Online Appendix to “Family Health Behaviors”

Itzik Fadlon and Torben Heien Nielsen

Appendix A: Institutional Background

Health insurance in Denmark is a universal scheme in which almost all costs are covered by the government. The few exceptions that entail a limited degree of out-of-pocket expenses include medical services provided by dentists, physiotherapists, psychologists, and chiropractors, as well as prescription drug co-insurance payments for prescriptions outside of hospitals as we describe below. The provision of public health insurance in Denmark is decentralized to the local government, specifically regions, that engage in common agreements with primary-care professionals and with non-hospital medical specialists and also fund public hospitals in the secondary healthcare sector. We describe the primary-care and the secondary-care sectors successively.

Primary Care. The main providers in the primary-care sector are general practitioners (GPs), who act as gatekeepers to the healthcare system, e.g., in terms of referring patients to hospitals and specialists. GPs are organized in private self-employed businesses and are reimbursed according to a fee-for-service schedule. The union of general practitioners (*Praktiserende lægers organization*) and the regional administration (*Regionernes Lønnings- og Takstnævn*) negotiate the annual fees for specific services, which are funded by regional and state taxes.

Each patient is assigned one GP, whose main responsibilities include medical consultations, non-specialized treatments, and provision of preventive care. For doing so, GPs are eligible to prescribe drugs for both treatment and prevention purposes. Patients pay no out-of-pocket costs for standard services provided by the GP, but there is some degree of co-insurance payments for medication prescribed by GPs. Specifically, until March 2000 patients paid 50% of pharmacy sale prices, with a reduced rate of 25% for drugs that treat life-threatening or chronic conditions. In March 2000 the payment scheme introduced a deductible with decreasing marginal co-insurance rates beyond the deductible amount. For annual expenses on prescription drugs up to DKK 865 (in 2012 rates as an example) patients would pay the full amount; and for additional expenses patients would pay 50% in the range of DKK 865-1,410, 25% in the range of DKK 1,410-3,045, and 15% for expenses over DKK 3,045.[1]

Secondary Care. The main entities in secondary care are public hospitals, to which patients are referred either by their GP or following visits to emergency rooms. Public hospitals operate as independent units with their own budgets funded via taxes by the regional government. Until the late 1990s hospitals were entirely funded by block grants and fee-for-service reimbursement schedules. From 1999, however, inspired by the American healthcare system, the funding gradually switched toward a scheme based on Diagnosis-Related Groups (DRGs). Within this scheme each patient's case is categorized into a DRG, and each DRG has prospectively set payment rates based on the average resources used to treat patients in that DRG. Initially, 10% of hospitals' budgets were funded through the DRG system.

[1] Additionally, there are annual caps for the chronically ill (so that, e.g., in 2012 these patients were fully reimbursed for expenses above DKK 3,555), and retirees can apply for means-tested reimbursements from the municipality.

This share increased to 20% in 2004 and is today between 50% and 70% depending on the region.

The main challenge within the hospital sector in recent decades has been long waiting lists for specialized treatments, which through the 1990s was addressed by increasing the degree of flexibility in individuals' hospital choice. Specifically, in the early 1990s patients were offered flexible hospital choice within regions, which was later extended to flexible choice nationally, and was finally expanded to the eligibility to choose private hospitals in case there were no availabilities in public hospitals. However, private hospitals account for only 2.5% of all hospital beds in the secondary sector and provide only very specialized services. Visits to private hospitals that are not due to public hospitals' unavailability are paid out-of-pocket on a fee-for-service basis. Still, patients rarely pay the full amount of these expenses, as many are covered by supplementary private insurance through their employers (who have tax incentives to provide these policies).

Appendix B: Alternative Control Group

In Appendix Table 11 we provide estimates for the effects of non-fatal cardiovascular shocks on the consumption of preventive care by spouses and adult children, based on an alternative matched control group as a robustness check. In this appendix, we describe in detail the matching procedure that we use to construct this control group.

The estimation is performed in three steps:

(I) *Predicting cardiovascular risk:* Using data on the entire Danish population aged 20-90 from the years 1994-2014, we estimate a probit model for the one-year probability of experiencing a first heart attack or stroke. Guided by the medical convention for calculating cardiovascular risk (De Backer et al. 2003, Pencina et al. 2014), we include as right-hand side variables age fixed effects, gender, and hypertension and diabetes statuses based on lagged indicators for condition-specific consumed medications. We also include year fixed effects as well as education as the best predictor available in our data that has been shown to strongly correlate with smoking (see, e.g., Cutler and Lleras-Muney 2010), which is also used in risk predictions by medical professionals. For each individual, we then calculate the lagged probability of experiencing the first heart attack or stroke based on this model. This provides each individual in our sample with a measure of underlying cardiovascular risk.

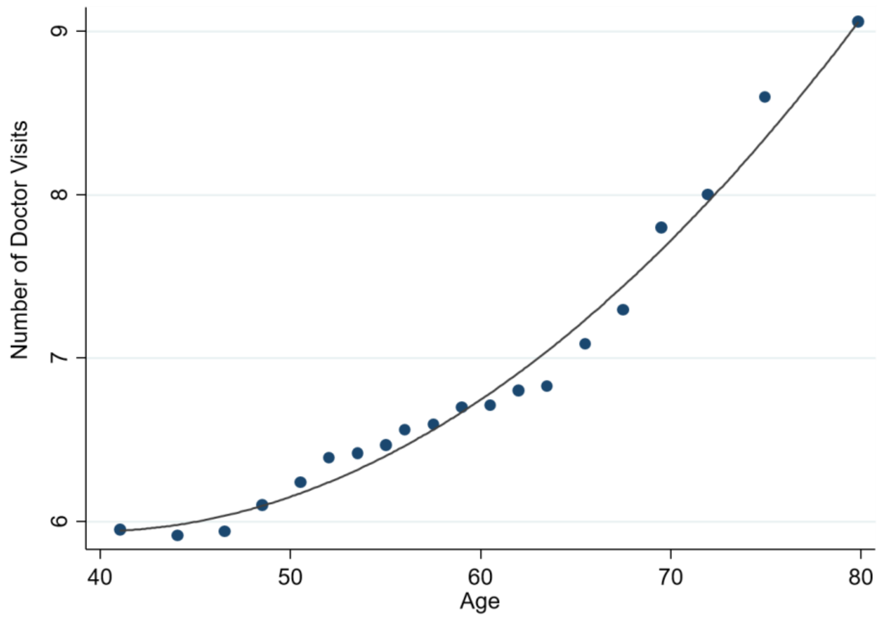
(II) *Creating match cells:* Within each calendar year, we create exact match cells by grouping the studied household units (i.e., spouses or parent-child pairs) based on the predicted underlying cardiovascular risk of the different family members and the lagged dependent variable to capture baseline utilization of preventive care and health behaviors.

