Parental Health Penalty on Adult Children's Employment: Gender Differences and Long-Term Consequences

Jiayi Wen[*], **Haili Huang**

Wang Yanan Institute for Studies in Economics (WISE)
and School of Economics, Xiamen University

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Abstract

This study examines the gender-specific and enduring impacts of parental health shocks on adult children's employment in China, where both formal care and health insurance are limited. Using an event-study approach, we establish a causal link between parental health shocks and a notable decline in female employment, which persists for at least six years following the shock. Male employment, however, exhibits minimal change on average, although this conceals an increase among poor families, indicating a channel beyond heightened informal care. Our findings underscore the consequences of "growing old before getting rich" for developing countries.

Keywords: Gender Inequality, Female Labor Supply, Health Shock, Aging
JEL: D13, I10, J22, O15

[*]E-mail: wjyecon@gmail.com. School of Economics and Wang Yanan Institute of Studies in Economics (WISE), Xiamen University, Fujian, China. We acknowledge the grants from the National Natural Science Foundation of China (Grant No. 71903167, Grant No. 72133004) and The Fundamental Research Funds for the Central Universities (Grant No. 20720221013). All remaining errors are the authors'.

1 Introduction

Developed and developing countries alike are experiencing demographic shifts toward an aging population. As evidenced by the seventh national census of China in 2021, 264 million people are aged 60 and above, representing 18.70% of the total population, a figure that has doubled since 2000. Remarkably, this number surpasses the entire population of the world's fifth most populous nation. Projections indicate that this proportion will escalate to a staggering 40% by 2050. Within this context, health risks have emerged as a prominent concern given their upward trajectory across the life cycle.

This study examines the repercussions of co-resident parental health shocks on the labor supply of younger cohorts in China. Amid limited provision of market and public insurance, family has a vital role in risk-sharing in developing countries (Fafchamps and Lund, 2003; Liu, 2016). Through this mechanism, health risks stemming from population aging may extend their reach beyond the elderly and impact younger generations. Intrahousehold specialization can induce the unequal distribution of costs, particularly for females (Dercon and Krishnan, 2000). While existing studies examining developed countries have found no evidence of such spillover effects (Rellstab et al., 2020), do parental health shocks affect the labor market outcomes of the younger generation in developing countries? Do these effects disproportionately impact females? If so, to what extent does these impact persist? Empirical answers to these questions can help elucidate the costs of family risk-sharing and offer insights into the potential dividends of market and social insurance reform in the developing world.

We begin by emphasizing the dual shortage in formal care and health insurance faced in many developing countries, positing that parental health shocks can generate a substitution effect that elevates the demand for informal care and diminishes labor supply, and an income effect that arises from heightened medical costs and motivates labor participation. These effects have significance non only for theoretical considerations but also for empirical and econometric practices. Empirically, existing research has predominantly scrutinized the substitution effect, specifically gauging the link between informal care and female labor supply. However, it is essential to consider income effects for a comprehensive understanding of the overall repercussions of parental health shocks on adult children. Methodologically, these dual effects presents challenges to the conventional instrumental variable (IV) approach, rendering it unable to satisfy monotonicity and exclusion restriction assumptions.

In response, this study uses the staggered difference-in-differences (DiD) design to elicit empirical causal evidence. The ideal dataset would require detailed and longitudinal information on both parental health and adult children's economic and demographic variables.

The longitudinal dataset must also be mature with sufficient waves to capture any long-term effects and provide credible pre-trend tests. It must also be nationally representative to shed light on the global consequences of population aging. Our empirical analysis uses the China Family Panel Studies (CFPS) dataset spanning from 2012 to 2020, which uniquely meets these requirements. Following recent studies (García-Gómez et al., 2013; Dobkin et al., 2018; Rellstab et al., 2020), we define parental health shock as the initial hospitalization of either adult children's parents or parents-in-law. This measure mitigates reporting bias inherent in subjective health variables. We also demonstrate that hospitalization, while capturing severe symptoms, exhibits prevalence, persistence, and a significant upward trajectory with age.

We investigate labor supply effects along the extensive margin, gauged through employment status, and the intensive margin, measured by weekly work hours conditional on employment. Our benchmark results reveal a noteworthy decline in female employment rates, with an average reduction of 3.7 percentage points subsequent to parental health shocks. Conversely, no significant change is found in male adult children's employment. The results also find no significant changes in work hours. The shifts observed on the extensive margin are particularly likely to have scarring effects on future labor market performance. Therefore, an important concern is whether females affected by parental health shocks are capable of quickly returning to employment. Additionally, any negative impact can also be fueled by the persistent nature of health shocks at older ages. We then undertake a dynamic analysis using the event-study approach. Strikingly, our investigation demonstrates no evidence of recovery in female employment after at least 6 years following a shock, which constitutes the maximum span of our sample.

Subgroup analysis reveals strong gradients in employment effects based on individual income and family wealth. Notably, some subgroups exhibit an increase in employment following a parental shock, indicating strong income effects. Specifically, females with median income experience a 4.77 percentage point decline in employment following a parental health shock, whereas the effects turn positive for those with top decile income, and the male labor income gradient is not evident. Interestingly, a male employment gradient emerges along household wealth, wherein males from households with assets below the median witness a 1.90 percentage point surge in employment post-shock. Strikingly, this effect escalates to 6.02 percentage points for those from households with assets below the quintile threshold. However, no such gradients are found for females. These positive effects cannot be explained by the rising demand for informal care and are consistent with the presence of income effects. We also explore heterogeneous effects based on education, marital status, the distinction between parental and parent-in-law shocks, and urban and rural residents.

Our robustness analyses reveals significant employment impacts of adult children's own hospitalization, supporting the validity of using hospitalization as a measure for health shocks. Remarkably, females recover more quickly from their own health shocks. We also follow the recent methodological advancements in DiD with treatment effect heterogeneity (Callaway and Sant'Anna, 2021) to rule out the "forbidden comparison", which is particularly pertinent to our context due to our keen focus on dynamic effects (Sun and Abraham, 2021). We also restrict the sample to individuals with no prior hospitalization records to examine more unanticipated shocks. Additionally, our robustness analysis narrows the sample to adult children of prime working age, a cohort that is expected to demonstrate robust labor market attachment and resilience to adverse shocks. We also control for functions fully saturated in the age of each parent, which is a critical factor associated with parental health and children's labor supply. The results remain robust across these various tests.

This study pioneers a causal exploration into the impact of parental health shocks on the labor supply of adult children within the context of developing countries. While research has demonstrated the sensitivity of female labor supply to spouses' health shocks (Coile, 2004; Fadlon and Nielsen, 2021) or from young children (Gould, 2004; Eriksen et al., 2021; Breivik and Costa-Ramón, 2022), this study investigates a new source of risk, parental health shocks, with particular concern for rapidly aging populations.[1] Previous studies examining developed countries have indicated no effects or minimal gender disparities. For instance, in the Netherlands, Rellstab et al. (2020) found no significant effect of parental hospitalization on children's labor supply, attributing it to the country's well-established formal care system. In Austria, Frimmel et al. (2023) determined that both female and male employment decreased following a parental stroke, with a modest gender difference.[2] Interestingly, the authors found that the decline disappeared following the liberalization of the formal care market.

The consequences of parental health shocks loom large and unequal in developing countries, where societies have strong cultural family ties and insufficient formal insurance. Our findings reveal a persistent effect of approximately four percentage points that is specific to women, highlighting the nontrivial and uneven burden associated with population aging. A concurrent paper by Brito and Contreras (2024) provides complementary evidence from Chile, finding a negative and persistent impact of initial parental cancer hospitalization on female employment, and a weakly positive effect for males. Their estimates cover families regardless of residential arrangements, whereas our results are based on families that reside

[1]Notably, Wang et al. (2023) identified small and insignificant spillover effects from spouses' health shocks in China, defining health shocks as the first onset of a cardiovascular or cerebrovascular health event.

[2]The authors revealed a decline of 2 percentage points at 10% significance level for females and 1.5 percentage points at 5% level for males; however, a larger gap in earnings decline is also evident.

together.[3]

More broadly, this study contributes to the research regarding the adverse effects of health shocks on labor supply, such as Bound et al. (1999), García-Gómez (2011), Cai et al. (2014), Dobkin et al. (2018), among others. These studies primarily focused on the impact of shocks on individuals themselves. Notably, Dobkin et al. (2018) employed the event study approach and provide early evidence based on a quasi-experimental design. They found that individuals' employment rate decreases by 8.9 percentage points one year following hospitalization and by 11.1 points three years later.

This study also contributes to the literature on the effect of informal care on female labor supply, such as Van Houtven et al. (2013), Crespo and Mira (2014), Schmitz and Westphal (2017). It is essential to note that informal care, which constitutes the substitution effect within our framework, represents just one of the mechanisms through which parental health shocks take effect. We aim to assess the comprehensive influence of parental health shocks across both genders, with the income effect emerging as a pivotal determinant of gender differences. Additionally, this study reveals the potential econometric issues of ignoring the income effect. Research on informal care typically adopts parental health as an IV for care provision (Bolin et al., 2008; Crespo and Mira, 2014), where the presence of income effects may invalidate the exclusion restriction and monotonicity assumption.

Another contribution of this study is revealing the enduring impacts using a quasi-experimental event-study design. Studies on the effects of informal care employing the IV method typically estimate a static effect. To our knowledge, Schmitz and Westphal (2017) are the only exception, which carefully selected control variables based on the conditional independence assumption. Our strategy relies on the parallel trend assumption and we provide credible tests prior to the event. The implied costs of population aging may be far greater and more uneven than those suggested by short-term effects, providing valuable insights on the potential benefits that policies on long-term care insurance could yield.

Finally, this study offers a new perspective to the literature on intrahousehold labor division and the gender gap, which typically attributed specialization to factors such as children, social norms, and marriage quality (Yamaguchi et al., 2014; Bertrand et al., 2015; Juhn and McCue, 2017). We highlight the role of parental health under population aging.

The next section introduces the study's background. Section 3 describes the data and econometric methods, followed by empirical results and robustness tests in Section 4 and 5. Section 6 concludes.

[3] In addition to employment, the authors focus on the effects of health shocks on earnings, whereas we look at work hours.

