Propagation and Insurance in Village Networks

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Firms in developing countries are embedded in supply chains and labor networks. These linkages may propagate or attenuate shocks. Using panel data from Thai villages, we document three facts: as households facing idiosyncratic shocks adjust their production, these shocks propagate to other households on both the production and consumption sides; propagation is greater via labor than supply chain links; and shocks in denser networks and to more central households propagate more, while access to formal or informal insurance reduces propagation. Social benefits from expanding safety nets may be higher than private benefits.
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Most small enterprises in low- and middle-income countries (LMICs) are owned by individual, un-hedged households. The resulting lack of separation between production and consumption means that shocks to household spending needs can lead to production-side adjustments (LaFave and Thomas, 2016). These adjustments may in turn affect other firms, propagating shocks through local production networks. Additionally, because households in LMICs often engage simultaneously in self-employment (running a firm) and wage work (working for others' firms), these production networks go beyond supply chains. They also involve firm-to-firm linkages through a labor market network of employer-employee transactions. As shocks propagate, there may be both production- and consumption-side indirect effects on the owners of other, linked, firms. Considering these responses — direct and indirect, and production- and consumption-side — together, as well as the mechanisms behind them — propagation through supply chains or labor networks— is crucial for a full understanding of the welfare consequences of shocks and of safety nets.

We shed light on these issues, documenting three key facts. First, we empirically show that health shocks to households lead to direct production-side adjustments, which in turn propagate through local networks, affecting linked household firms in terms of production-side outcomes (e.g., local transactions, income) as well as consumption. Second, we distinguish propagation via supply chains vs. via less-studied labor networks, and show that the extent of labor network propagation is greater. Third, we examine how the economic environment (network structure, access to formal and informal insurance, and frictions in labor markets) determines to what extent shocks are amplified or attenuated.

The setting we study is uniquely suited to answer these questions. The Townsend Thai data, constructed from 14 years of monthly panel survey data on households in 16 rural and peri-urban Thai villages, contain detailed information regarding intra-village transactions and transfers among family-operated businesses. We use these data to construct employer-employee (labor) and supply chain (goods) networks. We also have detailed information on firm performance and household consumption. The data additionally allow us to identify large, idiosyncratic shocks in the form of changes in health status, and to construct a valid counterfactual to obtain causal estimates of these shocks' effects. These elements together provide an ideal setting to shed light on the role of networks in propagating and mitigating shocks.

We show that an idiosyncratic shock has a significant effect on the business activities of the directly affected household. We follow Fadlon and Nielsen (2019) and compare the changes in outcomes of shocked households before versus after the shock, to those of other "control" households, who will experience a similar shock, but later on.[1] Shocked households are able to smooth consumption (non-health household spending). However, this smoothing is achieved by reducing business spending (by 23%), and almost entirely cutting demand for external labor (by 79%). This finding validates that in our setting, separation between the consumption and production sides of households' balance sheets fails to hold.

A lack of consumption-production separation has also been found in other settings (see, e.g., LaFave and Thomas 2016; Jones et al. 2022). Our contribution is to document three main findings. First, we leverage variation in the proximity of a given household to the shocked household through pre-shock networks to causally identify indirect impacts via propagation. We compare changes in outcomes before versus after the shock, between those more exposed (closer to the shocked household in the pre-shock network) and less exposed (farther away). Thus, we are able to quantify propagation at the micro level and trace its evolution within a network (village).

We find that idiosyncratic shocks propagate on both the production and consumption sides. Households with greater indirect exposure see larger falls in total

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

transactions (a 21% decline for a unit change in closeness), and falls in income and consumption (12% and 4% declines for a unit change in closeness, respectively). The indirect effects are largest for households located one link away from the shocked households. However, these first-order indirect effects also propagate further, affecting households located two or more links away from the shocked household.

The presence of negative indirect effects demonstrates that the strategies used to smooth consumption against direct shocks (cutting business spending on goods and labor) are not only costly from a private perspective — the business scale of the shocked household is slow to recover — but also from a social perspective: the transactions, income and consumption of other households are also depressed. To the best of our knowledge, documenting *consumption-side* effects of firm-to-firm propagation is new in the literature.

We also provide evidence on how shocks propagate differentially through supply chain and employer-employee networks. We show that the propagation effect is stronger in labor market networks. While inputs and outputs can also be traded outside the village, the local nature of labor markets and the high monetary and non-pecuniary costs of temporary migration (Lagakos et al., 2022) may prevent indirectly-shocked households from adapting to the labor market disruptions caused by health shocks. This finding is consistent with the theoretical insights of Baqaee and Farhi (2020) that propagation is more severe when inputs are not substitutable, and demonstrates that the effects of propagation via supply chain networks cannot be directly extrapolated to propagation via other linkages. While the propagation role of supply chains in advanced economies is increasingly well understood (e.g., Carvalho et al. 2021), the role of labor market networks has not, to our knowledge, been studied.

To contextualize our results, we show that propagation occurs in the context of rigid local networks, in which the links between households exhibit a substantial level of persistence. Indirectly shocked households do not appear to replace "broken" links even two years after a shock. Existing links are vulnerable to shocks, and replacing these links appears challenging: suppliers struggle to find new customers when a client suffers a shock, and workers cannot easily find new jobs when existing employers scale back demand. As shocks propagate through village networks, their effects have important aggregate consequences. A back-of-the-envelope calculation yields a multiplier of 1.2.

We additionally show how features of the economic environment affect the indirect consequences of shocks. We first focus on whether shocks have larger indirect effects in denser networks. It is *a priori* ambiguous how the density of a network will affect propagation: on one hand, households interacting in denser networks may be able to replace broken links with the shocked household more quickly and easily. However, they may also be more exposed to the indirect effects of such shocks. Using cross-village variation in density across the 16 village networks in our data, we find that the indirect effects of idiosyncratic shocks on

the *average* non-shocked household are amplified in denser networks, even after controlling for network size. Likewise, shocks to more central households—those with more links to other households before the shock—lead to larger negative indirect effects. These results suggest that network structure mediates the extent to which idiosyncratic negative shocks can translate into aggregate demand shocks, and that policies aiming to minimize aggregate fluctuations due to idiosyncratic shocks should be targeted either at more interconnected local economies or at central households.[2] These findings suggest a tradeoff: while increasing linkages among households may strengthen the insurance capacity of networks (Feigenberg, Field and Pande, 2013), increased links may also decrease resilience to propagation. Likewise, encouraging links with central households may promote information diffusion (Beaman et al., 2021), but may increase shock propagation as well.

We provide suggestive evidence on the roles of formal and informal insurance in attenuating propagation. Shocks to households who are better insured, formally or informally, propagate less. Access to insurance for directly shocked households can prevent costly production-side actions which drive propagation. These results underscore the distinct roles of village networks: supply-chain and labor networks contribute to propagation, while insurance networks contribute to attenuation.

Our work contributes to the literature analyzing the aggregate implications of microeconomic shocks. Existing evidence largely focuses on the propagation of sectoral shocks via supply chain networks among larger firms in high-income countries.[3,4] In LMICs, small, family-operated firms make intertwined consumption and production decisions; are exposed to shocks not typically faced by large firms (e.g, shocks to family health and wealth); and participate in multiple networks. A unique setting allows us to document the consequences of these shocks for the local economy. In our setting, households smooth consumption amid idiosyncratic shocks by making costly production-side adjustments, which, in turn, negatively affect the consumption levels of other households.

Finally, our results provide insights related to the mechanisms behind multiplier effects—a change in aggregate activity greater than the direct effect alone. Several studies document multiplier effects arising from large inflows of cash into local economies in both high- and low-income countries (e.g., Nakamura and Steinsson 2014; Chodorow-Reich 2019; Egger et al. 2021). Our approach enables us to

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[2]Allen and Gale (2000) and Elliott, Golub and Jackson (2014) examine the role of network structure theoretically in the context of links between financial intermediaries while Bigio and La'o (2020) examine the case of input-output networks. To the best of our knowledge the role of network structure in propagation among smaller firms has not previously been examined nor have these effects been tested empirically.

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

[4]Also related are several studies of the adjustment of larger, formal firms in LMICs to shocks, e.g. Khanna, Morales and Pandalai-Nayar (2022), who examine how supply chain networks among registered (i.e., larger) establishments within an Indian state respond to the aggregate shock of COVID-19 border closures, and Felix (2022) who studies the labor market consequences of trade exposure in Brazil for formal sector workers.

contribute with a novel insight: *idiosyncratic* shocks can amplify and generate multiplier effects similar to those generated by large inflows of resources. In addition, while recent work in LMIC contexts identifies equilibrium channels such as wages (see, e.g., Egger et al. 2021; Franklin et al. 2021; Breza and Kinnan 2021) and prices (e.g., Burke, Bergquist and Miguel 2019; Cunha, De Giorgi and Jayachandran 2019) as important for indirect effects, less is known about locally heterogeneous exposure to indirect effects via supply-chain and labor networks.[5] Detailed data on networks enables us to estimate within-village spillovers that are *net* of any effects on wages or prices; a crucial distinction for policy (Guren et al., 2021).

I. Context and Data

A. Household data

Our data come from the Townsend Thai Monthly Survey, which covers 16 villages in Northern and Central Thailand. The sample includes approximately 45 households per village, representing on average 42% of the village population (Townsend, 2010). A baseline interview was conducted from July to August 1998, collecting information on the demographic and financial situation of the households. Monthly updates began in September 1998.[6] We use data which covers the period between September 1998 and December 2012 and focus our analysis on the subset of 510 households who responded to the interview throughout all survey waves.

Panels A and B of Table A1 characterize sample households in terms of demographic and financial characteristics. All households in the sample operate a family firm. Households derive income mostly from family farms, but they also operate off-farm businesses. At the same time, they supply labor to other firms in the village or other external employers. In addition, part of total income comes from the receipt of government transfers and/or gifts from other households.

B. Network data

The data includes detailed information on transactions among households capturing different economic links. In each survey wave, interviewees identify all households in the village with whom they have conducted a given type of transaction.[7] We aggregate the monthly transactions by year to elicit three types of

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

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

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

networks — supply chain, labor, and financial — for each year in the sample. The fact that households operate firms, sell and buy labor to other firms, and provide and receive transfers to and from other households in their village, defines three types of firm-to-firm linkages: input and output (supply chain), employer-employee (labor market), and gifts (financial). Specifically, supply-chain networks capture transactions of final and intermediate goods across firms operated by households in the same village. Labor networks capture cross-household labor flows within the village.[8] Financial networks are defined by gifts and loans across households in the same village. We also construct kinship networks, which are measured in the initial survey wave. Appendix Figure A1 depicts these networks for one sample village in one year.

Panel C of Table A1 summarizes network participation across the sample as a whole. Just above half (51%) of the households transact in the local village supply-chain network by trading inputs and final outputs, with 1.36 connections on average per year; and 66% exchange labor with other households in the village, with 3.33 connections on average. An average of 38% of households participate in the financial (i.e., gift/loan) village networks, and 77% of households have at least one relative in the village.

Households participate in several networks within a given period. For those linked through gift/loan networks, over 60 % also transact in supply-chain networks and over 76% of them transact in labor markets. Over 77% of households transacting in the village supply-chain networks also sell/purchase labor locally, and 46% are linked through local financial markets.

Table A1 Panel D concerns the size of sample villages and firms. The average village has 161 households. There is evidence of excess kurtosis in the firm size distribution (measured via gross revenues): average village-level kurtosis is 10 (excess kurtosis of 7). Following Gabaix (2011), excess kurtosis suggests that idiosyncratic shocks may have important aggregate effects in our setting.

C.   Constructing idiosyncratic shocks

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

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[8]Outgoing labor flows (labor sales) arise when some members work in the household business and others work for other local businesses, or when the same household member(s) divide their time between the household business and working in local labor markets. Incoming labor flows (labor purchases) arise when household businesses hire from the local labor market.

hitting other connected households via propagation. Additionally, the timing of these shocks is—as we show below—plausibly exogenous.

We identify shocks as follows (see Appendix B for details). For each household, we identify the month with the highest level of health spending over the panel.[9] We focus on the largest shocks, because they pose a significant financial burden to the household. To account for potential anticipation effects, we define the beginning of an event by subtracting the number of months preceding the episode of high health spending during which household members reported health symptoms from the month corresponding to the episode. Thus, although we use health spending data to identify shocks, the *timing* of the onset of the shock is coded based on changes in health status.[10]

We restrict this search to health spending episodes occurring during years 3–12 of the panel (out of 14 years). This enables us to observe at least two years both pre- and post-shock. By construction, we identify one shock per household, ensuring that the propensity of experiencing a shock is not correlated with characteristics such as health, income or network position. We exclude shocks related to childbirth, which may be planned/anticipated before the onset of any symptoms, leaving 476 shocks.

D. *Characteristics of the shocks*

Relationship between health spending and health status. A natural question is how our spending-based shock measure correlates with changes in health status. Figure B2 shows that self-reported symptoms co-move with health spending, confirming that shocks are identifying decreases in household health. Figure B3 reports the distribution of behaviors and symptoms reported by shocked households during the year following a shock and during non-shock periods. Usage of health facilities, particularly hospitalization, is substantially higher after shocks, and there is a greater likelihood of a household member suspending their daily activities for one, seven, 14 or 21 days, consistent with shocks capturing sharp changes in health. In addition there is a higher incidence of uncommon symptoms (which tend to be more severe in this context).

Magnitude of the shock. These shocks represent a substantial financial burden to affected households. On average, the highest level of monthly health spending within 6 months after the onset of the shock (THB 7647) accounted for 99% of the average monthly consumption during the 6 months preceding the shock (THB 7696) and was substantially larger than the average monthly food consumption during this period (THB 2915).

Are the shocks idiosyncratic? Our analysis requires that the events be id-

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

[10]Appendix Figure B1 shows that, prior to the sudden increase in health spending, the median number of consecutive months in which households report any health symptoms is three. More details are in Appendix B.

iosyncratic and their occurrence be uncorrelated with trends in household behavior or network/sectoral shocks. The top panel of Appendix Figure B4 shows that event start dates are spread evenly through the periods in the sample. Indeed, in over 87% of the cases, shocks affected only one household per village at any one month (bottom panel). In the bottom panel of Appendix Table B1, we formally show that village-level trends have null predictive power on the the occurrence of these events ($p = 0.39$).