(III) *Estimating the spillover effects:* We then estimate equation (1) by only changing the control group to consist of all households that are included in the match cells of the treated households, where we weight the control households by their relative representation within a match cell.

Consequently, this control group consists of families with similar underlying cardiovascular risk as the treatment group, but who either never experience a shock or experience a shock at a later period (compared to treatment households). Note that never-affected households (approximately 94% of control units for prime-age spouses) dominate the control group, which provides us with a broader set of comparable control households. While the control group expands, the treatment group reduces to a majority subset of treated households for whom exact matches were found. This incomplete overlap between treated households in our original design and treated households in the alternative design implies that estimations should naturally not perfectly align across designs. We ran the regressions in step (III) both with and without covariates, but these specifications are similar by construction as the same covariates (age fixed effects, gender, year fixed effects, and education) are used in the matching algorithm. Lastly, whereas our focus is on statin consumption, we also illustrate this exercise for cholesterol testing among the small sub-sample of treated households for whom we have blood test indicators (namely, those who reside in Greater Copenhagen). Since the requirement of exact matching on the set of observables that we use reduces the probability of finding a match, we utilize for this smaller sample a cruder match on risk deciles rather than on exact underlying risk to retain a sufficient number of treatment households in the analysis.

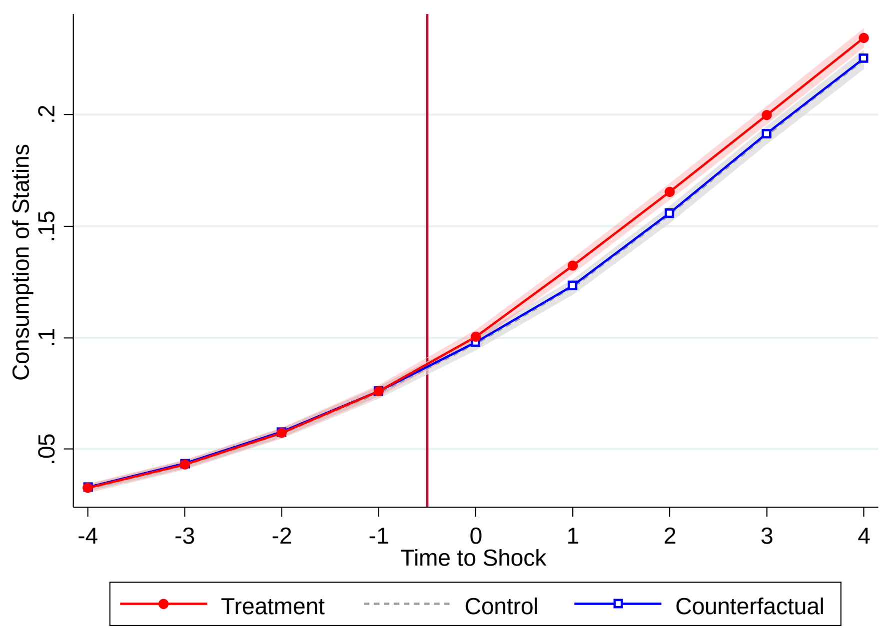
Overall, the current alternative design provides results that are similar to the results from our main research design described in Section 2 of the manuscript. A key advantage of the main design is its simplicity, as it matches households on one dimension only, namely, the timing at which the shock is realized. Creating the alternative control group requires much more refined matching on observables for the various members of the household, to obtain pre-trend comparability.

Appendix Figure 1: Interactions with the Medical System by Age



Notes: This figure plots averages for the number of doctor visits per individual within a year as a function of age. The blue dots represent raw means for each of the equal-sized age bins in the range of 40 to 80; the solid line represents the best quadratic fit (based on the individual-level data).

Appendix Figure 2: Effects of Cardiovascular Shocks on Older Spouses' Consumption of Preventive Care

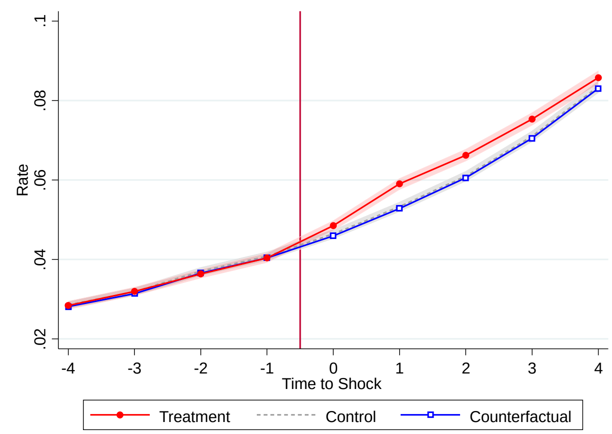


Notes: This figure displays changes in the consumption of preventive care among older spouses (of ages 55-85) in response to family cardiovascular shocks by plotting means of the raw data. The x-axis denotes time with respect to the shock, normalized to period 0. For the treatment group, period 0 is when the actual shock occurs; for the control group, period 0 is when a "placebo" shock occurs (while their actual shock occurs in period 5). The dashed gray line plots the behavior of the control group (along with the corresponding 95-percent confidence intervals). To ease the comparison of trends, from which the treatment effect is identified, we normalize the level of the control group's outcome to the pre-shock level of the treatment group's outcome (in period $t = -1$). This normalized counterfactual is displayed by the blue line and squares. The red line and circles plot the behavior of the treatment group (along with the corresponding 95-percent confidence intervals).

