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Time to Give

Poor Health as a Trigger for Inter-Vivos Transfers

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Time to Give: Poor Health as a Trigger for Inter-Vivos Transfers

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Abstract

In this study we investigate the role of health status as a predictor of wealth transfers between households (inter-vivos transfers). To that end, we use high-quality administrative data for the whole population of the Netherlands. Using data on hospital intakes, we construct a measure of health shocks based on the existing literature. We use this measure to carry out an event study that exploits the randomness in the exact timing of the shock. Our results show a significant and positive increase in the probability of giving during the years following a health shock. Further analysis indicates that this result does not reflect the presence of an exchange motive for giving, and that it does point to the use of inter-vivos transfers as an instrument to avoid inheritance taxes. Our results have relevant implications for social policies that redistribute wealth across individuals as well as for the design of gift and inheritance tax schedules.

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1 Introduction

Inter-vivos transfers (IVTs), *i.e.* transfers of resources among private individuals and households, play a crucial role in the well-being of families and society as a whole. They provide a means to redistribute economic resources among households and across generations. As a consequence, they often have important implications for the efficiency and effectiveness of social benefit programs that redistribute wealth across society via public transfers. As argued by *e.g.* Boileau and Sturrock (2023) and Suari-Andreu *et al.* (2024) among others, IVTs are likely to offset or complement existing public transfers. For instance, when directed from parents to children, IVTs will at least partially offset programs like public pensions that redistribute wealth from younger to older generations. In addition, when offered in exchange for informal care provision, IVTs will complement public long-term care programs that may be in place. Potentially, the presence and the motives behind IVTs have relevant implications for any public policy that redistributes wealth among households.

IVTs and inheritances are specially relevant nowadays since their frequency and volume have been increasing in recent years and are predicted to keep doing so in the near future (Palomino *et al.*, 2022). This increase is related, both as a cause and as a consequence, to an observed growth in wealth inequality within and across generations. It leads, in turn, to a rise in the overall share of wealth accumulated via transfers and inheritances (Alvaredo *et al.*, 2017; Elinder *et al.*, 2018). Therefore, studying the circumstances and motives behind inter-vivos giving between households is important for two reasons. First, as an input for the optimal design of social insurance and redistributive public transfers and, second, for understanding the dynamics of economic inequality.

In this study, we contribute to the literature by paying particular attention to the role of health as a trigger for IVTs. Health can play a role because transitions to poor health are very likely to lead individuals to revise their mortality expectations and/or see their care needs increase (Baji and Bíró, 2018). These two effects can increase the utility of giving while still alive *vis-à-vis* other possible uses of wealth. In turn, utility from giving can respond to different underlying giving motives. Understanding the role of poor health as a trigger for IVTs will help us shed light on the relative importance of the different giving motives identified by the literature.

As explaned by Kopczuk (2007) and Suari-Andreu *et al.* (2019) among others, the economic literature identifies three different motives why individuals may derive utility from giving. First, an exchange motive, *i.e.* when the transfer is regarded as means of exchange for informal care (*e.g.* Bernheim *et al.*, 1985; Perozek, 1998). Second, an altruistic motive, *i.e.* when the amount of the transfer depends on the marginal utility of wealth of the receiver (*e.g.* Becker, 1974; Laitner, 2002). Third, a warm-glow motive, *i.e.* when the transfer responds to the pure desire to give and not to the economic position or the actions of the receiver (*e.g.* Hurd, 1989; De Nardi and Yang, 2014).

Investigating the role of health as a trigger for IVTs provides a better understanding of the circumstances leading to giving and on the relative importance of the above-mentioned underlying motives. That is because poor health can lead to exchange of IVTs for informal care. Furthermore, as explained by Kvaerner (2023), Bonekamp and Wouterse (2023), and Suari-Andreu *et al.* (2024) reduced mortality due to poor health can lead to IVTs responding to a warm-glow or an altruistic motive. In the present study, we pay special attention to identifying the exchange motive and leave the identification of the altruistic and warm-glow motives for future work. Understanding and discerning among giving motives is important because the resulting implications for social policy and taxation depend on the relative importance of each of them.

In addition to the above-mentioned giving motives identified by the literature, when poorer health leads to substantial reduction in the expected life-time horizon, individuals may start giving while alive to divide their estate and avoid inheritance taxes. A few studies show evidence of estate planning at the end of life using IVTs (Kopczuk, 2007; Erixson and Escobar, 2020; Sturrock *et al.*, 2022; Suari-Andreu *et al.*, 2024). All of these studies assume that deteriorating health and revised mortality lead to transfers related to estate planning. However, these studies do not test this assumption directly and evidence of the effect of health on IVTs is very scarce.

To study the relation between health and IVTs, we employ high quality administrative data provided by Statistics Netherlands (CBS) on the whole Dutch population for the years between 2007 and 2019, both included. Importantly for the purpose of this study, CBS provides data on hospitalisations (sourced by the hospital discharge register) and on monetary gifts between individuals (sourced by the tax authorities). Following García-Gómez *et al.* (2013), Rellstab *et al.* (2020), and Bonekamp and Wouterse (2023), we define health shocks as unplanned hospitalizations that are not preceded by any hospitalization in the two years previous. Out of the whole population, we select individuals who are at least fifty years of age and suffer a health shock at any point between 2007 and 2019. Following Kleven *et al.* (2019) and Borusyak *et al.* (2024) among others, we apply an event study design by regressing the incidence (and amount) of IVTs on a set of leads and lags of the health shock. This strategy allows comparing individuals who suffer a health shock with similar individuals who have not suffered it yet. The identification of the effect hinges on the change in health being unexpected and on the randomness of the exact timing of the shock.

There are two important additional advantages of using the CBS data. First, they provide information on the main diagnosis associated to each hospitalization; and second, they allow connecting all individuals in our sample to their children. These features allow us to test the relative importance of the exchange motive for giving by extending our baseline analysis in different ways. First of all, using data on diagnoses, and building on the previous work by Bonekamp and Wouterse (2023), we classify health shocks according to the extent to which they lead to physical disability and/or increased mortality. If the exchange motive is relevant, then shocks that lead to physical but not to increased mortality should have a stronger effect on inter-vivos giving. If shocks leading to increased mortality have a stronger effect, then altruism and/or warm-glow giving should more important than the exchange motive.

Next to the above-mentioned extension of the baseline results, we use the link between parents and children to expand our analysis in two additional ways. First, we re-estimate our baseline regression accounting for the geographical proximity between parents and children. The literature suggests that proximity to the parental household is a good predictor of informal care provision from children to parents (Bonsang, 2009; Fu, 2019). Therefore, if the exchange motive is relevant then individuals with children living close to them should be more likely to give after a health shock. Second, we re-estimate our baseline regression separately for individuals with and without daughters.[1] The literature indicates that informal care provision from children to parents is strongly gendered since female children appear to be more likely to provide care than male children (Carmichael and Charles, 2003; Schmitz and Westphal, 2017). Therefore, if the exchange motive is relevant then individuals with daughters should be more likely to give after a health shock.

The Dutch context is specially interesting to conduct the present study. That is because in the Netherlands individuals are generally very well protected against income and wealth drops following illness and disability (García-Gómez *et al.*, 2013; Koning and Lindeboom, 2015). In addition, out-of-pocket medical expenditures are generally very small (Bakx *et al.*, 2016). That means that health shocks are unlikely to strongly impact the financial position of individuals, thus making it more plausible that IVTs take place after a health shock for the reasons and motives outlined above. An important exception in terms of health-related out-of-pocket expenditures are co-payments related to nursing home use. As explained by Tenand *et al.* (2021), nursing home use in the Netherlands is associated with a means-based co-payment, which may create an additional incentive to give wealth after a health shock to avoid or reduce the co-payment.

An additional interesting feature of the Dutch context is that, as explained by Sturrock *et al.* (2022) and Suari-Andreu *et al.* (2024), the Netherlands has a progressive gift and inheritance tax system that provides incentives for individuals to give while still alive to reduce the overall tax bill on their state. To check whether the tax motive is an important driver of IVTs taking place after a unexpected worsening in health, we use changes in the tax schedule that took place over the years we observe. These are changes in the amount of the one-off exemption for transfers from parents to children. We exploit these changes by comparing individuals who suffer a health shock at different points in time.

Our study contributes to the existing literature by being the first to thoroughly examine the effect of health shocks on inter-vivos giving and use this to investigate the relative importance of different giving motives. To our knowledge only two studies have done something similar in the past. First, Kvaerner (2023) uses Dutch and Norwegian administrative data to estimate the effect of health shocks on giving. Using the Dutch data he employs a difference-in-difference estimator and finds that changes in health occurred in 2013 increased the probability of giving

[1] Both of these analyses are conditional on having at least one child living outside of the household and controlling for the number of children and their age.

in the subsequent three years. He then uses the estimates of his analysis to calibrate the intensity of the bequest motive in a structural model of life-cycle saving and consumption. Second, Schaller and Eck (2023) use survey data for the USA to examine changes in IVTs and informal care within families following wealth shocks, job loss, widowhood, and health shocks. Interestingly, they find that parental health shocks decrease IVTs from parents to children but increase IVTs from children to parents. This finding however must be interpreted in the American institutional context. The latter is very different from the Dutch one in that out-of-pocket medical expenditures tend to be higher and individuals are significantly less protected against sickness and disability.

Besides contributing to the above-mentioned strands of literature on IVTs, giving motives, and estate planning, the present study is also related and contributes more generally to the literature on health and household finance. Within this literature, there are studies focusing on the effect of health on income and employment (*e.g.* García-Gómez, 2011; García-Gómez *et al.*, 2013; Dobkin *et al.*, 2018; Blundell *et al.*, 2023), wealth (*e.g.* Attanasio and Emmerson, 2003; Michaud and Van Soest, 2008; Bonekamp and Wouterse, 2023), and consumption (*e.g.* Finkelstein *et al.*, 2013; Kools and Knoef, 2019). Additionally, we contribute to the literature studying the effects of parental health on the economic situation of children (*e.g.* Rellstab *et al.*, 2020; Brito and Contreras, 2023; We and Huang, 2024).

Our baseline results show a positive and significant increase in the probability of giving IVTs following a health shock. The effect is very small on the same year of the shock. However, we observe an increase of 11% in the probability of giving at least one transfer on the year after the shock compared to the year right before the shock. After that, the effect gradually decreases and fades out by the fourth year after the shock. Interestingly we do not find an effect when looking at the intensive margin. For that, we consider the number of transfers per year and the total amount transferred per year. These results appears to be robust to an alternative specification using fixed effects and to the application of a novel alternative method proposed by Borusyak *et al.* (2024). An heterogeneity analysis shows that the effect increases with the intensity of the shock, and that it is stronger for older individuals, females, and singles.

Further analysis shows that the health shocks we observe have a considerable effect on mortality and nursing home entry. However, following the classification by Bonekamp and Wouterse (2023), we find that it is mortality-increasing shocks, rather than disabling shocks, that drive the increase in IVTs that we observe. Furthermore, we find no significant differences between individuals with and without adult children living nearby and also not between individuals with and without daughters. All these results indicate that the exchange motive is not likely to be the driver of the increase in IVTs that we observe following a health shock. This result is in line with the findings by Rellstab *et al.* (2020). Finally, our analysis exploiting the changes in the tax regime over time do indicate that our baseline result is at least partially driven by tax avoidance. That is, the use IVTs as an instrument to avoid inheritance taxes. This result is in line with the findings by Sturrock *et al.* (2022) and Suari-Andreu *et al.* (2024).

Our results indicate that health is a relevant predictor of IVTs giving; that IVTs do not respond to the exchange motive thus they are not part of a mechanism complementary with the existing formal long-term care programmes; and that individuals are using the structure of the gift and inheritance tax schedule to decrease the overall tax burden on their estate. Further work is necessary to investigate the relevance of the altruistic and warm-glow motives for giving IVTs.

The remainder of the document is structured as follows. Section 2 presents several relevant aspects of the Dutch institutional context; Section 3 describes the data; Section 4 presents the empirical strategy; Section 5 presents the results; and Section 6 concludes.

2 Institutional Context

2.1 Healthcare System

The healthcare system in the Netherlands is a combination of public and private in which every adult has a mandatory private healthcare insurance. However, private health care insurance companies are obliged by the government to accept everyone for a basic and affordable insurance package. Individuals with low income can apply for a subsidy to be able to afford the premium. Out of pocket payments are very small and are usually related to a mandatory deductible for specialist care. This system is complemented with a universal and comprehensive public insurance for long-term care. As a consequence, virtually all care is provided with full or nearly full coverage in the Netherlands. For more details about the Dutch health care system and the relevance of out of pocket medial expenditures, see Bakx *et al.* (2016).[2] For more details about the public long-term care insurance, see Bakx *et al.* (2023).

An important aspect of the public long-term care insurance is that it covers access to nursing homes. However, this access requires a co-payment depending on household wealth and income. Once individuals become eligible for a nursing home they have to pay a either a low-rate co-payment or a high-rate co-payment. Each of these is determined by specific schedules and ceilings. The low-rate co-payment applies during the first six months of a stay and for individuals who still have a partner living at home, while the high-rate co-payment applies to all other residents. Depending on household income and wealth, the low-rate copayment can go up to 1,052 Euro. The high-rate co-payment is capped at 2,887 Euro, and is typically not much higher than the living expenses an average individual would incur when continuing to live alone.[3] For more details about costs related to nursing home access in the Netherlands, see Tenand *et al.* (2021) and Bergeot and Tenand (2023).

[2]Bakx *et al.* (2016) show that out of all medical expenditures in the Netherlands, only about 5% are out of pocket.

[3]According to Statistics Netherlands, the average out-of-pocket costs for long-term care varied between 500 and 600 per person per month in the last decade. For yearly incomes of at most 10,000 Euro, it varied between 50 and 150 Euro per month. For incomes of above 50,000 Euro, the average contribution varied between 500 and 800 Euro per month. This includes both nursing homes and home care.

2.2 Sickness and Disability Insurance

When employees become unable to work for health reasons in the Netherlands, they are granted a maximum of two years of sickness benefits. During this period, employers are legally not allowed to dismiss the sick employee and obliged to pay (part of) the sickness benefits. The replacement rate cannot be lower than 70% of the last wage. If the 70% replacement rate puts the individual below the minimum wage, he/she will get the minimum wage instead. For the first year, most employers actually cover 100% of the last wage, which is more generous than the replacement rate of unemployment insurance benefits. After a year, the replacement rate may decrease depending on the sector, but never below 70%. Some sectors, such as construction, maintain a 100% replacement for the second year as well.

When the health problems are permanent, individuals are covered by the disability benefit system. Similar, to sickness benefits, disability benefits tend to be more generous than unemployment insurance benefits. The exact replacement rate depends on the degree of disability, which established by a medical screening. In the large majority of cases, the replacement rate lies between 75% and 100%. For more information on sickness benefits, see Been *et al.* (2024). For more information on disability benefits, see Koning and Vethaak (2021) and Koning and van Lent (2024).

2.3 Pension System

The Dutch pension system is composed of three pillars. The first pillar consists of a uniform public pension that is capped at a monthly amount equal to 70% of the minimum wage. Individuals accumulate 2% of the maximum amount for every year that they work or live in the Netherlands. The payments start at the statutory retirement age. The second pillar consist of occupational semi-private pensions. They are managed at the sectoral level and over 90% of employees have access to them. Once the occupational pension scheme is offered by the employer, the employee is obliged by law to take it. Payments are typically substantially larger than the public pension and start at the actual moment of retirement. The latter is in most cases before or at the statutory retirement age. Finally, the third pillar consists of private pension plans offered by banks and insurance companies. The third pillar is typically very small, with the majority of individuals relying mostly on the occupational pension. When combining all three pillars together, Knoef *et al.* (2016) show that the net replacement rate is close to 100%. They conclude that poverty an income uncertainty are less often a problem among retiress than among the working age population. For more deatils on the Dutch pension system, see Knoef *et al.* (2016) and Jensen *et al.* (2020).

2.4 Taxation of Gifts and Inheritances

Gifts and inheritances are taxed in the Netherlands according to a progressive tax schedule that changes depending on the relation between the giver and the receiver. The lowest tax rate (10%

to 20%, depending on the amount transferred) is for transfers from parents to children. Above that are transfers from grandparents to grandchildren (18% to 36%), and then the rest (30% to 40%).[4] There are relevant exemptions for both gifts and inheritances. For gifts, there is a 5,000 Euro exemption per child per year and one-time exemption of 50,000 per child if used for home purchase or studies. This exemption was temporarily increased to 100,000 Euro between October 2013 and December 2014 and then reintroduced in 2017. Importantly, gifts only count towards the inheritance if they take place less than six months before the death of the giver. This means that individuals can use the yearly and the one-time gift exemptions to avoid taxes by apportioning their estate and giving it gradually to their heirs while still alive. For more details on gift and inheritance taxation in the Netherlands, see Sturrock *et al.* (2022) and Suari-Andreu *et al.* (2024).

