CHAPTER 16

FIELD EXPERIMENTS, SOCIAL NETWORKS, AND DEVELOPMENT

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16.1 Introduction

The study of social networks has become an important part of the economics tool kit. Modeling agents as independent actors misses the countless ways in which actions are shaped by the traits, beliefs, and decisions of their peers. Similarly, in order to measure general equilibrium effects of a policy intervention, it is crucial to understand not only how a beneficiary is affected directly, but how an intervention spills over to others in the agent's community.

Measuring spillovers and peer effects is notoriously difficult, especially using observational data (Manski, 1993). Social relationships are often endogenous, and connected individuals are likely to have similar characteristics and experience common shocks. Thus, it should be no surprise that following the rise of experimental methods in microeconomics, many researchers have also begun to use experiments to tackle questions involving social networks and to revisit insights from foundational non-experimental papers.[1]

The way in which networks have been studied using experiments has evolved as the literature has matured. One of the simplest ways to measure spillovers is to use the spatial variation induced from randomization to measure geographical spillovers. Another common technique is to record baseline membership in groups such as kinship, caste, ethnicity, classroom, or borrowing groups and to again use variation

[1] Foundational non-experimental papers include Foster and Rosenzweig (1995), Conley and Udry (2010), McMillan and Woodruff (1999), Banerjee and Munshi (2004), Petersen and Rajan (1994), and Townsend (1994), among many others.

in treatment status or group-level treatment intensity to identify social effects. A more recent, but growing subset of the literature has begun to move away from mutually exclusive and symmetric group memberships towards incorporating more complex network structures and even global network (i.e., graph- or community-level) characteristics. Usage of network data allows researchers to ask three key questions. First, how does the embedding of individuals in a social network shape economic outcomes? Second, what is the effect of the community-level network structure on those same outcomes? Finally, how are networks themselves molded by external forces?

Much of the material for this chapter is drawn from developing countries. Arguably, development economics has led other subfields in the adoption of economic experiments. Part this trend is surely due to the feasibility and cost associated with field work in these settings. But, in addition, developing countries are likely to be places where social networks and spillovers play an especially important role. When formal institutions do not work very well, informal, network-based institutions often arise as substitutes. Further, other frictions such as barriers to technology adoption are potentially severe in developing countries, and network-based solutions may generate large welfare gains.

The current body of experimental economics research highlights the profound and diverse ways in which social structures shape economic decision-making. I begin in Section 16.2 with a discussion of the methodological challenges involved in conducting social network experiments along with recent innovations. Section 16.3 turns to one of the most well-studied topics, social learning and diffusion. Next, I present research measuring interactions of social network relationships with other-regarding preferences in Section 16.4, followed by work on peer monitoring and enforcement in Section 16.5. Both altruism and the scope for network-based contract enforcement are essential ingredients for risk-sharing, which is analyzed in Section 16.6. Section 16.7 explores arguably the newest experimental literature, which studies the formation and change of networks. A discussion of promising avenues for new work concludes the chapter.

16.2 Design and Implementation issues

Much has been written to assist researchers in evaluating, designing, and implementing field experiments and can be applied directly to experiments with networks (Harrison and List 2004; and Duflo et al. 2007, for example). However, there are some issues that are distinct to network experiments, including elements of identification, experimental design, inference, network measurement, and network sampling. This discussion is related to other chapters in this volume, notably "Some Challenges in the Empirics of the Effects of Networks" by Boucher and Fortin and "Networked Experiments: A Review of Methods and Innovations" by Aral.

16.2.1 Identification

Before moving to more practical considerations, it is important to first discuss the fundamental problem of identification in the presence of spillovers.

Identification in randomized experiments is extremely simple when the stable unit treatment value assumption (SUTVA) of Rubin (1978) holds (i.e., when an individual is unaffected by the treatment status of others). For example, in an experiment with binary treatment assignment (i.e., treatment vs. control), randomization ensures that the potential outcomes of the members of each group are identical. Then the causal treatment effect is simply the difference in the mean outcomes between the groups.