Are the shocks exogenous? Column 1 of Appendix Table B1 shows within-household correlations of the probability of experiencing a shock in period $t + 1$ and contemporaneous financial characteristics, standardized with respect to the sample mean and standard deviation. We are unable to reject the null of joint significance ($p = 0.281$). In addition, the null is only rejected in one out of 13 cases at 10%.[11] The point estimates are also small; the largest point estimate suggests that a 1 standard deviation increase in the value of land at t would increase the probability of experiencing a shock in $t + 1$ only by 0.002. Column 2 reports p-values of the null hypothesis that the 12 lags of each household characteristic do not precede the onset of the shock (i.e., a Granger causality test). This hypothesis does not only require that households' characteristics in t do not predict the occurrence of shocks in $t + 1$, but also that shocks occurring up to a year before the shock do not predict shock occurrence. We only reject this hypothesis at the 10% level for 1 out of the 13 variables. These patterns demonstrate that the timing of shocks is orthogonal to pre-shock family and business financial decisions. The next section discusses our empirical approach and addresses other potential identification concerns.

II. The direct effects of idiosyncratic shocks

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

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

We compare the behavior of households who experienced a shock in period t (i.e., treated households) to that of households from the same age group and village who did not experience a shock at time t, but did experience a similar shock

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

later on, in period $t + \Delta$ (control households).[12] Treated households are those who experienced the shock during the first half of the panel; control households experienced a shock during the second half.

We employ a difference-in-difference approach to compare changes in outcomes before and after the shock, between treatment households (who experienced an actual shock) and control households (who are assigned a placebo shock Δ periods before the occurrence of their actual shock).[13] The underlying assumption is that, in the absence of the shock, the treatment and control groups would have followed parallel trends, which we validate by testing for lack of systematic differences before the shock (parallel pre-trends).

A. Estimation

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

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

where $y_{i,t}$ denotes the outcome for household i at t. Household- and month-fixed effects (α_i and δ_t) absorb time-invariant household characteristics and aggregate time-varying shocks. $Treatment_i$ is a time-invariant indicator of whether the household is in the treatment group. As each household is either observed in the treatment or comparison group, $Treatment_i$ is absorbed by the household-fixed effects. Time to treatment is denoted by $\tau_{i,t}$ and is measured in half years to increase precision. X is a vector of time-varying demographic characteristics including the number of male and female household members, age of the household head, and maximum years of schooling in the household. The coefficients of interest are $\{\beta_\tau\}_{\tau=-4}^{\tau=3}$, which compare differences in changes in outcomes with respect to the period immediately preceding the shock ($\tau = -1$) between households in the treatment and control groups. We focus on a two-year (i.e., four-half year) time window before and after the shock, because our panel is fully balanced during this period. We also use a more parsimonious differences-in-difference specification to compute average effects over a two-year post period:

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

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[12]Δ is determined by taking the midpoint between the months associated with the first and last shocks in each age group-village bin. Its value is approximately five years. See Appendix B.B2 for details.

[13]Thus, if a household in the control group experiences the actual shock in t'', its placebo shock is assigned to period $t'' - \Delta$. See Appendix B.B2 for additional details.

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 to account flexibly for serial correlation.

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

B. Direct effects: Results

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

Figure 1 reports flexible difference-in-difference estimates of equation (1). Since in some cases statistical precision is limited due to our empirically demanding estimation strategy, the figure reports both 90% and 95% confidence intervals. 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.[14] Panel (c) shows that non-health consumption remains steady.

The remaining panels show that the shocks affect the household's production-side decisions. Panel (d) shows that, compared to households in the control

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

group, hired labor usage declines for shocked households,[15] and Panel (e) shows that input spending falls. Finally, Panel (f) shows that the reduction in input spending coincides with a reduction in revenues. The graphical evidence also documents parallel pre-trends for all six outcomes.[16]

The onset of the declines in input spending and revenues coincide with the sharp increase in health spending induced by the shock in the half-year of the shock's occurrence. Over this same period, we observe a smooth trajectory of non-health spending. This suggests that shocked households meet short-term liquidity needs in part by drawing down working capital. The effects on input spending and revenues also persist over time. This may partly reflect the suspension of key activities in the household due to the shock, as shown in panel (f) of Appendix Figure A2. A short-term health spending need can also generate persistent effects on business spending through other channels. For instance, households may relinquish their fixed assets, inventories or livestock, inducing a persistent reduction of a business's scale, especially when they exhaust other sources of liquidity. By event period 2, household assets appear to decline (see Appendix figure A5b), although the declines are not estimated with precision.[17] Likewise, shocked firms may not necessarily face the same demand for their products after the shock if consumers deepen their links with other providers. Both channels—reduction in business investment and suspension of activities—suggest that, unlike the case of large firms owned by a diversified set of investors, the business outcomes of small firms owned by un-hedged households respond to health shocks affecting household endowments; a behavior that is inconsistent with the separation theorem (Benjamin, 1992).

To provide a quantitative assessment of the overall impact of the health shocks, in Table 1 we report difference-in-difference estimates of the effect of the shock on outcomes over a 24-month post-shock period, corresponding to equation (2). Panel A restricts the treatment sample to shocks occurring in the first half of the period, ensuring the control group is never treated before or during the comparison window. Column 1 shows that the shocks are associated with a significant increase in the likelihood of reporting a health issue and column 2 shows that the shock leads to a large increase in health spending. While this is by construction, the magnitude, approximately THB 530, is notable, representing a roughly 350% increase relative to the baseline mean (THB 151.8). Column 4 shows that during the two years following the shock, total spending increases for shocked households, relative to control households, by approximately THB 608 on average, an amount close to the effect on health spending. Thus, in terms of non-health spending

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[15]One concern with Figure 1d is that at event period -4, hired labor appears higher for treated households, relative to those experiencing a placebo shock. Appendix Figure A3 reports results including one extra half year in the pre-period. The pre-trend at event time -4 appears to be a one-off deviation, ruling out the concern that systematic pre-trends drive the results.

[16]Appendix Figure A4 shows the same dynamics in the raw data.

[17]Column 3 in panel A on Appendix Table A15, finds a large though not significant decline in total non-cash assets (fixed assets, livestock and inventories).

(column 3), shocked households appear to fully buffer the shocks.

Buffering consumption may entail costly adjustments by shocked households (Chetty and Looney, 2006). Indeed, affected households significantly decrease spending on business inputs (column 5) and reduce the use of external labor (column 6).[18,19] Households also appear to reduce the use of labor provided by household members (column 7), though the effect is not significant when we restrict to shocks occurring in the first half of the period. As a result of these reduced investments in inputs and labor, there is a decrease in the revenues from family enterprises, as seen in column 8, albeit imprecisely estimated (p-value = 0.134).[20]

Panel B reports results that also include early-shocked households as controls for late-shocked households,[21] which roughly doubles the number of events. Reassuringly, the estimates are very similar to those in panel A but are estimated with more precision.[22]

Robustness

Panel A of Table A3 shows robustness to alternative definitions of health shocks.[23] Columns 1 and 2 show that using the largest *change* in health spending throughout the panel to identify episodes of high health spending yields very similar results to the main specification, which is based on the highest spending *levels*.[24] One concern is that our approach may include shocks that, based on their magnitude, are innocuous, despite being the largest shocks experienced by the households throughout the panel. In columns 3 and 4, we exclude shocks associated with expenditure levels that fall in the bottom 75% of the post-shock health spending distribution for control households; the results are similar to those of our main specification. We also employ two alternative ways of selecting shocks:

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[18]Households may also engage in other strategies to cope with the shocks, such as receiving gifts, borrowing, relinquishing fixed and liquid assets, and using unpaid external labor. See Appendix B.B5. We discuss the effects on gifts in more detail in Section IV.C.

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

[20]The point estimates of the effects on business spending and revenues are quite similar at approximately 1,642 THB and 1,514 THB, respectively, suggesting that business profits are unaffected. This can be a consequence of the reduced business scale induced by the decline in business spending. The zero effect on profits may also reflect households responding to the reduction in earnings by taking costly actions that in the short run reduce costs but are harmful in the long term (e.g., deferring needed maintenance of equipment).

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

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

[23]See Appendix Section B.B2 for a detailed description of each shock definition.

[24]This similarity reflects the fact that while we use spending data to identify the episodes (either maximum levels or changes), we use symptoms data to calculate the onset of the shocks; see footnote 10 and Appendix B.

a household-specific benchmark (health spending larger than the pre-shock average food consumption) and a common benchmark (health spending larger than the sample mean plus one standard deviation). Columns 5 to 8 show that we obtain qualitatively similar results. Both approaches select larger shocks and thus yield larger effect sizes, but also use a smaller set of shocks, reducing statistical power.[25]

In columns 3 and 4 in Appendix Table A4, we define shocks based on whether a household member suspended their main activities for at least one week. We use a one-week threshold based on Gertler and Gruber (2002), who show that only severe health shocks yield effects on household spending. As opposed to our main specification, which by construction captures episodes of financial stress with potential effects on time allocation, this alternative definition captures shocks to time availability with potential financial implications. Reassuringly, the results are qualitatively similar.[26] The results are robust to allowing for multiple, non-overlapping shocks per household, as shown in Appendix Table A5.

The results are also robust to using alternative control groups (see Appendix B.B2 for details). Appendix Table A6 columns 1 and 2 show that the results are robust to randomly allocating the placebo shocks. Columns 3 and 4 use all not-yet-shocked households in the same village as controls to shocked households at time t using a stacked difference-in-difference specification. This specification, which uses the not-yet-treated as the control group, is recommended in the recent difference-in-difference literature (Baker, Larcker and Wang, 2022). Columns 5 and 6 show robustness to using not-*currently*-shocked households as controls in a standard two-way fixed effects specification, and columns 7 and 8 report point estimates using Callaway and Sant'Anna (2021)'s estimator with households treated in the second half of the sample as controls.[27] Reassuringly, results using these specifications are consistent with the main specification. Likewise, Appendix Table A7 shows that the effects are unchanged when using an unbalanced panel of households for the main specification.

Finally, Appendix Table A8 presents co-movements between health status and spending using all households in the sample and all survey periods. As expected, changes in health status coincide with changes in health spending. Moreover, changes in health status driven by uncommon health conditions (those more likely to coincide with the spending shocks that we study) predict larger changes in health spending and declines in business spending.

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[25]In Appendix Table A5 we show that power is increased in these specifications when we include multiple, non-overlapping shocks per household (columns 3 and 4). However, doing so comes at the cost of imposing two additional identification assumptions. First, shocks experienced earlier on should not affect the probability of experiencing another health shock in the future and second, the effects of earlier shocks should not have long-lasting effects on the trajectories of outcomes that can lead to violations of the parallel trends assumption.

[26]Note that these effects are less precisely estimated, because this alternative definition identifies fewer shocks. We provide a brief discussion and robustness to alternative definitions in Appendix Section B.B2 and Appendix Table A4.

[27]The corresponding event-study estimates are reported in Appendix Figure A6.

III. Economic networks and the propagation of shocks

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

A. Identifying propagation effects

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

Throughout our sample period, we observe multiple health shocks per village. We construct a data set capturing information of non-shocked households before and after each health shock in the sample. For each event, we take two years of pre- and post-shock observations of households living in the same village as the directly shocked household.[28] We then stack the observations into a data set at the household (i) by time (t) by event (j) level for each village.

We combine this data set with information on network connections between the shocked household (j) and other households (i) in the village, measured during the year preceding the shock to household j. We use pre-shock networks as links may respond to economic shocks themselves. The assumption is that households who transacted with the shocked household during the pre-period, on average, would have been more likely to transact with the shocked households in the post-period, in the absence of the shock.[29]

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

[29] This is consistent with the evidence of persistence in the village networks discussed in Section III.C 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

We measure exposure as the inverse distance in the undirected village network between household i and the shocked household j: $Closeness_{i,j} = \frac{1}{dist_{i,j}}$.[30] As households are further away in the network from shocked households, exposure (closeness) decreases. We begin by computing overall closeness based on transactions in the supply-chain or labor-market networks, because households can be exposed through either network. To distinguish between exposure in the supply-chain and labor-market networks, we also compute measures of closeness in each separate network (see Section III.C).

We elicit economic networks using survey instead of census data (Chandrasekhar and Lewis, 2016). Thus, it is possible that we underestimate the closeness of some sample households to shocked households.[31] Because we may be underestimating exposure—classifying some households as not or less-exposed when they are actually (more) exposed—our results could be biased toward 0. Thus, we interpret our magnitudes as *lower bounds* of the indirect effects of idiosyncratic shocks on other households.

Not all shocked households are active in the local markets for goods and not all shocked households employ or work for other villagers. Thus, we analyze the propagation of shocks through village networks by focusing only on events corresponding to the 410 households who traded in either the supply-chain or labor-market networks during the year preceding their shock out of the 476 shocks in our sample.[32]

We estimate the following generalized difference-in-difference specification:

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

and the parsimonious difference-in-difference specification:

$$y_{i,t,j} = \beta Post_{t,j} \times Closeness_{i,j} + \gamma Closeness_{i,j} + \mathbf{X}_{i,t,j}\kappa$$
(4)
$$+ \alpha_i + \omega_j + \delta_t + \theta_{\tau(j)} + \delta_t \times Degree_i + \epsilon_{i,t,j}$$

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

Townsend, 2007) or between firms (Aaronson et al., 2004) in local economic networks.

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

[31]See footnote 7 for a discussion of this issue.

[32]In Appendix Table A10 we report similar results when we also use shocks to unconnected households (columns 3 and 4), coding closeness equal to 0 for all non-shocked households.

(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.[33] The coefficients of interest in equation (3) are β_τ, which capture relative changes in outcomes corresponding to half year τ with respect to the half year preceding the event ($\tau = -1$) associated with one additional unit of closeness (i.e., between more- vs. less-exposed households). In the generalized difference-in-difference specification, equation (4), $Post_{t,j}$ takes the value of 1 during the two years following the shock to household j and 0 for the pre-period. The coefficient of interest, β, captures differences in outcomes associated with one additional unit of closeness, with respect to the pre-period.

