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Insurance and Propagation in Village Networks

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Insurance and Propagation in Village Networks *

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Abstract

The intra-household linkages that comprise networks in village economies serve the important role of providing insurance. However, the flip side of these linkages is that they also propagate shocks. We show that when one household experiences a significant health shock, it propagates to other linked households via the village network. Because the shocked household is imperfectly insured, it adjusts production decisions–drawing down working capital, cutting input spending, and reducing labor hiring–hence affecting households who supply inputs and labor to them. We find that upstream businesses close to shocked households in the supply chain network experience reduced local sales and increased inventories. Likewise, workers closer to the shocked households in the labor network have lower probability of working locally and reduced earnings. Networks appear to be rigid, at least in the short run; linked households are unable to form new linkages when existing links experience negative shocks. A simple back of the envelope exercise suggests that the total indirect costs may be as large or larger than the direct costs. These results suggest that social (village-level) gains from expanding health insurance may be substantially higher than private (household-level) gains.

Keywords: Entrepreneurship, Risk sharing, Propagation, Production networks

JEL Classification: D13, D22, I15, Q12

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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 consumption goods, these households buy and sell labor and other productive inputs with each other (Braverman and Stiglitz, 1982). They are also engaged in borrowing and lending, as well as providing and receiving gifts (Udry 1994; Townsend 1994; Samphantharak and Townsend 2018). Effectively, these interactions between households constitute local financial, supply chain, and labor networks in village economies. This paper studies the dual role of these local networks in providing insurance and propagating shocks.

In principle, with complete markets, idiosyncratic shocks are fully insured. In such an environment, production decisions (e.g., investment in fixed capital, purchase of raw material, and hiring of workers) would be independent of consumption decisions (e.g., spending and supplying labor). Shocks would not affect production and hence no propagation to suppliers and workers through production networks would take place. However, a large body of evidence has demonstrated that labor, credit, and insurance markets are in fact not complete (e.g., Hayashi et al. 1996; Benjamin 1992; LaFave and Thomas 2016; Dillon and Barrett 2017). In the absence of complete markets, a new set of shocks is created in the economy that would not be present if there had been full insurance. In such a situation, households may need to diversify their production activities (Banerjee and Duflo, 2007). This is costly as it requires forgoing gains from specialization and economies of scale (e.g., workers may need to be specifically trained for each employer; lumpy assets may be un- or underutilized); there is a welfare loss as a result. In addition, the impact from propagation through supply chain or labor networks could be irreversible as suppliers and customers may switch to other partners or other activities permanently. These costly adjustments could have been avoided had there been full insurance.

This paper leverages a unique dataset to distinguish these network effects: positive (insurance) and negative (propagation). The Townsend Thai data, constructed from 14 years of monthly panel surveys, allow us to identify idiosyncratic shocks to households' budgets and, with detailed information regarding transactions across family-operated businesses, to construct local networks.

Of course, shocks to household endowments are not typically exogenous, which makes it challenging to empirically identify their causal effects. To overcome this, we exploit variation in the timing of episodes of sudden increases in health spending to construct idiosyncratic shocks to household expenses, which generate pressure on the budget. We show that, conditional on ever experiencing health spending shocks, their *timing* is exogenous and that, moreover, this timing is uncorrelated across households. We also show that the shocks are severe—they are twice as large as household average per capita food consumption and coincide with sharp increases in inpatient care. We also show that the shocks are mostly related to the illness of elderly individuals or children, as opposed to prime aged adults engaged in household production. Thus, they are primarily capturing a shock to financial needs, as opposed to an illness-induced change in labor supply. We distinguish these events in the analysis to unravel different responses depending on the nature of the shock.

We first analyze the effect of these idiosyncratic shocks on household consumption, production, and financing decisions. To account for cross-household differences in household and business characteristics, we follow Fadlon and Nielsen (2019), and construct counterfactuals for affected households using households that experience the same shock, but not contemporaneously.

Our first finding is that the shocks are smoothed on the consumption side. We find neither significant nor substantial changes in food consumption in the aftermath of the shocks. 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 24% 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 networks of gifts and loans (which we call "financial networks") provide insurance, this informal insurance is partial: incoming transfers cover approximately two-thirds of the spending needs of shocked households. As a result, in order to fully smooth food consumption, shocked entrepreneurs draw down their working capital to finance the shocks. Indeed, we find that shocked households substantially reduce input spending (24% decrease), and almost entirely cut their demand for external labor (78% decrease). They also reduce the work hours of (non-sick) family workers allocated to family businesses (12% decrease). This overall decrease in productive activities leads not only to a reduction in input expenses but to an average 10% reduction in revenues, relative to the pre-period, due to scaling back. Thus, shocks to households' consumption needs affect production-side decisions, in violation of the separation theorem (Benjamin, 1992).

Yet, the results are more subtle and revealing. First, in the case of shocks to households with limited participation in financial networks (gifts, loans) during the year preceding the shocks—i.e., households that transacted with few households in the village (below the median), input spending and revenues decreased by 30% and 19%, respectively. In contrast, these decreases were fully attenuated in the case of higher-participation households. Second, we find that 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. In other words, financial gifts cannot mitigate the impact as such household needs to cease production activities regardless of the receipt of gifts. That is, relative to shocks affecting elderly or children (i.e., mostly financial shocks), shocks related to the illness of prime-age household members (i.e., labor endowment shocks) are related to lower inflows of gifts from other households and larger declines in production. In a sense, there is not a market for individual specific labor input into household production, so naturally the separation hypothesis fails.

We next turn to studying further the impact of these shocks on other local businesses and workers. Our empirical strategy relies on variation in the proximity of a given household to the shocked household, through the 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 shocked household in the pre-period network) and less-exposed households (i.e., those that are further away from shocked household in the pre-period network). As the health shocks originally reduced demand for both inputs and labor, we analyze the transmission of shocks through two types of connections: the local supply chain networks (i.e., networks of households selling and purchasing raw material or intermediate goods) and labor networks (i.e., networks of households providing and hiring labor).

The shocks, despite being idiosyncratic in origin, nonetheless propagate to other households through local economic networks. Businesses closer to shocked households in the supply chain networks experience reduced sales and consequently increase inventories due to their (indirect) exposure to the shock. These increased inventories are costly as households incur additional cost of financing and storage. Similarly, workers closer to shocked households in the labor network experience a fall in the probability of working for local employers, and reduction in total hours allocated to wage labor. As a result, total household labor earnings decline.

As expected, the magnitude of the indirect effect experienced by a particular 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 magnitude of the indirect effects of a given shock may be nearly as large, or larger, than the direct effects of that shock. These findings suggest that the social gains of participating in insurance networks—or of providing publicly-funded insurance—are larger than the private gains.

One explanation of these results is the existence of frictions in the markets for goods and labor. For instance, suppliers may not be able to find new customers when their clients suffer a shock. Likewise, workers may struggle finding new jobs when their employers face health shocks.[1] Indeed, we show that supply chain, labor, and financial links are quite persistent: those pairs of households that transacted in the baseline are substantially more likely to transact 10 years later, relative to households that did not transact in the baseline. Importantly, baseline kinship relationships are strong predictors of trade, highlighting the importance of contract-enforcement barriers to trade across households (Ahlin and Townsend, 2007; Johnson et al., 2002).

Lastly, we do not find significant indirect effects on consumption upstream—i.e.,households who provided inputs or labor to (directly) shocked households.[2] Thus, households were able to smooth out the indirect effects of exposure to local shocks. How are households buffering these shocks? We find evidence of ex-post adjustments to mitigate lost sales and labor earnings. Indirectly affected households shifted away resources from activities that tend to be more vulnerable to shocks to other households—retail businesses and off-household labor—towards farm-related businesses, which tend to sell most of their output outside the village.[3] In contrast, households do not appear to buffer these indirect shocks by receiving or loans. One explanation is that as shocks propagate, the effectiveness of intra-village insurance reduces. Idiosyncratic shocks become more like aggregate shocks. Indeed, by comparing the effects of health shocks on gift receipt to those of a sectoral shock affecting shrimp farmers in our sample (Giannone and Banternghansa, 2018), we find that gift receipt is substantially higher in the case of idiosyncratic shocks. Another explanation might be moral hazard. The more a given household is connected to downstream insured households, the less the incentive to be diligent and join at some cost into risk sharing networks. Such a household does not bear all the costs of its underinsurance. To mitigate this moral hazard, downstream households have less insurance.

This paper makes several contributions. First, previous studies have provided evidence of non-

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

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

[3] For example, rice farmers tend to sell rice to cooperatives, sometimes at government regulated prices, and shrimp farmers tend to sell their products to international markets.

separability of household consumption (labor supply) and production (input or labor demand) decisions in developing countries.[4] We build on this literature by showing that idiosyncratic shocks to household spending can affect production decisions of shocked households but also 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, this paper complements the empirical literature studying the firm-to-firm propagation of regional or sectoral shocks through production networks.[5] We leverage on the context of family-owned firms to show that granular shocks to family expenditure can propagate to other firms. This distinction —sectoral 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. Moreover, recent macroeconomic models highlight the importance of both granular shocks and propagation in explaining aggregate fluctuations (Gabaix, 2011; Acemoglu et al., 2012).

This paper also contributes to the literature studying the role of local economic networks in developing countries (Bramoullé et al., 2016; Chuang and Schechter, 2015; Munshi, 2014). In particular, previous studies have analyzed the ability of households to use local networks to buffer shocks (Townsend, 1994; Kinnan and Townsend, 2012; Angelucci and De Giorgi, 2009).[6] We contribute to previous 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. Such findings provide novel implications regarding the importance of policies to expand health insurance in developing countries. Previous 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). One novel implication of our results is that expanding insurance may lead to even larger social (village-level) welfare gains. Moreover, our results suggest that, from a methodological perspective, local spillovers should be taken into account when analyzing the incidence of both consumption- and production-side shocks.

[4] 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.

[5] 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., 2016), trade shocks (Tintelnot et al., 2018; Huneeus, 2019), and sectoral or regional shocks (Caliendo et al., 2017).

[6] Other studies have documented the crucial role of local networks in the adoption of technologies (Beaman et al., 2018; Banerjee et al., 2013), 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).

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. Sections 4 and 5 analyze the direct and indirect effects of the shocks on business performance and labor earning. Sections 6 and 7 analyze potential explanations for propagation and the strategies to cope with indirect shocks, respectively. Sections 8.1 compares the responses to idiosyncratic shocks versus sectoral shocks. Finally, Section 9 concludes.

2 Data and Context

2.1 Household data

The data 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.[7] The baseline interview was conducted in July to August 1998, collecting information on demography 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.[8] The sample in this paper covers the period between September 1998 and December 2012. We focus our analysis on the subset of 509 households that responded to the interview throughout all survey waves.

Table 1 characterizes the sample households in terms of their demographic, financial and business characteristics. It shows that 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,[9] and 30% from personal lenders, including relatives.

[7]There is one exception. One sampled village in Srisaket has the total population less than 45 and all households are included in the survey.

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

[9]There are different types of financial institutions operating in these markets. The most prominent institution is the

Table 1: Summary statistics

Panel A: Household baseline characteristics	**N**	**Mean**	**S.D.**	**10th %ile**	**90th%ile**
Number of household members	509	4.54	1.87	2	7
Number of adults	509	2.87	1.38	1	5
Household head age	507	51.95	13.45	35	70
Average age	509	34.14	12.11	21	52
Household head is a male	507	0.77	0.42	0	1
Years of schooling: Household head	504	4.49	2.59	3	7
Years of schooling: Household maximum achievement	509	8.19	3.64	4	14
Years of schooling: Household average	509	5.09	2.17	3	8
Panel B: Household finance (annual data)	**N**	**Mean**	**S.D.**	**10th %ile**	**90th%ile**
Net Income in THB:					
Farm	7635	134389	1378506	-150	316500
Off-farm family business	7635	19095	115540	0	40700
Labor	7635	52816	108492	0	152222
Total from operations (farm+off-farm + labor)	7635	173327	618277	4974.07	410723
Net 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

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.

2.2 Economic networks data

The Townsend Thai Monthly Survey contains detailed information on transactions between households and captures different types of economic inter-linkages. During each survey wave, interviewees identify any households in the village with whom they have conducted a given type of transactions.[10] This information allows us 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 inputs and intermediate goods across business of households in the same village. Third, we also recovered labor networks which capture employer-employee relationships across households in the same village. Finally, as the baseline survey asks each interviewee to lists all their first-degree relatives living in the village, we are able to elicit time-invariant baseline kinship networks.

Figure 1 depicts different networks for a sample village. It shows that there is a lot of variation in the number of households interacting in different markets. For instance, the financial network is rather thin and involves only a reduced number of households (nodes). In contrast, there is a great degree of interconnection across households in the case of the supply chain and labor networks.

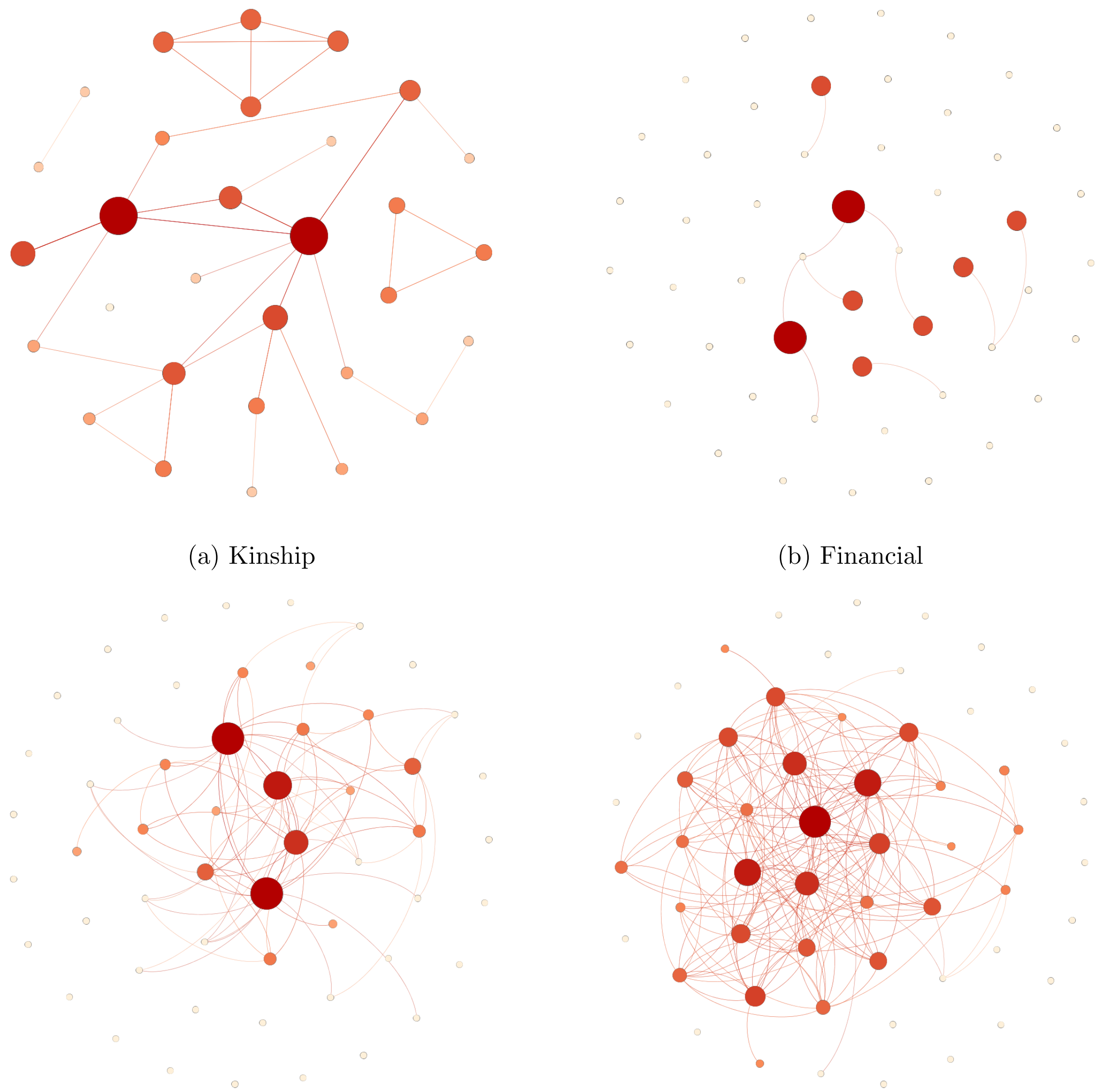
Table 2 shows that, on average, 35% of the households in the sample participate in the local financial networks on a given year—i.e., give or receive any gift or loan from other households in the village. In contrast, 48% of the households transact in the local village markets for inputs and final output, and 62% provide or purchase labor from/to other households in the village. There are some important differences by sectors. Farm-oriented households, those who obtain at least 50% of their income from farm-related activities,[11] tend to participate relatively more in the local labor markets than households that obtain most of their income from off-farm businesses. In contrast, they tend to transact less in the local market for inputs and final output. One explanation is that agricultural output is more likely to be exported to other areas in Thailand or abroad,[12] while off-farm businesses, typically retail, obtain revenues from intra-village sales.

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.

[10]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.

[11]These activities include cultivation of a variety of crops, livestock, fishing, and shrimp

[12]For instance, shrimp is one of Thailand's main exports; rice is also typically sold to cooperatives.



(a) Kinship

(b) Financial

(c) Sales

(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 between businesses operated by households in the same village. Labor networks include relationships through paid and unpaid labor between households in the same village.

Table 2: Summary statistics: Economic networks

	All		Farm		Non farm	
	Mean	**S.D.**	**Mean**	**S.D.**	**Mean**	**S.D.**
Baseline kinship networks: Degree (Number of links)	2.36	2.19	2.57	2.16	2.41	2.35
Baseline kinship networks: Access (any link)	0.77	0.42	0.83	0.38	0.70	0.46
Financial networks: Degree	0.65	1.36	0.65	1.19	0.99	2.18
Financial networks: Access	0.35	0.48	0.37	0.48	0.43	0.50
Sales networks: Degree	1.26	2.64	0.99	1.37	4.76	5.79
Sales networks: Access	0.48	0.50	0.56	0.50	0.74	0.44
Labor-market network: Degree	3.07	4.42	4.55	5.18	2.72	4.24
Labor-market network: Access	0.62	0.49	0.77	0.42	0.59	0.49

Note: The table reports degree centrality–number of links– and access to different type of networks. All networks are unvalued and undirected. 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.