3 Data

To estimate the effect of health shocks on inter-vivos giving, we employ Dutch administrative data provided by Statistics Netherlands (CBS). The datasets we employ come from different administrative sources but they are all provided to us by CBS. An encrypted social security number allows merging all the different datasets together. The data have yearly frequency and cover the full Dutch population for the period between 2007 and 2019, both years included.[5] They contain rich information on hospital intakes, inter-vivos transfers (IVTs), and demographic characteristics such as age, gender, and family composition.

We use the data on hospital intakes to construct our health shock measure and the data on IVTs to construct our dependent variable. Since our empirical strategy requires comparing individuals who suffer health shocks at different points in time, we select only individuals who suffer a health shock at some point during our period of observation. In addition, we select only individuals who are at least fifty years of age. That is because below that age the incidence of health shocks is significantly low and we are particularly interested in individuals who have adult children living outside of the parental household.

3.1 Health Shocks

To measure health shocks we follow García-Gómez *et al.* (2013), Rellstab *et al.* (2020), and Bonekamp and Wouterse (2023) and define a health shock as an unplanned hospitalisation that is not preceded by any other hospitalisation in the years previous. For the baseline analysis, we follow the literature and use a period of two years. We then test whether results change when increasing it to up to five years. To implement this definition, we use data on hospital intakes sourced to CBS by the hospital discharge register. For each hospital intake, the data contain information on the date, whether it was planned or not, and the main diagnosis associated

[4]The tax schedule was reformed in 2010. Before that, it was similar to the description we provide in the main text but had a more progressive structure. For details, see Suari-Andreu *et al.* (2024).

[5]Our period of analysis is determined by the availability of the data on IVTs.

Table 1: Health Shock Diagnoses ICD-10

Category	Frequency	Percentage
Infectious diseases	50,796	2.30%
Neoplasms	90,020	4.07%
Blood diseases	21,893	0.99%
Endocrine, nutritional and metabolic diseases	40,159	1.82%
Mental and behavioural disorders	18,994	0.86%
Diseases of the nervous system, eye and ear	79,330	3.59%
Diseases of the circulatory system	645,275	29.19%
Diseases of the respiratory system	183,090	8.28%
Diseases of the digestive system	222,911	10.09%
Diseases of the skin	20,018	0.91%
Diseases of the musculoskeletal system	45,848	2.07%
Diseases of the genitourinary system	82,736	3.74%
Ill-defined conditions	347,781	15.74%
Consequences of external causes	334,494	15.13%
Other	26,888	1.21%

Notes: Diagnoses are classified according to ICD-10 categories. For further information, see WHO (2016).

Table 2: Background Characteristics Before Health Shock

Female	50.42%
Age	
50-59	23.51%
60-69	26.12%
70-79	24.90%
80-89	20.75%
90+	4.72%
Household structure	
One-person household	30.57%
Couple without children	48.15%
Couple with children	13.99%
Single parent	3.32%
Other	0.42%
Institutionalised household	3.56%
Household size	1.96
Presence of children inside the household	17.31%
Number of children inside the household	0.26
Presence of children outside the household	76.59%
Number of children outside the household	1.79

Notes: All statistics are provided the year before the health shock. All statistics are provided in percentages except those for household size and the number of children. Given that the latter are continuous variables, we provide the average for the year before the shock.

with the intake. Diagnoses are classified according to the 10th revision of the International Statistical Classification of Diseases and Related Health Problems (ICD-10), assembled by the World Health Organisation (WHO, 2016).

As mentioned above, out of the whole Dutch population we select only individuals who are fifty or older and who suffer a health shock at some point between 2007 and 2019. These selection criteria leave us with a total of 2,210,233 individuals that we follow over time. We observe individuals for the first time in 2007 if by then they are fifty years of age or older. Otherwise they enter our selection the year they become fifty if that is 2019 or earlier. Importantly, when individuals suffer more than one health shock over the period we observe, we consider only the first shock. The only reasons why we stop observing individuals are death and outmigration. Since CBS also provides data from the death register, we also have access to information on date and cause of death.

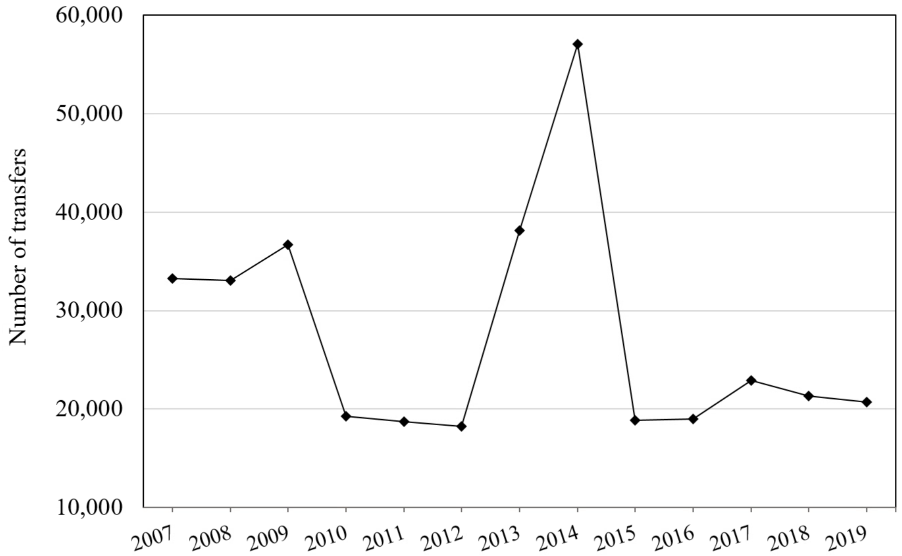
Table 1 shows how the health shocks we observe are distributed across the aggregate ICD-10 categories. Besides the ill-defined conditions category, the most frequent ones are diseases of the circulatory system (29.19%), which refers to cardiovascular diseases, consequences of external causes (15.13%), which refers to accidents and violence, and diseases of the digestive system (10.09%). Table 2 provides the background characteristics of individuals who suffer a health shock as measured on the year before the shock. It shows that males and females are similarly likely to suffer a health shocks, and that slightly more than half of the shocks occur at ages above 70. Interestingly for ages above 60, we see that there are less health shocks as age increases. That is because, due to death, our selection includes relatively less observations in the older age categories. Furthermore, Table 2 also shows that the most common household structure in our selection is couple without children, but that most individuals have children outside of the households (slightly more than 75%). There is also a non-negligible share of one-person households, *i.e.* close to one third of the total selection.

3.2 Inter-Vivos Transfers

To measure IVTs we use data sourced to CBS by the tax authorities. The latter collect information on private gifts in the form of wealth transfers between individuals. Everyone is obliged to report these transfers in the yearly tax statement and commercial banks report to the tax authorities. For each transfer, the data contain information on the relationship between the giver and the receiver, the transferred amount, the year of the transfer, and whether a transfer made use of any of the existing tax exemptions. It is important to note that we do not observe most transfers made under the yearly 5,000 tax exemption threshold.

During the period of study, we observe a total of 357,144 transfers. Out of these, 88.93% are from parents to children. Figure 1 shows that these transfers are not evenly distributed over time and that they clearly respond to changes in the gift and inheritance tax schedule. More specifically, there was a change in the tax schedule in 2010 that made it less progressive and increased the tax rate for the average giver. In addition, as we mention in Section 2.4, a one-off

Figure 1: Frequency of IVTs over Time



Notes: For each year, the vertical axis provides the total number of IVTs in the subpopulation selected for this study.

exemption was introduced in October 2013 for transfers up to 100 thousand Euro as long as they are destined to the purchase of a home. This was first introduced as a temporary measure (just between October 2013 and December 2014) and then as a permanent measure in 2017.

For the empirical analysis, we create a dummy that takes value one if an individual gives at least once in a given year. Table 3 shows the percentage of observations for which this dummy equals one when pooling all individual-year observations together. It does so separately by age, gender, and marital status groups. Table 3 shows that the percentage of individuals who give in a particular year is rather low. That is because even if, when aggregated across individuals, transfers are significant in their frequency and volume, they are still a rare happening for the average individual in a given year. Therefore, the probability that a particular individual gives in a particular year is always small. Furthermore, Table 3 also shows that, within our selection, males are more likely to give than females. Within the female group, singles are more likely to give than married individuals while it is the other way around for males. Importantly, we see that the probability of giving significantly increases with age. Since age correlates with the dependent variable and the main explanatory variable, it is important to take it into account in the empirical strategy, as we explain further in Section 4.

Finally, Table 4 shows the distribution of yearly transferred amounts by gender and marital status when pooling together all selected individual-year observations. These are amounts conditional on giving and aggregated by individual and year. This means that if an individual gives more than one IVT in a particular year, we add the amounts of all IVTs given by that individual within that year. As expected, the distributions are positively skewed with medians around 50 to 70 thousand Euro and means around 100 to 130 thousand Euro. Table 4 shows as

Table 3: Inter-Vivos Transfers by Gender, Marital Status, and Age

	Age groups				
	50-59	60-69	70-79	80-89	90+
Female single	0.17%	0.59%	0.98%	1.37%	1.42%
Female couple	0.08%	0.14%	0.21%	0.53%	1.06%
Male single	0.13%	0.50%	0.94%	1.58%	2.11%
Male couple	0.42%	1.18%	1.23%	1.59%	2.22%

Notes: Percentages of individuals who give are calculated pooling together all selected individual-year observations.

Table 4: Yearly Transferred Amounts by Gender and Marital Status

	Mean	p5	p25	p50	p75	p90	p95
Female single	99	10	32	68	114	209	280
Female couple	95	10	26	52	100	200	254
Male single	106	10	28	60	110	203	280
Male couple	133	12	27	53	103	210	306

Notes: Amounts are calculated pooling together all selected individual-year observations. They are conditional on giving and they are provided in thousands of Euro.

well that on average males tend to give larger IVTs than females. Among females, singles give larger IVTs than married individuals, while among males this is true around the median but not on average.

It is very important to note here that the IVTs used to construct Tables 3 and 4 are assigned to a particular individual who is registered as the giver of the transfer. However, the decision to give may be made at the household level. In our baseline analysis we use data at the individual level, *i.e.* we estimate the effect of receiving a negative health shock on the probability that the individual who receives the shock engages in inter-vivos giving. However, to account that for the possibility that the giving decision is made at the household level, we re-estimate our baseline analysis considering transfers at the household level. That is, we estimate the effect of receiving a negative health shock on the probability that anyone in the household of the individual that receives the shock engages in IVTs.

4 Empirical Strategy

4.1 Event Study Specification

Estimating the effect of health shocks on IVTs implies two very important empirical challenges. First, individuals usually have knowledge about their health status and about the health consequences of health behaviour. Therefore, they can form expectations about their future health path and already adjust their economic behaviour before a change in health is observed in the data. For this reason, and as explained in Section 3.1, we use a measure of health shocks that aims to rule out anticipation effects. The second is that individuals who suffer health shocks are potentially different from those who do not. Most importantly, some of these differences are likely to be along dimensions that are unobserved.

To deal with these empirical issues, we employ an event study design. We do so by setting up the following regression equation:

$$IVT_{ist} = \alpha + \sum_{j \neq -1} \beta_j \mathbf{I}\left[j = t\right] + \sum_{k} \gamma_k \mathbf{I}\left[k = age_{ist}\right] + \sum_{y} \delta_y \mathbf{I}\left[y = s\right] + \epsilon_{ist}, \tag{1}$$

where IVT_{ist} is a dummy indicating whether individual i gives at least one IVT in calendar year s and at a distance t (in years) from the shock. We also use as dependent variables the number of transfers per year and the total amount given per year. The first term to the right of the constant term is a full set of time-to-event dummies, the second term is a full set of age dummies (same categories as in Tables 2 and 3), while the third term is a full set of calendar year dummies. Finally, ϵ_{ist} denotes the error term.

The set of time-to-event dummies contains leads and lags of the health shock ranging from five years before to five years after the shock. We omit the time-to-event dummy at $t = -1$, meaning that the event time coefficients measure the impact of health shocks relative to the year just before the health shock occurs. This strategy allows comparing individuals who suffer a health shock, for up to five years after the shock, with similar individuals who have not suffered it yet. It is important to control for age since, as explained in Section 3 the prevalence of both health shocks and IVTs correlate with age. In addition, it is also important to include year dummies in the specification, since there are potentially important time effects as observed in Figure 1. For robustness, we also estimate an extended version of Equation 1 that includes gender, marital status, and household structure (measured the year before the shock) as control variables.

4.2 Identifying Assumptions

The identification of the effect via the estimation of Equation 1 hinges on the shock being unexpected and on the timing of the shock being random over the time window considered. Conditional on these assumptions, individuals who have not suffered a shock yet can be used as a counterfactual for individuals who have already suffered it. Kleven *et al.* (2019) use this same method to analyse the employment effects of having children. An important advantage of our study is that, compared to the birth of a child, sudden changes in health are more likely to be unexpected and their exact timing is more likely to be random. Nevertheless, there are three aspects regarding the assumptions of the model that require some discussion.

First, the health shock measure described in Section 3.1 may not totally rule out anticipation effects. That is because, even if the hospitalisations we use are unplanned and not preceded by any other hospitalisation in the two years previous, individuals are likely to have previous knowledge about their health that could lead to anticipation effects. For that reason, we re-estimate our baseline analysis adjusting the health shock definition to windows of three, four, and five years without any hospitalisation before the shock occurs. A drawback of extending the time window in the shock definition is that for every extra year we lose one year of data in our analysis. That is because the data on hospital admissions is only available from 2005 onwards.

Second, it is relevant to note that the specification in Equation 1 is the absence of fixed effects at the individual level. A static two-way fixed effects model would include both calendar-year and individual fixed effects. However, in the dynamic specification, *i.e.* a specification including a full set of time-to-event dummies, one cannot include two-way fixed effects since the simultaneous inclusion of time-to-event dummies, individual fixed effects, and calendar year dummies creates a collinearity problem. Following Kleven *et al.* (2019) we solve this problem by excluding the fixed effects at the individual level and relying on the random timing assumption. To the extent that this assumption holds, individuals who suffer a shock at different points in time will be on average comparable to each other.

Third, next to no anticipation effects and random timing, an additional important assumption for identification of the main effect in Equation 1 is that of homogeneity of the treatment effect. This requires somewhat more discussion here since this is an assumption that empirical studies using event study designs often impose implicitly without an *ex ante* justification. In recent years, a growing methodological literature highlights the potential consequences of this assumption for the estimation of causal effects using event studies. Rather than summarizing this literature here, we focus on the recent contribution by Borusyak *et al.* (2024). We rely on it to discuss the implications of the homogeneity assumption for the method and data we employ in the present study.[6]

Technically speaking, because of the dynamic specification, Equation 1 does not assume homogeneity by time since treatment. This is different from a more restrictive static two-way fixed effect model that would assume all treatment effects to be the same regardless of the time to event. As explained by Borusyak *et al.* (2024), if the homogeneity assumption does not hold in the static model, there will be a bias in the estimation due to the so-called "forbidden comparisons". These are comparisons between treated individuals and individuals who have

[6] Contributions to this literature include De Chaisemartin and d'Haultfoeuille (2020), Sun and Abraham (2021), Goodman-Bacon (2021), Callaway and Sant'Anna (2021), Roth (2022), Rambachan and Roth (2023), Roth and Sant'Anna (2023), Schmidheiny and Siegloch (2023), and De Chaisemartin and d'Haultfoeuille (2024) among others.

been already treated earlier. In this study we solve this problem by including the time-to-event dummies in the specification thus making the analysis dynamic instead of static. A consequence of that is that we cannot include individual fixed effects in the specification due to the collinearity problem mentioned above. Instead of using individual fixed effects we rely on the random timing assumption to be able to make comparisons across individuals.

However, Equation 1 does assume homogeneity across individuals and calendar years. As explained by Borusyak *et al.* (2024) this can lead to spurious identification of dynamic effects. That is because, in the absence of never-treated observations, the number of individuals who are not treated (and thus can serve as a comparison group) decreases over time and may eventually become zero. If the estimation of the dynamic effects relies on calendar years with no untreated individuals, the estimator then generates a comparison group by extrapolation of treatment effects across observations. This extrapolation relies on imposing homogeneity of treatment effects across individuals and will lead to a bias when this homogeneity does not hold.

Even if it is important to consider it, this bias related with the homogeneity assumption in dynamic models is unlikely to be a problem for our estimation. That is because we observe a large number of individuals over a large period of time (thirteen years) while we estimate dynamic effects for a ten year moving window. This means we do not have any particular time-to-event dummy that is estimated using only calendar years in which there is no untreated individuals.

