Action, Function, & Structure: Interpreting Network Effects on Behavior in Rural Malawi

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ESA Working Paper No. 07-12

February 2007

Agricultural Development Economics Division

The Food and Agriculture Organization
of the United Nations

www.fao.org/es/esa

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Abstract

A long series of ethnographic and sociological studies on kinship systems and information flows in developing societies has portrayed networks as varying structurally, serving multiple functions, and expressing themselves in different types of interaction. Little of this earlier work has informed empirical research in demography or development-related research. In stead, the latter operationalize social networks in relatively narrow ways, allowing for little overlap between multiple networks, and focusing on a subset of potential causal mechanisms. In an effort to pull the empirical literature closer to its qualitative forbearer, we use data from the Malawi Diffusion and Ideation Change Project to test how conversation networks and transfer networks overlap. We offer some predictions regarding how these overlapping networks might individually or jointly influence distinct outcome including ownership of livestock, planning innovative crops and HIV testing. Our sample of women from Malawi, interviewed in 3 rounds across a 6-year period, also enables us to question the inter-temporal stability of network effects. Our findings highlight: (a) how networks based on different actions appear nonetheless consistent with diverse behavioral outcomes; (b) how there is relatively little overlap between conversational and transfer networks; and (c) how there is considerable instability in temporal effects of conversational networks.

Key Words: Agricultural innovation, social networks, risk diversification, HIV testing.
JEL: J1, O1, O3, D8, D85.

Acknowledgements: Paper prepared for presentation at the FAO-sponsored workshop on “Migration, Transfers and Household Economic Decision Making”, January 11-12, 2007, in Rome, Italy. The data used in this article were collected with the support of the following grants: NIH/NICHD RO1-HD372-276 and RO1-HD41713 to Susan Watkins; Population Aging Research Center (PARC), University of Pennsylvania, National Institute of Aging (NIA) PARC Core Grant Pilot Study Project on Inter-Generational Resource Flows in a Poor Sub-Saharan Population, to Susan Watkins and Jere Behrman; NIH Center for AIDS Research, University of Pennsylvania, Pilot and Feasibility Grant Program, to Watkins and Behrman. We thank Flore Gubert, Paul Winters and FAO Workshop participants for helpful comments and suggestions.

SECTION 1. Introduction

Over the last few decades, interest in the relationship between social networks and behavior, often referred to as "social network effects," has diffused from sociology—the original disciplinary home—to sister-disciplines. The attractions of social network approaches seem fairly obvious. They claim to reduce the "asocial" nature of classical empirical models, where the latter are perceived as inadequately explaining or explaining away, observed behavior across a range of activities. More specifically, such approaches allow researchers to show how, given pronounced informational constraints in developing countries—peoples' infrequent exposure to trustworthy newspapers, radio, TV the internet, libraries, etc.—individuals come to rely on information from those they talk to. Alternatively, social network approaches shed light on the resistance that many developing country residents appear to have to innovative (non-traditional) behaviors, whether because those residents see some moral danger in those behaviors, or because of the financial risks inherent in living in a poor society without formal insurance mechanisms.

A key problem in the social networks literature has been the development of two distinct spheres of research activity, one broadly associated with sociology (and demography), the other with economics, including work by non-economists. Each builds on the recognition that individuals operate within a variety of different types of networks. Each also acknowledges what Udry and Conley (2004) describe as “functional networks” –that discrete networks may have discrete functions . The problem is that differential disciplinary cultures have emerged with respect to how networks are "operationalized"—that is, how analysts should translate the general axiom that networks affect behavior into measurable variables.

Demographers and sociologists writing on developing societies, for example, have tended to explore social networks through the lens of "conversational networks," where the analytic orientation is toward assessing the impact on behavior of the flow of information ("social learning effects"—Montgomery and Casterline 1996:153) and the flow of moral messages ("social influence" effects—ibid: 155). The associated empirical literature has been largely limited to questions about family planning or AIDS (Entwisle, Rindfuss, et al 1997; Kohler 1997; Kohler, Behrman and Watkins 2001; Buhler and Kohler 2003; Helleringer and Kohler 2005; Helleringer 2006).[1]

Economists and developing country-focused family sociologists, on the other hand, have tended to focus more on risk sharing networks, and how transfers enable risk sharing for poor households in situations where formal insurance

[1] There is also an increasing body of work on the effects of sexual networks on the spread of HIV (e.g., Morris and Kretzschmar 1997; Handcock and Jones 2003; Morris 2004). We do not review this here since we consider it to be a quite distinct literature. That is, although sexual networks likely fall under the umbrella of conversational networks and in some cases transfer networks (Poulin 2006), the stochastics of viral transmission for given sexual contact, and their effects on aggregate disease spread, raise a very different set of substantive issues than the transmission of information or some other material resource. The latter are our focus here.

markets are missing or incomplete (Platteau 1991; Deaton 1992; Coate and Ravallion 1993;(Deaton 1992; Fafchamps and Gubert 2006; Ligon, Thomas, and Worrall 2002; Townsend 1994). This body of work builds on a long series of observations by non-economists, from early exchange theorists in both anthropology and sociology (Mauss 1925/1967; Malinowski 1922; Homans 1958; Blau 1964) to more recent studies of the structure of extended family transfer systems in general (Stecklov 2002; Weinreb 2006), the nature of risk-sharing among the LDC elderly (e.g., Adamchak et al 1991; Apt 1993; Brouwer, Hoorweg and Van Liere 1997; Burman 1996; Cattel 1997; Hoddinott 1992; Mitchell 1956; Peil 1991; Siqwano-Ndulo 1998; Weinreb 2002) and among those directly and indirectly affected by AIDS (e.g., Kabila and Anderson 1993; Seeley et al 1993; Urassa et al 1997; Ntozi et al 1999). Either way, the general point is that the focus on individual and household survival through risk sharing has inevitably oriented economists and family sociologists toward an operationalization of network-related hypotheses through transfers-related behaviors. This is true even though in some cases the transfers are "assumed" to be the mechanism underlying correlated consumption patterns rather than specifically measured and identified in the data.

Historically, there has been very little crossover between these literatures on the potential behavioral effects of social learning, social influence, and risk sharing, particularly in the empirical literature.[2] This has important implications. First, and as already implied, although there are clear structural commonalities between the two general types of network approaches, the differential disciplinary focus means that analyses have been more likely to privilege one type of network explanation over another. That is, it is often unclear how one should distinguish between social learning, social influence, and risk sharing effects where, for example, these three functions are embodied in a unitary mass of "network partners" who share common interests, attendance at weekly religious meetings, kinship ties that carry certain financial obligations, frequent conversations at the local water-pump or maize mill, and so on. More specifically, as we discuss below, although there are standard (but not unproblematic) ways in which sociologists and demographers have come to differentiate *social learning* from *social influence* effects, there are no established ways of which we are aware for distinguishing social influence from risk-sharing/insurance effects. The result is that, in relation to a given variable of interest, it makes it difficult to make a clear causal case for one over the other when one is talking about observed effects on behavior, though the two effects are conceptually quite distinct, and have equally distinct implications for the design of public policy.

A second problem arises with differences in key behaviors of interest (which is linked to distinct disciplinary cultures but not a necessary result of those

[2] Recently, this has begun to change. A number of studies have utilized rich network data from Ghana to examine how behaviors related to the adoption of new technologies are influenced by networks. Conley and Udry (2003), for example, focus on the powerful social learning effects of networks on the decision to adopt a new technology. And Udry and Conley (2004) identify several distinct types of networks including labor, information, land and finance, and seek to understand both the degree of overlap between these different networks and their individual roles in affecting behavior.

distinctions). Whatever the behavioral outcome, we can expect the three causal mechanisms associated with networks to operate either jointly or independently. Equally, however, we can also expect that different types of behaviors, even different types of investment behaviors, are likely to draw on different types of network effects. In other words, the relative weight of social learning, social influence, and risk sharing, will vary across different types of behavior. As a result, in evaluating a farmer's adoption of a new seed, his/her decision to sell livestock, the labor migration of a member of his/her household, his/her decision to get an HIV test, or a host of other decisions, we should expect to find a different hierarchy in the observed effects of social learning, social influence, and risk-sharing (we describe this in greater detail below).[3] Given these shifting explanatory weights, analyses which privilege one of these at the expense of the other *do not appear to us to be an effective way to test general hypotheses about social network effects*. Nor may they be fully valid tests of that specific behavior.