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. Episodes of severe health issues are among the largest idiosyncratic shocks that may affect household finance and labor supply (Gertler and Gruber, 2002; Genoni, 2012). In this paper, we rely on idiosyncratic events associated with high levels of health spending to identify episodes of high financial stress. Because these shocks are uncorrelated across households, we are able to separate these idiosyncratic shocks from aggregate shocks that could affect economic activity through changes in the markets for final goods, intermediate inputs, and labor. Focusing on idiosyncratic shocks is important for two reasons. First, it allows us to analyze shocks that are insurable through local networks, and to understand whether individual responses to such shocks vary with access to local insurance networks. Second, by ruling out immediate general equilibrium effects, we can test whether these shocks propagate through local economic 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 then identify survey wave registering the highest amount of monthly health spending throughout the panel. We focus on the largest shocks as we want to restrict the analysis to shocks that pose a financial burden to the household. Because we would like to make comparisons of the responses to these shocks across households before and after the episodes, we restrict the search to periods between years 2-12 in the panel (out of 14 years of monthly data). This enables us to observe at least 2 years of pre- and post-shock behavior for all households. Following this approach we identified 505 episodes of non-zero sudden increases in monthly health spending, one per household.[13]

It is possible that events of high medical spending are actually planned. For instance, households may save or borrow for a surgery before it takes place, when a family member starts experiencing symptoms. For some households, our dataset allows us to identify the health symptoms affecting household members, and whether these symptoms were also reported in periods preceding the increases in health spending. Appendix Figure A1 shows that, prior to the sudden increase, the median number of consecutive months in which households report any health symptoms is 3 months. Thus, we code the beginning of each event three months before the observed spike on total health spending, in order to account for potential anticipation effects.[14]

3.1 Characteristics of the shocks.

Relationship between health spending and health status. Figure 2 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 both health spending and the self-reported

[13]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. An alternative way of identifying shocks would be to identify households who report not having been able to work due to illness. Ongoing research by Hendren et al. (2018) follows this approach using the same dataset. However, our approach differs in two ways. First, we are interested on 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. Second, there are several households with members having permanent conditions, and thus it is not possible to determine when there is a shock by only looking at symptoms data.

[14]For a sub-sample of 434 households reporting a health symptom at the time of the spending spike, we compute the number of consecutive times that the household reported any symptom during a two-month window preceding the spike in health spending. On average, households reported having symptoms 6 months before the spike. However, the mean is influenced by the subset of households who seem to have permanent health conditions. One fifth of the events relate to household members that reported symptoms for at least one year before the spikes in health spending.

symptoms vary simultaneously, confirming that the events are correlated with decreases in household health endowments. Appendix Table A2 reports the distribution of types of health symptoms reported by shocked households during the two years around the shock, during off-shock periods, and during all the sample periods. Relative to the off-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. In addition, note that the probability of reporting symptoms as well as health spending start increasing a quarter before the spike. This pattern supports our assumption that an event starts a quarter before the observed spike.

Magnitude of the shock. Figure 2 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.

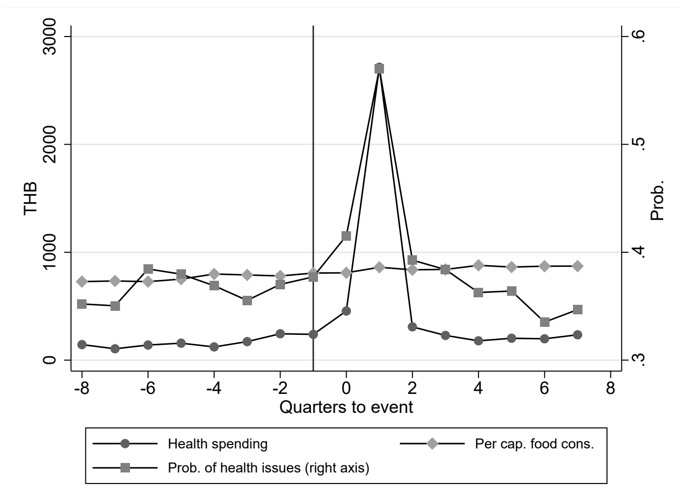


Figure 2: Health status and spending before and after health shocks.

Note: The figure reports averages of health and total spending for periods before and after the health shocks (left axis). The right axis reports 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. All averages are computed over a balanced panel of 505 households.

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, a large share of shocks are related to illness of non-prime age household members. Indeed, Appendix Table A1 shows that, on average, affected individuals spent most of their days helping with households chores rather than working for their family businesses.[15] Moreover, the distribution of type of symptoms around the shock matches more closely that of older individuals (see Appendix Table A2). Thus, for this subsample of shocks affecting non-prime age household members, we interpret the shocks as financial shocks. However, there is a great degree of variation in the age of the household member to which the shock is related. In contrast, around 40% of the events relate to household members in prime-working age, and we interpret these subset of shocks as shocks to labor endowments. Our analysis will exploit this variation to distinguish the effects of different types of shocks.

Are the shocks idiosyncratic? Our analysis requires that the events are idiosyncratic and their occurrence is uncorrelated with trends in household behavior. The top panel of Appendix Figure A3 presents the distribution of the months associated to 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.

To formally test whether village-level trends explain the occurrence of these events, we regress first differences in the probability of experiencing a shock on a given month on its first lag and village-month fixed effects according to the following specification:

$$P_{i,v,t} - P_{i,v,t-1} = \rho(P_{i,v,t-1} - P_{i,v,t-2}) + (\mathbf{X}_{i,v,t-1} - \mathbf{X}_{i,v,t-2})\Sigma + \theta_{v,t} + \epsilon_{i,v,t}$$

where $P_{i,v,t}$ denotes the probability that household i, in village v, suffers the shock at time t, and θ_v represents village-year fixed effects. To test whether the shocks were correlated with trends in household-finance variables, we also include a vector of lagged changes in business income, household debt, consumption, assets, and inflows and outflows of cash ($\mathbf{X}_{iv,t-1} - \mathbf{X}_{iv,t-2}$).

The bottom panel of Appendix Table A3 shows that, conditional on lagged event occurrence, village-specific trends do not significantly capture relevant variation in the probability of experiencing a shock. For instance, the R^2 of the model that only includes month fixed effects (Column (1)) is almost identical to the R^2 of the model including village-month fixed effects (Column (2)). Column (3) from Appendix Table A3 shows that there is no evidence of statistical correlation between household time-varying variables and the probability of experiencing a shock.

[15] For instance, they reported performing housework activities in 23 days during the month preceding the shock, while only 12 days in livestock activities, 7 days in cultivation, and 2 days in the provision of paid labor outside the home. These activities are not mutually exclusive, so the total days add up to more than 30.

4 Direct effects of idiosyncratic shocks

4.1 Identification strategy

Estimating the effects of idiosyncratic shocks on household outcomes requires a valid comparison group. In particular, we would like to compare changes in outcomes before and after the shock between shocked households and otherwise-similar households who were not simultaneously exposed to such shock. Such an approach relies on the assumption that the trends in outcomes between shocked and comparison households are parallel. However, this assumption could be violated as household-finance variables may have different trends depending on the stage of the household life cycle (Fadlon and Nielsen, 2019). For instance, Silva et al. (2019) show that risk-taking behavior of entrepreneurs varies substantially along the life cycle. Such differences may end up generating different trends in household finance decisions.

Appendix Figure A4 plots the trajectories of household assets, debt, revenues, and consumption by household mean age at baseline.[16] It shows that there are not only differences in levels, but importantly differences in trends. For instance, younger households tend to accumulate assets and debt at a faster pace than older households, and they also seem to increase the scale of their businesses more than their older peers. Thus, we would like to account for such differences and compare post-shock responses of shocked households to the behavior of unaffected households, who were in a similar stage of the life cycle, but who did not simultaneously suffer the shock.

We follow Fadlon and Nielsen (2015, 2019)'s approach to construct a valid counter-factual for shocked households. We compare the behavior of households that belong to age group c, in village v, that experienced a shock in period t, to the behavior of households from the same age group and village who did not experience the shock at time t, but experienced a similar shock later on in period $t + \Delta_{c,v}$. By comparing households in the same cohort during the same time period, this approach analyzes households that in the absence of the shock would be facing similar economic decisions. Moreover, by choosing a large enough value for $\Delta_{c,v}$, this approach rules out the possibility that the comparison households experience a shock around t.

We begin by computing the average age in the household at baseline (1997).[17] We then group households in two age bins (below and above the median household age). Given our sample size, we

[16] We compute household mean age by taking an average of the ages of all household members.

[17] 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. We do, however, report robustness checks defining cohorts based on the age of the household head.

choose two age bins to ensure that we have multiple observations per bin. Next, for each age group within each village, we split the panel in two equal-length sub-samples $\{\theta^1_{c,v}, \theta^2_{c,v}\}$ by taking the midpoint between the months associated to the first and last shocks in the panel ($\Delta_{c,v}$).[18] Thus, the first sub-sample for age group c in village v ($\theta^1_{c,v}$) includes observations from households that experienced the shock between $\underline{t} = 24$ and $t_{med} = 24 + \Delta_{c,v}$. Conversely, the second sub-sample of cohort c in village v includes observations from periods $t_{med} + 1$ to period $\bar{t} = 148$. Note that we exclude the first and last 24 months in the sample from the placebo-assignment process to ensure that we observe all households at least 24 months before and after the actual and placebo shocks.

The construction of the comparison is based on the following intuition. Consider two households i and j from age group c in village v. Household i suffers a shock earlier, in period $t' \in \theta^1_{c,v}$ (first sub-sample), while household j suffers a shock $\Delta_{v,c}$ months later in period $t'' \in \theta^2_{c,v}$ (second sub-sample). Consider now the behavior of households i and j in the months preceding the shock to household i. As they are likely to be in the same stage of the life cycle, both households should be facing similar financial decisions. The main difference in period t' is that household i suffers a shock and household j doesn't. Thus, the behavior of household j around period t' serves as a counter-factual for the behavior of household i in the absence of the shock. Because the timing of the shocks is evenly distributed over time (see Appendix Figure A3), for each household experiencing a shock in the first half of the panel, we can compare its behavior around the shock to that of other households that experienced the shock around period $t'' = t' + \Delta_{v,c}$.

We operationalize this intuition by allocating a placebo shock to the comparison group. In the case of households that were shocked in the later part of the sample ($\theta^2_{c,v}$), we allocated a placebo shock $\Delta_{c,v}$ periods before they experienced the shock. Thus, if household j experiences the actual shock in $t'' \in \theta^2_{c,v}$, we allocated a placebo shock to j in period $t'' - \Delta_{c,v}$. As 244 households out of 505 shocked households experienced the shock in the earlier part of the panel, this process includes 244 shocked households and 244 placebo households.

To increase power and exploit all the variation associated to shocks to households in the second half of the sample, we use the behavior of household i around the time in which household j (t'') suffers the shock as a counter-factual for household's j choices in the absence of the shock. In this case, the comparison group consists of households that suffered the shock earlier on and their corresponding placebo event starts in $t' + \Delta_{c,v}$. We then show that including these households does not modify the point estimates, but it substantially increases statistical power.

[18] We define $\Delta_{c,v}$ as $\Delta_{c,v} = \frac{t^{max}_{c,v} - t^{min}_{c,v}}{2}$. On average, each sub-sample covers 66 months.

We end up observing 505 actual events and 505 placebo events. Thus, each household is observed once in the treatment sample, and once, $\Delta_{c,v}$ periods apart, in the placebo sample. Figure 3 plots means of health spending and the self-reported probability of experiencing health symptoms for the treatment and placebo groups. It shows that the comparison group does not experience any change in health spending or health status around the placebo shock. In the case of the treatment sample, the sharp increase in health spending seems to be driven by spending on inpatient and outpatient care. The magnitude of the increase in health spending suggests that health shocks were quite severe.

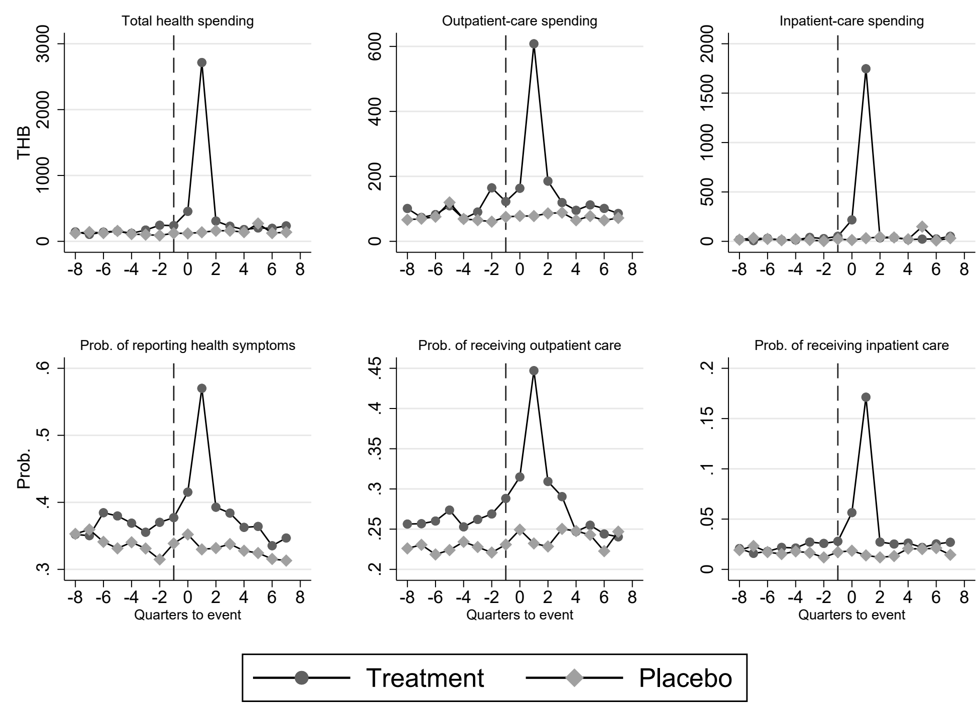


Figure 3: Health status and spending in the treatment and placebo samples

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. All averages are computed over a balanced panel of 505 households that ever experienced a health shock.

4.2 Estimation

Having identified a comparison group, we then estimate the following flexible difference-in-differences specification following Fadlon and Nielsen (2019):

$$
\begin{aligned}
y_{i,s,t} = & \sum_{h=-8,h\neq-1}^{h=7} \beta_h \mathbf{I}[\tau_{i,t}=k] \times T_s + \sum_{h=-8,h\neq-1}^{h=7} \theta_h \mathbf{I}[\tau_{i,t}=k] \\
& + \gamma T_s + X_{i,s,t}\kappa + \alpha_i + \delta_t + \epsilon_{i,s,t}
\end{aligned} \tag{1}
$$

Here, $y_{i,s,t}$ denotes the outcome of interest corresponding to household i, during period t, in sample $s \in \{Treatment, Placebo\}$. We control for household time-invariant characteristics and aggregate time-variant shocks by including household fixed effects (α_i) and month fixed effects (δ_t). We denote T_s as an indicator of whether the observation belongs to the treatment ($T_s = 1$) or placebo group ($T_s = 0$). Time to treatment is denoted by $\tau_{i,t}$ and is measured in quarters to increase precision. X is a vector of time-variant demographic characteristics including the number of male and female household members, age of household head and maximum years of schooling in the household. The coefficients of interest are $\{\beta_h\}_{h=-8}^{h=7}$. They compare differences in changes in outcomes with respect to the period preceding the shock ($\tau = -1$) between households in the treatment and comparison group. We focus on a two-year time window before and after the shocks as our panel is fully balanced during such period.

We complement equation (1) with a more parsimonious differences-in-difference specification:

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

In this case, $Post_{i,t}$ is an indicator that takes the value of 1 in periods following the shock, and 0 otherwise. The parameter of interest is β which simply compares differences in outcomes before and after the shock, between households in the treatment group and the comparison group. In both specifications, we cluster our standard errors at the household level as our main source of variation comes from cross-household variation in the timing of events, and to flexibly account for serial correlation (Bertrand et al., 2004).

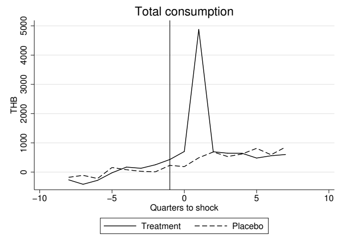
Note that our approach addresses two issues that may arise in simple event-study panel regressions without a placebo group—i.e., when researchers regress outcomes on time and household fixed effects and a post-shock dummy. A simple event-study approach would use all the households who have not experienced the shock at period t as a control group for those that did. This is problematic in our setting for two reasons. First, trends in outcomes may vary by age due to different trajectories along the life cycle, violating the parallel-trends assumption. By constructing a placebo group within age group and village, our approach makes comparisons of households with similar pre-shock trends. Second, while simple event-study panel regressions are well-powered to estimate immediate effects, effects in later periods are imprecisely estimated, as the size of the control group reduces in periods that are further away from the event. In contrast, our approach allows us to make comparisons of longer-term behavioral responses as we have a fixed comparison group for each post-shock period. We come back to this discussion in Section 4.3.3 when we discuss the robustness of our estimates to alternative specifications.

4.3 Results

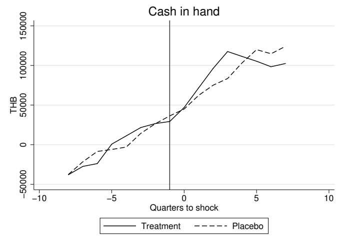
Graphical evidence: To illustrate the sources of variation behind our identification strategy, we begin by providing graphical evidence of changes in household outcomes before and after the shock, for the treated and placebo groups.

Figure 4a shows that, relative to the placebo households, the shocked households experience a sharp increase in total consumption. Figures 4b and 4c show that while the stock of household liquid assets remains unchanged after the shock, households experience a sudden increase in incoming gifts from other households. In terms of magnitudes, the increase in incoming gifts does not seem to be fully cover the spending needs due to the shock. As households are only partially insured through gift/transfer networks, the shocks seem to affect business spending and production. Figure 4d shows that, with respect to households in the placebo group, input spending slows down after the shock in the case of the shocked households. In a similar way, Figure 4e shows that labor usage declines. Finally, Figure 4f shows that the slow down in input spending coincides with a slow down in revenues after the shock in the case of shocked households. Overall, the graphical evidence suggests that despite the receipt of gifts and transfers, the shocks to household health endowments ended up affecting household production decisions.

