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Health of Elderly Parents, their Children's Labor Supply, and the Role of Migrant Care Workers

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Abstract

We estimate the impact of parental health on adult children's labor market outcomes. We focus on health shocks that increase care dependency abruptly. Our estimation strategy exploits the variation in the timing of shocks across treated families. Empirical results based on administrative data show a significant negative impact on the labor market activities of children. This effect is more pronounced for daughters and for children who live close to their parents. Informal caregiving is the most likely mechanism. The effect is significantly muted after a liberalization of the formal care market, which sharply increased the supply of foreign care workers.
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1 Introduction

Improvements in health care and declines in fertility have led to a global trend of rising longevity and an increasing share of older people. Among OECD countries, the share of the population older than 65 years is expected to rise from currently 17% to 27% in 2050 (OECD, 2019). Since older age is characterized by the emergence of health problems, age-related adverse health events become more frequent in an aging population. These events may reach beyond the affected individual and impact other family members too, including (adult) children.

From a theoretical point of view, children's labor market response to parental health problems is ambiguous. On the one hand, they could reduce their own labor supply to free up time for taking care of their parents. On the other hand, children could focus on supporting their parents by providing financial resources to compensate for the earnings loss associated with the health deterioration as well as to cover care-related spending. This could result in an expansion of their labor supply. Which type of transfer dominates is unclear and not straightforward to quantify. The relationship between parental health and children's labor supply also depends on the availability of formal care services. If formal care is a viable substitute to informal care provided by family members (Mommaerts, 2022), expansions of the formal care market may mitigate any negative effect of parental health shocks on children's labor supply.

We are interested (i.) in the consequences of a deterioration in parental health on the labor market outcomes of their adult children, and (ii.) whether an increase in the availability and affordability of formal care provided by migrant care workers affects the relationship between parental health and adult children's labor market activities.

In the first part of the paper, we examine the causal relationship between parental health conditions and children's labor market outcomes. The identification of this relationship is challenging. First, it is difficult to construct an adequate counterfactual scenario since health conditions are usually endogenous and often change gradually. Second, rich longitudinal individual-level data on health and labor market outcomes for both parents and their children are needed. We tackle these challenges by leveraging high-quality administrative data and by identifying causal effects of two types of sudden and unexpected health shocks, namely strokes and heart attacks (myocardial infarction).[1] Specifically, we employ a quasi-experimental event study design that is based on the comparison of children whose parents *all* experience the same health shock, but at different points in time. Our identifying assumption is that the exact timing of the health shock is as good as random. This strategy allows us to cleanly estimate the effect of deteriorating parental health on the employment and earnings of their adult children.

[1]Strokes and heart attacks are commonly considered as sudden and severe health events that are unlikely to be anticipated (see, e.g., Chandra and Staiger, 2007; Doyle, 2011; Druedahl and Martinello, 2022; Dobkin et al., 2018; Fadlon and Nielsen, 2021).

We show that both strokes and heart attacks are severe health shocks with permanent consequences for parental healthcare spending and labor productivity. One important difference between these two conditions is their differential impact on care dependency. We find that the effect of a stroke on an objective measure of care dependency is ten times larger than the effect of a heart attack. Our estimated effects of parental health shocks on adult children's labor market participation and earnings reflect this gradient. Our baseline estimates show significant negative effects of parental strokes on their adult children's employment and labor earnings. We observe a statistically significant drop of around 2.5 percentage points (3.5%) in children's employment one year after the parental stroke with a slight recovery thereafter. Children's labor earnings decrease more permanently due to the parental stroke. One year after the stroke, earnings are 3.9% lower relative to their pre-shock level. Three years after the stroke, the negative effect is 5.7%. In contrast, we find that heart attacks do not affect children's labor market outcomes. This result provides a first indication that the parental need of care is the relevant factor that explains the negative labor market effects we observe after a stroke. For both types of parental health shocks we do not find negative effects on the overall or mental health of children. Thus, it appears that the negative effect of a parental stroke on the labor market outcomes of their children cannot be explained by the worsening health or increased mental stress of the children.

Probing further into the relevant mechanism behind the labor supply response of the children to a parental stroke, we analyze the response across different subgroups. Specifically, we repeat our analysis for a sample of children whose parents had already retired before the stroke. For these parents, the stroke has no impact on labor earnings (no income shock). Here, we obtain estimated treatment effects of similar magnitude as compared to our baseline estimates. This finding indicates that income compensation through adult children plays only a minor role in our population and is in line with explanations based on the provision of informal care driving the labor market response of adult children. Further support for this interpretation comes from our results on treatment effect heterogeneity by geographical parent-child distance. Children who live close to their parents at the time of the stroke experience negative labor market effects, while the effects on children who live further away are not statistically different from zero. Geographical distance determines the opportunity cost of providing time, but not money. Taking together, this evidence suggests that the provision of informal care is the relevant mechanism behind children's labor market response.

Our analysis also uncovers a gender gradient in the labor market consequences of a parental health shock: For daughters, the estimated average drop in employment and earnings in the three years following the parental stroke amounts to 3.0 and 6.3%, respectively, relative to the pre-mean. The reduction is considerably smaller for sons, with 2.0 and 2.7%, respectively. Thus, our results demonstrate how gender inequality in unpaid

informal care work contributes to gender inequality in the paid labor market.[2]

In the second part of the paper, we go one step further and contribute to the question on how public policies can address the negative labor market effects stemming from worsening parental health conditions. In 2007, the Austrian government enacted a reform that liberalized formal care work at private homes and exempted migrant care workers from pre-existing labor market restrictions for citizens of Central and Eastern EU member countries (Oesterle and Bauer, 2016). We show that this reform led to a large positive supply shock of formal care provided by migrant care workers. Guided by research showing that informal and formal caregivers act as close substitutes (Mommaerts, 2022), we exploit this policy change and implement a triple-difference (DDD) strategy along three dimensions: (a) between time periods before and after the health shock, (b) between families that experience a stroke compared to a heart attack, and (c) between families that experience the shock before or after the implementation of the reform. This strategy is based on the idea that children whose parents suffer from a heart attack are largely unaffected by the care reform, because their parents do not develop caring needs in any comparable way to those who suffer from a stroke. At the same time, children whose parents have a heart attack should be subject to the same general changes in the labor market over time. Our results show significantly positive and economically relevant labor market effects from the reform, suggesting that the care reform, with its large increase in the availability of formal care, helped children to avoid the negative impact of a parental stroke. In sum, this result indicates that formal care provided by migrants substitutes for adult children's care provision, a factor that can be influenced by regulations of the formal care market and respective migration policies.

The substitutability between informal and formal care has to be interpreted against the background of prevailing social norms. Caring for sick parents is an important duty in many cultures. Austria is a typical example for a conservative continental European welfare state, characterized by traditional family values and family-based assistance dynamics (Akerlof and Kranton, 2000; Esping-Andersen, 1999). These traditional norms and values also include elderly care. Figure 1 shows the opinion of individuals regarding the question *"Who should take care of an elderly parent?"* for several countries. In Austria, as well as in other more family-based welfare states such as Germany, around 50% of all respondents believe that it is the role of adult children to take care of their elderly parents. In Scandinavian and Benelux countries, only around 20% of all survey respondents agree with this statement. Our estimation results show that the degree of substitutability between informal care provided by children and formal care providers is high enough to provide scope for policy interventions on the formal care market, even in

[2]This mirrors the literature on gender inequality in labor market outcomes resulting from important life events in the private domain, for example from the arrival of children (Kleven, Landais, and Søgaard, 2019; Kleven, Landais, Posch, Steinhauer, and Zweimüller, 2019).

the context of strong family-based social norms.

This paper makes several contributions to the literature. First, it expands upon existing literature on the impact of negative health shocks on labor market outcomes. Most of the existing studies focus on the effects of a health shock on *own* (i.e., of the affected person) labor market outcomes (see, e. g., Halla and Zweimüller, 2013; Garcia-Gómez et al., 2013; Gupta et al., 2015; Dobkin et al., 2018; Parro and Pohl, 2021). Fewer studies look at the effects of health shocks on *other family members'* labor market outcomes, with a majority focusing on *spouses*.[3] The impact on the labor supply of *adult children* is less understood. An exception is Rellstab et al. (2020) who find no effect of an unexpected parental hospitalization on either children's employment or earnings based on administrative data from the Netherlands. They rationalize their finding with the extensive public availability of formal LTC in the Netherlands, which has one of the highest LTC expenditure per capita levels among the OECD (OECD, 2017). Our setting exploits exogenous variation in the availability of formal LTC to test this argument, and our evidence is in line with their findings for the period *after* the liberalization of the Austrian formal care market. Relatedly, Fevang et al. (2012) examine how the labor market outcomes of children evolve around the time of parental death based on Norwegian register data. They find that children's employment declines in the years before their parents die, especially among daughters. In a recent study, Chen and Lin (2022) show that employment among children in Taiwan decreases substantially when parental LTC needs arise, followed by further decreases in the long run. The increase in parental LTC needs results from a smoothly decaying health process rather than a sudden health decline.

Second, we contribute to an extensive literature on the relationship between care provision by family members and their labor market outcomes (see Bauer and Sousa-Poza (2015) for a review). Most studies in this literature find a negative (although often small) effect of informal caregiving on labor force participation or the probability of employment (see, for e. g., Heitmueller (2007); Bolin et al. (2008); Leigh (2010); Ciani (2012); Crespo and Mira (2014); Schmitz and Westphal (2017); Heger and Korfhage (2020); Simard-Duplain (2022)). Results on the intensive margin consistently point to a negative effect of informal caregiving on working hours (Spiess and Schneider, 2003; Bolin et al., 2008; Leigh, 2010; Van Houtven et al., 2013; Heger and Korfhage, 2020). To observe the pro-

[3]In this context, effects depend on the severity of the health shock. Specifically, for non-fatal health shocks, Dobkin et al. (2018) find no evidence of a response of spousal earnings and Fadlon and Nielsen (2021) find only a modest reduction in spousal employment and earnings. Garcia-Gómez et al. (2013) show that husbands decrease their employment while there is no effect for wives when their partner experiences a health shock. In contrast, Jeon and Pohl (2017) find that men and women reduce their employment and experience earnings losses after their spouses are diagnosed with cancer. When looking at fatal shocks instead, spousal labor supply sometimes even increases to compensate for the income losses caused by the death of a partner (Fadlon and Nielsen, 2021). The size of the effect tends to vary across sexes and how well the institutional system insures against income losses. Breivik and Costa-Ramón (2021) study the impact of children's health shock on parental labor market outcomes. The authors find negative and persistent effects, which are stronger for mothers than for fathers.

vision of care, they usually employ survey data on self-reported care obligations. While this provides a viable indicator for informal caregiving (though potentially not bias-free, see Black et al. (2017)), it is often challenging to exploit quasi-experimental variation using this data. In contrast to this literature, we employ an objective measure of care dependency and use a design-based approach exploiting data from various administrative sources. To infer the effect of informal caregiving on children's labor market outcomes, we compare how children's responses vary between two types of health shocks that differ greatly in their impacts on parental care dependency.

Third, we contribute to the question whether public policies can help adult children to avoid the negative labor market consequences from informal care provision. There are two studies that investigate the role of policies that increase the generosity and coverage of public LTC provision. Løken et al. (2017) study an expansion of public spending on formal care provided to the elderly in their homes in Norway. In response to the reform, they find that adult daughters increase their labor supply at the intensive margin, and reduce the number of sickness absences. Relatedly, Coe et al. (2015) find that an increase in the LTC insurance coverage reduces informal caregiving and increases full-time employment of adult children. In contrast, we are the first to show that more liberal immigration policies targeted towards foreign care workers can mitigate the negative labor market effects stemming from an increase in parental care needs. Hereby, we provide evidence that liberalizing the labor market access of migrant care workers is an important policy measure for governments to support high levels of employment even in the context of rising LTC requirements. Our results are in line with a recent study by Chen and Lin (2022). They show that a reform that decreased the criteria to hire an international care worker in Taiwan increased the employment of eligible children by 12 percentage points. Related to this, Cortés and Pan (2013) study the impact of a policy change that increased the availability of foreign domestic workers on young mothers and find that their labor force participation increases with the reform.[4]

[4]Moreover, there are several studies that do not analyze policy reforms but rather investigate the impact of less-educated immigration on native women's labor market and fertility outcomes relying on the variation in the concentration of immigrants across regions and time. Cortés and Tessada (2011) and Farré et al. (2011) document that less-educated and female immigration, respectively, decreases the time native women spent on housework and increases their time devoted to market work. Less-educated immigration reduces the gender gap in the propensity to work long hours and increases the relative earnings of women in occupations that disproportionately reward overwork (Cortés and Pan, 2019). Furthermore, an increase in less-educated immigration results in a weakening of the negative correlation between fertility and work (Furtado and Hock, 2010) and an increase in native women's fertility and their likelihood of working long hours (Furtado, 2015). Finally, Peri et al. (2015) show that the inflow of immigrants in the local labor market caused women to delay retirement and to increase their labor supply relative to men. Butcher et al. (2022) find that an increase in the less-educated foreign-born labor force share reduces institutionalization among the elderly. These studies do not exploit variation in the number of migrant care workers, but rather focus on broader groups of (less-educated) immigrant workers. Furthermore, they often focus on individuals that are, very broadly measured, more likely to face demand for care, such as individuals with at least one old living parent or mothers of young children. In contrast, our study provides more direct evidence on the effect of a large inflow of migrant care workers

The remainder of this paper is organized as follows. Section 2 describes the institutional background in Austria and the data used for our analysis. Section 3 outlines our research design and spells out the identification assumptions. Section 4 presents the estimated effect of a health shock on own health and labor productivity. Section 5 presents our estimates of main interest, the impact of a parental health shock on adult children's labor market outcomes. Section 6 provides evidence to uncover the causal mechanism underlying our baseline estimates. Section 7 shows how treatment effects are affected by a liberalization of formal care work that increased the supply of migrant care workers. Section 8 concludes the paper.

2 Institutional background and data sources

2.1 Institutional setting

Austria has a comprehensive social insurance system. It provides universal access to high-quality healthcare with mandatory public health insurance. The LTC system is jointly organized by different federal levels and rests on three main pillars: First, at the national, there is an LTC allowance program. Second, there is a publicly coordinated but decentralized market for formal LTC. Third, there is informal care provided by families. In 2016, care-dependent individuals in Austria received the following types of LTC: informal care provided exclusively by the family (42%), formal home-based care provided by mobile services (32%), residential care (16%), 24-hour home care provided by privately paid assistants (5%), and day care (2%) (Famira-Mühlberger, 2020).

2.1.1 LTC allowance program

The cornerstone of the Austrian LTC policy is a comprehensive care allowance scheme (so-called *Pflegegeld*). This is a legal entitlement for every citizen who is in need of care in order to provide funding for care-related expenses. It grants cash on a needs — but not means — tested basis (Firgo et al., 2020). The eligibility is determined by accredited public doctors and based on individual requirements for personal services, assistance, and care (Riedel and Kraus, 2010). The law defines seven levels of care need and corresponding monthly cash benefits of the allowance range from Euro 150.3 at level one to Euro 1,688.9 at level seven (in 2016). Level one is granted when care requirements exceed 65 hours per month, while level seven corresponds to more than 180 hours of needed care per month (see Appendix Table A.1). We use data on care allowance receipt to observe parental care dependency.

Overall, 5.3% of all Austrians received this care allowance in 2016. The allowance is a cash (and not in-kind) benefit that can be used freely. Thus, it can be used for informal

on families all confronted with a severe health shock.

and formal care arrangements alike. While the care allowance certainly helps families to cover certain expenses and to pay for formal care arrangements, the collected benefits are often insufficient to afford full formal care provision (Riedel and Kraus, 2010). Thus, co-financing as well as informal care provision by family members is common.[5] Overall, the legal framework encourages (at least partly) the informal provision of care by family members, which mirrors preferences of the population about who should take care of an elderly person (see Figure 1).

2.1.2 Formal care

The second main element of the Austrian LTC system consists of different formal care arrangements, which are the responsibility of the nine federal provinces. There are two main types: Institutional and home-based care. The former is usually provided by provinces and municipalities, or by religious and other nonprofit organizations. These services typically include care in residential homes, nursing homes, day-care centers, and night-care centers. Home-based services are predominantly provided by nonprofit organizations. They include, among others, home (nursing) care, mobile therapeutic services, home-delivered meals, transport, house cleaning and laundry, and help on weekends. In 2007, a policy reform enabled private households to hire care workers. This policy reform aimed to increase the availability and affordability of formal care in a home-based setting called "24-hour care" provision. A keystone of this reform was the exemption of foreign care workers from the existing labor market restrictions for citizens of Central and Eastern EU member countries. This resulted in a large inflow of foreign care workers to Austria. Today, this home-based care is almost exclusively provided by self-employed live-in migrant care workers from Slovakia and Romania (Oesterle and Bauer, 2016). In Section 7, we exploit this policy change to examine whether this expansion of the formal care market in Austria helped adult children to mitigate the negative effects of a parental health shock.

2.1.3 Informal care

The *Austrian Time Use Survey* (*Zeitverwendungserhebung*) is the best available data source to assess the extent of informal care. The survey data used, conducted between March 2008 and April 2009 (Statistik Austria, 2009), shows that 2.23% of the population provides informal care for the elderly. The average caregiver provides about 9.4 hours per week. The strongest predictor for informal care supply is the age of the caregiver. Both the likelihood of provision and the hours supplied rise with his or her age. The participation rates vary across sexes, with 3.10% among women and 1.18 among men. Thus, about 72% of all caregivers are female. At the intensive margin, the gender divide

[5]Before 2018, there existed a legal system of recourse to the assets of individuals in inpatient LTC and their relatives (so-called "Pflegeregress"). Under "Pflegeregress", the wealth and income of the spouse or children could be confiscated to cover the costs of institutional care.

is less pronounced (women: 9.7 hours, men: 8.7 hours). Finally, there is also variation across educational attainment groups. Individuals with only compulsory education are more than twice as likely to provide informal care as compared to those with tertiary education (3.20 versus 1.36%). All these patterns are also present in a regression-based analysis (see Appendix Table A.2). Another survey among Austrians ($N = 16,279$) with caring needs conducted in 2014 finds that two-thirds of the respondents received care from close relatives (Famira-Mühlberger, 2017). Around 30% of the care was provided by the spouse, 24% by the daughter, and 12% by the son.

2.2 Administrative data sources

To study the effect of the health of elderly parents on their children's labor supply, we combine several administrative date sets: the *Upper Austrian Health Insurance Fund* (UAHIF) database, the *Austrian Social Security Database* (ASSD) and data from the *Austrian Ministry of Finance*. The UAHIF is the main statutory health insurance provider in the federal state of Upper Austria.[6] Around 75% of the Upper Austrian population is insured via the UAHIF. The remaining 25% is comprised of self-employed individuals, farmers, civil servants, and workers in the railway and mining industries, who are insured with other institutions. Importantly, pensioners continue to be insured with the UAHIF when they have been insured prior to their retirement. The UAHIF database comprises individual-level information on the universe of health care service utilization in the in-patient and outpatient sector. This comprises detailed information on expenditures for medical attendance and medical drugs. The ASSD provides us with information on labor market behavior such as employment and earnings, pension, and unemployment spells as well as a range of demographic characteristics. Unfortunately, our data does not contain information on the hours worked or the hourly wage rate. Finally, data from the *Austrian Ministry of Finance* allows identifying all care allowance recipients of Upper Austria on an annual basis. Our data cover the period from 1998 to 2017. The only exception is information on care allowance receipt, which is only available until 2012.