2 Background

Formal care systems in most developing countries are deficient. For instance, the Chinese government officially launched long-term care insurance (LTCI) pilot programs in 15 cities in 2016, expanding to 49 cities by 2023. However, this initiative only covers a small fraction of the 293 prefecture-level cities in China. Due to funding issues, LTCI is primarily available to urban workers with formal employment. Urban residents without formal employment and the vast rural population still face significant long-term care risks in their later lives.

Alternatively, the family serves as a crucial insurance for long-term care risks. This risk sharing mechanism operates not only for couples but also extends to younger cohorts. Our calculations demonstrate that 20% of males were cared for by their adult children at age 50. This figure rises to 30% at age 60 and 50% at age 70.[4] While family risk sharing generates benefits such as consumption smoothing, its costs are less clear as the shadow price is difficult to quantify. With the lack of a formal care system, parental health shocks elevate the demand for informal care and increase the opportunity cost of adult children's employment. Moreover, such costs may not be evenly assumed and tend to disproportionately fall on the shoulders of women.

Notably, developing countries also face a concurrent dearth in health insurance. For instance, during the 1990s, more than 90% of rural residents in China were not covered by health insurance (Huang and Liu, 2023). Public health insurance for rural residents, called the New Cooperative Medical Scheme, was not rolled out until 2003, and per capita subsidies are minimal, at 380 Chinese yuan (around 62 US dollars) in the year 2014.

Consequently, deteriorating parental health tends to inflict burdensome medical expenses, leading to a stronger incentive to work. When this income effect is sufficiently large, individuals may increase their labor supply following a parental health shock. This theoretical intuition has significant implications for empirical analysis. In terms of econometrics, the question regarding the impact of informal care on female labor supply has been examined often using parental health as an IV. This income effect poses a threat to the exclusion restriction and monotonicity assumption, rendering IV estimates biased. In terms of the economic significance of empirical evidence, individuals with lower savings or higher wages are more likely to increase their labor supply following a shock. As our study focuses on gender difference in the impact of parental health shocks, it is crucial to consider both effects because significant wage inequality exists across genders.

Based on a directed acyclic graph (DAG), Figure 1 summarizes the conventional IV approach for estimating only the substitution effect (shown in red edges) and the threat to

[4] Figure 2 provides more details.

IV assumptions generated by the income effect (shown in blue edges). Parental health (H) is commonly used as an instrument for estimating the effect of informal care (C) on employment (Y) to overcome the bias caused by omitted variables (U). However, medical expenditure (M) threatens the validity of this instrument as it introduces an additional channel.[5]

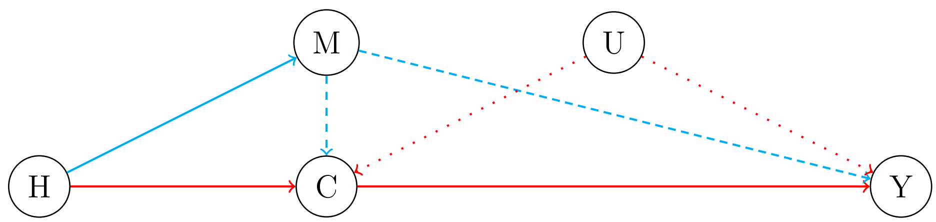

Figure 1: Income and Substitution Effects of Parental Health Shocks

Notes: This directed acyclic graph (DAG) provides a guide regarding the causal relationships between parental health (H), informal care (C), medical expenditure (M), employment (Y), and unobserved omitted variables (U). Red edges illustrate the intuition of using parental health shocks as an instrumental variable (IV) to examine the employment effect of informal care. Blue edges show the additional mechanism by which parental health may affect adult children's informal care and employment decisions, which threatens the IV approach.

3 Data and Econometric Method

3.1 Data and Measures

3.1.1 Data

To quantify the impact of parental health risks on adult children's labor supply, ideal data must meet three key requirements. First, it should provide detailed, longitudinal information on parental health and children's labor market outcomes. Datasets meeting this criterion are rare. For instance, while the China Health and Retirement Longitudinal Study provides rich information on elderly health, it offers limited data on the children's side, consequently restricting the number of variables that can be controlled for in any regressions at the children's level. As another example, the China Labor-force Dynamics Survey (CLDS) provides detailed longitudinal information on labor market outcomes and children's demographics. However, the only available health measure is self-rated. Second, given our focus on the broader implications of population aging, the data must be nationally representative to accurately assess overall impact. Although several sources of health insurance claim data

[5] Whether the exclusion restriction or the monotonicity assumption is violated is nuanced. For instance, consider estimating the effect of hours spent on informal care on employment. If parental health only affects the intensive margin of labor supply (hours) but not the extensive margin (employment), the exclusion restriction tends to hold but the monotonicity assumption is violated since poor health may reduce the hours spent on informal care.

are available in China, providing administrative information on specific individuals' health, these samples are typically drawn from a limited number of developed cities and tend not to include rural populations, among which family risk sharing is more prevalent. Finally, to capture long-term effects and conduct a credible examination of parallel trends, which is essential for DiD design, the longitudinal dataset must present a sufficient number of waves over time.

This study uses data from the CFPS covering 2012 to 2020. The CFPS is administered by the Institute of Social Science Survey at Peking University and is a comprehensive, nationally representative household panel survey encompassing diverse subjects such as economic activities, educational outcomes, family relationships, population migration, and health. The sample includes 25 provinces, with data at individual, household, and community levels. The CFPS survey design has a particular focus on households, and detailed information for parents and children is available. Although the survey covers 25 provinces instead of all, it includes 94.5% of the population in mainland China, providing us with the opportunity to quantify the impact of population aging nationwide. The CFPS officially began in 2010 and has accumulated six waves of data over a decade, uniquely meeting the research requirements outlined previously.

The CFPS defines its main sample according to the baseline 2010 survey, including *genetic members* who are respondents satisfying two conditions that include (1) having economic ties and (2) being immediate family or relatives who have lived together for over 3 months. According to the survey handbook, the definition of economic ties essentially concerns co-residence. Therefore, parents that co-resided with their adult children in 2010 are included as genetic members into the sample. An advantageous design of CFPS is that the genetic members are permanently tracked and followed by the CFPS even if they departed the household in subsequent waves. Parents that did not co-reside with their children in 2010 are labeled core members rather than genetic members, and their information was only gathered in waves that included co-residence. This survey design prevents over-extrapolation, as cohabiting families are likely to differ from families that live apart.[6] The Survey Design subsection 5.4 details the implications for internal validity and provides robust evidence for our baseline findings.

[6]According to China's Census 2010, about 50% of parents older than 65 lived with their children. Co-residence remains a significant living arrangement also in other developing countries. Against theoretical prediction of a decline, the trends of co-residence (for older 65+ parents) in many developing countries have been found to be stable at around 50% on average (Ruggles and Heggeness, 2008).

3.1.2 Measures

This study uses the initial hospitalization of either parents or parents-in-law as health shock events. Initial hospitalization events have been widely used in recent research on the impact of health shocks, such as García-Gómez et al. (2013), Dobkin et al. (2018), and Rellstab et al. (2020). As noted previously, datasets providing comprehensive information on children often include limited details regarding parental health. Self-rated health has typically been used in studies concerning informal care and female labor supply. Hospitalization experience is plausibly more objective and less prone to reporting bias and captures health shocks that are typically more severe and enduring. For comparison purposes, we also present results based on the variable of self-rated health decline. Due to data constraints, we are unable to identify the specific symptoms leading to hospitalization. Instead, we offer results based on health shocks preceded by years of no hospitalization records as robustness tests, where health shocks are more likely to be unanticipated.

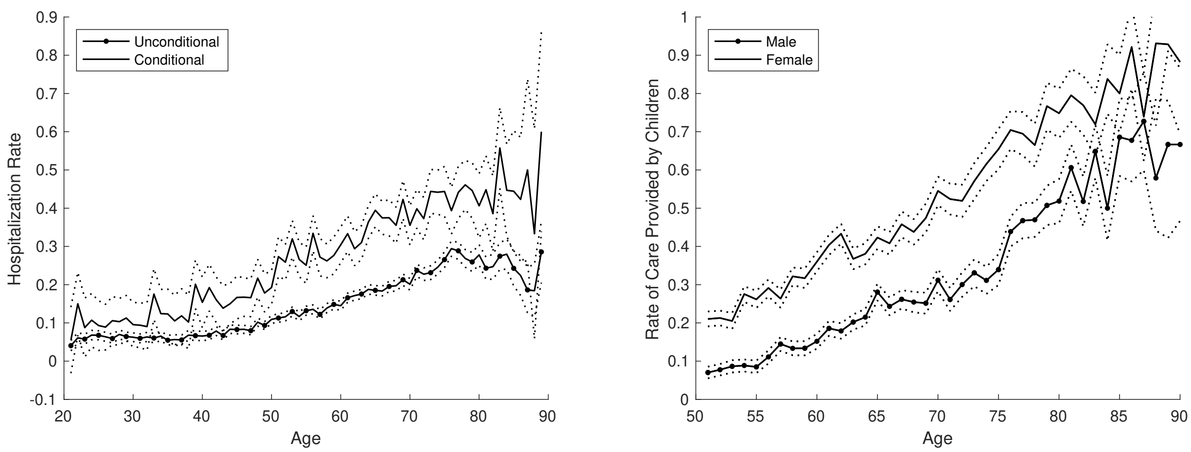

Figure 2: Trends in Hospitalization Risk and Likelihood of Informal Care

Notes: The left figure presents the trends in hospitalization and the right figure shows the trends in informal care provided by children. The conditional rate demonstrates the likelihood in the current period conditioned on having in-patient care experience in the previous interview. A detailed definition of the informal care variable is provided in Subsection 4.4. Confidence intervals are at the 95% level.

Hospitalization tends to indicate more severe health shocks; however, its frequency may be in question, wherein if hospitalization occurs rarely, its overall impact may be inconsequential. The unconditional hospitalization rate presented in Figure 2, indicating the proportion of individuals that experienced hospitalization in the past year, shows a clear rising trend over the life cycle, reaching approximately 20% at the age of 70. More notably, a distinctive feature of parental health shocks is the tendency to be more persistent than shocks at younger ages. The conditional hospitalization rate shown in Figure 2 illustrates the hospitalization rate of the current period conditioned on having reported in-patient care experience in the previous interview. This rate is higher than the unconditional rate and rises notably faster over the life cycle. As a result, parental hospitalization becomes more common and increasingly persistent as individuals age. Coupled with the rising dependency ratio due to birth control policies such as the Later, Longer, Fewer policy in the 1970s and the One-Child policy in the 1980s, the informal care burden induced by parental health shocks is considerable.