Third, and by extension, disciplinary differences in the types of networks that researchers of different stripes choose to focus on, may make – or be based on – implicit assumptions about the timing of network effects on given types of behavior. In relation to some types of behavior, for example, we can expect a lagged effect of social leaning, social influence or risk-sharing; in others, a more immediate effect. Either way, ignoring the temporal nature of networks may be problematic.

Our key aim in this paper is to examine the joint relationship between each of the three core causal mechanisms of social networks—social learning, social influence, and risk sharing—and three selected behavioral outcomes, while grappling with the three problems introduced thus far. The paper has four core sections. We begin by more explicitly conceptualizing the relationship between the three basic network functions, allowing us to develop a simple informal model that relates the different network types to behavioral decisions in LDCs. In the second, we describe the longitudinal Malawian dataset which we use to explore these ideas, both in terms of its unique network-rich attributes and its constraints. Finally, we present a series of analyses, examine the results, and discuss the implications of our findings.

SECTION 2. Multiple networks and their behavioral impacts

People circulate in, and can be connected to each other through, multiple networks with quite different structures, functions, and types of social action/interaction. Some network *structures*, for example, may be based on kinship ties, agnatic or affinal. Others may be based on formal membership in a different type of group, such as a church, ROSCO, local development group, or football club. Others, yet, may be premised on residential patterns and the resultant affiliation with institutions associated with that community (e.g., close

[3] To generate a full causal story one could also plausibly argue that none of these decisions are independent. For now, however, we choose to ignore that added layer of complexity.

familiarity with, and developing interdependency on, a small cluster of immediate neighbors, a local school, a particular agricultural extension officer, and so on). Similarly, social *action/interaction* associated with networks refers to the way that, in terms of day-to-day activities, people *express their connections to each other*. Examples of these expressive actions include conversation, gifting and exchange (respectively, 1- and 2-way transfers), sex, the collective performance of meaningful rituals, and so on.

In addressing the potential behavioral effects of networks, the combination of membership in multiple networks—sometimes referred to as "network multiplexity"—and the recognition that a given network connection often expresses itself in different types of social action, has important theoretical and empirical implications. The reason is that it explicitly recognizes the fact that networks are based on activities that may generate important social, economic and health benefits (or costs) that are related *or unrelated* to the specific activities and *intended function* of the network, in particular where we – outsider researchers and analysts – identify that function. For example, marriage patterns in some regions of India expand kinship networks across long distances. One function of such "structures" is that households can diversify their income stream and reduce risk (Rosenzweig and Stark 1989). Undoubtedly, the same structure created by the actions of marriage may provide many opportunities for conversations in which social learning and influence occur. (Arguably, there may well be a relationship between the value of transfers between two given kin members, and their respective level of social influence on each other). Along the same vein, in addition to the flow of information, conversational networks occurring around the local market can also index friendship and support networks. In other words, the diverse types of social action through which people express their connections to each other create an overlapping set of functions for any given type of network, including risk-sharing, social learning, and social influence.[4]

Understanding the overlap between, and potentially multiple functions of, different types of networks, goes to the heart of our discussion and analysis. In particular, and as implied by our prior discussion, three general types of social network functions—each indexing a discrete causal mechanisms vis-à-vis behavior—are of primary importance here: social learning, social influence, and risk sharing/insurance networks. We now define these conceptually (later we discuss how to operationalize them in models).

As conceived by Montgomery and Casterline (1996:153), for any given individual, social learning refers to the communication of an "information set . . . which summarizes his or her knowledge of all factors that might bear on decisions." The implication is that these pieces of information come from other people, whether

[4] This distinction between a network's function(s) and the actions that social actors use to express their relationship with someone in their network, makes no assumptions regarding the directionality of the relationship.

through active social interaction, or through more passive exposure to various types of media. In the most general sense, then, social learning *networks* characterize any type of relationship through which information flows from one individual to another. It is the primary network mechanism underlying the diffusion of ideas and technology, and we can reasonably assume that its effect on behavior is monotonic if not linear. [5]

Finally, risk-sharing/insurance networks refer to a particular network function through which households share risk and provide mutual support against illness, shocks, and other events that affect welfare. Risk-sharing arrangements in developing countries are a necessary response to the lack of formal insurance and pension markets, and while they may not function perfectly, they may offer some insurance for households to smooth consumption.

Our focus is on two types of network structures which embody three main functions and a number of different types of observed expressive action (as defined above). The first structure, conversational networks, ***cn***, describe people that are assumed to be centrally located in a person's social network, because respondents name them as people with whom they talk about one of two important subjects. As we show below, it is from these ***cn*** data that we extract our measures of social learning and social influence. The second type of network is that in which the core observed action is "transfers," or ***tn.*** Specifically, the ***tn*** defines the people on whom an individual can rely for different types of support (e.g., monetary assistance, other materials, time), or the people to whom an individual would provide similar types of support. Here, too, there is likely to be both overlap and difference, both between potential givers and receivers in the transfer network, and between the different types of help than can be given or received. The transfer network then carries out many functions including one that has been proposed, insurance. Transfer networks are indexed by tn_1 and tn_2.

Figure 1 about here

In Figure 1 we illustrate how the various social networks, whose underlying function is defined by the basic actions, can be used to examine network relationships and eventually network impacts. As implied by the relative sizes of ***cn*** and ***tn*** in Figure 1, it seems reasonable to assume that the total conversational network tends to be larger than the transfer network. Similarly, it seems reasonable to assume that there is some overlap between these two types of networks, and that this overlap will be positively correlated with the level of "familism" in a given setting. By this we refer to the extent to which day-to-day action (including interaction)—inevitably involving *both meaningful conversations and mutual assistance*—tends to occur around members of one's nuclear and

[5] Information effects include social influence effects which refer to a more general set of social factors such as "the desire to avoid conflict within groups" (Montgomery and Casterline 1996: 155), power-related factors, the impact of deviants, and so on. Thus, social influence networks deal less with information in its narrow sense than with the meta- or moral message that may be communicated simultaneously with the information—in particular, what is legitimate, moral practice.

extended family (natal and affinal). This area is indexed by ***cntn***. Ultimately, the relative sizes of ***cn*** and ***tn*** and their degree of overlap are both open to empirical investigation.[6]

Both ***cn*** and ***tn*** (and ***z***, see note 1) can generate a “social network effect” insofar as they signal how some dimension of social interaction affects someone's preferences, attitudes, and behavior independent of that person's own attributes (schooling, wealth, residence, and so on).

What complicates researchers' analytic endeavor is that data collection resources, including the respondent’s time, are limited, meaning that researchers typically collect data about only a subset of network types ***cn*** or ***tn***. Net of the measurement issues already mentioned, this raises a number of concerns.

Assume, for example, that the total social network characteristics of a given individual *i*, net of *c*, and following the discussion above, is

$$SN_i' = cn_i' + tn_i'$$

where

$$cn_i' = cn_{i1} + cn_{i2} + cn_{i3} \ldots + cn_{in} \text{ , and}$$

$$tn_i' = tn_{i1} + tn_{i2} + tn_{i3} \ldots + tn_{in}$$

where numeric subscripts 1,2, 3 . . . n refer to subtypes of network actions and effects ***cn*** and ***tn***.

Actual analysis of social networks effects rarely come close to SN_i'. Rather, they use a partial measure (*sn*), such as

$$sn_i = cn_{i1} + cn_{i2}$$

The difference between the underlying construct SN_i' and the actual indicator sn_i seems substantively important since it includes all of $cn_{i3} \ldots + cn_{in}$ and tn_i'. This raised a number of questions about the scope of observable network effects.

First, say, for example, that a research project only collected data on a conversational network related to "issue 1", that is, cn_{i1}, but researchers develop an interest in "issue 2." Is there sufficient overlap between cn_{i1} and an unobserved cn_{i2} that they can productively use data on cn_{i1} in order to explore network effects related to "issue 2"? In other words, is there sufficient "discursive proximity" between cn_{i1} and cn_{i2} that interactants draw on the same overall conceptual domain and, therefore, inhabit the same network space (in terms of Figure 1, the degree of discursive proximity would be positively correlated with

[6] Beyond conversational and transfer networks, one can also envisage a multitude of other types of network effects playing a role in affecting behavior. These are collectively indexed by Z which includes the entire space of social interaction. Thus, Z includes interactional networks specific to certain domains such as sexual networks or business networks. This third type also includes more general types of interactional effects which are not usually considered network effects, but in our view should be since it indexes the area in which a given individual circulates, engaging in an unceasing series of "interaction rituals" (Collins 2004), some of which escalate into areas cn and tn, but most of which do not. Note that from here on, notwithstanding its conceptual—and potentially empirical—importance as a type of social interaction effect, we do not directly address it. The main reason is that we have no direct way of measuring it.

the relative size of the shared cn_1/cn_2 area). Moreover, is there an advantage to using cn_{i1} to explore network effects related to issue 2 in order to avoid endogeneity issues related to network-selection? Presumably some of the same measures would be available, including all personal attributes of the network partners, and the density of their network.