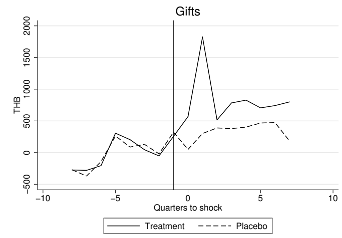
To provide a more-formal assessment of the impact of the health shocks on household outcomes, we report difference-in-difference estimates of the effect of the shocks based on equations (1) and (2). We organize the discussion of the effects of the shocks on different dimensions of household finance by referring to the accounting identity corresponding to the statement of cash flows for each household. The accounting identity states that outflows of resources must equal inflows of resources plus changes in cash holding. Under this logic, the shocks to health spending generate large outflows of resources which can be financed through four types of adjustments. First, the



(a) Total consumption

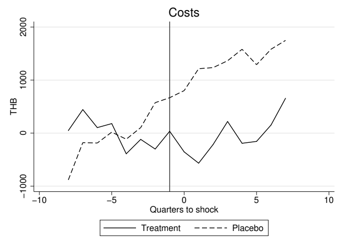


(b) Cash in hand

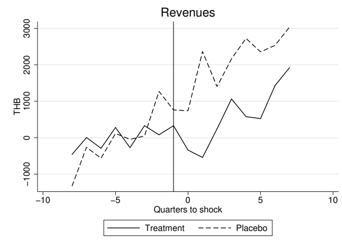


(c) Incoming gifts/transfers

(d) Hired labor (hrs/month)



(e) Input spending



(f) Revenues

Figure 4: Changes on 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 quarter preceding the sharp increase in health spending. Total consumption spending includes health spending. Savings is computed by subtracting total income from total spending. Revenues include income streams from all household enterprises and exclude earnings from providing wage labor to other households.

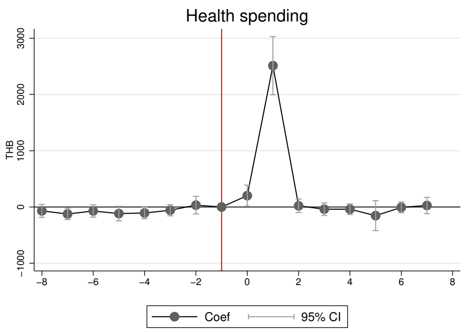
shock could crowd out non-health consumption. Second, households may liquidate their assets to finance the spending needs triggered by the shocks. Third, households may receive other inflows of resources either from gifts from other households, government transfers, or loans. Finally, the shocks could affect household production decisions by reducing hired labor or business investments to cope with the shocks.

4.3.1 Effects on consumption, assets, and transfers

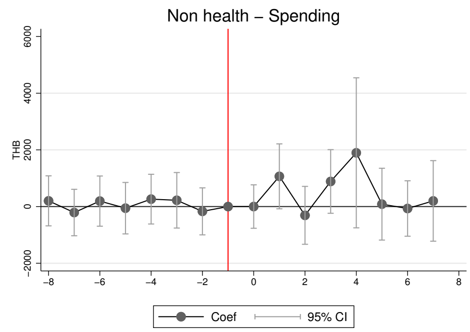
The top panels of Figure 5 analyze the changes in spending patterns due to the shock by plotting flexible difference-in-differences estimates corresponding to equation (1). The top figures show short run increases in both health spending and non-health spending due to the shock. There are also differences in the dynamics of the effects. By construction, the increase in health spending is strongest in the second quarter following the beginning of the event (Figure 5a).[19] The effect on health spending has dissipated by the following quarter (6 months after the dated onset of the

[19] Recall that, to address possible anticipation effects/planning for health events, we date the onset of the shock to 3 months prior to the increase in health spending.

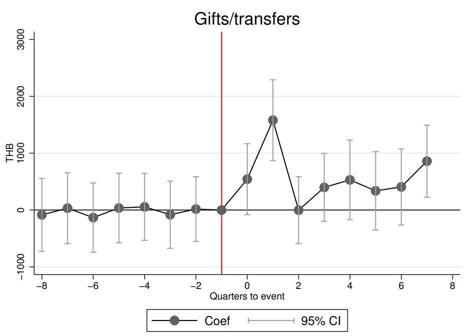
event). In contrast, although less precisely estimated, the increases in non-health spending (Figure 5b) persist up to 5 quarters after the beginning of the event. The likely explanation is that health shocks triggered other types of spending needs, such as goods or services related to the recovery or consequences of the illness (e.g., a special diet, in-home care, or funerals).[20]



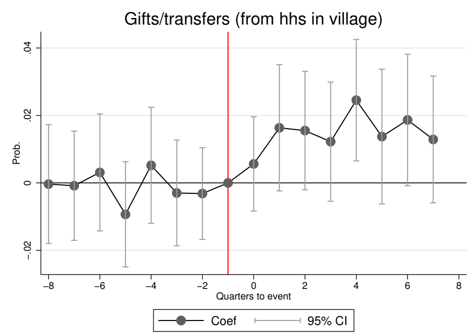
(a) Health spending



(b) Non-health spending



(c) Probability of receiving a gift from households in the village



(d) Total gifts/transfers received

Figure 5: Effects of idiosyncratic shocks on spending, and gift/transfer receipt

Note: The Figure plots flexible difference-in-differences coefficients associated to equation (1). 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 24 month's before and after the shock. All specifications control for household time-variant demographic characteristics, as well as household, month, and village-year fixed effects. 95% confidence intervals are computed using standard errors clustered at the household level. Non-health spending includes the value of home-produced goods used for consumption in a given period.

Panel A of Table 3 reports difference-in-differences estimates of the effect of the shock on household spending, corresponding to equation (2). Column (2) shows that during the two years following the shock, on average, total spending increased in the case of shocked households, relative to placebo households. This increase is twice as high as the average increase in health spending. As

[20] Health spending captures spending in inpatient, and outpatient care, medicines, and transportation to medical facilities. However, it does not capture other expenses such as food and at home-care services.

discussed above, this likely reflects a combination of spending to help the affected person recover, as well as funerals. Column (5) shows that there are neither substantial nor significant effects on food consumption, suggesting that, at least in that dimension, shocked households were able to buffer the shocks. Overall, the results imply that the shocks generated strong pressures on household budgets without crowding out food consumption. We analyze the mechanism through which consumption smoothing is achieved below.

We then turn to analyzing whether the shocked households use their assets to finance their spending needs. Panel B of Table 3 shows that households did not significantly rely 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 don't find significant changes in household fixed assets. While savings decrease, the decrease is not significant over the two-year post program period. One explanation is that incoming gifts could have provided extra income to cope with the shocks.

Next, we analyze whether households financed their severe spending needs with external sources of liquidity. Figure 5(c) shows that the probability of receiving gifts or transfers from other households in the village increases after the shocks. This increase in gifts from other local households highlights the importance of local informal insurance networks; when idiosyncratic shocks occur, other unaffected households respond by providing gifts or transfers to affected households. This evidence is consistent with the idea that the effects of idiosyncratic shocks can be (at least partially) smoothed through local risk-sharing networks (Samphantharak and Townsend, 2018; Townsend, 1994; Kinnan and Townsend, 2012).

While we do not observe the exact amount of the gifts received from every household in the village, we do observe the total amount of gifts and transfers received by each household, regardless of the source.[21] Figure 5d shows that treated households experience a sharp increase in the amount of gifts and transfers in the aftermath of the shock. The increases occur within the first two quarters following the shocks. Although they decay in subsequent quarters, they persist up to two years after the shock. Panel C of Table 3 shows that although incoming gift increased, there were no detectable effects on borrowing. One interpretation is that obtaining credit from banks or community-based

[21] We observe the number of transactions received from within the village (see Figure 5). To avoid overburdening households in our sample, we only collect the amount of the largest gifts received or made by each households in each month. However, we have the total amount of gifts in aggregate, including gifts from inside and outside the village, as well as transfers from the government.

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	382.0***	812.1***	430.1	398.3	31.77
	(52.50)	(288.8)	(281.6)	(267.7)	(53.40)
Baseline mean (DV)	147.3	5889.3	5742.0	3093.2	2648.8
Observations	46019	46019	46019	46019	46019
Number of households	503	503	503	503	503
R-Squared	0.201	0.164	0.153	0.112	0.716

Panel B: Effects on household savings and assets

	(1)	(2)	(3)	(4)	(5)
	Savings	Cash in hand	Livestock	Inventories	Fixed Assets
Post X Treatment	-1046.1	12718.9	78.18	287.8	-6048.5
	(1011.5)	(16271.3)	(1687.7)	(4117.6)	(5596.1)
Baseline mean (DV)	5775.4	438272.6	27357.0	127995.6	94767.4
Observations	86576	46019	46019	46019	46019
Number of households	503	503	503	503	503
R-Squared	0.132	0.869	0.802	0.883	0.755

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	0.0160**	0.0227***	584.0***	32.39	661.8**
	(0.00661)	(0.00815)	(153.5)	(251.5)	(325.5)
Baseline mean (DV)	0.0209	0.0260	2390.8	-61.08	2909.1
Observations	46019	46019	46019	46019	46019
Number of households	503	503	503	503	503
R-Squared	0.182	0.132	0.233	0.115	0.132

Panel D: Effects on family businesses

	(1)	(2)	(3)	(4)	(5)
	Costs	Hired labor (Hrs/Month)	HH Labor (Hrs/Month)	Biz. Assets	Revenues
Post X Treatment	-1291.5**	-11.79*	-18.43***	939.0	-1596.0**
	(546.1)	(6.223)	(6.304)	(1787.9)	(682.8)
Baseline mean (DV)	7255.0	14.81	139.6	31021.8	14683.7
Observations	46019	46018	46018	46019	46019
Number of households	503	503	503	503	503
R-Squared	0.774	0.708	0.711	0.869	0.600

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

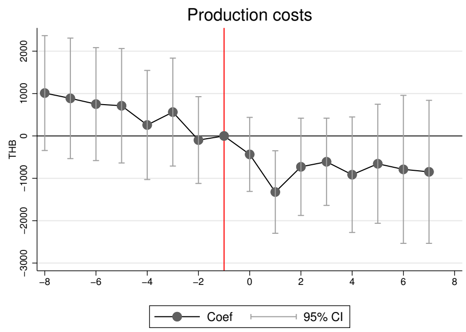
Note: The Table reports estimates of β from equation (2) for different outcomes. Each column reports differences between treatment and placebo households in changes in outcomes before and after the shock. All regressions control for household demographic characteristics, household and village-month fixed 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.

organizations is costly or entails a significant amount of delay. For instance, village funds do not meet often enough to evaluate loan applications. Thus, households may not be able to finance time-sensitive needs with formal or quasi-formal loans.

Note also that Column (5) shows that the post-shock increase in inflows (gifts and loans) only covers around two-thirds of the increase in total spending. This pattern suggests that despite having access to informal insurance networks, shocked households were not fully insured against the health shocks. As food consumption did not respond to the shock, shocked households may have achieved consumption smoothing by cutting back on other types of spending. However, we do not observe such spending cuts, implying that households must decrease spending on their business activities. Moreover, the dynamics of these effects suggest that households used gifts to finance immediate expenses, and could rely on alternative sources of financing subsequent expenses related to the shock. The results suggest that households may follow a pecking order when it comes to financing or coping with adverse shocks; they first rely on gifts, which might be less costly, and then turn to resources meant to fund their family businesses, which could compromise future income. We explore that possibility in the next subsection.

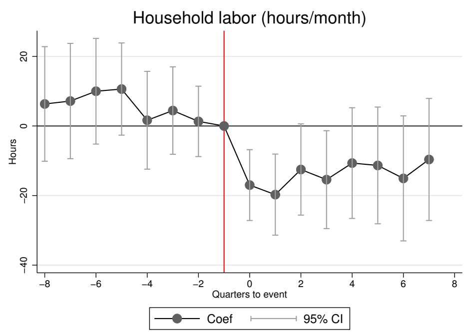
4.3.2 Effects on production

Figure 6 shows that, relative to the case of the placebo households, in the aftermath of the shock affected households decrease spending on business inputs (Figure 6a), reduce the use of external labor (Figure 6b), and reduce the use of labor provided by household members (Figure 6c). As a result, revenues from family businesses decline (Figure 6d). Note that in most cases, the sharpest declines coincide with the sudden increase in health spending at $\tau = 1$.

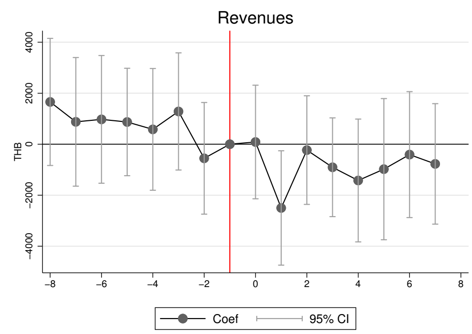


(a) Input spending

(b) Hired labor (hrs/month)



(c) Labor from household members (hrs/month)



(d) Revenues from household businesses

Figure 6: Effects of idiosyncratic shocks on business outcomes

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 24 months before and after the shock. All specifications control for household time-variant demographic characteristics, as well as household and village-month fixed effects. 95% confidence intervals are computed using standard errors clustered at the household level. Costs and revenues exclude costs and earnings associated with the provision of labor to other households or firms.

Consistently, Panel D of Table 3 shows that over the two-year period following the shock, the average reduction in business expenditure was substantial and coincides with reductions in labor demand and labor provided by household members. As a result, there is a decrease in the revenues from family enterprises. Note that the reduction in revenues is larger than the reduction in costs, suggesting that these responses were costly as they implied reductions in profits. Thus, although households seem to have insured consumption against these shocks, consumption smoothing came at the cost of a decline in household production.

4.3.3 Robustness

Robustness to alternative specifications. Our results are stable across a battery of alternative specifications. Appendix Table A4 shows that the results are not sensitive to controlling for time-varying demographic characteristics, such as education, household size, and gender composition; nor to including village-specific time trends.

Robustness to alternative definitions of the beginning of the shock. Throughout our analysis, we assume that each event starts the quarter before we observe the peak in health spending. One rational for this is to account for potential anticipation effects that could bias the results towards zero. Appendix Table A5 reports results from two alternative specifications that vary the definition of the beginning of the effect. 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. Similarly, Panel B reports estimates of the effects of the health shocks assuming that the event started two quarters before the observed peak. Reassuringly, the estimates are qualitatively similar to those from our main specifications.

Robustness to alternative definitions of placebo groups. We report three robustness checks concerning the construction of the placebo group for our analysis. In our main specification, we use the average household age to construct household cohorts in each village. This approach would be problematic if the relevant economic decisions of the households were more aligned with the heads age as opposed to the other household members. Appendix Figure A5 reports means before and after the shock for the treatment group and a placebo group which was constructed by allocating a placebo shock to households in the same village with household heads in a similar age bin at baseline. In all cases, the results are qualitatively similar to those from our main specifications. Panel A in Appendix Table A6 shows that the point estimates of the average effects of the shock are quite similar to those from our main specification.

Second, our results don't seem to be driven by our method of assigning the placebo shock. Our main specification assigns placebo shocks $\Delta_{v,c}$ periods away from the actual shocks, between village-cohort bins. An alternative approach would have been 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. Appendix Figure A6 reports means before and after the shock for the treatment group and a placebo group for which the shock was randomly allocated using a uniform distribution between the months associated to the first and last shock in each village. Panel B in Appendix Table A6 reports results of the average effect of the shock using this specification. In all cases, the results are qualitatively similar to those from our main specifications.

Third, our main analysis used 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 used households who experienced the shock in the earlier periods as a comparison group for households who suffered the shock in later periods. This approach could be problematic if large shocks that occurred earlier on could end up distorting the long-term trajectories of outcomes of placebo households. Panel C of Appendix Table A6 reports results of estimates that only consider shocks occurring in the first half of the sample, and placebo shocks that occurred before the households in the placebo group experience the shock. By doing so, we make sure that the placebo group is composed of only households that have not suffered the shock yet. In this case, the results are only generated by comparisons between 244 households who suffered the shocks in the earlier sub-sample, and their respective comparison households who suffered the shocks later on. Reassuringly, we obtain similar point estimates, though less precise as we reduced the number of events and observations by half. Appendix Figure A7 shows that the patterns in the dynamics of the effects are similar to those of our main specification.

Finally, we also report results from a simple panel specification using only data related to shocked households corresponding to a 24 month window before and after each event. We regress the outcome of interest on household fixed effects, village-month and cohort-month fixed effects and an indicator identifying pre- and post-shock periods. Panel D of Appendix Table A6 reports estimates of the effect health shocks on household outcomes following this approach. The results are qualitatively similar, though less precisely estimated. Unlike our main empirical approach, the simple panel approach does not ensure that households in the comparison group do not suffer a shock in any of the periods in the analysis window. As a result, the simple panel approach is only well-powered to detect immediate effects as some households may experience the shock only some months apart of each other. Thus, it is not surprising that we obtain larger magnitudes for outcomes that respond almost immediately to the shocks such as spending and gifts. Despite this issue, it is reassuring that we find decreases in costs that are close to being significant ($p - value < 0.13$).

4.3.4 Non separability: Unpacking the effects of health shocks on household production

In summary, our results show that idiosyncratic shocks affecting household endowments ended up affecting household production decisions. This result is consistent with evidence of incomplete insurance or labor markets and lack of separability of consumption (and labor supply) decisions and household production (Benjamin, 1992; Samphantharak and Townsend, 2018). However, it is still unclear whether the declines in business activities are due incompleteness in local insurance or labor markets. The former would imply that households with lower access to informal insurance may not be able to rely on transfers to smooth out shocks, and would have to reduce input spending. In contrast, the latter suggests that the declines in production could arise among those households for whom the shock was mainly a shock to labor endowments. If households can't replace the sick worker even if they receive enough transfers to cover the cost of health care—i.e., there might not be a market for individual-specific labor input into home production, then a shock to labor supply would end up affecting households production. In this section, we test the empirical salience of both mechanisms.

