

Supplemental Appendix: Effects of Parental Death on Labor Market Outcomes

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A Supplementary outcomes and robustness checks

A.A Inheritance

Our empirical design focuses on the first parental death. As described in Section II, after the first parental death, the entire estate of the deceased parent is usually held by their spouse for married couples. Thus, by focusing on the first parental death, this channel is less relevant in driving the effect of parental death on adult children’s labor market outcomes. However, to ensure that bequest is not a significant driver of our results, we conduct a heterogeneity analysis exploiting information on parental wealth before death (bequests are not observed directly). We regress adult children’s earnings on the interaction term between a treatment indicator and parental wealth one year before the first parental death. If parents are wealthy, adult children tend to receive more bequests. If bequests are driving the earnings decline, we would expect the interaction term to be negative.

We report the results by gender of parental death and adult children in Table B.4. We find no meaningful impact of any of the interaction terms on adult children’s earnings. As an alternative, we also interact the treatment indicator with adult children’s own assets and regress the interaction term on adult children’s earnings. The coefficient of the interaction term measures the impact of own wealth increase on adult children’s earnings after parental death. We still do not find an economically meaningful impact on individual earnings. Collectively, this evidence suggests that the bequest channel does not drive the decline in earnings after the first parental death.²⁹

A.B Effects on cohabitation and fertility

Parental death could also affect adult children’s labor market outcomes by influencing family behavior. For example, if parental death changes cohabitation and marital status, or affects fertility,

²⁹As a further robustness check, we also replicate the results by only using the wealth of the deceased parents and the wealth of the widowed parents separately. The interaction between the treatment and wealth of the widowed parents can provide suggestive evidence if widowed parent uses inter-vivo transfers to induce more care from their adult children, and thus, affect adult children’s labor market outcomes. We do not find any effects supporting this argument.

we would expect resulting effects on adult children's labor market outcomes. Figure B.17 shows the effect of parental death on fertility and cohabitation.

We find an economically small and negative effect of mothers' or fathers' deaths on men's and women's cohabitation status. Additionally, we find that first parental death reduces the fertility rate of adult children. Compared to matched controls, the total number of children is 0.015 (0.02) lower for men (women) after losing a mother, while the total number of children is around 0.006-0.008 lower after paternal death. Since higher fertility is associated with lower employment and earnings, particularly for women, a reduction in fertility would predict an increase in employment and earnings. Similarly, marriage and cohabitation are typically associated with lower employment and earnings, so a reduction in cohabitation status would also predict an increase in employment and earnings. These findings suggest that changes in relationship status and fertility behavior cannot explain the observed deterioration in labor market outcomes for men and women after parental death.

A.C Effects on spouses

Parental death may not only affect the labor market outcomes of daughters and sons, it could also have a spillover effect on the labor market outcomes of daughters-in-law and sons-in-law. In this subsection, we explore the spillover effect of parental death on the earnings of daughters-in-law and sons-in-law. Because the treated individuals are men and women who lost their first parent-in-law, the sample is restricted to those who cohabit or are married. Our control group is again obtained through nearest-neighbor matching. For each treated individual, we identify an observationally similar matched control who is also married or cohabiting, but who does not lose either of their parents-in-law in the sample window. When obtaining the matched controls, we also control for whether the individual's own parents are still alive to ensure that the two groups are comparable in terms of their own parent death status.

We find that the death of parents-in-law has a negative impact on the earnings of men and women with young children (children below age 6), which is shown in Figure B.18. Specifically, compared to men, women with young children experience a larger decline in earnings, more than 2%. This suggests that the death of parents has a greater spillover effect on female spouses through the informal childcare channel.

A.D Preference change: Type of work and leisure

Parental death could also affect labor market outcomes by altering adult children's preferences for work. Specifically, a body of literature outside economics considers the impact of bereavement on individuals' values in life (Lehman et al., 1993; Park, 2010; Umberson, 2003). Based on these discussions, parental death and the associated grief could influence individuals' preferences or values, potentially leading them to make different choices, such as prioritizing time with family over work.

First, after parental death, adult children may switch to occupations or positions they enjoy more, even if they earn less. To test whether this channel is operative, we examine the impact of parental death on job changes and present the results in Table B.5. We find no economically meaningful effect of parental death on occupational switching or transitions between firms and sectors, suggesting that parental death is unlikely to affect labor market choices by altering preferences for certain types of jobs.

Second, after losing a family member, people may value time spent with the rest of their family more, leading them to adjust their relative preference between the economic benefits of work and leisure. If this were the case, we would expect adult children to be more likely to form a family and have more children to enjoy time with family. However, as described in Section A.B, we do not find evidence of such an effect. Taken together, our results do not lend support to the hypothesis that parental death influences adult children's labor market outcomes through altered preferences for specific job types or leisure.

A.E Heterogeneity analysis

We conduct additional heterogeneity analyses to examine the effect of parental death based on whether parents and adult children live in the same region, the reasons for parental death, the ages of both parents and children at the time of death, and different time periods. The results are presented in Figure B.19 and Table B.6.

We find that if parents and adult children live in the same region before parental death, the effect of parental death on adult children's earnings is greater (Figure B.19). To further examine this result, we create a categorical variable based on an intensive measure of distance, specifically whether parents and adult children live in: 1) the same municipality (99 regions), 2) the same NUTS3 region (but not the same municipality; there are 11 NUTS3 regions in Denmark), 3) the same NUTS2 region (but not the same NUTS3 region; there are 5 NUTS2 regions in Denmark), or 4) a greater distance. This approach gives us four levels of distance, and we find that the negative

earnings effects of parental death decrease monotonically with regional distance (Figure B.20). This result can be explained by both the family support and mental health channels. In particular, parents are more likely to provide informal childcare and receive eldercare if they live close to their adult children. Additionally, living in the same region may suggest a stronger relationship between parents and children, meaning that adult children may suffer more from mental health problems after parental death.

Furthermore, we find that the effects of parental death are similar across different causes of death, such as heart attack, stroke, car accident, etc., suggesting no heterogeneous effects conditional on sudden death (see Table B.6, Columns 1 and 2). We also find a larger impact of parental death when parents die at a relatively younger age and when children are relatively young at the time of parental death (see Table B.6, Columns 3-6). This could be due to the family support and mental health channels: parents are more likely to provide informal childcare when they are young and healthy; adult children suffer more mental health problems when parents die unexpectedly at a relatively young age; and young children are more likely to need informal childcare from parents and may struggle more mentally with parental death. Finally, we find that the effects are comparatively larger in more recent time periods (after 2005).

A.F Alternative specification

As an alternative to the combination of fixed effects and a control group of matched nearest-neighbors, another widely used identification strategy relies on a control group of individuals who experience the same treatment but Δ years later. This approach is applied in, for example, Fadlon and Nielsen (2019, 2021). The control groups in Fadlon and Nielsen (2019, 2021) consist of individuals who will be treated in the future, outside the estimation window. Our combination of fixed effects and a control group of matched nearest-neighbors is similar to the approach used in Fadlon and Nielsen (2019): Our control group does not lose either of their parents within the estimation window, but they will lose their parents in the future, though it may be due to sudden or non-sudden causes. In the data section, we show that the group of children whose parents die suddenly is very similar to the group whose parents die non-suddenly.

One may still worry that even if adult children whose parents die suddenly appear similar to those whose parents die expectedly on aggregate, the anticipatory effect of non-sudden deaths could still bias our results. For example, if we find a matched control for a treated individual whose parent dies suddenly, and the control individual's parents are both alive during the window but are seriously ill, the parental illness could generate an anticipatory effect for the control group.

This anticipatory effect may lead the matched control to downward adjust their employment and earnings in response to the parental illness. If this is the case, comparing the treated individual’s employment and earnings behavior to their matched controls would result in a downward-biased estimate of the negative impact of parental death. As such, our match-control estimates would provide a lower bound of the true effect.

As a robustness check, we follow [Fadlon and Nielsen \(2021\)](#) and apply their alternative empirical strategy, which relies on future-treated individuals as the control group. We define individuals born in the same cohort using a 5-year age bin and consider a time gap in treatment of 6 years. For example, we use individuals whose first parent died suddenly in 2010 as the control group for those who lost their parents in 2004. We then compare the labor market trajectories of the two groups over the period from 2001 to 2009. We estimate the same regression equations as in our main specifications and report the results in Figures [B.21](#) and [B.22](#). Specifically, Figure [B.21](#) compares the employment and earnings results obtained using the matched control group with those using the soon-to-be-treated individuals as controls. We find that the point estimates from the two specifications are similar, and their confidence intervals overlap. We also observe a more pronounced gender difference in the participation margin, as shown in panels (a) and (b).

In the context of our paper, we prefer a combination of fixed effects and a control group of matched nearest-neighbors because it allows the treatment and control to be more comparable among all observables, which ensures the precision of the main analysis and heterogeneity analyses performed on various subsamples of our data.

A.G External validity

In this section, we first provide evidence suggesting that the effect of parental death may extend to non-sudden deaths and then discuss the implications of our findings for understanding the impact of parental death in other countries.

In Figure [B.23](#), we present event studies for first, *non*-sudden parental deaths, similar to those for sudden deaths shown in Figure [B.5](#). Quantitatively, the average reductions in labor supply and earnings following *non*-sudden parental deaths are very similar to those following sudden deaths. Furthermore, as shown in Panel (d), there is a sharp increase in mental health-related prescriptions at the time of parental death. These descriptive results suggest that the effects of parental death are very similar regardless of whether the cause is sudden or *non*-sudden. However, in contrast to Figure [B.5](#), Figure [B.23](#) reveals clear evidence of pre-trends. The increase in mental health prescriptions and the negative effects on labor market outcomes appear to begin

even before *non*-sudden parental deaths. These pre-trends likely reflect the fact that parents dying of non-sudden causes are often ill for a significant period before death, and parental illness may influence child outcomes.

In this paper, we use administrative data from Denmark, an OECD country with extensive publicly provided childcare and eldercare. Denmark is comparable to other EU countries in terms of GDP, longevity, public provision of childcare and eldercare, and its healthcare system. Therefore, we would expect the effects of parental death to have external validity for other EU countries with similar institutional frameworks, such as Norway, Sweden, Finland, the Netherlands, and France.

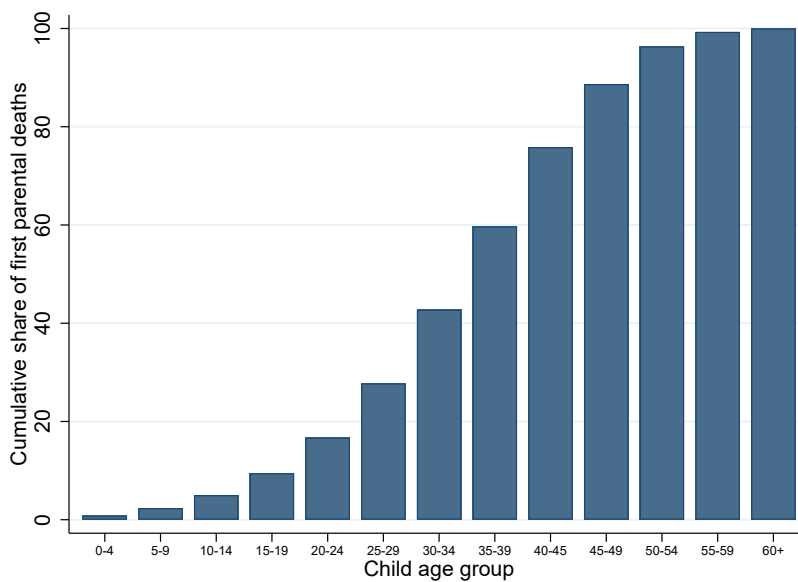
In the context of other countries, where there is limited publicly provided childcare or eldercare, as well as a weaker healthcare system (including mental health support), we anticipate that the effects of parental death would be more pronounced compared to Denmark. Specifically, we expect people in these countries to face larger labor market penalties due to: 1) the loss of informal childcare, as there is less public childcare available; 2) the added burden of caring for remaining parents, due to inadequate pension and long-term care systems; and 3) the deterioration of mental health, given the lack of sufficient mental health services. Thus, we believe our estimates could serve as an informative lower bound when considering the effects of parental death in other countries.