Second, and related, is there sufficient overlap between cn_i' and tn_i' that, given data on both, a well-chosen $cn_{i1} + cn_{i2}$ can effectively capture the total network effects. In other words, do conversational networks capture a substantial enough portion of the social learning and social influence effects which are an inherent part of insurance networks? Or can we empirically identify two distinct types of social learning and social influence effects, one related to the transmission of moral messages, the other to engagement in a transfer network?

Both of these questions relate to the conceptualization and operationalization of networks. Our main aim here is to explore how this conceptualization of multiple networks and their functions affects behavioral outcomes. Behaviors of different sorts are expected to be affected differently by different types of networks because each combination of behavior and type of network offers a different balance of incentives and disincentives. For example, behavior that is heavily responsive to financial risk can be expected to respond stronger to changes in the transfer dimension of networks. Behavior that depends on information flow is likely to depend on the social learning dimension of networks. Behavior that is morally risky is likely to be more sensitive to social influence dimensions of networks. This concern with identifying the particular effects of each network is shared with, and motivated by, Udry and Conley (2004).

We address these expected differences empirically in this paper by choosing three distinct behavioral outcomes: 1) current value of livestock holdings; 2) whether an innovative crop has been planted in the past year; and 3) whether an HIV test has been taken to date. Each of these, we assume, has a different relationship to both observed ***cn*** and ***tn***, allowing us to test general hypothesis about the effect of overlapping networks, and more specific hypotheses about the relative importance of information versus insurance mechanisms underlying such effects. In addition, our measures of ***cn*** allow us to separate out social influence from social learning, providing insight into the main function of observed networks. Furthermore, since each of the outcomes we examine is important in its own right the analysis as a whole offers insight into how rural Malawian households make a range of human and physical capital investment decisions.

Before providing more specific information on our expectations vis-à-vis these three outcomes, and describing them in more detail, we introduce the data.

SECTION 3. Data and the selection of outcome variables

i. Data

We explore these ideas empirically using data from the Malawi Diffusion and Ideation Change Project (MDICP), an ongoing longitudinal research project conducted in three rural areas of Malawi.

Malawi is an appropriate setting for this research for a number of reasons. First, it is a poor country, with a GNI PPP (per capita) of 620 USD, relative to an East Africa average of 1,020 USD, and an HIV prevalence approaching 14 percent of adults (PRB 2005). Each of these has likely implications for the structure and role of informal insurance systems. Indeed, both historical and contemporary sources point to the importance of these systems (Frazer 1914, Young 1932/1970; Mitchell 1956; Hirschman 1990; Mtika 2000; Anders 2002).

Second, since Malawi achieved independence in 1963, it has been spared the civil wars—and, until recently, any extreme manifestation of ethnic tension (Kaspin 1995)—which have affected, at one time or another, most of its neighbors, and many other countries in sub-Saharan Africa. This relative political stability has also likely had implications for network composition and structure, since it has allowed for the relative freedom of movement from an area associated with one's own ethnic group into another area, with consequences for patterns of interaction and the transmission of information.

Third, since the end of Malawi's one-party era in 1993/4 (associated with the rule of Kamuzu Banda's Malawi Congress Party), both state and society have undergone a series of transformations (see collected papers in Englund 2002). Over the course of the MDICP fieldwork we have (anecdotally) noticed rapid changes in the range of crops planted and available for sale in local markets, and (empirically observed) changes in the acceptability of family planning and other types of health-related behavior. As these are innovations, and overlap with the three outcomes discussed below, understanding changes in them goes to the heart of understanding the behavioral effects of social networks.

Sampling and the initial wave of the MDICP survey data collection was conducted in 1998. Roughly 1500 ever-married women were interviewed. Follow-up surveys were fielded on the full sample in 2001 and 2004, and data collection has just been completed on a 4th round, though we do not use those data here.

While various aspects of the MDICP survey instrument have changed over the waves, its network dimension has remained relatively stable. That is, in all three rounds, data have been collected about two types of ego-centric conversational networks. The first describes up to four people with whom individuals have talked about AIDS. The second describes up to four people with whom individuals have talked about family planning (in MDICP-1 and MDICP-2) and religion (MDICP-3 and MDICP-4). In each case, where a respondent mentioned having talked to someone about one of these issues, she was asked a series of follow-up questions about that person. At the end of the questionnaire, she was then asked how well each of her named network partners knew each other.

Aggregating these follow-up questions over a complete network allows us to construct several measures which describe the attributes of a person's conversational network. These include the overall size of the conversational network; the proportion of the conversational network that is not part of the respondent's nuclear family; the proportion of the conversational network that resides in the respondent's own village ; the proportion of the network with no schooling whatsoever, as opposed to the proportion with primary, secondary, or higher; and the density of the network, meaning the extent to which the named network partners are reported to be each other's confidants as well.

Each of these variables can be estimated for each of the two conversational networks referred to in each survey wave, and across the three waves, allowing us to compare the characteristics of two conversational networks cross-sectionally and across time. In addition, we can also estimate, though only cross-sectionally: the compositional overlap between the two different types of conversational networks: ie. the proportion of individual network partners (NWPs) who are mentioned in one conversational network, but who also feature in another.

Data on transfer networks have also been collected by the MDICP, though not in such detail. In the 2004 survey people were asked a series of questions about who they considered the most important donor (to them) of monetary assistance, material assistance, informal assistance, and overall, and who was the most important recipient of their assistance (same categories). The answers were coded into four main categories—no reported transfers; member of one's nuclear family; member of the extended family; non-kin—eventually collapsing the final two after seeing that they were statistically indistinguishable in all models. Thus, the 2004 transfer data don’t allow us to evaluate the frequency and value of transfers, or the likelihood of transfers between the respondent and a given type of relative or friend. They do allow us, however, to distinguish zero-transfer networks from those which are focused on close kin to those focused on more distant kin or non-kin. As our main analytic emphasis is on the overlap between different network structures and functions, this distinction represents a key advantage over alternative formulations.

ii. Our outcome variables and specific hypotheses

As mentioned above, we explore the extent to which different types of networks have variable ***cn*** and ***tn*** effects—allowing us to test our general hypothesis about the effect of overlapping networks, and more specific hypotheses about the relative importance of information versus insurance mechanisms underlying such effects—across different types of behavior using three distinct behavioral outcomes. Here we lay out our specific expectations, summarizing them in Table 1.

Table 1 about here

Value of livestock holdings provide a baseline transfer network model for two reasons. First, because the MDICP does not include network information specific to livestock (e.g., we did not list individuals with whom the respondents had "chatted about livestock") this model explores the effects of an unrelated conversational network on some behavior of interest, as described above (problem 1). Moreover, we assume that there is quite low "discursive proximity" between the AIDS and religion-related conversational networks for which we have information, and the unobserved livestock-related conversational network. Second, because the ownership of livestock has been an important savings and insurance mechanism among African households for generations, we would expect few social learning (cn^{si}) effects in this case even if we had collected information about respondents' livestock-related conversational networks. Rather, because livestock is still a potentially important type of wealth in these rural areas, a change in livestock holdings has implications for the rest of the extended family or transfer network and is likely to have some relationship to insurance networks. Consequently, in this case we would expect to see some effect of transfer network characteristics on livestock holdings but no conversational network effects whatsoever. In short, our expectation is that tn_i = ++ (the double plus ("++") sign in Table 1 signals that we expect a strong relationship, and $cn_i = 0$ (where both $cn^{sl}_i = 0$ and $cn^{si}_i = 0$).

