NBER WORKING PAPER SERIES

PROPAGATION AND INSURANCE IN VILLAGE NETWORKS

Cynthia Kinnan
Krislert Samphantharak
Robert Townsend
Diego A. Vera Cossio

Working Paper 28089
http://www.nber.org/papers/w28089

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
November 2020, Revised July 2022

We thank numerous colleagues and seminar audiences for helpful suggestions. Townsend gratefully acknowledges research support from the University of Thai Chamber of Commerce, the Thailand Research Fund, the Bank of Thailand, and the Eunice Kennedy Shriver National Institute of Child Health and Human Development (NICHD) (grant number R01 HD027638). Opinions, findings, conclusions, and recommendations expressed here are those of the authors and do not necessarily reflect the views of the Bank of Thailand, the Inter-American Development Bank, or the National Bureau of Economic Research.

Propagation and Insurance in Village Networks
Cynthia Kinnan, Krislert Samphantharak, Robert Townsend, and Diego A. Vera Cossio
NBER Working Paper No. 28089
November 2020, Revised July 2022
JEL No. D13,D22,I15,O1,Q12

ABSTRACT

In village economies, small firm owners facing idiosyncratic shocks adjust production by cutting spending and reducing employment. House-holds with whom they trade inputs and labor scale back their own businesses and reduce consumption. As effects reverberate through local economies, the aggregate indirect adverse effects are larger than the direct effects. Propagation is more severe when transmitted through labor networks as opposed to material supply-chain networks, and goes beyond input-output/sectoral considerations as it varies with network position, closeness to a shocked household, and network density. Participation in gift-giving insurance networks mitigates direct and hence indirect effects. Supply chain and labor networks are fragile as the broken links are not easily replaced, leading to persistent damage. Social gains from better-targeted safety nets are substantially higher than private gains.

Cynthia Kinnan
Department of Economics
Tufts University
8 Upper Campus Road
Medford, MA 02155
and NBER
cynthia.kinnan@tufts.edu

Robert Townsend
Department of Economics, E52-538
MIT
77 Massachusetts Avenue
Cambridge, MA 02139
and NBER
rtownsen@mit.edu

Krislert Samphantharak
School of Global Policy and Strategy
University of California San Diego
9500 Gilman Drive
La Jolla, CA 92093-0519
krislert@ucsd.edu

Diego A. Vera Cossio
Inter-American Development Bank
1300 New York Ave, NW
Washington, DC 20577
diegove@iadb.org

1 Introduction

Economies are exposed to shocks at many levels, ranging from the aggregate (such as booms or recessions) to the level of individual households or firms (such as an inheritance windfall or illness). Shocks that do not directly affect all agents may nonetheless propagate to others. For example, in advanced economies the role of supply chains in spreading economic shocks is increasingly well understood (e.g., Carvalho et al. 2021). However, material input-output linkages are likely only part of the story. In low- and middle-income countries (LMICs), households with multiple members typically engage simultaneously in self-employment—running small and medium enterprises—and in wage work. This creates additional firm-to-firm linkages through labor: a household firm hit with an adverse shock may cut both material inputs and hired labor, reducing income and consumption among other households running their own firms. Thus, one can construct both networks of supply chain material transactions and networks of these labor transactions. In advanced economies, firms may rely on formal insurance and hedging contracts to cope with shocks. In LMICs, households running firms may engage in informal risk-sharing. Thus, one can construct another network, of insurance transactions.

We empirically study how these supply chain, labor, and insurance networks interact and contribute to the propagation or mitigation of shocks. To the best of our knowledge, this is a relatively unexplored question. A related open question is whether a household's ability to cope with direct shocks mitigates propagation. Firms—particularly small, household-owned firms—are heterogeneous in the extent to which they participate in different networks and to which they can rely on markets for insurance, goods, and labor to mitigate shocks. This heterogeneity may give rise to instability: even if on average firms can buffer shocks, those with the greatest exposure or the least ability to buffer may disproportionately drive aggregate impacts.

Just as there are different types of networks, networks vary in their structure. Yet little is known about the empirical role played by network structure in mediating propagation. Denser networks may ease trade but may also increase exposure to shocks. Shocks to firms which are central in a network, but not necessarily large, may propagate less if such central firms are well insured, or more if they have rigid or complex networks. Understanding these relationships can paint a more complete picture of the aggregate benefits of safety nets such as employment protection programs (e.g., the US Paycheck Protection Program), workfare (e.g., India's National Rural Employment Guarantee Scheme), health insurance, cash transfers, etc. Such programs may benefit from better targeting of those with key roles in the underlying networks.

Understanding how idiosyncratic shocks propagate through networks requires identifying shocks which meet several criteria: exogeneity, a scale of shock large enough to "move the needle," and idiosyncrasy (i.e., that the direct impact of the shock is isolated to a given firm). It also requires granular data on who is linked to who, how, and when; as well as production- and consumption-side panel data. And to measure how network structure amplifies or mitigates shock propagation, we need to observe multiple networks.

We use a dataset that is uniquely suited to answer these questions. The Townsend Thai data, constructed from 14 years of monthly panel survey data on households in rural and peri-urban Thai villages, contain detailed information regarding transactions among family-operated businesses, which we use to construct labor and supply chain networks. The data also allow us to identify large, idiosyncratic shocks to households' budgets and labor endowments in the form of shocks to health status, and to construct a valid counterfactual to obtain causal estimates. These elements together provide an ideal setting to shed light on the role of networks in both propagating and mitigating shocks.

We first show that idiosyncratic shocks have significant effects on the business activities of the shocked household. We follow Fadlon and Nielsen (2019) and compare the changes in outcomes of shocked households before and after the shock to those of other households who will experience a similar shock, but later on.[1] Shocked households reduce business spending by 23%, and almost entirely cut their demand for external labor (by 79%). These results are robust to alternative definitions of the onset of a shock; when we define shocks using interruptions to usual daily activities rather than spending; and when we consider alternative control groups and estimation procedures.[2]

This paper is not the first to show that separation between the consumption and production sides of households' balance sheets fails to hold (see, e.g., Benjamin 1992; LaFave and Thomas 2016; Jones et al. 2022). Our key contribution is to show that the business-side adaptations by the directly-hit household lead to *indirect* impacts on other, linked, local businesses and workers. To causally identify these impacts, our first empirical strategy leverages variation in the proximity of a given household to the shocked household through pre-shock economic networks. We compare changes in outcomes before and after the shock, between those more exposed (closer to the shocked household in the pre-shock network) and less exposed (farther away). Households with greater exposure see larger falls in total upstream and downstream transactions (a 21% decline for a unit change in closeness), and therefore falls in income and con-

[1] The outcomes of these "later shocked" households inform the counterfactual outcomes of the shocked households. For more discussion of how we construct the control group and the placebo shocks assigned to the control group, see Section 3.

[2] Specifically, we provide results of a regression-based stacked difference-in-difference approach that uses not-yet-treated households in the same village as controls, and Callaway and Sant'Anna (2021)'s doubly robust difference-in-difference estimator.

sumption (12% and 4% declines for a unit change in closeness, respectively).[3] A back-of-the-envelope calculation suggests roughly 170 Thai baht of indirect effects for each 100 baht of direct effects, implying a multiplier of 1.7.

Two alternative identification strategies yield similar results: one in which we compare those close to a shocked household vs. those close to a household who will suffer a shock later on; and a triple-difference specification using those with close connections to households experiencing a placebo shock as controls.

We provide novel evidence on how different types of networks contribute to propagation. Transactions decline equally when shocks spread through supply-chain and labor-market networks. But the indirect effects on income and consumption are more severe for exposure through labor-market networks, holding exposure through supply-chain networks constant. This suggests that while inputs and final output can also be traded outside the village, the local nature of labor markets may prevent indirectly shocked households from adapting to the disruptions caused by the health shocks.[4]

Due to the potential amplifying effects of multiple, possibly interacting, market frictions, an omnibus test for the presence of market failures is particularly useful. In the absence of market failures, the aggregate effects of idiosyncratic shocks can be summarized by the market share of the shocked firm/operating household or, in a roundabout economy, the input-output multiplier(Baqaee and Farhi, 2020; Hulten, 1978). We bring this idea to the data and show that, after controlling for a shocked household's market share, a

[3]We find indirect effects both upstream and downstream, as the costly adjustments taken by the directly-shocked households reverberate through the local network. Although the shocks dissipate through the networks, we find effects on indirectly-connected households (two or more links away from the shocked household) as well as those directly linked.

[4]We also show that there is no indirect treatment effect on the provision of uncompensated labor or on net gifts and loans, ruling out the concern that "propagation" is simply a relabeling of the linked households providing insurance.

household's pre-shock degree centrality and the village's pre-shock network density predict larger indirect effects. Thus, efficiency is rejected: market frictions serve to amplify negative idiosyncratic shocks.

Our setting enables us to shed light on the link between shock propagation and mitigation through insurance and labor markets. The health episodes we identify may shock both spending needs and labor endowments. Further, households vary in their vulnerability to each channel. We first show that hired labor declines more when a household with limited ability to substitute household with external labor is hit. However, business spending declines equally, regardless of a household's ability to substitute household labor, suggesting that there are other market frictions limiting a household's ability to mitigate shocks. We next show that shocks to households with limited participation in insurance networks lead to large direct, and indirect, effects. In contrast, when well-insured households are shocked, they buffer the direct shocks by receiving transfers from other households; as a result, the direct and indirect effects are more muted. Thus, supply-chain and labor networks propagate negative shocks, but insurance networks mitigate propagation.

We further show that propagation occurs in a context rigid yet vulnerable networks. Existing links are vulnerable to shocks, and replacing these links appears challenging: suppliers struggle to find new customers when their clients suffer a shock, and workers cannot easily find new jobs when existing employers scale back demand. The indirect effects persist even two years later.

Our results are novel in several ways. First, they provide insights related to the mechanisms behind multiplier effects—an increase in aggregate activity greater than the direct effect alone. Several studies document multiplier effects arising from large inflows of cash into local economies (Nakamura and Steinsson, 2014; Chodorow-Reich, 2019). Our approach enables us to contribute with a novel insight: *idiosyncratic* shocks can amplify and generate multiplier effects similar to those generated by large inflows of resources. In addition, while recent work in developing contexts identifies equilibrium channels such as wages (see, e.g., Egger et al. 2021; Franklin et al. 2021) and prices (e.g., Burke et al. 2019; Cunha et al. 2019) as important for indirect effects, less is known about locally heterogeneous exposure to indirect effects via production networks.[5] Detailed data on networks enables us to estimate within-village spillovers that are *net* of any effects on aggregate demand affecting wages or prices; a crucial distinction for policy (Guren et al., 2021).

Second, existing evidence on how disturbances propagate through global supply-chain networks comes largely from sectoral shocks and larger firms in high-income countries.[6] Less is known about local propagation though this is receiving increased attention and supply chain disruptions persist. In LMICs, small, family-operated firms participate in multiple networks; are exposed to shocks not typically faced by large firms (e.g, shocks to family health and wealth); and whose consumption and production decisions are intertwined.

Our contribution is to show that different networks serve distinct roles. First, both supply-chain and labor-market networks increase a firm's exposure to *others'* shocks, but shocks spread through labor-market networks can lead to more severe indirect effects. Thus, extrapolating supply-chain results to effects via labor market linkages may underestimate the degree of propagation. Second, while shocks propagate through production networks, risk-sharing networks play a mitigating role, helping households buffer their *own* shocks, and

[5] Angelucci and De Giorgi (2009) and Moscona and Seck (2021) show that cash transfers via public programs spill over to non-eligible households via risk sharing networks; their focus is not on production-side network spillovers, nor on shocks that are *prima facie* idiosyncratic to individual households.

[6] See for instance, Carvalho et al. (2021); Barrot and Sauvagnat (2016); Caliendo et al. (2017); Dhyne et al. (2021); Huneeus (2019).

in turn limiting their spread.[7] Our results suggest a key policy implication: expanding insurance against shocks affecting households' spending needs can have large returns in terms of aggregate production.

Finally, observing multiple, independent village networks allows us to provide novel empirical evidence on the link between network structure and propagation.[8] We show that the aggregate impact of idiosyncratic shocks is larger in villages with denser networks. Following Baqaee and Farhi (2020), this finding indicates the presence of market failures in the economy, providing a novel omnibus test of misallocation. In addition, this finding illustrates an important policy tradeoff: encouraging more interconnection in village economies may improve diffusion of information and encourage the adoption of new technology (Banerjee et al., 2013; Beaman et al., 2021) and strengthen insurance networks (Feigenberg et al., 2013), but can also increase propagation.

2 Context and Data

2.1 Household data

The data used in this study come from the Townsend Thai Monthly Survey, which covers approximately 45 households per village, representing 42% percent of the village population. A baseline interview was conducted from July to August 1998, collecting information on the demographic and financial situation of the households as well as ecological data on the villages. Monthly

[7] We also contribute by studying the extent to which the presence of complementarities between external and household-provided labor amplify propagation.

[8] Allen and Gale (2000) and Elliott et al. (2014) examine the role of network structure theoretically in the context of links between financial intermediaries while Bigio and La'o (2020) examine the case of input-output networks. To our knowledge the role of network structure in propagation in village economies has not previously been examined nor have these effects been tested empirically.

updates began in September 1998.[9] The sample in this paper covers the period between September 1998 and December 2012. We focus our analysis on the subset of 509 households who responded to the interview throughout all survey waves. Table A1 Panels A and B characterizes sample households in terms of demographic and financial characteristics. While households derive income mostly from family farms, they also operate off-farm businesses and provide labor to other households and businesses. Roughly 13% of total income comes from the receipt of government transfers and/or gifts from other households.

2.2 Network data

The data includes detailed information on transactions among households capturing different economic interlinkages. In each survey wave, interviewees identify all households in the village with whom they have conducted a given type of transaction.[10] We aggregate the monthly transactions by year to elicit three types of networks for each year in the sample. Appendix Figure A1 depicts both networks for one sample village in one year.

First, we recover the supply-chain networks that capture transactions of output, inputs, and intermediate goods across businesses involving households in the same village. Second, we recover labor networks that capture employer-employee relationships within the village. Panel C of Table A1 shows statistics on network participation across the sample as a whole. On average, just above half (51%) of the households transact in the local village supply-chain network

[9] For more detail about the survey, see Samphantharak and Townsend (2010).

[10] The set of transactions includes relinquishment of assets, purchases or sales of inputs or final goods, provision of paid and unpaid labor, and giving and receiving gifts and loans. As is typically the case in networks based on survey data, our networks may look thinner than those that would be elicited using census data (Chandrasekhar and Lewis, 2016). We discuss the implications for our research design in Section 4.

by trading inputs and final outputs (with 1.26 connections on average per year), and 66% provide to or purchase labor from other households in the village, with 3.33 connections on average. We also recover information on local financial networks defined by gifts and loans across households in the same village, which tend to be sparser (see Appendix Figure A1).

Households participate in several networks in a given period. For those linked through gift/loan networks, over 60% also transact in supply-chain networks and over 70% of them transact in labor markets. Over 77% of households transacting in the village supply-chain networks also sell/purchase labor locally, and 45% are linked through local financial markets. Likewise, over 59% and 43% of households buy or provide labor locally transact in the supply-chain and gift/loan village networks, respectively. Kinship networks also overlap with these transaction-based networks; see Section 5.3.

Panel D concerns the size of sample villages and firms. The average village has 161 households. There is evidence of excess kurtosis in the firm size distribution (measured via gross revenues): average village-level kurtosis is 10 (excess kurtosis of 7). Following Gabaix (2011), excess kurtosis suggests that idiosyncratic shocks may have important aggregate effects in our setting.

2.3 Constructing idiosyncratic shocks

To understand how shocks propagate to other households through village networks, we focus on idiosyncratic events associated with high levels of health spending, which correspond to periods of high financial stress. These shocks are well suited for our analysis for several reasons. First, serious health shocks affect household finances and labor supply (Gertler and Gruber, 2002; Genoni, 2012; Hendren et al., 2018); the large magnitude of such shocks improves statistical power and moreover such shocks are of *prima facie* importance. Second, because these shocks are uncorrelated across households (as shown below), we are able to separate the direct idiosyncratic effect from indirect effects hitting other connected households via propagation. Additionally, the timing of these shocks is—as we show below—exogenous.

We identify shocks as follows (see Appendix B for details). For each household, we identify the month with the highest level of health spending over the panel.[11] We focus on the largest shocks, because they pose a significant financial burden to the household. To account for potential anticipation effects, we define the beginning of an 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. Thus, although we use spending data to identify shocks, the *timing* of the onset of the shock is coded based on changes in health status.[12]

We restrict this search to episodes occurring during years 3–12 of the panel (out of 14 years). This enables us to observe at least two years both pre- and post-shock. We identify 505 shocks, 1 per household. We exclude shocks related to childbirth, which may be anticipated, leaving 470 shocks.