Overall, we argue that studying these questions within the context of the current data and framework is not only important in its own right but is also likely to carry significant implications for understanding similar effects in other contexts.

B Additional figures and tables

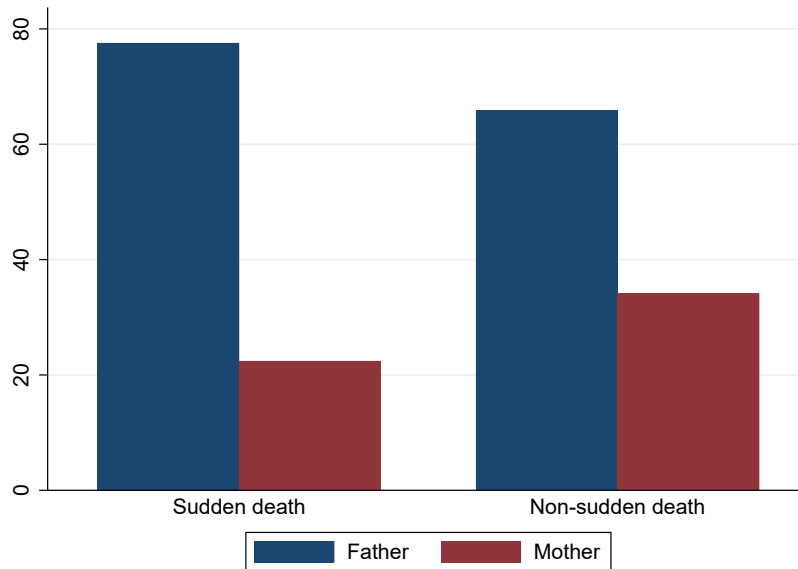
B.A Additional descriptives

Figure B.1: Percentage of people with deceased parents by age: Denmark



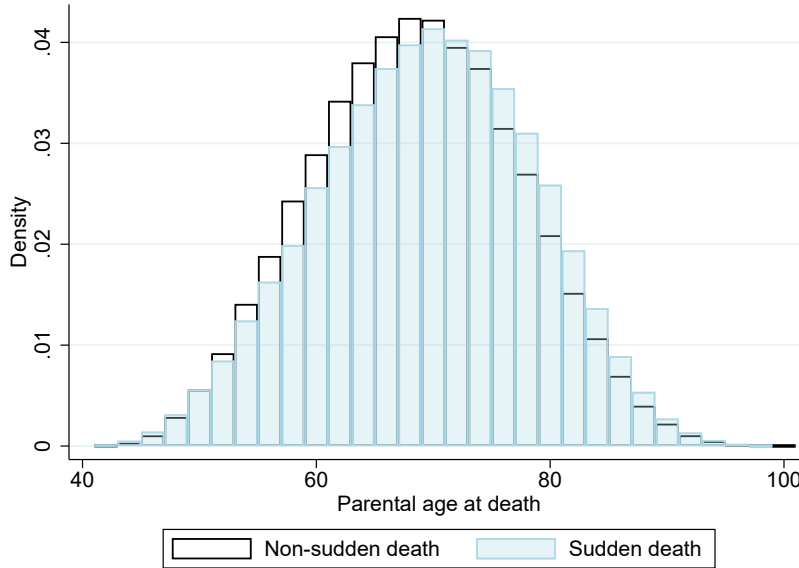
Notes: This figure shows the cumulative percentage of people who have lost their parents by age between 1985 and 2014. We consider all causes of death for this figure. Source: Danish population registry, BEF, and the cause of death registers, DODSAARS/DODSAARG.

Figure B.2: Gender of first deceased parent



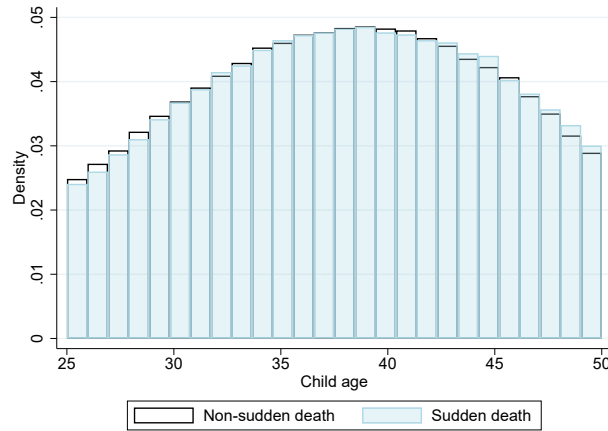
Notes: This figure presents the gender composition of the first deceased parent by sudden deaths vs. nonsudden death. Sudden deaths include heart diseases, cerebrovascular diseases, and traffic or other (external) accidents. Non-sudden deaths include the remainder of deaths. We include the first parental deaths occurring between 1985 and 2014 for children aged 25-50 in the year of the first parental death and with two known parents. See Table [B.1](#) for details on the sample.

Figure B.3: Age of first deceased parent distribution



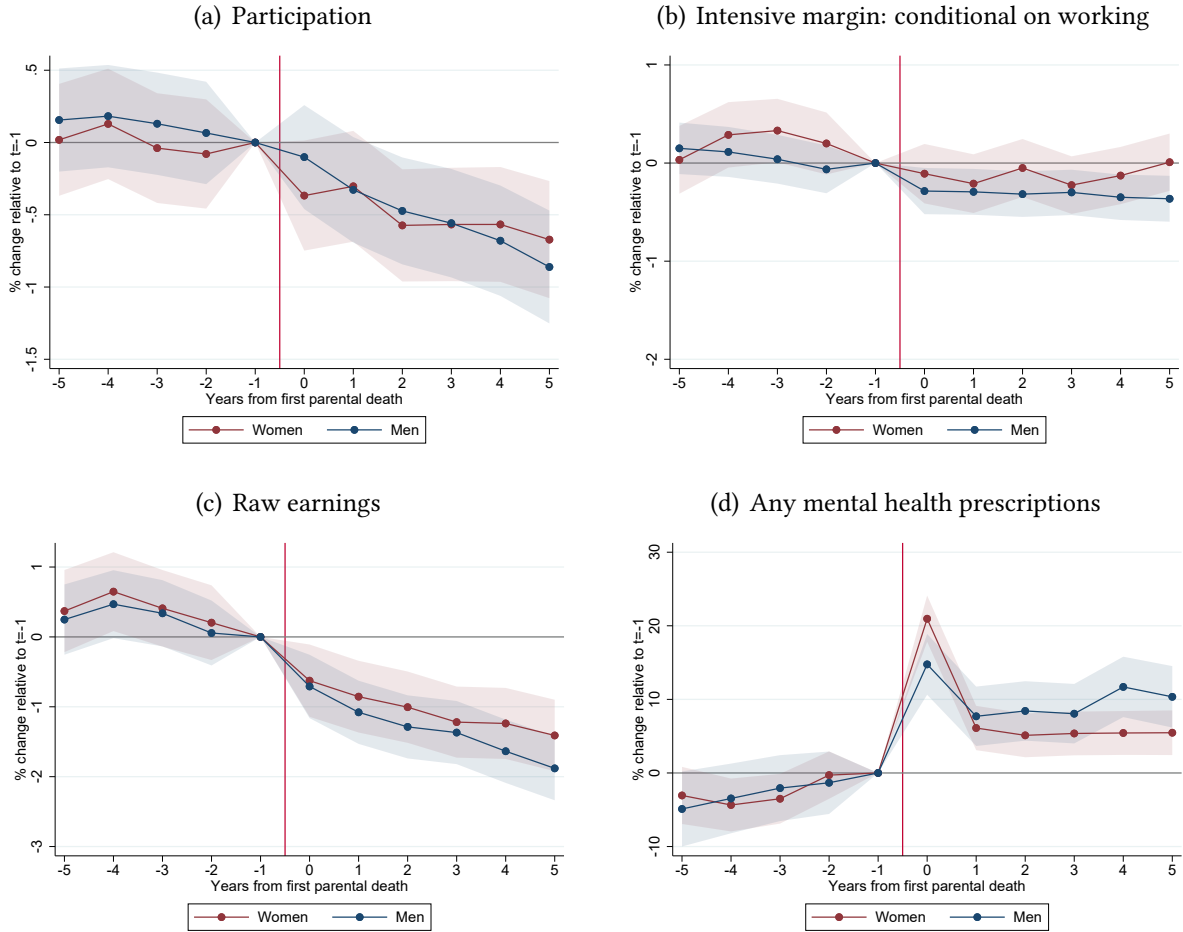
Notes: This figure plots the age distribution of the first deceased parent by sudden vs. nonsudden death. We include the first parental deaths occurring between 1985 and 2014 for children aged 25-50 in the year of first parental death and with two known parents. See Table [1](#) for sample sizes. Bars including fewer than 5 individuals are dropped due to data confidentiality restrictions.

Figure B.4: Adult children age distribution when first parent deceased



Notes: This figure plots the age distribution of adult children at the time of first parent death and by first parent sudden vs. nonsudden death. We include the first parental deaths occurring between 1985 and 2014 for children aged 25-50 in the year of first parental death and with two known parents. See Table [1](#) for sample sizes. Bars including fewer than 5 individuals are dropped due to data confidentiality restrictions.

Figure B.5: Descriptives: Effect of sudden parental death



Notes: This figure plots the estimated coefficients from Equation 1 for men's and women's labor market outcomes, and if using any mental health prescriptions. We follow Kleven, Landais and Sogaard (2019, p. 188) and convert the estimated coefficients into percentage change relative to the baseline. Participation is measured as strictly positive ATP contributions. The intensive margin is based on ATP contributions, similar to Kleven, Landais and Sogaard (2019). ATP-pension contributions are paid proportionally to hours worked. Earnings are inflated to 2020 levels and include earnings from both employment and self-employment. The sample consists of all sudden, first parental deaths from 1985 to 2014 for children aged 25-50 in the year of first parental death and with two known parents. The prescription data are available from 1995. 95%-confidence intervals indicated.