Manski (2013) highlights the limits to identification when SUTVA no longer holds. Suppose now, in an experiment with simple binary treatments, that an individual may be affected either directly or indirectly by the treatment status of n members of that individual's community. Then, what Manski (2013) calls the *effective treatment* status of an individual no longer takes binary values (own treatment vs. control) but instead can take up to 2^n possible values (capturing the binary treatment status of each group member). If n is large relative to the number of participants in the experiment, there is no hope of identifying the treatment response for all possible treatment configurations without making stronger assumptions.

Given the intractability of this identification problem, in most cases researchers must place restrictions on the nature of the reference group and how spillovers are transmitted through a network. One common workhorse example is the linear-in-means peer effects model of Manski (1993). Bramoullé et al. (2009) extend the Manski (1993) model to the case of peer effects mediated by the social network.[2] Dieye et al. (2014) combine this model with experimental variation in cash transfers to study peer effects in schooling decisions in Colombia.[3]

It is important to keep in mind for the remainder of the chapter that in most analyses of spillovers in experiments, the researchers are making specific assumptions in the background about the nature of those spillovers. It is also important to note that one way to get around the identification problem of Manski (2013) is to treat each independent network or group as the unit of analysis (see Hudgens and Halloran 2008).[4]

16.2.2 Measurement

Defining the "true" social network is notoriously difficult and costly. One of the simplest network proxies to collect is geography, such as neighborhood indicators or spatial

[2] Also see Calvó-Armengol et al. (2009) and Lee et al. (2010), among others, for examples of model-based identification of social network spillovers.

[3] Note that in any estimation of the effect of the mean outcome of the peer group on an individual's own outcome, a type of exclusion restriction must be satisfied. That is, the experimental variation must affect the individual in only the ways specified by the model.

[4] In this spirit, Baird et al. (2014) present a two-stage experimental design combining within- and across-community randomization.

distances. However, geographical measures are only coarse proxies for bilateral ties and do not always map well to networks elicited through survey methods.[5] Another frequently used method is to employ common demographic characteristics (e.g., ethnicity, religion, caste, or kinship) or group memberships (e.g., churches, firms, social clubs, microfinance groups, classrooms) to proxy for social closeness. Again, this approach is limited, as it only allows for in- versus out-group comparisons.

Many network-based questions require more detailed network data, which in field studies is generally obtained from household surveys. In these surveys, researchers ask respondents to name individuals with whom they engage in a variety of activities such as risk-sharing, advice, or kinship.[6] Due to the difficulty of measuring networks, the recentness of the experimental networks literature, and setting-specific considerations, there is no consensus definition of what constitutes a link between two nodes. For example, some projects use actual historical interactions, others hypothetical interactions (i.e., financial transfers or advice). Many papers work with undirected graphs, while others instead take the directionality of interactions more seriously. Similarly, some research focuses on only one dimension of interaction, while others work with multigraphs over many dimensions of interactions.

Ideally, independent of the sampling strategy for the experiment itself, researchers would survey every member in each study network. This process often requires two steps, where an initial community-level census records the identities of all network nodes. A second survey then captures lists of each respondent's connections, matched to the census. Such surveys are often very costly, and are sometimes infeasible (for example, in urban areas, it is simply not possible to survey the entire graph), requiring researchers to sample subsets of nodes from the network.

16.2.3 Network Sampling

Measuring the network requires sampling sufficient numbers of nodes to generate accurate information about longer paths through the network. In general, small sampling rates will overestimate path lengths between arbitrary nodes and underestimate some node-specific measures such as degree. Chandrasekhar and Lewis (2011) show that sampling the network can cause nonclassical measurement error that systematically biases regressions that use network statistics as regressors. Even worse, the sign of the bias may depend on the underlying network.

[5] In their setting, Beaman et al. (2015) find geography-based centrality to be poorly correlated with survey-based measures of network centrality. Conley and Udry (2010) use differences between geographical and information neighbors for identification in their (nonexperimental) study of social learning.

[6] Other data sources have also been cleverly used for networks data. Leider et al. (2009) use online tasks to elicit networks of college students. Others have used mobile phone calling networks (Blumenstock et al. 2012), for example.

