Online Appendix: Propagation and Insurance in Village Networks

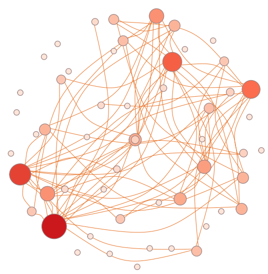
Cynthia Kinnan, Krislert Samphantharak, Robert Townsend
and Diego Vera-Cossio

A1. Supportive evidence

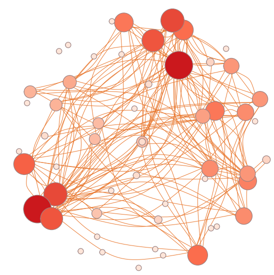
Table A1—: Summary statistics

	count	mean	sd	p10	p90
Panel A: Household baseline characteristics					
Number of household members	510	4.53	1.87	2	7
Number of adults	510	2.87	1.38	1	5
Age (household head)	508	52.00	13.49	35	70
Age (household average)	510	34.19	12.14	21	52
Household head is male	508	0.77	0.42	0	1
Years of schooling: Household head	505	4.49	2.59	3	7
Years of schooling: Household maximum achievement	510	8.19	3.64	4	14
Years of schooling: Household average	510	5.09	2.17	3	8
Panel B: Household finance (annual data)	count	mean	sd	p10	p90
Farm	7650	134203.22	1377160.98	-151	316242
Off-farm business	7650	19061.31	115429.66	0	40654
Labor	7650	48537.08	102427.94	0	141428
Total from operations (farm+off-farm + labor)	7650	516020.23	2490777.97	15228	1104350
Gifts/transfers	7650	23935.48	184141.89	-11632	75635
Total net income (Operations+Gifts/Transfers)	7650	539955.71	2497465.40	29614	1116092
Food consumption	7650	32916.51	21912.78	11865	60521
Total consumption	7650	98030.54	99438.08	24189	204476
Total Assets (THB)	7650	2345327.56	7351009.41	168188	4660295
Fixed Assets/ Total Assets (%)	7650	53.12	27.12	13	88
Total debt/Total assets (%)	7650	11.60	21.42	0	27
Panel C: Village Networks	count	mean	sd	p10	p90
Supply chain network: Degree (number of links)	7650	1.36	2.71	0	3
Supply chain network: Participation (any link)	7650	0.51	0.50	0	1
Labor-market network: Degree	7650	3.33	4.51	0	9
Labor-market network: Participation	7650	0.66	0.46	0	1
Financial network: Degree	7650	0.70	1.40	0	2
Financial network: Participation	7650	0.38	0.48	0	1
Baseline kinship network: Degree	7650	2.36	2.19	0	6
Baseline kinship network: Participation	7650	0.77	0.42	0	1
Panel D: Village and firm size	count	mean	sd	p10	p90
Number of households in the village	16	160.95	89.61	74	330
Village-level average firm size	240	341048.59	397630.43	59966	620106
Village-level standard deviation of firm size	240	618846.47	1452881.89	69877	1222209
Village-level kurtosis of average village firm size	240	10.13	5.92	4	19

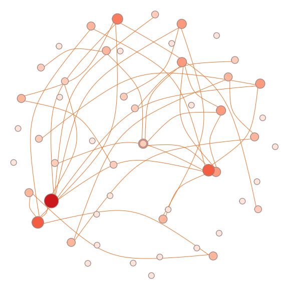
Note: Panel A reports summary statistics for baseline demographic characteristics. Panel B reports household financial characteristics (annual averages using balanced panel of 509 households). Farm income includes income from agriculture, livestock, fish and shrimp. Off-farm income excludes earnings from labor provision. In both cases income is net of operation costs. Gifts and transfers include transactions from households inside and outside the village and receipt of government transfers. Consumption includes spending and consumption of home production. In Panel C, all networks are unweighted and undirected. Kinship networks are measured at baseline; transaction networks are measured on an annual basis. Financial networks are based on gifts and loans between households in the same village. Supply chain networks include transactions of raw material and intermediate goods between businesses operated by households in the same village. Labor networks include paid and unpaid labor between households in the same village. Degree: Number of households with whom each household transacted in each year. Access=1 if the household has participated in the network in a given year; 0 otherwise. Panel D reports characteristics at the village level (16 villages). Firm size statistics are computed at village-year level using gross annual revenues as a proxy for firm size.



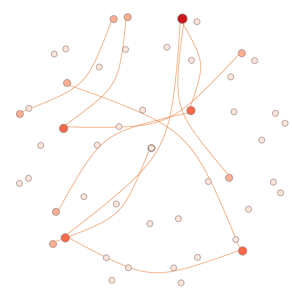
(a) Supply chain



(b) Labor



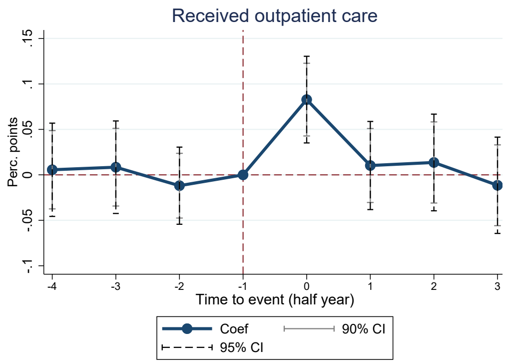
(c) Kinship



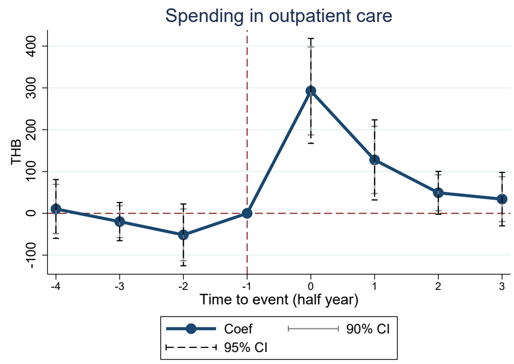
(d) Financial

Figure A1. : Socioeconomic Networks for a sample village

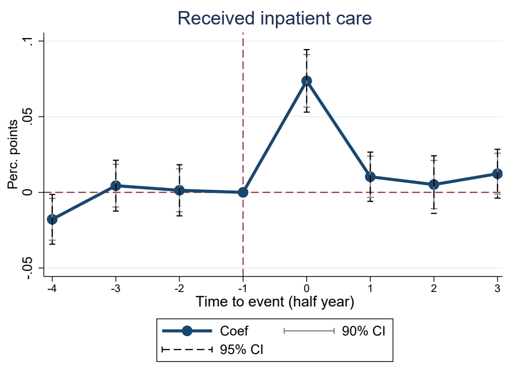
Note: The Figure depicts undirected, unweighted networks corresponding to a sample village in our sample. Each dot represents a node. The size of the node increases with the number of links of each node. Each link represents whether two households have transacted during the reference period. The transaction networks are measured on an annual basis. The reference period for is 2005. Supply chain networks include transactions of raw material and intermediate goods as well as final goods between businesses operated by households in the same village. Labor networks include relationships through paid and unpaid labor between households in the same village. Kinship networks are measured at baseline in 1998, while transaction networks are measured on an annual basis. Financial networks are constructed based on gifts and loans between households in the same village.



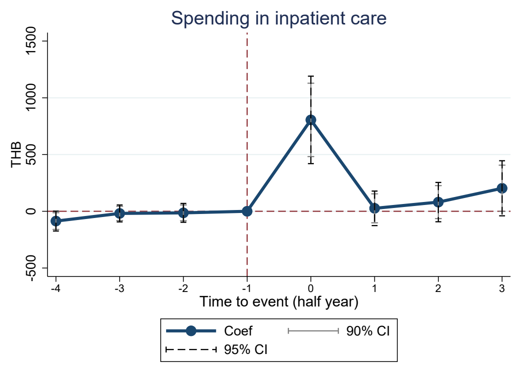
(a) Outpatient care (Prob.)



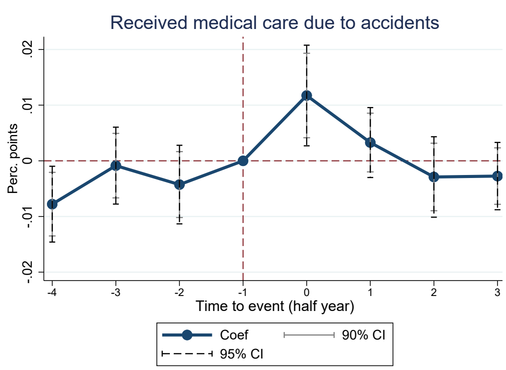
(b) Outpatient care (Spending)



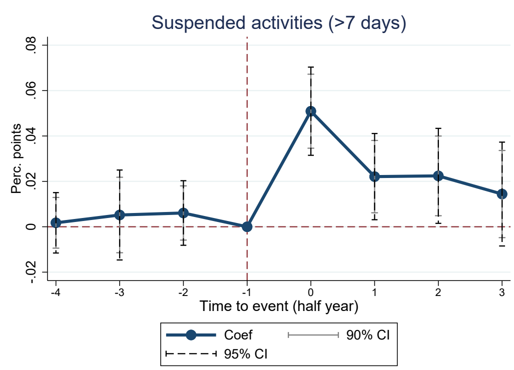
(c) Inpatient care (Prob.)



(d) Inpatient care (Spending)



(e) Accidents (Prob.)



(f) Suspension of main activities (Prob.)

Figure A2. : Direct effects of health shocks

Note: Each dot represents differences between treatment and control households in changes in outcomes relative to the period preceding the beginning of the shock ($\tau = -1$).The estimating sample includes 2 years before and after the shock divided in half-year bins. All specifications control for household time-variant demographic characteristics, as well as household and month fixed effects. 90% and 95% confidence intervals are computed using standard errors clustered at the household level.

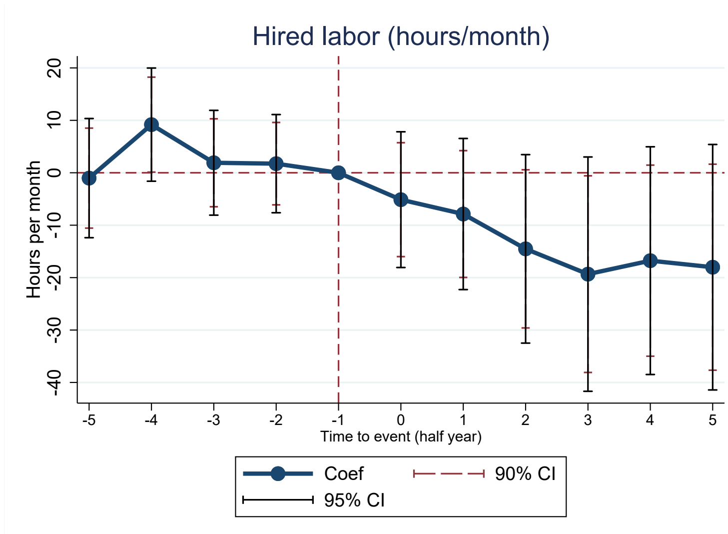
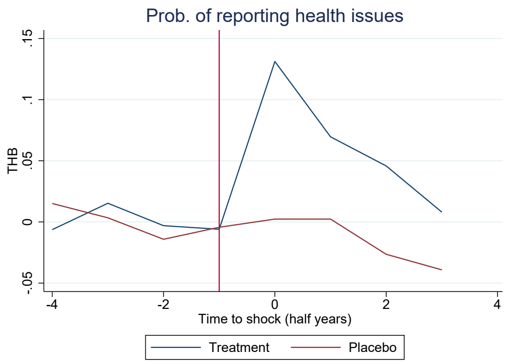
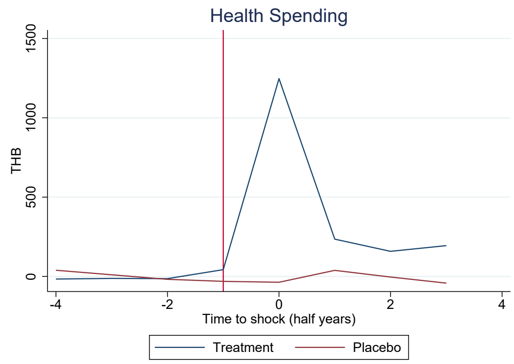


Figure A3. : Hired labor (extended analysis window)

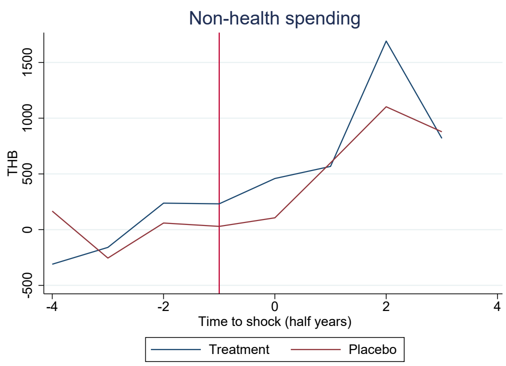
Note: Each dot represents differences between treatment and control households in changes in outcomes relative to the period preceding the beginning of the shock ($\tau = -1$).The estimating sample includes 5 half years before and after the shock divided in half-year bins. All specifications control for household time-variant demographic characteristics, as well as household and month fixed effects. 90% and 95% confidence intervals are computed using standard errors clustered at the household level.