Figure A.1 in Appendix A shows that for the estimation of each time-to-event dummy we rely on at least eight years of data. It also shows that, by construction, in the year 2007 we only observe individuals who are either just treated or not treated yet, while in the year 2019 we only observe individuals who have already been treated. This means also that for the year 2019 we have no control group. Even if our analysis is unlikely to suffer a bias due to the homogeneity assumption, for robustness and completeness we re-estimate our baseline result using an imputation estimator proposed by Borusyak *et al.* (2024). This is an estimator for the time-to-event dummies that does not rely on the homogeneity assumption. In addition, as explained further below in this section, we check for heterogeneity of our baseline results over a range of relevant background variables.

4.3 Alternative Methods

The existing literature on the effect of health shocks has applied strategies that are similar to the one we consider here. For instance, García-Gómez *et al.* (2013) and Rellstab *et al.* (2020) apply a difference-in-difference approach combined with propensity score matching. García-Gómez *et al.* (2013) use this method to study the effect of health shocks on income, while Rellstab *et al.* (2020) use it to study the effect of parental health shocks on the children's labour supply. The matching is used to construct a treatment group of individuals who suffer health shocks and a comparable control group of individuals who do not.

An advantage of this method is that the control group is formed by individuals who are never treated. This prevents the above-mentioned problem of extrapolation related with the homogeneity assumption. However, an important disadvantage of this method is that the matching is based on observable characteristics. Therefore any unobserved variable that simultaneously affects both health shocks and IVTs will generate a bias. For instance, individuals who suffer a health shock is a selection of individuals who may have poorer health in general, which can in different unobserved ways affect their capacity to accumulate wealth and to engage in IVTs.

Next to this method, Fadlon and Nielsen (2019), Kvaerner (2023), and Bonekamp and Wouterse (2023) apply a different adaptation of the difference-in-difference estimator. They do so by comparing individuals who suffer a health shock with a control group of individuals who suffer it a set number of years into the future. This is similar to our method. It has the advantage of assuming that the timing of the shock is random over a shorter period of time. However, this implies the choice a fixed time frame. That is, a rather arbitrarily set window of time between the shock in the treatment group and in the control group. In doing so, this method exploits fewer variation compared to the method we use in the present study.

For instance, in applying this method Kvaerner (2023) uses only health shocks that occurred in 2013. He then examines whether they lead to the giving of IVTs in the following year, while using individuals who suffer a shock in 2015 as the control group. To make our results more reliable and generalisable, we exploit much larger variation by choosing to apply the method described in Section 4.1 based on Kleven *et al.* (2019). This method uses time-to-event dummies on a panel of individuals who experience a shock at different times within the period between 2007 and 2019.

4.4 Baseline Extensions

Our baseline analysis consists of the estimation of Equation 1 under the assumptions explained in Section 4.2. We then expand our analysis in a number of ways. First of all, we start by checking the heterogeneity of our baseline results by age, gender, marital status, and wealth and income of the giver. For that purpose, all variables that could be affected by the health shock (marital status, wealth, and income) are measured on the year of the shock.

We then follow Bonekamp and Wouterse (2023) and classify health shocks by diagnosis (*i.e.*, using the diagnoses listed in Table 1) according to the extent to which they lead to physical disability and/or increased mortality. We do so by following their classification, which we compare with our own estimations of the effect of health shocks on mortality and disability. For our own estimations, we rely on the same Equation 1 but using death and nursing home entry as dependent variables respectively. We do this because if we find that disabling shocks are more likely to lead to giving, then the exchange motive is the likely explanation. Conversely, if it is the more lethal shocks have a stronger effect on giving, then the altruistic and/or the warm-glow motive are likely more important.

To further investigate the relative importance of the exchange motive for giving, we re-estimate our baseline regression accounting for the geographical proximity between parents and children and also separately for individuals with and without daughters. As explained in the introduction, we do this because the literature predicts that if parental giving is directed towards children that live close and/or to daughters instead of sons, then the observed transfers are likely to be the result of the exchange motive.

5 Results

5.1 Baseline

Figure 2 shows the results of our baseline analysis. More specifically, it shows the estimates we obtain for the time-to-event dummies on the probability of giving at least one . Interestingly, the estimates for the years before the shock (*i.e.* $t = -5$ to $t = -2$) are not significantly different from zero, indicating the absence of anticipation effects.[7] The year in which the shock takes place, *i.e.* $t = 0$, the estimated effect is not significantly different from zero. That is likely because the shocks we observe can take place at any time within a year. Therefore, it can be that individuals do not have the time to react within the same calendar year. We then see how the estimates become clearly positive for the years $t = 1$ and $t = 2$. At $t = 1$ there is a 11.37% increase in the probability of giving compared to the year before the shock, while at $t = 2$ there is an increase of 8.94%.[8] Both coefficients are statistically significant at the 0.1% level. At $t = 3$ the effect is still statistically significant but clearly smaller (increase of 3.59% with respect to $t = -1$) and then it becomes statistically indistinguishable from zero again for the years $t = 4$ and $t = 5$. These results show a clearly positive and significant effect of receiving a health shock on giving via IVTs that peaks in the year after the shock. They are in line with those by Kvaerner (2023), since he also finds a positive effect of health shocks on the probability of giving.[9]

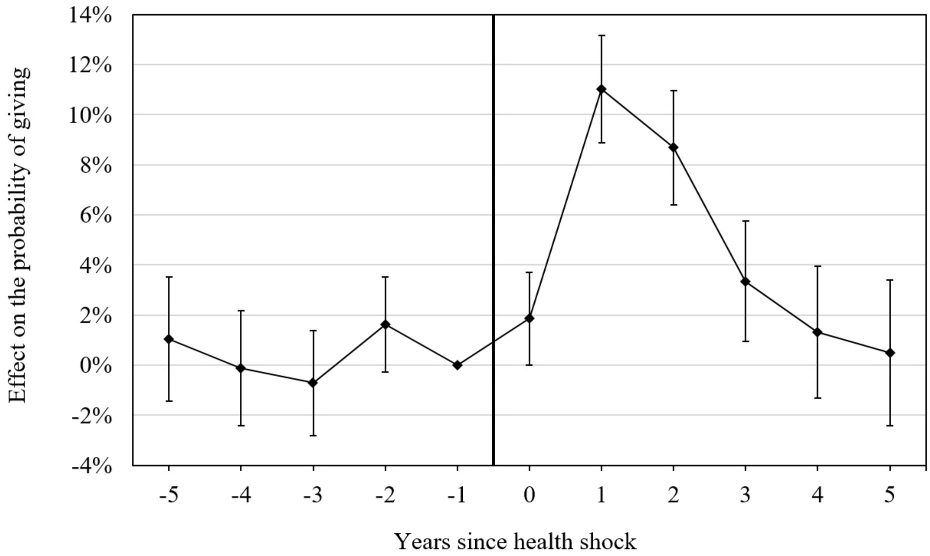
Figures 3 and 4 show the results we obtain when looking at the intensive margin of giving, *i.e.* using the yearly number of IVTs given and the total yearly amount given in Euros as dependent variable respectively. That is, without separating the extensive and the intensive margins of giving. The results we find are in both cases not significantly different from those in Figure 2. In both cases, the effect at $t = 1$ goes up to 14% but the patterns drawn by the time-to-event estimates are indistinguishable from that in Figure 2. Figures B.1 and B.2 in B show the results we obtain when conducting the same analysis but making it conditional on giving, *i.e.* only using individual-year observations for which the IVTs dummy is one. Figure B.1 shows that there is an effect of health shocks on the number of transfers per year. However

[7] If there were anticipation effects, the estimates for $t - 5$ to $t - 2$ would be negative and approaching zero the closer to the reference year $t - 1$.

[8] We provide results as percentage increase with respect to the value of the dependent variable at $t = -1$. We do this because, as show in Table 3, the baseline probability of giving is very small for reasons explained in Section 3.2, which also means that the estimated coefficients are also small in absolute value. Among all individual-year observations used for the regression in Figure 2, 0.79% have a value one for the dependent variable at $t = -1$.

[9] Note however that he applies a difference-in-difference estimator using health shocks occurred only in 2013. Health shocks occurred in 2015 are used as a control group. His health shock measure is built using data on diagnoses but does not consider the previous hospital history of the individual.

Figure 2: Effect of Health Shocks on Probability of Inter-Vivos Giving



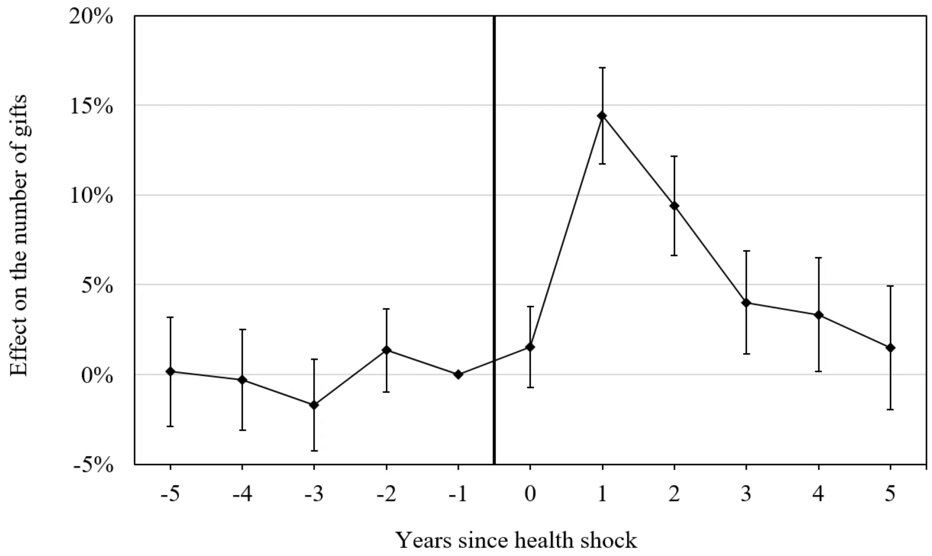
Notes: Effects are estimated using the year previous to the health shock as a reference category. They are provided as a percentage change with respect to the average in that year. The regression equation includes a set of age dummies, a set of year dummies, gender, and family structure as control variables. Point estimates are surrounded by 95% confidence intervals. Standard errors are clustered at the individual level. The estimation is based on 2,210,233 individuals and 17,123,310 individual-year observations.

the effect is small and not clearly significantly different from zero. At $t = 1$ for instance the effect is statistically significant but the estimated increase in the number of transfers is just above 3%, which is very small.[10] Figure B.2 shows that effects are even less statistically significant when using the amount given as a dependent variable. In that case, only the dummy at $t = 1$ is significant but the point estimate is again very small, *i.e.* just over 2%.[11] These results strongly suggest that the baseline effects we estimate are a reflection of the extensive margin rather than the intensive margin. This indicates that individuals do not usually give IVTs, but then they are more likely to do so after suffering a negative health shock. That is instead of a situation in which individuals would usually give IVTs and then increase their intensive margin of giving after receiving a health shock.

As mentioned in Section 3.2, in our data IVTs are assigned to a particular individual who is registered as the giver of the transfer. However, the decision to give may be made at the household level. To account for this, we re-estimate our baseline analysis considering transfers at the household level. That is, using as a dependent variable a dummy that takes value one if anyone within the household engages in IVTs. Figure B.3 in Appendix B shows that when we do this we find stronger effects compared to Figure 2 (the estimates for $t = 1$ and $t = 2$ are 12.76% and 10.50% respectively) which are also more precisely estimated. However, the results are not significantly different from those in Figure 2. Important to note is as well that all results

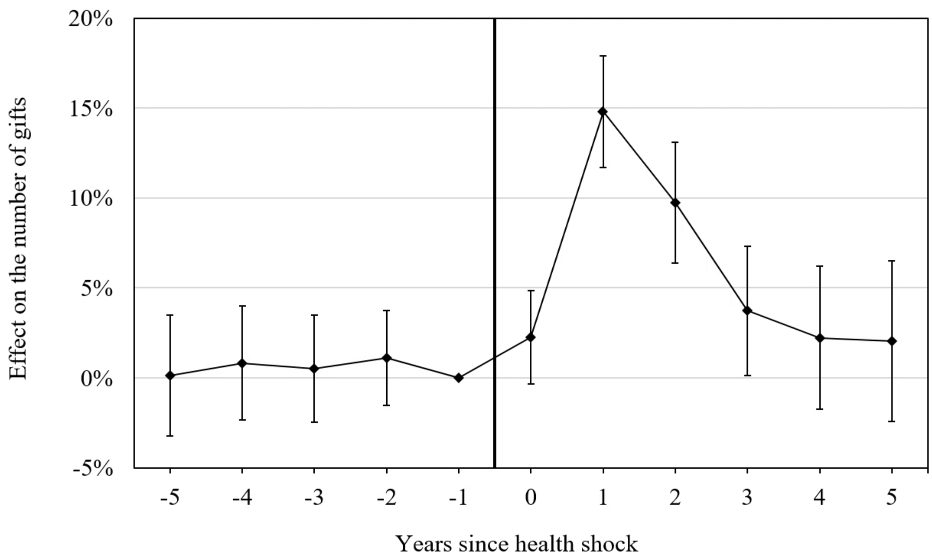
[10]The average number of transfers at $t = -1$ is 1.92.
[11]The average amount transferred at $t = -1$ is 89,161 Euro.

Figure 3: Effect of Health Shocks on Number of Gifts



Notes: Effects are estimated using the year previous to the health shock as a reference category. They are provided as a percentage increase with respect to the average in that year. The regression equation includes a set of age dummies, a set of year dummies, gender, and family structure as control variables. Point estimates are surrounded by 95% confidence intervals. Standard errors are clustered at the individual level. The estimation is based on 2,210,233 individuals and 17,123,310 individual-year observations.

Figure 4: Effect of Health Shocks on Amount Given



Notes: Effects are estimated using the year previous to the health shock as a reference category. They are provided as a percentage increase with respect to the average in that year. The regression equation includes a set of age dummies, a set of year dummies, gender, and family structure as control variables. Point estimates are surrounded by 95% confidence intervals. Standard errors are clustered at the individual level. The estimation is based on 2,210,233 individuals and 17,123,310 individual-year observations.

we mention in this section do not significantly change when the control variables mentioned in Section 4.1 (gender, marital status, and household structure) are included in the equation. Results do not change either when we include the diagnosis associated with the health shock as a control variable. Furthermore, the baseline result also holds when performing placebo tests that assume the shock to take place at $t = -1$ and $t = -2$ instead of at $t = 0$.[12]

As mentioned in Section 4.2, in our baseline estimation we do not include individual fixed effects in (see Equation 1) to avoid a collinearity problem (thus relying on the random timing of the shock to make comparisons across individuals) and we rely on the assumption of homogeneity across individuals and calendar years. To test these assumptions, we first re-estimate our baseline analysis including individual fixed effects, but then dropping two time-to-event dummies from the estimation to avoid the collinearity problem. Then we also re-estimate the baseline analysis again using the imputation method by Borusyak *et al.* (2024), which does not rely on the assumption of homogeneity of the treatment effect. These two additional analyses are provided in Figures B.6 and B.7 in Appendix B.

Figure B.6 shows that the results we obtain when including fixed effects in the specification are not fundamentally different from those in Figure 2. The point estimates are of similar magnitude and are not significantly different from those in Figure 2. We do see that the estimates are less precise, leading to substantially wider confidence intervals. This has to do with the fact that we are using substantially less variation in this estimation, *i.e.* we are comparing individuals only with themselves in the two years previous to the shock and thus ruling out comparisons across individuals.

Figure B.7 provides the results of the imputation method proposed by Borusyak *et al.* (2024). This method uses untreated observations to impute a counterfactual for treated observations. It then estimates an effect based on comparisons between the treated observations and the counterfactual. The imputation model includes both year and individual fixed effects. In the estimation of the time-to-event dummies ranging from $t = 0$ to $t = 5$ all the years previous to the shock are used as a reference category. The estimates for these variables show a pattern very similar to that in Figure 2, indicating that the baseline effects are not affected by the homogeneity assumption. As we explain in Section 4.2, this assumption is unlikely to cause a problem in our analysis because none of the time-to-event dummies is estimated using only calendar years without untreated individuals.[13]

Next to this analysis, the method proposed by Borusyak *et al.* (2024) also offers the option of estimating the pre-trend coefficients, *i.e.* the coefficients for the $t < 0$ dummies. We provide these estimates as well in Figure B.7.[14] They are estimated using two years as a reference category (*i.e.*, $t = -5$ and $t = -4$) because of the inclusion of fixed effects in the estimation. The estimates we obtain for the time-to-event dummies ranging from $t = -3$ to $t = -1$ appear

[12]See Figures B.4 and B.5 in Appendix B. These figures show how when we treat $t = -1$ and $t = -2$ as shock years, the effect still does not peak until $t = 1$.

