Propagation and Insurance in Village Networks *

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Abstract

In village economies, it is well known that networks can smooth shocks. Less acknowledged is that production networks between local firms can propagate shocks. In Thailand, a significant idiosyncratic shock to one household firm propagates via supply-chain and labor networks. Imperfectly insured firm owners adjust production decisions—cutting input spending and reducing hiring—affecting those with whom they trade inputs and labor. Those linked to shocked firms experience reduced local transactions, earnings, and consumption. These declines persist over several years. The total magnitude of indirect effects may be larger than direct effects, and the social gains from expanding safety nets may be substantially higher than the private gains.

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1 Introduction

Local linkages between households ("village networks") are understood to play an important role in many domains, including, but not limited to, risk sharing, geographic and social mobility, information diffusion and technology adoption.[1] While the important role of village networks in many domains is increasingly well-understood, several important gaps remain. One relates to the role of *production-side* village networks involved in the exchange of productive inputs and labor. It is well known that shocks to a household's income can and do affect *that household's* businesses (see, e.g., Rosenzweig and Binswanger 1993; Samphantharak and Townsend 2010). But how do the resulting adjustments affect the other businesses and workers with whom a shocked household transacts? Whether and how shocks propagate through production networks in village economies matters for assessing the economic incidence of policies or technologies that affect different households/firms differentially.[2]

There is a growing empirical literature studying the firm-to-firm propagation of regional or sectoral shocks through multinational production networks.[3] Recent macroeconomic models have explored the link between granular shocks and aggregate fluctuations (Gabaix, 2011; Acemoglu et al., 2012; Farhi and Baqaee, 2020). However, it is nonobvious whether propagation will occur similarly in local networks in emerging market settings. A high degree of local in-

[1]See, e.g., Udry 1994; Townsend 1994; Munshi 2003; Banerjee et al. 2013; Beaman et al. 2018; de Janvry et al. 2019. Social networks also play a role in redistributing public programs, either independently of deliberate effort to harness them (e.g., Angelucci and De Giorgi 2009; Vera-Cossio 2020), or as part of such efforts (e.g., Maitra et al. 2020; Hussam et al. 2020).

[2]As we discuss below, all households in our data operate at least one family firm, so we use the terms firm and household interchangably.

[3]The literature in international trade studies the propagation of shocks through production networks in the aftermath of natural disasters (Barrot and Sauvagnat, 2016; Carvalho et al., 2021), trade shocks (Tintelnot et al., 2018; Huneeus, 2019), and sectoral or regional shocks (Caliendo et al., 2017).

formation, repeat interaction, and norms against opportunistic behavior could, in principle, mean that supply chains in these village networks function more smoothly, with less propagation, than in networks composed of large firms. Or, liquidity and informational constraints could bind more tightly in village networks, making propagation more severe. A large share of firms across the world are small and family-operated (Beck et al., 2005), and thus exposed to shocks affecting family endowments. The welfare consequences of propagation may be very different in a context where firms are owned, not by diversified shareholders, but by households with low and un-hedged incomes.

Additionally, there remains much we do not know about how the microstructure of village networks relates to networks' effects on risk-sharing and propagation. There are many ways in which households can be connected in village economies, and it is important to understand which types of links primarily serve an insurance role, which might serve a propagation role, and which serve other roles (information diffusion, etc.).[4] And the question of how overall network structure matters for insurance and propagation of shocks is also a relatively open one.[5] As pointed out by Breza et al. (2019), shedding light on these questions is challenging. To understand how different network linkages affect production- and consumption-side outcomes, one needs to observe both granular network data (who is linked to who, and how) and panel data on the production and consumption side of household balance sheets. Moreover, understanding the propagation effects of network connections re-

[4]Of course, these roles are not mutually exclusive; we discuss overlap between our network measures below.

[5]Banerjee et al. (2013) study empirically how network structure affects information diffusion in villages in India. Elliott et al. (2014) examine this question theoretically in the context of links between financial intermediaries; 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 is not well understood.

quires 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 household so that its propagation effects can be measured).

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, exogenous and idiosyncratic shocks to households' budgets in the form of shocks to health spending needs. These elements together provide a setting that is uniquely able to shed light on the role of networks in propagating and mitigating shocks.

We first show that, in violation of the separation theorem, idiosyncratic consumption-side shocks have significant effects on the business activities of the shocked household. When hit by a health spending shock, entrepreneurs reduce business spending to smooth consumption, in essence financing the shock out of the business budget. They substantially reduce input spending (23% decrease), and almost entirely cut their demand for external labor (79% decrease).

This paper is not the first to show that the separation theorem fails (see, e.g., Benjamin 1992). 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 relies on variation in the proximity of a given household to the shocked household through pre-period economic networks. We undertake a generalized difference-in-difference analysis: comparing changes in outcomes before and after each shock, between more-exposed households (those that are closer to a shocked household in the pre-shock network) and less-exposed households (those farther away). Closer households, with greater exposure, see larger falls in total transactions (a 21% decline for a unit change in closeness), and therefore falls in income (12% decline for a unit change in closeness). An alternative identification strategy in the spirit of Fadlon and Nielsen (2019), in which we compare those close to a shocked household (the treated group) to those who are close to a household who suffer a shock, but at a different point in time (the control group), yields similar results.

Although the indirect effects dissipate through the network, there are non-negligible propagation effects on indirectly-connected households (two or more links away from the shocked household) as well as those directly connected (one link away). When, due to a shock, directly-connected households reduce sales of goods or labor (outgoing transactions to the shocked household), this leads to declines in income, which in turn translate into fewer purchases (incoming transactions) from other households, triggering further propagation through the network. In sum, the shocks that we study generate indirect effects both upstream and downstream, as the costly adjustments taken by the directly-shocked household reverberate through the local network.

Our results are robust to a battery of robustness checks. Our main specification examining direct shock effects uses only shocks occurring in the first half of the sample period to address concerns about difference-in-difference with staggered timing (Goodman-Bacon, 2018); however the results are similar when we include a broader set of shocks. We also obtain similar results when we consider alternate definitions of the onset of a shock and of the placebo group. To support our interpretation that the episodes we identify are largely shocks to spending needs rather than to labor endowments, we show that the results remain similar when we use only shocks to non-working-age household members. Turning to the indirect effects, we show that indirect exposure via the labor network is associated with a drop in labor transactions and no effect on supply chain transactions, and vice versa for the supply chain network. We also include flexible controls for network centrality-by-time effects. These checks alleviate the concern that indirect exposure is picking up other differences between exposed and unexposed households. We also show that there is no treatment effect on the provision of uncompensated labor, ruling out the concern that "propagation" is simply a relabeling of the linked households providing insurance. In addition, we use a second research design comparing those close to a shocked household to those close to a placebo household, which yields similar effects.

To understand the mechanisms of this propagation, we show that propagation occurs in a context of rigid/persistent networks: *ceteris paribus*, households that transacted at baseline are substantially more likely to transact even ten years later, relative to households that did not transact at baseline. Kinship ties are strong predictors of trade, highlighting the importance of time-invariant barriers to trade across households (Emerick, 2018). Thus, suppliers struggle to find new customers when their clients suffer a shock, and workers struggle to find new jobs when existing employers scale back demand. Frictions leading to rigid labor networks are particularly important: proximity to the shocked household via the labor market network is most strongly associated with indirect effects on income and consumption.[6] Indirect effects persist even four years after the shock, suggesting that, in a context of rigid networks, the

[6]Evidence of frictional slack in goods and labor markets is also shown by Egger et al. (2019) in the context of rural Kenya.

recovery from indirect shocks can be sluggish.

To understand how the impacts of health shocks are related to incompleteness in insurance markets, we show that directly-shocked households receive incoming gifts and loans to partially buffer the shock; however, this interpersonal insurance is incomplete. We further show that the direct and indirect consequences of shocks vary with access to informal insurance. Shocks to households with limited access to informal insurance reduce costs and revenues by 29% and 22%, respectively. In contrast, for households with higher informal insurance participation, these decreases are almost fully attenuated. Thus, households that are not well-integrated into local informal insurance networks are most vulnerable. In turn, shocks to less-insured households appear to generate larger indirect effects, though these differences are imprecisely estimated.

Turning to the role of overall network structure, using cross-village variation in pre-shock network density, we find that *ceteris paribus*, denser networks are associated with more propagation, suggesting that there may be a tradeoff to interventions which aim to increase social capital.

As expected, the magnitude of the propagation effect experienced by a single indirectly-affected household is smaller than the magnitude of the effect felt by the household directly experiencing the health shock. But, the indirect effects hit many more households. As a result, a simple back-of-the-envelope exercise suggests that the total village-level magnitude of the indirect effects of a given shock may be as large or larger than the direct effects of that shock, with a multiplier of approximately 1.5.[7] Thus, analyses of shocks to households

[7] Our estimated multiplier is quite similar in magnitude to those estimated by Egger et al. (2019) in Kenya and by Nakamura and Steinsson (2014) and Suárez Serrato and Wingender (2016) in the US. See Section 5 for details.

and their businesses—or of shock-mitigating policies such as health insurance, bankruptcy, cash transfers or small business aid in times of crisis—which do not take into account the indirect effects may significantly underestimate the total impact.

2 Context and Data

2.1 Household data

The data used in this study come from the Townsend Thai Monthly Survey. The survey 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. The subsequent monthly updates began in September 1998.[8] The sample in this paper covers the period between September 1998 and December 2012. We focus our analysis on the subset of 509 households that responded to the interview throughout all survey waves.

Table 1 characterizes the sample households in terms of their demographic, financial and business characteristics. It shows that while households derive income mostly from family farms, they also operate off-farm businesses and provide labor to other households or businesses. In addition, 13% of total income comes from the receipt of government transfers and/or gifts from other households. Households allocate around 50% of their resources to consumption, and use the remainder to accumulate assets, which are evenly distributed between liquid and fixed assets. In terms of access to financial markets, in a

[8]For more detail about the Townsend Thai Monthly Survey, see Samphantharak and Townsend (2010).

given year, 83% of households report borrowing from any source, 48% from formal or quasi-formal financial institutions, and 30% from personal lenders, including relatives.

2.2 Network data

The survey contains detailed information on transactions among households and captures different types of economic inter-linkages. During each survey wave, interviewees identify any and all households in the village with whom they have conducted a given type of transaction.[9] We aggregate the monthly transaction data by year to elicit three types of village networks, for each year in the sample.

First, we recover the supply chain networks that capture transactions of output, inputs and intermediate goods across businesses of households in the same village. Second, we recover labor networks that capture employer-employee relationships within the village.[10] Appendix Figure B1 depicts both networks for a sample village. Panel C of Table 1 shows statistics on network participation across the sample as a whole. On average, just under half (48%) of the households transact in the local village supply-chain network by transacting inputs and final output (with 1.26 connections on average), and 62% provide or purchase labor to/from other households in the village, with 3.07 connections on average. We also recover information on local financial networks defined by gifts and loans across households in the same village, which

[9]The set of transactions include the relinquishment of assets, purchases or sales of inputs or final goods, the provision of paid and unpaid labor, and giving and receiving gifts and loans.

[10]As is typically the case in networks based on survey instead of census data, our networks may look thinner than the networks that would be elicited using census data (Chandrasekhar and Lewis, 2017). We discuss the implications of this source of bias for our research design in Section 3.2.

tend to be more sparse (see Appendix Figure B1).

Households tend to participate in several networks in a given period. Among those linked through financial networks, over 60% also transact in local supply-chain networks and over 70% of them transact in local labor markets. Over 77% of households transacting in the village supply-chain networks also sell or purchase labor locally, and 45% are linked through local financial markets. Likewise, over 59% and 43% of households that buy or provide labor locally transact in the supply-chain and financial village networks, respectively. Local kinship networks also overlap with these transaction-based networks; see Section 4.4.