3 Research design

3.1 Empirical strategy

The goal of our analysis is to estimate the dynamic causal effect of parental health on the family members' labor market and health outcomes, with a focus on adult children. The identification of causal effects is challenging in this setting: The health status of a person might be anticipated and endogenous, both with respect to his or her own, and with

[6]Unfortunately, the health insurance data is only available from Upper Austria. Upper Austria has around 1.5 million inhabitants, which makes it the third-largest federal state of Austria.

respect to other family members' outcomes. First, if health is gradually deteriorating, both affected parents and their children may anticipate future care requirements and start adjusting their labor market behavior, as well as their human capital formation even before a certain health deterioration takes place. We tackle this issue by exploiting sudden health shocks. We focus on strokes and heart attacks experienced by an individual for the first time. Second, parents' health status might be related to unobservable characteristics that potentially correlate both with their own and their family members' outcomes. To address this issue, our identification builds on a homogeneous group of families, who all experience the same parental health shock, but at different points in time. Thus, they differ only in the timing of this sudden deterioration of parental health. In other words, our research design exploits the randomness in the timing of the parental health shock, which is similar to that used by Druedahl and Martinello (2022) and Fadlon and Nielsen (2021, 2019).

We employ the following event-study model with cohort-specific time and individual fixed effects:

$$O_{i,t} = \kappa_{<-12} I\left\{t - \tau_i < -12\right\} + \sum_{\substack{k=-12 \\ k\neq -4}}^{12} \cdot \kappa_k \cdot I\left\{t - \tau_i = k\right\} + \lambda_{c,t} + \psi_i + \varepsilon_{i,t}, \tag{1}$$

where $O_{i,t}$ denotes the outcome of individual i observed in quarter t. We separately estimate the model for parents and children. A parent has an own health shock whereas a child experiences a health shock of his or her parent at time τ_i. The number of quarters relative to the quarter of the shock is denoted by $k = t - \tau_i$ and, hence, $I\left\{t - \tau_i = k\right\}$ are indicators for time relative to the quarter of the shock. We omit the event time dummy at $k = -4$, standardizing its coefficient to zero. $\lambda_{c,t}$ and ψ_i are quarter-by-cohort and individual fixed effects, respectively. By cohort, we refer to the birth year of individual i. We report robust standard errors clustered at the individual- and the family-level (as a robustness).

The parameter κ_k is the dynamic treatment effect k quarters to the shock relative to the reference period four quarters before the shock. Parameters for $k \geq 0$ refer to effects after the shock. In this type of event-study model it is important to assure that the dynamic treatment effects are separately identified from secular time fixed effects. In a setup with no never-treated units, like ours, i.e. all individuals experience a parental health shock at some point, dynamic effects are only identified up to a linear trend (Borusyak et al., 2021). Restricting the effect window by the binning of endpoints introduces restrictions to avoid underidentification. In our model, we restrict the treatment effects to be constant before twelve quarters prior to the shock with the binned treatment indicator $I\left\{t - \tau_i < -12\right\}$. Intuitively, binning assigns the individual-quarter observations outside of the effect window to the control group that pins down the secular time trends

(Schmidheiny and Siegloch, 2020).

To interpret the event-study coefficients κ_k as causal effects of the parental health shock requires the identifying assumption that, conditional on cohort-specific time and individual fixed effects, the timing of the shock is uncorrelated with the outcome variable. This implies that the outcome would have evolved in a parallel way for individuals in the same cohort that are treated at different points in time in absence of the health shock. Our event-study design allows to examine patterns in outcomes in the quarters up to three years before the shock, which provides a test for the plausibility of our assumption. The quarter-by-cohort fixed effects included in our model control non-parametrically for cohort-specific life-cycle and time patterns in the outcome variable. This is important because both parents and children have potentially complex counterfactual life-cycle patterns, i.e. the parental health shock hits them, on average, around the time of retirement and of family formation, respectively.

We further must preclude diverging expectations and differential anticipatory responses before the parental health shock across households who are treated at different points in time (Fadlon and Nielsen, 2019). Again, patterns in the estimated effects in the quarters prior to the parental health shock can help to assess the plausibility of this assumption. Furthermore, in our baseline estimations we assume that treatment effects are homogeneous across cohorts with different shock quarters τ_i unlike in Sun and Abraham (2021). We regard this homogeneity assumption as plausible in our case, since families with an early versus late shock should not be systematically different. Nevertheless, we replicate our baseline results using the interaction weighted estimator of Sun and Abraham (2021) (see Appendix Section C). This alternative approach, allows for heterogeneous treatment effects (as discussed, for instance, in Callaway and Sant'Anna (2021) or Sun and Abraham (2021)).[7] Both estimation approaches provide very comparable results. We interpret this as evidence that the assumption of homogenous treatment effects across cohorts is reasonable.

Finally, it is important to note that our event-study approach uses a varying control group over k, the quarters relative to the quarter of the shock (Druedahl and Martinello, 2022). As a robustness check, we implement the approach of Fadlon and Nielsen (2021, 2019), which relies on a fixed control group over k. More precisely, we compare the outcomes of families who experience a health shock in time τ with those of families who experience the same shock twelve quarters in the future, i.e. in $\tau + 12$. We use the latter group as an explicit control group and assign them a placebo shock at time τ. Based on this treatment and control group, we estimate a dynamic difference-in-differences (DD) model. The results from this robustness check are described in Appendix Section D.

[7]Furthermore, we examine heterogeneous treatment effects due to the policy reform in 2007. In Section 7, we distinguish between families with different treatment timing relative to the liberalization of the formal care market in the year 2007.

3.2 Analysis sample and descriptive statistics

We construct our sample based on all legitimate children. This guarantees that we have a link between children and both parents. We then restrict our analysis to families in which at least one parent experiences a stroke or heart attack for the first time between 1998 and 2017. These health shocks are identified by WHO's ICD-10 codes ($I63$ and $I21$, respectively) available in the UAHIF data. In defining the first shock, we face the limitation that we do not observe health outcomes before 1998. We only consider the event of the first observable shock, and in our analysis sample we focus on non-fatal shocks.[8] Furthermore, we only include children in our data who are older than 23 years at the time of the shock. This ensures a certain level of attachment to the labor market throughout the time window of our analysis. Thus, our main sample comprises families that experienced a parental health shock with at least one child above age 23 at the time of the stock.

In our administrative data we cannot link parents to children when those are born prior to 1974. This limitation combined with the fact that we do not observe health shocks before 1998 makes the parents in our estimation sample cases of relatively early stroke/heart attack. In our data, the median age at first stroke is 62 years, compared with 73 years in the general population. The corresponding median ages for first heart attacks are 68 and 59 years, respectively. Web Appendix Figure A.1 shows that there is still a substantial mass to the left of our median age in the age distribution of first stroke and heart attacks in the overall population. This sample of "younger" parents has its advantages and disadvantages. While the non-representativeness of our estimation sample reduces external validity, the health shocks in our population are very likely to be unexpected. Our "younger" parents have "younger" children. They are about 30 years old at the time of the parental shock, and we can study them at an important life period, i.e., at the beginning of their working careers.

The main labor market outcomes in our analysis are employment and earnings. We organize individual observations at the quarterly level and define employment by an indicator that is equal to one if the individual is employed at the quarter date (February 10, May 10, August 10, November 10). Earnings refer to real earnings in euros (2000 prices) in the quarter with the main employer. For non-employed individuals, earnings are set to 0. Note that the ASSD does not provide information on working hours. Thus, our earnings measure reflects changes in wages and hours.[9]

Table 1 provides summary statistics of our sample. It reports average characteristics at four quarters before the respective health shock. In total, we observe 2,763 individuals

[8] In our main sample, we define non-fatal shocks as shocks from which the affected parent does not die within a week. We provide a robustness analysis using only families in which the affected parent is alive over the entire time horizon of our analysis (survival $\geq$ 3 years) in Appendix Section E.

[9] Unfortunately, we do not observe other sources of income in our data such as rental/capital income or government benefits.

who experience a stroke, and 3,437 individuals who experience a heart attack for the first time between 1998 and 2017. Parents suffering from a stroke are, at an average of 61.1 years, somewhat older than those experiencing a heart attack (58.7 years). Around one-third of parents with a stroke and one-quarter of those with a heart attack are female. Around 38% (51%) of the parents suffering a stroke (heart attack) are employed and have average earnings of Euro 2,597 (Euro 3,755) per quarter one year prior to the health shock. About 6% (2%) of all parents hit by a stroke (heart attack) received a care allowance one year prior to the shock.

Their spouses are, on average, around two years younger. Their employment rate is of similar magnitude but their average earnings are lower. Children are around 29 to 30 years old when the parental health shock occurs. They are much more attached to the labor market one year prior to the parental health shock compared to their affected parents with employment rates well above 70% and higher quarterly earnings. In sum, our sample comprises 9,407 children.

4 Estimation results: parents

4.1 Parental stroke

We start by looking at the outcomes of the affected parent around the time of the stroke. Figure 2 depicts the estimated effect of a stroke on various health- and employment-related outcomes on a quarterly basis from three years before to three years after the stroke. As defined above, the effect at event time k is relative to the reference period four quarters before the shock. The figure includes 95% confidence bands around the event coefficients. Panels (a) to (d) show the estimated effect of the stroke on the number of days in the hospital, total health-care expenses, spending on medical drugs, and care allowance receipt, respectively. We find large and highly significant impacts of the stroke on all of these outcomes. Days spent in the hospital increase by, on average, 14 days and total health expenses rise by around Euro 8,000 per quarter at the time of the shock relative to a pre-shock mean of 0.9 days and Euro 689 per quarter, respectively. The effects remain positive and statistically significant in the subsequent quarters, but are substantially smaller in size. Spending on medical drugs increases permanently by around Euro 70 per quarter due to the stroke. Finally, the share of care allowance recipients increases by 14 percentage points with the stroke and remains high several quarters thereafter. This represents a sharp increase, given that only 6% of all parents hit by a stroke received a care allowance one year prior to the shock. It underpins the lasting effect of a stroke on long-term care dependency.

Importantly, the patterns observed in Panels (a) to (d) in Figure 2 indicate no significant pre-shock effects on the respective outcome except in the three quarters immediately

preceding the shock. This provides support for the assumption that there are no general differences in the underlying trends for individuals who are shocked at different points in time. The only notable exception is within the three quarters before the shock. Parental health outcomes slightly worsen in this period, as indicated by small increases in hospitalizations and health-related spending.[10] We also discuss these pre-shock health problems again when presenting our results based on an alternative estimation method (Fadlon and Nielsen, 2021, 2019), which provides a very transparent way to deal with this (see Appendix Section D).

Next, we examine the effect on labor market outcomes of the affected parent around the time of the stroke. Panels (e) and (f) in Figure 2 display the impact of the stroke on parental employment and quarterly earnings, respectively. We observe significant impacts of the shock on these labor market outcomes. The percentage of affected parents who are in employment drops by around 12 percentage points in the quarters after the shock. The effect is relatively constant throughout the post-period. Given that the employment rate is 38% prior to the stroke, this constitutes a reduction in employment of around 30%. The negative effect on quarterly earnings is gradually increasing in absolute terms and amounts to a reduction of around 30% two years after the stroke relative to the pre-shock earnings. Overall, a stroke severely and persistently affects the labor market outcomes of the parent. Again, the patterns observed in Panels (e) and (f) in Figure 2 show no pre-trends up to three quarters before the shock. The decline in parental employment and labor earnings mirrors the development in health-related outcomes around the time of the stroke.

4.2 Parental heart attack

We now turn to the estimated effect of a parental heart attack on parents' health and labor market outcomes (see Figure 3). Like in the case of strokes, we find large and significant effects of heart attacks on all outcomes.[11] However, some important differences emerge. For instance, parents with a heart attack experience an increase of an average of eight days spent in the hospital in the quarter of the health shock, which is about half of the effect for a stroke. Spending on prescription drugs is particularly large in the year following the heart attack, but drops to a substantially lower level thereafter. Most importantly, parents suffering from a heart attack do not experience such a substantial increase in their care dependency as stroke patients. To be more precise, the share of parents who receive

[10] A potential explanation for this pattern is a clustering of so-called *transient ischemic attacks* (TIAs). The medical literature discusses a pattern where strokes can sometimes be preceded by *transient ischemic attacks* (see, e.g., Johnston et al., 2000). Furthermore, the small uptick in care allowance receipt before the shock is related to the fact that this data is only available annually, which makes a clear assignment to a specific quarter difficult.

[11] We also see here some increases in the number of days in the hospital and health-care spending in the quarter prior to the shock. This is most likely due to the treatment of *angina pectoris*, which refers to chest pain or discomfort occurring when the heart muscle does not get as much blood as it needs.

a care allowance increases by 1.4 percentage points due to a heart attack, compared to 14 percentage points due to a stroke. This difference in the need for care suggests that any potential negative consequences on the labor market or health outcomes of children due to caregiving should be more pronounced after a parental stroke compared to a heart attack.

Panel (e) in Figure 3 shows a significant reduction in the employment rate of around 10 percentage points (20%) due to a heart attack. Quarterly earnings decrease substantially as a consequence of a heart attack as illustrated in Panel (f). The reduction in earnings averages around 20% three years after the heart attack relative to the earnings before the shock. In sum, the negative effects on parental labor market outcomes due to a heart attack are similar in absolute but smaller in relative terms compared to the effects due to a stroke.

5 Estimation results: children

5.1 Children after parental stroke

We now turn to the effect of parental strokes on children's labor market outcomes. Our results in Figure 4 show a significant negative effect of a parental stroke on adult children's employment and labor earnings. In Panel (a), we observe a statistically significant drop of around 2.5 percentage points (or 3.5%) in children's employment one year after the parental stroke with a slight recovery thereafter. At the same time, children's labor earnings decrease due to the parental stroke. One year after the shock, quarterly earnings are a statistically significant reduced by Euro 175 (see Panel b). Three years after the shock, the negative effect builds up to Euro 257. This corresponds to a reduction of 3.9 and 5.7%, respectively, relative to the average quarterly earnings prior to the shock.

Panel (a) of Table 2 provides the average effect of a parental stroke on children's labor market outcomes over the post period, i.e. over the twelve quarters after the shock. The average employment effect is a reduction by 1.8 percentage points (or 2.5%). The average effect on children's labor earnings is a decrease by Euro 176 per quarter (or 3.9%). Both effects are statistically significant at the 1 percent level.[12]

When comparing our measured reductions in children's labor supply to the estimated effect of the stroke on parental health our results appear sensible. Panel (d) of Figure 2

[12]Table A.3 replicates the analysis for additional labor market outcomes of the children. A parental stroke leads to a statistically significant reduction in the number of days employed per quarter by 1.6 days (or 2.4%), in the daily wage by Euro 1.8 (or 3.5%), and in the likelihood of full-time employment by 1.4 percentage points (or 1.7%). We obtain almost unchanged results when we cluster the standard errors at the family-level (instead of the individual-level) to account for treated children being from the same family, i.e. having the same treated parent, and, hence, are experiencing the same shock (compare Appendix Figure A.3 with Figure 4). Also using yearly (instead of quarterly) indicators does not change our overall findings (compare Appendix Figure A.4 with Figure 4).

shows that after a stroke parental care dependency increases by around 14 percentage points. For children, we find a reduction in employment of 1.8 percentage points after the stroke (Panel (a) of Table 2). To completely stop working in response to parental care dependency seems rather costly for adult children in their prime working age (children are around 30 years old in our sample). Thus, a considerably lower percentage point change in children's employment compared to the percentage point change in parental care dependency seems reasonable. Furthermore, we consistently find the earnings response to be larger than the employment response. For children in their prime working age a reduction in earnings seems more likely than to stop working altogether, and mirrors the response of Austrian mothers after giving birth (see e.g. Kleven, Landais, Posch, Steinhauer, and Zweimüller (2019)).

5.2 Children after parental heart attack

Next, we turn to the labor market outcomes of children whose parent had a heart attack, as displayed in Figure 5. In contrast to a parental stroke, we do not find a statistically significant labor market effect for children after a parental heart attack. As outlined above, the main difference between the two conditions is that the impact on care dependency is a magnitude larger after a stroke than after a heart attack. Thus, the null result following a heart attack is a first piece of evidence pointing to parental care dependency as the causal mechanism underlying the negative impact of a parental stroke on adult children's labor supply.[13]

6 Mechanism and treatment effect heterogeneity

To probe further into the mechanism behind the negative labor market response following a stroke, we inspect treatment effect heterogeneity through a series of tests. In principle, there are two main mechanisms through which a parental health shock could affect adult children's labor market outcomes: First, children reduce their labor supply to free up time to provide informal care to their parents. Second, children transfer financial resources to their parents to compensate for the earnings loss associated with the health shock. This would cause an increase in their own labor supply. In the following, we employ a sample of parents who are already retired at the time of the stroke to differentiate between the "care shock" and the "income shock". We also inspect the effect of a parental stroke on health outcomes of adult children to study the potential consequences of a parental health shock besides those on labor outcomes.

[13] In Appendix Section B, we additionally present results for the labor market outcomes of spouses.

6.1 Children of already retired parents ("no income shock")

As documented in Section 4.1, parents suffer a substantial loss in labor earnings after the health shock. In principle, adult children's labor supply could provide insurance against an income shock to the parent, i.e. when parents experience an income reduction due to a stroke, their children may compensate for the income loss by increasing their labor supply. If this was the case, our estimates in Figure 4 would present a mix of two opposing forces: A reduction in labor supply due to the increase in (informal) care, and an increase in labor supply to compensate for the parental income loss. To address this issue, we repeat our analysis in Figure 4 for a group in which the income channel is shut off: Parents who were already retired prior to the stroke should not experience a loss in income. Figure 6 displays the results from this analysis. The effect for children whose affected parents were already retired is similar to the effect for the entire sample. This indicates that income compensation through adult children plays only a minor role in our population. Thus, this result is in line with explanations based on the provision of informal care that causes the negative labor market effects for adult children.

6.2 Health outcomes of children after a parental stroke

A parental health shock may also have adverse consequences for the well-being of the children. The deterioration of their parents' health *per se* might have a direct negative impact: Children may worry about their parent and develop stress-induced health issues. Further, the provision of informal care is known to be psychologically and physiologically challenging (Bom et al., 2019). We examine health outcomes of daughters and sons after a parental stroke. Any impact on children's health could represent another causal channel beyond an "allocation of time" effect through which a parental health shock could affect children's labor supply. Figure 7 shows the effect of a parental stroke on two measures for children's health. The dependent variable in Panel (a) is a broad measure for total health care spending (sum of medical attendance in the outpatient sector plus medical drugs). In Panel (b), the outcome is a binary indicator for the use of medical drugs related to the nerve system (e.g. anti-depressants). We do not find evidence of negative health consequences for adult children resulting from a parental stroke. This speaks against a causal mechanism that operates through increased mental stress following a parental stroke on adult children's labor supply. We cannot rule out that children's health suffers from providing LTC in the longer run.

6.3 Who cares more?

We now investigate whether treatment effects are heterogeneous across different groups of children. We distinguish between subgroups based on the sex of the child and the

geographic distance between parent and child.[14] In Table 2, we summarize for these sub-samples the effects of a parental stroke over the entire post-shock period.

6.3.1 Sex of the child

The estimates in Panel (B1) of Table 2 provide evidence that the negative labor market effect of a parental stroke is more pronounced for daughters than for sons. Daughters experience, on average, a statistically significant reduction in their quarterly earnings of Euro 205. Employment of daughters is also reduced by 2 percentage points. The corresponding point estimates for sons also shows a statistically significant reduction, but of smaller magnitude. Relating the point estimates to the pre-shock mean shows that the effects for daughters are much more pronounced.[15] Specifically, we estimate the relative drop in employment and earnings for daughters to amount to 3.0 and 6.3%, respectively. For sons, the reduction relative to their pre-mean is 2.0 and 2.7%, respectively.

To study the dynamics of the effects in more detail, we present the effects over the twelve quarters before and after a parental stroke by the sex of the child in Appendix Figure A.2. One year after the parental stroke, daughters are statistically significant 4.4 percentage points (or 6.7%) less likely to be employed. While this employment differential dissipates over time, we find that the earnings losses of daughters remain relatively constant throughout the post-shock period. Three years after the stroke, daughters earn 9.1% less relative to their earnings before the shock. For sons, we observe much smaller negative effects, which are not statistically distinguishable from zero. Comparing estimates on an annual basis, we find the difference between daughters and sons to be statistically significant for the first two years after the stroke. This set of results is in line with survey evidence showing that women are more likely to perform informal care.