[13]We show this in Appendix A.

[14]Note that this means that Figure B.7 provides the results of two separate analysis. We provide both in the same figure for convenience.

to be not significantly different from zero. Note that the standard errors are large since this analysis uses substantially less variation.[15]

5.2 Heterogeneous Effects

We begin the heterogeneity analysis by re-estimating our baseline result for different values of the intensity of the health shock. As explained in Sections 3.1 and 4.2, the operationalization we borrow from the literature to measure health shocks takes anticipation effects into account. It does so by defining shocks as unscheduled hospitalisations not preceded by any hospitalisation in the two years previous. Since this may not totally rule out anticipation effects, we re-estimate our baseline analysis adjusting the health shock definition to windows of three, four, and five years without any hospitalisation before the health shock occurs.

Enlarging this time window means that the shock is likely to be unexpected to a larger extent and thus more intense. Each time we enlarge the time window by one year we are looking at an increasingly smaller sub-sample of shocks compared to the baseline analysis. In addition, as explained in Section 4.2, expanding this window also implies losing observations since we only have data on hospitalisations from 2005 onwards. The data on IVTs are available from 2007, therefore expanding the time window above two years means losing a year of data for each year added to it.

Figure 5 does show that the effect reported in Figure 2 increases with the intensity of the shock. The different lines in the figure show the estimates we obtain for the time-to-event dummies using definitions of the shock based on time windows of two to five years. The line using the two year window is the same as that one reported in Figure 2. For the years before the shock, we see that all estimates are close to zero and they are not significantly different among them. After the shock, we see that all estimations follow pattern similar to that in Figure 2.

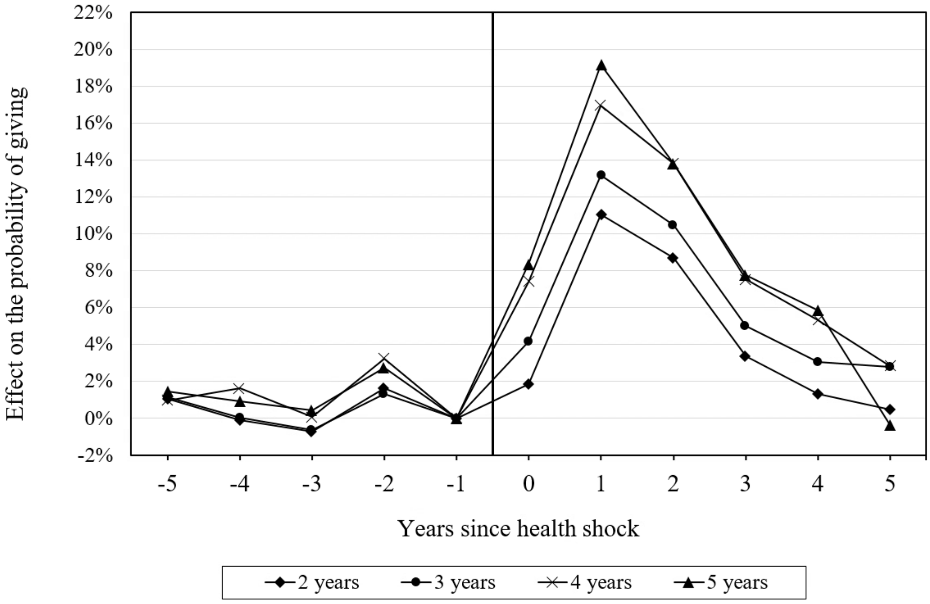
Interestingly, the size of the effect increases with the intensity of the shock; *i.e.*, the larger the time window we use to define the shock, the greater the observed effect. This pattern is evident at $t = 0$ and becomes even more pronounced at $t = 1$. The estimates are significantly different from each other at $t = 1$. For the estimation with the largest time window (five years) the effect peaks at a magnitude of just above 19.48%.[16] These results indicate that our approach to empirically measure health shocks does meaningfully capture expectations about future health status. However, as already mentioned, increasing the shock intensity implies losing years of data. In addition, selecting the size of the window still remains an arbitrary choice. Therefore, for most of the analysis we follow the literature and stick to a two year window.

We continue the heterogeneity analysis by re-estimating our baseline results by different values of relevant demographic variables, *i.e.* age, gender, and marital status. Figure 6 shows

[15] The inclusion of fixed effects mean that only variation within individuals is used. In addition, as shown in Figure A.1, not all individuals are observed four and five years previous to the event.

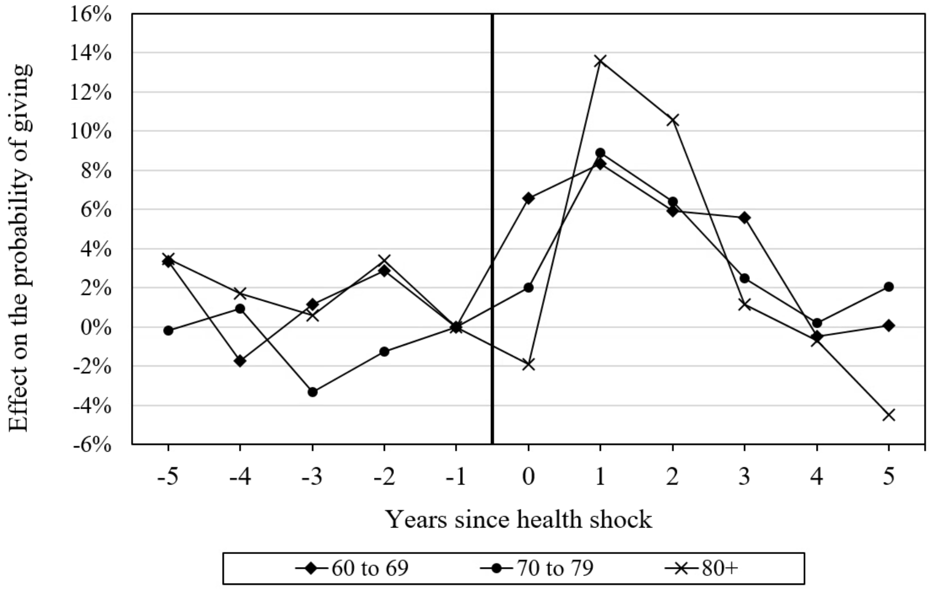
[16] We do not report confidence intervals in Figure 5 to avoid cluttering. Significance levels are similar to those in the baseline analysis for all estimations reported in Figure 5. At $t = 1$ and $t = 2$ the estimate obtained with the five-year window is significantly different in statistical sense when compared to those obtained with the two- and three-year window definitions.

Figure 5: Effect on the Probability of Inter-Vivos Giving by Shock Intensity



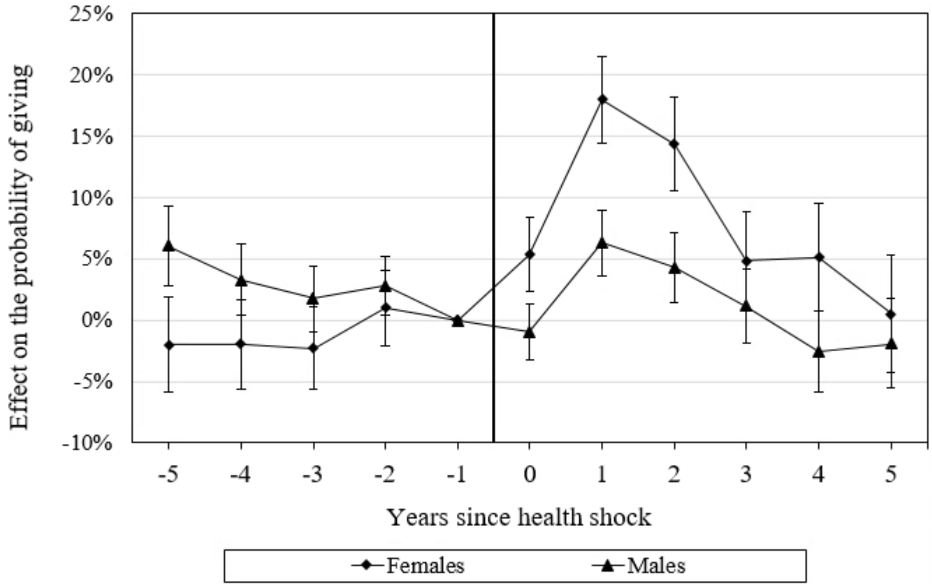
Notes: Effects are estimated in the same way as in Figure 2 but for four different regressions. Each of them using different time windows to determine the period before the health shock that is set to be free of hospitalisations. Confidence intervals are not provided in the figure to avoid cluttering. For more information, see main text.

Figure 6: Effect of Health Shocks on Probability of Inter-Vivos Giving
- Heterogeneity by Age -



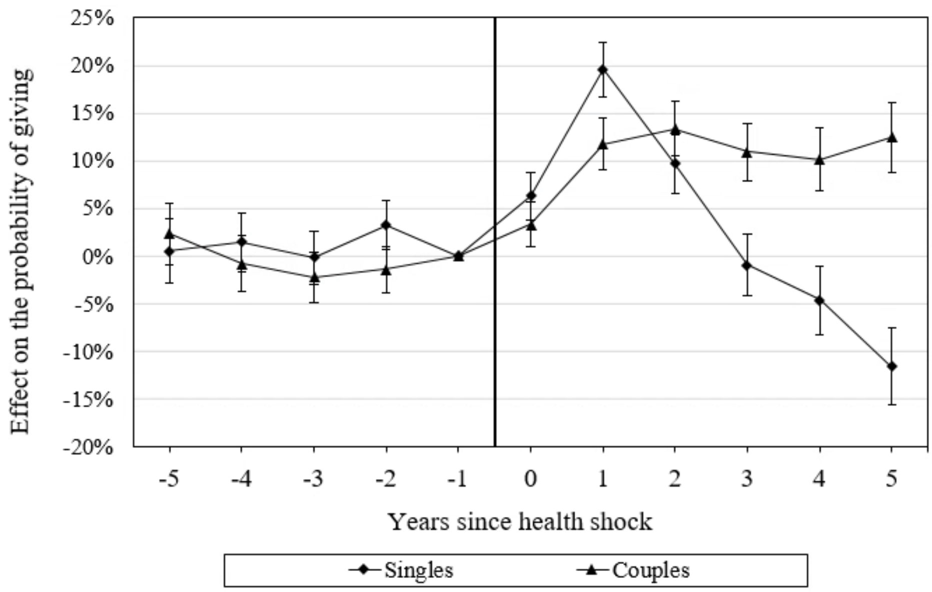
Notes: Effects are estimated in the same way as in Figure 2 but for three different regressions. Each of them using different age selection. Confidence intervals are not provided in the figure to avoid cluttering. For more information, see main text.

Figure 7: Effect of Health Shocks on Probability of Inter-Vivos Giving
- Heterogeneity by Gender -



Notes: Effects are estimated in the same way as in Figure 2, but using two different regressions. Each of them corresponding to a different gender subgroup. Point estimates are surrounded by 95% confidence intervals. For more information, see main text.

Figure 8: Effect of Health Shocks on Probability of Inter-Vivos Giving
- Heterogeneity by Marital Status -



Notes: Effects are estimated in the same way as in Figure 2, but using two different regressions. Each of them corresponding to a different marital status subgroup. Point estimates are surrounded by 95% confidence intervals. For more information, see main text.

the results we obtain when running separate regressions for different age groups.[17] The results in Figure 6 show that, for all three age groups provided (60 to 69, 70 to 79, and 80+), the coefficient estimates show a pattern largely similar to that one observed in Figure 2. For all three groups, the coefficients for the time-to-event dummies before $t = -1$ are not significantly different from zero. That is the case even though the point estimates for each group are further apart from each other than in Figure 5. For the years after the shock, the two younger groups follow a similar pattern that shows a peak at $t = 1$ that is slightly lower than that in Figure 2 (just over 8%). For the older group (80+) the estimate at $t = 1$ is 13.59%. It is significantly different in statistical sense compared with the estimates of the same time-to-event dummies for the two younger groups.

Figures 7 and 8 provide the baseline results split by gender and marital status. Regarding the results by gender, Figure 7 shows that the effect is significantly stronger for females compared to males. For females, the point estimate at $t = 1$ is just 18.61%, while for males it is significantly lower at 6.31%. Figure 8 shows a stronger effect for singles at $t = 1$ (19.58%). Interestingly, this difference fades out in the subsequent years. That is, the effect for singles quickly drops while that for couples remains stable between 10% and 15%. For the interpretation of these results it is important to note that marital status is measured at $t = -1$. However, it can change at any point before or after $t = -1$. If we re-estimate the results in Figure 8 using only individuals who do not experience a change in marital status, we find an effect for singles that is smaller at $t = 1$ than that reported in Figure 8. It becomes not significantly different from zero after that.[18] While for couples we find that the effect is not significantly different from zero for any of the time-to-event dummies. The stronger effect for singles may partially explain the stronger effect for females, since the latter are more often single at older ages. Finding a stronger effect for singles is in line with the bequest literature, since the latter shows that individuals in a couple tend to bequeath wealth to the spouse while it is singles that tend to bequeath to children (*e.g.* Hurd and Smith, 2002; Van Ooijen *et al.*, 2015; Alonso-García *et al.*, 2022).

5.3 Mortality-Increasing vs Disabling Health Shocks

As a first step to investigate the relative importance of the three giving motives mentioned in the introduction (*i.e.* the altruistic, the exchange, and the warm-glow motives) we classify the health shocks we observe according to the extent to which they lead to increased mortality and/or physical disability. Following the previous work by Bonekamp and Wouterse (2023), we do so using the data on diagnoses associated to the health shocks (see Table 1). The idea behind this distinction is that if disabling shocks are more important for explaining the result we find

[17] Estimates for the age group from 50 to 59 do not follow a clear pattern and are all not significantly different from zero. For this reason, and to minimize the amount of information within Figure 6, results for this age group are not provided. The lack of significance is likely due to the lower variation in the IVT and the health shock variables among this age group. If we join this group with the 60 to 69 group, the results are very similar to those obtained when using the 60 to 69 group alone.

[18] It is important to note here that we do observe quite a few transitions in marital status in the data. Mostly from married to widowed. After selecting only individuals who do not experience a transition in marital status, roughly half of the observations used in the baseline remain in the analysis.

in Figure 2, then the exchange motive is likely to be important. If mortality-increasing shocks are more important in explaining that result, then the explanation is more likely to be one of altruism and/or warm-glow triggered by the awareness of an increased probability of death.[19] [20]

To classify health shocks according to their effect on mortality and disability, Bonekamp and Wouterse (2023) use Dutch administrative and cross-sectional survey data to estimate how particular diagnoses are associated with death and with self-reported limitations in activities of daily living. Following the result of their analysis, they choose four different diagnosis across the mortality-disability spectrum. These are neoplasms (very mortality-increasing but below median disabling effect), mental and behavioural disorders (very mortality-increasing and very disabling), diseases of the musculoskeletal system (not mortality-increasing and relatively disabling compared to other non-mortality-increasing diagnoses), and consequences of external causes (median level for both mortality-increasing and disabling effects).[21] They validate their choice of diagnoses by also estimating their effect on demand for home care using Dutch panel survey data.

For our analysis, we follow the classification by Bonekamp and Wouterse (2023), which we also crosscheck with our own data. For that purpose, we start by estimating the average effect of health shocks on death and nursing home entry. In the data provided by CBS, we observe whether an individual lives in a nursing home. We use this as a proxy for disability.[22] Examining the effect of health shocks on death is additionally relevant since increased mortality could potentially explain why the effect estimated in Figure 2 phases out after $t = 3$.