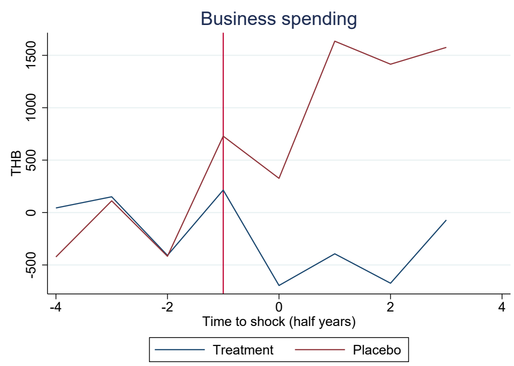
(a) Reported symptoms



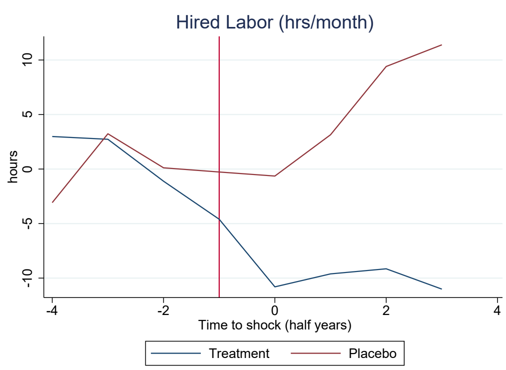
(b) Health Spending



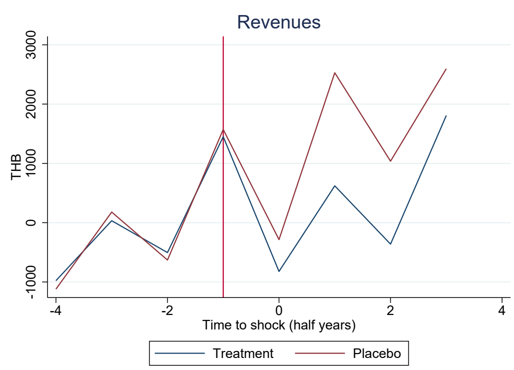
(c) Non-health Spending



(d) Business spending



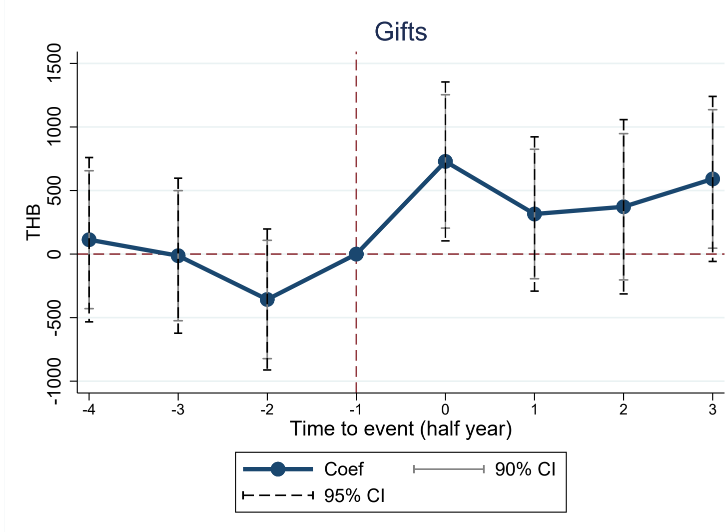
(e) Hired labor (hrs/month)



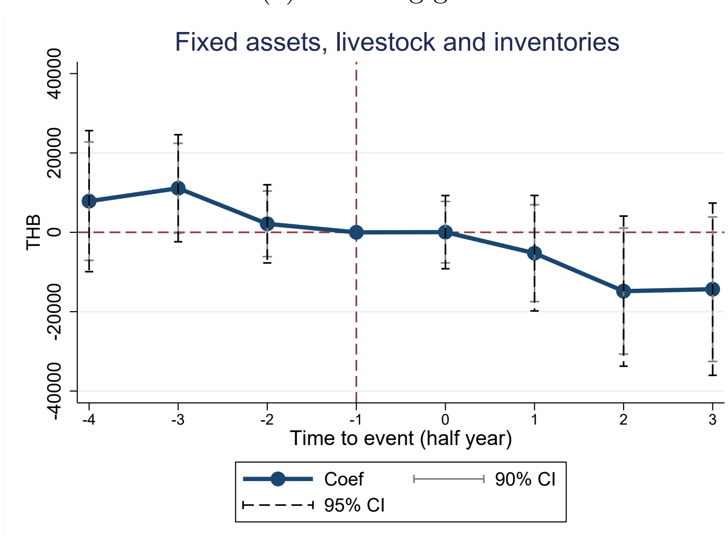
(f) Revenues

Figure A4. : Changes in household outcomes before and after the shock

Note: The Figure plots means of average monthly health spending, total consumption, business spending, hired labor, household labor and revenues for the four half-years preceding and following the shock. All variables are normalized with respect to the pre-shock mean. Period $\tau = -1$ denotes the half-year preceding the shock onset. Total consumption spending includes health spending. Revenues include income streams from all household enterprises and exclude earnings from providing wage labor to other households.



(a) Incoming gifts



(b) Fixed assets, livestock, and inventories

Figure A5. : Effects on incoming transfers and assets

Note: The figure reports coefficients from equation 1. Each dot represents differences between treatment and control households in changes in outcomes relative to the period preceding the beginning of the shock ($\tau = -1$).The estimating sample includes 2 years before and after the shock divided in half-year bins. All specifications control for household time-variant demographic characteristics, as well as household and month fixed effects. 90 and 95% confidence intervals are computed using standard errors clustered at the household level.

Table A2—: Direct effects on housework

Panel A: Using shocks occurring during the first half of the sample	(1) # of hh members	(2) # of days
Post X Treatment	-0.08	-3.06
	(0.06)	(1.79)
Baseline mean (DV)	2.93	81.31
Observations	23015.00	23015.00
Number of events	249.00	249.00
Adj. R-Squared	0.80	0.77

Panel B: Using all shocks	(1) # of hh members	(2) # of days
Post X Treatment	-0.09	-3.17
	(0.04)	(1.19)
Baseline mean (DV)	3.03	85.40
Observations	43925.00	43925.00
Number of events	476.00	476.00
Adj. R-Squared	0.78	0.76

Note: The Table reports estimates of β from equation (2) for different outcomes. Each column reports differences between treatment and control households in changes in outcomes before and after the shock. All regressions control for household demographic characteristics, household and month fixed effects. # of days is computed by adding across household members the number of days in which household member performed housework activities (e.g., cooking, cleaning, taking care of children, etc.) Standard errors are clustered at the household level.

Table A3—: Direct and indirect effects: Alternative shock definitions

Panel A: Direct Effects on household spending

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Max. Changes		Excluding small shocks		Health exp.>Avg. Food exp.		Health exp.>mean + SD	
	Health	Business	Health	Business	Health	Business	Health	Business
PostXTreat	463.6	-1644.4	671.5	-1947.8	829.5	-3669.1	720.0	-2945.4
	(79.38)	(772.7)	(115.2)	(901.3)	(143.9)	(1951.3)	(120.5)	(1535.1)
Baseline mean (DV)	140.3	6980.7	169.1	8129.8	218.0	10387.8	228.9	9983.4
Observations	22544	22544	20073	20073	7616	7616	8874	8874
Number of events	232	232	182	182	87	87	104	104
Adj. R-Squared	0.0606	0.803	0.0506	0.787	0.0500	0.819	0.0896	0.753

Panel B: Indirect Effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Max. Changes		Excluding small shocks		Health exp.>Avg. Food exp.		Health exp.>mean + SD	
	# Transactions	Income	# Transactions	Income	# Transactions	Income	# Transactions	Income
Post X Closeness	-0.294	-859.6	-0.344	-659.7	-0.400	-809.4	-0.196	-284.0
	(0.0787)	(395.5)	(0.0718)	(496.9)	(0.158)	(1065.4)	(0.103)	(936.4)
Baseline mean (DV)	1.414	10744.3	1.242	11980.1	1.118	14506.5	1.055	14635.8
Observations	431785	431785	318189	318189	72400	72400	107224	107224
Number of events	407	407	296	296	147	147	183	183
Adj. R-Squared	0.376	0.203	0.351	0.193	0.331	0.194	0.354	0.187

Note: The table reports direct and indirect effects using alternative definitions of shocks. Columns 1 and 2 show results corresponding to a definition of shocks based on the timing of symptoms that coincide with the largest monthly change in health spending. Columns 3 and 4 report results from our main specification but excluding shocks associated to a post-shock six-month cumulative health spending falls within the bottom 75% of the post-shock cumulative health spending distribution among control households. Columns 5 and 6 report results of a shock definition based on whether health spending is larger than the average food consumption for each household. Columns 7 and 8 report results of an alternative shock definition based on whether health spending exceeds its sample average by more than one standard deviation. Standard errors in parentheses.

Table A4—: Direct and indirect effects: Shocks based on suspended activities

Panel A: Direct effects on spending

	(1)	(2)	(3)	(4)	(5)	(6)
	Suspension of activities due to sickness					
	> 0 days		≥ 7 days		$>$ mean $+1$sd days	
	Health	Business	Health	Business	Health	Business
PostXTreat	42.47	-1163.8	385.9	-2261.0	432.1	-1887.6
	(107.7)	(844.8)	(121.1)	(1211.8)	(139.0)	(999.7)
Baseline mean (DV)	184.0	6076.7	180.2	5698.4	207.5	6058.4
Observations	13309	13309	10825	10825	9901	9901
Number of events	215	215	129	129	118	118
Adj. R-Squared	0.0443	0.743	0.0795	0.836	0.0769	0.847

Panel B: Indirect effects

	(1)	(2)	(3)	(4)	(5)	(6)
	Suspension of activities due to sickness					
	> 0 days		≥ 7 days		$>$ mean $+1$sd days	
	# Transactions	Income	# Transactions	Income	# Transactions	Income
Post X Closeness	-0.179	-745.3	-0.333	-518.2	-0.252	-313.7
	(0.0886)	(512.4)	(0.0846)	(681.7)	(0.0952)	(607.7)
Baseline mean (DV)	1.443	8910.8	1.233	10988.5	1.230	11293.2
Observations	224393	224393	131478	131478	110857	110857
Number of events	352	352	229	229	216	216
Adj. R-Squared	0.405	0.182	0.396	0.199	0.409	0.210

Note: The table reports direct and indirect effects using alternative definitions of shocks based on a household member suspending their primary activities for at least X days. Columns 1 and 2 report results for $X > 0$, columns 3 and 4 report results for $X \geq 7$ and columns 5 and 6 report results for $X \geq$ average disruption length in days (9 days). Standard errors in parentheses.

