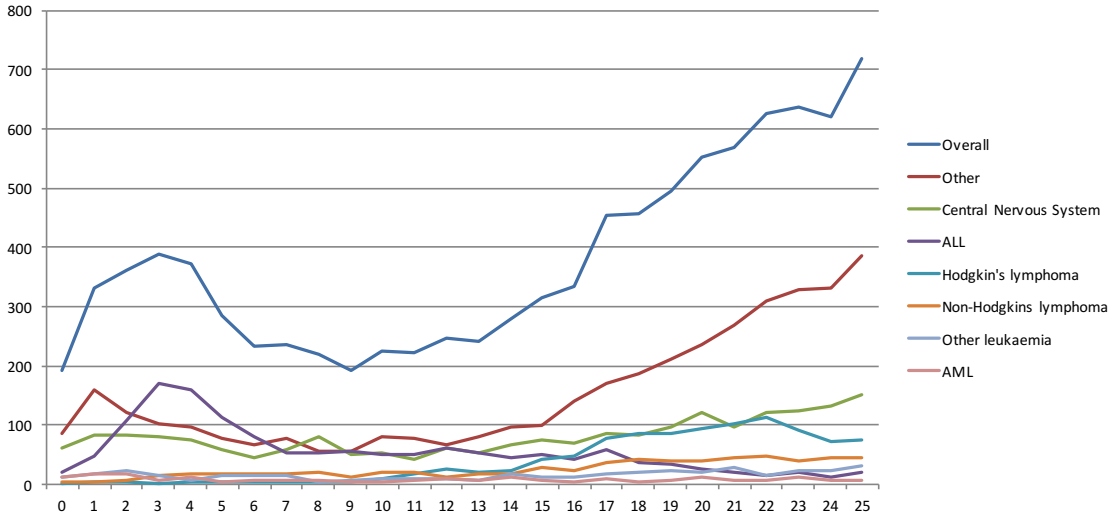

Figure 10: Age-distribution of cancers during years 1998-2017.

B Additional Tables

	Mother earnings			Father earnings		
	ALL or LBL	CNS	Other cancers	ALL or LBL	CNS	Other cancers
-4	-13.69	-986.7	152.4	-812.0	1009	56.70
	(618.9)	(629.7)	(370.3)	(773.0)	(758.7)	(486.2)
-3	235.7	14.66	-44.49	-419.9	460.6	113.0
	(512.1)	(521.4)	(332.5)	(623.8)	(612.8)	(386.6)
-2	102.8	-530.1	-244.4	-399.0	328.6	-39.37
	(395.3)	(383.2)	(239.1)	(461.4)	(397.7)	(281.7)
0	-5195***	-2868***	-2713***	-2258***	-1463***	-1127***
	(440.9)	(379.1)	(296.5)	(490.7)	(423.8)	(324.0)
1	-10127***	-5637***	-4049***	-4485***	-3032***	-1672***
	(593.8)	(566.5)	(395.6)	(743.9)	(653.1)	(480.8)
2	-5825***	-3652***	-1773***	-2748***	-2426***	-912.0
	(625.4)	(673.6)	(430.2)	(859.9)	(800.2)	(584.9)
3	-2996***	-3056***	-955.4**	-1065	-1494	-316.4
	(706.5)	(747.8)	(477.9)	(971.2)	(942.4)	(687.9)
4	-2412***	-3179***	-842.3	-666.1	-1652	-263.9
	(826.0)	(800.4)	(534.8)	(1130)	(1129)	(771.1)
5	-2077**	-3609***	-803.2	-1723	-973.5	114.8
	(931.8)	(950.6)	(605.4)	(1348)	(1340)	(893.1)
Constant	1456	8286***	5934***	4344	5902	4925*
	(1504)	(2144)	(1213)	(3619)	(3632)	(2705)
Observations	18078	17549	36363	17539	16951	35154
R-squared	0.303	0.272	0.284	0.270	0.288	0.283
Number of id	786	763	1581	777	749	1558
Calendar Year FE	YES	YES	YES	YES	YES	YES
Child-Age FE	YES	YES	YES	YES	YES	YES
Parent-Age FE	YES	YES	YES	YES	YES	YES
Mean at l = -1	19572	21748	22605	35874	36036	35779