Controls include household fixed effects (α_i); month fixed effects (δ_t); shocked-household fixed effects (ω_j); time-to-shock fixed effects ($\theta_{\tau(j)}$), which account for village-specific time-varying shocks during the analysis window corresponding to the shock to household j; and a vector of time-varying demographic characteristics ($\mathbf{X}_{i,t,j}$).[34] We also control for time-varying trends for more-central households, who could also be more likely to be close to other households, by including interactions of the number of links of household i ($Degree_i$) during the year preceding the shock to j with time fixed effects. Thus, we are in essence comparing two households equally well-connected to the network, one of whom happens to be closer to the shocked household. We use two-way clustered standard errors at the event level j and household level i to allow for flexible correlation across households during the periods preceding and following event j and across responses of the same household i to different events. In order to focus on indirect effects, we drop observations of directly shocked households (where $i = j$) from the analysis and exclude observations of households who experienced their own shock within a year before or after the shock to household j.

The identifying assumption underlying our strategy for estimating indirect effects is that, in the absence of the shock to household j, the outcomes of households i and i', with differential closeness to j, would have evolved following parallel trends, conditional on the vector of controls included in equations (3) and (4). We validate this by testing for (lack of) differences in the pre-period: for $\tau < 0$, we verify that β_τ is not different from 0.[35]

B.   Results: Propagation through economic networks

Figure 2 presents flexible difference-in-difference estimates following equation 3.[36] Panel A analyzes total transactions. After a health shock, households who are

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

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

[35]As discussed below, Panel B of Table 2 shows that proximity in the labor market does not predict changes in supply-chain transactions and vice versa is additional evidence in support of this assumption.

[36]Since in some cases statistical precision is limited due to our empirically demanding estimation strategy, the figure reports both 90% and 95% confidence intervals.

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 III.C) demonstrates that such reorganization of local ties is very difficult, at least over the span of 1–2 years.

Table 2 Panel A shows difference-in-difference estimates corresponding to equation (4). It documents significant post-shock declines in the number of monthly transactions in the supply-chain (column 1) and labor-market networks (column 2), and in total transactions (column 3). These effects are large, representing declines of 19%, 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 820, or 8% of the pre-period mean. In turn, consumption spending falls by THB 207, or 2.7% of its pre-period mean (column 5).[37] The fall in consumption is smaller than the fall in income, suggesting that indirectly shocked households can partly, but not completely, smooth their indirect shock exposure. Moreover, the consumption fall increases over time, suggesting the depletion over time of consumption-smoothing strategies (e.g., savings).[38]

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[37]Recall that these are effects associated with moving from *Closeness* = 0 (unconnected to the directly shocked household) to *Closeness* = 1 (directly linked). The mean level of *Closeness* = 0.42 (see Table 4), so the average indirect effect is 42% of the coefficient.

[38]Why are directly shocked households able to smooth their consumption, while *indirectly* hit households are not? One explanation is that directly shocked households see economically and statistically significant increases in transfers (see Appendix Figure A5a), while indirectly shocked households do not (see Appendix Table A12). This difference in turn may be due to the fact that 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. We return to this issue in Appendix B.B5.

C.   Mechanisms

EFFECTS OF SUPPLY-CHAIN VS. LABOR-MARKET EXPOSURE

In Table 2 Panel B, we examine whether the effect of exposure through the supply-chain network has different effects than exposure through the labor-market network. If proximity through the supply chain (labor) network is associated with changes in input/output (hired labor) transactions, and not vice versa, this is supportive of the identification assumption, because many plausible confounds (e.g., differential trends between closer vs. more-distant households) would manifest in both sets of outcomes. Because the two networks are correlated, we analyze the effect of pre-shock exposure to one controlling for pre-shock exposure to the other.[39] 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 0.24. There is no effect on input/output transactions associated with proximity through the labor-market network. Analogously, column 2 shows that proximity through the labor-market network has a negative and significant effect (-0.20) 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 or the labor-market network generates negative and significant effects on the total number of transactions of -0.22 in both cases.

Columns 4 and 5 show that proximity via the labor-market network is associated with large and significant drops in both income and consumption, while the corresponding effects of proximity via the supply-chain network are small and insignificant. Thus, while shocks propagate through both networks, the severity of the impacts are larger when shocks transmit through labor-market networks. This result underscores the importance of distinguishing between networks and suggests estimates that consider supply-chain networks alone may be lower bounds. **Why do we observe greater propagation via labor networks?** A possible explanation is that, although the absolute effect of exposure via supply-chain networks on input/output transactions is similar to the effect through labor-market networks on labor transactions, the effect on labor-market transactions is larger in relative terms. When shocks propagate through the labor-market, labor-market transactions fall by 43% relative to the pre-period level, while, when the shock propagates through the supply chain, the decline in transactions of inputs and final goods represents 23% of the pre-period mean (see columns 1 an 2 in panel B of Table 2). It may also be more difficult for households to adjust along the intensive margin: in the goods market, fewer but larger within-village transactions or more out-of-village transactions may substitute for the loss of transactions

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[39]On average, 43% of households share a direct or indirect link to the shocked households through both networks, 17% are directly or indirectly linked to the shocked household through only the supply-chain network, 13% are directly or indirectly connected to the shocked households through only the labor-market network and 27% of households are not connected to the shocked households either network.

with the shocked household. In the labor market, working more hours for other employers or forming new employment connections may be more difficult, due to concerns of adverse selection (Barwick et al., 2019), finite time in the day, travel costs, etc. In addition, as we discuss below, shocks reverberate through the networks, reducing the demand for labor at the local level and the local availability of jobs. Selling labor in other villages can be difficult, as temporary migration can be costly (Bryan, Chowdhury and Mobarak, 2014).

The ripple effects of shocks

Indirectly connected households. The declines in overall transactions that we observe are strongest for directly connected households—those one link from the shocked households—but affect indirectly connected households as well (see Figure 3).[40] When, due to a shock, those linked directly to shocked households reduce sales of goods or labor (outgoing transactions to the shocked household), this leads to declines in income and consumption, though less-precisely estimated. As households are consumers but also operate firms, these indirect effects translate into fewer purchases (incoming transactions) of goods, inputs and labor from other households, triggering further propagation through the network. As a result, what was initially a negative shock to one household turns into a negative aggregate demand shock.

Upstream vs. downstream propagation Table 3 shows that the fall in overall transactions documented above is driven by falls in both outgoing transactions (sales of inputs and labor) and incoming transactions (purchases/hiring). In sum, the health shocks that we study generate indirect effects both upstream and downstream, as the costly adjustments taken by the directly shocked household reverberate through local networks. Shocks that are *prima facie* idiosyncratic are spread to other connected households.

Dynamics and persistence.

The indirect effects that we document appear to persist over time, despite the transitory increase in health spending due to the health shocks. This is also true when we expand the post-shock analysis window as in Appendix Figure A7.[41] The existence of relatively persistent declines in business activity among directly shocked households, the inability of households to immediately replace broken

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

[41]Note, however, that a causal interpretation of the results based on a larger analysis window impose additional assumptions. By construction, none of the non-shocked households at event period $\tau = 0$ suffered a direct shock within two years. Thus, a longer-term analysis as in Figure A7 requires assuming that no other household suffered a shock during the post shock period. This assumption is less plausible for longer analysis windows.

links, and the reduced demand for labor at the village level may explain the highly persistent effects on indirectly shocked households.

While the direct effect on health spending peaks within a half-year of the changes in health status, there is evidence of longer term health consequences (see Figures 1a and b). In addition, directly shocked households reduced business spending and relinquished business assets (see Figures 1e and A5b), reducing the business scale. To the extent that replacing such assets takes time, the reduction in business scale may have generated a persistent decrease in labor demand, which could explain the persistent *indirect* effects.

In addition, as directly shocked households reduce their demand for workers and physical inputs, indirectly shocked households may struggle to replace the broken links. Thus, frictions in rewiring economic networks may lead to a large degree of persistence. To document the degree of rigidity in the local networks, we construct a dyadic data set that includes indicators of whether each pair of sample households (dyads) transacted in year t either in the local goods, labor, or financial market and estimate the extent to which past transactions predict future transactions, conditional on measures of similarity and connections based on kinship networks at baseline. (See Appendix B.B4 for details.) Appendix Table A9 shows that labor-market and supply-chain networks exhibit a striking degree of rigidity over time. Column 4 in Panel B shows that dyads linked through the labor-market network at period $t − 1$ are 33 percentage points more likely to transact in period t, relative to unconnected dyads at $t − 1$. This level of persistence is an order of magnitude above the probability that two randomly chosen nodes in the network transact in a given year in the labor market (0.0612) or supply chain (0.0508); the persistence in the supply-chain and labor-market networks is also greater than that seen in the gift and loan network (Panel C).[42]

Finally, as discussed in Section III.C, as the direct effects of the shocks ripple through the networks, the local aggregate demand may have declined. This decline in aggregate demand may have stronger consequences in local labor markets. To the extent that replacing local employers with external employers is costly, the decline in local economic activity may generate these persistent impacts.

D. Robustness

The indirect effects are robust to a battery of alternative specifications. Appendix Table A10 shows results controlling for village-month fixed effects (columns 1 and 2), including shocks to unconnected households during the pre-period (columns 3 and 4), using an unbalanced panel of households to estimate indirect effects (columns 5 and 6), and excluding shocks to large firms to attenuate issues of granularity as in Gabaix (2011) (columns 7 and 8).[43] The results are

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[42]Our results on persistence in labor market networks echo Felix (2022), who documents highly inelastic within-market cross-firm substitution in Brazil.

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

also robust to utilizing alternative identification strategies (see Appendix Section B.B3 for details). Columns 1 and 2 of Appendix Table A11 report estimates from a triple-difference specification using the placebo shocks from Section II.A as a control group.[44] Columns 3 and 4 show results from an alternative identification strategy that parallels our strategy for estimating the direct effects. In this alternative strategy, we compare households who are indirectly shocked for the first time at time t to a control group of households who will be indirectly exposed to a shock for the first time several years in the future. The results are remarkably similar to those from our main specification. Finally, Panel B of Appendix Table A3 shows that our main effects are qualitatively similar, albeit less precisely estimated, when we use alternative definitions of shocks.[45]

IV. The aggregate effects of idiosyncratic shocks

So far, our analysis shows that idiosyncratic shocks propagate through economic networks, and that shocks spread through labor market networks appear to have larger negative indirect effects on income and consumption. A natural question is whether these microeconomic effects can have important aggregate implications, and, if so, which features of local networks contribute to the amplification or attenuation of these shocks. In this section, we provide evidence to answer these questions.

A. The multiplier effect of idiosyncratic shocks

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

Table 4 summarizes the key values. The indirect effect on consumption associated with a 1-unit change in *Closeness*, from column 5 in Panel A in Table 2, is a fall of -207 baht. The median level of *Closeness* in the village network is 0.42 and the median number of indirectly exposed households (i.e., households who are connected to the shocked household via the network) is 23.[46] The implied

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[44]In this case, we append data on households with different degrees of closeness to placebo shocks to our data set on indirectly shocked households and fully interact equation (4) with a Treatment/Placebo dummy. Columns 9 and 10 of Appendix Table A10 report the coefficient on the triple interaction ($Post \times Closeness \times Treatment$).

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

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

total indirect effect using mean values is $-207 \times 0.42 \times 23 = -1999.62$ baht per month.

From column 5 in Panel A in Table 1, the fall in business costs for a directly affected household is -1642 baht, so the indirect effects using median closeness represent a multiplier effect of 1.23 (see column 1 in Panel C in Table 4). For comparison, Egger et al. (2021) estimate a consumption-expenditure multiplier of 2.4 from cash transfers in Kenya, while in the United States, Nakamura and Steinsson (2014) estimate an "open economy relative multiplier" of 1.5, Suárez Serrato and Wingender (2016) estimate a local income multiplier of government spending of 1.7 to 2, and Chodorow-Reich (2019) suggest a spending multiplier of 1.8 based on a survey of multiple studies. 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. Relative to previous estimates of multipliers in developing countries (e.g., Egger et al. 2021), we exploit *within-village* variation in exposure to shocks based on distance to the shocked household in the village network. Thus, our estimates of indirect effects are net of any changes in prices (which would not differ by network distance). As such our multiplier estimate may be a lower bound, consistent with our estimate being at the lower end of the range of other recent estimates.

Because our calculations are based on economic responses of indirectly shocked households, *net* of changes in prices or other village aggregate adjustments, our multiplier estimates constitute novel partial equilibrium benchmarks for developing countries. The multiplier we estimate is therefore informative about the degree of frictions in insurance, labor, and goods markets.[47] 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.

B. *Network structure and the propagation of shocks*

How does the structure of local networks affect propagation? Do shocks to agents who differ in their network position propagate to different extents? Understanding which economic microstructures are conducive to the amplification or attenuation of idiosyncratic shocks is relevant for targeting social protection (e.g., cash transfers), forecasting under what circumstances shocks will propagate to a greater or lesser extent, and gaining a deeper understanding of the ways in which networks function.

We shed light on these questions by estimating alternative specifications which, instead of exploiting variation in closeness to the shocked household *within* the village, use *cross-village* variation in shock exposure, combined with pre-post variation. We consider two dimensions of network exposure: the network's pre-shock

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[47]The value of partial equilibrium multipliers is highlighted by Guren et al. (2021), who argue that partial equilibrium multipliers can be directly linked to specific mechanisms and be subject to a clearer interpretation as opposed to general equilibrium multipliers which capture multiple mechanisms.

density, and the degree centrality of the directly shocked household. Specifically, we estimate the following model:

$$y_{i,t,j} = \beta Post_{t,j} \times \text{Network Exposure}_j + \gamma Post_{t,j} \times \text{Market share}_j$$

(5) $$+ \mathbf{X}_{i,t,j}\kappa + \theta_{\tau(j)} + \alpha_i + \omega_j + \delta_t + \delta_t \times \text{Network Exposure}_j + \epsilon_{i,t,j}$$

where, as above, the unit of observation is a household i in period t around the shock to household j. Network Exposure$_j$, which does not vary across households i, measures either the shocked household's degree or the network's density during the pre-shock period, based on the transactions in the labor market and the market of inputs and final output. ω_j absorbs village-level variables that are invariant around the analysis window, including the main effect of Network Exposure$_j$. We allow outcomes to vary over time differentially for villages with different structures by including time-fixed effects δ_t interacted with Network Exposure$_j$. 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, as well as a measure of the household's market share in the local economy.[48]

The parameter of interest is β, which captures the post- vs. pre-shock differences in changes in outcomes of households in villages with higher pre-shock network density (or shocked household degree centrality), relative to households in villages with lower pre-shock network density/shocked household degree centrality. It is useful to compare equation 4 with equation 5. Equation 4 leverages within-village variation, comparing households with a distant, or no, network connection to the shocked household versus those who are closer in the network. This specification traces out how the direct effects travel through the network. Equation 5 instead compares average outcomes of non-directly-shocked households in villages where network exposure differs; this specification sheds light on the indirect effects on the *average* non-shocked household, as a function of network characteristics.