2.3 Constructing idiosyncratic shocks

In order 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 finance 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 effects hitting other, connected, households via propagation. Additionally, the timing of these shocks are—as we show below—exogenous, allowing us to understand their causal effects.

We identify shocks as follows (see Appendix A for details). For each household, we identify the month with the highest level of health spending throughout the panel.[11] We focus on the largest shocks as they pose a significant financial burden to the household. To facilitate measuring responses to the shocks by comparing households' behavior before and after the episodes, we restrict the search to years 3-12 in the panel (out of 14 years). This enables us to observe at least two years of both pre- and post-shock behavior for all households. We identify 505 shocks, one per household. After excluding shocks related to childbirth, which may be anticipated, we are left with 469 shocks.[12]

2.4 Characteristics of the shocks.

Relationship between health spending and health status. Appendix Figure A4 shows that health spending (left axis) and self-reported symptoms (right axis) co-move, confirming that shocks are correlated with decreases in household health endowments. Appendix Table A1 reports the distribution of types of health symptoms reported by shocked households during the two years around the shock, during non-shock periods, and during all the sample periods.

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 total monthly consumption during the 6 months preceding the shock (THB 6113) and was substantially larger than the average monthly total

[11] Thailand has a universal health insurance program, so these expenses are above and beyond those covered. See Appendix C.

[12] 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. Appendix Figure A1 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. We present robustness checks varying the beginning of the shock in Section A.2.1.

household 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. The top panel of Appendix Figure A3 presents the distribution of months associated with the beginning of each event. It shows that the event start dates are spread through all the periods in the sample and suggests that the events are indeed idiosyncratic. In Appendix Table A3 we formally show that village-level trends have null predictive power on the the occurrence of these events.

3 Direct and indirect effects of idiosyncratic shocks

3.1 Direct effects

To understand the indirect effects of shocks via network propagation, we first must understand how they affect the *directly* shocked household. Because the networks that we study are defined by cross-household transactions of input, 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 idea, we follow Fadlon and Nielsen (2019) and exploit the plausibly random variation in the *timing* of severe health shocks. We compare the behavior of households that experienced the shock in period t (i.e., treated households), to the behavior of households from the same age group and village that did not experience a shock at time t, but experienced a similar shock later on, in period $t + \Delta$ (control households). We denote as treated households 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 experienced a placebo shock). (See Appendix A.2 for additional details.) The underlying identification assumption is that, in the absence of the shock, the treatment and control group would have followed parallel trends, which we validate tusing event-study specifications that test for lack of systematic differences before the shock ("parallel pre-trends").

3.1.1 Estimation

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

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

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 households 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 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 preceding the shock ($\tau = -1$) between households in the treatment and control group. We focus on a two-year (i.e., four-half year) time window before and after the shock as our panel is fully balanced during this period. We also use a more parsimonious differences-in-difference specification:

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

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 as our main source of variation comes from cross-household variation in the timing of events and to flexibly account for serial correlation (Bertrand et al., 2004).

Note that our approach addresses two 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 that were shocked before t. This could be problematic in our setting since such "staggered event timing" specifications may suffer from bias when effects are heterogeneous over time (Goodman-Bacon, 2018; Baker et al., 2021). 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 246 shocks that occurred earlier in the sample. Moreover, trends in outcomes may vary by age due to different trajectories along the life cycle. By constructing a comparison group within age group and village, our approach makes comparisons of households with similar pre-shock trends.

3.1.2 Direct effects: Results

A shock to health spending, which entails large outflows of resources, can be financed in a number of ways. Here we focus on changes in household production decisions — reducing spending on hired labor and/or business inputs to free up resources to meet the shock — as such dimensions are linked to cross-household transactions that determine local economic networks.[13]

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, and Panel (c) shows an increase in total expenditure of a similar magnitude, indicating that non-health consumption remains steady.

[13] *A priori*, shocks may affect household labor endowments as well as spending needs. In Appendix A.2.2 we argue that the spending effect is more first-order in our setting as a majority of shocks affect non-prime-aged individuals.

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. Panel (e) shows that input spending falls after the shock. Finally, Panel (f) shows that the slowdown in input spending coincides with a slowdown in revenues after the shocks. Note that the sharp declines in input spending and revenues coincide with the sharp increase in spending induced by the shock. Thus, shocked households meet short-term liquidity needs in part by drawing down working capital, inconsistent with the separation theorem. The graphical evidence also documents parallel pre-trends: for all six outcomes, there are no spuriously significant "effects" prior to the spending shock.[14]

To provide a quantitative assessment of the overall impact of the health shocks, in Table 2 we report difference-in-difference estimates of the effect of the shock on outcomes, corresponding to equation (2). Panel A examines household spending. Column 1 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.[15] Column 2 shows that during the two years following the shock, on average, total spending increases for shocked households, relative to control households, by approximately THB 620, an amount close to the effect on health spending. Thus, in terms of non-health spending, shocked households appear to fully buffer the shocks.

Buffering consumption may entail costly adjustments by shocked households as in Chetty and Looney (2006). Indeed, in order to buffer non-health

[14] Appendix Figure B2 shows the same dynamics in the raw data.

[15] Note that the effects of the shock on health spending are averaged across 24 months in this specification.

consumption, affected households significantly decrease spending on business inputs (column 3) and reduce the use of external labor (column 4). Households also appear to reduce the use of labor provided by household members (column 5), though the effect is not significant. As a result of reduced investment in inputs and labor (columns 3-5), there is a decrease in the revenues from family enterprises, as seen in column 6. (The effect on revenues has a p-value of 0.107.) To increase precision, panel B reports results also including early-shocked households as controls for late-shocked households,[16] which nearly doubles the number of events. Reassuringly, the point estimates are very similar to those in panel A but are estimated with more precision.

Table B1 shows that the results are robust to using alternative definitions of the shock onset attenuating concerns about anticipation. In addition, the results are robust to randomly allocating the placebo shocks, and to using standard two-way fixed effect approaches to compute the effects of the shocks (see Section A.2.1 for details). In addition, Table B2 reports results based on the subset of shocks that affected older household members (age above the median age of 57). The responses to shocks hitting these non-prime-age adults are similar in magnitude to the response in the full sample, suggesting that the costly adjustments in response to the shocks are not solely due to reductions in household labor, but rather are driven by the expenditure shock.

In summary, in the face of a shock, households buffer non-health consumption, but do so at the cost of significantly reducing business spending. (Households may also engage in other strategies to cope with the shocks; see Section 4.2.) These declines in business spending and labor demand have broader con-

[16]Note that even after including more events, 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-differences frameworks that tend to arise when treatment status varies over time.

sequences for other households. We next turn to examining the effects of these shocks on *other* households, via propagation through local economic networks.

3.2 Economic networks and the propagation of idiosyncratic shocks

The results above show that health shocks meet the necessary criteria to understand propagation: their timing is exogenous, their occurrence is idiosyncratic, and the shocks have substantial effects on household production decisions. Given the significant degree of inter-linkage 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 that trade with shocked households. Second, shocks could propagate through local labor networks: as supply and demand for outside labor decreases due to the shocks, households that exchange labor with shocked households could suffer falls in hours, earnings and revenue.

3.2.1 Identifying propagation effects

We exploit two sources of variation to test if 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 network (or both). We assess the propagation of idiosyncratic shocks to other local family businesses by comparing households who, before the shock, shared closer market inter-linkages with household j's businesses to those who were un- or less-connected to household j before the shock, before and after the shock to household j.

Throughout our sample period, we observe multiple health shocks per village. We construct a dataset 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 of the directly shocked households.[17] We then stack the observations into a dataset at the household (i) by time (t) by event level (j), for each village.

We combine this dataset 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 that 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. This is consistent with the evidence of persistence in the village networks discussed in Section 4.4, and with evidence of the importance of time-invariant determinants of economic 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.

We measure exposure as the inverse distance in the undirected village network[18] between household i and the shocked household j: $Closeness_{i,j}$ =

[17]We restrict the analysis to two years before and after the shock, first, to be consistent with the analysis of the direct effects of the shocks; second, we only have a fully balanced panel during this time window.

[18]We focus on undirected networks because the shock can propagate both up- and downstream as we document below, in Section 3.2.2.

$\frac{1}{dist_{i,j}}$.[19] 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 networks as 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 4).

We elicit economic networks using survey instead of census data (Chandrasekhar and Lewis, 2017). Thus, it is possible that we underestimate the closeness of some sample households to shocked households.[20] Because we may be underestimating exposure—classifying some households as un- or less-exposed when they are actually (more) exposed—our results could be biased towards zero. Thus, we interpret our magnitudes as *lower bounds* of the indirect effects of idiosyncratic shocks on other households.

Finally, not all shocked households are active in the local markets for goods, and not all shocked households employ other villagers for their businesses. Thus, we analyze the propagation of shocks through village networks by focusing only on events corresponding to the 391 households that traded in either the supply chain or labor market networks during the year preceding their shock; these represents 83% of all the shocks in our sample.

With these caveats in mind, we estimate the following difference-in-difference specifications:

[19]This measure equals one if household i directly traded with the shocked household j and zero 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. By undirected networks we mean that we do not distinguish between incoming vs. outgoing transactions. Likewise, we weight each transaction equally for our calculations.

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

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

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.[21] 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 one during the two years following the shock to household j, and zero for the pre-period. The coefficient of interest, β, captures differences in outcomes with respect to pre-period, associated with one additional unit of closeness.

We control for household fixed effects (α_i), time (month) fixed effects (δ_t) shocked-household fixed effects (ω_j), time-to-shock fixed effects ($\theta_{\tau(j)}$), which accounts for village-specific time-varying shocks during the analysis window corresponding to the shock to household j, and a vector of time-varying de-

[21]Below we consider several definitions of *Closeness*: proximity in the overall network pooling supply chain and labor market, as well as proximity in one network or the other.

mographic characteristics ($\mathbf{X}_{i,t,j}$).[22] We also control for time-varying shocks affecting more central households, which could also be more likely to be close to other households, by including interactions of the number of links of household i ($Degree_{i,j}$) during the year preceding the shock to j with time fixed effects. 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. As we are focusing on indirect effects, we drop observations of directly shocked households j from the analysis. We also exclude observations of households that experienced their own shock within a year before and after the shock to household j.

The identifying assumption underlying our strategy of 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 along parallel trends *ceteris paribus*, i.e., conditional on the vector of controls included in equation 3 and 4. We validate this identifying assumption by testing for a lack of differences in the pre-period; namely, for $\tau < 0$, we verify that β_τ is not different from zero.

In thinking about the identifying assumption, recall that equations 3 and 4 control for household fixed effects; shocked-household fixed effects; and $Degree_{i,j} \times month$ fixed effects, which allow for a common shock to all households with a given network degree to experience a common shock. 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.

[22] We control for household size, gender composition, average age and schooling.

3.2.2 Results: Propagation of shocks 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 4.4) demonstrates that such reorganization of local ties is very difficult, at least over the span of 1-2 years.

Table 3 shows difference-in-difference estimates corresponding to equation (4).[23] It documents significant post-shock declines in the number of monthly transactions in the supply-chain (column 1) and labor-market networks (col-

[23] In Appendix table B3, we re-estimate equation 3, including village-by-month fixed-effects ($v_{v,t}$) to control for potential village-and-time-specific shocks. The results are quite similar to those from the main specification which control for village-time fixed effects in the analysis window corresponding to each event.

umn 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 1267, or 12% of the pre-period mean. In turn, consumption spending falls by THB 304, or 4.2% of its pre-period mean (column 5).[24] The fall in consumption is smaller than the fall in income, suggesting that indirectly shocked households are able to partly, but not completely, smooth their indirect shock exposure.