6.3.2 Geographic distance between child and parent

Distance is an important determinant for the cost of caregiving.[16] We expect that children living close to their parents are *ceteris paribus* more likely to provide care and reduce their labor market activities to a larger extent. To test this hypothesis, we construct two subsamples of children. One sample contains only children who live in the same postal code as the affected parent ("local children"), while the other sample comprises those living further away ("distant children"). We measure the postal code four quarters prior to the

[14]Heterogeneity by child parity and the number and gender of siblings would be interesting as well. However, due to data limitations on children born before 1974, we cannot measure these variables without error. Therefore, we refrain from analyzing heterogeneity by child parity and the number and gender of siblings.

[15]Pre-shock mean refers to the mean of the respective outcome variable four quarters prior to the parental health shock.

[16]Konrad et al. (2002) study game-theoretically the residential choice of siblings and predict that first-born children locate further away from their parents to avoid care responsibilities.

shock.

Panel (B2) of Table 2 depicts the effects on the labor market outcomes of local and distant children separately. As expected, we find the treatment effect to be larger in both absolute and relative terms for children living close to their parents. Over the entire post-shock period (i.e., over the 12 quarter after the stroke), these children are, on average, 2.1 percentage points less likely employed and earn around Euro 166 less per quarter. For children living further away, estimated treatment effects are not statistically different from zero. Looking at the relative effects, the difference between local and distant children becomes even more apparent. In sum, the results from variation in geographic distance are in line with explanations based on the provision of informal care driving the negative labor market response of adult children.

7 Liberalization of the formal care market

In our analysis above, we uncover negative impacts of a parental stroke on the labor market activities of children based on health shocks that occurred between 1998 and 2017. In 2007, in the middle of our sample period, a substantial liberalization of the formal care market took place, sharply increasing the supply of foreign care workers in Austria. We investigate now whether this policy intervention attenuated the negative effects of a parental stroke on adult children's labor market activities.

7.1 The 2007 reform

In July 2007, the Austrian government implemented a set of reforms to liberalize the formal care market. A specific goal was to allow private households to hire care workers. Therefore, a legal basis for home care work (the so-called "24-hour care work") was established. This type of service had previously existed, but without any legal basis. The reform turned this small gray economy into a formalized system of privately hired home care workers. The second cornerstone of the reform was the exemption of foreign care workers from Central and Eastern EU member states from the existing labor market restrictions, which applied to citizens of the new EU member states (during the so-called "transition phase"). With this reform, care workers from the new EU member states (henceforth EU-11) gained access to the Austrian care market.[17] They predominantly offer their care services as self-employed under the new legal framework of the 24-hour care work (Oesterle and Bauer, 2016). Furthermore, the government introduced an additional

[17]This comprises Central, Eastern and Baltic European member states that accessed the EU in 2004 and thereafter: the Czech Republic, Estonia, Latvia, Lithuania, Hungary, Poland, Slovenia, and the Slovak Republic in 2004; Bulgaria, Romania in 2007; and Croatia in 2013. The exemption of EU-11 nationals is regulated in the *Auslaenderbeschaeftigungsverordnung* (link). The legality of home care work is set forth in the *Hausbetreuungsgesetz* (link).

allowance for 24-hour care work, complementing the standard LTC allowance program.

7.2 Inflow of foreign care workers

There is no ideal data source to document the inflow of migrant care workers. However, the combination of several data sources leads to clear conclusions. Figure 8 documents the size of the inflow of female foreign care workers to Austria using various administrative data sources.[18] The panels on the left display the group of EU-11 nationals, whereas the panels on the right refer to the group of Austrian nationals. The two top Panels, (a) and (b), show the total number of *employed* female care workers for the two groups.[19] For neither group do we observe a discontinuous increase in dependent employment after the reform in 2007. In the two middle panels, we display the number of *all self-employed* females from two different data sources. In circles, we show the number of self-employed females as registered in the ASSD and, in squares, we show the number of self-employed females recorded in the BALI (budget, labour market and benefit receipt information) online database. While the ASSD also tracks the number of self-employed individuals prior to 2007, it comes with the disadvantage of not recording the sector the self-employed are working in. BALI does record the sector but is not available prior to 2008. Both series in Panel (c) display a strong and almost identical increase in the number of self-employed women from the EU-11 after the reform rising from 10,000 in 2008 to around 40,000 in 2012. Studying the series in circles from the ASSD, we see that this rise in EU-11 self-employed females starts immediately after the implementation of the care reform in 2007. In contrast, Panel (d) shows only a small increase over time for Austrian self-employed women, with no trend break around the time of the reform.

In the bottom panels we exploit the fact that BALI records the economic sector, allowing us to distinguish between all self-employed women, and those working in "other personal service activities" (which is the category under which self-employed care workers fall). Panel (e) shows that almost all self-employed women from the EU-11 work in the care sector and confirms the strong increase in the number of self-employed care workers from the EU-11 after 2007. In contrast, almost no increase in self-employed Austrian women working in "other personal service activities" takes place around that time. To conclude, Figure 8 documents a strong inflow of predominantly self-employed foreign care workers to Austria after the care reform in 2007. This led to a significant overall employment growth in this sector.[20]

[18] Appendix Figure F.5 replicates the analysis for male care workers. It shows that male (foreign) care workers play basically no role on the formal care market in Austria. This is the reason why we focus on females only.

[19] This includes all employees in the ASSD with a NACE category of 8710 (residential nursing care activities), 8730 (residential care activities for the elderly and disabled), 8790 (other residential care activities), or 8810 (social work activities without accommodation for the elderly and disabled).

[20] It is possible that some of the self-employed foreign care workers who registered after 2007 worked

7.3 Why did self-employed foreign care workers become so popular?

A driving force behind the rise in demand for self-employed care workers from the EU-11 is their relatively low remuneration: First, the costs for an equivalized day of formal care provided by a self-employed care worker are around 20% lower compared to hiring a dependent care worker (Famira-Mühlberger, 2017).[21] Furthermore, wages of self-employed *foreign* care workers are much lower compared to domestic care workers. While we are not able to compare wages of the EU-11 self-employed and Austrian care workers in our data, we can use post-reform tax return data to study income differentials between the two groups. We find the annual gross income of self-employed females from the EU-11 to be substantially lower compared to dependent female care workers (see Appendix Figure F.3).

Finally, finding and hiring a self-employed care worker from the EU-11 was made simple and easy. After the reform, private agencies assisting Austrian families in the placement of a self-employed foreign care worker mushroomed. Famira-Mühlberger (2017) estimates that around 600 of those agencies existed in Austria in 2016. In addition, the reform of 2007 was covered extensively by Austrian newspapers.[22] Thus, the salience of the reform was large and most Austrian families with caring responsibilities were aware of the new possibilities to find formal care.

Overall, the reform of 2007 has drastically changed the availability and affordability of formal care in Austria. In the following, we exploit this policy change to examine whether this positive supply shock muted the negative labor market effects on adult children after a parental stroke.

7.4 A triple-difference estimation approach of the 2007 reform

We now ask if a parental stroke has a different impact on adult children's labor supply before and after the reform in 2007. In a first step, we simply split the sample by the year of the shock. Columns (1) and (2) of Panel (B3) in Table 2 summarize the event study results for children whose parents suffered a stroke from 2003 to 2007, whereas columns (3) and (4) display the results for those, who experienced a parental strokes between 2008 and 2012.

The effects of a parental stroke on children's employment and earnings are sizable and negative if the stroke occurred *prior* to the implementation of the reform. In contrast,

illegally in Austria prior to the reform. In Appendix F.1 we examine this possibility in detail, and find this effect to be small compared to the overall increase of EU-11 self-employed women after the reform.

[21] The authors show that this is mostly due to the legal status as self-employed, which means that these workers are not subject to collectively bargained wages or rigid working time provisions that apply to dependent care workers.

[22] We provide an analysis of press articles reporting about the reform in Appendix Section F.2.

parental strokes that occurred in the years thereafter do not affect the employment or earnings of their children.[23] However, we cannot rule out that other changes between these two time periods apart from the care reform took place, which may have affected children's labor market response to parental strokes (e.g., changes in the overall economic or labor market situation).

To account for other potential time-varying factors that impacted labor market outcomes apart from the reform, we want to have a "control" group of individuals whose labor market outcomes provide a reasonable counterfactual for children who experience a parental stroke in absence of the reform. Our results in Section 4.2 suggest that a parental heart attack has a negligible effect on the need for care, especially relative to parents who experience a stroke. Therefore, the increased availability of formal care workers after the care reform should be irrelevant (or matter little) for the labor market responses of children whose parents experience a heart attack. At the same time, children with a parental heart attack should be subject to the same general changes in the labor market over time.

We exploit this idea to estimate a triple-difference (DDD) model that utilizes variation in parents' need for as well as availability of formal care along three dimensions: (a) between time periods before and after the health shock, (b) between families that experience a stroke compared to a heart attack, and (c) between families that experience the shock before or after the implementation of the reform:

$$\begin{aligned} O_{i,t} = \gamma_0 + \gamma_1 Post_{it} + \gamma_2 Stroke_i + \gamma_3 AfterReform_t \\ + \gamma_4 Post_{it} \cdot Stroke_i + \gamma_5 Post_{it} \cdot AfterReform_t + \gamma_6 Stroke_i \cdot AfterReform_t \\ + \eta Post_{it} \cdot Stroke_i \cdot AfterReform_t + \lambda_{c,t} + \epsilon_{i,t}, \quad (2) \end{aligned}$$

The binary variable $Stroke_i$ takes the value 1 if the parent of individual i has a stroke, and 0 if the parent has a heart attack. $Post_{it}$ takes the value 1 for quarters after the parental health shock of individual i, and 0 before. $AfterReform_t$ is an indicator equal to one if the year is after the reform and 0 if before. $\lambda_{c,t}$ are quarter-by-cohort fixed effects. Standard errors are robust and clustered at the individual level.

The main parameter of interest is η. If the increased availability of care workers muted the negative effects of a parental stroke on their children's labor market outcomes, then the coefficient of the triple interaction term $Post_{it} \cdot Stroke_i \cdot AfterReform_t$, η, would be positive. The identifying assumption is that, on average, the difference between the outcomes of families experiencing a stroke and families experiencing a heart attack would have evolved similarly before and after the implementation of the reform in the absence of the health shock.

[23] We also examine whether strokes before the reform happen to be more severe, but we do not find any meaningful differences in changes of parental health outcomes suffering from a stroke before versus after the reform.

7.4.1 Examining pre-trends

To provide supportive evidence for the parallel trend assumption, we examine pre-trends. We look at labor market outcomes prior the respective health shock as well as across all pre-reform years. First, we explore whether children experiencing a parental heart attack vs. a parental stroke followed similar employment and earnings trends prior the health shock. Panel (a) of Figure 9 plots quarterly employment rates of children with a parental stroke and parental heart attack for up to 12 quarters prior to the shock. The rates are conditional on sex and quarter-by-cohort fixed effects. Panel (b) provides an equivalent figure for earnings. For both groups employment and earnings evolve with parallel trends over time, with negligible differences in most quarters prior the shock. Second, we study whether the two groups of children follow similar labor market trends *before* the reform. Panel (c) and (d) of Figure 9 plot employment and earnings in pre-shock quarters for each of the five years before the reform. We see (conditional on sex and quarter-by-cohort fixed effects) strong evidence for parallel pre-trends in both outcomes. We conclude that children with parents suffering from a heart attack appear to be a suitable counterfactual for children whose parents had a stroke prior to the respective shock and reform.

7.4.2 Estimated effect of the 2007 reform

In our estimation of treatment effects, we include all children whose parents suffered a stroke or heart attack in the five years before or after the reform in 2007.[24] Our sample comprises all observations in the 12 quarters before and after the parental health shock occurs. We focus on children who we observe in all of these quarters. Clearly, children who experience a parental stroke shortly before the reform might still be affected by the increased availability of formal care after 2007. For example, a child whose parent had a health shock in 2006 might switch to 24h care work after the 2007 reform and, hence, within our 3-year post-shock period. To account for this fact, we exclude individuals which are partly affected by the reform in an alternative estimation set-up. We define a sample of "only early health shocks", who experienced a parental health shock before 2005. These units are unaffected by the reform throughout the 3-year post-shock period.

Table 3 displays the estimated coefficient on the triple interaction term in the DDD model described in equation (2). In Panel A we consider children who experience a parental health shock in the five years around the reform. Columns (1) and (2) show positive and economically significant coefficients for both labor market outcomes. The coefficient of interest is statistically significant at the ten percent level in the case of employment. Our result in column (1) implies that the increased availability and affordability of formal care due to the reform in 2007 reduced the negative effect of parents' care dependency on children's employment by 3.6 percentage points. In columns (3) and

[24] We provide additional results in which we narrow down this window to four years.

(4), we use "only early health shocks", who cannot benefit from the reform throughout the three years after their parents' health shock. We observe that both coefficients increase in size and statistical significance. We find that the reform significantly reduced the negative effect of a sudden increase in parental dependency on children's employment by 6.2 percentage points. Similarly, the negative effect on children's quarterly labor earnings decreases significantly in size by Euro 373. In absence of the reform, experiencing a parental stroke compared to a heart attack leads to a decrease in children's employment and quarterly earnings by 3.9 percentage points and Euro 247, respectively (see estimate for γ_4 in Table 3). Based on "only early health shocks", these reductions are 6.5 percentage points and Euro 464, respectively. Hence, the negative effect of a parental stroke on labor market outcomes are almost offset with the reform. In Panel B, we restrict the sample to children who experienced a parental health shock in the four years before and after the reform. The results are similar in size and significance.

To sum up, we find that the care reform with its large increase in the availability of formal care helped children to avoid the negative impact of a parental stroke on their labor market outcomes. This indicates that migrant care workers who provide 24-hour care at home act as substitutes for adult children's informal care, a factor that can be influenced by migration policies. Unfortunately, in our setting it is difficult to exactly quantify the impact of migrant supply on care prices, because this market was only legally introduced with the reform. Furthermore, the vast majority of migrant care workers are self-employed, which means we cannot observe their wages in our data (see also Section 7.3. The migrant care workers also often work on specific rotational shifts: They come as live-in carers and return to their home country on a biweekly rotation (Oesterle and Bauer, 2016). This makes it difficult to assess the exact price and quantity of their labor supply, which would be necessary to calculate elasticities.

Finally, the substitution from informal care to more professional care workers may also have an effect on the well-being and quality of treatment of the parent. Therefore we also look at parental mortality risk one to five years after the health shock before and after the reform. We re-estimate the DDD model described in equation (2) with the probability of dying within one year up to within five years after the health shock. Appendix Table F.1 summarizes the estimates of this model. For the five-year window-sample, we do not find any significant reform effects on parental mortality risk (Panel A). When restricting to "only early health shocks" (Panel B), however, we find that the reform reduces mortality risk between 3 and 5 years after a stroke by approximately 1.5 percentage points. Thus, we find some evidence that a substitution to more professional care workers does not only raise the labor supply of adult children but also slightly improves the survival of the parent in need of care.

7.4.3 Placebo checks

We run two placebo checks to test for the plausibility of our results. First, we limit our analysis to observations *before* the respective health shock, and examine whether pre-shock differences between children with a parental stroke and a parental heart attack changed with the 2007 reform. Specifically, we estimate the following equation:

$$O_{i,t} = \delta_0 + \delta_1 Stroke_i + \delta_2 AfterReform_t + \delta_3 Stroke_i \cdot AfterReform_t + \lambda_{c,t} + \epsilon_{i,t}, \quad (3)$$

where the binary variable $Stroke_i$ takes the value 1 if the parent of individual i has a stroke, and 0 if the parent has a heart attack. $AfterReform_t$ is an indicator equal to one if the year is after the reform and 0 if before. $\lambda_{c,t}$ are quarter-by-cohort fixed effects. Standard errors are robust and clustered at the individual level. Table 4 displays results from this placebo DD estimation. Column (1) and (2) restrict our sample to all pre-shock observations in the five years before ($AfterReform = 0$) and after 2007 ($AfterReform = 1$). Estimates listed in column (3) and (4) use a more narrow four year window. Across columns, we see that the 2007 reform had no significant effect on pre-shock differences between children with a parental stroke and those with a parental heart attack. In our second placebo test, we re-estimate the sample split of Panel B3 of Table 2, but this time examine whether the impact of a parental heart attack on children's labor market outcomes differs between years before and after the reform. In contrast to parental strokes, we do not find significant effects of a parental heart attack on children's labor market outcomes prior to the reform (see Appendix Table A.5). Furthermore, we continue to find similar effects sizes also in the after-reform period. Thus, while the reform with its large expansion in the availability of formal care had an effect on children experiencing a parental stroke, it has been mostly irrelevant for those after a parental heart attack.

8 Conclusions

Many societies around the world are aging rapidly, facing the prospects of a population pyramid turning upside down. One key challenge of an aging population is the increasing need for care. LTC provisions in many OECD countries are characterized by a mix of informal and formal care arrangements. Adult children are often important providers of informal care to their parents, which in turn may affect their labor market outcomes and well-being. Understanding the impact of providing care to a sick relative is crucial when designing and evaluating LTC policies.

Against this background, we examine how parental health shocks affect the labor market and health outcomes of their adult children. Our study exploits sudden parental health shocks (strokes and heart attacks) to evaluate their effect on the employment and earnings of adult children. Estimating event study models, we find significant negative

effects of strokes on the labor market outcomes of adult children, but not of heart attacks. The negative labor market effects after a stroke carry a gender gradient. For sons, we find a 2 percent reduction in employment, and a 2.7 percent reduction in earnings over the three years following a parental stroke. For daughters, the effect is considerably larger, with a drop in employment and earnings of 3.0% and 6.3%, respectively.

All the evidence points to informal caregiving as the most likely causal channel. First, the negative labor market effect is only observed for strokes but not for heart attacks. The main difference between these two conditions is that the observed impact on care dependency is a magnitude larger after a stroke than after a heart attack. Second, we find stronger effects for children who live closer to their parents. Third, we find estimates for children of retired parents (no income shock) to be very comparable to the ones of our overall sample.

Finally, we examine whether public policies can mitigate the negative labor market effects stemming from a parental health shock. We exploit a reform that loosened restrictions on privately hired (foreign) care workers, which led to a sharp increase of live-in migrant care workers in Austria. Using a triple-difference strategy, we find that children who are exposed to the increased availability of formal care are able to avoid the negative labor market effects of a parental stroke. This suggests that the availability and affordability of formal care matters for the decision of which care mode families choose, something policymakers can manipulate. It seems plausible that this home-based version of formal care we analyze provides a care option which aligns well with the prevailing family-based social norms towards LTC. Overall, we show that migrant care workers can substitute informal care, which is mostly performed by female family members.

Our study has important implications for public policy. First, it shows that the well-known gender inequality in informal care work spills over to gender inequality in the paid labor market. Second, we document how a liberalization of the formal care market can reduce the costs of a parental health shock in terms of adult children labor market activities. We recommend considering both aspects when designing LTC policies. If policy makers aim to reduce negative labor market effects of affected (female) relatives, we suggest an expansion and/or subsidy of formal LTC.

We see two important limitations of our study. First, our research design is not suited to study long-run effects of parental health shocks and subsequent need for care. As a consequence, we cannot quantify the life-cycle impact of this shock. Second, due to data availability we focus on "younger" parents and children. Our population of children is about 30 years old at the time of the parental shock. We consider this an important life period to study since these children are at the beginning of their working careers. However, in future research it would be interesting to also study the impact of health shocks of older parents, which would allow to identify effects along the entire children's life cycle.

