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ABSTRACT

In village economies, insurance networks are key to smoothing shocks, while production networks can propagate them. The interplay of these networks is crucial. We show that a significant health expenditure shock to one household propagates to other linked households via supply-chain and labor networks. Imperfectly insured households adjust production decisions---cutting input spending and reducing labor hiring---affecting households with whom they trade inputs and labor. Household businesses proximate to shocked households in the supply chain network experience reduced local sales, and those proximate in the labor network experience a lower probability of working locally. As a result, indirectly shocked households' earnings and consumption fall. These declines persist over several years because networks are rigid: households appear unable to form new linkages when existing links experience negative shocks. Propagation is a function of access to insurance networks: well-insured households do not cut spending when hit by shocks, leading to minimal propagation. A simple back-of-the-envelope exercise suggests that the total magnitude of indirect effects may be larger than the direct effects and that social (village-level) gains from expanding safety nets such as health insurance may be substantially higher than private (household-level) gains.

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1 Introduction

Households in developing countries are both consumers and producers (Banerjee and Duflo 2007; Samphantharak and Townsend 2010). In addition to purchasing goods and services for consumption, they exchange productive inputs with each other (Braverman and Stiglitz, 1982). These inputs flow through local supply chain networks (networks of households buying and selling output, raw material or intermediate goods) and labor networks (networks of households providing and hiring labor).

Households are also exposed to shocks, such as health, weather and business demand, affecting both consumption needs and production. These may be smoothed by transfers or loans from other households, via self-insurance, or not at all. Linkages between households mean that what happens to one household has the potential to propagate to others, too. This paper documents that this potential is empirically relevant. Local networks of gifts and loans (which we call "financial networks") provide insurance, as previous work has shown (e.g., Udry 1994; Townsend 1994; Samphantharak and Townsend 2018). On the other hand, when financial networks fall short of full insurance, supply chain and labor networks propagate shocks.

We leverage a unique dataset to examine network effects—both positive (insurance) and negative (propagation). The Townsend Thai data, constructed from 14 years of monthly panel surveys to households in rural and peri-urban Thai villages, allow us to identify idiosyncratic shocks to households' budgets. Using detailed information regarding gifts, loans and transactions across family-operated businesses, we are able to observe labor, supply chain, and financial networks, and study their evolution over time and their role in mitigating and/or propagating shocks.

Of course, shocks to household endowments are not typically exogenous, which makes it challenging to empirically identify their causal effects. To overcome this challenge, we exploit variation in the timing of episodes of sudden increases in health spending and construct counterfactuals for shocked households using households that experience similar shocks, but at different points in time (Fadlon and Nielsen, 2019). These sudden increases in health spending are idiosyncratic shocks to household endowments, which constitute exogenous and inescapable obligations that generate pressure on household budgets. We show that, conditional on ever experiencing a health spending shock, their *timing* is exogenous and uncorrelated across households. We also show that the shocks are severe—they are twice as large as average per capita food consumption and coincide with sharp increases in inpatient care.

Summary of Results

We first analyze the direct effects of idiosyncratic shocks on household consumption, production, and financing decisions. We find that directly-shocked households are able to smooth the shock on the consumption side. We find neither significant nor substantial changes in non-health consumption for these households in the aftermath of a shock. This consumption-side smoothing is achieved, in part, through intra-village insurance. Shocked households were more likely to receive transfers from other households in the village, constituting a 29% increase in total incoming transfers relative to the pre-shock periods. This result highlights the importance of local financial networks in providing insurance against idiosyncratic shocks.

However, while local financial networks provide insurance, this informal insurance is incomplete.[1] As a result, in order to fully smooth consumption, shocked entrepreneurs reduce business spending: in essence, financing the shock out of the business budget. They substantially reduce costs (23% decrease), and almost entirely cut their demand for external labor (79% decrease). This decrease in productive activities leads to an average 12% reduction in revenues, relative to the pre-period. Shocks to the health of prime-aged members could also affect labor endowments; however, we show that even shocks to children and elderly household members affect production. Thus, shocks to households' consumption needs affect production-side decisions, in violation of the separation theorem (Benjamin, 1992).

The impacts of health shocks are related to incompleteness in insurance and labor markets. Shocks to households with limited access to informal insurance (that is, with limited participation in gift and loan networks during the year preceding the shock) reduce costs and revenues by 34% and 27%, respectively. In contrast, for households with higher informal insurance participation, these decreases are almost fully attenuated. Thus, households that are not well-integrated into local informal insurance networks are most vulnerable and so, in turn, are their network connections. Additionally, the declines in production are larger when the shocks also affect household labor endowments and thus are unlikely to be fully insured by local financial networks. Namely, shocks related to the illness of prime-age household members (i.e., financial and labor endowment shocks) are associated with significant declines in hired labor and business costs, while shocks affecting elderly or children (i.e., mostly financial shocks) affect business revenues but do not significantly affect hired labor or costs. Thus there appears to be a missing market for individual-specific labor

[1] In the half-year of the shock, less than half of the spending need is met by transfers: see Figure 3.

inputs into household production, so naturally, the separation hypothesis fails.

We next examine the *indirect* impact of these shocks: the effect on other local businesses and workers. Our empirical strategy relies on variation in the proximity of a given household to the shocked household through pre-period economic networks. We undertake a generalized difference-in-difference analysis: comparing changes in outcomes before and after each shock, between more-exposed households (i.e., those that are closer to a shocked household in the pre-period network) and less-exposed households (those farther away). Closer households, with greater exposure, see larger falls in total transactions (a 21% decline for a unit change in closeness), and therefore a fall in income (12% decline for a unit change in closeness). They do not experience a change in incoming gifts. Given a fall in income and no offsetting change in gifts, consumption falls (4% decline for a unit change in closeness). These indirect effects are largely driven by shocks to underinsured households, suggesting that, by protecting directly hit households, local financial networks can also reduce or prevent propagation.

Propagation occurs in a context of rigid networks: suppliers struggle to find new customers when their clients suffer a shock, and workers struggle to find new jobs when existing employers face shocks. We document that links are quite persistent: *ceteris paribus*, households that transacted at baseline are substantially more likely to transact ten years later, relative to households that did not transact at baseline. Kinship relationships are strong predictors of trade, highlighting the importance of time-invariant contract-enforcement barriers to trade across households (Ahlin and Townsend, 2007; Johnson et al., 2002). Frictions leading to rigid labor networks are particularly important in the Thai setting, as proximity to the shocked household via the labor market network is most strongly associated with indirect effects on income and consumption.[2] These indirect effects persist even two years after the shock, suggesting that, in a context of rigid networks, the recovery from indirect shocks can be sluggish.

As expected, the magnitude of the indirect effect experienced by a single household is smaller than the magnitude of the effect felt by the directly-hit household (i.e., the one experiencing the health shock). But, the indirect effects hit many more households. As a result, a simple back-of-the-envelope exercise suggests that the total village-level magnitude of the indirect effects of a given shock may be as large or larger than the direct effects of that shock, with a multiplier (i.e.,

[2]Evidence of frictional slack in goods and labor markets is also shown by Egger et al. (2019) in the context of rural Kenya and by Breza et al. (2020) for rural India. Chandrasekhar et al. (2020) present a model of network-based hiring arising from informational frictions.

a ratio of the total indirect fall in consumption to the direct reduction in business spending) of approximately 1.5.

Contributions to the Literature

Previous studies have provided evidence of non-separability of household consumption (labor supply) and production (input or labor demand) decisions in developing countries.[3] We build on this literature by showing that idiosyncratic shocks to household spending can affect not only the production decisions of shocked households but also those of other (non-shocked) households. Our findings emphasize the dual role of networks in understanding non-separability and its consequences: risk-sharing networks provide insurance, but production networks increase the risk of propagation.

In turn, we complement the empirical literature studying the firm-to-firm propagation of regional or sectoral shocks through production networks.[4] We leverage our unique data and the context of family-owned firms (micro and small enterprises) to show that, in line with recent macroeconomic models linking granular shocks to aggregate fluctuations (Gabaix, 2011; Acemoglu et al., 2012; Farhi and Baqaee, 2020), granular shocks to health spending affect family enterprises and propagate to other local firms. Our ability to focus on local and granular shocks is important as a large share of firms across the world are small and family-operated (Beck et al., 2005; Banerjee and Duflo, 2007; La Porta et al., 1999; Bertrand et al., 2008), and thus exposed to shocks affecting family endowments. The fact that we uncover a multiplier (i.e., a ratio of indirect to direct effects stemming from a change in demand) that is similar to multiplier estimates for demand shocks in the US (Nakamura and Steinsson, 2014; Suárez Serrato and Wingender, 2016) and in Kenya (Egger et al., 2019) suggests that our results have applicability beyond the specific context of Thai villages, and that, in settings of imperfect insurance and rigid supply-chain and labor networks, idiosyncratic shocks can lead to similar multipliers as those generated by aggregate shocks.

This paper also contributes to the literature studying local economic networks in developing countries (e.g., Bramoullé et al. (2016); Chuang and Schechter (2015); Munshi (2014)). Previous studies have analyzed the ability of households to use local networks to buffer shocks (Townsend,

[3]See for example Benjamin 1992; Dillon and Barrett 2017; Dillon et al. 2019; Samphantharak and Townsend 2010; LaFave and Thomas 2016; Samphantharak and Townsend 2018, among others.

[4]There is a growing literature in international trade studying the propagation of shocks through production networks in the aftermath of natural disasters (Barrot and Sauvagnat, 2016; Carvalho et al., 2020), trade shocks (Tintelnot et al., 2018; Huneeus, 2019), and sectoral or regional shocks (Caliendo et al., 2017).

1994; Kinnan and Townsend, 2012; Angelucci and De Giorgi, 2009)[5]; we build on these studies by showing that access to informal insurance not only mitigates the direct impact of idiosyncratic shocks, but also reduces the degree of propagation to *other* households. This result underscores the importance of expanding safety nets in developing countries. Studies analyzing the direct effects of health shocks on households highlight the potential household-level gains from expanding insurance (Gertler and Gruber, 2002; Genoni, 2012; Fadlon and Nielsen, 2015; Dercon and Krishnan, 2000); however, to our knowledge the spillover benefits of such programs via reduced shock propagation, have not previously been acknowledged. Moreover, our findings show that, from a methodological perspective, local spillovers should be taken into account when analyzing the incidence of both consumption and production-side shocks:[6] households who do not directly experience a shock are cannot be used as a control group without understanding if and how they may be *indirectly* affected by the shock.

The rest of the paper proceeds as follows. Section 2 describes the dataset and the process to elicit local networks. Section 3 discusses the steps to compute the idiosyncratic shocks. Section 4 analyzes the direct effects of the shocks on business performance and labor earnings, while Section 5 analyzes the indirect effects. Section 6 analyzes potential explanations for propagation. Section 7 offers a simple back-of-the-envelope calculation to put the effects in context, and Section 8 concludes.

2 Context and Data

2.1 Characterization of the environment

Our approach to empirically studying the direct and indirect effects of idiosyncratic shocks, and considering possible policy responses, rests on the standard theoretical characterization of village economies. This characterization delivers several testable implications.

The timeline for production is as in Moll (2014) and Samphantharak and Townsend (2018): given previously accumulated capital k and current shocks z, a household running a firm decides on hiring labor and purchasing intermediate inputs to produce output at the end of the period,

[5] Other studies have documented the crucial role of local networks in the adoption of technologies (Beaman et al., 2018; Banerjee et al., 2013; Emerick, 2018), reducing adverse selection through peer referrals (Beaman and Magruder, 2012), the diffusion of information (Banerjee et al., 2019) and overcoming enforcement problems (Chandrasekhar et al., 2018).

[6] Angelucci and De Giorgi (2009) make the important point that shocks propagate via risk sharing but, to our knowledge the scope for propagation via *production* networks has not been highlighted.

subject to potential collateral constraints on financing. The household-as-firm is forward-looking and maximizes current plus discounted expected utility over consumption and leisure. In this setting, expenditure shocks are interpreted as an exogenous and inescapable obligation that hits during the production period, at the time of intermediate and labor decisions. Instead of operating a firm, the household can supply wage labor, as is standard in the endogenous occupation choice literature (Lloyd-Ellis and Bernhardt, 2000; Buera et al., 2011). One can refer to profits or wage earnings as (endogenous) income.[7]

The across-period problem considers capital accumulation, with $k' \neq k$, and financial assets a. However, unlike Bewley (1981) or Aiyagari (1994), risk-sharing transfers can take place to hedge *ex ante* shocks z. For example, in Samphantharak and Townsend (2018), transfers are determined as a solution to the full risk-sharing problem of a "planner" determining gifts and investment. As in Silva et al. (2019), households invest in real assets for which there is a market each period and in financial assets.

Another key feature of the environment is the set of agents a household-as-firm can transact with in the local markets. In Chandrasekhar et al. (2018), households experience participation shocks which determine whether in a given period they can participate in insurance networks (paying a premium or receiving indemnities) and trade in markets for safe and risky assets, or not.[8] Participation could be determined by exogenous shocks, or rather modeled as endogenous, e.g., with moral hazard if there is an effort cost to joining and/or costly participation under noisy *ex ante* signals of within-period income. We go a bit further here and conceptualize this as a multi-period problem with correlated costs.

Likewise, supply-chain network decisions can be viewed as endogenous choices that entail a fixed cost of interacting with new firms.[9] Thus, supply-chain networks may be thin and persistent. Likewise, for wage labor, we have in mind that there is a cost to forming new employment contracts. Both types of costs could either subtract from economic resources or could be non-pecuniary. Finally, there may be another production sector a household can choose: selling at small economy-

[7]In practice, a household consists of multiple members, with multiple sources of income, but this is easily incorporated as a household with a unitary utility function engaged in multiple activities.

[8]Households that can participate in markets when markets are thin are most valued in terms of their Pareto weight, receiving higher than average consumption.

[9]Throughout the paper we adopt the terminology of the literature studying supply chain networks. We refer to households providing inputs or labor *to* shocked households as upstream households. Likewise, we refer to households purchasing inputs or labor *from* shocked households as downstream households.

fixed (given) prices, though this might have lower returns.

Conceptual Framework for Welfare. Community solutions to such a planner's problem can be termed constrained-optimal as they consider the various possible constraints. However, there may be scope for improvement. In Chandrasekhar et al. (2018), welfare can be improved by optimally-targeted liquidity injections,[10] in effect, allowing increased insurance. Potential spillovers or externalities are internalized in the planner's problem. Here again, there may be scope for improvement if instead the village economy is viewed as consisting of competitive markets. In that case, pecuniary and non-pecuniary externalities would make competitive equilibria not constrained optimal.[11] The various costs outlined above could be brought down by broader participation in cross-village insurance platforms and non-relationship-based matching platforms for production and labor. We see in the data that membership in networks is correlated with kin, and so non-kin are particularly vulnerable even within a village.

Exact welfare statements and particular remedies require additional steps in modeling and take us beyond the fact-finding mission of this paper. We should note, however, that adjustments can be costly in utility terms. Consumption, which is steady, may be achieved by costly mechanisms (Chetty and Looney, 2006). Households may have diversified income sources *ex ante* at the expense of higher returns, or *ex post* as a consequence of shocks (Morduch, 1995). We use the loss of income as a back-of-the-envelope measure of spillovers, but this is only a proxy for impact and not welfare.

Empirical implications. Our characterization of the economic environment suggests several testable implications. First, the extent to which households can access state contingent assets (gifts and loans) for risk smoothing will determine the extent to which they need to smooth shocks via business disinvestment; that is, the extent to which the separation theorem fails. Secondly, the nature of the shock—whether only affecting available cash on hand, or also affecting the household labor endowment—will also affect the extent of separation failure since pure cash shocks may be more insurable than shocks to labor endowments.

Turning to implications for indirectly shocked households, the extent of propagation is expected to depend, first, on the households' proximity to the shocked household in the pre-shock network, which determines its exposure to the shock and, second, on the extent of network rigidity, which determines a household's ability to find new buyers or employers in the network. Additionally, the extent of propagation may depend on whether a household was connected to the directly shocked

[10] A new measure of financial centrality is used to determine who would optimally get these, see footnote 8.