Table 4: Effects on labour market earnings by cancer type. All estimates include individual fixed effects. Clustered (family level) standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

	Mother earnings by schooling			Father earnings by schooling		
	Primary	Secondary	Tertiary	Primary	Secondary	Tertiary
-4	349.8	244.7	-1297*	371.7	-21.75	-55.07
	(622.5)	(326.7)	(701.3)	(705.4)	(374.5)	(1030)
-3	-272.8	286.6	-389.6	-176.7	114.1	-33.89
	(539.2)	(273.2)	(626.4)	(611.3)	(307.7)	(831.8)
-2	-191.5	-23.67	-736.3	-55.60	-24.46	-272.2
	(367.3)	(193.6)	(490.0)	(418.7)	(223.9)	(606.1)
0	-1606***	-2946***	-5134***	-1769***	-1383***	-1419**
	(432.9)	(212.3)	(577.7)	(428.3)	(257.3)	(656.6)
1	-2019***	-5590***	-8536***	-2153***	-2598***	-3044***
	(620.0)	(312.0)	(759.4)	(678.5)	(380.0)	(959.1)
2	-320.9	-3122***	-4488***	-1944**	-1252***	-2172*
	(702.8)	(332.2)	(857.6)	(831.2)	(444.8)	(1130)
3	196.4	-1570***	-3360***	-756.9	-320.1	-1373
	(745.7)	(369.7)	(944.6)	(944.7)	(512.6)	(1313)
4	640.1	-1625***	-2482**	-1889*	-99.81	-338.7
	(898.6)	(419.0)	(1017)	(1090)	(581.5)	(1496)
5	620.2	-1742***	-1703	-1521	274.1	-648.8
	(1011)	(470.9)	(1197)	(1278)	(686.0)	(1757)
Constant	-2431	4765***	12748***	-2169	-2024	8344*
	(2124)	(1278)	(3249)	(5022)	(1589)	(4769)
Observations	8717	44459	18814	12446	41644	15554
R-squared	0.184	0.284	0.381	0.167	0.274	0.431
Number of id	379	1933	818	575	1827	682
Calendar Year FE	YES	YES	YES	YES	YES	YES
Child-Age FE	YES	YES	YES	YES	YES	YES
Parent-Age FE	YES	YES	YES	YES	YES	YES
Mean at l = -1	9717	17315	30885	25471	33678	55796

Table 5: Effects on labour market earnings by parental schooling. All estimates include individual fixed effects. Clustered (family level) standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

	Mother earnings		Father earnings	
	Child dies	Child survives	Child dies	Child survives
-4	-451.1	-138.2	-401.3	40.40
	(789.0)	(323.5)	(1081)	(422.3)
-3	-915.6	37.84	-80.60	-14.86
	(706.7)	(273.9)	(984.3)	(334.8)
-2	-1138**	-55.94	215.0	-83.81
	(472.2)	(206.7)	(557.2)	(242.5)
0	-4033***	-2915***	-1391*	-1205***
	(522.7)	(223.6)	(710.6)	(261.6)
1	-10198***	-5033***	-3050***	-2647***
	(897.8)	(309.1)	(988.5)	(377.3)
2	-6828***	-2640***	-651.0	-1750***
	(1085)	(328.5)	(1175)	(439.2)
3	-5968***	-1324***	2222	-954.8*
	(1142)	(365.6)	(1412)	(505.7)
4	-4995***	-1277***	3649**	-990.2*
	(1249)	(415.0)	(1663)	(596.4)
5	-4546***	-1308***	3246	-763.7
	(1428)	(483.3)	(2032)	(708.1)
Constant	17597***	7552***	19482***	7482*
	(2437)	(1781)	(4732)	(4106)
Observations	7222	56143	6865	54349
R-squared	0.242	0.288	0.236	0.255
Number of id	314	2441	310	2402
Calendar Year FE	YES	YES	YES	YES
Child-Age FE	YES	YES	YES	YES
Parent-Age FE	YES	YES	YES	YES
Mean at l = -1	20489	21588	33088	36176

Table 6: Effects on labour market earnings by child survival. All estimates include individual fixed effects. Clustered (family level) standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