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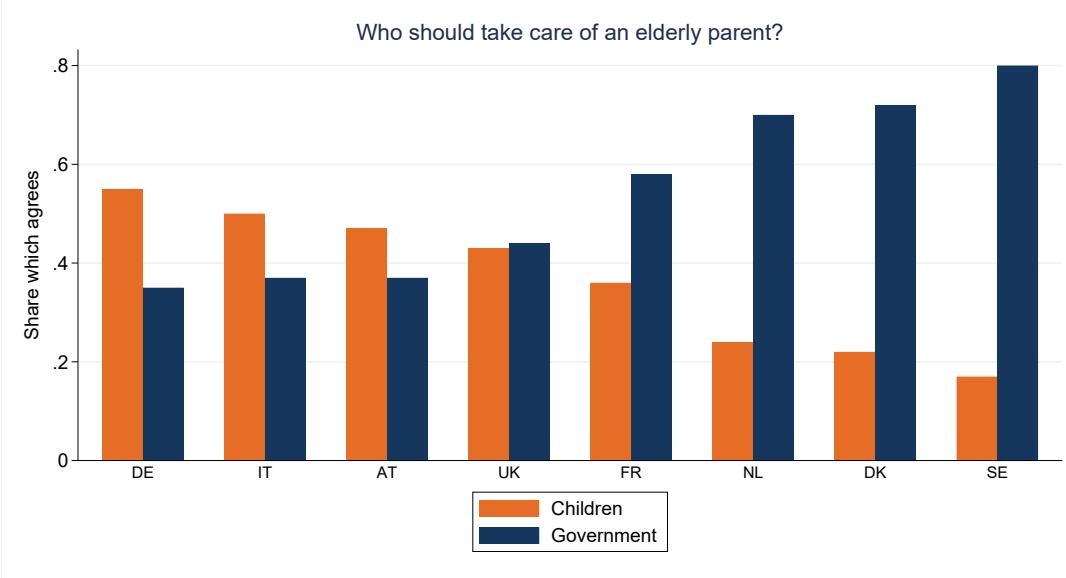
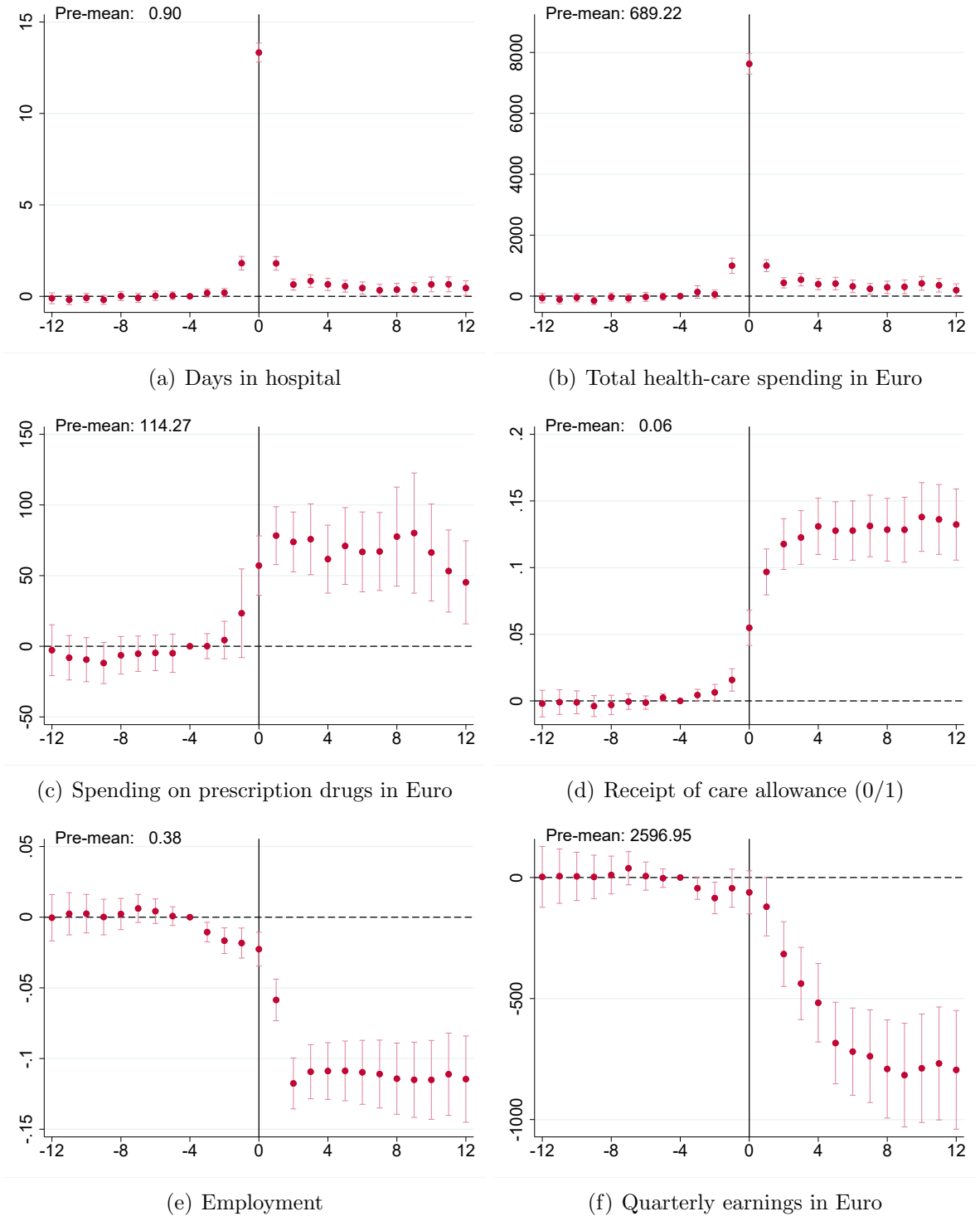
9 Figures (to be placed in the article)

Figure 1: Social norm regarding the care provision to an elderly parent in selected countries



Notes: This figure is based on data from the Eurobarometer 67.3 (2007), which interviewed 28,660 citizens of the 25 countries in the European Union after the 2004 enlargement. All respondents were residents in the respective country, and aged 15 and over. The original survey questions is as follows 'Imagine an elderly father or mother who lives alone and can no longer manage to live without regular help because of her or his physical or mental health condition. In your opinion, what would be the best option for people in this situation?'. Respondents are given six options to choose from: (1) 'They should live with one of their children', (2) 'One of their children should regularly visit their home, in order to provide them with the necessary care', (3) 'Public or private service providers should visit their home and provide them with appropriate help and care', (4) 'They should move to a nursing home',(5) 'It depends', and (6) 'None of these'. The graph shows the share of respondents (by country) who gave answer (1) or (2) (in orange), or answer (3) or (4) (in blue), respectively.

Figure 2: Impact of parental stroke on parental health and employment



(a) Days in hospital

(b) Total health-care spending in Euro

(c) Spending on prescription drugs in Euro

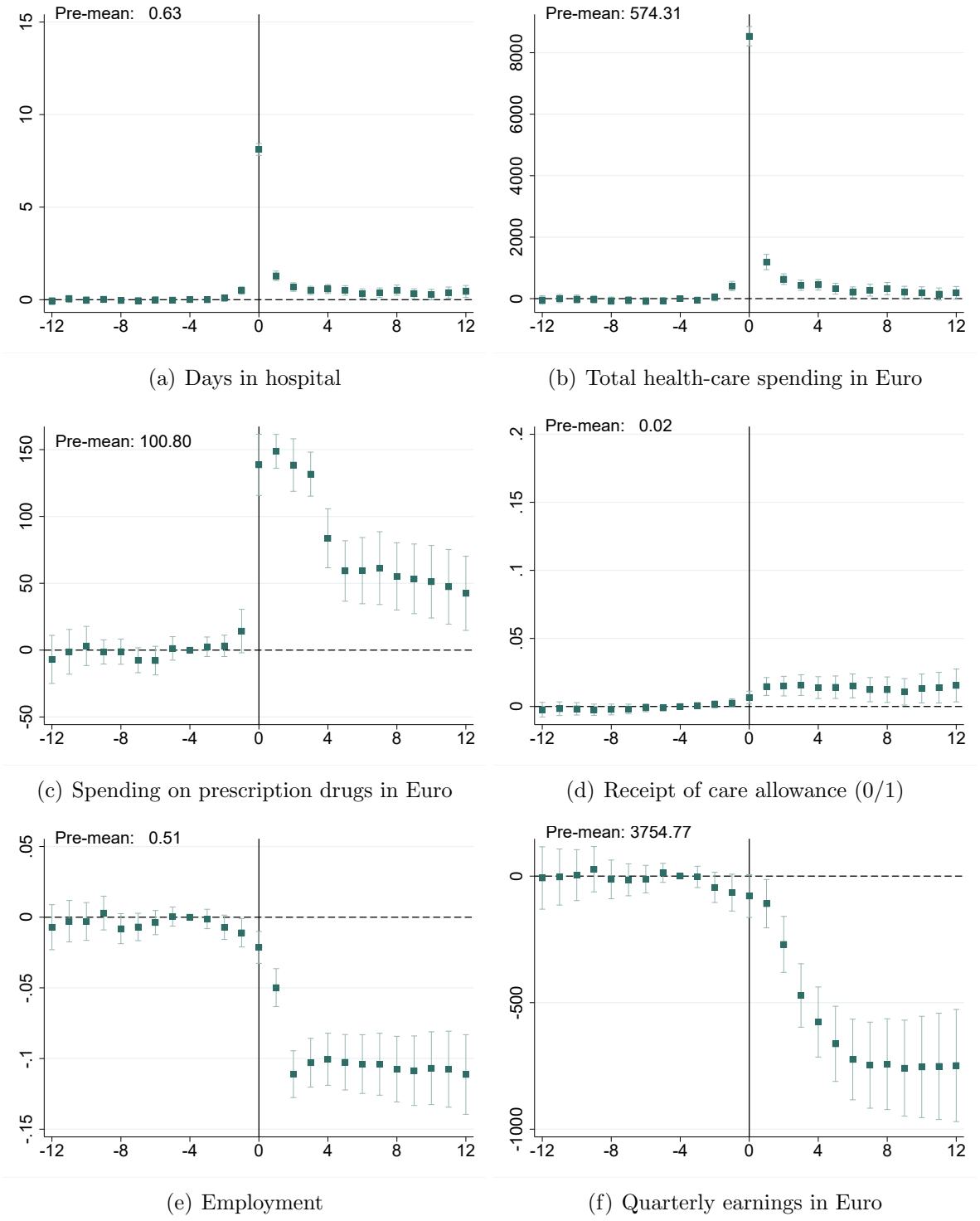
(d) Receipt of care allowance (0/1)

(e) Employment

(f) Quarterly earnings in Euro

Notes: Each panel of the figure shows the event-study coefficients estimated from equation (1) (including 95 percent confidence intervals) of the parental stroke on the respective parental health or labor market outcome. Standard errors are clustered at the individual level. *Pre-mean* refers to the mean of the respective outcome variable four quarters prior to the parental stroke.

Figure 3: Impact of parental heart attack on parental health and employment



(a) Days in hospital

(b) Total health-care spending in Euro

(c) Spending on prescription drugs in Euro

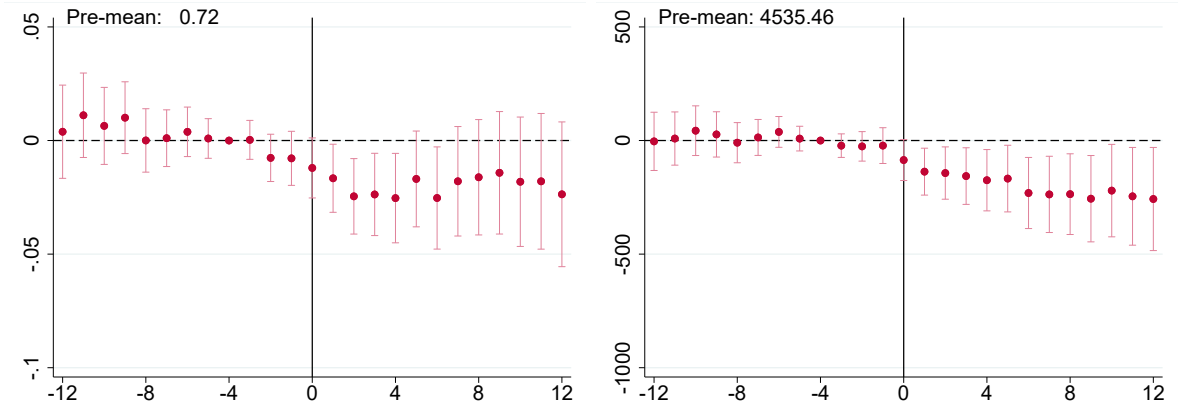
(d) Receipt of care allowance (0/1)

(e) Employment

(f) Quarterly earnings in Euro

Notes: Each panel of the figure shows the event-study coefficients estimated from equation (1) (including 95 percent confidence intervals) of the parental heart attack on the respective parental health or labor market outcome. Standard errors are clustered at the individual level. *Pre-mean* refers to the mean of the respective outcome variable four quarters prior to the parental heart attack.

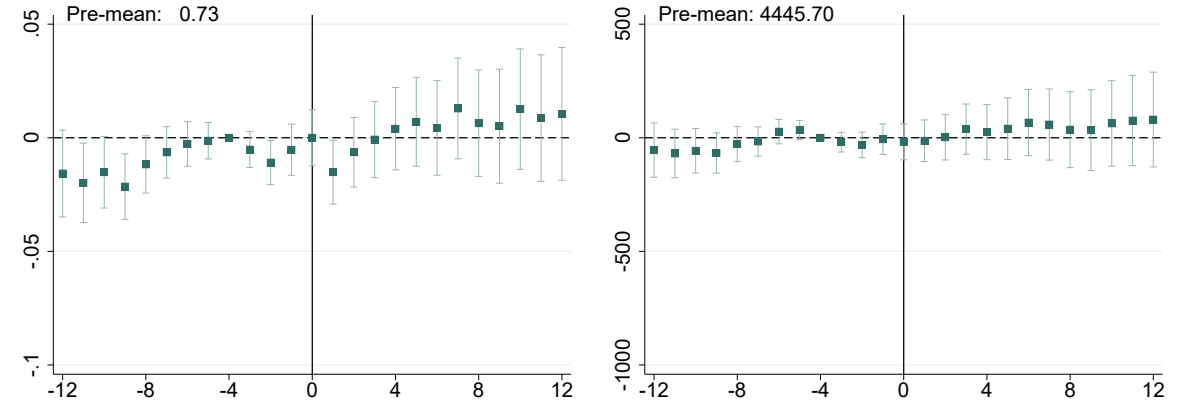
Figure 4: Impact of parental stroke on children's labor market outcomes



(a) Employment (b) Quarterly earnings in Euro

Notes: Each panel of the figure shows the event-study coefficients estimated from equation (1) (including 95 percent confidence intervals) of the parental stroke on the respective labor market outcome of the child. Standard errors are clustered at the individual level. *Pre-mean* refers to the mean of the respective outcome variable four quarters prior to the parental stroke.

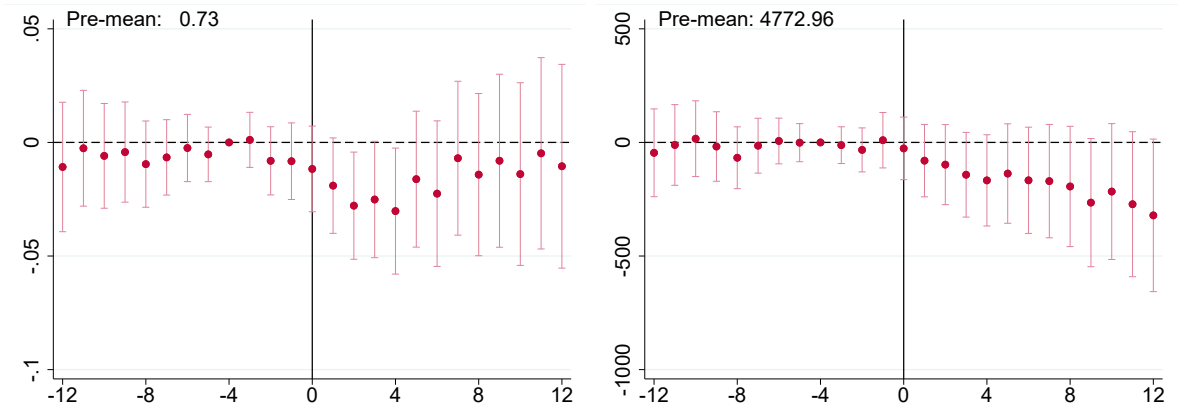
Figure 5: Impact of parental heart attack on children's labor market outcomes



(a) Employment (b) Quarterly earnings in Euro

Notes: Each panel of the figure shows the event-study coefficients estimated from equation (1), including 95 percent confidence intervals of the parental heart attack on the respective labor market outcome of the child. Standard errors are clustered at the individual level. *Pre-mean* refers to the mean of the respective outcome variable four quarters prior to the parental heart attack.

Figure 6: Children with retired parent: Impact of parental stroke on children's labor market outcomes

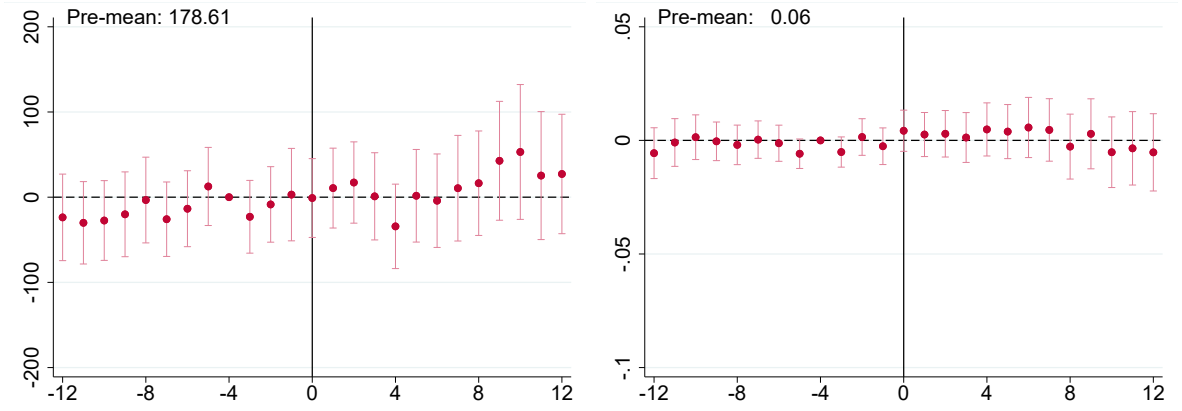


(a) Employment

(b) Quarterly earnings of daughters in Euro

Notes: Each panel of the figure shows the event-study coefficients estimated from equation (1) (including 95 percent confidence intervals) of the parental stroke on the respective labor market outcome of the child. Standard errors are clustered at the individual level. *Pre-mean* refers to the mean of the respective outcome variable four quarters prior to the parental stroke.