[11] Remedies consist of better market design (Kilenthong and Townsend, 2011; Jain and Townsend, 2014).

household via the labor market network, supply chain network, or both, since the two networks may exhibit differential degrees of frictions such as moral hazard and, in turn, differential rigidity.

2.2 Household data

The data we use in this study come from the Townsend Thai Monthly Survey. The survey follows a sample of households from 16 randomly selected villages in four provinces in Thailand: Chachoengsao and Lopburi provinces in the Central region and Buriram and Sisaket in the Northeastern region. On average, the survey covers approximately 45 households per village, representing 42% percent of the village population.[12] The baseline interview was conducted from July to August 1998, collecting information on the demographic and financial situation of the households as well as ecological data of the villages. The subsequent monthly updates began in September 1998 and had continued through November 2017.[13] The sample in this paper covers the period between September 1998 and December 2012. We focus our analysis on the subset of 509 households that responded to the interview throughout all survey waves.

Table 1 characterizes the sample households in terms of their demographic, financial and business characteristics. Consistent with our characterization of village economies, it shows that while households derive income mostly from family farms, they also operate off-farm businesses and provide labor to other households or businesses. In addition, 13% of their total income comes from the receipt of government transfers and/or gifts from other households. Out of their income, households tend to allocate around 50% of their resources to consumption, and use the remaining resources to accumulate assets, which are evenly distributed between liquid and fixed assets. In terms of access to financial markets, on a given year, 83% of the households report borrowing from any source, 48% from formal or quasi-formal financial institutions,[14] and 30% from personal lenders, including relatives.

[12] There is one exception: one sampled village in Sisaket has a total population less than 45, so all households are included in the survey.

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

[14] There are different types of financial institutions operating in these markets. The most prominent institution is the Bank for Agriculture and Agricultural Cooperatives (BAAC). Community-driven institutions such as cooperatives, production credit groups and village funds also an important source of loans in the village.

2.3 Networks data

The Townsend Thai Monthly Survey contains detailed information on transactions among households and captures different types of economic inter-linkages. During each survey wave, interviewees identify any and all households in the village with whom they have conducted a given type of transaction.[15] We aggregate the monthly transaction data by year to elicit three types of village networks, for each year in the sample. First, we recovered information regarding intra-village financial networks, which include the provision and receipt of gifts and loans. Second, we recovered the supply chain networks that capture transactions of output, inputs and intermediate goods across businesses of households in the same village. Third, we also recovered labor networks that capture employer-employee relationships across households in the same village. Finally, as the baseline survey asks each interviewee to list all their first-degree relatives living in the village, we are able to elicit time-invariant baseline kinship networks.[16]

Figure 1 depicts different networks for a sample village. It suggests a significant degree of variation in the number of households interacting in different markets. The financial network appears rather thin. In contrast, there appears to be a great degree of interconnection across households in the supply chain and labor networks.

Of course, depictions of one village are only suggestive. Panel C of Table 1 shows average participation across the sample as a whole. On average, 35% of the households in the sample participate in the local financial networks in a given year—i.e., give or receive at least one gift or loan from other households in the village. Just under half (48%) of the households transact in the local village supply-chain network by transacting inputs and final output, and 62% provide or purchase labor to/from other households in the village. Out of these networks, households seem to be particularly well connected to local labor networks. On average, households transact with three

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

[16] As it is usually the case in networks based on survey instead of census data (Chandrasekhar and Lewis, 2017), our networks may look thinner than the networks that would be elicited using census data (unavailable to us). For instance, suppose that in the population network, household i is connected to j through connections with households k and h, such that $i \rightarrow k \rightarrow h \rightarrow j$. Suppose that households i and j are in the survey sample but households k and h are not. While we are able to recover the links between household i and k and households h and j, we would not be able to recover the links between households k and h. As a result, i and j would appear unconnected in the sampled network when they are in fact indirectly connected. Thus, we would be underestimating the distance between households i and j. We discuss the implications of this source of bias for our research design in Section 5.

different households during a year. Households also tend to participate in several networks, in a given period. For instance, among those that trade on financial networks, over 60% also transact in local supply-chain networks and over 70% of them transact in local labor markets. Over 77% of households transacting in the village supply-chain networks also sell or purchase labor locally, and 45% trade in local financial markets. Likewise, over 59% and 43% of households that buy or provide labor locally transact in the supply-chain and financial village networks. In addition, as we discuss in the next section, local kinship networks also overlap with these transaction-based networks.

2.4 Rigidities in local networks

Our characterization of the village economy suggests that costs related to the creation of new links may limit the participation of some households in local networks. Indeed, Panel C of Table 1 shows that while an significant share of households transacts in the village networks, there are also households that do not participate in the networks. There is evidence from other contexts suggesting that market frictions may prevent transactions across businesses. For instance, Johnson et al. (2002) finds that access to adequate institutional infrastructure (e.g., well-functioning courts) encouraged business owners to try new suppliers in post-Communist countries. In the Thai setting, Ahlin and Townsend (2007) provide evidence highlighting the importance of social ties and local sanctions in the context of joint-liability loans, for which commitment is crucial. Other sources of frictions may include product specificity (Barrot and Sauvagnat, 2016), or barriers to trade related to racial/ethnic differences of owners of small businesses (Aaronson et al., 2004).

If the aforementioned barriers are empirically important, one should observe a large degree of persistence in the economic networks. To test for rigidities in the local networks, we construct a dyadic dataset including indicators of whether each pair of sample households (dyads) transacted in year t either in the local goods, labor or financial market. We then use this dataset to estimate the following model:

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

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.[17] 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).[18] 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 2 presents estimates of equation (1). There is an important degree of persistence in the labor-market and supply chain networks, with raw auto-correlation coefficients of 0.46 and 0.42 (see column (1) in each sub-panel). These are substantially higher than that of the financial network (0.26). In the case of the labor market and the supply chain networks, having transacted during the previous period explains one-fifth of the overall variation in the current probability of trading. This pattern contrasts sharply with the case of the transactions in the financial markets (gifts and loans) as transactions in period $t-1$ only explain 7% of the overall variation in the probability of transacting at t. One explanation is that financial networks are less active, and are probably responding to either unexpected business opportunities or shocks. We explore this premise in Section 4. Persistence remains substantial after controlling for village-year fixed effects, suggesting that economic linkages respond mostly to within-village variation (see column (2) in each sub-panel).

In columns (3) and (4), we analyze whether persistence is related to kinship relationships, differences in demographic characteristics or differences in endowments (net worth). Although, in all three networks, controlling for baseline kinship links reduces the persistence coefficients, they are still high. Persistence does not seem to respond to including measures of differences in terms of demographic characteristics or initial wealth. In all cases, pairs that share kinship connections are 10 percentage points more likely to trade. The probability of trade in the supply chain and labor networks does not respond to differences in distance or wealth between the two households. In contrast, the probability of trading in the local financial network increases when households are different in terms of demographic characteristics, but decreases when there are differences in baseline wealth in the pair. This pattern highlights two features of local financial networks. First, among those households with similar wealth, households that differ in demographic characteristics

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

[18] 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.

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.

In sum, the labor-market and supply chain networks exhibit a striking degree of persistence over time. One implication of this persistence is that the effects of shocks which propagate via these networks may be quite persistent, an implication that we test in Section 5.

3 Constructing idiosyncratic shocks

Our goal is to examine how household production decisions respond to idiosyncratic shocks to household wealth and labor endowments, and whether these shocks propagate to other households through village economic networks. In this paper, we focus on idiosyncratic events associated with high levels of health spending to identify episodes of high financial stress.[19] Analyzing these shocks is important for three reasons. First, it has been shown that health shocks affect household finance and labor supply (Gertler and Gruber, 2002; Genoni, 2012; Hendren et al., 2018). Second, because these shocks are uncorrelated across households, we are able to separate these idiosyncratic shocks from aggregate shocks that could directly affect economic activity through changes in the markets for final goods, intermediate inputs, and labor. Finally, it allows us to analyze shocks that are, in principle, insurable through local networks, and so to understand whether individual responses to such shocks vary with access to local insurance networks.

We identify the shocks as follows. On a monthly basis, we compute health spending as the sum of spending on medicines, transportation to medical facilities, and fees related to either inpatient or outpatient care. For each household, we identify the month with the highest amount of monthly health spending throughout the panel. We focus on the largest shocks as we want to restrict the

[19] Thailand has a universal health insurance program, so these expenses are above and beyond those covered. The insurance program essentially covers expenses related to basic healthcare services. These services 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`

analysis to shocks that pose a financial burden to the household.[20] To facilitate measuring responses to the shocks by comparing households' behavior before and after the episodes, we restrict the search to years 2-12 in the panel (out of 14 years of monthly data). This enables us to observe at least two years of both pre- and post-shock behavior for all households. Following this approach, we identify 505 episodes of sudden increases in monthly health spending, one per household.[21]

We exclude 32 events related to possible pregnancies/births from our analysis as the associated changes in health spending are likely to be anticipated.[22] After excluding possible pregnancies, there are 473 shocks. In addition, for a subset of 405 of these events, we were able to identify the health symptoms affecting household members at the time of the events, and when these symptoms were initially reported. Appendix Figure A1 shows that, prior to the sudden increase in health spending, the median number of consecutive months in which households report any health symptoms is three months. 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.[23] In the case of the 85 households for which we could not identify the beginning of the symptoms,[24] we coded the beginning of the event as three months before the episode of high total health spending. We present robustness checks varying the beginning of the shock in Section 4.3.1.

3.1 Characteristics of the shocks.

Relationship between health spending and health status. Appendix Figure A3 depicts total household health spending (left axis) and the probability of reporting health symptoms around the events of financial stress (right axis). The figure shows that health spending and self-reported symptoms co-move, confirming that the events are correlated with decreases in household health

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

[21] In some cases, our approach identified more than one sudden increase per household–i.e., increases of the same magnitude. In such cases, we only focus on the first increase to avoid sample selection issues due to repeated shocks.

[22] As we do not observe pregnancies or births directly, we do so by excluding events that coincide with the inclusion of a new child in the household roster within 12 months of the sudden increase in health spending.

[23] 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.

[24] There were 19 households for which symptoms were repeatedly reported for two years or more, and 68 households for which we did not find information related to symptoms.

endowments. Appendix Table A1 reports the distribution of types of health symptoms reported by shocked households during the two years around the shock, during non-shock periods, and during all the sample periods. Relative to non-shock periods, there is a lower incidence of transitory symptoms such as headaches, colds, cough or influenza during shock periods. In contrast, during the periods related to the shocks, there is a higher incidence of other less common symptoms which might be more severe.

Magnitude of the shock. Appendix Figure A3 also shows that episodes of high health spending represent a substantial financial burden for the households: on average, such increase in health spending is twice as large as the monthly average per-capita food expenditure, and represents 18% of average monthly household income. The figure also demonstrates that food consumption is smoothed even as health spending spikes, which we confirm via regression analysis below.

Shocks to household budget or household labor supply? The shocks are related to a substantial increase in spending needs but also to substantial declines in health status. Appendix Figure A2 shows that 50% of the shocks affected family members that were 52 or older, and that 10% of the shocks affected children under the age of 18. Thus, the majority of shocks are related to illness of non-prime age household members. Indeed, Appendix Table A2 shows that in the month before experiencing the shocks, on average, affected individuals spent most of their days helping with household chores rather than working in family businesses. Moreover, the distribution of type of symptoms around the shock matches more closely that of older individuals (see Appendix Table A1). Thus, for the subsample of shocks affecting non-prime age household members, we interpret the shocks as financial shocks. In contrast, around 40% of the events relate to household members in prime-working age, and we interpret this subset of shocks as affecting both spending needs and labor endowments. Below we test whether "financial only" shocks have different effects than "financial plus labor" shocks.

Are the shocks idiosyncratic? Our analysis requires that the events be idiosyncratic and their occurrence be uncorrelated with trends in household behavior. The top panel of Appendix Figure A4 presents the distribution of months associated with the beginning of each event. It shows that the event start dates are spread through all the periods in the sample and suggests that the events are indeed idiosyncratic. Indeed, the bottom panel shows that in over 87% of the cases, the shocks affected only one household per village at the same time.[25]

[25] We formally test whether village-level trends explain the occurrence of these events in Appendix Table A3.

4 Direct effects of idiosyncratic shocks

4.1 Research Design

Estimating the effects of idiosyncratic shocks on household outcomes requires a valid comparison group. Ideally, we would like to compare changes in outcomes before and after the shock between shocked households and otherwise-similar households who, by chance, were not simultaneously exposed to such a shock. We follow Fadlon and Nielsen (2019) and exploit the plausibly random variation in the timing of severe health shocks to approximate this ideal scenario. That is, we compare households that suffer a shock in a given period with households who will suffer a similar shock, but later in time.

We follow the approach of Fadlon and Nielsen (2015, 2019) to construct counterfactuals for shocked households. We compare the behavior of households that experienced the shock in period t (i.e., treated households), to the behavior of households from the same age group and village that did *not* experience the shock at time t, but experienced a similar shock later on, in period $t + \Delta$ (control households). We then undertake a difference-in-difference approach to estimate the effect of the shock by computing differential changes in outcomes, before and after the treated household's shock, between treatment and control households. The underlying identification assumption is that, in the absence of the shock, the outcomes of households in the treatment and control group would have followed parallel trends. By comparing households in the same village and age group, we isolate contemporaneous village-specific shocks and potential differences in the trajectories of business and household-finance outcomes that could vary along the life cycle (Silva et al., 2019). We validate the identifying assumption using flexible difference-in-difference estimates that allow us to analyze whether there were systematic differences across treatment and control groups before the occurrence of the shocks.

We operationalize this approach in three steps. First, we split households into two age groups—i.e., below and above the median household age at baseline (1997).[26] 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

[26] 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.

$t_{med} = \underline{t} + \Delta$ 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).[27] 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 A4), the placebo shocks occur within the domain of the actual shocks. As 243 out of 473 shocked households experienced a shock in the earlier part of the panel, this process yields 243 households in the treatment group and 230 in the control group.

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

Figure 2 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. 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.

[27] We define Δ as $\Delta = \frac{\bar{t} - \underline{t}}{2}$ for each age-group-village bin. On average, each sub-sample covers 66 months. We exclude shocks occurring during the first and last 24 survey waves to ensure that we observe pre and post outcomes for at least two years for all households—i.e., $\underline{t} >= 24$ and $\bar{t} <= 148$.

4.2 Estimation

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

$$y_{i,t} = \sum_{\tau=-4,\tau\neq-1}^{\tau=3} \beta_\tau \mathbf{I}[t=\tau] \times Treatment_i + \sum_{\tau=-4,\tau\neq-1}^{\tau=3} \theta_\tau \mathbf{I}[t=\tau] \qquad (2)$$
$$+ \gamma + X_{i,t}\kappa + \alpha_i + \delta_t + \epsilon_{i,t}$$

where $y_{i,t}$ denotes the outcome of interest corresponding to household i, during period t. We control for time-invariant household characteristics and aggregate time-varying shocks by including household fixed effects (α_i) and month fixed effects (δ_t). We denote $Treatment_i$ as an indicator of whether the observation belongs to the treatment ($Treatment_i = 1$) or control ($Treatment_i = 0$) group. Time to treatment is denoted by $\tau_{i,t}$ and is measured in half-years to increase precision. X is a vector of time-varying demographic characteristics including the number of male and female household members, age of the household head and maximum years of schooling in the household. The coefficients of interest are $\{\beta_\tau\}_{\tau=-4}^{\tau=3}$, which compare differences in changes in outcomes with respect to the period preceding the shock ($\tau = -1$) between households in the treatment and control group. We focus on a two-year (i.e., four-half year) time window before and after the shock as our panel is fully balanced during this period.

