NBER WORKING PAPER SERIES

HEALTH SPILLOVERS:
THE BROAD IMPACT OF SPOUSAL HEALTH SHOCKS

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Working Paper 33994
http://www.nber.org/papers/w33994

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
July 2025

This project was supported by an SSHRC Institutional Grant (SIG). We thank Alex Hoagland, Adriana Lleras-Muney, Clementine Van Effenterre, Andrea Velasquez and seminar participants at the BSE Summer Forum, Workshop on Risky Behaviours at the LSE, Exeter, CEA, CHESG, RIDGE, ASHEcon, University of Toronto, Erasmus School of Economics, AES, SED and IEA for their comments. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

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NBER Working Paper No. 33994
July 2025
JEL No. I12

ABSTRACT

In this paper we provide new evidence on the health spillover effects of health shocks within couples. Using administrative data from the Netherlands and a matching event-study framework, we estimate the causal effect of experiencing a health shock within a couple on the health of the initially unaffected partner. Our findings reveal a significant deterioration in the partner's health outlook, characterized by substantial increases in hospital visits, overnight stays, and mortality. The health decline is broad in scope, encompassing higher risk of infections, accidents, and digestive and cardiovascular conditions. This deterioration is accompanied by substantial increases in stress, anxiety and depression for both men and women, as well as sleep disorders for women. These effects are not driven by a heavy caregiving load, financial distress or worsening of health behaviors. On the contrary, the adverse outcomes persist despite suggestive positive changes, including increased exercise for both men and women, and reduced alcohol consumption among women.

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I. Introduction

Health shocks are among the most disruptive life events, carrying profound economic and health consequences not only for the affected individual but also for their close social network.[1] Approximately 7% of the population in high-income countries visits a hospital in a given year (CDC, 2018; CIHI, 2021; Statistics-Netherlands, 2023), and by age 55, at least 42.6% of the population will have experienced a hospitalization event at least once (Statistics-Netherlands, 2023). Health shocks lead to permanent health deterioration and declines in employment and income (García-Gómez et al., 2013; Dobkin et al., 2018). These shocks have spillovers on other family members, specifically partners, whose labor force participation and preventive care have been found to be shaped by their affected spousal's health (Lee, 2020; Fadlon and Nielsen, 2021; Jeon and Pohl, 2017; Jeon et al., 2020; Fadlon and Nielsen, 2019; Hoagland, 2022; Chen et al., 2022; Fadlon et al., 2025).

Economic models of health typically conceptualize it as an individual choice, shaped by personal preferences, resources, and constraints (Grossman, 1972; Murphy and Topel, 2006). While a growing body of research incorporates social interactions—such as peer and network effects in behaviors like smoking, drinking, obesity, and unprotected sex (Cawley and Ruhm, 2011), or the influence of social ties on the spread of infectious diseases (Hill et al., 2010)—the social dimensions of health, particularly within couples, remain relatively underexplored in this literature. Spouses share resources, habits, caregiving responsibilities, and emotional bonds, all of which suggest that one partner's health may significantly influence the other's well-being. Ignoring such interdependence may underestimate the broader spillover effects of health shocks with implications for insurance design and policy interventions (Fadlon et al., 2025).

This paper presents evidence of a mechanism of direct health interdependence within couples, independent of information, behavioral, or resource-related channels. We estimate the health effects of having a partner who experiences a health shock, and we delve into the coping mechanisms that it triggers. We use rich administrative data from the Netherlands from 1994 to 2021 that link resident and household structure records for 26.5 million residents, to employment, hospital discharge, mortality, medication and survey data on health, health behaviors and caregiving. To identify the counterfactual outcomes of couples experiencing health shocks, we use a matching event-study framework. We define a health shock as a hospital visit, excluding those related to childbirth or pregnancy, after at least five years with no prior hospital visit, and we restrict our attention to shocks whose onset cannot be predicted, following Rellstab et al. (2020). We focus on individuals who experienced a health shock between the ages of 25 and 55, and on

[1] See Dobkin et al., 2018; García-Gómez, 2011; Meyer and Mok, 2019; Wagstaff, 2007; García-Gómez et al., 2013; Lee, 2020; Jeon and Pohl, 2017; Fadlon and Nielsen, 2021; Breivik and Costa-Ramón, 2024; Adhvaryu et al., 2023; Brito and Contreras, 2023; Rellstab et al., 2020 , among others.

couples who were together both at the time of the health shock and three years prior.

We find that after a health shock, the partner's health declines significantly. Hospital visits rise and remain high; five years after the shock, the initially unaffected partner is 11.2% and 9.6% more likely to visit a hospital, for women and men, respectively.[2] There is no evidence that these hospital visits are unnecessary or wasteful; rather, they reflect a genuine decline in health. For example, we find that initially unaffected female and male partners are 7.9% and 9.0% more likely to themselves experience a severe health shock– defined as an unexpected hospital visit for which admission was classified as urgent and which entailed a hospital stay of at least three nights. The conditions leading to these admissions are diverse, with increased likelihoods of nervous, circulatory, digestive, and respiratory system issues, as well as injuries. This deterioration in health also translates into increased mortality risk. Five years after the initial shock, the one-year probability of dying is 3.2% higher for women and 1.6% higher for men.

We combine our sample with self-reported measures of emotional well-being from health surveys and prescription drug records to examine the spillover effects on mental health. Analysis of the survey data reveals that women, in particular, report feeling more nervous and experiencing greater sadness following their partner's health shock. These patterns align with a higher incidence of depression and anxiety, as reflected in a sustained increase in the likelihood of filling prescriptions for medications related to stress, anxiety, and depression among both men and women. The deterioration in mental health is especially pronounced when the partner's initial shock is severe—defined as having an above-median mortality risk—and results in a higher likelihood of hospital admissions for mental health conditions in the short term.

The magnitude of spillover effects from a partner's health shock differs substantially between physical and mental health, particularly for women. For instance, the increase in hospital visits for the initially healthy partner amounts to about 14% of the increase observed in the sick partner for women and 12% for men. In contrast, the spillovers on mental health are much larger. The rise in antidepressant use among healthy partners reaches 48% of the increase seen in sick partners for women and 18.6% for men. Similarly, the likelihood of reporting feelings of sadness is even more pronounced—105% of the effect observed in sick partners for women and 87% for men. This substantial decline in mental health may contribute to a higher risk of physical health problems. Indeed, we find that the deterioration in physical health is more severe among individuals who are prescribed mental health medications.

We then examine the potential role of several mechanisms and behavioral responses. First, we show that in our setting, the initial health shock does not lead to significant

[2]Regarding the effects on the person experiencing the initial health shock, consistent with prior research, we find that it leads to a sustained 80% increase in hospital visits, a decline in survival rates by 3.6 pp for women and 4.4 pp for men, and reductions in employment (2.3 pp for women, 2.7 pp for men) and income (3.2% for women, 4% for men) five years later.

caregiving responsibilities for the partner. There is only a short-lived increase in reports of informal caregiving during the year of the shock, and these responsibilities are not reported to be time-intensive. Moreover, when we classify shocks based on caregiving intensity—above and below the median—we find no meaningful differences in the partner's risk of hospitalization between these two groups. However, when it comes to survival outcomes, female partners do experience an increased mortality risk following high-intensity caregiving shocks, whereas no such effect is observed for male partners. By contrast, health spillovers are larger when the initial shock involves a diagnosis with lower survival probability. In these cases, partners experience higher rates of hospitalization and mortality. Third, our survey data suggest that the observed health deterioration is not driven by worsening health behaviors such as reduced exercise or increased smoking and drinking. In fact, women report reductions in alcohol consumption, while men report exercising more, both at the extensive and intensive margins. However, women also report poorer sleep quality, as indicated by increased use of medication for sleep disorders. Finally, we find little evidence that financial distress plays a significant role in this context. On average, we observe no economically meaningful changes in employment status, hours worked, or personal and household income. Overall, these results show that even in settings with generous social insurance and welfare systems that ease financial strain and caregiving responsibilities, a partner's health shock still has significant negative spillovers on the health and well-being of those around them.

We probe the validity of our identification strategy with multiple exercises. A natural threat to our identification relates to the possibility that, given assortative matching in health outlooks, the health of the initially healthy partner was already on track to deteriorate even in the absence of the other partner's health shock. To address this concern, we first show that there is no evidence of anticipation of the shock in "unmatched" health and economic variables such as medication prescriptions, employment, hours worked and income. Second, we examine the health trends of extended family members such as parents and siblings, with whom genetic endowments and health habits are shared, and observe no differential trends in hospital visits. Third, we extend the pre-period analysis by five years, a period in which we do not have restrictions on hospital visits for either the control or treatment groups. In this extended pre-period, we do not observe any meaningful differences between the groups in the rate of hospitalizations. Fourth, we restrict our sample to a subset of shocks that are either more likely to be sudden or less likely to be the result of correlated health behaviors, such as acute appendicitis events, injuries, emergency room admissions, and shocks with uniform incidence across the week. We find that the patterns and magnitudes are very similar. Finally, we also show that our results are robust to changes to our sample restrictions, econometric specification, and definition of a health shock.

We contribute to the literature that studies the role of families as determinants of health and health behaviors. Fadlon and Nielsen (2019); Hodor (2021); Hoagland (2022) examine the effects of health shocks within families and their consequences for medical spending and health care utilization. A key mechanism driving this relationship is the role of information shared within families. They find that such shocks update family members on the incidence and health effects of certain conditions, and lead to behavioral changes. Our contribution to this literature is to provide evidence of the direct health deterioration that results from a partner's health shock, which in turn can influence care decisions, behaviors, and health investments. Specifically, we show that anxiety, stress and depression are major drivers of the decline in health, in a context with generous health insurance and welfare. In addition, improvements in behaviors and habits do not offset the decline in health. Our work also extends a rich literature in medicine and public health that has documented the correlation between a broad range of spousal shocks and the future health decline of the partner (see Christakis and Allison (2006); Stroebe et al. (2007); Awad and Voruganti (2008), among others).

This paper also contributes to the extensive literature on the broader consequences of health shocks. Prior research has documented effects on labor market outcomes for individuals themselves, their spouses, parents, and adult children, as well as impacts on crime (García-Gómez et al., 2013; Breivik and Costa-Ramón, 2024; Adhvaryu et al., 2023; Lee, 2020; Jeon and Pohl, 2017; Brito and Contreras, 2023; Rellstab et al., 2020; Peijnenburg et al., 2024). We contribute to this literature by documenting important long-term health effects for spouses, even in a context where employment and income margins are not affected.

The remainder of the paper is organized as follows. In Section II. we describe our data, sample selection and empirical strategy. Section III. presents the results and is followed by the robustness checks in Section IV. Section V. discusses the implications of our findings and concludes.

II. Empirical Strategy and Data

The key identification challenge in this paper is to estimate the counterfactual outcomes for couples who experienced a health shock, had the shock not occurred. Health shocks are not randomly distributed across the population; they are influenced by factors such as medical care, health investments, habits, and risky behaviors, all of which correlate with socio-economic variables. Moreover, given the strong assortative matching within couples, income, education, and health are highly correlated between partners (Schwartz, 2013; Guner et al., 2018). A substantial body of literature documents the health gradients associated with income and education, even in contexts with universal healthcare systems like that of the Netherlands (Cutler et al., 2008; De Gelder et al., 2017). As a result, the incidence and timing of health shocks can confound other underlying health and economic trends. This creates a challenge in appropriately constructing a control group.

We address this challenge by first selecting a subset of shocks deemed unexpected by physicians (Rellstab et al., 2020), and employing a matching event-study specification following Adams et al. (2024). Specifically, using rich administrative data, we estimate these counterfactuals by matching each couple in which one person experienced a health shock in year $t = k$, to a couple from the comparison group with identical demographics and similar economic variables during year $t = k - 3$ relative to the shock. The identification assumption in this setting is that, in the absence of the partner's shock, both the initially healthy treated and control pair would exhibit the same health trends. This research design is particularly well suited to our outcome variables, especially when compared to alternative approaches that use yet-to-be-treated individuals as controls. For example, when examining effects on mortality, individuals who have not yet experienced a shock are, by definition, still alive—limiting the ability to construct relevant counterfactuals.

II.A Data and Context

Our data are linked administrative records from Statistics Netherlands for the entire Dutch population from October 1st 1994 to December 31st 2021. We use the Municipality Register (GBA), the hospital discharge register (LMR-LBZ), employment records (EWLBUS, POLISBUS, SPOLISBUS), the register of dispensed prescribed drugs (MEDICIJNTAB), and health surveys (POLS, GECON, GEMON).

Health and Social Insurance in the Netherlands

The Dutch health and social insurance system provides strong institutional support for individuals and families facing health shocks. Health insurance is universal, comprehensive, and mandatory, covering general practitioner (GP) visits, specialist and hospital care, mental health services, and medication. Regarding sick leave and disability, in the event of illness, employers must pay at least 70% of an employee's wages for up to two years, after which long-term disability benefits may be available through the national insurance system. Caregiving support is also provided: workers are entitled to short-term leave—up to twice their weekly working hours per year—typically paid at 70% of their wage, to care for a sick child, partner, or parent. Long-term caregiving leave allows up to six times the weekly working hours over 12 weeks, usually unpaid but with job protection. Finally, long-term care is provided under the WLZ Act, which guarantees access to institutional and home-based care for those with severe, chronic needs, funded through mandatory insurance contributions.

Resident and Household Register

The Municipality Register reports the date of birth, date of death and most recently recorded gender for all individuals registered in the Dutch Municipality Register at any point since October 1st, 1994, until December 31st 2021, for a total population of 26.5 million residents. The individual information is linked to household records that list household characteristics for every household formation recorded since October 1st, 1994. A separate household record exists after every change in household composition. For every household, we observe (i) the start and end dates of the recorded household, (ii) the individual identifiers of all household members, and (iii) the household type and the role of each person in the household. We use these data to identify couples in our sample.

Hospital and Prescriptions Data

The hospital discharge register contains data on both inpatient and day care patients of all general and university hospitals and most of the specialized hospitals in the Netherlands from 1995 to 2019. Each year, there are approximately two million hospitalizations of some 1.6 million distinct individuals.For the entire Dutch population, we observe (i) whether the individual entered the hospital, (ii) whether it was an acute admission, (iii) the admission and discharge date, and (iv) the main diagnosis.

The register of dispensed prescribed drugs reports whether any individual received a particular drug at least once in a given year, for all prescribed drugs covered by the basic insurance package, from 2006 to 2021.[3] We use these data to construct indicators for whether individuals were prescribed different types of drugs in any given year.

Health Surveys

These data provide cross-sectional information on lifestyle, behaviors and a broad set of health measures for a sample of the population. For the years 1997–2009, we use the Continuing Survey on Living Conditions (POLS) and its health module. For the years 2010–2021 we only use the Health Survey (GECON), which was conducted independently during this period. These surveys provide self-reported information on smoking and alcohol consumption habits, sport activities, and mental health.

We also merge our data to the Public Health Monitor survey (GEMON), which provides information on health measures and informal care provision for a large sample of respondents in the years 2012, 2016, 2020 and 2022.[4]

Labor Market Data

The employment records report information for approximately half of all jobs held

[3]As benzodiazepines were excluded from the basic health insurance package starting from 2009 - with some few exceptions - we only have information on sleeping medication and tranquilizers for the years 2006–2008.

[4]Public Health Monitor 2012, 2016, and 2020 of the Community Health Services, Statistics Netherlands and the National Institute for Public Health and the Environment.

in the Netherlands for the years 1995–2005 (EWLBUS) and for the universe of jobs for the years 2006–2021 (POLISBUS, SPOLISBUS). For every job recorded in the data, we observe (i) the employee and firm identifiers, (ii) the start and end dates of the employment contract, (iii) the total number of days and hours worked, (iv) gross salary, gross overtime pay and one-time monetary rewards, and (v) sector and type of contract. The employment records provide the economic outcomes: namely, employment, hours and income.

II.B Sample Construction

We define our treated sample as all couples in which at least one of the spouses ever suffered an unexpected health shock. For each couple, we select the first such shock to occur in our records as the main event for our analysis. We restrict the treated sample to couples in which the age of both spouses is between 25 and 55 at the time of the health shock. Additionally, we consider only couples formed at least three years prior to the health shock. Finally, we restrict the sample to couples in which neither spouse nor any child residing in the household has any hospital visits recorded for the five years preceding the exact date of the main health shock used in the analysis.[5] To make sure that this restriction captures all hospital visits in this period, we restrict the sample to couples that are continuously observed in the Netherlands for at least the five years before the health shock. As our records begin in 1995, this restriction effectively implies that the health shocks in our final sample occurred in 2000 and onward. We also exclude cases in which both spouses experienced a health shock concurrently.

We impose similar sample restrictions on the control sample, such that all control couples also satisfy the conditions related to age, couple's tenure and previous hospital visits relative to the year of the health shock of their matched treated couple. The restriction on previous hospital visits for the control sample is particularly important in our context given our focus on the initially healthy partner's health, as it ensures that we are matching couples in which the partners are comparably healthy. Notably, as mentioned before, this restriction does not allow us to perform an empirical strategy using only treated couples, as we need to impose the condition of no hospital visits for a similar number of years in both treated and control couples.[6]

In Table 1, columns 1 and 2, we present summary statistics for 4.6 million people, corresponding to the population of 25 to 55-year-olds who have been in a couple for at least three years, in 2015. The average age in this sample is 41.7 for women and 42.6 for men, with 25.3% of the population under 35 for women and 21.2% for men. Partners

[5]As young children have a much higher frequency of hospital visits for less serious conditions, we construct this restriction as no hospital visits entailing a hospitalization of one or more nights for children 0-4 years old, and as any hospital visit for children 5 years old and older.

[6]An extreme example of this restriction is that when examining the effects on mortality, not yet treated controls will necessarily be alive, and the same argument follows for hospital visits.

of women are, on average, 2.9 years older, while for men, they are 2.2 years younger. Women in this population earn approximately 50% less employment income than their partners.[7] Employment rates for women are 72.5%, and for men, they are 80.5%. The average family size in this population is 3.5. Next, columns 3 to 6 of Table 1 restrict the sample to the population of couples who experienced a shock. Our matched sample corresponds to columns 3 and 4, whereas columns 5 and 6 correspond to the unmatched sample. As expected, relative to the general population, there is a negative selection in the population that experiences a health shock: income levels and hours worked are lower, and age is higher.

II.B.1 Health Shocks

Over our observed period, on average, 7.6% of the adult population 25 to 55 years old, has a hospital visit in a given year. This share is higher for women, of whom 8.2% experience a hospital visit in a year, as compared to 6.9% of men (see Table 1). Around half of these visits entail an overnight stay in the hospital. Following Rellstab et al. (2020), we study health shocks whose onset is exogenous and cannot be medically predicted. Additionally, under our health shock definition, we exclude all hospital visits related to pregnancy or childbirth. This group of diagnoses account for 41.8% of all observed hospital visits. The incidence of this shock in our population is 3% a year.