Figure 7: Impact of parental stroke on children's health outcomes

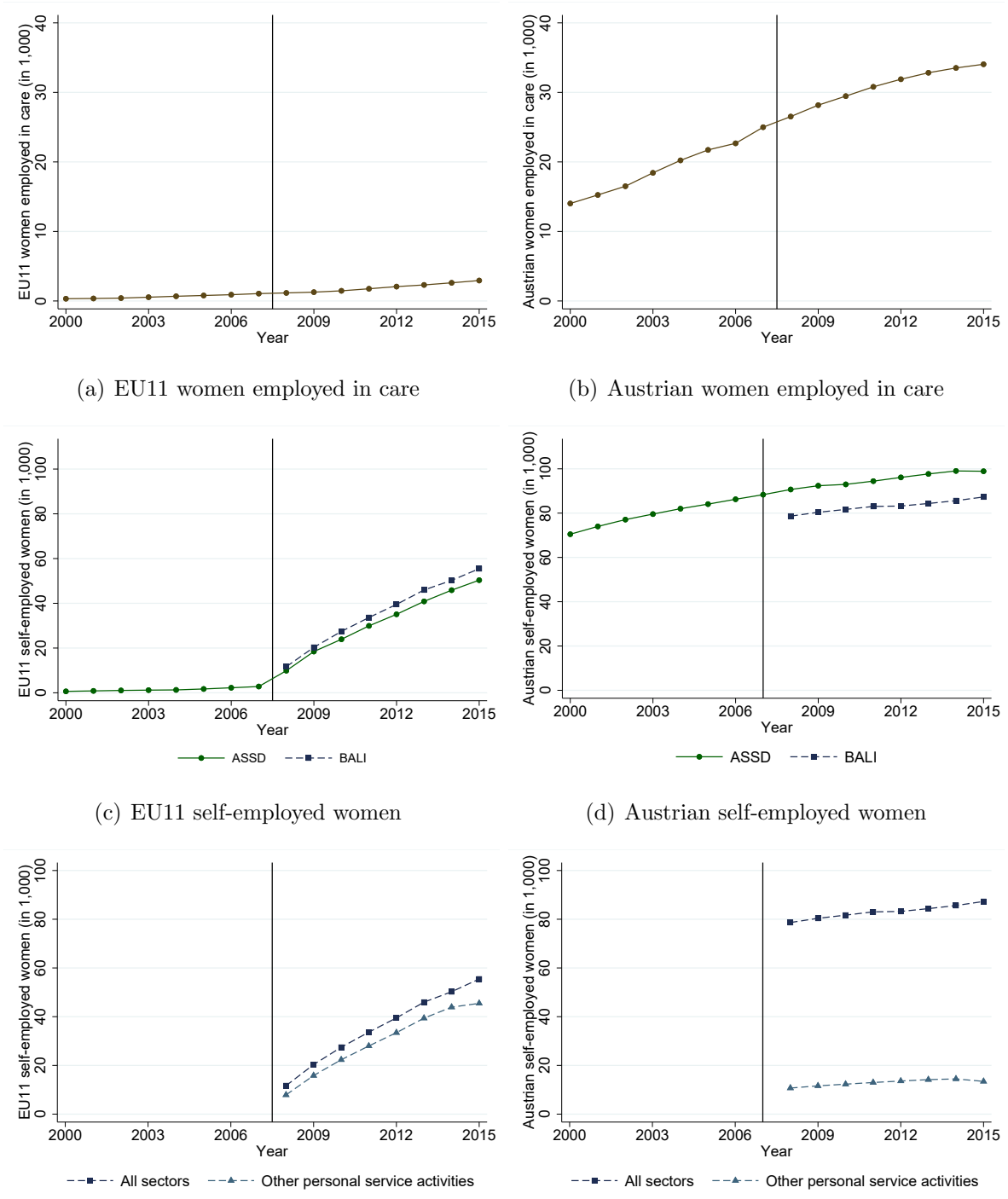


(a) Total health spending in Euro

(b) Probability of antidepressant use

Notes: Each panel of the figure shows the event-study coefficients estimated from equation (1), including 95 percent confidence intervals of the parental stroke on the respective health outcome of the child. Standard errors are clustered at the individual level. *Pre-mean* refers to the mean of the respective outcome variable four quarters prior to the parental stroke.

Figure 8: Number of female care workers in Austria



(a) EU11 women employed in care

(b) Austrian women employed in care

(c) EU11 self-employed women

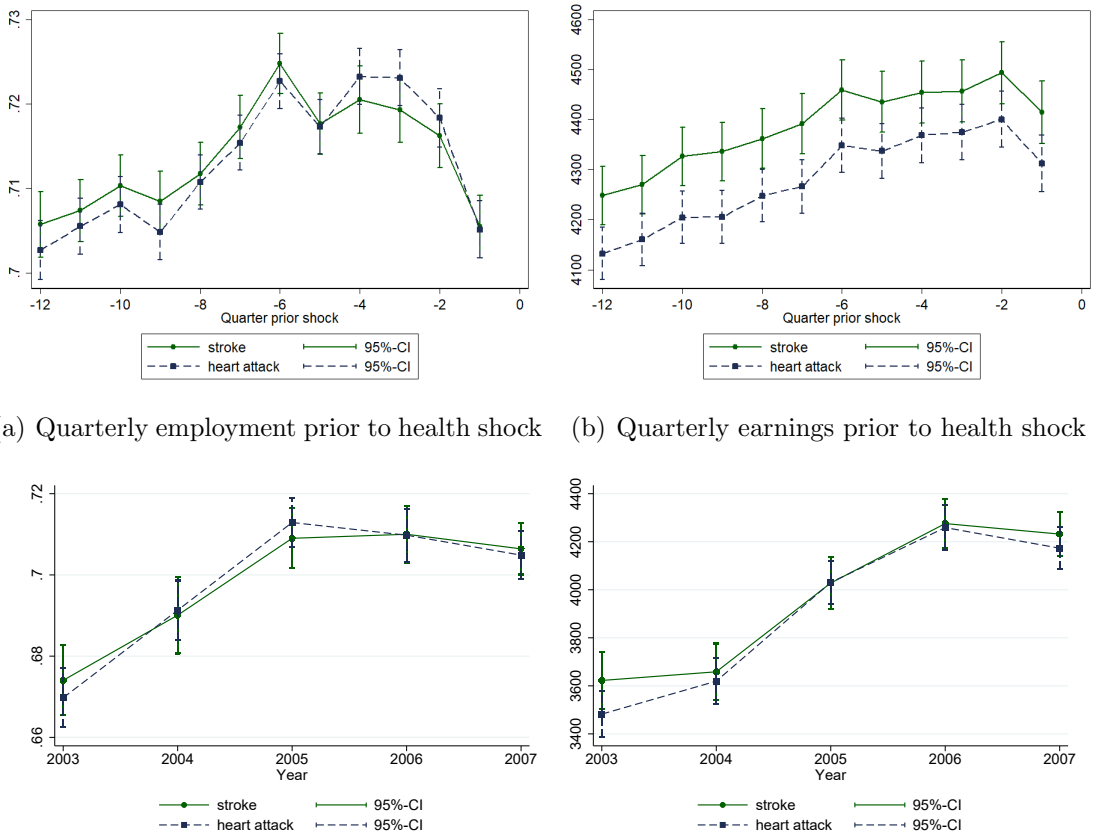
(d) Austrian self-employed women

(e) EU11 self-employed women by sector

(f) Austrian self-employed women by sector

Notes: The panels of this figure show the average number of female care workers in Austria from 2000 to 2015. For each year we take the average over the total number employed at four reference dates. The panels to the left (to the right) refer to EU11 nationals (Austrians). Panels (a) and (b) include all employees in the NACE categories 8710 (residential nursing care activities), 8730 (residential care activities for the elderly and disabled), 8790 (other residential care activities), and 8810 (social work activities without accommodation for the elderly and disabled). Panels (c) and (d) include all self-employed women from two different data sources. Panels (e) and (f) distinguish between all self-employed women and those in "Other personal service activities" based on the data. *Source*: Austrian Social Security Database, BALI (budget, labour market and benefit receipt information) online database.

Figure 9: Labor market trends of children with a parental heart attack vs. a parental stroke



(a) Quarterly employment prior to health shock

(b) Quarterly earnings prior to health shock

(c) Annual employment before the reform

(d) Annual earnings before the reform

Notes: These figures inspect the common trend in labor market outcomes of children with a parental heart attack vs. a parental stroke prior the health shock (see Panels a and b) as well as before the reform (Panels c and d). All estimates control for sex and quarter-by-cohort fixed effects, with clustered standard errors at the individual level. Panel (a) shows quarterly employment of children with a parental stroke and heart attack for each of the 12 quarters prior to the shock. Panel (b) provides an equivalent graph for earnings. Panel (c) plots annual employment in pre-shock quarters for each of the five years before the reform. Panel (d) provides an equivalent graph for annual earnings.

Table 1: Descriptive statistics of analysis sample prior to the shock

	Parent has stroke			Parent has heart attack		
Variable	Treated	Spouse	Child	Treated	Spouse	Child
Age	61.10	58.82	30.12	58.65	56.52	29.10
Female	0.32	0.70	0.48	0.24	0.77	0.48
Labor market outcomes:						
Employed	0.38	0.38	0.72	0.51	0.46	0.73
Earnings	2,597	2,011	4,535	3,755	2,334	4,446
Health outcomes:						
Days in hospital	0.90	0.48	0.21	0.63	0.48	0.24
Total health care spending	689	412	179	574	403	188
Spending on prescription drugs	114	73	21	101	75	24
Care allowance	0.06	0.02	-	0.02	0.02	-
Observations	2,763	2,603	4,206	3,437	3,295	5,201

Notes: This table presents the means of key variables in our analysis sample four quarters before the parental health shock. All variables are measured per quarter. All monetary values in the table are expressed in real 2010 Euros. Please note that data on care allowance receipt is only available until 2012, and some health care records only start in 2005.

Table 2: Impact of parental strokes on children's labor market outcomes

	(1)	(2)	(3)	(4)
	Employed	Earnings	Employed	Earnings
Panel A. Pooled				
Post	−0.018***	−176.189***		
	(0.006)	(45.505)		
Effect rel. to pre-mean (%)	−0.025***	−0.039***		
	(0.008)	(0.010)		
Pre-mean	0.72	4,535		
Observations	143,156	136,118		
Panel B. By subgroups				
B1. By sex of the child				
	Female		Male	
Post	−0.020*	−205.325***	−0.015**	−151.979**
	(0.010)	(68.086)	(0.006)	(61.640)
Effect rel. to pre-mean (%)	−0.030**	−0.063***	−0.020**	−0.027**
	(0.016)	(0.020)	(0.008)	(0.011)
Pre-mean	0.66	3,283	0.77	5,705
Observations	69,298	65,843	73,858	70, 256
B2. By geographic distance between child and parent				
	Local		Distant	
Post	−0.021**	−165.829**	−0.006	−133.062
	(0.008)	(64.089)	(0.012)	(83.734)
Effect rel. to pre-mean (%)	−0.029**	−0.036***	−0.009	−0.031
	(0.011)	(0.013)	(0.018)	(0.020)
Pre-mean	0.73	4,631	0.69	4,326
Observations	64,801	61,898	48,205	45,630
B3. By year of parental stroke				
	2007 and before		After 2007	
Post	−0.046**	−334.470**	0.011	−92.050
	(0.020)	(137.629)	(0.012)	(85.956)
Effect rel. to pre-mean (%)	−0.064**	−0.082***	0.016	−0.020
	(0.027)	(0.032)	(0.017)	(0.019)
Pre-mean	0.72	4,086	0.72	4,575
Observations	19,727	19,727	46,255	46,255

Notes: This table shows the impact of parental strokes on children's labor market outcomes based on equation $O_{i,t} = \beta Post_{it} + \lambda_{c,t} + \psi_i + \upsilon_{i,t}$, where $Post_{it}$ is an indicator equal to 1 in the quarters after the shock ($k \geq 0$) and 0 otherwise. The dependent variable in columns (1) and (3) is equal to one if the child is employed in a given quarter. In columns (2) and (4), the outcome is the quarterly earnings of the child in Euro. *Pre-mean* refers to the mean of the respective outcome variable four quarters prior to the parental health shock. Standard errors are clustered at the individual level and reported in parentheses; those for the relative effects are bootstrapped (500 replications). Geographic distance between parent and child in Panel B2 is defined as local if they live in the same municipality one year prior to the stroke and as distant otherwise. Geographic distance is missing for some children for whom we do not observe either their own or their parents' location. 2007 and before in Panel B3 refers to the years from 2003 to 2007, after 2007 to the years from 2008 to 2012.

Table 3: A triple-difference estimation of children's labor market outcomes

	(1)	(2)	(3)	(4)
	All		Only "early" health shocks	
	Employed	Earnings	Employed	Earnings
Panel A: Five-year window around the reform				
$\gamma_4 : Post \cdot Stroke$	-0.039^{**}	-247.217^{*}	-0.065^{**}	-463.537^{**}
	(0.018)	(140.204)	(0.028)	(196.702)
$\eta : Post \cdot Stroke \cdot AfterReform$	0.036^{*}	157.823	0.062^{**}	373.221^{*}
	(0.021)	(164.952)	(0.030)	(215.188)
Observations	100,725	100,725	81,200	81,200
Panel B: Four-year window around the reform				
$\gamma_4 : Post \cdot Stroke$	-0.039^{**}	-219.520	-0.074^{**}	-465.982^{*}
	(0.019)	(155.409)	(0.036)	(250.524)
$\eta : Post \cdot Stroke \cdot AfterReform$	0.042^{*}	212.236	0.077^{**}	458.129^{*}
	(0.024)	(184.483)	(0.039)	(269.607)
Observations	76,900	76,900	57,375	57,375

Notes: The dependent variable in columns (1) and (3) is equal to one if the child is employed in a given quarter. In columns (2) and (4), the outcome is the quarterly earnings of the child in Euro. This table displays the estimated coefficients γ_4 and η in the DDD model presented in equation (2). Appendix Table A.4 provides the estimated coefficients for all variables in the model. The dependent variable in columns (1) and (3) is equal to one if the child is employed in a given quarter. In columns (2) and (4), the outcome is the quarterly earnings of the child in Euro. Standard errors are clustered at the individual level and reported in parentheses. *, ** and *** indicate significance at 10-, 5- and 1-percent levels, respectively.

Table 4: Placebo check of triple-difference estimation: Only pre-shock periods

	(1)	(2)	(3)	(4)
	5y-window		4y-window	
	Employed	Earnings	Employed	Earnings
$\delta_1 : Stroke$	0.011	-30.445	0.010	-68.743
	(0.016)	(141.695)	(0.017)	(153.627)
$\delta_3 : AfterReform \cdot Stroke$	-0.014	92.120	-0.011	166.219
	(0.019)	(173.611)	(0.020)	(186.752)
Observations	61,015	61,015	47,780	47,780

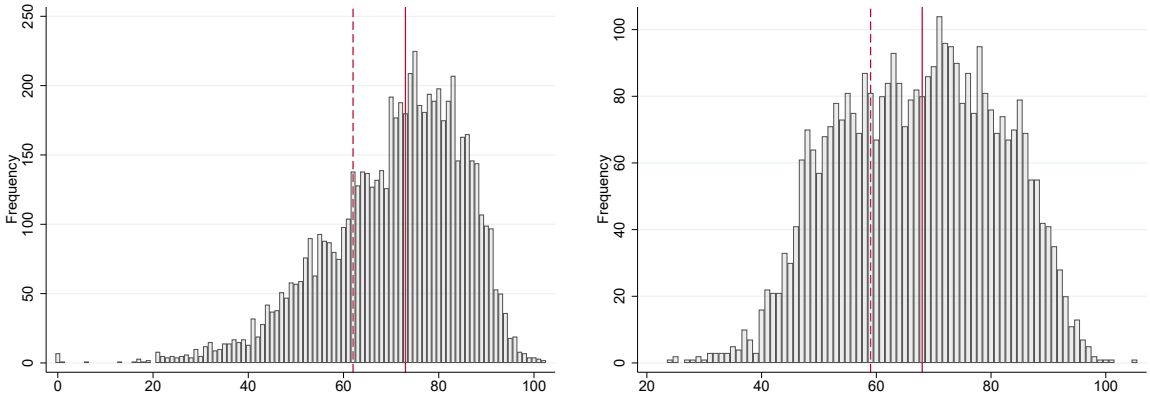
Notes: The dependent variable in columns (1) and (3) is equal to one if the child is employed in a given quarter. In columns (2) and (4), the outcome is the quarterly earnings of the child in Euro. In columns (1) to (2), the analysis period is 2003 to 2012, for columns (3) to (4) it is 2004 to 2011. Standard errors are clustered at the individual level and reported in parentheses. *, ** and *** indicate significance at 10-, 5- and 1-percent levels, respectively.

Web appendix

This Web Appendix provides additional material discussed in the unpublished manuscript "Health of Parents, their Children's Labor Supply, and the Role of Migrant Care Workers" by Wolfgang Frimmel, Martin Halla, Jörg Paetzold, and Julia Schmieder.

A Additional figures and tables

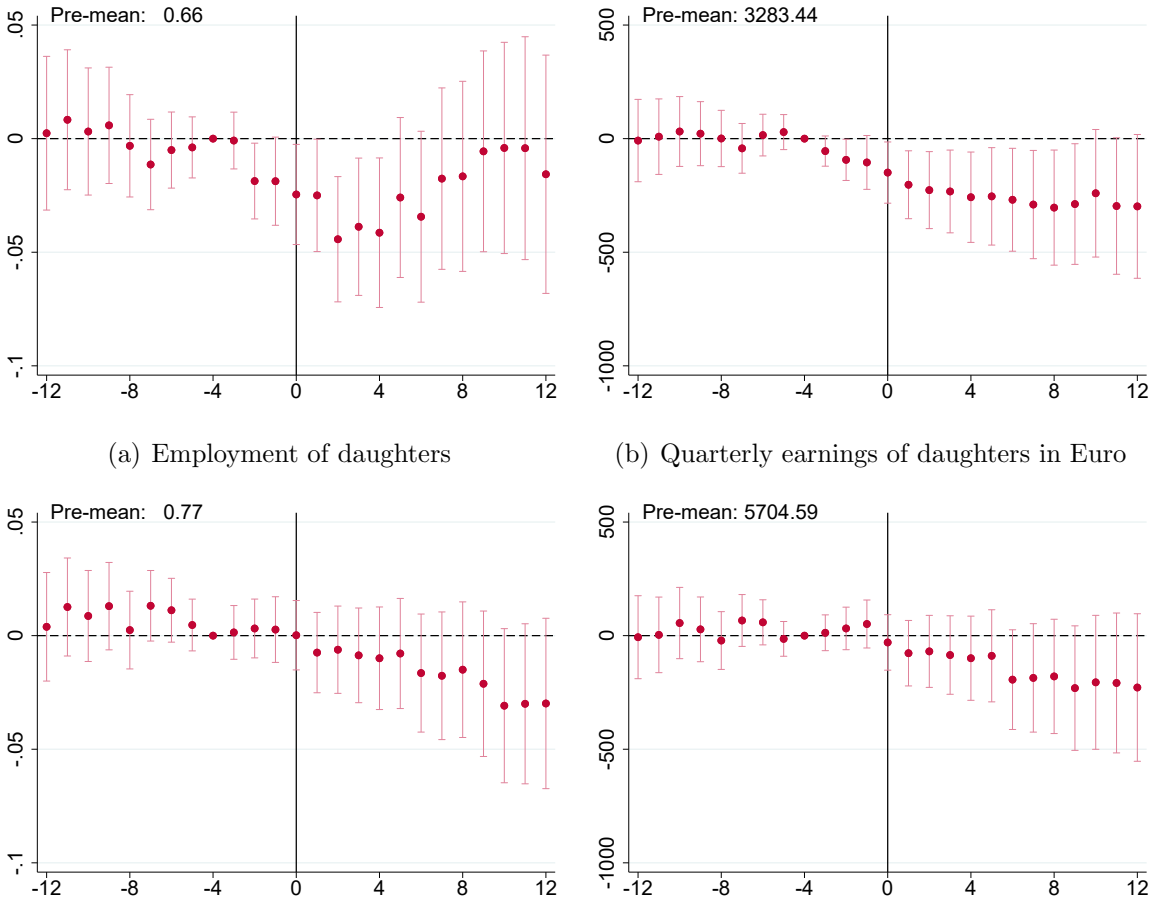
Figure A.1: Age distribution at parental health shock



(a) Strokes (b) Heart attack

Notes: These figure shows the age distribution of the first parental health shock in the total population. The left panel shows the age at first stroke, and the right panel the age at first heart attack. In each case, the solid vertical line indicates the median age in the total population, while the dashed line indicates the median age in our estimation sample. The median age at first stroke in the total population is 73 years relative to 62 years in our sample. The median age at first heart attack in the total population is 68 years relative to 59 years in our estimation sample.