We complement equation (2) with a more parsimonious differences-in-difference specification:

$$y_{i,t} = \beta Post_{i,t} \times Treatment_i + \theta Post_{i,t} + X_{i,t}\kappa + \alpha_i + \delta_t + \epsilon_{i,t} \qquad (3)$$

where $Post_{i,t}$ is an indicator that takes the value of 1 in periods following the shock, and 0 otherwise. The parameter of interest, β, compares differences in outcomes before and after the shock, between households in the treatment group and the comparison group. In both specifications, we cluster standard errors at the household level as our main source of variation comes from cross-household variation in the timing of events.[28]

4.3 Results

Outflows of resources must equal inflows of resources plus changes in cash holdings. Thus, a shock to health spending, which entails large outflows of resources, can be financed through four types

[28] This also flexibly accounts for serial correlation within the household over time (Bertrand et al., 2004).

of (non-mutually exclusive) adjustments. First, the shock could crowd out non-health spending. Second, households may liquidate assets to finance spending needs. Third, households may receive other inflows of resources, either as gifts from other households, government transfers, or loans. Finally, the shock could affect household production decisions — reducing hired labor or business input spending to free up resources to meet the shock.

Figure 3 reports flexible difference-in-difference estimates following the specification in equation (2). Panel (a) shows that, relative to the control households, the shocked households experience a sharp increase in total expenditure. Panel (b) shows that shocked households experience a sudden increase in incoming gifts from other households. This increase in gifts from other local households highlights the importance of local informal insurance networks; when idiosyncratic shocks occur, other households respond by providing gifts or transfers to the affected household.

During the first six-month period after the shock, the increase in gifts and loans only covers around one-half of the increase in spending needs due to the shock.[29] As households are only partially insured through gift/transfer networks, the shocks affect other household financial decisions, namely production-side decisions. Panel (c) shows that there is a decline in fixed assets after the shock. Panel (d) shows that, compared to households in the control group, labor usage declines for shocked households. Panel (e) shows that input spending falls after the shock. Finally, Panel (f) shows that the slowdown in input spending coincides with a slowdown in revenues after the shocks. Note that the sharp declines in input spending and revenues coincide with the sharp increase in spending induced by the shock. One implication is that, as fixed assets can be hard/costly to liquidate quickly, households may have to meet short-term liquidity needs by drawing down working capital.

The dynamics of these effects suggest that households use gifts to (partly) finance immediate expenses, and appear to rely on alternative sources for financing remaining expenses related to the shock. Thus households may follow a pecking order when it comes to financing or coping with adverse shocks; they first rely on gifts, which may be less costly, and then turn to resources meant to fund their family businesses, which could compromise future income. We explore this possibility in the next subsection.

The graphical evidence suggests that despite the receipt of gifts and transfers, health-driven

[29]While gifts and loans are also received in later post-shock periods, we show below that households appear constrained in their ability to borrow and so, even if these future transfers are anticipated, they cannot be used to meet immediate spending needs.

shocks to household spending affect production decisions, inconsistent with the separation theorem. The graphical evidence also documents parallel pre-trends: for all six outcomes, there are no spuriously significant "effects" prior to the spending shock.[30]

To provide a quantitative assessment of the overall impact of the health shocks, in Table 3 we report difference-in-difference estimates of the effect of the shock on outcomes, corresponding to equation (3). Panel A examines household spending. Column 1 shows that the shock leads to a large increase in health spending. While this is by construction, the magnitude is notable, representing a roughly 350% increase relative to the baseline mean. Column 2 shows that during the two years following the shock, on average, total spending increases for shocked households, relative to control households, by an amount close to the effect on health spending, though the effect is not significant at conventional levels. Columns 3 to 5 shows that there are neither substantial nor significant effects on non-health spending (total, non-food, and food, respectively). Thus, in terms of non-health consumption spending, shocked households were able to buffer the shocks, despite strong pressures on household budgets.

We next turn to analyze whether the shocked households draw down assets to finance their spending needs. Panel B of Table 3 shows that households did not rely significantly on either deposits in financial institutions or cash in hand to cover their health expenses. We also fail to detect significant changes in inventory or livestock, which are traditional proxies for buffer-stock savings. Similarly, we do not find significant changes in household fixed assets. While savings and the stock of fixed assets decrease, the decrease is not significant over the two-year post-shock period. Thus, incoming gifts allow households to (partly) cope with the shocks and, as we show below, the remainder of the shock is buffered by reducing business spending.

Panel C of Table 3 shows that although incoming gifts significantly increased, there were no detectable effects on borrowing. One interpretation is that obtaining credit from banks or community-based organizations is costly and/or entails a significant amount of delay. For instance, village funds do not meet often enough to evaluate loan applications quickly.

Panel D of Table 3 shows that affected households significantly decrease spending on business inputs (column 1) and reduce the use of external labor (column 2). Households also appear to reduce the use of labor provided by household members (column 3), though the effect is not significant. There is no significant effect on business assets (column 4). As a result of reduced investment in inputs and labor (columns 1-3), there is a decrease in the revenues from family enterprises, as seen

[30]Appendix Figure A5 shows the same dynamics in the raw data.

in column 5. (The effect on revenues has a p-value of 0.106.) Thus, households insure consumption against health shocks, but consumption smoothing comes at the cost of a decline in household business investment and revenues.

4.3.1 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. Appendix Table A4 replicates the results from the previous section and shows results that are quantitatively similar, but estimated with higher precision since we now use 473 shocks as opposed to only 243, as in Table 3. By adding more shocks we are able to detect significant declines in fixed assets, household labor, and revenues.

Alternative definitions of shock onset. Throughout our analysis, we use the first consecutive month in which households reported experiencing health symptoms within the six months preceding the peak in health spending, in order to account for potential anticipation effects that could bias the results. Appendix Table A5 reports results from two alternative definitions of the beginning of the shock. Panel A reports estimates of the effects of the health shocks assuming that the beginning of the event coincided with the peak in health spending. This approach provides a lower bound since households may have adjusted their behavior before the peak. Panel B reports estimates assuming that the event started six months before the observed peak. In both cases, the estimates are qualitatively similar to those from our main specifications.

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

We also report results from a panel specification in the spirit of Gertler and Gruber (2002) and Jack and Suri (2014), where we regress the outcome of interest on an indicator of whether household i experienced a shock during the past 12 months, a vector of demographic characteristics, household fixed effects and village-month fixed effects. Panel B of Table A6 reports estimates following this approach. It is worth noting that, as opposed to our main specification, this approach ignores age-specific trends and does not guarantee that the comparison households—non-shocked households at time t—do not suffer a shock within 12 months of the shocks to the focal household. Reassuringly, we obtain qualitatively similar results.

4.3.2 Non-separability: Unpacking the effects on household production

The results above show that idiosyncratic shocks to household endowments also affect household production decisions. This is a violation of the separation that would occur with complete markets.This could be due to frictions in either local insurance markets and/or labor markets. Incomplete local insurance would imply that (some) households are unable to rely on transfers to smooth out shocks, and would have to reduce input spending. Incomplete labor markets would suggest that (some) households are unable to replace a sick worker due to missing markets for individual-specific labor. In this section, we examine both mechanisms.

The role of insurance networks. Although gifts and transfers provide households with insurance against shocks, not all households have equal access to this mechanism. Un- or under-insured households may have to cut input spending in order to smooth consumption. This suggests that shocked households who can rely more on local risk-sharing networks should have smaller adjustments to their production decisions. To test this idea, we use data on intra-village provision and receipt of gifts or loans during the year preceding the shock. We classify households with relatively more pre-shock connections in their networks (above median) as having "high" access to informal insurance, and others as having "low" access to informal insurance.[31] We then estimate the following triple differences model:[32]

[31]We perform this exercise for households in both the treatment and control group. For the control group, we construct the measure based on the number of transactions during the year preceding the placebo shock.

[32]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. The results using only households shocked in the first part of the period as treated are similar, see Table A7.

$$
\begin{aligned}
y_{i,t} =& \beta_1 Post_{i,t} \times Treatment_i + \beta_2 Post_{i,t} \times Treatment_i \times High_i \\
&+ \theta_1 Post_{i,t} + \theta_2 Post_{i,t} \times High_i + X_{i,t}\kappa + \alpha_i + \delta_t + \epsilon_{i,t}
\end{aligned} \tag{4}
$$

where $y_{i,t}$, $Treatment$ and $Post$ are defined as before. $High_i$ takes the value of 1 for households with high access to informal networks before the shock (either actual or placebo). The coefficients β_1 captures the effects of the shock for households with low access to insurance networks, and β_2 captures the differential effect for households with high access to intra-village insurance. The sum $\beta_1 + \beta_2$ captures the total effect of the shock for high-access households.

Panel A in Table 4 reports estimates of the direct effects of the shock on gift receipt and business outcomes by access to informal insurance. Households with high access to informal insurance are more likely to receive gifts from other households than those with low access, although the differences are not precisely estimated (see columns 3 and 4). Moreover, the statistically significant declines in input spending (column 8) and business revenues (column 9) for low-access households are almost fully offset for high-access households. The results suggest that households with limited access to insurance drive most of the declines in business activities, suggesting that incompleteness in local insurance markets may lead to non-separability of household spending and production decisions. Conversely, improvements in access to risk smoothing may reduce the extent of non-separability and thus reduce propagation.

The role of labor market frictions. To shed light on the role of labor market frictions, we examine the effect of shocks to different household members. Illnesses of non-prime-aged household members are unlikely to directly affect labor supply; such shocks are primarily financial and can, in principle, be smoothed by relying on gifts and transfers.[33] In contrast, the illness of prime-age members may decrease labor supply even if gifts fully cover health spending needs. This distinction would be irrelevant if shocked households could substitute household labor with external sources of labor. However, if households cannot substitute household and market labor, then shocks to household labor supply will affect production decisions (Benjamin, 1992). Thus we use an equation analagous to equation (4) to test whether shocks related to prime-age household members have

[33] Of course, financial shocks may lead to indirect effects on labor supply of non-shocked household members, if, for instance, they need to allocate time to take care of the sick household member. The underlying assumption here is that the direct financial component of the shocks is relatively more salient than the possible indirect effects. We return to this discussion in Section 6.

differential effects on business activity.[34]

Panel B of Table 4 shows that the spending increases resulting from the shocks to non-prime-age members are larger than those of prime-age members (see columns 1 and 2 for health and total spending, respectively). In turn, while shocks to non-prime-age members led to large significant increases inflows of gifts and transfers, shocks to prime-age household members lead to smaller and non-significant increases in gift receipt (column 5). However, when we consider impacts on production, we find that the shocks to prime-age members led to a larger and statistically significant drop in the household's own labor supply (column 7). This decrease is twice as large as for shocks related to illness of non-prime age household members and is not compensated by an increase hired labor. In addition, both input spending and revenues decrease accordingly (column 9).

Taken together, the heterogeneous effects analyses suggest that both insurance and labor markets are incomplete: households with less baseline access to local gift and transfer networks show more evidence of separation failure, and shocks to prime-aged household members, despite generating smaller spending needs, lead to larger impacts on business decisions.

We next turn to examining the effects of these shocks on *other* households, via propgation.

5 Economic networks and the propagation of idiosyncratic shocks

The results above show that health shocks affect household production. Given the significant degree of inter-linkage in the study villages, we next ask whether these shocks propagate to other households. Motivated by the previous findings, we analyze two propagation channels. First, shocks could propagate through local supply chain networks: health shocks lead to decreases in the supply and demand for inputs, which could lead to reductions in sales and revenue for those households that trade with shocked households. Second, shocks could propagate through local labor networks: as supply and demand for outside labor decreases due to the shocks, households that exchange labor with shocked households could suffer falls in hours, earnings and revenue.

5.1 Identification strategy

We exploit two sources of variation to test if idiosyncratic health shocks propagated to other agents in the local economy. First, we use variation in the timing of each household-level shock. Second,

[34] In the case of the placebo shock, we identify the household member who reports the symptoms in the actual shock and assume that the placebo shock would affect the corresponding household member.

a household's exposure may depend on their network connections to the shocked household via the supply chain or labor network (or both). Thus, we use variation from households in the village with different levels of exposure to the shocked household. We assess the propagation of idiosyncratic shocks to other local family businesses by comparing households who, before the shock, shared market inter-linkages with household j's businesses (exposed households) to those who were unconnected to household j before the shock (unexposed households), before and after the shock to household j.

Throughout our sample period, we observe multiple health shocks per village. We construct a dataset capturing information of non-shocked households before and after each health shock in the sample. For each event, we take two years of pre- and post-shock observations of households living in the same village of the directly shocked households.[35] We then stack the observations into a dataset at the household (i) by time (t) by event level (j), for each village.

We combine this dataset with information on network connections between the shocked household (j) and other households (i) in the village, measured during the year preceding the shock to household j. We use pre-shock networks as links may respond to economic shocks themselves. The assumption is that households that transacted with the shocked household during the pre-period, on average, would have been more likely to transact with the shocked households in the post-period, in the absence of the shock. This is consistent with the evidence of persistence in the village networks discussed in Section 2.4, and with evidence of the importance of time-invariant determinants of economic connections such as kinship relations (Kinnan and Townsend, 2012), race or caste (Munshi, 2014), and the existence of economic frictions such as contracting issues that may limit trade between households (Ahlin and Townsend, 2007), or between firms (Aaronson et al., 2004) in local economic networks.

We measure exposure by constructing an index of closeness in the village networks as the inverse distance in the undirected, unvalued network,[36] between household i and the shocked household j: $Closeness_{i,j} = \frac{1}{dist_{i,j}}$.[37] As non-shocked households are further away in the network from shocked households, exposure (closeness) decreases. We begin by computing overall closeness based on

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

[36] Below, we also present results using directed networks.

[37] This measure equals one if household i directly traded with the shocked household j and zero if household i does not have any direct or indirect connections with the shocked household. The geodesic distance between two unconnected nodes is $dist_{i,j} = \infty$ and so their closeness equals 0 in that case. By undirected networks we mean that we do not distinguish between incoming vs. outgoing transactions. Likewise, we weight each transaction equally for

transactions in the supply chain or labor networks as households can be exposed through either network. To distinguish between exposure in the supply chain and labor market networks, we also compute measures of closeness in each separate network.

We elicit economic networks using survey instead of census data (Chandrasekhar and Lewis, 2017). Thus, it is possible that we underestimate the closeness of some sample households to the shocked households.[38] Because we could be underestimating exposure—classifying some households who are unexposed when they are actually exposed—our results could be downward biased towards zero. Thus, we interpret our magnitudes as *lower bounds* of the indirect effects of idiosyncratic shocks on other households.

Finally, not all shocked households are active in the local markets for goods, and not all shocked households employ other villagers for their businesses. Thus, we analyze the propagation of shocks through village networks by focusing only on the events corresponding to the 391 households that traded in either the supply chain or labor market networks during the year preceding their shock.[39]

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

$$
\begin{aligned}
y_{i,t,j} = &\sum_{\tau=-4,\tau\neq-1}^{\tau=4} \beta_\tau \mathbf{I}[t=\tau] \times Closeness_{i,j} + \gamma Closeness_{i,j} + \mathbf{X}_{i,t,j}\kappa \\
&+ \alpha_i + \omega_j + \delta_t + \theta_\tau + \delta_t \times Degree_{i,j} + \epsilon_{i,t,j} \qquad (5) \\
y_{i,t,j} = &\beta Post_{t,j} \times Closeness_{i,j} + \gamma Closeness_{i,j} + \mathbf{X}_{i,t,j}\kappa \\
&+ \alpha_i + \omega_j + \delta_t + \theta_\tau + \delta_t \times Degree_{i,j} + \epsilon_{i,t,j} \qquad (6)
\end{aligned}
$$

where y denotes the outcome of interest for household i in village v at time t. In the "event-study" specification (equation (5)), $\tau_{j,t}$ denotes a half-year, which may precede ($h < 0$) or follow ($h >= 0$) the shock to household j. $Closeness_{i,j}$ denotes inverse distance to the shocked household during the year preceding the shock to j.[40] The coefficients of interest in equation (5) are β_τ, which capture relative changes in outcomes corresponding to quarter τ with respect to the quarter 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 (6), $Post_{t,j}$ takes the value of one during the two years following the shock to household j, and zero

our calculations.

[38] See footnote 16 for a discussion of this issue.

[39] This represents 83% of all the shocks in our analysis.