Heterogeneity by access to gifts and transfers networks. Although gifts and transfers allow households to insure against the shocks, not all households have access to this mechanism. Thus, liquidity-constrained households may have cut back on input spending. In such case, households who can rely more on local risk-sharing networks should have smaller adjustments in their production decisions. We take this idea to the data by exploiting rich information regarding pre-shock transactions across villagers. We proxy for access to informal insurance networks by identifying the number of transactions with different households in the village that involved either providing or receiving gifts or loans during the year preceding the shock. We classify the households with the most pre-shock connections in their networks (top 50%) as households with "high" access to informal insurance, and classify the households with fewer links (bottom 50%) as households with "low" access to informal insurance.[22] We then estimate the following triple differences model:

$$\begin{aligned} y_{i,s,t} =& \beta_1 Post_{i,t} \times Treated_s + \beta_2 Post_{i,t} \times Treated_s \times High_{i,s} + \theta_\tau \times High_{i,s} \\ &+ \gamma Treated_s + \sigma High_{i,s} + X_{i,s,t}\kappa + \alpha_i + \delta_t + \epsilon_{i,s,t} \end{aligned} \tag{3}$$

In this case, the unit of observation is the outcome of household i, in sample s at time t. As before, $Treatment$ indicates whether each observation belongs to the treatment or placebo samples, and $Post$ indicates the post-shock periods. $High_{i,s}$ takes the value of 1 in the case of households with higher access to informal networks. The coefficients of interest are β_1 and β_2. β_1 captures the effects of the shock in the case of households with lower access to insurance networks, and β_2 captures the differential effect between shocks to households with higher and lower access to intra-village insurance. Finally, $\beta_1 + \beta_2$ capture the effect of the shock in the case of higher-access households.

Panel A in Table 4 reports triple-differences estimates of the direct effects of the shock on gift receipt and business outcomes by access to informal insurance. While we do not find statistically significant differences on the effect of the shock on gift receipt, the differences are substantial. For instance, households with access to informal insurance networks receive 50% more gifts and loans than those with limited access to informal insurance. Moreover, the statistically significant decline in business revenues for uninsured households is almost offset when households have higher access to informal insurance (Column 9). The results suggest that households with limited access to insurance are the ones driving most of the declines in business activities, and are consistent with the idea that incompleteness in local insurance markets may lead to non-separability of household spending and production decisions.

Heterogeneity by who suffers the illness. Shocks related to illness of non-prime-aged household members are unlikely to directly affect labor supply. Thus, such shocks are primarily financial shocks that can be smoothed by relying on gifts and transfers. In contrast, shocks related to illness of prime-age members may also decrease labor supply even if financial gifts fully covered health spending. This distinction would be irrelevant if shocked households could substitute household labor input into home production with external sources of labor input. However, if shocked

[22] Note that we perform this exercise for households in the treatment and placebo sample. In the case of the placebo sample, we construct our measure of access to informal insurance based on the number of transactions in the local financial networks during the year preceding the placebo shock.

households can't substitute household labor, then the shocks to household labor supply should end up affecting production decisions as in (Benjamin, 1992). Using this distinction, we follow equation (3) to test whether shocks that are less insurable —those related to prime-age household members— are more likely to lead to reductions in business activity.[23]

Panel B of Table 4 shows significant financial needs triggered by the shock, with the increases resulting from the shocks to non-prime-age members being larger than those of prime-age members (see Columns (1) for health spending and (2) for total spending). Consistently, while shocks to non-prime-age members led to large significant increases inflows of gifts and transfers, shocks to prime-age household members only lead to smaller and non-significant increases in gift receipt (Column (5)). When we consider the impacts on production, however, we find that the shocks to prime-age members led to a larger and statistically significant drop in household's own labor supply (Column (7)). This decrease is twice as severe than the shocks related to illness of non-prime age household members. Consequently, both household's input spending and revenues decreased accordingly (Column (9). Altogether, the results suggest that local gift and transfer networks are more likely to be relevant for the shocks on household spending but not to the shocks on household production.

[23] In the case of the placebo shock $s = Placebo$, we identify the household member who reports the symptoms in the actual shock. We then assume that the placebo shock would affect the same household member affected by the actual shock.

Table 4: 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+Loans	Hired labor	HH Labor	Costs	Revenues
Post X Treatment (β_1)	350.4***	749.1	0.0144*	0.0145*	554.1	-5.751	-18.92**	-2116.1***	-2886.8***
	(66.80)	(461.0)	(0.00783)	(0.00875)	(374.3)	(5.097)	(8.089)	(564.5)	(791.7)
Post X Treatment X High Access (β_2)	80.22	89.22	0.00360	0.0182	217.2	-14.07	1.870	1801.3	2898.4*
	(103.4)	(603.0)	(0.0121)	(0.0155)	(559.5)	(8.697)	(12.51)	(1127.9)	(1480.7)
Effect: High Access ($\beta_1 + \beta_2$)	430.6***	838.4**	0.0180*	0.0327**	771.4	-19.82**	-17.05*	-314.8	11.57
P-val: High Access	0.00	0.02	0.08	0.02	0.12	0.05	0.08	0.76	0.99
Baseline mean (DV)	148.7	5905.9	0.0211	0.0262	2905.4	14.93	140.1	7323.8	14774.3
Observations	45422	45422	45422	45422	45422	45422	45422	45422	45422
R-Squared	0.205	0.166	0.186	0.136	0.134	0.710	0.712	0.775	0.602

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+Loans	Hired labor	HH Labor	Costs	Revenues
Post X Treatment (β_1)	520.3***	1079.6***	0.0144	0.0178	1150.1**	-9.660	-7.729	-1025.1	-1303.8
	(102.6)	(392.0)	(0.00904)	(0.0118)	(555.9)	(8.089)	(10.13)	(704.9)	(953.1)
Post X Treatment X Prime-working age (β_2)	-215.5*	-582.0	0.00178	0.00765	-906.1	-8.248	-10.64	-1150.7	-1188.8
	(117.9)	(541.6)	(0.0128)	(0.0157)	(697.9)	(13.09)	(14.92)	(1298.3)	(1544.0)
Effect: Prime-working age ($\beta_1 + \beta_2$)	304.8***	497.5	0.0162	0.0254**	243.9	-17.91	-18.36*	-2175.8**	-2492.5**
P-val: Prime-working age	0.00	0.22	0.10	0.03	0.57	0.17	0.07	0.04	0.04
Baseline mean (DV)	134.5	5542.8	0.0202	0.0252	2797.8	16.20	138.5	6872.3	13999.4
Observations	36157	36157	36157	36157	36157	36157	36157	36157	36157
R-Squared	0.160	0.234	0.172	0.134	0.147	0.738	0.731	0.771	0.601

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

Note: The Table reports estimates of β_1 and β_2 from equation (3) 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.

5 Economic networks and the propagation of idiosyncratic shocks

The results from the previous section show that health shocks can affect household production. As there is a large degree of inter-linkages in the study villages, it is natural to ask whether these shocks propagated to other households. Motivated by the previous findings we analyze two propagation channels. First, the shocks could propagate through the local supply chain networks. The health shocks led to decreases in the demand for inputs, which could lead to reductions on sales for those households that trade with shocked households. Second, the shocks could propagate through the local labor networks. As demand for outside labor decreased due to the shocks, the health shocks could affect households that provided labor to shocked households.

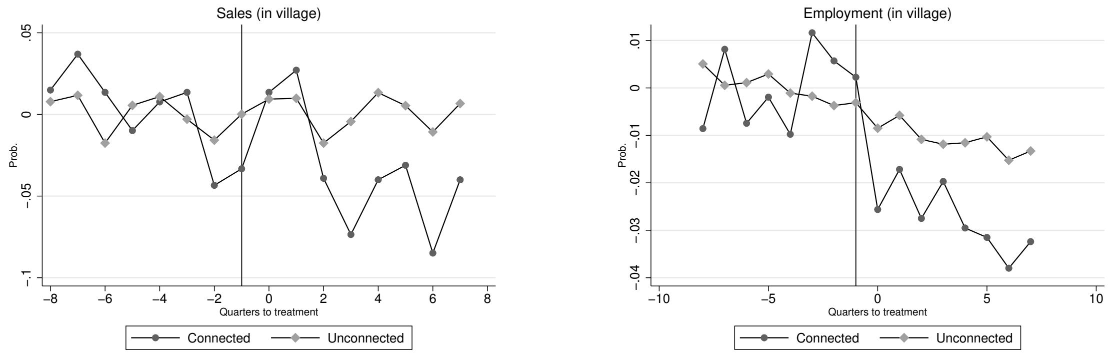
5.1 Identification strategy

We exploit two sources of variation to test if idiosyncratic health shocks propagated to other agents in the local economy. First, we can think of each shock as a natural experiment potentially affecting other actors in the local economy. Thus, within each village, we exploit the variation in the timing of each household-level shock. Second, not every household in the village is equally exposed to these shocks, and their exposure may depend on their relationship with the shocked household, either in the sales or the labor networks. Thus, by identifying households in the village with different type of pre-shock economic ties to the shocked households, we can exploit variation in indirect exposure to the shock.

We assess the propagation of idiosyncratic shocks to other local family businesses by comparing changes in behavior of households who, before the shock, shared market inter-linkages with household j's businesses (exposed households) to that of households who were unconnected to household i before the shock (unexposed households), before and after the shock to household j. We analyze two different ways of exposure to the shock: local sales and labor-market connections.

To illustrate the intuition of our identification strategy, we analyze first a case study. For each village, we selected one shock based on two criteria: shock size and the number of links of the shocked households in the supply chain and labor networks, respectively.[24] We then compare the

[24] For each village we constructed rankings based on the severity of health-spending shocks. We computed total health spending during the post-shock periods (two years) for each household in the village and kept the five households with the largest shocks in each village. Among these households, we selected the one with the highest number of connections in the village supply chain network (highest degree centrality) during the year preceding the shock. We repeated this process in the case of the labor networks.



(a) Probability of selling to local clients

(b) Probability of working for local employers

Figure 7: Local sales and employment before and after large shocks to central households
Note: The Figure plots means of the variable of interest as a function of time to the shock, by type of connections with the shocked household. All means are normalized with respect with the pre-event mean. We use 16 shocks, one shock per village. The right panel uses the largest shock to a central household in the supply chain network, in each village. The left panel uses the largest shock to a central household in the labor network in each village. Connected households are those who share a link with the shocked household during the pre-shock periods, or those who shared a link to a household that shared a link to the shocked household, and so on. Unconnected households are those who, during the pre-period, did not share a link with either the shocked household or a household that was connected to the shocked household. Directly shocked households are excluded from the calculations.

probability of selling either goods or labor to other businesses in the same village, before and after the shock, between households that are connected and unconnected to the shocked households through the relevant networks.[25]

The left panel of Figure 7 shows that, relative to unconnected households, households with connections to the shocked household in the supply chain network reduce the probability of a sale in the village during the post-shock periods. Similarly, the right panel of Figure 7 plots the probability of working for a local employer, before and after the shock, for households with and without labor-market connections to the shocked household. We also find a decrease in local employment in the aftermath of the shock in the case of connected households. These patterns suggest that idiosyncratic health shocks can propagate to other households through supply chain and labor networks.

Throughout our sample period we observe several health shocks. To make full use of the information, we extend our empirical approach to exploit variation from several shocks by constructing 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.[26] We then stack all the observations

[25] We focus on undirected, unvalued networks as the propagation of the shocks can go in different dimensions.

[26] We restrict the analysis to two years before and after the shock for two reasons. First, we would like to be

into a dataset at the household (i), time (t) and event level (j), village per village.

We combine this dataset with data regarding economic connections between the shocked household (j) and other households 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 (Huneeus, 2019). The idea 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 absence of the shock. Such idea is consistent 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 will come back to this issue in Section 6.2.

We then recover a measure of treatment exposure by constructing and index of closeness in the supply chain network as the inverse distance to the shocked household: $Closeness_{i,j} = \frac{1}{dist_{i,j}}$. This measure takes the value of one if household i directly traded with the shocked household j. As non-shocked households are further away in the network from shocked households—i.e., they transact with other households that transact with the shocked household— exposure (closeness) decreases. Finally, $Closeness_{i,j}$ takes the value of zero if household i does not have any direct or indirect connections with the shocked household.[27] We replicate the same procedure in the case of the labor network.

One limitation in our setting is that we elicit economic networks using survey data (Chandrasekhar and Lewis, 2017). Thus, it is possible that our measures of connections underestimate the closeness of some sample households to the shocked households.[28] Because we could be underestimating exposure, our results could be biased towards zero. Thus, we interpret our magnitudes as lower bounds of the indirect effects of idiosyncratic shocks on other households.

Finally, not all shocked households are active in the local markets for goods, and not all shocked

consistent with the analysis of the direct effects of the shocks. Second, we only have a fully balanced panel during these time window.

[27]The geodesic distance between two unconnected nodes is not defined, i.e., $dist_{i,j} = \infty$.

[28]For instance, suppose that in the population network, household i is connected to j through connections with households k and h, such that $i \rightarrow k \rightarrow h \rightarrow j$. Imagine now 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.

households employ other villagers for their businesses. Thus, our main specification analyzes propagation through the supply chain network by focusing only on the events corresponding to the 287 households who participated in the supply chain network during the year preceding their shock. In a similar way, we test for propagation through labor networks by analyzing the subset of 241 shocks to business owners that traded goods, and hired locally during the pre-period.

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

$$
\begin{aligned}
y_{i,t,j} = & \sum_{h=-8,h\neq-1}^{h=7} \beta_h \mathbf{I}[\tau_{j,t}=k] \times Closeness_{i,j} + \gamma Closeness_{i,j} + \mathbf{X}_{i,t,j}\kappa \\
& + \alpha_i + \omega_j + \delta_t + \theta_\tau + \upsilon_{v,t} + \epsilon_{i,t,j} \qquad (4) \\
y_{i,t,j} = & \beta Post_{t,j} \times Closeness_{i,j} + \gamma Closeness_{i,j} + \mathbf{X}_{i,t,j}\kappa \\
& + \alpha_i + \omega_j + \delta_t + \theta_\tau + \upsilon_{v,t} + \epsilon_{i,t,j} \qquad (5)
\end{aligned}
$$

Here, y denotes the outcome of interest for household i in village v at time t. In equation (4), $\tau_{j,t}$ denotes quarters preceding and following the shock to household j. $Closeness_{i,j}$ denotes inverse distance to the shocked household during the year preceding the shock to j. The coefficients of interest in equation (4) are β_h, which capture relative changes in outcomes corresponding to quarter h with respect to the quarter preceding the event ($h = -1$), between more- and less-exposed households. In the case of the more-parsimonious specification in equation (5), $Post_{t,j}$ takes the value of one during the two years following the shock to household j, and zero for the pre-period. The coefficient of interest is β. It captures differences in outcomes with respect to pre-period, for exposed households relative to unexposed households.

In both cases, we control for household fixed effects (α_i), shocked-household fixed effects (ω_j), time-to-shock fixed effects (θ_τ), and a vector of time-varying demographic characteristics ($\mathbf{X}_{i,t,j}$).[29] We also control for village-month fixed effects ($\upsilon_{v,t}$), in order to provide estimates that are net off potential village-specific shocks. We use two-way clustered standard errors at the event level j and household i levels to allow for flexible correlation across households during the periods preceding and following such event j, and across responses of the same household in different events. We drop observations of directly shocked households from the analysis.

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

5.2 Results: Propagation of shocks through economic networks

5.2.1 Propagation of health shocks through the supply chain networks.

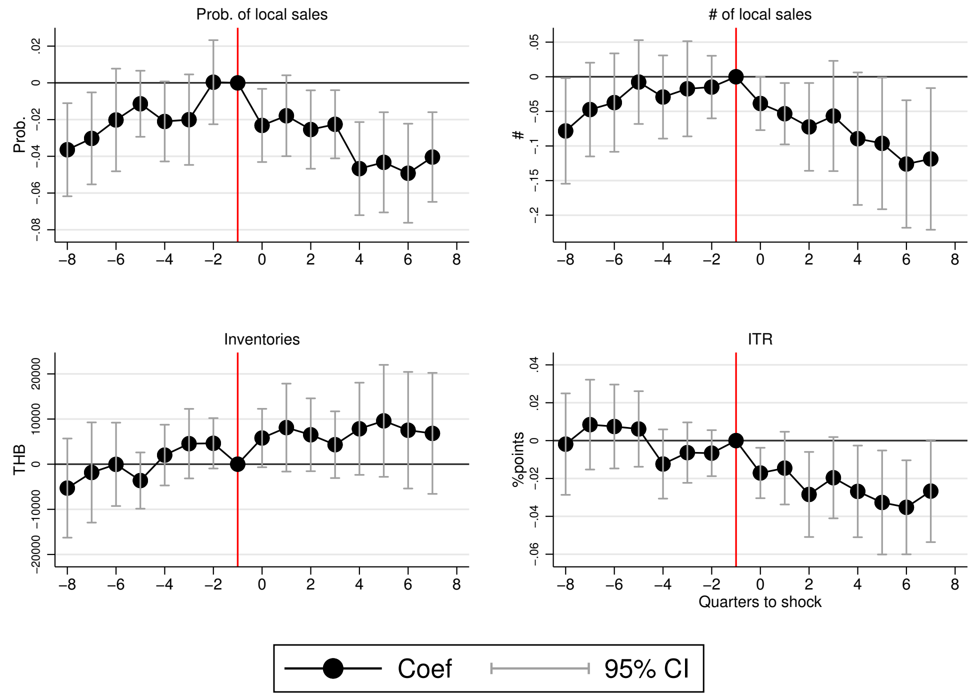


Figure 8: Propagation through supply chain networks

Note: The Figure presents flexible difference-in-differences 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-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 quarter preceding the shock (-1) between more- and less-exposed households. Standard errors are two-way clustered at the household (i) and shock level (j). ITR: Inventory turnover ratio (Inventories/Total revenues)

Figure 8 presents flexible difference-in-differences estimates following the specification of equation 4. It shows that households with connections with shocked households reduce the sales to local customers due to the health shocks. As sales of indirectly shocked businesses decline, their inventories increase leading to declines in inventory turnover (ITR).[30]

Panel A of Table 5 supports the graphical evidence with difference-in-differences estimates corresponding to (5). It documents a post-shock decline in the probability of selling output, and in the number of sales to local customers. These effects represent a 20% and 16% decline relative to the pre-period mean, respectively. Column (3) presents a substantial increase in inventories—an

[30]We compute the Inventory turnover ratio as the ratio of production costs to the stock of inventories and livestock.