2.4 Characteristics of the shocks

Relationship between health spending and health status. A natural question is how our spending-based shock measure correlates with changes in health status. Figure B9 shows that self-reported symptoms and health

[11] Thailand has a universal health insurance program covering (some) health costs, so these expenses are above and beyond those covered; see Appendix C.

[12] Appendix Figure B8 shows that, prior to the sudden increase in health spending, the median number of consecutive months in which households report any health symptoms is three. More details are in Appendix B.

spending co-move, confirming that shocks are identifying decreases in household health. Figure B10 reports the distribution of types of symptoms reported by shocked households during the year following a shock and during non-shock periods. Usage of health facilities is substantially higher after shocks, showing that the shocks capture severe changes in health. In addition, the likelihood of visiting a health facility due to an accident is substantially higher during post-shock periods, and there is a higher incidence of uncommon symptoms, showing that shocks capture unexpected changes in health status.

Magnitude of the shock. The shocks represent a substantial financial burden to the households. On average, the highest level of monthly health spending within 6 months after the onset of the shock (THB 5314) accounted for 87% of the average monthly per-capita consumption during the 6 months preceding the shock (THB 6113) and was substantially larger than the average monthly per-capita food consumption during this period (THB 2817).

Are the shocks idiosyncratic? Our analysis requires that the events be idiosyncratic and their occurrence be uncorrelated with trends in household behavior or network/sectoral shocks. The top panel of Appendix Figure B11 shows that event start dates are spread evenly through the periods in the sample. Indeed, the bottom panel shows that in over 87% of the cases, shocks affected only one household per village at any one month. In the bottom panel of Appendix Table B14, we formally show that village-level trends have null predictive power on the the occurrence of these events ($p = 0.39$).

Are the shocks exogenous? Column 1 in Appendix Table B14 shows within-household correlations of the probability of experiencing a shock in period $t + 1$ and contemporaneous financial characteristics. We are unable to reject the null of joint significance ($p = 0.27$). In addition, column 2 reports $p-$values of the null hypothesis that the 12 lags of each household characteristic do not precede the onset of the shock (i.e., a Granger causality test). We only reject this hypothesis at the 10% level for 1 out of the 13 variables.[13] These patterns suggest that the timing of shocks is orthogonal to pre-shock family and business financial decisions. The next section discusses our empirical approach and addresses other potential concerns about endogeneity.

3 The direct effects of idiosyncratic shocks

To understand the indirect effects of shocks via network propagation, we first must understand how they affect the *directly* shocked household. Because the networks we study are defined by cross-household transactions of inputs, output, and labor, our first-stage analysis focuses on estimating the direct effects of shocks on business spending, labor demand, and production.

Estimating the effects of idiosyncratic shocks on household outcomes requires a valid comparison group. We would like to compare shocked households and otherwise-similar households who, by chance, were not simultaneously exposed to a shock. To implement this comparison, we follow Fadlon and Nielsen (2019) and exploit plausibly random variation in the *timing* of severe health shocks.

We compare the behavior of households who experienced a shock in period t (i.e., treated households) to that of households from the same age group and village who did not experience a shock at time t, but did experience a similar shock later on, in period $t + \Delta$ (control households).[14] Treated households

[13] Health spending in period t is negatively correlated with the onset of the shock in $t+1$. This is by construction: when a shock starts (i.e., when when a household member starts reporting symptoms) at time $t+1$, households are very likely to experience their largest level of health spending, hence spending in the previous month, t, will be lower.

[14] Δ is calibrated by taking the midpoint between the months associated with the first and last shocks in each age group-village bin. See Appendix B.2 for a detailed description

are those who experienced the shock during the first half of the panel; control households experienced a shock during the second half.

We use a difference-in-difference approach to compare changes in outcomes, before and after the shock, between treatment households (who experienced an actual shock) and control households (who are assigned a placebo shock, Δ periods before the occurrence of their actual shock).[15] The underlying assumption is that, in the absence of the shock, the treatment and control groups would have followed parallel trends, which we validate using event-study specifications that test for lack of systematic differences before the shock.

3.1 Estimation

We estimate the following generalized difference-in-difference specification, following Fadlon and Nielsen (2019):

$$y_{i,t} = \sum_{\tau=-4,\tau\neq-1}^{\tau=3} \beta_\tau \mathbf{I}[t=\tau] \times Treatment_i + \sum_{\tau=-4,\tau\neq-1}^{\tau=3} \theta_\tau \mathbf{I}[t=\tau] \tag{1}$$
$$+ X_{i,t}\kappa + \alpha_i + \delta_t + \epsilon_{i,t}$$

where $y_{i,t}$ denotes the outcome for household i at t. Household- and month-fixed effects (α_i and δ_t) absorb time-invariant household characteristics and aggregate time-varying shocks. $Treatment_i$ is a time-invariant indicator of whether the household is in the treatment group. As each household is either observed in the treatment or comparison group, $Treatment_i$ is absorbed by the household-fixed effects. Time to treatment is denoted by $\tau_{i,t}$ and is measured

of the process.

[15]Thus, if a household in the control group experiences the actual shock in t'', its placebo shock is assigned to period $t'' - \Delta$. See Appendix B.2 for additional details.

in half years to increase precision. X is a vector of time-varying demographic characteristics including the number of male and female household members, age of the household head, and maximum years of schooling in the household. The coefficients of interest are $\{\beta_\tau\}_{\tau=-4}^{\tau=3}$, which compare differences in changes in outcomes with respect to the period immediately preceding the shock ($\tau = -1$) between households in the treatment and control groups. We focus on a two-year (i.e., four-half year) time window before and after the shock, because our panel is fully balanced during this period. We also use a more parsimonious differences-in-difference specification to compute average effects over a two-year post period:

$$y_{i,t} = \beta Post_{i,t} \times Treatment_i + \theta Post_{i,t} + X_{i,t}\kappa + \alpha_i + \delta_t + \epsilon_{i,t} \qquad (2)$$

where $Post_{i,t}$ is an indicator that takes the value of 1 in periods following the shock and 0 otherwise. The parameter of interest, β, compares differences in outcomes before and after the shock between households in the treatment group and the comparison group. In both specifications, we cluster standard errors at the household level, because our main source of variation comes from cross-household variation in the timing of events and we want to account flexibly for serial correlation.

Note that our approach addresses several issues that may arise in simple event-study panel regressions without a stable comparison group—i.e., when researchers regress outcomes on time- and household-fixed effects and a post-shock dummy. A simple event-study approach would use all the households who do not experience a shock at period t as a control group for those that did, even those who were shocked before t. This could be problematic in our setting, because such "staggered event timing" specifications may suffer from bias when effects are heterogeneous over time (Goodman-Bacon, 2018; Baker et al., 2022). Our design, by virtue of using a control group which is never treated before/during the comparison window, avoids these concerns. However, this advantage comes at the cost of statistical power and limits the number of available post-period observations as we only analyze the subset of 247 shocks that occurred earlier in the sample. Moreover, trends in outcomes may vary by age and village due to different trajectories along the life cycle; by constructing a comparison group within age group and village, our approach constructs a comparison group of households with similar pre-shock trends and similar risk profiles.

3.2 Direct effects: Results

A health shock can affect households in a number of ways. Here we focus on changes in household production decisions—reducing spending on hired labor and/or business inputs—because such dimensions are linked to cross-household transactions that determine local economic networks.

Figure 1 reports flexible difference-in-difference estimates following the specification in equation (1). Panel (a) shows that, relative to control households, shocked households experience a large and significant increase in the probability of reporting health symptoms. Panel (b) shows that this coincides with a sharp increase in total health expenditure.[16] Panel (c) shows that

[16] Appendix Figure A2 provides evidence that the onset of the shock coincides with changes in health status and health spending that are likely severe and unexpected. Panels (a)–(e) show that the usage of and spending on outpatient and inpatient care increase and that the probability of receiving medical care due to an accident also increases. These patterns suggest that the shocks generate immediate spending needs that squeeze a household's budget. The non-financial consequences of the shocks may be persistent: panel (f) of Appendix Figure A2 shows that the shocks increased the probability of suspending activities for more than a week and that even though this effect declines over time, it persists for two years after the onset of the shock.

non-health consumption remains steady.

The remaining panels show that the shocks affect the household's production-side decisions. Panel (d) shows that, compared to households in the control group, hired labor usage declines for shocked households, and Panel (e) shows that input spending falls. Finally, Panel (f) shows that the reduction in input spending coincides with a reduction in revenues. Note that the declines in input spending and revenues coincide with the sharp increase in health spending induced by the shock within a year from the shock occurrence. This suggests that shocked households meet short-term liquidity needs in part by drawing down working capital. The effects on input spending also persist over time, likely reflecting the suspension of key activities in the household due to the shock as shown in panel (f) of Appendix Figure A2.[17] Both channels—reduction in business investment and suspension of activities—suggest behavior that is inconsistent with the separation theorem, reflecting the presence of market frictions. The graphical evidence also documents parallel pre-trends for all six outcomes.[18]

To provide a quantitative assessment of the overall impact of the health shocks, in Table 1 we report difference-in-difference estimates of the effect of the shock on outcomes over a 24-month post-shock period, corresponding to equation (2). Panel A restricts the treatment sample to shocks occurring in the first half of the period, ensuring the control group is never treated before or during the comparison window. Column 1 shows that the shocks are asso-

[17]Note that a one-time health spending need can also generate persistent effects on business spending. For example, shocked firms may not necessarily face the same demand for their products after the shock as consumers deepen their links with other providers. Likewise, households may relinquish fixed assets, inducing a persistent reduction of a business's scale (column 3 in panel A on Appendix Table A10, finds large though not significant declines in assets.).

[18]Appendix Figure A3 shows the same dynamics in the raw data.

ciated with a significant increase in the likelihood of reporting a health issue and column 2 shows that the shock leads to a large increase in health spending. While this is by construction, the magnitude, approximately THB 540, is notable, representing a roughly 350% increase relative to the baseline mean. Column 4 shows that during the two years following the shock, total spending increases for shocked households, relative to control households, by approximately THB 620 on average, an amount close to the effect on health spending. Thus, in terms of non-health spending (column 3), shocked households appear to fully buffer the shocks.

Buffering consumption may entail costly adjustments by shocked households (Chetty and Looney, 2006). Indeed, in order to buffer non-health consumption, affected households significantly decrease spending on business inputs (column 5) and reduce the use of external labor (column 6).[19,20] Households also appear to reduce the use of labor provided by household members (column 7), though the effect is not significant when we restrict to shocks occurring in the first half of the period. As a result of these reduced investments in inputs and labor, there is a decrease in the revenues from family enterprises, as seen in column 8, albeit imprecisely estimated (p-value = 0.107).[21]

[19]Households may also engage in other strategies to cope with the shocks, such as gift reception, borrowing, relinquishing fixed and liquid assets, and using unpaid external labor. See Appendix B.4. We discuss the effects on gift reception in more detail in Section 5.2.2.

[20]An alternative mechanism is that non-shocked household members reduced the time allocated to the operation of family businesses in order to take care of the shocked household member. However, Appendix Table A2 shows that the overall household-level number of days dedicated to housework appears to decline as well.

[21]The point estimates of the effects on business spending and revenues are quite similar at approximately 1,650 THB and 1,530 THB, respectively, suggesting that business profits are unaffected. This can be a consequence of the reduced business scale induced by the decline in business spending. As businesses reduce their scale and their demand for inputs, these adjustments can lead to propagation as we discuss in Section 4. The zero effect on profits may also reflect households responding to the reduction in earnings by taking costly actions that in the short run reduce costs but are harmful in the long term (e.g., deferring needed maintenance of equipment).

Panel B reports results that also include early-shocked households as controls for late-shocked households,[22] which roughly doubles the number of events. Reassuringly, the estimates are very similar to those in panel A but are estimated with more precision.[23]

3.2.1 Robustness

Panel A of Table A3 shows robustness to alternative definitions of health shocks.[24] Columns 1 and 2 show that using the largest *change* in health spending throughout the panel to identify episodes of high health spending yields very similar results to the main specification, which is based on the highest spending *levels*.[25] One concern is that our approach may include shocks that, based on their magnitude, are innocuous, despite being the largest shocks experienced by the households throughout the panel. In columns 3 and 4, we exclude shocks associated with expenditure levels that fall in the bottom 75% of the post-shock health spending distribution for control households; the results are similar to those of our main specification. We also employ two alternative ways of selecting shocks: a household-specific benchmark (health spending larger than the pre-shock average food consumption) and a common benchmark (health spending larger than the sample mean plus one standard deviation). Columns 5 to 8 show that we obtain qualitatively similar results. Both approaches select larger shocks and thus yield larger effect sizes, but also

[22] The treatment status of control households is held fixed around the 24-month analysis window around each event. This addresses potential biases in difference-in-difference frameworks that tend to arise when treatment status varies over time.

[23] The effects on household labor and revenues are now significant at conventional levels, and all other outcomes remain significant.

[24] See Appendix Section B.2.1 for a detailed description of each shock definition.

[25] This similarity reflects the fact that while we use spending data to identify the episodes (either maximum levels or changes), we use symptoms data to calculate the onset of the shocks; see footnote 12 and Appendix B.

use a smaller set of shocks, reducing statistical power.[26]

In columns 9 and 10, we define shocks based on whether a household member suspended their main activities for at least one week. We use a one-week threshold based on Gertler and Gruber (2002), who show that only severe health shocks yield effects on household spending. As opposed to our main specification, which by construction captures episodes of financial stress with potential effects on time allocation, this alternative definition captures shocks to time availability with potential financial implications. Reassuringly, the results are qualitatively similar.[27] The results are robust to allowing for multiple, non-overlapping shocks per household, as shown in Appendix Table A4.

The results are also robust to using alternative control groups. Appendix Table A6 columns 1 and 2 show that the results are robust to randomly allocating the placebo shocks. Columns 3 and 4 use all not-yet-shocked households in the same village as controls to shocked households at time t using a stacked difference-in-difference specification. This specification, namely that the control group should be the not-yet-treated, is recommended in the recent difference-in-difference literature (Baker et al., 2022). Columns 5 and 6 show robustness to using not-*currently*-shocked households as controls in a standard two-way fixed effects specification, columns 7 and 8 report point estimates using Callaway and Sant'Anna (2021)'s estimator with households treated in the second half of the sample as controls,[28] and columns 9 and 10 show that the

[26]In Appendix Table A4 we show that power is increased in these specifications when we include multiple, non-overlapping shocks per household (columns 3 and 4). However, doing so comes at the cost of imposing two additional identification assumptions. First, shocks experienced earlier on should not affect the probability of experiencing another health shock in the future and second, the effects of earlier shocks should not have long-lasting effects on the trajectories of outcomes that can lead to violations of the parallel trends assumption.

[27]Note that the effects in columns 9 and 10 are less precisely estimated, because this alternative definition identifies fewer shocks. We provide a brief discussion and robustness to alternative definitions in Appendix Section B.2.1 and Appendix Table A5.

[28]The corresponding event-study estimates are reported in Appendix Figure A4.

effects are unchanged when using an unbalanced panel of households for the main specification. Reassuringly, results using these specifications are consistent with the main specification. Details are in Appendix B.2.1.

Finally, Appendix Table A7 presents co-movements between health status and spending using all households in the sample and all survey periods. As expected, changes in health status coincide with changes in health spending. Moreover, changes in health status driven by uncommon health conditions (those more likely to happen around the shocks that we study) predict larger changes in health spending and declines in business spending.

4 Economic networks and the propagation of shocks

The results above show that health shocks meet the necessary criteria for understanding propagation: their timing is exogenous, their occurrence is idiosyncratic, and the shocks have substantial effects on household production decisions. Given the significant degree of interlinkage in the study villages, we next examine whether these shocks propagate to other households. We analyze two propagation channels. First, shocks could propagate through local supply-chain networks: health shocks lead to decreases in the supply and demand for inputs, which could lead to reductions in sales and revenue for those households who trade with shocked households. Second, shocks could propagate through local labor networks: as supply and demand for outside labor decrease due to the shocks, households who exchange labor with shocked households could suffer falls in hours, earnings, and revenue.

4.1 Identifying propagation effects

We exploit two sources of variation to test whether idiosyncratic health shocks propagated to other agents in the local economy. First, we use variation in the timing of each household-level shock. Second, we use the fact that a household's exposure will depend on their network connections to the shocked household, via the supply-chain or labor-market networks, or both. We assess the propagation of idiosyncratic shocks to other local family businesses by comparing households who, before the shock, shared closer market interlinkages with household j's businesses to those who were not or less-connected to household j before the shock, before vs. after the shock to household j.