	Earnings gap				
	Absolute	Relative	Primary schooling	Secondary schooling	Tertiary schooling
-4	-346.4	-0.0190	-0.00966	0.0253	-0.115**
	(443.5)	(0.0258)	(0.0780)	(0.0314)	(0.0551)
-3	-45.39	-0.00647	-0.0715	0.0337	-0.0590
	(376.8)	(0.0213)	(0.0640)	(0.0255)	(0.0470)
-2	-215.8	-0.0123	-0.0435	0.00599	-0.0397
	(275.0)	(0.0152)	(0.0441)	(0.0175)	(0.0355)
0	-1983***	-0.0933***	0.0241	-0.0905***	-0.150***
	(299.7)	(0.0156)	(0.0413)	(0.0178)	(0.0379)
1	-3327***	-0.156***	0.0856	-0.176***	-0.217***
	(433.4)	(0.0218)	(0.0590)	(0.0250)	(0.0526)
2	-1608***	-0.0821***	0.0618	-0.110***	-0.0825
	(501.9)	(0.0252)	(0.0740)	(0.0286)	(0.0612)
3	-1296**	-0.0700**	0.0177	-0.0721**	-0.113*
	(563.0)	(0.0283)	(0.0857)	(0.0330)	(0.0671)
4	-1211*	-0.0566*	0.0622	-0.0617	-0.113
	(635.7)	(0.0320)	(0.0987)	(0.0378)	(0.0743)
5	-1366*	-0.0579	0.0780	-0.0815*	-0.0756
	(738.4)	(0.0370)	(0.114)	(0.0437)	(0.0863)
Constant	-404.0	-0.163	-0.418	-0.285	1.100***
	(2664)	(0.168)	(0.325)	(0.318)	(0.225)
Observations	69644	69644	8126	43076	18442
R-squared	0.107	0.186	0.227	0.206	0.161
Number of id	3084	3084	364	1904	816
Calendar Year FE	YES	YES	YES	YES	YES
Child-Age FE	YES	YES	YES	YES	YES
Parent-Age FE	YES	YES	YES	YES	YES

Table 7: Effects on the earnings gap between genders. All estimates include individual fixed effects. Clustered (family level) standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

C Results based on the month of diagnosis

Results for subsamples by the diagnosis month can be found from Table 8.

D Cohort-heterogeneous effects

As the cancer shock of a child could be considered more exogenous treatment than for instance some political measures, it is interesting to compare the results of modern, more robust methods to the more traditional methods. In this case the use of methods like standard Two Way Fixed Effects should in theory be less problematic

	January-March			October-December		
	(1)	(2)	(3)	(4)	(5)	(6)
	Mother earnings	Father Earnings	Relative wage gap	Mother earnings	Father Earnings	Relative wage gap
-4	-731.1	-231.1	0.0235	78.65	-206.8	0.0009
	(573.8)	(714.4)	(0.00528)	(620.6)	(768.3)	(0.0057)
-3	-710.5	-104.0	0.0499	115.2	-201.2	0.008
	(518.5)	(596.9)	(0.0431)	(492.6)	(591.8)	(0.0046)
-2	-843.7**	583.4	-0.0205	-299.7	88.41	-0.0243
	(383.1)	(400.3)	(0.0300)	(369.3)	(426.5)	(0.0310)
0	-6033***	-1865***	-0.195***	-1207***	-211.3	-0.0382
	(451.6)	(473.6)	(0.0332)	(445.1)	(493.1)	(0.0312)
1	-4979***	-1683**	-0.149***	-7313***	-3200***	-0.187***
	(579.4)	(681.2)	(0.0426)	(612.4)	709.3)	(0.045)
2	-2294***	-699.4	-0.0733	-4354***	-1837**	-0.120**
	(612.8)	(823.6)	(0.0474)	(688.8)	(853.4)	(0.0527)
3	-1100	303.6	-0.0716	-3205***	524.0	-0.125**
	(674.2)	(976.6)	(0.0541)	(718.4)	(964.3)	(0.0575)
4	-1438*	222.9	-0.0696	2507***	-300.1	-0.108*
	(762.2)	(1134)	(0.0618)	(796.8)	(1146)	(0.0643)
5	-1670*	173.1	-0.0707	-2507	-120.9	-0.111
	(890.5)	(1311)	(0.0728)	(914.0)	(1350)	(0.075)
Constant	12580***	2455	-0.357	2887	4358	0.539
	(1934)	(3731)	(0.847)	(3658)	(3696)	(0.381)
					YES	
Observations	17848	17198	17198	18124	17475	17475
R-squared						
Number of id	776	765	765	788	774	774
Calendar Year FE	YES	YES	YES	YES	YES	YES
Child-Age FE	YES	YES	YES	YES	YES	YES
Parent-Age FE	YES	YES	YES	YES	YES	YES