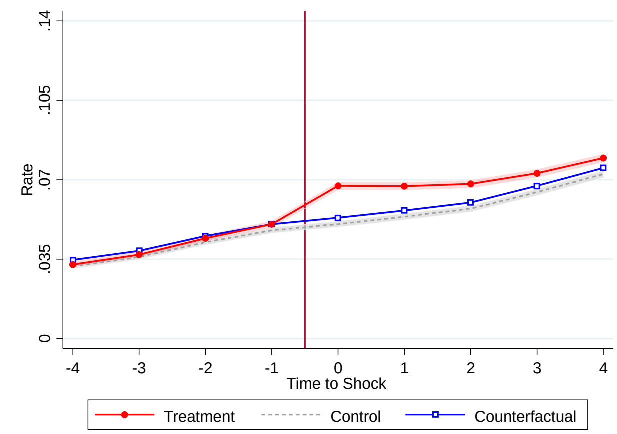
Appendix Figure 3: Effects of Spousal Death on Health Behaviors

Increased Awareness of Health

A. Hospital Medical Observation for Conditions that Are Ruled Out

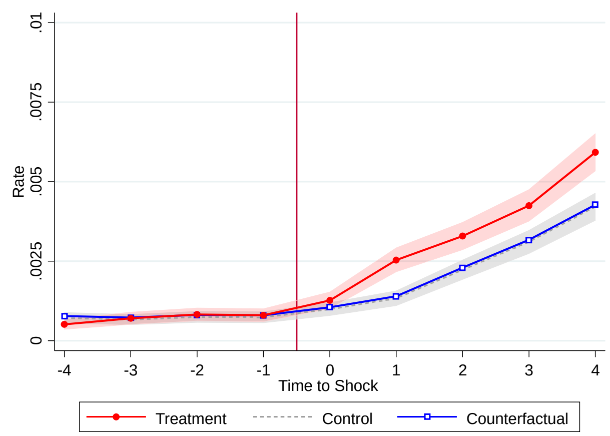


B. Non-Hospital Urgent Care Contacts

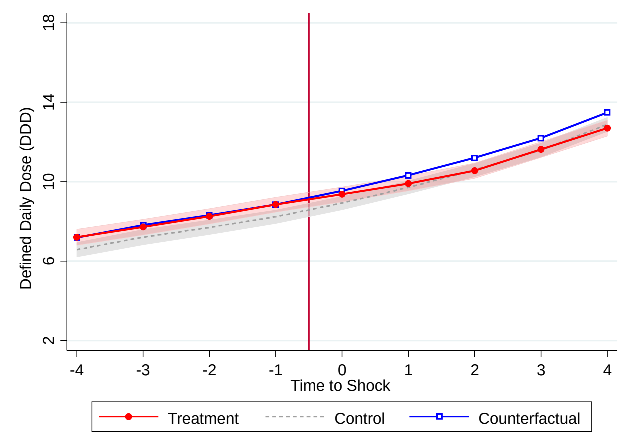


Declines in Harmful Behaviors and Medication

C. Consumption of Medication for Smoking Cessation

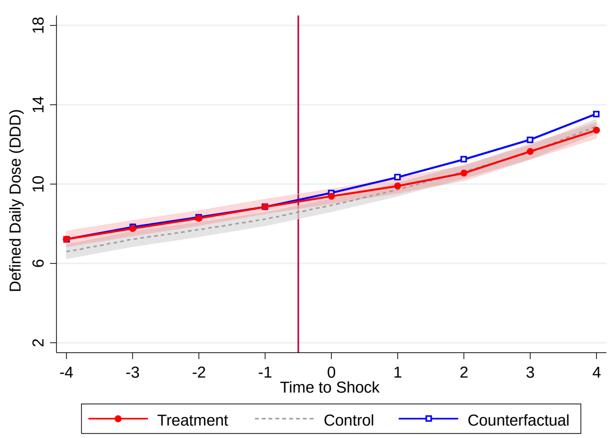


D. Opioid Dosage

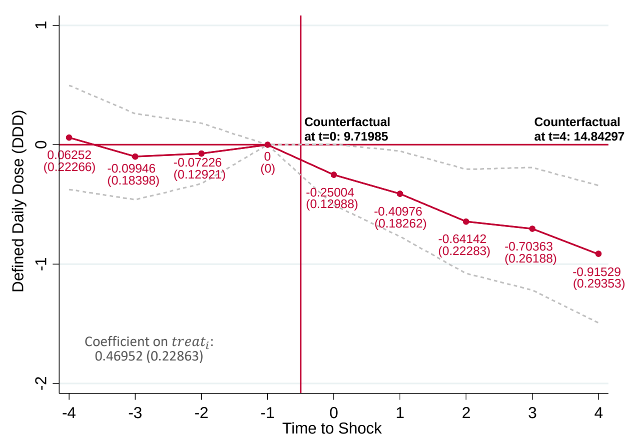


E. Opioid Dosage – Excluding Events with Prescription Opioid Poisoning as Cause of Death

E.1



E.2



Notes: These figures display changes in health behaviors in response to spousal death. The figures in panels A-E.1 plot means of the raw data. The x-axis denotes time with respect to the event, normalized to period 0. The dashed gray line plots the behavior of the control group (along with the corresponding 95-percent confidence intervals), and the normalized counterfactual (constructed by normalizing the level of the control group's outcome to the level of the treatment group's outcome in the pre-period $t = -1$) is displayed by the blue line and squares. The red line and circles plot the behavior of the treatment group (along with the corresponding 95-percent confidence intervals). The figure in panel E.2 plots the regression estimates for δ_r from equation (1) along with their 95-percent confidence intervals. We include as controls in this regression age fixed effects, calendar year fixed effects, gender, and education, and we report in parentheses robust standard errors clustered at the household level.