The sign of β is theoretically ambiguous. When we consider variation in network density, on one hand, in denser networks the average household has more transaction partners, potentially making it easier to make up for the loss of transactions with the directly shocked household. On the other hand, being connected with more transaction partners increases overall indirect exposure to shocks, and may make shocks more "*de facto* aggregate." Similarly, a shock hitting a higher-centrality household may on one hand be better-insured or, due to homophily, the contacts of a higher-centrality household may also be more central/better insured; these mechanisms would suggest less propagation. On the other hand, a higher-centrality household has, by definition, more first-degree connections,

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

which may cause the shock to propagate more.[49]

RESULTS: NETWORK DENSITY, POSITION AND PROPAGATION

We first examine the effect of network density on shock propagation: Panel A of Table 5 shows that shocks in denser networks propagate more: a 1 SD increase in the network's pre-shock network density results in 0.054 (5.4%) fewer input/output transactions in the post-shock period, 0.027 (5.8%) fewer labor market transactions, and 0.08 (5.5%) fewer overall transactions (columns 1–3; all significant at the 1% level). The corresponding effect on income is a fall of 330 THB (3.1%) and the effect on consumption is a decline of 160 THB or 2.1% (columns 4 and 5), also statistically significant at 1%.

Turning to the effect of the network position of the shocked household, Panel B in Table 5 indicates that when a more central household is shocked, greater propagation results. A 1 standard deviation (SD) increase in the degree of the shocked household leads to an average of 0.037 (3.7%) fewer input/output transactions per household in the post-shock period relative to the pre-shock period, 0.046 (10%) fewer labor market transactions, and 0.083 (5.7%) fewer overall per-household transactions (columns 1–3; all significant at 5% or better). Accordingly, a 1 SD increase in the degree of the shocked household leads to a differential fall in income of 273 THB (2.5%), significant at 1% (column 4). In column 5, the point estimate indicates a differential fall in consumption of 72 THB, or 0.9%, significant at 10%.

These results show that both the structure of the network and the location of the shocked household within it matter. Shocks to more centrally located households lead to greater propagation, as do shocks to denser networks, even after controlling for a household's market share in the local economy. These findings suggest a tradeoff in promoting network interlinkages: while increasing linkages among households may strengthen the insurance capacity of networks (Feigenberg, Field and Pande, 2013), such increased links may also decrease resilience to propagation. Likewise, encouraging links with central households may promote information diffusion (Beaman et al., 2021), but may increase indirect exposure to shocks via propagation as well. These results suggest that policy efforts targeted at attenuating the effects of shocks to central households may have high social returns, as they may prevent propagation. We explore this idea further in the next section.

C. *Access to insurance and attenuation of propagation.*

The above discussion illustrates features of the economic environment which mediate the propagation of shocks, namely network density and the position of

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[49]Note that, because we are using only *network-level* variation in network density and in the centrality of the shocked household, we will not mechanically measure more propagation by virtue of more High-*Closeness* individuals, as we would if we used a specification like equation 3.

the shocked household. It is also well known that insurance can help to buffer health shocks. Both formal insurance (Finkelstein et al., 2012) and informal risk-coping mechanisms (de Weerdt and Dercon, 2006) may play a role by helping directly-hit households buffer shocks. By allowing shocked households to cope *without* scaling back business activities, insurance can stop propagation before it starts.[50,51]

<center>THE ROLE OF INFORMAL INSURANCE</center>

We investigate the role of informal risk coping using heterogeneity in the strength of local insurance networks. We do so by allowing the direct and indirect effects of the shocks to vary based on whether the directly shocked household exhibits gift-returns co-movements—a measure of engagement with risk sharing networks proposed by Samphantharak and Townsend (2018)—during the pre-period that are above or below the sample median.[52]

The results appear in Table A13. Panel A reports direct effects.[53] Columns 1-2 confirm that the severity of health shocks, as measured by symptoms or spending, does not differ by the baseline insurance network participation of the shocked household. Column 3 shows that the magnitude of incoming gifts/loans is roughly twice as high for high-insurance households (however the difference is not statistically significant). Reflecting this greater access to insurance, the falls in hired labor and business spending are roughly one third as large for high-insurance households (cols 4-5). In order to examine heterogeneous effects with more statistical precision, column 6 reports on an inverse covariance weighted (ICW) index of labor and spending. The index falls by less than half much, 0.05 vs. 0.13 standard deviations (SD), for high- vs low-insurance households ($p = 0.0745$ for a one-sided test).[54]

Panel B presents the indirect effects, based on an specification similar to (5)

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[50]Indeed, we document a role for informal insurance in our context, namely an increase in incoming gifts for directly shocked households in the first half year after the shock (see Appendix Figure A5a). However, this increase does not fully make up for the sharp increase in health spending over the same time frame (see Figure 1b). On average, the incoming gifts account for roughly two-thirds of the increase in health spending during the first post-shock half year.

[51]The finding that directly shocked households see increases in net gifts and loans may raise the question of whether the indirect effect on consumption (see Table 2, column 6) could be a consequence of a decline in cash on hand/liquidity arising from helping the directly shocked household. However, Appendix Table A12 shows that neither transfers nor loans given by the indirectly shocked household to other households increase following the shock.

[52]Because sections IV.C and IV.D together estimate three different sets of heterogeneity analyses for direct effects and a corresponding effect for indirect effects, one may worry that some of the effects will be spuriously significant. Therefore, for each set of heterogeneity analyses, we construct an inverse covariance weighted index of key outcomes and compute Anderson (2008) *q*-values across the three indices for direct effects and the three indices for indirect effects. These are reported in tables A13, A14 and A17.

[53]Specifically, we estimate direct effects by estimating β_1 and β_2 from equation B6, which is a modified version of (2). Details appear in Appendix B.B6

[54]We discuss one-sided p-values in this section in keeping with the one-sided alternative that better access to insurance *mitigates* direct and indirect effects of shocks. Tables A13-A17 also report two-sided p-values.

but that allows the indirect effects of shocks hitting denser networks (a proxy for increased amplification of the shock) to vary by the shocked household's pre-shock engagement in insurance networks.[55] Both labor and input/output transactions, hence total transactions, fall to a lesser extent for indirectly exposed households when the directly shocked household has above-median access to informal insurance (cols 1-3). The falls in income and spending are also lower (cols 4-5). An ICW index of total transactions, income and spending falls by .023 vs .038 SD when the shocked household is well insured vs not ($p = 0.086$). In sum, when shocks hit households with better access to informal insurance, both the direct and indirect effects are mitigated. This result emphasizes the distinct roles of economic networks: production networks (e.g., supply chains and employer-employee networks) appear to amplify idiosyncratic shocks but insurance networks contribute to attenuate their effects.

The role of formal insurance

To explore the role of formal insurance in mediating direct and indirect effects of shocks, we explore heterogeneity across the four provinces in our sample. As documented by Gruber, Hendren and Townsend (2014), Thailand's "30 Baht" program, which increased funding available to hospitals to care for the poor and reduced copays to 30 Baht ($0.75), was implemented more intensively and was more impactful in poorer provinces.[56] Thus, we examine whether the direct and indirect effects of shocks differ between the poorer provinces in the sample (Buriram and Sisaket), which were more intensively targeted by the 30 Baht program, vs. the richer provinces (Chachoengsao and Lopburi). Of course, there are many other important differences across provinces. While *level* differences across provinces or villages will be absorbed by the household fixed effects, this analysis should be regarded as suggestive.

Table A14 presents the results. Direct effects are presented in Panel A. Despite health shocks in poorer provinces being slightly more severe as measured by symptoms (col 1), the rise in health spending is less than half as large (col 2, $p < 0.01$). Given that the shocks require less out of pocket expenditure, incoming gifts/loans are smaller in poorer provinces (col 3). Cols 4 and 5 show that in the poorer provinces, where formal health insurance was more robust, the falls in both hired labor and business transactions are roughly one third the size as in other provinces. Accordingly, the index of labor and spending falls by .0362 SD in poorer provinces vs .128 SD in richer provinces ($p = 0.062$).

Panel B presents the indirect effects. Transactions (labor, goods, and total) fall to a lesser extent for indirectly shocked households in poorer provinces (cols 1-3). Accordingly, income and consumption also fall to a lesser extent. An index of total transactions, income and spending falls by 0.086 SD in richer provinces vs.

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[55]We estimate indirect effects by estimating β_1 and β_2 from equation B7, which is a modified version of equation (5). Details appear in Appendix B.B6

[56]A key aim of the policy was to reduce geographical disparities in the provision of public healthcare.

0.031 SD in poorer provinces ($p < 0.01$). Thus, in areas where formal insurance was implemented more robustly, direct effects of health shocks are buffered and therefore, propagate to a lesser extent.

D.   The role of labor market incompleteness.

Frictions in the market for labor may drive the direct—and in turn, indirect—effects of shocks. The lost labor of an ill or injured household member (and other members taking care of them) may not easily be replaced by hired labor. We find declines in hired labor coupled with similar declines in labor provided by household members among directly shocked households (cols 6-7 of Table 1), which are not offset with incoming free external labor (col 5, Panel A of Appendix Table A15). Moreover, Appendix Table A16 shows that when a shock hits working-age members—those more likely to operate businesses—there is a large decline in hired labor and a corresponding decline in business spending. This suggests that there may be complementarities between labor provided by household members and hired labor and hence, when household labor declines due to a shock, demand for hired labor will fall as well.

To further investigate to what extent demand-side frictions in local labor markets mediate propagation, we split the sample by the degree of complementarity between household and hired labor, $c_i^{h,l}$, measured as the pre-shock co-movement between the idiosyncratic component of labor provided by household members (h) and labor hired externally (l). If labor-market frictions play a role, we expect to see larger effects for those households with above-median $c_i^{h,l}$ (see Appendix Section B.B6 for details).

Panel A in Table A17 first shows that the severity of the shock, measured via symptoms and by spending on health, is not different for high- vs. low-$c_i^{h,l}$ households (cols 1-2). The degree of informal insurance is also similar (col 3). However, the fall in hired labor is over 6 times larger for high-$c^{h,l}$ households (col 4) ($p = 0.0734$). In contrast, the direct effects on business spending are similar (col 5), indicating that the difference is specific to labor, not goods, demand.

However, turning to the indirect effects (Panel B), there is little evidence of heterogeneity by the degree of complementarity ($c_i^{h,l}$) of the shocked household: the falls in transactions, and consumption are similar when high- and low-$c_i^{h,l}$ households are shocked. Although the indirect effects on income appear to differ (though not significantly), the impacts on the ICW index of total transactions, income and spending are remarkably similar. The results, in sum, suggest that while labor market frictions play a role in shock propagation, as shown by the greater effects of labor market vs supply chain exposure in Table 2 Panel B, the degree of internal-external labor complementarity of the shocked household is not a key mediator of these indirect effects. This may indicate that the predominant channel for propagation is the expenditure-side impacts of health shocks combined with supply-side frictions in local labor markets (e.g. costs of migrating outside of

the village), rather than impacts to household labor endowments and attendant frictions on the demand for hired labor.

V.   Concluding remarks

In developed economies, firms are often owned by diversified investors and embedded in global supply chains. In contrast, small firms in LMICs are often operated by un-hedged households and are tightly linked to other businesses and households through local economic networks defined by labor as well as product market transactions. These key differences may imply that the way in which firms cope with shocks to their owners, and the mechanisms through which these shocks propagate through the local economy may also be different to the behavior documented in the literature studying larger firms.

Our analysis reveals three novel facts. The first is that the adjustments that households make to cope with severe idiosyncratic shocks not only affect their business outcomes, but also the incomes and spending levels of other *other* households. In our setting, health shocks put substantial pressure on household budgets and labor endowments. As a result, shocked households adjust production: drawing down working capital, cutting input spending, and reducing labor hiring. These adjustments propagate the shock to other households through interlinkages in local supply chain and labor market networks. The aggregate indirect effects imply a consumption multiplier of approximately 1.23.

The second fact we document is that, while these shocks propagate through both local supply chain and local labor markets, propagation through labor linkages is more consequential in terms of indirect effects on consumption. This distinction points at important labor market frictions: once a link between and employer and an employee is broken, it is quite hard to replace it, either locally or by selling/buying labor outside the network.

Thirdly, we document that economic microstructures also shape propagation: shocks in denser networks propagate more, as do shocks to actors who are central in networks. Additionally, access to insurance (either formal or informal) mediates the spread of shocks, by decoupling the strategies that households use to cope with shocks and their productive decisions. As such, insurance appears to mitigate propagation by "stopping it before it starts".

Our findings suggest several interventions that may be beneficial. First, investing in preventative heath to reduce the severity of shocks benefits not only the household whose health improves, but also others linked in the local network. In addition, improved safety nets may help cut the link between production and the strategies that business owners use to cope with shocks, preventing granular shocks from propagating. Safety nets such as workfare (e.g., India's National Rural Employment Guarantee Scheme), health insurance (e.g. Thailand's 30 Baht Program), cash transfers (e.g, Mexico's Oportunidades), and others benefit not only the directly targeted household but also its network connections; either by reducing propagation or by reducing its negative second-order effects. Second,

where such programs are not universal, they may benefit from targeting those with key roles in the underlying networks, or areas (networks) where the economic activities of their members are more interconnected (i.e., higher network density). Electronic payment platforms that identify key players in the network structure could allow insurers to better target recipients who are key nodes in local networks.