Table 8: Some results for different months of diagnosis. All estimates include individual fixed effects. Clustered (family level) standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

than in simulations like the ones performed by for example Baker et al. (2021), since the units observed are often states or firms that differ considerably and only a small amount are treated in each year. On the other hand, as there are more cohorts in my sample as in some other applications where the units observed are countries or states, it is plausible that there could be differences between the cohorts.

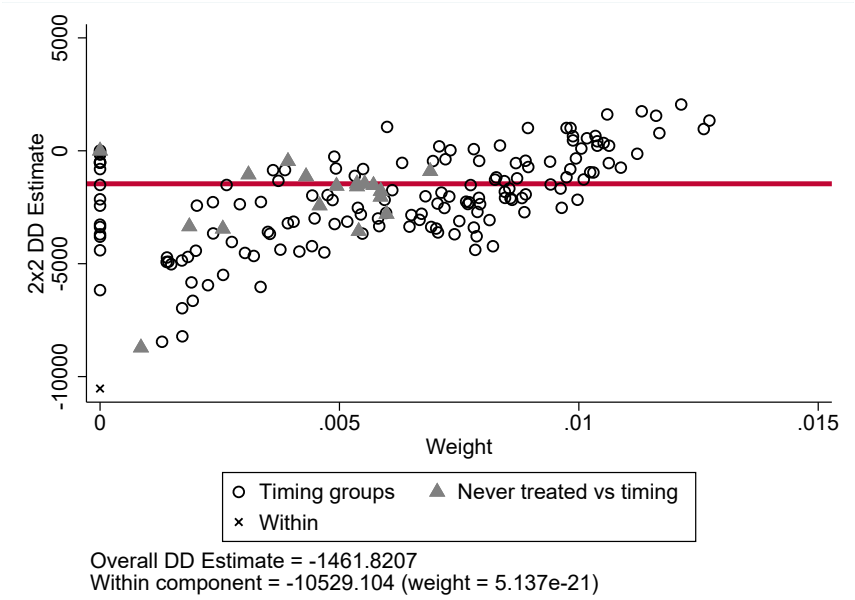

Figure 11: Goodman-Bacon decompositions for the sample. The outcome variable is mother wage earnings. Each observation denotes one comparison made between different treatment and control group. X-axis shows the weight of each estimate, y-axis shows the size of the estimate. Within -estimate denotes the differences caused by control variables, Goodman-Bacon (2018) notes that if the weight of this component is substantial, it should be examined closer.

The Stata command based on Goodman-Bacon (2018) plots all 2x2 components of the differences-in-differences estimates against their weights. The interpretation of the Goodman-Bacon decomposition is not entirely straightforward. As Figure D shows, the weights between the components differ substantially. However, the estimates are fairly similar and are not driven by one observation having a large weight. The estimate in the figure includes observations from periods not included in the main estimates, so it is not directly comparable to those estimates.

Many newer approaches are aimed to solve problems caused by dynamic effects or cohort specific heterogeneity in treatment effects. In Figure 12 results for mother's

wage earnings using the standard TWFE event study estimator, CS estimator and IW-estimator are presented. While there are small differences in magnitudes, the main results stay the same: insignificant pre-trends, large short-run effects and significant effects even 5 years after the diagnosis. The differences between these estimators are not substantial, which indicates that the traditional TWFE-estimator does not lead to large bias in this case, unlike in the cases examined by Baker et al. (2021).