One approach that researchers have taken is to conduct full network surveys among only those individuals participating in one of the experimental treatments (i.e., individuals showing up to lab games). However, this method may miss a large fraction of potential links. If a fraction ψ is sampled, then information about approximately ψ^2 of the total links in a network is captured. If $\psi = 0.75$, then only 56% of potential links are recorded.[7] Chandrasekhar and Lewis (2011) highlight that first conducting a census of the community and then allowing sampled respondents to list connections with any other community members can dramatically improve coverage. Rates increase to approximately $1-(1-\psi)^2$. With $\psi = 0.75$, coverage under this method is 94%.[8] For lower levels of coverage due to sampling, Chandrasekhar and Lewis (2011) introduce a graphical reconstruction method for reducing bias.

There are some cases, often involving only direct social connections, where naive subsampling preserves the characteristics of interest. For example, measuring direct spillovers from a treated friend onto an untreated household only requires eliciting the direct connections between the treatment and spillover groups.[9] Banerjee et al. (2014a), who study dense urban communities where full networks surveys are impossible to implement, ask how access to credit changes a household's degree, which can be measured using survey responses from only treatment and control households.

However, taking only local measurements of the network places limitations on the types of questions that can be posed.[10] Subsampling considerations are particularly important when taking measurements of community-level (i.e., "global") network structures or when asking questions about indirect pathways of spillovers.[11]

16.2.4 Inference

One key problem with conducting inference along social networks is that outcomes of linked individuals are likely correlated. One common technique used in the broader experimental literature is to cluster standard errors to correct for within-group correlations. However, clustered standard errors require independence between members of different groups. It is an empirical regularity that real-world networks tend to contain one large connected component including the vast majority of nodes. For example, in the Banerjee et al. (2013) data, an average of 93% of households belong to the giant component in their villages.[12] Further, because clustered standard errors may suffer

[7] Related to Chandrasekhar and Lewis (2011), Manski (2013) suggests that this approach may be problematic, as treatment effects might flow through untreated individuals and then reflect back onto the treatment group.

[8] Reducing sampling rates to $\psi = 2/3$ only drops coverage to 89%.

[9] See Kremer and Miguel (2007) and Oster and Thornton (2012), for example.

[10] Measuring degree alone is likely to not be a sufficient proxy for more recursive or path-length dependent notions of centrality, for example.

[11] See Banerjee et al. (2013) or Alatas et al. (2014) for examples.

[12] Which is likely an understatement given the 50% sampling rate.

from small sample bias, the required number of distinct networks is large.[13] This is a second benefit, in addition to identification, from including many independent communities in an experimental design.

There are many types of research questions for which sampling a large number of independent graphs is not possible. Fafchamps and Gubert (2007) suggest using modified spatial standard errors, where spatial distance is replaced with network distance, and correlation between nodes decays with distance.

One standard component of the experimental design process that is tightly linked with inference is power calculations (Duflo et al. 2007). More work is needed by applied econometricians to construct analogous power calculations that incorporate network structure.

16.2.5 Other Implementation Considerations

In many experiments, researchers aim to measure the direct effects of an intervention on treated individuals and also the indirect effects of that treatment onto network neighbors or common group members. For these effects to be comparable, the potential outcomes of the treated and control groups need to be the same as the potential outcomes for the group receiving any spillovers. This creates design considerations when deciding how to define and sample the so-called "spillover" nodes.

The best method is to select the spillover group randomly from the same sample frame as the treatment group. This ensures that spillover nodes are exposed to treated versus control individuals with random intensity and that they have the same potential outcomes as the rest of the sample frame.

A second approach starts with defining the treatment and control groups, subsequently eliciting network connections from those individuals, and finally randomly sampling from that list of connections. Then spillovers can be measured by comparing the friends of treated individuals with friends of control individuals. Rather than collecting data on connections within the sample frame, this method relies instead on less intensive "snowball sampling." While the reduced form treatment effect on the friends is identified (subject to the discussion in Section 16.2.1), this group is likely to have different potential outcomes from the members of the sample frame themselves. One important difference is network centrality—individuals who are listed as friends by an arbitrary group of individuals are on average more central than that initial group of individuals. Thus, analyses using snowball sampling can overweight observations from more central nodes.