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

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Figures and Tables

(a) Prob. of reporting any symptoms

(b) Health spending

(c) Non health consumption spending

(d) Hired labor (hrs/month)

(e) Business spending

(f) Revenues
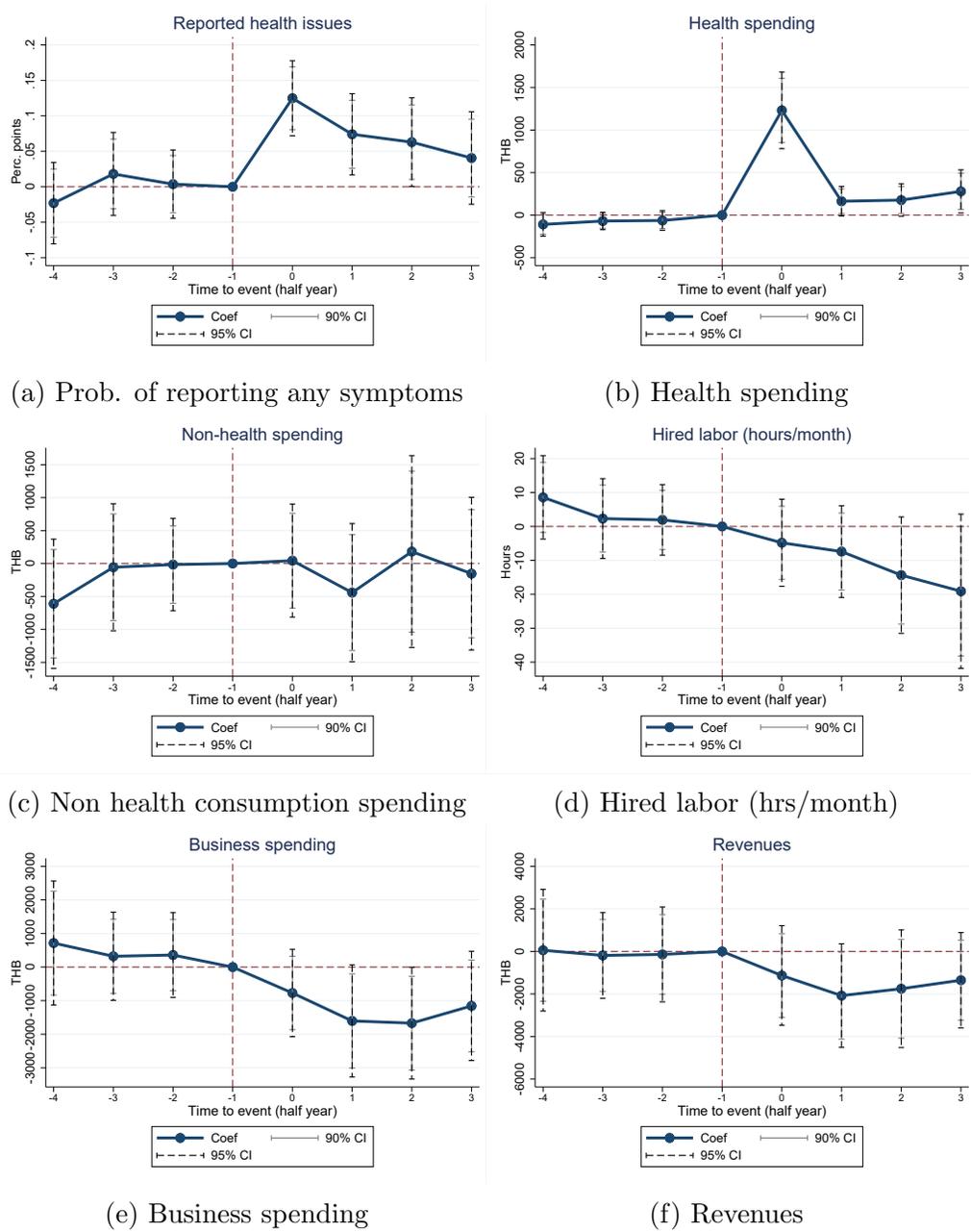
Figure 1. : Direct effects of health shocks

Note: Each dot represents differences between treatment and control households in changes in outcomes relative to the period preceding the beginning of the shock ($\tau = -1$).The estimating sample includes 2 years before and after the shock divided in half-year bins. All specifications control for household time-variant demographic characteristics, as well as household and month fixed effects. 90% and 95% confidence intervals are computed using standard errors clustered at the household level. Costs and revenues exclude costs and earnings associated with the provision of labor to other households or firms. All variables measured in THB are winsorized with respect to the 99% percentile.

Table 1—: Effects on health, spending and family businesses

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

	(1) Reported symptoms	(2) Health spending	(3) Non-health spending	(4) Total spending	(5) Business Spending	(6) Hired labor	(7) Household labor	(8) Revenues
Post X Treatment	0.0764	530.5	77.56	608.0	-1642.2	-14.49	-11.63	-1514.6
	(0.0229)	(90.29)	(335.3)	(358.8)	(793.5)	(7.648)	(8.589)	(1011.3)
Baseline mean (DV)	0.361	151.8	5260.4	5412.2	7307.5	17.58	151.7	14304.5
Observations	23014	23015	23015	23015	23015	23015	23015	23015
Number of events	249	249	249	249	249	249	249	249
Adj. R-Squared	0.232	0.0487	0.148	0.155	0.789	0.600	0.713	0.641

Panel B: Using all shocks

	(1) Reported symptoms	(2) Health spending	(3) Non-health spending	(4) Total spending	(5) Business Spending	(6) Hired labor	(7) Household labor	(8) Revenues
Post X Treatment	0.0839	411.9	234.5	646.4	-1325.3	-9.918	-15.05	-1771.9
	(0.0167)	(61.90)	(331.2)	(336.8)	(515.2)	(4.885)	(6.457)	(664.4)
Baseline mean (DV)	0.345	158.2	5770.4	5928.6	7172.5	15.81	140.3	14302.3
Observations	43923	43925	43925	43925	43925	43925	43925	43925
Number of events	476	476	476	476	476	476	476	476
Adj. R-Squared	0.226	0.0443	0.0993	0.108	0.758	0.685	0.657	0.570

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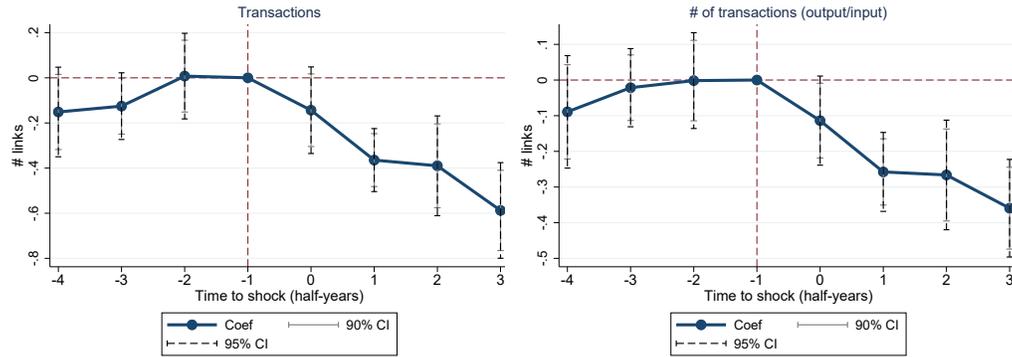
(a) Total transactions

(b) Supply-chain (sales) network transactions
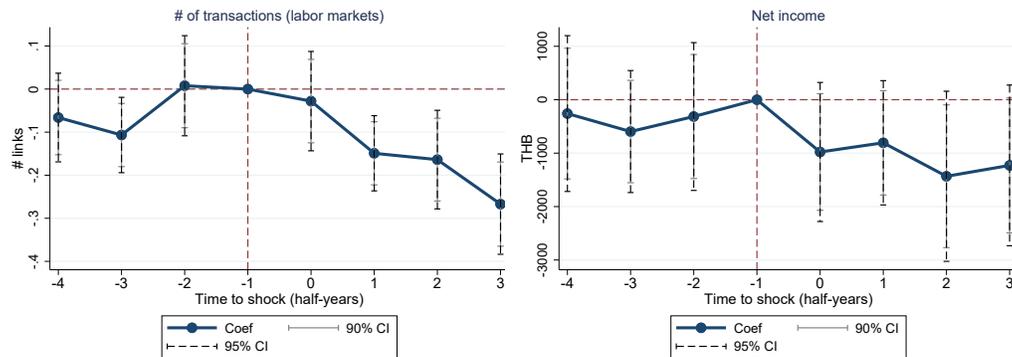
(c) Labor network transactions

(d) Total income
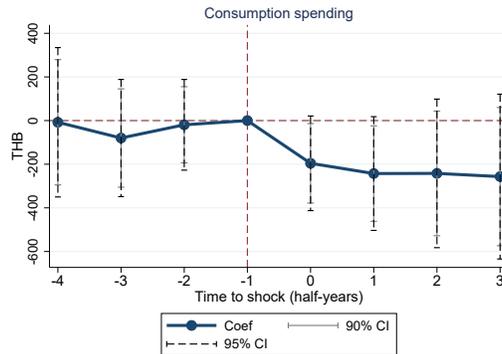
(e) Consumption Spending

Figure 2. : Indirect effects on transactions, income and consumption

Note: The Figure presents flexible difference-in-difference estimates of the indirect effects of idiosyncratic shocks on local businesses, following equation (3). All regressions include household fixed effects, event fixed effects, month fixed effects, village- and year-fixed effects, and household size, household average age and education, and the number of adult males and females in each household. Each dot captures differences in changes in outcomes with respect to the half-year preceding the shock (-1) between more- and less-exposed households. Standard errors are two-way clustered at the household (i) and shock level (j). All variables measured in THB are winsorized with respect to the 99% percentile.

Table 2—: Propagation of idiosyncratic shocks

Panel A: Propagation effects through village networks.

	(1) Input/Output	(2) Hired Labor	(3) All transactions	(4) Total Income	(5) Total Spending
Post X Closeness	-0.19	-0.11	-0.30	-820	-207
	(0.06)	(0.04)	(0.08)	(412)	(151)
Observations	434,145	434,145	434,145	434,145	434,145
R-squared	0.44	0.23	0.37	0.21	0.64
Pre-period Mean	0.989	0.462	1.451	10729	7433
Number of events	410	410	410	410	410

Panel B: Propagation effects through supply-chain and labor-market networks.

	(1) Input/Output	(2) Hired Labor	(3) All transactions	(4) Total Income	(5) Total Spending
Post X closeness (supply-chain network)	-0.24	0.02	-0.22	106	155
	(0.06)	(0.04)	(0.08)	(464)	(174)
Post X closeness (labor-market network)	-0.02	-0.20	-0.22	-1,233	-501
	(0.06)	(0.04)	(0.08)	(423)	(153)
Observations	434,145	434,145	434,145	434,145	434,145
R-squared	0.44	0.23	0.37	0.21	0.64
Pre-period Mean	0.989	0.462	1.451	10729	7433
Number of events	410	410	410	410	410

Note: Panel A 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. Panel B 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. Standard errors are two-way clustered at the household (i) and event (j) level.

Table 3—: Propagation effects on outgoing and incoming transactions

	(1)	(2)	(3)	(4)	(5)	(6)
	Input/Output		Labor		All transactions	
	Outgoing	Incoming	Outgoing	Incoming	Outgoing	Incoming
Post X Closeness	-0.07	-0.12	-0.08	-0.03	-0.16	-0.15
	(0.04)	(0.03)	(0.02)	(0.03)	(0.05)	(0.04)
Observations	434,145	434,145	434,145	434,145	434,145	434,145
R-squared	0.53	0.26	0.15	0.21	0.43	0.25
Pre-period Mean	0.495	0.494	0.178	0.284	0.673	0.778
Number of events	410	410	410	410	410	410

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.

(a) Total transactions

(b) Total income

(c) Spending
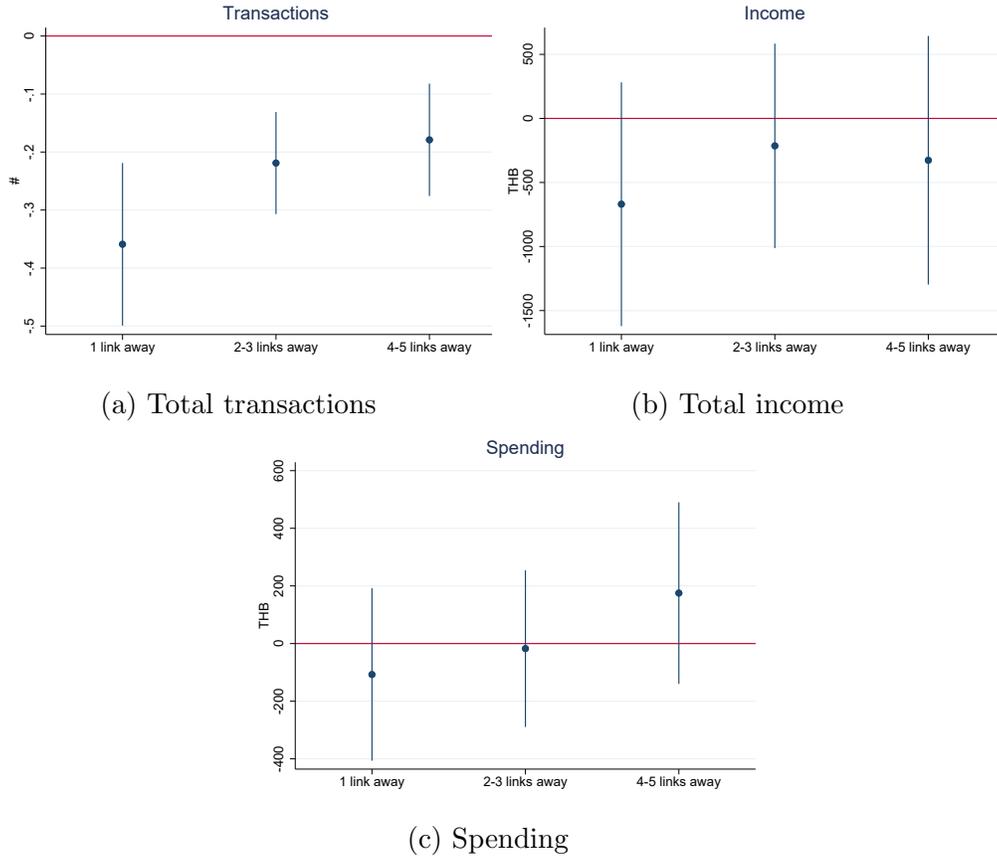
Figure 3. : Indirect effects by distance to shocked households

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 4—: Multiplier effects

Panel A: Pre-shock characteristics

	Median
Market share (shocked household)	0.028
Avg. closeness (shocked household)	0.422
# of indirectly exposed households (pre-shock)	23