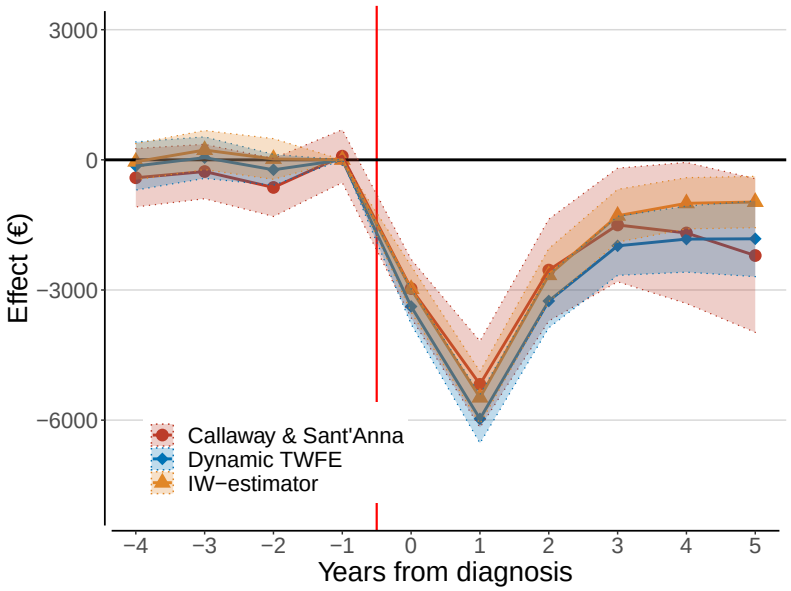

Figure 12: Comparison between aggregated CS estimator, IW-estimator and TWFE with dynamic effects, i.e. using Equation 1. CS includes controls for child and parent birth year. Outcome variable is mother earnings. Treatment happens at $l = 0$. For TWFE and IW period $l = -1$ excluded. 95 % confidence intervals included.

E Regressions using additional controls

Results using additional controls in addition to Equation (1) can be found in Table 9.

	Mother			Father			
	Mother earnings	Mother log earnings	Mother employment probability	Father earnings	Father log earnings	Father employment probability	Cancer penalty
-4	-431.0	-0.0643	-0.00347	515.8	0.0872	0.0140*	-0.0179
	(347.3)	(0.0889)	(0.0104)	(431.2)	(0.0594)	(0.00762)	(0.0278)
-3	-34.33	-0.0207	-0.00457	406.4	0.0464	-0.000336	-0.0104
	(281.8)	(0.0742)	(0.00913)	(340.8)	(0.0497)	(0.00676)	(0.0226)
-2	-338.3*	-0.0721	-0.000807	22.51	-0.0115	0.00224	-0.00911
	(201.8)	(0.0569)	(0.00721)	(233.4)	(0.0383)	(0.00548)	(0.0159)
0	-3423***	-0.403***	-0.0207***	-1638***	-0.116***	0.00102	-0.0957***
	(220.3)	(0.0520)	(0.00695)	(237.8)	(0.0381)	(0.00523)	(0.0161)
1	-5732***	-1.073***	-0.0458***	-2836***	-0.224***	-0.00374	-0.157***
	(310.1)	(0.0720)	(0.00859)	(364.3)	(0.0524)	(0.00665)	(0.0224)
2	-2938***	-0.596***	-0.0259***	-1880***	-0.113*	-0.00369	-0.0859***
	(339.4)	(0.0781)	(0.00973)	(441.6)	(0.0614)	(0.00749)	(0.0253)
3	-1517***	-0.325***	-0.0205*	-1127**	-0.0586	0.000597	-0.0629**
	(381.6)	(0.0864)	(0.0105)	(512.8)	(0.0656)	(0.00831)	(0.0285)
4	-1280***	-0.276***	-0.0189	-1057*	-0.0244	0.00672	-0.0593*
	(429.2)	(0.0960)	(0.0116)	(587.1)	(0.0745)	(0.00903)	(0.0321)
5	-1288***	-0.321***	-0.0187	-917.8	-0.0626	-0.00393	-0.0695*
	(490.9)	(0.108)	(0.0129)	(686.0)	(0.0856)	(0.0103)	(0.0366)
Constant	14127***	3.510***	0.249**	3650**	6.653***	-0.0374	-0.468***
	(1900)	(0.713)	(0.106)	(1701)	(0.329)	(0.0360)	(0.172)
Observations	47709	47709	47709	47673	47673	47673	47673
R-squared	0.278	0.126	0.101	0.121	0.094	0.098	0.183
Number of id	3079	3079	3079	3078	3078	3078	3078
Calendar Year FE	YES	YES	YES	YES	YES	YES	YES
Child-Age FE	YES	YES	YES	YES	YES	YES	YES
Parent-Age FE	YES	YES	YES	YES	YES	YES	YES
Additional controls	YES	YES	YES	YES	YES	YES	YES