Table A5—: Direct effects: allowing for multiple, non overlapping shocks per household.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Shock(health>food spending)				Shock(activities)			
	Single shock		Multiple shocks		Single shock		Multiple shocks	
	Health	Business	Health	Business	Health	Business	Health	Business
Post X Treatment	829.5	-3669.1	975.3	-4377.7	385.9	-2261.0	289.6	-2060.3
	(143.9)	(1951.3)	(145.8)	(1745.4)	(121.1)	(1211.8)	(98.31)	(1011.6)
Baseline mean (DV)	218.0	10387.8	316.5	12423.8	180.2	5698.4	197.0	5974.4
Observations	7616	7616	12624	12624	10825	10825	11086	11086
Number of events	87	87	184	184	129	129	162	162
Adj. R-Squared	0.0500	0.819	0.123	0.778	0.0795	0.836	0.0716	0.745

Note: The table reports results corresponding to specifications that allow for a single shock occurrence per household (the first shock) and multiple, non-overlapping shocks per households. Columns 1 to 4 report results based on shocks related to episodes of high health spending (larger than the household average food consumption). Columns 5 to 8 report results related to the shock definition based on having suspended activities for at least 7 days. Standard errors in parentheses. Direct effects using a single shock are estimated using equation (2) while direct effects that allow for multiple shocks are estimated using equation (B1).

Table A6—: Direct effects: Robustness to alternative control groups.

	Randomly selected placebo group		**Using not-yet-treated as controls**		**Using not currently treated as controls**		**Callaway & San't anna (2021)**	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Health	Business	Health	Business	Health	Business	Health	Business
Treatment Effect	411.9	-1325.3	410.6	-1354.7	846.7	-741.9	401.1	-1312.5
	(61.9)	(515.2)	(57.91)	(384.8)	(124.8)	(314.5)	(68.26)	(683.8)
Baseline mean (DV)	158.2	7172.5	140.5	6601.3	162.1	7599.9	68.23	4847.0
Observations	43925	43925	135476	135476	21792	21792	N.A.	N.A.
Number of events	476	476	361	361	472	472	249	249
Adj. R-Squared	0.0443	0.758	0.0529	0.781	0.0354	0.791	N.A.	N.A.

Note: The table reports results corresponding to alternative specifications using different control groups and estimation strategies. Columns 1 and 2, report estimates using our main specification (equation (2)), but using control whose placebo shock is allocated at random. Columns 3 and 4, use a stacked differences-in-difference specification under which the control group for each household is made up of households in the same village that had not been treated yet, at the time of the onset of the shock based on equation (B2). Columns 5 and 6, present results using a standard two-way fixed effects specification withing 2 years of the onset of the shock in which the control group is made up of households in the sample who were not simultaneously treated based on equation (B3). Columns (7) and (8) report (Callaway and Sant'Anna, 2021)'s doubly-robust difference-in-difference estimates using households treated in the second half of the sample as controls for households treated earlier on. See Appendix Section B.B2 for details. Standard errors in parentheses.

Table A7—: Direct effects: Robustness to using an unbalanced panel

	(1) Health spending	(2) Business spending
Post X Treatment	426.8	-1429.8
	(64.11)	(658.9)
Baseline mean (DV)	153.7	6770.1
Observations	26861	26861
Number of events	296	296
Adj. R-Squared	0.0690	0.804

Note: The table reports estimates from our main specification (using shocks in the first half of the panel) using an unbalanced panel of 709 households (including 199 who either left the sample or entered the sample later on as replacements). Columns 1 and 2, report estimates using our main specification (equation (2)). See Appendix Section B.B2 for details. Standard errors in parentheses.

Table A8—: Spending co-movements with health status

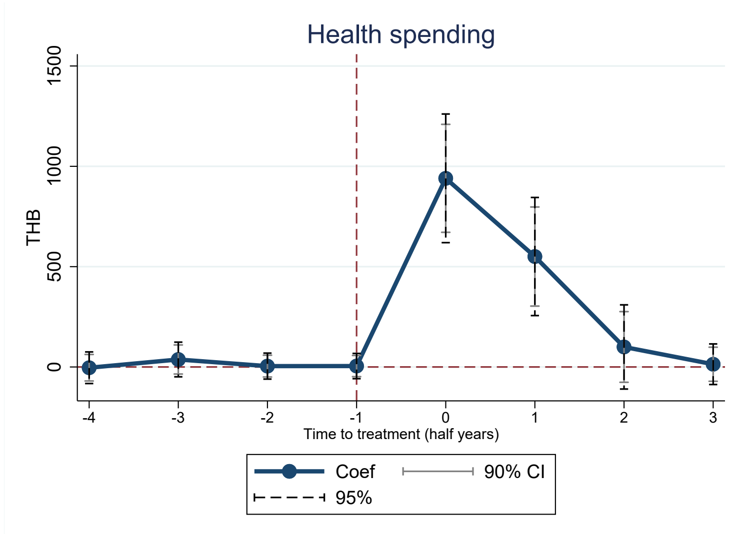
Panel A: Symptom - Health spending comovements

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Δ Experienced any symptom	438.3		445.7		448.3		
	(44.56)		(45.85)		(44.53)		
Δ Experienced uncommon symptoms		734.0		750.0		744.9	777.5
		(117.4)		(121.1)		(117.1)	(116.0)
DV mean (no symptoms)	-1.997	-0.574	-1.997	-0.574	-1.997	-0.574	-1.997
Observations	87110	87720	84380	84929	84380	84929	84929
Adj. R-Squared	0.00658	0.00496	0.00654	0.00496	0.00581	0.00433	0.0100

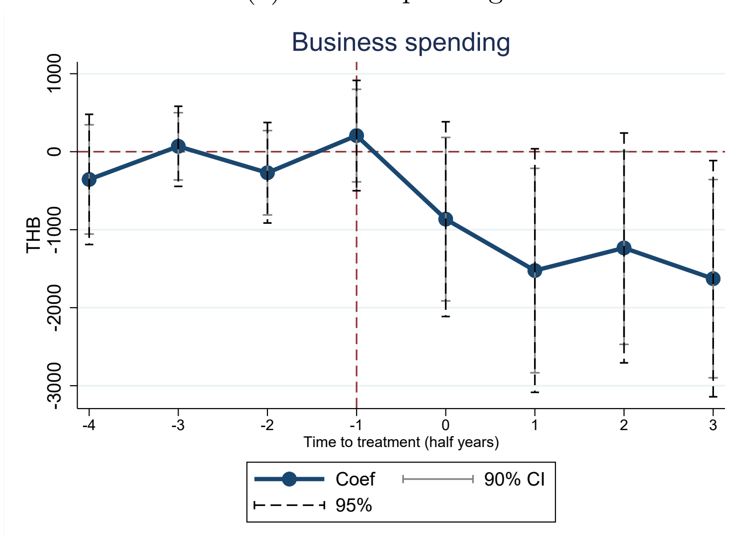
Panel B: Symptom - Business spending comovements

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Δ Experienced any symptom	-97.15		-101.4		-54.37		
	(95.73)		(97.60)		(95.44)		
Δ Experienced uncommon symptoms		-434.0		-441.7		-392.4	-418.4
		(200.8)		(205.8)		(205.1)	(205.3)
DV mean (no symptoms)	99.84	96.05	99.84	96.05	99.84	96.05	99.84
Observations	87110	87720	84380	84929	84380	84929	84929
Adj. R-Squared	0.0192	0.0200	0.0195	0.0204	0.0657	0.0657	0.0658
Demographic characteristics	No	No	Yes	Yes	Yes	Yes	Yes
Village X month FE	No	No	No	No	Yes	Yes	Yes
Controls for other symptoms	No	No	No	No	No	No	Yes

Note: The table reports co-movements between health status and spending. The estimates correspond to Gertler and Gruber (2002)'s specification: $\Delta Spending_{i,v,t} = \beta \Delta \text{Health Status}_{i,v,t} + \delta_{v,t} + \epsilon_{i,v,t}$. Where $\Delta X_{i,v,t}$ measures the changes in X between months t and $t-1$, $\delta_{v,t}$ denotes village-month fixed effects, and ϵ denotes an error term. Standard errors are reported in parenthesis and are clustered at the household level.



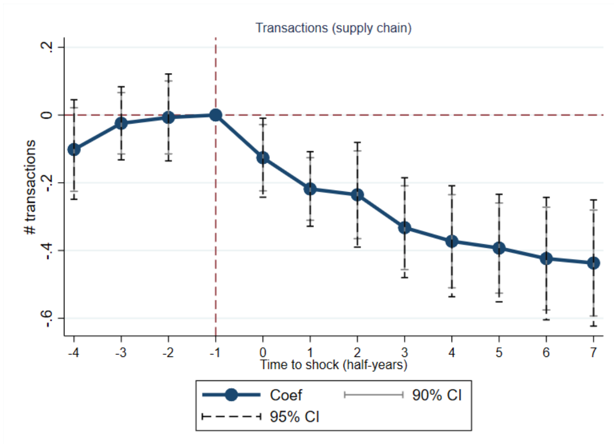
(a) Health Spending



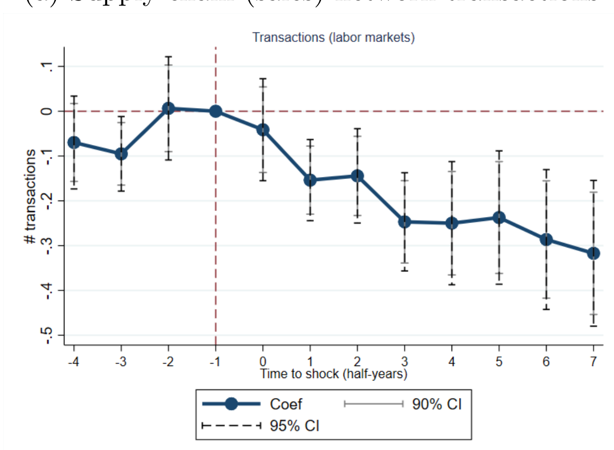
(b) Business Spending

Figure A6. : Event-study estimates using Callaway and Sant'Anna (2021)'s approach

Note: The figure depicts event-study estimates of the direct effects of shocks on health and business spending using Callaway and Sant'Anna (2021)'s doubly robust difference-in-difference estimator. The control group is made up of households who suffer a health shock during the second half of the panel. Estimations control for number of household members, average household age, and average household years of schooling. Confidence intervals are based on standard errors, clustered at the household level.



(a) Supply-chain (sales) network transactions



(b) Labor network transactions

Figure A7. : Persistent indirect effects of shocks on transactions.

Note: The Figure presents flexible difference-in-difference estimates of the indirect effects of idiosyncratic shocks on local businesses, following equation (3). All regressions include household fixed effects, event fixed effects, month fixed effects, village- and year-fixed effects, and household size, household average age and education, and the number of adult males and females in each household. Each dot captures differences in changes in outcomes with respect to the half-year preceding the shock (-1) between more- and less-exposed households. Standard errors are two-way clustered at the household (i) and shock level (j). We exclude shocks that occurred within 4 years of the end of the panel, to ensure a balanced panel throughout the analysis window.