Throughout our sample period, we observe multiple health shocks per village. We construct a data set capturing information of non-shocked households before and after each health shock in the sample. For each event, we take two years of pre- and post-shock observations of households living in the same village as the directly shocked household.[29] We then stack the observations into a data set at the household (i) by time (t) by event (j) level for each village.

We combine this data set with information on network connections between the shocked household (j) and other households (i) in the village, measured during the year preceding the shock to household j. We use pre-shock networks as links may respond to economic shocks themselves. The assumption is that households who transacted with the shocked household during the pre-period, on average, would have been more likely to transact with the shocked households in the post-period, in the absence of the shock.[30]

[29] We restrict the analysis to two years before and after the shock to be consistent with the analysis of the direct effects of the shocks and because we only have a fully balanced panel for this time window.

[30] This is consistent with the evidence of persistence in the village networks discussed in Section 5.3 and with evidence of the importance of time-invariant determinants of economic

We measure exposure as the inverse distance in the undirected village network between household i and the shocked household j: $Closeness_{i,j} = \frac{1}{dist_{i,j}}$.[31] As households are further away in the network from shocked households, exposure (closeness) decreases. We begin by computing overall closeness based on transactions in the supply-chain or labor-market networks, because households can be exposed through either network. To distinguish between exposure in the supply-chain and labor-market networks, we also compute measures of closeness in each separate network (see Section 5).

We elicit economic networks using survey instead of census data (Chandrasekhar and Lewis, 2016). Thus, it is possible that we underestimate the closeness of some sample households to shocked households.[32] Because we may be underestimating exposure—classifying some households as not or less-exposed when they are actually (more) exposed—our results could be biased toward 0. Thus, we interpret our magnitudes as *lower bounds* of the indirect effects of idiosyncratic shocks on other households.

Not all shocked households are active in the local markets for goods and not all shocked households employ or work for other villagers. Thus, we analyze the propagation of shocks through village networks by focusing only on events corresponding to the 391 households who traded in either the supply-chain or labor-market networks during the year preceding their shock; these represent

connections, such as kinship relations (Kinnan and Townsend, 2012), race, or caste (Munshi, 2014), and the existence of economic frictions such as contracting issues that may limit trade between households (Ahlin and Townsend, 2007) or between firms (Aaronson et al., 2004) in local economic networks.

[31] This measure equals 1 if household i has directly traded with the shocked household j and 0 if household i does not have any direct or indirect connections with the shocked household. The geodesic distance between two unconnected nodes is $dist_{i,j} = \infty$ and so their closeness equals 0 in that case. We focus on undirected networks because the shock can propagate both up- and downstream, as we document in Section 4.2. By undirected networks we mean that we do not distinguish between incoming vs. outgoing transactions. Likewise, we weight each transaction equally for our calculations.

[32] See footnote 10 for a discussion of this issue.

83% of all the shocks in our sample.[33]

We estimate the following difference-in-difference specifications:

$$
\begin{aligned}
y_{i,t,j} =& \sum_{\tau=-4,\tau\neq-1}^{\tau=4} \beta_\tau \mathbf{I}[t=\tau] \times Closeness_{i,j} + \gamma Closeness_{i,j} + \mathbf{X}_{i,t,j}\kappa \\
&+ \alpha_i + \omega_j + \delta_t + \theta_{\tau(j)} + \delta_t \times Degree_i + \epsilon_{i,t,j} \qquad (3) \\
y_{i,t,j} =& \beta Post_{t,j} \times Closeness_{i,j} + \gamma Closeness_{i,j} + \mathbf{X}_{i,t,j}\kappa \\
&+ \alpha_i + \omega_j + \delta_t + \theta_{\tau(j)} + \delta_t \times Degree_i + \epsilon_{i,t,j} \qquad (4)
\end{aligned}
$$

where y denotes the outcome of interest for household i in village v at time t around the shock suffered by household j. In the "event-study" specification (equation 3), τ denotes a half year, which may precede ($\tau < 0$) or follow ($\tau >= 0$) the shock to household j. $Closeness_{i,j}$ denotes inverse distance to the shocked household during the year preceding the shock to j.[34] The coefficients of interest in equation (3) are β_τ, which capture relative changes in outcomes corresponding to half year τ with respect to the half year preceding the event ($\tau = -1$) associated with one additional unit of closeness (i.e., between more- vs. less-exposed households). In the generalized difference-in-difference specification, equation (4), $Post_{t,j}$ takes the value of 1 during the two years following the shock to household j and 0 for the pre-period. The coefficient of interest, β, captures differences in outcomes associated with one additional unit of closeness, with respect to the pre-period.

Controls include household fixed effects (α_i); month fixed effects (δ_t); shocked-household fixed effects (ω_j); time-to-shock fixed effects ($\theta_{\tau(j)}$), which account

[33]In Appendix Table A8 we report similar results when we also use shocks to unconnected households (columns 3 and 4), coding closeness equal to 0 for all non-shocked households.

[34]Below we consider several definitions of *Closeness*: proximity in the overall network pooling the supply-chain and labor-market networks, as well as proximity in either network.

for village-specific time-varying shocks during the analysis window corresponding to the shock to household j; and a vector of time-varying demographic characteristics ($\mathbf{X}_{i,t,j}$).[35] We also control for time-varying trends for more-central households, who could also be more likely to be close to other households, by including interactions of the number of links of household i ($Degree_i$) during the year preceding the shock to j with time fixed effects. Thus, we are in essence comparing two households equally well-connected to the network, one of whom happens to be closer to the shocked household. We use two-way clustered standard errors at the event level j and household level i to allow for flexible correlation across households during the periods preceding and following event j and across responses of the same household i to different events. In order to focus on indirect effects, we drop observations of directly shocked households (where $i = j$) from the analysis and exclude observations of households who experienced their own shock within a year before or after the shock to household j.

The identifying assumption underlying our strategy for estimating indirect effects is that, in the absence of the shock to household j, the outcomes of households i and i', with differential closeness to j, would have evolved following parallel trends, conditional on the vector of controls included in equations (3) and (4). We validate this by testing for (lack of) differences in the pre-period; namely, for $\tau < 0$, we verify that β_τ is not different from 0.[36]

[35] These are household size, gender composition, average age, and years of schooling.

[36] As discussed below, Table 4, which shows that proximity in the labor market does not predict changes in supply-chain transactions and vice versa is additional evidence in support of this assumption.

4.2 Results: Propagation through economic networks

Figure 2 presents flexible difference-in-difference estimates following equation 3. Panel A analyzes total transactions. After a health shock, households who are more connected to shocked households differentially reduce the number of transactions with other households in the village. Prior to the shock, transactions are not different for closer vs. more-distant households. After the shock, however, transactions decline more for households who are closer to the shocked household. Panels B and C show that supply-chain and labor-network transactions, respectively, each exhibit the same pattern seen for total transactions. Panel D shows that, as local networks are shocked, total income declines for households closer to the shocked household. In all four cases, the pre-shock period shows no evidence of differential pre-trends. Finally, Panel E shows an analogous result for total consumption expenditure, which declines in the post-shock period (and exhibits no differential trend in the pre-period).

The effects on transactions, income, and spending are evident in all three half-year periods following the shock and do not appear to shrink in magnitude over time: the effects are quite persistent. In theory, indirectly hit households might attenuate these effects over time by finding new local trading partners. However, the evidence on the rigidity of local networks shown below (section 5.3) demonstrates that such reorganization of local ties is very difficult, at least over the span of 1–2 years.

Table 2 shows difference-in-difference estimates corresponding to equation (4). It documents significant post-shock declines in the number of monthly transactions in the supply-chain (column 1) and labor-market networks (column 2), and in total transactions (column 3). These effects are large, representing declines of 20%, 24%, and 21% relative to the pre-period means, respectively. Column 4 shows that these changes in turn reduce income: a one-unit increase in *Closeness* is associated with a fall in income of THB 1232, or 12% of the pre-period mean.[37] In turn, consumption spending falls by THB 293, or 4% of its pre-period mean (column 5).[38] The fall in consumption is smaller than the fall in income, suggesting that indirectly shocked households can partly, but not completely, smooth their indirect shock exposure.

4.2.1 Ripple effects of shocks

Indirectly connected households. The effects that we observe are strongest for directly connected households—those one link from the shocked households—but affect indirectly connected households as well (see Appendix Figure A5).[39] When, due to a shock, those linked directly to shocked households reduce sales of goods or labor (outgoing transactions to the shocked household), this leads to declines in income and consumption. As households are consumers but also operate firms, these indirect effects translate into fewer purchases (incoming transactions) of goods, inputs and labor from other households, triggering further propagation through the network.

Upstream vs. downstream propagation Table 3 shows that the fall in

[37]For comparison, Jarosch (2021) finds evidence of a 20% decline in the net-present value of income over a 20-year period after a job loss using data from Germany. Our estimates are smaller, but computed over a much shorter period. We discuss the role of persistence in Section 5.3.

[38]Recall that these are effects associated with moving from $Closeness = 0$ (unconnected to the directly shocked household) to $Closeness = 1$ (directly linked). The mean level of $Closeness = 0.42$, so the average indirect effect is 42% of the coefficient.

[39]Figure A5 plots indirect effects decomposing the measure of closeness into 4 categories: directly connected households (1 link away from the shocked households), households who are 2 or 3 links away from the shocked households, those who are 4 or 5 links away from the shocked households, and the base category, those who are 6 or more links away in the network, including those who are unconnected to the shocked household. Although the effects dissipate through the network, there are non-negligible propagation effects on indirectly connected households.

overall transactions documented above is driven by falls in both outgoing transactions (sales of inputs and labor) and incoming transactions (purchases/hiring). In sum, the health shocks that we study generate indirect effects both upstream and downstream, as the costly adjustments taken by the directly shocked household reverberate through local networks. Shocks that are prima facie idiosyncratic are spread to other connected households.

4.2.2 Effects of supply-chain vs. labor-market exposure

In Table 4, we examine whether the effect of exposure through the supply-chain network has different effects than exposure through the labor-market network. If proximity through the supply chain (labor) network is associated with changes in input/output (hired labor) transactions, and not vice versa, this is supportive of the identification assumption, because many plausible confounds (e.g., differential trends between closer vs. more-distant households) would manifest in both sets of outcomes. Because the two networks are correlated, we analyze the effect of exposure to one controlling for the effect of the other.[40] Column 1 shows that, conditional on proximity in the labor-market network, a 1-unit increase in proximity in the supply-chain network is associated with a significant fall in input/output transactions of 0.23. There is no effect on input/output transactions associated with proximity through the labor-market network. Analogously, column 2 shows that proximity through the labor-market network has a negative and significant effect (-0.21) on transactions involving paid labor, while there is no effect seen via the supply-chain

[40] On average, 41% of households share a direct or indirect link to the shocked households through both networks, 16% are directly or indirectly linked to the shocked household through only the supply-chain network, 13% are directly or indirectly connected to the shocked households through only the labor-market network and 30% of households are not connected to the shocked households either network.

network. In column 3, proximity via the supply-chain network or the labor-market network generates negative and significant effects on the total number of transactions of -0.20 and -0.24, respectively.

Columns 4 and 5 show that proximity via the labor-market network is associated with large and significant drops in both income and consumption, while the corresponding effects of proximity via the supply-chain network are small and insignificant. Thus, while shocks propagate through both networks, the severity of the impacts are larger when shocks transmit through labor-market networks. This result underscores the importance of distinguishing between networks and suggests estimates that consider supply-chain networks alone may be lower bounds.

Why do we observe greater propagation via labor networks? A possible explanation is that, although the absolute effect of exposure via supply-chain networks on input/output transactions is similar to the effect through labor-market networks on labor transactions, the effect on labor-market transactions is larger in relative terms. Labor-market transactions fall by 44.6% relative to the pre-period level, while the decline in transactions of inputs and final goods represents 23% of the pre-period mean. It may also be more difficult for households to adjust along the intensive margin: in the goods market, fewer but larger within-village transactions or more out-of-village transactions may substitute for the loss of transactions with the shocked household. In the labor market, working more hours for other employers may be more difficult, due to finite time in the day, travel costs, etc. In addition, as shocks reverberate through the networks, the demand for labor at the local level reduces and so does the local availability of jobs. Selling labor in other villages can be difficult, as migration can be costly (Bryan et al., 2014).

4.2.3 Robustness

The indirect effects are robust to a battery of alternative specifications. Appendix Table A8 shows results controlling for village-month fixed effects (columns 1 and 2), including shocks to unconnected households during the pre-period (columns 3 and 4), using an unbalanced panel of households to estimate indirect effects (columns 5 and 6), and excluding shocks to large firms to attenuate issues of granularity as in Gabaix (2011) (columns 7 and 8).[41] The results are also robust to utilizing alternative identification strategies (see Appendix Section B.3 for details). Columns 9 and 10 report estimates from a triple-difference specification using the placebo shocks from Section 3.1 as a control group.[42] Columns 11 and 12 show results from an alternative identification strategy that parallels our strategy for estimating the direct effects. In this alternative strategy, we compare households who are indirectly shocked for the first time at time t to a control group of households who will be indirectly exposed to a shock for the first time several years in the future. The results are remarkably similar to those from our main specification. Finally, Panel B of Appendix Table A6 shows that our main effects are qualitatively similar, albeit less precisely estimated, when we use alternative definitions of shocks.[43]

[41] We drop shocks to firms with revenues (over the 12 months preceding the shock) that are above the median revenues among shocked firms.

[42] In this case, we append data on households with different degrees of closeness to placebo shocks to our data set on indirectly shocked households and fully interact equation (4) with a Treatment/Placebo dummy. Columns 9 and 10 of Appendix Table A8 report the coefficient on the triple interaction ($Post \times Closeness \times Treatment$).

[43] This is largely due to the fact that more-stringent definitions of shock identify fewer shocks.

4.3 The multiplier effect of idiosyncratic shocks

What is the total magnitude of indirect effects relative to that of direct effects? The former are larger on a per-household basis, but the latter can potentially affect many more households. In order to compare their overall magnitudes, and so obtain an estimate of the overall "multiplier effect' of the fall in spending associated with the shock, we perform a back-of-the-envelope exercise to estimate the total indirect fall in consumption for each baht of reduced business spending by directly affected households.

Table 5 summarizes the key values. The indirect effect on consumption associated with a 1-unit change in *Closeness*, from column 5 in Panel A in Table 2, is a fall of -293 baht (significant at 10%). The median level of *Closeness* in the village network is 0.42 and the median number of indirectly exposed households (i.e., households who are connected to the shocked household via the network) is 23.[44] The implied total indirect effect using mean values is $-293 \times 0.42 \times 23 = -2830$ baht per month.

From column 5 in Panel A in Table 1, the fall in business costs for a directly affected household is -1653 baht, so the indirect effects using median closeness represent a multiplier effect of 1.71 (see column 1 in Panel C in Table 5). For comparison, Egger et al. (2021) estimate a consumption-expenditure multiplier of 2.4 from cash transfers in Kenya, while in the United States, Nakamura and Steinsson (2014) estimate an "open economy relative multiplier" of 1.5, Suárez Serrato and Wingender (2016) estimate a local income multiplier of government spending of 1.7 to 2, and Chodorow-Reich (2019) suggest a spending multiplier of 1.8 based on a survey of multiple studies.

[44] We prefer medians to means, because the median may be less sensitive to networks with a high number of connections or many distant (low-*Closeness*) connections, where the linear specification for *Closeness* may be less appropriate. However, in our data the median and mean are in practice very similar.

Barrot and Sauvagnat (2016) find that \$1 of lost sales at the supplier level leads to \$2.40 of lost sales at the customer level. These estimates are quite similar to ours despite their very different data and methods.

One key distinction is that we exploit within-village variation in exposure to shocks based on distance to the shocked household in the village network. Thus, our estimates of indirect effects are net of any changes in prices (which would not differ by network distance) and as such our multiplier estimate may be a lower bound; this is consistent with our estimate being at the lower end of the range of other recent estimates. While our multiplier estimates are admittedly back-of-the-envelope, they demonstrate that, because the indirect effects are economically meaningful and affect many households for each directly affected household. Thus, the total indirect effects are of a similar order of magnitude, and perhaps larger than, the direct effect itself.

5 Market failures and propagation

Thus far the paper has documented the existence of important indirect effects of idiosyncratic shocks. These effects may capture first-order reactions stemming from shocked households transacting less with other households and second-order effects due to market failures that amplify the indirect effects. Baqaee and Farhi (2020) note that, in the absence of market failures, there are no second-order propagation effects. In the no-distortion case, a sufficient statistic to compute the aggregate implications of idiosyncratic shocks is the shocked firm's market share (Hulten, 1978).