Figure A.2: Impact of parental stroke on children's labor market outcomes by sex



(a) Employment of daughters

(b) Quarterly earnings of daughters in Euro

(c) Employment of sons

(d) Quarterly earnings of sons in Euro

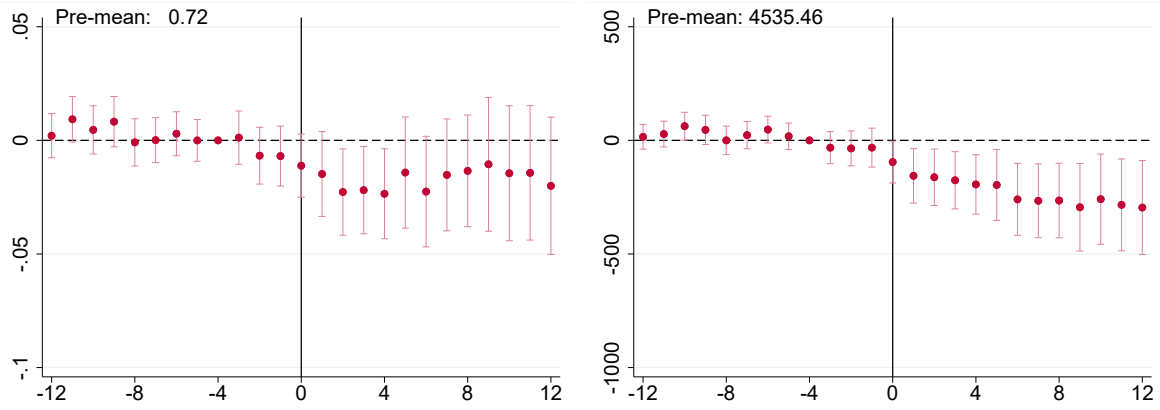
Notes: Each panel of the figure shows the event-study coefficients estimated from equation (1) (including 95 percent confidence intervals) of the parental stroke on the respective labor market outcome of the child. Standard errors are clustered at the individual level. *Pre-mean* refers to the mean of the respective outcome variable four quarters prior to the parental stroke.

Table A.1: Cash benefits of care allowance scheme in 2016

(1) Level	(2) Care requirements in hours per month	(3) Benefit in Euro*	(4) Share of total recipients**
1	$65 < x \leq 95$ hours	150.30	0.26
2	$95 < x \leq 120$ hours	290.00	0.24
3	$120 < x \leq 160$ hours	451.80	0.18
4	$160 < x \leq 180$ hours	677.60	0.14
5	>180 hours and †	920.30	0.11
6	>180 hours and ‡	1,285.20	0.04
7	>180 hours and #	1,688.90	0.02

Notes: * Per month for year 2016. † Extraordinary high level of care required.‡ Timely unpredictable care measures during day and night, or constant presence of carer required because person would be at risk. # No goal-directed movements of the person's four extremities or equivalent condition. ** The total number of recipients was 455, 354, Source: *Statistik Austria*.

Figure A.3: Impact of parental stroke on children's labor market outcomes - family cluster



(a) Employment

(b) Quarterly earnings in Euro

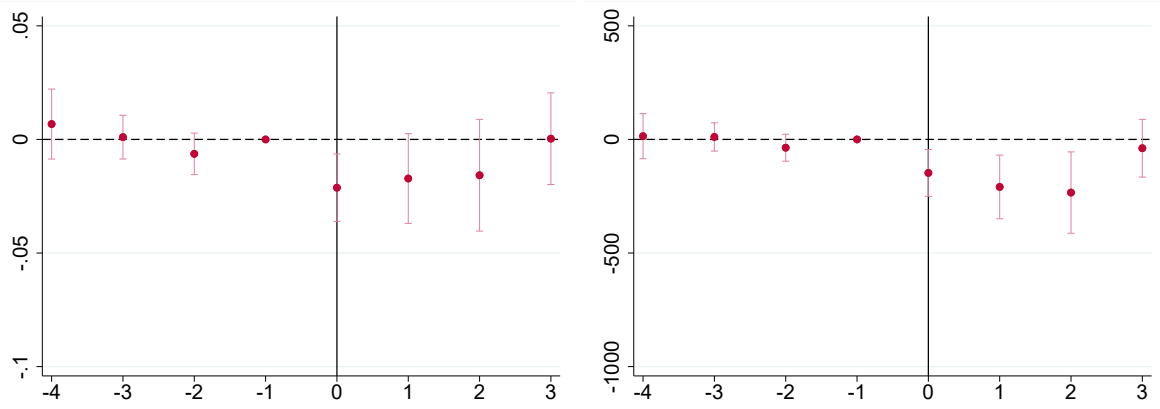
Notes: This Figure shows the main labor market effects of parental strokes with clustering on the household level rather than individual level to account for the fact that many children are part of the same family. Each panel shows the event-study coefficients estimated from equation (1) and the corresponding 95 percent confidence intervals of the parental stroke on the respective labor market outcome of the child. Standard errors are clustered at the family level. *Pre-mean* refers to the mean of the respective outcome variable four quarters prior to the parental stroke.

Table A.2: Socio-economic determinants of informal care provision

	(1) Hours> 0 OLS	(2) Hours/week Tobit	(3) Hours/week Truncated reg.
Sex (Base group: male)			
Female	0.018***	9.718***	7.606
	(0.003)	(1.859)	(13.408)
Age (Base group: below 25 years of age)			
25 - 39	0.009**	7.755*	35.512
	(0.004)	(4.429)	(52.925)
40 - 54	0.023***	15.319***	39.449
	(0.005)	(4.262)	(52.322)
$\geq$ 55	0.028***	17.381***	40.636
	(0.004)	(4.182)	(52.276)
Education (Base group: compulsory education)			
Apprenticeship	−0.005	−0.832	−5.618
	(0.005)	(2.039)	(12.461)
Upper secondary	−0.005	−1.946	−24.195
	(0.005)	(2.120)	(19.743)
Tertiary	−0.012*	−6.467*	−124.521
	(0.007)	(3.575)	(95.547)
Missing information	−0.008**	−93.025	
	(0.004)	(.)	
Urbanization (Base group: low population density)			
Medium	−0.002	−0.551	−3.941
	(0.004)	(1.786)	(12.200)
High	−0.009**	−4.639**	6.326
	(0.004)	(2.079)	(13.681)
Constant	0.002	−65.696***	−78.081
	(0.004)	(6.124)	(78.654)
Number of observations	8,234	8,234	184
Mean of dependent variable	0.022	0.211	9.435
(Pseudo) R-squared	0.011	0.037	

Notes: This table summarizes estimations of the socio-economic determinants of informal care provision in Austria based on data from the *Austrian Time Use Survey* (*Zeitverwendungserhebung*) 2008/2009. Further details on the data source are provided in (Statistik Austria, 2009). In column (1), the sample comprises all individuals. The dependent variable is a binary indicator equal to one if the individual provides any informal care (hours> 0) and zero otherwise. The method of estimation is OLS, and reported standard errors are robust to heteroskedasticity of unknown form. In column (2), the sample comprises all individuals. The dependent variable is equal to the number of hours of informal care individuals provide per week. To account for censoring of the dependent variable, a Tobit model is used. In column (3), the sample comprises only the 184 individuals who provide any informal care (hours> 0). The dependent variable is equal to the number of hours of informal care individuals provide per week. To account for the truncated sample a truncated regression model is used. (Robust) standard errors are reported in parentheses. *, ** and *** indicate significance at 10-, 5- and 1-percent levels, respectively.

Figure A.4: Impact of parental stroke on children's labor market outcomes: Year-panel



(a) Employment

(b) Quarterly earnings in Euro

Notes: This Figure shows the main labor market effects of parental strokes with relative year indicators instead of quarterly indicators. Each panel shows the event-study coefficients estimated from equation (1) adapted for relative yearly indicators (instead of quarters) and the corresponding 95 percent confidence intervals. Standard errors are clustered at the individual level.

Table A.3: Impact of parental strokes on children's labor market outcomes, further outcomes

	(1) Days employed	(2) Daily wage	(3) Full-time employed
Post	-1.625^{***}	-1.846^{***}	-0.014^{*}
	(0.567)	(0.491)	(0.008)
Effect rel. to pre-mean (%)	-0.024^{***}	-0.035^{***}	-0.017^{*}
	(0.009)	(0.009)	(0.010)
Pre-mean	66.4	52.9	0.80
Observations	136,118		61,256

Notes: This table shows the impact of parental strokes on children's labor market outcomes by subgroups based on equation $O_{i,t} = \beta Post_{it} + \lambda_{c,t} + \psi_i + \upsilon_{i,t}$, where $Post_{it}$ is an indicator equal to 1 in the quarters after the shock ($k \geq 0$) and 0 otherwise. The dependent variable in column (1) is the number of days employed per quarter, in (2) the daily wage in Euro, and in (3) an indicator for full-time employment. *Pre-mean* refers to the mean of the respective outcome variable four quarters prior to the parental stroke. Standard errors are clustered at the individual level and reported in parentheses; those for the relative effects are bootstrapped (500 replications). *, ** and *** indicate significance at 10-, 5- and 1-percent levels, respectively.

Table A.4: A triple-difference estimation of children's labor market outcomes, full estimation output

	(1)	(2)	(3)	(4)
	All		Only "early" health shocks	
	Employed	Earnings	Employed	Earnings
A: Five-year window around the reform				
$\gamma_1 : Post$	−0.002	−72.999	0.058*	446.594*
	(0.016)	(132.693)	(0.034)	(253.144)
$\gamma_2 : Stroke$	0.007	112.272	−0.009	124.254
	(0.020)	(163.439)	(0.032)	(237.873)
$\gamma_3 : AfterReform$	−0.023	−46.923	0.038	664.465*
	(0.023)	(236.629)	(0.046)	(376.678)
$\gamma_4 : Post \cdot Stroke$	−0.039**	−247.217*	−0.065**	−463.537**
	(0.018)	(140.204)	(0.028)	(196.702)
$\gamma_5 : Post \cdot AfterReform$	0.004	52.984	−0.056	−457.262
	(0.020)	(170.114)	(0.037)	(285.250)
$\gamma_6 : Stroke \cdot AfterReform$	−0.004	−118.561	0.012	−129.959
	(0.024)	(216.788)	(0.035)	(277.036)
$\eta : Post \cdot Stroke \cdot AfterReform$	0.036*	157.823	0.062**	373.221*
	(0.021)	(164.952)	(0.030)	(215.188)
Observations	100,725	100,725	81,200	81,200
B: Four-year window around the reform				
$\gamma_1 : Post$	−0.012	−127.267	0.070	768.479**
	(0.018)	(148.596)	(0.045)	(355.497)
$\gamma_2 : Stroke$	0.010	142.753	−0.010	205.910
	(0.022)	(181.790)	(0.042)	(317.586)
$\gamma_3 : AfterReform$	−0.033	20.700	0.047	1105.460**
	(0.024)	(247.190)	(0.057)	(455.391)
$\gamma_4 : Post \cdot Stroke$	−0.039**	−219.520	−0.074**	−465.982*
	(0.019)	(155.409)	(0.036)	(250.524)
$\gamma_5 : Post \cdot AfterReform$	0.017	46.650	−0.065	−834.137**
	(0.021)	(185.129)	(0.048)	(391.781)
$\gamma_6 : Stroke \cdot AfterReform$	−0.000	−314.672	0.020	−378.149
	(0.027)	(244.143)	(0.045)	(356.574)
$\eta : Post \cdot Stroke \cdot AfterReform$	0.042*	212.236	0.077**	458.129*
	(0.024)	(184.483)	(0.039)	(269.607)
Observations	76,900	76,900	57,375	57,375

Notes: This table displays the estimated coefficients for the DDD model presented in equation (2). The dependent variable in columns (1) and (3) is equal to one if the child is employed in a given quarter. In columns (2) and (4), the outcome is the quarterly earnings of the child in Euro. Standard errors are clustered at the individual level and reported in parentheses. *, ** and *** indicate significance at 10-, 5- and 1-percent levels, respectively.

Table A.5: Placebo check of triple-difference estimation: Only heart attacks (before and after reform)

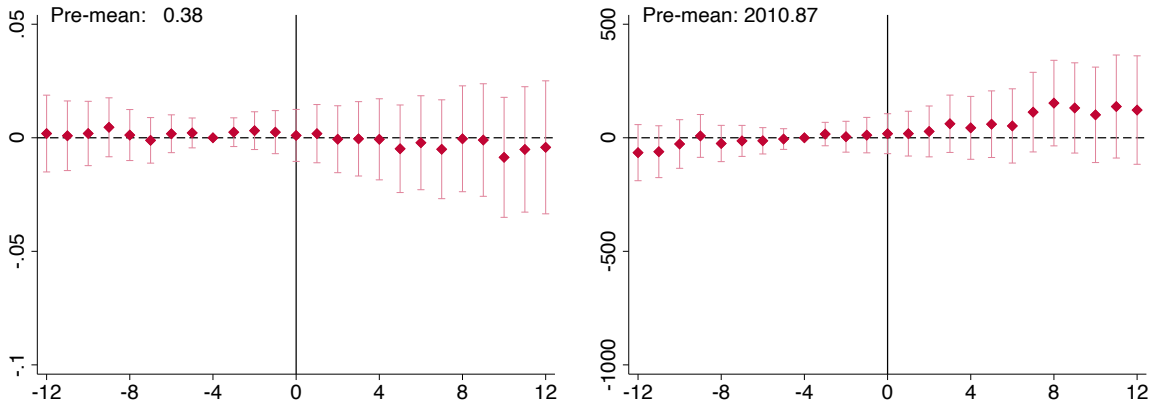
	(1) Employed	(2) Earnings	(3) Employed	(4) Earnings
By year of parental heart attack				
	Before reform		After reform	
Post	−0.015	−2.554	0.003	−92.887
	(0.015)	(106.334)	(0.012)	(83.614)
Effect rel. to pre-mean (%)	−0.022	−0.001	0.005	−0.022
	(0.022)	(0.028)	(0.016)	(0.021)
Pre-mean	0.72	3,981	0.72	4,400
Observations	28,930	28,930	46,255	46,255

Notes: This table shows the impact of parental heart attacks on children's labor market outcomes based on equation $O_{i,t} = \beta Post_{it} + \lambda_{c,t} + \psi_i + \upsilon_{i,t}$, where $Post_{it}$ is an indicator equal to 1 in the quarters after the shock ($k \geq 0$) and 0 otherwise. The dependent variable in columns (1) and (3) is equal to one if the child is employed in a given quarter. In columns (2) and (4), the outcome is the quarterly earnings of the child in Euro. *Pre-mean* refers to the mean of the respective outcome variable four quarters prior to the parental health shock. Standard errors are clustered at the individual level and reported in parentheses; those for the relative effects are bootstrapped (500 replications). Before reform refers to the years from 2003 to 2007, after reform to the years from 2008 to 2012.

B Spouses after partner's stroke

In this section, we study the impact of the health shock on spousal labor supply. We will focus on strokes, since those are found to cause large increases in care dependency and triggered the strongest reactions of children's labor supply.[1] As in the case of children, we study labor market outcomes of the spouse three years before and after the stroke. Figure B.1 displays event-study coefficients and standard errors estimated from equation (1) for employment and quarterly earnings. We find no significant effects on spousal labor market outcomes. This result might be explained by the low labor market attachment of the spouse prior to the shock (employment rates are around 38 percent, see Table 1). Furthermore, it is *a priori* unclear whether a spouse reacts to such a shock with decreasing labor supply, in order to provide informal care, or with an increase of labor supply (to compensate for the household income shock, or to raise financial resources to buy formal care). Another explanation might be the salience of financial incentives regarding public pension claims among spouses. In Austria, dropping out of the labor force before reaching the statutory retirement age leads to considerable reductions in pension claims, an effect especially salient for the spouses in our sample (they are around 58 years old at the time of the health shock). Furthermore, care work is sometimes physically challenging, making younger family member (e.g. adult children) more suitable to step in. Overall, our results are in line with Dobkin et al. (2018) who find no evidence of a response of spousal earnings, as well as with Fadlon and Nielsen (2021) who find no economically significant reduction in spousal employment and earnings after a health shock.

Figure B.1: Spouse's labor market outcomes: Strokes



(a) Employment

(b) Quarterly earnings in Euro B

Notes: Each panel of the figure shows the event-study coefficients estimated from equation (1) (including 95 percent confidence intervals) of a stroke on the labor market outcome of the spouse. Standard errors are clustered at the individual level. *Pre-mean* refers to the mean of the respective outcome variable four quarters prior to the parental stroke.

[1] The results are similar for heart attacks.

C Heterogeneous treatments across cohorts

In this section, we relax our assumption of homogeneous treatments across cohorts. In our baseline estimations we assume that treatment effects are homogeneous across cohorts with different shock quarters. In our setting with variation in treatment timing, this implies that our estimations are not contaminated by effects from other periods in the two-way fixed effects regressions (see for example Callaway and Sant'Anna (2021) or Sun and Abraham (2021) for alternative estimators to account for treatment effect heterogeneity). We regard this assumption as plausible, since families with different timings in health shocks should not be systematically different and due to independence of families, i.e. the timing of a shock in one family is independent from the health shock in another unknown family, treatment timing should be uncorrelated. Nevertheless, as a robustness check we implemented the interaction weighted estimator suggested by Sun and Abraham (2021). Appendix Table C.1 replicates our baseline estimates from Table 2. Our results remain basically unchanged for children's employment and slightly increase in size for children's earnings.

Table C.1: Impact of parental strokes on children's labor market outcomes using interaction weighted estimators by Sun and Abraham (2021)

	(1)	(2)	(3)	(4)
	Employed	Earnings	Employed	Earnings
Panel A. Pooled				
Post	-0.018^{***}	-184.783^{***}		
	(0.006)	(47.161)		
Observations	139,043	132,685		
Panel B. By subgroups				
B1. By sex of the child				
	female		male	
Post	-0.020^{*}	-204.501^{***}	-0.015^{**}	-167.875^{***}
	(0.010)	(70.458)	(0.006)	(63.699)
Observations	67,297	64,142	71,757	68,471
B2. By geographic distance between child and parent				
	local		distant	
Post	-0.021^{**}	-172.628^{**}	-0.006	-128.019
	(0.008)	(65.835)	(0.012)	(87.729)
Observations	62,876	60,281	46,830	44,475
B3. By year of parental stroke				
	2007 and before		after 2007	
Post	-0.044^{**}	-220.083^{*}	0.008	-111.238
	(0.018)	(112.564)	(0.011)	(78.794)
Observations	18,354	18,354	42,888	42,888

Notes: This table shows the impact of parental strokes on children's labor market outcomes based on equation $O_{i,t} = \beta Post_{it} + \lambda_{c,t} + \psi_i + \upsilon_{i,t}$ using, where $Post_{it}$ is an indicator equal to 1 in the quarters after the shock ($k \geq 0$) and 0 otherwise. Interaction weighted estimates of dynamic treatment effects are obtained from the tool provided by Sun (2021). The number of observation decreases due to exclusion of the last-treated cohort, which serves as the control cohort. The dependent variable in columns (1) and (3) is equal to one if the child is employed in a given quarter. In columns (2) and (4), the outcome is the quarterly earnings of the child in Euro. Geographic distance between parent and child in Panel B2 is defined as local if they live in the same municipality one year prior to the stroke and as distant otherwise. Geographic distance is missing for some children for whom we do not observe either their own or their parents' location. 2007 and before in Panel B3 refers to the years from 2003 to 2007, after 2007 to the years from 2008 to 2012.

D Empirical approach with explicit control group

Our event-study approach uses a varying control group over k, the quarters relative to the quarter of the shock (Druedahl and Martinello, 2022). In this section, we implement as a robustness check the approach of Fadlon and Nielsen (2021, 2019), which relies on a fixed control group over k. More precisely, we compare the outcomes of individuals whose family experiences a parental health shock in time τ with those of families who experience the same shock twelve quarters in the future, i.e. in $\tau+12$. This is particularly useful because parental health outcomes slightly worsen in the three quarters right before the health shock (see Section 5.1). In our main approach, these individual-quarter observations can enter the control group in any event quarter. Exploiting a fixed control group of families which experiences the health shock twelve quarters in the future, we can transparently show the dynamic effects for the event quarters in which this control group provides an "absolutely clean" counterfactual. In particular, allowing for anticipation effects up to three quarters before the shock, the control group provides a reasonable counterfactual up to event quarter nine.