A different version of this problem also arises in experimental designs where the initially treated individuals are not randomly assigned at the individual level. An example is a village-level treatment where information about a preventative health

[13] Angrist and Pischke (2008) suggest a rule of thumb of 42. For smaller numbers, see Canay et al. (2014).

product is given to the head nurse in each treated village. One could measure spillovers onto the friends of the nurse, but those friends are not likely representative.

16.2.6 Endogeneity

One final consideration that is present in most empirical networks projects is the endogeneity of social connections. While this chapter describes many experiments that are overlaid on top of real-world networks, it is hard to escape the fundamental endogeneity of observed network structures. While some laboratory experiments are able to exogenously construct networks of different shapes (e.g., Choi et al. 2011) or to include individual-specific fixed effects (Chandrasekhar et al., 2014), this is generally not possible in many field settings. In Section 16.7, I discuss measurements of how networks change and evolve. This line of research is a key first step in solving the endogeneity problem.

16.3 Social Learning and Diffusion

Social learning is one of the most studied areas of the experimental social networks literature. This volume's chapter "Learning in Social Networks" by Golub and Sadler presents a thorough treatment of the theory. While most field experiments measure the joint information transmission and adoption decisions in a single outcome, laboratory experiments have the benefit of being able to closely control and measure the quality of signals, the content of information transmitted to and between subjects, and the network structure itself.

16.3.1 Testing Theories of Social Learning

Social learning is a core focus of Choi, Gallo, and Kariv's chapter, "Networks in the Laboratory." Here I discuss a small selection of papers in order to highlight the strengths of the laboratory for studying social learning. A dialogue between laboratory and field experiments is important to advance our understanding of how social learning works "in the wild."

Choi et al. (2012) and Choi et al. (2005) are key early studies that aimed to test the predictions of Bayesian learning presented in Gale and Kariv (2003). The authors experimentally formed networks of three individuals and randomly assigned private information to the nodes under different conditions. Participants took an action, which was observable to their network links each period. The authors find broad support for Bayesian theories, but they do note that over time, as the origin of information was harder to ascertain, the error rate increased.

Others have tested departures from the Bayesian model such as DeGroot learning, where as a rule-of-thumb, individuals naively average the beliefs of their peers. Chandrasekhar et al. (2012) formed seven-person networks of three varieties, chosen for the different learning patterns predicted under each model. Each node initially received a private signal, and every period entered a guess. Network neighbors where able to observe these guesses. At the network level, DeGroot performed better, explaining 76% of actions in contrast to 62% for Bayesian. DeGroot also performed better than a relaxation of the Bayesian model, where players were allowed to have incomplete information about the types of the other players. Grimm and Mengel (2014) randomly varied both the network structure and information about the network structure. They also find that DeGroot performed better than Bayesian. However, at odds with the DeGroot model, participants were able to partially account for the correlation in the beliefs of their neighbors when informed of the network structure. Also see Mueller-Frank and Neri (2013).

Möbius et al. (2015) test DeGroot learning models in a setting where individuals could communicate more than just their beliefs. They seeded signals in a real-life network of college students that they had measured in previous work. Students could update their guesses with the researchers at any time over the four-day study period, and were freely able to discuss the game with one another. The authors find that students were able to avoid double-counting information originating at a single source. This is consistent with participants communicating information to others along with the source of that information (tagging).

16.3.2 Social Learning and Technology Adoption

Studying social learning outside of the lab inevitably limits the questions that can be asked. In field applications, researchers generally take the network as given and only observe the final diffusion decision, rather than a path of intermediate beliefs. Is is also difficult to observe direct communication, both *who* spoke with whom and *what* was communicated. Further, it is often difficult to measure *a priori* beliefs about new technologies. Thus, results of diffusion experiments depend crucially on the network structure, the priors of the community, and the characteristics of the product that is being introduced.

One of the oldest empirical networks literatures has been the study of the adoption of agricultural technologies (such as fertilizer or high-yielding seeds) and basic or preventative health technologies (such as deworming drugs or bednets). Seminal work includes Foster and Rosenzweig (1995) and Conley and Udry (2010), and a recent wave of field experiments further explores this question.