To test the role of market failures in propagation, we note that the direct effects correspond to a decline in business spending of 22.4% relative to the pre-shock mean. The indirect effects, evaluated at mean closeness, imply a decline of 1.68% relative to the pre-shock mean; this yields an elasticity of 0.074 (see Panel C of Table 5). We follow Baqaee and Farhi (2020) and compare this elasticity to the median market share of shocked firms, 0.028,[45] which suggests that the first-order effects account for less than 50% of the aggregate effects of idiosyncratic health shocks.[46]

5.1 Network structure and propagation

Our setting offers an additional test for the presence of market failures. If there were no market failures—i.e., no second-order propagation effects—the shocked firm's market share would be a sufficient statistic to compute the aggregate implications of idiosyncratic shocks. Then, after controlling for the shocked firm's market share, the magnitude of indirect effects would not depend on the characteristics of the shocked firm nor of the local economic environment. We test this implication by estimating alternative specifications which, instead of exploiting variation in closeness to the shocked household within the village, use cross-village variation in shock exposure along two dimensions. First, we use the shocked household's degree—the number of links that it had in the pre-period network, which is the number of households in the village who were exposed to the shock—as a measure of exposure. Second, to understand how the overall village-level structure matters for the extent of propagation, we exploit cross-village variation in network density (based on

[45] Following Hulten (1978), we compute a firm's market share to be the firm's non-labor revenues as a share of aggregate value added.

[46] At the 10% level, we are able to reject the null of an elasticity equal to the median market share against the alternative of an elasticity exceeding the median market share of shocked households (see Panel C of Table 5). Moreover, using the indirect effects on income evaluated at mean closeness (5% with respect to the pre-shock mean), we get a much larger elasticity (0.22), which strengthens our conclusion.

pre-shock transactions). We estimate the following model:

$$
\begin{aligned}
y_{i,t,j} = &\beta Post_{t,j} \times \text{Network Exposure}_j + \gamma Post_{t,j} \times \text{Market share}_j \\
&+ \mathbf{X}_{i,t,j}\kappa + \theta_{\tau(j)} + \alpha_i + \omega_j + \delta_t + \epsilon_{i,t,j}
\end{aligned} \tag{5}
$$

where, as above, the unit of observation is a household i in period t around the shock to household j; ω_j absorbs village-level variables that are invariant around the analysis window. Network Exposure$_j$ denotes exposure based on either the shocked household's degree or on network density during the pre-period. The vector X includes the interaction of the number of households in the village (number of nodes in the network) with $Post_{t,j}$ to account for potential contemporaneous shocks correlated with village size.

The results are reported in Table 6. Panel A indicates that a 1 standard deviation (SD) increase in the degree of the shocked household leads to an average of 0.035 (3.4%) fewer input/output transactions in the post-shock period relative to the pre-shock period, 0.045 (9.3%) fewer labor market transactions, and 0.08 (5.4%) fewer overall transactions (columns 1–3; all significant at 5% or better). Accordingly, a 1 SD increase in the degree of the shocked household leads to a differential fall in income of 277 THB (2.6%), significant at 1% (column 4). In column 5, the point estimate indicates a differential fall in consumption of 78 THB, or 1.1%, significant at 10%.

Panel B shows the results for variation by degree density. A 1 SD increase in the network's pre-shock degree density results in 0.054 (5.4%) fewer input/output transactions in the post-shock period, 0.025 (5.3%) fewer labor market transactions, and 0.078 (5.6%) fewer overall transactions (columns 1–3; all significant at the 1% level). The corresponding effect on income is a fall of 381 THB (3.6%) and the effect on consumption is a decline of 162 THB or

2.2% (columns 4 and 5). All of the results in Panel B are significant at 1%.

In summary, both the location of the shocked household within the network and the network's overall structure matter. Shocks to more-connected households lead to greater propagation as do shocks to denser networks, even after controlling for a household's market share in the local economy. These findings suggest an important trade-off: while increasing linkages among households may strengthen the insurance capacity of networks (Feigenberg et al., 2013), our results show that such increased links may also decrease resilience to propagation. Encouraging links with central households may promote information diffusion (Beaman et al., 2021), but may increase shock propagation as well. The results also demonstrate that both households and networks are heterogeneous in their propagation propensity. Understanding this variation can shed new light on sources of fragility.

Additionally, this dependence of propagation on attributes of shocked households and local network structure demonstrates that the shocked household's market share is not a sufficient statistic for aggregate effects, revealing the presence of distortion(s) in the economy. Specifically, Baqaee and Farhi (2020) show that, as long as the elasticities of substitution of firms in an economy are equal across firms, the propagation of shocks should not be related to network structure. The fact that propagation is larger in denser networks suggests that there is a large degree of heterogeneity in the ability of firms to substitute inputs amid a shock, and points out to the role of market failures preventing the mitigation of shocks (e.g., incomplete labor and insurance markets). We study the role of these frictions below.

5.2 Lack of smoothing and the propagation of shocks

Our setting enables us to analyze how a household's ability to smooth negative shocks is linked to the degree of propagation. The indirect effects of shocks stem from their direct effects: because the directly shocked household cannot perfectly smooth its shock, it reduces investment in its business. This failure of smoothing may result from failures in labor markets, financial/insurance markets, or both. We discuss each in turn.

5.2.1 Labor market incompleteness.

Frictions in the market for labor may drive the direct—and in turn, indirect—effects of shocks. The lost labor of an ill or injured household member (and of other household members taking care of them) may not easily be replaced by hired labor. Such frictions may arise from constraints in the ability to hire/ supply non-family labor (Jones et al., 2022). Indeed, we find declines in hired labor coupled with similar declines in labor provided by household members (columns 6 and 7 in Table 1) among directly shocked households, which are not offset with incoming free external labor (column 5, Panel A of Appendix Table A10). We confirm this result when we split the sample based on whether an adult of working age was directly affected by the shock.[47] Appendix Table A12 shows that when the shock hits working-age members—those more likely to operate businesses—there is a large decline in hired labor and a corresponding decline in business spending. This result suggests that there may be complementarities between labor provided by household members and hired

[47]We computed the age of affected household members during their actual and placebo shocks (Δ, periods away from the actual shock). We then separate households into two groups based on whether the shocked household member was an adult of working age (18 to 60 years old, based on Thailand's retirement age).

labor.

To understand whether labor market frictions mediate propagation, we split the sample by the degree of complementarity between household and hired labor, measured as the pre-shock co-movement between the idiosyncratic component of labor provided by household members (h) and labor hired externally (l). Let $c_i^{h,l}$ denote this co-movement. If labor-market frictions play a role, we expect to see larger effects for those households with above-median $c_i^{h,l}$ (see Appendix Section B.5 for details).

Columns 1–3 of Table A9, Panel A present the results. The severity of the shock, measured as spending on health, is not different for high- vs. low-$c_i^{h,l}$ households (column 1). The fall in hired labor is over 3 times larger for high-$c^{h,l}$ households (column 2), although the difference is not precisely estimated ($p = .194$). However, the direct effects on business spending are remarkably similar (column 3). Thus, while labor-market frictions do seem to lead to more-severe direct effects on hired labor, they do not fully account for the declines in direct spending. Turning to indirect effects in Panel B, when a high-$c^{h,l}$ household is shocked, the indirect falls in transactions and consumption are larger (columns 1 and 3), but the fall in income (column 2) is if anything smaller. The results, in sum, suggest that while labor market frictions play a role in mediating shock propagation, other factors are at play as well.

5.2.2 Incomplete insurance markets.

Informal insurance can help to buffer health shocks (de Weerdt and Dercon, 2006); indeed, we document an increase in incoming gifts for directly shocked households in the first half year after the shock (see Appendix Figure A6).[48]

[48] This may raise the concern that the indirect effect on consumption (see Table 2, column 6) could be a consequence of a decline in cash on hand/liquidity arising from helping the

However, this increase does not fully make up for the sharp increase in health spending over the same time frame (see Figure 1b).[49] If incomplete access to insurance plays a role in propagation, shocks to un- or under-insured households may trigger larger declines in business activities and hence greater propagation. To test this idea, we split the sample of shocked households into those with high vs. low pre-shock engagement in informal insurance networks.[50]

Columns 4–6 in Panel A of Appendix Table A9 report direct effects on health spending, gift and loan receipt, and business spending by access to informal insurance. Column 4 shows that shocks to the two groups are of similar severity: the associated effects on health spending are almost identical. Yet the responses to the shock differ: column 5 shows that households with high access to informal insurance experience a significant increase in gifts and loans after a shock, while the effect is small and insignificant for low-access households. There are significant declines in input spending (column 6) for low-access households, whereas these declines are small and insignificant for better-insured households. These patterns suggest that incompleteness in local insurance markets may be a driver of non-separability of household spending and production decisions.[51]

Next, to investigate whether shocks to less-insured households propagate

directly shocked household. Appendix Table A11 shows that neither transfers nor loans given by the indirectly shocked household to other households increase following the shock, which suggests that households engage in risk sharing with households whose economic activities are ex-ante unrelated. Moreover, the evidence below that *better-insured* shocks propagate *less* helps to rule out this concern.

[49]The incoming gifts account for roughly two-thirds of the increase in health spending during the first post-shock half year.

[50]Engagement in insurance is measured by pre-shock co-movements between the idiosyncratic component of asset returns and net gifts (Samphantharak and Townsend, 2018). See Appendix Section B.5 for details.

[51]Smoothing shocks via financial markets other than insurance, via changes in financial or tangible assets is also possible. However, we find little evidence of adjustment along these margins; see Appendix section B.4.

differently, we estimate the effect of *indirect* exposure to shocks. As above, we allow the effect to differ by whether the *directly shocked* household has a high vs. low level of insurance. Panel B of Table A9 presents the results. The effect on transactions is similar across the two samples (column 4), but when the shocked household had low access to insurance in the pre-period, the fall in income associated with 1 unit greater *Closeness* is approximately 60% larger (column 5). In turn, the consumption of indirectly affected households falls by 2.5 times more when exposed to an uninsured shock (column 6).[52] In sum, although differences across shocks to households with high and low access to informal insurance cannot be estimated with precision, the magnitudes suggest that incompleteness in informal insurance markets plays a role in the direct and indirect effects of idiosyncratic shocks.[53] In sum, different networks play different roles: shocks propagate through labor and production networks and are mitigated through risk-sharing networks.

5.3 Rigidities in local networks

The presence of frictions affecting directly shocked households, discussed above, plays a key role in propagation. However, an additional condition for shocks to propagate is that the indirectly affected households not be able to frictionlessly shift to new transaction partners.[54]

[52] This decline is consistent with the finding of no impact on incoming gifts among indirectly shocked households, possibly because, as idiosyncratic shocks become aggregate, the effectiveness of local insurance networks declines. See Appendix section B.4.

[53] While we cannot reject equality of effects on business spending by access to insurance at conventional levels (p-value=0.102), we are able to reject the null of equal or lower effects for households with access to insurance at 10% (p-value=0.051). Likewise we are able to reject the null of smaller indirect effects on consumption associated to shocks to insured households at 10% (p-value=0.093).

[54] Evidence from other contexts suggests market frictions may limit transactions across businesses. For instance, Johnson et al. (2002) highlights the role of limited legal enforcement. Other frictions may stem from product specificity (Barrot and Sauvagnat, 2016),

Frictions in rewiring economic networks may lead to a large degree of persistence. To test for rigidities in the local networks, we construct a dyadic data set that includes indicators of whether each pair of sample households (dyads) transacted in year t either in the local goods, labor, or financial market and estimate the extent to which past transactions predict future transactions, conditional on measures of similarity and connections based on kinship networks at baseline. (See Appendix B.6 for details.) Appendix Table A13 presents the results, showing that labor-market and supply-chain networks exhibit a striking degree of rigidity over time. For instance, column 8 shows that dyads linked through the labor-market network at period $t-1$ are 33 percentage points more likely to transact in period t, relative to those who did not transact in $t-1$. This level of persistence is an order of magnitude above the probability that two randomly chosen nodes in the network transact in a given year in the labor market (0.061) or supply chain (0.051); the persistence in the supply-chain and labor-market networks is also greater than that seen in the gift and loan network (columns 9–12).[55]

6 Concluding remarks

Local networks among households are understood to help to smooth consumption in the face of risks that households face directly (health, income, etc). Yet such insurance networks are imperfect and not everyone participates. Because many households in low- and low-middle-income countries are also en-

relationship specificity (Elliott, 2015), and market power (Grant and Startz, 2022).

[55]One implication is that the indirect effects of shocks that propagate via these networks may be quite persistent. Indeed, Figure A7 reports event-study estimates of equation 3 over a larger post-period time span of 4 years (8 half years). It suggests that the network disruptions induced by the shocks are persistent in both supply-chain (Panel A) and labor-market (Panel B) networks, showing no evidence of dissipating even 4 years post shock.

trepreneurs, they are also embedded in another set of local networks, in supply chains and in labor markets. These networks may *increase* exposure to shocks faced by local trading partners. We leverage variation in the timing of health shocks to quantify the interaction across these networks in mitigating and propagating risk.

Health shocks put substantial pressure on household budgets and labor endowments. As a result, under-insured shocked households adjust production: they draw down working capital, cut input spending, and reduce labor hiring. These adjustments propagate the shock to other households through interlinkages in local supply-chain and labor-market networks. The aggregate indirect effects imply a consumption multiplier of approximately 1.7.

We provide evidence that both direct and indirect effects are mitigated by access to local risk-sharing networks. Because such access is heterogeneous, shocks to underinsured households propagate. Network structure also shapes propagation, with shocks in denser networks propagating more. These findings are relevant in their own right and moreover serve as an omnibus test of misallocation: in an efficient economy the shocked firm's share of village value added would be a sufficient statistic for the extent of aggregate impacts (Hulten, 1978). That is not the case here. Small players, who are nevertheless central in networks, can be a risk factor for the village economy, creating *de facto* aggregate shocks.

Our findings suggest interventions that may be beneficial. First, investing in preventative heath to reduce the severity of shocks benefits not only the household whose health improves, but also others in the local network. In addition, improved safety nets may help prevent granular shocks from propagating via the incomplete insurance channel. Given that the ability to buffer idiosyncratic shocks increases with the number of households participating in the insurance network, limited local networks alone may be unable to diversify severe idiosyncratic risk. Formal commercial insurance contracts or social insurance could enable better risk coping and thus reduce propagation. Electronic payment platforms that identify key players in network structure could allow insurers to better target recipients who are key nodes in networks.

Because fully insuring against all idiosyncratic shocks is infeasible, policy interventions should also strive to improve the functioning of local labor markets and to make production networks less rigid and more diversified. Interventions to improve contract enforcement (Fazio et al., 2020) or to broaden the extent of product and factor markets beyond the local village market (Park et al., 2021) may reduce the rigidity and sparsity of supply-chain and labor-market networks and hence mitigate propagation and persistent adverse impact.

References

Aaronson, D., R. W. Bostic, P. Huck, and R. Townsend (2004). Supplier relationships and small business use of trade credit. *Journal of Urban Economics 55*(1), 46 – 67.

Ahlin, C. and R. M. Townsend (2007). Using repayment data to test across models of joint liability lending. *The Economic Journal 117*(517), F11–F51.

Allen, F. and D. Gale (2000). Financial contagion. *Journal of Political Economy 108*(1), 1–33.

Angelucci, M. and G. De Giorgi (2009). Indirect effects of an aid program: how do cash transfers affect ineligibles' consumption? *American Economic Review 99*(1), 486–508.

Baker, A. C., D. F. Larcker, and C. C. Wang (2022). How much should we trust staggered difference-in-differences estimates? *Journal of Financial Economics 144*(2), 370–395.

Banerjee, A., A. G. Chandrasekhar, E. Duflo, and M. O. Jackson (2013). The diffusion of microfinance. *Science 341*(6144).

Baqaee, D. R. and E. Farhi (2020). Productivity and misallocation in general equilibrium. *The Quarterly Journal of Economics 135*(1), 105–163.

Barrot, J.-N. and J. Sauvagnat (2016). Input Specificity and the Propagation of Idiosyncratic Shocks in Production Networks. *The Quarterly Journal of Economics 131*(3), 1543–1592.

Beaman, L., A. BenYishay, J. Magruder, and A. M. Mobarak (2021). Can network theory-based targeting increase technology adoption? *American Economic Review 111*(6).

Benjamin, D. (1992). Household composition, labor markets, and labor demand: Testing for separation in agricultural household models. *Econometrica 60*(2), 287–322.