In Tables 2 and 3, we present the most common diagnoses that we will consider as health shocks in our analysis. For women, these are: malignant breast tumors (8.4%), benign tumors in the uterus (8.3%), and appendicitis (4.3%). There is variation in the average onset of these shocks, with cancer appearing at age 45.6, whereas appendicitis affects women earlier, on average, at 39.3 years old. The severity of the shock also exhibits variation, both in the mortality risk and in the duration of the hospital stay length they entail. For breast cancer, the five-year survival probability is 85.7%, the lowest among the most common shocks. However, in the year of diagnosis, on average, patients only spend 0.75 nights in the hospital, with hospital visits increasing to 2.8 in the year after. On the other hand, acute appendicitis has a 98.7% five-year survival rate but entails 3.25 hospital nights in the year of diagnosis. For men, the most common shocks are heart attacks (6.9%), followed by appendicitis (4.7%), cardiac arrhythmias (4.1%), and benign tumors in the digestive system (3.1%). Similarly, across the most common shocks, five-year survival rates vary from 89.7% to 98.7%, and average hospital stays in the year of the shock range from 0.1 to 5.6 nights. When aggregating into diagnostic categories, in Tables C1 and C2, we find that cancer is the largest group (37%) for women, followed

[7] For 2018 the unadjusted earning gaps in the Netherlands was 47.5%, higher than the 39.6% in the European Union (EU). The hourly wage gender gap was 15.6%, compared to the 16.2% in the EU (2018).

by injuries (14.9%). For men injuries are the top category (26.5%), followed by cardiovascular conditions (22.2%).

II.C Matching

We match our treated and control samples at the couple level in year -3 relative to the health shock. We use an exact match on the gender of each spouse, age of each spouse, employment status of each spouse, an indicator for in-year fertility, number of children living in the household, region of residence, number of alive parents for each spouse, and year. Then, we apply a probability-score match on the employment income percentile of each spouse, employment income of each spouse, total hours worked of each spouse, employment tenure of each spouse, income ratio, family size, tenure of the couple, an indicator for whether the main individual had a different partner in the two years before the shock, and indicators for the presence of children in age groups 0-3, 4-11, and 12 and above in the household. We successfully match 85% of our shocks to both men and women, to control couples.[8] The matched sample consists of 163,215 unexpected hospital visits for men and 171,006 for women (columns 3 and 4 in Table 1).[9]

II.D Event-Study Specification

We identify the health and economic consequences of health shocks by comparing the evolution of each outcome y among couples who experience the event in index year t with the evolution of that outcome among the matched comparison group:

$$y_{imt} = \alpha_i + \sum_{t=-5, t\neq -2}^{t=5} \beta_t D_t + \sum_{t=-5, t\neq -2}^{t=5} \gamma_t D_t T_i + \varepsilon_{imt} \qquad (1)$$

Here, y_{imt} represents the outcome for individual i in match m at time to treatment t. $D_t = (\text{time to treatment}_t = t)$ denotes a set of dummies for each time period between $t = -5$ and $t = +5$. T_i is an indicator for treated individuals, and α_i is an individual fixed effect. Our coefficients of interest are γ_t, and we cluster standard errors at the individual level.

III. Spillover Effects on Partners

In this section, we examine the extent to which a partner's health shock affects the initially healthy spouse and explore the mechanisms and responses that explain or offset these changes.

[8] We only restrict couples to be together in $t = -3$ and $t = 0$.

[9] The unmatched sample (columns 5 and 6 in Table 1), has higher income, is younger, and has lower employment rates, for both partners.

III.A Spillover Effects on Physical Health

We start analyzing whether experiencing a partner's health shock increases the probability of future hospital visits.[10] As shown in Panel a of Figure 1, we find a significant and persistent rise in hospital visits for the initially healthy partner over the five years following the shock.[11] Specifically, in Panel a of Figure 1, we estimate that by year five, partners of shocked individuals experience 11.2% and 9.6% higher rates of hospital visits compared to the control group for women and men, respectively.[12]

This increase in the rate of hospital visits may not necessarily be the result of a health decline, but rather an increase in detection of pre-existing conditions or increased demand of healthcare services. Indeed, recent research suggests that family members' healthcare use and spending may disproportionately respond to health shocks experienced by others in the household, with such responses not necessarily being necessary or resulting in health benefits (Hoagland, 2022). As a result, it is plausible that some of the effects observed in Figure 1 could stem from this behavior rather than changes in health status. To evaluate this hypothesis, we focus on the effects on a subset of hospital visits that are more severe, including overnight stays (Panel b), hospitalizations resulting from diagnoses deemed unexpected by physicians (Panel c), and urgent visits for unexpected diagnoses that also entail three or more nights at the hospital (Panel d). Across these three categories, we observe significant short and long-term increases in the incidence of more serious hospitalizations. This would suggest that the observed patterns likely reflect a health deterioration, rather than a behavioral response.

Similarly, in Figure 2 we show that a broad range of diagnostic categories drives the increase in hospitalizations. For both women and men, there is an increase in the incidence of cancer, digestive, cardiovascular, musculoskeletal, and respiratory conditions, as well as injuries. Furthermore, this health deterioration leads to a higher mortality risk. In Figure 3 we find evidence that compared to the control group, these initially healthy partners face lower survival rates for the next five years after the shock. Specifically, at the five year mark, the increase in one-year mortality risk for women is 3.2% and 1.6% for men. All evidence that would point towards a health deterioration in this population.

[10] See Appendix A for estimates of the direct effect of a health shock on health and economic outcomes of the sick person.

[11] As we match the treated and control groups at the year level, we need to impose the restriction of no hospital visits until the time of the shock at the year level for the control group. Instead, this restriction is imposed until the precise day of the health shock for partners in the treated sample. For this reason, we cannot meaningfully interpret our estimates in year zero for all outcomes related to hospital visits. Matching at the daily level would allow us to estimate meaningful effects for year zero, but it is computationally unfeasible.

[12] We compute percent effects as the ratio between the coefficient in $t = k$ divided by the outcome variable in the control group in $t = k$. The Online Appendix shows the corresponding coefficients from the results figures, control means and sample sizes.

III.B Spillover Effects on Mental Health

Experiencing a partner's health shock imposes a significant emotional toll, and may manifest in heightened stress, anxiety, depression, and sleep disturbances (Schulz and Sherwood, 2008; Pinquart and Sörensen, 2003). In this section, we document these psychological spillovers, using both prescription data and self-reported measures of emotional well-being.

I. Depression

We start by investigating changes in the incidence of depression. To do so, we link individuals in our sample to the registry of dispensed prescription drugs and construct an indicator for whether they received a prescription for antidepressants.[13] In Panel A of Figure 4, we find that women experience a gradual increase in the likelihood of being prescribed antidepressants. By year five, they are 5.9% more likely to be taking antidepressants than women in the control group. For men, we estimate increases that persist for up to three years after the shock, peaking at 7.6%, but dissipating thereafter. In Panel b of Figure 4, we find that after the shock, initially healthy women also have an increased likelihood of stating they felt sad or depressed in the past four weeks.[14]

The severity of some of these experiences is high enough to entail a hospital visit. Panel c of Figure 4 shows the probability of a hospital visit with a mental health diagnosis. We find an increased probability of hospital visits for mental health conditions in the year following severe shocks–those characterized by above-median mortality rates–for both women and men. In contrast, we find no evidence of a similar effect for non-severe shocks.

The size of these spillovers in mental health from the initially sick partner on the healthy partner are substantially larger than those observed in terms of hospitalization and mortality. While the increase in hospital visits by the healthy partner is approximately 14% of the increase experienced by the sick partner for women, and 12% for men, when it comes to the effects on the increased use of antidepressant medication, the corresponding rates are much higher: 48% for women and 18.6% for men. Similarly, the rate at which healthy partners report feeling sad is 105% of the effect observed in sick partners for women, and 87% for men.

II. Stress and Anxiety

Another important dimension of worsening mental health is increased stress and anxiety. In Panel d of Figure 4, we find that the probability of filling a prescription for stress or anxiety management rises immediately after the shock for both women and

[13]We identify antidepressants as prescribed drugs classified with ATC code N06A.

[14]Self-reported sadness is derived from linked survey data, which corresponds to 0.12% of our sample. Further details on the data and the linkage process are provided in Section III.C.2.

men.[15] This increase, which remains relatively constant even five years after the shock, corresponds to a 3.5 and 9.1% rise for women and men, respectively. Using our linked survey data further supports this result. In Panel e of Figure 4, we note that after the shock, healthy partners have an increased likelihood of reporting feelings of nervousness in the past four weeks. Additionally, higher stress levels are reflected in increased use of medications targeting cardiovascular and digestive conditions—such as hypertension, ulcers, and diarrhea—that are commonly triggered or exacerbated by stress (Figure 5).

This evidence suggests that there is a substantial decline in mental health after a partner's health shock. These mental health effects not only persist over time but can also contribute to the physical health decline (Moussavi et al., 2007). Increased stress triggers the body's endocrine responses, which can disrupt various systems, including the cardiovascular and immune systems, thereby increasing the risk of both physical and psychiatric disorders (McEwen, 1998).[16] In Figure 6, we split the sample by whether we observe a prescription for either stress, anxiety or depression. We observe that the increases in hospital stays are substantially larger when individuals are prescribed medicine for mental health conditions, and this is particularly strong for men. On the other hand, the relationship in the opposite direction, is less clear. Figure B1 shows that increases in prescriptions for stress, anxiety and depression are not a function of hospitalization rates.

III.C Mechanisms and Responses

To better understand the factors driving these health changes, we examine the impact of caregiving burden, changes in health behaviors, and economic distress as potential contributors.

III.C.1 Caregiving and Shock Severity

Caregiving for a partner can impose a significant burden (Schulz and Sherwood, 2008). The physical demands of caregiving, especially over extended periods, may lead to mus-

[15] We identify stress medication as all prescribed drugs classified with ATC codes NC07A (Beta-blocking agents), N02B (Analgesics and antipyretics, other than opioids and anti-migraine preparations) and N03A (Antiepileptics).

[16] Physiological effects can arise from stressor-elicited endocrine responses, particularly the hypothalamic-pituitary-adrenocortical (HPA) axis and the sympathetic-adrenal-medullary (SAM) system. The HPA axis is activated by cortisol, which regulates various physiological processes including anti-inflammatory responses, metabolism, and gluconeogenesis. In contrast, the SAM system responds by releasing catecholamines, which work with the autonomic nervous system to regulate systems such as the cardiovascular, pulmonary, hepatic, skeletal muscle, and immune systems. These processes potentially increase the risk of physical and psychiatric disorders. For example, experimental research in animals shows that stress contributes to the initiation and progression of select tumors and coronary artery disease (Rozanski et al., 1999; Antoni et al., 2006), and research in humans has also shown that traumatic events can both trigger the onset of cancer (Palesh et al., 2007) and increase its mortality (Elwert and Christakis, 2008).

culoskeletal injuries, exacerbate preexisting conditions, and contribute to the neglect of a healthy lifestyle (Bauer and Sousa-Poza, 2015). In some cases, these burdens persist even beyond the caregiving period, as health shocks can have lasting effects on both health and economic outcomes (García-Gómez et al., 2013). Moreover, in severe cases, the transition from caregiving to bereavement introduces additional challenges, as individuals navigate both psychological distress and financial strain (Van den Berg et al., 2011; Hodor, 2021).

To assess the role of caregiving burden and shock severity in driving our results, we conduct a series of complementary exercises. We begin by documenting caregiving responses using the GEMON survey, to which we match 0.3% of individuals. Given the much smaller sample size, we pool years together for our regression analysis, and include survey year, region of residence in $t = -3$ and age as controls. These controls, which were part of the propensity score matching in our main specification, contribute to balance treatment and control groups in this exercise, where the sample size is too small to construct the exact and propensity matching of our main analysis. We then implement two heterogeneity analyses: the first based on the predicted caregiving burden of the shock, and the second on its severity, measured by the associated mortality risk.

Figure 7 shows a marked increase in reports of informal caregiving during the year of the shock—26.4% for women (6.6 percentage points) and 60% for men (7.8 percentage points), respectively, relative to baseline rates of 25% and 13% for the control in the same year. However, this increase appears to be short-lived: reports of current caregiving are no longer statistically significant in the post-shock period (Panel c). Additionally, we find no significant rise in reports of a heavy caregiving load (Panel d), suggesting that caregiving demands are generally not time-intensive—likely reflecting the availability of generous long-term care supports in the Netherlands (Jongen, 2017; Schut et al., 2013; Rellstab et al., 2020).

We next examine whether health spillovers differ by the predicted intensity of caregiving required. Using survey-based caregiving reports by diagnosis, we classify conditions as having high or low caregiving needs, based on whether they fall above or below the median caregiving burden.[17] Figure 8 shows that hospitalization rates do not differ significantly by caregiving intensity, either at the time of the health shock or in subsequent years. However, when it comes to survival outcomes, female partners experience an increased mortality risk following high-intensity caregiving shocks, whereas no such effect is observed for male partners.

To illustrate the importance of shock severity and potential bereavement, we examine

[17] We identify all couples in the population in which both partners are between 25 and 55 years old at the time one partner experiences a health shock. The healthy partner is then matched to the GEMON survey (2012, 2016, 2020, 2022), and we compute the average number of reported hours of informal care they provide in the year of the shock and in the two subsequent years. For each diagnosis with at least 10 matched couples, we calculate the median number of caregiving hours, disaggregated by sex. We classify a diagnosis as having high caregiving needs if the average number of informal care hours provided by the healthy partner exceeds the overall median.

heterogeneity by the severity of the health shock, proxied by its associated mortality risk. We define a severe or high-mortality shock as a diagnosis with an above-median ten-year mortality rate, and a low-mortality (non-severe) shock as one below the median.[18] Panels (a) and (b) of Figure 9 show that spillovers onto hospitalization rates are higher following high-severity shocks compared to low-severity ones, particularly for women. In the first year after the shock, hospitalization rate increases are 38% higher for women and 22% higher for men in the high-severity group relative to the low-severity group. These differences grow over time, and by year five, the risk nearly doubles for high-severity shocks compared to low-severity ones. Specifically, the probability of hospitalization rises to 18.5% for women and 14.9% for men following a high-severity shock, compared to 9.4% and 7.3%, respectively, after a low-severity shock. The effects on survival are even more pronounced. As shown in panels (c) and (d), by year five, the mortality rates of partners are 2.8 times higher for women and 4.3 times higher for men when the initial health shock is severe compared to when it is not. These patterns suggest that the severity of the initial shock—and the associated mortality risk—are critical drivers of health deterioration in partners.

Finally, Fadlon et al. (2025) show that in the United States, a health shock among *older* couples reduces the likelihood that the initially affected partner provides informal care to the unaffected partner, potentially leading to downstream consequences for the household. In contrast, as shown in Figure B2, we find no comparable change in our context: the probability that the affected partner provides care remains statistically unchanged following the shock. Opposite to Fadlon et al. (2025), we find an increase in the intensive margin of informal care provision when the initially affected partner is the men. These differences may reflect both the older age of the U.S. sample—implying more severe and incapacitating shocks—and differences in institutional context. In particular, the more limited availability of publicly funded short- and long-term care in the U.S. increases households' reliance on informal caregiving (Barczyk and Kredler, 2018).

III.C.2 Health Behaviors

Behavioral responses may play a crucial role in shaping health outcomes, as family health spillovers influence individual health behaviors. On one hand, some evidence suggests that health shocks can lead to lasting positive changes; for example, Fadlon and Nielsen (2019) find that spouses permanently improve their health behaviors following nonfatal heart attacks and strokes. On the other hand, the stress and burden of a partner's health shock may lead individuals to adopt harmful coping strategies, such as increased smoking, reduced physical activity or sleep, poor dietary choices, and diminished adherence to

[18] We identify all individuals in the population who experience a health shock between ages 25 and 55. For each diagnosis, we calculate the average ten-year mortality rate, separately by sex. Diagnoses with mortality rates above the median are classified as high-mortality risk.

medical regimens, all of which can contribute to their own health decline (Firth et al., 2020).

A. Exercise, Smoking and Drinking

We first analyze changes in risky behaviors, such as smoking, drinking, and physical activity. To study the role of health behaviors, we link our sample to nationally representative health surveys conducted by Statistics Netherlands. We match 0.12% of individuals, and this linkage is balanced across treatment and control groups, time to shock, and sex. Given this smaller sample size, we pool observations and estimate a difference in difference regression, rather than the full event-study, and include survey year, region of residence in $t = -3$ and age as controls.

As shown in Table 4, we find no evidence of increased unhealthy habits following a partner's health shock. Instead, our estimates suggest that women become more likely to report never drinking and increase their exercise levels. For men, we observe a rise in both the frequency and intensity of exercise around the time of the shock. Smoking rates remain unchanged.

B. Sleep

A substantial body of research underscores the importance of sleep as a key determinant of health (Luyster et al., 2012; Grandner, 2022). Increased stress and depression have been strongly linked to declines in sleep quality and duration (Kalmbach et al., 2018; Oh et al., 2019). To examine sleep disorders, we focus on the period from 2006 to 2008, when sleeping medications were covered by public health insurance.[19] Due to data limitations, we cannot construct a full 11-year panel for sleep medication use, so we include a control for calendar year in our specification.

Panel f of Figure 4 shows an immediate increase in the likelihood of filling a prescription for sleep medication, and this increase is more than twice the size for women (22.0%) compared that of men (4.9%). Moreover, for women, this effect remains high and statistically significant for the years following the shock.[20]

III.C.3 Economic Distress

Finally, financial distress can be a potential contributing factor to the health decline. In our context, however, this decline occurs despite no significant changes in labor market outcomes following the initial shock. This finding aligns with previous research in the Netherlands and is confirmed by our results in Figure 10. We find no evidence of employment adjustments, either at the extensive or intensive margin. Given our sample size,

[19] We classify sleep medications as drugs with ATC codes N05B (Anxiolytics) and N05C (Hypnotics and sedatives).

[20] Figure B3 presents estimates from an alternative specification without individual fixed effects. Compared to our preferred model, these estimates are larger in magnitude but more precise in the post-shock period.

we can rule out employment effects larger than 0.0028pp for women and 0.0038pp for men. These estimates remain robust even when we stratify the sample by shock severity (Figure B4) or by income level (Figure B6). When estimating changes in total household income we find small permanent declines in total income of around 2.2% for initially healthy men, and 5.9% for women (Panel d of Figure 10). This income decline is similar across income terciles (Figure B7), and it is likely an overestimation given that we do not include disability or welfare transfers these households may be receiving.

When examining health spillovers among couples from different income groups, Figure B5 reveals an income gradient in the first year following the shock: partners in the bottom tercile of household income experience an increase in hospitalizations more than twice as large as that of those in the top tercile. However, this gap diminishes over time, and by the fifth year after the shock, there is no meaningful difference in spillovers.

Overall, our findings show that a partner's health shock has significant and persistent spillover effects on the initially healthy spouse, leading to both physical and mental health deterioration. The decline in physical health is especially pronounced when accompanied by mental health disorders. While caregiving responsibilities do arise, they appear to be relatively short-lived in this context—likely due to the availability of robust long-term care support. Relatedly, economic distress does not appear to be a major contributing factor. Instead, our analysis points to a combination of depression and stress responses, behavioral adaptations—such as worsened sleep—and the mortality risk associated with the initial shock as key mechanisms driving these health declines.

IV. Robustness

In this section we provide evidence of the validity of our identification strategy and conduct different robustness checks.

IV.A Identification and Regression Specification

A natural threat to our identification relates to the possibility that, given assortative matching in health outlooks, the health of the initially healthy partner would have deteriorated even in the absence of the other partner's health shock. To address this concern, we show that there is no evidence of anticipation of the shock in "untargeted" health and economic variables such as medication prescriptions, employment, hours worked and income. This can be observed when we examine Figure 10 for economic outcomes and Figures 4 and 5 for medication. The coefficients of the pre-trends are statistically insignificant both individually and jointly. There is also no clear pattern or trend in the data.

Second, in Figure 11, we extend our sample to include an additional five years in the pre-period, covering the time frame from ten to six years before the shock, during which no restrictions on hospitalizations are imposed. This analysis shows no evidence of differences in hospitalization rates, which would suggest different health trajectories.