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

for the pre-period. The coefficient of interest, β, captures differences in outcomes with respect to pre-period, associated with one additional unit of closeness.

We control for household fixed effects (α_i), shocked-household fixed effects (ω_j), time-to-shock fixed effects (θ_τ), which vary across villages and over time, and a vector of time-varying demographic characteristics ($\mathbf{X}_{i,t,j}$).[41] We control for village- and month-fixed effects. We also control for time-varying shocks affecting more central households, which could also be more likely to be close to other households, by including interactions of the number of links of household i ($Degree_{i,j}$) during the year preceding the shock to j with month-fixed effects. We use two-way clustered standard errors at the event level j and household level i to allow for flexible correlation across households during the periods preceding and following event j and across responses of the same household i to different events. As we are focusing on indirect effects, we drop observations of directly shocked households j from the analysis. We also exclude observations of households that experienced their own shock within a year before and after the shock to household j.

The identifying assumption underlying our strategy of estimating indirect effects is that, in the absence of the shock to household j, the outcomes of households i and i', with differential closeness to j, would have evolved along parallel trends *ceteris paribus*, i.e., conditional on the vector of controls included in equation 5 and 6. We validate this identifying assumption by testing for a lack of differences in the pre-period; namely, for $\tau < 0$, we verify that β_τ is not different from zero.

5.2 Results: Propagation of shocks through economic networks

Figure 4 presents flexible difference-in-difference estimates following the specification of equation 5. After a health shock, households who are more connected to shocked households differentially reduce the number of transactions with other households in the village supply-chain (sales) networks. Panel A analyzes total transactions. Prior to the shock, transactions through the are not different for closer vs. more distant households. After the shock, however, sales network transactions decline more for households who are closer to the shocked household. Panels B and C show that sales and labor network transactions, respectively, each exhibit the same pattern seen for total transactions. Finally, Panel D shows that, as local networks are shocked, total income declines for households closer to the shocked household. In all four panels, the results for the pre-shock period show no evidence of differential pre-trends. Figure 5 shows an analogous result for total consumption expenditure, which declines in the post-shock period (and exhibits no differential trend in the

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

pre-period).

The effects on transactions, income and spending are evident in all three half-year periods following the shock and do not exhibit evidence of shrinking in magnitude over time. Thus 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 above (section 2.4) demonstrates that such reorganization of local ties is very difficult, at least over the span of 1-2 years.

Panel A of Table 5 shows difference-in-difference estimates corresponding to (6). It documents significant post-shock declines in the number of monthly transactions in the supply-chain (column 1) and labor-market networks (column 2), and in total transactions (column 3). These effects are large, representing declines of 20%, 24% and 21% relative to the pre-period means, respectively. Column 4 shows that these changes, in turn, reduce income: a one-unit increase in *Closeness* is associated with a fall in income of THB 1236, or 11.7% of the pre-period mean. As seen in column 5, incoming gifts and loans do not change in response to indirect exposure to shocks. With income falling and gifts and loans not filling the gap, consumption spending falls by THB 294, or 4.1% of its pre-period mean (column 6).[42] The fall in consumption is smaller than the fall in income, suggesting that indirectly shocked households were able to partly, though not completely, smooth out the health shocks affecting their networks.[43]

The reason the (non-health) spending of directly-shocked households does not fall in response to a health shock (see Section 4), while the consumption of indirectly-shocked households does fall, relates to the differing response of incoming gifts. Directly shocked households see economically and statistically significant increases in transfers, while indirectly shocked households do not. (It is worth noting that, in addition to receiving transfers, directly shocked households take other, costly, steps to buffer consumption, namely scaling back on business activities.) Two factors, not mutually exclusive, may explain the divergence in transfer behavior. First, the direct shocks are large increases in health spending, associated with changes in health symptoms. These shocks are salient and relatively unlikely to raise concerns of effort (moral hazard), verifiability (hidden income), etc. The indirect shocks, on the other hand, arise from reductions in supply and demand

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

[43] In Appendix table A8, we re-estimate equation 5, including village-by-month fixed-effects ($\upsilon_{v,t}$) to control for potential village-and-time-specific shocks. The results are quite similar to those from the main specification.

facing household businesses. Such shocks are likely less salient and potentially more subject to concerns of effort and verifiability, and hence potentially less insurable. Second, 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.

5.2.1 Propagation of health shocks through supply chain vs. labor networks

In Panel B of Table 5, we examine whether the effect of exposure through the supply chain network has different effects than exposure through the labor market network. Because the two networks are correlated, we analyze the effect of exposure to one controlling for the effect of the other.[44] 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 0.228. There is no effect on input/output transactions associated with proximity through the labor network. Analogously, column 2 shows that proximity through the labor market network has a negative and significant effect (-0.21) on transactions involving paid labor, while there is no effect seen via the supply chain network. The fact that proximity through the supply chain (labor) network is associated with changes in input/output (hired labor) transactions, and not vice versa, is supportive of the identification assumption, as confounds (e.g., differential trends between closer vs. more distant households) would likely manifest in both sets of outcomes.

In column 3, proximity via the supply chain network and the labor market network both have negative and significant effects on the total number of transactions (-0.206 and -0.244, respectively). In column 4, proximity via the labor market network is associated with a large and significant drop in income, while the effect of proximity via the supply chain network is small and insignificant. Column 5 shows that neither networks' proximity is associated with a large or significant effect on gifts/transfers. Finally, in column 6, proximity via the labor market network is associated with a large and significant drop in consumption spending, while the effect of proximity via the supply chain network is small and insignificant. We return to the interpretation of this difference in the impacts of exposure via the supply chain vs. labor markets in Section 6.2.

Transactions in vs. transactions out. Motivated by the fact that directly shocked households

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

reduce their demand for inputs and labor, we have focused our discussion on "upstream" propagation, to households selling goods or labor to the directly shocked household. However, directly shocked households also see falls in revenues (Table 3, Panel D, col 5), so there are likely to be negative indirect "downstream" effects on indirectly shocked households' ability to purchase inputs and labor. We investigate these effects in Appendix Table A9, which separately presents effects on outgoing/upstream transactions (columns 1 to 6) and incoming/downstream transactions (columns 7 to 12). Panel A shows that, using the measure of network proximity which combines the supply chain and the labor market networks, the fall in outgoing transactions documented above is matched by a fall in incoming transactions (input purchases and labor hiring, as well as total incoming transactions) which is similar in magnitude; both sets of effects are statistically significant. In Panel B, we distinguish closeness through the supply chain network and the labor market network. Controlling for closeness in the labor market network, closeness in the supply chain network is associated will falls in both outgoing purchases and incoming sales. Controlling for closeness in the supply chain network, closeness in the labor market network is associated with falls in both outgoing labor provision and incoming labor hiring. Both networks independently predict falls in total transactions. In sum, the health shocks we study generate indirect effects both upstream and downstream, as the costly adjustments taken by the directly shocked household reverberate through the local economic networks.

6 Propagation Mechanisms

6.1 Access to insurance and the propagation of shocks

Section 4 presented suggestive evidence that idiosyncratic shocks were more likely to trigger declines in business activities when the shocked household had limited access to informal insurance. This suggests that shocks to uninsured households may propagate more to other households. To test this hypothesis, we estimate the following difference-in-difference model:

$$y_{i,t,j} = \beta_1 Post_{t,j} \times Closeness_{i,j} + \beta_2 Post_{t,j} \times Closeness_{i,j} \times Access_j \tag{7}$$

$$+ \beta_3 Post_{t,j} \times Access_j + \beta_4 Closeness_{i,j} \times Access_j + \gamma Closeness_{i,j} \tag{8}$$

$$+ X_{i,t,j}\kappa + \alpha_i + \omega_j + \delta_t + \theta_\tau + \epsilon_{i,t,j}$$

where $Access_j$ is an indicator of whether directly shocked household j had above-median pre-period access to informal insurance networks, measured by the number of transfers and loans exchanged with other households in the village during the year preceding the shock. As above, $Closeness_{i,j}$ denotes inverse distance between household i and directly shocked household j during the year preceding the shock. For simplicity and to maximize power, we focus on overall closeness. The coefficient β_1 measures the change in outcomes after the shock associated with a one-unit change in proximity to the shocked household when that shocked household has below-median access to informal insurance, and β_2 captures the differential indirect effects when the shocked household had above-median access to informal insurance networks. The total indirect effect associated with a one-unit change in proximity to a shocked household with above-median access to informal insurance networks is $\beta_1 + \beta_2$.

Table 6 provides evidence that shocks to less-insured households propagate differently, compared with those to more-insured households. Column 1 shows that input/output transactions by indirectly affected households fall by 0.26 (from a base of 0.999) when the shocked household had low access to insurance in the pre-period. When the shocked household had high access to insurance, however, the fall in sales is much smaller (the differential effect is significant at the 1% level) and the net effect is no longer significant. For hired labor (column 2), the fall for indirectly affected households is 0.107 (from a base of 0.470) when the shocked household had low access to insurance in the pre-period; in this case, the fall when the shocked household had high access to insurance is similar. Column 3 shows that the fall in total transactions (summing input/out sales and hired labor), is 0.367 (from a base of 1.47) when the shocked household had low access to insurance in the pre-period; when the shocked household had high access to insurance, the fall in sales is reduced by 0.09 percentage points, although this difference is not precisely estimated it accounts for 25% of the decline in total transactions in the case of shocks to uninsured households.

Column 4 shows that income displays the same pattern: when the shocked household had low access to insurance in the pre-period, the fall in income associated with 1 unit greater Closeness is 1654 baht. When the shocked household had high access to insurance, the fall in income is lower by 1031 baht (the differential effect is significant at the 1% level) and the net effect is not significant. Column 5 shows that incoming gifts to indirectly shocked households do not respond, regardless of the baseline insurance status of the directly shocked household. Finally, column 6 shows that the consumption spending of indirectly affected households falls by 321 baht, or roughly 5%, when the shocked household had low access to insurance in the pre-period (the p-value of this effect is

0.134). When the shocked household had high access to insurance, however, the fall in consumption spending is reduced by 94 baht (the differential effect is significant at the 1% level).

6.2 Total spillovers and access to insurance

Our results highlight that the extent to which shocks propagate is a function of whether the directly-shocked household has access to informal insurance, with shocks to well-insured households being buffered more successfully and hence spreading less through networks. To assess how the total indirect impacts of shocks relate to informal insurance, we calculate how much shocks to more- vs. less-insured households propagate. Households with low access to insurance have a mean (median) of 21.42 (17) contacts and mean (median) closeness of 0.39 (0.38). The figures for those with high (i.e., above median) pre-shock access to insurance are similar, with a mean (median) of 21.14 (18) contacts and mean (and median) closeness of 0.46.

Using the figures from Table 6, column 6, the consumption effect associated with a one-unit increase is *Closeness* to a poorly-insured shocked household is a fall of -1654 baht (significant at 1%). The implied total indirect effect from poorly-insured households using mean values is therefore $-1654 \times 0.39 \times 21.42 = -13817$ baht per month. Using median values instead gives an implied total indirect effect of -10,685.

For well-insured households, the effect associated with a one-unit increase is *Closeness* is a fall of -622.2 baht (not significantly different from zero). The implied total indirect effect from well-insured households using mean values is -6051 baht per month. Using median values instead gives an implied total indirect effect of -5152. Thus, the aggregate "spillover" effect is nearly twice as large when the shock hits a poorly-insured household.

Taken together, these results suggest that when households facing health spending shocks have better *ex-ante* access to informal insurance networks, the propagation of idiosyncratic shocks to other households is meaningfully reduced. This, in turn, suggests that policies that improve informal insurance—e.g., by reducing frictions due to moral hazard, limited commitment, hidden income or asymmetric information—will have spillover benefits even to those whose direct access to risk sharing does not improve.

The cash vs. labor costs of health shocks. The direct-exposure results in table 4 show that while input spending and revenues of the shocked household decline less when that household has access to insurance, hired labor is affected even for high-insurance-access households (column 6). Likewise, the propagation results in Table 6 show no significant differences in labor-market transactions (column 2) as a function of the insurance status of the directly-shocked household. These results suggest that while incoming transfers help shocked households with expenses, they are unable to compensate for the time required to take care of the ill household member. As illnesses to prime-aged members induce a decline in hired labor provided by household members (Table 4, Panel B, col 6), this suggests that household labor and hired labor are complements, namely that household labor is required to supervise or monitor hired labor. This is further evidence of labor-market imperfections driving propagation, and moreover, such imperfections blunting the ability of gifts and transfers to smooth the impact of shocks.

A related finding appears in Table 5, Panel B, namely that the indirect-effect declines in income and consumption associated with exposure via the labor market network are large and highly significant while those associated with exposure via the supply chain network are indistinguishable from zero. This suggests that households connected to the shocked household via the labor market network may suffer a double impact, namely, reduced labor demand via an income effect as the directly hit household scales back, as well as a further hit due to the complementarity between household and hired labor.

Propagation via insurance? It is also possible that households connected to shocked households could have provided transfers in order to help them attenuate the consequences of the shocks. Thus, the indirect effect on consumption (Table 5, column 6) could be a consequence of a decline in cash on hand/liquidity arising from helping the directly shocked household. However, Appendix Table A10, shows that neither transfers nor loans given by the indirectly shocked household to other households increase following the shock.If anything, the point estimates suggest that the provision of transfers to other households declined among higher-exposure households, perhaps because they were experiencing negative indirect effects themselves. Moreover, the effect on consumption, while meaningful, is less in both absolute and percentage terms than the effect on income (column 4), suggesting that the fall in consumption can be fully explained by the fall in income stemming from reduced local sales in the input/output and labor networks.

7 Putting the findings in context

In this section, we offer two exercises to help benchmark the magnitude of our results. First, we show how the propagation of household-level shocks compares with that of a sector-wide shock. Second, we perform a simple back-of-the-envelope exercise to estimate the total magnitude of indirect vs.

direct effects on revenues.

7.1 Aggregate vs. idiosyncratic shocks

To illustrate the idea that local networks may be less able to provide insurance against aggregate shocks, Figure 6 provides a graphical comparison of the direct responses to idiosyncratic vs. aggregate shocks. We consider the timing of the European Union ban on Thai shrimp imports, which was announced in May 2002 and directly affected over 30% of the households in Chachoengsao, the shrimp-producing province in our dataset.[45] The "sectoral" line in the figure depicts differences in changes in incoming gifts, before and after the EU ban, between shrimp farmers and non-shrimp farmers.[46] The "idiosyncratic" line depicts changes in incoming gifts before and after health shocks, relative to a placebo group as in equation (3). There is only a small, insignificant increase in gifts within a year of the shrimp ban, in marked contrast to the sudden increase in gift inflows in the aftermath of the idiosyncratic shocks. This simple comparison suggests that the effectiveness of local risk-sharing networks depends on the nature of the shock and echoes our finding that, while households directly hit by idiosyncratic health shocks see an increase in gifts (as seen in the figure), indirectly-shocked households do not, as the indirect effects are quasi-aggregate, hitting many households in the network.

7.2 The multiplier effect of idiosyncratic shocks

As documented above, idiosyncratic health shocks have both direct costs (analyzed in section 4) and indirect costs (analyzed in sections 5 and 6). The former are larger on a per-household basis, but the latter can potentially affect many more households. In order to compare their overall magnitude, and so obtain an estimate of the overall "multiplier effect' of the fall in spending associated with the shock', we perform a simple calculation of the total indirect fall in consumption for each baht of reduced business spending by directly affected households.

The indirect effect on consumption associated with a 1 unit change in *Closeness*, from Table 5, Panel A, column 6 is a fall of -308.9 baht (significant at 10%). The mean (median) level of *Closeness* in the village network is 0.42 (0.43) and the mean (median) number of indirectly exposed households

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

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

(i.e., households who are connected to the shocked household via the network) is 21.23 (16).[47] The implied total indirect effect using mean values is therefore $-308.9 \times 0.42 \times 21.23 = -2754$ baht per month. Using median values instead gives an implied total indirect effect of -2125.