While parental health shocks are prevalent and persistent at older ages, they only have spillover effects if children genuinely care about their parents and devote time to providing care. In Figure 2, the graph on the right demonstrates that parents receiving informal care from their children account for a large and rising share. Approximately 50% of mothers at the age of 70 received care from their children, based on calculations for each individual parent. If we consider the union of parents and parents-in-law, the likelihood of caregiving for at least one parent in each household tends to be even higher.

This study uses CFPS data from 2012 to 2020 to construct the sample. We exclude the 2010 wave due to differing questions concerning labor market outcomes. We merge the databases of individuals, family relationships, and economic conditions for each year, extracting relevant information regarding health, labor market outcomes, and parent–child relationships from the merged data. The sample includes each adult child as the basic unit of observation that is matched with parental information. After obtaining the full sample, we restrict our final sample to include adult children who: (1) are aged between 16 and 64 years, (2) appeared in at least 2 survey waves, (3) have parents at least 50 years old.

Table 1: Definitions of Main Variables

VARIABLES	DESCRIPTION
Adult Children	
Employment status	1 for employed, 0 for unemployed or out of the labor force
Weekly work hours	Average number of hours worked per week
Age	Respondent's age
Self-rated health	Respondent's self-rated health status with five alternatives
Gender	1 for male, 0 for female
Years of education	Number of years of education completed by the respondent
Marital status	1 for married (having a spouse), 0 for unmarried, cohabiting, divorced, or widowed
Log of family assets	Natural logarithm of the respondent's family assets
Having children under 6	1 if the respondent has children below the age of 6, 0 otherwise
Number of children	Respondent's number of children
Urban/Rural	Residence classification as urban or rural
Parents	
Average age	Average age combining parents and parents-in-law
Hospitalization	If either parents or parents-in-law ever had a hospitalization experience
Self-rated health deterioration	If either parents or parents-in-law ever had reported worsening health

Notes: This table presents the main variables used for this study's estimations and their definitions.

The final dataset is an unbalanced panel dataset with 30,165 observations. Table 1 presents explanations of each variable used in subsequent analysis, and Table 2 presents the descriptive statistics. Appendix A reports additional results by parental hospitalization status.

As shown in Table 2, the average age of the respondents' father and mother was approximately 63 years; 75% of adult children have ever experienced a self-rated parental health deterioration, while approximately 41% have ever experienced a parental hospitalization. Approximately 53% of the respondents were male, and 47% were female, with an average age of 35.6 years. At the time of the interview, 88% of the respondents were employed, working an average of 48 hours per week. Approximately 85% of respondents were married, and 34% had a child under the age of 6.

Table 2: Descriptive Statistics of Main Variables

Variable	Observations	Mean	Std	Min	Max
Children					
Employment status	30,165	0.881	0.324	0	1
Weekly work hours	19,552	48.36	19.46	0.100	100
Age	30,165	35.61	8.740	16	64
Self-rated health	30,165	2.733	1.102	1	5
Gender	30,165	0.528	0.499	0	1
Years of education	30,165	9.450	4.273	0	23
Marital status	30,165	0.845	0.362	0	1
Log of family assets	30,165	12.59	1.38	0	17.75
Having children under 6	30,165	0.338	0.473	0	1
Number of children	30,165	1.331	0.921	0	7
Urban/ Rural	30,165	0.488	0.500	0	1
Parents					
Average age	30,165	63.22	9.526	25	99
Hospitalization	30,165	0.409	0.492	0	1
Self-rated health deterioration	29,859	0.749	0.434	0	1

Notes: This table presents descriptive statistics of main variables for the estimation sample. The number of observations 30,165 is the sum of 15,921 observations of males and 14,244 of females in the main analyses. Parental hospitalization is defined as an absorbing state which equals 1 after the first hospitalization observed in the sample period for the union of parents or parents-in-law. Parental self-rated health decline is defined similarly based on the first self-rated health deterioration during the sample period.

3.2 Econometric Model

3.2.1 Baseline Model

To analyze the impact of parental health shocks, our baseline econometric model adopts the staggered DiD approach. The econometric model is formulated as follows in a two-way fixed effect specification:

$$Y_{it} = \tau D_{it} + Z'_{it}\beta + \lambda_t + \alpha_i + \epsilon_{it} \tag{1}$$

where Y_{it} represents the labor supply of child i in period t at either the extensive or the intensive margin. When the child's parents or parents-in-law have ever experienced a health shock, the variable D_{it} is assigned a value 1. Z_{it} represents the set of control variables related to the adult children, their families, and their parents, including age, marital status, education, logarithm of family assets, self-rated health, having children under 6 years old, urban-rural classification, and a quadratic age function of each parent and parent-in-law.[7] Individual and time fixed effects are captured by α_i and λ_t, respectively. τ captures the average changes in labor supply for individuals who have experienced parental health shocks.

Notably, instead of average treatment effect (ATE), our quasi-experimental design can only identify the average treatment on the treated (ATT), which represents the average effect of parental health shocks on the labor supply of children that had experienced the shocks. This is because the validity of our identification strategy relies on the conditional parallel trend assumption rather than purely random treatment assignment. For instance, parents with low socioeconomic status are more likely to experience health shocks, and their adult children may also have lower employment rates. While our identification strategy does not require that the adult children in the treatment group have the same employment as the control group (only that they shared similar employment trends had the shocks not occurred), nevertheless, the treatment and control groups may systematically differ, resulting in different ATT and ATE.

3.2.2 Long-Term Effects

To estimate long-term effects, this study employs the dynamic staggered DID approach (i.e., the event study method). The econometric model is presented as follows:

$$Y_{it} = \sum_{p=k_1} \mu_p D_{it}^p + \sum_{q=k_2} \tau_q D_{it}^q + Z_{it}'\beta + \lambda_t + \alpha_i + \epsilon_{it} \tag{2}$$

The model compares the differences in labor supply between treatment and control groups in each period against the benchmark period, which is set as 2 years prior to the initial hospitalization. $D_{it}^q = \mathbf{1}\{t - E_i = q\}$ is an indicator function, where E_i represents the period of event occurrence for the respondent i, and similarly for for D_{it}^p. The value of D_{it}^q is equal to 1 when the respondent i in period t is q years away from the occurrence of parental health shock, where $k_1 \in \{-4, -6, -8\}$ and $k_2 \in \{0, 2, 4, 6, 8\}$. Coefficient τ_q captures the dynamic effects of parental health shocks on adult children's labor supply and μ_p captures the pre-trends.

We use the series of coefficients μ_p to determine whether a significant difference is evident in labor supply trends between treatment and control groups prior to the parental health shock. If a significant difference exists, the parallel trend assumption is most likely to be

[7] As the main sample covers adult children since age 16, we control for years of schooling. We find that 6.4% of the individuals in our sample have temporal variation in this variable. The control variable self-rated health refers to one of the children.

violated, and the trend of labor supply of the control group cannot serve as a counterfactual reference for the treatment group following the shock. If the trends are similar prior to the parental health shock, it is plausible that these trends would continue had the shock not occurred to the treatment group. If any concurrent changes in labor supply occur following parental health shocks, the results are most likely driven by the health shock event.

4 Empirical Results

4.1 Baseline Results

Table 3 presents our baseline results for the staggered DiD regression. While our primary focus is on the extensive margin of labor supply, measured by individuals' employment status, we also provide results concerning the intensive margin based on weekly work hours conditional on employment.[8] Despite the composition change in the employed group before and after the shock, it remains interesting to know whether individuals who remain in the labor market experienced lower average work hours compared with the pre-shock average. For instance, if the work hours of females who stayed in the labor market were also shorter, this would imply additional parental health shock consequences at the intensive margin.

Table 3: Baseline Results Estimated Using Staggered DiD

	Work Hours		Employment	
	Male	Female	Male	Female
Parental health shock	-0.7743	0.3633	0.0034	-0.0373***
	(0.7936)	(0.9910)	(0.0084)	(0.0143)
Age	0.0059	0.2649	0.0239***	0.0604***
	(0.6433)	(1.0803)	(0.0076)	(0.0127)
Age squared	0.0014	-0.0144	-0.0003***	-0.0009***
	(0.0065)	(0.0091)	(0.0001)	(0.0001)
Marital status	1.1762	-3.1059**	0.0241*	-0.1327***
	(1.1887)	(1.5663)	(0.0142)	(0.0266)
Years of schooling	0.4862	0.2431	-0.0031	0.0068
	(0.4168)	(0.3743)	(0.0039)	(0.0060)
Self-rated health	0.3588	0.3991	-0.0103***	-0.0076*
	(0.2715)	(0.3425)	(0.0029)	(0.0045)
Log family assets	-0.5618*	0.0194	0.0014	0.0134***
	(0.3021)	(0.3576)	(0.0031)	(0.0050)
Having children under 6	-1.2050*	-0.8227	0.0055	-0.0392***
	(0.6401)	(0.7855)	(0.0063)	(0.0121)
Urban/Rural	0.4205	1.8586	0.0190*	-0.0174
	(0.9894)	(1.3480)	(0.0102)	(0.0196)
Observations	11,117	8,435	15,921	14,244
R-squared	0.026	0.015	0.011	0.038
Mean of dependent variables	50.50	45.54	0.940	0.814

Notes: This table presents the baseline estimates using the staggered DiD approach. *** p<0.01, ** p<0.05, * p<0.1. Second-order polynomial functions of parent or parent-in-law age are also controlled for. Standard errors clustered at the individual level are in parentheses.

[8]Different waves of CFPS include differing variables concerning work hours. We harmonize these statistics into weekly hours, and a few outliers emerge; therefore, the weekly work hours variable is winsorized at the 99 percentile.