Bigio, S. and J. La'o (2020). Distortions in production networks. *The Quarterly Journal of Economics 135*(4).

Bryan, G., S. Chowdhury, and A. M. Mobarak (2014). Underinvestment in a profitable technology: The case of seasonal migration in bangladesh. *Econometrica 82*(5), 1671–1748.

Burke, M., L. F. Bergquist, and E. Miguel (2019). Sell low and buy high: arbitrage and local price effects in Kenyan markets. *The Quarterly Journal of Economics 134*(2), 785–842.

Caliendo, L., F. Parro, E. Rossi-Hansberg, and P.-D. Sarte (2017, 12). The Impact of Regional and Sectoral Productivity Changes on the U.S. Economy. *The Review of Economic Studies 85*(4), 2042–2096.

Callaway, B. and P. H. Sant'Anna (2021). Difference-in-differences with multiple time periods. *Journal of Econometrics 225*(2), 200–230.

Carvalho, V. M., M. Nirei, Y. U. Saito, and A. Tahbaz-Salehi (2021). Supply chain disruptions: Evidence from the Great East Japan earthquake. *The Quarterly Journal of Economics 136*(2).

Chandrasekhar, A. and R. Lewis (2016). The econometrics of sampled networks. Working paper, Stanford University.

Chetty, R. and A. Looney (2006). Consumption smoothing and the welfare consequences of social insurance in developing economies. *Journal of public economics 90*(12), 2351–2356.

Chodorow-Reich, G. (2019, May). Geographic cross-sectional fiscal spending multipliers: What have we learned? *American Economic Journal: Economic Policy 11*(2), 1–34.

Cunha, J. M., G. De Giorgi, and S. Jayachandran (2019). The price effects of cash versus in-kind transfers. *The Review of Economic Studies 86*(1).

de Weerdt, J. and S. Dercon (2006). Risk-sharing networks and insurance against illness. *Journal of development Economics 81*(2), 337–356.

Dhyne, E., A. K. Kikkawa, M. Mogstad, and F. Tintelnot (2021). Trade and

domestic production networks. *The Review of Economic Studies 88*(2).
Egger, D., J. Haushofer, E. Miguel, P. Niehaus, and M. Walker (2021). General equilibrium effects of cash transfers: experimental evidence from Kenya. *Econometrica*.
Elliott, M. (2015). Inefficiencies in networked markets. *American Economic Journal: Microeconomics 7*(4).
Elliott, M., B. Golub, and M. O. Jackson (2014). Financial networks and contagion. *American Economic Review 104*(10).
Fadlon, I. and T. H. Nielsen (2019, September). Family health behaviors. *American Economic Review 109*(9), 3162–91.
Fazio, D., T. Silva, J. Skrastins, et al. (2020). Economic resilience: spillovers, courts, and vertical integration. Technical report.
Feigenberg, B., E. Field, and R. Pande (2013). The economic returns to social interaction: Experimental evidence from microfinance. *Review of Economic Studies 80*(4), 1459–1483.
Franklin, S., C. Imbert, G. Abebe, and C. Mejia-Mantilla (2021). Urban public works in spatial equilibrium: Experimental evidence from Ethiopia.
Gabaix, X. (2011). The granular origins of aggregate fluctuations. *Econometrica 79*(3), 733–772.
Genoni, M. E. (2012). Health shocks and consumption smoothing: Evidence from indonesia. *Economic Development and Cultural Change 60*(3).
Gertler, P. and J. Gruber (2002). Insuring consumption against illness. *The American Economic Review 92*(1), 51–70.
Goodman-Bacon, A. (2018). Difference-in-differences with variation in treatment timing. Technical report, National Bureau of Economic Research.
Grant, M. and M. Startz (2022). Cutting out the middleman: The structure of chains of intermediation. *NBER Working Paper 30109*.
Guren, A., A. McKay, E. Nakamura, and J. Steinsson (2021). What do we learn from cross-regional empirical estimates in macroeconomics? *NBER Macroeconomics Annual 35*(1), 175–223.
Hendren, N., A. Shenoy, and R. Townsend (2018). Household responses to negative health shocks in thailand. Mimeo.
Hulten, C. R. (1978). Growth accounting with intermediate inputs. *The Review of Economic Studies 45*(3), 511–518.
Huneeus, F. (2019). Production network dynamics and the propagation of shocks. Working paper.
Jarosch, G. (2021, February). Searching for job security and the consequences of job loss. Working Paper 28481, National Bureau of Economic Research.
Johnson, S., J. McMillan, and C. Woodruff (2002, April). Courts and Relational Contracts. *Journal of Law, Economics, and Organization 18*(1).

Jones, M., F. Kondylis, J. Loeser, and J. Magruder (2022). Factor market failures and the adoption of irrigation in Rwanda. *American Economic Review 112*(7).

Kinnan, C. and R. Townsend (2012). Kinship and financial networks, formal financial access, and risk reduction. *American Economic Review 102*(3).

LaFave, D. and D. Thomas (2016). Farms, families, and markets: New evidence on completeness of markets in agricultural settings. *Econometrica 84*(5), 1917–1960.

Moscona, J. and A. A. Seck (2021). Social structure and redistribution: Evidence from age set organization.

Munshi, K. (2014, November). Community networks and the process of development. *Journal of Economic Perspectives 28*(4), 49–76.

Nakamura, E. and J. Steinsson (2014). Fiscal stimulus in a monetary union: Evidence from US regions. *American Economic Review 104*(3), 753–92.

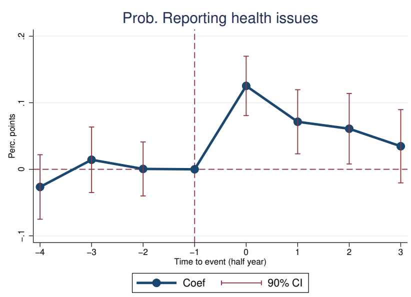
Park, S., Z. Yuan, and H. Zhang (2021). Technology adoption and quality upgrading in agricultural supply chains: A field experiment in Vietnam.

Samphantharak, K. and R. M. Townsend (2010, December). *Households as Corporate Firms*. Cambridge University Press.

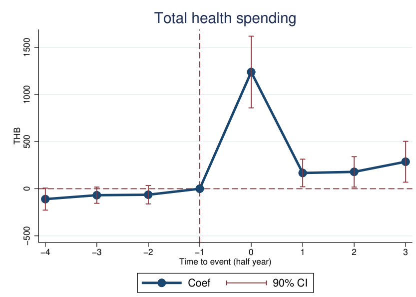
Samphantharak, K. and R. M. Townsend (2018, February). Risk and return in village economies. *American Economic Journal: Microeconomics 10*(1).

Suárez Serrato, J. C. and P. Wingender (2016, July). Estimating local fiscal multipliers. Working Paper 22425, National Bureau of Economic Research.

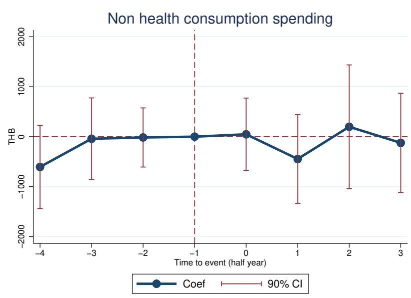
Figures and Tables



(a) Prob. of reporting any symptoms

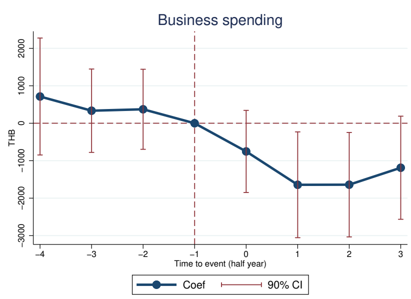


(b) Total health spending

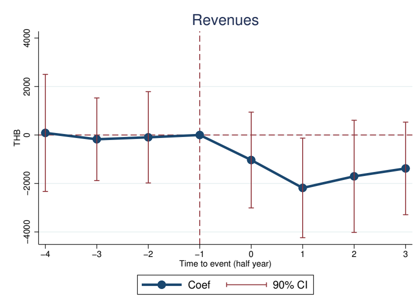


(c) Non health consumption spending

(d) Hired labor (hrs/month)



(e) Business spending (inputs)



(f) Revenues

Figure 1: 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% confidence intervals are computed using standard errors clustered at the household level. Costs and revenues exclude costs and earnings associated with the provision of labor to other households or firms. All variables measured in THB are winsorized with respect to the 99% percentile.

Table 1: Effects on spending and family businesses

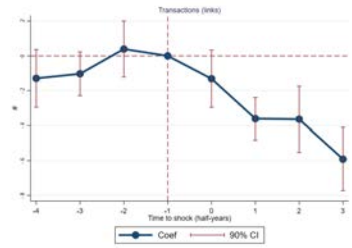
Panel A: Using shocks occurring in the first half of the sample.

	(1) Any health issues	(2) Health Spending	(3) Non health spending	(4) Total Spending	(5) Biz. Spending	(6) Hired labor (Hrs/Month)	(7) HH Labor (Hrs/Month)	(8) Revenues
Post X Treatment	0.0765	536.1	82.83	619.0	-1653.5	-14.66	-10.90	-1531.5
	(0.0231)	(91.62)	(339.4)	(363.6)	(797.5)	(7.730)	(8.608)	(1018.6)
Baseline mean (DV)	0.361	152.4	5277.6	5430.0	7333.5	17.73	151.8	14335.4
Observations	22804	22805	22805	22805	22805	22805	22805	22805
Number of events	247	247	247	247	247	247	247	247
Adj. R-Squared	0.230	0.0489	0.147	0.154	0.790	0.601	0.714	0.644

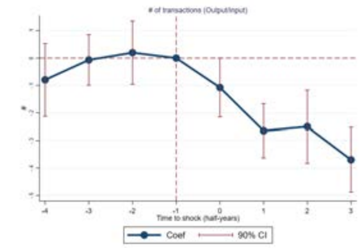
Panel B: Using all shocks

	(1) Any health issues	(2) Health Spending	(3) Non health spending	(4) Total Spending	(5) Biz. Spending	(6) Hired labor (Hrs/Month)	(7) HH Labor (Hrs/Month)	(8) Revenues
Post X Treatment	0.0857	411.6	240.7	652.2	-1342.0	-9.934	-15.13	-1774.0
	(0.0167)	(61.47)	(336.3)	(342.0)	(514.3)	(4.937)	(6.485)	(666.9)
Baseline mean (DV)	0.344	157.8	5767.7	5925.6	7194.1	15.92	140.2	14348.4
Observations	43244	43246	43246	43246	43246	43246	43246	43246
Number of events	470	470	470	470	470	470	470	470
Adj. R-Squared	0.225	0.0438	0.0985	0.107	0.759	0.684	0.657	0.571

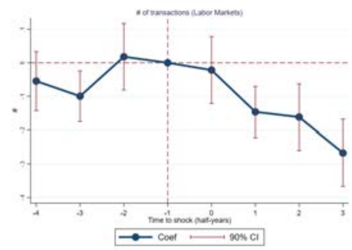
Note: The Table reports estimates of β from equation (2) for different outcomes. Each column reports differences between treatment and control households in changes in outcomes before and after the shock. All regressions control for household demographic characteristics, household and month fixed effects. Standard errors are clustered at the household level. Costs, labor, and revenues are aggregated across all businesses operated by household members, and exclude revenues and costs of wage labor provision to other businesses or households. Hired labor and labor provided by household members are measured in hours/month.



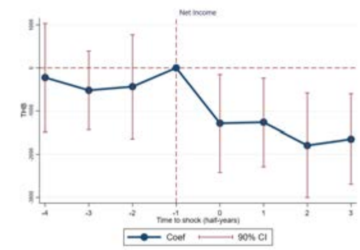
(a) Total transactions



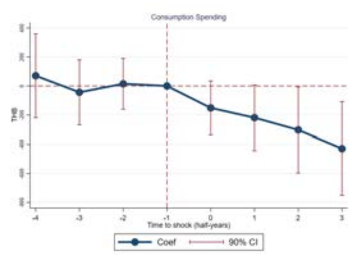
(b) Supply-chain (sales) network transactions



(c) Labor network transactions



(d) Total income



(e) Consumption Spending

Figure 2: Indirect effects on transactions, income and consumption

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). All variables measured in THB are winsorized with respect to the 99% percentile.

Table 2: Propagation of idiosyncratic shocks

Propagation through village networks

	(1) Input/Output	(2) Hired labor	(3) All transactions	(4) Income	(5) Total spending
Post X closeness (village network)	-0.20	-0.11	-0.31	-1,232	-293
	(0.06)	(0.04)	(0.08)	(422)	(156)
Observations	411,535	411,535	411,535	411,535	411,535
R-squared	0.44	0.23	0.37	0.20	0.64
Pre-period Mean	0.997	0.469	1.466	10301	7220
Number of events	391	391	391	391	391

Note: 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, and month fixed effects, 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 3: Propagation effects on outgoing and incoming transactions

	(1)	(2)	(3)	(4)	(5)	(6)
	Input/output		Labor		Total transactions	
	Outgoing	Incoming	Outgoing	Incoming	Outgoing	Incoming
Post X closeness (village network)	-0.08	-0.12	-0.09	-0.03	-0.16	-0.15
	(0.04)	(0.03)	(0.03)	(0.03)	(0.05)	(0.04)
Observations	411,535	411,535	411,535	411,535	411,535	411,535
R-squared	0.53	0.27	0.15	0.22	0.44	0.25
Pre-period Mean	0.496	0.500	0.181	0.288	0.678	0.788
Number of events	391	391	391	391	391	391

Note: 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, and 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 4: Propagation of idiosyncratic shocks through supply-chain and labor-market networks

	(1) Input/Output	(2) Hired Labor	(3) All transactions	(4) Total Income	(5) Total Spending
Post X closeness (supply-chain network)	-0.23	0.02	-0.20	-107	57
	(0.07)	(0.04)	(0.08)	(468)	(173)
Post X closeness (labor-market network)	-0.03	-0.21	-0.24	-1,283	-477
	(0.07)	(0.04)	(0.08)	(426)	(157)
Observations	411,535	411,535	411,535	411,535	411,535
R-squared	0.44	0.23	0.37	0.20	0.64
Pre-period Mean	0.997	0.469	1.466	10301	7220
Number of events	391	391	391	391	391

Note: The Table presents estimates of β from equation a variation of (4) where $Closeness_{i,j}$ denotes inverse distance to the shocked household during the year preceding the shock to j, by type of network. Each coefficient captures differences in changes in outcomes before and after the shock between more- and less-exposed households, through supply-chain and labor-market networks. Each regression 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. Standard errors are two-way clustered at the household (i) and event (j) level.

Table 5: Multiplier effects

Panel A: Pre-shock characteristics			
		Median	
Market share (shocked household)		0.028	
Avg. closeness (shocked household)		0.421	
# of indirectly exposed households (pre-shock)		23	
Panel B: Treatment effects			
		Point estimate	
Direct effects on business spending (THB)		-1653	
Direct effects on business spending (% relative to pre-shock mean)		-22.5	
Indirect effects on consumption spending (THB)		-293	
Indirect effects on consumption spending at mean closeness (THB)		-123	
Indirect effects on consumption spending (% of pre-shock means)		-1.68	
Panel C: Aggregate effects			
	Estimate	95% CI	90% CI
Multiplier	1.71	[0.245, 7.866]	[0.470, 5.569]
Elasticity	0.074	[0.011, 0.322]	[0.021, 0.241]
Elasticity>Median Market share (p−value)	0.086		

Note: Panel A reports medians across shocks. Median market share is computed as the ratio of a shocked household's total revenues during the year preceding its shock (excluding labor income) divided by village aggregate value added measured during the same period as in (Hulten, 1978). Avg. Closeness and the # of indirectly exposed households are computed across all non-shocked households in the same village of a shocked household. Households who suffer a direct shock themselves within a year of the indirect shock are excluded from the calculations. Panel B reports direct and indirect treatment effects based on column 5 of Table 1 and column 5 in Table 2. Panel C reports back-of-the-envelope calculations. Confidence intervals are based on percentiles of 500 bootstrap replications.