Appendix Table 1: Summary Statistics of Analysis Sample

		Year	Age	Education (Months)	Share Female	Number of Individuals
Non-Fatal Health Shocks						
Spouses						
Prime Age (25-55)	Treatment	2002	46.7	155.4	0.72	20,381
	Control	2002	45.8	156.6	0.704	28,699
Older (55-85)	Treatment	2002.2	65.7	136	0.64	37,828
	Control	2002.1	64.6	139	0.60	36,392
Adult Children						
Younger (25-40)	Treatment	2002	33.4	169	0.492	63,323
	Control	2001.9	33.1	170	0.492	68,437
Older (40-65)	Treatment	2002.4	44.6	166.3	0.46	39,783
	Control	2002.3	44.1	167.4	0.463	32,926
All Coworkers	Treatment	2001.7	48.2	161.2	0.374	52,388
	Control	2001.6	48.1	161.3	0.379	68,841
Close Coworkers	Treatment	2001.8	52.8	154.2	0.323	2,703
	Control	2001.6	51	156.6	0.327	3,522
Sons and Daughters In-Law	Treatment	2002.1	38.3	168.6	0.494	71,352
	Control	2002	37.3	169.3	0.49	69,889
Fatal Health Events						
Spouses	Treatment	1996.5	63.2	118.3	0.72	255,994
	Control	1996.4	62.4	119.9	0.70	341,329
Adult Children	Treatment	2003.7	41.2	166.6	0.47	324,594
	Control	2003.7	40.5	167.5	0.473	395,861

Notes: This table presents means of key variables in our analysis sample based on data from period 0. For each event, the treatment group is comprised of individuals whose family member (or peer) experienced a shock in some calendar year, to whom we match as a control group individuals from the same cohorts whose family member (or peer) experienced the same shock but five years later (Δ=5). Our primary sample of non-fatal health shocks is comprised of all households in which an individual experienced a heart attack or a stroke for the first time and survived for the four-year analysis horizon. The main close family circles that we study are spouses (based on matches of all married and cohabiting couples prior to the shock) and their adult children (based on matches available for individuals born after 1960). We additionally study two distant circles of family members and peers. The first is the sample of sons and daughters in-law (to whom we collectively refer as "children in-law"), which includes the spouses of the adult children whose parents experience a cardiovascular shock. The second is the sample of coworkers based on matched employer-employee register data, where we define workplaces using physical establishment units. To approximate peers with whom individuals are more likely to interact, we focus on "close" coworkers in the following way. From our sample of individuals who experience a health shock, we identify those who, during the pre-shock periods from t=-4 to t=-1, have worked in smaller workplaces where the number of employees was equal to or lower than the sample's 25th percentile (of approximately 20). We then identify their coworkers who have been employed in a similar occupation class, and who are close to these individuals in terms of age (with an age gap of 5 years or less). We exclude from this sample any coworker who is also a family member. Our secondary sample of death events includes all families in which one member died between 1985 and 2011. For these events we study spouses and children, whose respective samples are constructed in the same way as described above.

Appendix Table 2: Heterogeneity in Spousal Responses to Cardiovascular Shocks

	Cholesterol Testing	Statin Consumption for Sub-sample from Column 1	Statin Consumption	Statin Consumption	Statin Consumption by Previously Tested	Statin Consumption by Former Spouses
	(1)	(2)	(3)	(4)	(5)	(6)
Treat x Post	0.01628	0.00379			0.02542	0.00235
	(0.00462)	(0.00332)			(0.01361)	(0.00424)
Treat x Post x	-0.00002	0.00801		0.01316		
High Risk	(0.00597)	(0.00526)		(0.00273)		
Treat x Post x			-1.45039	-0.92161		
Risk Gap			(0.41284)	(0.44584)		
Number of Observations	214,037	189,927	930,448	930,448	21,856	61,304
Number of Clusters	20,924	20,924	101,237	101,237	2,635	7,572

Notes: This table studies heterogeneity in spousal responses to cardiovascular shocks along different dimensions. Column 1 estimates equation (3) and analyzes how spouses' responses in cholesterol testing vary by whether the spouse's own predicted cardiovascular risk is above or below the median. In this regression, the post-shock years also include period 0, in which the dynamic analysis found a large immediate effect, and the sample comprises residents of Greater Copenhagen for whom data on blood tests are available. Column 2 provides a similar analysis but where the outcome variable is spouses' statin consumption. It replicates column 1 from Table 3 (which was estimated for the full population) for the restricted sub-sample of Greater Copenhagen residents, where the point estimates display a similar response gradient in risk though we lose precision due to small sample sizes. Columns 3 and 4 estimate equation (3) to study how spousal responses in statin consumption vary by the similarity of their predicted baseline cardiovascular risk to that of their partners who experience the shock. Column 3 interacts the treatment effect with this risk gap, and column 4 also adds an interaction with an indicator for whether the spouse's own predicted risk is above or below the median. For the constrained sample of Greater Copenhagen residents, column 5 estimates equation (2) and analyzes statin consumption responses by spouses whose cholesterol levels had been already tested, similar to column 3 from Table 3. Further restricting the sample to those who have been tested just before the shock (in period -1), the results provide evidence for spillover responses even among the small sub-sample of spouses with the most updated information on own cardiovascular risk. Column 6 estimates equation (2) and analyzes statin consumption among former spouses, where we define former couples as individuals who were linked through marriage or cohabitation in period -5 but are no longer linked in period -1. We include as controls age fixed effects, calendar year fixed effects, gender, and education, and we report in parentheses robust standard errors clustered at the household level.

Appendix Table 3: Testing Alternative Mechanisms for Spousal Responses

	Presence of Children		Severity of Shock		Income Loss			
					Mean Changes		By Sick Spouse's Share of Income	
	Statin Consumption	Statin Consumption	Statin Consumption	Sick Spouse's Labor Supply	Statin Consumption	Household Income	Statin Consumption	Household Income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat x Post	0.01149		0.01809	-0.03308	0.01742	-9,273	0.00643	-8,255
	(0.00264)		(0.00157)	(0.00240)	(0.00144)	(607)	(0.00272)	(1,644)
Treat x Post x	-0.00869	-0.00450						
Child Below 18	(0.00322)	(0.00360)						
Treat x Post x		0.00063						
Own Age		(0.00022)						
Treat x Post x			-0.000032	-0.00106				
Hospital Days			(0.000035)	(0.00009)				
Treat x Post x							-0.00022	-4,405
Sick Primary Earner							(0.00358)	(2,238)
Counterfactual						443,765		
Percent Change						-2.09		
Number of Observations	392,640	392,640	955,722	955,722	955,174	955,174	332,891	332,891
Number of Clusters	44,302	44,302	104,047	104,047	104,044	104,044	37,724	37,724