The results show that after a parental health shock to parents or parents-in-law, female adult children's employment rate decreases by an average 3.73 percentage points, which is statistically significant at the 1% level. To put this into context, this impact is comparable in magnitude to that of having children under 6 years of age, which has an effect of 3.92 percentage points. In addition, due to intrahousehold specialization, being married results in a 13.27 percentage point lower female employment rate, and parental health shocks further widen this gap by 28%. In contrast, no significant change in male adult children's employment is evident following a parental health shock.

We also do not find statistically significant effects of parental health shocks on work hours conditional on employment. The literature on the effects of informal care has typically found less evidence regarding the intensive margin, such as Ettner (1996) and Van Houtven et al. (2013). This result is also consistent with the broader labor supply literature that has largely indicated smaller elasticity of the intensive margin compared with the extensive margin.

The results also reveal evidence of intrahousehold specialization at intensive and extensive margins. While married females have lower employment rates than single women by 13.27 percentage points, married males' employment is 2.41 points higher than singles. Regarding the intensive margin, employed married females work 3.1 hours less per week than single women, whereas married males work 1.2 hours more.

4.2 Dynamic Effects and Parallel Trends

4.2.1 Long-Term Effects

The baseline results reflect changes in labor supply relative to the benchmark period that are averaged over all periods following a parental health shock. However, how long did the impact persist? In particular, after females became unemployed, how long did it take for them to return to the labor market? These long-term effects are particularly relevant. As shown in Figure 2, parental health shocks are more persistent than those that occur at younger ages, such as those concerning young children or adult spouses. This particular feature tends to have sustained impacts on female employment. Furthermore, under intrahousehold specialization, which is prevalent in developing countries, informal care interrupts females' human capital accumulation, which can lead to deteriorating labor market conditions and may further strengthen intrahousehold specialization, generating reinforcing and unequal impacts on females.

Table 4 presents the dynamic effects of parental health shocks estimated using the event study approach. The results reveal that female adult children experienced a 3.44 percentage point decline in employment in the year of the parental health shock and 3.18 percentage points 2 years after. Although the estimates for subsequent years are not statistically significant, which is primarily attributable to larger standard errors, no clear trend of recovery is evident for up to at least 6 years according to the magnitude.[9]

Table 4: Dynamic Effects Estimated by Event Study

	Work Hours		Employment	
	Male	Female	Male	Female
Before at least 6 years	0.6897	3.5272*	0.0018	0.0148
	(1.6659)	(2.0721)	(0.0188)	(0.0309)
Before 4 years	-0.6627	3.7196	0.0063	0.0139
	(1.2497)	(1.5769)	(0.0132)	(0.0204)
Before 2 years	-	-	-	-
	-	-	-	-
Year of the shock	-0.9675	1.0416	-0.0032	-0.0344**
	(0.9340)	(1.1368)	(0.0096)	(0.0153)
After 2 years	-0.3877	0.4665	-0.0018	-0.0318*
	(1.0774)	(1.3482)	(0.0110)	(0.0186)
After 4 years	-0.2550	-0.5341	0.0070	-0.0251
	(1.2851)	(1.5986)	(0.0132)	(0.0226)
After at least 6 years	-0.1924	-0.3040	-0.0144	-0.0385
	(1.5541)	(1.9150)	(0.0170)	(0.0289)
Observations	11,117	8,435	15,921	14,244
R-squared	0.027	0.015	0.012	0.038
Mean of dependent variables	50.50	45.54	0.940	0.814

Notes: This table presents the estimates of dynamic effects. At least 6 years combines the effects of 6 years and 8 years. *** p<0.01, ** p<0.05, * p<0.1. Control variables are the same as the baseline regression. Standard errors clustered at the individual level are in parentheses.

Overall, there is no evidence of a recovery trend in the employment rate of female adult children following a parental health shock within the maximum observation period in our sample. These results indicate a sustained long-term impact of parental health shocks on female labor supply. In contrast, the results do not indicate a significant change in male employment rate following the shock. Additionally, there is no evidence of a dynamic impact on work hours.

4.2.2 Parallel Trend Assumption

The core assumption of the DiD method is that the labor supply trend of the control group can be considered as counterfactual for the treatment group if there were no treatment. Table 4 demonstrates no significant differential trend in employment between the control and treatment group in 2 to 6 years prior to a parental health shock, for both males and females. It seems unlikely that treatment group employment would experience a sudden and coincidental unattributable change in parallel to the occurrence of a parental health shock. The most plausible explanation for these changes in employment is the parental health shock. Figure 3 visualizes the results of the event study estimation, clearly revealing a significant decline

[9]The effects of at least 6 years before and after the shock combine the effects of 6 years and 8 years due to the limited number of 8 year observations.

in female employment following the parental health shock and no significantly different pre-trends.

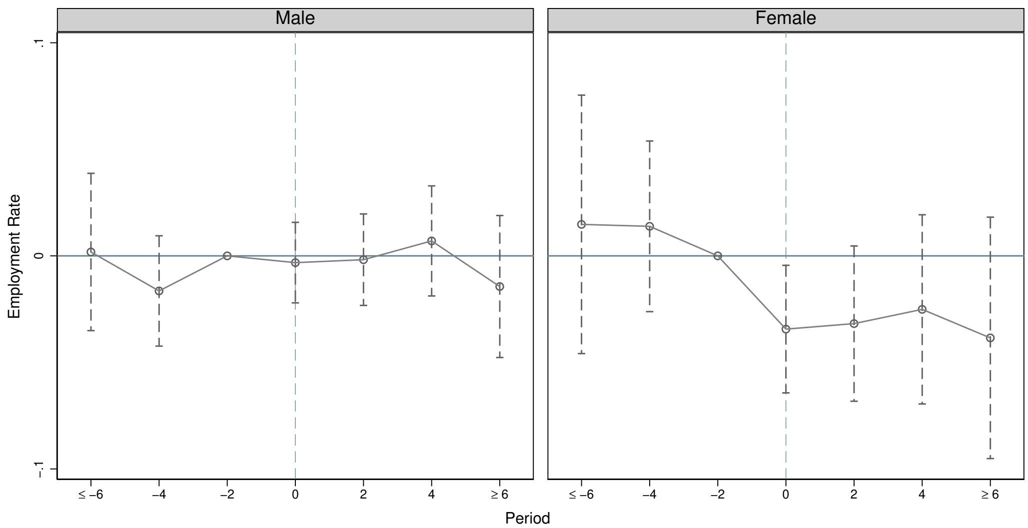

Figure 3: Dynamic Effects and Parallel Trends

Notes: This figure illustrates the trends of differing employment rates between the treatment and control group and for male and female adult children, respectively. Confidence intervals are at the 95% confidence level.

4.3 Effects by Subgroups

4.3.1 Evidence of Income Effects

While one of the main implications of parental health shocks is the increased demand for informal care, which reduces work incentives, health shocks may also generate nontrivial medical costs, particularly when health shocks are defined as hospitalization. Consequently, parental health shocks may not uniformly imply a lower employment rate because of a higher marginal utility of consumption, which is a key additional channel beyond the substitution effect that has been examined by the literature on the labor supply effects of informal care. To empirically investigate this mechanism, we explore how the employment effect of parental health shocks differs across individual income and household wealth.

The upper panel of Table 5 presents the heterogeneous effects by individual income.[10] We find that the employment rate decreased by 4.77 percentage points for females with median income, which is larger than our previous estimates of 3.73 percentage points. However, for women with every one percent higher income, the likelihood of non-employment is reduced

[10]We construct the variable of log income as each individual's income from employment averaged over all periods; thus, the variable is time-invariant. We also subtract the median of log income to facilitate the interpretation of the main effect.

by 0.04 percentage points. For example, for a female with an annual income of 48,000 yuan, the decrease in employment rate following a parental health shock will be as small as -0.77 percentage points. Meanwhile, the employment rate decreased by as much as 8.05 percentage points for females with incomes that were lower than the median, whereas the effect for those with higher-than-median income was significantly smaller. Notably, the estimates reveal a positive effect for females with incomes that were higher than 52,692 yuan.[11] While this income gradient is also qualitatively consistent with a setting without income effects, as the heterogeneous effects can be driven by different opportunity costs of informal care, the positive employment effect cannot be explained by only the substitution effect.

Table 5: Employment Effects by Individual Income and Household Wealth

	Male	Female	Male	Female
Individual Income				
Parental health shock	0.0012	-0.0477^{***}	0.0065	$-.0805^{***}$
	(0.0092)	(0.0171)	(0.0153)	(0.0220)
Shock × log income	0.0049	0.0399^{***}		
	(0.0101)	(0.0147)		
Shock × $\mathbf{1}\{\text{income} > \text{median}\}$			-0.0089	0.0519^{*}
			(0.0171)	(0.0290)
Mean income, Yuan	44,452	29,820	44,452	29,820
Median income, Yuan	36,000	24,000	36,000	24,000
Observations	12,802	9,697	12,802	9,697
Dep. mean	0.949	0.840	0.949	0.840
Household Assets				
Parental health shock	-0.0085	-0.0388^{**}	-0.0231^{*}	-0.0387
	(0.0104)	(0.0189)	(0.0140)	(0.0285)
Shock × log asset	-0.0346^{***}	0.0006		
	(0.0133)	(0.0230)		
Shock × $\mathbf{1}\{\text{asset} < \text{median}\}$			0.0399^{**}	-0.0005
			(0.0178)	(0.0328)
Mean assets, Yuan	263,510	263,510	263,510	263,510
Median assets, Yuan	239,080	239,080	239,080	239,080
Observations	11,560	10,275	11,560	10,275
Dep. mean	0.939	0.803	0.939	0.803

Notes: This table demonstrates how parental health shocks affected the employment of individuals with different incomes and household assets, excluding individuals from the top quartile for results by assets. *** p<0.01, ** p<0.05, * p<0.1. Control variables are the same as the baseline regression. Standard errors clustered at the individual level are in parentheses.

The results for household wealth provide additional evidence regarding the income effects of parental health shocks. The lower panel of Table 5 shows that the females from households with median assets experienced a 3.88 percentage point decline in employment following a shock, and this effect did not differ significantly across households with different wealth level. Interestingly, while male employment did not change for median households, it increases by 0.0346 percentage points for households with every one percent fewer assets.[12] In particular, the employment rate of males from households with lower-than-median assets increased by 1.68 percentage points following a parental health shock, in contrast to those with higher-

[11]The top decile of female income in our sample was 56,000 yuan.