We also examine the health trends of extended family members such as parents and siblings, with whom genetic endowments and health habits are shared. This exercise is possible given the richness of Dutch municipal registers, which allow to identify the parents and siblings of the individuals in our sample, in order to follow the health outcomes of the entire family tree. In Figure 12 we plot event study estimates for hospitalizations. We find there is not any meaningful difference between sibling and parents across treatment and control groups before the onset of the shock. This evidence further suggests that the health effects we estimate for the initially healthy partner, are the result of the shock measured at $t = 0$, and not the result of an underlying and contemporaneous health decline.

As an additional exercise, we show that our results remain consistent when we examine a subset of shocks that are either more likely to be sudden or less likely to result from correlated health behaviors. To demonstrate this, we construct four different sets of shocks. First, we restrict our shocks to those that begin in the emergency room (42%) or have an urgent code from the referring physician. Second, we consider "undeferrable" shocks, that occur with the same frequency across all days of the week (4.2%).[21] Third, we limit our sample to acute appendicitis events (4.5%). Fourth, we restrict our sample to injuries (20.6%). Figure B8 presents the results on hospitalizations for these sets of shocks. We find that, the patterns are similar to our baseline specification across all definitions, even when the sample size is substantially smaller and the severity of the shocks varies significantly. Results for appendicitis are smaller and shorter lived, which is expected given the overall low severity and ambulatory nature of the shock.

An alternative explanation is that our effects are the result of common shocks (i.e car accident, poisonings, etc) rather than spillovers. To address this concern, in Figure B9, we exclude couples who experienced hospital visits less than one week or one month apart, respectively. We find that our results remain invariant to this exclusion. Similarly, a concern is that our results might reflect correlated risks rather than spillover effects. In Figure B10 we provide evidence against this hypothesis by examining effects on hospitalizations for diagnosis categories different from the initial shock. For these cases, we find substantial increases in the probability of a hospitalization, consistent across multiple diagnosis categories. Specifically, we observe that 92% of the hospitalizations arise from conditions where this was not the category of the partner's initial shock. As a placebo

[21] Specifically, for each shock diagnosis, we use the full hospital records and test if the proportion of weekend admissions is equal to the proportion of weekday admissions (Rellstab et al., 2020). We identify a shock diagnosis as "undeferrable" if we do not reject the null hypothesis at the 1% level. Most of the shocks classified as"undeferrable" are injury diagnoses.

check we look at the effect on hospitalizations with a diagnosis for a congenital condition, which cannot be affected by the spillovers. In Figure B11 we see that there is no evidence of effects on this margin.

Finally, to assess the sensitivity of our results to our shock definition and specification, in Figure B12 we run different specifications where we include match fixed effects rather than individual fixed effects, and additional versions where we add year fixed effects. The results are very stable across these changes in specifications. Furthermore, in Figure B13 we further refine the definition of the shock to assess the sensitivity of the effects to different definitions. The first definition includes only shocks that entail a hospital stay of three days or longer, and the second adds the further restriction of an urgent intake. We find that our estimated spillovers are present across all shock definitions and that the point estimates and dynamics are very similar.

IV.B Event-Study Specification

The literature on identification using differences in differences and event-study strategies has highlighted several threats that can arise in some applications (De Chaisemartin and d'Haultfoeuille, 2020; Callaway and Sant'Anna, 2021; Sun and Abraham, 2021; Borusyak et al., 2024). A key insight from this literature is that using already treated units as future controls can lead to biased estimates in contexts where effects are dynamic. This is not a concern in our application, as we do not allow treated couples to be part of the control group after their treatment. In our context, however, our control group is composed of never treated and not yet treated individuals. This aligns with the treatment effect we aim to estimate, which corresponds to the effects of having a partner who experiences a health shock at $t = 0$, rather than no shock at all.

This literature also highlights that bias can arise in contexts where treatment is not absorbing and can be turned on and off. Here, we define treatment as absorbing, meaning a couple will always be considered treated when one of the partners receives a health shock. In a context with heterogeneous dynamic paths by time or cohort, event-study estimates can mismeasure dynamic effects. This is potentially a concern in our application as we study shocks over a long period (1999 to 2020) and a wide age range (25 to 55 years old). To assess the extent to which our estimates are driven by period and cohort composition, rather than average treatment dynamics, we split our sample into: i) shocks that occurred in the first half versus the second half of our period, and ii) by age, distinguishing between those below and above 40 years old. Figures B14 and B15 illustrate these analyses. The results indicate that the magnitude of the coefficients and the overall dynamics remain consistent across the different data splits.

Finally, papers in the literature on health shocks often use a sample of only treated individuals and leverage the timing of the shock (e.g. Fadlon and Nielsen (2021)). This strategy is suited to study outcomes such as income, employment, or spending, among others. However, for the outcomes we study, this strategy does not yield relevant counterfactuals. An extreme example is the case of survival. By construction, the not-yet-treated individual will be alive when used as a control. Also for outcomes such as hospitalization, the control group will be constrained to be healthier up to the time they become treated.

V. Discussion

In this paper, we show how an individual's health is directly influenced by the well-being of their partners. Following a health shock within a couple, we observe a sustained increase in hospital visits, a higher likelihood of hospitalization, and elevated mortality rates for the initially healthy partner. The health decline is broad in scope and is accompanied by increased stress and mental health strain. These findings underscore the interconnectedness of health within families and challenge traditional models that view health and health-related behaviors as purely individual experiences. In particular, our results highlight spousal illness as a significant risk factor with long-term consequences, especially for mental health, where spillover effects are notably large.

Our analysis is set in a context with generous and universal healthcare, as well as comprehensive disability and caregiving support. In such a setting, financial stress and caregiving burdens are likely to be lower than in countries with less extensive safety nets. As a result, our estimates likely represent a lower bound on the average effects of spousal health shocks. Taken together, these insights emphasize the importance of extending medical attention and support to patients' partners, recognizing them as part of the broader care network.

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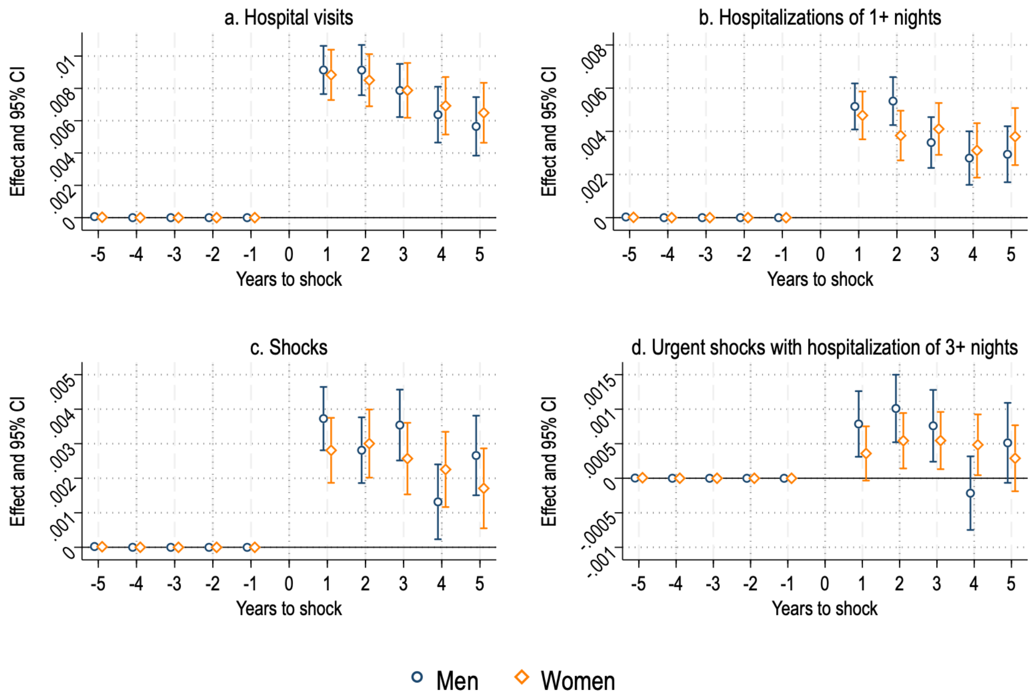
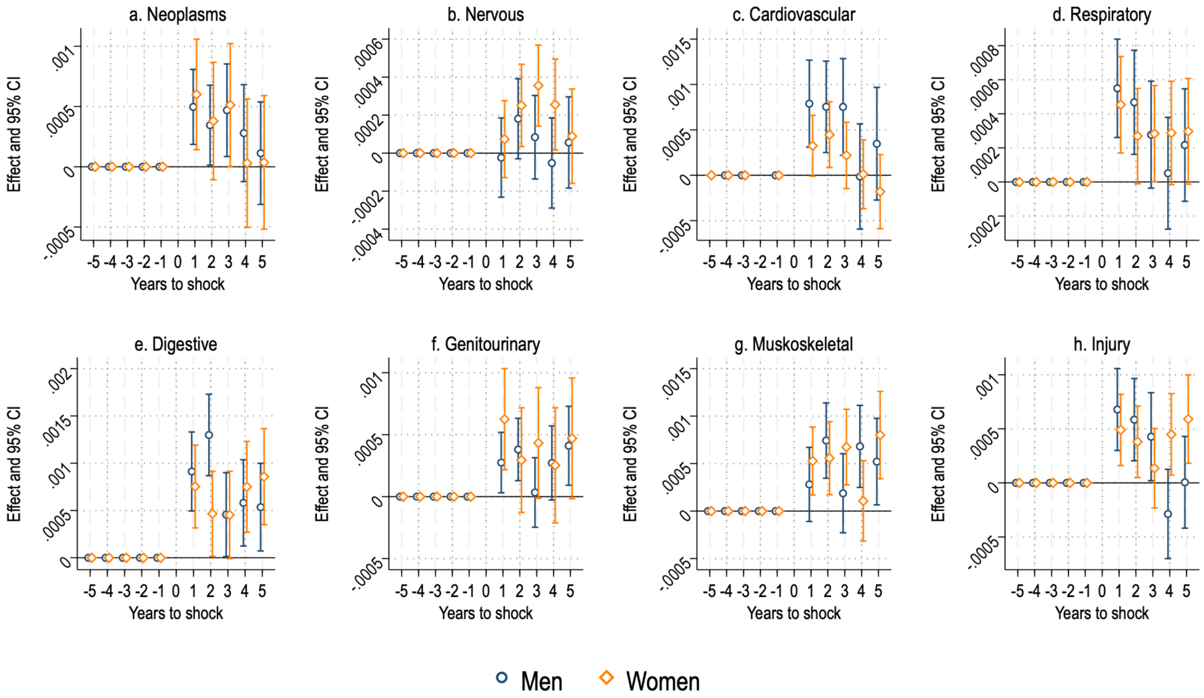
VI. Figures

Figure 1: Health Spillover Effects



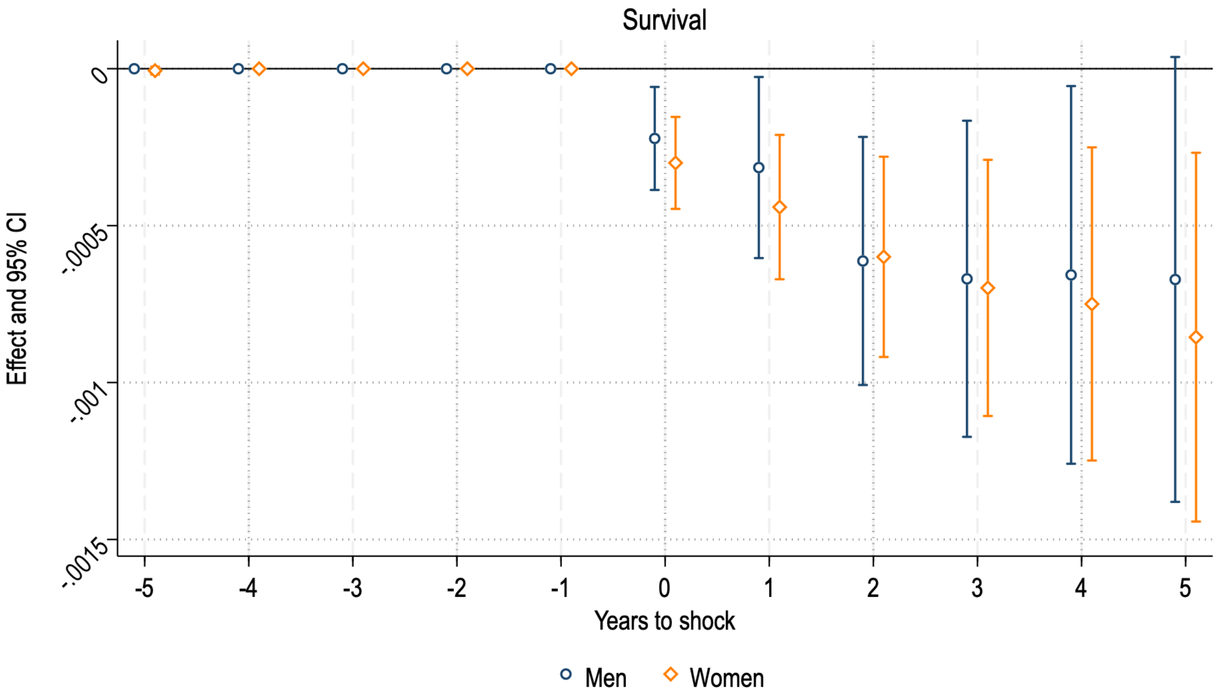
Notes: This figure shows our estimates for the coefficients γ_t from Eq. 1. Specifically, it shows the effect of a health shock in $t = 0$ on hospital visits, hospitalizations, shocks and urgent shocks that were followed by a hospitalization of at least 3 nights in the subsequent five years for the partner of the shocked individual. Hospital visits include all visits to the hospital, with the exception of visits with a diagnosis related to pregnancy or childbirth. Hospitalizations include all hospital visits, with the exception of visits with a diagnosis related to pregnancy or childbirth, for which the patient was discharged one day or more after admission. Following García-Gómez et al. (2013) shocks identify hospital visits whose onset is exogenous and cannot be medically predicted, based on the diagnosis. Urgent shocks with a hospitalization of 3 or more nights include all shocks for which the hospital visit started at the Emergency Room or that have an urgent code from the referring physician, and for which the patient was discharged three days or more after admission. Coefficients for $t = -5$ to -1 are zero by construction due to our sample restrictions. Coefficients for $t = 0$ cannot be interpreted meaningfully given the different sample restrictions imposed on partners in the treated and control groups. In particular, we match treated and control couples at the yearly level due to computational feasibility. This implies that we need to impose the restriction of no previous hospital visits until the year of the shock for partners in the control sample, while the restriction is imposed until the precise date of the shock for partners in the treated sample. Standard errors are clustered at the individual level. This figure is referenced in Section III.A.

Figure 2: Hospitalizations by Diagnosis Category: Initially Healthy Partner



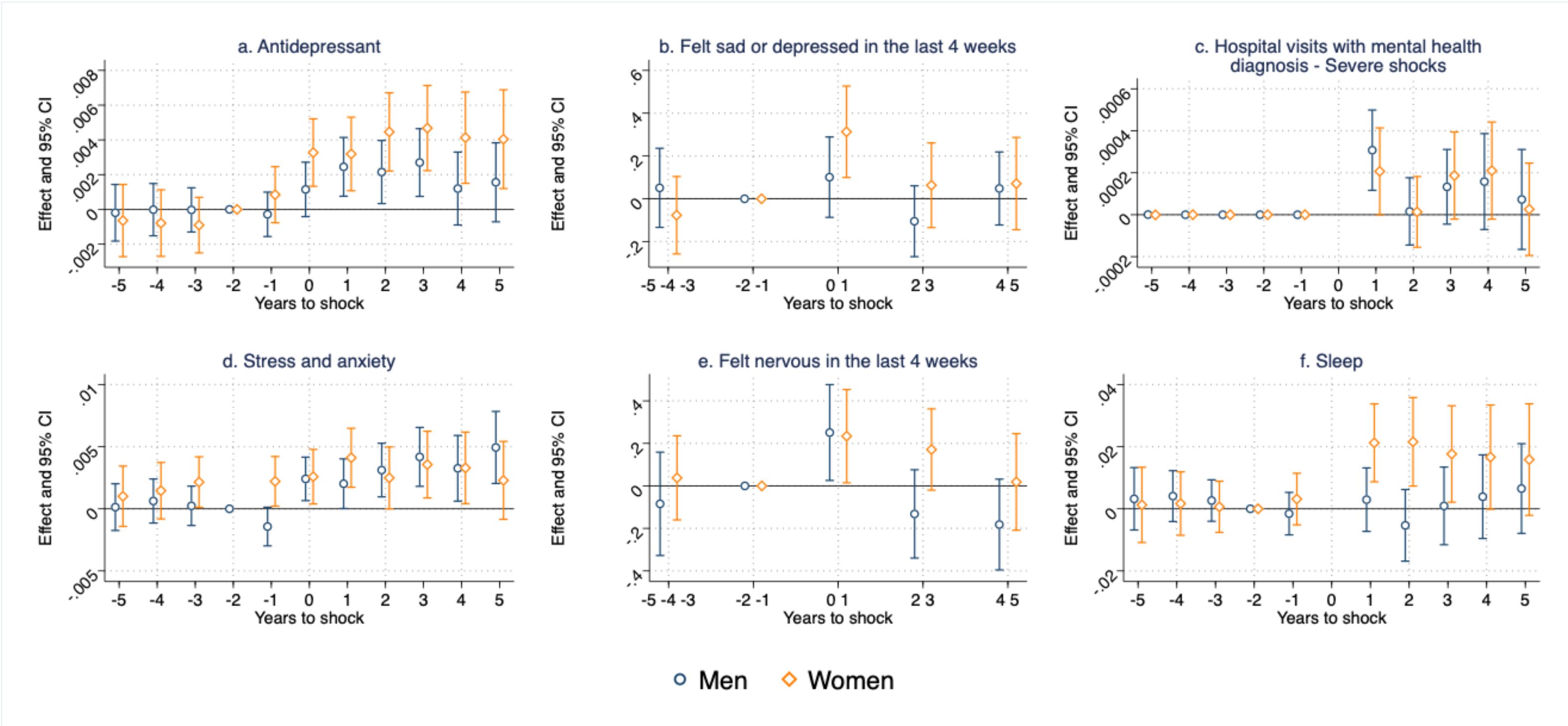
Notes: This figure shows our estimates for the coefficients γ_t from Eq. 1. Specifically, it shows the effect of a health shock in $t = 0$ on hospitalizations by broad diagnosis category in the subsequent five years for the partner of the shocked individual. Hospitalizations include all hospital visits, with the exception of visits with a diagnosis related to pregnancy or childbirth, for which the patient was discharged one day or more after admission. We show results for hospitalizations in the following diagnosis categories: neoplasms (ICD-9 codes 140-239), diseases of the nervous system and sense organs (320-389), diseases of the circulatory system (390-459), diseases of the respiratory system (460-519), diseases of the digestive system (520-579), diseases of the genitourinary system (580-629), diseases of the musculoskeletal system and connective tissue (710-739), injury (800-999). Coefficients for $t = -5$ to -1 are zero by construction due to our sample restrictions. Coefficients for $t = 0$ cannot be interpreted meaningfully given the different sample restrictions imposed on partners in the treated and control groups. In particular, we match treated and control couples at the yearly level due to computational feasibility. This implies that we need to impose the restriction of no previous hospital visits until the year of the shock for partners in the control sample, while the restriction is imposed until the precise date of the shock for partners in the treated sample. Standard errors are clustered at the individual level. This figure is referenced in Section III.A.