We expect the planting of an innovative crop in the last year to draw on a very different combination of information and transfers-related effects. Specifically, as long acknowledged in the development literature, crop innovation is an inherently risky activity. Prospective innovators therefore need highly specific information about the crop, about its performance in local soil, its labor, water and fertilizer demands, its promised net value, and so on – information that may be gained from networks (Boahene et al 1999). In all these things, the planting of an innovative crop differs from livestock ownership. While, as above, we have no specific information on a "crops-related" conversational network, we assume that the discursive proximity between AIDS/religion and new crops is closer than to AIDS/religion and livestock ownership, since they draw on specific modern types of information, and/or the willingness to talk about those types of issues. In short, we should be able to identify conversational network effects, including social learning effects, but also social influence effects insofar as potential innovators would likely want the moral support of their neighbors and kin. In addition, however, since potential innovators would likely also need some insurance that, in case the new crop fails, they and their dependents will not be left destitute, we should also see some effect of transfer networks. In other words, as depicted in Table 1, we should find strong signs of social learning effects (cn^{sl}_i = ++), a weaker sign of social influence effect (cn^{si}_i = +), and also some sign of transfer networks (tn_i = +).

The third outcome, relating to reporting having undergone an HIV test prior to the survey, should draw on a different set of networks effects than the two prior outcome variables. The main reason is that because people constitute ideas about risk socially, and the decision to have an HIV test is premised on the respondent treating information about HIV—publicly available through posters, education campaigns, etc—as being relevant to them, we expect to find strong conversational network effects, particularly since we have specific information on an AIDS-related conversational network. Moreover, given the specific informational components related to decisions about whether or not to have an HIV test, we expect to find effects of both social learning and social influence. On the other hand, to the extent that (i) people in this setting tend to overestimate their likelihood of infection (Helleringer and Kohler 2005), (ii) the results of an HIV test have some implications for relations among extended family members (e.g., if I find out if I am positive, can I be sure that my extended family will take care of me and then my children?), and (iii) one important determinant of health-related behavior, including testing, appears to be "self-efficacy", we also expect to find a mild effect of transfer networks. In summary, in relation to HIV testing, we expect to find cn^{sl}_i = ++, cn^{sl}_i = ++, and tn_i = + (in other words, the relative strength of the two network structures are $cn_i > tn_i$). In addition, however, because the conversational network data are, in part, built on questions about AIDS networks, we expect to find more powerful *cn* effects than in relation to planting of an innovative crop.

SECTION 4. Empirical approach & analysis

i. Empirical approach

Our analysis is conducted in two distinct stages. The first stage is primarily descriptive and aims to examine the stability of network characteristics across separate conversational domains and time. Consequently, using simple correlational analysis, we look at whether the same person appears in both topics as well as the degree of correlation in the characteristics of the networks, $\boldsymbol{cn}_i$. We also focus on how each type of conversational network, $\boldsymbol{cn}_{i,t}$, is correlated over two time periods.

The second stage of analysis focuses on the effect of conversation and transfer networks on the three behavioural outcomes described above. Our empirical strategy is to use basic cross-sectional regressions to test for the effect of networks of each type on the outcomes, in combination with village-level fixed effects that allow us to control for unobserved heterogeneity between villages. Given the analytic focus on decisions regarding livestock ownership, planting of innovative crops, and HIV testing, this allows us to reduce the effect on the estimates of unobserved community level factors such as land quality, water availability, the availability of local health clinics, and the activities of local health workers and agricultural extension officers, etc., thereby increasing the plausibility of the estimates.[7]

[7] Note that data constraints stop us from using the individual-level fixed-effects that Helleringer and Kohler (2005) bring to bear on these data. The reason is that questions on crop innovation and HIV testing were asked only in the 3rd wave, and the livestock (and network) slightly differed in the 3rd wave from prior waves.

1. Multiple networks and validity

We use two approaches to explore the degree to which conversational networks in one dimension are similar to those in a second dimension. In the first, we estimate the extent to which individual network partners (NWPs)—that is the same individuals—are mentioned in the two types of conversational networks. In the second, we identify the extent to which the same *types* of people are mentioned across the two conversational networks.

i. Overlap of individual NWPs

Of all persons mentioned in the conversational networks from round 1 of the survey, only 14% are mentioned in both types of conversational networks. This figure shows almost no variation across the 3 rounds. Given what we assumed was the considerable "discursive proximity" between family planning and AIDS this is less overlap than we expected. Similarly, the lack of variation over time also surprises us since we assumed that there would be greater discursive proximity between AIDS and family planning than AIDS and religion.[8]

A different statistic is offered by looking within each network type in each round and calculating the proportion of the network composed of people also in the other network types. We find that roughly 30% of the composition of each network is composed of persons also in the "other" network. However, this average varies somewhat across rounds and type of question. Whereas the proportion of persons mentioned in the AIDS and FP networks that are also reported in the other network is almost identical in rounds 1 and 2, this is not the case in round 3. In round 3, we find the proportion of persons in the AIDS network also reported in the religion network is 37%, but only 28% of those mentioned in the religion network are reported in the AIDS network. This discrepancy suggests that people with whom our Malawian respondents speak about religion are not as likely to also be the people with whom they speak about AIDS. In other words, **there is higher discursive proximity between AIDS and family planning than between AIDS and religion**.

ii. Overlap in terms of network characteristics

Another perspective on network overlap is to examine the extent to which the characteristics of the two types of network $\boldsymbol{cn}_i$ overlap, rather than to look at the proportion of members shared by the two network types. We examine each of these within each round of data as shown in Table 2.

Table 2 about here

The strongest correlation is for the proportion of the network with no education. The correlations within each round are between 60-70%. An intermediate group

[8] This validity of our second assumption is questionable on other grounds too. Specifically, where, as appears to be the case (e.g., Trinitapoli and Regnerus 2006), individuals are as likely to frame AIDS in religious terms as in health-related terms, the discursive proximity of AIDS to both religion and family planning is likely to be equal.

of correlations is represented on network size (number), the proportion who are not family members (kin), and the proportion co-resident in the same village (village). These are mostly in the 40-50% range across the 3 rounds. Finally, the lowest correlations—and least stable across the three rounds—are for the network density measures. These fluctuate between 37.4% in round 1, 35% in round 2 and 11.3% in round 3.

Overall, we do not think it surprising to see that the correlations across characteristics are much higher than the correlations of the specific members across the networks because these reported networks are only a sample of the respondents' total network. If we assume that the characteristics of the members of this total network are likely to be correlated, this implies that they are selected into certain types of cn_i on the basic of unobserved characteristics (e.g., a familial experience with AIDS, a known interest in religious issues, and so on). So even when the respondent is populating his specific conversational networks cn_1 and cn_2 with different individuals, **they tend to share outward characteristics**, in particular a given educational profile and, to a somewhat lesser extent, residential and kin characteristics.

iii. Relationship between conversational and transfer networks

We have a much harder time assessing the overlap between the conversation and transfer networks. Our strategy is to test which if any dimensions of the conversational networks explain whether transfers are made at all and, if so, whether they are sent to close kin or to non-close kin or friends. We do this using a simple multinomial regression model where the three outcomes for the dependent variable are 1) no transfer sent, 2) largest transfer to close kin and 3) largest transfer to non-close kin. The omitted category is no transfer. The only explanatory variables included are the conversational network variables because this is not a causal model (we thus could use chi-squared tests of tabulated data instead). We include the five conversational network characteristics. Our analysis is based solely on round 3, since the MFTP data collected between rounds 1 and 2 of the MDICP include only half the MDICP sample.

Our findings are shown in Table 3. The size of conversational networks is associated with the probability *receiving* transfers from close kin (p=0.01) or non-kin (p=0.00) versus not receiving any transfers. Surprisingly, the effect of conversational network size on transfers *sent* to close kin is negative (p=0.04). Thus, the size of conversational networks appears associated with whether a person is engaged in transfers, but the relationship appears positive and stronger for transfers received than for transfers sent.

Table 3 about here

Another strong association is related to the educational composition of the conversational network. We find that the as the proportion of the conversational network with no education rises, probability of transfers from either close or non-close kin versus no transfers falls and both are highly significant (p<0.01). Thus, households with low education conversational networks are less likely to be receiving transfers. In contrast, households with educated conversational networks report sending larger transfers to close kin (p=0.00), but not to non-close kin (p=0.84).

A weaker relationship was also identified in relation to the conversational NWPs' residential characteristics. In particular, the higher the proportion of the network resident in the respondent's village, the less likely the respondent is to have received transfers from either close or non-close kin, relative to zero transfers, though only the latter effect is significant (p=0.02). Somewhat surprisingly, given theories about how and why individuals invest in transfer relationships, there is no clear effect on transfers sent.