Table 5: Propagation through supply chain networks

Panel A : All shocks to households who participated in the sales network (pre-shock)

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Prob. Any sale	# of sales	Inventories	ITR	Revenues	Cons. Spending
Post X Closeness (Sales Network)	-0.018***	-0.049*	20,257.826***	-0.037***	943.676	48.388
	(0.006)	(0.026)	(4,945.555)	(0.009)	(604.920)	(132.095)
Observations	421,224	421,224	421,224	421,224	421,224	421,224
R-squared	0.569	0.726	0.860	0.594	0.588	0.608
Pre-period mean	0.113	0.301	116800	0.0994	12976	6950
Number of events	287	287	287	287	287	287

Panel B : Excluding households experiencing shocks around each event

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Prob. Any sale	# of sales	Inventories	ITR	Revenues	Cons. Spending
Post X Closeness (Sales Network)	-0.012	-0.041	21,316.858***	-0.059***	866.654	99.330
	(0.010)	(0.029)	(7,432.576)	(0.017)	(1,089.594)	(221.127)
Observations	187,240	187,240	187,240	187,240	187,240	187,240
R-squared	0.610	0.785	0.892	0.578	0.654	0.646
Pre-period mean	0.113	0.304	104916	0.110	13608	6804
Number of events	286	286	286	286	286	286

Panel C: Large shocks (Post-period Health Spending above median)

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Prob. Any sale	# of sales	Inventories	ITR	Revenues	Cons. Spending
Post X Closeness (Sales Network)	-0.024*	-0.082**	17,947.045**	-0.094***	-79.605	398.812
	(0.013)	(0.041)	(8,992.160)	(0.028)	(1,704.982)	(308.343)
Observations	91,895	91,895	91,895	91,895	91,895	91,895
R-squared	0.605	0.789	0.892	0.590	0.615	0.655
Pre-period mean	0.109	0.271	128504	0.146	17166	8425
Number of events	133	133	133	133	133	133

Panel D: Case study (largest shock to a central household in each village)

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Prob. Any sale	# of sales	Inventories	ITR	Revenues	Cons. Spending
Post X Closeness (Sales Network)	-0.035	-0.177**	24,926.398**	-0.073*	-2,365.440	330.203
	(0.027)	(0.074)	(10,258.587)	(0.038)	(2,566.461)	(495.231)
Observations	22,178	22,178	22,178	22,178	22,178	22,178
R-squared	0.669	0.840	0.964	0.691	0.559	0.687
Pre-period mean	0.099	0.270	143588	0.122	15130	7914
Number of events	16	16	16	16	16	16

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

Note: The Table presents estimates of β from equation (5). $Closeness_{i,j}$ 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 the supply chain network. 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. Panel A includes all the events associated to households with pre-shock participation in the local market for goods. Panel B excludes those households that experienced a health shock themselves during the event-analysis window. Panel C includes only events that implied post-shock health spending levels larger than the pre-period average per-capita consumption (annual). Panel D only includes the largest 16 shocks associated to highly connected households, one per village. Each regression controls for household, event, village-year and month fixed effects as well as time-varying demographic characteristics. Standard errors are two-way clustered at the household (i) and event (j) level.

18% increase relative to the average pre-period stock of inventories. The increases in inventories coincides with a 50% decrease in the inventory turnover ratio, relative to the pre-shock period. This overall decrease on local sales can be costly, as there is evidence suggesting that households in developing countries may face storage costs and/or losses due to spoilage of products (Burke et al., 2018).

Column (5) in Table 5 shows that there are not significant effects on revenues, and Column (6) shows that more-exposed households did not significantly change their consumption patterns, relative to their less-exposed peers. These results suggest that indirectly shocked households were able to smooth out the health shocks affecting their providers. We discuss the mechanisms of this result in more detail in Section 7.

The previous results survive a battery of robustness checks. One concern is that, just by chance, some households may experience shocks around the time of the shocks to other households in their villages. For instance, if household i suffers a direct shock around the shock to household j, it is unclear whether our estimates would capture direct or indirect responses. To test the sensitivity of the results to this issue, in Panel B, we drop from the sample all the observations corresponding to households that experience a health shock during any of the 24 post-shock months associated to each event. While this exercise reduces the number of observations and hence reduces precision, the results are both quantitatively and qualitatively similar to those of Panel A. Second, to make sure that the effects are driven by large shocks —i.e., those which are more likely to generate decreases in the demand of inputs and subsequently lead to spillovers, in Panel C, we restrict the sample to the 50% largest shocks, based on increases in health spending. As expected, the patterns are even stronger.

Finally, our pooled difference-in-differences approach differs from standard approaches as we include several events leading to the possibility of repeated observations in the dataset. Panel D presents results from our case study which analyzes only one shock per village—i.e., the largest shock to a central household, and does not include repeated observations. Reassuringly, all results are qualitatively similar, and even stronger in magnitude.

5.2.2 Propagation of health shocks through labor networks

Figure 9 presents flexible difference-in-differences estimates of the indirect effects of health shocks on other households. It shows that workers from more-exposed households experience a sharp decline in the probability of working for other households in the village during the post-shock periods, relative to workers from less-exposed households. This decline on local labor provision coincides with a reduction in total monthly work hours, and with a decline in wage earnings, one year after the shock. Panel A of Table 6 supports the evidence from the graphical analysis. It documents

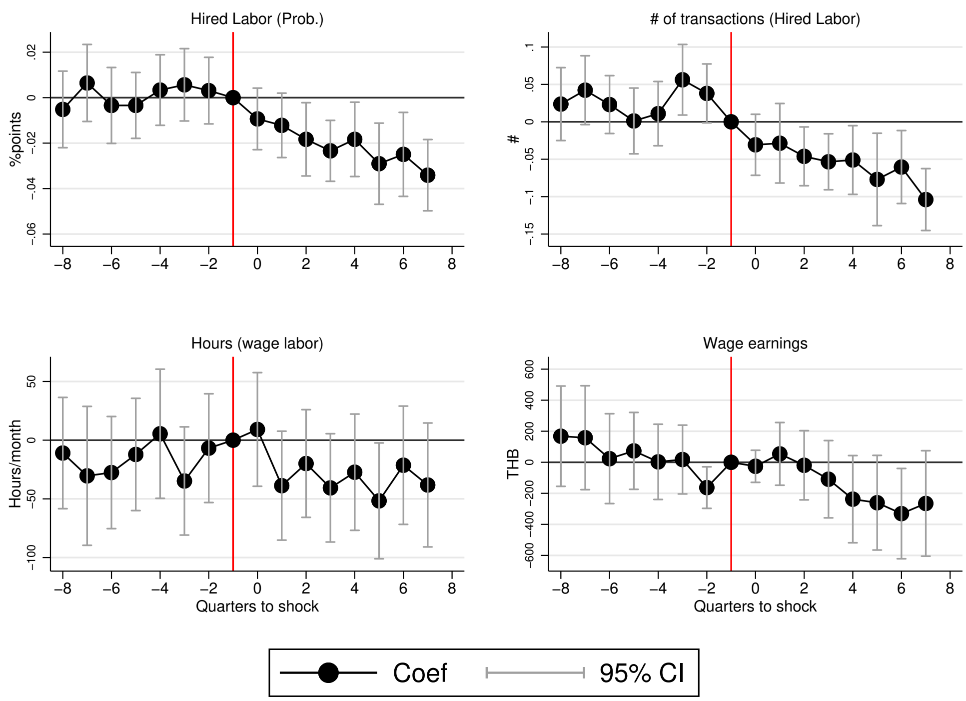


Figure 9: Propagation through labor networks

Note: The Figure presents flexible difference-in-differences 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-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 quarter 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.

a 2-percentage-point decline in the probability of working for other households which represents a drop of 25% with respect to the pre-period mean. This decrease coincides with an average 13-hour decrease in monthly wage labor, which represents 10% with respect to the baseline mean.These results are robust across alternative specifications.

It is worth noting that while we are not able to detect significant average decreases in earnings due to the shocks in Panel A, Panels B and C document substantial declines in earnings corresponding to higher-exposure households. These declines are precisely estimated and account for 16 and 20% of the pre-period averages. Finally, there are neither substantial nor significant changes in consumption during the post-shock periods in the case of higher-exposure households, relative to their lower-exposure peers. Overall, the results suggest that idiosyncratic health shocks propagated through the labor networks, but indirectly affected households were able to smooth out such shocks.

Table 6: Propagation through labor networks

Panel A : All shocks to households who sell and hire locally (pre-period)

	(1)	(2)	(3)	(4)	(5)
VARIABLES	Prob. hired labor (in village)	Labor sales (in village)	Wage labor (hours/month)	Earnings	Cons. Spending
Post X Closeness (Labor-market Network)	-0.021***	-0.078***	-13.455*	-162.244	76.591
	(0.005)	(0.016)	(7.397)	(114.084)	(98.297)
Observations	353,720	353,720	353,720	353,720	353,720
R-squared	0.233	0.225	0.114	0.726	0.589
Pre-period mean	0.0868	0.198	140.6	3121	6422
Number of events	241	241	241	241	241

Panel B : Excluding households experiencing shocks around each event

	(1)	(2)	(3)	(4)	(5)
VARIABLES	Prob. hired labor (in village)	Labor sales (in village)	Wage labor (hours/month)	Earnings	Cons. Spending
Post X Closeness (Labor-market Network)	-0.016**	-0.059**	-13.820	-494.372***	-54.953
	(0.007)	(0.024)	(10.256)	(189.570)	(175.944)
Observations	154,677	154,677	154,677	154,677	154,677
R-squared	0.255	0.268	0.168	0.757	0.634
Pre-period mean	0.0818	0.197	135.1	3073	6341
Number of events	240	240	240	240	240

Panel C: Large shocks (Post-period Health Spending above median)

	(1)	(2)	(3)	(4)	(5)
VARIABLES	Prob. hired labor (in village)	Labor sales (in village)	Wage labor (hours/month)	Earnings	Cons. Spending
Post X Closeness (Labor-market Network)	-0.024**	-0.056*	-18.500	-547.469*	55.903
	(0.010)	(0.028)	(16.805)	(319.701)	(273.385)
Observations	70,114	70,114	70,114	70,114	70,114
R-squared	0.242	0.214	0.204	0.700	0.654
Pre-period mean	0.0705	0.139	170	4290	7998
Number of events	100	100	100	100	100

Panel D: Case study (largest shock to a central household in each village)

	(1)	(2)	(3)	(4)	(5)
VARIABLES	Prob. hired labor (in village)	Labor sales (in village)	Wage labor (hours/month)	Earnings	Cons. Spending
Post X Closeness (Labor-market Network)	-0.045**	-0.108**	18.203	-39.212	739.287
	(0.017)	(0.039)	(29.179)	(258.467)	(590.490)
Observations	22,178	22,178	22,178	22,178	22,178
R-squared	0.246	0.219	0.170	0.816	0.658
Pre-period mean	0.071	0.136	162.9	4004	7395
Number of events	16	16	16	16	16

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

Note: The Table presents estimates of β from equation (5). Each coefficient captures differences in changes in outcomes before and after the shock between more- and less-exposed households, through the labor network. $Closeness_{i,j}$ denotes inverse distance to the shocked household during the year preceding the shock to j. 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. Panel A includes all the events associated to households with pre-shock participation in the local market for goods and labor. Panel B excludes those households that experienced a health shock themselves during the event-analysis window. Panel C includes only events that implied post-shock health spending levels larger than the pre-period average per-capita consumption (annual). Panel D only includes the largest 16 shocks associated to highly connected households, one per village. Each regression controls for household, event, village-year and month fixed effects as well as time-varying demographic characteristics. Standard errors are two-way clustered at the household (i) and event (j) level.

6 Propagation Mechanisms

6.1 Access to insurance and the propagation of shocks

Section 4 presented suggestive evidence that the idiosyncratic shocks were more likely to trigger declines in business activities in the case of shocked households with limited access to informal insurance. One implication is that shocks to uninsured households should propagate more to other households. To test this hypothesis, we estimate the following pooled difference-in-differences model:

$$\begin{aligned} y_{i,t,j} = {} & \beta_1 Post_{t,j} \times Closeness_{i,j} + \beta_2 Post_{t,j} \times Closeness_{i,j} \times Access_j \\ & + \beta_3 Post_{t,j} \times Access_j + \gamma Closeness_{i,j} + X_{i,t,j}\kappa + \alpha_i + \omega_j + \delta_t + \theta_\tau + \upsilon_{v,t} + \epsilon_{i,t,j} \end{aligned} \tag{6}$$

In this case, $Access_j$ is an indicator of whether the shock corresponds to a household with above-median pre-period access to informal insurance networks j, measured by the number of transfers or loans excahnged with other households in the village during the year preceding the shock. As above, $Closeness_{i,j}$ denotes inverse distance between i and the shocked household during the year preceding the shock. The coefficient of interest is β_3 which captures the sensitivity of the indirect effects of the shock to whether the shocked household had ex-ante access to informal insurance networks.

Table 7 provides evidence that shocks to uninsured households propagate more to other households. Column (3) in Panel A shows that health shocks to uninsured households lead to substantial reductions on inventory turnover (ITR), and that such effects are attenuated in the case of shocks to households with pre-period access to informal insurance. We observe similar patterns in the case of local sales, although the latter effects are imprecisely estimated (Column (1)). We further find that households with low baseline access to insurance experience a significant increase in inventories (Column (2)), although the differential effect for households with access to insurance is not significant.

In Panel B we examine the propagation of these shocks through labor networks. We find evidence of differential effects on labor provision, based on whether the shocked household had access to insurance. Shocks to uninsured households more severely reduced local employment of other households (Column (1) shows the intensive margin and Column (2) shows the extensive margin). This decrease is almost completely offset when the shocked household had access to

Table 7: Propagation of health shocks and access to informal insurance of shocked households

Panel A: Propagation through sales networks by access to informal insurance of shocked household

	(1)	(2)	(3)	(4)	(5)
VARIABLES	# of sales	Inventories	ITR	Revenues	Cons. Spending
(1) Post X Closeness (Sales Network)	-0.073	37,952.928**	-0.085***	933.489	252.289
	(0.066)	(15,391.084)	(0.024)	(1,376.359)	(320.404)
(2) Post X Closenes X Access (shocked household)	0.049	9,341.792	0.040***	1,650.250***	-99.649
	(0.066)	(14,752.863)	(0.006)	(537.885)	(279.433)
Observations	179,292	179,292	179,292	179,292	179,292
R-squared	0.795	0.871	0.554	0.659	0.636
Pre-period mean	0.333	95155	0.122	13565	6827
Number of events	239	239	239	239	239

Panel B: Propagation through labor networks by access to informal insurance of shocked household

	(1)	(2)	(3)	(4)	(5)
	Labor provision in village				
VARIABLES	Prob.	Labor sales	Wage labor (hours)	Wage Earnings	Cons. Spending
(1) Post X Closeness (Labor Network)	-0.036***	-0.112*	-1.604	-103.302	202.803
	(0.013)	(0.063)	(16.050)	(386.522)	(313.704)
(2) Post X Closenes X Access (shocked household)	0.021*	0.055	-8.062	-330.949	-207.526
	(0.012)	(0.060)	(14.633)	(322.803)	(293.489)
Observations	150,498	150,498	150,498	150,498	150,498
R-squared	0.272	0.282	0.193	0.741	0.636
Pre-period mean	0.0855	0.202	137.7	3171	6570
Number of events	203	203	203	203	203

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

Note: The Table reports estimates of β_1 and β_2 from the pooled difference-in-differences equation (6) 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, village-year 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.

informal insurance. (In Columns (3) - (5) we examine earnings and spending, but these effects are not significant.) Taken together, these results suggest that *ex-ante* upstream access to informal insurance networks minimizes the propagation of idiosyncratic shocks to other households.

6.2 Rigidities in local markets

In a context of frictionless, competitive markets for goods and labor, idiosyncratic decreases in the demand for labor or goods should not affect other businesses or households; sellers should be able to easily find new customers, and laid-off workers should easily find new employers. However, there is evidence suggesting that market frictions may prevent transactions across businesses. For instance, Johnson et al. (2002) found 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. Appendix Table A11 reports probabilities of trade between households during the last three years of the panel (2012-2014) by whether they transacted 10 years earlier during the first three years of the sample (1999-2001), by type of transaction. It shows that the probability of transacting during the last three years of the panel is substantially higher for pairs of households that transacted earlier in the sample, and is almost negligible in the case of pairs that did not trade at baseline. This pattern is evident in the local goods, labor, and financial markets.

To further 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.

$$\begin{aligned} Link_{i,j,t} =& \rho Link_{i,j,t-1} + \gamma_1 Kinship_{i,j} + \gamma_2 \text{Demographic distance}_{i,j} \\ &+ \gamma_3 \text{Net-Worth distance}_{i,j} + \delta_{v,t} + \alpha_i + \alpha_j + \epsilon_{i,j,t} \end{aligned} \tag{7}$$

Here, $Link_{i,j,t}$ 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.[31] We include controls for distance with respect to demographic characteristics, which we approximate by obtaining 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 the households members at baseline. We then take logs of the resulting norm. We also include a measure of distance between each pair of households based on baseline net worth (e.g., total assets net of liabilities). We do

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

so by taking logs of the squared net-worth difference within each pair. Finally, we also include household specific fixed effects. The parameter of interest is ρ, it captures the persistence of the economic interactions between each pair of sample households.

Table 8 presents estimates of equation (7) for several specifications, by type of transactions. 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), which are substantially higher than that of the financial network (0.26). In the case of the labor-market and the supply chain network, 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 as transactions in period $t-1$ only explain 7% of the overall variation in the probability of transacting. One explanation is that financial networks are less active, and are probably responding to either unexpected business opportunities or shocks, which is consistent with the results from Section 4. In all cases, persistence is still substantial after controlling for village-year fixed effects, which suggests 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. This is consistent with previous evidence on the importance of kinship networks in village economies (Kinnan and Townsend, 2012; Samphantharak and Townsend, 2010; Angelucci and De Giorgi, 2009).

Finally, the probability of trade in the supply chain and labor networks does not seem to respond to differences in distance or wealth between the two households. In contrast, the probability of trading in the local financial network increases as 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 substantially in demographic characteristics are more likely to transact. This suggest that one motive for trading is insurance/diversification, as the type and occurrence of the shocks may vary with demographic characteristics. Second, similarly wealthy households are more likely to trade. Thus, one explanation for observing only partial insurance through local networks in this setting could be related to inequality: poorer households may be less likely to benefit from substantial transfers from wealthier households.