Our strategy to construct the control group is as follows. We start with a cohort of individuals of the same birth cohort and define individuals from families experiencing a shock in a (reference) quarter τ as the treatment group. The control group is given by the set of individuals in the same birth cohort, whose families experience a parental health shock twelve quarters in the near future, in $\tau+12$. We then assign a placebo shock at τ to the individuals in the control group. It is important to hold the birth cohort of the treated and control group fixed to make sure that they are at the same stage of their life-cycle at date h. We repeat the construction of the control group for every combination of birth cohort and reference quarter τ and construct weights such that the treatment and control group size is balanced within each cell.

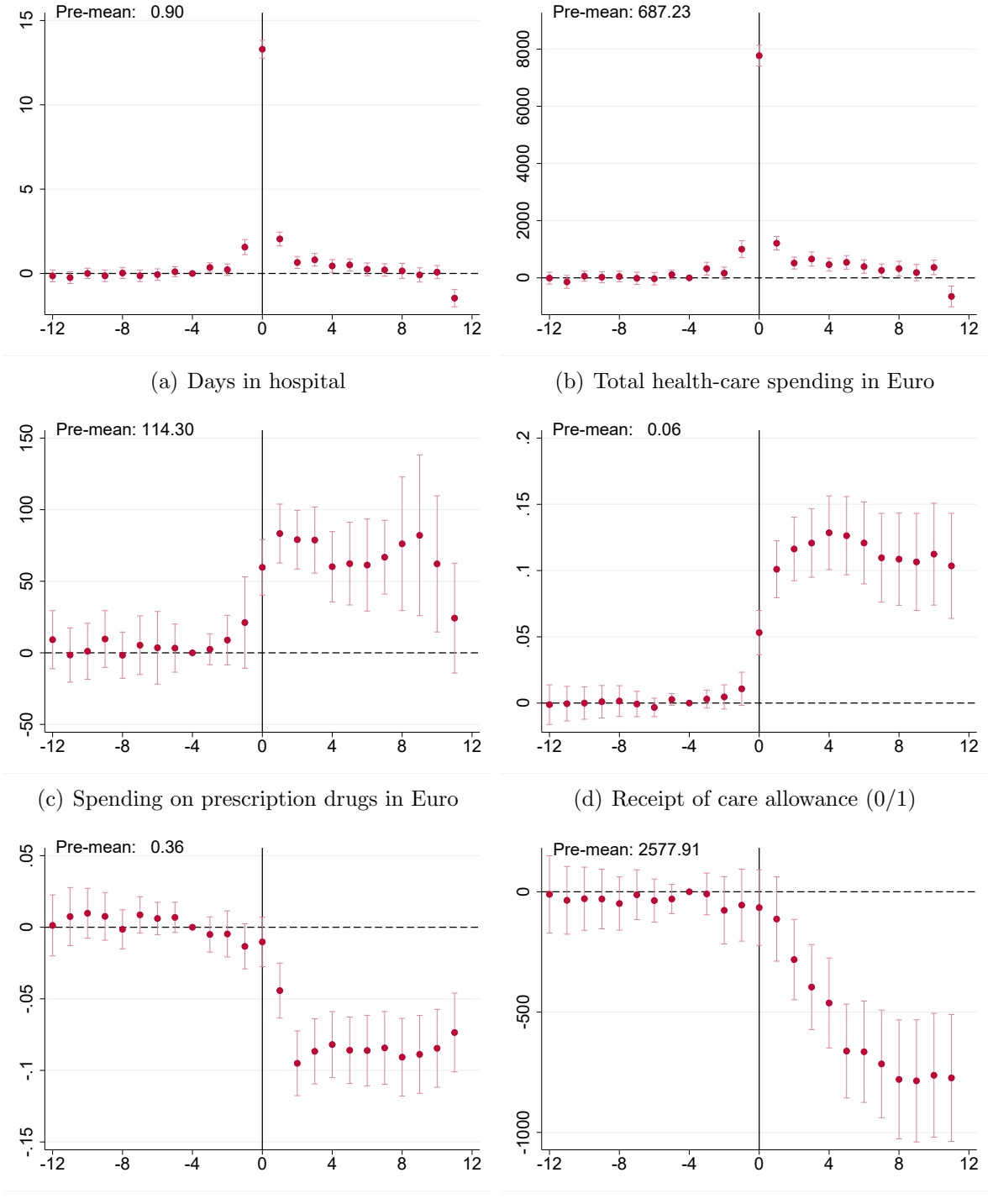
The reference date is such that for the treatment group it refers to the actual date of the shock, whereas for the control group it refers to the date of the placebo shock. Based on this treatment and control group, we estimate the dynamic difference-in-differences (DD) model:

$$O_{i,t} = \theta D_i + \sum_{k=-12}^{11} \gamma_k I\{t-\tau_i = k\} + \sum_{\substack{k=-12 \\ k\neq -4}}^{11} \delta_k D_i * I\{t-\tau_i = k\} + \lambda_t + \upsilon_{i,t}, \qquad (4)$$

where O_{it} is the outcome of individual i in quarter t, k measures the number of quarters relative to the reference quarter, D_i is an indicator equal to one if the parent has a health shock at $k=0$ and equal to zero if the shock is at $k=12$, $I\{.\}$ is the indicator function, and $\upsilon_{i,t}$ is the error term. The parameter θ estimates the overall mean difference in the outcome between treated and controls, the parameters γ_k measure the quarterly time profile of the outcome in the control group and δ_k measure the difference in time profiles between the treated and the control group relative to the reference quarter.

The results from this robustness check are illustrated in Appendix Figures D.1, D.2, D.3, and D.4. In sum, the estimated coefficients for the dynamic treatment effect up to nine quarters after the reference quarter are very similar across the two approaches: Compare Figures 4 and D.2 for children's labor market outcomes. This suggests that the pre-shock health problems do not lead to any relevant anticipatory effects.

Figure D.1: Impact of parental stroke on parental health



(a) Days in hospital

(b) Total health-care spending in Euro

(c) Spending on prescription drugs in Euro

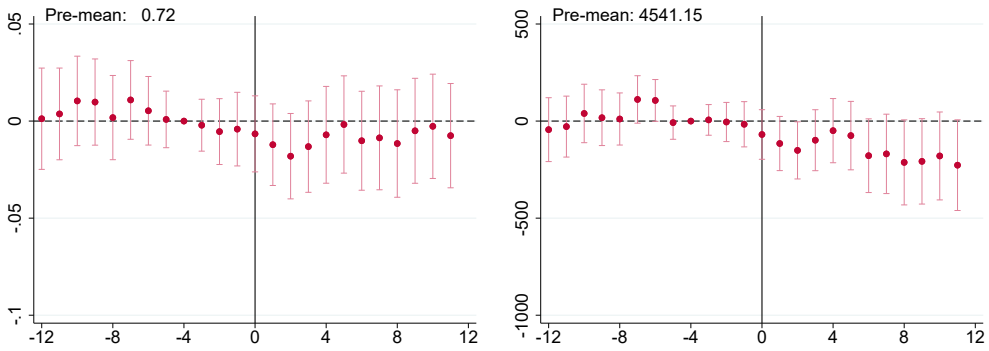
(d) Receipt of care allowance (0/1)

(e) Employment

(f) Quarterly earnings in Euro

Notes: In each panel we compare the difference in parental outcomes between parents who have a health shock at the reference date and parents who have a health shock twelve quarters after the reference date based on equation (4). We assign a placebo shock to the later group at the reference date. The x-axis denotes the quarters relative to the reference date. We normalize the difference between these two groups in -4 and control for time fixed effects. This approach is inspired by Fadlon and Nielsen (2021, 2019). We include 95 percent confidence intervals based on standard errors clustered at the individual level. *Pre-mean* refers to the mean of the respective outcome variable four quarters prior to the parental stroke.

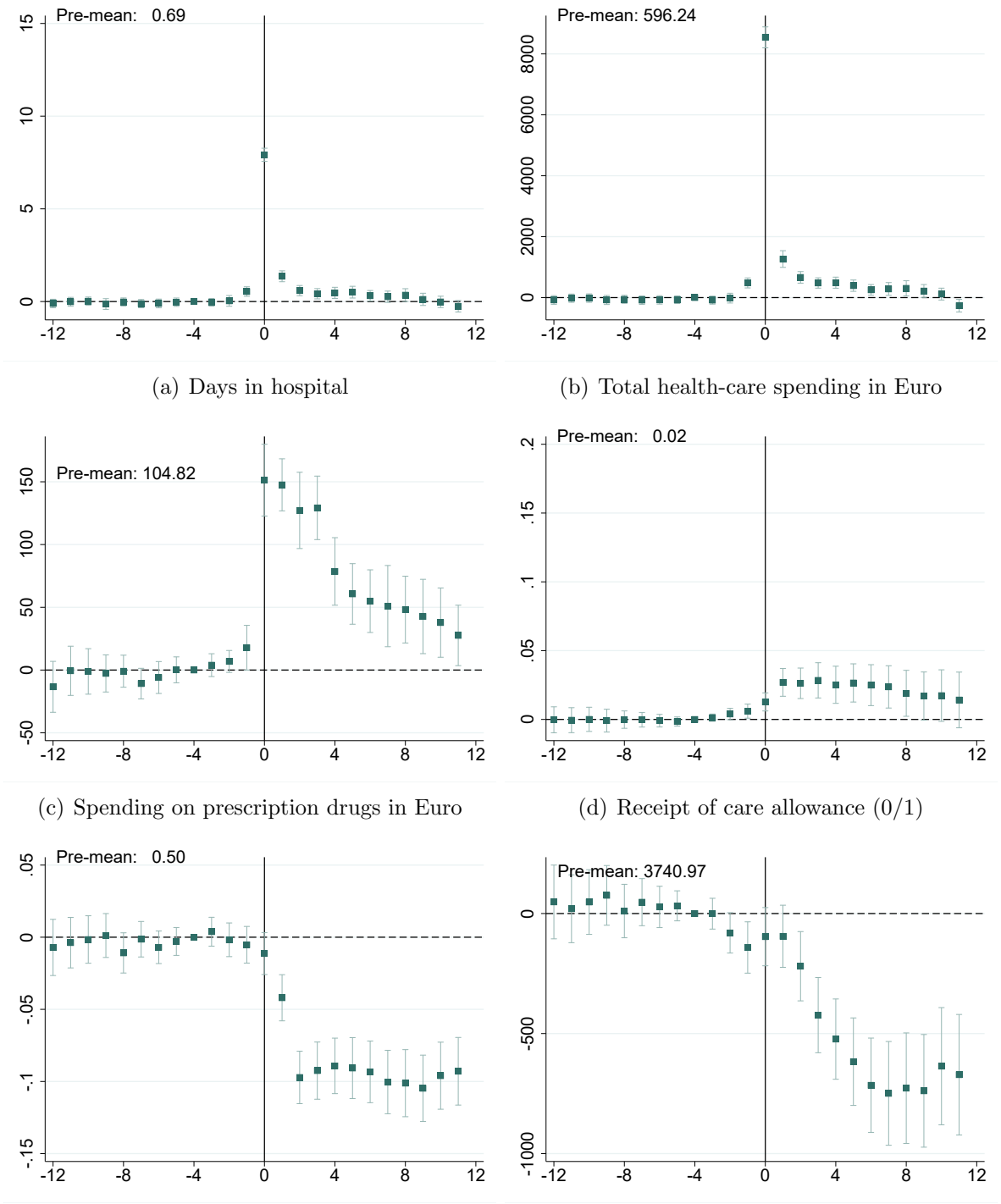
Figure D.2: Impact of parental stroke on children's labor market outcomes



(a) Employment (b) Quarterly earnings in Euro

Notes: In each panel we compare the difference in children's outcomes between children who have parents with a health shock at the reference date and children who have parents with a health shock twelve quarters after the reference date based on equation (4). We assign a placebo shock to the later group at the reference date. The x-axis denotes the quarters relative to the reference date. We normalize the difference between these two groups in -4 and control for time fixed effects. This approach is inspired by Fadlon and Nielsen (2021, 2019). We include 95 percent confidence intervals based on standard errors clustered at the individual level. *Pre-mean* refers to the mean of the respective outcome variable four quarters prior to the parental stroke.

Figure D.3: Impact of parental heart attack on parental health



(a) Days in hospital

(b) Total health-care spending in Euro

(c) Spending on prescription drugs in Euro

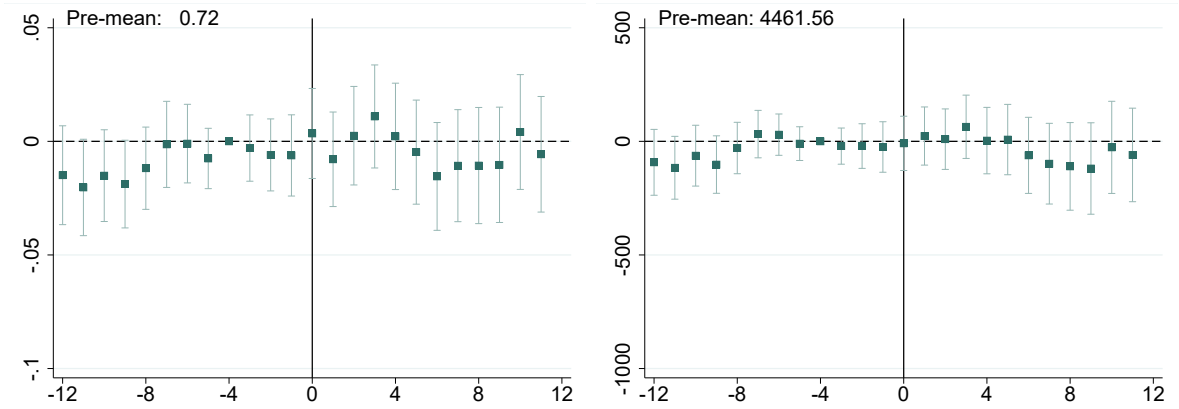
(d) Receipt of care allowance (0/1)

(e) Employment

(f) Quarterly earnings in Euro

Notes: In each panel we compare the difference in parental outcomes between parents who have a health shock at the reference date and parents who have a health shock twelve quarters after the reference date based on equation (4). We assign a placebo shock to the later group at the reference date. The x-axis denotes the quarters relative to the reference date. We normalize the difference between these two groups in -4 and control for time fixed effects. This approach is inspired by Fadlon and Nielsen (2021, 2019). We include 95 percent confidence intervals based on standard errors clustered at the individual level. *Pre-mean* refers to the mean of the respective outcome variable four quarters prior to the parental stroke.

Figure D.4: Impact of parental heart attack on children's labor market outcomes



(a) Employment

(b) Quarterly earnings in Euro

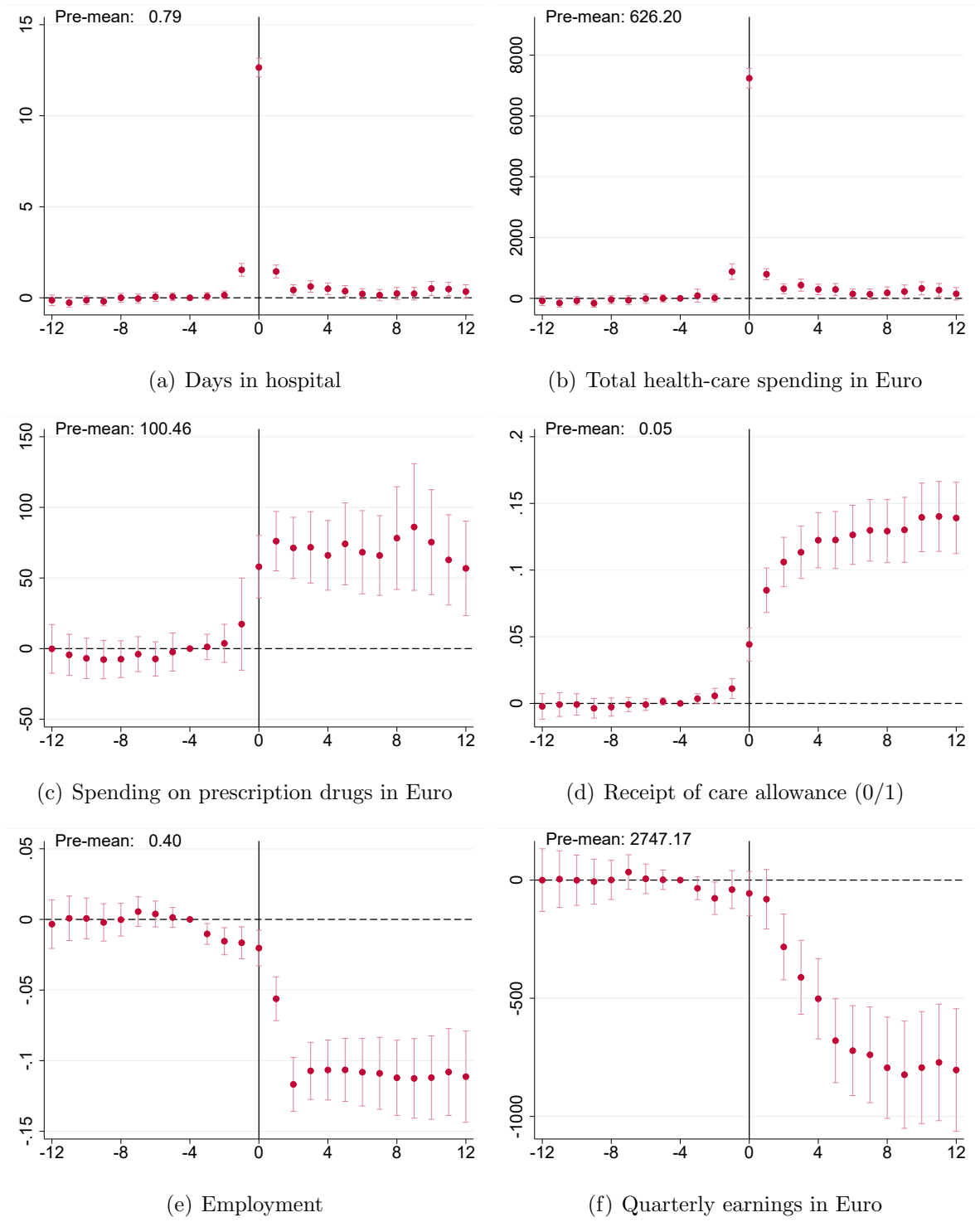
Notes: In each panel we compare the difference in children's outcomes between children who have parents with a health shock at the reference date and children who have parents with a health shock twelve quarters after the reference date based on equation (4). We assign a placebo shock to the later group at the reference date. The x-axis denotes the quarters relative to the reference date. We normalize the difference between these two groups in -4 and control for time fixed effects. This approach is inspired by Fadlon and Nielsen (2021, 2019). We include 95 percent confidence intervals based on standard errors clustered at the individual level. *Pre-mean* refers to the mean of the respective outcome variable four quarters prior to the parental stroke.

E Excluding cases where the affected parent dies within three years

As described in the main text, for our baseline sample we employ all parental strokes and heart attacks where the affected parent survives at least one week after the health shock. In the following, we show that our results do not change when using a more restrictive sample definition. Specifically, we now keep only parent-child pairs where the affected parent survives over the entire time horizon of our analysis (survival $\geq$ 3 years). It is important to note that only around 10% of the affected parents in our baseline sample are dropped when introducing this restriction. This has to do with the fact that we focus on the first parental stroke (or heart attack) in our baseline sample. As a result, affected parents in our baseline sample are relatively young (around 60 years old) and, thus, the survival rates are quite high.