One "non-result" is also notable. Although we expected that the proportion of conversational networks composed of non-close kin would be associated with transfers – particularly to or from non-close kin – the observed relationship between the kinship-type of the conversational network and the kinship level of transfers was relatively weak. The only marginally significant relationship suggests that conversational networks composed of large proportions of non-close kin raise the probability that the largest transfers sent are to close kin (p=0.07), relative to zero transfers.

In summary, our overall impression is that **the overlap between these two network dimensions—*cn* and *tn*—is relatively limited**, although the overlap that is identified offers useful insights into the substantive content of each of the networks. This result is not necessarily inconsistent with those reported by Udry and Conley (2004) who note a “substantial” overlap between different networks within a given village. It may well be that focusing on within a single village increases the overlap across network types.

iv. Conversational network correlations over time

Here we discuss results obtained by simple bivariate correlational analysis of five CN measures between 1998 and 2001, 2001 and 2004, and between 1998 and 2004. The results—presented in Table 4—are shown for the combined CN measures as well as separately for AIDS and non-AIDS network measures.

Table 4 about here

There are several rather striking features found in these results. The correlations are overall quite low and never greater than 57%, highlighting the degree of fluidity among conversational network measures over time. However, the degree of fluidity varies considerably by type of CN network characteristic. At the highest end of fluidity (lowest correlation) we find that network density has under 10% correlation and even a negative correlation in some cases. On the other hand, the educational characteristics are much more stable and reach 57%.

When we compare the correlations across conversation types (AIDS versus non-AIDS), we note that the correlations over both adjacent survey rounds are very similar on the AIDS *cn* but much less so on the non-AIDS *cn*. That is, with the exception of the *cn* educational characteristics, the rounds 2-3 correlation on the non-AIDS *cn* is lower than the rounds 1-2 correlation on the same network. This is not surprising given that in rounds 1 and 2 this network focused on conversations related to family planning, but in round 3 it switched to conversations about religion. Consequently the reduction in the correlation highlights the fact that people are involved in different types of networks for different types of conversations, with implications for measurement of overall network effects.

Furthermore, the ranking of correlations for both AIDS and non-AIDS *CN* are similar across the different characteristics of *cn*. The most stable characteristic is the proportion with no education. There is a considerable drop to the next least fluid which is the proportion of non-close kin in the *cn*, followed by the proportion of *cn* in the same village and then the number of persons in the *cn*. The least stable is the density of the *cn* which is actually negative in some cases but generally between 5-10%.

It is interesting to consider these results in light of the earlier correlations we found for the characteristics of networks across conversation topics within each round (Table 2). Based on that comparison we conclude that networks **appear to be more stable across topics of conversation at a given point in time than they are within a topic across time**. This is substantively important and has analytic implications which we discuss below.

2. How multiple networks affect behavioral outcomes

As described above, in order to explore the effects of these partially overlapping but overall different types of networks on behavioral outcomes, we purposively selected three dependent variables which vary on the type of social network effects which we expected to find. Before describing our results, we lay out the specific characteristics of the variables:

1. *Current value of livestock in 2004* – The variable refers to the estimated monetary value in Malawi kwacha of the household's total livestock holdings, including cattle, goats, pigs, chickens and ducks.

2. *Planted innovative crops in the 2003-04 growing season* – We identified crop innovators by combining information collected in both 1998 and 2004. That is, in the 2004 data, individuals were asked which crops they had planted the previous growing season (ie. usually the late fall and winter of 2003). Meanwhile, in a 1998 village-level questionnaire, village headmen were asked about any new crops which had been planted in their villages. Based on this information, we were able to identify region-specific innovators. In Balaka these were respondents who had planted sweet potato. In Mchinji and Rumphi, they were residents who had planted soya. Across the three areas 12-13 percent of households fell into this (binomial) category. Although this

variable is less than ideal[9], we argue that the relatively low prevalence of people using these crops (12-13%) means that not much has changed in the 4-5 years since they were labeled "new." In addition, while the lack of information on seed varieties means that we get some false negatives in the "have-not-planted-new-crops" category, we should get very few false positives in the smaller "*have*-planted-new–crops" category.

3. *Had an HIV test prior to MDIC-3* – This is a simple binomial variable based on the question: "have you ever had an HIV test?"

Note that we make no explicit assumption about how accurate the responses were to any survey question on which these variables are based. As discussed elsewhere (Weinreb 2006), there can be significant and surprising sources of bias across a wide range of questions in developing country surveys. Our working assumption throughout this analysis is that the overall level of response error falls within an acceptable range, though there is some evidence that error is high in relation to AIDS and network questions (Bignami-van Assche, Reniers and Weinreb 2003).

i. Baseline results

Descriptive statistics on the dependent and independent variables in our models are shown in the Appendix. We begin with a set of baseline models in order to examine how sociodemographic controls are related to the three main outcome variables. These include indicators of wealth (a composite measure of durables and housing stock), age, marital status, region of residence, and educational attainment. These controls are important because women will vary in the extent to which they are exposed to sources of information; process the information accordingly; and have need for insurance depending on their financial resources and marital status. Note that we use casewise deletion to facilitate analysis of the impact of each subsequent model on the estimated coefficients (although the main results are substantively similar when we do not employ casewise deletion). Baseline results across the three dependent variables are presented in columns (1), (5), and (9) of Table 5, respectively.

Table 5 about here

The baseline model of livestock ownership (column 1) shows that there is a strong, positive and almost monotonic association between wealth and livestock ownership. The effects of education are also positive as women with higher education levels report greater livestock value than women with no education,

[9] There are two main problem with this variable. First, there is a 4-5 year gap between the lists of innovative crops and the decision to plant them (which we subsequently measure 1 year afterwards). Second, we lack information on the particular seed varieties that our respondents planted. This means that we cannot distinguish people who use new seed varieties for traditional crops from those who use older and more established seeds. This seems particularly relevant to the 90 percent of households who planted maize since there are several types of maize seeds available in the MDICP research sites (e.g., MH18, MH17, NSCM 41, chitute).

and the differences are significant or marginally so in all cases. Among women with some education, however, the differences in livestock ownership appear insignificant. Women in the youngest age category, under 30, appear to have significantly less valuable livestock holdings than women 30 and over, although it is women 40-50 who report the highest values. Finally, there is no relationship between marital status and reported livestock ownership.

The baseline model on the planting of innovative crops (column 5) shows a different type of wealth effect. All wealth categories above the poorest are associated with a higher probability of innovation, except that the difference between richest and poorest women is the only insignificant coefficient (p=0.10). Thus, there is some indication of an inverse-U shaped relationship. Surprisingly, there is no clear relationship between education on the decision to innovate: women in all three education categories are no more likely than none educated women to plant innovative crops. Women in all categories above 30 are less likely to plant innovative crops than women under 30, although the effect is insignificant for women 40-50 and most negative for women over 50. Finally, marital status, once again, has no impact on innovative crop usage.

Column 9 presents baseline results vis-à-vis the probability of HIV testing. We see that it is generally larger for better off households, although the effect is not linear and none of the categories are significantly different than the least wealthy group. In contrast, education has a very strong positive and monotonic effect on HIV testing, with women in each age group significantly more likely to report having been tested. Age, on the other hand, has no relationship to HIV testing in these data. We find no clear pattern with women 40 and over less likely to report an HIV test, but not significantly different than women under 30. HIV testing is also strongly related to marital status with married women reporting far lower rates of testing (p=0.00).

ii. The contribution of network variables

We now examine the three subsequent models for each outcome to understand how *tn*, *cn* and both *tn* and *cn* affect the outcomes. We first note that inclusion of any of the network measures has little impact on the coefficients in the baseline model. In some cases the coefficient value changes but rarely does it shift much and almost never is the significance much affected. Therefore, we focus our attention on the network variables themselves.

In column 2, we find that not giving transfers at all or giving to extended family or friends reduces the value of livestock owned. The former coefficient is significant (p=0.02) and is likely due to the fact that almost no one can avoid making some transfers unless they are facing serious economic difficulties – poverty that goes beyond our wealth controls. The substantive magnitude of the coefficient suggests that households that make no transfers report 34% less value in livestock than those who transfer to nuclear households. The coefficient on transfer to non-close kin indicates that those whose largest transfer is to non-close kin report 24% less value in livestock holdings than those women who report a largest transfer to close kin, however this last coefficient is not significant (p=0.20).