Table 6: Propagation and network characteristics

Panel A:Village-level variation in degree of shocked household

	(1) Input/Output	(2) Hired labor	(3) All transactions	(4) Income	(5) Consumption
Post X Degree (z-score)	-0.035	-0.045	-0.080	-277.522	-78.466
	(0.014)	(0.015)	(0.022)	(87.554)	(40.804)
Observations	452,115	452,115	452,115	452,115	452,115
R-squared	0.423	0.209	0.355	0.203	0.627
Pre-period Mean	0.996	0.467	1.463	10321	7234
Number of events	389	389	389	389	389

Panel B: Village-level variation in pre-period network density

	(1) Input/Output	(2) Hired labor	(3) All transactions	(4) Income	(5) Consumption
Post X Density (z-score)	-0.054	-0.025	-0.078	-381.301	-162.741
	(0.015)	(0.015)	(0.023)	(96.880)	(34.763)
Observations	452,115	452,115	452,115	452,115	452,115
R-squared	0.423	0.209	0.355	0.203	0.627
Pre-period Mean	0.996	0.467	1.463	10321	7234
Number of events	389	389	389	389	389

Note: Panels A and B report results corresponding to equation (5) using degree centrality of the shocked household and network density as proxies of village-level exposure to shocks, respectively. All regressions include interactions of the post-shock indicator with village size (number of households) and pre-shock Domar weights (market share) of the shocked household computed by dividing the shocked household gross revenues during the 12 months preceding the shock by the village-level aggregate value added during a similar time frame). Standard errors are clustered at the event level.

Online Appendix: Propagation and Insurance in Village Networks Online Appendix

A Supportive evidence

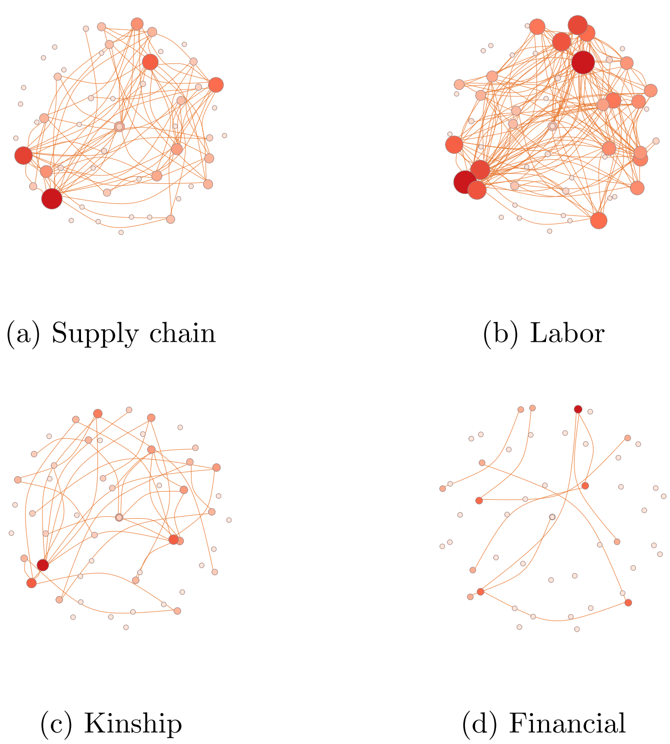


Figure A1: Socioeconomic Networks for a sample village

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.

Table A1: Summary statistics

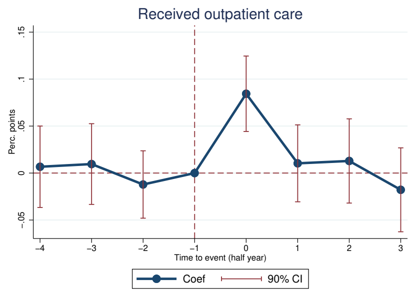
Panel A: Household baseline characteristics	N	Mean	S.D.	10th %ile	90th%ile
Number of household members	510	4.53	1.87	2	7
Number of adults	510	2.87	1.38	1	5
Age (household head)	508	52.00	13.49	35	70
Age (household average)	510	34.19	12.14	21	52
Household head is male	508	0.77	0.42	0	1
Years of schooling: 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)	N	Mean	S.D.	10th %ile	90th%ile
Net Income in THB:					
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
Consumption in THB					
Food	7650	32916.51	21912.78	11865	60521
Total consumption	7650	98030.54	99438.08	24189	204476
Household Assets and Debt					
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

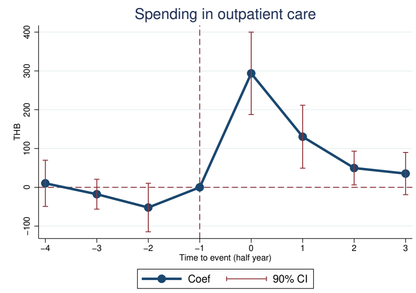
Panel C: Village networks	N	Mean	S.D.	10th %ile	90th%ile
Supply chain (sales) network: Degree (number of links)	7650	1.36	2.71	0	3
Supply chain (sales) network: Participation (any link)	7650	0.51	0.50	0	1
Labor-market network: Degree	7650	3.33	4.51	0	9
Labor-market network: Participation	7650	0.66	0.46	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	N	Mean	S.D.	10th %ile	90th%ile
Number of households in the village (Baseline census)	16	160.95	89.61	74	330
Village-level average firm size (based on annual gross revenues)	240	341049	397630	59966	620106
Village-level standard deviation of firm size (based on annual gross revenues)	240	618847	1452882	69877	1222209
Village-level kurtosis of average Village firm size (based on annual gross revenues)	240	10.13	5.92	4	19

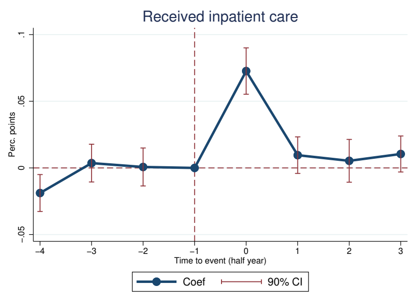
Note: Panel A reports summary statistics on demographic characteristics measured at baseline. Panel B reports household financial characteristics based on annual averages using a balanced panel of 509 households. Farm income includes income from agriculture, livestock, fish and shrimp. Off-farm income excludes earnings from labor provision. In both cases income is net of operation costs. Gifts and transfers include transactions from both households inside and outside the village, as well as receipt of government transfers. Consumption includes spending and consumption of home production. In Panel C, all networks are unvalued and undirected; all links have equal weight and the direction of the transaction is not considered. Kinship networks are measured at baseline; transaction networks are measured on an annual basis. Financial networks are constructed 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 relationships through paid and unpaid labor between households in the same village. Degree: Number of households with whom each household transacted in each year. Access: Takes the value of 1 if the household has participated in the network in a given year and 0 otherwise. Panel D, reports characteristics at the village level (16 villages). Firm-size statistics are computed at village-year level.



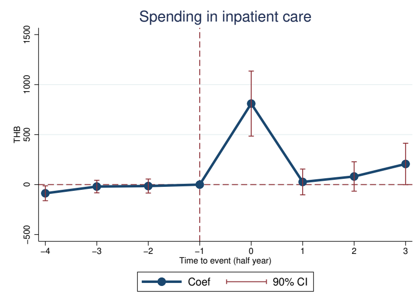
(a) Outpatient care (Prob.)



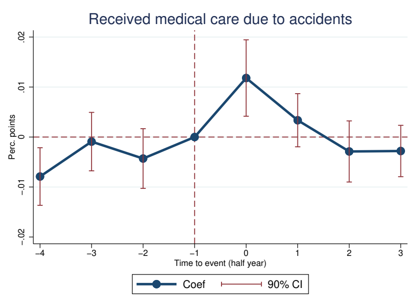
(b) Outpatient care (Spending)



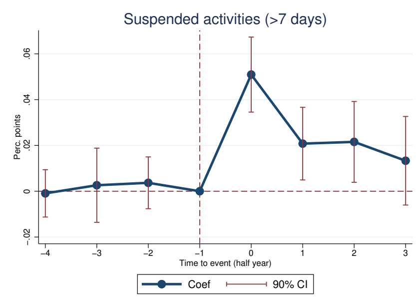
(c) Inpatient care (Prob.)



(d) Inpatient care (Spending)



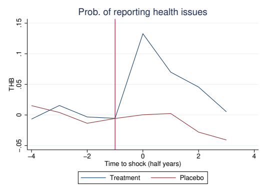
(e) Accidents (Prob.)



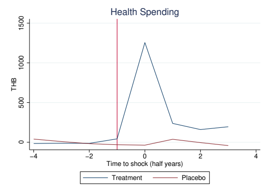
(f) Suspension of main activities (Prob.)

Figure A2: Direct effects of health shocks

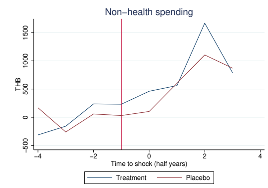
Note: Each dot represents differences between treatment and control households in changes in outcomes relative to the period preceding the beginning of the shock ($\tau = -1$).The estimating sample includes 2 years before and after the shock divided in half-year bins. All specifications control for household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



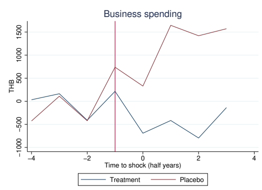
(a) Reported symptoms



(b) Health Spending

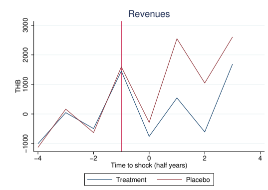


(c) Non-health Spending



(d) Business spending

(e) Hired labor (hrs/-month)



(f) Revenues

Figure A3: 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.

Table A2: Direct effects on housework

Panel A: Using shocks occurring during the first half of the sample

	(1)	(2)
	# of hh members	# of days
Post X Treatment	-0.0870	-3.326
	(0.0572)	(1.781)
Baseline mean (DV)	2.932	81.22
Observations	22805	22805
Number of events	247	247
Adj. R-Squared	0.801	0.775

Panel B: Using all shocks

	(1)	(2)
	# of hh members	# of days
Post X Treatment	-0.100	-3.344
	(0.0387)	(1.199)
Baseline mean (DV)	3.034	85.42
Observations	43246	43246
Number of events	470	470
Adj. R-Squared	0.780	0.757

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

	Max. Changes		Highest Spending (excluding small spending levels)		Health Spending >Avg. Food Spending		Health Spending¿mean + SD		Suspended activities > 7 days	
	(1) Health Spending	(2) Business Spending	(3) Health Spending	(4) Business Spending	(5) Health Spending	(6) Business Spending	(7) Health Spending	(8) Business Spending	(9) Health Spending	(10) Business Spending
Post X Treatment	467.4	-1662.5	664.6	-1896.8	829.5	-3669.1	719.7	-2947.2	398.9	-2267.4
	(81.02)	(780.7)	(114.6)	(897.6)	(143.9)	(1951.3)	(120.4)	(1535.8)	(121.7)	(1238.7)
Baseline mean (DV)	141.0	7032.4	169.0	8113.8	218.0	10387.8	228.9	9983.4	173.1	5759.5
Observations	22250	22250	20055	20055	7616	7616	8871	8871	10681	10681
Number of events	228	228	184	184	87	87	104	104	128	128
Adj. R-Squared	0.0610	0.803	0.0507	0.788	0.0500	0.819	0.0896	0.753	0.0792	0.836

Panel B: Indirect Effects

	Max. Changes		Highest Spending (excluding small spending levels)		Health Spending >Avg. Food Spending		Health Spending >mean + SD		Suspended activities >7 days	
	(1) # Transactions	(2) Income	(3) # Transactions	(4) Income	(5) # Transactions	(6) Income	(7) # Transactions	(8) Income	(9) # Transactions	(10) Income
Post X Closeness	-0.307	-1224.4	-0.350	-1200.2	-0.446	-1149.9	-0.230	-663.1	-0.351	-916.4
	(0.0804)	(415.2)	(0.0742)	(499.8)	(0.166)	(1104.2)	(0.107)	(932.3)	(0.0880)	(669.8)
Baseline mean (DV)	1.427	10381.4	1.249	11531.1	1.116	14158.0	1.062	14508.1	1.252	10780.3
Observations	408709	408709	303440	303440	68781	68781	105907	105907	127003	127003
Number of events	389	389	284	284	142	142	181	181	224	224
Adj. R-Squared	0.379	0.201	0.357	0.193	0.336	0.195	0.357	0.188	0.395	0.196

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. Columns 9 and 10 report results based on alternative shock definition based on the number of days that a household members suspended activities (>7 days). Standard errors in parentheses.

Table A4: Direct effects: allowing for multiple, non overlapping shocks per household.

	Shock (Spending>food spending)				Shock (Activities)			
	Single Shock		Multiple shocks		Single Shock		Multiple shocks	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Health spending	Business spending	Health spending	Business spending	Health spending	Business spending	Health spending	Business spending
Post X Treatment	829.5	-3669.1	975.4	-4367.8	398.9	-2267.4	294.1	-2111.1
	(143.9)	(1951.3)	(145.8)	(1743.8)	(121.7)	(1238.7)	(97.63)	(1020.8)
Baseline mean (DV)	218.0	10387.8	316.5	12423.8	173.1	5759.5	197.3	6000.6
Observations	7616	7616	12622	12622	10681	10681	11038	11038
Number of events	87	87	184	184	128	128	162	162
Adj. R-Squared	0.0500	0.819	0.123	0.778	0.0792	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 (6).

Table A5: Direct and indirect effects: Shocks based on suspended activities

Panel A: Direct Effects

	Suspension of activities due to sickness (household level)					
	At least one day		7 or more days		One sd above mean disruption time	
	(1) Health Spending	(2) Business Spending	(3) Health Spending	(4) Business Spending	(5) Health Spending	(6) Business Spending
PostXTreat	48.44	-1205.8	398.9	-2267.4	435.7	-1943.0
	(110.2)	(859.3)	(121.7)	(1238.7)	(141.1)	(1044.4)
Baseline mean (DV)	184.2	6099.3	173.1	5759.5	205.0	6120.6
Observations	13261	13261	10681	10681	9757	9757
Number of events	215	215	128	128	117	117
Adj. R-Squared	0.0442	0.743	0.0792	0.836	0.0767	0.847

Panel B: Indirect Effects

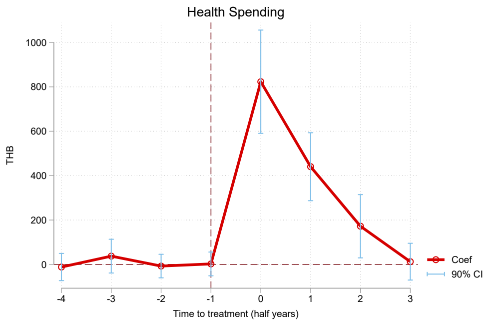
	Suspension of activities due to sickness (household level)					
	At least one day		7 or more days		One sd above mean disruption time	
	(1) # Transactions	(2) Income	(3) # Transactions	(4) Income	(5) # Transactions	(6) Income
Post X Closeness	-0.179	-753.3	-0.351	-916.4	-0.284	-593.7
	(0.0904)	(513.1)	(0.0880)	(669.8)	(0.0967)	(570.2)
Baseline mean (DV)	1.442	8914.7	1.252	10780.3	1.247	11189.6
Observations	222319	222319	127003	127003	108869	108869
Number of events	351	351	224	224	215	215
Adj. R-Squared	0.404	0.182	0.395	0.196	0.408	0.207

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.

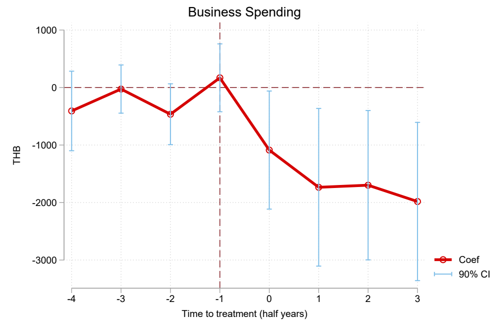
Table A6: Direct effects: Robustness to alternative control groups.

	Randomly selected placebo group		**Using not-yet-treated as controls**		**Using not currently treated as controls**		**Callaway & San't anna (2021)**		**Main spec. with unbalanced panel**	
	(1) Health Spending	(2) Biz. Spending	(3) Health Spending	(4) Biz. Spending	(5) Health Spending	(6) Biz. Spending	(7) Health Spending	(8) Biz. Spending	(9) Health Spending	(10) Biz. Spending
Treatment effect	467.1	-1016.5	416.6	-1383.4	844.8	-720.5	362.0	-1625.8	434.9	-1438.9
	(64.64)	(444.5)	(59.09)	(389.2)	(126.0)	(315.1)	(59.45)	(719.1)	(64.52)	(664.6)
Baseline mean (DV)	196.5	7542.0	140.8	6661.3	161.7	7626.1	68.52	4886.5	143.9	6796.4
Observations	43155	43155	132698	132698	21409	21409	N.A.	N.A.	26559	26559
Number of events	470	470	353	353	470	470	247	247	292	292
Adj. R-Squared	0.0590	0.787	0.0533	0.782	0.0247	0.793	N.A.	N.A.	0.0576	0.804

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 (7). 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 (8). 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. Columns 9 and 10 report 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). See Appendix Section B.2.1 for details. Standard errors in parentheses.