Figure 9 shows the results we obtain when estimating Equation 1 using as a dependent variable a dummy that takes value one if an individual dies in a particular year. By selection, individuals do not die before the health shock, therefore the estimated effects for all time-to-event dummies at $t < 0$ are zero. In this case we do not calculate percentage changes thus the estimates are measured in percentage points. We do not provide confidence intervals since all estimated effects are extremely significant and thus the confidence intervals are so narrow they overlap with the point estimates in the figure. Important to note for the interpretation of this figure is that among all individuals we observe, 28.42% of them die at some point between health shock and $t = 5$. At the year of the shock we see that 10.45% of individuals die. Out of those who are still alive at $t = 1$, about 6.94% die on that year. From $t = 2$ onward the percentage of surviving individuals who die at particular time-to-event stabilizes at just under 6%. These

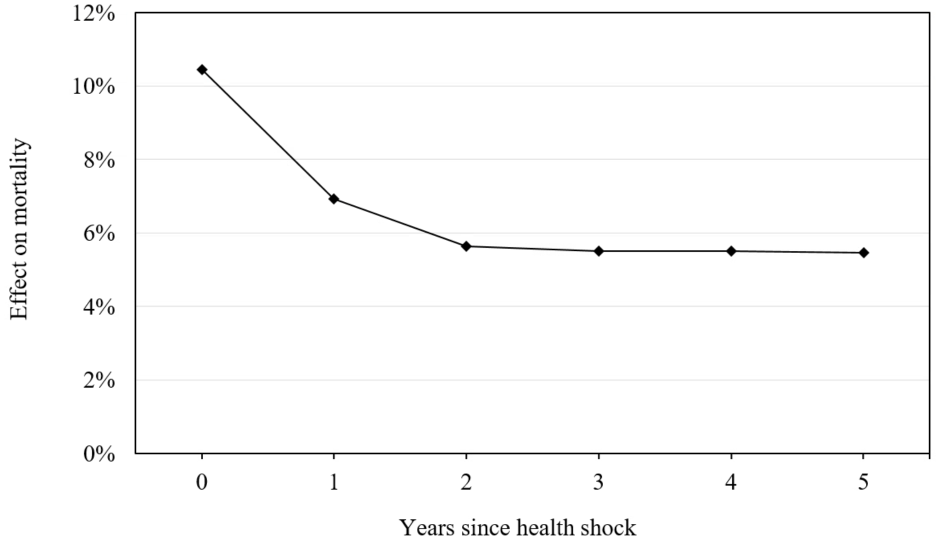
[19] The underlying theoretical mechanism here is that increased mortality leads to a revised saving and consumption path that will likely lead to inter-vivos transfers if individuals have a preference for giving relative to other possible uses of wealth. For exposition and discussion of this theoretical mechanism, see Hurd *et al.* (2011); Bonekamp and Wouterse (2023), and Kvaerner (2023).

[20] It can be of course that inheritance tax avoidance also plays a role, as explained by Suari-Andreu *et al.* (2024) among others. However, it is also true that the altruism and warm-glow motives are not incompatible with the tax motive.

[21] For a scatter plot showing the mortality and disabling effects estimated by Bonekamp and Wouterse (2023), see Figure 1 in Bonekamp and Wouterse (2023). We provide that figure as Figure C.1 in Appendix C.

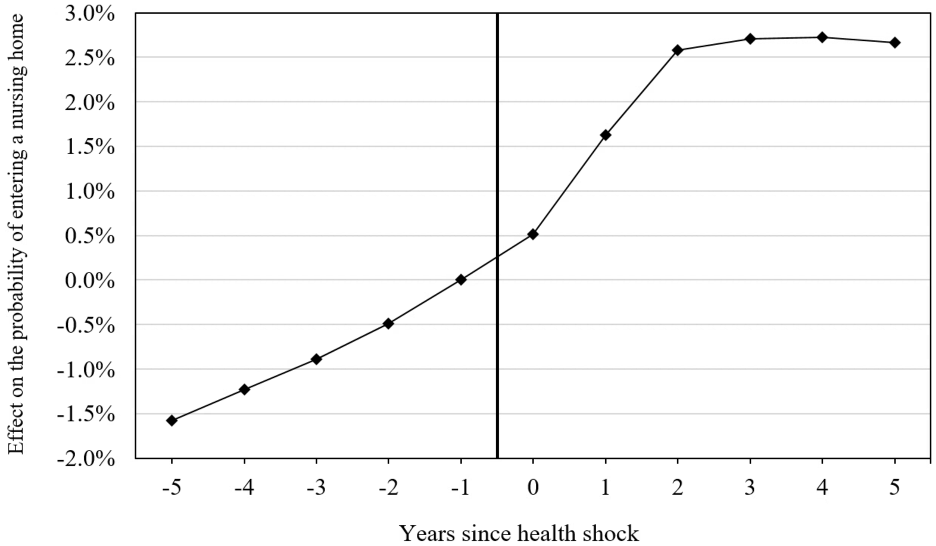
[22] For the role of physical and mental disability as a predictor of nursing home entry, see Headen Jr (1993), Fong *et al.* (2015), and Roquebert and Tenand (2024) among others.

Figure 9: Effect of Health Shocks on Probability of Death



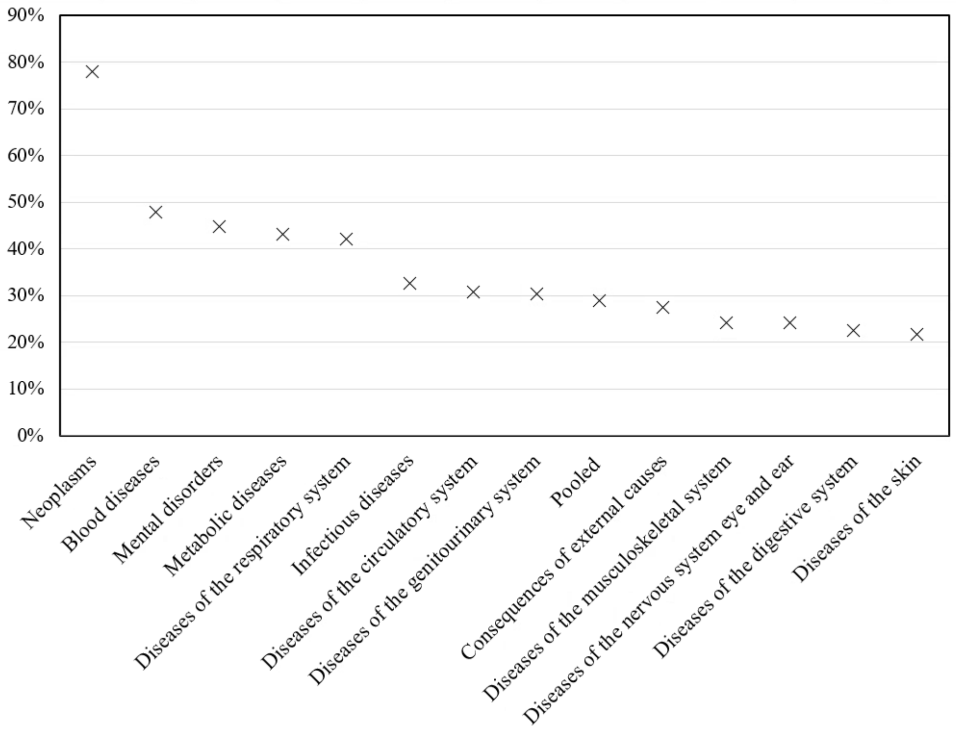
Notes: Effects are estimated using the year previous to the health shock as a reference category. The regression equation includes a set of age dummies, a set of year dummies, gender, and family structure as control variables. Standard errors are clustered at the individual level. The estimation is based on 2,210,233 individuals and 17,123,310 individual-year observations.

Figure 10: Effect of Health Shocks on Probability of Nursing Home Entry



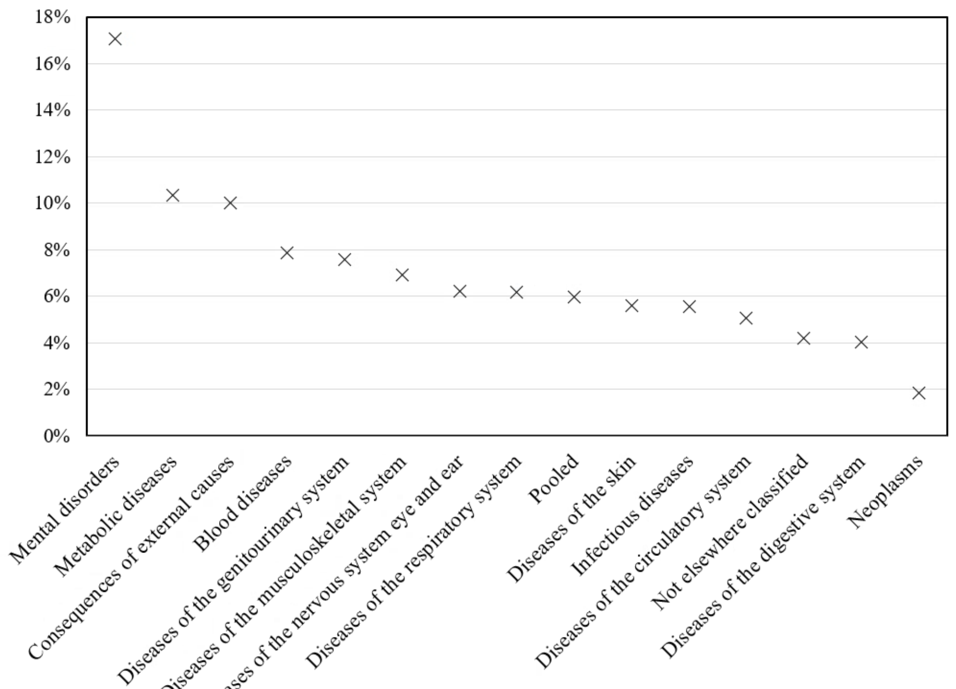
Notes: Effects are estimated using the year previous to the health shock as a reference category. The regression equation includes a set of age dummies, a set of year dummies, gender, and family structure as control variables. Standard errors are clustered at the individual level. The estimation is based on 2,210,233 individuals and 17,123,310 individual-year observations.

Figure 11: Effect of Health Shocks on Probability of Death
- Heterogeneity by Diagnosis -



Notes: Effects are estimated as in Figure 9, but separately by diagnoses and using as a dependent variable a dummy that takes value one at $t = 0$ if the individuals dies anywhere between $t = 0$ and $t = 5$.

Figure 12: Effect of Health Shocks on Probability of Nursing Home Entry
- Heterogeneity by Diagnosis -



Notes: Effects are estimated as in Figure 10, but separately by diagnoses and using as a dependent variable a dummy that takes value one at $t = 0$ if the individuals is at a nursing home at any point between $t = 0$ and $t = 5$.

results show that, on average, health shocks have a substantial effect on mortality. The effect decreases substantially conditional on surviving up to $t = 1$.

Regarding the effect of health shocks on nursing home entry, Figure 10 shows the results we obtain when using as a dependent variable a dummy that takes value one if an individual lives in a nursing home in a particular year.[23] For ease of interpretation, is important to note that the percentage of individuals at a nursing home at $t = -1$ is 3.56%. Interestingly, the years before the shock show important anticipation effects. Importantly, we see that there is an acceleration of the trend at $t = 1$ and $t = 2$, showing that the probability of living in a nursing home increases by 2.58% points by $t = 2$ compared to $t = -1$. These results show that, even if individuals are already likely to be in a nursing home before the shock, the latter does seem to have an effect on nursing home entry. This indicates that at least some of the shocks we observe have a disabling effect.

We turn now to Figures 11 and 12, where we report the same estimations as in Figures 9 and 10 but broken down by the diagnosis categories provided in Table 1. In this case we use only time-to-event dummies from $t = -5$ until $t = 0$, and we give the dummy at $t = 0$ value one if the individual dies (Figure 11) or is at a nursing home (Figure 12) at any point between $t = 0$ and $t = 5$. We report the results only for the $t = 0$ dummies, which show the increase (in percentage points) in the probability of the event taking place at any point between $t = 0$ and $t = 5$ compared to the probability at $t = -1$.[24] Figure 11 shows that neoplasms (which refers to tumours and cancer) are clearly the health shock diagnosis with the highest mortality effect. Out of all individuals we observe who suffer a health shock related to neoplasms, close to 80% are dead by $t = 5$.[25] Simultaneously, neoplasms appear to have the smallest effect on nursing home entry, as shown in Figure 12. Therefore, for this particular diagnosis our estimations are clearly in line with the classification by Bonekamp and Wouterse (2023), confirming that neoplasms are a shock that is very mortality-increasing but not disabling.

Regarding the other diagnoses selected by Bonekamp and Wouterse (2023), the results for mental disorders also appears to be in line with their classification. It has a strong effect on mortality (around 45% percentage point increase relative to $t = -1$) while it is clearly the most disabling of all diagnoses. For musculoskeletal diseases, we see that this diagnosis has a relatively small effect on mortality while its effect on nursing home entry is above average.[26] Among the four diagnoses with a lowest mortality effect, it is the one with the largest effect on nursing home entry. For consequences of external causes, which mostly refer to health shocks related to

[23]Note that in this case that, different from Figure 9, the dependent variable taking value one does not mean the individual is removed from the panel the year after. In this case, the dependent variable takes value one for all the years an individual lives in a nursing home. In addition, the dependent variable can also take value one before the shock. Here as well we provide the effect in percentage points difference with respect to $t = -1$. Confidence intervals are again excluded due to extreme statistical significance.

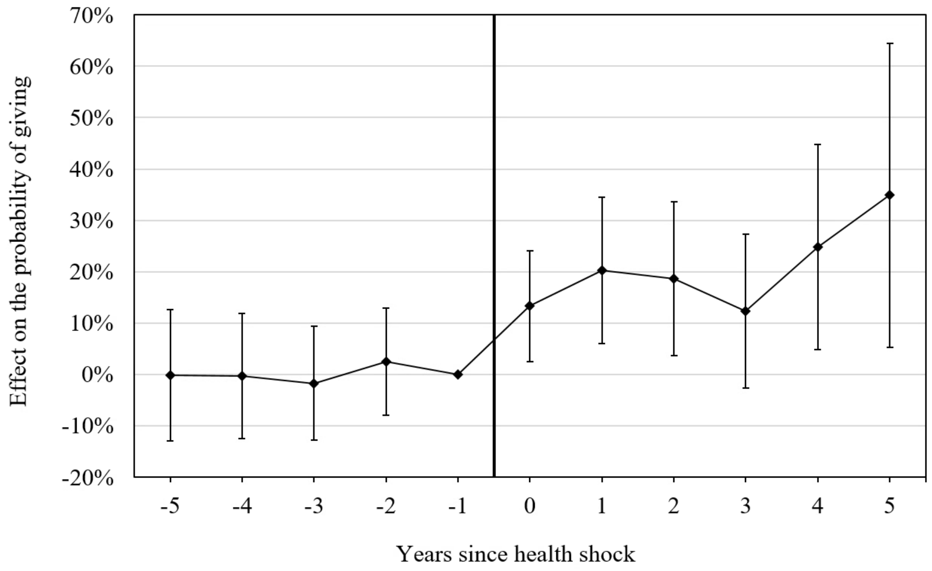
[24]In Figures 11 and 12 we also exclude the confidence intervals due to the extremely small standard errors.

[25]Note that this should not be interpreted as the odds of dying from cancer, since here we are focusing on unplanned hospitalisations not preceded by any hospitalisation in the two years previous. This means that we are capturing a particular selection of cancer diagnoses.

[26]As the average effect we refer to the *pooled* effect provided in Figures 11 and 12. That is the effect on mortality and nursing home entry when pooling all diseases together as in Figures 9 and 10.

Figure 13: Effect of Health Shocks on Probability of Inter-Vivos Giving

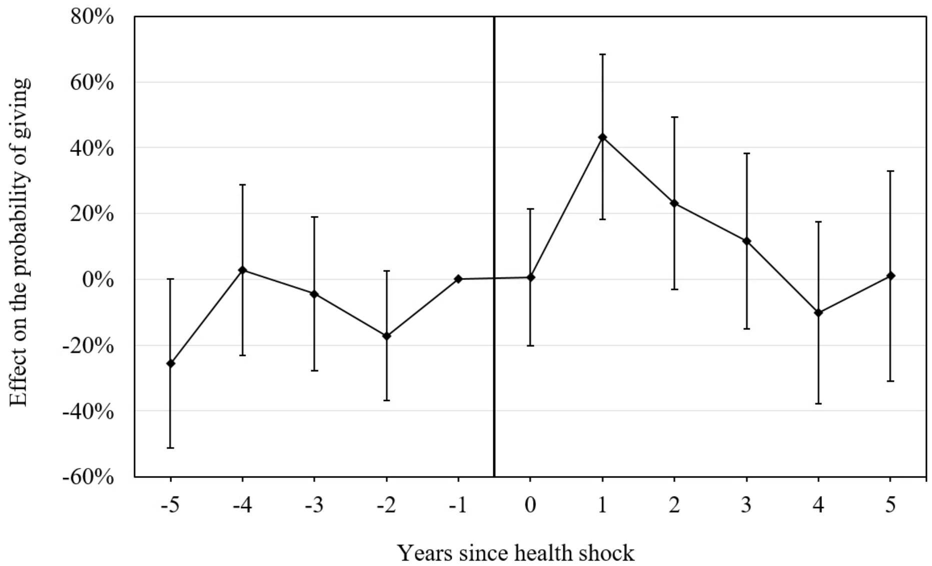
- Neoplasms -



Notes: Effects are estimated using the year previous to the health shock as a reference category. They are provided as a percentage increase with respect to the average in that year. Point estimates are surrounded by 95% confidence intervals. Standard errors are clustered at the individual level. The estimation is based on individuals suffering a health shock with neoplasms as the main diagnosis.