Notes: This table studies heterogeneity in spousal responses to cardiovascular shocks along different dimensions. Columns 1-2 estimate equation (3) and analyze whether spouses' responses in statin consumption vary by the presence of younger children. Column 1 interacts the treatment effect with an indicator for the presence of children younger than 18, and column 2 also adds an interaction with the spouse's own age. Similar results are found for other age thresholds (12 and 6). In these regressions we include prime-age spouses (ages 25-55) who are more likely to have younger children. Columns 3-4 estimate equation (3) to study whether spousal responses vary by the severity of the family shock, as defined by the number of hospitalization days. Similar results are found if severity is defined by hospitalization days being above or below the median, and if we further interact the treatment effect with the sick spouse's age at the time of the shock. Column 3 and 4 jointly show that while those who experience more severe shocks are more likely to drop out of the labor force (and potentially require more caregiving), spouses' health investments do not vary by this dimension. Column 5-8 investigate responses by income losses. First, columns 5-6 estimate equation (2) and show that the investments in spousal health are present even though households experience very small relative income losses (compared to the counterfactual), taking into account all income sources and government transfers. Second, columns 7-8 further show using specification (3) that while households in which the sick person was the primary earner experience larger income losses, spousal health investments do not vary by this dimension. These regressions include prime-age sick spouses (of ages 25-55) who were more likely to participate in the labor force in the pre-period, but similar results are found when we include all households and define income shares for each household member using income from any source (not only from labor earnings). We include as controls age fixed effects, calendar year fixed effects, gender, and education, and we report in parentheses robust standard errors clustered at the household level.

Appendix Table 4: Family Effects of Cardiovascular Shocks—Different Physicians

	Spouses' Statin Consumption					Adult Children's Statin Consumption				
	Different Matched GP	Number of Patients Overlapped		Share of Patients Overlapped		Different Matched GP	Number of Patients Overlapped		Share of Patients Overlapped	
		Less than 50	Less than 20	Less than 0.05	Less than 0.02		Less than 50	Less than 20	Less than 0.05	Less than 0.02
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Time to Shock:										
-4	-0.00187	-0.00132	-0.00118	-0.00220	-0.00207	-0.00004	-0.00005	-0.00005	-0.00000	-0.00003
	(0.00190)	(0.00204)	(0.00216)	(0.00199)	(0.00209)	(0.00033)	(0.00034)	(0.00034)	(0.00033)	(0.00034)
-3	-0.00164	-0.00119	-0.00153	-0.00169	-0.00179	-0.00082	-0.00007	-0.00006	-0.00005	-0.00007
	(0.00176)	(0.00189)	(0.00201)	(0.00184)	(0.00195)	(0.00031)	(0.00031)	(0.00032)	(0.00031)	(0.00032)
-2	-0.00171	-0.00113	-0.00109	-0.00123	-0.00086	0.00021	0.00023	0.00018	0.00028	0.00023
	(0.00147)	(0.00158)	(0.00167)	(0.00153)	(0.00162)	(0.00025)	(0.00026)	(0.00027)	(0.00030)	(0.00026)
-1	0	0	0	0	0	0	0	0	0	0
	0	0	0	0	0	0	0	0	0	0
0	0.00389	0.00332	0.00367	0.00352	0.00342	0.00114	0.00111	0.00113	0.00104	0.00109
	(0.00171)	(0.00184)	(0.00193)	(0.00181)	(0.00187)	(0.00032)	(0.00032)	(0.00033)	(0.00032)	(0.00033)
1	0.00681	0.00676	0.00673	0.00631	0.00595	0.00289	0.00266	0.00277	0.00274	0.00276
	(0.00237)	(0.00253)	(0.00267)	(0.00249)	(0.00259)	(0.00046)	(0.00047)	(0.00048)	(0.00046)	(0.00047)
2	0.01037	0.01003	0.01030	0.00867	0.00831	0.00356	0.00340	0.00339	0.00351	0.00348
	(0.00293)	(0.00312)	(0.00333)	(0.00306)	(0.00322)	(0.00058)	(0.00059)	(0.00060)	(0.00059)	(0.00060)
3	0.01403	0.01432	0.01514	0.01286	0.01440	0.00491	0.00473	0.00483	0.00491	0.00490
	(0.00340)	(0.00363)	(0.00388)	(0.00356)	(0.00376)	(0.00070)	(0.00071)	(0.00072)	(0.00071)	(0.00072)
4	0.01066	0.01072	0.01224	0.00952	0.00973	0.00632	0.00615	0.00619	0.00626	0.00622
	(0.00383)	(0.00409)	(0.00436)	(0.00401)	(0.00423)	(0.00083)	(0.00084)	(0.00085)	(0.00084)	(0.00085)
Treat	-0.00091	0.00043	0.00123	0.00046	0.00125	-0.00051	-0.00052	-0.00059	-0.00056	-0.00052
	(0.00221)	(0.00237)	(0.00253)	(0.00231)	(0.00243)	(0.00036)	(0.00037)	(0.00038)	(0.00037)	(0.00038)
Number of Observations	238,779	204,201	176,040	214,515	188,865	1,296,423	1,236,303	1,182,555	1,254,375	1,207,953
Number of Clusters	24,373	21,033	18,268	22,017	19,483	72,855	70,850	68,866	71,400	69,761

Notes: This table reports dynamic difference-in-differences estimates for the evolution of household responses to cardiovascular shocks using specification (1). It displays estimates for the δ_r parameter vector of the interaction between the treatment indicator and the indicators for time with respect to the shock from -4 to +4, where the baseline period is -1. We also report the estimate for the coefficient β on the variable $treat_i$. In this table, we analyze only households in which the family members whose behaviors we study do not share the same doctor with the person who experiences the cardiovascular shock. The data allow matching patients to their general practitioner (GP) since any service provided to a patient by a GP documents the GP's identifier and whether he or she is the patient's assigned GP. The analysis of family members with different matched GPs is reported in column 1 for spouses and in column 6 for children. As the different physicians may share clinics which could lead to information flows across doctors, we further guarantee the separation of healthcare providers by studying only physicians whose patient overlap is minimal. Specifically, we exclude observations for whom the GP of the person that experienced the shock treated a non-negligible portion of the patients of the GP that is assigned to the family member. Columns 2-3 and 7-8 include only observations where patient overlap falls below a threshold number (where the average number of patients per GP is 1,279), and column 4-5 and 9-10 include only observations where patient overlap falls below a threshold share. Overall, we find similar-magnitude effects among these households, suggesting that the spillover is not likely to be driven by the responses of a family physician who may provide primary-care to several members of the household and aggregate information across them. The regressions include as controls age fixed effects, calendar year fixed effects, gender, and education, and we report in parentheses robust standard errors clustered at the household level.