Table B.1: Summary statistics for adult children at $t = -1$, split by gender and suddenness of first parental death

	First death mother		First death father	
	Sudden mean	Non-sudden mean	Sudden mean	Non-sudden mean
Age	37.65	36.67	36.78	36.99
Male	0.53	0.52	0.53	0.52
Share with college or above	0.20	0.22	0.21	0.22
Share with high school	0.49	0.52	0.51	0.52
Share without high school	0.31	0.27	0.28	0.26
Cohabitation	0.71	0.70	0.71	0.70
Number of children	1.43	1.37	1.38	1.39
Age of youngest child	8.43	7.69	7.79	7.79
Share with children under 6	0.32	0.34	0.34	0.34
Mother age	66.82	64.12	65.05	64.36
Father age	69.39	66.85	69.63	68.49
Mother married	0.83	0.80	0.85	0.81
Father married	0.87	0.85	0.85	0.81
First death age	67.82	65.12	70.63	69.49
Employment	0.83	0.84	0.84	0.84
Intensive margin	1018.54	1017.14	1015.71	1021.54
Annual earnings	293.35	300.22	297.79	304.95
N	46273	180149	160013	347908

Notes: This table shows the summary statistics for children in the calendar year preceding first parental death, split by both parental gender and suddenness of parental death. We include the first parental deaths occurring between 1985 and 2014 for children aged 25-50 in the year of first parental death and with two known parents. All statistics are derived from Danish population-level register data.

Table B.2: Summary statistics for adult children at $t = -1$ for sudden, first parental deaths

	First death mother: Daughters mean	First death father: Daughters mean	First death mother: Sons mean	First death father: Sons mean
Age	37.59	36.73	37.71	36.83
Male	0.00	0.00	1.00	1.00
Share with college or above	0.24	0.26	0.16	0.17
Share with high school	0.45	0.46	0.53	0.55
Share without high school	0.31	0.28	0.31	0.28
Cohabitation	0.74	0.74	0.68	0.68
Number of children	1.57	1.51	1.31	1.26
Age of youngest child	9.02	8.35	7.81	7.20
Share with children under 6	0.32	0.35	0.32	0.34
Mother age	66.77	64.99	66.86	65.10
Father age	69.33	69.56	69.44	69.69
Mother married	0.83	0.85	0.83	0.86
Father married	0.87	0.84	0.87	0.85
First death age	67.77	70.56	67.86	70.69
Employment	0.83	0.84	0.83	0.84
Intensive margin	978.30	974.07	1054.56	1052.33
Annual earnings	243.13	246.58	338.55	342.96
N	21919	74991	24354	85022

Notes: This table shows the summary statistics for children in the calendar year preceding a sudden and first parental death, split by both child and parent gender. We include the first parental deaths occurring between 1985 and 2014 for children aged 25-50 in the year of the first parental death and with two known parents. All statistics are derived from Danish population-level register data.

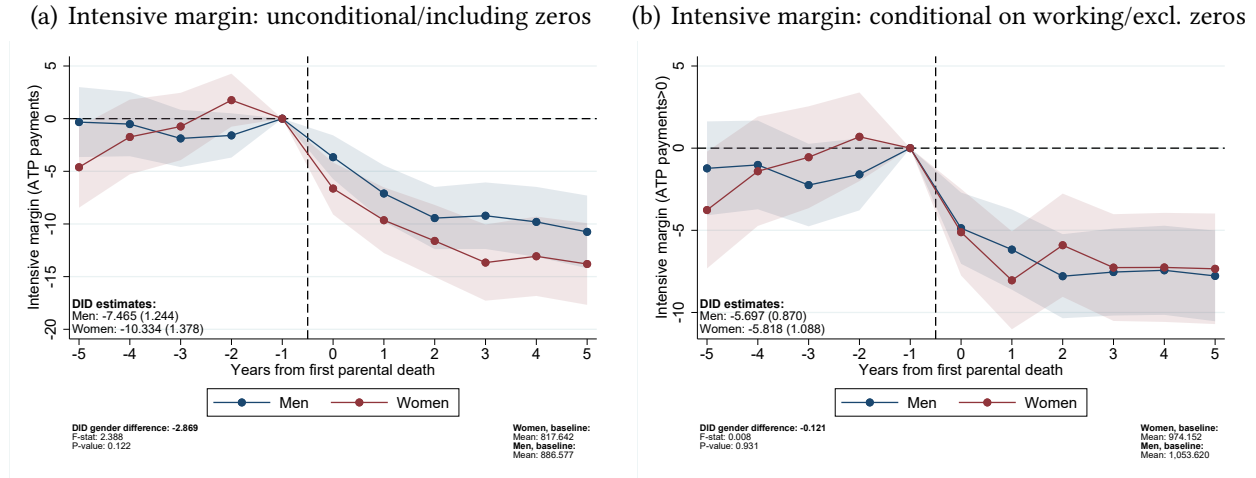
Table B.3: Summary statistics for treatment and their matched controls at $t = -1$

	Treatment	Control
<i>Panel A: Targeted variables</i>		
Age	36.979	36.966
Male	0.530	0.530
Share with college or above	0.207	0.209
Share with high school	0.506	0.536
Share without high school	0.287	0.255
Cohabitation	0.708	0.726
Number of children	1.391	1.386
Age of youngest child	7.936	7.841
Share with children under 6	0.336	0.338
Mother age	65.444	65.253
Father age	69.574	69.195
Employment	0.840	0.840
Intensive margin	854.191	854.192
Annual earnings (1000 DKK)	299.973	304.434
Public sector	0.286	0.277
<i>Panel B: Non-targeted variables</i>		
Mother any hospital	0.548	0.499
Father any hospital	0.642	0.481
No. GP visits	5.571	5.262
Any hospital visits	0.318	0.301
Any psychologist visits	0.010	0.007
Any psychiatrist visits	0.012	0.012
Any mental health pres.	0.111	0.103
Any alc. abuse pres.	0.007	0.005
Any opioid pres.	0.052	0.044
N	206282	206282

Notes: This table presents summary statistics for treated individuals and their matched controls one year before the parental death of the treated individual. We include first parental deaths occurring between 1985 and 2014 for individuals who were aged 25–50 at the time of the first parental death and had two known parents. All statistics are derived from Danish population-level register data.

B.B Additional main results

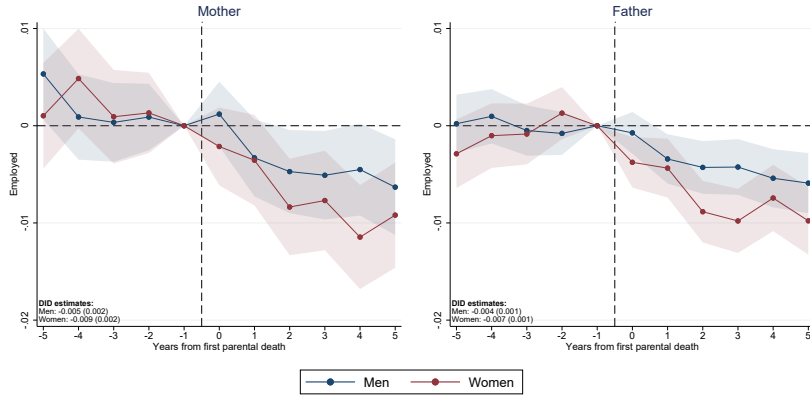
Figure B.6: Effect of parental death by gender: Intensive margins



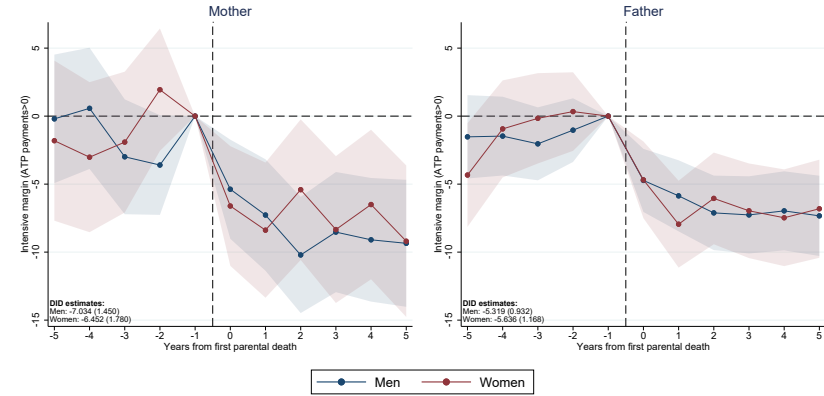
Notes: This figure plots the estimated coefficients from Equation 2 for men's and women's intensive margins of labor supply. Intensive margin is the amount of ATP contributions. ATP-pension contributions are paid proportionally to hours worked. We include those with no ATP-payments as zeros in Panel (a), and exclude them in Panel (b). The sample consists of all sudden, first parental deaths from 1985 to 2014 and their matched controls for children aged 25-50 in the year of first parental death and with two known parents. See Table B.2 for details on the sample. 95%-confidence interval indicated. Standard errors are clustered at the individual-by-match ID level.

Figure B.7: Effects by parental and child gender interaction

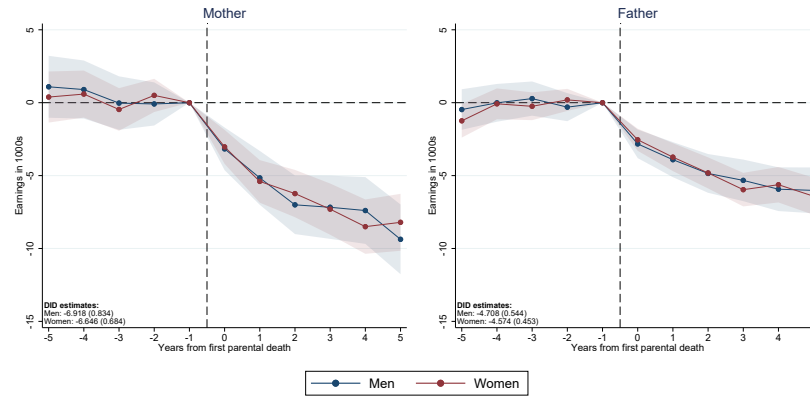
(a) Participation



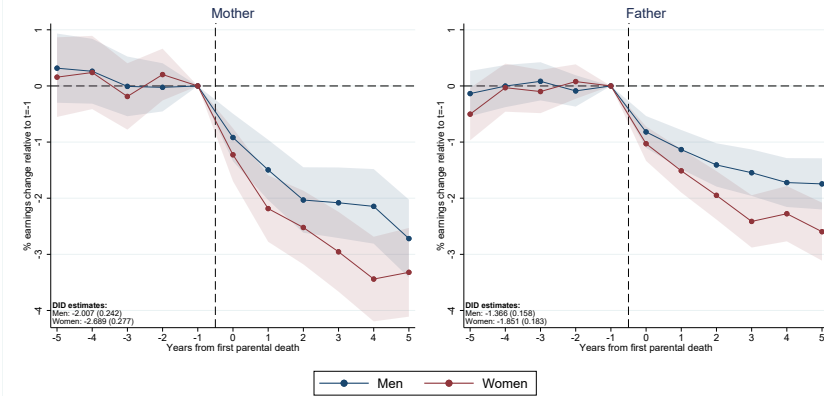
(b) Intensive margin: conditional on working