(a) Health Spending



(b) Business Spending

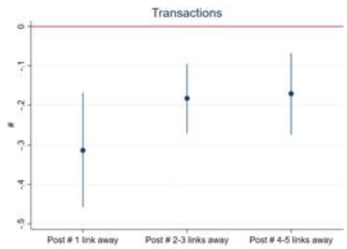
Figure A4: Event-study estimates using Callaway and Sant'Anna (2021)'s approach

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 household 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.

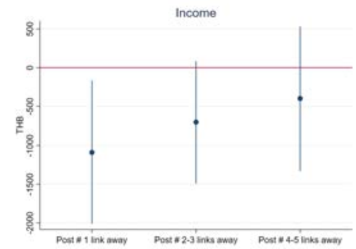
Table A7: Spending co-movements with health status

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Symptom - Health spending comovements							
	D.Health spending	D.Health spending	D.Health spending	D.Health spending	D.Health spending	D.Health spending	D.Health spending
D.Experienced any symptom	431.3		438.6		441.2		
	(44.99)		(46.31)		(44.90)		
D.Experienced uncommon symptoms		734.9		747.1		742.3	775.8
		(119.8)		(123.6)		(119.4)	(118.1)
DV mean (no symptoms)	-1.997	-0.574	-1.997	-0.574	-1.997	-0.574	-1.997
Observations	83266	83875	80647	81195	80647	81195	81195
Adj. R-Squared	0.00645	0.00504	0.00640	0.00499	0.00576	0.00444	0.00997
Panel B: Symptom - Business spending comovements							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	D.Business spending	D.Business spending	D.Business spending	D.Business spending	D.Business spending	D.Business spending	D.Business spending
D.Experienced any symptom	-118.9		-122.8		-81.49		
	(98.10)		(100.0)		(97.75)		
D.Experienced uncommon symptoms		-427.5		-433.4		-380.3	-413.2
		(203.7)		(208.8)		(207.0)	(207.4)
DV mean (no symptoms)	99.84	96.05	99.84	96.05	99.84	96.05	99.84
Observations	83266	83875	80647	81195	80647	81195	81195
Adj. R-Squared	0.0191	0.0199	0.0195	0.0205	0.0635	0.0635	0.0637
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

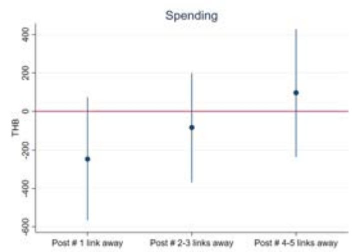
Note: The table reports co-movements between health status and spending. The estimates correspond to Gertler and Gruber (2002)'s specification: $\Delta Spending_{i,v,t} = \beta \Delta \text{Health Status}_{i,v,t} + \delta_{v,t} + \epsilon_{i,v,t}$. Where $\Delta X_{i,v,t}$ measures the changes in X between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



(a) Total transactions



(b) Total income



(c) Spending

Figure A5: Indirect effects by distance to shocked households

Note: The figure depicts indirect effects of the shocks based on distance to the shocked household in the pre-shock network. The coefficients correspond to a regression of the dependent variable on a Post shock indicator, distance-to-shocked household dummies, and interactions of the Post-shock indicator and the distance dummies. The base distance category is households that are more than 5 links away from the shocked households or that are unconnected to the shocked household. All regressions include household fixed effects, event fixed effects, month fixed effects, household size, household average age and education, the number of adult males and females in each household, and control for degree centrality interacted with month fixed effects. 95% confidence intervals are based on standard errors that are two-way clustered at the household (i) and shock level (j). All variables measured in THB are winsorized with respect to the 99% percentile.

Table A8: Indirect effects: Robustness to alternative specifications

	Diff-in-diff specification (Post X Closeness)								Triple Difference		Fadlon & Nielsen approach	
	Village-month FE		Unconnected households		Unbalanced panel		Only shocks to small firms		(Post X Closeness X Treatment)		(Post X Treatment)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES	Transactions	Income	Transactions	Income	Transactions	Income	Transactions	Income	Transactions	Income	Transactions	Income
Treatment effects	-0.19	-816	-0.18	-912	-0.24	-1,232	-0.24	-1,362	-0.18	-1,570	-0.22	-1,206
	(0.04)	(437)	(0.06)	(402)	(0.06)	(422)	(0.08)	(494)	(0.09)	(488)	(0.11)	(632)
Observations	411,535	411,535	445,621	445,621	411,535	411,535	195,822	195,822	832,155	832,155	20,730	20,716
R-squared	0.44	0.26	0.37	0.20	0.38	0.20	0.39	0.24	0.37	0.20	0.39	0.22
Pre-period Mean	0.936	10301	0.919	10571	0.709	9116	1.021	9119	1.389	10496	1.520	7202
Number of events	391	391	421	421	391	391	190	190	458	458	474	473

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. Columns 9 and 10 report triple difference estimates corresponding to equation (9) 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.3.1 for details). In this case, we winsorized the number of transactions corresponding to the supply-chain networks. Columns 11 and 12 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.3.2 for details). Standard errors are two-way clustered at the household (i) and event (j) level.

Figure A6: Incoming gifts

Note: The figure reports coefficients from equation 1 for incoming gifts/transfers. 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% confidence intervals are computed using standard errors clustered at the household level.

Table A9: Direct and indirect effects by household- and hired-labor complementarities and by engagement in risk-sharing networks

Panel A: Direct effects

	By hired-household labor complementarities			*By engagement in risk-sharing networks*		
	(1)	(2)	(3)	(4)	(5)	(6)
	Total hh spending in health	Hired Labor	Costs	Total hh spending in health	Gifts+Loans	Costs
(1) Low	383.7	-3.193	-1244.9	411.2	280.2	-2350.6
	(82.28)	(4.201)	(702.5)	(73.27)	(360.6)	(973.6)
(2) High	438.9	-16.48	-1435.0	416.2	736.9	-409.0
	(80.99)	(9.075)	(747.6)	(109.0)	(415.4)	(594.3)
Difference (2)-(1)	-55.15	13.29	190.1	-5.048	-456.7	-1941.6
S.E. Difference	107.5	10.22	1025.7	129.2	526.1	1186.0
P-value(H0:Difference=0)	0.608	0.194	0.853	0.969	0.386	0.102
P-value(H0:Difference≤0)	0.304	0.0972	0.427	0.484	0.193	0.0512
Baseline mean (DV)	157.8	15.92	7194.1	152.9	2906.3	7617.2
Observations	43246	43246	43246	39835	39835	39835
Adj.R-Squared	0.0438	0.684	0.759	0.0424	0.0463	0.762

Panel B: Indirect effects

	By hired-household labor complementarities			*By engagement in risk-sharing networks*		
	(1)	(2)	(3)	(4)	(5)	(6)
	Transactions	Income	Spending	Transactions	Income	Spending
(1) Low	-0.294	-1016.0	-201.5	-0.271	-1626.8	-457.6
	(0.0888)	(538.9)	(187.5)	(0.109)	(520.9)	(194.3)
(2) High	-0.330	-1437.0	-392.0	-0.379	-1038.8	-168.0
	(0.0995)	(500.8)	(186.4)	(0.0890)	(518.2)	(193.2)
Difference (2)-(1)	0.0363	420.9	190.5	0.108	-588.0	-289.6
S.E. Difference	0.106	615.8	204.0	0.123	576.9	218.9
P-value(H0:Difference=0)	0.733	0.495	0.351	0.380	0.309	0.187
P-value(H0:Difference≤0)	0.367	0.247	0.176	0.190	0.154	0.0934
Baseline mean (DV)	1.466	10301.5	7220.2	1.466	10301.5	7220.2
Observations	411535	411535	411535	383404	383404	383404
Adj.R-Squared	0.372	0.202	0.638	0.370	0.202	0.636

Note: Panel A reports estimates of β_1 and β_2 from equation (10) in section B.5. 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 (11). In columns 1 to 3, we split the sample by the degree of complementarity between household and hired labor, measured as the pre-shock co-movements between the idiosyncratic component of labor provided by household members and labor hired externally. In columns 4 to 6, we split the sample by access to informal insurance networks measured as pre-shock co-movements between the idiosyncratic component of returns over assets and net gifts, as in Samphantharak and Townsend (2018). In both cases "High" and "Low" refers to the directly-shocked household and is defined with respect to the median (above vs. below) of the pre-shock gift-returns co-movements and internal-external labor comovements. Standard errors are clustered at the household level.

Table A10: Response to shocks: coping mechanisms

Panel A: Direct effects

	(1) Gifts/Transfers	(2) Loans	(3) Fixed Assets	(4) Cash in Hand	(5) Unpaid labor (Hrs/Month)
Post X Treatment	560.7	70.40	-6053.9	-13452.2	1.832
	(214.4)	(239.4)	(5984.8)	(22948.1)	(1.557)
Baseline mean (DV)	1938.1	271.7	123322.2	368842.9	6.197
Observations	22805	22805	22805	22805	22805
Number of events	247	247	247	247	247
Adj. R-Squared	0.165	0.00972	0.892	0.882	0.212

Panel B: Indirect effects

	Gifts/Transfers	Loans	Fixed Assets	Cash in Hand	Unpaid labor (Hrs/Month)
Post X Closeness (village network)	-90.16	-99.89	-6430.7	-11242.7	-0.974
	(123.1)	(120.2)	(4482.9)	(20699.1)	(1.023)
Baseline mean (DV)	2274.0	67.80	124816.9	410614.0	5.841
Observations	411535	411535	411535	411535	411535
Number of events	391	391	391	391	391
Adj. R-Squared	0.141	0.0385	0.805	0.815	0.269

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 A11: Indirect effects of health shocks on gift/transfers to other households

	(1) # of gifts	(2) Gift ($ THB)	(3) Gift+Loans ($ THB)
Post X Closeness (village network)	-0.00602	-65.36	-90.7
	(0.00686)	(56.02)	(64.91)
Baseline mean (DV)	0.0287	905.2	1019.9
Observations	411535	411535	411535
Number of events	391	391	391
Adj. R-Squared	0.0633	0.302	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 A12: Heterogeneous direct effects by age of shocked household member

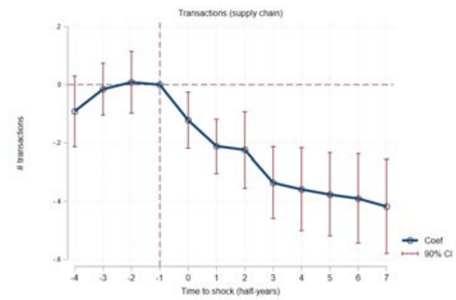
	(1) Health spending	(2) Business spending	(3) Hired labor (Hrs/Month)	(4) HH Labor (Hrs/Month)
Non working age	567.3	-700.0	0.365	-11.75
	(116.3)	(705.9)	(1.366)	(10.08)
Working age (18-60)	376.8	-1739.6	-19.23	-6.196
	(56.72)	(772.9)	(10.50)	(8.468)
Difference	190.5	1039.6	19.60	-5.551
S.E. Difference	129.1	1046.7	10.79	13.27
P-value Difference	0.141	0.321	0.0701	0.676
Baseline mean (DV)	154.8	7276.8	17.35	143.5
Observations	37100	37100	37100	37100
Adj.R-Squared	0.0427	0.773	0.691	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.

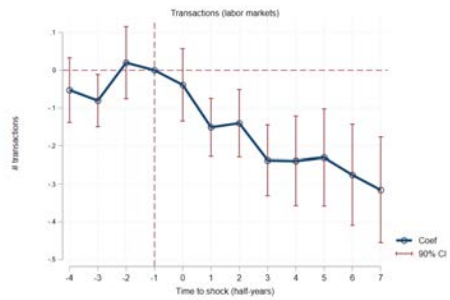
Table A13: Persistence in transaction networks, by network type

	Probability of a direct link at t											
	Supply chain				Labor markets				Gifts/loans			
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Lag Prob. of link at $t-1$ (ρ)	0.469	0.460	0.378	0.378	0.427	0.401	0.333	0.333	0.260	0.258	0.209	0.209
	(0.015)	(0.014)	(0.011)	(0.011)	(0.012)	(0.013)	(0.011)	(0.011)	(0.015)	(0.015)	(0.013)	(0.013)
Kinship connection			0.100	0.100			0.110	0.110			0.091	0.091
			(0.006)	(0.006)			(0.007)	(0.007)			(0.006)	(0.006)
Demographic (log euclidean distance)				-0.019				-0.112				0.138
				(0.119)				(0.130)				(0.071)
Net worth (log squared differences)				-0.037				-0.006				-0.035
				(0.027)				(0.031)				(0.017)
Mean DV	0.0508				0.0612				0.0122			
Observations	234,192	234,192	234,192	234,192	234,192	234,192	234,192	234,192	234,192	234,192	234,192	234,192
Adjusted R-squared	0.221	0.227	0.268	0.268	0.189	0.207	0.241	0.241	0.067	0.069	0.102	0.102
Village-Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Household i FE	NO	NO	YES	YES	NO	NO	YES	YES	NO	NO	YES	YES
Household j FE	NO	NO	YES	YES	NO	NO	YES	YES	NO	NO	YES	YES

Note: The table presents regression coefficients following the specification in equation (12). 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 1,5 and 9 present raw correlations, columns 2,6,and 10 include village-year fixed effects. Columns 3,7 and 11 control for kinship first-degree connections. Columns 4,8, and 12 also control 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 re-scaled 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.



(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). All variables measured in THB are winsorized with respect to the 99% percentile. We exclude shocks that occurred within 4 years of the end of the panel, to ensure a balanced panel throughout the analysis window.

B Identifying shocks and their effects

B.1 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 35 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 405 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 85 households for which we could not identify the beginning of the symptoms,[1]

[1] There were 19 households for which symptoms were repeatedly reported for two years

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 B13 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.

or more, and 68 households who lack information related to symptoms.

B.1.1 Characteristics of shocks

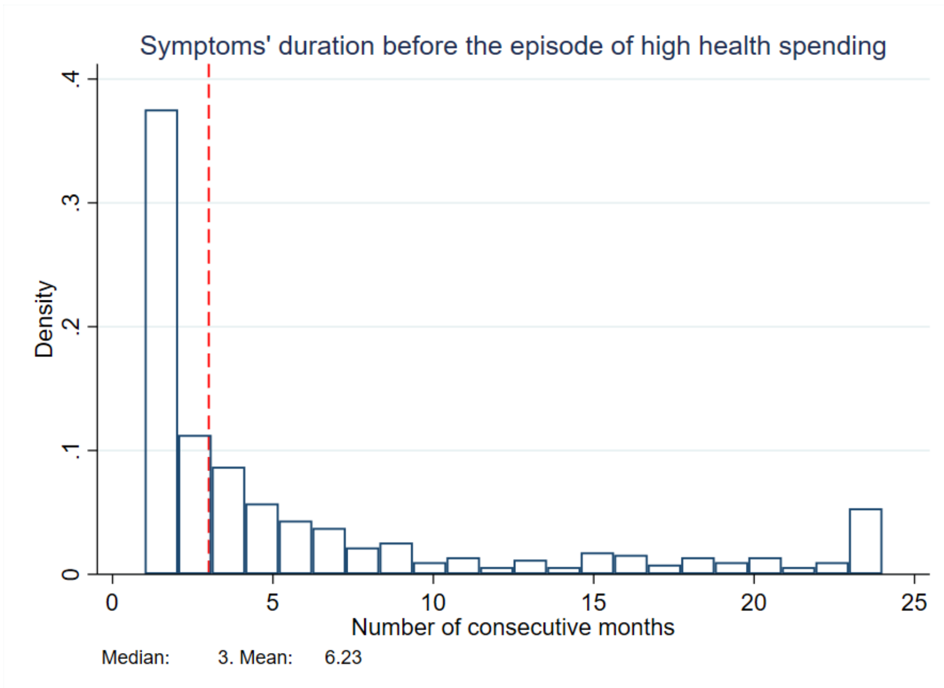


Figure B8: Distribution of symptom duration before the episodes of high health spending

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.