Panel B: Treatment effects

	Point estimate
Direct effects on business spending (THB)	-1642
Direct effects on business spending (% relative to pre-shock mean)	-22.5
Indirect effects on consumption spending (THB)	-207
Indirect effects on consumption spending at mean closeness (THB)	-88
Indirect effects on consumption spending (% of pre-shock means)	-1.16

Panel C: Aggregate effects

	Estimate	95 % CI	90 % CI
Multiplier	1.23	[-0.327, 6.423]	[-0.141, 4.145]

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

Table 5—: Propagation and network characteristics

Panel A: Aggregate shock amplification and network density

	(1) Input/Output	(2) Hired Labor	(3) All transactions	(4) Total Income	(5) Total Spending
Post X Density (z-score)	-0.054	-0.027	-0.080	-330.226	-160.752
	(0.014)	(0.014)	(0.022)	(94.026)	(33.558)
Observations	477,316	477,316	477,316	477,316	477,316
R-squared	0.422	0.207	0.355	0.208	0.635
Pre-period Mean	0.989	0.460	1.449	10745	7448
Number of events	410	410	410	410	410

Panel B: Aggregate shock amplification and shocked household degree

	(1) Input/Output	(2) Hired Labor	(3) All transactions	(4) Total Income	(5) Total Spending
Post X Degree (z-score)	-0.037	-0.046	-0.083	-273.657	-72.502
	(0.014)	(0.015)	(0.021)	(86.045)	(39.693)
Observations	477,316	477,316	477,316	477,316	477,316
R-squared	0.422	0.207	0.355	0.208	0.635
Pre-period Mean	0.989	0.460	1.449	10745	7448
Number of events	410	410	410	410	410

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

ONLINE APPENDIX: PROPAGATION AND INSURANCE IN VILLAGE NETWORKS

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

A1.   Supportive evidence

Table A1—: Summary statistics

Panel A: Household baseline characteristics

	count	mean	sd	p10	p90
Number of household members	510	4.53	1.87	2	7
Number of adults	510	2.87	1.38	1	5
Age (household head)	508	52.00	13.49	35	70
Age (household average)	510	34.19	12.14	21	52
Household head is male	508	0.77	0.42	0	1
Years of schooling: Household head	505	4.49	2.59	3	7
Years of schooling: Household maximum achievement	510	8.19	3.64	4	14
Years of schooling: Household average	510	5.09	2.17	3	8

Panel B: Household finance (annual data)

	count	mean	sd	p10	p90
Farm	7650	134203.22	1377160.98	-151	316242
Off-farm business	7650	19061.31	115429.66	0	40654
Labor	7650	48537.08	102427.94	0	141428
Total from operations (farm+off-farm + labor)	7650	516020.23	2490777.97	15228	1104350
Gifts/transfers	7650	23935.48	184141.89	-11632	75635
Total net income (Operations+Gifts/Transfers)	7650	539955.71	2497465.40	29614	1116092
Food consumption	7650	32916.51	21912.78	11865	60521
Total consumption	7650	98030.54	99438.08	24189	204476
Total Assets (THB)	7650	2345327.56	7351009.41	168188	4660295
Fixed Assets/ Total Assets (%)	7650	53.12	27.12	13	88
Total debt/Total assets (%)	7650	11.60	21.42	0	27

Panel C: Village Networks

	count	mean	sd	p10	p90
Supply chain network: Degree (number of links)	7650	1.36	2.71	0	3
Supply chain network: Participation (any link)	7650	0.51	0.50	0	1
Labor-market network: Degree	7650	3.33	4.51	0	9
Labor-market network: Participation	7650	0.66	0.46	0	1
Financial network: Degree	7650	0.70	1.40	0	2
Financial network: Participation	7650	0.38	0.48	0	1
Baseline kinship network: Degree	7650	2.36	2.19	0	6
Baseline kinship network: Participation	7650	0.77	0.42	0	1

Panel D: Village and firm size

	count	mean	sd	p10	p90
Number of households in the village	16	160.95	89.61	74	330
Village-level average firm size	240	341048.59	397630.43	59966	620106
Village-level standard deviation of firm size	240	618846.47	1452881.89	69877	1222209
Village-level kurtosis of average village firm size	240	10.13	5.92	4	19

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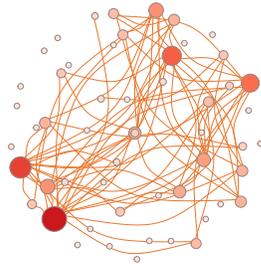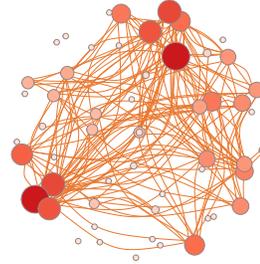
(a) Supply chain   (b) Labor
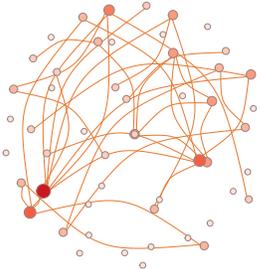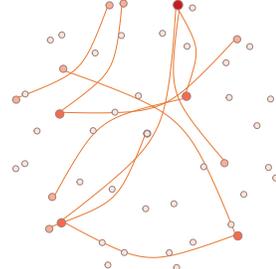
(c) Kinship   (d) Financial

Figure A1. : Socioeconomic Networks for a sample village

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

(a) Outpatient care (Prob.)

(b) Outpatient care (Spending)

(c) Inpatient care (Prob.)

(d) Inpatient care (Spending)

(e) Accidents (Prob.)

(f) Suspension of main activities (Prob.)
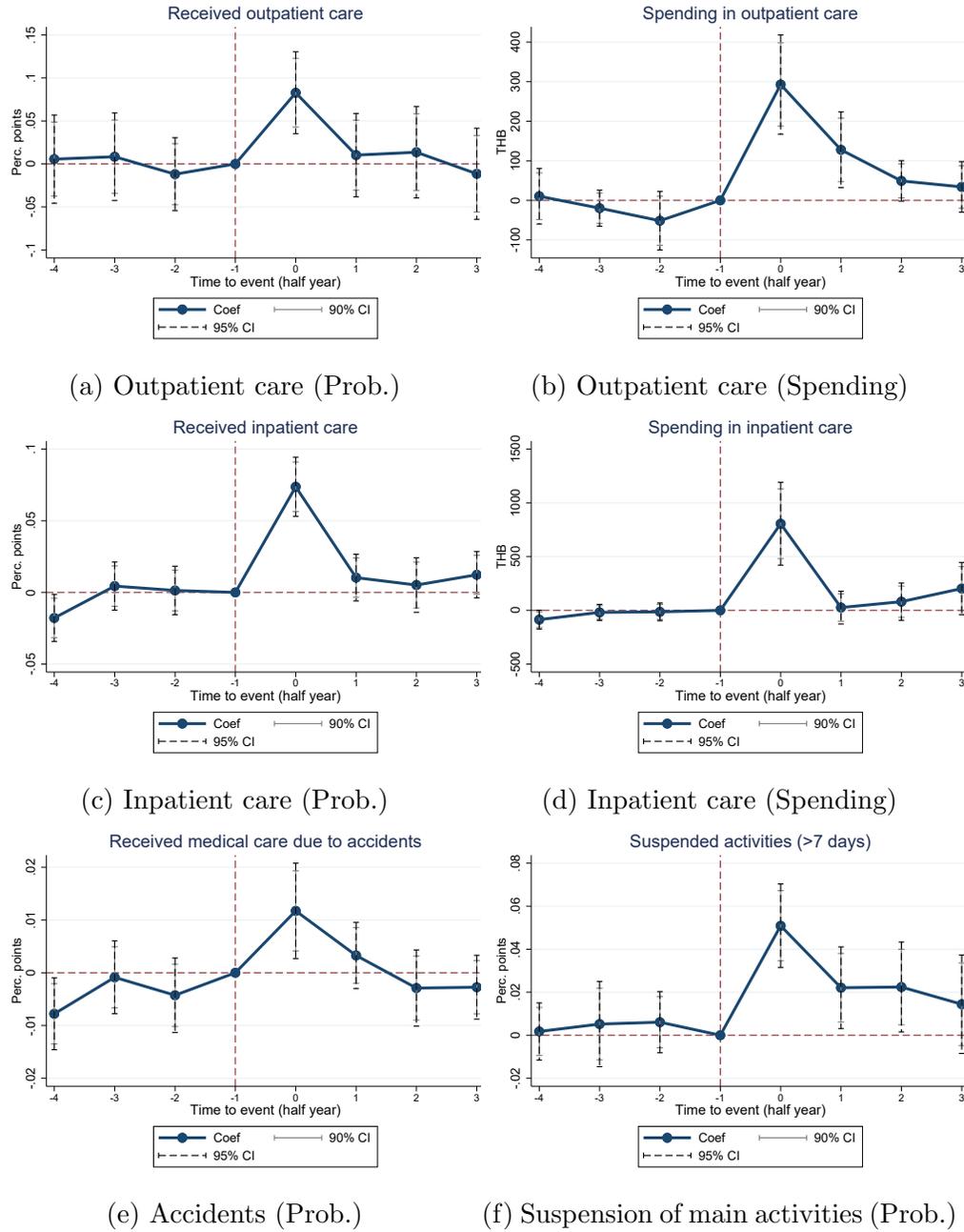
Figure A2. : Direct effects of health shocks

Note: Each dot represents differences between treatment and control households in changes in outcomes relative to the period preceding the beginning of the shock ($\tau = -1$). The estimating sample includes 2 years before and after the shock divided in half-year bins. All specifications control for household time-variant demographic characteristics, as well as household and month fixed effects. 90% and 95% confidence intervals are computed using standard errors clustered at the household level.
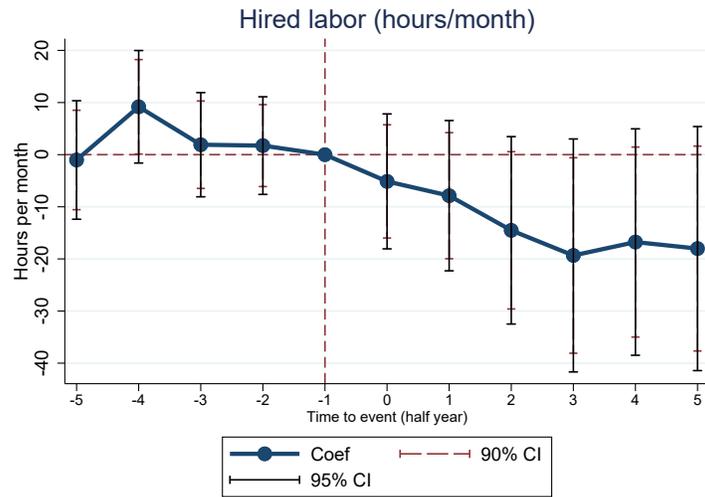
Figure A3. : Hired labor (extended analysis window)

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

(a) Reported symptoms

(b) Health Spending

(c) Non-health Spending

(d) Business spending

(e) Hired labor (hrs/month)
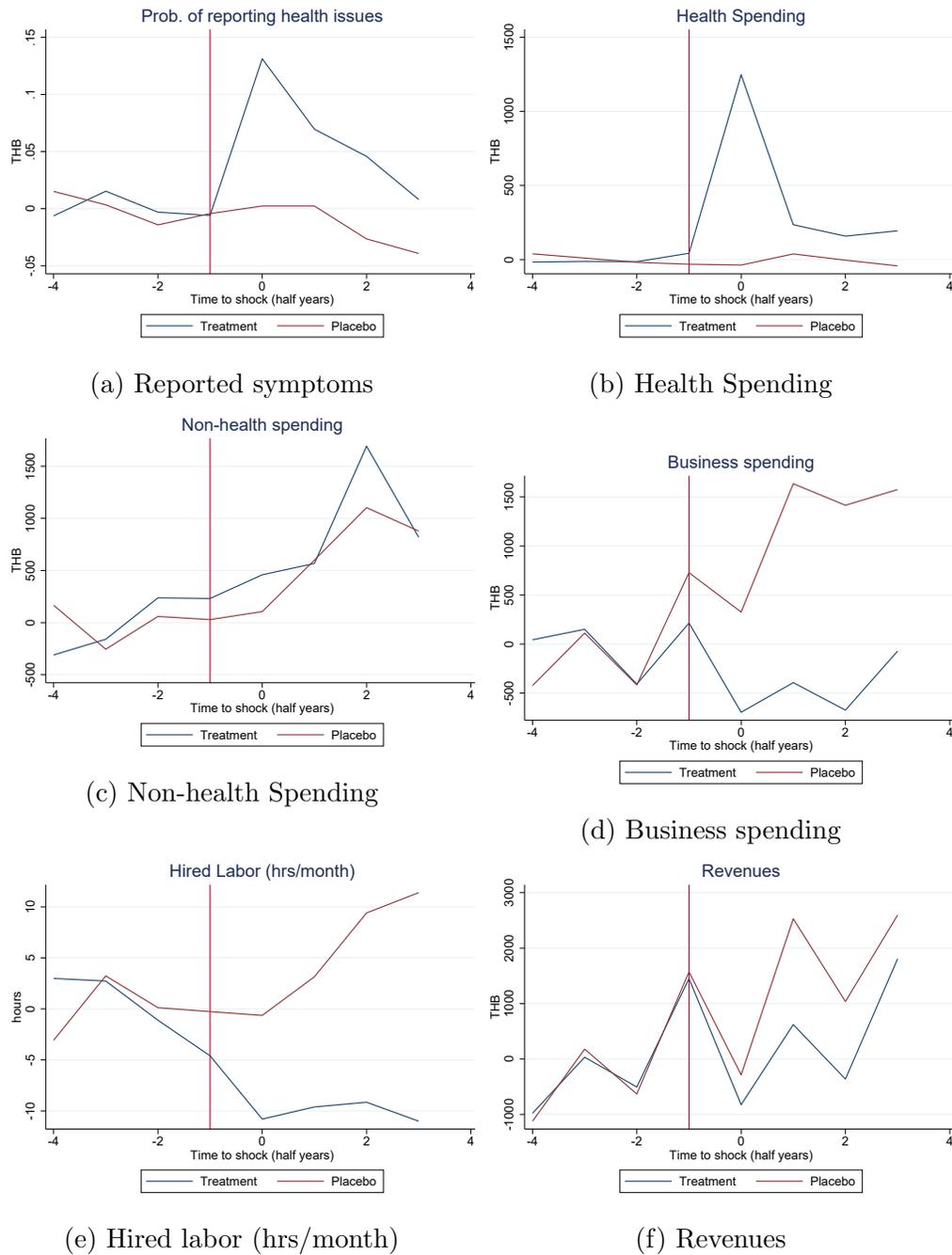
(f) Revenues