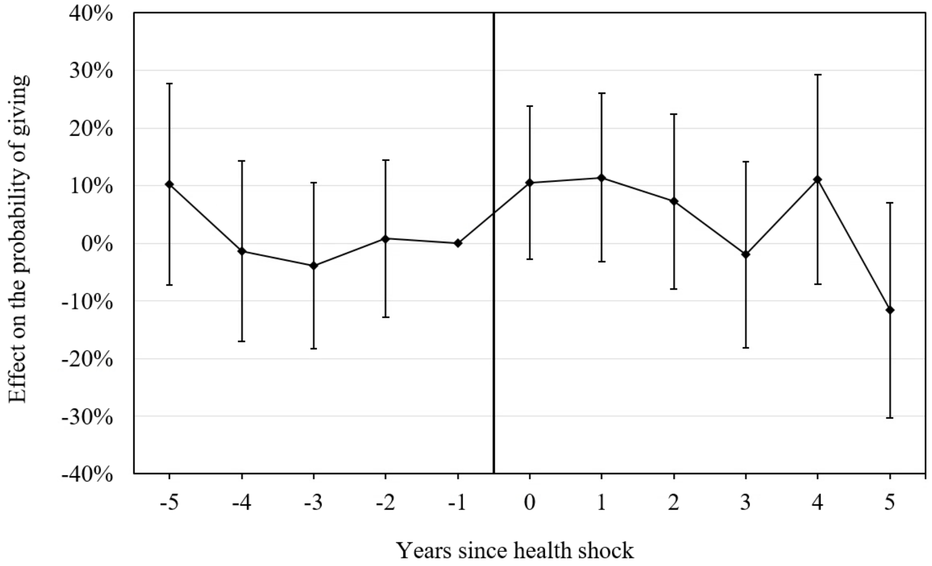
Figure 14: Effect of Health Shocks on Probability of Inter-Vivos Giving

- Mental Disorders -



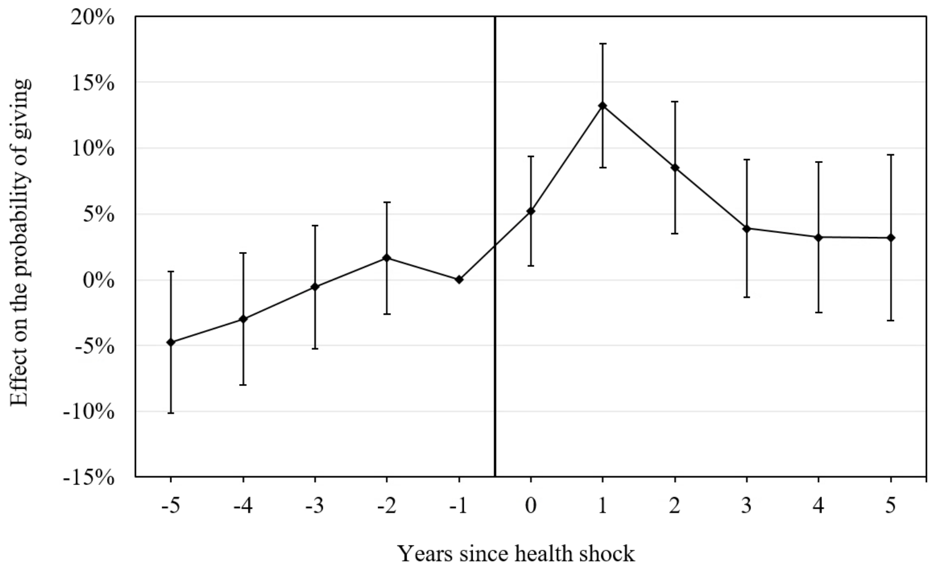
Notes: Effects are estimated using the year previous to the health shock as a reference category. They are provided as a percentage increase with respect to the average in that year. Point estimates are surrounded by 95% confidence intervals. Standard errors are clustered at the individual level. The estimation is based on individuals suffering a health shock with mental disorders as the main diagnosis.

Figure 15: Effect of Health Shocks on Probability of Inter-Vivos Giving - Musculoskeletal Diseases -



Notes: Effects are estimated using the year previous to the health shock as a reference category. They are provided as a percentage increase with respect to the average in that year. Point estimates are surrounded by 95% confidence intervals. Standard errors are clustered at the individual level. The estimation is based on individuals suffering a health shock with musculoskeletal diseases as the main diagnosis.

Figure 16: Effect of Health Shocks on Probability of Inter-Vivos Giving - Consequences of External Causes -



Notes: Effects are estimated using the year previous to the health shock as a reference category. They are provided as a percentage increase with respect to the average in that year. Point estimates are surrounded by 95% confidence intervals. Standard errors are clustered at the individual level. The estimation is based on individuals suffering a health shock with consequences of external causes as the main diagnosis.

accidents, we see a mortality effect that is very close to the average and a disabling effect that is somewhat above the average (just under percentage points).

Having estimated the effects on mortality and nursing home entry by diagnosis, we turn now to estimating the effect on IVTs for selected diagnoses. For that we stick to the classification by Bonekamp and Wouterse (2023). Our analyses reported in Figures 11 and 12 shows that this classification is largely robust. Figure 13 shows the estimated effect of health shocks on giving via IVTs for individuals who suffer a health shock with neoplasms as the main diagnosis, which is the most deadly health shock in our data. There are three relevant features of this figure that are worth noting. First, the estimated effects are substantially larger than those in Figure 2. For instance the effect at $t = 0$ is already 13.31% (positive and statistically significant), indicating that individuals already give on the same calendar year of the shock. At $t = 1$ the size of the effect already nearly doubles. It then increases up to 34.89% at $t = 5$. Second, the confidence intervals appear to be substantially wider than in the baseline results. This is likely the case because here we are looking at a smaller subgroup of individuals. This holds also for all of regressions that estimate effects for particular diagnoses. Third, quite remarkably, the effect does not seem to decrease the further away from $t = 0$. Given that neoplasms have the strongest effect on mortality, the fact that we do not observe a phase out of the effect here suggests that mortality is not what explains the phasing out of the effect in Figure 2.

Figure 14 shows that health shocks related to mental disorders have a very strong effect at $t = 1$ (the point estimate is 43.29%, the largest of all the estimates we provide in this results section) which already becomes not significantly different from zero from $t = 2$ onwards. This indicates that mental disorders have a very strong effect on giving that quickly fades out. Neurodegenerative diseases that cause a general decline in cognitive abilities may be the reason why individuals are more likely to engage in inter-vivos giving shortly after the shock but not any more after that. Figure 15 shows that for musculoskeletal diseases, which are likely to cause limitations in activities of daily living but not death, we do not estimate any significant effects. Finally, Figure 16 shows that for consequences of external causes the estimated effects are not significantly different from the baseline in Figure 2.

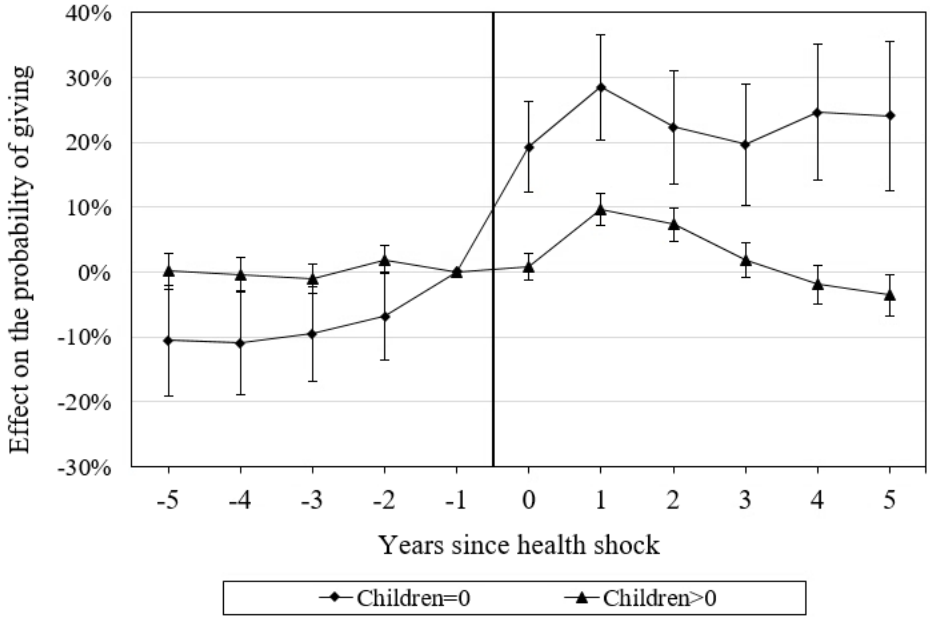
The results presented in this section indicate that it is mortality-increasing health shocks, rather than disabling shocks, that lead to IVTs. This suggests that the exchange motive is not likely to be the driver of the results reported in Figure 2. It suggests that is rather an altruistic and/or warm-glow motive that is activated as a result of the increased mortality and shortened lifetime horizon.

5.4 Child-Level Interactions

To further investigate the relevance of the exchange motive for giving as an explanation for our baseline results, in this section we link the dataset employed so far with data on the children of the individuals who suffer a health shock. We start by estimating the effect separately for individuals with and without children. We focus here on adult children living outside of the

Figure 17: Effect of Health Shocks on Probability of Inter-Vivos Giving

- Heterogeneity by Presence of Children -



Notes: Effects are estimated in the same way as in Figure 2 but for two different regressions. One for individuals without children living outside of the household and the other for individuals with at least one child living outside of the household. Point estimates are surrounded by 95% confidence intervals. For more information, see main text.

parental household. Figure 17 shows that there is a significant effect for both individuals with and without children. Somewhat surprisingly, the effect is stronger for individuals without children. While the effect for individuals with children outside of the household is comparable to the baseline reported in Figure 2, the effect for individuals without children is substantially larger, reaching a point estimate of 28.79% at $t = 1$.

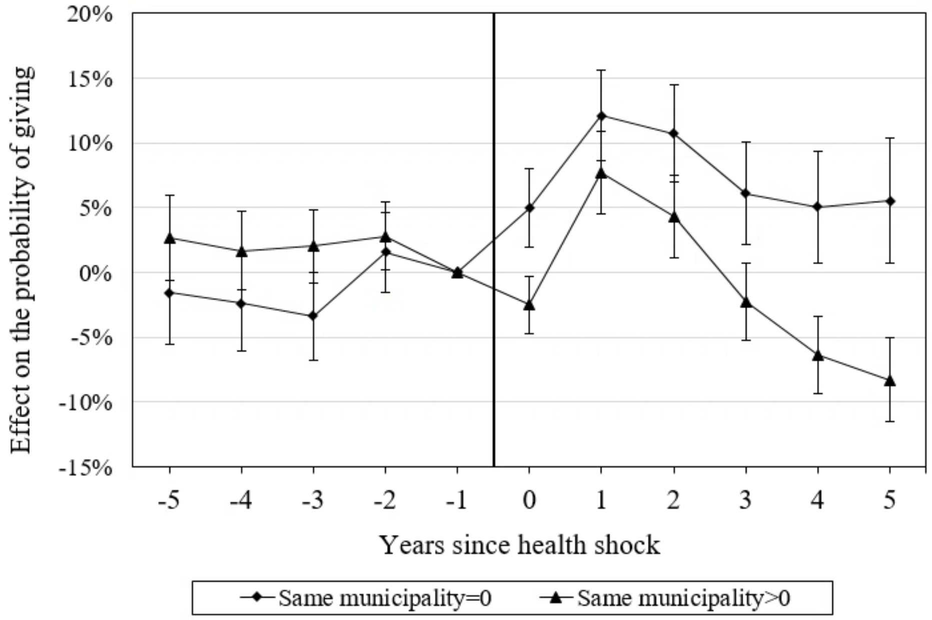
Note however that the literature on the bequest motive has often concluded that having children is not necessarily a predictor of the intensity of the bequest motive, indicating that individuals without children also derive utility from giving to other family or non-family members.[27] Also important to note is that most individuals in our data (76.83%) do have children, that individuals who have children are significantly more likely to engage in IVTs (5.85% of individuals with children give at least one IVT during the period we observe, compared to 2.32% of individuals without children), and that 88.93% of all transfers we observe are from parents to children. For these reasons we leave the investigation of the motives of individuals without children for future work and focus here on the motives of individuals with children.

As mentioned in the introduction, existing literature suggests that proximity to the parental household is a good predictor of informal care provision from children to parents (Bonsang, 2009; Fu, 2019). Therefore, if the exchange motive is a relevant driver of the results we obtain,

[27]See for instance, Hurd (1989), Hurd and Smith (2002), Lockwood (2012), Suari-Andreu *et al.* (2019), and Suari-Andreu *et al.* (2024).

Figure 18: Effect of Health Shocks on Probability of Inter-Vivos Giving

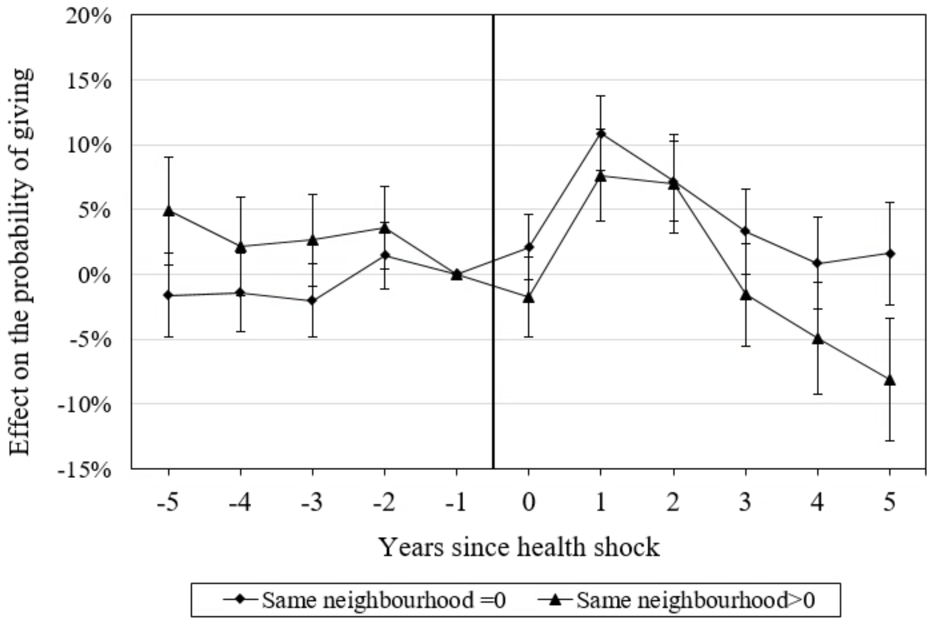
- Heterogeneity by Distance to Children I -



Notes: Effects are estimated in the same way as in Figure 2 but for two different regressions. One for individuals with adult children living in their same municipality and the other one for individuals without. Both are conditional on having at least one child living outside of the parental household. Point estimates are surrounded by 95% confidence intervals. For more information, see main text.

Figure 19: Effect of Health Shocks on Probability of Inter-Vivos Giving

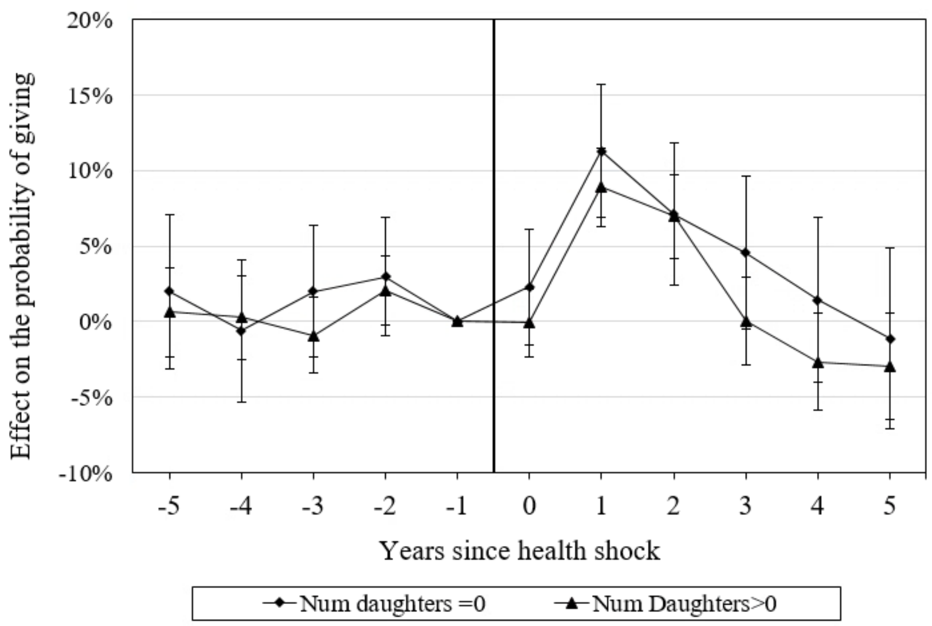
- Heterogeneity by Distance to Children II -



Notes: Effects are estimated in the same way as in Figure 2 but for two different regressions. One for individuals with adult children living in their same neighbourhood and the other one for individuals without. Both are conditional on having at least one child living outside of the parental household. Point estimates are surrounded by 95% confidence intervals. For more information, see main text.