The effects that we observe are strongest for directly connected households—those that were one link away from the shocked households—but affect indirectly connected households as well (see Appendix Figure B3).[25] 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, which in turn translate into fewer purchases (incoming transactions) from other households, triggering further propagation through the network. Indeed, Table 4 shows that the fall in outgoing transactions documented above is matched by a fall in incoming transactions (input purchases and labor 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

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

[25]Figure B3 plots indirect effects decomposing the measure of closeness into 4 categories: directly connected households (1 link away from the shocked households); households that are 2 or 3 links away from the shocked households; those that are 4 or 5 links away from the shocked households; and (as the base category) those that are 6 or more links away in the network, including those that are unconnected to the shocked household. Although the effects dissipate through the network, there are non-negligible propagation effects on indirectly connected households.

are *prima facie* idiosyncratic are spread to other connected households. We return to the multiplier effect of these idiosyncratic shocks in Section 5.

3.2.3 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 placebo 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. Details are in Appendix A.3.

The results appear in Table 5. Column 1 reveals a drop in input/output transactions of 0.214, very close to the estimate of 0.200 from table 3. The effect on hired labor (column 2) is imprecisely estimated, but the effect on total transactions (column 3) of -0.278 is quite similar to the -0.315 from table 3. The effects on income and consumption, THB -1426.3 and -351, respectively, are also quite close to the estimates from Table 3 (THB -1267.1 and -303.6). 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.

4 Propagation Mechanisms

4.1 Propagation via supply chain vs. labor networks

In Table 6, 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, as 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.[26] 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 0.227. There is no effect on input/output transactions associated with proximity through the labor network. Analogously, column 2 shows that proximity through the labor market network has a negative and significant effect (-0.210) on transactions involving paid labor, while there is no effect seen via the supply chain network. In column 3, proximity via the supply chain network and the labor market network both have negative and significant effects on the total number of transactions (-0.206 and -0.244, respectively).

Columns 4 and 5 show that proximity via the labor market network is associated with large and significant drops in income and consumption, re-

[26]On average, 41% of households share a direct or indirect link to the shocked households through both, supply-chain and labor-market network, 16% are directly or indirectly linked to the shocked household only through the supply-chain network, 13% are directly or indirectly connected to the shocked households only through the labor network and 30% of households are neither connected to the shocked households through the supply-chain nor labor network.

spectively, while the corresponding effects of proximity via the supply chain network are small and insignificant.[27]

4.2 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 B4 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%.[28] Columns 2 to 4 show that although borrowing increases and fixed and liquid assets decline, the changes are not significant.[29] 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 present

[27]A possible explanation is that, although the absolute effect of propagation through supply chain networks on input/output transactions is similar to the propagation effect through labor market networks on labor transactions, the effect on labor market transactions is larger in relative terms: the decline in labor market transactions represents a 44.6% decline relative to the pre-period number of labor market transactions, while the decline in transactions of inputs and final goods represents 23% of the pre-period mean. Households close to the shocked household via the labor market network may suffer a double impact, namely, reduced labor demand via an income effect as the directly hit household scales back, as well as a further hit due to the complementarity between household and hired labor, as household labor can be required to supervise or monitor hired labor. Indeed, Table 2 shows that household labor seems to decline due to the direct effect of the shocks (see column 5).

[28]Note that this is on the same order as the direct effect on health spending in Table 2; however, comparing Figure 1, Panel c and Figure B4 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.

[29]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.

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 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.[30]

4.3 Informal insurance and the propagation of shocks

Informal insurance can help to buffer health shocks (De Weerdt and Dercon, 2006); this suggests that shocks to uninsured households may be more likely to trigger declines in business activities and hence propagate more to other households. To test this idea, we use data on intra-village provision and receipt

[30]To demonstrate that local networks may be less able to insure aggregate shocks, Figure B5 compares the network responses to idiosyncratic vs. aggregate shocks using the 2002 EU ban on Thai shrimp imports. See appendix B.1.1 for details.

of gifts during the year preceding the shock. We split the sample of shocked households into those with high vs low pre-shock access to informal insurance. See Appendix B.2 for details.

Panel A in Appendix Table B5 reports estimates of the direct effects of the shock on gift and loan receipt and business outcomes by access to informal insurance. Households with high access to informal insurance experienced a substantial increase in gifts and loans.[31] This increase, in contrast, is small and non-significant in the case of low-access households (column 2). Moreover, there are statistically significant declines in input spending (column 3) and hired labor (column 4) in the case of low-access households, but these declines are small and not significant in the case of better-insured households. For input spending, the difference between the effects on low- and high-access households is significant at 10% (p- value=0.09). In addition, although there is a significant decline in revenues in the case of high-access households (column 5), the decline in revenues in the case of low-access households is 1.7 times that of high-access households. The results suggest that households with limited access to insurance drive most of the declines in business activities, suggesting that incompleteness in local insurance markets may lead to non-separability of household spending and production decisions. Conversely, improvements in access to risk smoothing may reduce the extent of non-separability and thus reduce propagation.

Next, to investigate whether shocks to less-insured households propagate differently, we estimate a version of equation 3 where we allow the effect of indirect exposure to vary by the *directly shocked* household's baseline access

[31] We test whether the the indirect effect on consumption (Table 3, column 6) could be a consequence of a decline in cash on hand/liquidity arising from helping the directly shocked household. Appendix Table B7 shows that neither transfers nor loans given by the indirectly shocked household to other households increase following the shock.

to insurance (See Appendix B.2.) Panel B of Appendix Table B5 presents the results. When the shocked household had low access to insurance in the pre-period, the fall in income associated with 1 unit greater *Closeness* is 1705 baht. When the shocked household had high access to insurance, the fall in income is 1016.3 baht. That is, the propagation effects on income when the shocked household has low access to informal insurance are 1.67 times the propagation effects on income when the shocked household has higher access to insurance (col 4). Moreover, the consumption spending of indirectly affected households falls by 462 baht, or roughly 6%, when the shocked household had low access to insurance in the pre-period. When the shocked household had high access to insurance, however, the fall in consumption spending is reduced by only 275 baht (col 5). In sum, although the differences across shocks to households with high and low access to informal insurance cannot be estimated with precision, the magnitude of these differences suggest that informal insurance markets may mitigate the direct and indirect effects of idiosyncratic shocks.

4.4 Rigidities in local networks

Our results suggest that the costs related to the creation of new links may limit the participation of some households in local networks. Indeed, Panel C of Table 1 shows that while an significant share of households transact in village networks, this share is not 1. There is evidence from other contexts suggesting that market frictions may prevent transactions across businesses.[32]

[32]E.g., Johnson et al. (2002) finds that adequate institutional infrastructure (e.g., well-functioning courts) encouraged business owners to try new suppliers in post-Communist countries. Ahlin and Townsend (2007) provide evidence highlighting the importance of social ties and local sanctions in the context of joint-liability loans, for which commitment is crucial. Other sources of frictions may include product specificity (Barrot and Sauvagnat, 2016) or relationship specificity (Elliott, 2015).

If these frictions are empirically important, one should observe a large degree of persistence in the economic 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 and estimate the extent to which past transactions predict future transactions, conditional on measures of similarity. (See Appendix B.3 for details.) Table B8 presents the results. The labor-market and supply chain networks exhibit a striking degree of rigidity over time. One implication is that the effects of shocks which propagate via these networks may be quite persistent. Figure B6 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 networks (panel b), showing no evidence of dissipating even 4 years post-shock.

4.5 Village-level determinants of propagation

The results presented so far exploit *individual-level* variation in exposure to a given shock, between households in a village. It is also of interest to know how *village-level* variations such as differences in in network structure or the position of the directly shocked individual matter for the extent of propagation. Therefore we estimate two alternative specifications which, instead of exploiting variation in closeness to the shocked household within the village, exploit cross-village exposure to the shocks. First, we use the number of links that the shocked household had in the pre-period network—i.e., the number of households in the village that were exposed to the shock—as a measure of exposure. This specification sheds light on how a shock propagates when the shocked household is more vs. less connected.

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 pre-shock transactions). We expect that, the denser the network (the more interconnected households are in a village), the higher is the potential for propagation. Thus, we compare changes in outcomes before and after each health shock, between households in more- and less-exposed villages.

To understand the role of these village-level determinants we estimate the following model:

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

where, as in our main approach to estimate propagation effects, the unit of observation is a household i in period t around the shock to household j. As only one household was directly hit at a time, 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.[33] 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 Panels A and B of Table 7. Panel A indicates that a 1 standard deviation (SD) increase in the degree of the shocked household leads to an average of 0.026 (2.5%) fewer input/output transactions in the post-shock period relative to pre-shock, 0.044 (9.3%) fewer labor market transactions, and 0.0695 (4.7%) fewer overall transactions (columns 1-3; all significant at 5% or

[33] In both cases, we use standardized z-scores to obtain results on a common scale.

better). Accordingly, a 1 SD increase in the degree of the shocked household leads to a differential fall in income of 364 THB (3.4%), significant at 1% (column 4). In column 5, the point estimate indicates a differential fall in consumption of 72 THB, (0.99%), however this is not different from zero at conventional levels (the p-value is .114).

Panel B shows the results for variation by degree density. A 1 SD increase in the network's pre-shock degree density is causes a 0.043 (4.2%) fewer input/output transactions in the post-shock period, 0.041 (8.6%) fewer labor market transactions, and 0.083 (5.6%) fewer overall transactions (columns 1-3; all significant at the 1% level). The corresponding effect on income is a fall of 391 THB (3.7%), and the effect on consumption is a decline of 148 THB or 2% (columns 4 and 5). All of the results in Panel B are significant at 1%.

These results show that both the structure of the network and location within the network matter: denser networks lead to greater propagation as do shocks to more-connected households.

5 Putting the findings in context: The multiplier effect of idiosyncratic shocks

In this section, we perform a simple back-of-the-envelope exercise to estimate the total magnitude of indirect vs. direct effects on revenues and use this exercise to benchmark our results.

As documented above, idiosyncratic health shocks have both direct costs (analyzed in section 3) and indirect costs (analyzed in sections 3.2 and 4). The former are larger on a per-household basis, but the latter can potentially affect many more households. In order to compare their overall magnitude, and so obtain an estimate of the overall "multiplier effect' of the fall in spending associated with the shock', we perform a simple calculation of the total indirect fall in consumption for each baht of reduced business spending by directly affected households.

The indirect effect on consumption associated with a 1 unit change in *Closeness*, from Table 3, Panel A, column 5 is a fall of -303.5 baht (significant at 10%). The mean (median) level of *Closeness* in the village network is 0.42 (0.43) and the mean (median) number of indirectly exposed households (i.e., households who are connected to the shocked household via the network) is 21.23 (16).[34] The implied total indirect effect using mean values is therefore $-303.5 \times 0.42 \times 21.23 = -2706$ baht per month. Using median values instead gives an implied total indirect effect of -2088.

From Table 2, Panel A, column 3, the fall in business costs for a directly affected household is -1757.4 baht, so the indirect effects using mean and median closeness represent multiplier effects of 1.54 and 1.19, respectively. For comparison, Egger et al. (2019) estimate a consumption-expenditure multiplier of 1.7 from cash transfers in Kenya, while in the US, 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 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. Note that a key distinction with other studies 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

[34]We report medians as well as means since the median is 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.

changes in prices (which would not be differential between closer and less-close households) 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, the total indirect effects are of a similar order of magnitude, and perhaps larger than, the direct effect itself.