(c) Raw earnings

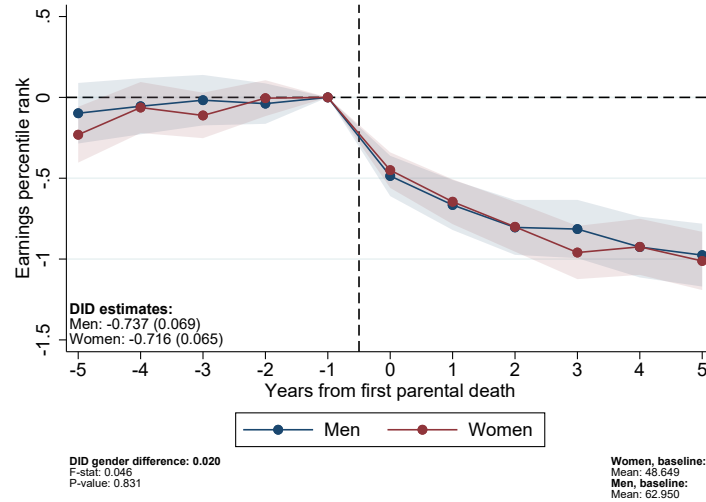


(d) Indexed earnings



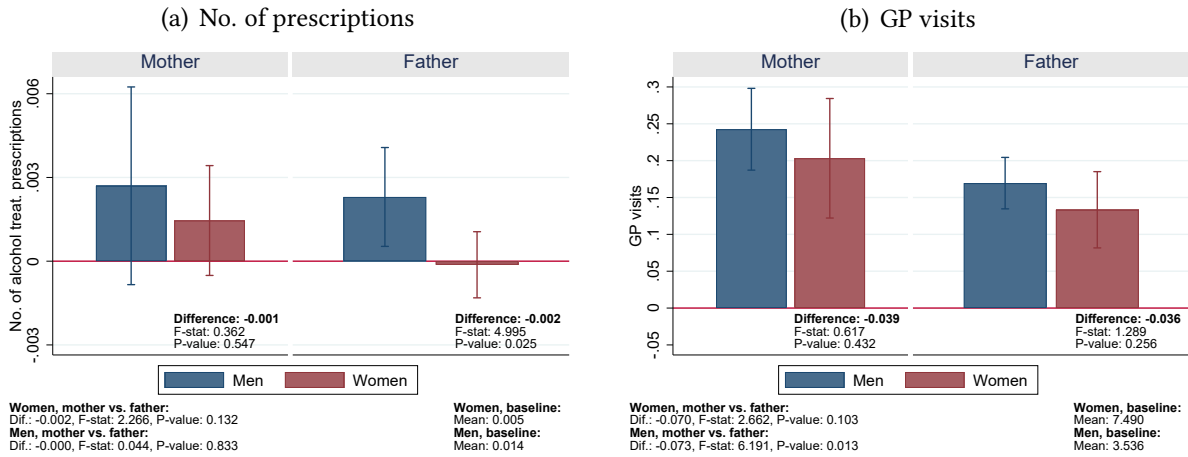
Notes: This figure plots the estimated coefficients from Equation 4 for men's and women's earnings. Participation is measured as strictly positive ATP contributions; intensive margin is the amount of ATP contributions. ATP-pension contributions are paid proportionally to hours worked. Earnings are indexed by the average earnings of men and women one year before parental death. The sample consists of all unexpected, first parental deaths from 1985 to 2014 and their matched controls for children aged 25-50 in the year of first parental death and with two known parents. Effects are within 5 years after the first parental death. 95%-confidence interval indicated. Standard errors are clustered at the individual-by-match ID level.

Figure B.8: Effect of parental death by gender: Earnings percentile ranks



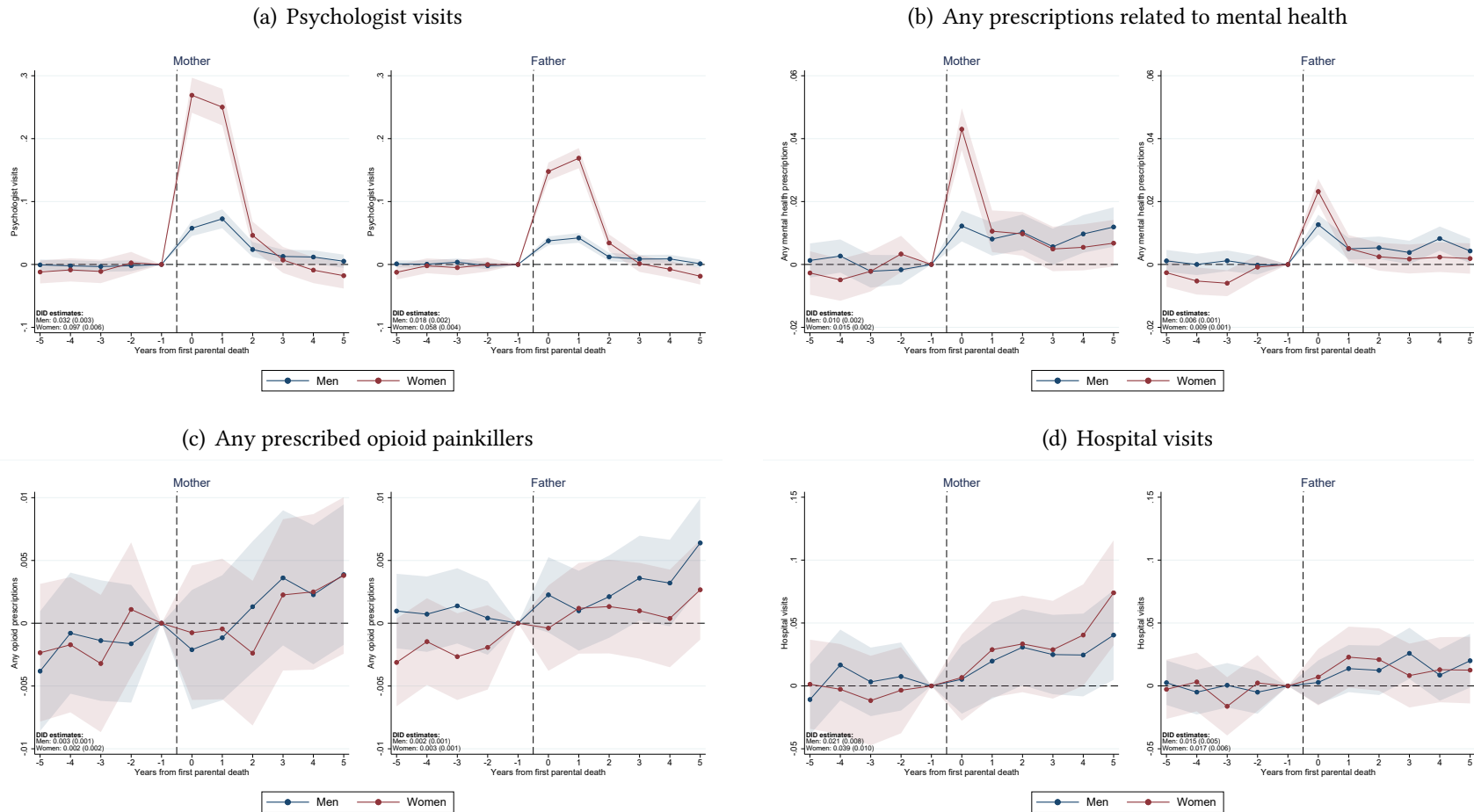
Notes: This figure plots the estimated coefficients from Equation 2 for men's and women's percentile earnings rank. Earnings are ranked within calendar years across the population aged 20-55. The sample consists of all unexpected, first parental deaths from 1985 to 2014 and their matched controls for children aged 25-50 in the year of first parental death and with two known parents. See Table B.2 for details on the sample. 95%-confidence interval indicated. Standard errors are clustered at the individual-by-match ID level.

Figure B.9: Treatment of alcohol addiction and GP visits, mothers' vs. fathers' deaths



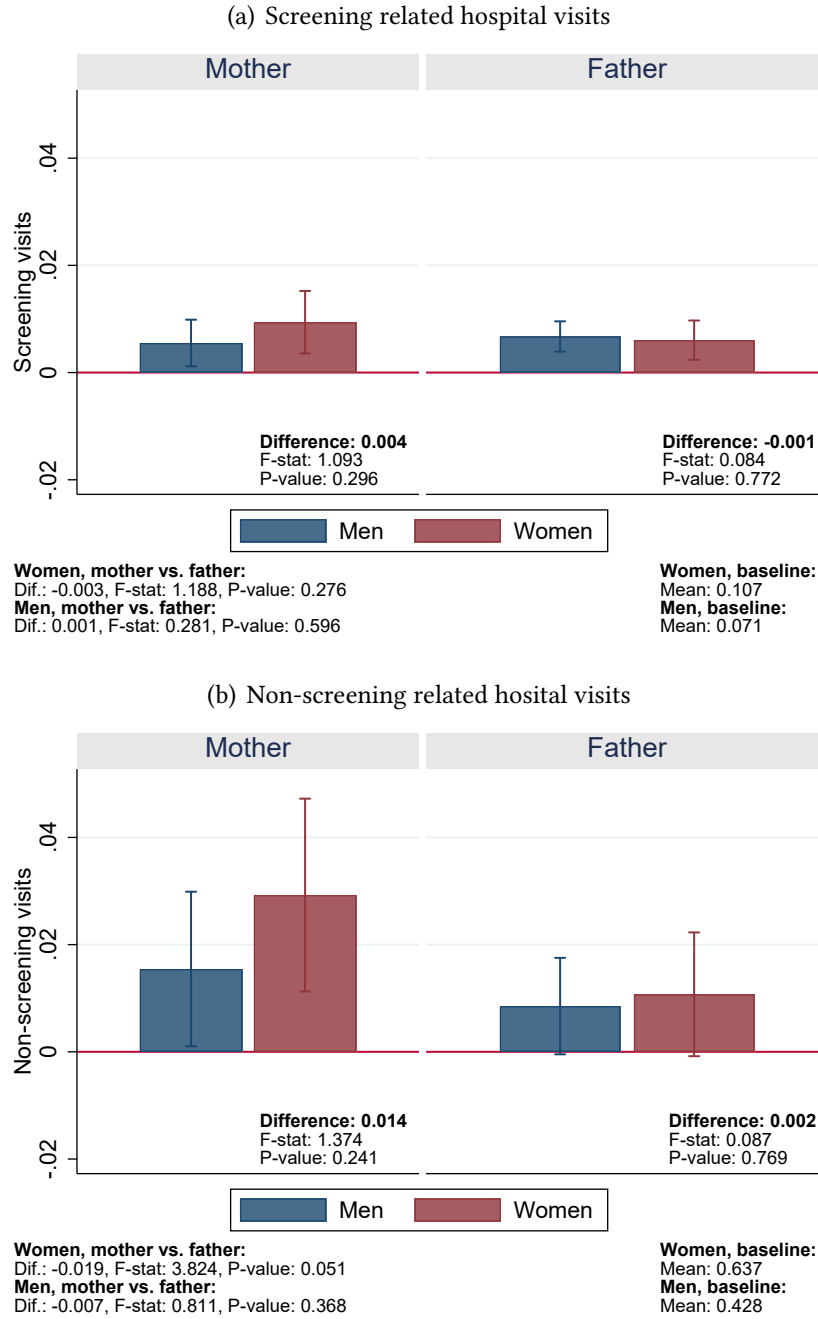
Notes: This figure plots the estimated coefficients from Equation 4 for men's and women's prescriptions related to alcohol addiction, as well as men's and women's visits to GP. All medical prescriptions are classified into ATC codes. We classify ATC-codes N07BB as prescriptions related to the treatment of alcohol addiction. Effects are within 5 years after the first parental death. 95%-confidence interval indicated.