Figure 3: Health Spillover Effects: Survival Rates



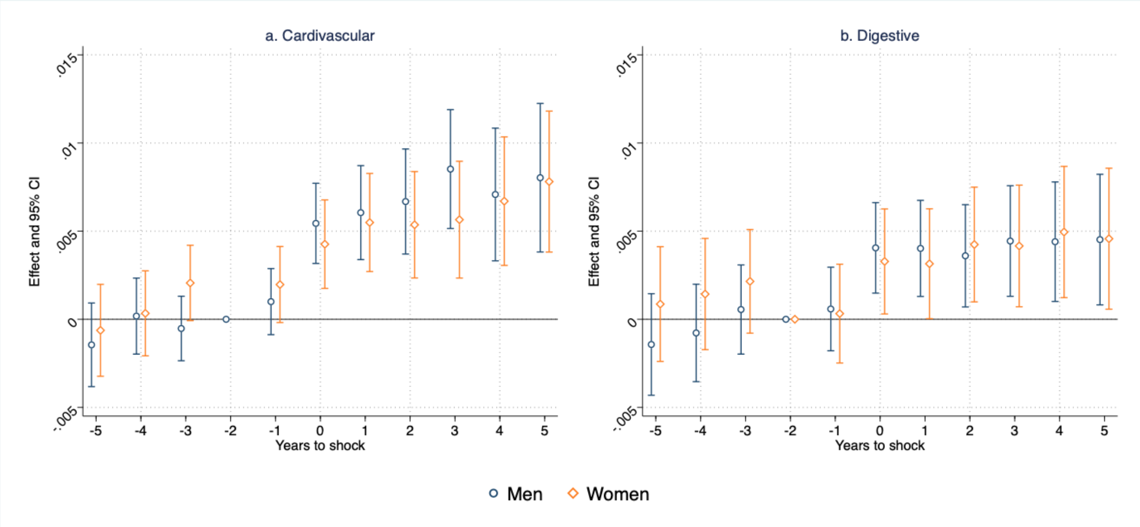
Notes: This figure shows our estimates for the coefficients γ_t from Eq. 1. Specifically, it shows the effect of a health shock in $t = 0$ on survival in the subsequent five years for the partner of the shocked individual. We construct survival based on individual's year of death. Coefficients for $t = -5$ to -1 are zero by construction due to our sample restrictions. Standard errors are clustered at the individual level. This figure is referenced in Section III.A.

Figure 4: Health Spillover Effects: Mental Health



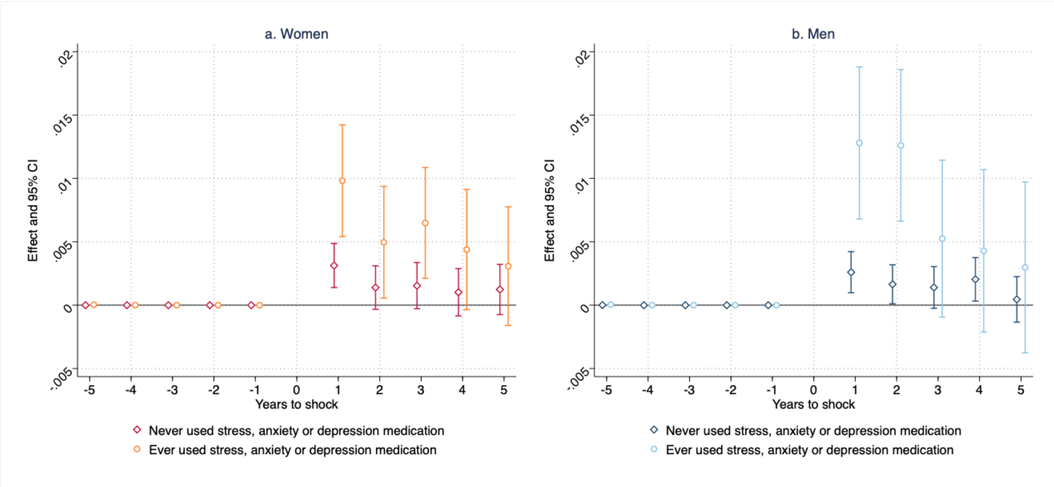
Notes: The graphs in Panels a and d of this figure show our estimates for the coefficients γ_t from Eq. 1, for the effect of a health shock in $t = 0$ on consumption of antidepressants and stress management medication in the subsequent five years for the partner of the shocked individual. We construct the outcomes as indicators equal to 1 if the individual filled any prescription for a stress medication (ATC codes NC07A, N02B, N03A, N05B, N05C, N06A) or antidepressants (ATC code N06A) during the year. Panel f shows our estimates for the coefficients γ_t from a specification analogous to Eq. 1, where we add a control for calendar year. We make this adjustment as we only observe the prescription of sleeping medication for three years (2006–2008), when they were covered by public health insurance, so we cannot construct a panel of sleeping medication usage for the full 11-year period around the time of the shock. Specifically, the figure shows the effect of a health shock in $t = 0$ on the prescription of sleeping medication (ATC codes N05B and N05) in the subsequent five years for the partner of the shocked individual. The indicator for sleep medication is equal to 1 if the individual filled any prescription for a drug in this category during the year. Panels b and e show our estimates for the coefficients γ_t from a specification analogous to Eq. 1, where we pool together single years to treatment. Specifically these panels show the effect of health shocks in $t = 0$ on two self-reported mental health measures for the partner of the shocked individual. Respondents answered the following two questions on a scale of 1 to 5 based on frequency:"In the past 4 weeks I felt depressed and gloomy" and "In the past 4 weeks I felt very anxious". We standardize these variables and estimate the effects of the health shock in the subsample which we are able to link to the health survey. Panel c shows our estimates for the coefficients γ_t from Eq. 1 for hospital visits with a mental health diagnosis (ICD9 codes 290-319) for the subsample of initial shocks classified as severe. We classify shocks as severe if the mortality risk associated with the diagnosed condition for the control group is above the median. Coefficients for $t = -5$ to -1 are zero by construction due to our sample restrictions. Coefficients for $t = 0$ cannot be interpreted meaningfully given the different sample restrictions imposed on partners in the treated and control groups. In particular, we match treated and control couples at the yearly level due to computational feasibility. This implies that we need to impose the restriction of no previous hospital visits until the year of the shock for partners in the control sample, while the restriction is imposed until the precise date of the shock for partners in the treated sample. This figure is referenced in Section III.B.

Figure 5: Health Spillover Effects: Medication Prescriptions



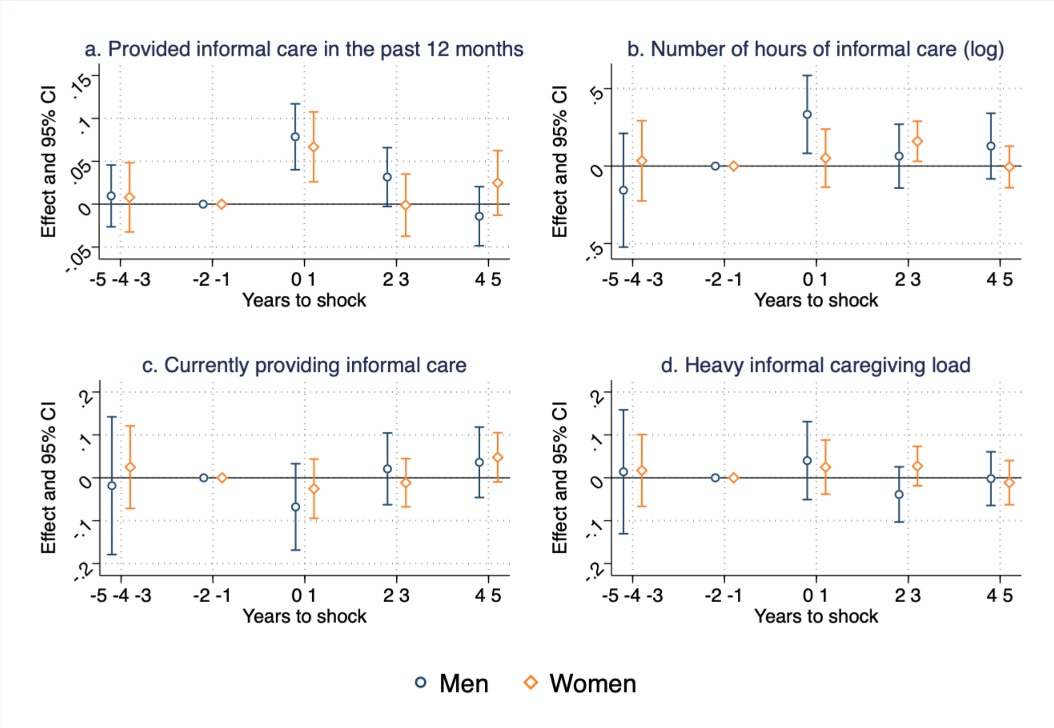
Notes: This figure shows our estimates for the coefficients γ_t from Eq. 1. Specifically, it shows the effect of a health shock in $t = 0$ on the prescription of medication for cardiovascular (ATC codes C) and digestive (ATC codes A) conditions in the subsequent five years for the partner of the shocked individual. The indicators for are equal to 1 if the individual filled any prescription for a drug in this category during the year. Standard errors are clustered at the individual level. This figure is referenced in Section III.B.

Figure 6: Hospitalization Effects by Stress, Anxiety or Depression Medication Use



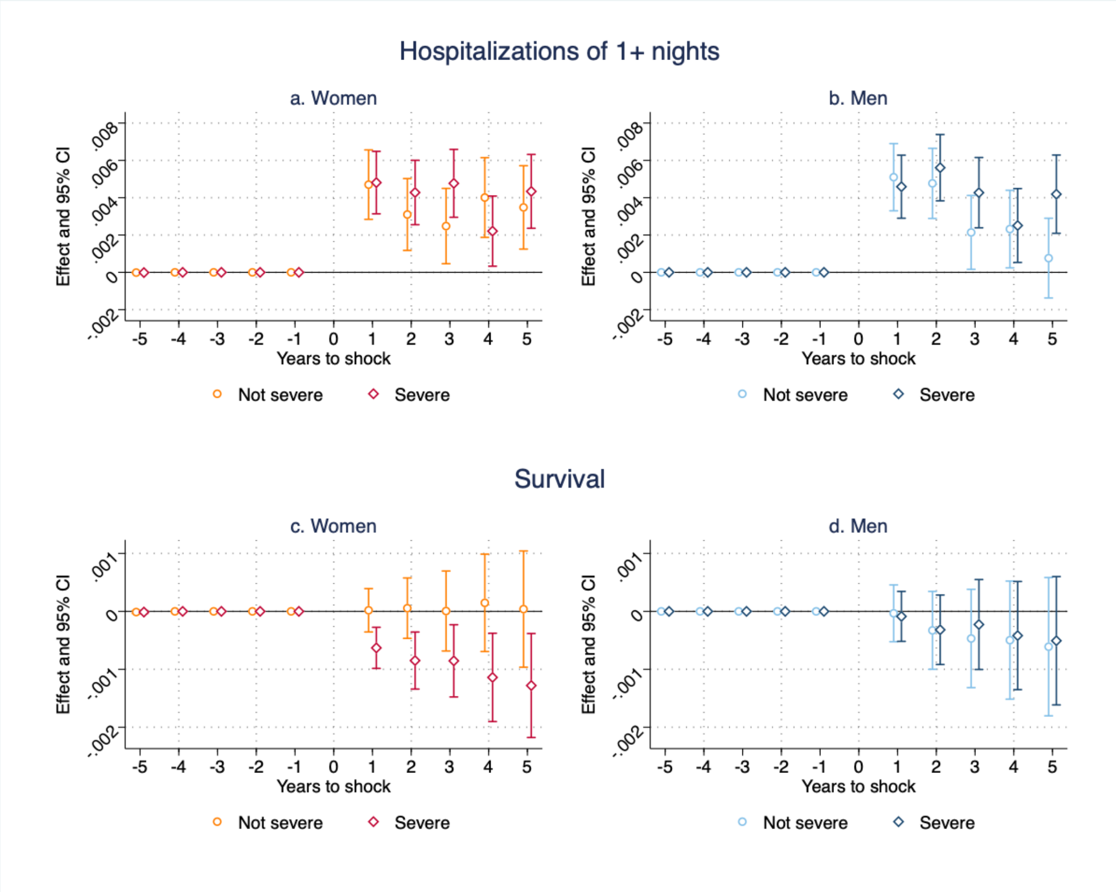
Notes: This figure shows our estimates for the coefficients γ_t from Eq. 1. Specifically, it shows the effect of a health shock in $t = 0$ on hospitalizations in the subsequent five years for the partner of the shocked individual. Hospitalizations include all hospital visits, with the exception of visits with a diagnosis related to pregnancy or childbirth, for which the patient was discharged one day or more after admission. The effects are shown separately by stress, anxiety, or depression medication use. An individual is classified as having used stress, anxiety or stress medication if a prescription for any such medication is ever recorded between $t = -5$ and $t = 5$. Coefficients for $t = -5$ to -1 are zero by construction due to our sample restrictions. Coefficients for hospitalizations in $t = 0$ cannot be interpreted meaningfully given the different sample restrictions imposed on partners in the treated and control groups. In particular, we match treated and control couples at the yearly level due to computational feasibility. This implies that we need to impose the restriction of no previous hospital visits until the year of the shock for partners in the control sample, while the restriction is imposed until the precise date of the shock for partners in the treated sample. Standard errors are clustered at the individual level. This figure is referenced in Section III.B.

Figure 7: Informal Caregiving by the Healthy Partner



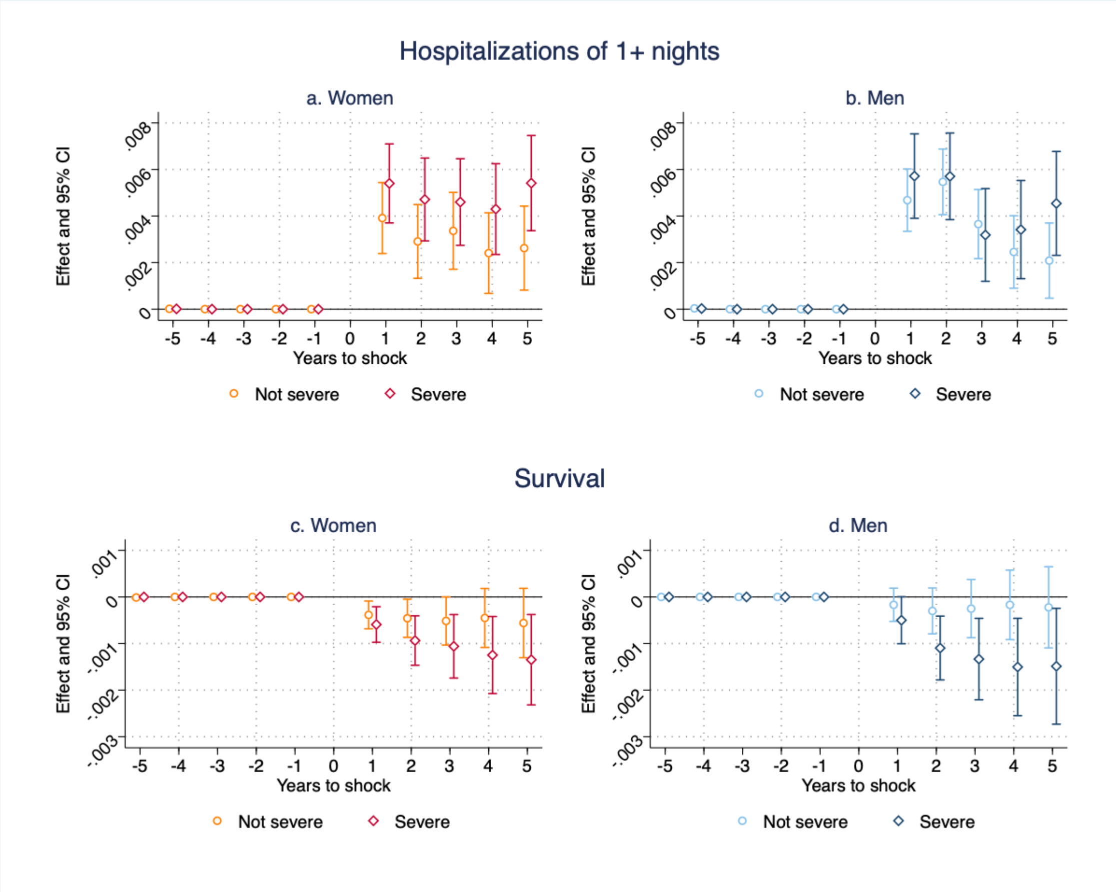
Notes: This figure shows our estimates for the coefficients γ_t from a specification analogous to Eq. 1, where we pool together single years to treatment and include for survey year, region of residence in $t = -3$ and age as controls. Specifically it shows the effect of health shocks in $t = 0$ on a set of measures of informal caregiving provision in the subsequent five years for the partner of the shocked individual. Provided informal care in the past 12 months is an indicator for wether the individual reports any informal caregiving provision in the previous year. Hours of informal care provided are conditional on the individual providing any care. Currently providing informal care is an indicator for wether the individual reports any informal caregiving provision at the time of the interview. Heavy informal caregiving load is an indicator that captures informal caregiving provision for more than 8 hours per week or that has lasted for more than 3 months. The variable that captures informal care provision in the past 12 months is only available in survey years 2012, 2016 and 2020; all other variables are available in all survey years. This figure is referenced in Section III.C.1.

Figure 8: Health Spillovers by Caregiving-Need Intensity



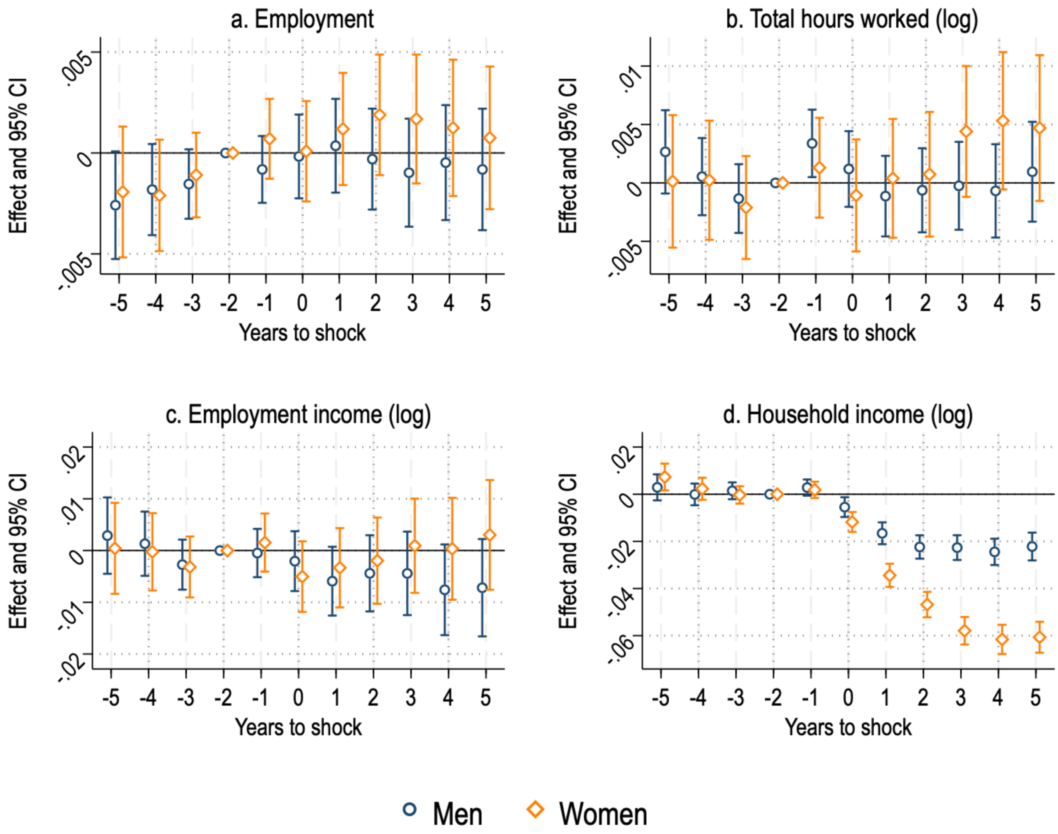
Notes: This figure shows our estimates for the coefficients γ_t from Eq. 1. Specifically, it shows the effect of a health shock in $t = 0$ on hospitalizations and survival in the subsequent five years for the partner of the shocked individual. Hospitalizations include all hospital visits, with the exception of visits with a diagnosis related to pregnancy or childbirth, for which the patient was discharged one day or more after admission. We construct survival based on individual's year of death. The effects are shown separately by caregiving-need intensity of the initial shock. We classify shocks as high caregiving-need if the reported informal caregiving provision associated with the diagnosed condition for the control group is above the median. Coefficients for $t = -5$ to -1 are zero by construction due to our sample restrictions. Coefficients for hospitalizations in $t = 0$ cannot be interpreted meaningfully given the different sample restrictions imposed on partners in the treated and control groups. In particular, we match treated and control couples at the yearly level due to computational feasibility. This implies that we need to impose the restriction of no previous hospital visits until the year of the shock for partners in the control sample, while the restriction is imposed until the precise date of the shock for partners in the treated sample. Standard errors are clustered at the individual level. This figure is referenced in Section III.C.1.