[12]Because wealthy families tend to face different employment decisions, we exclude individuals with assets above the top quartile.

than-median assets, where employment decreased by 2.31 percentage points.

Figure 4 visualizes the income and wealth gradients, with the results estimated by sequentially excluding individuals with top percentile income or assets.[13] As the sample is restricted to individuals with lower incomes, the effects on male employment remained close to zero. However, while the average impact on female employment was -3.73 percentage points, that for females with income below the first quintile was as large as -10.11 percentage points.

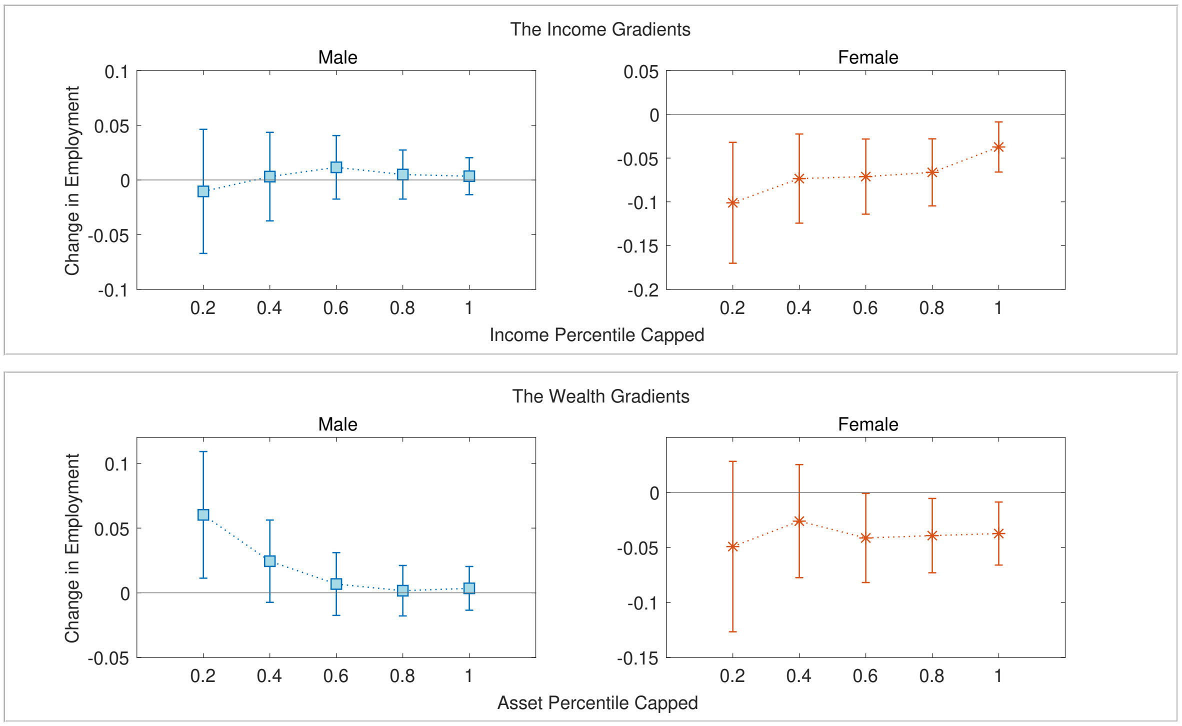

Figure 4: Income and Asset Gradients by Restricted Sample

Notes: This figure illustrates the income and wealth gradients in the employment effects of parental health shocks. The results are estimated using a restricted sample with income or assets capped at corresponding percentiles. Confidence intervals are at the 95% confidence level.

Wealth gradients reveal a different picture, wherein female employment effects remained largely stable regardless of the sample restriction. Notably, while male employment did not change in the full sample, a significant increase is found among males from families with less than the lower quintile of asset distribution, with a 6 percentage point increase.

Overall, we determine that the positive employment effects of parental health shocks cannot be explained by the higher demand for informal care alone. These effects indicate the presence of income effects and are crucial to capture the differential effects across genders.

[13]We provide estimates by each quintile in Appendix B. While the estimates are noisier due to small sample size, the gradients are qualitatively the same.

4.3.2 Other Heterogeneity

Table 6 further presents heterogeneous employment effects based on education, marital status, parents' type, and urban-rural residence. Adult children with higher education tend to earn higher labor income, with a higher opportunity cost of caregiving, which we expect to result in milder reductions in employment following a parental health shock. Considering the income effect, we expect this negative impact to be further diminished or even reversed. Regarding marital status, intrahousehold specialization implies that the negative employment effect for married females may be larger. However, household economy of scale may also indicate the availability of more resources for risk-sharing, reducing the necessity for wives to exit the labor market. Concerning the shocks to which parents, if individuals receive greater altruistic utility from caring for their own parents and decision-making is noncooperative, it is more likely that such health shocks have a greater impact than those of parents-in-law. This heterogeneity is also related to concerns regarding the higher likelihood of co-residence with husbands' parents. Finally, formal care is extremely rare in rural areas of China, and rural residents have notably lower labor earnings compared with urban residents. Therefore, the gender gap concerning the burden of informal care could be unequal across rural and urban areas.

Table 6: Employment Effects by Subgroups

	Male	Female		Male	Female
College	0.0141	-0.0263	High School	0.0015	-0.0409***
	(0.0183)	(0.0306)		(0.0095)	(0.0162)
	$2,924^{\dagger}$	2,680		12,997	11,564
	$0.958^{\ddagger}$	0.888		0.936	0.797
Married	0.0092	-0.0365**	Singles	0.0112	-0.0978*
	(0.0090)	(0.0151)		(0.0267)	(0.0500)
	12,725	12,777		3,196	1,467
	0.950	0.810		0.898	0.854
Own Parents	0.0025	-0.0316	Parents-in-Law	0.0059	-0.0445***
	(0.0089)	(0.0274)		(0.0255)	(0.0168)
	14,262	4,273		1,796	10,112
	0.938	0.816		0.954	0.814
Urban, All	-0.0096	-0.0345*	Rural, All	0.0151	-0.0378*
	(0.0121)	(0.0210)		(0.0112)	(0.0194)
	7,959	7,151		7,962	7,093
	0.933	0.814		0.947	0.815
Urban, Earners	-0.0090	-0.0529**	Rural, Earners	0.0107	-0.0757***
	(0.0128)	(0.0238)		(0.0131)	(0.0248)
	6,787	5,601		6,015	4,096
	0.942	0.844		0.956	0.836

Notes: This table presents heterogeneous effects based on education, marital status, types of parental health shocks, and urban-rural residence. For urban versus rural, All refers to our main sample with 15,921 observations of males and 14,244 of females, and Earners refers to individuals who reported labor earnings for at least one wave. See Appendix Section C for details. *** p<0.01, ** p<0.05, * p<0.1. † Number of observations are reported in the third row of each panel. ‡ Dependent variables' means are reported in the fourth row of each panel. Control variables are the same as the baseline regression. Standard errors clustered at the individual level are in parentheses.

The first row of Table 6 shows that the employment of females with a high school degree or below are the most negatively affected, while males with a college degree demonstrate a positive effect, although the number is not precisely estimated. Once again, this positive effect is difficult to be explained by the substitution effect alone. The second row of Table 6 shows no evidence that parental health shocks affected male employment, regardless of marital status. In contrast, single females were more affected than married females, although the estimates for single females are relatively imprecise, partially due to the smaller sample size. Finally, we do not find greater changes in employment in response to own parents' health shocks, indicating a high level of cooperation within families.

Interestingly, we do not find an urban-rural gap using the full sample. The urban-rural gap only emerges when we exclude individuals who did not report any labor earnings in any waves. A possible explanation is that these non-earners are more likely to engage in informal jobs, with work arrangements that can be more flexible, and the extensive margin of their labor supply can be less responsive to parental health shocks. Rural residents tend to be associated with a higher share of non-earners. Therefore, excluding this population can lead to larger employment responses.[14]

4.4 Elder Care

The impact of informal care on female labor supply, as an important mechanism through which parental health shocks affect adult children's labor supply, has received extensive attention in previous literature. This study also investigates changes in informal care provision following parental health shocks, exploiting information from the survey question: "Who took care of you when you were sick in the past year?" Possible answers to this question include: spouse, children or their spouses, grandchildren or their spouses, other family members, friends, social services, caregivers, and parents. We generate a dependent variable based on this question to identify whether care was provided by the children or their spouses. However, the survey does not differentiate whether the care is provided by parents' own child or the child-in-law. To examine gender differences, the following analysis focuses on a subsample of unmarried children.

Table 7 presents the estimates of the changes in the probability of eldercare provided by unmarried children after parental health shocks. The findings reveal that the likelihood of eldercare increased significantly for males and females after parental health shocks, suggesting that the predominant reason for the decline in female employment may stem from

[14]It can be helpful to consider individuals as never-takers, compliers and always-takers of employment. While urban residents are more likely to be always-takers due to their higher income levels, rural residents are more likely to be compliers as well as never-takers. These never-takers may already have weak attachment to the labor market, and parental health shocks may subsequently have a smaller impact for them. Please see Appendix C for more discussion on the sample of nonearners.

weaker attachment to the labor market. A reminder is that couples may exhibit different behaviors regarding eldercare provision due to intrahousehold specialization. Therefore the results based on singles are only provided as indicative facts, and any extrapolation should be approached with caution.

Table 7: Effects of Parental Health Shocks on Unmarried Children's Eldercare Provision

		Male	Female
Effects on Eldercare	Coefficients	0.2435***	0.1675***
		(0.0379)	(0.0555)
	Observations	3,476	1,716
	Dep. Mean	0.327	0.332
Effects on Employment	Coefficients	0.0112	-0.0978*
		(0.0267)	(0.0500)
	Observations	3,196	1,467
	Dep. Mean	0.898	0.854
Implied Effects of Eldercare on Employment	Coefficients	0.046	-0.584

Notes: This table presents results examining the effect of parental health shocks on the likelihood of unmarried adult children's eldercare provision. Control variables are the same as the baseline regression. Standard errors clustered at the individual level are in parentheses.