Table 8: Persistence in transaction networks, by network type

	Sales				**Labor-Market**				**Gift / 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)				-0.000				-0.001				0.001**
				(0.001)				(0.001)				(0.001)
Net-worth distance (log of squared differences)				-0.000				-0.000				-0.000**
				(0.000)				(0.000)				(0.000)
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
Village-year FE	NO	YES	YES	YES	NO	YES	YES	YES	NO	YES	YES	YES
Household i FE	NO	NO	YES	YES	NO	NO	YES	YES	NO	NO	YES	YES
Household j FE	NO	NO	YES	YES	NO	NO	YES	YES	NO	NO	YES	YES

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

Note: The table presents regression coefficients following the specification in equation (7). 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. 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.

6.3 Alternative propagation mechanisms

Our analysis of the direct effects of health shocks in Section 4 suggests that they led to a reduction in the demand of inputs and labor, in the case of shocked households. Moreover, the results from Section 5.2 suggest that such shocks propagated to other households through the local supply chain and labor networks. However, it is possible that other mechanisms could explain the results. For instance, 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 local sales could be a consequence of a decline in liquidity. Appendix Table A9, shows that neither transfers nor loans to other households differentially increased due to the shocks, in the case of households that were closer to the shocked households in either the supply chain (Panel A) or the labor networks (Panel B). If anything, the provision of transfers to other households declined among higher-exposure households.

Another alternative propagation mechanism is that as the health shocks affected household production, these idiosyncratic shocks disrupted production supply chains. For example, Carvalho et al. (2016) find that the Great East Japan Earthquake disrupted supply chains of intermediate inputs, and ended up affecting firms from non-earthquake areas which were customers of firms in affected areas. As our main analysis uses undirected networks to prevent measurement error issues and boost statistical power, it is important to verify that the indirect effects of the health shocks are indeed driven by households that would have sold either goods or labor to the shocked households. To do so, we distinguish between exposure to health shocks to other households through the supply- vs. demand-side of each network. We compute measures of closeness to the directly shocked households based on whether household i purchased goods from household j prior to household j's shock, and whether household i sold inputs to household j during the pre-periods. In a similar way, we distinguish between the households that, during the pre-periods, hired members of household j, and households that provided labor to shocked households j.

Appendix Table A10 reports estimates of the indirect effects of the health shocks based on the type of connection to the shocked households following an specification similar to equation (5). Reassuringly, Columns (1) to (4) show that the main results are driven by households for which the health shocks represented a reduction in the *demand* for goods (Panel A) and labor (Panel B). For instance, Column (4) from Panel A shows that the health shocks decreased the inventory-turnover ratio (ITR) in the case of households that were more likely to sell their output to shocked households, and that there are neither significant nor substantial changes in the case of the households that were more likely to purchase output from shocked households. Similarly, Panel B shows that the negative indirect effects of the health shock on the provision of labor are driven by the households who are more likely to have *sold* labor to the shocked households, and not by those who were more likely to *purchase* labor from shocked households in the pre-period.

7 Coping with Shock Propagation

Although idiosyncratic health shocks adversely affect the sales and employment of non-shocked households, consumption does not significantly respond to shocks to others. This result suggests that households were able to smooth out indirect shocks.[32] However, such adjustment mechanisms may themselves be costly.

Effects on gift receipt, fixed assets, and livestock. Appendix Table A7 reports estimates of the indirect effects of health shocks on the probability of receiving transfers from other households in the village, the number of transfers from other households in the village, and the total amount of received transfers and loans from any source. Across specifications, the results show that more exposed households did not receive more gifts from other households in the aftermath of the shock. This suggests that, as the idiosyncratic shocks propagate and hence become *de facto* aggregate, indirectly affected households cannot rely on local insurance networks for smoothing.

Reallocation. In the Thai context, many rural households are endowed with land and likely to conduct farming activities for household consumption. For those who do farming for business, they tend to sell most of their crops (rice) or products from their ponds (shrimp) to customers outside their villages (e.g., cooperatives or exporting firms). As a result, farm activities might be less exposed to the propagation of shocks to other households in the village, although more exposed to regional or international shocks. In contrast, households that mostly obtain revenues from providing wage labor or operating non-farm businesses —e.g., retail— tend to serve local employers and customers. In the aftermath of the shocks, households who were indirectly hit by idiosyncratic shocks may reallocate resources to activities that provides a buffer to consumption shocks or activities that are less vulnerable to shocks to other households in the village, namely, farming activities (both for household's own consumption and for commercial purposes).

[32]For example, indirectly affected households could have received gifts from other households. Another way of smoothing out these shocks is by reallocating resources across the economic activities performed by the households.

Table 9 reports estimates of the indirect effects of the health shocks on businesses revenue of other households following equation (5), over the subsample of households that do not experience a direct health shock in the 24 months following to shocks to their neighbors. It shows that during the post-shock periods, revenues from farm-related activities (agriculture, livestock and shrimping) increased in the case of more-exposed households. In the case of households that were more exposed to through the supply chain network, Panel A shows that the increase in farm revenues coincides with decreases in non-farm businesses, although the latter are not precisely estimated. In the case of households that were more exposed to the health shocks through the labor network, Panel B shows that the increases in agricultural revenues coincide with decreases in wage earnings. Appendix Table A8 shows that these patterns are robust to estimating the effects using all households in the sample, only large shocks, and only data corresponding to the case study.

The results suggest that households tend to reallocate resources to activities that are less vulnerable to within-village shocks. Previous studies have argued that one way of managing risk ex ante in rural settings is through income diversification (Reardon et al., 1992; Morduch, 1995). The results in this section suggest that, as there are barriers to trade leading to rigidities in networks, income diversification may allow households to reallocate resources across activities as an ex post mitigation strategy. One implication is that the propagation of idiosyncratic shocks through local networks may slow down the process of structural transformation and changes in occupational choice away from farm to non-farm activities (Banerjee and Newman, 1998).

Table 9: Indirect effects on businesses, by sector

	Farm		Off-farm		Wage Labor	
Panel A : Sales networks						
	Revenues	Costs	Revenues	Costs	Revenues	Costs
	(1)	(2)	(3)	(4)	(5)	(6)
Post X Closeness (Sales Network)	702.960***	218.182***	-184.179	-48.060	-125.313	-5.704
	(213.183)	(70.864)	(1,035.)	(915.103)	(211.799)	(26.948)
Observations	187,240	187,240	187,240	187,240	187,240	187,240
R-squared	0.324	0.339	0.813	0.835	0.729	0.479
Pre-period mean	4400	1032	6155	4987	3534	183.3
Number of events	286	286	286	286	286	286
Panel B: Labor-market networks						
	Farm		Off-farm		Wage Labor	
	Revenues	Costs	Revenues	Costs	Revenues	Costs
	(1)	(2)	(3)	(4)	(5)	(6)
Post X Closeness (Labor-market Network)	711.936***	186.121**	-757.306	-325.547	-494.372***	-35.215
	(226.490)	(73.339)	(526.799)	(507.873)	(189.570)	(32.742)
Observations	154,677	154,677	154,677	154,677	154,677	154,677
R-squared	0.339	0.353	0.820	0.842	0.757	0.462
Pre-period mean	4226	980.2	5965	4860	3073	159
Number of events	240	240	240	240	240	240

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

Note: The Table presents estimates of the indirect effect of the idiosyncratic health shocks on revenues of other households. Farm businesses: Includes agricultural activities, livestock and fishing and shrimping. Off-farm businesses: Include non-farm businesses but excludes revenues from wage labor. Each regression controls for household, event, village-year 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. Panel A includes events corresponding to shocks to households who traded in the local supply chain network during the year preceding the shock. Panel B includes events corresponding to shocks to households who traded and hired locally during the year preceding each shock. Standard errors are two-way clustered at the household (i) and event (j) level.

8 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.

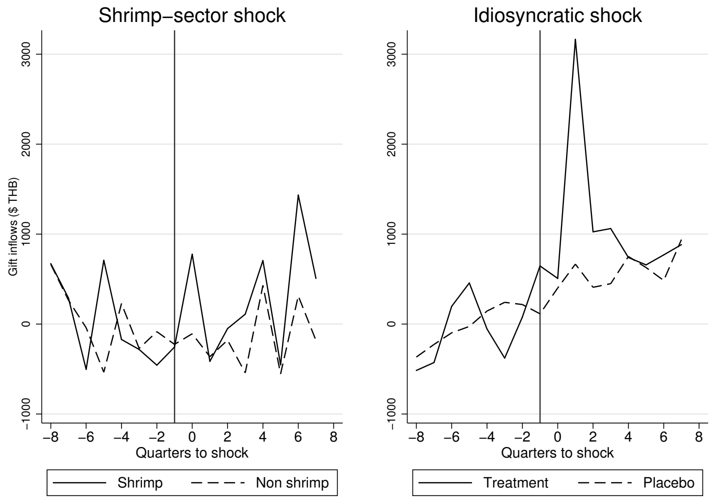
8.1 Comparing responses to idiosyncratic versus sectoral shocks

The propagation of economic shocks has been mostly studied in the context of sectoral or regional shocks. However, we focus on granular shocks affecting one household at the time. This distinction is important for two reasons. First, a large share of firms in developing countries are family-owned and are not only exposed to sectoral or regional shocks but also to idiosyncratic consumption-side shocks. Second, the nature of the shock may lead to different economic reactions. For instance, as local networks may be more effective at providing insurance against idiosyncratic shocks than sectoral shocks.

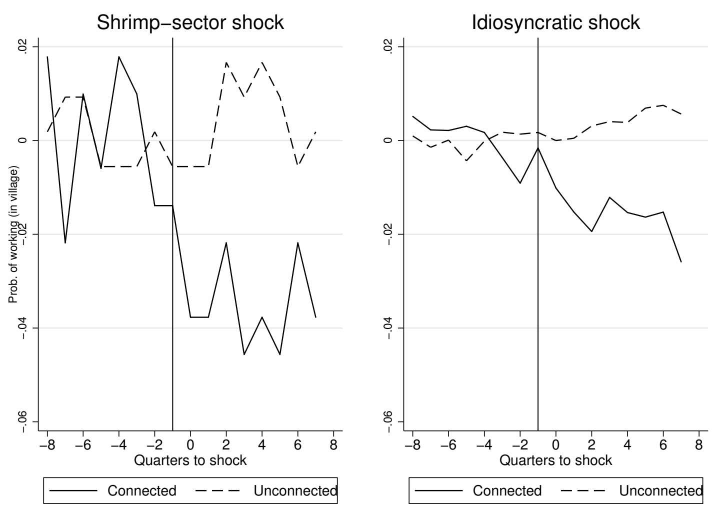
We illustrate the latter point by graphically comparing the responses to idiosyncratic shocks in our setting to the responses to sectoral shocks. For this, we follow Giannone and Banternghansa (2018) and exploit the fact that our dataset overlaps with the introduction of shrimp import ban which prohibited imports of Thai shrimp to the European Union.[33] We begin by comparing gift inflows before and after the ban to shrimp farmers relative to non-shrimp households, in the four shrimp-producing villages in our dataset. The top-left panel of Figure 10 shows that gifts to shrimp-producing households don't seem to increase during the post-ban periods, relative to non-shrimp households. We then compare these responses to changes in gift-inflows in the aftermath of our idiosyncratic health shocks, relative to placebo households in the 4 shrimp-producing villages in our sample. The top-right panel of Figure 10 shows that the increase in gifts following the idiosyncratic shocks is substantially larger than the one associated to the sectoral shock induced by the EU shrimp ban. This result is consistent with the idea that local risk-sharing networks are more effective at insuring idiosyncratic shocks.

Next, we analyze the relative magnitude of the propagation of idiosyncratic shocks, relative to sectoral shocks. For this, we focus on propagation through labor networks as shrimp farming is a

[33] 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. The shock directly affected 30% of shrimp-producing villages in our sample.



(a) Direct effects on gift receipt



(b) Propagation of shocks through local labor networks

Figure 10: Comparison of direct and indirect effects by type of shock
Note: The figures depict means of gift inflows (top panel) and the probability of working for local employers (bottom panel), by subgroups before and after the shock. For comparison, the variables are re-scaled to represent differences from pre-shock periods. Panel A depicts the direct effects of sectoral (left-side panel) and idiosyncratic shocks (right panel). Panel B reports indirect effects of sectoral (left panel) and idiosyncratic shocks (right-side panel). The European Union import ban on Thai shrimp was announced in May 2002. The figure only presents means for the four villages with shrimp production in the Chachoengsao province.

labor-intensive activity.[34] The bottom-left panel of Figure 10 shows that the probability of working for a local employer decreases dramatically in the case of households who were connected (either directly or indirectly) to shrimp farmers through the pre-shock labor networks. In contrast, the

[34]Indeed, shrimp-farmers are more likely to be central nodes in the local labor networks.

bottom-right panel shows smaller declines in local employment in the aftermath of idiosyncratic health shocks to employers. Overall, these patterns highlight the importance of distinguishing between idiosyncratic and sectoral shocks when analyzing propagation. Once again, the results also show that local financial networks are an important source of insurance against idiosyncratic shocks, and against the propagation of such shocks.

8.2 The multiplier effect of idiosyncratic shocks

As documented above, idiosyncratic health shocks have both direct and indirect costs. The former are larger, but the latter can potentially affect many more households. In order to compare their overall magnitude, and so obtain an estimate of the overall consequences, we perform a simple calculation.

One challenge is to choose a suitable outcome variable. While consumption is arguably the bottom-line welfare measure, it is not appropriate in our context as households take other costly measures in order to smooth consumption over the short term. These methods, involving reduced investments in businesses, will then depress income and hence consumption in the long term, however our identification strategy is not able to detect long-term effects. Therefore, we focus on revenues as an alternate measure. The direct effect on revenues, from Table 3, Panel D, column 5 is a fall of 1596 baht per month (significant at 5%).

To capture the indirect effects, for sales networks we focus on off-farm businesses since, as discussed above, farm businesses are less affected and may see offsetting reallocation effects. The indirect effect on off-farm businesses from Table 9, Panel A, column 3, is a fall of 184 baht (not significantly different from zero) per month per unit change in *Closeness*. For the labor network, we use the effect on wage revenue (Table 9, Panel B, column 5), which is a fall in wage revenue of 494 baht per month (significant at 1%) per unit change in *Closeness*.

The mean level of *Closeness* in the supply chain network is 0.336 and the average number of indirectly exposed households (i.e., households who are connected to the shocked household via the supply chain network) is 10.30. Thus the implied total indirect effect is $-184 \times 0.336 \times 10.3 = -636.9$ baht per month, which represents 39.9% of the direct effect of 1596 baht per month.

The mean level of *Closeness* in the labor market network is 0.360 and the average number of indirectly exposed households (i.e., households who are connected to the shocked household via the supply chain network) is 10.46. Thus the implied total indirect effect is $-494 \times 0.36 \times 10.46 = -1881.6$ baht per month, which represents 117.9% of the direct effect of 1596 baht per month.

Summing across the effects which propagate via the sales and labor notworks, the total implied indirect effect is -1882 baht per month, which is 158% of the direct effect.

While these numbers 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.

9 Concluding remarks

We study the dual role of networks in providing insurance and in propagating idiosyncratic shocks. If markets are complete, then idiosyncratic shocks are fully insured. Shocks do not affect production and hence no propagation through production networks. In the absence complete markets, a new set of shocks is created in the economy that would not be there if there had been full insurance. In such situation, households may need to diversify their production activities, which is costly as they have to forgo gains from specialization and economies of scale (e.g., workers may need to be specifically trained for each employer). The impact from propagation through supply chain or labor networks could be long lasting as suppliers and customers may switch to other partners or activities. These costly adjustments could have been avoided if there is full insurance.

Empirically, we use variation in the timing of severe shocks on health spending experienced by households in Thai villages. We find no impacts on food consumption. Smoothing is largely achieved through local gift and loan networks. However, insurance is partial for some households that need to adjust their production decisions–drawing on their working capital, cutting input spending, and reducing labor hiring, hence propagating the shocks to other households. Upstream businesses close to the underinsured households in the supply chain network experience reduced local sales and increased inventories. Likewise, workers closer to the underinsured households in the labor network experience declined probability of working locally and reduced earnings. We find evidence of ex-post adjustments of these upstream households, shifting resources towards activities with lower exposure to local shocks.

Our study suggests that two sets of policies might be needed: (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 and inevitably take place. First, full insurance can then 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 and the shared risk remains covariate. Formal commercial insurance contracts (through companies operating nationally or beyond) or social insurance (through central government) could allow better insurance and better prevention of propagation through production and labor networks. Second, in order to mitigate the impacts from propagation through supply chain and labor networks, one can consider broadening the extent of product and factor markets beyond that local village market via regional or economy-wide platforms, to take advantage of more systematic multi-lateral matching, yet dealing with and exacerbating underlying information frictions.

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A Appendix Figures and Tables

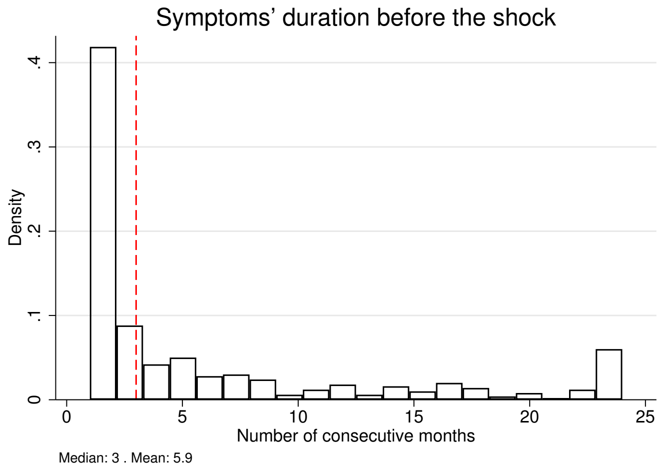


Figure A1: Distribution of pre-spike symptom duration

Note: The figure plots the distribution of the number of consecutive months prior to the spikes in health spending for which any household member reported health symptoms.

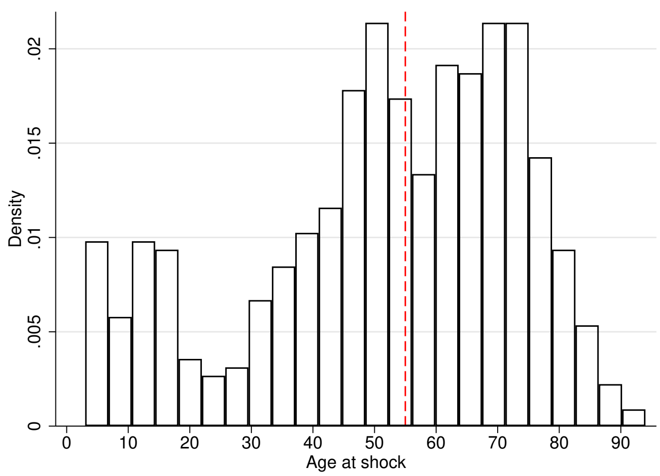


Figure A2: Age at shock

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 434 shocks for which we found households reporting health symptoms.