Figure 9: Health Spillovers by Shock Severity: Proxied by Mortality Risk



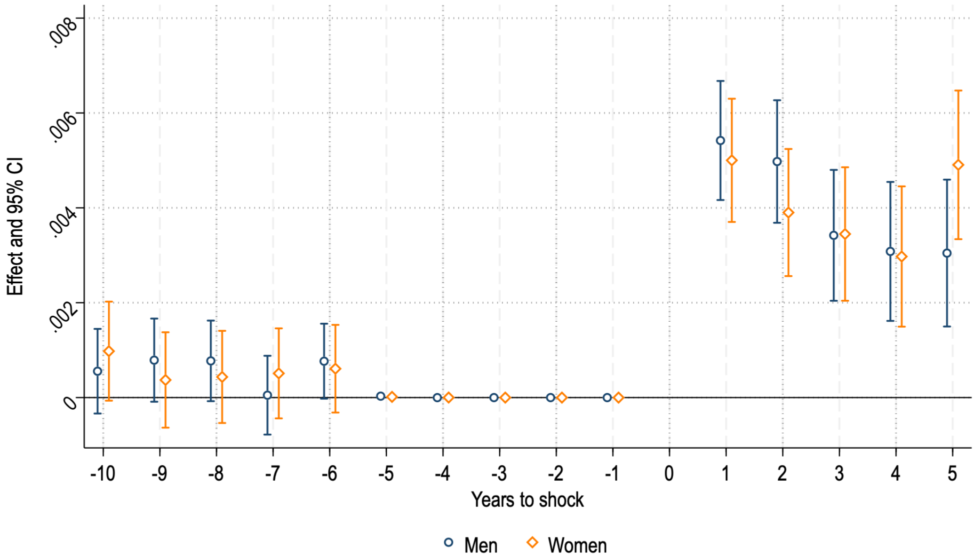
Notes: This figure shows our estimates for the coefficients γ_t from Eq. 1. Specifically, it shows the effect of a health shock in $t = 0$ on hospitalizations and survival in the subsequent five years for the partner of the shocked individual. Hospitalizations include all hospital visits, with the exception of visits with a diagnosis related to pregnancy or childbirth, for which the patient was discharged one day or more after admission. We construct survival based on individual's year of death. The effects are shown separately by severity of the initial shock. We classify shocks as severe if the mortality risk associated with the diagnosed condition for the control group is above the median. Coefficients for $t = -5$ to -1 are zero by construction due to our sample restrictions. Coefficients for hospitalizations in $t = 0$ cannot be interpreted meaningfully given the different sample restrictions imposed on partners in the treated and control groups. In particular, we match treated and control couples at the yearly level due to computational feasibility. This implies that we need to impose the restriction of no previous hospital visits until the year of the shock for partners in the control sample, while the restriction is imposed until the precise date of the shock for partners in the treated sample. Standard errors are clustered at the individual level. This figure is referenced in Section III.C.1.

Figure 10: Economic Spillovers: Initially Healthy Partner



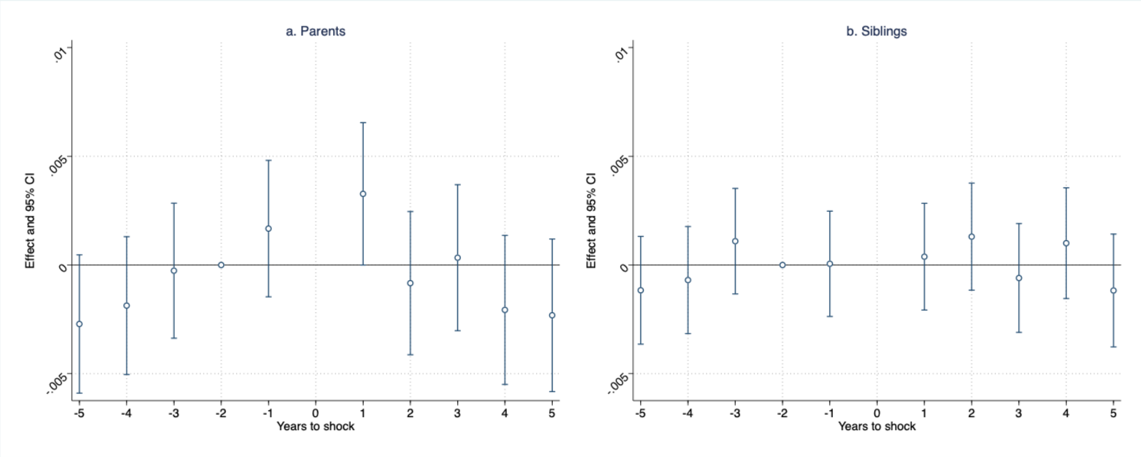
Notes: This figure shows our estimates for the coefficients γ_t from Eq. 1. Specifically, it shows the effect of a health shock in $t = 0$ on employment, the log of the total number of hours worked, the log of employment income, and the log of household income, in the subsequent five years for the partner of the shocked individual. Employment is only defined for years 2006 onward, as we do not have data on the universe of jobs for the previous years. Total hours worked and employment income are conditional on the individual being employed; these variables are defined for the entire period. Standard errors are clustered at the individual level. This figure is referenced in Section III.C.3.

Figure 11: Extended Pre-period: Hospitalizations



Notes: This figure shows our estimates for the coefficients γ_t from Eq. 1. Specifically, it shows the effect of a health shock in $t = 0$ on hospitalizations in the subsequent five years for the partner of the shocked individual. Coefficients for $t = -5$ to -1 are zero by construction due to our sample restrictions. Coefficients for hospitalizations in $t = 0$ cannot be interpreted meaningfully given the different sample restrictions imposed on partners in the treated and control groups. Standard errors are clustered at the individual level. This figure is referenced in Section IV.A.

Figure 12: Health Evolution of Other Family Members



Notes: This figure shows our estimates for the coefficients γ_t from Eq. 1. Specifically, it shows the effect of a health shock in $t = 0$ on hospitalizations in the subsequent five years for relatives of the partner of the shocked individual. Hospitalizations include all hospital visits, with the exception of visits with a diagnosis related to pregnancy or childbirth, for which the patient was discharged one day or more after admission. Panel a shows these effects for the parents of the partner of the shocked individual, while Panel b shows these effects for their siblings. The indicator for hospitalizations are equal to 1 if any of the parents (or any of the siblings) experienced the event during the year. Standard errors are clustered at the individual level. This figure is referenced in Section IV.A.

VII. Tables

Table 1: Summary Statistics

Variable	**General Population 2015**		**Matched Sample at t=-1**		**Unmatched Sample at t=-1**	
	Women	Men	Women	Men	Women	Men
Age	41.7	42.6	41.8	43.8	40.3	43.0
Age 25-35	25.3%	21.2%	20%	14%	25%	15%
Age 36-45	34.7%	35.1%	38%	34%	42%	38%
Age 46-55	40.0%	43.7%	42%	52%	32%	47%
Age of partner	44.6	40.4	43.8	41.5	42.3	39.4
Employment income	22,314	41,898	17,330	37,294	23,991	44,797
Employment income of partner	42,193	22,057	23,468	10,914	33,815	17,908
Employed	72.5%	80.5%	77.9%	85.3%	62.4%	67.5%
Employed partner	78.6%	73.4%	86.0%	78.3%	60.5%	55.6%
Total hours per week	21.2	32.2	16.5	27.9	21.3	31.7
Divorce rate (one year)	2.7%	2.9%				
Divorce rate (five years)	9.8%	10.7%				
Family size	3.5	3.5	3.4	3.4	3.6	3.7
Main earner in the household	25.3%	75.8%	19.7%	79.0%	29.0%	70.3%
Hospital visit rate	8.2%	6.9%				
Hospitalization rate	4.3%	3.4%				
Any shock during the year	3.0%	2.9%				
Urgent shock with 3+ nights hosp.	0.5%	0.6%				
Survival rate (one year)	99.9%	99.9%				
Survival rate (five years)	99.4%	99.2%				
Observations	2,405,357	2,214,204	171,006	163,215	30,315	29,832

Notes: The Table reports summary statistics by gender. The first two columns report summary statistics for the population of 25-55-year-olds who had been in a couple for at least three years, in 2015. The third and fourth columns report statistics for the sick individuals in our main sample of analysis in the year preceding the health shock, $t = -1$. This corresponds to couples where both spouses are 25-55 years old in $t = 0$ with at least 3 years of couple's tenure by that time, and in which neither spouse nor any children residing in the household had any hospital visit for the 5 preceding years. The fifth and sixth columns report statistics for individuals who suffer a health shock and satisfy our sample restrictions, but for whom the matching algorithm does not find a suitable match among control couples. Employment is only defined for years 2006 onward, as we do not have data on the universe of jobs for the previous years. Total hours worked per week and employment income are conditional on the individual being employed; these variables are defined for the entire period. The variable main earner in the household is constructed as an indicator which is equal to one if the individual is employed and earns more than their spouse. Separations and health-related variables are not meaningful for the shocked population due to our sample restrictions. This Table is referenced in Section II.B.

Table 2: Most Common Diagnostic Codes: Women

Main Diagnoses in t=0	**Total Number of Diagnoses**	**Share of Total Diagnoses**	**Share of Total Diagnoses in Control**	**Avg Age at Diagnosis in Control**	**Avg Nights in Hospital in Control**	**Avg Num Hosp. Visits Next Years in Control**	**5-Year Survival Probability in Control**
Malignant neoplasm of female breast	14,286	8.4%	5.5%	45.6	0.8	2.8	85.7%
Uterine leiomyoma	14,111	8.3%	1.9%	44.6	2.7	0.2	98.6%
Acute appendicitis	7,373	4.3%	0.9%	39.3	3.3	0.2	98.7%
Other disorders of eyelids	7,283	4.3%	0.9%	47.1	0.0	0.2	98.6%
Noninflammatory disorders of cervix	4,603	2.7%	0.8%	38.8	0.2	0.2	98.5%
Benign neoplasm of other parts of digestive system	3,705	2.2%	0.6%	46.8	0.5	0.5	97.4%
Other disorders of synovium, tendon and bursa	3,587	2.1%	1.0%	42.9	0.1	0.2	98.8%
Carcinoma in situ of breast and genitourinary system	3,448	2.0%	0.5%	43.1	1.0	0.6	97.9%
Lipoma	3,346	2.0%	0.4%	43.2	0.2	0.2	99.0%
Benign neoplasm of breast	3,025	1.8%	0.3%	40.9	0.2	0.2	98.4%
Fracture of ankle	2,809	1.6%	0.4%	43.4	2.2	0.4	98.7%
Cholelithiasis	2,530	1.5%	2.9%	40.5	1.9	0.2	98.7%
Benign neoplasm of ovary	2,518	1.5%	0.3%	41.2	2.6	0.2	98.9%
Inflammatory disease of cervix, vagina and vulva	2,494	1.5%	0.3%	39.3	0.4	0.2	98.1%
Pneumonia, organism unspecified	2,477	1.4%	0.4%	41.5	5.3	0.4	92.7%
Average in control				**39.1**	**0.1**	**0.1**	**98.4%**

Notes: The Table reports the most common diagnoses that we consider as health shocks - i.e. hospital visits whose onset is exogenous and cannot be medically predicted, excluding visits related to pregnancy and childbirth - for the sick women in our main sample. For each diagnosis we report the total number of $t = 0$ shocks in the sample of sick individual and their share of total shocks. We also report a set of statistics based on the full sample of possible controls - i.e. the control sample which passes our sample restrictions, but that is not necessarily matched to a treated couple. Specifically, for each diagnosis we report the share of total shock diagnoses, the average age at diagnosis, the average number of nights spent in the hospital immediately following the diagnosis, the average number of hospital visits one year after a diagnosis, and the 5-year survival probability. The bottom line in the table reports these statistics for the full control sample. This Table is referenced in Section II.B.1.

Table 3: Most Common Diagnostic Codes: Men

Main Diagnoses in t=0	Total Number of Diagnoses	Share of Total Diagnoses	Share of Total Diagnoses in Control	Avg Age at Diagnosis in Control	Avg Nights in Hospital in Control	Avg Num Hosp. Visits Next Years in Control	5-Year Survival Probability in Control
Acute myocardial infarction	11,187	6,85%	1,95%	49.4	4.5	0.4	94,8%
Acute appendicitis	7,711	4,72%	1,02%	41.3	3.2	0.2	98,5%
Cardiac dysrhythmias	6,666	4,08%	2,17%	48.6	1.5	0.7	95,4%
Benign neoplasm of other parts of digestive system	5,051	3,09%	0,94%	49.4	0.3	0.5	96,3%
Lipoma	4,064	2,49%	0,55%	45.0	0.1	0.1	98,2%
Sprains and strains of ankle and foot	3,949	2,42%	0,47%	42.0	0.8	0.1	98,7%
Angina pectoris	3,435	2,10%	0,87%	49.8	1.4	0.5	97,3%
Symptoms involving respiratory system and other chest symptoms	3,429	2,10%	4,81%	46.1	0.8	0.3	97,1%
Other forms of ischaemic heart disease	3,315	2,03%	0,70%	49.7	3.9	0.5	97,5%
Other disorders of synovium, tendon and bursa	3,293	2,02%	0,85%	44.4	0.3	0.2	98,4%
Abscess of anal and rectal regions	3,166	1,94%	0,50%	42.8	1.2	0.5	97,3%
Fracture of ankle	2,892	1,77%	0,45%	42.7	2.3	0.4	98,3%
Pneumonia, organism unspecified	2,697	1,65%	0,46%	45.5	5.6	0.5	89,7%
Fracture of radius and ulna	2,492	1,53%	0,35%	44.1	1.4	0.3	98,4%
Pilonidal cyst	2,379	1,46%	0,34%	37.7	0.5	0.2	98,4%
Average in control				**41.4**	**0.04**	**0.1**	**98.0%**

Notes: The Table reports the most common diagnoses that we consider as health shocks - i.e. hospital visits whose onset is exogenous and cannot be medically predicted, excluding visits related to pregnancy and childbirth - for the sick men in our main sample. For each diagnosis we report the total number of $t = 0$ shocks in the sample of sick individual and their share of total shocks. We also report a set of statistics based on the full sample of possible controls - i.e. the control sample which passes our sample restrictions, but that is not necessarily matched to a treated couple. Specifically, for each diagnosis we report the share of total shock diagnoses, the average age at diagnosis, the average number of nights spent in the hospital immediately following the diagnosis, the average number of hospital visits one year after a diagnosis, and the 5-year survival probability. The bottom line in the table reports these statistics for the full control sample. This Table is referenced in Section II.B.1.

Table 4: Short-Term Coping Mechanisms and Behavior Changes

	Smoking	Never Drinks	Any Sport	Sports 5h+
Panel a: Healthy Women				
Post Shock	-0.003	0.087*	-0.024	0.067*
	(0.038)	(0.050)	(0.045)	(0.037)
Observations	3,239	1,094	2,923	2,012
Panel b: Healthy Men				
Post Shock	-0.049	0.013	0.087*	0.122***
	(0.040)	(0.035)	(0.045)	(0.046)
Observations	3,332	1,067	3,033	2,082

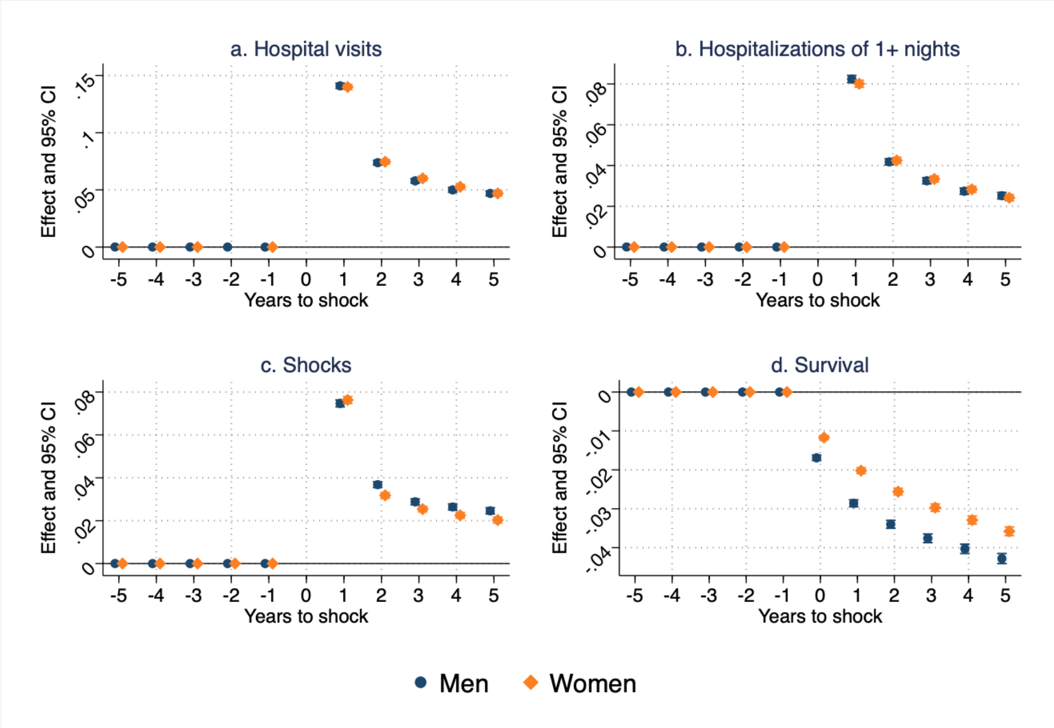
Notes: The Table reports our estimates from a Difference-in-Differences regression of short-term outcomes from the Health Surveys. Specifically, we report the coefficient of the interaction term between an indicator for the treated group and an indicator for years 0 and 1 relative to the shock, which captures the short term effect of the health shock. Smoking indicates whether the person smoked in the past year. Never Drinks is an indicator equal to 1 if the respondent stated that they never drank alcohol in the previous year. Any sport is an indicator equal to 1 if the respondent indicated that they spent any time doing sports in the previous week or in the average week, depending on the survey year. Sports 5h+ is an indicator equal to 1 if the respondent indicated that they spent 5 or more hours doing sports. The number of observations varies across outcomes due to different response rates and to the survey used. For the outcomes related to physical activity, we use the Continuing Survey on Living Conditions (POLS) in years 1997–2008. The other outcomes are not available in the general survey so we use the health module of the survey, which is only administered to a subsample of respondents. We use the Health Survey for all outcomes for the years 2009–2021. Robust standard errors are reported in parenthesis. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. This Table is referenced in Section III.B.