Table A9—: Persistence in transaction networks, by network type

Panel A: Supply chain transactions

	(1)	(2)	(3)	(4)
Lag Prob. of link at $t-1$ (ρ)	0.469	0.460	0.378	0.378
	(0.015)	(0.014)	(0.011)	(0.011)
Kinship connection			0.100	0.100
			(0.006)	(0.006)
Demographic (log euclidean distance)				-0.019
				(0.119)
Net worth (log squared differences)				-0.037
				(0.027)
Observations	234,192	234,192	234,192	234,192
Adjusted R-squared	0.221	0.227	0.268	0.268
Mean DV	0.0508	0.0508	0.0508	0.0508

Panel B: Labor market transactions

	(1)	(2)	(3)	(4)
Lag Prob. of link at $t-1$ (ρ)	0.427	0.401	0.333	0.333
	(0.012)	(0.013)	(0.011)	(0.011)
Kinship connection			0.110	0.110
			(0.007)	(0.007)
Demographic (log euclidean distance)				-0.112
				(0.130)
Net worth (log squared differences)				-0.006
				(0.031)
Observations	234,192	234,192	234,192	234,192
Adjusted R-squared	0.189	0.207	0.241	0.241
Mean DV	0.0612	0.0612	0.0612	0.0612

Panel C: Gifts/loans transactions

	(1)	(2)	(3)	(4)
Lag Prob. of link at $t-1$ (ρ)	0.260	0.258	0.209	0.209
	(0.015)	(0.015)	(0.013)	(0.013)
Kinship connection			0.091	0.091
			(0.006)	(0.006)
Demographic (log euclidean distance)				0.138
				(0.071)
Net worth (log squared differences)				-0.035
				(0.017)
Observations	234,192	234,192	234,192	234,192
Adjusted R-squared	0.067	0.069	0.102	0.102
Village-Year FE	YES	YES	YES	YES
Household i FE	NO	NO	YES	YES
Household j FE	NO	NO	YES	YES
Mean DV	0.0122	0.0122	0.0122	0.0122

Note: The table presents regression coefficients following the specification in equation (B5). We model the probability that a pair of households $\{i, j\}$ trades in year t as a function of whether the couple traded in period $t-1$, by type of transaction. Columns 1 presents raw correlations, column 2 includes village-year fixed effects. Column 3 adds kinship first-degree connections as a control. Column 4 controls for differences in baseline demographic characteristics, differences in baseline wealth (e.g., assets net of liabilities), and household fixed effects. The coefficients of Demographic and Net-worth distance are rescaled by 100. All regressions are estimated over a sample of dyads of households included in the survey sample that responded in all 172 monthly waves of the survey. Standard errors are two-way clustered at the household i and j levels, and are presented in parentheses.

Table A10—: Indirect effects: Robustness to alternative specifications

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Village-month FE		Unconnected households		Unbalanced panel		Only shocks to small firms	
	Transactions	Income	Transactions	Income	Transactions	Income	Transactions	Income
Post X closeness	-0.18	-552	-0.18	-597	-0.24	-820	-0.24	-1,017
(village networks)	(0.04)	(418)	(0.05)	(404)	(0.06)	(412)	(0.08)	(474)
Observations	434,145	434,145	478,578	478,578	434,145	434,145	207,286	207,286
R-squared	0.44	0.26	0.38	0.21	0.38	0.21	0.40	0.24
Pre-period Mean	0.927	10729	0.903	11120	0.698	9477	0.991	9637
Number of events	410	410	449	449	410	410	200	200

Note: Columns 1 to 8 present estimates of β from equation (4). Each coefficient captures differences in changes in outcomes before and after the shock between more- and less-exposed households, through village networks. Standard errors are two-way clustered at the household (i) and event (j) level.

Table A11—: Indirect effects: Robustness to alternative estimation approaches

	(1)	(2)	(3)	(4)
	Triple difference		Fadlon & Nielsen approach	
	Transactions	Income	Transactions	Income
Post X closeness (village networks) X indirect exposure	-0.18	-1,427		
	(0.09)	(504)		
Post X Indirect exposure			-0.24	-1,184.00
			(0.11)	(782.99)
Observations	874,404	874,404	21,120	21,106
R-squared	0.36	0.20	0.39	0.21
Pre-period Mean	1.382	10834	1.507	7459
Number of events	462	462	481	480

Note: Columns 1 and 2 report triple difference estimates corresponding to equation (B4) of a triple interaction between closeness to the shocked household, a post-shock dummy, and an indicator of whether the shock is an actual shock or a placebo shock (see Appendix Section B.B3 for details). In this case, we winsorized the number of transactions corresponding to the supply-chain networks. Columns 3 and 4 report estimates corresponding to equation (2) using the subsample of households with a direct or indirect connection to the shocked household; the control group is households with a direct or indirect connection to a control household (see Appendix Section B.B3 for details). Standard errors are two-way clustered at the household (i) and event (j) level.

Table A12—: Indirect effects of health shocks on gift/transfers to other households (outflows)

	(1) # of gifts	(2) Gift ($ THB)	(3) Gift+Loans ($ THB)
Post X Closeness (village network)	-0.0136	-83.52	-110.9
	(0.00879)	(53.45)	(61.55)
Baseline mean (DV)	0.0306	928.6	1043.2
Observations	434145	434145	434145
Number of households	410	410	410
Adj. R-Squared	0.0587	0.300	0.231

Note: The Table presents estimates of the indirect effect of the idiosyncratic health shocks on gifts and transfers provided to other households in the village. The Table presents estimates of β from equation (4). $Closeness_{i,j}$ denotes inverse distance to the shocked household during the year preceding the shock to j. Each coefficient captures differences in changes in outcomes before and after the shock between more- and less-exposed households, through village networks. Each regression includes household (i), event j, month fixed effects (odd columns), and village-month (even columns), as well as demographic characteristics such as household size, average age, education and number of male and female adults. Standard errors are two-way clustered at the household (i) and event (j) level.

Table A13—: Direct and indirect effects by participation in risk-sharing networks.

Panel A: Direct effects	(1) Any health symptom	(2) Health spending	(3) Incoming gifts/loans	(4) Hired labor	(5) Business spending	(6) ICW Index (4-5)
Low participation (insurance networks) X Post X Treatment	0.0780	399.5	362.7	-13.42	-2191.9	-0.128
	(0.0245)	(72.27)	(361.7)	(7.202)	(935.6)	(0.0510)
High participation (insurance networks) X Post X Treatment	0.0907	420.2	824.7	-7.331	-565.5	-0.0509
	(0.0225)	(117.0)	(428.6)	(5.405)	(537.6)	(0.0339)
Difference	0.0127	20.74	462.0	6.092	1626.4	0.0774
S.E. Difference	0.0318	139.6	515.1	6.572	1081.2	0.0535
P-value(H0: Difference=0)	0.690	0.882	0.370	0.354	0.133	0.149
P-value(H0: Difference<0)	0.345	0.441	0.185	0.177	0.0666	0.0745
Q-value (H0: Difference<0)						0.0810
Baseline mean (DV)	0.352	160.1	2910.4	16.30	7611.8	0.0181
Observations	40745	40747	40747	40747	40747	40747
Adj.R-Squared	0.231	0.0445	0.0540	0.691	0.759	0.713

Panel B: Indirect effects	(1) Hired labor	(2) # transactions Input/Output	(3) All	(4) Income	(5) Spending	(6) ICW Index (3-5)
Low participation X Post X Density transactions (z-score)	-0.0512	-0.0556	-0.107	-381.8	-163.6	-0.0376
	(0.0280)	(0.0312)	(0.0452)	(240.4)	(55.72)	(0.0113)
High participation X Post X Density transactions (z-score)	-0.0272	-0.0301	-0.0573	-226.2	-129.9	-0.0232
	(0.0296)	(0.0334)	(0.0521)	(196.9)	(55.93)	(0.0110)
Difference	-0.0240	-0.0255	-0.0496	-155.6	-33.70	-0.0144
S.E. Difference	0.0252	0.0261	0.0379	167.2	62.21	0.0106
P-value(H0:Difference=0)	0.341	0.328	0.192	0.353	0.588	0.177
P-value(H0:Difference<0)	0.170	0.164	0.0960	0.176	0.294	0.0885
Q-value (H0:Difference<0)						0.0980
Baseline mean (DV)	0.460	0.989	1.449	10745.4	7447.7	-0.0322
Observations	448772	448772	448772	448772	448772	448772
Adj.R-Squared	0.219	0.421	0.359	0.210	0.634	0.451

Note: Panel A reports estimates of β_1 and β_2 from equation (B6) in section B.B6. Each column reports differences between treatment and control households in changes in outcomes before and after the shock. Panel B presents estimates of β_1 and β_2 from equation (B7). "High" and "Low" denote whether the directly shocked household exhibits gift-returns co-movements during the pre-period that are above (high) or below (low) the sample median. Standard errors are clustered at the household level.

Table A14—: Direct and indirect effects by differential exposure to formal insurance

Panel A: Direct effects

	(1) Any health symptom	(2) Health spending	(3) Incoming gifts/loans	(4) Hired labor	(5) Business spending	(6) ICW Index (4-5)
Less poor provinces X Post X Treatment	0.0582	559.9	997.4	-14.96	-1939.8	-0.128
	(0.0215)	(110.3)	(426.0)	(8.021)	(795.8)	(0.0515)
Poorer provinces X Post X Treatment	0.112	218.2	164.7	-3.490	-665.3	-0.0362
	(0.0256)	(44.00)	(319.6)	(4.476)	(678.8)	(0.0293)
Difference	0.0534	-341.7	-832.6	11.47	1274.4	0.0915
S.E. Difference	0.0334	119.0	527.6	9.108	1047.0	0.0591
P-value(H0: Difference=0)	0.110	0.00429	0.115	0.208	0.224	0.122
P-value(H0: Difference<0)	0.0551	0.00214	0.0576	0.104	0.112	0.0612
Q-value (H0: Difference<0)						0.0810
Baseline mean (DV)	0.345	158.2	2852.2	15.81	7172.5	0.00255
Observations	43923	43925	43925	43925	43925	43925
Adj.R-Squared	0.234	0.0449	0.0563	0.686	0.762	0.712

Panel B: Indirect effects

	(1) Hired labor	(2) # transactions Input/Output	(3) All	(4) Income	(5) Spending	(6) ICW Index (3-5)
Less poor provinces X Post X Density transactions (z-score)	-0.142	-0.0810	-0.223	-1270.9	-341.0	-0.0864
	(0.0352)	(0.0446)	(0.0629)	(416.8)	(124.2)	(0.0193)
Poorer provinces X Post X Density transactions (z-score)	-0.0490	-0.0439	-0.0929	-322.6	-127.0	-0.0315
	(0.0256)	(0.0278)	(0.0424)	(178.6)	(43.15)	(0.00897)
Difference	-0.0934	-0.0371	-0.131	-948.3	-214.0	-0.0549
S.E. Difference	0.0271	0.0376	0.0519	374.0	116.9	0.0179
P-value(H0:Difference=0)	0.000617	0.324	0.0122	0.0116	0.0678	0.00235
P-value(H0:Difference<0)	0.000309	0.162	0.00612	0.00580	0.0339	0.00118
Q-value (H0:Difference<0)						0.004
Baseline mean (DV)	0.460	0.989	1.449	10745.4	7447.7	-0.0322
Observations	477316	477316	477316	477316	477316	477316
Adj.R-Squared	0.231	0.445	0.382	0.224	0.637	0.469

Note: Panel A reports estimates of β_1 and β_2 from equation (B6) in section B.B6. Each column reports differences between treatment and control households in changes in outcomes before and after the shock. Panel B presents estimates of β_1 and β_2 from equation (B7) "Poor" and "Less poor" denote whether the shock occurred in a relatively poorer province or a less poor one, based on the average province-level income. Standard errors are clustered at the household level.