Appendix Table 5: Robustness Checks for Closeness of Peers

Panel A: Age Gap

Max. Years of Age Gap:	7	6	5	4	3
Treat x Post	0.01136	0.01123	0.01349	0.01437	0.01184
	(0.00490)	(0.00527)	(0.00559)	(0.00601)	(0.00678)
Number of Observations	64,192	56,816	49,336	40,864	32,592
Number of Clusters	4,046	3,785	3,498	3,138	2,684

Panel B: Workplace Size

Max. Number of Employees:	24	23	22	21	20	19	18	17	16
Treat x Post	0.00765	0.00919	0.00959	0.01153	0.01349	0.01428	0.01244	0.01474	0.01884
	(0.00486)	(0.00500)	(0.00521)	(0.00539)	(0.00559)	(0.00577)	(0.00602)	(0.00646)	(0.00668)
Number of Observations	64,320	60,384	56,736	53,336	49,336	45,344	41,488	37,840	34,408
Number of Clusters	4,192	4,027	3,856	3,682	3,498	3,286	3,094	2,900	2,713

Notes: This table reports difference-in-differences estimates for coworkers' responses to cardiovascular shocks using specification (2). The table provides as robustness checks estimations that perturb the thresholds of age gap and workplace size in our definition of "close" coworkers. Panel A perturbs the age gap between coworkers and the person that experiences the shock around our choice of 5 years; and panel B perturbs the workplace size around our choice of 20 employees (the sample's 25th percentile). We include as controls age fixed effects, calendar year fixed effects, gender, and education, and we report in parentheses robust standard errors clustered at the workplace level.

Appendix Table 6: Family Members' Health Behaviors following Fatal Events

	Spouses' Consumption of Medication for Smoking Cessation when C.o.d is Autoimmune Disease	Adult Children				
		Hospital Medical Observation for Conditions that Are Ruled Out	Non-Hospital Urgent Care Contacts	Consumption of Medication for Smoking Cessation	Statin Use when Cause of Death is Cardiovascular	Diagnostic Radiology when Cause of Death is Cancer
	(1)	(2)	(3)	(4)	(5)	(6)
Treat x Post	0.00243	0.00190	0.00146	0.00041		
	(0.00119)	(0.00041)	(0.00065)	(0.00009)		
C.o.d x Post					0.00469	0.74009
					(0.00084)	(0.29324)
Counterfactual	0.00162	0.06343	0.06171	0.00373		
Baseline *t*=-1					0.01364	10.16599
Number of Observations	19,663	6,276,868	3,002,647	5,764,516	2,597,547	2,612,139
Number of Clusters	2,105	306,841	188,719	287,943	167,579	228,835

Notes: This table reports estimates for family members' responses to death events. In column 1, using equation (2), we estimate the consumption of medication that treats nicotine dependence by individuals whose spouse's cause of death was autoimmune disease. Columns 2 to 4 estimate equation (2) for different behavioral outcomes of adult children, which are indicated at the top of each column. Counterfactual levels in the periods following the event are calculated using these estimations. Our data use agreement excludes some information on drug prescriptions for the sample of adult children (as opposed to spouses). Drug dosage is part of the excluded data, so that responses in prescription opioid doses are the one outcome for which we cannot provide the corresponding estimation for adult children. Columns 5 and 6 estimate specifications of equation (5) for adult children. Column 5 compares statin consumption by individuals whose parent died of cardiovascular disease to that by individuals whose parent died of any other cause; column 6 compares expenditure on diagnostic radiology by individuals whose parent died of cancer to that by individuals whose parent died of any other cause. In these regressions, we indicate baseline levels in period -1 among the sub-sample of households with the studied cause of death. We include as controls age fixed effects, calendar year fixed effects, gender, and education, and we report in parentheses robust standard errors clustered at the household level.

Appendix Table 7: Severe Health Conditions following Family Health Events

	Non-Fatal Cardiovascular Shocks								Spousal Death	
	Spouses				Adult Children					
	Incidence of Own Cardiovascular Shock		Any Hospitalization		Incidence of Own Cardiovascular Shock		Any Hospitalization		Any Hospitalization	Major Conditions
	Prime Age	Older	Prime Age	Older	Younger	Older	Younger	Older		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treat x Post	-0.00020	-0.00029	0.00313	0.00058	0.00019	0.00021	0.00156	0.00121	0.02031	0.00364
	(0.00043)	(0.00067)	(0.00293)	(0.00243)	(0.00014)	(0.00030)	(0.00183)	(0.00232)	(0.00138)	(0.00083)
Counterfactual	0.00532	0.01941	0.30344	0.37237	0.00144	0.00398	0.31222	0.26995	0.37427	0.10540
Number of Observations	441,720	667,980	441,720	667,980	1,179,387	647,667	1,179,387	647,667	2,230,731	2,230,731
Number of Clusters	44,302	65,661	44,302	65,661	67,460	40,690	67,460	40,690	210,431	210,431