6 Concluding remarks

Local networks are well understood to serve a consumption smoothing purpose. We document that they also serve another role, propagating idiosyncratic shocks. We leverage variation in the timing of health expenditure shocks to document consumption smoothing, i.e., no impacts on non-health consumption expenditure, for directly shocked households. However, these shocks are only partially insured via gifts and loans and, as a result, shocked households adjust their production decisions—drawing down working capital, cutting input spending, and reducing labor hiring—in order to achieve consumption smoothing. These adjustments propagate the shock to other households through interlinkages in local supply-chain and labor networks. Businesses close to the shocked household in the supply chain network experience reduced local sales, and workers closer to the shocked household in the labor network experience declines in the probability of working locally and reduced earnings. As a result, consumption falls for these indirectly shocked households. The indirect effects imply a consumption multiplier of approximately 1.5. We also provide suggestive evidence that both direct and indirect effects are attenuated when shocked households are better insured through local risk-sharing networks.

Our findings suggest that (at least) two sets of interventions might be beneficial. First, improved safety nets may help prevent granular shocks from propagating to become *de facto* aggregate. Given that the ability to share idiosyncratic shocks increases with the number of households participating in the insurance network, local networks alone may be unable to diversify severe idiosyncratic risk. Formal commercial insurance contracts or social insurance could allow better risk-coping and thus reduce propagation. In addition, electronic payment platforms that ease the flow of resources across villages may expand and strengthen informal risk-sharing networks (Jack and Suri, 2014).

Of course, fully insuring all idiosyncratic shocks is surely infeasible. This suggests a need for policy interventions to make production networks less rigid and more diversified. These may include 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). Such investments may reduce the rigidity and sparsity of supply chain and labor networks and hence mitigate propagation.

Shocks to entrepreneurs of the type studied in this paper are widespread in both high- and low-income countries. Moreover, the coronavirus pandemic, while to a large extent an aggregate shock, has significant idiosyncratic aspects due to variation in household infection risks and realizations (Jordan et al., 2020), the ability to work from home (Angelucci et al., 2020), and the extent to which different workers and sectors of the economy are affected by shutdowns and social distancing (Daly et al., 2020). Understanding the propagation of shocks is crucial for designing policies to mitigate future crises.

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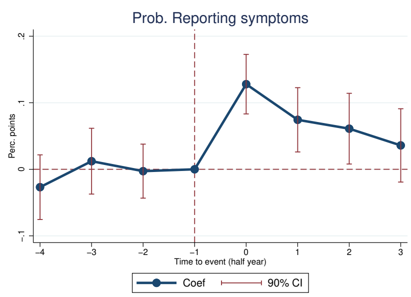
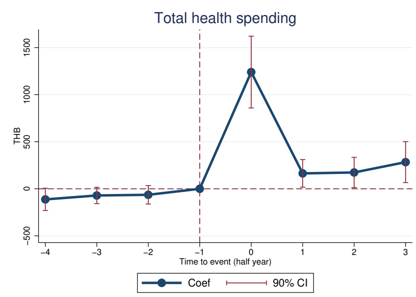
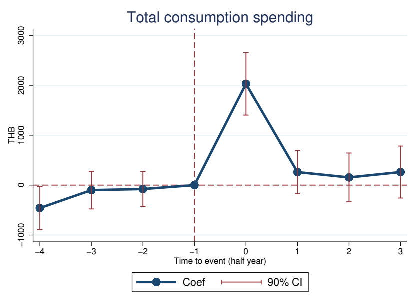
Figures



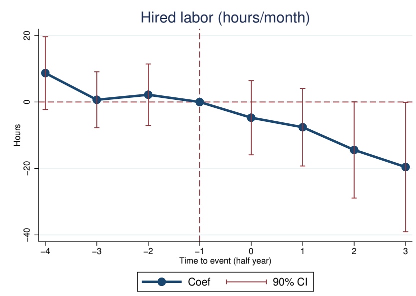
(a) Prob. of reporting any symptoms



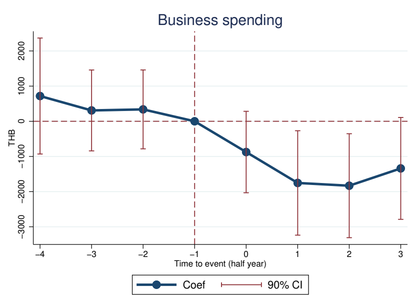
(b) Total health spending



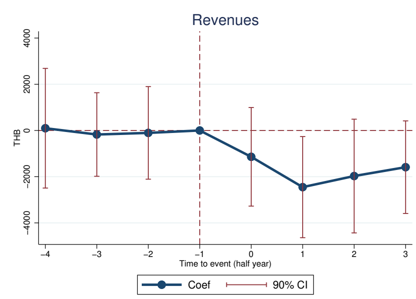
(c) Total consumption spending



(d) Hired labor (hrs/month)



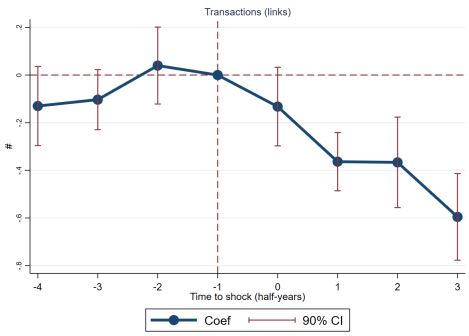
(e) Business spending (inputs)



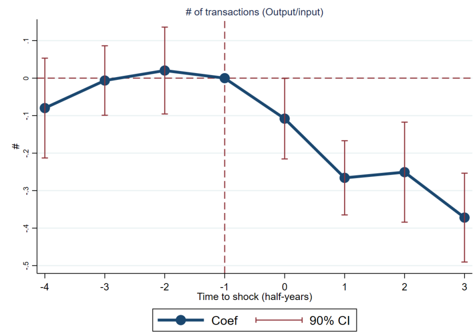
(f) Revenues

Figure 1: Direct effects of health shocks

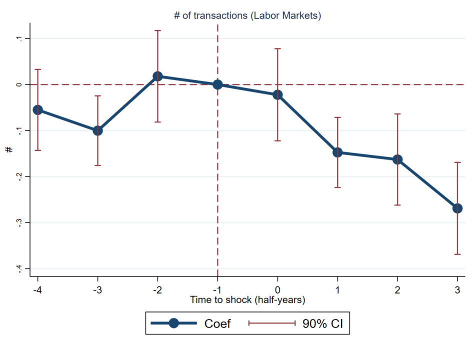
Note: Each dot represents differences between treatment and placebo households in changes in outcomes relative to the period preceding the beginning of the shock ($\tau = -1$).The estimating sample includes 2 years before and after the shock divided in half-year bins. All specifications control for household 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.



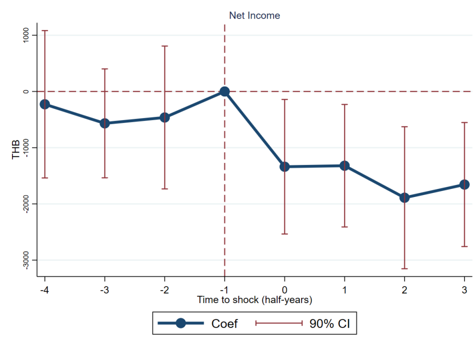
(a) Total transactions



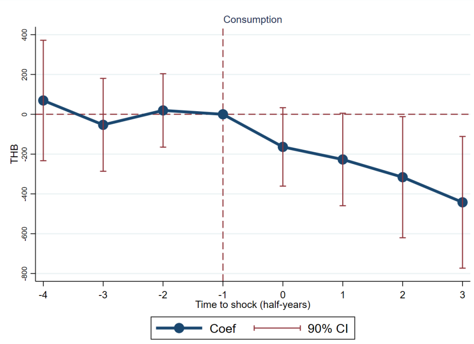
(b) Supply-chain (sales) network transactions



(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.

Tables

Table 1: Summary statistics

Panel A: Household baseline characteristics					
	N	**Mean**	**S.D.**	**10th %ile**	**90th%ile**
Number of household members	509	4.54	1.87	2	7
Number of adults	509	2.87	1.38	1	5
Household head age	507	51.95	13.45	35	70
Average age	509	34.14	12.11	21	52
Household head is a male	507	0.77	0.42	0	1
Years of schooling: Household head	504	4.49	2.59	3	7
Years of schooling: Household maximum achievement	509	8.19	3.64	4	14
Years of schooling: Household average	509	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	7635	134389	1378506	-150	316500
Off-farm family business	7635	19095	115540	0	40700
Labor	7635	52816	108492	0	152222
Total from operations (farm+off-farm + labor)	7635	173327	618277	4974.07	410723
Gift/transfers	7635	24107	183826	-11613	75706
Total net income (Operations+Gifts/Transfers)	7635	197434	644150	16241	446693
Consumption in THB					
Food	7635	32952	21915	11931	60559
Total consumption	7635	98149	99486	24330	204512
Household Assets and Debt					
Total Assets (THB)	7635	2448596	7431394	194277	4817110
Fixed Assets/ Total Assets (%)	7635	53	27	13	88
Total debt/Total assets (%)	7635	12	21	0	27
Households with outstanding loans (%)	7635	83	38	0	100
Households with outstanding loans from institutions (%)	7635	48	50	0	100
Households with outstanding loans from personal lenders (%)	7635	30	46	0	100
Panel C: Village networks (annual data)					
	N	**Mean**	**S.D.**	**10th %ile**	**90th%ile**
Baseline kinship networks: Degree (Number of links)	8344	2.36	2.19	0	6
Baseline kinship networks: Access (any link)	8344	0.77	0.42	0	1
Financial networks: Degree	8344	0.65	1.36	0	2
Financial networks: Access	8344	0.35	0.48	0	1
Sales networks: Degree	8344	1.26	2.64	0	3
Sales networks: Access	8344	0.48	0.50	0	1
Labor-market network: Degree	8344	3.07	4.42	0	9
Labor-market network: Access	8344	0.62	0.49	0	1

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.

Table 2: Effects on spending and family businesses

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

	(1) Health Spending	(2) Total spending	(3) Business spending	(4) Hired labor (Hrs/Month)	(5) HH Labor (Hrs/Month)	(6) Revenues
Post X Treatment	539.7	623.3	-1757.1	-14.33	-11.05	-1714.7
	(92.26)	(366.4)	(829.4)	(7.498)	(8.731)	(1062.5)
Baseline mean (DV)	152.6	5451.0	7610.2	18.11	154.1	14939.0
Observations	22709	22709	22709	22708	22708	22709
Number of events	246	246	246	246	246	246
R-Squared	0.0490	0.154	0.782	0.578	0.712	0.620

Panel B: All shocks

	(1) Health Spending	(2) Total spending	(3) Business spending	(4) Hired labor (Hrs/Month)	(5) HH Labor (Hrs/Month)	(6) Revenues
Post X Treatment	413.1	655.2	-1410.6	-9.754	-15.32	-1850.3
	(61.65)	(344.0)	(537.8)	(4.824)	(6.562)	(697.1)
Baseline mean (DV)	158.0	5937.9	7462.1	16.14	142.0	14960.4
Observations	43151	43151	43151	43150	43150	43151
Number of events	469	469	469	469	469	469
R-Squared	0.0439	0.107	0.749	0.667	0.657	0.541

Note: The Table reports estimates of β from equation (2) for different outcomes. Each column reports differences between treatment and placebo 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, assets 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.