From Table 3, Panel D, column 1, the fall in business costs for a directly affected household is -1783.4 baht, so the indirect effects using mean and median closeness represent multiplier effects of 1.54 and 1.19, respectively. For comparison, Egger et al. (2019) estimate a consumption-expenditure multiplier of 1.7 from cash transfers in Kenya, and in the US, Nakamura and Steinsson (2014) estimate an "open economy relative multiplier" of 1.5 and Suárez Serrato and Wingender (2016) estimate a local income multiplier of government spending of 1.7 to 2. These estimates are quite similar to ours despite very different data and methods.

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.

8 Concluding remarks

We study the dual role of networks—providing insurance and propagating idiosyncratic shocks. Such shocks are widespread in both high- and low-income countries. Moreover, the novel coronavirus (COVID-19), while to a large extent an aggregate shock, has significant idiosyncratic aspects due to variation in household infection risks and realizations (Jordan et al., 2020), the ability to work from home (Dingel and Neiman, 2020; Angelucci et al., 2020), and the extent to which different workers and sectors of the economy are affected by shutdowns and social distancing (Daly et al., 2020). Thus the importance of understanding the ramifications of such shocks has, if anything, increased in the COVID-19 era.

If markets were complete, idiosyncratic shocks would be fully insured. Shocks would not affect production and hence no propagation would take place through production networks. However, in the absence of complete markets, spending-side shocks affect production-side decisions and, in turn, ripple out to other households. In such a situation, indirectly-affected households will see falls in local transactions and income and so are forced to cut consumption. The impact of propagation

[47] We report medians as well as means since the median is less sensitive to networks with a high number of connections or many distant (low-*Closeness*) connections, where the linear specification for *Closeness* may be less appropriate.

through supply chain or labor networks could be long-lasting as suppliers and customers may eventually switch to other partners or activities. This is potentially costly as there may be fixed costs of each partner (e.g., workers may need to be specifically trained for each employer). These costly adjustments could have been avoided if there were full insurance.

Empirically, we use variation in the timing of severe shocks on health spending experienced by households in Thai villages. We document consumption smoothing, i.e., no impacts on non-health consumption expenditure for directly shocked households. This smoothing is partially achieved through local gift and loan networks. However, insurance is partial and, as a result, shocked households need to adjust their production decisions–drawing down working capital, cutting input spending and reducing labor hiring. This propagates the shock to other households. Businesses close to the shocked household in the supply chain network experience reduced local sales. Workers closer to the shocked household in the labor network experience declines in the probability of working locally and reduced earnings. As a result, their consumption falls.

Our findings suggest that two sets of interventions might be beneficial: (1) policies that prevent granular shocks from propagating and causing aggregate fluctuations, and (2) policies that mitigate the indirect, propagated shocks, if they cannot be prevented. First, improved safety nets can help prevent granular shocks from amplifying to become aggregate. Given that the ability to share idiosyncratic shocks increases with the number of households participating in the insurance network, local networks alone may not be enough to diversify this idiosyncratic risk, such that the shared risk remains covariate. Formal commercial insurance contracts (through companies operating nationally or beyond) or social insurance (through central governments) could allow better risk-coping and better prevention of propagation through production and labor networks. Second, in order to mitigate the impacts of propagation through supply chain and labor networks, one can consider broadening the extent of product and factor markets beyond the local village market via regional or economy-wide platforms, to take advantage of more systematic multilateral matching.

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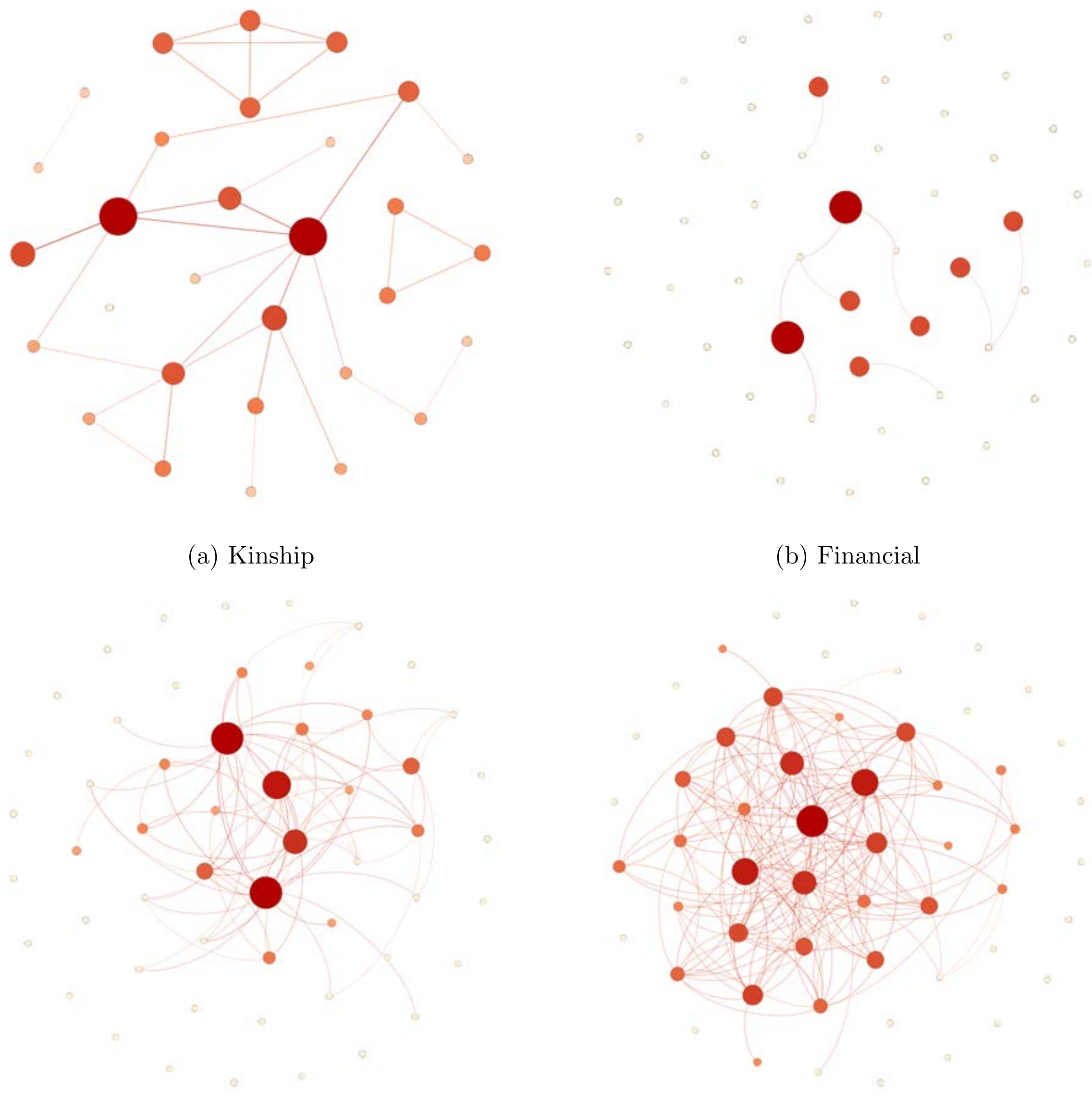
Figures



(a) Kinship

(b) Financial

(c) Supply chain

(d) Labor

Figure 1: 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 nodes are connected through kinship at baseline [Panel (a)], or whether they have transacted during the reference period [Panels (b) to (d)]. The reference period for Panels (b) to (d) is 2005. 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. 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.

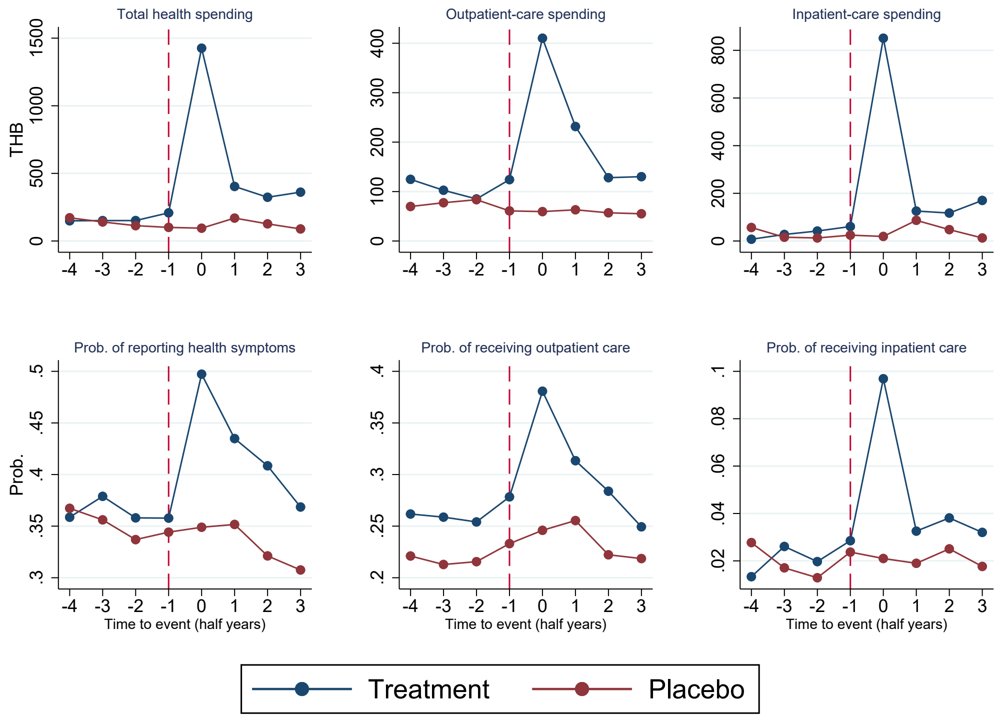
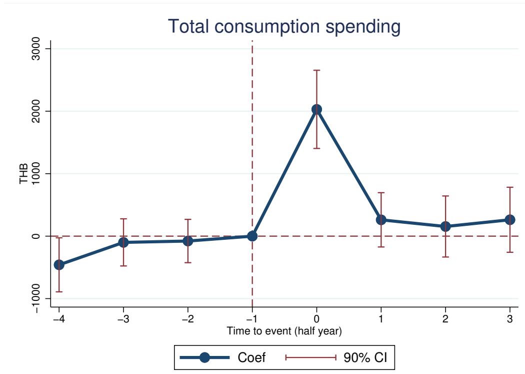
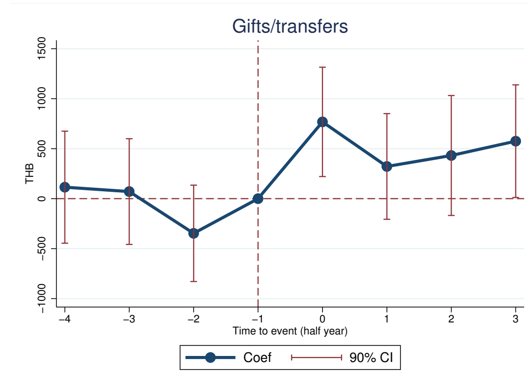


Figure 2: Health status and spending in the treatment and placebo samples

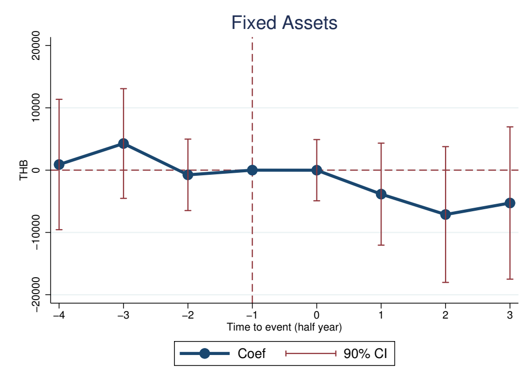
Note: The figure reports averages of health and total spending for periods before and after the health shocks (left axis). The right axis reports probabilities of reporting health symptoms before and after the shocks. The horizontal axis represents normalized time with respect to the event realization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



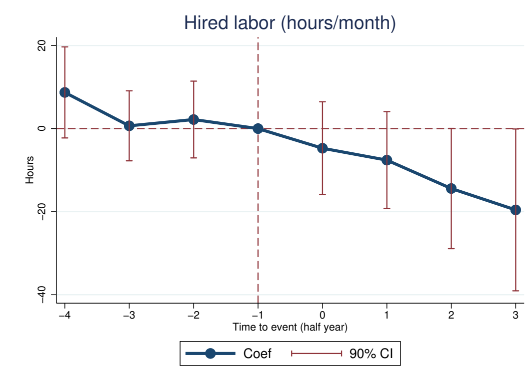
(a) Total consumption spending



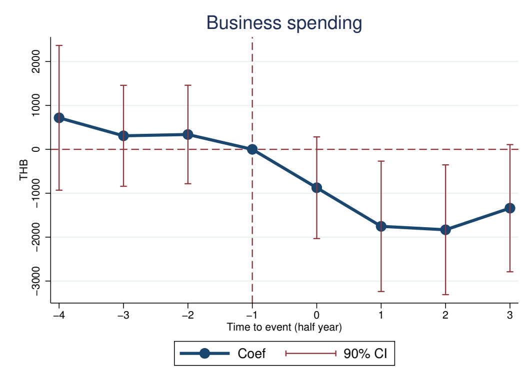
(b) Incoming gifts/transfers



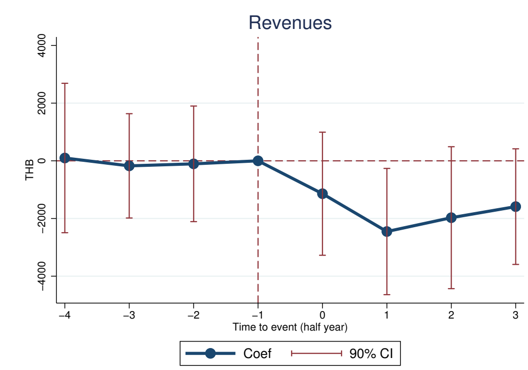
(c) Fixed assets



(d) Hired labor (hrs/month)



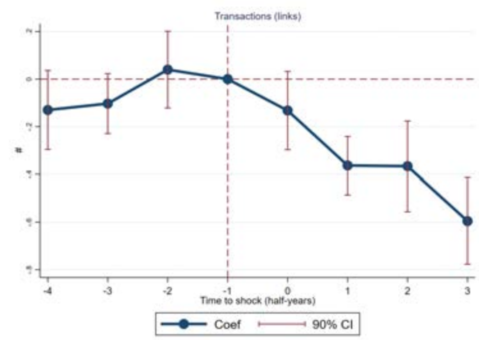
(e) Input spending



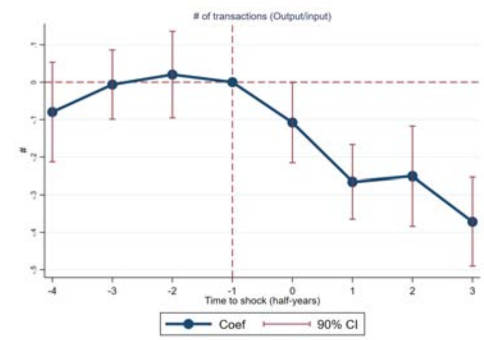
(f) Revenues

Figure 3: Changes in household outcomes before and after the shock

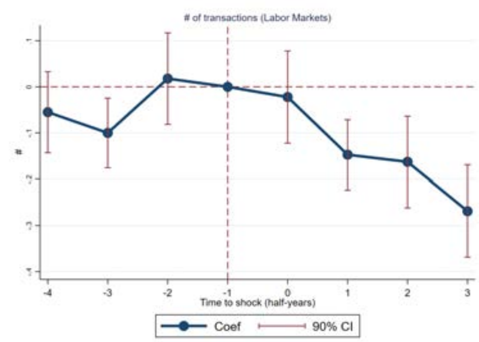
Note: Each dot represents differences between treatment and placebo households in changes in outcomes relative to the period preceding the beginning of the shock ($\tau = -1$).The estimating sample includes 2 years before and after the shock divided in half-year bins. All specifications control for household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



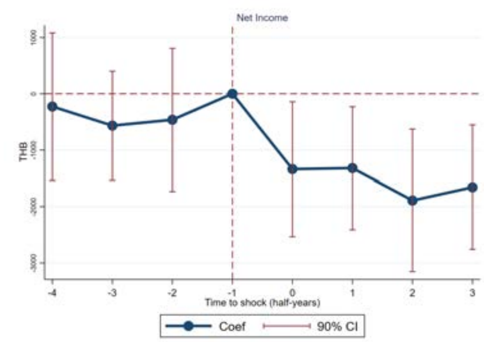
(a) Total transactions



(b) Sales network transactions



(c) Labor network transactions



(d) Total income

Figure 4: Indirect effects on transactions and income

Note: The Figure presents flexible difference-in-difference estimates of the indirect effects of idiosyncratic shocks on local businesses, following equation (5). 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).