Since our data enable us to estimate the effects of parental health shocks on both eldercare provision and employment, it is also feasible to estimate the effect of informal care on employment if one is willing to impose the monotonicity and exclusion restriction assumptions. The implied effects of informal care on employment were 0.046 for males and -0.584 for females. For comparison, Crespo and Mira (2014) revealed effects between -0.45 and -0.65 for females in southern European countries and Carmichael and Charles (2003) found an effect of -0.356 for primary female caregivers within the household but no significant gender difference. However, our result is significantly larger than the -0.04 reported by Schmitz and Westphal (2017). Once again, exclusion and monotonicity assumptions may not be satisfied due to the income effect generated by parental health shocks. Nevertheless, we provide this result for comparison, as this IV estimate differs from previous estimates in that it is based on the causal first-stage and causal reduced-form estimates obtained via DiD, rather than ordinary least squares assuming the exogeneity of parental health shocks.[15]

5 Robustness Tests

5.1 Treatment Effects Heterogeneity

The identification of the baseline results primarily relies on variations in labor supply of the treatment group relative to the control group before and after a parental health shock.

[15] Specifically, our first-stage and reduced-form regressions of the IV estimation do not rely on (conditional) exogeneity of parental health shocks but on conditional parallel trends, which is plausibly weaker.

In practice, the control group includes the following cases: (1) observations of individuals who never experienced a parental health shock during the sample period; (2) observations prior to the parental health shock of individuals who experienced a shock in a later period of the sample; (3) observations after the parental health shock of individuals who have already experienced a shock. Goodman-Bacon (2021) noted that in the third case, if the effects of the event are not homogeneous (e.g. dynamic effects), the estimation results of the staggered DiD can be biased. Sun and Abraham (2021) further demonstrated that estimates of the event study are also biased. In our context, it is highly unlikely that adult children's labor supply changes would remain constant following a parental health shock. Therefore, our previous results may be biased even if the parallel trend assumption holds.

As a robustness test, we present results based on the two-step approach proposed by Callaway and Sant'Anna (2021) (CS-DiD). The effects are first calculated based on each granular 2×2 DiD, which are then aggregated. Specifically, this method removes the post-shock observations of individuals who have already experienced the shock from the control group. We also select individuals who never experienced a parental health shock during the sample period as the control group to yield cleaner and more interpretable estimation results.

Table 8: Dynamic Effects Based on Callaway and Sant'Anna (2021)

	Work Hours		Employment	
	Male	Female	Male	Female
Before at least 6 years	-	-1.8239	0.0463	-0.0792
	-	(5.6074)	(0.0386)	(0.0884)
Before 4 years	-2.3983	-0.9773	-0.0072	0.0017
	(2.9548)	(3.8554)	(0.0258)	(0.0406)
Before 2 years	3.5283	-2.5839	0.0104	-0.0093
	(2.7132)	(3.2860)	(0.0167)	(0.0254)
Year of the shock	-1.9458	2.3140	-0.0097	-0.0381**
	(1.4548)	(1.6309)	(0.0113)	(0.0174)
After 2 years	-1.8710	-0.8657	0.0022	-0.0472*
	(1.9528)	(2.6355)	(0.0148)	(0.0249)
After 4 years	2.2409	-6.9264**	-0.0040	-0.0432
	(2.2191)	(3.3901)	(0.0205)	(0.0353)
After at least 6 years	0.3224	0.6186	-0.0443	-0.0502
	(3.4675)	(4.1383)	(0.0350)	(0.0523)
observations	5,226	3,730	10,063	9,186
Dep. Mean	50.78	46.12	0.942	0.805

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Control variables are the same as the baseline regression. Standard errors clustered at the individual level are in parentheses.

Table 8 presents our new results, revealing that the employment rate of the treated female decreased by 3.81 percentage points in the year of the shock and 4.72 percentage points 2 years after the shock compared with females who did not experience a parental health shock. Results for males exhibit no significant changes. Although the use of a more stringent control group reduces the sample size and increases the standard errors of our estimation results, the CS-DiD estimates are quite similar to the baseline estimates, demonstrating the robustness of our results. Figure 5 visualizes the results, once again revealing no significant difference in employment trends prior to parental health shocks between treatment and control groups.

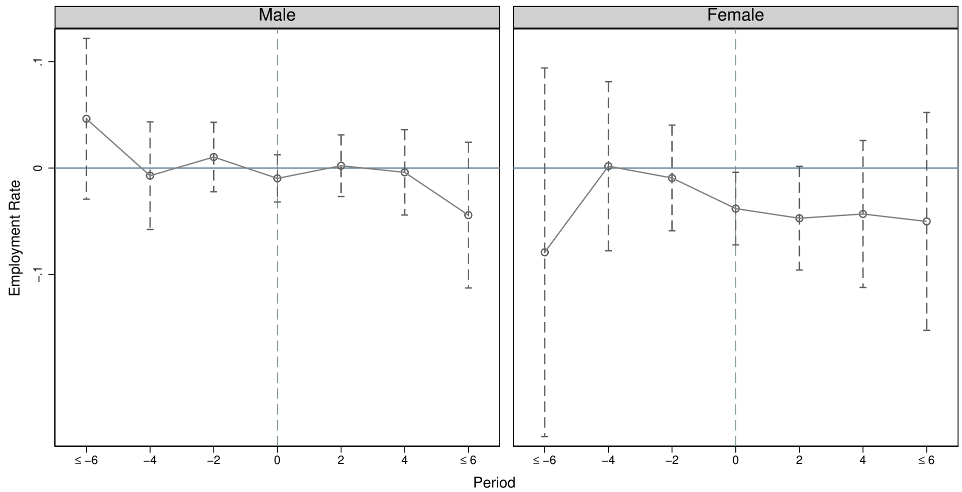

Figure 5: Dynamic Effects Based on Callaway and Sant'Anna (2021)

Notes: This figure presents the results estimated by the method proposed by Callaway and Sant'Anna (2021). Confidence intervals are at the 95% confidence level.

5.2 Unanticipated Hospitalization

Our primary analysis defines a parental health shock as the initial hospitalization observed within the sample period. With a maximum of five waves of data, this approach allows us to track dynamic changes in individuals' labor supply for up to four waves following a parental health shock. It also enables us to examine labor supply trends for up to four waves (approximately 8 years) prior to the shock.

Despite these advantages, one concern may be whether adult children were able to anticipate their parents' hospitalization. If so, they might adjust their behavior prior to the hospitalization, such as increasing their labor supply to save enough money for in-patient costs. While this concern is conceptually plausible, whether this bias exists and the size of the magnitude is an open empirical question. If it does exist, changes in labor supply due to anticipation could invalidate the parallel trend assumption and fail our pre-trend tests.

Empirically, we do not observe systematically different trends in employment between the treatment and control groups for males or females. Indeed, while households may have some leeway in deciding when to seek medical attention for certain diseases, severe health issues necessitating in-patient treatments (e.g. stroke or heart disease) are difficult to defer intertemporally. Moreover, if any intertemporal decision concerning hospitalization was possible, it would likely occur within months rather than across the several years that our research design focuses on. Due to data limitations, we are unable to identify the specific reasons for hospitalization. Nevertheless, we provide additional robust evidence by restricting the sample to parental health shocks that occurred following years of no hospitalization, where anticipated hospitalization is less likely.

Table 9: Results Based on a Restricted Sample with No Prior Hospitalization Records

	(1)	(2)	(3)	(4)
Male				
Parental health shocks	0.0034	0.0015	0.0097	0.0079
	(0.0084)	(0.0089)	(0.0111)	(0.0158)
Observations	15,921	12,051	9,621	8,250
R-squared	0.011	0.010	0.009	0.009
Dep. Mean	0.940	0.942	0.944	0.945
Female				
Parental health shocks	−0.0373***	−0.0423***	−0.0639***	−0.0819**
	(0.0143)	(0.0153)	(0.0206)	(0.0324)
Observations	14,244	10,798	8,554	7,318
R-squared	0.038	0.039	0.044	0.044
Dep. Mean	0.814	0.807	0.807	0.805
No prior hospitalization records				
At least 1 period (2 years)		Yes		
At least 2 periods (4 years)			Yes	
At least 3 periods (6 years)				Yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. Control variables are the same as the baseline regression. Standard errors clustered at the individual level are in parentheses.

Table 9 presents results based on a restricted sample including individuals with no hospitalization records for at least some periods prior to the initial observed hospitalization. The estimates remain quite robust, and if anything, the negative effects for females appear to be even larger as the sample restriction becomes more stringent. Meanwhile, the effects for men remain close to zero across all specifications.

5.3 Measure of Hospitalization

One of the motivations to focus on hospitalization rather than self-rated health deterioration concerns biased reporting. Self-rated health is subjective and references for comparison can vary across both individuals and over time (Lindeboom and Van Doorslaer, 2004). Alternatively, the variable of hospitalization is derived from the question : "*During the past one year, have you ever been hospitalized?*", where hospitalization is defined as "*Stayed in ward for at least one night due to diseases or accidents*". Therefore, answers to this question have the advantage of requiring no comparisons with others and being more objective. Nevertheless, this measure is still self-reported. If the reporting bias is random and not systematic, the classical measurement error will generate attenuation bias and our estimates will tend to be underestimated. Regarding any systematic reporting bias, it seems more plausible that individuals may fail to report hospitalization rather than reference a nonexistent experience. In this case, our findings are also likely to be underestimated because some treated individuals may be misclassified as untreated.

Another potential bias could arise from missing hospitalization experience as the question asks only about the year before the interview, while the data were collected biannually, which may logically miss parental health shocks in even years prior to the interview year. With this type II error leading to potential false negatives, some actual treated observations are misclassified as untreated and the treatment effect is likely to be underestimated. Our robustness check in Table 9 partially mitigates this concern. By restricting the sample to observations without hospitalization records prior to the initial one in longer periods, the likelihood of this bias tends to be smaller. For instance, in the absence of previous hospitalization for at least 6 years, the likelihood that hospitalization occurred only in the even but not odd years relative to the interview date should be minimal. Notably, the estimates become larger as we restrict the sample to longer periods without prior hospitalization. The pattern is consistent with the existence of false negative bias, although we refrain from arguing that this is the only rationale behind the findings in Table 9 .