16.3.2.1 Health and Agricultural Technology Adoption with Peer Data

Kremer and Miguel's (2007) study of the decision to purchase deworming drugs was one of the first field experiments in economics to measure adoption spillovers. The authors measure the treatment spillovers from a large randomized school-level deworming program in 75 Kenyan schools (Miguel and Kremer 2007). Three years after the program started, they collected social network information from a representative set of parents in their sample frame. Parents were asked to name up to five connections in each of several categories: friends, relatives, social contacts with children in primary school, and individuals with whom they spoke about health. The authors also collected information on the schools (and school treatment status) of the children of each of the reported links. They find that conditional on the total number of social connections, parents with two additional connections to treated schools were one standard deviation less likely to take up deworming drugs.

Miller and Mobarak (2014) study the decision to purchase improved cookstoves in 42 Bangladeshi villages and also find negative adoption spillovers. In the first round of their two-stage field experiment, they randomly offered new stove technologies at a randomized set of prices. In the second round, they returned to market the stoves to control group households and measured spillovers. Close social ties of round one participants were elicited in the original baseline survey. The authors instrument product purchase in round one with the random price variation and find that round two adoption decreased substantially with the number of friends who adopted in round one. They suggest that in their setting, it was both hard to learn about the benefits and that those benefits were not very large.

Oster and Thornton (2012) also analyze the diffusion of an unfamiliar product that was difficult to learn how to use. In their setting, menstrual cups were randomly introduced to adolescent girls attending four schools in Nepal. At baseline, the authors elicited links between each girl and the other students in the sample frame. They find that those girls in the treatment group who happened to have a higher share of friends who were also treated were more likely to use the technology. However, this effect was only present for the first six months, suggesting that information spillovers accelerated the adoption process.

Many other health adoption experiments that do not directly measure social network characteristics find strong evidence of (positive) spillovers based on geographical proximity to randomly treated households. Dupas (2014) finds that individuals were more likely to purchase insecticide-treated bednets if they had more neighbors who had previously received subsidies for their use. Banerjee et al. (2010) observe that villages bordering areas that received incentives for vaccination also had higher numbers of children receiving their full course of vaccines. Cohen et al. (2015) find that when neighbors were randomly exposed to rapid diagnostic tests (RDTs) for malaria, households increased their own demand for them. In an RCT in Morocco, Devoto et al. (2011) helped treated households obtain a private water tap. They find higher adoption rates among control households in areas that had higher densities of treated households. Finally, Godlonton and Thornton (2012) find that when individuals in rural Malawi were given incentives to take an HIV test, geographic neighbors were more likely to also learn their own HIV status (see also Ngatia 2012).

Taking these studies together, the results are decidedly mixed. Not surprisingly, the setting-specific benefits and ease of adoption appear to be of first-order importance. However, this raises a host of new questions including how the complexity of the information and uncertainty about the quality of the information affect both how people learn and the ultimate extent of learning.

16.3.2.2 Technology Adoption with Full Networks Data

Three relatively recent projects study diffusion using detailed network maps of the study communities.

Banerjee et al. (2013) trace out the adoption of microfinance and Cai et al. (2015) the adoption of crop insurance. These papers take very different, though complementary, approaches to studying social learning. Banerjee et al. (2013) measured take-up months after initial community members were informed about microfinance, allowing for many periods of information transmission along both short and long paths through the network. The authors are exactly interested in these longer paths. Cai et al. (2015) measured spillovers only days after seeding information, and therefore isolate direct, local spillovers from initially informed to uninformed households.

The Banerjee et al. (2013) project involved a detailed networks survey about 12 dimensions of relationships administered in 75 rural Indian villages. The authors partnered with a microfinance institution (MFI), which chose to enter 42 of these villages. Though the authors do not have randomized experimental variation in who was initially reformed, they argue that the fixed rule (based on demographic characteristics) used by the MFI gives quasi-exogenous variation in the network properties of those individuals who were first informed (injection points). They find that village-level adoption was higher when the injection points were more central in the network. Further the best-performing measure of centrality, which they call diffusion centrality, takes into account transmission through both short and long paths.[14,15] The authors also use a structural model to disentangle mechanisms behind the peer effect. They test whether individuals were differentially more likely to adopt if the links from whom they learned also chose to participate (endorsement effect). They do not find evidence of an endorsement effect, but find that individuals who adopted were more likely to pass information to others.