Figure 20: Effect of Health Shocks on Probability of Inter-Vivos Giving

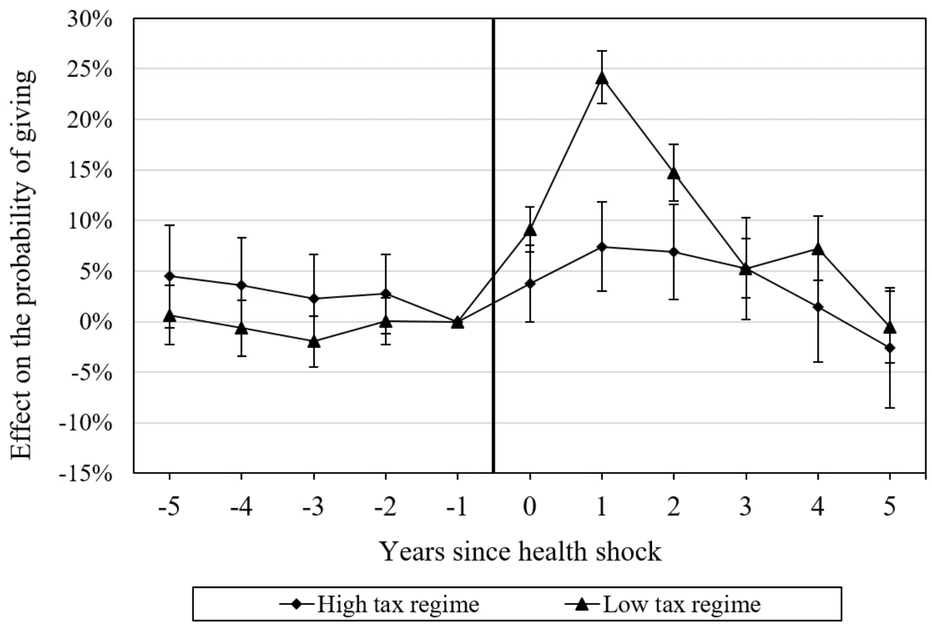
- Heterogeneity by Presence of Daughters -



Notes: Effects are estimated in the same way as in Figure 2 but for two different regressions. One for individuals with at least one daughter living outside of the household and the other for individuals without. Point estimates are surrounded by 95% confidence intervals. For more information, see main text.

Figure 21: Effect of Health Shocks on Probability of Inter-Vivos Giving

- Heterogeneity by Tax Regime -



Notes: Effects are estimated in the same way as in Figure 2 but for two different regressions. One for individuals who suffer a health shock in the years 2010, 2011, 2014, or 2015 (high tax regime), and another group for individuals who suffer a health shock in the years 2012, 2013 or the 2016-2019 period (low tax regime). Point estimates are surrounded by 95% confidence intervals. For more information, see main text.

individuals with adult children living nearby should be more likely to give IVTs after a health shock. The CBS data provides information on the municipality and the neighbourhood where households live. Leveraging this information, we re-estimate our baseline results for individuals with children living in the same municipality and individuals without. We conduct the same analysis at the neighbourhood level. These analyses are conditional on having at least one child living outside of the household. Figure 18 shows that, up to $t = 2$ the differences between the two groups are not statistically significant. After that the effect is significantly larger for individuals without adult children living in the same municipality, which is the opposite of what we would expect if the exchange motive for giving was a relevant explanation for our baseline results. Figure 19 shows similar results at the neighbourhood level, even though in this case the effects for the two groups are only significantly different at $t = 5$.

In addition to distance from the parental household, another predictor of informal care identified by the literature is the presence of daughters. Previous studies, such as Carmichael and Charles (2003) and Schmitz and Westphal (2017) among others, show that due to prevailing gender norms female children appear to be more likely to provide care to their parents than male children. Figure 20 provides the results we obtain when estimating our baseline results separately for individuals with and without at least one daughter. All results are here again conditional on having at least one child living outside of the household. Figure 20 shows that the results for both groups are not significantly different from each other. For both groups, the estimates are comparable to the baseline results provided in Figure 2.[28] All results in this section indicate that the exchange motive for giving IVTs is unlikely to be the mechanism behind our baseline results. This is inline with the results shown in Section 5.3, which show that results not significantly different from zero when considering a disabling health shock.

5.5 Differential Tax Regimes

As noted in the literature (Kopczuk, 2007; Erixson and Escobar, 2020; Sturrock *et al.*, 2022; Suari-Andreu *et al.*, 2024), an additional potentially relevant motive behind inter-vivos transfers is inheritance tax avoidance. As we explain in Section 2.4, the Netherlands has a gift an inheritance tax regime that does provide an incentive to give while alive to avoid inheritance taxes. That is because there are both yearly and one-off exemptions for IVTs as long as they take place at least six months before death. If the transfers take place less than six months before the death of the giver, then they are considered as part of the inheritance for tax purposes. Besides these exemptions, the progressive nature of the tax schedule also means that dividing an estate into different parts that are taxed separately leads to a lower overall tax burden.

As we also mention in Section 2.4, within the years that we cover in this study there is time variation in the one-off exemption for IVTs directed to children. That is, between October

[28]It is important to note here that, like all results provided in the whole of Section 5, the results including child-level interactions do not change when we add gender, marital status, and household structure as control variables. For the results in this Section 5.4 we add as well the number of children living outside of the household as a control. That also does not change the results.

2013 and December 2014 the one-off 50,000 Euro (which is only valid in case the transfer is used for home purchase or studies) was temporarily increased to 100,000 Euro. This measure was then reintroduced in 2017. These changes provide interesting variation to check whether the increase in giving that we see after a health shock reflects avoidance of inheritance taxes as a result of increased mortality. As mentioned in Section 4, one of the assumptions of the empirical model is that the exact timing of the health shocks is random. Conditional on this assumption, individuals are randomly assigned to higher or lower tax regime for the one-off exemption. Taking into account that our baseline result shows that the effect of the health shock takes place mostly at $t = 1$ and $t = 2$, we create two groups. One with individuals who suffer a health shock in the years 2010, 2011, 2014, or 2015 (higher tax regime) and one with individuals who suffer a health shock in the years 2012, 2013, or the 2016-2019 period (lower tax regime).[29]

Figure 21 shows that there is an effect for both the higher and the lower tax years. However, the effect is clearly larger for the lower tax years. For that group, the point estimate for $t = 1$ is 24.14%, while for the group facing higher taxes for the one-off exemption the estimate is just 7.41%. This difference is large and statistically significant. These results indicate that individuals do use IVTs as a mechanism to avoid inheritance taxes. This result is in line with the previous work by Sturrock *et al.* (2022) and Suari-Andreu *et al.* (2024), both of which show evidence using Dutch data that points towards the use of IVTs for inheritance tax avoidance. However, none of these studies considers the role of health shocks explicitly. Importantly, the presence of a tax motive does not rule out the possible existence of an altruistic and/or warm-glow motives. That is because when individuals avoid taxes, they are increasing the total size of their estate, which they may do to fulfill an altruistic and/or warm-glow motive. We leave the further exploration of these motives for future work.

6 Conclusion

In this study we investigate the role of health as a driver of inter-vivos transfers (IVTs). Drawing on a rich dataset from Dutch administrative records, we define negative health shocks as unplanned hospital intakes that are not preceded by any hospital intake in the two years previous. Applying an event time study design, we estimate the impact of these health shocks on inter vivos giving. We find a positive and significant effect at the extensive margin that peaks in the year after the shock and then gradually fades out. For the year after the shock we find an increase in the probability of giving at least one transfer a year of 11.37% compared to the year previous to the shock. We find no effect at the intensive margin. The result we find is in line with the previous work by Kvaerner (2023), who also finds a positive effect using a dif-

[29] Note that the years 2012 and 2016 are the years before the two tax reductions. However, we group them with the lower tax years because the baseline result shows that the effect is not observed until $t = 1$. We do the opposite with the year 2014. It is a lower tax year but we group with the higher tax years because the exemption was reduced again in 2015 and 2016. The years before 2010 are excluded because in those years other aspects of the tax schedule were different.

ferent methodology. The effect we estimate appears to increase with the intensity of the shock (*i.e.* when we increase the pre-shock time window free of hospital intakes), and it appears to be stronger for older individuals, for women, and for singles. In the latter case, the effect is stronger for singles the year after the shock, but after that it is actually significantly stronger for individuals in a couple.

Besides looking at whether health is a trigger for IVTs, we also use this analysis to investigate the role of different giving motives identified by the literature. In doing so, we focus on mostly investigating the role of the exchange motive. To do so, we follow different strategies. First, building on Bonekamp and Wouterse (2023), we classify the health shocks according the their effect on mortality and their effect on disability. We find that mortality increasing shocks have an effect on inter vivos giving, while disabling shocks do not. Second, we re-estimate our baseline analysis comparing individuals with and without children living nearby the parental household. We find that the effect is slightly stronger for individuals without children living nearby. Third, we re-estimate our baseline analysis comparing individuals with and without daughters. We find that the effects are not significantly different. All of these results indicate that the exchange motive is not likely to be the mechanism behind our baseline results.

In addition to exploring the role of the exchange motive, we exploit changes over time in the gift an inheritance tax regime to investigate the role tax avoidance in explaining our results. We find that when health shocks occur in years in which one-off tax exemptions for transfers are higher they are more likely to lead to giving of IVTs. This result indicates that, following an increase in expected mortality induced by a negative health shock, individuals are likely to use IVTs as an instrument to avoid gift and inheritance taxes. This result is in line with previous work by Kopczuk (2007), Erixson and Escobar (2020), Sturrock *et al.* (2022), and Suari-Andreu *et al.* (2024), all of which find evidence indicating the use of IVTs to avoid estate taxes. Interestingly, all of these studies assume (without testing for it) that deteriorating health and revised mortality lead to transfers related to estate planning. Our results provide evidence in favour of this assumption.

Future work is needed to investigate the role of two other giving motives identified by the literature, *i.e.* altruism and warm-glow giving. As explained by Kvaerner (2023), Bonekamp and Wouterse (2023), and Suari-Andreu *et al.* (2024), reduced mortality due to poor health can lead to IVTs responding to an altruistic or warm-glow motive. Our results suggest that these two motives can be relevant but do not provide any insight allowing us to distinguish between the two. Future work should conduct an analysis at the children level using data on income and wealth for the latter to see how these condition the receipt of transfers after a parental health shock. This would require a different analysis and approach than the one we apply on this paper and therefore we leave it for future work.

The implications of our findings are threefold. First, they shed light on the circumstances leading to wealth transfers among households. In this way, they underscore the necessity to consider the behavioural responses to health shocks when designing policies that imply wealth redistribution among households. Second, the results indicate that the exchange motive is not a relevant driver of the observed inter-vivos transfers. This suggests that individuals receive sufficient formal care and/or that existing informal care provided from children to parents is not rewarded with wealth transfers while the parents are still alive. This indicates that the transfers respond to altruism and/or warm glow giving, which has implications for how they ought to be taxed. Third, we find evidence indicating that IVTs are used as an instrument to avoid taxation, which has important implications for the design of the tax and inheritance regime. For instance, this evidence implies that a measure that policymakers can easily use to influence this behaviour would be a change in the look-back period for IVTs to be counted as part of an inheritance. As we explain in Section 2.4, in the Netherlands IVTs do not count as part of the inheritance if the take place more than six months before death. An extension of this period would limit the chances of using IVTs to avoid estate taxes following an increase in mortality.

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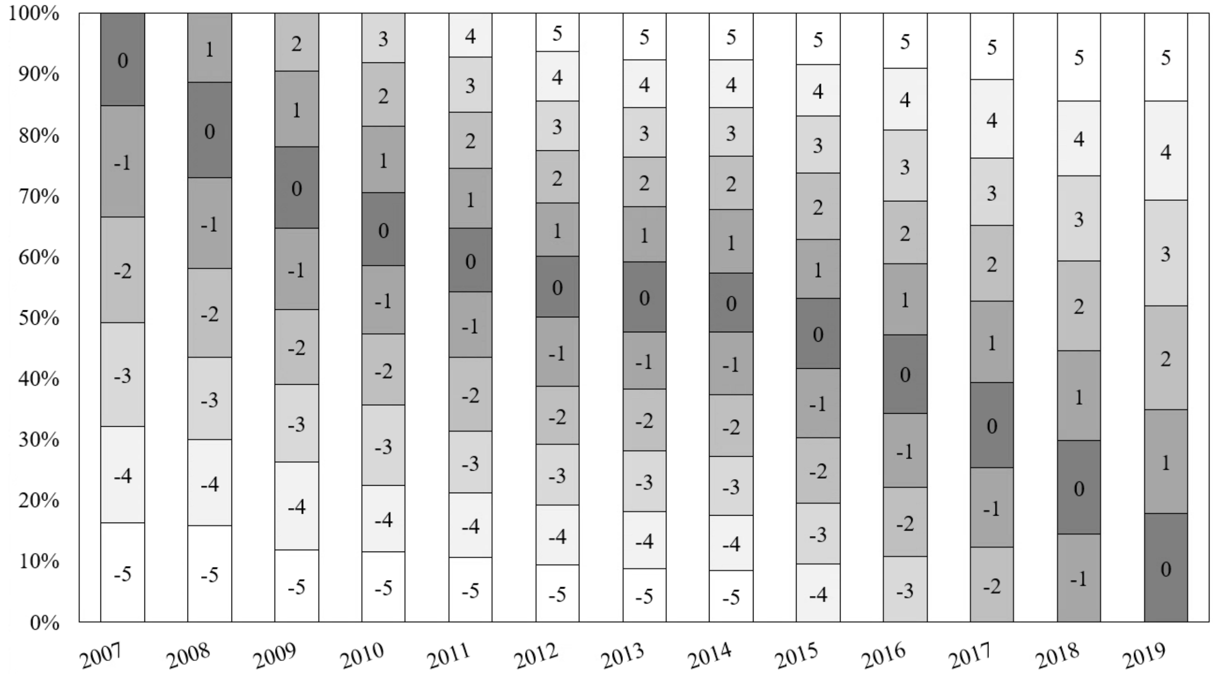
Appendices

A Data Structure

Figure A.1: Data Structure by Calendar Year and Time to Event



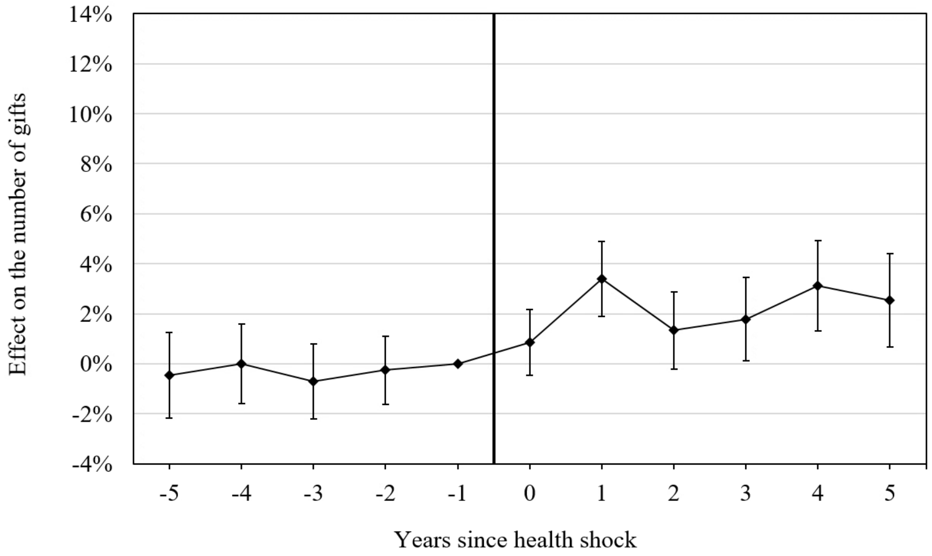
Notes: Sections within columns show the proportion of observations within a calendar year that correspond to a particular value for the time-to-event variable.