Table A15—: Response to shocks: coping mechanisms

Panel A: Direct effects

	(1) Gifts/Transfers	(2) Loans	(3) Fixed Assets livestock & inventories	(4) Cash in Hand	(5) Unpaid labor (Hrs/Month)
Post X Treatment	571.6	77.30	-13601.9	-11824.0	1.813
	(213.3)	(236.1)	(9610.5)	(22788.9)	(1.544)
Baseline mean (DV)	1936.6	266.7	223388.5	369550.7	6.152
Observations	23015	23015	23015	23015	23015
Number of events	249	249	249	249	249
Adj. R-Squared	0.166	0.00995	0.923	0.882	0.212

Panel B: Indirect effects

	Gifts/Transfers	Loans	Fixed Assets livestock & inventories	Cash in Hand	Unpaid labor (Hrs/Month)
Post$\times$Closeness (village network)	-101.8	-146.7	-14778.2	-11323.3	-1.186
	(151.6)	(120.7)	(6365.0)	(20939.4)	(0.948)
Baseline mean (DV)	2351.3	82.01	253322.2	434897.1	5.807
Observations	434145	434145	434145	434145	434145
Number of households	410	410	410	410	410
Adj. R-Squared	0.147	0.0372	0.879	0.813	0.292

Note: Panel A reports estimates of β from equation (2) for different outcomes. Each column reports differences between treatment and control households in changes in outcomes before and after the shock. Panel B presents estimates of β from equation (4). $Closeness_{i,j}$ denotes inverse distance to the shocked household during the year preceding the shock to j. Each coefficient captures differences in changes in outcomes before and after the shock between more- and less-exposed households through village networks. Each regression in Panel B includes household (i), event j, and month fixed effects, as well as demographic characteristics such as household size, average age, education and number of male and female adults. Incoming unpaid labor is in hours/month. All standard errors are two-way clustered at the household (i) and event (j) level.

Table A16—: Heterogeneous direct effects by age of shocked household member

	(1) Health spending	(2) Business spending	(3) Hired labor (Hrs/Month)	(4) Household Labor (Hrs/Month)
Non working age	577.8	-685.3	0.145	-11.63
	(118.5)	(708.6)	(1.423)	(10.04)
Working age (18-60)	370.0	-1711.9	-19.08	-6.668
	(56.16)	(769.7)	(10.39)	(8.483)
Difference	207.8	1026.6	19.23	-4.960
S.E. Difference	130.4	1044.2	10.70	13.26
P-value Difference	0.112	0.326	0.0731	0.709
Baseline mean (DV)	155.1	7253.2	17.21	143.6
Observations	37694	37694	37694	37694
Adj.R-Squared	0.0436	0.772	0.693	0.674

Note: The Table reports estimates of β from equation (2) for different outcomes. Each column reports differences between treatment and control households in changes in outcomes before and after the shock. All regressions include controls for demographic characteristics such as household size, average age, education and number of male and female adults. Working age: Indicator that takes the value of one if household i's shock was suffered by a household member whose age was between 18 and 60 years old - the Thai retirement age. Standard errors are clustered at the household (i) level.

Table A17—: Direct and indirect effects by internal-external labor complementarities.

Panel A: Direct effects

	(1) Any health symptom	(2) Health spending	(3) Incoming gifts/loans	(4) Hired labor	(5) Business spending	(6) ICW Index (4-5)
Low complementarities X Post X Treatment	0.0836	457.5	627.0	-2.407	-1020.9	-0.0452
	(0.0216)	(67.20)	(344.2)	(3.219)	(705.8)	(0.0273)
High complementarities X Post X Treatment	0.0882	369.5	491.6	-16.35	-1516.4	-0.137
	(0.0242)	(97.92)	(438.7)	(8.927)	(740.3)	(0.0454)
Difference	0.00457	-87.97	-135.5	-13.94	-495.5	-0.0922
S.E. Difference	0.0317	114.2	552.0	9.591	997.2	0.0528
P-value(H0: Difference=0)	0.885	0.441	0.806	0.147	0.619	0.0812
P-value(H0: Difference¡0)	0.443	0.221	0.403	0.0734	0.310	0.0406
Q-value (H0: Difference<0)						0.0810
Baseline mean (DV)	0.345	158.2	2852.2	15.81	7172.5	0.0212
Observations	43923	43925	43925	43925	43925	43925
Adj.R-Squared	0.227	0.0455	0.0520	0.686	0.759	0.727

Panel B: Indirect effects

	(1) Hired labor	(2) # transactions Input/Output	(3) All	(4) Income	(5) Spending	(6) ICW Index (3-5)
Low complementarities X Post X Density transactions (z-score)	-0.0502	-0.0384	-0.0886	-216.9	-139.6	-0.0297
	(0.0285)	(0.0311)	(0.0465)	(180.1)	(55.45)	(0.00987)
High complementarities X Post X Density transactions (z-score)	-0.0290	-0.0367	-0.0657	-381.7	-152.7	-0.0289
	(0.0280)	(0.0297)	(0.0439)	(217.4)	(50.61)	(0.0103)
Difference	-0.0212	-0.00174	-0.0229	164.7	13.10	-0.000837
S.E. Difference	0.0243	0.0257	0.0355	151.3	59.82	0.00995
P-value(H0:Difference=0)	0.385	0.946	0.519	0.277	0.827	0.933
P-value(H0:Difference¡=0)	0.192	0.473	0.260	0.138	0.413	0.466
Q-value (H0:Difference<0)						0.185
Baseline mean (DV)	0.460	0.989	1.449	10745.4	7447.7	-0.0322
Observations	477316	477316	477316	477316	477316	477316
Adj.R-Squared	0.219	0.425	0.362	0.211	0.635	0.453

Note: Panel A reports estimates of β_1 and β_2 from equation (B6) in section B.B6. Each column reports differences between treatment and control households in changes in outcomes before and after the shock. Panel B presents estimates of β_1 and β_2 from equation (B7). "High" and "Low" denote whether the directly shocked household exhibits hired-internal labor co-movements during the pre-period that are above (high) or below (low) the sample median. Standard errors are clustered at the household level.

Identifying shocks and their effects

B1. Identifying shocks

Here we provide additional details related to identifying idiosyncratic health shocks.

We identify shocks as the month with the highest level of reported health spending throughout the panel. We compute monthly health spending as the sum of spending on medicines, transportation to medical facilities, and spending on either inpatient or outpatient care.

In some cases, our approach identified more than such episode per household–i.e., two levels of spending of the same magnitude. In such cases, we focus on the first episode to avoid sample selection issues due to repeated shocks, and to ensure that the responses to the shocks are not driven by responses to preceding large shocks.

To identify and exclude events related to pregnancy and childbirth, we exclude the 32 events that coincide with the inclusion of a new child in the household roster within 12 months of the sudden increase in health spending.

To account for potential anticipation effects, we define the beginning of each event by subtracting the number of months preceding the episode of high health spending during which household members reported health symptoms from the month corresponding to the episode. For example, if the episode of high health spending was recorded in month 100 and the symptoms started being reported three months before, the beginning of the event is month 97. For 406 events, we can identify the health symptoms reported at the time of the events, and when these symptoms were first reported. In the case of the 70 households for which we could not identify the beginning of the symptoms,[58] we coded the beginning of the event as three months before the episode of high total health spending (the median period between the observed increases in health spending and the first time symptoms were reported).

Figure B6 plots means of health spending and the self-reported probability that at least one household member experienced health symptoms over time, for the treatment and control groups. It shows that the control group does not experience any change in health spending or health status around the placebo shock, as expected. In the case of the treatment group, the sharp increase in health spending coincides with sharp increases in spending on inpatient and outpatient care. The magnitude of the increase in health spending suggests that health shocks were quite severe. The figure also demonstrates that, prior to the shock, the treatment and control groups are on similar trajectories in terms of spending, symptoms, and probability of receiving care, supporting the parallel trends assumption.

[58]There were 12 households for which symptoms were repeatedly reported for two years or more, and 68 households who lack information related to symptoms.

Characteristics of shocks

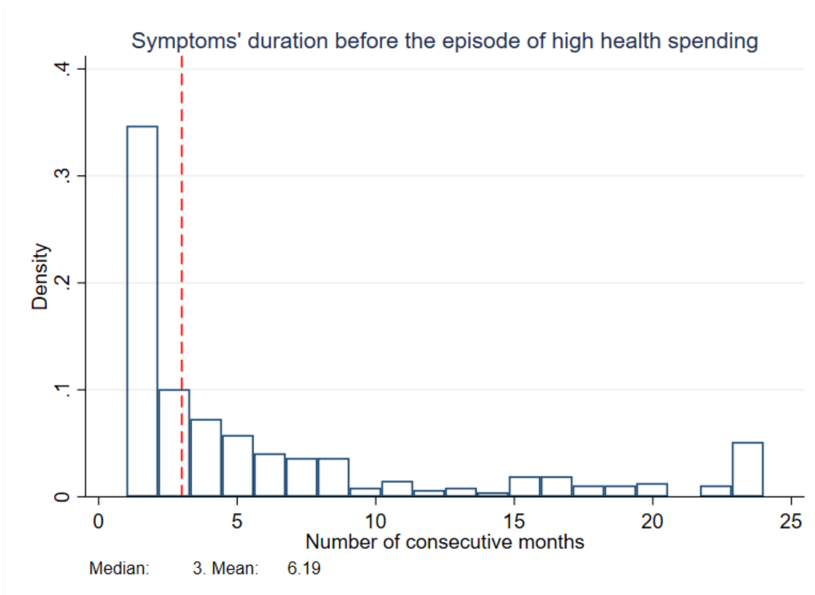


Figure B1. : Distribution of symptom duration before the episodes of high health spending

Note: The figure plots the distribution of the number of consecutive months prior to the episodes of high health spending for which at least one household member reported health symptoms. The dashed vertical line denotes the median number of consecutive months reporting symptoms before the episode of high health spending. The last bar to the right captures the density of symptoms that were experienced 24 months or more before the episode of high health spending.

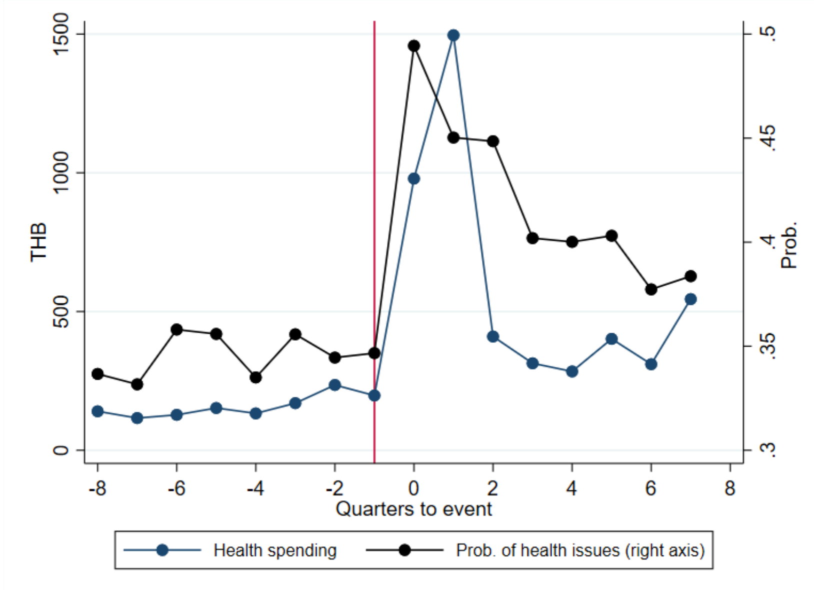


Figure B2. : Health status and spending before and after health shocks.

Note: The figure reports averages of health and total spending for periods before and after the health shocks (left axis). The right axis reports the probability that at least one household member reports health symptoms in a given month, before and after the shocks. The horizontal axis represents normalized time with respect to the event realization (time 0). Each time bin corresponds to quarters. All averages are computed over a balanced panel.