In contrast to the effect of transfers, the model presented in column 3 shows that conversational network variables have no significant association with value of reported livestock. The largest observed effect is that of the share of network partners with no education. This is associated with less valuable livestock holdings, although this coefficient is also not significant (p=0.11).

Finally, in column 4 we see that when both network measures are included simultaneously, there is almost no change in the estimated coefficients on either set of coefficients, supporting our expectation that transfer behavior, the primary "action" associated with a risk sharing network, indexes a very different function in relation to livestock, overlapping very little with any function associated with conversational networks.

The results for innovative crops used present a different picture. Beginning with the model that introduces only transfer variables (column 6), we find that households with no transfers are less likely to innovate (p=0.13) than those who transfer to close kin. While not significant, this is consistent with our expectation that innovation requires some form of insurance in case of failure. Reinforcing this finding, we find that households whose largest transfer is to extended kin are more likely to innovate. Again, although this coefficient is not significant in itself (p=0.15), a joint test for the combined effect of the two transfer variables on the decision to use innovative crops shows that their contribution to the model is significant (p=0.05).

In contrast, the model shown in column 7 indicates that conversational network variables are not strong predictors of innovative crop use. Of the five discrete measures, only the proportion with no education is even marginally significant (p=0.09) in explaining the decision to innovate (this effect is also in the expected direction since it implies that information -- in this case about crop innovation – is likely to flow through a more educated network). Aside from this variable, however, *cn* is a poor determinant of crop innovation. This remains the case in the final model where we simultaneously explore the effects of both *cn* and *tn* networks.

Our final set of models shows the effect of network variables on HIV testing. In column 10 we find that not making any transfer *reduces* the odds of testing by about 4 percent relative to making the largest transfer to close kin, while reporting the largest transfer to extended kin *raises* the odds of HIV testing by about 4 percent. Tested jointly, transfer networks significantly affect HIV testing (p=0.03).

The effect of *cn* is tested in the next set of models. In both columns 11 and 12, results show that *cn* variables have considerably stronger effects on HIV testing than on the other two outcomes. Network size is positively associated with HIV testing (p=0.03) and the larger the share of non-educated network partners the less the probability of HIV testing (p=0.02), suggesting a social learning effect.

Density is marginally significant and suggests that individuals with sparser (less dense) *cns* are *less likely* to do HIV testing.

When, as seen in column 12, both network variables are added, the change in the results suggests that there is more overlap between the *functional* attributes of *cn* and *tn* when it comes to HIV testing than in relation to the other two outcomes. In particular, we see that when *cn* and *tn* are both included, the significance of no transfers falls considerably. In contrast, the significance of the *cn* variables changes very little.

iii. The effects of social learning versus social influence

As described earlier, one of our key aims in this paper is differentiate social learning effects on our three dependent variables from social influence effects. Both are enshrined in the *cn* variables. The key to differentiating their effects, building on Kohler (. . .) and Helleringer and Kohler (2005), is to leverage information about network density in order to identify networks in with multiple weak ties. Following Granovetter (1973), these are assumed to index social learning effects, whereas social influence effects should be associated with "density."

In order to explore whether such effects can be indentified here, we specified a further series of models which, in addition to the full "*cn*/*tn*" specification of models 4, 8, and 12, introduce an interaction between density and three important characteristics of conversational networks: the size of one's network, the proportion with no education, and the proportion from within the village. We then retested the model and jointly and individually tested the interactions. In no case did any of the interactions individually or jointly approach any level of significance or near-significance (results available from the authors). Thus, the results imply that most of the effects of *cn* are related to a social learning rather than social influence mechanism.

iv. The impact of temporal variation in networks on behavior

We explored possible temporal variation in network effects, in particular, whether network information collected in prior rounds of the MDICP might improve our ability to determine current behavior. This makes conceptual sense since it allows us to test whether there are lagged *cn* effects on the three behavioral outcomes.

We used two approaches (results not shown, but available from the authors). In the first, we reestimated the full models depicted in Table 5's columns 4, 8, and 12, but added the *cn* variables measured in the previous round (2001) as additional explanatory variables. Results showed that networks measured contemporaneously with the outcome variables were jointly significant (p=0.00), although some of the coefficients and their significance levels declined. However, none of the individual *cn* variables (as measured in the past) was significant; nor were they jointly significant (p=0.57).

In the second approach we reestimated the models with *only the past cn* variables – that is, without the current ones. Here, too, we found that the no single *cn* variable was significant; nor were they jointly insignificant (p=0.83). In summary, at least in relation to these three outcome variables, information on past networks provides little additional information.

v. The differential effects of AIDS and non-AIDS networks on HIV-testing

An important epistemological concern with respect to the effects of observed *cn* on HIV testing (columns 9-12) is that to the extent that AIDS *cn* networks are endogenous to all things related to HIV, we may be producing biased estimates of *cn* effects. In order to explore whether this is the case, we reestimated two versions of model 12, one with all *cn* information from the AIDS network, the other with all *cn* information from the religion network. We assumed that the relatively low levels of discursive proximity between these two conversational domains – in addition to the fact that the MDICP questionnaire placed all questions about religion and the religion network before any question about AIDS or the AIDS network – meant that we could treat religion NWPs as exogenous to questions about HIV testing.

Results are somewhat surprising. In particular, HIV testing is significantly related to aspects of religion-related *cn* (joint test p=0.03), but not to AIDS *cn (p=0.83)*. Specifically, in the analysis with religion-specific *cn* variables, we find a positive but marginally significant effect of *cn* size (.023 [p=.077]) and strong negative effect of proportion of *cn* with no formal education (-.10 [p=.04]). These are substantively the same effects as those seen in the full model (column 12, Table 5). In the analysis with AIDS-specific *cn* variables, however, the effect of network size retains the same point size but is not significant (.014 [p=.32]), and the effect of proportion of cn with no education drops in terms of the magnitude of the effect and the significance (-.058 [p=.16]).

These results are difficult to interpret. One explanation for this somewhat surprising result can be found in the conceptual distinction made earlier between an expressive *action* associated with a given type of network and the *function* of that network. Simply, the MDICP distinguishes between two types of conversational networks on the basis of AIDS- and religion-related conversations. The discrete religion and AIDS network models de facto verify the extent to which each of these social *actions* is related to AIDS or religion *functionally* is open to empirical verification. These two models suggest it is dangerous to assume that there is a relationship. In other words, the fact that we refer to a given conversational network as an AIDS network does not mean that functionally it is necessarily related to AIDS. An alternative explanation rests on the possibility that conversation networks about AIDS are themselves a product of the concern with testing or not testing. In that case, those not testing might show larger networks and any positive information effect of networks might be eliminated in the final results.

vi. Discussion

On a general level, analytic results are largely consistent with the general expectations laid out in Table 1. In particular: we found *tn* effects on the value of livestock holdings, but no *cn* effects; and we found both *cn* and *tn* effects on innovations in crop use and HIV testing. Beyond this general level, however, results are considerably more complicated. We now pull some of the general themes together.

First, there are stronger observed network effects overall where the focus of the analysis is reported HIV testing than where it is the use of innovative crops or changes in the value of livestock holdings. This implies that there is a higher correlation between the network-related action (reported conversations about religion and AIDS) and HIV-related network functions than between the latter and crop- or livestock-related decisions.

Second, and representing an important qualification to the first finding, conversational network data are not so specific that they cannot capture affects of networks on other behavioral outcomes. This has been seen in two ways. (1) Observed *cn* effects on crop innovation are in the expected direction, even though the *cn* data are supposed to be specific to religion and AIDS. (2) The last set of analyses show how reasonable assumptions about the relationship between network-related actions and functions are not always valid. Thus, for example, the closer association between <u>religion</u> *cn* characteristics and HIV testing than <u>AIDS</u> *cn* characteristics and HIV testing. Either way, the overlap between apparent conversational domains and overall network function has clear implications for other types of network analysis since it implies that, in the absence of network data specific to a given domain, subject, or idea, researchers may be able to use network data specific to some other domain.

Third, although observed *cn* effects are consistent with expectations, there is some evidence that the social learning mechanism is much more influential on these behavioral outcomes than social influence. In fact, in analyses of innovative crop use and HIV testing there is virtually no sign of social influence effects whatsoever.