Figure A.1 provides an overview of the structure of the data that we use to estimate the parameters in Equation 1. As mentioned in Section 3, we follow 2,210,233 individuals over a period of 13 years and we consider a time-to-event window of five years before and five years after the health shock. This means that every year a share of individuals is treated. However, we do not observe the same values for the time-to-event variable in every calendar year. In the first year we observe only individuals who are either just treated or not treated yet. Over time we observe more individuals who have been already treated and less who are not treated yet. This happens up to the last year, 2019, in which we observe only treated individuals. This means that in this last year there is no reference group to which the treated individuals can be compared.

Within every calendar year, the shares for each value of the time-to-event variable appear to be very equally distributed. Only for three years (2012 to 2014) we observe all time-to-event values. The total number of individual-year observations we observe is 17,123,310. The number of individual-year observations per calendar year increases somewhat from 2007 until 2014, and then it slightly decreases up to 2019. However the differences are small, with every year's share of total observations being between 5% and 9%. Regarding the number of observations for each of the time-to-event values, by definition the number of observations with a time-to-event value of zero is equal tot he number of individuals we observe (2,210,233). The number of observations corresponding to each time-to-event value gradually decreases the further away we move from the shock. The decrease in the number of observations when moving an extra year away from the shock (in either direction) is in all cases around 200,000 to 300,000. For the most distant values, *i.e.* $t = -5$ and $t = 5$, the number of individual-year observations is still around one million.

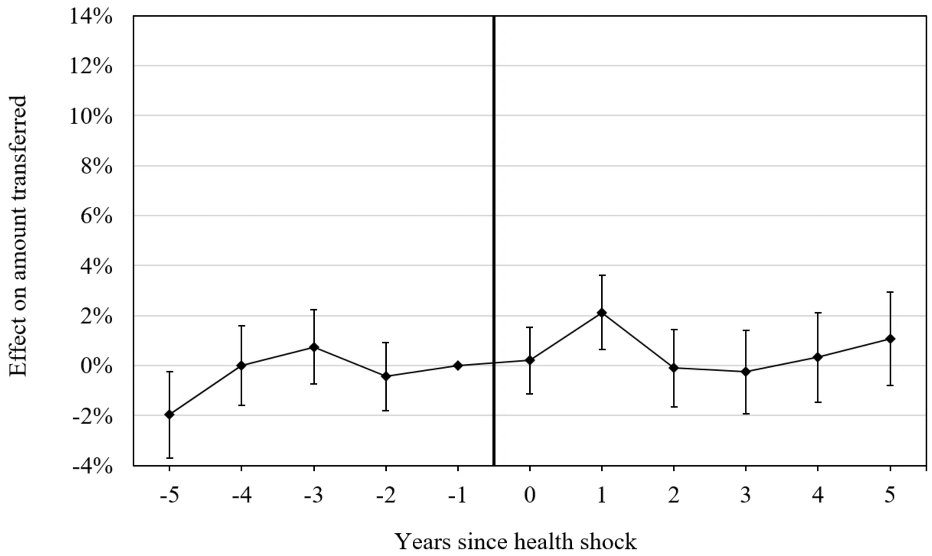
B Additional Results

Figure B.1: Effect of Health Shocks on Number of Gifts (Censored)



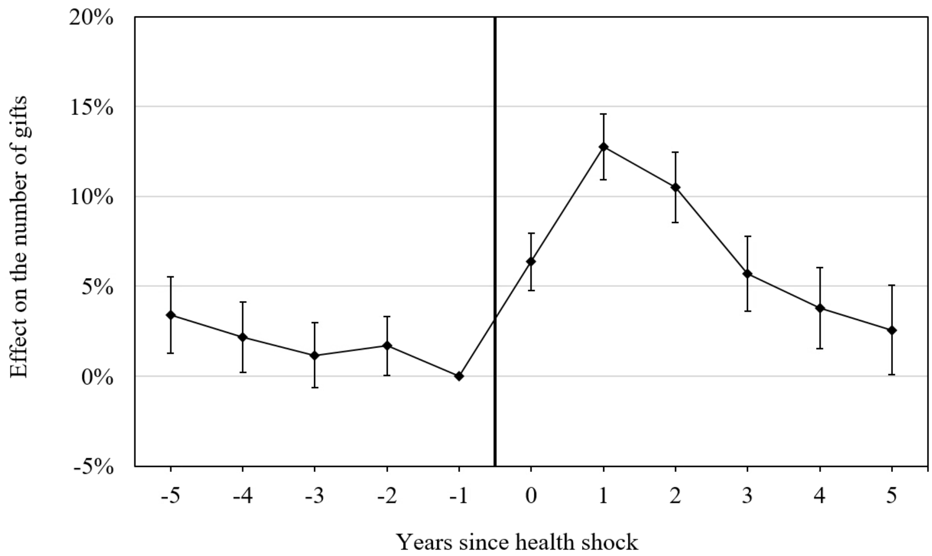
Notes: Effects are conditional on giving and are estimated using the year previous to the health shock as a reference category. They are provided as a percentage increase with respect to the average in that year. The regression equation includes a set of age dummies, a set of year dummies, gender, and family structure as control variables. Point estimates are surrounded by 95% confidence intervals. Standard errors are clustered at the individual level. The estimation is based on 89,012 individuals and 138,886 individual-year observations.

Figure B.2: Effect of Health Shocks on Amount Given (Censored)



Notes: Effects are conditional on giving and are estimated using the year previous to the health shock as a reference category. They are provided as a percentage increase with respect to the average in that year. The regression equation includes a set of age dummies, a set of year dummies, gender, and family structure as control variables. Point estimates are surrounded by 95% confidence intervals. Standard errors are clustered at the individual level. The estimation is based on 88,426 individuals and 137,497 individual-year observations. The top 1% of observations are removed to avoid the influence of outliers on the result.

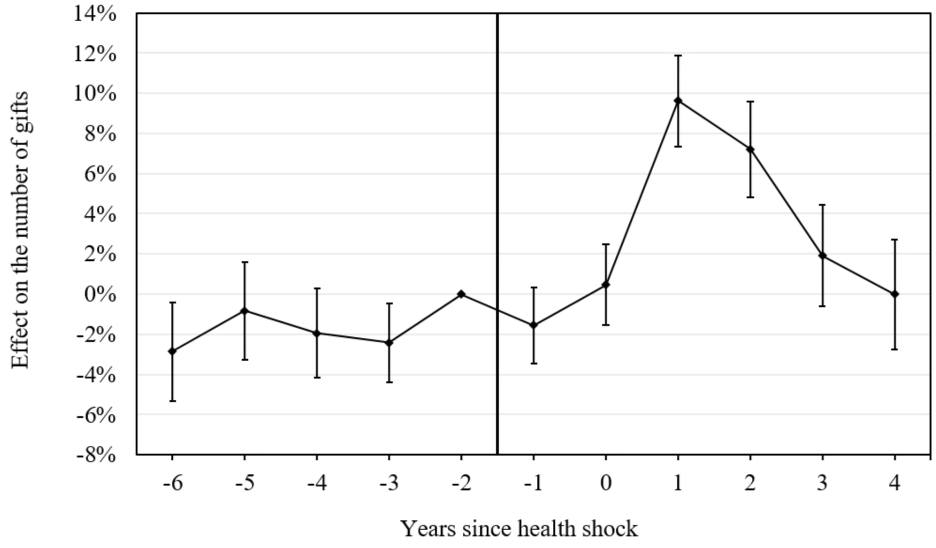
Figure B.3: Effect of Health Shocks on Probability of Inter-Vivos Giving
- Household Level -



Notes: Effects are estimated using the year previous to the health shock as a reference category. They are provided as a percentage increase with respect to the average in that year. The regression equation includes a set of age dummies, a set of year dummies, gender, and family structure as control variables. Point estimates are surrounded by 95% confidence intervals. Standard errors are clustered at the individual level. The estimation is based on 2,210,233 individuals and 17,123,310 individual-year observations.

Figure B.4: Effect of Health Shocks on Probability of Inter-Vivos Giving

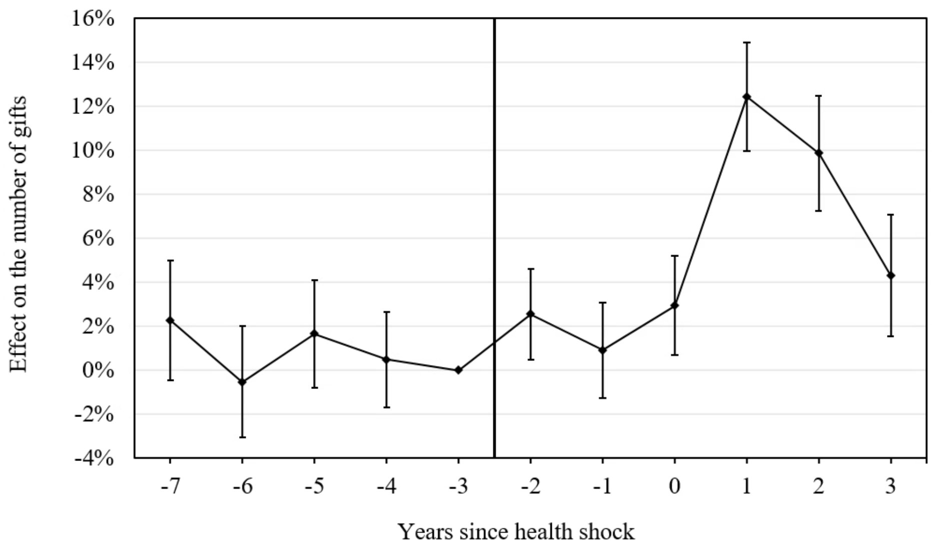
- Placebo ($t = -1$) -



Notes: Effects are estimated using the second year before health shock as a reference category. They are provided as a percentage increase with respect to the average in that year. Point estimates are surrounded by 95% confidence intervals. Standard errors are clustered at the individual level. The estimation is based on 2,210,233 individuals and 17,263,675 individual-year observations.

Figure B.5: Effect of Health Shocks on Probability of Inter-Vivos Giving

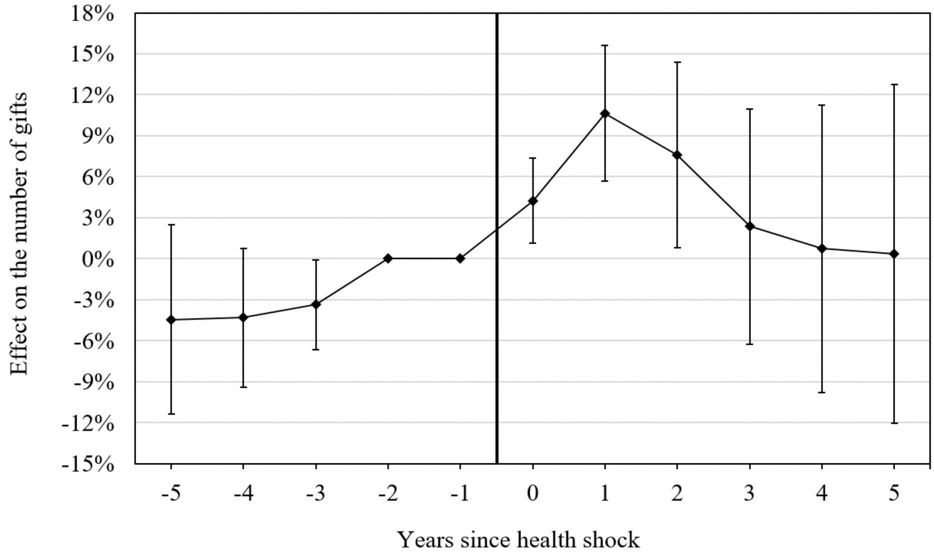
- Placebo ($t = -2$) -



Notes: Effects are estimated using the third year before the health shock as a reference category. They are provided as a percentage increase with respect to the average in that year. Point estimates are surrounded by 95% confidence intervals. Standard errors are clustered at the individual level. The estimation is based on 2,210,233 individuals and 17,022,112 individual-year observations.

Figure B.6: Effect of Health Shocks on Probability of Inter-Vivos Giving

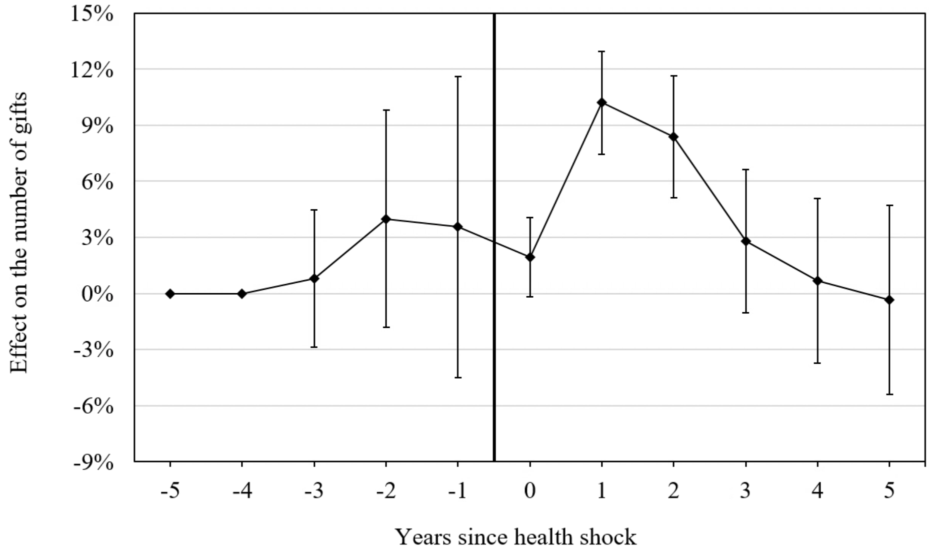
- Fixed Effects -



Notes: Effects are estimated using the two years previous to the health shock as a reference category. They are provided as a percentage increase with respect to the average for those two years. The regression equation includes a set of age dummies, a set of year dummies, gender, and family structure as control variables. Point estimates are surrounded by 95% confidence intervals. Standard errors are clustered at the individual level. The estimation is based on 2,210,233 individuals and 17,123,310 individual-year observations.

Figure B.7: Effect of Health Shocks on Probability of Inter-Vivos Giving

- No Homogeneity -

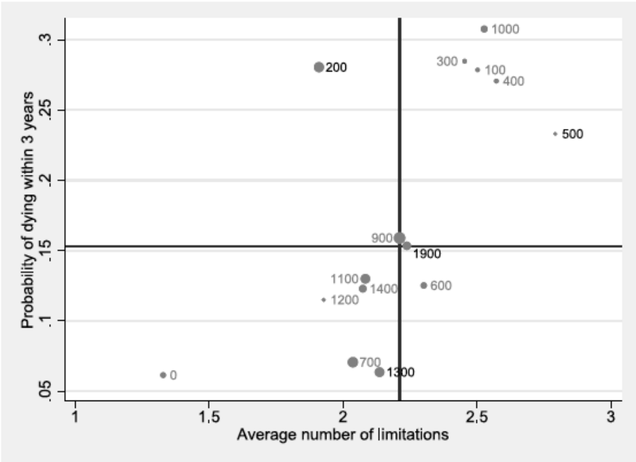


Notes: Effects for the time-to-event dummies $t = 0$ to $t = 5$ are estimated using all year previous to the health shock as a reference category. They are provided as a percentage increase with respect to the average for those years. Effects for the time-to-event dummies $t = -3$ to $t = -1$ are estimated using $t = -4$ and $t = -5$ as a reference category. The regression equation includes a set of age dummies, a set of year dummies, gender, and family structure as control variables. Point estimates are surrounded by 95% confidence intervals. Standard errors are clustered at the individual level. The estimation is based on 2,243,015 individuals and 15,395,351 individual-year observations.

C Health Shock Classification

Figure C.1: Mortality and Disability Across Health Shock Diagnoses

(Bonekamp and Wouterse, 2023)



0	Not admitted
100	Infectious and parasitic diseases
200	Neoplasms
300	Blood and blood forming organs and certain disorders involving the immune mechanism
400	Endocrine, nutritional and metabolic diseases
500	Mental and behavioral disorders
600	Nervous system
700	Eye and adnexa
900	Cirulatory system
1000	Respiratory system
1100	Digestive system
1200	Skin and subcutaneous tissue
1300	Musculoskeletal system and connective tissue
1400	Genitourinary system
1900	Injury, poisoning and certain other consequences of external causes

Notes: This figure corresponds with Figure 1 in Bonekamp and Wouterse (2023). Effects of diagnoses on death are estimated using administrative data from Statistics Netherlands. Effects of diagnoses on disability are estimated using Dutch cross-sectional survey data. For more details, see Bonekamp and Wouterse (2023).