A Direct Effects of Health Shocks

We document here the effects on the person experiencing the initial health shock, this constitutes the *treatment* for the initially healthy partner. Similar to a large body of literature using comparable identification strategies (e.g., Meyer and Mok (2019); Dobkin et al. (2018); Jeon and Pohl (2017); García-Gómez (2011); among others), in Figures A1 and A2, we find that following a health shock, there is a permanent decline in health status and labor market attachment. We estimate increases in the probability of future hospital visits (80%), hospitalizations (84%), and future health shocks (92% for women and 107% for men) even five years after the original event. The health deterioration for sick partners has spillovers to conditions other than the initially diagnosed one. In Figure A3, we document that there are increases in the incidence of diagnoses across multiple categories. Survival rates five years after the shock are 3.6 percentage points (pp) and 4.3 pp lower for women and men, respectively.

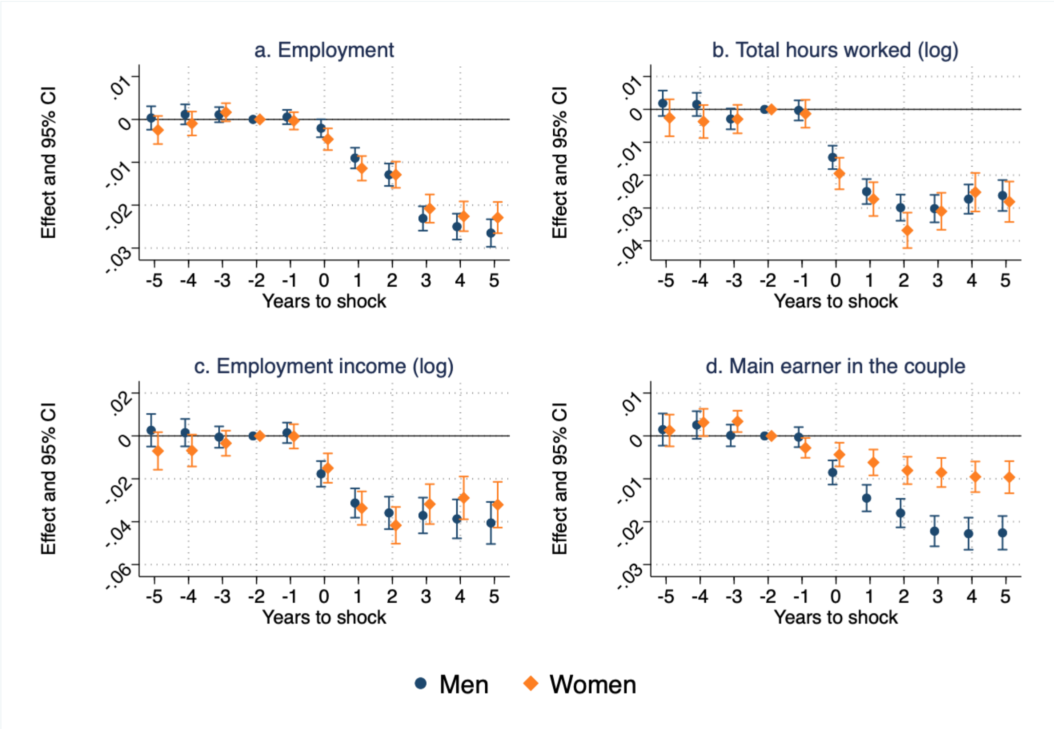
In terms of economic effects, we find that these partners experience significant and permanent declines in employment rates, with their employment rate being 2.3 and 2.7 percentage points lower five years after the shock for women and men, respectively (Panel a of Figure A2). Within our study window of five years, we do not estimate any recovery from this decline in labor force attachment. The decline in employment is also greater for more severe shocks. Specifically, we divide the sample into two groups based on the mortality risk associated with the diagnosed condition. We find that among individuals in the higher mortality group, the decrease in employment is 4.5 percentage points for men and 5.1 percentage points for women, compared to 1.3 and 0.7 percentage point, respectively, in the lower mortality group (Figure A4). In terms of the intensive margin for those employed, we also estimate permanent declines in hours (Panel b of Figure A2). The drop is largest in the second year after the shock, after which hours stabilize in years three to five with a decline of 2.5% to 3.1%. Both of these changes translate into a permanent decline in employment income of 3.2% for women and 4% for men (Panel c of Figure A2). Overall, the pattern of these economic effects reflects the local institutional context, where individuals enjoy an up to two-years period of job protection at 70% of their original salary following a severe health event, followed by disability insurance for workers with a disability that limits their earning capacity by 35% or more.

Figure A1: Health Effects of Own Health Shock



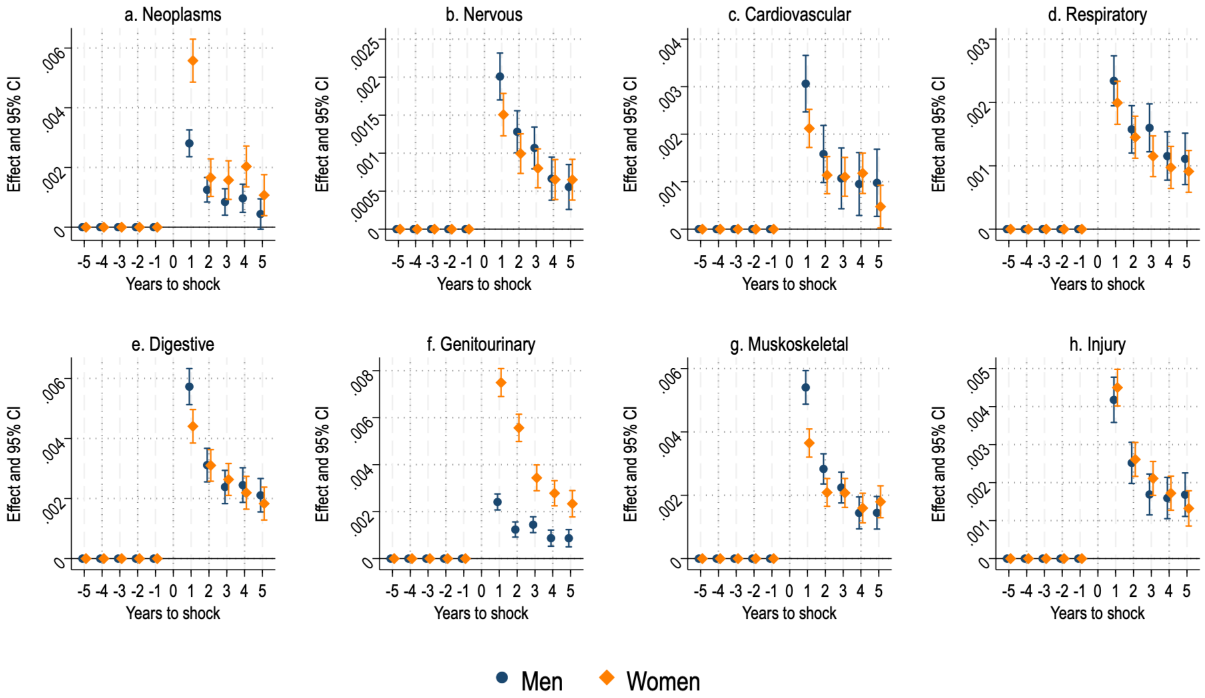
Notes: This figure shows our estimates for the coefficients γ_t from Eq. 1. Specifically, it shows the effect of a health shock in $t = 0$ on hospital visits, hospitalizations, health shocks, and survival in the subsequent five years for the shocked individual. Hospital visits include all visits to the hospital, with the exception of visits with a diagnosis related to pregnancy or childbirth. Hospitalizations include all hospital visits, with the exception of visits with a diagnosis related to pregnancy or childbirth, for which the patient was discharged one day or more after admission. Following García-Gómez et al. (2013) shocks identify hospital visits whose onset is exogenous and cannot be medically predicted, based on the diagnosis. We construct survival based on individual's year of death. Coefficients for $t = -5$ to -1 are zero by construction due to our sample restrictions. Coefficients for $t = 0$ for hospital visits, hospitalizations, and health shocks are outside of the scale of the graph. The coefficient is 1 for all specifications by construction. Standard errors are clustered at the individual level. This figure is referenced in Appendix A.

Figure A2: Economic Effects of Own Health Shock



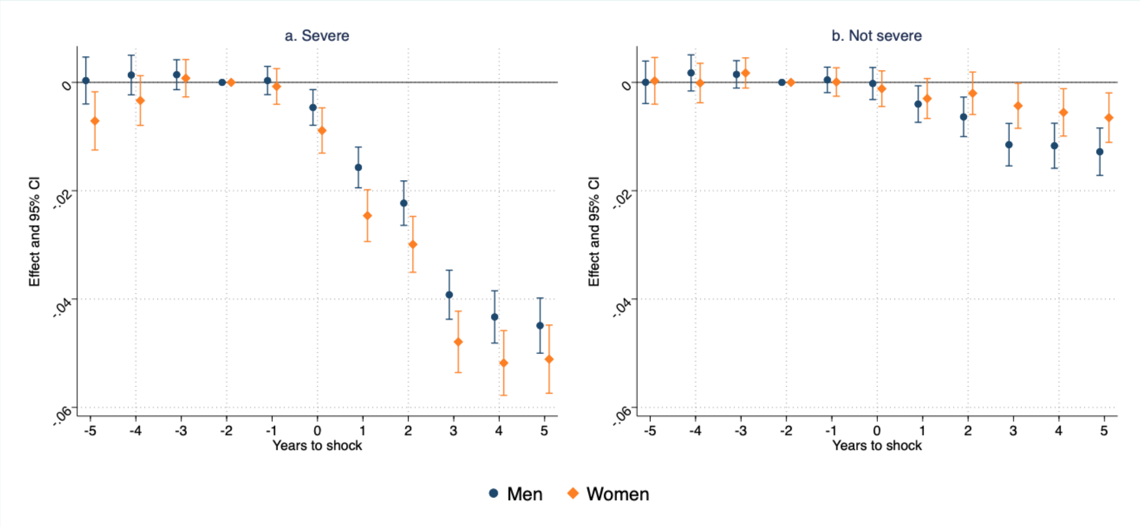
Notes: This figure shows our estimates for the coefficients γ_t from Eq. 1. Specifically, it shows the effect of a health shock in $t = 0$ on employment, the log of the total number of hours worked, the log of employment income, and an indicator for the main earner in the couple in the subsequent five years for the shocked individual. Employment is only defined for years 2006 onward, as we do not have data on the universe of jobs for the previous years. Total hours worked and employment income are conditional on the individual being employed; these variables are defined for the entire period. The variable main earner in the couple is constructed as an indicator which is equal to one if the individual is employed and earns more than their spouse. Standard errors are clustered at the individual level. This figure is referenced in Appendix A.

Figure A3: Initial Health Shock Diagnosis Categories



Notes: This figure shows our estimates for the coefficients γ_t from Eq. 1. Specifically, it shows the effect of a health shock in $t = 0$ on hospitalizations by broad diagnosis category in the subsequent five years for the shocked individual. For each diagnosis category, we exclude individuals for whom the initial shock diagnosis belongs to that same category. For example, when we look at the effects on hospitalizations with a cardiovascular diagnosis, we exclude individuals who experienced a heart attack in $t = 0$. Coefficients for $t = -5$ to -1 are zero by construction due to our sample restrictions. Coefficients for $t = 0$ cannot be interpreted meaningfully given the different sample restrictions imposed on individuals in the treated and control groups. In particular, we match treated and control couples at the yearly level due to computational feasibility. This implies that we need to impose the restriction of no previous hospital visits until the year of the shock for individuals in the control sample, while the restriction is imposed until the precise date of the shock for individuals in the treated sample. Standard errors are clustered at the individual level. This figure is referenced in Appendix A.

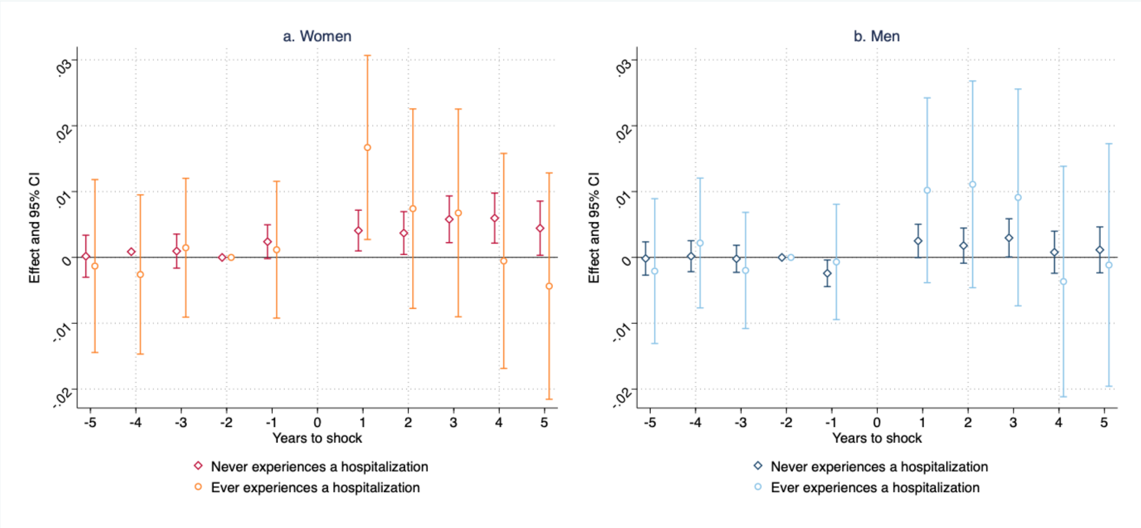
Figure A4: Employment Effects by Shock Severity



Notes: This figure shows our estimates for the coefficients γ_t from Eq. 1. Specifically, it shows the effect of a health shock in $t = 0$ on employment in the subsequent five years for the shocked individual. The effects are shown separately by severity of the initial shock. We classify shocks as severe if the mortality risk associated with the diagnosed condition for the control group is above the median. Standard errors are clustered at the individual level. This figure is referenced in Appendix A.

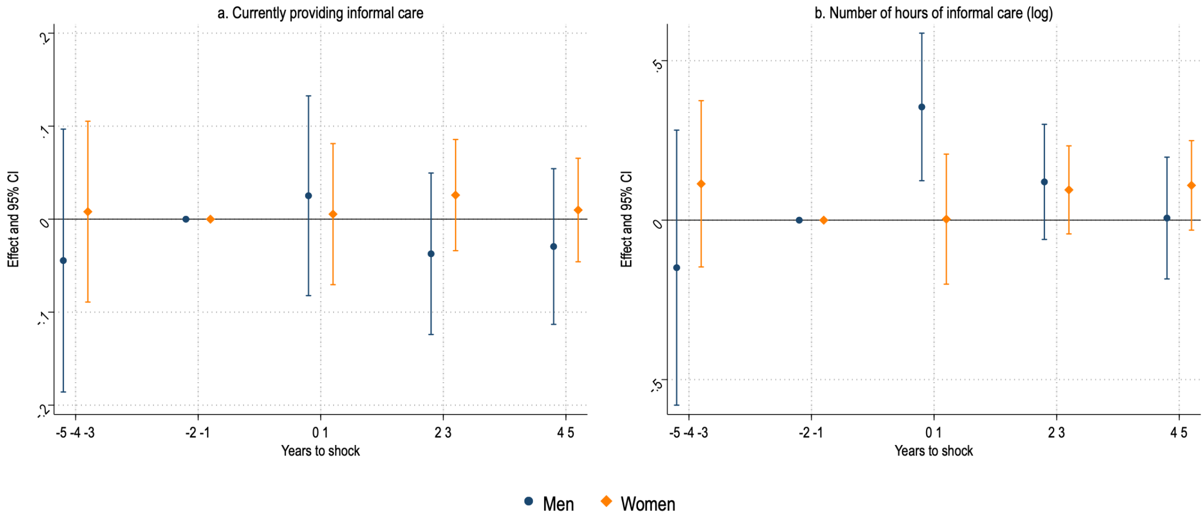
B Additional Figures

Figure B1: Stress, Anxiety and Depression Medication Use by Ever Hospitalization Status



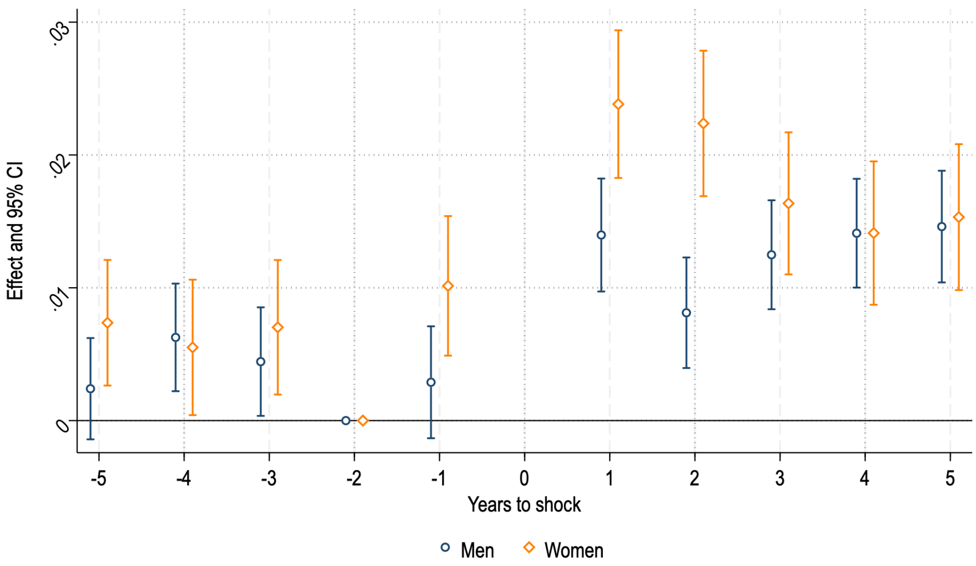
Notes: This figure shows our estimates for the coefficients γ_t from Eq. 1. Specifically, it shows the effect of a health shock in $t = 0$ on stress, anxiety and depression medication use in the subsequent five years for the partner of the shocked individual. The effects are shown separately by hospitalization status. An individual is classified as having experienced a hospitalization if they were ever hospitalized between $t = 0$ and $t = 5$. Hospitalizations include all hospital visits, with the exception of visits with a diagnosis related to pregnancy or childbirth, for which the patient was discharged one day or more after admission. Standard errors are clustered at the individual level. This figure is referenced in Section III.B.