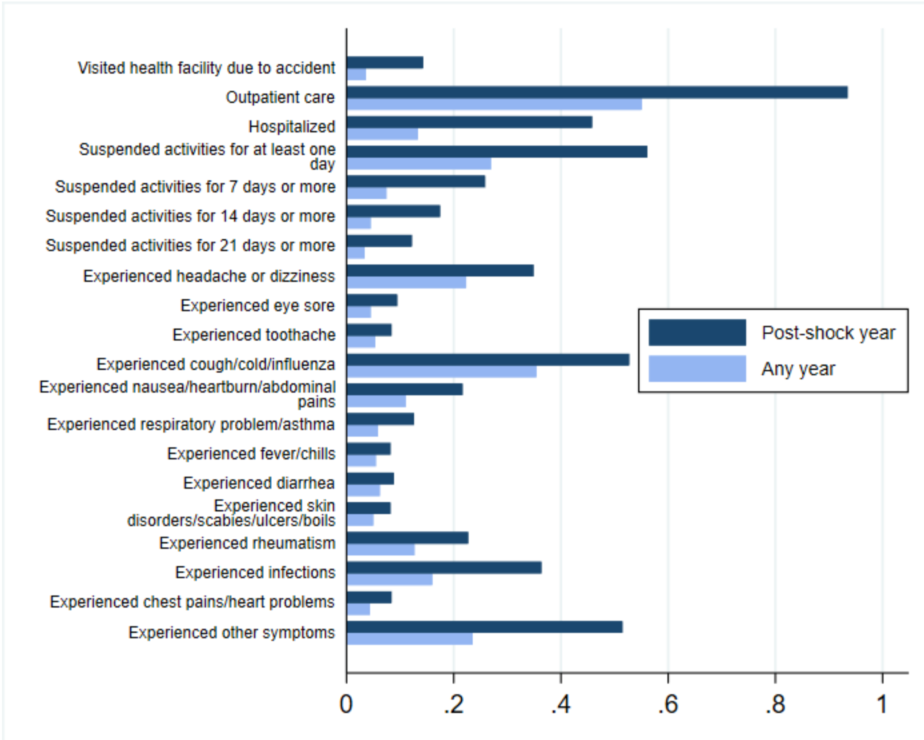
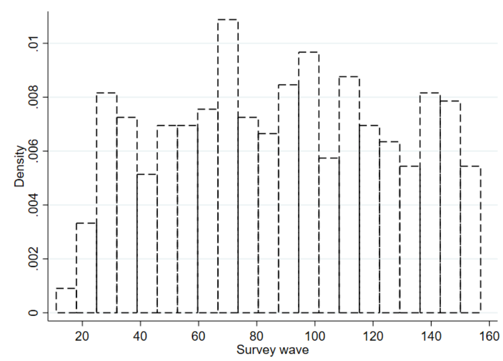
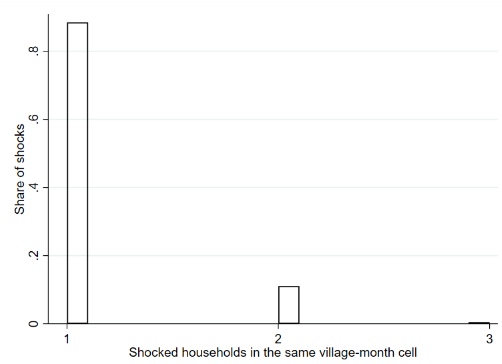


Figure B3. : Incidence of health conditions during shock and non-shock periods.

Note: The figure reports the proportion of symptoms experienced during the year following the episodes of high-health spending and during any year. The sample includes all households that ever experience a health shock according to our main shock definition.



(a) Distribution of initial event periods



(b) Distribution of shocks by number of simultaneously affected households in the same village

Figure B4. : Distribution of events by initial event period and number of affected households

Note: The top panel plots a histogram capturing the distribution of survey months associated the beginning of the health shocks across the full sample period.The bottom panel plots the distribution of events by the number of households simultaneously affected in the same village.

Table B1—: Timing of health shocks and village and household characteristics

	(1) Shock occurrence at t+1	(2) P-value (Granger causality - 12 lags)
Income	0.000487	0.276
	(0.000391)	
Business Revenues	0.000951	0.883
	(0.000687)	
Business Spending	-0.000798	0.715
	(0.000663)	
Non health consumption	0.0000399	0.760
	(0.000197)	
Health spending	-0.000404	0.587
	(0.000224)	
Borrowing	0.000452	0.830
	(0.000475)	
Lending	-0.000422	0.182
	(0.000349)	
Incoming gifts	-0.000366	0.511
	(0.000445)	
Outgoing gifts	0.00000542	0.948
	(0.000281)	
Livestock	-0.000317	0.0887
	(0.000490)	
Cash in hand	-0.000557	0.375
	(0.000448)	
Fixed assets	0.000142	0.0873
	(0.000412)	
Land	0.00222	0.200
	(0.00165)	
Observations	87210	77755
Adj. R-Squared	-0.00411	
P-value (Joint significance)	0.281	
P-value (Hausman Test Village X month fixed effects)	0.203	

Note: Column 1 reports OLS coefficients from a regression of the probability that a shock occurs on $t+1$ on lagged household and business characteristics, controlling for household and village fixed effects. The bottom panel reports p-values of an F-stest of joint significance of all regressors, and p-values for the joint significance of the village fixed effects computed using a Hausman specification test. All regressors are standardized with respect to the sample mean and standard deviation. Column 2 reports p-values corresponding to a test of joint significance of the 12 lags of each household and business outcomes. These p-values are computed based on the coefficients of a regression of the probability of experiencing a shock at $t+1$ on the first 12 lags of household and business characteristics, controlling for household and business fixed effects. Standard errors are clustered at the household level to control for serial correlation.

Table B2—: Time use in pre-shock periods: Count of days dedicated to different activities

	Number of days per month	More than 15 days
	Average	Share
Cultivation	3.43	0.08
Livestock	6.51	0.21
Fish/Shrimp	1.13	0.02
Off-farm businesses	1.83	0.07
Housework	22.88	0.78
School or training	2.05	0.05
Village organizations/positions	0.15	0
Funerals/weddings	0.56	0
Labor exchange outside home	0.02	0
Free labor outside home	0.38	0.01
Paid labor outside home	3.92	0.12
Looking for a job	0.03	0
Sick	0.1	0

Note: The table reports participation in several activities for a subsample of individuals that reported being sick during the periods in which their household experienced the shock. Column 1 reports the number of days in which household members reported participating in each activity, during the month preceding the shock. Column 2 reports the share of affected individuals that dedicated more than 15 days to each activity, during the month preceding the shock. The sample is restricted to the month-preceding the shock and corresponds only to household members that reported being sick during the shock. These activities are not mutually exclusive, so the total days per month across categories add up to more than 30.

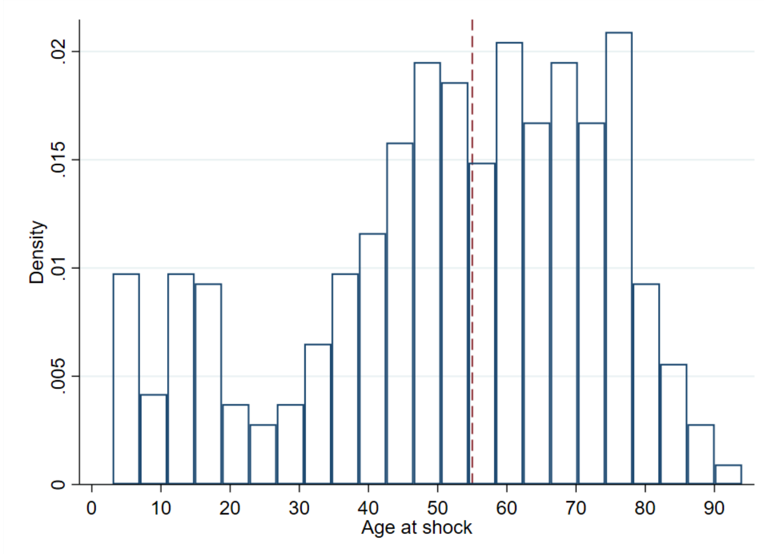


Figure B5. : Age at shock

Note: The figure plots a histogram capturing the distribution of age of family members reporting health symptoms during the month associated to the beginning of each shock. The figure includes observations corresponding to the 405 shocks for which we found households reporting non-pregnancy/non-birth health symptoms. The dashed vertical line denotes the median age of household members reporting symptoms during the month preceding the beginning of each shock.

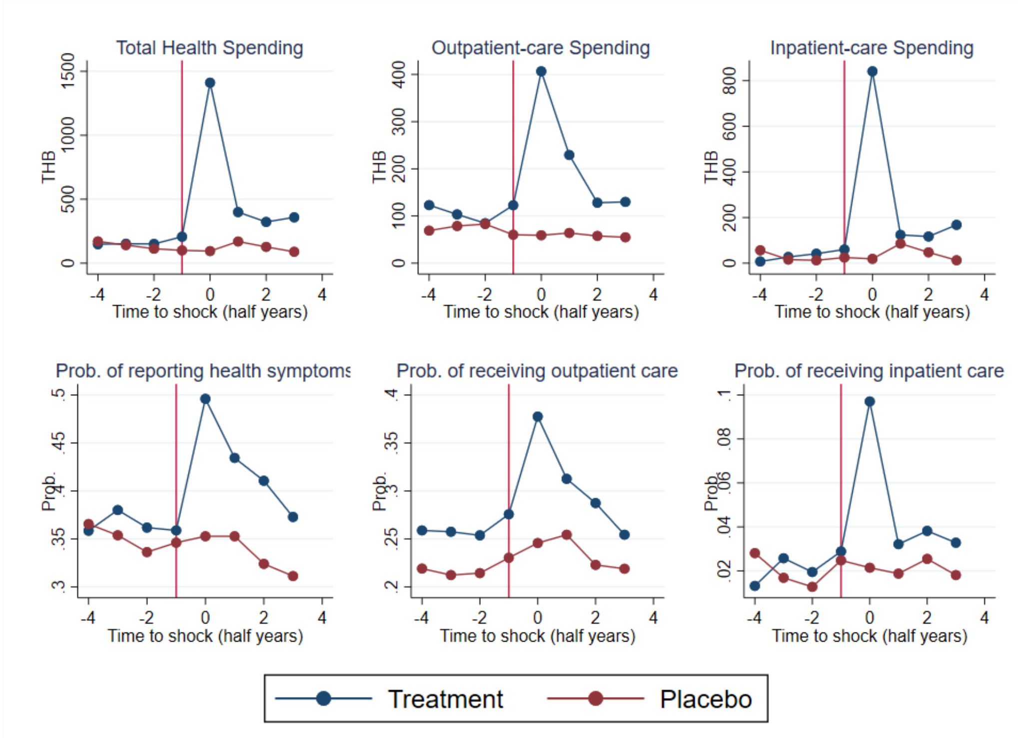


Figure B6. : Health status and spending in the treatment and control samples

Note: The figure reports averages of health and total spending for periods before and after the health shocks (left axis). The right axis reports probabilities of reporting health symptoms before and after the shocks. The horizontal axis represents normalized time with respect to the event realization (time 0). Each time bin corresponds to half years.

B2. Treatment and control groups for direct effects

We implement our approach for estimating the direct effects of idiosyncratic shocks in three steps. First, we split households into two age groups—i.e., below and above the median household age at baseline (1997).[59] By comparing households in the same village and age group, we isolate contemporaneous village-specific shocks and potential differences in the trajectories of business and household-finance outcomes that could vary along the life cycle. Given our sample size, we choose two age group bins to ensure that we have multiple observations per bin in each village.

Second, for each age group within each village, we split the panel in two equal-length sub-samples $\{\theta^1, \theta^2\}$ by taking the midpoint between the months associated to the first and last shocks in each age group-village bin (Δ), such that those households suffering a shock between periods $\underline{t}$ and $t_{med} = \underline{t} + \Delta$ belong to the treatment group (θ^1), and those experiencing the shock between periods t_{med} and $\bar{t}$ belong to the control group (θ^2).[60] By construction, there is no overlap between the two groups.

Third, we assign a placebo shock to each household in the control group Δ periods before they experienced their actual shock. Thus, if a household in the control group experiences the actual shock in t'', its placebo shock is assigned to period $t'' - \Delta$. Because the timing of the shocks is evenly distributed over time (see Appendix Figure B4), the placebo shocks occur within the domain of the actual shocks. As 249 out of 476 shocked households experienced a shock in the earlier part of the panel, this process yields 249 households in the treatment group and 227 in the control group.