Notes: This table reports difference-in-differences estimates using equation (2) for the effects of health events on family members' severe health conditions based on hospital contacts. Counterfactual levels in the periods following the event are calculated using these estimations. The outcome "major conditions" represents an indicator for a hospital contact related to any severe condition included in the Charlson Comorbidity Index (Charlson et al. 1987): Acute Myocardial Infarction, Cerebrovascular Disease, Chronic Pulmonary Disease, Congestive Heart Failure, Cancer, Dementia, Diabetes with chronic complications, Diabetes without complications, AIDS/HIV, Hemiplegia or Paraplegia, Metastatic Carcinoma, Mild Liver Disease, Moderate or Severe Liver Disease, Peptic Ulcer Disease, Peripheral Vascular Disease, Renal Disease, and Rheumatologic Disease (Connective Tissue Disease). This index is a weighted sum of the number of specific diagnoses in a given year, which was originally designed to predict ten-year mortality and is now widely used as a measure of adverse health (see, e.g., Ho and Pakes 2014 and Finkelstein et al. 2016). Similar results are found when we narrow the analysis to conditions included in the Iezzoni Chronic Conditions (Iezzoni et al. 1994), another widely-studied set of illnesses (see, e.g., Welch et al. 2011 and Finkelstein et al. 2016), as well as when we study the Charlson numerical index itself (instead of illness indicators). We include as controls age fixed effects, calendar year fixed effects, gender, and education, and we report in parentheses robust standard errors clustered at the household level.

Appendix Table 8: Effects of Cardiovascular Shocks for Different Values of the Bandwidth Δ

	Value of Bandwidth Δ					Value of Bandwidth Δ				
	3	4	5	6	7	3	4	5	6	7
Time to Shock:										
-4	0.00035	0.00025	-0.00070	-0.00156	-0.00112	0.00083	0.00081	-0.00120	-0.00062	0.00009
	(0.00112)	(0.00109)	(0.00106)	(0.00103)	(0.00103)	(0.00113)	(0.00110)	(0.00106)	(0.00104)	(0.00104)
-3	-0.00080	-0.00012	-0.00035	-0.00211	-0.00152	-0.00040	0.00015	-0.00006	-0.00151	-0.00076
	(0.00100)	(0.00100)	(0.00099)	(0.00096)	(0.00095)	(0.00101)	(0.00100)	(0.00099)	(0.00096)	(0.00095)
-2	-0.00006	-0.00000	0.00117	-0.00068	-0.00056	0.00016	0.00025	0.00127	-0.00029	-0.00020
	(0.00080)	(0.00082)	(0.00082)	(0.00079)	(0.00078)	(0.00081)	(0.00082)	(0.00082)	(0.00079)	(0.00078)
-1	0	0	0	0	0	0	0	0	0	0
	0	0	0	0	0	0	0	0	0	0
0	0.00419	0.00364	0.00427	0.00248	0.00418	0.00392	0.00321	0.00388	0.00208	0.00369
	(0.00103)	(0.00102)	(0.00101)	(0.00102)	(0.00099)	(0.00104)	(0.00103)	(0.00101)	(0.00102)	(0.00099)
1	0.00598	0.00476	0.00596	0.00442	0.00479	0.00519	0.00363	0.00475	0.00313	0.00336
	(0.00143)	(0.00142)	(0.00141)	(0.00141)	(0.00139)	(0.00142)	(0.00142)	(0.00141)	(0.00140)	(0.00138)
2	0.00759	0.00737	0.00680	0.00726	0.00784	0.00627	0.00575	0.00512	0.00514	0.00549
	(0.00186)	(0.00182)	(0.00182)	(0.00177)	(0.00176)	(0.00189)	(0.00181)	(0.00181)	(0.00177)	(0.00176)
3		0.01086	0.01155	0.01086	0.01282		0.00841	0.00914	0.00792	0.00939
		(0.00225)	(0.00222)	(0.00219)	(0.00216)		(0.00225)	(0.00221)	(0.00218)	(0.00215)
4			0.01402	0.01321	0.01535			0.01110	0.00969	0.01105
			(0.00260)	(0.00256)	(0.00253)			(0.00260)	(0.00255)	(0.00253)
Treat	0.00034	0.00045	0.00103	0.00341	0.00225	-0.00081	-0.00088	-0.00055	0.00116	-0.00040
	(0.00119)	(0.00124)	(0.00125)	(0.00119)	(0.00121)	(0.00121)	(0.00125)	(0.00126)	(0.00121)	(0.00123)
Number of Observations	240,093	282,672	321,552	336,528	346,932	236,663	278,776	316,971	331,803	341,919
Number of Clusters	28,167	32,400	35,728	37,392	38,548	27,762	31,947	35,219	36,867	37,991
No Controls	X	X	X	X	X					
Controls						X	X	X	X	X

Notes: This table reports dynamic difference-in-differences estimates for the responses to family cardiovascular shocks for different choices of the bandwidth Δ using specification (1). As an illustration, we study the evolution of statin consumption responses among prime-age spouses. The tables display estimates for the δ_r parameter vector of the interaction between the treatment indicator and the indicators for time with respect to the shock, where the baseline period is -1. We also report the estimate for the coefficient β on the variable $treat_i$. The table includes non-fatal cardiovascular shocks with the requirement that the individual that experienced the shock survived for four years across all columns for comparability (and hence regressions run up to period 4). To further ensure comparability across bandwidths, we require that the range of calendar years in which households experienced the shock—which determines the composition of the treatment/control groups for any choice of bandwidth—would be similar across choices of Δ. This guarantees that it is always the same treatment group whose responses are investigated, and that only the control group changes across columns. The upper bound for included years is governed by the highest value of Δ analyzed (of seven years), so that the last shock year is 2003 due to the survival requirement and the fact that the death records are truncated at the year 2014. The latter restriction accounts for the difference between our estimates here for Δ=5 and those reported in Table 1, as the treatment group in the former is a subset of the treatment group in the latter (since the analysis here is constrained by the highest Δ of seven years). We first report raw regressions with no controls for all households, and we then report regressions with controls for all households for whom we have information on education, as we include in our set of controls age fixed effects, calendar year fixed effects, gender, and education. We report in parentheses robust standard errors clustered at the household level.