Figure B.10: Event Study: Psychologist visits, prescriptions, and hospital visits, mothers' vs. fathers' deaths



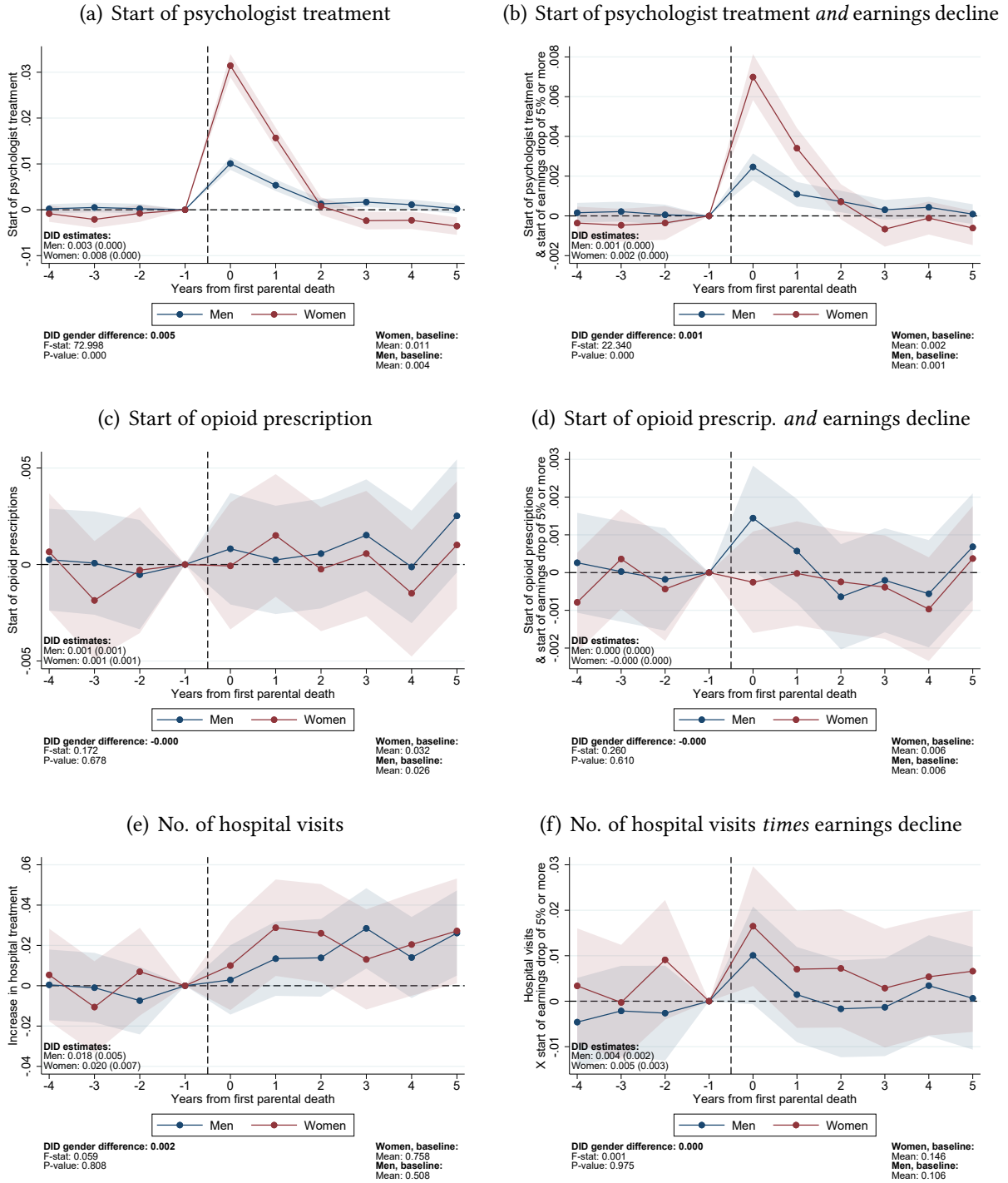
Notes: This figure plots the estimated coefficients from Equation 2 for men's and women's psychologist visits, any prescriptions related to mental health, any prescribed opioid painkillers, and hospital visits. Data on psychologist visits are available from 1990-2019. All medical prescriptions are classified into ATC codes. We classify ATC-codes N05*, N06A*, N06B*, and N06C* as mental health related, ATC-codes N02A* as opioid painkillers. The prescription data are available from 1995. Hospital visits include both in- and outpatient visits at non-psychiatric hospital wards, and these data are available from 1994-2018. Effects are within 5 years after the first parental death. 95%-confidence interval indicated. Standard errors are clustered at the individual-by-match ID level.

Figure B.11: Screening vs. non-screening, mothers' vs. fathers' deaths



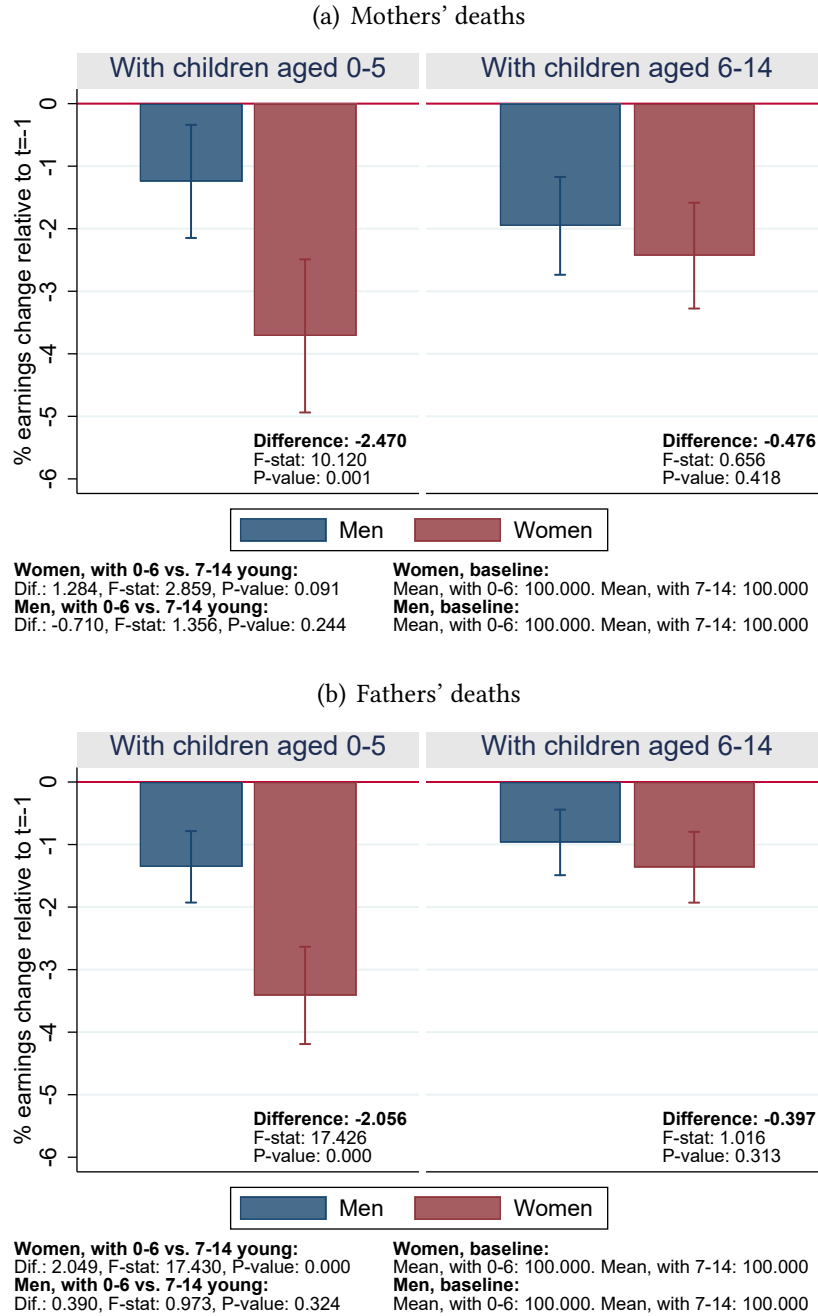
Notes: This figure plots the estimated coefficients from Equation 4 for men's and women's hospital visits, divided by screening and non-screening diagnoses. We consider the following ICD-10 codes (including their subcategories) as self-screening: "Z00: General examination and investigation of persons without complaint and reported diagnosis" and "Z01: Other special examinations and investigations of persons without complaint or reported diagnosis". Hospital visits include both in- and outpatient visits at non-psychiatric hospital wards, and these data are available from 1994-2018. Effects are within 5 years after the first parental death. 95%-confidence interval indicated.

Figure B.12: Co-occurrence of earnings declines and health treatment start



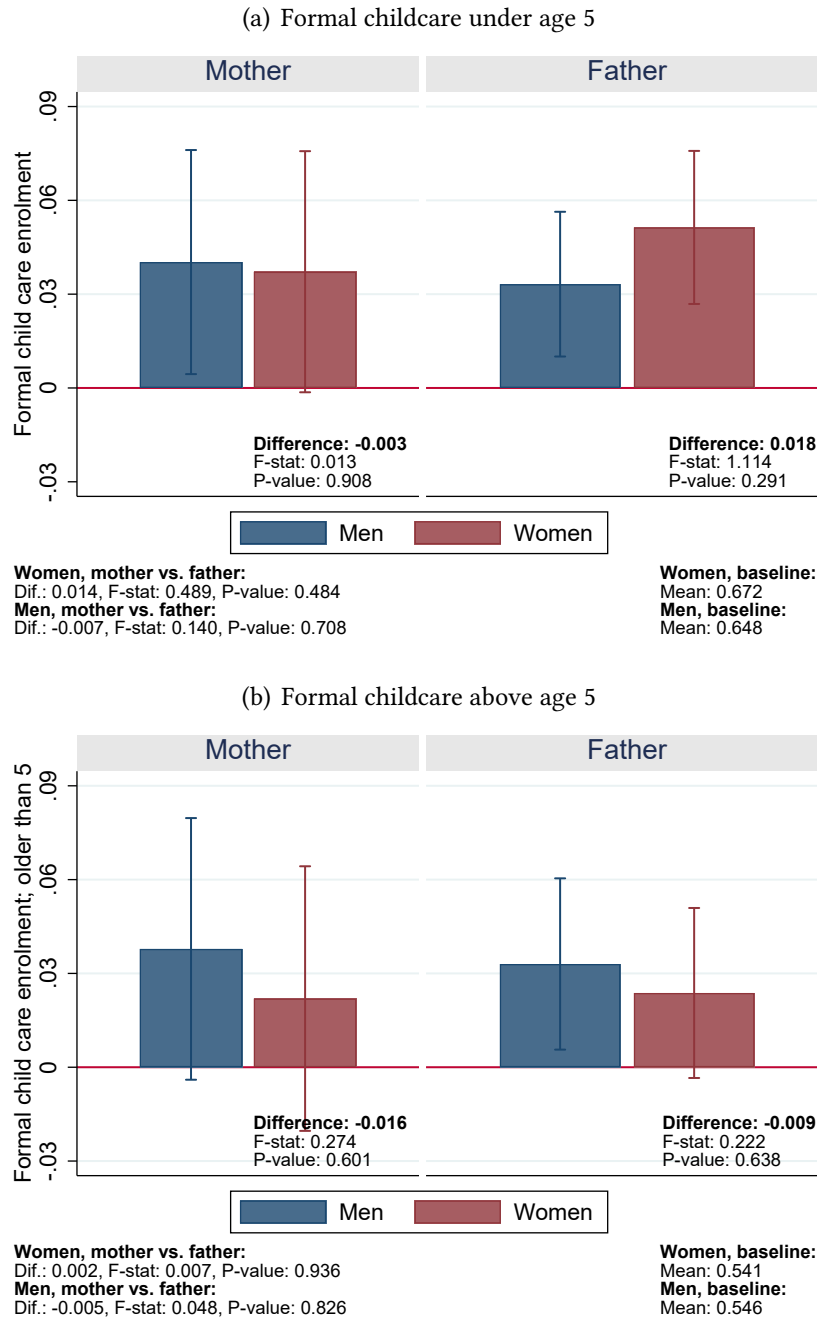
Notes: This figure plots the estimated coefficients from Equation 4 where outcomes are indicators for medical treatment (and counts of hospital treatment) as well as their interactions with the onset of earnings declines. We classify ATC-codes N05*, N06A*, N06B*, and N06C* as mental health related, ATC-codes N02A* as opioid painkillers. The prescription data are available from 1995, so we restrict all analyses in this figure to begin in 1995 for consistency. We drop year -5 to define start of treatment in year -4 before parental death. 95%-confidence interval indicated. Standard errors are clustered at the individual-by-match ID level.