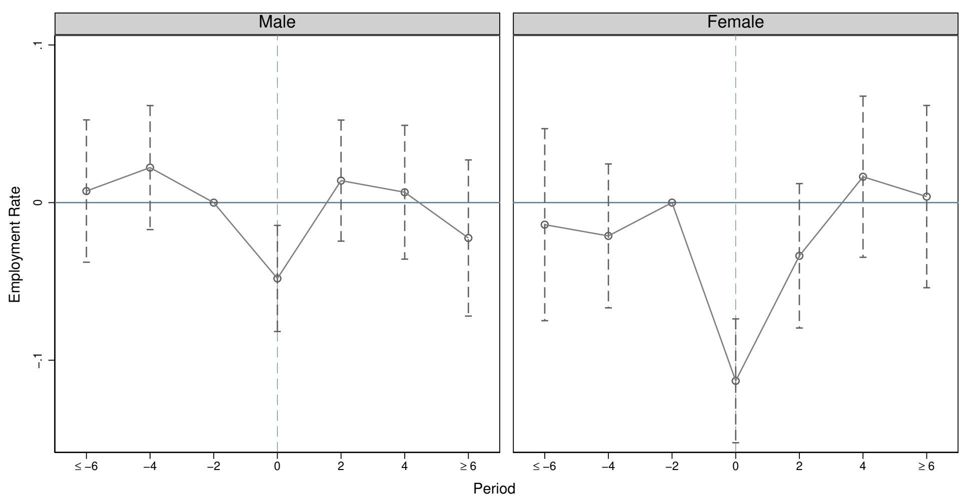

Figure 6: Employment Effects of Adult Children's Own Hospitalization

Notes: This figure presents the effects of individuals' initial personal hospitalization on employment, replacing parental hospitalization in the employment equation with adult children's own hospitalization. Confidence intervals are at the 95% confidence level.

The above discussions indicate that false negatives are conceptually more likely than false positives, indicating that the estimated treatment effects could be underestimated. We also seek additional empirical evidence regarding the validity of the hospitalization measure. Specifically, we explore how adult children's own hospitalization affected their employment as a cross validation for this measure, based on the same sample and same identification strategy, replacing parental hospitalization with adult children's personal hospitalization in the employment equations. The findings reveal that male and female employment experienced notable declines following personal hospitalization. The short-term impact in the year following the shock was 5.77 for males and 11.27 for females, and the average post-shock impact was 4.23 and 7.79 percentage points for males and females respectively. These results indicate that the hospitalization measure indeed carries important information related to health.

Another remarkable finding is that males and females appear to recover rapidly from personal health shocks, as Figure 6 reveals, which underscores the persistent impacts of parental health shocks. Heuristically, the results also indicate that the persistent nature of parental health shocks, rather than depreciation of human capital, is the primary hindrance to females' return to employment.[16]

5.4 Survey Design

This subsection discusses the implications of the CFPS survey design concerning the internal validity of our findings. First, due to social norms in China, adult children are more likely to live with the husband's parents, which implies that parents' health shocks are more likely to come from husbands' own parents and parents-in-law for wives. Our DiD strategy primarily relies on comparisons between individuals of the same gender; therefore, this issue should not pose a significant threat. However, differing sources of health shocks from parents/parents-in-law do affect our interpretation of gender difference in the estimated effects. Conceptually, adult children may care more about their own parents' well-being, resulting in a larger behavioral response to shocks.[17]

To empirically test this concern, we separately examine the employment effects of health shocks from respondents' own parents and parents-in-law. As shown in Table 6, employment effects for males remain statistically and economically insignificant, and while the effect from own parents appears statistically insignificant for females, it may be attributed to a lack of statistical power due to a smaller sample size. However, its magnitude is comparable, if not smaller, than the effect from parents-in-law. Reassuringly, we also observe a similar gender gap when restricting the sample to singles. These findings suggest that the gender difference

[16] We check the average age at which parental and own health shocks occur initially in the sample, i.e. the age since which individuals received treatments. Own hospitalization happened at age 38.0 on average while parental hospitalization at 35.2. Therefore it is unlikely that the persistent impact of parental health shocks is driven by a group of older adult children.

[17] Especially if the family is non-cooperative or the utility is not perfectly transferable even under cooperation.

in our results is unlikely to be driven by a higher co-residence rate with husbands' parents.

Attrition is another concern related to the survey design. As previously described, CFPS permanently tracks *genetic* parents, even if they no longer reside with their children, which largely mitigates concerns of parents' moving out. However, parents were not followed if they moved into nursing homes or passed away. Our benchmark analyses are based on a sample with available parental information, implicitly restricting the sample to children whose parents were alive and not in nursing homes. Therefore, attrition bias cannot be fully ruled out. However, because our main regressions operate at the children's level, we can address the remaining attrition issue conveniently. Regarding missing parental information, it is essential to note that the treatment variable is absorbing, while parental ages are easy to extrapolate. Therefore, we conduct a robustness test by including observations of children when their parents were no longer followed. Column (5) of Table 10 and 11 shows that the results are convincingly similar to our main specifications.

The remaining concern is related to the move-in sample, referring to parents who did not initially co-reside with their children in 2010 being defined as *core members* instead of *genetic members*. As a result, their information is unavailable before the time of co-residence, and the observations of their children in the main sample are selected. In Column (7) of Tables 10 and 11, we restrict the sample to children whose parents co-resided with them at least two years prior to their initial hospitalization, excluding data from move-in families. The estimates remain robust across genders, with the parental health penalty on female employment even appearing to be slightly larger.

5.5 Prime Working Age

The main analysis confines the age range of adult children between 16 and 64 years old to gauge the overall impact of parental health shocks. Younger individuals' labor supply may be influenced by attending school, while those aged 50 and above could be affected by retirement policies since the statutory retirement ages are 50 for female workers, 55 for female officials, and 60 for males. It is crucial to understand whether our findings are driven by these groups, which often exhibit weaker labor market attachment. Moreover, our previous results are more concerning if they persist among prime-age workers. To address this concern, we further restrict the sample to individuals aged 25 to 50 years old. As shown in Column (3) of Table 10, the average employment rate for prime-age female workers following parental health shocks decreased by 3.5 percentage points, which is statistically significant at the 5% level and close to our baseline estimate of 3.73 percentage points.

5.6 More Rigorous Controls

In the benchmark analysis, the age of each parent or parent-in-law is included in the regression in a quadratic form. Adult children's years of schooling are controlled for as a continuous variable with a linear slope. Parents' age and children's education are critical variables in determining adult children's labor supply and may have nonlinear effects. This robustness test allows both variables to be fully saturated. Column (4) in Table 10 shows that under stricter control of these variables, females' employment decreases by 3.77 percentage points due to the parental health shock, which is significant at the 1% level.

5.7 Self-Rated Health Decline

This study defines parental health shocks as initial hospitalizations. Self-rated health is a common health measure and has been widely used in previous literature. In this subsection, we explore results using parents' initial self-rated health deterioration as health shocks. The last column in Table 10 presents results based on this definition, revealing no evidence of a significant impact from this health shock measure, even on female employment. While self-rated health may capture more comprehensive health changes compared with hospitalization, it may also include milder health changes. It is also likely to be subject to greater measurement errors. These results highlight the importance of using a more objective health measure and considering the consequences of more severe health shocks when assessing the impact of elderly health shocks.

5.8 Potential Bad Controls

Our main regressions control for family assets, which is an important variable related to household socioeconomic status, because it is likely to correlate with both parental hospitalization and adult children's employment. Moreover, the variable is time-varying and cannot be addressed by individual fixed effects.[18] However, this variable may partially absorb the impacts of parental health shocks. Similarly, while adult children's own health is an important predictor of their labor supply, it may also correlate with the hospitalization of their parents.

To examine these trade-offs, we implement robustness tests for our main results by excluding these variables. As Column (6) in Table 10 and Table 11 show, the estimates remain robust and notably similar to the baseline results.

[18] In particular, around 2015, there were significant boom and bust of Chinese stock markets.

Table 10: Robustness Tests, Employment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Male								
Parental health shock	0.0034	-0.0081	0.0101	0.0047	0.0037	0.0034	0.0015	
	(0.0084)	(0.0114)	(0.0086)	(0.0085)	(0.0083)	(0.0085)	(0.0089)	
Self-rated health decline								0.0014
								(0.0093)
Observations	15,921	10,063	13,647	15,921	17,346	15,921	12,051	15,773
R-squared	0.011	-	0.008	0.035	0.010	0.011	0.010	0.011
Dep. mean	0.940	0.942	0.949	0.940	0.941	0.940	0.942	0.941
Female								
Parental health shock	-0.0373***	-0.0429**	-0.0350**	-0.0377***	-0.0368***	-0.0374***	-0.0423***	
	(0.0143)	(0.0185)	(0.0149)	(0.0146)	(0.0139)	(0.0143)	(0.0153)	
Self-rated health decline								-0.0025
								(0.0153)
Observations	14,244	9,186	11,806	14,244	15,700	14.244	10,798	14,086
R-squared	0.038	-	0.025	0.058	0.039	0.037	0.039	0.037
Dep. mean	0.814	0.805	0.841	0.814	0.817	0.814	0.807	0.815
CS-DiD		Yes						
Prime working age			Yes					
More rigorous controls				Yes				
No attrition					Yes			
Excluding bad controls						Yes		
Co-residence prior to shock							Yes	

Notes: This table presents estimates of various robustness tests using staggered DiD. *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at the individual level are in parentheses.

Table 11: Robustness Tests, Work Hours

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Male								
Parental health shock	-0.7743	-0.7742	-0.6635	-0.5263	-0.9535	-0.7489	-0.6497	
	(0.7936)	(1.1820)	(0.8459)	(0.8026)	(0.7733)	(0.7935)	(0.8476)	
Parental self-rated shock								1.4476*
								(0.8693)
Observations	11,117	5,226	9,592	11,117	12,229	11,117	8,411	11,013
R-squared	0.026	-	0.024	0.057	0.021	0.026	0.027	0.027
Dep. mean	50.50	50.78	50.77	50.50	50.46	50.50	50.48	50.52
Female								
Parental health shock	0.3633	-0.8741	0.0575	0.3828	0.4813	0.3632	0.0632	
	(0.9910)	(1.4861)	(1.0514)	(1.0173)	(0.9656)	(0.9910)	(1.0541)	
Parental self-rated shock								-0.0104
								(1.1044)
Observations	8,435	3,730	7,228	8,435	9,410	8,435	6,288	8,334
R-squared	0.015	-	0.013	0.066	0.014	0.015	0.016	0.015
Dep. mean	45.54	46.12	45.80	45.54	45.46	45.54	45.44	45.58
CS-DiD		Yes						
Prime working age			Yes					
More rigorous controls				Yes				
No attrition					Yes			
Excluding bad controls						Yes		
Co-residence prior to shock							Yes	

Notes: This table presents estimates of various robustness tests using staggered DiD. *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at the individual level are in parentheses.