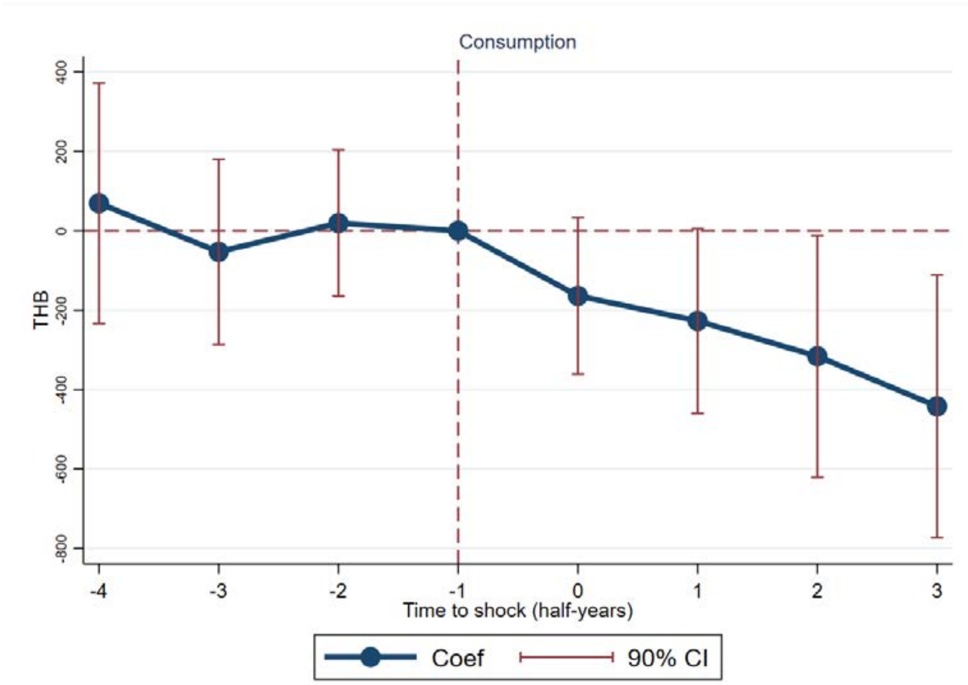


Figure 5: Indirect effects on total consumption spending

Note: The Figure presents flexible difference-in-difference estimates of the indirect effects of idiosyncratic shocks on local businesses, following equation (5). 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; that is, the coefficient on $\mathbf{I}[\tau_{j,t} = k] \times Closeness_{i,j}$, where $Closeness_{i,j}$ denotes inverse distance to the shocked household during the year preceding the shock to j. Standard errors are two-way clustered at the household (i) and shock (j) level.

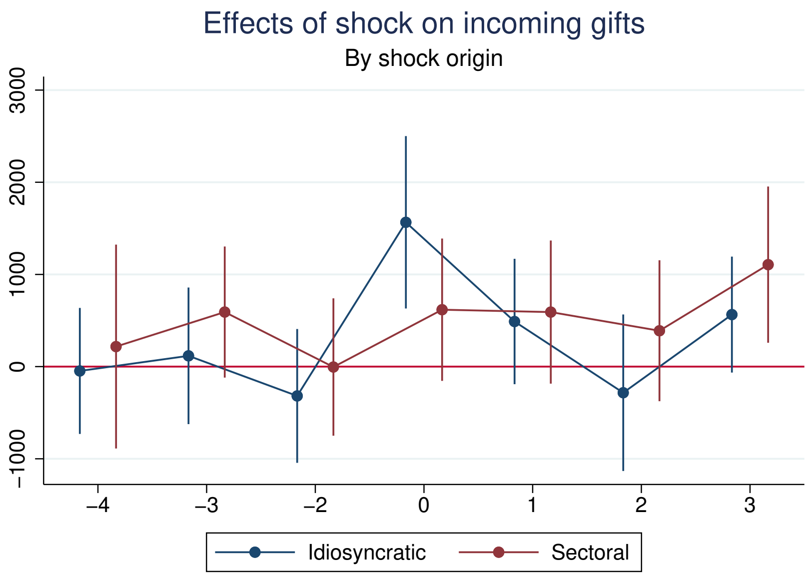


Figure 6: Effects on gift reception by type of shock

Note: The figures depict flexible difference-in-difference estimates of the effects of idiosyncratic shocks on incoming gifts (in blue) and of the EU shrimp ban on incoming gifts (in red). The figures focus on households in the 4 villages corresponding to the Chachoengsao province, where most of the shrimp activity takes place. The European Union import ban on Thai shrimp was announced in May 2002. The the effects of idiosyncratic shocks are estimated using the specification detailed in equation (3). The effects of the shrimp-ban shock are estimated using a regression of incoming gifts on month fixed effects, normalized with respect to May of 2002, and interactions of month fixed effects with an indicator that takes the value of 1 if the household had a shrimp farm before the shock. In this case, the effects of the shock are captured by the plotted interaction coefficients. In both cases, standard errors are clustered at the households level and are used to plot 90% confidence intervals.

Tables

Table 1: Summary statistics

	N	Mean	S.D.	10th %ile	90th%ile
Panel A: Household baseline characteristics					
	N	**Mean**	**S.D.**	**10th %ile**	**90th%ile**
Number of household members	509	4.54	1.87	2	7
Number of adults	509	2.87	1.38	1	5
Household head age	507	51.95	13.45	35	70
Average age	509	34.14	12.11	21	52
Household head is a male	507	0.77	0.42	0	1
Years of schooling: Household head	504	4.49	2.59	3	7
Years of schooling: Household maximum achievement	509	8.19	3.64	4	14
Years of schooling: Household average	509	5.09	2.17	3	8
Panel B: Household finance (annual data)					
	N	**Mean**	**S.D.**	**10th %ile**	**90th%ile**
Net Income in THB:					
Farm	7635	134389	1378506	-150	316500
Off-farm family business	7635	19095	115540	0	40700
Labor	7635	52816	108492	0	152222
Total from operations (farm+off-farm + labor)	7635	173327	618277	4974.07	410723
Gift/transfers	7635	24107	183826	-11613	75706
Total net income (Operations+Gifts/Transfers)	7635	197434	644150	16241	446693
Consumption in THB					
Food	7635	32952	21915	11931	60559
Total consumption	7635	98149	99486	24330	204512
Household Assets and Debt					
Total Assets (THB)	7635	2448596	7431394	194277	4817110
Fixed Assets/ Total Assets (%)	7635	53	27	13	88
Total debt/Total assets (%)	7635	12	21	0	27
Households with outstanding loans (%)	7635	83	38	0	100
Households with outstanding loans from institutions (%)	7635	48	50	0	100
Households with outstanding loans from personal lenders (%)	7635	30	46	0	100
Panel C: Village networks					
	N	**Mean**	**S.D.**	**10th %ile**	**90th%ile**
Baseline kinship networks: Degree (Number of links)	8344	2.36	2.19	0	6
Baseline kinship networks: Access (any link)	8344	0.77	0.42	0	1
Financial networks: Degree	8344	0.65	1.36	0	2
Financial networks: Access	8344	0.35	0.48	0	1
Sales networks: Degree	8344	1.26	2.64	0	3
Sales networks: Access	8344	0.48	0.50	0	1
Labor-market network: Degree	8344	3.07	4.42	0	9
Labor-market network: Access	8344	0.62	0.49	0	1

Note: Panel A reports summary statistics about demographic characteristics, measured at baseline. Panel B reports household financial characteristics based on annual averages using a balanced panel of 509 households. Farm income includes income from agriculture, livestock, fishing and shrimping. Off-farm income excludes earnings from labor provision. In both cases income is net of operation costs. Gifts and transfers include transactions from both households inside and outside the village, as well as the receipt of government transfers. Consumption includes spending but also consumption of home production. In Panel C, all networks are unvalued and undirected; all links have equal weight and the direction of the transaction is not considered. Kinship networks are measured at baseline, while transaction networks are measured on an annual basis. Financial networks are constructed based on gifts and loans between households in the same village. Supply chain networks include transactions of raw material and intermediate goods between businesses operated by households in the same village. Labor networks include relationships through paid and unpaid labor between households in the same village. Degree: Number of households with whom each household transacted in each year. Access: Takes the value of 1 if the household has participated in the network during a given year and 0 otherwise.

Table 2: Persistence in transaction networks, by network type

	Probability of a direct link											
	Supply chain				**Labor Market**				**Gift or Loans**			
VARIABLES	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Lagged Prob. of link (ρ)	0.469***	0.460***	0.379***	0.379***	0.426***	0.401***	0.333***	0.333***	0.260***	0.258***	0.209***	0.209***
	(0.015)	(0.014)	(0.011)	(0.011)	(0.012)	(0.013)	(0.011)	(0.011)	(0.015)	(0.015)	(0.013)	(0.013)
Kinship connection			0.099***	0.099***			0.109***	0.109***			0.091***	0.091***
			(0.006)	(0.006)			(0.007)	(0.007)			(0.006)	(0.006)
Demographic (log distance)				-0.014				-0.107				0.141**
				(0.120)				(0.131)				(0.071)
Net-worth distance (log of squared differences)				-0.037				-0.006				-0.035**
				(0.027)				(0.031)				(0.017)
Village fixed effects	No	Yes	Yes	Yes	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Year fixed effects	No	Yes	Yes	Yes	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Household fixed effects	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes
Observations	233,240	233,240	233,240	233,240	233,240	233,240	233,240	233,240	233,240	233,240	233,240	233,240
R-squared	0.221	0.227	0.268	0.268	0.189	0.207	0.241	0.241	0.067	0.069	0.102	0.102

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

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

Table 3: Effects on spending, assets, transfers, and family businesses

Panel A: Effects on Spending					
	(1)	(2)	(3)	(4)	(5)
			Non-health		
	Health	Total	Total	Non-Food	Food
Post X Treatment	534.8***	585.1	50.36	9.039	41.32
	(91.18)	(367.4)	(344.4)	(314.0)	(69.65)
Baseline mean (DV)	152.6	5451.0	5298.3	2903.7	2394.7
Observations	22709	22709	22709	22709	22709
Number of households	451	451	451	451	451
R-Squared	0.0489	0.154	0.146	0.0886	0.676
Panel B: Effects on household savings and assets					
	(1)	(2)	(3)	(4)	(5)
	Savings	Cash in hand	Livestock	Inventories	Fixed Assets
Post X Treatment	-1211.1	-16081.6	-1035.3	-7771.7	-5094.7
	(1847.5)	(22697.4)	(2467.5)	(7114.0)	(5471.4)
Baseline mean (DV)	5278.6	370556.0	36674.4	101918.1	88184.4
Observations	22709	22709	22709	22709	22709
Number of households	451	451	451	451	451
R-Squared	0.0627	0.885	0.926	0.885	0.854
Panel C: Effects on gifts, transfers and debt					
	(1)	(2)	(3)	(4)	(5)
	Gifts from village hhs				
	Prob.	count	Gifts/Transfers	Borrowing	Gifts+Loans
Post X Treatment	0.0103	0.0140	570.4***	113.0	668.1*
	(0.006)	(0.009)	(220.5)	(258.8)	(398.7)
Baseline mean (DV)	0.0192	0.0249	1957.3	234.4	2736.5
Observations	22709	22709	22709	22709	22709
Number of households	451	451	451	451	451
R-Squared	0.140	0.0695	0.159	0.0105	0.0399
Panel D: Effects on family businesses					
	(1)	(2)	(3)	(4)	(5)
	Costs	Hired labor	HH Labor	Biz. Assets	Revenues
Post X Treatment	-1783.4**	-14.33*	-10.89	1510.0	-1744.4
	(842.6)	(7.504)	(8.809)	(2101.4)	(1075.5)
Baseline mean (DV)	7610.2	18.11	154.1	33376.9	14939.0
Observations	22709	22708	22708	22709	22709
Number of households	451	451	451	451	451
R-Squared	0.781	0.578	0.712	0.911	0.620

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

Note: The Table reports estimates of β from equation (3) for different outcomes. Each column reports differences between treatment and placebo households in changes in outcomes before and after the shock. All regressions control for household demographic characteristics, household and month fixed effects. Standard errors are clustered at the household level. Costs, labor, assets and revenues are aggregated across all businesses operated by household members, and exclude revenues and costs of wage labor provision to other businesses or households. Hired labor and labor provided by household members are measured in hours/month.

Table 4: Heterogeneity in the effects of health shocks

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

	(1) Health Spending	(2) Total spending	(3) Prob. Gift (in village)	(4) # of Gifts (in village)	(5) Gifts	(6) Hired labor	(7) HH Labor	(8) Costs	(9) Revenues
Post X Treatment (β_1)	402.8***	628.9	0.00574	0.00494	458.5**	-8.895	-18.65*	-2596.8***	-4070.7***
	(65.57)	(520.5)	(0.00427)	(0.00556)	(206.7)	(6.467)	(9.830)	(824.1)	(1099.7)
Post X Treatment X High Access (β_2)	82.76	124.2	0.00594	0.0169	1.537	-5.949	7.040	2053.1*	3947.1**
	(108.9)	(630.5)	(0.0110)	(0.0156)	(351.9)	(10.57)	(14.69)	(1231.9)	(1647.3)
Effect: High Acceess ($\beta_1 + \beta_2$)	485.6	753.1	0.0117	0.0218	460.0	-14.84	-11.61	-543.8	-123.6
P-val: High Access	0.000	0.0650	0.259	0.138	0.109	0.0952	0.294	0.555	0.919
Baseline mean (DV)	148.8	6055.5	0.0203	0.0258	2248.3	16.93	144.2	7524.5	15228.3
Observations	37325	37325	37325	37325	37325	37325	37325	37325	37325
Adj. R-Squared	0.0435	0.101	0.113	0.0605	0.138	0.677	0.649	0.743	0.542

Panel B: Effects of the shocks by age of ill household member (prime age 18-60)

	(1) Health Spending	(2) Total spending	(3) Prob. Gift (in village)	(4) # of Gifts (in village)	(5) Gifts	(6) Hired labor	(7) HH Labor	(8) Costs	(9) Revenues
Post X Treatment (β_1)	563.6***	1064.4***	0.00896	0.00883	533.7*	-1.136	-5.741	-1468.8	-1962.8*
	(112.1)	(403.6)	(0.00920)	(0.0126)	(292.5)	(2.999)	(12.56)	(894.9)	(1134.8)
Post X Treatment X Prime-working age (β_2)	-255.8**	-935.0	-0.00244	0.00165	-325.8	-17.03*	-4.344	-746.8	-726.2
	(123.7)	(574.3)	(0.0119)	(0.0159)	(363.2)	(9.626)	(15.63)	(1293.4)	(1642.1)
Effect: Prime-working age ($\beta_1 + \beta_2$)	307.8	129.4	0.00652	0.0105	207.8	-18.17	-10.09	-2215.6	-2689.0
P-val: Prime-working age	0.000	0.754	0.444	0.335	0.339	0.0785	0.280	0.0186	0.0297
Baseline mean (DV)	135.7	5632.1	0.0194	0.0246	2134.2	18.50	142.9	7040.2	14449.8
Observations	29478	29478	29478	29478	29478	29478	29478	29478	29478
Adj. R-Squared	0.0376	0.168	0.0738	0.0399	0.116	0.706	0.659	0.735	0.525

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

Note: The Table reports estimates of β_1 and β_2 from equation (4) for different outcomes by access to informal insurance networks, in Panel A, and by whether the shocks relate to illness of non-prime-age or prime-age family members, in Panel B. Each column reports differences between treatment and placebo households in changes in outcomes before and after the shock, and interactions of these differences with the relevant variable capturing heterogeneity. High Access equals to 1 if the number of transfers or loans given/received to/from other households in the village during the year preceding the shock are above the sample median. *Prime working age* equals to 1 if the shock relates to the illness of a family member of age 18 to 60. Hired and household labor are measured in hours per month. All regressions control for household demographic characteristics, household and village-month fixed effects as well as flexible time-to-treatment trends by access to informal insurance. Standard errors are clustered at the household level.