Figure B.13: Earnings by child age group, mothers' vs. fathers' deaths



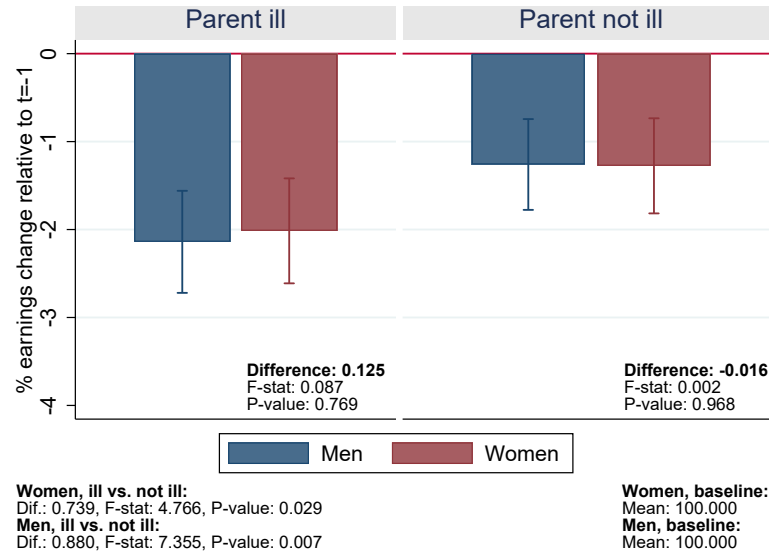
Notes: This figure plots the estimated coefficients from Equation 4 for men's and women's earnings, but where the sample is divided depending on whether individuals' youngest child is aged 5 or younger, or 6 to 14 years. Earnings are indexed by the average earnings of men and women one year before parental death. The sample consists of all unexpected, first parental deaths from 1985 to 2014 if the individual experiencing parental loss has a child aged 14 or younger. Effects are within 5 years after the first parental death. 95%-confidence interval indicated.

Figure B.14: Formal childcare, mothers' vs. fathers' deaths



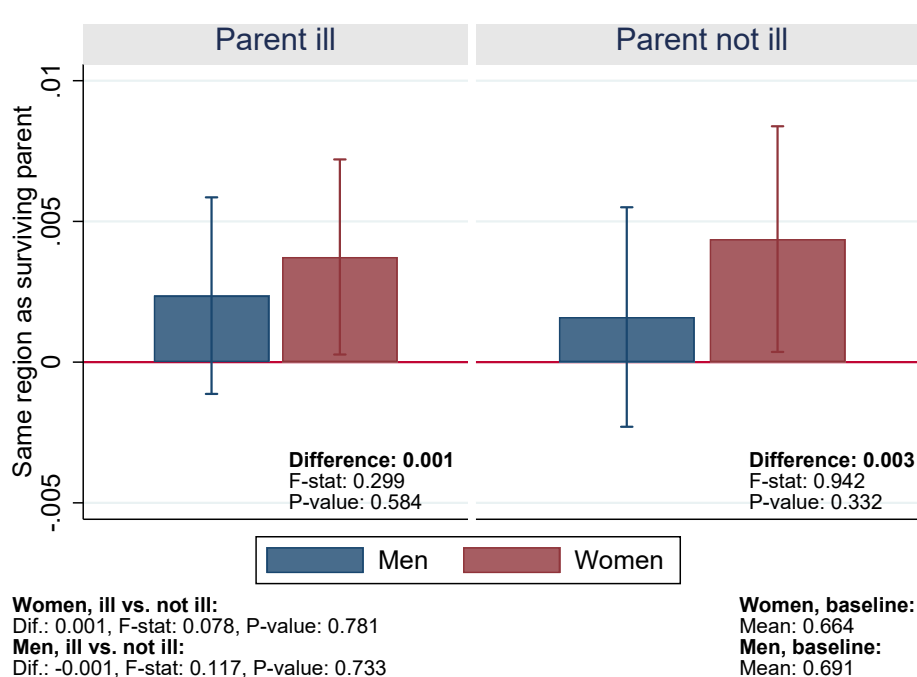
Notes: This figure plots the estimated coefficients from Equation 4 for men's and women's uptake of formal childcare. The data on formal childcare have low coverage before 2005, so we only include data from 2005-2019. Effects are within 5 years after the first parental death. 95%-confidence interval indicated.

Figure B.15: Earnings by surviving parents' health status before first parental death



Notes: This figure plots the estimated coefficients from Equation 4 for men's and women's earnings. Earnings are indexed by the average earnings of men and women one year before parental death. We divide the sample by the surviving parents' health status, based on their hospital visits 5 years before the first parental death. Hospital visits include in- and outpatient visits at non-psychiatric hospital wards; these data are available from 1994-2018. We restrict the sample to men and women without young children, i.e., children age below 6. Effects are within 5 years after the first parental death. 95%-confidence interval indicated.

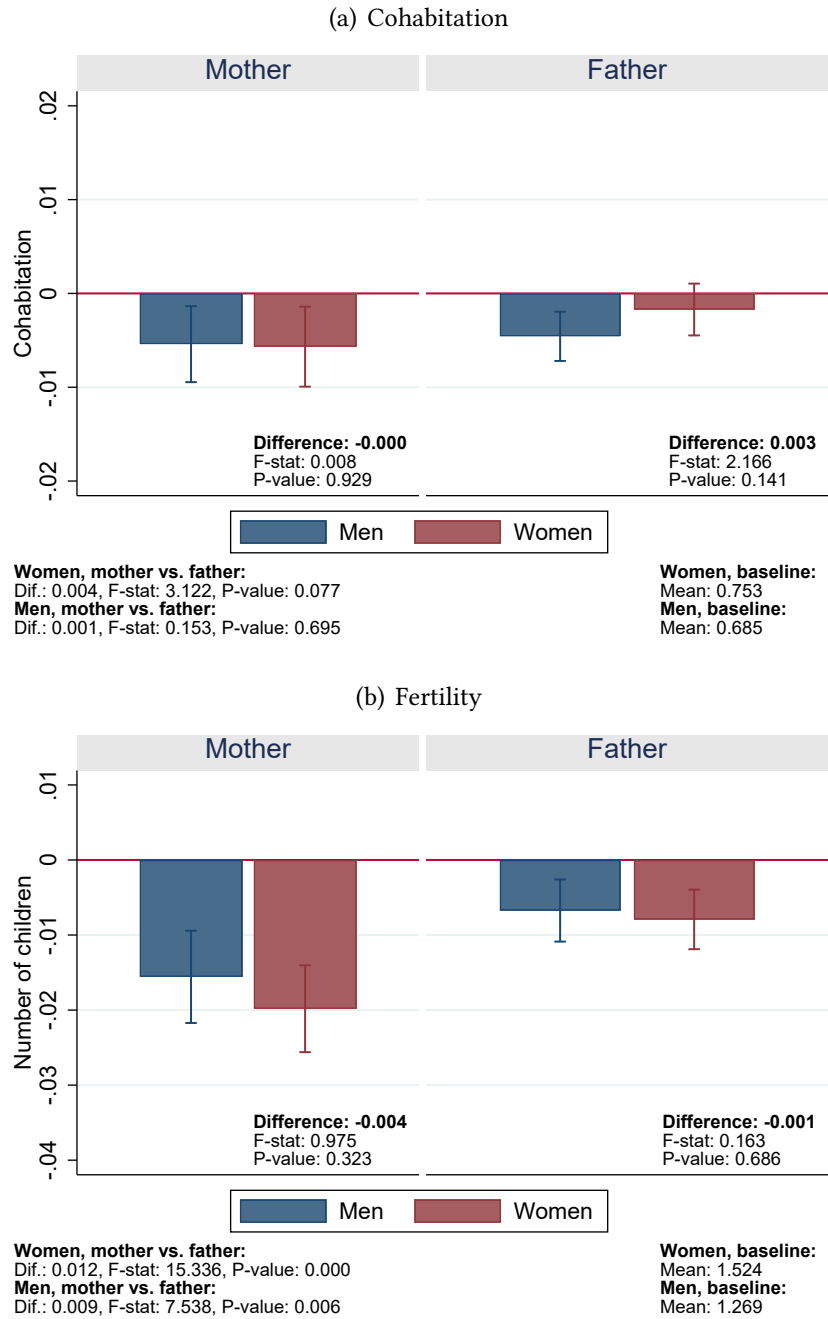
Figure B.16: Same region of residence as surviving parent



Notes: This figure plots the estimated coefficients from Equation 3 for men's and women's living in the same region with surviving parents by parental health status. We use the 11 NUTS3 regions in Denmark for this exercise. Detailed regional data are available from 1985 and hospital data until 2018. Half of the individuals are treated, and the other half are matched controls. Effects are within 5 years after the first parental death. 95%-confidence interval indicated.