Cai et al. (2015) collected network data from rice farmers in 185 villages in China and invited a randomly selected group of farmers to intensive information sessions meant to encourage crop insurance take-up. A few days later, they returned and offered the insurance to a second group of farmers. The sessions did increase take-up and resulted in large spillovers to direct network connections. Because the second group was approached so soon after the information sessions, the spillovers likely isolate the effect of direct communications between the initially informed and their network

[14] For T periods of information diffusion, diffusion centrality measures the expected number of times all individuals hear about information originating at a common source.

[15] Degree, which is a local measure of centrality (i.e., number of links) does not perform as well.

neighbors. The spillover effect was half as large as the information session effect, and using pricing experiments, was measured to be equivalent to a 13% price reduction. In order to separate learning from an endorsement effect, they introduced randomized defaults into their experimental design, which increased the likelihood of purchase. Using these defaults as an instrument for take-up, they find small but imprecisely measured endorsement effects.

In a new paper studying the adoption of a productivity-enhancing agricultural technology, Beaman et al. (2015) mapped networks in 200 villages in Malawi and experimentally varied who in the network received training in the new technology. In one treatment arm, the authors selected who to initially inform based on simulations of threshold diffusion models, in which individuals need to learn about the technology through multiple network neighbors before adopting themselves. In their benchmark treatment, the initial seeds were selected by agricultural extension officers. The authors find that their model-driven approach increased adoption by 3–4 percentage points (on a base of 10% in the benchmark group) and reduced the chance of no individual in a village adopting.

When information about new technologies is scarce, these papers illustrate in three different settings how the success of diffusion and adoption is closely tied to the network properties of who is initially informed. From a policy perspective, they suggest that mis-allocation of information may be extremely costly, and that it is possible to use network properties to substantially improve outcomes. More work is needed to develop cost-effective tools to accomplish this.

16.3.2.3 Strategic Information Diffusion

The field experiments discussed thus far show a range of diffusion outcomes including negative, positive, and an absence of spillovers. Strategic motivations may be an additional mechanism hindering or aiding diffusion. Many types of goods such as jobs, popular concert tickets, or limited-quantity subsidies are rival. Conversely, some products exhibit complementarities in the number of end users such as technology platforms. It is natural to speculate that rivalries or complementarities should affect incentives to pass information and overall adoption rates.

Banerjee et al. (2012) study a setting of social learning about rival goods in 60 of the Banerjee et al. (2013) network villages. In each village, 10 randomly chosen households were notified about an opportunity to earn half a day's wage playing laboratory experiments. Invitees were also informed that they could invite others, but that participation had a strict cap of 24 subjects. When the researchers arrived to conduct the laboratory experiments, many individuals who were not directly invited were waiting to play. Indeed, individuals with a greater share of initially invited friends showed up. To capture rivalry in the decision of whom to invite, the authors estimate a diffusion model. They find that the patterns of participation were consistent with invited households only spreading information to peripheral (low degree) friends. Transmission rates to high-degree friends were very low, in comparison.

Better understanding the causes and implications of strategic passing is fertile ground for future experimental work.

16.3.3 Information Extraction

While many social learning applications investigate how information injected from outside flows through a network, the social network may also serve as an information reservoir. Many actors such as governments, employers, lenders, and NGOs may benefit from being able to tap into sources of information. A recent set of labor and credit market experiments investigate whether the network does in fact possess valuable information and if individuals can be provided with incentives for truthful revelation.

Pallais and Sands (2014) conducted job referral experiments in an online labor market. They asked previous employees to refer workers and compared the referred applicants to applicants who responded to an online job posting. On average, the referred workers had better observables and also performed better conditional on observables. The authors find evidence that the effect was driven by selection and that referral work quality was positively related to the work quality of the referrer.[16] A natural follow-up question is how much of this observed selection effect comes from selection in the network formation process (if more productive workers sort into relationships with other productive workers) versus information acquisition (referrers can better use the social network to learn about the quality of other workers). Further, what are the underlying incentives for individuals to refer some members of their social network and not others?