Table 5: Propagation through village networks

Panel A: Propagation through village networks						
	(1)	(2)	(3)	(4)	(5)	(6)
	Input/Output	Hired labor	All transactions	Income	Incoming gifts	Consumption
Post X closeness (village network)	-0.199***	-0.115**	-0.314***	-1,236.121***	-107.381	-294.199*
	(0.061)	(0.044)	(0.077)	(450.332)	(125.041)	(160.936)
Observations	410,578	410,578	410,578	410,578	410,578	410,578
R-squared	0.440	0.231	0.374	0.197	0.140	0.620
Pre-period Mean	0.999	0.470	1.469	10486	2339	7265
Number of events	391	391	391	391	391	391
Panel B: Propagation through supply-chain and labor-market networks						
	(1)	(2)	(3)	(4)	(5)	(6)
	Input/Output	Hired labor	All transactions	Income	Incoming gifts	Consumption
Post X closeness (supply-chain network)	-0.228***	0.022	-0.206**	-73.483	-251.662	65.746
	(0.065)	(0.040)	(0.081)	(489.309)	(160.727)	(174.830)
Post X closeness (labor-market network)	-0.034	-0.210***	-0.244***	-1,323.731***	177.292	-485.050***
	(0.066)	(0.043)	(0.083)	(447.973)	(138.280)	(159.340)
Observations	410,578	410,578	410,578	410,578	410,578	410,578
R-squared	0.535	0.274	0.483	0.316	0.254	0.819
Pre-period Mean	0.999	0.470	1.469	10486	2339	7265
Number of events	391	391	391	391	391	391

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

Note: The Table presents estimates of β from equation (6). $Closeness_{i,j}$ 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 in Panel A, and through supply-chain and labor-market networks in Panel B. Each regression includes household (i), event j, and month fixed effects, as well as demographic characteristics such as household size, average age, education and number of male and female adults. Standard errors are two-way clustered at the household (i) and event (j) level.

Table 6: Propagation of health shocks and access to informal insurance of shocked households

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Input/Output	Hired labor	All transactions	Income	Incoming gifts	Consumption
Post X Closeness (village network)	-0.260***	-0.107**	-0.367***	-1,654.198***	-40.953	-321.272
	(0.066)	(0.048)	(0.085)	(565.595)	(150.804)	(213.899)
Post X Closeness X Insurance (shocked household)	0.116***	-0.026	0.090	1,031.979***	-90.126	93.541***
	(0.030)	(0.078)	(0.130)	(64.348)	(59.874)	(10.928)
Effect: High Access ($\beta_1 + \beta_2$)	-0.144	-0.133	-0.277	-622.2	-131.1	-227.7
P-val: High Access	0.442	0.233	0.376	0.198	0.141	0.622
Observations	405,073	405,073	405,073	405,073	405,073	405,073
R-squared	0.0498	0.0482	0.0150	0.267	0.371	0.287
Pre-period Mean	0.999	0.470	1.469	10486	2339	7265
Number of events	386	386	386	386	386	386

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

Note: The Table reports estimates of β_1 and β_2 from the pooled difference-in-difference equation (7) for different outcomes by access to informal insurance networks of the shocked household. Access to informal insurance : Number of transfers or loans given/received to/from other households in the village (during the year preceding the shock) above the sample median. Each regression controls for household, event, and month fixed effects as well as time-varying demographic characteristics. The estimating sample excludes households who suffered a direct health shock during any of the 24 months following the shocks to other households in their village. Standard errors are two-way clustered at the household (i) and event (j) level.

A Appendix Figures and Tables

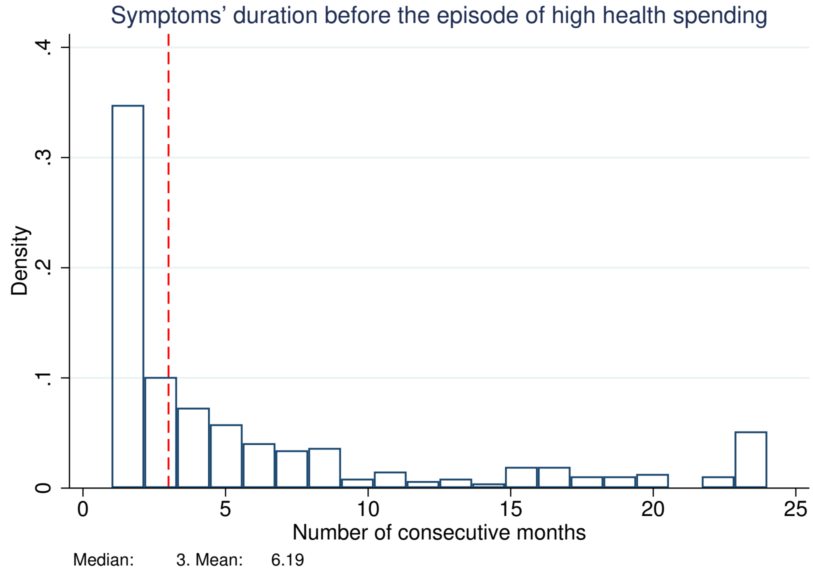


Figure A1: Distribution of symptom duration before the episodes of high health spending

Note: The figure plots the distribution of the number of consecutive months prior to the episodes of high health spending for which at least one household member reported health symptoms. The dashed vertical line denotes the median number of consecutive months reporting symptoms before the episode of high health spending.

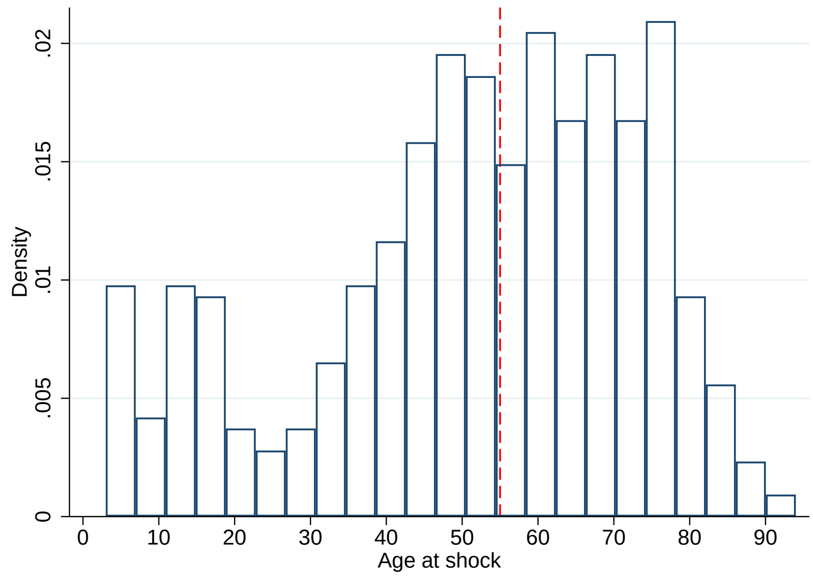


Figure A2: Age at shock

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

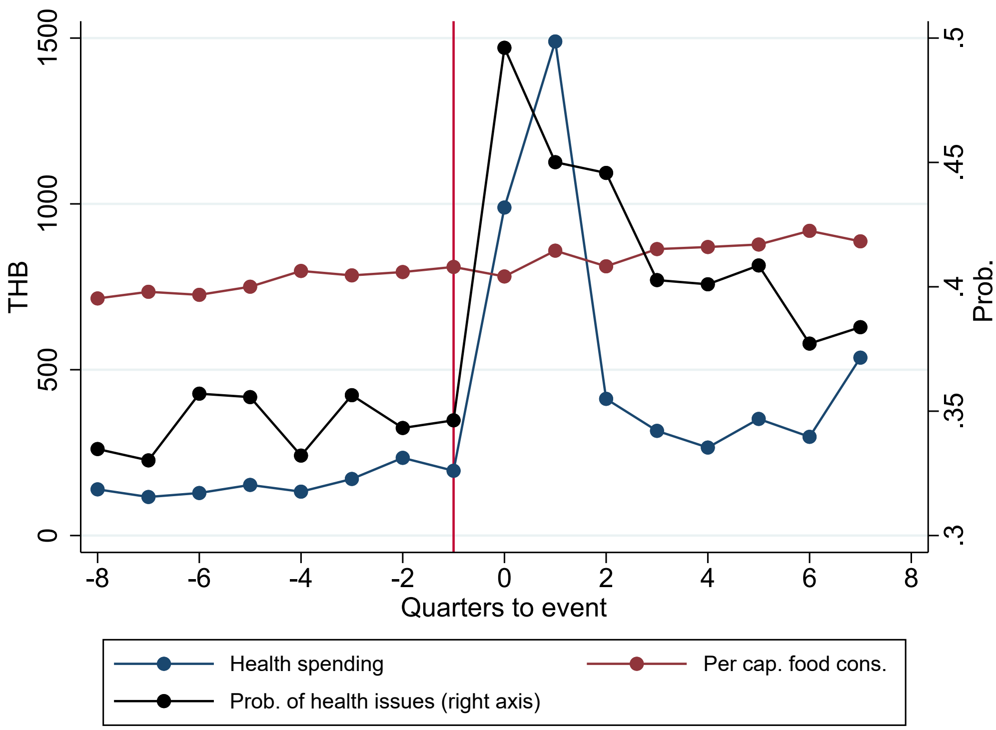


Figure A3: Health status and spending before and after health shocks.

Note: The figure reports averages of health and total spending for periods before and after the health shocks (left axis). The right axis reports the probability that at least one household member reports health symptoms in a given month, before and after the shocks. The horizontal axis represents normalized time with respect to the event realization (time 0). Each time bin corresponds to quarters. All averages are computed over a balanced panel of 505 households.

Table A1: Incidence of health conditions by type of symptom

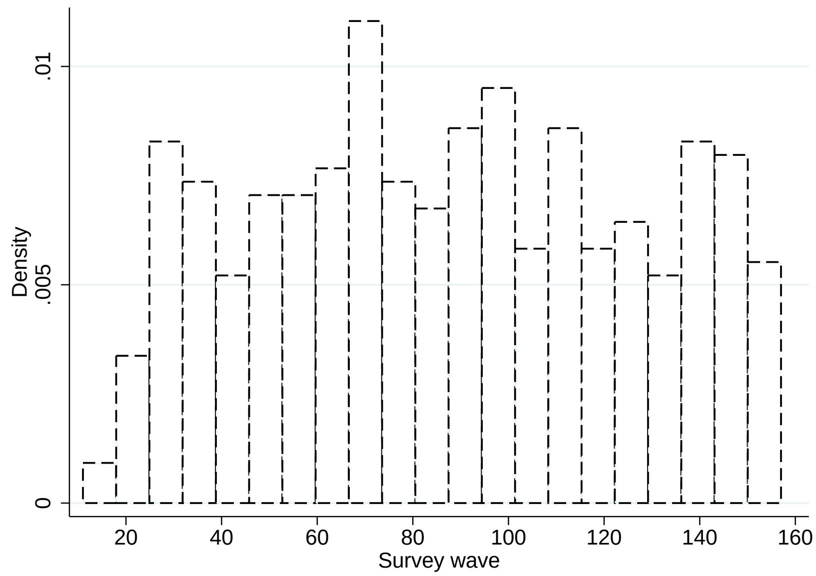
Condition	Shock periods	Non-shock periods	All periods			
			All	Prime working age	Elderly	Children
	(1)	(2)	(3)	(4)	(5)	(6)
Headache/dizziness	9.28	12.03	11.46	15.09	11.96	4.76
Eye sore	1.33	2.05	1.92	1.73	2.09	2.40
Tootache	1.36	1.77	1.72	2.22	0.84	3.00
Cough/cold/influenza	18.35	23.82	22.87	18.67	8.28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

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

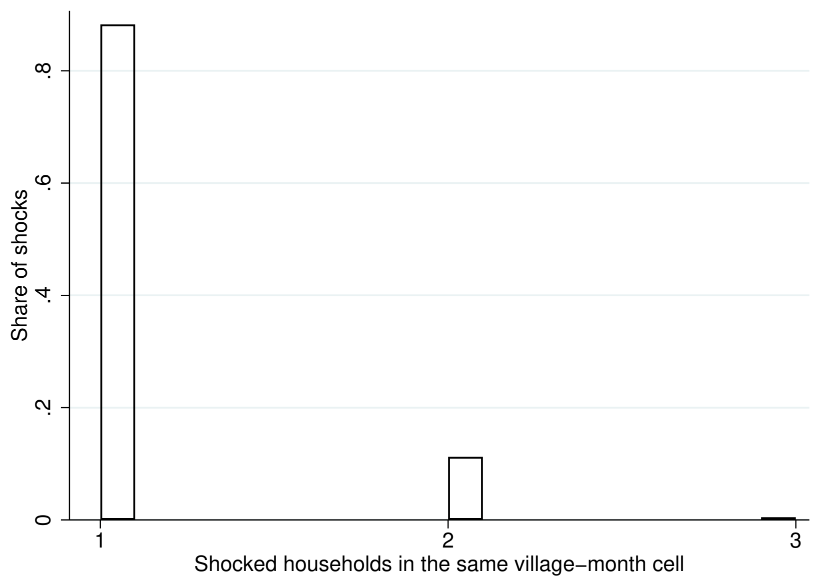
Table A2: Time use in pre-shock periods: Count of days dedicated to different activities

	Number of days per month	More than 15 days
	Average	Share
Cultivation	3.43	0.08
Livestock	6.55	0.21
Fish/Shrimp	1.13	0.02
Off-farm business	1.83	0.07
Housework	22.85	0.78
School or training	2.06	0.05
Positions in village organizations	0.15	0.00
Funerals/Weddings	0.56	0.00
Labor exchange outside home	0.02	0.00
Unpaid labor outside home	0.39	0.01
Paid labor outside home	3.94	0.12
Looking for a job	0.03	0.00
Sick	0.10	0.00

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



(a) Distribution of initial event's period



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

Figure A4: Distribution of events by initial event's periods and number of affected households

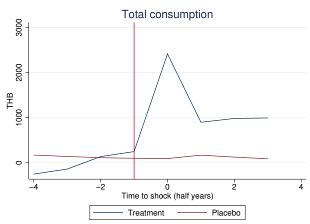
Note: The top panel plots a histogram capturing the distribution of survey months associated the beginning of the health shocks across the full sample period.The bottom panel plots the distribution of events by the number of households simultaneously affected in the same village.

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

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

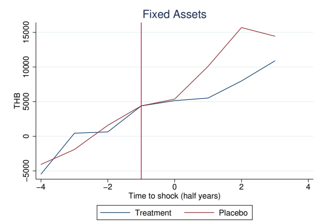
$***p < 0.01, **p < 0.05, *p < 0.1$

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



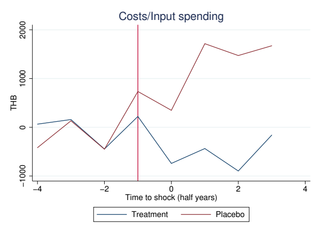
(a) Total consumption

(b) Assets

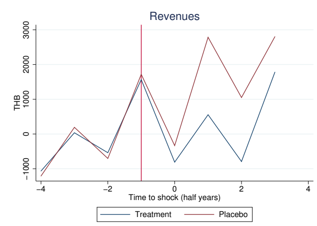


(c) Incoming gifts/transfers

(d) Hired labor (hrs/month)



(e) Input spending



(f) Revenues

Figure A5: Changes in household outcomes before and after the shock

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

Table A4: Robustness to including shocks occurring in the second half of the sample.