Figure B2: Informal Caregiving by the Sick Partner



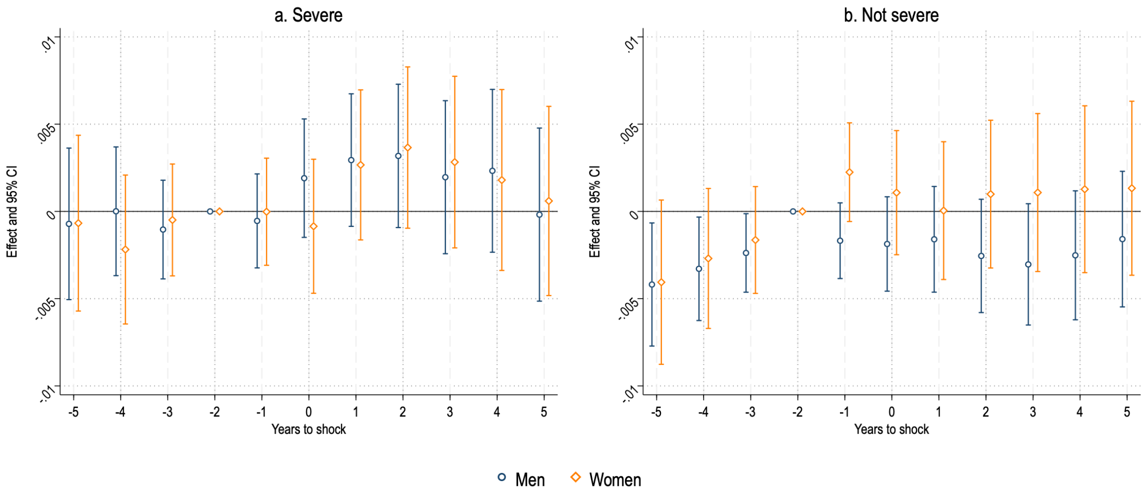
Notes: This figure shows our estimates for the coefficients γ_t from a specification analogous to Eq. 1, where we pool together single years to treatment. Specifically it shows the effect of health shocks in $t = 0$ on a set of measures of informal caregiving provision in the subsequent five years for the shocked individual. Currently providing informal care is an indicator for wether the individual reports any informal caregiving provision at the time of the interview. Hours of informal care provided are conditional on the individual providing any care. This figure is referenced in Section III.C.1.

Figure B3: Health Spillover Effects: Sleep Medication



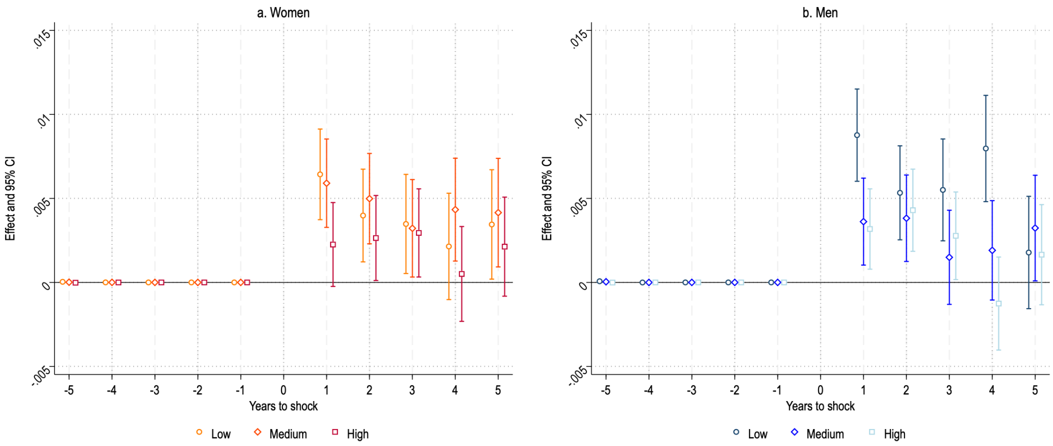
Notes: This figure shows our estimates for the coefficients γ_t from a specification analogous to Eq. 1, where we control for calendar year and we do not include individual FE. Specifically, the figure shows the effect of a health shock in $t = 0$ on the prescription of sleeping medication (ATC codes N05B and N05) in the subsequent five years for the partner of the shocked individual. The indicators for are equal to 1 if the individual filled any prescription for a drug in this category during the year. This figure is referenced in Section III.C.2.

Figure B4: Healthy Partner Employment Effects by Shock Severity



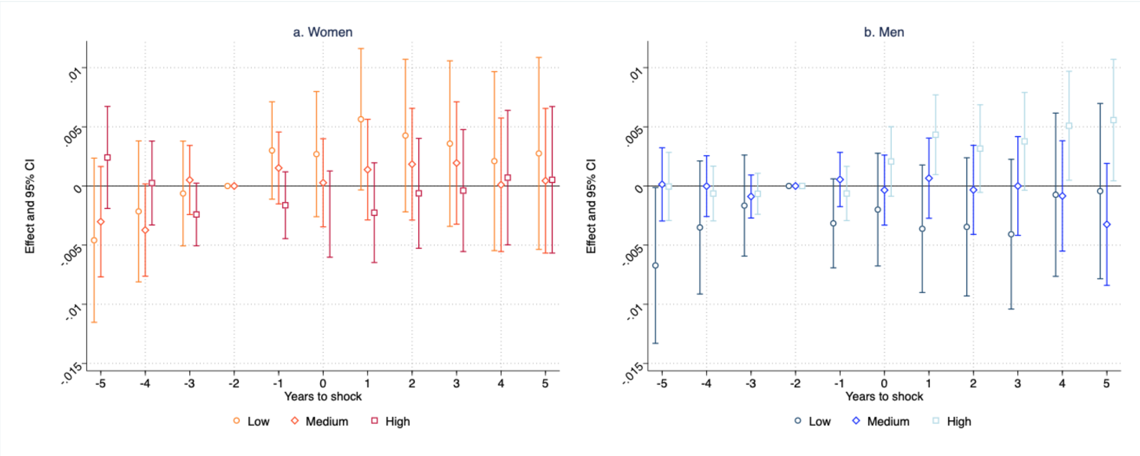
Notes: This figure shows our estimates for the coefficients γ_t from Eq. 1. Specifically, it shows the effect of a health shock in $t = 0$ on employment in the subsequent five years for the partner of the shocked individual. The effects are shown separately by severity of the initial shock. We classify shocks as severe if the mortality risk associated with the diagnosed condition for the control group is above the median. Employment is only defined for years 2006 onward, as we do not have data on the universe of jobs for the previous years. Standard errors are clustered at the individual level. This figure is referenced in Section III.C.3.

Figure B5: Healthy Partner Hospitalization Rates by Income



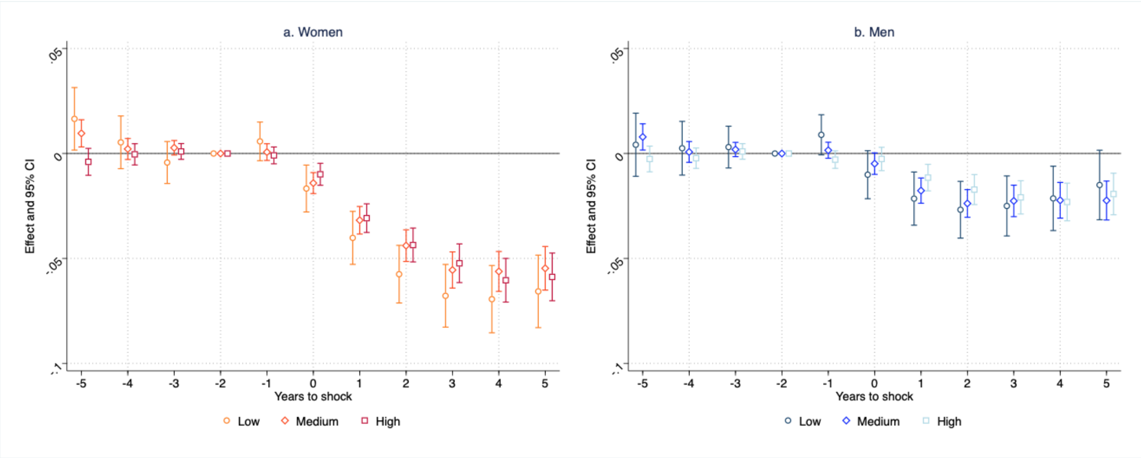
Notes: This figure shows our estimates for the coefficients γ_t from Eq. 1. Specifically, it shows the effect of a health shock in $t = 0$ on hospitalizations in the subsequent five years for the partner of the shocked individual. The effects are shown separately by couple employment income tercile in $t = -3$. Coefficients for $t = -5$ to -1 are zero by construction due to our sample restrictions. Coefficients for $t = 0$ cannot be interpreted meaningfully given the different sample restrictions imposed on partners in the treated and control groups. In particular, we match treated and control couples at the yearly level due to computational feasibility. This implies that we need to impose the restriction of no previous hospital visits until the year of the shock for partners in the control sample, while the restriction is imposed until the precise date of the shock for partners in the treated sample. Standard errors are clustered at the individual level. This figure is referenced in Section III.C.3.

Figure B6: Healthy Partner Employment Rates by Income



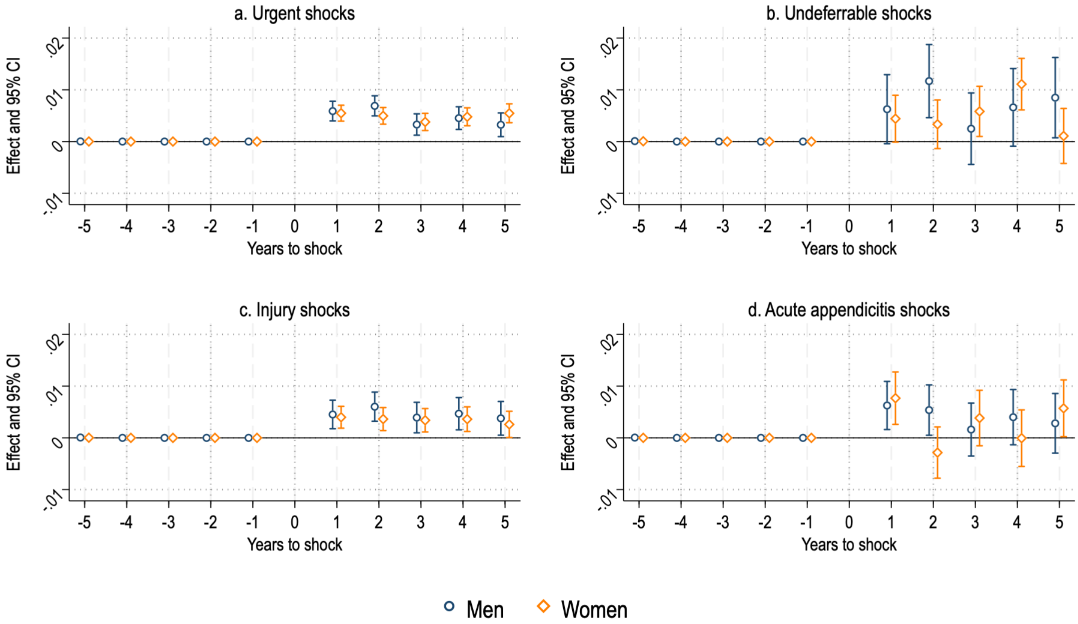
Notes: This figure shows our estimates for the coefficients γ_t from Eq. 1. Specifically, it shows the effect of a health shock in $t = 0$ on employment in the subsequent five years for the partner of the shocked individual. The effects are shown separately by couple employment income tercile in $t = -3$. Employment is only defined for years 2006 onward, as we do not have data on the universe of jobs for the previous years. Standard errors are clustered at the individual level. This figure is referenced in Section III.C.3.

Figure B7: Household Income by Income



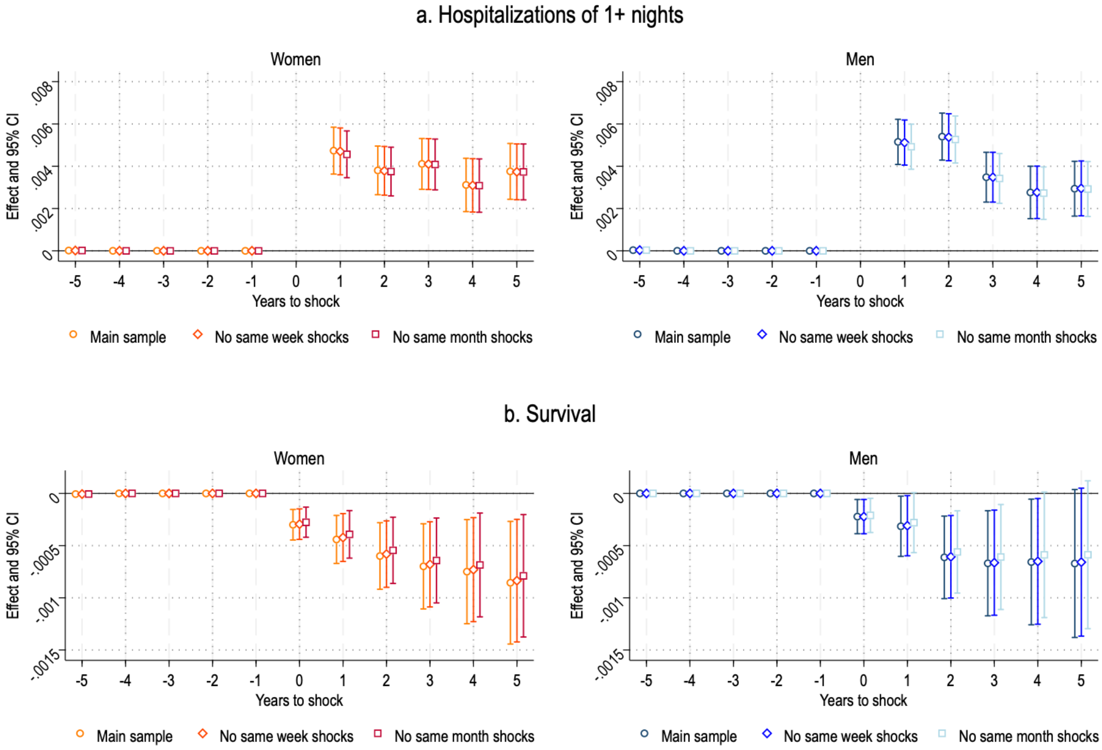
Notes: This figure shows our estimates for the coefficients γ_t from Eq. 1. Specifically, it shows the effect of a health shock in $t = 0$ on household income in the subsequent five years for the partner of the shocked individual. The effects are shown separately by couple employment income tercile in $t = -3$. Standard errors are clustered at the individual level. This figure is referenced in Section III.C.3.

Figure B8: Health Spillover Effects: Subset of “Sudden” Shocks



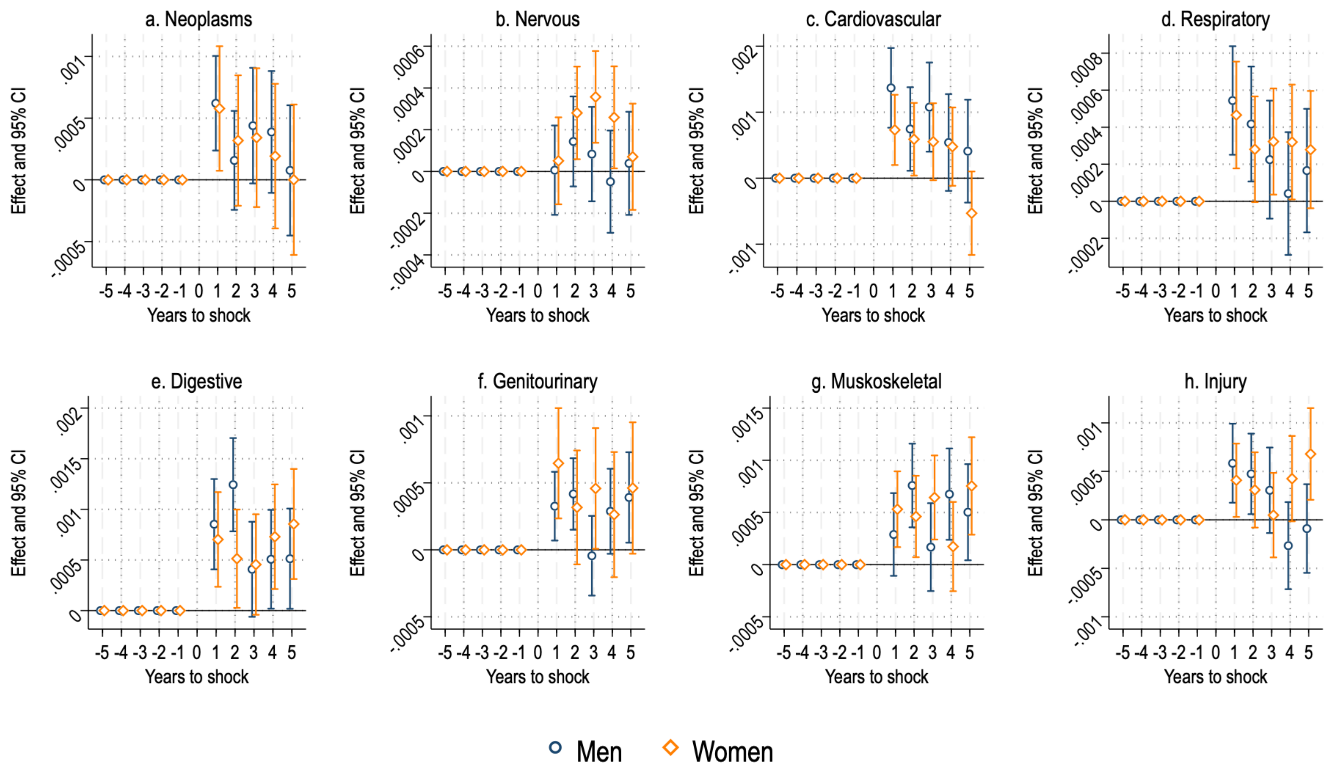
Notes: This figure shows our estimates for the coefficients γ_t from Eq. 1. Specifically, it shows the effect of a health shock in $t = 0$ on hospitalizations in the subsequent five years for the partner of the shocked individual. Hospitalizations include all hospital visits, with the exception of visits with a diagnosis related to pregnancy or childbirth, for which the patient was discharged one day or more after admission. Each panel restricts to a different set of shocks which are less likely to be expected or correlated with the partner's health. Urgent shocks are shocks for which the hospital visit starts at the Emergency Room or that have an urgent code from the referring physician. Undeferrable shocks correspond to shock diagnoses that occur with the same frequency across all days of the week. Specifically, for each shock diagnosis, we use the full hospital records and test if the proportion of weekend admissions is equal to the proportion of weekday admissions (Rellstab et al., 2020; Dobkin et al., 2018). We identify a shock diagnosis as undeferrable if we do not reject the null hypothesis at the 1% level. Injury shocks include all shocks with an injury diagnosis (ICD-9 codes 800-959). Acute appendicitis shocks are hospital visits with a diagnosis for acute appendicitis (ICD-9 code 540). Coefficients for $t = -5$ to -1 are zero by construction due to our sample restrictions. Coefficients for $t = 0$ cannot be interpreted meaningfully given the different sample restrictions imposed on partners in the treated and control groups. In particular, we match treated and control couples at the yearly level due to computational feasibility. This implies that we need to impose the restriction of no previous hospital visits until the year of the shock for partners in the control sample, while the restriction is imposed until the precise date of the shock for partners in the treated sample. Standard errors are clustered at the individual level. This figure is referenced in Section IV.A.