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

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

(a) Incoming gifts

(b) Fixed assets, livestock, and inventories
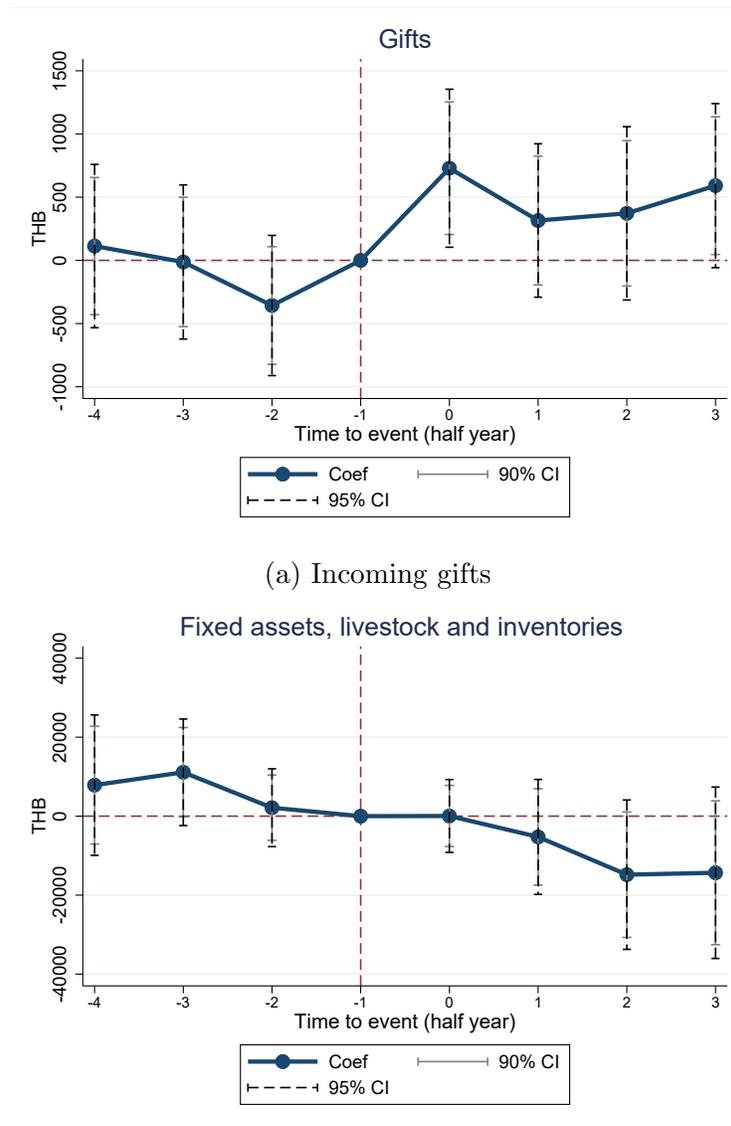
Figure A5. : Effects on incoming transfers and assets

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

Table A2—: Direct effects on housework

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

	(1) # of hh members	(2) # of days
Post X Treatment	-0.08 (0.06)	-3.06 (1.79)
Baseline mean (DV)	2.93	81.31
Observations	23015.00	23015.00
Number of events	249.00	249.00
Adj. R-Squared	0.80	0.77

Panel B: Using all shocks

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

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

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

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

Panel A: Direct Effects on household spending

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

Panel B: Indirect Effects

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

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

Panel A: Direct effects on spending

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

Panel B: Indirect effects

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

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

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

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

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

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

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

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

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

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

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

Table A8—: Spending co-movements with health status

Panel A: Symptom - Health spending comovements

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

Panel B: Symptom - Business spending comovements

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

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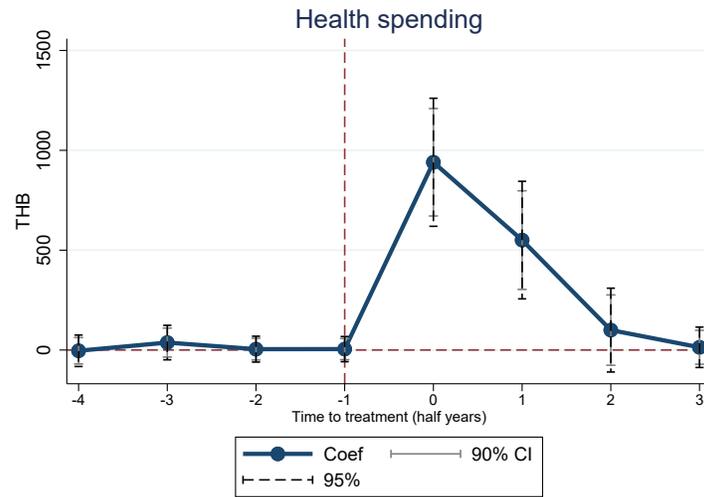
(a) Health Spending
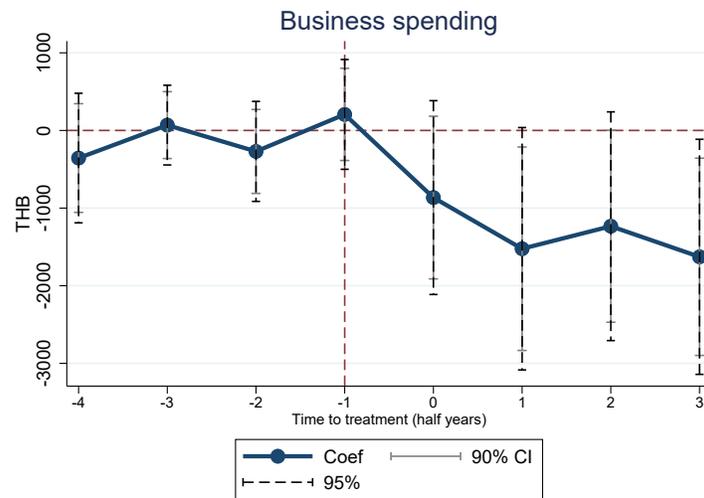
(b) Business Spending

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

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

(a) Supply-chain (sales) network transactions

(b) Labor network transactions
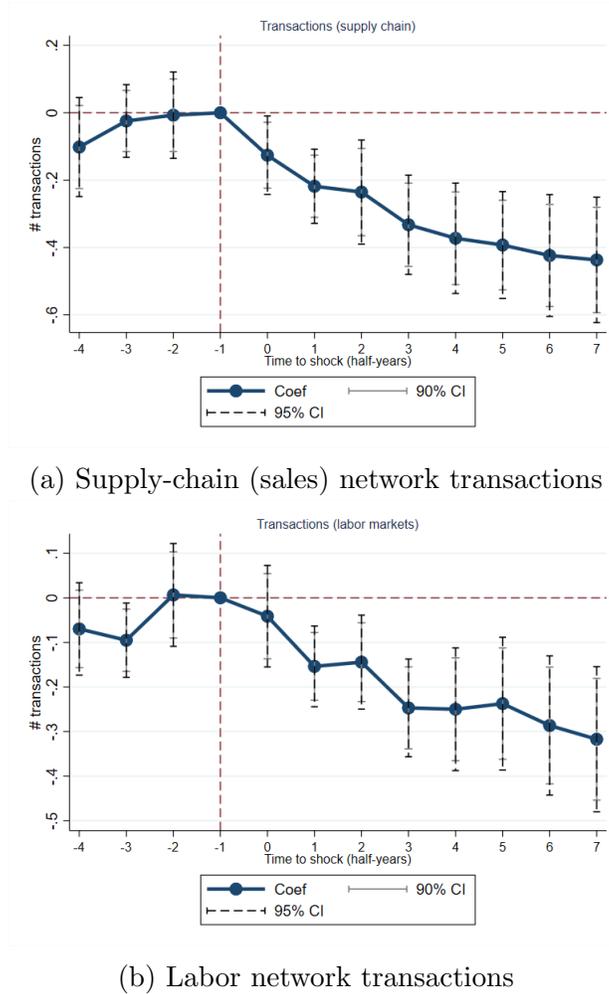
Figure A7. : Persistent indirect effects of shocks on transactions.

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

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

Panel A: Supply chain transactions

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

Panel B: Labor market transactions

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

Panel C: Gifts/loans transactions

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

Note: The table presents regression coefficients following the specification in equation (B5). We model the probability that a pair of households $\{i,j\}$ trades in year t as a function of whether the couple traded in period $t-1$, by type of transaction. Columns 1 presents raw correlations, column 2 includes village-year fixed effects. Column 3 adds kinship first-degree connections as a control. Column 4 controls for differences in baseline demographic characteristics, differences in baseline wealth (e.g., assets net of liabilities), and household fixed effects. The coefficients of Demographic and Net-worth distance are 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.

Table A10—: Indirect effects: Robustness to alternative specifications

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

Table A15—: Response to shocks: coping mechanisms

	Panel A: Direct effects				
	(1) Gifts/Transfers	(2) Loans	(3) Fixed Assets livestock & inventories	(4) Cash in Hand	(5) Unpaid labor (Hrs/Month)
Post X Treatment	571.6 (213.3)	77.30 (236.1)	-13601.9 (9610.5)	-11824.0 (22788.9)	1.813 (1.544)
Baseline mean (DV)	1936.6	266.7	223388.5	369550.7	6.152
Observations	23015	23015	23015	23015	23015
Number of events	249	249	249	249	249
Adj. R-Squared	0.166	0.00995	0.923	0.882	0.212
	Panel B: Indirect effects				
	Gifts/Transfers	Loans	Fixed Assets livestock & inventories	Cash in Hand	Unpaid labor (Hrs/Month)
Post$\times$Closeness (village network)	-101.8 (151.6)	-146.7 (120.7)	-14778.2 (6365.0)	-11323.3 (20939.4)	-1.186 (0.948)
Baseline mean (DV)	2351.3	82.01	253322.2	434897.1	5.807
Observations	434145	434145	434145	434145	434145
Number of households	410	410	410	410	410
Adj. R-Squared	0.147	0.0372	0.879	0.813	0.292

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

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

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

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

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

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

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

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

Identifying shocks and their effects

B1. Identifying shocks

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

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

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

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

To account for potential anticipation effects, we define the beginning of each event by subtracting the number of months preceding the episode of high health spending during which household members reported health symptoms from the month corresponding to the episode. For example, if the episode of high health spending was recorded in month 100 and the symptoms started being reported three months before, the beginning of the event is month 97. For 406 events, we can identify the health symptoms reported at the time of the events, and when these symptoms were first reported. In the case of the 70 households for which we could not identify the beginning of the symptoms,[57] we coded the beginning of the event as three months before the episode of high total health spending (the median period between the observed increases in health spending and the first time symptoms were reported).

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

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

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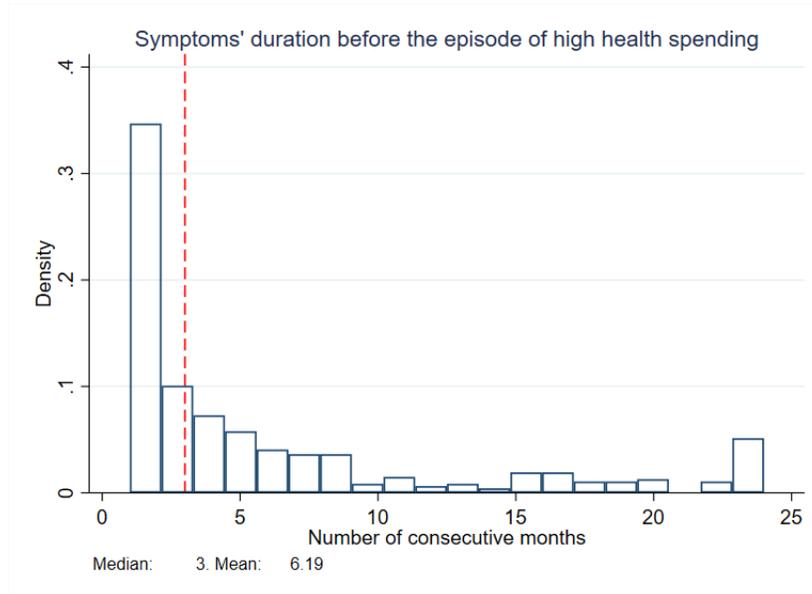
Figure B1. : Distribution of symptom duration before the episodes of high health spending

Note: The figure plots the distribution of the number of consecutive months prior to the episodes of high health spending for which at least one household member reported health symptoms. The dashed vertical line denotes the median number of consecutive months reporting symptoms before the episode of high health spending. The last bar to the right captures the density of symptoms that were experienced 24 months or more before the episode of high health spending.
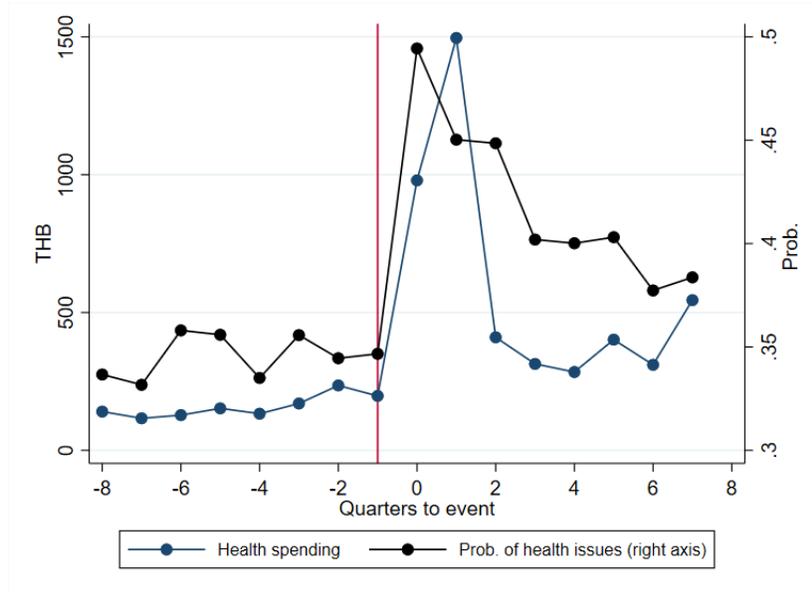
Figure B2. : Health status and spending before and after health shocks.