Appendix Table 9: Dynamic Family Effects of Cardiovascular Shocks—Alternative Control Group

	Spouses' Statin Consumption				Spouses' Cholesterol Testing		Adult Children's Statin Consumption			
	Prime Age (Ages 25-55)		Older (Ages 55-85)				Younger (Ages 25-40)		Older (Ages 40-65)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Time to Shock:										
-4	0.00071	0.00071	0.00027	0.00027	-0.00197	-0.00206	0.00001	0.00001	0.00011	0.00011
	(0.00071)	(0.00071)	(0.00127)	(0.00126)	(0.00338)	(0.00338)	(0.00007)	(0.00007)	(0.00029)	(0.00030)
-3	-0.00032	-0.00032	0.00055	0.00055	0.00370	0.00367	-0.00000	-0.00000	0.00032	0.00032
	(0.00063)	(0.00063)	(0.00114)	(0.00113)	(0.00349)	(0.00349)	(0.00008)	(0.00008)	(0.00031)	(0.00031)
-2	0.00077	0.00077	0.00049	0.00049	0.00068	0.00069	0.00013	0.00013	0.00025	0.00025
	(0.00051)	(0.00051)	(0.00092)	(0.00092)	(0.00342)	(0.00342)	(0.00007)	(0.00007)	(0.00025)	(0.00025)
-1	0	0	0	0	0	0	0	0	0	0
	0	0	0	0	0	0	0	0	0	0
0	0.00313	0.00313	0.00126	0.00126	0.02561	0.02569	0.00071	0.00071	0.00122	0.00122
	(0.00091)	(0.00091)	(0.00119)	(0.00119)	(0.00392)	(0.00392)	(0.00018)	(0.00018)	(0.00052)	(0.00052)
1	0.00494	0.00491	0.00619	0.00617	0.01694	0.01683	0.00169	0.00169	0.00448	0.00446
	(0.00127)	(0.00126)	(0.00168)	(0.00167)	(0.00400)	(0.00400)	(0.00028)	(0.00028)	(0.00080)	(0.00080)
2	0.00658	0.00649	0.00875	0.00876	0.01214	0.01197	0.00279	0.00278	0.00576	0.00573
	(0.00161)	(0.00160)	(0.00206)	(0.00205)	(0.00420)	(0.00420)	(0.00038)	(0.00038)	(0.00102)	(0.00101)
3	0.00928	0.00913	0.00753	0.00758	0.00900	0.00874	0.00364	0.00361	0.00828	0.00824
	(0.00196)	(0.00194)	(0.00236)	(0.00235)	(0.00441)	(0.00440)	(0.00048)	(0.00048)	(0.00125)	(0.00124)
4	0.01046	0.01022	0.00975	0.00991	0.01064	0.01025	0.00489	0.00485	0.00867	0.00861
	(0.00224)	(0.00221)	(0.00262)	(0.00261)	(0.00457)	(0.00456)	(0.00056)	(0.00056)	(0.00146)	(0.00145)
Counterfactual at *t*=4	0.06554	0.06578	0.20421	0.20406			0.00810	0.00815	0.03640	0.03646
Percent Change	15.96	15.54	4.76	4.85			60.37	59.47	23.81	23.61
Counterfactual at *t*=0					0.13524	0.13516				
Percent Change					18.93	19.01				
Number of Matched Treated Households	15,842	15,842	27,164	27,164	10,668	10,668	49,551	49,551	26,762	26,762
Number of Obs.	4,126,305	4,126,305	6,486,231	6,486,231	21,307,138	21,307,138	8,667,418	8,667,418	3,674,905	3,674,905
Number of Clusters	305,662	305,662	309,696	309,696	493,357	493,357	418,511	418,511	179,542	179,542
No Controls	X		X		X		X		X	
Controls		X		X		X		X		X

Notes: This table reports dynamic difference-in-differences estimates for the evolution of household responses to cardiovascular shocks using an alternative control group that matches households on observable pre-shock risk factors. This approach expands our control group to a broader set of comparable households, which includes those who never experience the health shock despite having similar underlying risk, as well as those who experience the shock but within other Δ bandwidths. Note that due to the matching requirement, the treatment group reduces to a majority subset of treated households for whom exact matches were found, so that there is incomplete overlap across treated households in our original design and treated households in the current alternative design (and hence also across estimations). The matching procedure that we use is described in detail in Appendix A. Using specification (1), this table displays estimates for the δ_r parameter vector of the interaction between the treatment indicator and the indicators for time with respect to the shock from -4 to +4, where the baseline period is -1. Counterfactual levels are calculated using this estimation. We include as controls age fixed effects, calendar year fixed effects, gender, and education, and we report in parentheses robust standard errors clustered at the household level.

Appendix Table 10: Dynamic Effects of Cardiovascular Shocks on Statin Consumption

	Close Coworkers (1)	Nearby In-Laws (2)
Time to Shock:		
-4	-0.00230	0.00016
	(0.00388)	(0.00062)
-3	-0.00198	0.00004
	(0.00341)	(0.00057)
-2	-0.00044	0.00023
	(0.00268)	(0.00046)
-1	0	0
	0	0
0	0.00632	0.00088
	(0.00295)	(0.00056)
1	0.01160	0.00178
	(0.00444)	(0.00082)
2	0.01001	0.00210
	(0.00555)	(0.00103)
3	0.01303	0.00373
	(0.00656)	(0.00125)
4	0.01586	0.00278
	(0.00764)	(0.00144)
Treat	-0.00483	-0.00015
	(0.00470)	(0.00070)
Counterfactual at *t*=4	0.11804	0.03233
Percent Change	13.44	8.60
Number of Observations	55,503	452,862
Number of Clusters	3,498	33,752

Notes: This table reports dynamic difference-in-differences estimates for network members' responses to cardiovascular shocks using specification (1). Close coworkers are defined as coworkers within the same occupation class and age range (with an age gap of 5 years or less) in smaller workplaces (in which the number of employees was equal to or lower than the sample's 25th percentile). Nearby in-laws are sons and daughters in-law who live closer to their parents in-law as defined relative to the median distance. The table displays estimates for the δ_r parameter vector of the interaction between the treatment indicator and the indicators for time with respect to the shock from -4 to +4, where the baseline period is -1. We also report the estimate for the coefficient β on the variable $treat_i$. Counterfactual levels are calculated using specification (1). We include as controls age fixed effects, calendar year fixed effects, gender, and education. We report in parentheses robust standard errors which are clustered at the workplace level in column 1 and at the household level in column 2.

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