Table 3: Propagation of idiosyncratic shocks

	(1) Input/Output	(2) Hired labor	(3) All transactions	(4) Income	(5) Total spending
Post X closeness (village network)	-0.200	-0.115	-0.315	-1,267.103	-303.551
	(0.062)	(0.044)	(0.078)	(443.764)	(160.860)
Observations	410,578	410,578	410,578	410,578	410,578
R-squared	0.440	0.231	0.374	0.198	0.621
Pre-period Mean	0.999	0.470	1.469	10486	7265
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 4: 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)	-0.078	-0.121	-0.087	-0.028	-0.165	-0.150
	(0.044)	(0.032)	(0.025)	(0.027)	(0.050)	(0.044)
Observations	410,578	410,578	410,578	410,578	410,578	410,578
R-squared	0.534	0.266	0.154	0.218	0.435	0.254
Pre-period Mean	0.497	0.501	0.182	0.288	0.679	0.790
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 5: Estimating propagation á la Fadlon and Nielsen (2019)

	(1)	(2)	(3)	(4)	(5)
	Input/Output	Hired labor	All transactions	Income	Consumption
Post X Treatment	-0.214	-0.0641	-0.278	-1426.3	-351.0
	(0.0853)	(0.0517)	(0.103)	(582.7)	(201.0)
Baseline mean (DV)	1.215	0.564	1.779	9292.2	6596.4
Observations	35111	35111	35111	35111	35111
Number of events	376	376	376	376	376
Adj. R-Squared	0.451	0.193	0.353	0.197	0.558

Note: The table reports results of estimating 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 placebo household. Standard errors are clustered at the household level.

Table 6: Propagation of idiosyncratic shocks through supply-chain and labor-market networks

	(1) Input/Output	(2) Hired labor	(3) All transactions	(4) Income	(5) Total spending
Post X closeness (supply-chain network)	-0.227	0.022	-0.206	-85.229	60.058
	(0.065)	(0.040)	(0.081)	(488.795)	(176.647)
Post X closeness (labor-market network)	-0.035	-0.210	-0.244	-1,332.916	-486.448
	(0.066)	(0.043)	(0.083)	(446.814)	(159.941)
Observations	410,578	410,578	410,578	410,578	410,578
R-squared	0.441	0.231	0.374	0.198	0.621
Pre-period Mean	0.999	0.470	1.469	10486	7265
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 7: Village-level determinants of propagation

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)	-0.0256	-0.0439	-0.0695	-363.9	-72.44
	(0.0130)	(0.0137)	(0.0192)	(91.83)	(45.77)
Baseline mean (DV)	1.012	0.474	1.486	10607.3	7316.4
Observations	453958	453958	453958	453958	453958
Number of events	391	391	391	391	391
Adj. R-Squared	0.422	0.207	0.353	0.197	0.609
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)	-0.0425	-0.0409	-0.0834	-391.4	-148.6
	(0.0150)	(0.0137)	(0.0205)	(111.4)	(34.88)
Baseline mean (DV)	1.012	0.474	1.486	10607.3	7316.4
Observations	453958	453958	453958	453958	453958
Number of events	391	391	391	391	391
Adj. R-Squared	0.422	0.207	0.353	0.197	0.609

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. Standard errors are clustered at the event level.

Propagation and Insurance in Village Networks

Online Appendix

A Identifying shocks and their effects

A.1 Identifying shocks

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

We identify shocks as the month with the highest reported health spending throughtout 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 one sudden increase per household–i.e., increases of the same magnitude. In such cases, we only focus on the first increase to avoid sample selection issues due to repeated shocks.

To identify and exclude events related to pregnancy and childbirth, we exclude the 36 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] 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 A5 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.

An alternative way of identifying shocks would be to identify households who report not having been able to work due to illness. Hendren et al. (2018) follow this approach using the same dataset. However, we follow a different approach as we are interested in extreme events that are related to severe financial needs. For instance, a worker could catch an infection and thus miss some time at work, but that may not necessarily imply large spending needs.

[1] There were 19 households for which symptoms were repeatedly reported for two years or more, and 68 households who lack information related to symptoms.

A.1.1 Characteristics of shocks

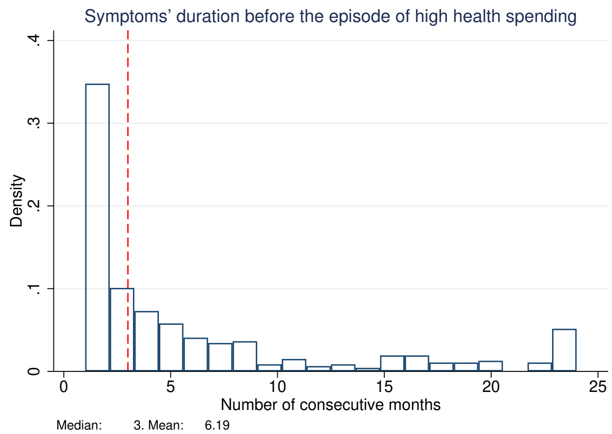


Figure A1: Distribution of symptom duration before the episodes of high health spending

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.

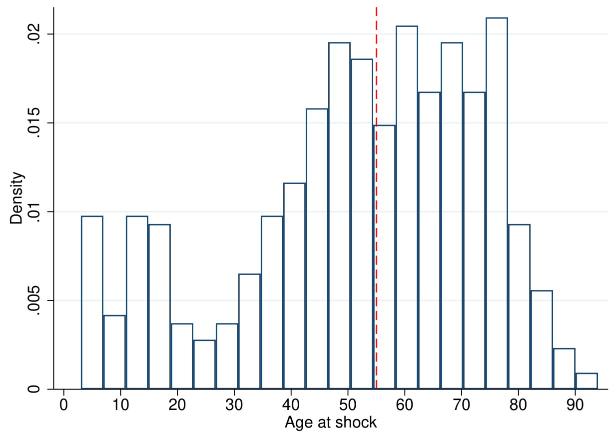


Figure A2: Age at shock

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.

Table A1: Incidence of health conditions by type of symptom

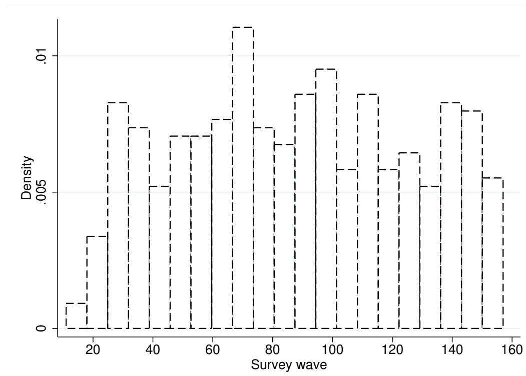
Condition	Shock periods	Non-shock periods	All periods			
			All	Prime working age	Elderly	Children
	(1)	(2)	(3)	(4)	(5)	(6)
Headache/dizziness	9.28	12.03	11.46	15.09	11.96	4.76
Eye sore	1.33	2.05	1.92	1.73	2.09	2.40
Tootache	1.36	1.77	1.72	2.22	0.84	3.00
Cough/cold/influenza	18.35	23.82	22.87	18.67	8.28	55.19
Nausea/heartburn/abdominal pain	4.77	5.11	5.13	6.05	5.15	3.69
Respiratory/asthma	4.91	3.55	3.76	3.63	4.71	2.31
Fever/chills	2.04	2.09	2.05	1.46	1.01	3.14
Diarrhea	1.11	2.01	1.83	1.77	1.01	2.51
Skin disorders/scabies/ulcers/boils	1.84	2.1	2.14	1.89	2.07	2.85
Rheumatism	10.89	9.42	9.61	8.74	15.95	0.09
Infections	7.64	7.45	7.44	9.56	5.51	6.65
Chest pains/heart problems	4.24	3.75	3.75	4.54	3.56	2.82
Others-uncommon conditions	32.24	24.88	26.32	24.65	37.84	10.61

Note: The table reports the proportion of symptoms reported during different time periods and sub-populations. Column (1) reports the distribution of reported symptoms during two years preceding and following the episodes of high-health spending. Column (2) reports the distribution of symptoms for periods that are within two years away of the events (non-shock). Columns (3) to (5) report the distribution of symptoms during all the survey waves by age groups. Prime working age: 18 to 60 years old. Elderly: 60 years old or older. Children: 17 years old of younger.

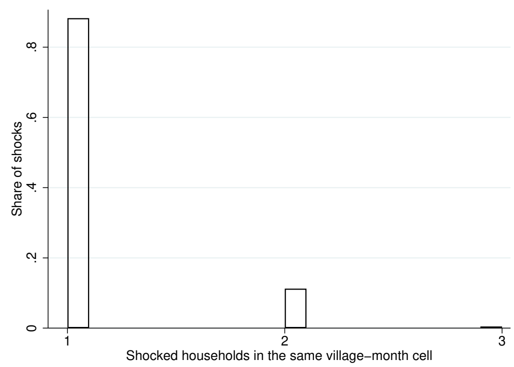
Table A2: Time use in pre-shock periods: Count of days dedicated to different activities

	Number of days per month Average	More than 15 days 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.



(a) Distribution of initial event periods



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

Figure A3: Distribution of events by initial event's periods 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.

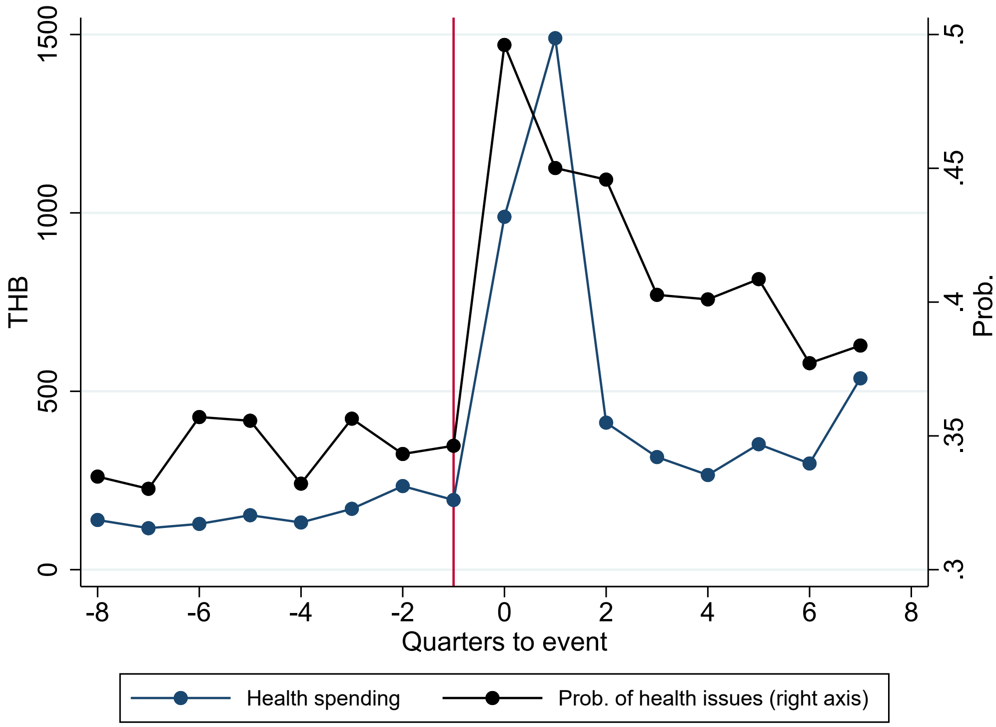


Figure A4: Health status and spending before and after health shocks.

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.