Table 9: Some results using additional control variables. Controls included: child being born, parent lives with the child, family lives in a big city. All estimates include individual fixed effects. Clustered (family level) standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

F Cancers in the sample

Lip, oral cavity and pharynx	110
Digestive organs	89
Respiratory and intrathoracic organs	34
Bone and articular cartilage	124
Melanoma and other malignant neoplasms of skin	129
Mesothelial and soft tissue	199
Breast	18
Female genital organs	28
Male genital organs	45
Urinary tract	159
Eye, brain and other parts of central nervous system	647
Thyroid and other endocrine glands	181
Ill-defined, other secondary and unspecified sites	36
Hodgkin's disease	144
Non-Hodgkin's lymphoma	146
Other lymphomas	45
Lymphatic leukaemia	777
Multiple Myeloma	8
Myeloid Leukaemia	121
Other leukaemia	82
Overall	3130

Table 10: Cancers in the estimation sample, by location/classification.

G ICD10-codes

The ICD10 Codes used in categorizing the cancers are the following:

Table 11

ALL and LBL:	C910, C8340, C8341, C8351, C8352, C88359
Central Nervous System Tumours:	C7
Other cancers:	Other C-codes (only malignous tumours)

H Distribution of effects

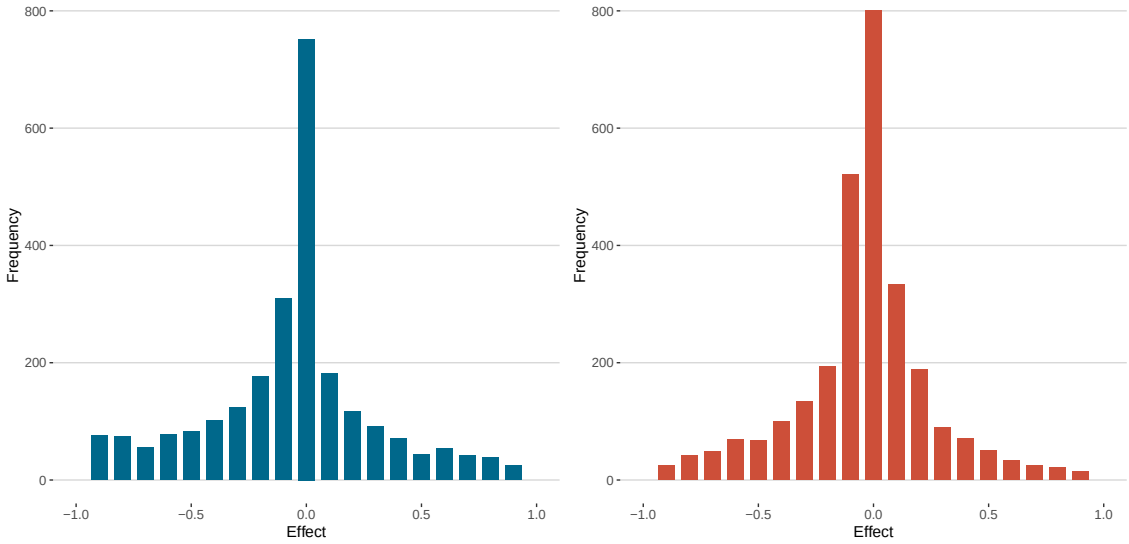

Figure 13: Effects on mothers' (left) and fathers' (right) individual earnings. Calculated by $\frac{Y^{-1}-Y^{1}}{\tilde{Y}^{t}}$, where $\tilde{Y}^{t}$ is the wage predicted by calendar year, age and child age.