By using households that experience a shock Δ periods (approximately 5 years) in the future, this process ensures that none of the households in the control group experienced a shock themselves during the analysis period. This is potentially important as households that experience illness are more likely to experience other illness episodes in the future (Hendren, Shenoy and Townsend, 2018). This approach reduces the threat of biases arising from contemporaneous shocks affecting the control group, but comes at the cost of precision since we do not exploit the occurrence of the actual shocks in the second part of the sample. To increase precision, we also report estimates exploiting the variation associated with shocks to households in the second half of the sample for robustness. In this case, the comparison group consists of households that suffered the shock earlier on and their corresponding placebo shock occurs in period $t' + \Delta$; Δ periods after their actual shock. Including this variation does not materially alter the point estimates, but

[59] One alternative way of assigning households into cohorts is by focusing on the age of the household head. However, that approach ignores the age structure of the household as in several cases several families are part of the household.

[60] We define Δ as $\Delta = \frac{\bar{t} - \underline{t}}{2}$ for each age-group-village bin. On average, each sub-sample covers 56 months. We exclude shocks occurring during the first and last 24 survey waves to ensure that we observe pre and post outcomes for at least two years for all households—i.e., $\underline{t} >= 24$ and $\bar{t} <= 148$.

it increases statistical power.

Another advantage of constructing the control group in this way is that the treatment-control comparisons are within-village. This ensures that village aggregate shocks are differenced out. If the timing of health shocks was endogenously chosen based on village-level conditions (for instance, timing a surgery for the post-harvest period when labor demand is low), then any spurious "effects" driven by endogenous timing based on village-level conditions will be common to the treatment and control, and will not affect the estimator.

Direct effects: Robustness

Robustness to using shocks occurring in the second half of the panel. Our main analysis uses households who experienced the shock in later periods as a comparison group for households that experienced the shock earlier on. To increase power, we also report results using households who experienced the shock in the earlier periods as a comparison group for households who suffered the shock in later periods. Panel B of Table 1 replicates the results from Section II and shows results that are quantitatively similar, but estimated with higher precision since we now use 476 shocks as opposed to only 249, as in Table 1. By adding more shocks we are able to detect significant declines in household labor, and revenues.

Robustness to defining shocks based on changes in spending. One concern is that because we identify events based on levels of spending, as opposed to sudden changes, the timing of the shock may be endogenous. We argue that this unlikely in our case: while the event is identified based on the maximum level of spending, the date of the event is a function of changes in health status. Columns 1 and 2 of Panel A in Appendix Table A3 show that our results are robust to identifying events using the highest monthly change in health spending, as opposed to the highest level of health spending. The coefficients are remarkably similar to those of our main specification suggesting that episodes with the maximum levels of spending are strongly correlated with the largest change in health spending.

Robustness to defining shocks based on household-specific thresholds. One concern is that our empirical approach could be simply picking up events associated to small, innocuous levels of spending. To show that this is unlikely, we use a specification that selects events during which the maximum level of health spending is relatively larger than a household's average level of food consumption. This approach is substantially more restrictive as it selects a total of 147 events (87 in the first half of the sample). Columns 5 and 6 of Panel A in in Appendix Table A3 show that the effects on health and business spending are larger, but imprecise.

Robustness to defining shocks based on global thresholds. Another concern is that despite the shocks being large , relative to a household's budget, they may not be large in general. We selected events based on whether health spending exceeded the sample average by at least 1 standard deviation. We chose the first shock in the case this approach identified multiple events for the same household.

Columns 7 and 8 show that the results are qualitatively similar to those in our main specification, but less precisely estimated due to the fact that this approach selects less events.

Robustness to defining shocks based on disruptions to main activities. Another concern is that households may select when to spend on health based and thus, the timing of the shocks that we analyze can be correlated with other determinants of business spending. To rule out these concerns, we report results of an alternative approach to identifying shocks based on the first time in the sample when a household member has to suspend activities for at least one day, for seven days, and for more days than 9.5 days—the average number of days of suspended activities in the sample. Appendix Table A4 reports results related to this alternative definitions. Consistent with Gertler and Gruber (2002), it shows that only severe shocks (those implying more days of suspended activities) lead to effects on spending.

Robustness to allowing a same household to experience multiple, non-overlapping shocks. One concern is that our empirical specification only analyzes one shock per household (the largest throughout the panel), which may limit power. An alternative approach is to allow for multiple shocks per household, under the idea that some households may be exposed multiple times throughout the panel. However, allowing for multiple shocks per household comes at the cost of imposing two additional identification assumptions. First, that shocks experienced earlier on do not affect the probability of experiencing another health shock in the future. Second, that the effects of earlier shocks do not have long-lasting effects on the trajectories of outcomes that can lead to violations of the parallel trends assumption.

We estimate the following equation:

(B1)
$$y_{i(k),t} = \beta Post_{i(k),t} \times Treatment_{i(k)} + \theta Post_{i(k),t} + X_{i(k),t}\kappa + \alpha_i + \delta_t + \epsilon_{i(k),t}$$

where $Post_{i(k),t}$ is an indicator that takes the value of 1 in periods following the k-th shock to household i, and 0 otherwise. In this case $k \geq 1$ for all treated households.[61] We exclude shocks that occur less than 24 months after the preceding shock, to minimize violations to the parallel trends assumption. As in our main specification we focus on a two-year time window before and after each shock. In Appendix Table A5 we report robustness to include multiple, non overlapping shocks per household based on two definitions of shocks: changes in health status that precede levels of health spending that are larger than the household-specific food consumption (columns 3 and 4) and shocks based on whether a household member had to suspend activities for more than 7 days due to illness (columns 7 and 8). The results in both cases are very similar to those from our specifications

[61] Households who do not experience any shock according to a given threshold are dropped from this specification.

that only allow for one shock (the first) per household. As expected, they are estimated with more precision.

Alternative definitions of comparison groups. We report three robustness checks that rely on different comparison groups for our analysis. Our main specification assigns placebo shocks Δ periods away from the actual shocks, within village-age groups bins. An alternative approach would be to randomly allocate the placebo event within each village bin. The main difference between these approaches is that our main specification ensures that the control group does not suffer a shock during the two-year comparison window. In contrast, the random assignment of the placebo event could coincide with other shocks. Columns 1 and 2 in Appendix Table A6 report results using the random placebo assignment, based on a uniform distribution between the months of the first and last shock in each village. The results are qualitatively similar to those from our main specifications.

In our main specification, the control group is made of households who will suffer a shock Δ periods into the future. This approach excludes some not-yet-treated households who will suffer a shock in less than Δ periods into the future. One advantage of this approach is that the control group size does not systematically vary across shocks occurring earlier vs. later in the sample. An alternative approach would be to use *all* the not-yet-treated households in the village at the time of each shock as controls. This approach would increase the size of the control group and statistical power, but the size of the control group will shrink in the case of shocks occurring later in the sample.

Specifically, we follow Baker, Larcker and Wang (2022) and construct a dataset at the event level h. Each dataset includes observations of the shocked household and not-yet-treated households in the same village and age group of the shocked household. Note that this is an alternative estimator that, as our main specification, also avoids the issues with traditional two-way fixed effects models (see Section 4.2. in Baker, Larcker and Wang (2022)). We then estimate:

$$y_{i,t,h} = \beta Post_{t,h} \times Treatment_{i,h} + \theta Post_{i,t,h} + X_{i,t,h}\kappa + \alpha_i + \delta_t + \epsilon_{i,t,h} \tag{B2}$$

Columns 3 and 4 in Appendix Table A6 report results from a stacked difference-in-difference specification using not-yet-treated households in the shocked household's village and age group as controls. Reassuringly, the results are similar to those from our main specification.

We also report results from the following two-way fixed effects panel specification:

$$y_{i,t} = \beta Post_{i,t} + X_{i,t}\kappa + \alpha_i + \delta_t + \epsilon_{i,t} \tag{B3}$$

Here, we regress the outcome of interest on a Post dummy over a sample of shocked households including 2 years before and after the shock. This specification uses households that are not simultaneously shocked as controls. Reassuringly, the results are very similar to those from our main specification (see columns 5 and 6 of Appendix Table A6).

Additionally, we report robustness to estimating treatment effects using Callaway and Sant'Anna (2021)'s difference-in-difference estimator. This specification utilizes households that were shocked in the first half of the sample as a treatment group and uses households treated in the second half of the sample as controls. By excluding already treated units from the control group, this approach allays concerns related to difference-in-difference designs with staggered entry into treatment (Goodman-Bacon, 2018).

Finally, in Appendix Table A7 we leverage shocks to a larger sample of households (including the 510 continuously-observed households that are always in the sample and adding 199 who either left the sample or entered the sample later on as replacements). Once again, results are similar to the main specification.

Co-movements of health status and spending. One concern is that the relationship between health spending and the timing of the shock is only a feature of the identification of the shocks. In Panel A of Appendix Table A8 we report the relationship between changes in health status and changes health spending using data from all the households in the sample and all time periods, controlling for village-month fixed effects to ensure that we are capturing within household's co-movements net of the influence of village-level shocks as in Gertler and Gruber (2002). Changes in health spending co move with changes in health status, suggesting that this relationship holds beyond the events that we analyze in our main specification. Interestingly, when we use changes in health status associated to uncommon health symptoms—those that are more prominent around the shocks used for our main specification—the changes in health spending seem substantially larger. Moreover, in Panel B, we show that these uncommon health conditions are the ones that also predict declines in business spending as we find in our main specification.

B3. Indirect effects: Alternative empirical approaches

Triple difference estimates of indirect effects

To allay any remaining concerns regarding the identifying assumption underlying equation 3 and 4, we present a second research design that uses the placebo shocks used as controls to identify the direct effects as controls to identify the indirect effects. We estimate the following equation:

$$\begin{aligned}y_{i,t,j} = {} & \beta_1 Post_{t,j} \times Closeness_{i,j} \times Treatment_i + \beta_2 Post_{t,j} \times Treatment_i + \\ & \beta_3 Post_{t,j} \times Closeness_{i,j} + \gamma_1 Closeness_{i,j} \times Treatment_i + \gamma_2 Closeness_{i,j} + \\ & \gamma_3 Treatment_i + \theta Post_{t,j} + \mathbf{X}_{i,t,j}\kappa + \alpha_i + \omega_j + \delta_t + \theta_\tau + \delta_t \times Degree_{i,j} + \epsilon_{i,t,j}\end{aligned} \tag{B4}$$

where we compare a household i with given closeness to a treated household j versus a household i' who is equally close to a control household j'. In this case, j' is a household who directly experience a shock, but later in the future. The parameter of interest, β_1, compares differences in outcomes before and after the shock, between a household close to a shocked household in the treatment group, versus the analogous change for a household close to a household in the control group.

The advantage of this specification is that it does not compare households who are closer vs. more distant to a given household but instead compares households who are equally close to a shocked household, with the difference that one is close to a household that suffers the shock earlier on ($Treatment_i$) and the other is close to a household that suffers a contemporaneous placebo shock, but will suffer an actual shock later in the future. The disadvantage, however, is that household's connected to households experiencing a placebo shock (i.e., the control group) may have already been exposed to an indirect shock or might as well be connected to households suffering an actual shock. These two issues may compromise the validity of the parallel trends assumption. In the next section, we discuss a more data-demanding identification strategy that circumvents these concerns. That said, Columns 7 and 8 in Appendix Table A10 report estimates that are very similar to those of our main specification.

Measuring indirect effects à la Fadlon and Nielsen (2019)

A potential concern with the first approach to measuring indirect effects is that we are comparing households who are closer vs. farther from the shocked household and, *a priori*, those with different network positions may be different. (Though recall that we are flexibly controlling for $Degree_{i,j} \times month$ fixed effects and that both groups exhibit parallel pre-trends.) An alternative approach, in the spirit of the design used to study direct effects, is to compare households that are close to a household (j) that experienced a shock in period t to households that were also close to a control household (j'): one whose shock occurs later in the data. In this design, both treatment and comparison households are similarly close to a shocked household but treated households are exposed to the shock during the analysis window while control households experience a placebo shock.