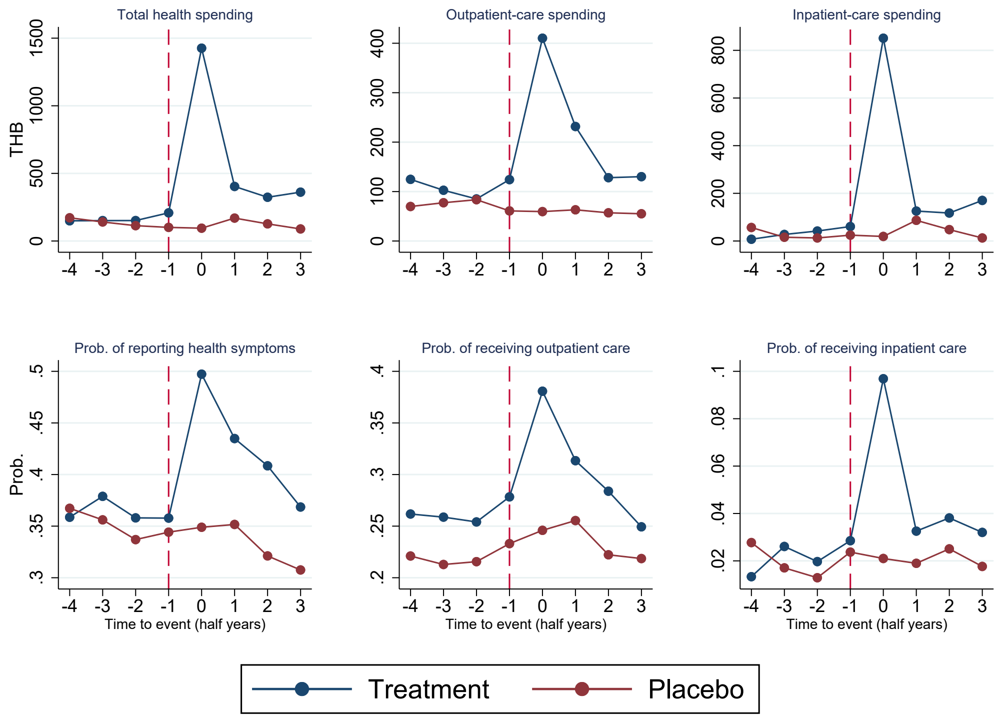


Figure A5: Health status and spending in the treatment and placebo samples

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.

Table A3: Timing of health shocks and village and household characteristics

VARIABLES	(1) Δ P(event)	(2) Δ P(event)	(3) Δ P(event)
Lagged Δ P(event)	-0.500	-0.501	-0.5010
	(0.000)	(0.001)	(0.0012)
Lagged Δ Total net operating income			0.0002
			(0.0009)
Lagged Δ Consumption spending			-0.0022
			(0.0017)
Lagged Δ Consumption of household production			0.0082
			(0.0913)
Lagged Δ Borrowing			-0.0008
			(0.0010)
Lagged Δ Lending			-0.0070
			(0.0039)
Lagged Δ Inflows (transfers)			0.0008
			(0.0010)
Lagged Δ Outflows (transfers)			0.0003
			(0.0004)
Lagged Δ Livestock value			0.0002
			(0.0005)
Lagged Δ Cash in hand			0.0005
			(0.0004)
Lagged Δ Fixed assets - excludes land			0.0006
			(0.0005)
Lagged Δ Land value			0.0003
			(0.0004)
Observations	80,750	77,163	77,163
R-squared	0.252	0.275	0.2754
Month FE	Yes	Yes	Yes
Village FE	No	Yes	Yes
Number of households	475	475	475
P-value (H0: Village trends = 0)		0.731	0.725

Note: The table reports OLS coefficients from changes in the the probability of suffering a shock on period t on lagged changes and village fixed-effects in columns 1 and 2. The bottom panel reports an F-test for the joint significance of the village fixed effects. Column 3 reports similar coefficients including lagged first-differences of household-finance variables. Standard errors are clustered at the household level to control for serial correlation.

A.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 (Silva and Townsend, 2019). 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 A3), 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 Hendren et al. (2018) show that households that experience illness are more likely to experience other illness episodes in the future. 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.

A.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 2 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 2. By adding more shocks we are able to detect significant declines in household labor, and revenues.

Alternative definitions of shock onset. Throughout our analysis, we use the first consecutive month in which households reported experiencing health symptoms to identify the onset of the shock and account for potential anticipation effects that could bias the results. Table B1 reports results from two alternative definitions of the beginning of the shock. Panel A reports estimates of the effects of the health shocks assuming that the beginning of the event started three months after reporting health symptoms. This approach provides a lower bound since households may have adjusted their behavior before the peak if they already experienced symptoms. Panel B reports estimates assuming that the event started three months before households started reporting symptoms. In both cases, the estimates are qualitatively similar to those from our main specifications.

Alternative definitions of comparison groups. We report two 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 placebo 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. Panel C of Table B1 reports 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.

We also report results from a two-way fixed effects panel specification in the spirit of Gertler and Gruber (2002) and Jack and Suri (2014), where we regress the outcome of interest on an indicator of whether household i experienced a shock during the past 12 months, a vector of demographic characteristics, household fixed effects and village-month fixed effects. Panel D of Table B1 reports estimates following this approach. It is worth noting that, as opposed to our main specification, this approach ignores age-specific trends and does not guarantee that the comparison households—non-shocked households at time t—do not suffer a shock within 12 months of the shocks to the focal household. Reassuringly, we obtain qualitatively similar results.

A.2.2 Shocks to household budget or household labor supply?

Appendix Figure A2 shows that 50% of the shocks affected family members that were 52 or older, and that 10% of the shocks affected children under the age of 18. Thus, the majority of shocks are related to illness of non-prime age household members. Appendix Table A2 shows that in the month before experiencing the shocks, on average, affected individuals spent most of their days helping with household chores rather than working in family businesses. For the subsample of shocks affecting non-prime age household members, we interpret the shocks as primarily financial shocks. In contrast, around 40% of the events relate to household members in prime-working age, and we interpret this subset of shocks as affecting both spending needs and labor endowments.

A.3 Treatment and control groups for indirect effects (alternative approach)

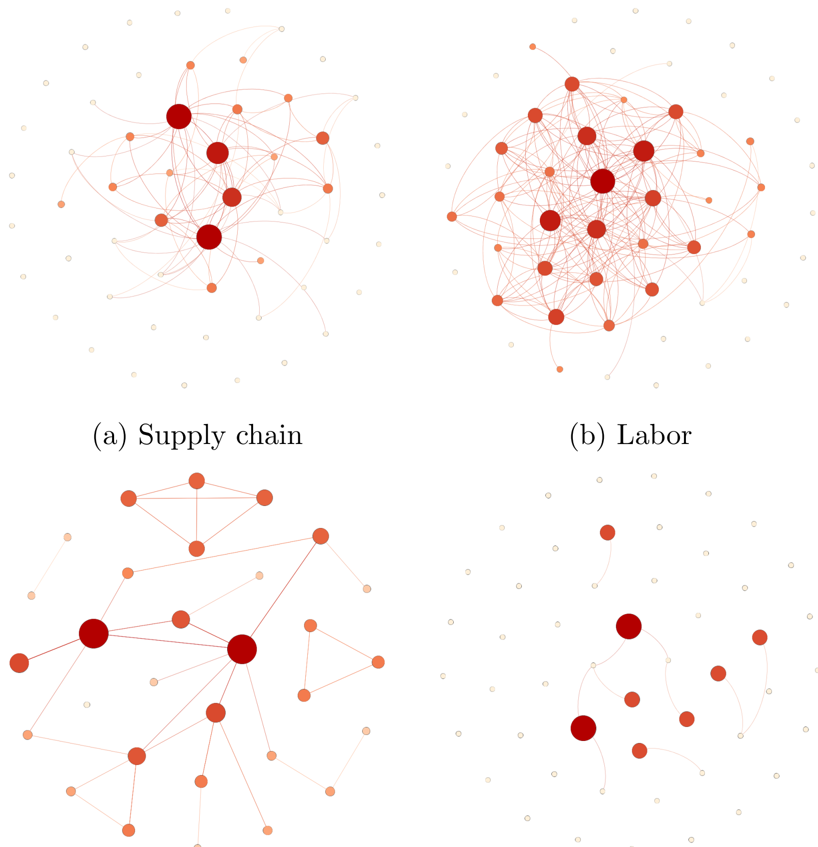
Here we discuss additional details related to measuring indirect effects à la Fadlon and Nielsen (2019). In the spirit of the design used to study direct effects, we compare households that are close to a household (j) that experienced a shock in period t to households that were also close to a placebo 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 to that of our approach in Section 3.1.1. 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 through direct or indirect pre-period links to the shocked household). We focus on households either directly or indirectly connected to shocked households through the pre-period network for two reasons. Fist, Figure B3 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.

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.

B Supportive evidence

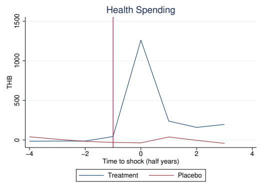


(a) Supply chain (b) Labor

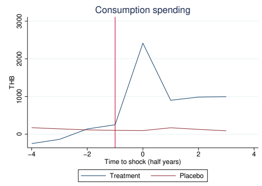
(c) Kinship (d) Financial

Figure B1: Socioeconomic Networks for a sample village

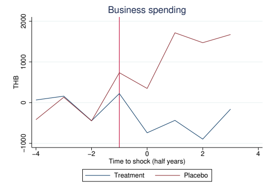
Note: The Figure depicts undirected, unweighted networks corresponding to a sample village in our sample. Each dot represents a node. The size of the node increases with the number of links of each node. Each link represents whether two households have transacted during the reference period. The transaction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



(a) Health Spending

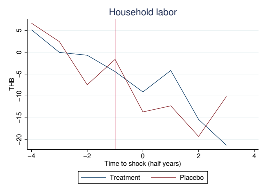


(b) Total consumption spending

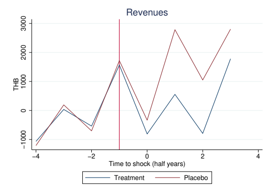


(c) Business spending

(d) Hired labor (hrs/-month)



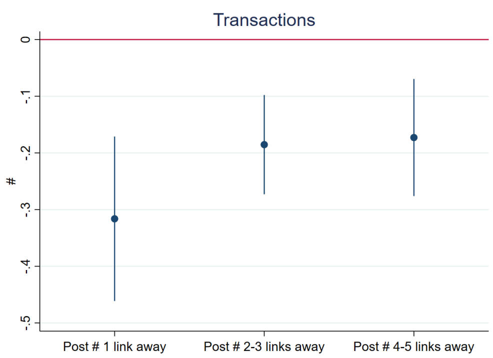
(e) Household labor (hrs/-month)



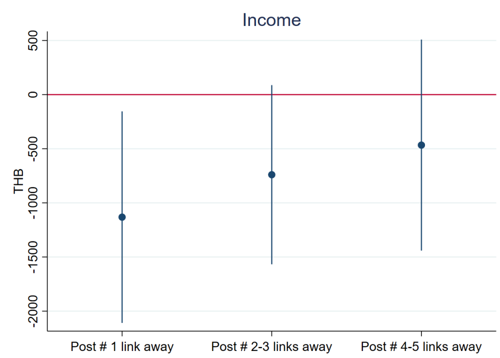
(f) Revenues

Figure B2: Changes in household outcomes before and after the shock

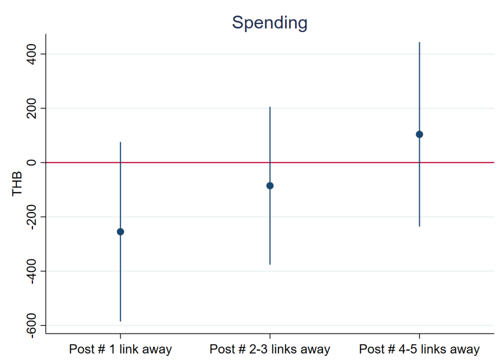
Note: The Figure plots means of average monthly consumption, savings, cash holdings, and incoming gifts for the four quarters preceding and following the shock. All variables are normalized with respect to the pre-shock mean. Period $\tau = -1$ denotes the half year preceding the shock onset. Total consumption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