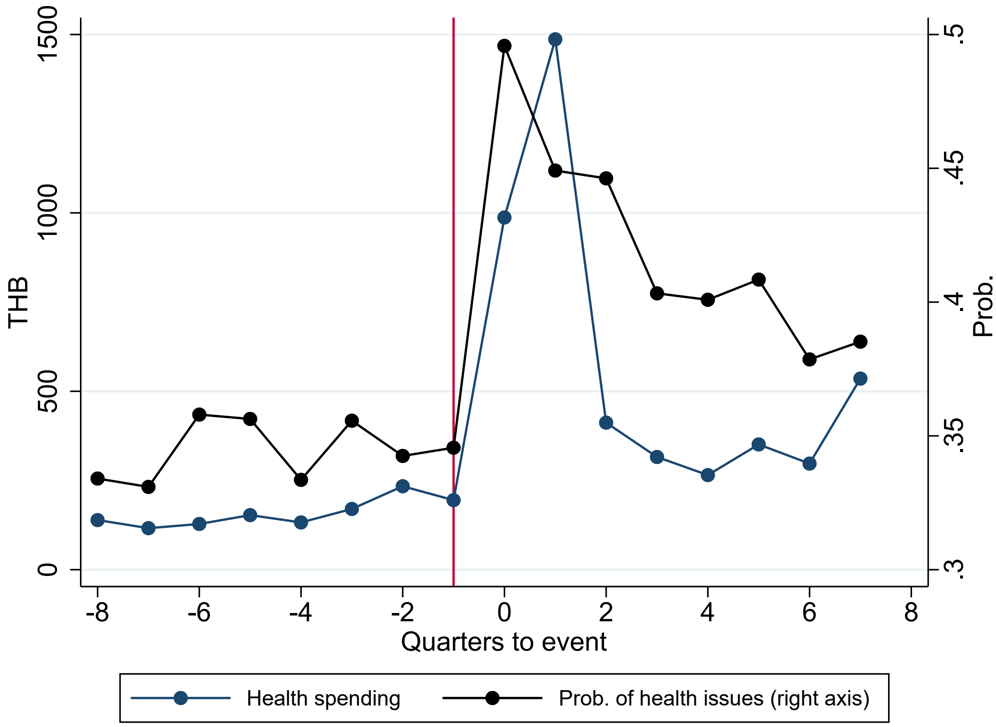


Figure B9: Health status and spending before and after health shocks.

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 of 505 households.

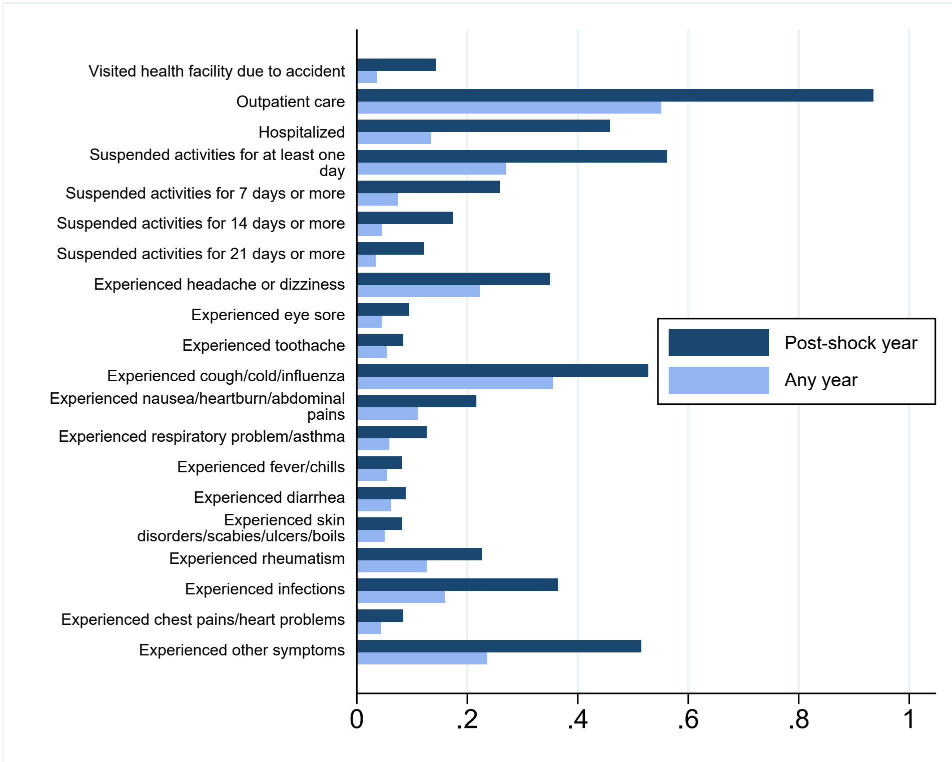
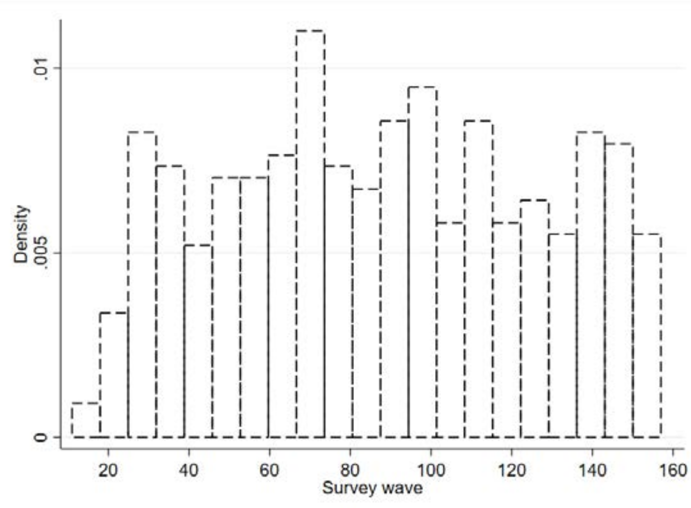
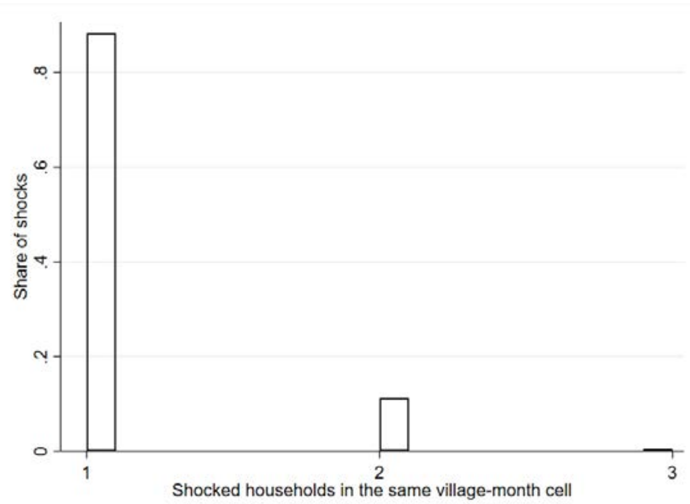


Figure B10: Incidence of health conditions during shock and off-shock periods.

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 B11: 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 B14: Timing of health shocks and village and household characteristics

	(1) Shock occurrence at t+1	(2) P-value (Granger causality - 12 lags)
Income	0.00201	0.130
	(0.00164)	
Business Reveneues	0.00325	0.665
	(0.00237)	
Business Spending	-0.00429	0.654
	(0.00356)	
Non health consumption	0.000310	0.810
	(0.00163)	
Health spending	-0.135	0.628
	(0.0763)	
Borrowing	0.00584	0.711
	(0.00599)	
Lending	-0.415	0.181
	(0.343)	
Incoming gifts	-0.00331	0.427
	(0.00412)	
Outgoing gifts	0.000849	0.957
	(0.0123)	
Livestock	-0.000660	0.0868
	(0.00103)	
Cash in hand	-0.0000856	0.514
	(0.0000702)	
Fixed assets	0.0000836	0.129
	(0.000289)	
Land	0.000138	0.578
	(0.0000942)	
Observations	83875	77755
Adj. R-Squared	-0.00458	
P-value (Joint significance)	0.267	
P-value (Hausman Test Village X month fixed effects)	0.392	

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. 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 B15: 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	6.55	0.21
Fish/Shrimp	1.13	0.02
Off-farm business	1.83	0.07
Housework	22.85	0.78
School or training	2.06	0.05
Positions in village organizations	0.15	0.00
Funerals/Weddings	0.56	0.00
Labor exchange outside home	0.02	0.00
Unpaid labor outside home	0.39	0.01
Paid labor outside home	3.94	0.12
Looking for a job	0.03	0.00
Sick	0.10	0.00

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.

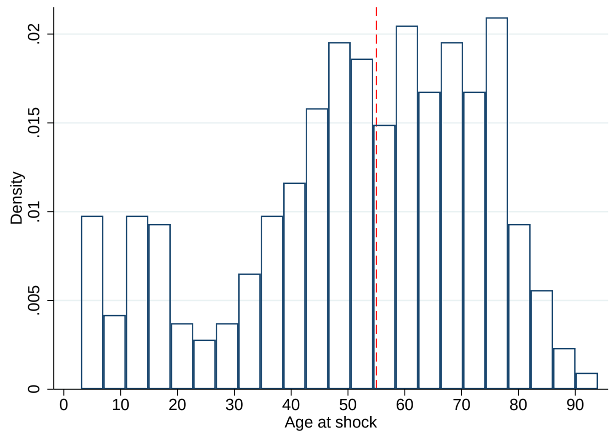


Figure B12: Age at shock

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.

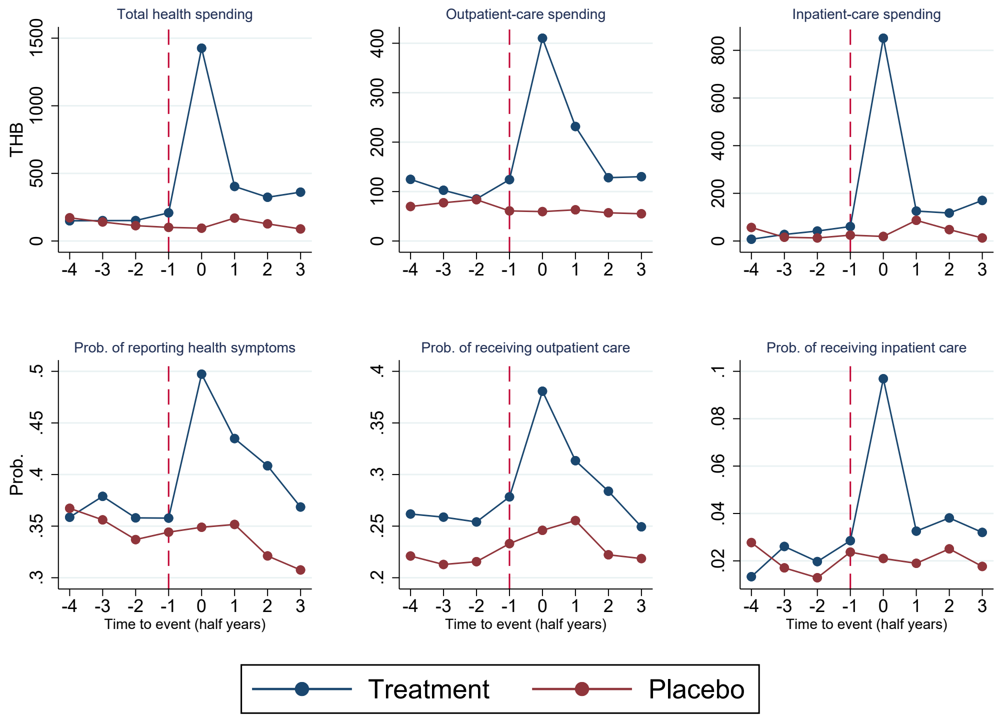


Figure B13: Health status and spending in the treatment and control samples

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 quarters.

B.2 Treatment and control groups for direct effects

We operationalize 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).[2] 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).[3] 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 B11), the placebo shocks occur within the domain of the actual shocks. As 243 out of 473 shocked households experienced a

[2]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.

[3]We define Δ as $\Delta = \frac{\bar{t} - \underline{t}}{2}$ for each age-group-village bin. On average, each sub-sample covers 66 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 $\bar{t} <= 148$.

shock in the earlier part of the panel, this process yields 243 households in the treatment group and 230 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 et al., 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 power, 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 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.

B.2.1 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 3 and shows results that are quantitatively similar, but estimated with higher precision since we now use 473 shocks as opposed to only 243, 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 142 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 suspended activities in the sample. Appendix Table A5 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:

$$y_{i(k),t} = \beta Post_{i(k),t} \times Treatment_{i(k)} + \theta Post_{i(k),t} + X_{i(k),t}\kappa + \alpha_i + \delta_t + \epsilon_{i(k),t} \tag{6}$$

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.[4] 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 A4 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 that only allow for one shock (the first) per household. As expected, they are estimated with more

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

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 et al. (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 et al. (2022)). We then estimate:

$$y_{i,t,h} = \beta Post_{t,h} \times Treatment_{i,h} + \theta Post_{i,t,h} + X_{i,t,h}\kappa + \alpha_i + \delta_t + \epsilon_{i,t,h} \quad (7)$$

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:

$$y_{i,t} = \beta Post_{i,t} + X_{i,t}\kappa + \alpha_i + \delta_t + \epsilon_{i,t} \quad (8)$$

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 columns 9 and 10 of Appendix Table A6 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 A7 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.

B.3 Indirect effects: Alternative empirical approaches

B.3.1 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:

$$\begin{aligned}y_{i,t,j} = {} & \beta_1 Post_{t,j} \times Closeness_{i,j} \times Treatment_i + \beta_2 Post_{t,j} \times Treatment_i + \\ & \beta_3 Post_{t,j} \times Closeness_{i,j} + \gamma_1 Closeness_{i,j} \times Treatment_i + \gamma_2 Closeness_{i,j} + \\ & \gamma_3 Treatment_i + \theta Post_{t,j} + \mathbf{X}_{i,t,j}\kappa + \alpha_i + \omega_j + \delta_t + \theta_\tau + \delta_t \times Degree_{i,j} + \epsilon_{i,t,j}\end{aligned} \tag{9}$$

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 A8 report estimates that are very similar to those of our main specification.

B.3.2 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 3.1. 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).[5]

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 9 and 10 of Appendix Table A8. The effect on total transactions (column 9) of -0.22 is quite similar to the -0.315 from table 2. The effects on income THB -1206 are also quite close to the estimates from Table 2 (THB -1232). 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.

[5]We focus on households either directly or indirectly connected to shocked households through the pre-period network for two reasons. Fist, Figure A5 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.

B.4 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 A10 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 570, or approximately 29%.[6] Columns 2 to 4 show that although borrowing increases and fixed and liquid assets decline, the changes are not significant.[7] 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 activities. Two other factors may help explain the divergence in transfer

[6]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 A6 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.

[7]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.

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.

B.5 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 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:[8]

$$\begin{aligned} y_{i,t} =& \beta_1 Post_{i,t} \times Treatment_i \times Low_i + \beta_2 Post_{i,t} \times Treatment_i \times High_i \\ &+ \theta_1 Post_{i,t} + \theta_2 Post_{i,t} \times High_i + X_{i,t}\kappa + \alpha_i + \delta_t + \epsilon_{i,t} \end{aligned} \tag{10}$$

where $y_{i,t}$, $Treatment$ and $Post$ are defined as in Section 3.1. $High_i$ 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 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 propagate differently, compared with those to better-insured households, we estimate the following model:

$$\begin{aligned} y_{i,t,j} =& \beta_1 Post_{t,j} \times Closeness_{i,j} \times Low_j + \beta_2 Post_{t,j} \times Closeness_{i,j} \times High_j \\ &+ \beta_3 Post_{t,j} \times High_j + \beta_4 Closeness_{i,j} \times High_j + \gamma Closeness_{i,j} \\ &+ X_{i,t,j}\kappa + \alpha_i + \omega_j + \delta_t + \theta_\tau + \epsilon_{i,t,j} \end{aligned} \tag{11}$$

where $High_j$ is an indicator of whether directly shocked household j had above-

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

median pre-period access to informal insurance networks, defined as above. Also as above, $Closeness_{i,j}$ denotes the inverse distance between household i and directly shocked household j during the year preceding the shock. The coefficient β_1 measures the change in outcomes after the shock associated with a one-unit 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 (10) and (11).

B.6 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:

$$\begin{aligned} Link_{i,j,t} = &\rho Link_{i,j,t-1} + \gamma_1 Kinship_{i,j} + \gamma_2 Demographic\ distance_{i,j} \\ &+ \gamma_3 Net\text{-}Worth\ distance_{i,j} + \delta_{v,t} + \alpha_i + \alpha_j + \epsilon_{i,j,t} \end{aligned} \tag{12}$$

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.[9] 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).[10] 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 A13 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 sub-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-

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

[10] 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.

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 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.4 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 sub-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 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.

C 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`