In the spirit of the design used to study direct effects, we compare households that are close to a household (j) that experienced a shock in period t to households that were also close to a control household (j'): one whose shock occurs later in the data. In this design, both treatment and comparison households are similarly close to a shocked household but treated households are exposed to the shock during the analysis window while control households experience a placebo shock.

The intuition of this approach is similar that of our approach in Section II.A. However, its implementation is more challenging. Because households share links with many households, some households may be indirectly exposed to shocks more than once. For this reason, we focus on the first shock to which a household is indirectly exposed throughout the panel (either directly or indirectly).[62]

With these modifications to the sample and to the definition of treatment (indirect exposure vs. direct exposure), we use the same specification as in equation (2) to estimate the effects of being indirectly exposed to a health shock. In this case, however, the sample only includes observations of households that were connected to a shocked household. The coefficient of interest, β, compares differences in outcomes before and after their first indirect exposure to a shock (actual or placebo), between households in the treatment group and the comparison group.

The advantage of this specification is that it does not compare households who are closer vs. more distant to a given household but instead compares households who are equally close to a shocked household, with the difference that one is close to a household that suffers the shock earlier on ($Treatment_i$) and the other is close to a household that suffers a contemporaneous placebo shock, but will suffer at a different time.

The results appear in columns 3 and 4 of Appendix Table A11. The effect on total transactions (column 3) of -0.24 is quite similar to the -0.30 from table 2. The effects on income THB -1184 are also quite close to the estimates from Table 2 (THB -820). The similarity of the two sets of results, using different designs for identifying indirect effects, serves as a sort of over-identification test, suggesting that both identifying assumptions are valid.

[62]We focus on households either directly or indirectly connected to shocked households through the pre-period network for two reasons. Fist, Figure 3 shows that there are non-negligible propagation effects to households that are more than one link away from the shocked households. Second, only focusing on households with a direct link to the shocked household reduces substantially the number of available observations. Note that this approach excludes households without connections to shocked households, so the number of observations drops.

B4. Persistence in transaction networks

To test for rigidities in the local networks, we construct a dyadic dataset including indicators of whether each pair of sample households (dyads) transacted in year t either in the local goods, labor or financial market. We then use this dataset to estimate the following model:

$$\begin{aligned} Link_{i,j,t} =& \rho Link_{i,j,t-1} + \gamma_1 Kinship_{i,j} + \gamma_2 Demographic\ distance_{i,j} \\ & + \gamma_3 Net\text{-}Worth\ distance_{i,j} + \delta_{v,t} + \alpha_i + \alpha_j + \epsilon_{i,j,t} \end{aligned} \tag{B5}$$

where $Link_{i,j,t}$ is an indicator of whether households i and j transacted in period t. $Kinship_{i,j}$ is an indicator that takes the value of 1 when households i and j share a direct link in the local kinship network (e.g., first-degree relatives), which is measured during the baseline survey in 1998.[63] We include controls for distance with respect to demographic characteristics and a measure of distance between each pair of households based on baseline net worth (e.g., total assets net of liabilities).[64] Finally, we also include household-fixed effects. The parameter of interest is ρ, which captures the persistence of the economic interactions between each pair of sample households.

Table A9 shows that there is an important degree of persistence in the labor-market and supply chain networks, with raw auto-correlation coefficients of 0.47 and 0.42 (see column (1) in each panel). These are substantially higher than that of the financial network (0.26). The estimated levels of persistence are also orders of magnitude above the probability that two randomly-chosen nodes in the network transact in a given year (0.051, 0.061 and 0.012 in the supply chain, labor market and gift/loan networks, respectively). In the case of the labor market and the supply chain networks, having transacted during the previous period explains one-fifth of the overall variation in the current probability of trading. This pattern contrasts sharply with the case of the transactions in the financial markets (gifts and loans) as transactions in period $t-1$ only explain 6.7% of the overall variation in the probability of transacting at t. One explanation is that financial networks are less active, and, as the results from Section B.B5 suggest, are probably responding to either unexpected business opportunities or shocks. Persistence remains substantial after controlling for village-year fixed effects, suggesting that economic linkages respond mostly to within-village variation (see column (2) in each panel).

In columns (3) and (4), we analyze whether persistence is related to kinship relationships, differences in demographic characteristics or differences in endowments (net worth). Although, in all three networks, controlling for baseline kinship links

[63] Two households share a link if they are first-degree relatives (including parents-in-law).

[64] Demographic distance is measured as the euclidean norm of a vector of household attributes capturing household size, gender and age composition, as well as average age and education corresponding to members of the household at baseline. We then take logs of the resulting norm. Net worth distance is constructed by taking logs of the squared net-worth difference within each pair.

reduces the persistence coefficients, they are still high. Persistence does not seem to respond to including measures of differences in terms of demographic characteristics or initial wealth. In all cases, pairs that share kinship connections are 10 percentage points more likely to trade. The probability of trade in the supply chain and labor networks does not respond to differences in distance or wealth between the two households. In contrast, the probability of trading in the local financial network increases when households are different in terms of demographic characteristics, but decreases when there are differences in baseline wealth in the pair. This pattern highlights two features of local financial networks. First, among those households with similar wealth, households that differ in demographic characteristics are more likely to transact, suggesting that one motive for trading is diversification, as shock type and occurrence may vary with demographics. Second, similarly wealthy households are more likely to trade, which suggest that, although diversification takes place, it is restricted to household pairs for whom insurance is more likely to be actuarially fair.

B5. Direct and indirect coping mechanisms

What, if any, coping mechanisms do households use when hit by the direct or indirect effects of health shocks? Appendix Table A15 examines the response of gifts, borrowing, fixed and liquid assets, and incoming unpaid labor. In principle, all of these mechanisms may be helpful in smoothing shocks, but it is an empirical question to what extent they are actually used.

Panel A presents results from direct shocks, corresponding to equation (2). Column 1 shows that incoming gifts increase by THB 571, or approximately 29%.[65] Columns 2 to 4 show that although borrowing increases and fixed and liquid assets decline, the changes are not significant.[66] Finally, column 5 shows that there is no response in terms of the amount of incoming unpaid labor. This is important as it demonstrates that the reductions in paid labor documented above are not reflections of a substitution to unpaid labor. Panel B presents results from indirect exposure to shocks, corresponding to equation 4. There are no significant effects associated with indirect shock exposure on any of the five mechanisms. This helps to explain why consumption falls for indirectly shocked households—other coping mechanisms appear to be unavailable.

Why do directly shocked households see economically and statistically significant increases in transfers, while indirectly shocked households do not? First note that, in addition to receiving transfers, directly shocked households take other costly steps to buffer consumption, namely scaling back on business activ-

[65] Note that this is on the same order as the direct effect on health spending in Table 1; however, comparing Figure 1, Panel c and Figure A5 shows that the *timing* of gifts does not match that of health spending; with gifts in the half-year of the shock meeting less than half of the roughly THB 2000 of spending needs in that half-year.

[66] Health spending needs emerge suddenly and so arranging for loans or asset sales may take too long; alternatively households may desire to preserve these financing options as last-resort buffer stocks and so finance the shock out of business investment instead.

ities. Two other factors may help explain the divergence in transfer behavior. First, the direct shocks are large increases in health spending, often associated with changes in health symptoms. These shocks are salient and relatively observable. The indirect shocks, on the other hand, arise from reductions in supply and demand facing household businesses. Such shocks are likely less salient and potentially more subject to concerns of effort and verifiability, hence potentially less insurable. Moreover, because the indirect shock, by its nature, affects many interlinked households, the shock becomes *de facto* aggregate, which makes the potential for insurance via gifts from other villagers more limited.

B6. Effects of health shocks by participation in informal insurance networks and by hired-household labor complementarities

To examine the effects of health shocks by participation in informal insurance networks, we follow Samphantharak and Townsend (2018) who observe that if households are active members of local insurance networks, incoming gifts should co-move with declines in household idiosyncratic income. We bring this idea to the data by using pre-shock time series data to estimate, household by household, the sensitivity of net incoming gifts to idiosyncratic income. Specifically, we regress net gift reception as a share of asset's on province-month fixed effects and recover the residuals of such regression. Next, for each household, we regress the residuals on Samphantharak and Townsend (2018)'s measure of idiosyncratic returns over assets using pre-period data and recover a household-specific measure of gifts-returns co-movements.

We then classify households with above median pre-shock gift-to-income sensitivity as having "high" access to informal insurance, and others as having "low" access to informal insurance. We replicate this process using pre-period data with respect to actual and placebo shocks. We then estimate a triple differences model, modifying equation 2 to allow the effect of a shock to vary by access to informal insurance:[67] To increase statistical precision, in these regressions we use households that experience a shock in the second half of the period as additional treatment observations, with the demographically similar households experiencing the shock in the first half as placebo observations.

$$\begin{aligned} \text{(B6)}\quad y_{i,t} =& \beta_1 Post_{i,t} \times Treatment_i \times Low_i + \beta_2 Post_{i,t} \times Treatment_i \times High_i \\ &+ \theta_1 Post_{i,t} + \theta_2 Post_{i,t} \times High_i + X_{i,t}\kappa + \alpha_i + \delta_t + \delta_t \times High_i + \epsilon_{i,t} \end{aligned}$$

where $y_{i,t}$, $Treatment$ and $Post$ are defined as in Section II.A. $High_i$ takes the value of 1 for households with high access to informal insurance networks before the shock (either actual or placebo); Low_i is defined analogously. The coefficient β_1 captures the effect of a shock for households with low access to insurance

[67]We estimate the gifts-to-income sensitivity using the 24 months preceding each shock (both actual and placebo).

networks, and β_2 captures the direct effect of a shock for households with high access.

Next, to investigate whether shocks to less-insured households lead to larger aggregate effects, we estimate the following model:

$$\begin{aligned}(\text{B7})\qquad y_{i,t,j} =& \beta_1 Post_{t,j} \times Density_j \times Low_j + \beta_2 Post_{t,j} \times Density_j \times High_j \\ &+ \gamma Post_{t,j} \times High_j + \mathbf{X}_{i,t,j}\kappa + \theta_{\tau(j)} + \alpha_i + \omega_j \\ &+ \delta_t + \delta_t \times Density_j + \delta_t \times High_j + \epsilon_{i,t,j}\end{aligned}$$

where $High_j$ is an indicator of whether directly shocked household j had above-median pre-period access to informal insurance networks, defined as above. Density measures the standardized network density of the shocked village during the year preceding the shock. The coefficient β_1 measures the change in outcomes after the shock associated with a one-standard-deviation change in proximity to the shocked household when that shocked household has below-median access to informal insurance ($Low_j = 1$), and β_2 captures the effect of indirect effects when the shocked household had above-median access to informal insurance networks ($High_j = 1$).

We repeat a similar approach to estimate the effects of shocks by a household's degree of complementarity between hired labor and labor provided by household members. For this, we regress total hours of hired labor and total hours of household-provided labor on province-time fixed effects and obtain the residuals. Next, for each household, we estimate the co-movements ($c_i^{h,l}$) between both residualized versions of household and hired labor using pre-period data. We next classify households on high vs. low complementarities based on whether $c_i^{h,l}$ is above or below the median. Finally, we estimate equations (B6) and (B7).

The Thai healthcare system

Thailand has a universal health insurance program, so these expenses are above and beyond those covered. Only 6% of households received insurance payments within three months of experiencing the shock. The insurance program covers expenses related to basic healthcare services, which include medical visits at registered primary healthcare facilities (which must be located in the same area as each patient's registered residential address), transferred patients from a primary facility to secondary or tertiary facilities for complicated cases, emergency cases at non-registered facilities, expenses for in-patients staying for less than 180 days for the same illness, and prescriptions of medicines as listed in the National List of Essential Drugs. For details, see Thailand's National Health Security Office (NHSO), Administrative Manual, 2014 (in Thai). `http://www.oic.go.th/FILEWEB/CABINFOCENTER3/DRAWER091/GENERAL/DATA0000/00000367.PDF`