I Results without Age Fixed Effects

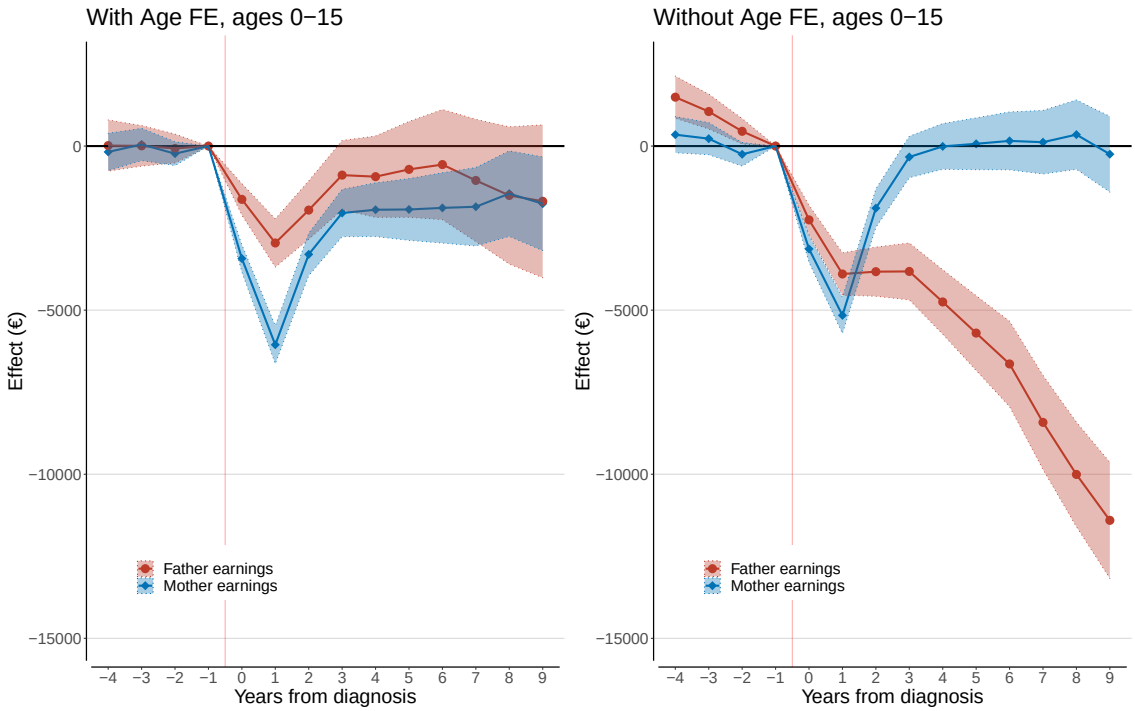

Figure 14

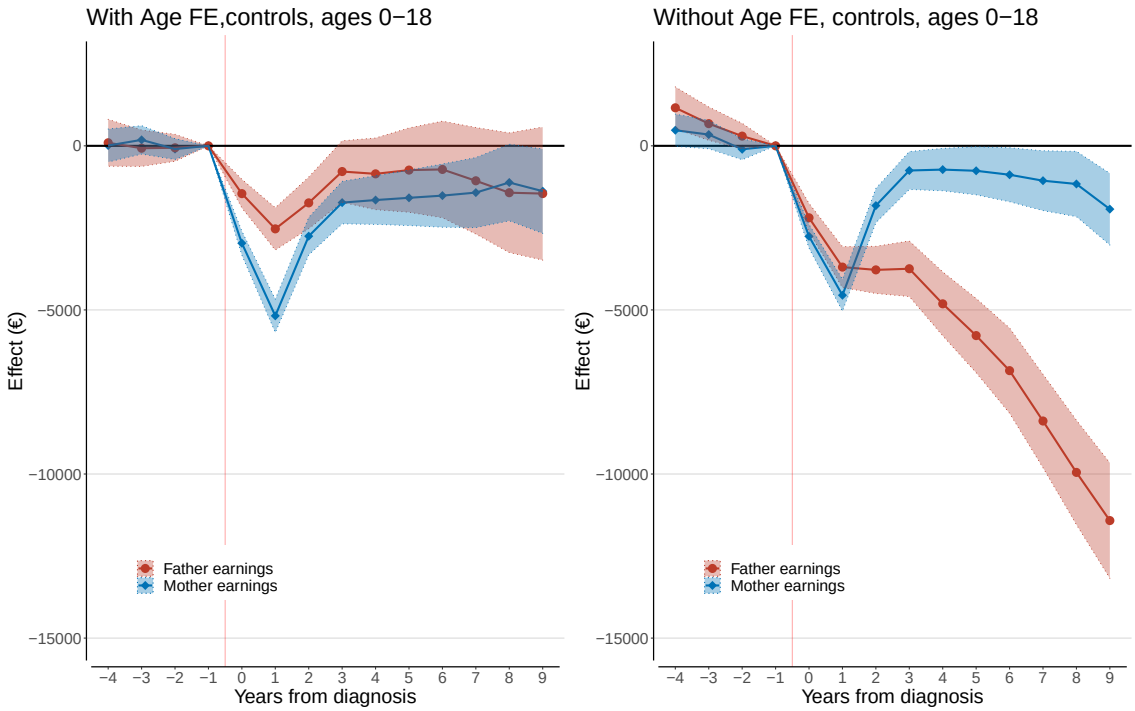

Figure 15

J Survival of the child

To examine the extent that the results are driven by the families experiencing a fatal shock, I estimate results separately based on the child's survival. There are still significant effects even when the fatal cases are dropped from the sample. This together with the results regarding the severity of cancer indicate that any cancer diagnosis does indeed have a significant effect on parental outcomes, and the results are not caused by a small portion of serious cases.

The death of a child is without a doubt a huge shock for the family. The cases leading to the death of the child can also be assumed to be among the most serious cancers that would have a large impact on the family even if the child survived, so the effect of the death is hard to quantify precisely. Regardless, diagnoses that prove fatal have huge impact on the labour market outcomes, as shown in Figure 16.