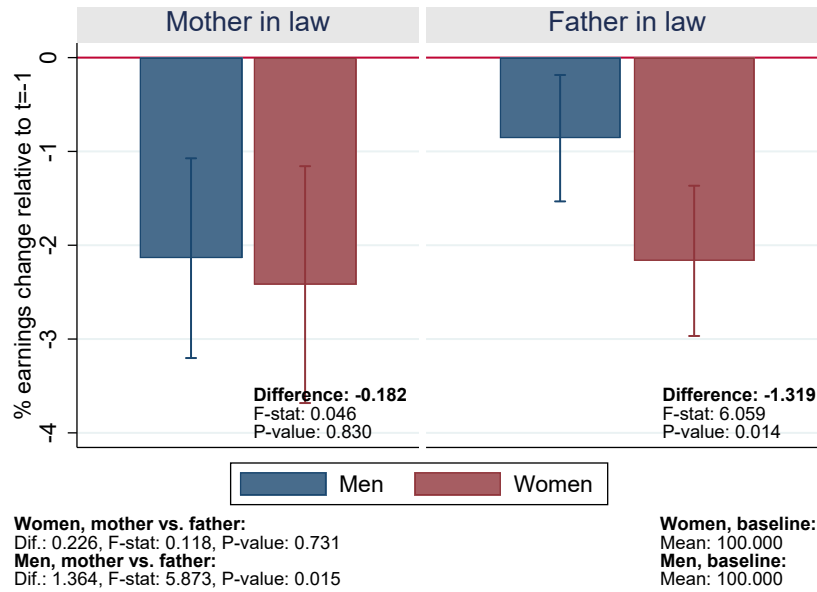
B.C Additional outcomes and robustness checks

Figure B.17: Cohabitation and Fertility, mothers' vs. fathers' deaths



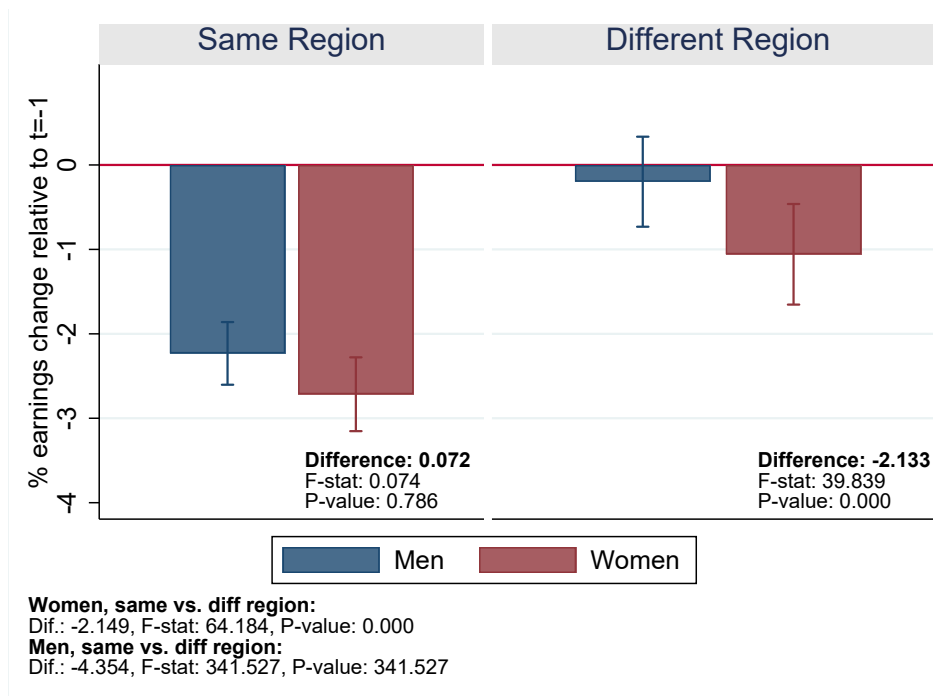
Notes: This figure plots the estimated coefficients from Equation 4 for men's and women's cohabitation rates and the number of children. The sample consists of all unexpected, first parental deaths from 1985 to 2014 and their matched controls. See Table B.2 for details on the sample. Effects are within 5 years after the first parental death. 95%-confidence interval indicated.

Figure B.18: Effects of parents-in-law death



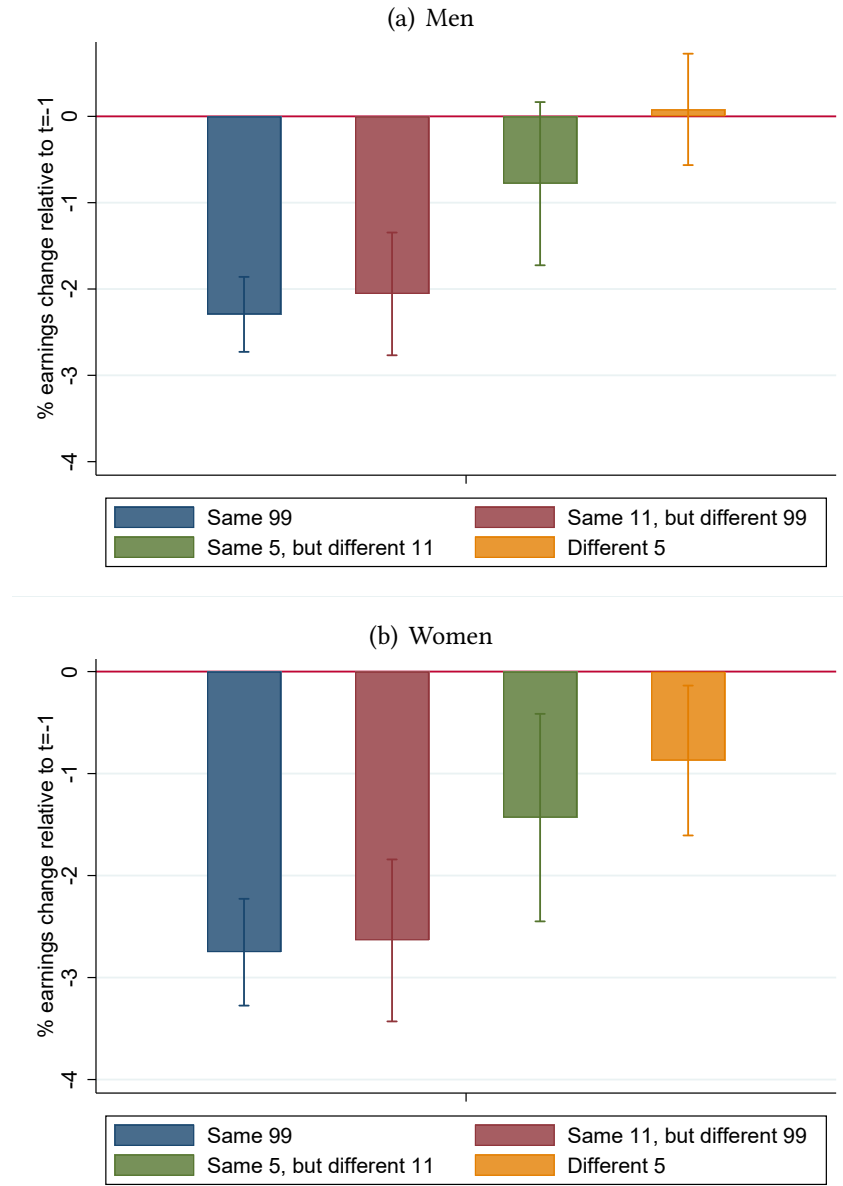
Notes: This figure plots the estimated coefficients from Equation 4 for men's and women's earnings, but considering the death of a parent-in-law instead. The sample is restricted to men and women with young children, i.e., below age 6 in the year of parental death, and from the year 1985 onward because spouse IDs are fully consistent from then. Effects are within 5 years after the first parental death. 95%-confidence interval indicated. Standard errors are clustered at the individual-by-match ID level.

Figure B.19: Heterogeneity in treatment effect on earnings by home region



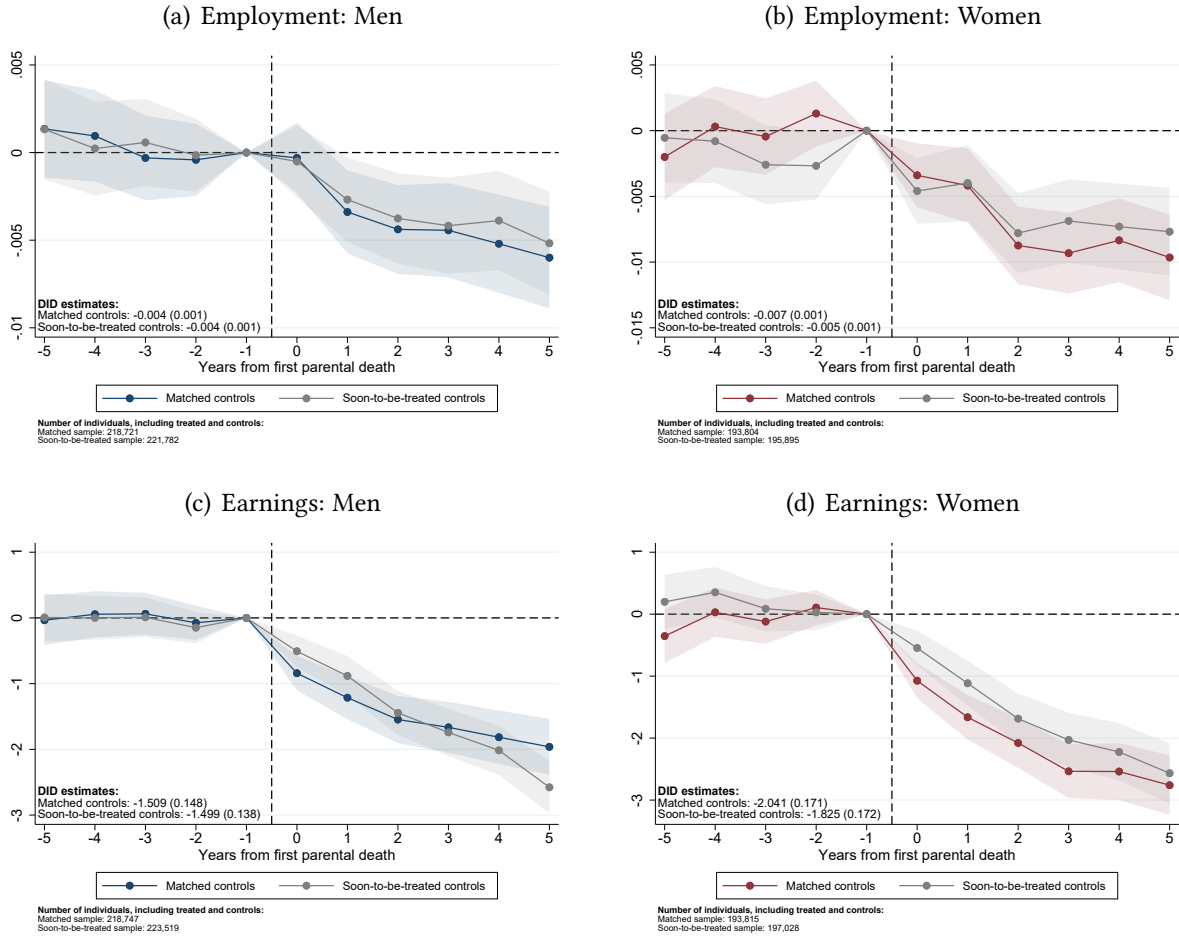
Notes: This figure plots the estimated coefficients from Equation 3 for men's and women's earnings, but where we split the sample depending on whether or not the children live in the same region as their deceased parent. There are 5 regions in Denmark. Earnings are indexed by the average earnings of men and women one year before parental death. Detailed regional data are available from 1985. See Table B.2 for details on the sample. Effects are within 5 years after the first parental death. 95%-confidence interval indicated.