6 Conclusion

The proportion of the aging population is soaring in China and many other developed and developing countries. The increasing incidence of elderly health shocks, coupled with the prevalence of risk-sharing among families and a rising dependency ratios, tends to generate spillover effects to other family members with significant social costs.

This study provides causal evidence regarding the parental health penalty that is gender-specific and persistent. Using a unique dataset that has recently matured for dynamic analysis, this study examines the causal effects of parental health shocks on adult children's labor supply. Unlike existing findings for developed countries, the empirical results reveal significant gender differences in employment changes following a parental health shock. Additionally, our dynamic analysis finds no evidence of recovery at least 6 years following the shock, in contrast to the quick return to employment after adult children's own health shocks. We also find that subgroups experience rising employment rates following a shock, showing evidence of the income effect. These findings indicate a concerning consequence of "growing old before getting rich" for developing countries and the necessity of improving social and market insurance systems.

Appendix A: Descriptive Statistics by Parental Hospitalization Status

The following table reports the descriptive statistics for the group of observations whose parents reported no hospitalization and whose parents reported the initial hospitalization. The group of no hospitalization includes observations prior to the initial parental hospitalization, including those never-treated in the sample period. The group of initial hospitalization includes observations whose parents reported hospitalization the first time in the sample period. The results show that initial hospitalization tends to happen at older ages of parents. Accordingly, the average age of adult children is higher and children's own health is worse. Sample with initial parental hospitalization are also more likely to be married and be urban residents. However, there is no statistically significant difference in both the extensive and intensive margins of labor supply, gender, education, family assets and having young children.

Table 12: Descriptive Statistics by Parental Hospitalization Status

	No Hospitalization		Initial Hospitalization			
Variable	Mean	Obs.	Mean	Obs	Difference	p-value
Employment status	0.876	17,823	0.877	4,501	-0.001	0.916
Weekly work hours	48.1	11,039	48.2	2,786	-0.1	0.837
Age	34.3	17,823	35.2	4,501	-0.9***	0.000
Self-rated health	2.69	17,823	2.77	4,501	-0.08***	0.000
Gender	0.53	17,823	0.53	4,501	-0.01	0.525
Years of education	9.58	17,823	9.47	4,501	0.10	0.142
Marital status	0.833	17,823	0.849	4,501	-0.015**	0.011
Log of family assets	12.51	17,823	12.54	4,501	-0.02	0.334
Having children under 6	0.367	17,823	0.355	4,501	0.013	0.110
Urban/ Rural	0.474	17,823	0.497	4,501	-0.023***	0.006
Parental Averaged Age	61.6	17,823	63.0	4,501	-1.4***	0.000

Notes: This table presents descriptive statistics of main variables for the group of no parental hospitalization and of initial parental hospitalization. The group of no hospitalization includes observations prior to the initial parental hospitalization, including those never-treated. The group of initial hospitalization includes observations whose parents reported hospitalization the first time in the sample period. Parental average age is defined as the average over the union of parents and parents-in-law. *** p<0.01, ** p<0.05, * p<0.1.

Appendix B: Income and Wealth Gradients

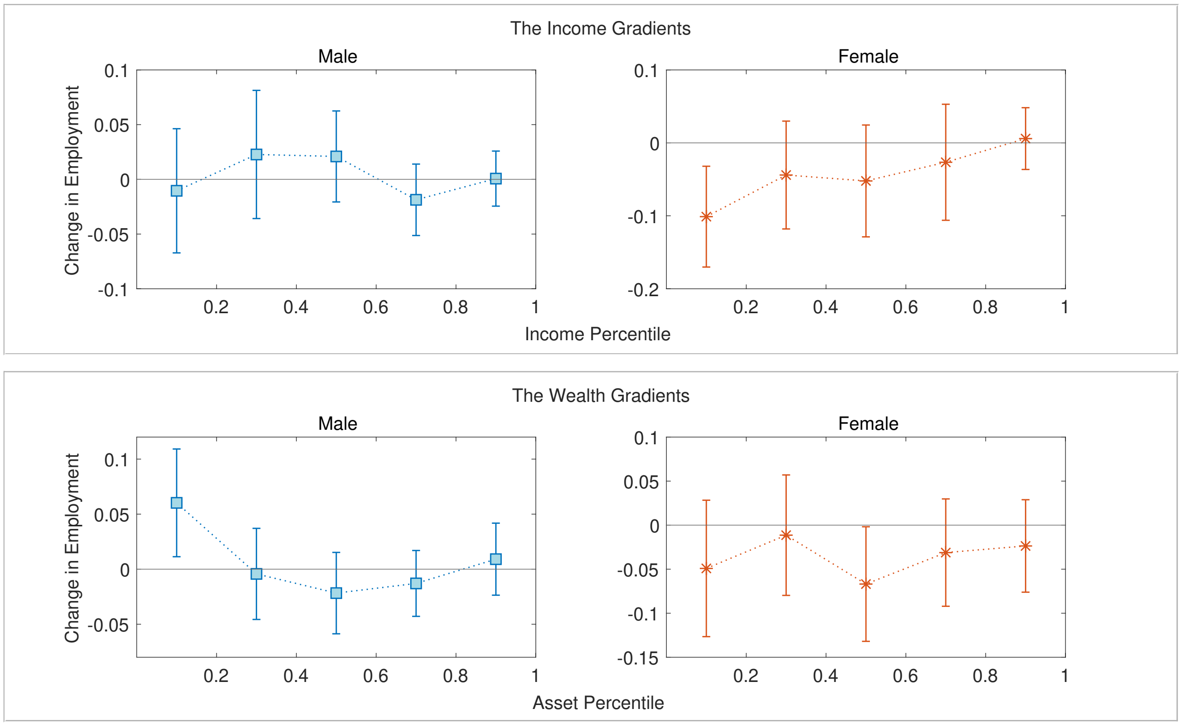

Figure 7: Gradients in Income and Asset Estimated by Quantiles

Notes: This figure shows the income and wealth gradients in the employment effects of parental health shocks. The results are estimated by individuals from each quintiles. Confidence intervals are at the 95% confidence level.

Appendix C: Sample without Reported Income

There is an implicit sample restriction in the analysis of income gradients in Table 5. This restriction includes individuals that have reported labor income for at least one wave. Specifically, to explore income gradients, we define each individual's "permanent" income as the average over all periods under employment. Then it is carried over to observations of the same individual when non-employed. Therefore, this income variable is different across individuals but remains fixed within individuals. This practice provides clean results about how the impacts differ across individuals. However, there are individuals that did not report labor income in any periods, for which we cannot define their average income. While this group of individuals are included in our main analysis, they are implicitly excluded in the analysis of income gradients.

While it is difficult to substantiate the exact reason of missing earnings, we find these individuals are more likely to be female and have lower years of schooling. Even within employed individuals, those with labor income have 10.1 years of schooling on average and

60% are male. Individuals never reported earnings appear to have 7.0 years of schooling on average and 46% are male. We conjecture that the excluded individuals are more likely to engage in informal employments and less attached to the labor market.

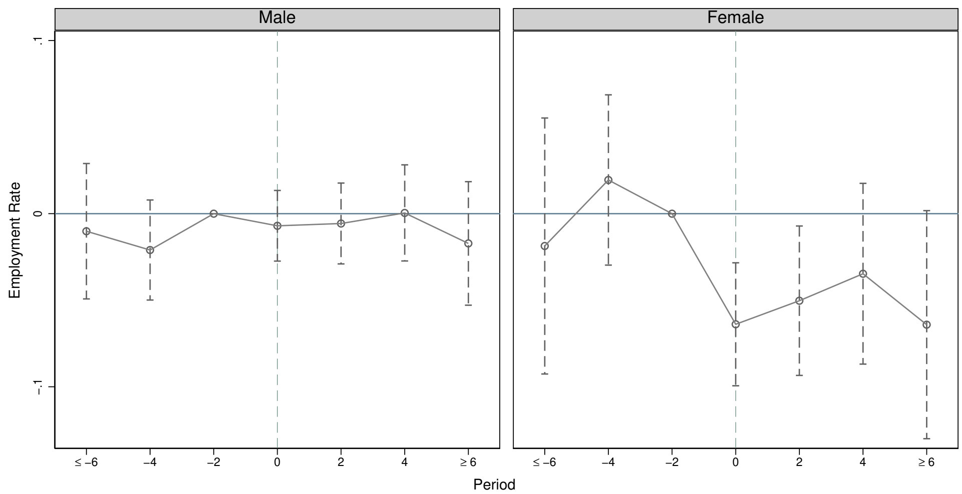

Figure 8: Dynamic Effects Excluding Individuals without Reported Income

Notes: This figure shows the results based on a sample excluding individuals who did not report earnigns in any waves. Confidence intervals are at the 95% confidence level.

To see the role of this sample restriction, we replicate our main analyses by imposing this sample restriction. The sample size is reduced to 12,802 observations of males and 9,697 observations of females. By staggered DiD, we find that the main effect of parental health shocks on male employment remains insignificant with a point estimate of 0.1. However, the decline of female employment appears to be significantly larger, reaching 6.30 percentage points as opposed to the 3.73 in our main results. Meanwhile, results on work hours remain insignificant for both males and females. Figure 8 shows the dynamic effects by event study and the pattern is similar to our main results, with a larger decline. The parallel trend assumption also holds under this sample restriction.

The larger effects by excluding the sample without earnings suggest that these individuals have smaller elasticity of labor supply. The underlying reason may be that these individuals are more likely to be "always-takers" of informal care, and their labor income is so low that the income effect of medical expenditure is minimal.

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