Table A1: Time use during the pre-shock periods: Count of days dedicated to different activities

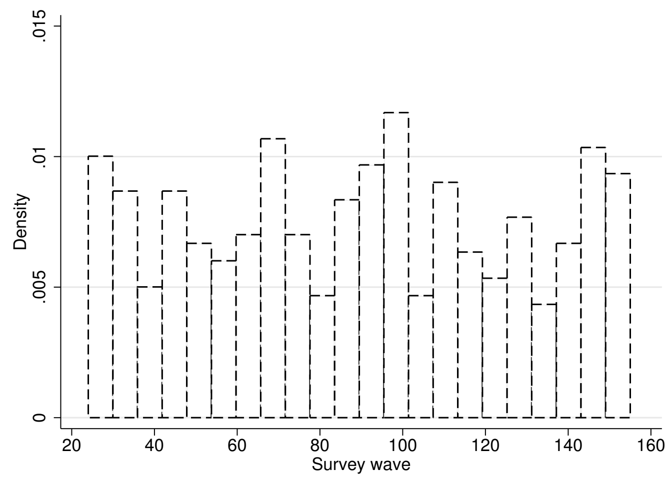
	Number of days per month	More than 15 days
	Average	Share
	(1)	(2)
Cultivation	7.14	0.16
Livestock	12.38	0.40
Fish/Shrimp	0.80	0.02
Off-farm business	2.04	0.06
Housework	23.33	0.82
School or training	2.59	0.05
Positions in village organizations	0.78	0.02
Funerals/Weddings	0.63	0.00
Labor exchange outside home	0.02	0.00
Unpaid labor outside home	0.30	0.01
Paid labor outside home	2.60	0.07
Looking for a job	0.01	0.00
Sick	0.25	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.

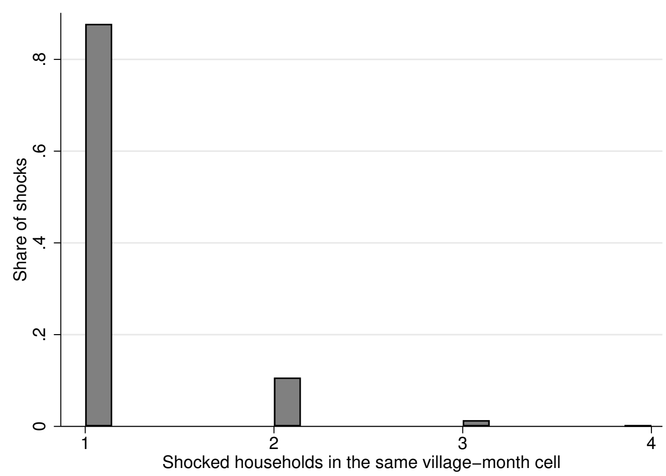
Table A2: Incidence of health conditions by type of symptom

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.



(a) Distribution of initial event's period



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

Figure A3: Distribution of events by initial event's periods and number of affected households

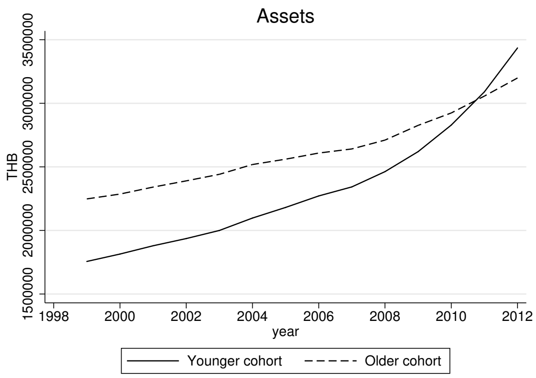
Note: The top panel plots a histogram capturing the distribution of survey months associated the beginning of the health shocks across the full sample period. The bottom panel plots the distribution of events by the number of households simultaneously affected in the same village.

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

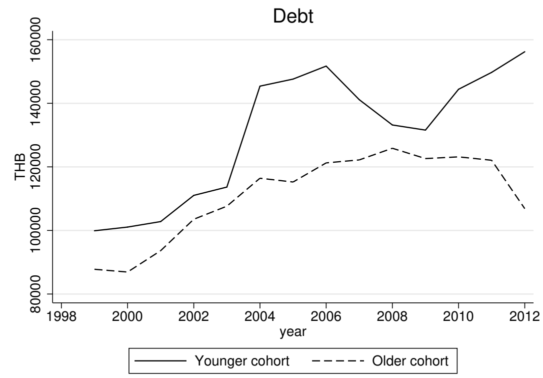
VARIABLES	(1) Δ P(event)	(2) Δ P(event)	(3) Δ P(event)
Lagged Δ P(event)	-0.500***	-0.501***	-0.5005***
	(0.000)	(0.001)	(0.0009)
Lagged Δ Total net operating income			0.0007
			(0.0008)
Lagged Δ Consumption spending			-0.0025
			(0.0018)
Lagged Δ Consumption of household production			-0.0478
			(0.0836)
Lagged Δ Borrowing			-0.0004
			(0.0010)
Lagged Δ Lending			-0.0048
			(0.0037)
Lagged Δ Inflows (transfers)			0.0006
			(0.0009)
Lagged Δ Outflows (transfers)			0.0002
			(0.0003)
Lagged Δ Livestock value			-0.0003
			(0.0005)
Lagged Δ Cash in hand			0.0003
			(0.0004)
Lagged Δ Fixed assets - excludes land			0.0008
			(0.0006)
Lagged Δ Land value			0.0002
			(0.0005)
Observations	86,530	82,693	82,693
R-squared	0.252	0.252	0.2523
Month FE	Yes	Yes	Yes
Village FE	No	Yes	Yes
Number of households	509	509	509
F-stat Village FE		0.776	0.470
P-val (Village FF)		0.567	0.955

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

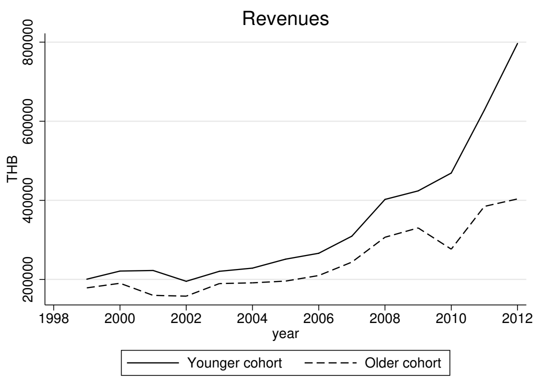
Note: The table reports OLS coefficients from changes in the the probability of suffering a shock on period t on lagged changes and village fixed-effects in columns 1 and 2. The bottom panel reports an F-test for the joint significance of the village fixed effects. Column 3 reports similar coefficients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



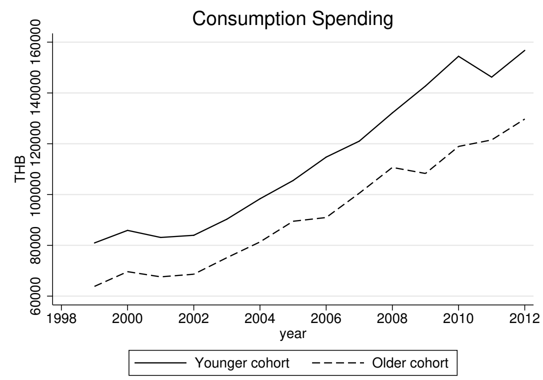
(a) Stock of Assets



(b) Stock of Debt



(c) Revenues from production



(d) Spending

Figure A4: Trends in assets, debt, revenues and spending by age group

Note: The figure plots averages of household-finance variables over time by age cohorts. The median household age is 34 years old. The younger cohort includes households that, at baseline , had a household average age below the cross-household median. Similarly, the older cohort includes households that, at baseline , had a household average age above the cross-household median.

Table A4: Robustness to alternative specifications

Panel A: Effects on Spending

	Health			Total			Non-health spending: Total			Non-health spending: Non-food			Non-health spending: Food		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Post X Treatment	367.047***	367.863***	382.006***	902.276***	853.187***	812.092***	535.229*	485.325	430.086	474.883	463.601	398.315	60.346	21.723	31.771
	(56.054)	(57.754)	(52.505)	(322.810)	(324.577)	(288.758)	(315.745)	(316.868)	(281.588)	(300.498)	(303.550)	(267.739)	(58.552)	(55.526)	(53.400)
Observations	47,704	46,048	46,019	47,704	46,048	46,019	47,704	46,048	46,019	47,704	46,048	46,019	47,704	46,048	46,019
R-squared	0.096	0.097	0.201	0.119	0.122	0.164	0.110	0.112	0.153	0.069	0.071	0.112	0.648	0.674	0.716
Pre Mean	143.7	143.7	143.7	5850	5850	5850	5706	5706	5706	3068	3068	3068	2638	2638	2638
Number of households	505	503	503	505	503	503	505	503	503	505	503	503	505	503	503

Panel B: Effects on household assets

	Savings			Cash in hand			Livestock			Inventories			Fixed assets		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Post X Treatment	-506.801	-628.575	-612.550	-2,215.983	5,270.183	12,718.889	556.092	513.126	78.181	294.601	1,226.103	287.776	-3,029.752	-5,658.411	-6,048.514
	(503.144)	(520.699)	(537.965)	(15,205.767)	(15,714.612)	(16,271.264)	(1,683.007)	(1,739.057)	(1,687.681)	(3,945.589)	(3,969.340)	(4,117.576)	(5,180.097)	(5,334.497)	(5,596.056)
Observations	47,704	46,048	46,019	47,704	46,048	46,019	47,704	46,048	46,019	47,704	46,048	46,019	47,704	46,048	46,019
R-squared	0.143	0.145	0.225	0.836	0.840	0.869	0.746	0.751	0.802	0.853	0.853	0.883	0.729	0.736	0.755
Pre Mean	5519	5519	5519	433309	433309	433309	26885	26885	26885	126880	126880	126880	93094	93094	93094
Number of households	505	503	503	505	503	503	505	503	503	505	503	503	505	503	503

Panel C: Effects on gifts, transfers and debt

	Gifts from households in the village: Prob.			Gifts from households in the village: Count			Gifts/Transfers			Borrowing			Gifts+Loans		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Post X Treatment	0.012**	0.013**	0.016**	0.017**	0.018**	0.023***	574.541***	631.467***	584.046***	140.783	64.682	32.392	761.499**	755.305**	661.775**
	(0.006)	(0.006)	(0.007)	(0.007)	(0.008)	(0.008)	(146.969)	(147.749)	(153.477)	(233.029)	(241.554)	(251.459)	(314.262)	(316.552)	(325.473)
Observations	47,704	46,048	46,019	47,704	46,048	46,019	47,704	46,048	46,019	47,704	46,048	46,019	47,704	46,048	46,019
R-squared	0.120	0.124	0.182	0.071	0.074	0.132	0.167	0.171	0.233	0.039	0.040	0.115	0.063	0.065	0.132
Pre Mean	0.0208	0.0208	0.0208	0.0257	0.0257	0.0257	2386	2386	2386	-50.82	-50.82	-50.82	2898	2898	2898
Number of households	505	503	503	505	503	503	505	503	503	505	503	503	505	503	503

Panel D: Effects on family businesses

	Costs			Hired labor (Hrs/Month)			HH Labor (Hrs/Month)			Biz. Assets			Revenues		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Post X Treatment	-1,341.366**	-1,329.192**	-1,291.477**	-10.513**	-10.747**	-11.794*	-13.115**	-15.859**	-18.425***	748.727	446.456	938.969	-1,682.156**	-1,752.652***	-1,595.958**
	(524.184)	(532.026)	(546.057)	(4.996)	(5.163)	(6.223)	(6.619)	(6.543)	(6.304)	(1,747.047)	(1,804.762)	(1,787.852)	(662.778)	(671.245)	(682.795)
Observations	47,704	46,048	46,019	47,691	46,047	46,018	47,691	46,047	46,018	47,704	46,048	46,019	47,704	46,048	46,019
R-squared	0.747	0.748	0.774	0.684	0.684	0.708	0.664	0.671	0.711	0.851	0.858	0.869	0.549	0.543	0.600
Pre Mean	7312	7312	7312	14.37	14.37	14.37	137.6	137.6	137.6	30454	30454	30454	14615	14615	14615
Number of households	505	503	503	505	503	503	505	503	503	505	503	503	505	503	503
Demographics	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Village X month FE	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes

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

Note: The Table reports OLS estimates of β from equation (2) for different outcomes. Three specifications are tested for each variable. First, a version of equation (2) that includes household and month fixed effects but neither includes demographic characteristics nor village-specific time trends. The second specification includes demographic characteristics and the third one includes both demographic characteristics and village-month fixed effects. Each column reports differences between treatment and placebo households in changes in outcomes before and after the shock. Standard errors are clustered at the household level.

Table A5: Robustness to alternative definitions of the beginning of the events

Panel A: Beginning of event coincides with the observed peak in health spending

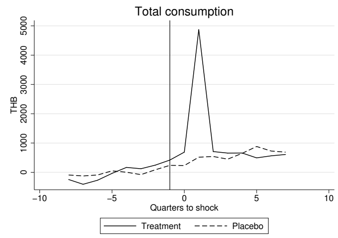
	(1) Health	(2) Total	(3) Non-health	(4) Non-food	(5) Food	(6) Savings	(7) Cash in Hand	(8) Livestock	(9) Inventories	(10) Fixed assets	(11) Prob. Gift (in village)	(12) # of Gifts (in village)	(13) Gifts/Transfers	(14) Borrowing	(15) Gifts+Loans	(16) Costs	(17) Hired labor	(18) HH Labor	(19) Biz. Assets	(20) Revenues
Post X Treatment	405.5***	774.4***	368.9	311.6	57.33	-2197.2*	11065.2	-577.7	427.5	-5334.1	0.0139**	0.0196**	469.5***	170.5	767.0**	-1345.2**	-13.06*	-19.64***	1183.7	-1657.4**
	(53.07)	(287.2)	(280.6)	(267.2)	(50.56)	(1209.6)	(16339.2)	(1714.5)	(4076.5)	(5638.8)	(0.00635)	(0.00792)	(150.8)	(235.1)	(311.4)	(578.3)	(6.798)	(6.344)	(1817.7)	(733.0)
Baseline mean (DV)	140.3	5808.7	5668.4	3049.7	2618.6	6264.3	429623.1	27956.1	124743.3	93170.3	0.0212	0.0259	2331.9	-43.82	2861.5	7173.0	15.23	140.7	31057.5	14483.8
Observations	46197	46197	46197	46197	46197	46197	46197	46197	46197	46197	46197	46197	46197	46197	46197	46197	46196	46196	46197	46197
Number of households	503	503	503	503	503	503	503	503	503	503	503	503	503	503	503	503	503	503	503	503
R-Squared	0.202	0.169	0.158	0.116	0.714	0.190	0.866	0.809	0.879	0.753	0.183	0.135	0.229	0.114	0.127	0.774	0.699	0.712	0.869	0.600

Panel B: Beginning of event starts 6 months before the observed peak in health spending

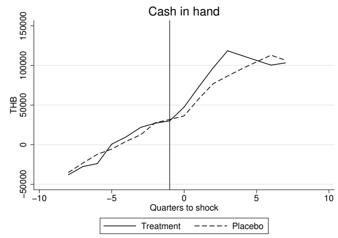
	(1) Health	(2) Total	(3) Non-health	(4) Non-food	(5) Food	(6) Savings	(7) Cash in Hand	(8) Livestock	(9) Inventories	(10) Fixed assets	(11) Prob. Gift (in village)	(12) # of Gifts (in village)	(13) Gifts/Transfers	(14) Borrowing	(15) Gifts+Loans	(16) Costs	(17) Hired labor	(18) HH Labor	(19) Biz. Assets	(20) Revenues
Post X Treatment	338.2***	741.1**	402.9	380.4	22.48	-2269.0**	13790.0	777.4	84.15	-6212.7	0.0171**	0.0230***	467.3***	83.45	534.9*	-969.6*	-9.289*	-13.51**	599.8	-1379.0**
	(43.58)	(295.5)	(293.5)	(280.0)	(54.87)	(1051.8)	(16567.9)	(1676.5)	(4117.5)	(5534.3)	(0.00691)	(0.00850)	(146.5)	(238.4)	(307.3)	(498.8)	(5.546)	(6.295)	(1814.1)	(611.7)
Baseline mean (DV)	167.1	5961.8	5794.7	3119.6	2675.1	6734.6	448051.4	26745.9	131481.1	96324.3	0.0205	0.0256	2467.4	-41.41	3025.1	7322.1	14.20	138.4	31004.1	14795.3
Observations	45720	45720	45720	45720	45720	45720	45720	45720	45720	45720	45720	45720	45720	45720	45720	45720	45718	45718	45720	45720
Number of households	503	503	503	503	503	503	503	503	503	503	503	503	503	503	503	503	503	503	503	503
R-Squared	0.167	0.164	0.154	0.112	0.720	0.164	0.873	0.797	0.888	0.757	0.184	0.136	0.235	0.112	0.131	0.776	0.720	0.713	0.868	0.600

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

Note: The Table reports OLS estimates of β from equation (2) for different outcomes. Each column reports differences between treatment and placebo households in changes in outcomes before and after the shock. All regressions include a vector of demographic characteristics as well as household and village-month fixed effects. Standard errors are clustered at the household level.