(a) Total transactions



(b) Total income



(c) Spending

Figure B3: 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 B1: Robustness: Alternative specifications (Direct shocks)

Panel A: Onset of shock: 3 months after a symptom is reported for the first time

	(1) Health Spending	(2) Total spending	(3) Biz. spending	(4) Hired labor (Hrs/Month)	(5) HH Labor (Hrs/Month)	(6) Revenues
Post X Treatment	499.6	780.7	-1620.8	-10.72	-8.918	-1205.2
	(83.60)	(327.1)	(827.1)	(5.631)	(8.996)	(1089.9)
Baseline mean (DV)	147.9	5397.2	7561.6	19.01	155.5	14643.0
Observations	22643	22643	22643	22642	22642	22643
Number of events	246	246	246	246	246	246
R-Squared	0.0534	0.172	0.778	0.557	0.710	0.618

Panel B: Onset of shock: 3 months before a symptom is reported for the first time

	(1) Health Spending	(2) Total spending	(3) Biz. spending	(4) Hired labor (Hrs/Month)	(5) HH Labor (Hrs/Month)	(6) Revenues
Post X Treatment	284.8	53.82	-1743.7	-15.31	-11.43	-1621.3
	(90.29)	(402.8)	(850.0)	(8.367)	(8.463)	(1076.9)
Baseline mean (DV)	214.5	5525.7	7618.8	17.17	151.6	14977.5
Observations	22742	22742	22742	22741	22741	22742
Number of events	246	246	246	246	246	246
R-Squared	0.0483	0.158	0.784	0.584	0.717	0.620

Panel C: Random assignment of placebo shocks

	(1) Health Spending	(2) Total spending	(3) Biz. spending	(4) Hired labor (Hrs/Month)	(5) HH Labor (Hrs/Month)	(6) Revenues
Post X Treatment	449.3	703.5	-972.1	-6.460	-14.62	-1667.3
	(64.62)	(324.4)	(473.8)	(5.607)	(6.011)	(633.2)
Baseline mean (DV)	198.1	6186.2	7707.3	17.13	142.1	15193.4
Observations	43194	43194	43194	43193	43193	43194
Number of events	469	469	469	469	469	469
R-Squared	0.0619	0.119	0.779	0.735	0.699	0.572

Panel D: Two-way fixed effect estimation (using only shocked households)

	(1) Health Spending	(2) Total spending	(3) Biz. spending	(4) Hired labor (Hrs/Month)	(5) HH Labor (Hrs/Month)	(6) Revenues
Post	523.3	104.8	-465.3	-1.906	-5.474	-970.0
	(88.60)	(333.7)	(246.9)	(1.370)	(3.205)	(429.7)
Baseline mean (DV)	161.9	6306.1	7938.9	18.92	142.9	15869.2
Observations	21362	21362	21362	21361	21361	21362
Number of events	469	469	469	469	469	469
R-Squared	0.0248	0.0765	0.781	0.764	0.762	0.582

Note: Panels A to C report OLS estimates of β from equation (2) for different outcomes under different specifications. Each column reports differences between treatment and placebo households in changes in outcomes before and after the shock. All regressions include a vector of demographic characteristics as well as household and month fixed effects. Panel A assumes that the shock starts three months after the health symptoms are reported. Panel B assumes that the shock starts three months before the symptoms are first reported. Panel C reports estimates based on a random selection of the timing of the placebo shocks. Panel D reports estimates of a regression of the dependent variable on household and time fixed effects and a dummy variable that takes the value of 1 if a household experienced the onset of the shock during the past 12 months. See section A.2.1 for more details. Standard errors are clustered at the household level.

Table B2: Robustness: Only including shocks to older adults (above the median age of 57)

Panel A: Using shocks to older adults occuring in the first half of the sample.

	(1) Health Spending	(2) Total spending	(3) Biz. spending	(4) Hired labor (Hrs/Month)	(5) HH Labor (Hrs/Month)	(6) Revenues
Post X Treatment	335.2	270.3	-2805.6	-31.69	-0.868	-1850.1
	(85.44)	(464.5)	(1599.0)	(18.64)	(13.47)	(1990.1)
Baseline mean (DV)	127.7	5432.7	7807.2	30.88	184.3	15291.6
Observations	8764	8764	8764	8764	8764	8764
Number of events	95	95	95	95	95	95
R-Squared	0.0513	0.200	0.728	0.620	0.769	0.612

Panel B: Using all shocks to older adults

	(1) Health Spending	(2) Total spending	(3) Biz. spending	(4) Hired labor (Hrs/Month)	(5) HH Labor (Hrs/Month)	(6) Revenues
Post X Treatment	305.4	-32.21	-2442.0	-18.02	-14.07	-2753.1
	(58.16)	(398.2)	(975.4)	(10.82)	(10.15)	(1222.4)
Baseline mean (DV)	120.5	6033.4	7598.8	22.86	166.7	15508.4
Observations	15598	15598	15598	15598	15598	15598
Number of events	164	164	164	164	164	164
R-Squared	0.0679	0.185	0.711	0.601	0.724	0.522

Note: The table reports estimates of β from equation (2) for different outcomes for the subsample of shocks affecting adults who were older than the median age of 57 among shocked adults. Each column reports differences between treatment and placebo households in changes in outcomes before and after the shock. Hired and household labor are measured in hours per month. All regressions control for household demographic characteristics, household and village-month fixed effects as well as flexible time-to-treatment trends by access to informal insurance. Standard errors are clustered at the household level.

Table B3: Robustness: Propagation through village networks (village X month FE)

VARIABLES	(1) Input/Output	(2) Hired labor	(3) All transactions	(4) Income	(5) Total spending
Post X closeness (village network)	-0.185	-0.083	-0.268	-825.242	-68.954
	(0.045)	(0.027)	(0.055)	(455.417)	(152.575)
Observations	410,578	410,578	410,578	410,578	410,578
R-squared	0.499	0.319	0.448	0.253	0.637
Pre-period Mean	0.999	0.470	1.469	10486	7265
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 village-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.

B.1 Coping mechanisms in response to shocks

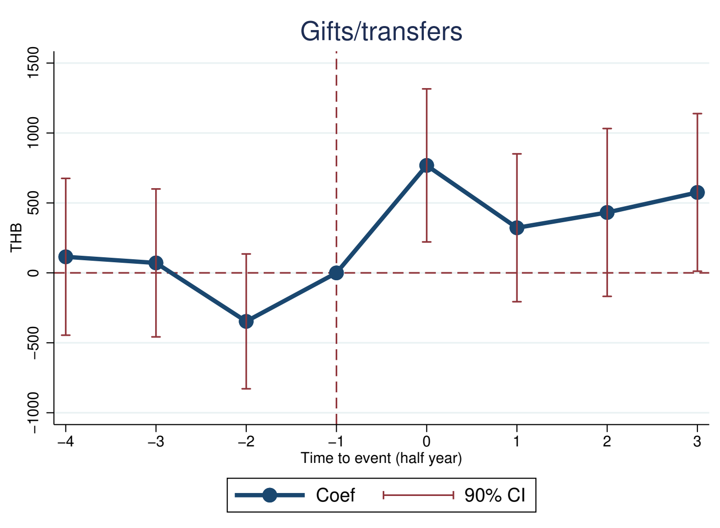


Figure B4: Incoming gifts

Note: The figure reports coefficients from equation 1 for incoming gifts/transfers. Each dot represents differences between treatment and placebo 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 B4: Response to shocks: coping mechanisms

Panel A: Direct effects					
	(1) Incoming gifts	(2) Borrowing	(3) Fixed assets	(4) Liquid assets	(5) Incoming unpaid labor
Post X Treatment	574.6	115.6	-6135.7	-16060.0	1.867
	(220.7)	(262.1)	(6069.7)	(22930.8)	(1.579)
Baseline mean (DV)	1957.3	234.4	124858.8	370556.0	6.253
Observations	22709	22709	22709	22709	22709
Number of events	246	246	246	246	246
R-Squared	0.159	0.0105	0.894	0.885	0.212
Panel B: Indirect effects					
	(1) Incoming gifts	(2) Borrowing	(3) Fixed assets	(4) Liquid assets	(5) Incoming unpaid labor
Post X closeness (village network)	-100.4	-87.00	-6289.6	-17631.4	-0.976
	(126.2)	(127.0)	(4544.9)	(20014.7)	(1.038)
Baseline mean (DV)	2295.0	19.37	125705.9	410834.6	5.876
Observations	410578	410578	410578	410578	410575
Number of events	391	391	391	391	391
R-Squared	0.137	0.0392	0.807	0.818	0.269

$***p < 0.01, **p < 0.05, *p < 0.1$

Note: Panel A reports estimates of β from equation (2) for different outcomes. Each column reports differences between treatment and placebo 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.

B.1.1 Idiosyncratic vs. aggregate shocks

To illustrate the idea that local networks may be less able to provide insurance against aggregate shocks, Figure B5 provides a graphical comparison of the direct responses to idiosyncratic vs. aggregate shocks. We consider the timing of the European Union ban on Thai shrimp imports, which was announced in May 2002 and directly affected over 30% of the households in Chachoengsao, the shrimp-producing province in our dataset.[4] The "sectoral" line in the figure depicts differences in changes in incoming gifts, before and after the EU ban, between shrimp farmers and non-shrimp farmers.[5] The "idiosyncratic" line depicts changes in incoming gifts before and after health shocks, relative to a placebo group as in equation (2). There is only a small, insignificant increase in gifts within a year of the shrimp ban, in marked contrast to the sudden increase in gift inflows in the aftermath of the idiosyncratic shocks. This simple comparison suggests that the effectiveness of local risk-sharing networks depends on the nature of the shock and echoes our finding that, while households directly hit by idiosyncratic health shocks see an increase in gifts (as seen in the figure), indirectly-shocked households do not, as the indirect effects are quasi-aggregate, hitting many households in the network.

[4] Giannone and Banternghansa (2018) show that the EU ban lead to significant declines in revenues, to spillovers to non-shrimp households, and to reallocation of resources towards non-shrimp businesses.

[5] In this case, we estimated a regression of incoming gifts on time-to-treatment fixed effects, the interaction of time-to-treatment fixed effects with an indicator of whether the households operated shrimp farms at baseline, and household fixed effects. We plot the coefficient associated with the interactions.

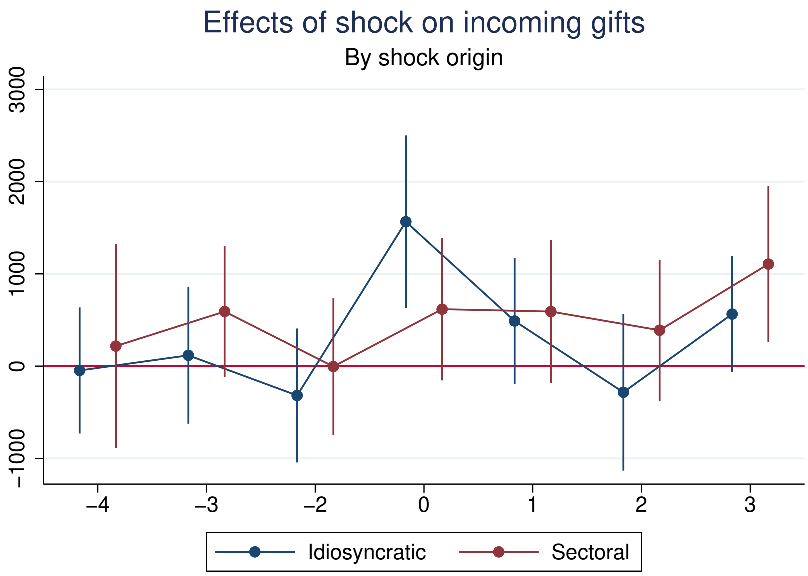


Figure B5: Effects on gift receipt by type of shock

Note: The figures depict flexible difference-in-difference estimates of the effects of idiosyncratic shocks on incoming gifts (in blue) and of the EU shrimp ban on incoming gifts (in red). The figures focus on households in the 4 villages corresponding to the Chachoengsao province, where most of the shrimp activity takes place. The European Union import ban on Thai shrimp was announced in May 2002. The the effects of idiosyncratic shocks are estimated using the specification detailed in equation (2). The effects of the shrimp-ban shock are estimated using a regression of incoming gifts on month fixed effects, normalized with respect to May of 2002, and interactions of month fixed effects with an indicator that takes the value of 1 if the household had a shrimp farm before the shock. In this case, the effects of the shock are captured by the plotted interaction coefficients. In both cases, standard errors are clustered at the households level and are used to plot 90% confidence intervals.