Note: The figure reports averages of health and total spending for periods before and after the health shocks (left axis). The right axis reports the probability that at least one household member reports health symptoms in a given month, before and after the shocks. The horizontal axis represents normalized time with respect to the event realization (time 0). Each time bin corresponds to quarters. All averages are computed over a balanced panel.
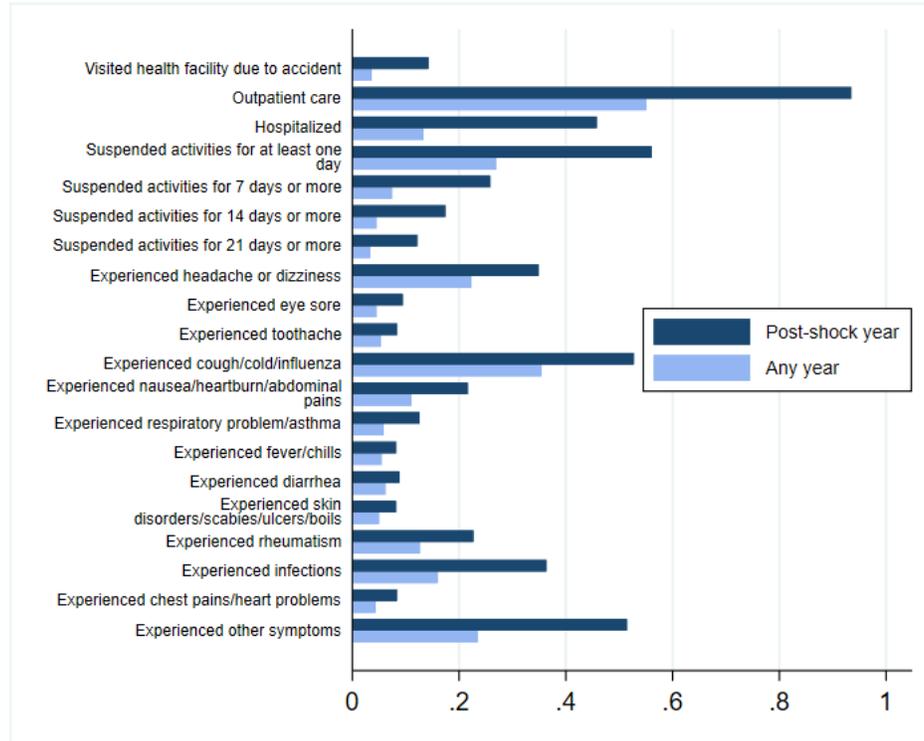
Figure B3. : Incidence of health conditions during shock and non-shock periods.

Note: The figure reports the proportion of symptoms experienced during the year following the episodes of high-health spending and during any year. The sample includes all households that ever experience a health shock according to our main shock definition.
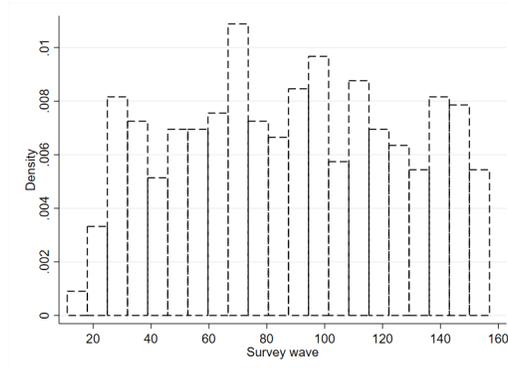
(a) Distribution of initial event periods
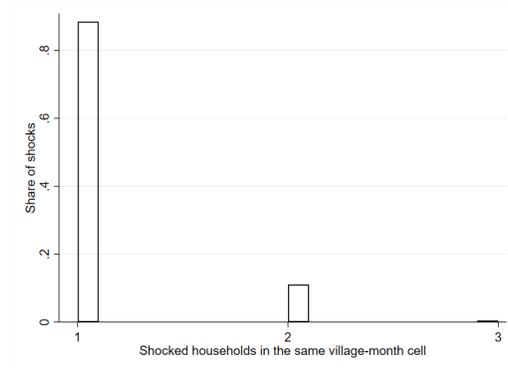
(b) Distribution of shocks by number of simultaneously affected households in the same village

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

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

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

	(1) Shock occurrence at t+1	(2) P-value (Granger causality - 12 lags)
Income	0.000487	0.276
	(0.000391)	
Business Revenues	0.000951	0.883
	(0.000687)	
Business Spending	-0.000798	0.715
	(0.000663)	
Non health consumption	0.0000399	0.760
	(0.000197)	
Health spending	-0.000404	0.587
	(0.000224)	
Borrowing	0.000452	0.830
	(0.000475)	
Lending	-0.000422	0.182
	(0.000349)	
Incoming gifts	-0.000366	0.511
	(0.000445)	
Outgoing gifts	0.00000542	0.948
	(0.000281)	
Livestock	-0.000317	0.0887
	(0.000490)	
Cash in hand	-0.000557	0.375
	(0.000448)	
Fixed assets	0.000142	0.0873
	(0.000412)	
Land	0.00222	0.200
	(0.00165)	
Observations	87210	77755
Adj. R-Squared	-0.00411	
P-value (Joint significance)	0.281	
P-value (Hausman Test Village X month fixed effects)	0.288	

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

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

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

Note: The table reports participation in several activities for a subsample of individuals that reported being sick during the periods in which their household experienced the shock. Column 1 reports the number of days in which household members reported participating in each activity, during the month preceding the shock. Column 2 reports the share of affected individuals that dedicated more than 15 days to each activity, during the month preceding the shock. The sample is restricted to the month-preceding the shock and corresponds only to household members that reported being sick during the shock. These activities are not mutually exclusive, so the total days per month across categories add up to more than 30.
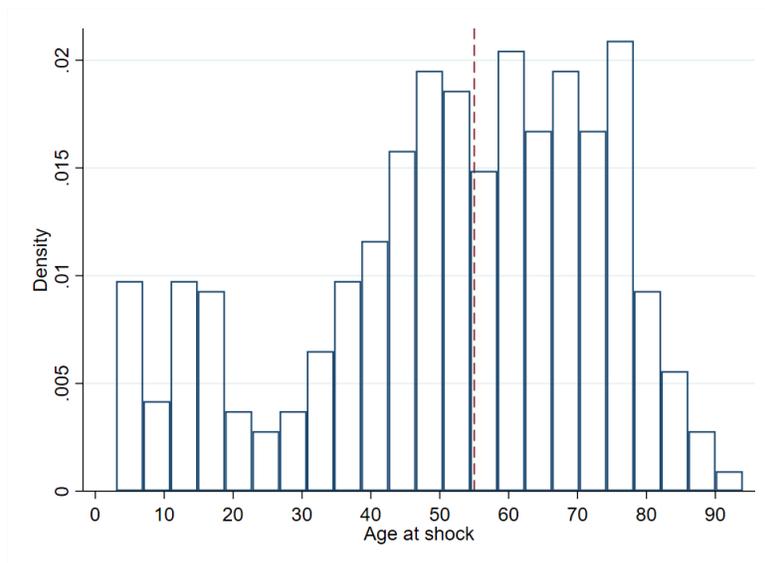
Figure B5. : Age at shock

Note: The figure plots a histogram capturing the distribution of age of family members reporting health symptoms during the month associated to the beginning of each shock. The figure includes observations corresponding to the 405 shocks for which we found households reporting non-pregnancy/non-birth health symptoms. The dashed vertical line denotes the median age of household members reporting symptoms during the month preceding the beginning of each shock.
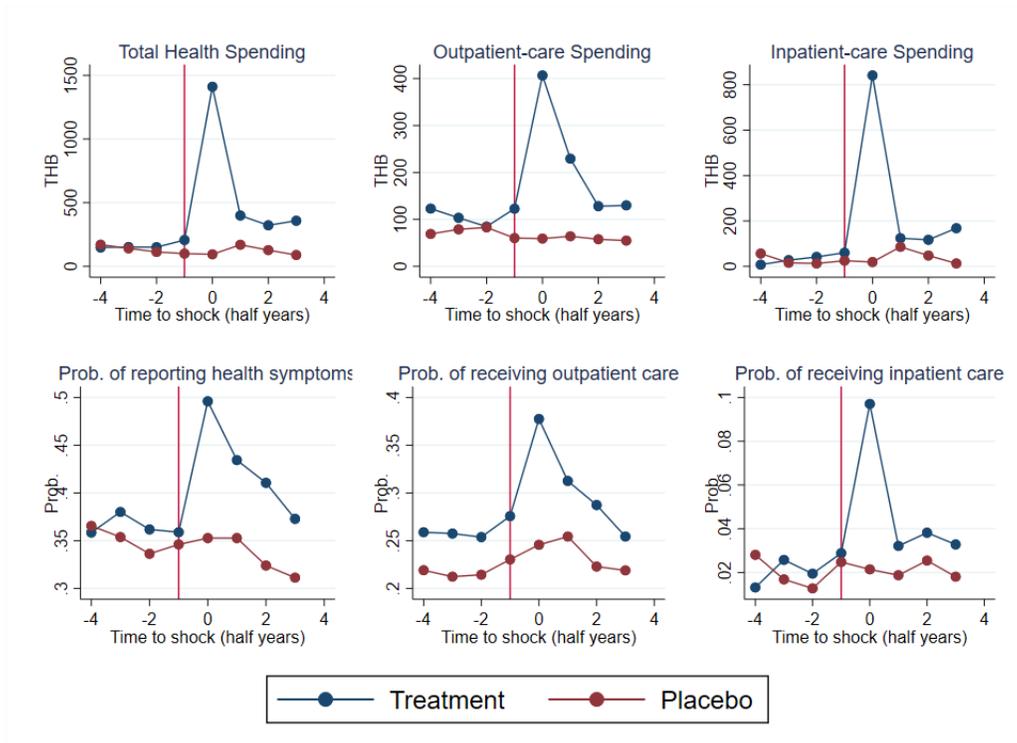
Figure B6. : Health status and spending in the treatment and control samples

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

B2.   *Treatment and control groups for direct effects*

We implement our approach for estimating the direct effects of idiosyncratic shocks in three steps. First, we split households into two age groups—i.e., below and above the median household age at baseline (1997).[58] By comparing households in the same village and age group, we isolate contemporaneous village-specific shocks and potential differences in the trajectories of business and household-finance outcomes that could vary along the life cycle. Given our sample size, we choose two age group bins to ensure that we have multiple observations per bin in each village.

Second, for each age group within each village, we split the panel in two equal-length sub-samples $\{\theta^1, \theta^2\}$ by taking the midpoint between the months associated to the first and last shocks in each age group-village bin (Δ), such that those households suffering a shock between periods $\underline{t}$ and $t_{med} = \underline{t} + \Delta$ belong to the treatment group (θ^1), and those experiencing the shock between periods t_{med} and $\bar{t}$ belong to the control group (θ^2).[59] By construction, there is no overlap between the two groups.

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

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

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[58]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.

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

it increases statistical power.

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

DIRECT EFFECTS: ROBUSTNESS

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

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

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

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

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

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

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

We estimate the following equation:

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

where $Post_{i(k),t}$ is an indicator that takes the value of 1 in periods following the k-th shock to household i, and 0 otherwise. In this case $k \geq 1$ for all treated households.[60] We exclude shocks that occur less than 24 months after the preceding shock, to minimize violations to the parallel trends assumption. As in our main specification we focus on a two-year time window before and after each shock. In Appendix Table A5 we report robustness to include multiple, non overlapping shocks per household based on two definitions of shocks: changes in health status that precede levels of health spending that are larger than the household-specific food consumption (columns 3 and 4) and shocks based on whether a household member had to suspend activities for more than 7 days due to illness (columns 7 and 8). The results in both cases are very similar to those from our specifications

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

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

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

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

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

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

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

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

$$(\text{B3}) \quad y_{i,t} = \beta Post_{i,t} + X_{i,t}\kappa + \alpha_i + \delta_t + \epsilon_{i,t}$$

Here, we regress the outcome of interest on a Post dummy over a sample of shocked households including 2 years before and after the shock. This specification uses households that are not simultaneously shocked as controls. Reassuringly, the

results are very similar to those from our main specification (see columns 5 and 6 of Appendix Table A6).

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

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

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

B3.   Indirect effects: Alternative empirical approaches

Triple difference estimates of indirect effects

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

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

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

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

Measuring indirect effects à la Fadlon and Nielsen (2019)

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

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

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

is indirectly exposed throughout the panel (either directly or indirectly).[61]

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

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

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

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

B4. Persistence in transaction networks

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

$$Link_{i,j,t} = \rho Link_{i,j,t-1} + \gamma_1 Kinship_{i,j} + \gamma_2 Demographic\ distance_{i,j}$$
$$\text{(B5)} \qquad + \gamma_3 Net\text{-}Worth\ distance_{i,j} + \delta_{v,t} + \alpha_i + \alpha_j + \epsilon_{i,j,t}$$

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.[62] 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).[63] Finally, we also include household-fixed effects. The parameter of interest is ρ, which captures the persistence of the economic interactions between each pair of sample households.

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

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

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

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

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

B5. *Direct and indirect coping mechanisms*

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

Panel A presents results from direct shocks, corresponding to equation (2). Column 1 shows that incoming gifts increase by THB 571, or approximately 29%.[64] Columns 2 to 4 show that although borrowing increases and fixed and liquid assets decline, the changes are not significant.[65] Finally, column 5 shows that there is no response in terms of the amount of incoming unpaid labor. This is important as it demonstrates that the reductions in paid labor documented above are not reflections of a substitution to unpaid labor. Panel B presents results from indirect exposure to shocks, corresponding to equation 4. There are no significant effects associated with indirect shock exposure on any of the five mechanisms. This helps to explain why consumption falls for indirectly shocked households—other coping mechanisms appear to be unavailable.

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

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

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

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

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

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

We then classify households with above median pre-shock gift-to-income sensitivity as having "high" access to informal insurance, and others as having "low" access to informal insurance. We replicate this process using pre-period data with respect to actual and placebo shocks. We then estimate a triple differences model, modifying equation 2 to allow the effect of a shock to vary by access to informal insurance:[66] 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.

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

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

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

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

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

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

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

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

THE THAI HEALTHCARE SYSTEM

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