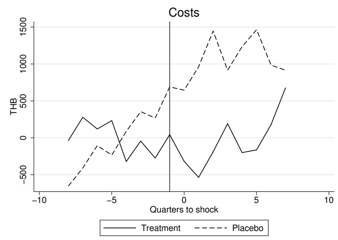
(a) Total consumption



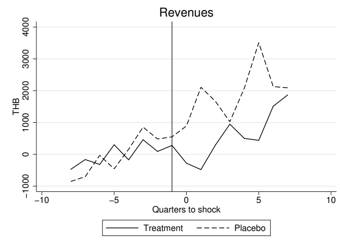
(b) Cash in hand

(c) Incoming gifts/transfers

(d) Hired labor (hrs/month)



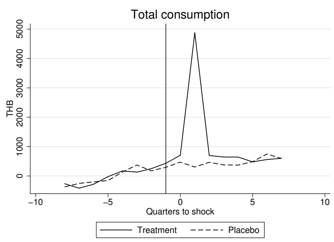
(e) Input spending



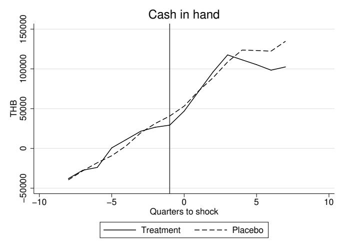
(f) Revenues

Figure A5: Changes on household outcomes before and after the shock - Placebo group based on household head baseline age

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 quarter preceding the sharp increase in health spending. Total consumption spending includes health spending. Savings is computed by subtracting total income from total spending. Revenues include income streams from all household enterprises and exclude earnings from providing wage labor to other households.



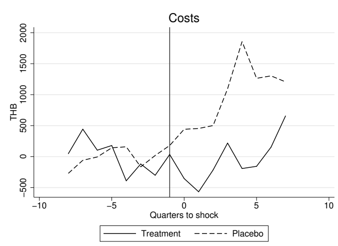
(a) Total consumption



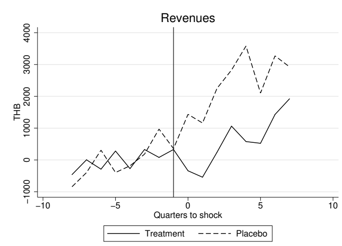
(b) Cash in hand

(c) Incoming gifts/transfers

(d) Hired labor (hrs/month)



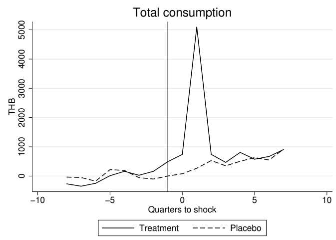
(e) Input spending



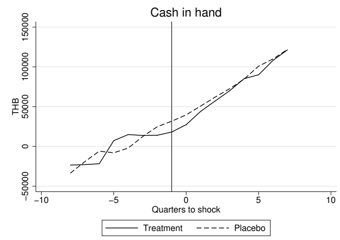
(f) Revenues

Figure A6: Changes on household outcomes before and after the shock - Randomly assigned placebo shock

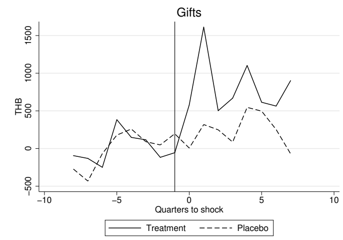
Note: The Figure plots means of average monthly consumption, savings, cash holdings, and incoming gifts for the four quarters preceding and following the shock. All variables are normalized with respect to the pre-shock mean. Period $\tau = -1$ denotes the quarter preceding the sharp increase in health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



(a) Total consumption

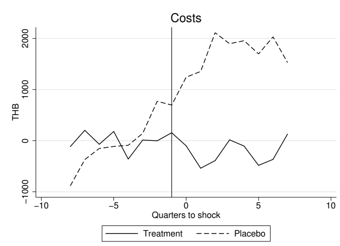


(b) Cash in hand

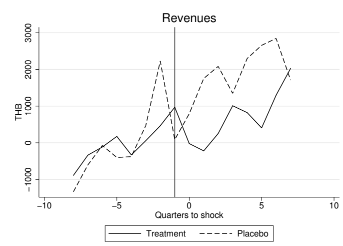


(c) Incoming gifts/transfers

(d) Hired labor (hrs/month)



(e) Input spending



(f) Revenues

Figure A7: Changes on household outcomes before and after the shock - Using only placebo shocks that happen before the actual 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 quarter preceding the sharp increase in health spending. Total consumption spending includes health spending. Savings is computed by subtracting total income from total spending. Revenues include income streams from all household enterprises and exclude earnings from providing wage labor to other households.

Table A6: Robustness to alternative placebo groups

Panel A: Cohorts based on household head's age at baseline

	(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 X Treatment	381.6***	637.5*	0.0139**	0.0195**	449.4***	-1047.5*	-6.564	-15.22**	-1207.9*
	(53.51)	(326.6)	(0.00626)	(0.00825)	(158.1)	(579.2)	(4.189)	(6.559)	(677.9)
Baseline mean (DV)	147.3	6001.7	0.0228	0.0286	2421.8	7526.1	15.32	141.1	15093.4
Observations	44910	44910	44910	44910	44910	44910	44909	44909	44910
Number of households	489	489	489	489	489	489	489	489	489
R-Squared	0.180	0.164	0.227	0.162	0.236	0.786	0.692	0.723	0.606

Panel B: Randomly assigned placebo shocks

	Spending		Gifts (in village)		Gifts (total)	Production			
	Health	Total	Prob.	Count		Costs	Hired Labor	HH Labor	Revenues
Post X Treatment	419.0***	870.8***	0.0141***	0.0176***	571.9***	-903.6**	-5.213*	-18.63***	-1751.5***
	(52.01)	(306.8)	(0.00425)	(0.00648)	(149.5)	(387.8)	(2.901)	(5.437)	(660.2)
Baseline mean (DV)	181.7	6204.7	0.0212	0.0277	2513.4	7395.9	15.53	137.9	14845.7
Observations	46177	46177	46177	46177	46177	46177	46176	46176	46177
Number of households	505	505	505	505	505	505	505	505	505
R-Squared	0.153	0.179	0.198	0.152	0.255	0.795	0.774	0.738	0.617

Panel C: Using only households who suffer the shock in the future as placebo households

	Spending		Gifts (in village)		Gifts (total)	Production			
	Health	Total	Prob.	Count		Costs	Hired Labor	HH Labor	Revenues
Post X Treatment	460.4***	664.5*	0.0119*	0.0161*	684.2***	-2095.9**	-20.03*	-14.36*	-2058.0*
	(76.64)	(352.8)	(0.00632)	(0.00844)	(207.9)	(883.1)	(10.55)	(8.465)	(1093.0)
Baseline mean (DV)	149.2	5443.7	0.0180	0.0238	1969.6	7344.4	16.89	149.4	14626.5
Observations	24447	24447	24447	24447	24447	24447	24446	24446	24447
Number of households	244	244	244	244	244	244	244	244	244
R-Squared	0.205	0.283	0.225	0.174	0.260	0.807	0.639	0.767	0.678

Panel D: Panel regression with household and month fixed effects

	Spending		Gifts (in village)		Gifts (total)	Production			
	Health	Total	Prob.	Count		Costs	Hired Labor	HH Labor	Revenues
Post	1073.8***	1651.7***	0.0135***	0.0247***	719.2***	-524.3	-1.494	-18.33***	-733.5
	(126.4)	(327.7)	(0.00437)	(0.00757)	(197.6)	(338.0)	(2.183)	(4.477)	(691.9)
Baseline mean (DV)	168.3	6228.9	0.0191	0.0246	2421.1	7629.8	17.12	141.8	15378.5
Observations	22841	22841	22841	22841	22841	22841	22839	22839	22841
Number of households	501	501	501	501	501	501	501	501	501
R-Squared	0.181	0.297	0.288	0.221	0.396	0.835	0.804	0.854	0.695

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

Note: The table reports difference-in-differences estimates corresponding to equation (2). 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: Effects of indirect shocks on gift receipt

Panel A: Sales Networks by different subsamples												
	All households			**Excluding simultaneous shocks**			**Large shocks (above median)**			**Case study**		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES	Any transfer	# of transfers	Transfers+Loans	Any transfer	# of transfers	Transfers+Loans	Any transfer	# of transfers	Transfers+Loans	Any transfer	# of transfers	Transfers+Loans
Post X Closeness (sales network)	0.002	-0.003	-428.803**	0.003	-0.003	-530.164*	-0.009	-0.015	-670.885*	0.004	-0.005	623.950
	(0.004)	(0.005)	(178.719)	(0.009)	(0.010)	(270.454)	(0.011)	(0.012)	(387.713)	(0.011)	(0.015)	(584.485)
Observations	421,224	421,224	421,224	187,240	187,240	187,240	91,895	91,895	91,895	22,178	22,178	22,178
R-squared	0.156	0.108	0.102	0.186	0.126	0.135	0.219	0.135	0.132	0.317	0.202	0.129
Pre-period mean	0.0224	0.0284	2965	0.0211	0.0267	2757	0.0244	0.0318	3081	0.0233	0.0309	3194
Number of events	287	287	287	286	286	286	133	133	133	16	16	16
Panel B: Labor-market networks by different subsamples												
	All households			**Excluding simultaneous shocks**			**Large shocks (above median)**			**Case study**		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES	Any transfer	# of transfers	Transfers+Loans	Any transfer	# of transfers	Transfers+Loans	Any transfer	# of transfers	Transfers+Loans	Any transfer	# of transfers	Transfers+Loans
Post X Closeness (labor-market network)	0.004	0.002	-232.599	0.009	0.005	-239.937	0.001	-0.005	-287.304	0.002	0.006	1,031.800
	(0.003)	(0.004)	(172.376)	(0.009)	(0.010)	(248.596)	(0.008)	(0.010)	(426.156)	(0.007)	(0.012)	(780.087)
Observations	353,720	353,720	353,720	154,677	154,677	154,677	70,114	70,114	70,114	22,178	22,178	22,178
R-squared	0.138	0.103	0.100	0.171	0.126	0.130	0.209	0.138	0.121	0.297	0.205	0.122
Pre-period mean	0.0212	0.0265	2911	0.0198	0.0248	2709	0.0224	0.0290	3045	0.0232	0.0292	3074
Number of events	241	241	241	240	240	240	100	100	100	16	16	16

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

Note: The Table presents estimates of the indirect effect of the idiosyncratic health shocks following the specification from equation 5, using different estimating sub-samples. Columns (1) to (3) use all households in the estimation. Columns (4) to (6) exclude observations from households that experienced a health shock within 24 months of a shock to other households. Columns (7) to (9) use only the largest shocks (total health spending above median) for estimation. Columns (10) to (12) report results associated to the largest shock to central households in each village (case study). Gifts+Loans: total amount of transfers from other households or the government plus total new credit. Each regression controls for household, event, village-year and month fixed effects as well as time-varying demographic characteristics. Panel A includes events corresponding to shocks to households who traded in the local supply chain network during the year preceding the shock. Panel B includes events corresponding to shocks to households who traded and hired locally during the year preceding each shock. Standard errors are two-way clustered at the household (i) and event (j) level.

Table A8: Robustness: Indirect effects of health shocks on revenues by sector and sub-samples

	All households						Large shocks (above median)						Case Study					
Panel A : Sales networks																		
	Farm		Off-farm		Wage Labor		Farm		Off-farm		Wage Labor		Farm		Off-farm		Wage Labor	
	Revenues	Costs	Revenues	Costs	Revenues	Costs	Revenues	Costs	Revenues	Costs	Revenues	Costs	Revenues	Costs	Revenues	Costs	Revenues	Costs
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
Post X Closeness (Sales Network)	664***	197***	285.420	-35.398	-49.289	11.033	1,211.188***	329.250***	-1,622.795	-1,279.052	-94.501	-14.243	2,318.103**	628.570**	-4,694.193*	-4,615.269*	597.319	68.417
	(203.863)	(62.257)	(584.244)	(524.289)	(149.426)	(21.576)	(288.371)	(106.078)	(1,607.472)	(1,455.524)	(330.613)	(43.013)	(902.338)	(255.503)	(2,428.239)	(2,405.197)	(485.649)	(70.201)
Observations	421,224	421,224	421,224	421,224	421,224	421,224	91,895	91,895	91,895	91,895	91,895	91,895	22,178	22,178	22,178	22,178	22,178	22,178
R-squared	0.296	0.301	0.768	0.790	0.713	0.438	0.303	0.325	0.822	0.839	0.680	0.473	0.297	0.304	0.806	0.827	0.815	0.643
Pre-period mean	4635	1152	5787	4622	3562	216.9	5492	1346	7150	5829	4747	236.4	5964	1613	6227	5026	4041	272.4
Number of events	287	287	287	287	287	287	133	133	133	133	133	133	16	16	16	16	16	16
Panel B: Labor-market networks																		
	All households						Large shocks (above median)						Case Study					
	Farm		Off-farm		Wage Labor		Farm		Off-farm		Wage Labor		Farm		Off-farm		Wage Labor	
	Revenues	Costs	Revenues	Costs	Revenues	Costs	Revenues	Costs	Revenues	Costs	Revenues	Costs	Revenues	Costs	Revenues	Costs	Revenues	Costs
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
Post X Closeness (Labor-market Network)	950.445***	326.254***	-757.004**	-305.530	-162.244	-36.311*	1,151.294***	280.008**	-816.979	-750.100	-547.469*	-27.546	1,469.603**	633.945***	-1,982.330*	-1,074.487	-39.212	-58.978
	(164.255)	(50.527)	(309.577)	(245.540)	(114.084)	(18.699)	(382.423)	(130.871)	(728.621)	(835.885)	(319.701)	(34.027)	(636.047)	(206.792)	(999.096)	(822.932)	(258.467)	(90.289)
Observations	353,720	353,720	353,720	353,720	353,720	353,720	70,114	70,114	70,114	70,114	70,114	70,114	22,178	22,178	22,178	22,178	22,178	22,178
R-squared	0.309	0.314	0.767	0.787	0.726	0.435	0.313	0.338	0.837	0.855	0.700	0.442	0.270	0.263	0.811	0.820	0.816	0.665
Pre-period mean	4331	1066	5397	4329	3121	197.7	5590	1370	6862	5606	4290	211	4640	1158	5997	4932	4004	256.7
Number of events	241	241	241	241	241	241	100	100	100	100	100	100	16	16	16	16	16	16

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

Note: The Table presents estimates of the indirect effect of the idiosyncratic health shocks on revenues and production costs following the specification from equation 5, using different estimating sub-samples. Columns (1) to (6) use all households in the estimation. Columns (7) to (12) use only the largest shocks (total health spending above median) for estimation. Columns (13) to (18) report results associated to the largest shock to central households in each village (case study). Farm businesses include agricultural, livestock and shrimping activities. Off-farm activities exclude the provision of wage labor to other households or firms. Each regression controls for household, event, village-year and month fixed effects as well as time-varying demographic characteristics. Panel A includes events corresponding to shocks to households who traded in the local supply chain network during the year preceding the shock. Panel B includes events corresponding to shocks to households who traded and hired locally during the year preceding each shock. Standard errors are two-way clustered at the household (i) and event (j) level.

Table A9: Indirect effects of health shocks on gift/transfers to other households

Panel A: Sales Networks				
	(1)	(2)	(3)	(4)
VARIABLES	Any transfer	# of transfers	Amount transfers	Transfers+Loans
Post X Closeness (sales network)	-0.005	-0.011*	-61.130	-61.393
	(0.003)	(0.006)	(76.433)	(84.756)
Observations	187,240	187,240	187,240	187,240
R-squared	0.138	0.163	0.325	0.247
Pre-period mean	0.0216	0.0251	869	1002
Number of events	286	286	286	286
Panel B: Labor-market Networks				
	(1)	(2)	(3)	(4)
VARIABLES	Any transfer	# of transfers	Amount transfers	Transfers+Loans
Post X Closeness (labor-market network)	-0.008**	-0.010***	-89.421	-82.883
	(0.003)	(0.004)	(76.744)	(91.600)
Observations	154,677	154,677	154,677	154,677
R-squared	0.140	0.160	0.312	0.235
Pre-period mean	0.0208	0.0242	799.8	939.2
Number of events	240	240	240	240

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

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. Each regression controls for household, event, village-year 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. Panel A includes events corresponding to shocks to households who traded in the local supply chain network during the year preceding the shock. Panel B includes events corresponding to shocks to households who traded and hired locally during the year preceding each shock. Standard errors are two-way clustered at the household (i) and event (j) level.

Table A10: Propagation of shocks by type of connection to shocked households

Panel A: Propagation by type of exposure (Sales Networks)

	(1)	(2)	(3)	(4)
	Purchases (in village)		Inventories	ITR
VARIABLES	Prob.	#		
Post X Closeness (sales to shocked household)	-0.018	-0.031	33,735.775***	-0.039*
	(0.015)	(0.049)	(11,794.491)	(0.020)
Post X Closeness (purchases from shocked household)	0.004	-0.009	73.209	-0.016
	(0.009)	(0.030)	(2,967.786)	(0.012)
Observations	187,334	187,334	187,334	187,334
R-squared	0.610	0.786	0.892	0.578
Pre-period mean	0.113	0.304	104916	0.110
Number of events	286	286	286	286

Panel B: Propagation by type of exposure (Labor-market Networks)

	(1)	(2)	(3)	(4)
	Provision (in village)		Work hours	Earnings
VARIABLES	Prob.	#		
Post X Closeness (labor provision to shocked household)	-0.023**	-0.110***	-21.271**	-295.116*
	(0.009)	(0.033)	(10.116)	(168.737)
Post X Closeness (labor reception from shocked household)	0.013*	0.054**	3.158	-84.464
	(0.007)	(0.025)	(10.303)	(167.261)
Observations	154,771	154,771	154,771	154,771
R-squared	0.255	0.268	0.168	0.757
Pre-period mean	0.0818	0.197	135.1	3073
Number of events	240	240	240	240

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

Note: The Table presents estimates of the indirect effect of the idiosyncratic shocks by closeness to the shocked households in the sales or purchase networks (Panel A), and in the labor provision and hiring networks (Panel B). Each regression controls for household, event, village-year 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. Panel A includes events corresponding to shocks to households who traded in the local supply chain network during the year preceding the shock. Panel B includes events corresponding to shocks to households who traded and hired locally during the year preceding each shock. Standard errors are two-way clustered at the household (i) and event (j) level.

Table A11: Probability of trade (endline) as a function of baseline trade (10 years apart)

Probability of trade (years 13-15)			
	Sales	Labor-Market	Gifts/Loans
No Link (years 1-3)	0.05	0.02	0.02
Link (years 1-3)	0.22	0.13	0.17

Note: The table presents raw probabilities that a pair of household transacts during the last 3 years of the sample (2012-2014) for pairs that reported transacting during the first three years of the sample (1999-2001). The probabilities are computed over a sample of dyads of households included in the survey sample that responded in all 172 monthly waves of the survey.