Figure B.20: Heterogeneity in treatment effect on earnings by regional proximity to parents



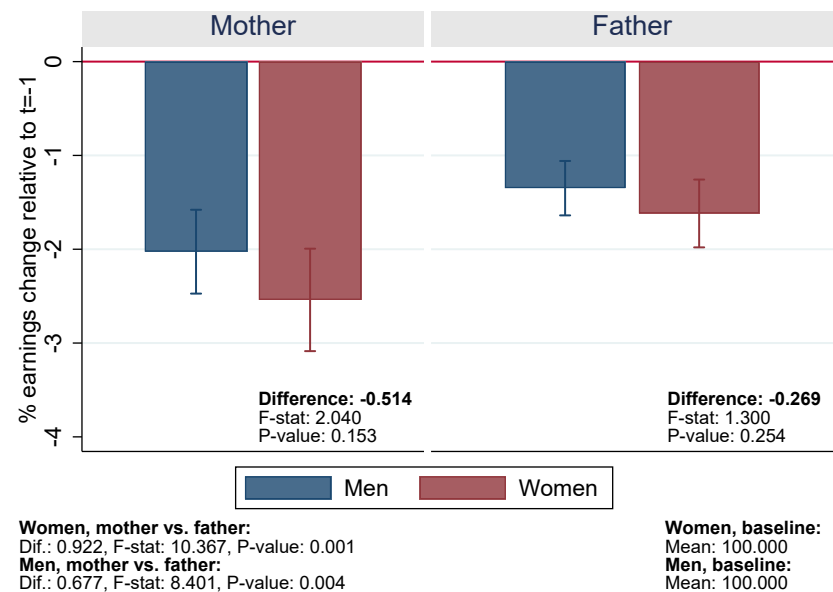
Notes: This figure plots the estimated coefficients from Equation [3](#) for men's and women's earnings, but where we split the sample depending on children's regional proximity to their deceased parent. We create an intensive measure of distance, that is whether parents and adult children live in the: 1) same municipality (**99** regions), 2) same NUTS3 region (but not same municipality; there are **11** NUTS3 regions in Denmark), 3) same NUTS2 region (but not same NUTS3 region; there are **5** NUTS2 regions in Denmark), 4) further distance than that. This approach gives us four levels of distance, increasing from the left to the right in the figure. Detailed regional data are available from 1985. See Table [B.2](#) for details on the sample. Effects are within 5 years after the first parental death. 95%-confidence interval indicated.

Figure B.21: Alternatively specification: Employment and earnings



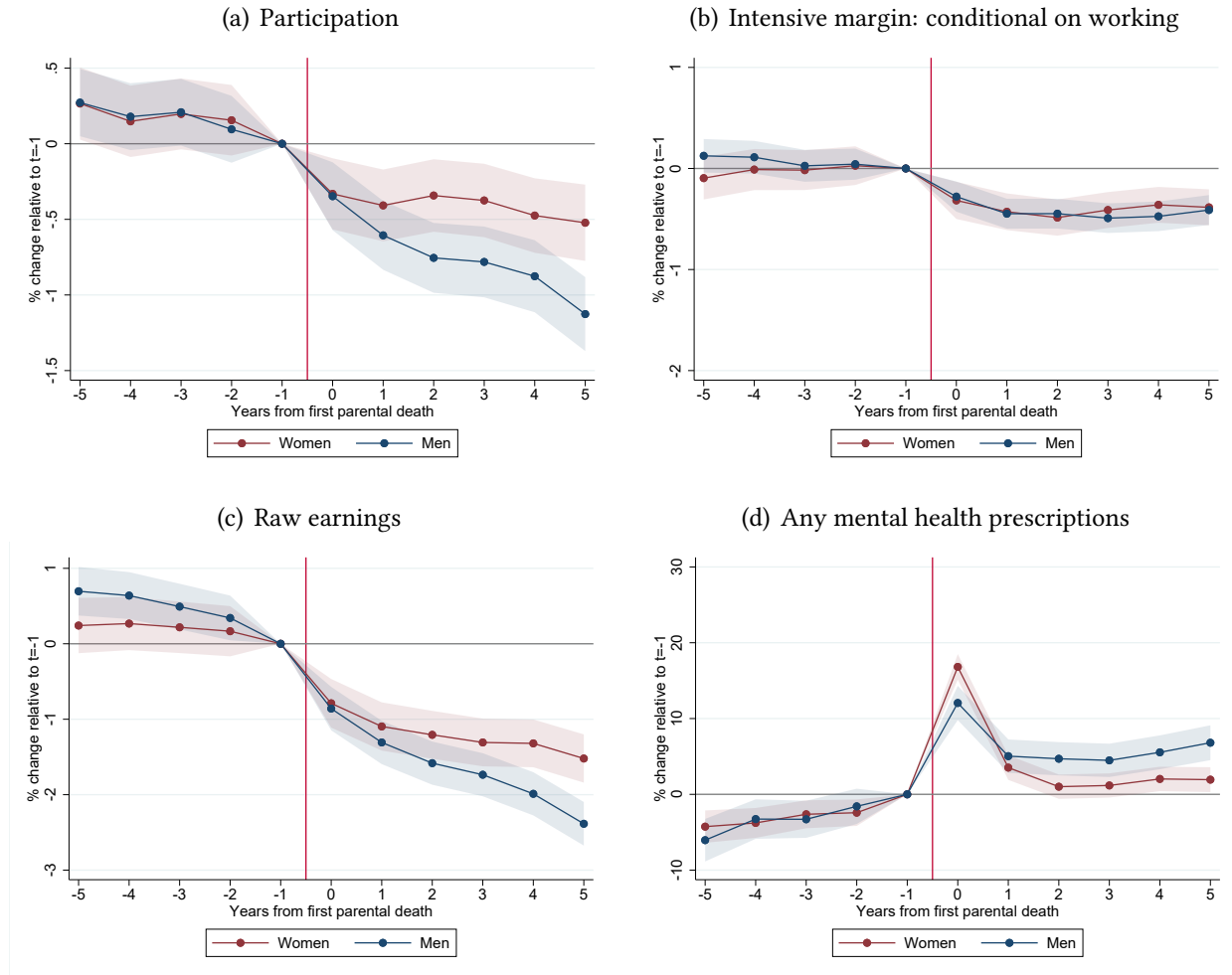
Notes: This figure plots the estimated coefficients from Equation 2 for men's and women's employment and earnings. Earnings are indexed by the average earnings of men and women one year before parental death. We use both our matched control as in the main text and construct an alternative control group following Fadlon and Nielsen (2021) to use individuals who are treated 6 years later as controls for those treated in any given year. The average effects are within 5 years after the first parental death. 95%-confidence interval indicated. Standard errors are clustered at the individual level in parentheses.

Figure B.22: Alternatively specification: Earnings, mothers' vs. fathers' deaths



Notes: This figure plots the estimated coefficients from Equation 4 for men's and women's earnings. Earnings are indexed by the average earnings of men and women one year before parental death. To construct an alternative control group, we follow Fadlon and Nielsen (2021) and use individuals who are treated 6 years later as controls for those treated in any given year. The average effects are within 5 years after the first parental death. 95%-confidence interval indicated. Standard errors are clustered at the individual level in parentheses.

Figure B.23: Descriptives: Effect of non-sudden parental death



Notes: This figure plots the estimated coefficients from Equation 1 for men's and women's labor market outcomes and if using any mental health prescriptions, but focusing on **non-sudden** deaths only. We follow Kleven, Landais and Sogaard (2019, p. 188) and convert the estimated coefficients into percentage change relative to the baseline. Participation is measured as strictly positive ATP contributions. The intensive margin is based on ATP contributions, similar to Kleven, Landais and Sogaard (2019). ATP-pension contributions are paid proportionally to hours worked. Earnings are inflated to 2020 levels and include earnings from both employment and self-employment. The sample consists of all non-sudden, first parental deaths from 1985 to 2014 for children aged 25-50 in the year of first parental death and with two known parents. The prescription data are available from 1995. 95%-confidence intervals indicated.

Table B.4: Parental net assets and earnings

	(1)	(2)	(3)	(4)
	First death mother		First death father	
VARIABLES	Men	Women	Men	Women
	Earnings	Earnings	Earnings	Earnings
Treatment $\times$ Parental Wealth	0.001 (0.001)	0.000 (0.000)	0.000 (0.001)	0.002 (0.000)
Treatment	-2.095 (0.412)	-2.542 (0.360)	-1.425 (0.202)	-2.246 (0.211)
Observations	535,784	482,170	1,870,428	1,649,796
Control mean men	102.3	102.3	102.3	102.3
Control mean women	103.4	103.4	103.4	103.4

Notes: This table shows the interaction effect of parental wealth and parental death on adult children's earnings by gender. The first two columns show the effect after mothers' death and the last two columns show the effect after fathers' death. Parental wealth includes savings, stocks, etc., as well as the value of any properties owned net of the debt in the property, all measured in the year before the first parental death. Values are inflated to 2020 levels and expressed in 1000 DKK.

Table B.5: Firm, sector, and occupation

	(1)	(2)	(3)	(4)	(5)	(6)
	Occ. switch		Firm-to-firm trans.		Public sector if employ.	
VARIABLES	Men	Women	Men	Women	Men	Women
Treatment	-0.00147 (0.00149)	-0.00214 (0.00152)	0.000824 (0.000922)	-0.000871 (0.000911)	-0.00185 (0.00108)	0.00166 (0.00147)
Observations	1,428,496	1,304,530	2,406,212	2,131,966	2,011,842	1,781,875
Pre-period mean	0.306	0.308	0.144	0.119	0.204	0.463

Notes: This table shows the effect of parental death on adult children's job switching behavior by gender. The first two columns show the effect of parental death on whether the treated individual switching to another occupation, the following two columns show the effect on whether the treated individual switching to another firm and the last two columns show the effect on whether the treated individual working in a public sector.

Table B.6: Heterogeneity in treatment effect

Dependent variable: Indexed earnings, % change relative to t=-1								
	(1) Men	(2) Women	(3) Men	(4) Women	(5) Men	(6) Women	(7) Men	(8) Women
Treat., heart	-1.662 (0.162)	-2.025 (0.187)						
Treat., stroke	-0.920 (0.243)	-2.137 (0.288)						
Treat., respiratory	-1.584 (2.122)	-0.196 (2.153)						
Treat., accident	-1.845 (0.407)	-1.950 (0.473)						
Treat., parent 70 years			-2.135 (0.183)	-2.521 (0.215)				
Treat., parent > 70 years			-0.862 (0.183)	-1.533 (0.209)				
Treat., child 38 years					-1.746 (0.189)	-2.182 (0.223)		
Treat., child > 38 years					-1.255 (0.190)	-1.887 (0.216)		
Treat., death years 1985-1994							-1.466 (0.227)	-1.841 (0.254)
Treat., death years 1995-2004							-1.166 (0.200)	-1.698 (0.238)
Treat., death years 2005-2014							-2.067 (0.255)	-2.702 (0.289)
Observations	2,406,212	2,131,966	2,406,212	2,131,966	2,406,212	2,131,966	2,406,212	2,131,966

Notes: This table shows heterogeneity in treatment effect by parental cause of death, by age of death parents, by age of children, and by time periods. Earnings are indexed by the average earnings of men and women one year before parental death. The sample consists of all unexpected, first parental deaths from 1985 to 2014 and their matched controls for children aged 25-50 in the year of first parental death and with two known parents. See Table [B.2](#) for details on the sample. Effects are within 5 years after the first parental death. Standard errors clustered at the individual-by-match ID level in parentheses