Appendix Figures E.1 and E.2 replicate our baseline results in Figures 2 and 4 but using this more restrictive sample. In sum, our baseline results remain primarily unchanged. Specifically, we find significant negative effects on employment as well as earnings of children. The magnitude of the effects is similar to our baseline sample. In sum, we find that restricting our analysis to parents who survive over the entire time horizon does not change our conclusions. However, in order to fully exploit all parent-child-quarter combinations in our data, we decided to keep the unrestricted sample as our baseline.

Figure E.1: Impact of parental stroke on parental health - Families with surviving parents only



(a) Days in hospital

(b) Total health-care spending in Euro

(c) Spending on prescription drugs in Euro

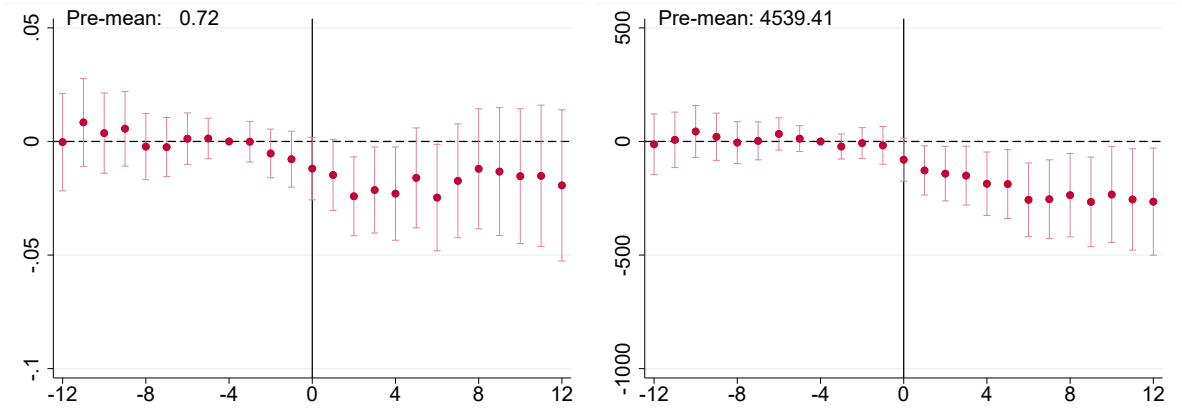
(d) Receipt of care allowance (0/1)

(e) Employment

(f) Quarterly earnings in Euro

Notes: Each panel shows the event-study coefficients estimated from equation (1) and the corresponding 95 percent confidence intervals. These are based on the sample of parents who survive the entire time horizon of our analysis (survival $\geq$ 3 years). Standard errors are clustered at the individual level. *Pre-mean* refers to the mean of the respective outcome variable four quarters prior to the parental stroke.

Figure E.2: Families with surviving parents only: Impact of parental stroke on children's labor market outcomes



(a) Employment

(b) Quarterly earnings in Euro

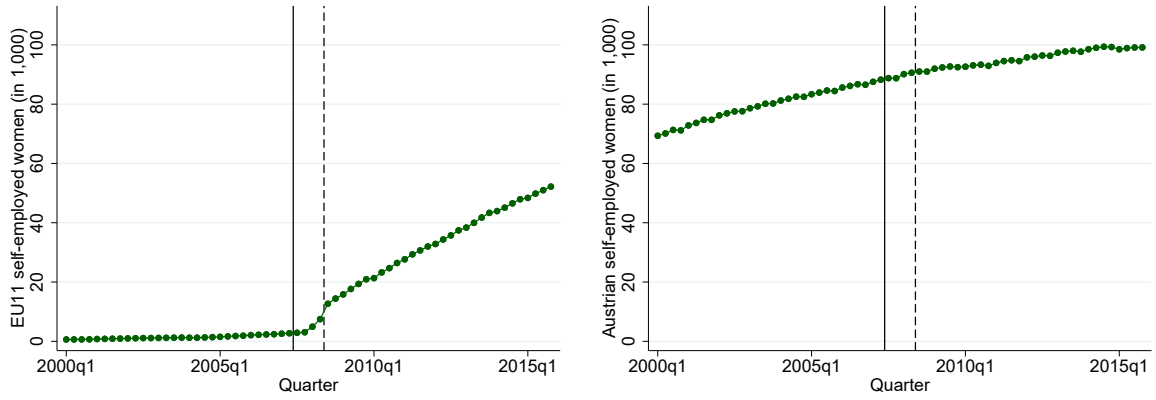
Notes: Each panel shows the event-study coefficients estimated from equation (1) and the corresponding 95 percent confidence intervals. These are based on the sample of children whose parents survive the entire time horizon of our analysis (survival $\geq$ 3 years). Standard errors are clustered at the individual level. *Pre-mean* refers to the mean of the respective outcome variable four quarters prior to the parental stroke.

F Additional results regarding the liberalization of foreign care workers in Austria

F.1 Size of the grey economy before the reform

It is possible that some of the self-employed foreign care workers who registered after 2007 worked illegally in Austria prior to the reform. So rather than inducing a real supply shock of care workers the reform might have just led to a legalization of foreign care workers who already provided their services on an unregistered basis. The home care reform was implemented on July 1, 2007 but there was a transitional period until June 30, 2008 in which amnesty was granted for unregistered home care workers (Larsen et al., 2009). In the left panel of Appendix Figure F.1 we plot the number of EU-11 self-employed women on a quarterly basis (ASSD). The number of EU-11 self-employed women steadily increases by around 1,000 to 2,000 per quarter after the implementation of the reform in the third quarter of 2007. In addition, we see a discrete jump in the number of EU-11 self-employed women around the time when amnesty ends. This number jumps from about 7,000 in the second quarter to 13,000 in the third quarter of 2008. We assume that the discrete increase by around 6,000 registered EU-11 self-employed women around the end of amnesty provides an upper bound for the legalization effect. This legalization effect appears to be small compared to the overall increase of EU-11 self-employed women after the reform, rising steadily to around 40,000 in 2012 (see Panel (e) Figure 8). This is corroborated by a comprehensive study based on interviews and field studies, which concludes that the post-reform number of foreign care workers considerably exceeds the estimated level of previous illegal employment in this sector (Oesterle and Bauer, 2016).

Figure F.1: Number of female self-employed in Austria by quarter



(a) EU11 self-employed women

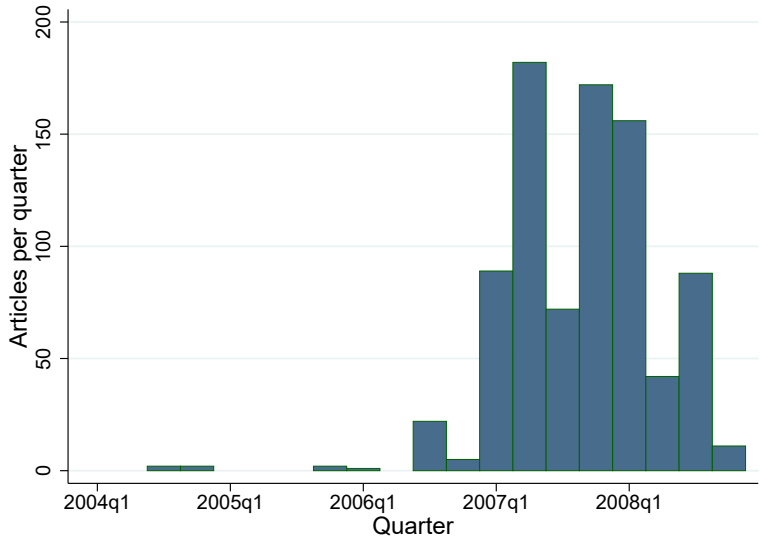
(b) Austrian self-employed women

Notes: The panels of this figure show the number of self-employed women in Austria for each quarter from 2000 to 2015. The panel to the left (to the right) refers to EU11 nationals (Austrians). The solid line highlights the quarter in which the home care work reform was implemented. The dashed line highlights the quarter until which amnesty was granted to illegal home care workers (Larsen et al. (2009)). *Source*: Austrian Social Security Database.

F.2 Salience of the reform

To change behavior as a response to the increased availability of foreign care workers, Austrian families with caring responsibilities must be aware of the 2007 reform. One way to assess the salience of the reform is to examine its press coverage. Figure F.2 displays the number of press articles mentioning the phrase *24 Stunden Pflege* (24h care work), an expression often used to describe the content of the reform in German.[1] We display the number of articles by quarter from January 2004 until December 2009, and for the major Austrian newspapers.[2] Figure F.2 shows a sharp increase in press articles about *24 Stunden Pflege* exactly around the time when the new law was discussed in parliament. Furthermore, the coverage of the reform continued to be high in the following year when the new law was implemented. This suggests that the salience of the reform was quite large, with many people knowing about the new possibilities regarding finding a (foreign) care worker.

Figure F.2: Press articles referring to the reform



Notes: This figure shows the number of press articles mentioning *24h Pflege*, an expression often used to describe the content of the 2007 reform. It displays the number of articles referring to the reform by quarter.

[1] Using other expressions associated with the reform give qualitatively similar results.

[2] We constructed the graph by scraping the newspaper archive *AOMlibrary* by the Austrian Press Agency (APA), which contains a full-text archive of most newspapers in Austria. The sample is restricted to large daily newspapers. This covers seven newspapers in total, including Kurier, Der Standard, Die Presse, and Krone.

F.3 Effect of the reform on the elderly

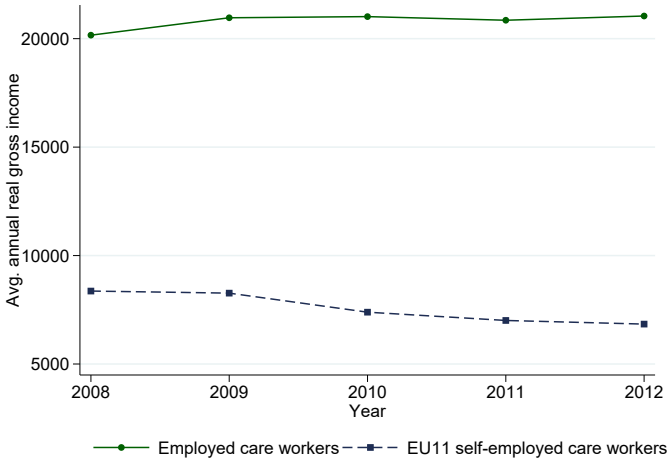
Table F.1: Mortality effects of the reform

	(1) Within 1 yr	(2) Within 2 yrs	(3) Within 3yrs	(4) Within 4yrs	(5) Within 5 yrs
Panel A: Five-year window around reform					
γ_1: Post	-0.017*	-0.012	-0.014	-0.017	-0.011
	(0.006)	(0.009)	(0.010)	(0.011)	(0.013)
γ_2: Stroke	-0.001	0.031**	0.041**	0.042**	0.061*
	(0.011)	(0.015)	(0.017)	(0.018)	(0.020)
γ_3: AfterReform	-0.033*	-0.021	-0.020	-0.024	-0.002
	(0.012)	(0.016)	(0.017)	(0.019)	(0.021)
γ_4: Post · Stroke	-0.001	-0.001	0.000	0.001	0.001
	(0.003)	(0.004)	(0.004)	(0.005)	(0.005)
γ_5: Post · AfterReform	0.008	-0.003	-0.010	-0.013	-0.014
	(0.007)	(0.010)	(0.012)	(0.013)	(0.015)
γ_6: Stroke · AfterReform	0.019	-0.001	-0.005	-0.002	-0.011
	(0.013)	(0.018)	(0.020)	(0.022)	(0.024)
η: Post · Stroke · AfterReform	-0.002	-0.004	-0.007	-0.010	-0.010
	(0.004)	(0.005)	(0.005)	(0.006)	(0.006)
Observations	68,975	68,975	68,975	68,975	68,975
Panel B: Five-year window around reform: Only "early" health shocks					
γ_1: Post	-0.017	-0.013	-0.020	-0.012	0.001
	(0.012)	(0.016)	(0.019)	(0.020)	(0.024)
γ_2: Stroke	0.007	0.053**	0.061**	0.068**	0.098*
	(0.012)	(0.021)	(0.025)	(0.027)	(0.030)
γ_3: AfterReform	-0.027	-0.015	-0.024	-0.010	0.023
	(0.021)	(0.027)	(0.031)	(0.032)	(0.035)
γ_4: Post · Stroke	0.002	0.002	0.004	0.005	0.004
	(0.003)	(0.005)	(0.005)	(0.006)	(0.006)
γ_5: Post · AfterReform	0.009	-0.002	-0.005	-0.020	-0.028
	(0.013)	(0.018)	(0.021)	(0.023)	(0.026)
γ_6: Stroke · AfterReform	0.013	-0.022	-0.024	-0.027	-0.048
	(0.014)	(0.024)	(0.028)	(0.030)	(0.033)
η: Post · Stroke · AfterReform	-0.007*	-0.007	-0.011*	-0.015**	-0.013*
	(0.004)	(0.006)	(0.006)	(0.007)	(0.008)
Observations	55,000	55,000	55,000	55,000	55,000

Notes: This table displays the estimated coefficients for the DDD model presented in equation 2. The dependent variable in columns (1) to (5) is equal to one if the parent dies within the indicated years after the health shock. Standard errors are clustered at the individual level and reported in parentheses. *, ** and *** indicate significance at 10-, 5- and 1-percent levels, respectively.

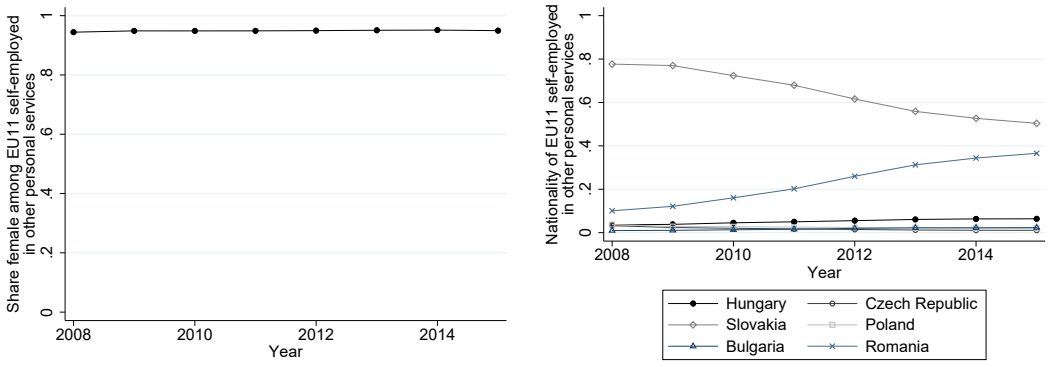
F.4 Additional figures concerning liberalization of the formal care market

Figure F.3: Annual income of dependent vs. self-employed care workers



Notes: This figure shows the average annual gross income of the EU-11 self-employed female care workers as well as the average annual gross earnings of dependent female care workers in Austria. Dependent female care workers include all employees in the 4-digit NACE categories 8710 (residential nursing care activities), 8730 (residential care activities for the elderly and disabled), 8790 (other residential care activities), and 8810 (social work activities without accommodation for the elderly and disabled). *Source*: Austrian Social Security Database & Austrian Tax Return Data.

Figure F.4: Characteristics of EU11 nationals employed in personal services

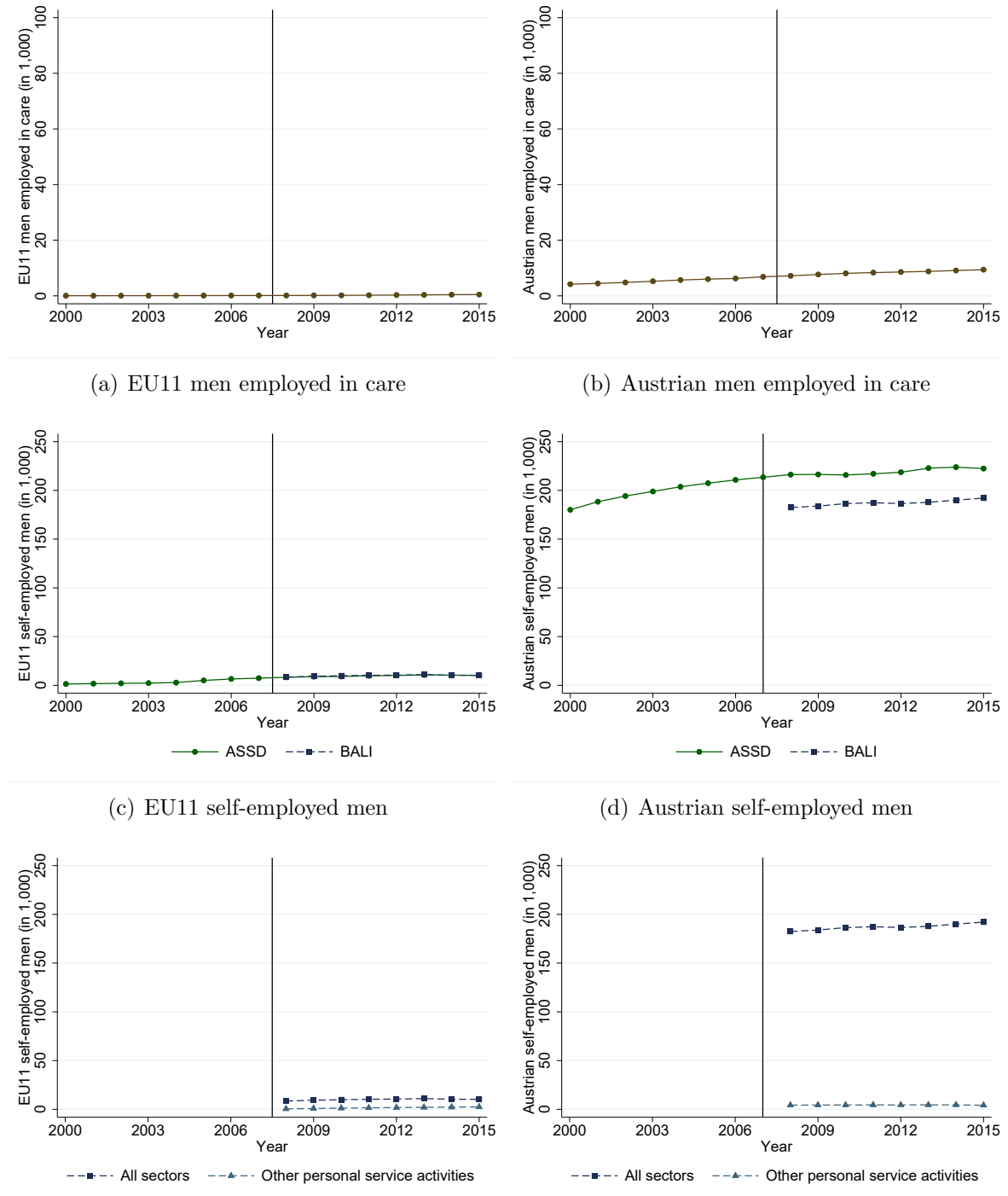


(a) Share female

(b) Nationality

Notes: The panels of this figure show the characteristics of EU11 self-employed in personal services in Austria from 2008 to 2015. The panel to the right does not include those with a nationality from the Baltic States or from Slovenia because their shares are negligibly small (below 0.01 in all years). *Source*: BALI (budget, labour market and benefit receipt information) online database.

Figure F.5: Number of male care workers in Austria



(a) EU11 men employed in care

(b) Austrian men employed in care

(c) EU11 self-employed men

(d) Austrian self-employed men

(e) EU11 self-employed men by sector

(f) Austrian self-employed men by sector

Notes: The panels show the average number of male care workers in Austria from 2000 to 2015. For each year we take the average over the total number employed at four reference dates. The panels to the left (to the right) refer to EU11 nationals (Austrians). Panels a) and b) include all employees in the NACE categories with care activities. Panels c) and d) include all self-employed men from two different data sources. Panels e) and f) distinguish between all self-employed men and those in "Other personal service activities" based on the BALI data. *Source*: Austrian Social Security Database, BALI (budget, labour market and benefit receipt information) online database.