B.2 Effects of health shocks by participation in informal insurance networks

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. We 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:[6]

$$\begin{aligned} y_{i,t} = & \beta_1 Post_{i,t} \times Treatment_i \times Low_i + \beta_2 Post_{i,t} \times Treatment_i \times High_i \\ & + \theta_1 Post_{i,t} + \theta_2 Post_{i,t} \times High_i + X_{i,t}\kappa + \alpha_i + \delta_t + \epsilon_{i,t} \end{aligned} \quad (6)$$

where $y_{i,t}$, $Treatment$ and $Post$ are defined as in Section 3.1.1. $High_i$ 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

[6]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.

following model:

$$
\begin{aligned}
y_{i,t,j} = {} & \beta_1 Post_{t,j} \times Closeness_{i,j} \times Low_j + \beta_2 Post_{t,j} \times Closeness_{i,j} \times High_j \\
& + \beta_3 Post_{t,j} \times High_j + \beta_4 Closeness_{i,j} \times High_j + \gamma Closeness_{i,j} \\
& + X_{i,t,j}\kappa + \alpha_i + \omega_j + \delta_t + \theta_\tau + \epsilon_{i,t,j} \qquad (7)
\end{aligned}
$$

where $High_j$ is an indicator of whether directly shocked household j had above-median pre-period access to informal insurance networks, defined as above. 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 ($High_j = 1$).

Panel B of Appendix Table B5 presents the results. We note that statistical power to detect heterogeneous effects is limited and that these results should be regarded as suggestive. Columns 1 to 3 show that there are similar declines in the transactions of indirectly affected households between shocks to households with high and low access to insurance. However, Column 4 shows that when the shocked household had low access to insurance in the pre-period, the fall in income associated with 1 unit greater *Closeness* is 1705 baht. When the shocked household had high access to insurance, the fall in income is 1016.3 baht. That is, the propagation effects on income when the shocked household has low access to informal insurance are 1.67 times the propagation effects on income when the shocked household has higher access to insurance. Finally, column 5 shows that the consumption spending of indirectly affected households falls by 462 baht, or roughly 6%, when the shocked household had low access to insurance in the pre-period. When the shocked household had high access to insurance, however, the fall in consumption spending is reduced by only 275 baht. These patterns are robust to measuring access to insurance networks by whether the shocked household transacted in the village gift/loans network during the preperiod (see Appendix Table B6).

Table B5: Effects of health shocks by participation in informal insurance networks

Panel A: Direct effects of the shocks by pre-period access to informal insurance					
	(1) Total hh spending in health	(2) Gifts+Loans	(3) Costs	(4) Hired labor (Hrs/Month)	(5) Revenues
(1) Low	412.2	343.1	-2471.9	-12.84	-2310.1
	(73.60)	(409.9)	(1019.5)	(7.042)	(1237.0)
(2) High	418.4	882.4	-424.2	-4.841	-1335.3
	(109.1)	(459.0)	(608.0)	(5.128)	(803.3)
Difference (2)-(1)	-6.206	-539.3	-2047.7	-7.997	-974.8
S.E. Difference	129.3	586.4	1230.0	8.032	1500.3
P-value Difference	0.962	0.358	0.0967	0.320	0.516
Baseline mean (DV)	153.1	2952.0	7897.9	13.52	15670.8
Observations	39740	39740	39740	39740	39740
Adj.R-Squared	0.0424	0.0382	0.752	0.541	0.545
Panel B: Indirect effects of the shocks by pre-period access to informal insurance of shocked household					
	(1) Input/Output	(2) Hired labor	(3) All transactions	(4) Income	(5) Total spending
(1) Low	-0.158	-0.114	-0.273	-1705.9	-462.4
	(0.0808)	(0.0660)	(0.111)	(545.9)	(202.0)
(2) High	-0.275	-0.107	-0.381	-1016.7	-187.2
	(0.0695)	(0.0519)	(0.0884)	(545.3)	(199.1)
Difference (2)-(1)	0.116	-0.00774	0.108	-689.2	-275.2
S.E. Difference	0.0892	0.0743	0.124	607.1	228.7
P-value Difference	0.194	0.917	0.384	0.257	0.230
Baseline mean (DV)	0.999	0.470	1.469	10486.0	7264.8
Observations	382590	382590	382590	382590	382590
Adj.R-Squared	0.435	0.231	0.370	0.195	0.618

Note: Panel A reports estimates of β_1 and β_2 from equation (2) for different outcomes. Each column reports differences between treatment and placebo households in changes in outcomes before and after the shock. Panel B presents estimates of β_1 and β_2 from equation (4). We split the sample by access to informal insurance networks. "High" and "Low" refers to the directly-shocked household. Households with above the median gifts-to-income sensitivity during the pre-period are coded as "High" while the rest as "Low". Hired labor is measured in hours per month. Standard errors are clustered at the household level.

Table B6: Robustness: Effects of health shocks by pre-period participation in informal insurance networks

Panel A: Direct effects of the shocks by pre-period access to informal insurance

	(1) Total hh spending in health	(2) Gifts+Loans	(3) Costs	(4) Hired labor (Hrs/Month)	(5) Revenues
(1) No access	358.7	499.3	-2148.3	-6.907	-3346.1
	(82.14)	(379.6)	(694.3)	(5.522)	(920.0)
(2) Access	491.1	778.9	-491.5	-13.55	-25.46
	(91.81)	(518.1)	(846.9)	(8.109)	(1095.4)
Difference (2)-(1)	-132.4	-279.6	-1656.8	6.642	-3320.7
S.E. Difference	121.3	609.7	1086.9	9.496	1442.2
P-value Difference	0.276	0.647	0.128	0.485	0.0217
Baseline mean (DV)	159.5	2862.7	7531.4	16.26	15058.9
Observations	42548	42548	42548	42548	42548
Adj.R-Squared	0.0439	0.0430	0.749	0.668	0.543

Panel B: Indirect effects of the shocks by pre-period access to informal insurance of shocked household

	(1) Input/Output	(2) Hired labor	(3) All transactions	(4) Income	(5) Total spending
(1) No access	-0.261	-0.107	-0.369	-1658.0	-319.1
	(0.0655)	(0.0481)	(0.0852)	(555.0)	(214.8)
(2) Access	-0.143	-0.133	-0.277	-686.1	-250.1
	(0.0895)	(0.0673)	(0.114)	(545.3)	(184.7)
Difference (2)-(1)	-0.118	0.0261	-0.0919	-971.8	-68.94
S.E. Difference	0.0996	0.0782	0.130	648.5	236.3
P-value Difference	0.237	0.739	0.480	0.135	0.771
Baseline mean (DV)	0.999	0.470	1.469	10486.0	7264.8
Observations	405073	405073	405073	405073	405073
Adj.R-Squared	0.441	0.231	0.374	0.196	0.621

Note: Panel A reports estimates of β_1 and β_2 from equation (2) for different outcomes. Each column reports differences between treatment and placebo households in changes in outcomes before and after the shock. Panel B presents estimates of β_1 and β_2 from equation (4). We split the sample by access to informal insurance networks. "High" and "Low" refers to the directly-shocked household. Households that transacted in the gifts/loans network during the pre-period are coded as "High" while the rest as "Low". Hired labor is measured in hours per month. Standard errors are clustered at the household level.

Table B7: Indirect effects of health shocks on gift/transfers to other households

	(1)	(2)	(3)
VARIABLES	Gifts provided	Total gifts	Gifts+Loans
Post X Closeness	-0.007	-45.810	-67.610
	(0.007)	(57.812)	(68.728)
Observations	410,578	410,578	410,578
R-squared	0.066	0.293	0.216
Pre-period Mean	0.0283	903.9	1035
Number of events	391	391	391

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.

B.3 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:

$$\begin{aligned} Link_{i,j,t} =& \rho Link_{i,j,t-1} + \gamma_1 Kinship_{i,j} + \gamma_2 Demographic\ distance_{i,j} \\ & + \gamma_3 Net\text{-}Worth\ distance_{i,j} + \delta_{v,t} + \alpha_i + \alpha_j + \epsilon_{i,j,t} \end{aligned} \tag{8}$$

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.[7] 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).[8] 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 B8 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-

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

[8]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.

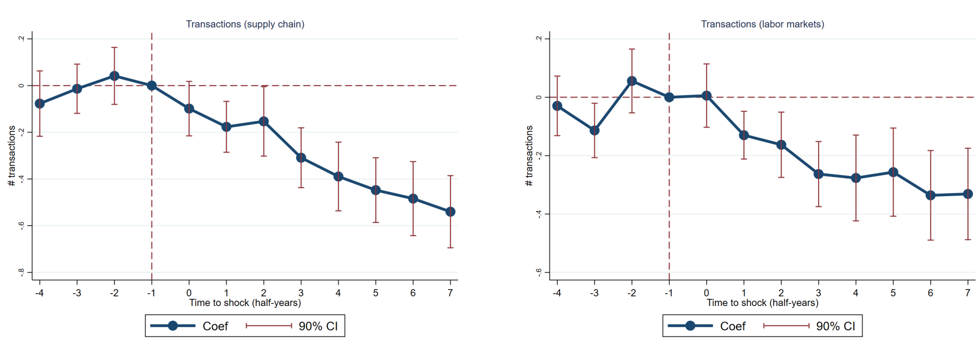
chosen nodes in the network transact in a given year (0.051, 0.061 and 0.012 in the supply chain, labor market and gift/loan networks, respectively). In the case of the labor market and the supply chain networks, having transacted during the previous period explains one-fifth of the overall variation 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 4.2 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.

Table B8: Persistence in transaction networks, by network type

	Probability of a direct link											
	Supply chain				**Labor market**				**Gifts or loans**			
VARIABLES	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Lagged Prob. of link (ρ)	0.469	0.460	0.379	0.379	0.426	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			0.099	0.099			0.109	0.109			0.091	0.091
			(0.006)	(0.006)			(0.007)	(0.007)			(0.006)	(0.006)
Demographic (log distance)				-0.014				-0.107				0.141
				(0.120)				(0.131)				(0.071)
Net-worth distance (log of squared differences)				-0.037				-0.006				-0.035
				(0.027)				(0.031)				(0.017)
Mean DV	0.051				0.061				0.0123			
Village fixed effects	No	Yes	Yes	Yes	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Year fixed effects	No	Yes	Yes	Yes	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Household fixed effects	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes
Observations	233,240	233,240	233,240	233,240	233,240	233,240	233,240	233,240	233,240	233,240	233,240	233,240
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

Note: The table presents regression coefficients following the specification in equation (8). 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. Column (1) presents raw correlations, Column (2) includes village-year fixed effects. Columns (3) and (4) control for kinship first-degree connections, 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 B6: 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.

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 inpatients 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`