Fourth, network effects measured contemporaneously with the outcome variables overwhelmingly dominate lagged effects of networks measured in past rounds. This is consistent with the relatively high levels of temporal instability in reported network characteristics and structure that was shown in Table 4. But more generally this instability signals the poor statistical properties of network variables, whether measured in terms of high levels of interviewer-related error

(Bignami-van Assche, Reniers, and Weinreb 2003) or indicators of reliability across time (White and Watkins 2000; Bignami-van Assche 2005). Either way, these properties weaken the analytic utility of network variables and suggest that future work might consider treating the measurement error directly – either through instrumental variables or structural equation modeling.

Finally, the results raise questions about how analysts ought to interpret networks effects. There are two distinct examples of this in the analyses. The first is related to the attenuation in selected *cn* and *tn* characteristics in the final analyses (columns 4, 8 and 12) in comparison to the two preceding models. We have mixed findings regarding attenuation – findings that do find some attenuation, particularly with HIV Testing, but the attenuation in coefficients is generally weaker than we anticipated. In other words, the percentage of a woman's *cn* which is resident in her village may index an informational determinant of behavior. But it does apparently index some element of the woman's local support network. A second example can be seen in the final model in Table 5. The coefficient on network size has a strong positive effect on women's likelihood of reporting having had an HIV test. Even if we discount the possibility that association is a mere artifact of measurement bias—ie., women who are more likely to admit talking about AIDS are also more likely to report having been tested—it is difficult to fully assign this result to the social learning mechanism of social networks effects since, presumably, there is some correlation between the number of conversational network partners and the *size* of their support network. In these data the latter is unobserved – even though we have some minimal data on actual transfer behavior. These two examples suggest that without distinct indicators of both social learning and risk sharing networks—each of which has very different interpretive implications—it is risky to infer a specific causal pattern to observed effects. And it is even riskier to claim knowledge of the relative magnitude of their effects.

Conclusions

There is good ethnographic and anecdotal evidence that not only do people *construct* networks out of a universe of social interactants but they construct particular types of networks for particular types of purposes. Networks, in other words, vary in form and function, and they do so by design. Here we have presented evidence that, irrespective of its particular form or function, social action within any given network – including conversation action that, in the case of "conversational networks," actually brings a network connection into being – is not necessarily related to the network's actual function. In addition, we have also shown that our *observed indicators* of network characteristics change over time, sometimes dramatically.

Using data on ego-centric networks from rural Malawi, we have attempted to explore the empirical implications of some of these ideas in relation to different types of behavior. Building on diverse network-oriented literatures in sociology, economics, and other network-friendly disciplines with foci on poorer countries, we have found that across conversational networks, there is some overlap in terms of specific network partners, some overlap between network partners of given characteristics across time, but far greater overlap in the cross-section. Consistent with more constructivist approaches to networks, we have also shown how conversational network characteristics appear to be affected by aspects of kin availability, and how they vary across levels of wealth. The underlying message of these descriptive sections is that indicators of social networks are partial, fluid, endogenous, but also at least partly predictable.

In a longer series of fixed effect analyses we have also shown, however, that the overlap between different types of network characteristics give network coefficients a dual quality. On one hand they facilitate certain types of network-informed analysis—we showed, for example, that data on AIDS and religion-related conversational networks have somewhat predictable effects on the decision to plant innovative crops even though the subject of conversation is completely unrelated to the outcome. Yet on the other hand, even our simple measures of the different network structures hints – at least in the case of HIV testing – that there is overlap between the networks limiting the easy interpretation of observed effects. Thus, for example, the difficulty in determining which of the two key social network mechanisms, information-transmission *versus* insurance, certain conversational network characteristics – such as conversational network size – actually represent.

Because networks and behaviors all vary over time, we have also explored whether the analysis of network effects should take into account the effects of temporal variation on behavioral outcomes. Our analysis has shown that conversational networks from the past have no import for current behavioral outcomes, in contrast to current conversational networks. Given the dearth of research on the effects of temporal changes in network characteristics on behavior, this is an important result. But it is also a surprising result since it is inherently commonsensical to think that currently reported behaviors should be, in part, the product of longer-term network characteristics. Indeed, we suspect that our answer to this particular question is affected by our inability to distinguish changes in *actual* network structures from temporal instability in the *measurement* of networks. In other words, though our analysis provides some indications that even if prior network information were available, they would not matter much, empirical research on networks must go much farther to validate this finding.

Whatever the answer to this final question about temporal effects, the levels of network fluidity in form and function, and the complicated relationship between individuals' actions within a network and that network's overall function, have clear and simple implications for data collection and, subsequently, for analysis. Specifically, in order to be able to parse out the discrete effects of different types of social networks, research instruments must include at least minimal indicators of those different types. Only thus can social networks approaches truly allow us to identify the ways in which individual preferences and action are socially embedded, whether through social learning and social influence effects, or through people's willingness and ability to risk some new action. Until such a time as those multiple indicators are available, and in the absence of information which would allow us to differentiate between different types of network effect, we may be as likely to misidentify the cause of the social network effect, as to identify it correctly.

Table 1. Expected weight of conversation and transfer network effects on three selected dependent variables

Behavior	***cn*** – social learning	***cn*** – social influence	Transfer Network
Livestock ownership	0	0	++
Crop innovation	++	+	+
HIV testing	++	++	+

Table 2. AIDS and Non-AIDS Network Characteristics Correlations

	Round 1	**Round 2**	**Round 3**
Number	0.422	0.520	0.419
Kin	0.484	0.494	0.547
Village	0.477	0.446	0.583
Education	0.603	0.677	0.698
Density	-0.374	0.350	0.113

Table 3. Relationship between transfers received and sent and conversational network characteristics. Multinomial regression coefficients using Malawi data

	Transfers Received		**Transfers Sent**	
	Close Kin	**Non-Close**	**Close Kin**	**Non-Close**
Number	0.100	0.193	-0.084	0.041
	[0.010]	[0.000]	[0.035]	[0.413]
Non-Close Kin	-0.299	0.052	0.363	0.301
	[0.130]	[0.864]	[0.070]	[0.256]
Village	-0.157	-0.615	0.179	-0.341
	[0.391]	[0.022]	[0.333]	[0.149]
Education	-1.524	-1.016	1.167	0.062
	[0.000]	[0.002]	[0.000]	[0.835]
Density	-0.004	0.056	0.029	0.017
	[0.881]	[0.096]	[0.237]	[0.610]
Constant	-0.376	-2.602	-0.575	-1.371
	[0.406]	[0.000]	[0.205]	[0.019]
Observations	1361	1361	1328	1328

Table 4. Total, AIDS and non-AIDS network measure correlations across time using Malawi data

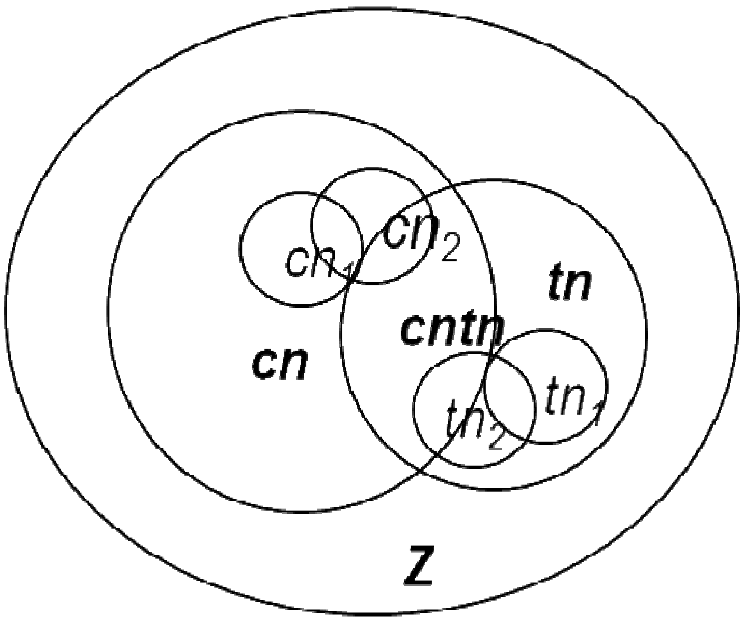
Both AIDS and non-AIDS			
	Round 1-2	**Rounds 2-3**	**Rounds 1-3**
Number	0.192	0.122	0.146
Kin	0.286	0.310	0.149
Village	0.215	0.247	0.139
Education	0.532	0.566	0.433
Density	0.092	-0.050	0.064
Only AIDS Networks			
	Round 1-2	**Rounds 2-3**	**Rounds 1-3**
Number	0.146	0.132	0.154
Kin	0.202	0.347	0.197
Village	0.142	0.196	0.108
Education	0.433	0.512	0.349
Density	0.117	-0.031	0.054
Only non-AIDS networks			
	Round 1-2	**Rounds 2-3**	**Rounds 1-3**
Number	0.143	0.062	0.101
Kin	0.261	0.194	0.110
Village	0.203	0.153	0.116
Education	0.470	0.514	0.385
Density	0.054	-0.015	0.025