Figure B9: Hospitalizations and Survival Rates Excluding Same Week or Same Month Shocks



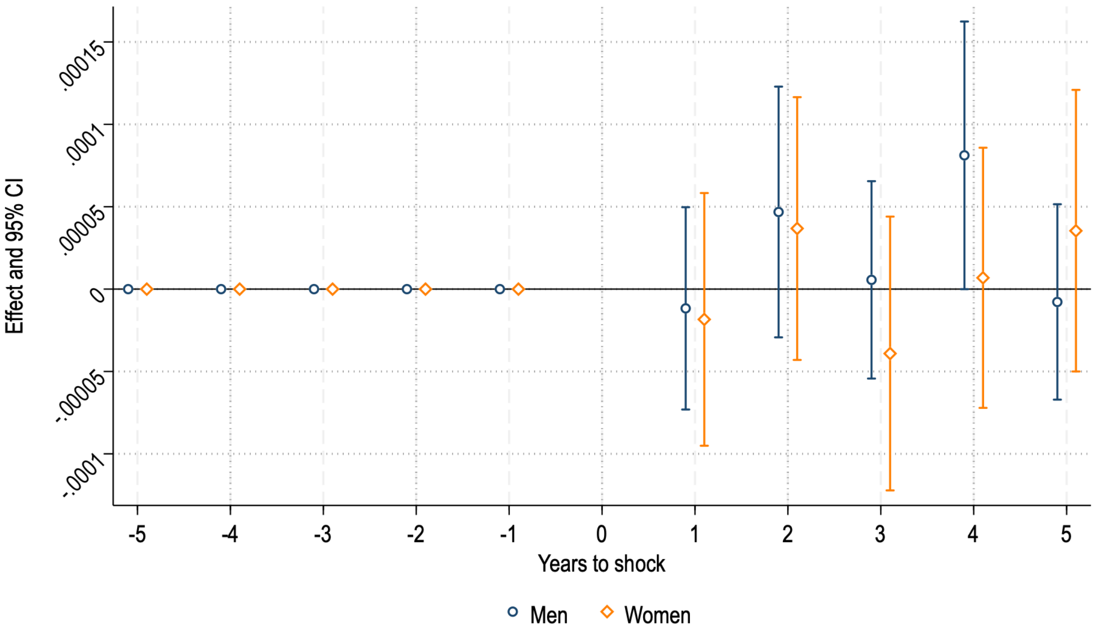
Notes: This figure shows our estimates for the coefficients γ_t from Eq. 1. Specifically, it shows the effect of a health shock in $t = 0$ on hospitalizations and survival in the subsequent five years for the partner of the shocked individual. The effects are shown separately for the main sample and for the subset of couples in which the partner of the shocked individual did not experience a shock within a month or a week of their partner's initial shock in $t = 0$. Coefficients for $t = -5$ to -1 are zero by construction due to our sample restrictions. For hospitalizations, coefficients in $t = 0$ cannot be interpreted meaningfully given the different sample restrictions imposed on partners in the treated and control groups. In particular, we match treated and control couples at the yearly level due to computational feasibility. This implies that we need to impose the restriction of no previous hospital visits until the year of the shock for partners in the control sample, while the restriction is imposed until the precise date of the shock for partners in the treated sample. Standard errors are clustered at the individual level. This figure is referenced in Section IV.A.

Figure B10: Health Spillover Effects: Hospitalizations for Categories Different that the Original Shock



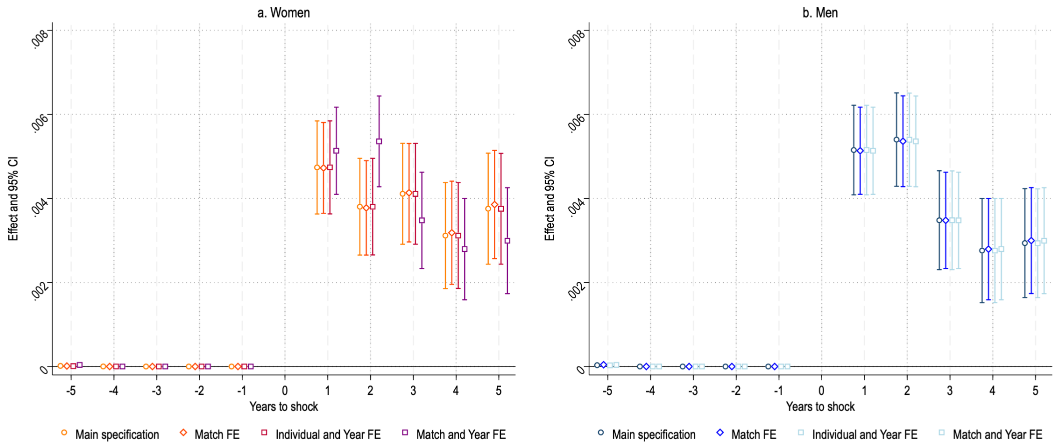
Notes: This figure shows our estimates for the coefficients γ_t from Eq. 1. Specifically, it shows the effect of a health shock in $t = 0$ on hospitalizations by broad diagnosis category in the subsequent five years for the partner of the shocked individual. For each diagnosis category, we exclude couples for whom the diagnosis of the sick individual's initial shock belongs to that same category. For example, when we look at the effects on hospitalizations with a cardiovascular diagnosis, we exclude couples in which the sick partner experienced a heart attack in $t = 0$. Coefficients for $t = -5$ to -1 are zero by construction due to our sample restrictions. Standard errors are clustered at the individual level. This figure is referenced in Section IV.A.

Figure B11: Hospitalizations for Congenital Diagnoses



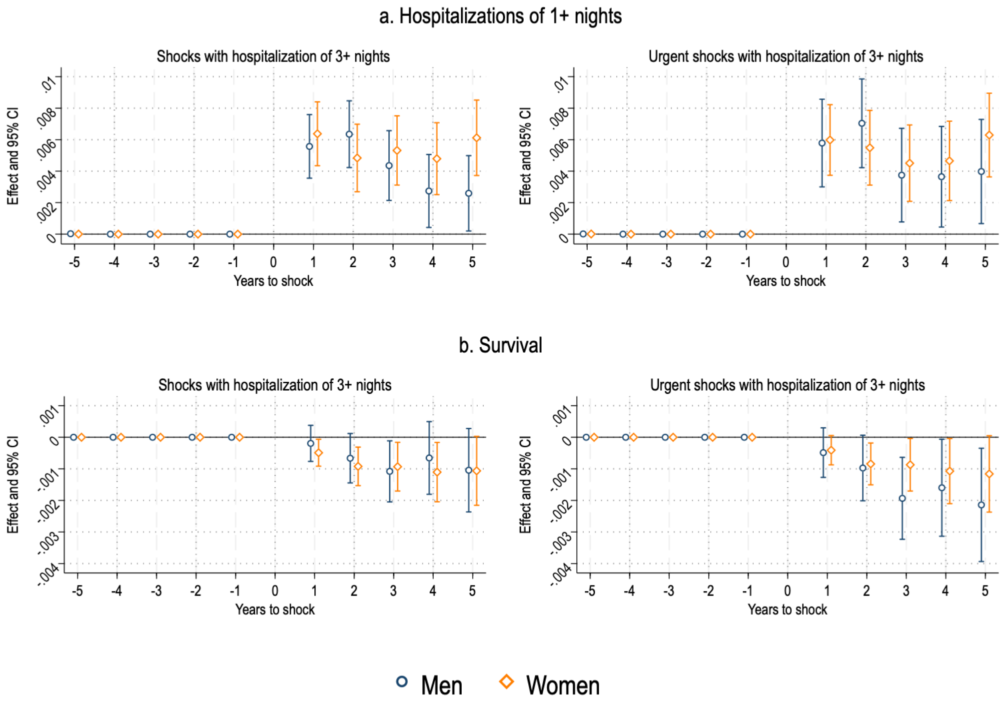
Notes: This figure shows our estimates for the coefficients γ_t from Eq. 1. Specifically, it shows the effect of a health shock in $t = 0$ on hospitalizations with diagnoses for congenital diseases in the subsequent five years for the partner of the shocked individual. Coefficients for $t = -5$ to -1 are zero by construction due to our sample restrictions. Coefficients in $t = 0$ cannot be interpreted meaningfully given the different sample restrictions imposed on partners in the treated and control groups. In particular, we match treated and control couples at the yearly level due to computational feasibility. This implies that we need to impose the restriction of no previous hospital visits until the year of the shock for partners in the control sample, while the restriction is imposed until the precise date of the shock for partners in the treated sample. Standard errors are clustered at the individual level. This figure is referenced in Section IV.A.

Figure B12: Health Spillovers: Different Regression Specifications



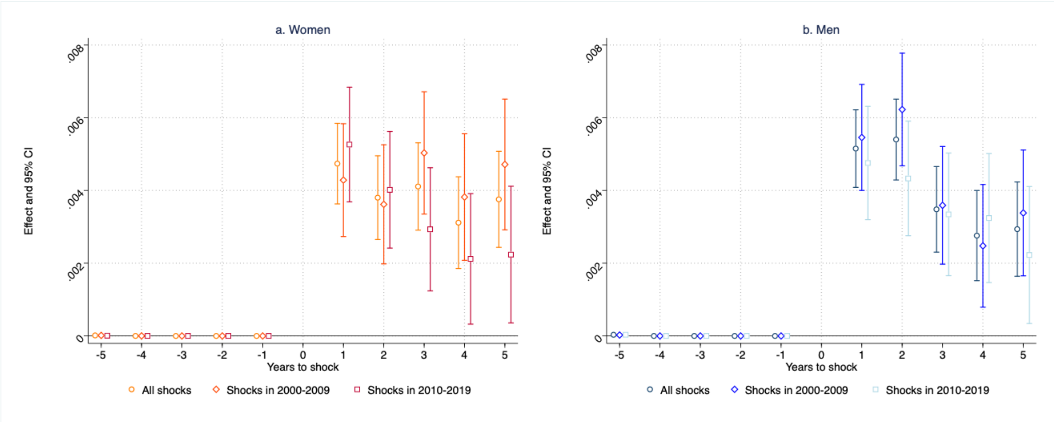
Notes: This figure shows our estimates for the coefficients γ_t from Eq. 1. Specifically, it shows the effect of a health shock in $t = 0$ on hospitalizations in the subsequent five years for the partner of the shocked individual. We also show our estimates for different specifications where we include match FE instead of individual FE, and where we add year FE. Coefficients for $t = -5$ to -1 are zero by construction due to our sample restrictions. Coefficients in $t = 0$ cannot be interpreted meaningfully given the different sample restrictions imposed on partners in the treated and control groups. In particular, we match treated and control couples at the yearly level due to computational feasibility. This implies that we need to impose the restriction of no previous hospital visits until the year of the shock for partners in the control sample, while the restriction is imposed until the precise date of the shock for partners in the treated sample. Standard errors are clustered at the individual level. This figure is referenced in Section IV.A.

Figure B13: Hospitalizations and Survival Rates By Shock Type



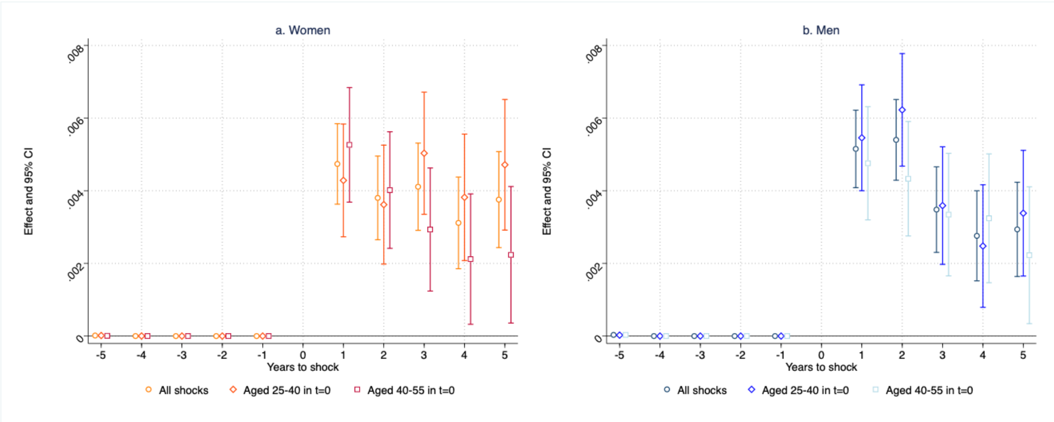
Notes: This figure shows our estimates for the coefficients γ_t from Eq. 1. Specifically, it shows the effect of a health shock in $t = 0$ on hospitalizations and survival in the subsequent five years for the partner of the shocked individual. The effects are shown separately for two different subsets of initial shocks: shocks followed by a hospitalization of at least 3 nights, urgent shocks followed by a hospitalization of at least 3 nights. We define as urgent, shocks for which the hospital visit starts at the Emergency Room or that have an urgent code from the referring physician. Coefficients for $t = -5$ to -1 are zero by construction due to our sample restrictions. For hospitalizations, coefficients in $t = 0$ cannot be interpreted meaningfully given the different sample restrictions imposed on partners in the treated and control groups. In particular, we match treated and control couples at the yearly level due to computational feasibility. This implies that we need to impose the restriction of no previous hospital visits until the year of the shock for partners in the control sample, while the restriction is imposed until the precise date of the shock for partners in the treated sample. Standard errors are clustered at the individual level. This figure is referenced in Section IV.A.

Figure B14: Health Spillovers: Early vs Late Shocks



Notes: This figure shows our estimates for the coefficients γ_t from Eq. 1. Specifically, it shows the effect of a health shock in $t = 0$ on hospitalizations in the subsequent five years for the partner of the shocked individual. The effects are shown separately for the main sample and for the subset of couples in which the shock happened in the years 2000-2009 and 2010-2019. Coefficients for $t = -5$ to -1 are zero by construction due to our sample restrictions. Coefficients in $t = 0$ cannot be interpreted meaningfully given the different sample restrictions imposed on partners in the treated and control groups. In particular, we match treated and control couples at the yearly level due to computational feasibility. This implies that we need to impose the restriction of no previous hospital visits until the year of the shock for partners in the control sample, while the restriction is imposed until the precise date of the shock for partners in the treated sample. Standard errors are clustered at the individual level. This figure is referenced in Section IV.B.

Figure B15: Health Spillovers: Younger vs Older Partners



Notes: This figure shows our estimates for the coefficients γ_t from Eq. 1. Specifically, it shows the effect of a health shock in $t = 0$ on hospitalizations in the subsequent five years for the partner of the shocked individual. The effects are shown separately for the main sample and for the subset of couples in which the partner of the shocked individuals was up to 40 years old at the time of the shock, and couples in which the partner was older than 40 years old. Coefficients for $t = -5$ to -1 are zero by construction due to our sample restrictions. Coefficients in $t = 0$ cannot be interpreted meaningfully given the different sample restrictions imposed on partners in the treated and control groups. In particular, we match treated and control couples at the yearly level due to computational feasibility. This implies that we need to impose the restriction of no previous hospital visits until the year of the shock for partners in the control sample, while the restriction is imposed until the precise date of the shock for partners in the treated sample. Standard errors are clustered at the individual level. This figure is referenced in Section IV.B.

C Additional Tables

Table C1: Most Common Diagnostic Groups: Women

Main Diagnosis Categories in t=0	**Total Number of Diagnoses**	**Share of Total Diagnoses**	**Share of Totall Diagnoses in Control**	**Avg Age at Diagnosis in Control**	**Avg Nights in Hospital in Control**	**Avg Num Hosp. Visits Next Years in Control**	**5-Year Survival Probability in Control**
Neoplasms	63,071	36.9%	17.1%	45.3	1.8	2.1	77.4%
Injury	25,446	14.9%	4.2%	42.7	2.8	0.4	96.5%
Digestive	19,854	11.6%	10.2%	41.5	2.0	0.5	97.2%
Genitourinary	17,574	10.3%	12.1%	40.7	1.2	0.3	98.4%
Cardiovascular	15,414	9.0%	6.9%	44.2	2.4	0.4	95.9%
Nervous	11,045	6.5%	6.4%	44.2	0.8	0.9	97.8%
Respiratory	5,617	3.3%	3.7%	39.6	2.3	0.5	96.1%
Muskoskeletal	4,937	2.9%	11.1%	43.8	1.0	0.6	98.6%
Infections	3,098	1.8%	0.7%	41.4	4.0	0.7	93.5%
Skin	2,390	1.4%	1.7%	41.5	1.1	2.0	98.2%
Symptoms	1,269	0.7%	8.6%	42.3	1.2	0.6	94.7%
Supplementary	907	0.5%	13.1%	43.0	0.6	1.9	89.4%
Mental	28	0.0%	0.7%	41.9	21.4	1.2	94.6%
Endocrine	15	0.0%	1.8%	41.6	2.2	1.0	96.0%
Average in control				**39.1**	**0.1**	**0.1**	**98.4%**

Notes: The Table reports the most common diagnostic groups for the diagnoses that we classify as health shocks - i.e. hospital visits whose onset is exogenous and cannot be medically predicted, excluding visits related to pregnancy and childbirth - for the sick women in our main sample. For each diagnostic group we report the total number of $t = 0$ shocks in the sample of sick individual and their share of total shocks. We also report a set of statistics based on the full sample of possible controls - i.e. the control sample which passes our sample restrictions, but that is not necessarily matched to a treated couple. Specifically, for each diagnostic group we report the share of total shock diagnoses, the average age at diagnosis, the average number of nights spent in the hospital immediately following a diagnosis in this group, the average number of hospital visits one year after a diagnosis, and the 5-year survival probability. The bottom line in the table reports these statistics for the full control sample. This Table is referenced in Section II.B.1.

Table C2: Most Common Diagnostic Groups: Men

Main Diagnosis Categories in t=0	**Total Number of Diagnoses**	**Share of Total Diagnoses**	**Share of Totall Diagnoses in Control**	**Avg Age at Diagnosis in Control**	**Avg Nights in Hospital in Control**	**Avg Num Hosp. Visits Next Years in Control**	**5-Year Survival Probability in Control**
Injury	43,292	26.5%	6.9%	43.8	3.0	0.3	96.2%
Cardiovascular	36,262	22.2%	12.8%	48.7	3.2	0.5	95.0%
Neoplasms	28,261	17.3%	10.6%	48.6	2.6	2.2	59.9%
Digestive	20,902	12.8%	12.8%	45.2	1.7	0.4	96.1%
Respiratory	6,531	4.0%	4.9%	42.7	2.3	0.4	95.4%
Nervous	6,393	3.9%	5.3%	46.6	1.1	0.7	96.5%
Skin	5,100	3.1%	2.3%	43.5	1.0	3.5	98.2%
Musculoskeletal	5,043	3.1%	15.9%	44.5	0.9	0.4	98.4%
Infections	4,200	2.6%	1.1%	44.0	3.9	0.7	93.8%
Symptoms	3,749	2.3%	11.2%	45.9	1.0	0.6	93.9%
Genitourinary	2,275	1.4%	3.9%	45.7	1.5	0.5	96.9%
Supplementary	311	0.2%	9.0%	46.8	0.8	1.5	86.3%
Endocrine	10	0.0%	1.2%	45.7	2.1	2.4	93.2%
Average in control				**41.4**	**0.04**	**0.1**	**98.0%**

Notes: The Table reports the most common diagnostic groups for the diagnoses that we classify as health shocks - i.e. hospital visits whose onset is exogenous and cannot be medically predicted, excluding visits related to pregnancy and childbirth - for the sick men in our main sample. For each diagnostic group we report the total number of $t = 0$ shocks in the sample of sick individual and their share of total shocks. We also report a set of statistics based on the full sample of possible controls - i.e. the control sample which passes our sample restrictions, but that is not necessarily matched to a treated couple. Specifically, for each diagnostic group we report the share of total shock diagnoses, the average age at diagnosis, the average number of nights spent in the hospital immediately following a diagnosis in this group, the average number of hospital visits one year after a diagnosis, and the 5-year survival probability. The bottom line in the table reports these statistics for the full control sample. This Table is referenced in Section II.B.1.