Panel A: Effects on Spending

	(1)	(2)	(3)	(4)	(5)
	Health	Total	Total	Non-health Non-Food	Food
Post X Treatment	431.6***	711.3**	279.7	249.4	30.30
	(53.09)	(350.6)	(346.0)	(325.5)	(61.10)
Baseline mean (DV)	147.3	6025.9	5878.5	3217.0	2661.5
Observations	37881	37881	37881	37881	37881
Number of households	471	471	471	471	471
R-Squared	0.0503	0.102	0.0932	0.0524	0.673

Panel B: Effects on household savings and assets

	(1)	(2)	(3)	(4)	(5)
	Savings	Cash in hand	Livestock	Inventories	Fixed Assets
Post X Treatment	-1287.6	-6886.9	-1179.9	-3086.1	-10366.0*
	(1253.1)	(16517.8)	(1982.7)	(5062.9)	(6056.6)
Baseline mean (DV)	6287.7	434066.6	29655.4	123317.6	95742.2
Observations	37881	37881	37881	37881	37881
Number of households	471	471	471	471	471
R-Squared	0.0813	0.849	0.775	0.834	0.759

Panel C: Effects on gifts, transfers and debt

	(1)	(2)	(3)	(4)	(5)
	Gifts from village hhs		Gifts/Transfers	Borrowing	Gifts+Loans
	Prob.	count			
Post X Treatment	0.00873*	0.0131*	479.8***	229.5	752.4**
	(0.00522)	(0.00724)	(171.9)	(266.7)	(347.1)
Baseline mean (DV)	0.0201	0.0255	2247.8	-30.78	2796.6
Observations	37881	37881	37881	37881	37881
Number of households	471	471	471	471	471
R-Squared	0.110	0.0585	0.136	0.0207	0.0382

Panel D: Effects on family businesses

	(1)	(2)	(3)	(4)	(5)
	Costs	Hired labor	HH Labor	Biz. Assets	Revenues
Post X Treatment	-1666.6***	-11.26**	-16.38**	-767.4	-2271.6***
	(607.2)	(5.427)	(7.310)	(1934.4)	(808.3)
Baseline mean (DV)	7447.5	16.80	143.6	33048.1	15110.3
Observations	37881	37880	37880	37881	37881
Number of households	471	471	471	471	471
R-Squared	0.743	0.677	0.645	0.880	0.541

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

Note: The Table reports OLS estimates of β from equation (3) for different outcomes. The estimating sample includes results 471 households in the treatment and control group. Each column reports differences between treatment and placebo households in changes in outcomes before and after the shock. All regressions control for household demographic characteristics, household and month fixed effects. Standard errors are clustered at the household level. Costs, labor, assets and revenues are aggregated across all businesses operated by household members, and exclude revenues and costs of wage labor provision to other businesses or households. Hired labor and labor provided by household members are measured in hours/month.

Table A5: Robustness to alternative definitions of the onset of the shocks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Beginning of event coincides with the observed peak in health spending									
	Health	Total	Prob. Gift (in village)	# of Gifts (in village)	Gifts/Transfers	Costs	Hired labor (Hrs/Month)	HH Labor (Hrs/Month)	Revenues
Post X Treatment	494.9***	741.4**	0.0104*	0.0130	550.3***	-1649.0**	-10.70*	-8.833	-1233.8
	(82.52)	(326.2)	(0.00612)	(0.00870)	(208.2)	(838.9)	(5.637)	(9.079)	(1102.4)
Baseline mean (DV)	147.9	5397.2	0.0201	0.0265	1977.3	7561.6	19.01	155.5	14643.0
Observations	22643	22643	22643	22643	22643	22643	22642	22642	22643
Number of households	451	451	451	451	451	451	451	451	451
R-Squared	0.0810	0.196	0.163	0.0929	0.175	0.784	0.570	0.718	0.629
Panel B: Beginning of event starts 6 months before the observed peak in health spending									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Health	Total	Prob. Gift (in village)	# of Gifts (in village)	Gifts/Transfers	Costs	Hired labor (Hrs/Month)	HH Labor (Hrs/Month)	Revenues
Post X Treatment	280.8***	9.094	0.00802	0.0104	470.1**	-1762.6**	-15.23*	-11.15	-1637.2
	(89.16)	(404.9)	(0.00664)	(0.00972)	(211.3)	(863.6)	(8.310)	(8.506)	(1088.1)
Baseline mean (DV)	214.5	5525.7	0.0178	0.0236	2030.9	7618.8	17.17	151.6	14977.5
Observations	22742	22742	22742	22742	22742	22742	22741	22741	22742
Number of households	451	451	451	451	451	451	451	451	451
R-Squared	0.0762	0.182	0.173	0.101	0.182	0.790	0.596	0.725	0.631

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

Note: The Table reports OLS estimates of β from equation (3) for different outcomes. Each column reports differences between treatment and placebo households in changes in outcomes before and after the shock. All regressions include a vector of demographic characteristics as well as household and month fixed effects. Standard errors are clustered at the household level.

Table A6: Robustness to alternative placebo groups and specifications

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Randomly assigned placebo shocks									
	Spending		Gifts (in village)		Gifts (total)	Production			
	Health	Total	Prob.	Count		Costs	Hired Labor	HH Labor	Revenues
Post X Treatment	450.3***	712.5**	0.00811*	0.00534	271.7*	-975.3**	-6.471	-14.57**	-1671.2***
	(64.81)	(324.2)	(0.00447)	(0.00705)	(147.7)	(474.3)	(5.619)	(6.006)	(633.7)
Baseline mean (DV)	198.1	6186.2	0.0226	0.0275	2375.8	7707.3	17.13	142.1	15193.4
Observations	43194	43194	43194	43194	43194	43194	43193	43193	43194
Number of households	472	472	472	472	472	472	472	472	472
R-Squared	0.0620	0.119	0.154	0.0698	0.156	0.779	0.735	0.699	0.572
Panel B: Panel regression with household and month fixed effects (Gertler and Gruber, 2002)									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Spending		Gifts (in village)		Gifts (total)	Production			
	Health	Total	Prob.	Count		Costs	Hired Labor	HH Labor	Revenues
Post	522.6***	94.54	-0.00506*	-0.00369	235.8*	-472.2*	-1.936	-5.541*	-986.0**
	(88.57)	(334.3)	(0.00288)	(0.00480)	(130.7)	(246.8)	(1.349)	(3.212)	(430.4)
Baseline mean (DV)	161.9	6306.1	0.0198	0.0257	2387.3	7938.9	18.92	142.9	15869.2
Observations	21362	21362	21362	21362	21362	21362	21361	21361	21362
Number of households	469	469	469	469	469	469	469	469	469
R-Squared	0.0248	0.0764	0.158	0.0836	0.184	0.781	0.764	0.762	0.582

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

Note: The table reports difference-in-difference estimates corresponding to equation (3). Panel A report estimates in which the placebo shocks are allocated randomly. Panel B reports estimates excluding households who suffered the shock in the second half of the survey and their respective placebo group. Standard errors are clustered at the household level. All regressions include a vector of demographic characteristics as well as household and village-month fixed effects.

Table A7: Heterogeneity in the effects of health shocks

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Health Spending	Total spending	Prob. Gift (in village)	# of Gifts (in village)	Gifts	Hired labor	HH Labor	Costs	Revenues
Post X Treatment (β_1)	458.3***	569.7	0.0135*	0.0120	523.4*	-14.12	-15.44	-2703.7**	-3712.4**
	(98.17)	(553.0)	(0.00706)	(0.00885)	(271.1)	(11.38)	(12.45)	(1261.5)	(1603.3)
Post X Treatment X High Access (β_2)	172.7	-65.72	-0.00740	0.00237	35.81	-1.011	12.38	1828.8	4037.4*
	(180.2)	(815.9)	(0.0130)	(0.0195)	(452.2)	(18.01)	(19.40)	(1709.7)	(2075.9)
Effect: High Access ($\beta_1 + \beta_2$)	631.0	504.0	0.00612	0.0143	559.2	-15.13	-3.060	-874.9	325.0
P-val: High Access	0.0000616	0.358	0.577	0.404	0.121	0.217	0.827	0.451	0.816
Baseline mean (DV)	154.7	5476.4	0.0193	0.0252	1952.0	18.25	154.6	7696.3	15039.3
Observations	22289	22289	22289	22289	22289	22289	22289	22289	22289
Adj. R-Squared	0.0495	0.154	0.141	0.0702	0.163	0.578	0.716	0.783	0.624

Panel B: Effects of the shocks by age of ill household member (prime age 18-60)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Health Spending	Total spending	Prob. Gift (in village)	# of Gifts (in village)	Gifts	Hired labor	HH Labor	Costs	Revenues
Post X Treatment (β_1)	698.3***	1209.4*	0.00562	0.00491	584.4	3.194	-6.243	-1714.7*	-2869.6**
	(195.4)	(618.9)	(0.0112)	(0.0181)	(379.3)	(2.699)	(14.53)	(901.8)	(1292.8)
Post X Treatment X Prime-working age (β_2)	-344.8*	-1081.4	0.00924	0.00979	-286.3	-31.88*	10.46	-674.4	1578.2
	(200.3)	(772.2)	(0.0149)	(0.0213)	(450.1)	(17.48)	(19.28)	(1729.6)	(2229.9)
Effect: Prime-working age ($\beta_1 + \beta_2$)	353.5	128.0	0.0149	0.0147	298.1	-28.69	4.222	-2389.1	-1291.4
P-val: Prime-working age	0.00	0.78	0.15	0.24	0.22	0.09	0.74	0.13	0.51
Baseline mean (DV)	138.2	5064.4	0.0171	0.0223	1838.1	19.34	157.6	6891.0	14090.5
Observations	17579	17579	17579	17579	17579	17579	17579	17579	17579
Adj. R-Squared	0.0336	0.155	0.0747	0.0380	0.118	0.603	0.722	0.746	0.587

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

Note: The Table reports estimates of β_1 and β_2 from equation (4) for different outcomes by access to informal insurance networks, in Panel A, and by whether the shocks relate to illness of non-prime-age or prime-age family members, in Panel B. Each column reports differences between treatment and placebo households in changes in outcomes before and after the shock, and interactions of these differences with the relevant variable capturing heterogeneity. High Access equals to 1 if the number of transfers or loans given/received to/from other households in the village during the year preceding the shock are above the sample median. *Prime working age* equals to 1 if the shock relates to the illness of a family member of age 18 to 60. Hired and household labor are measured in hours per month. All regressions control for household demographic characteristics, household and village-month fixed effects as well as flexible time-to-treatment trends by access to informal insurance. Standard errors are clustered at the household level.

Table A8: Propagation through village networks (village X month FE)

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Propagation through village networks						
VARIABLES	Input/Output Sales	Hired labor provision	All transactions	Income	Incomig gifts	Consumption
Post X closeness (village network)	-0.184***	-0.083***	-0.268***	-811.808*	-167.341	-65.344
	(0.045)	(0.027)	(0.055)	(456.340)	(123.281)	(151.918)
Observations	410,578	410,578	410,578	410,578	410,578	410,578
R-squared	0.499	0.319	0.448	0.253	0.187	0.636
Pre-period Mean	0.999	0.470	1.469	10486	2339	7265
Number of events	391	391	391	391	391	391
Panel B: Propagation through supply-chain and labor-market networks						
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Input/Output Sales	Hired labor provision	All transactions	Income	Incomig gifts	Consumption
Post X closeness (supply-chain network)	-0.190***	-0.014	-0.204***	-273.328	-278.366*	-19.238
	(0.049)	(0.026)	(0.057)	(418.882)	(151.786)	(134.974)
Post X closeness (labor-market network)	-0.051	-0.144***	-0.195***	-585.115*	80.119	-29.148
	(0.048)	(0.026)	(0.059)	(342.531)	(113.183)	(126.717)
Observations	410,578	410,578	410,578	410,578	410,578	410,578
R-squared	0.584	0.357	0.544	0.363	0.295	0.826
Pre-period Mean	0.999	0.470	1.469	10486	2339	7265
Number of events	391	391	391	391	391	391

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

Note: The Table presents estimates of β from equation (6). $Closeness_{i,j}$ 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 in Panel A, and through supply-chain and labor-market networks in Panel B. Each regression includes household (i), event j, and village-month fixed effects, as well as demographic characteristics such as household size, average age, education and number of male and female adults. Standard errors are two-way clustered at the household (i) and event (j) level.

Table A9: Propagation effects on downstream and upstream transactions

Panel A: Effects on downstream and upstream transactions												
	Outgoing transactions (Upstream)						**Incoming transactions (Downstream)**					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES	Input/output sales		Labor provision		Outgoing transactions		Input/output purchases		Labor hiring		Incoming transactions	
Post X closeness (village network)	-0.078*	-0.084**	-0.087***	-0.048***	-0.165***	-0.132***	-0.121***	-0.100***	-0.028	-0.036*	-0.150***	-0.136***
	(0.044)	(0.034)	(0.025)	(0.015)	(0.050)	(0.038)	(0.032)	(0.024)	(0.027)	(0.018)	(0.044)	(0.032)
Observations	410,578	410,578	410,578	410,578	410,578	410,578	410,578	410,578	410,578	410,578	410,578	410,578
R-squared	0.574	0.597	0.181	0.271	0.496	0.534	0.369	0.424	0.243	0.285	0.341	0.390
Pre-period Mean	0.497	0.497	0.182	0.182	0.679	0.679	0.501	0.501	0.288	0.288	0.790	0.790
Number of events	391	391	391	391	391	391	391	391	391	391	391	391
Panel B: Effects on downstream and upstream transactions by exposure in the supply-chain and labor-market network												
	Outgoing transactions (Upstream)						**Incoming transactions (Downstream)**					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES	Input/output sales		Labor provision		Outgoing transactions		Input/output purchases		Labor hiring		Incoming transactions	
Post X closeness (supply-chain network)	-0.086*	-0.072*	-0.017	-0.016	-0.103**	-0.088**	-0.142***	-0.118***	0.039	0.002	-0.104*	-0.116***
	(0.047)	(0.040)	(0.016)	(0.011)	(0.048)	(0.040)	(0.040)	(0.030)	(0.031)	(0.021)	(0.054)	(0.038)
Post X closeness (labor-market network)	0.002	-0.016	-0.109***	-0.059***	-0.107*	-0.075*	-0.036	-0.035	-0.101***	-0.085***	-0.136***	-0.119***
	(0.054)	(0.041)	(0.027)	(0.016)	(0.061)	(0.045)	(0.031)	(0.026)	(0.027)	(0.018)	(0.041)	(0.031)
Observations	410,578	410,578	410,578	410,578	410,578	410,578	410,578	410,578	410,578	410,578	410,578	410,578
R-squared	0.575	0.597	0.182	0.271	0.496	0.535	0.369	0.424	0.243	0.286	0.341	0.390
Pre-period Mean	0.497	0.497	0.182	0.182	0.679	0.679	0.501	0.501	0.288	0.288	0.790	0.790
Number of events	391	391	391	391	391	391	391	391	391	391	391	391
Village-Month FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

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

Note: The Table presents estimates of β from equation (6). $Closeness_{i,j}$ 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 in Panel A, and through supply-chain and labor-market networks in Panel B. 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 A10: Indirect effects of health shocks on gift/transfers to other households

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

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

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 (6). $Closeness_{i,j}$ 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 A11: Effects of indirect shocks on Savings and Assets

	(1)	(2)	(3)	(4)	(5)
VARIABLES	Savings	Cash in Hand	Livestock	Inventories	Fixed assets
Post X Closeness	417.250	-18,216.659	-2,201.766	-11,883.668**	-3,579.940
	(990.391)	(19,842.938)	(2,081.598)	(4,958.028)	(4,087.105)
Observations	410,578	410,578	410,578	410,578	410,578
R-squared	0.070	0.818	0.802	0.864	0.777
Pre-period Mean	5612	400531	29204	125374	92866
Number of events	391	391	391	391	391

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

Note: The Table presents estimates of β from equation (6). $Closeness_{i,j}$ 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, 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.