Table 5. Village Fixed-effect Model to test Network effects on change in women's livestock ownership, Innovative crop use, and HIV Testing (robust standard errors)

	Livestock Value				Innovative Crops Used				HIV Test			
	Base	TN	CN	TNCN	Base	TN	CN	TNCN	Base	TN	CN	TNCN
Wealth 2	4.989	3.016	4.205	2.425	0.103	0.102	0.101	0.1	0.038	0.037	0.03	0.029
	[0.799]	[0.878]	[0.835]	[0.905]	[0.007]	[0.008]	[0.009]	[0.009]	[0.213]	[0.231]	[0.328]	[0.337]
Wealth 3	27.191	25.314	26.871	25.122	0.081	0.083	0.078	0.079	0.057	0.057	0.048	0.049
	[0.166]	[0.209]	[0.190]	[0.233]	[0.045]	[0.042]	[0.057]	[0.052]	[0.103]	[0.101]	[0.162]	[0.155]
Wealth 4	107.91	105.372	107.538	105.15	0.136	0.141	0.132	0.137	0.049	0.053	0.038	0.041
	[0.035]	[0.040]	[0.036]	[0.041]	[0.008]	[0.006]	[0.010]	[0.008]	[0.266]	[0.231]	[0.392]	[0.346]
Wealth 5	56.959	52.887	57.053	52.999	0.127	0.123	0.127	0.123	0.04	0.035	0.04	0.036
	[0.041]	[0.065]	[0.052]	[0.077]	[0.001]	[0.002]	[0.001]	[0.002]	[0.234]	[0.294]	[0.234]	[0.281]
Wealth 6	318.808	307.546	318.32	307.266	0.073	0.076	0.071	0.073	0.039	0.04	0.028	0.029
	[0.000]	[0.000]	[0.000]	[0.000]	[0.110]	[0.102]	[0.127]	[0.116]	[0.388]	[0.382]	[0.541]	[0.520]
Ages 30-40	26.834	31.126	26.027	30.383	-0.061	-0.064	-0.049	-0.053	0.037	0.034	0.048	0.046
	[0.482]	[0.420]	[0.509]	[0.447]	[0.044]	[0.033]	[0.105]	[0.082]	[0.167]	[0.196]	[0.072]	[0.088]
Ages 40-50	75.081	76.146	74.578	75.698	-0.025	-0.027	-0.015	-0.017	-0.003	-0.004	0.01	0.008
	[0.072]	[0.068]	[0.079]	[0.075]	[0.476]	[0.443]	[0.679]	[0.641]	[0.931]	[0.890]	[0.760]	[0.794]
Ages 50+	53.006	56.121	51.851	55.196	-0.105	-0.107	-0.097	-0.098	-0.024	-0.025	-0.021	-0.021
	[0.210]	[0.185]	[0.231]	[0.202]	[0.009]	[0.007]	[0.017]	[0.015]	[0.564]	[0.549]	[0.615]	[0.604]
Married	-47.445	-52.851	-47.55	-52.896	-0.014	-0.012	-0.015	-0.013	-0.135	-0.135	-0.138	-0.137
	[0.405]	[0.350]	[0.414]	[0.360]	[0.735]	[0.770]	[0.707]	[0.742]	[0.001]	[0.001]	[0.000]	[0.001]
Primary Ed	15.284	15.355	17.159	17.121	0.008	-0.002	-0.011	-0.019	0.042	0.034	0.02	0.014
	[0.319]	[0.332]	[0.225]	[0.244]	[0.830]	[0.961]	[0.767]	[0.608]	[0.093]	[0.175]	[0.415]	[0.573]
Second. Ed	36.436	34.305	38.166	36.042	0.003	-0.008	-0.018	-0.026	0.101	0.092	0.072	0.066
	[0.418]	[0.445]	[0.403]	[0.428]	[0.958]	[0.865]	[0.718]	[0.590]	[0.025]	[0.043]	[0.103]	[0.140]
Post Second Ed	228.605	233.946	230.491	235.68	-0.026	-0.041	-0.041	-0.054	0.129	0.117	0.111	0.101
	[0.086]	[0.081]	[0.081]	[0.077]	[0.659]	[0.497]	[0.497]	[0.373]	[0.050]	[0.074]	[0.086]	[0.116]

(Table 5 continued)	Livestock Value				Innovative Crops Used				HIV Test			
No Transfers Sent	-	-39.213	-	-38.967	-	-0.04	-	-0.036	-	-0.04	-	-0.031
	-	[0.088]	-	[0.087]	-	[0.139]	-	[0.180]	-	[0.064]	-	[0.154]
Transfers to Non-Close	-	-75.105	-	-74.877	-	0.052	-	0.049	-	0.039	-	0.038
	-	[0.057]	-	[0.061]	-	[0.154]	-	[0.173]	-	[0.248]	-	[0.271]
Number	-	-	0.683	0.503	-	-	0.005	0.005	-	-	0.015	0.014
	-	-	[0.922]	[0.943]	-	-	[0.497]	[0.546]	-	-	[0.032]	[0.036]
Non-Close Kin	-	-	-0.066	-0.22	-	-	-0.059	-0.056	-	-	-0.032	-0.03
	-	-	[0.999]	[0.996]	-	-	[0.151]	[0.172]	-	-	[0.362]	[0.400]
Village	-	-	8.862	7.637	-	-	-0.039	-0.036	-	-	0.007	0.01
	-	-	[0.856]	[0.876]	-	-	[0.303]	[0.351]	-	-	[0.832]	[0.756]
Education	-	-	11.299	9.927	-	-	-0.093	-0.092	-	-	-0.088	-0.088
	-	-	[0.703]	[0.738]	-	-	[0.087]	[0.088]	-	-	[0.021]	[0.022]
Density	-	-	-0.443	-0.257	-	-	-0.002	-0.001	-	-	-0.006	-0.005
	-	-	[0.911]	[0.948]	-	-	[0.674]	[0.802]	-	-	[0.074]	[0.112]
Constant	58.423	92.446	50.452	84.608	0.18	0.195	0.264	0.268	0.192	0.21	0.224	0.229
	[0.360]	[0.142]	[0.608]	[0.395]	[0.001]	[0.001]	[0.008]	[0.009]	[0.000]	[0.000]	[0.013]	[0.012]
Observations	1263	1263	1263	1263	1263	1263	1263	1263	1251	1251	1251	1251
Number villages	121	121	121	121	121	121	121	121	121	121	121	121

Figure 1. Measurable indicators of social networks, and the relationship between them



Types of social networks:

cn = conversational network

- cn_1 = conversation topic 1
- cn_2 = conversation topic 2

tn = transfer network

- tn_1 = transfer type 1
- tn_2 = transfer type 2

Z = all social interaction (incl. non-verbal)

APPENDIX

Table of descriptive statistics

Variable	Mean	Std. Dev
Livestock Value	139.869	449.159
Crop Innovation	0.219	0.414
HIV Testing	1.099	9.573
Wealth 2	0.217	0.413
Wealth 3	0.161	0.368
Wealth 4	0.082	0.274
Wealth 5	0.228	0.420
Wealth 6	0.128	0.334
Ages 30-40	0.388	0.488
Ages 40-50	0.176	0.381
Ages 50+	0.086	0.280
Married	0.893	0.309
Region 2	0.300	0.458
Region 3	0.345	0.476
Primary Ed	0.534	0.499
Second. Ed	0.146	0.353
Post Second Ed	0.062	0.242
No Transfers Sent	0.394	0.489
Transfers to Non-Close	0.168	0.374
Number	5.257	1.849
Non-Close Kin	0.699	0.316
Village	0.609	0.348
Education	0.231	0.312
Density	9.164	3.172

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