The difference between fatal and nonfatal shocks is considerably larger for mothers' than fathers. In fact, for fathers there seems to be a positive effect on earnings some years after the shock. The effect seems to take place in multiple years instead of being just a single outlier. In theory this could be increased labour supply compensating for mothers' decreased earnings, but I will refrain from drawing strong conclusions from one barely significant coefficient, since testing large amounts of coefficients will cause some of them to inevitably be significant by pure chance. On the other hand, the effect can be observed in multiple years and the confidence intervals for this group are so large that to be significant, the effect would have to be quite large.

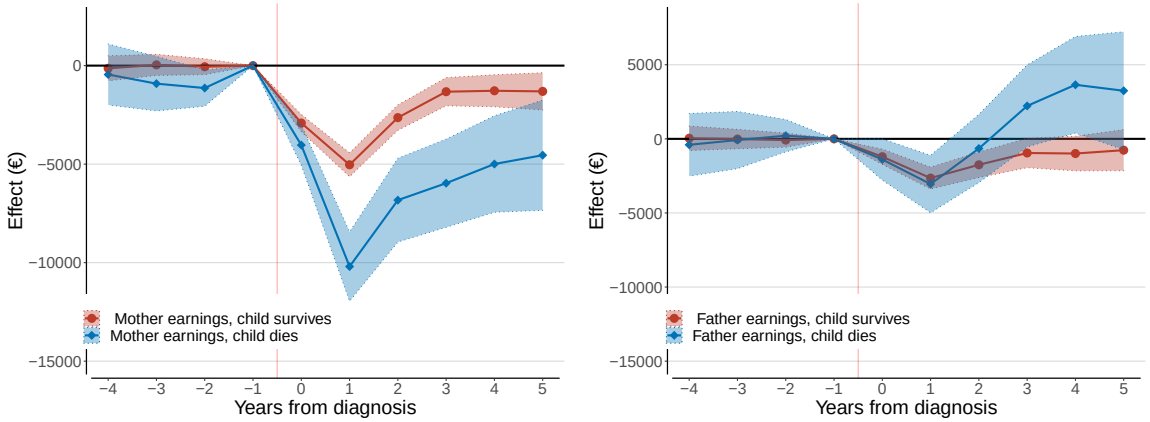

Figure 16: Effect on parental earnings by the survival of the child. Period $l = -1$ excluded, treatment happens at $l = 0$. 95 % confidence interval included.