Beaman and Magruder (2012) tackle these questions in a laboratory-in-the-field experiment in Kolkata, India. They recruited participants to perform a set of tasks and then asked them to refer other highly skilled workers under experimentally varied incentive structures. Some participants received fixed payments for each referral, while others received performance-linked incentives.[17] First, they observe a high level of family referrals in the fixed incentive treatment and a large decrease when participants were instead given performance incentives. Second, they find that more productive participants were better able to refer other high-performing workers, but only did so under performance pay.[18] This suggests that for some individuals, there was both variation in the skill of their network connections and substantial (likely private) information about worker quality. However, incentives were required to induce information revelation.

[16] One alternative explanation is that more central workers tend to be better workers. Even if workers referred random friends, those friends would be more central on average than a randomly sampled worker.

[17] In order for the performance treatment to only measure selection and not induce monitoring, when the referrers brought their referrals to the lab, they were unexpectedly given the maximum payment before the referral started working.

[18] In contrast, performance pay did not increase the referral quality of the less-productive participants.

In a related study, Beaman et al. (2014) consider the distributive consequences of referrals. They also randomized between fixed and performance-based incentives, but in addition, requested (randomized) gender-specific referrals. In absence of a requested gender, men were much more likely to refer men. The authors argue that men were able to refer qualified women when incentivized, and they speculate that men likely either received higher social benefits from referring other men or paid lower search costs.

Bryan et al. (2015) investigate similar issues in the context of credit market referrals in South Africa. However, unlike Beaman and Magruder (2012) they find that holding incentives to monitor fixed,[19] providing incentives for borrowers to refer credit-worthy individuals did not result in better loan performance. This result is not consistent with individuals having superior information about the credit-worthiness of their network connections relative to the lender.[20] It may also be the case that demand for loans among the referrer's friend was low, limiting the scope for private information.

To understand which network structures are best-suited to facilitate information aggregation, Alatas et al. (2014) use network data from 631 Indonesian communities. In a prior study, Alatas et al. (2012) investigated how policy makers could most effectively identify the poor for subsidy delivery given that incomes and asset holdings could not easily be verified. They randomized between methods including proxy-means tests (PMTs) and community targeting. Alatas et al. (2014) investigate which networks produced the most accurate poverty rankings. This type of village-level analysis is only possible because of the very large number of independent networks measured in the original study. They present and estimate a quasi-Bayesian model of learning about other people's wealth levels when those levels are changing over time. They find that (as predicted by the model) networks of higher densities (i.e., higher maximal eigenvalue and higher frequency of links, fixing network size) and with degree distributions better suited for diffusion (i.e., first-order stochastic dominance) produced better information. They also show that villages that were expected to know more under the model versus those that were expected to know less performed better at allocating resources in community targeting versus proxy means.

A natural follow-up question is which members of the social network have the best information (holding incentives fixed)? While Alatas et al. (2014), Banerjee et al. (2013), and Banerjee et al. (2014c) investigate this question descriptively and provide suggestive evidence that central individuals are better informed, there is scope for more causal experimental evidence.

The experimental literature on social learning shows the profound ways in which social networks can matter for diffusing, storing, and revealing information. However, in many ways, the literature has barely scratched the surface.

[19] Incentives to monitor are discussed in Section 16.5

[20] The authors mention that at the time of the study, South Africa had a robust credit scoring system and that there may not have been much scope for additional soft information revelation. Unlike in Beaman and Magruder (2012), only borrowers who satisfied the lender's underwriting criteria were given a loan.

16.4 Other-Regarding Preferences and Social Networks

A large laboratory literature on other-regarding preferences demonstrates high levels of baseline altruism among anonymous pairs. Other research shows that the social context of laboratory experiments also affects payoffs. In an early experiment, Hoffman et al. (1996) find that transfers in the dictator game increased when dictators were placed in situations involving less social isolation. Bohnet and Frey (1999) adopted the clever experimental design of changing the anonymity of the dictator and recipient before transfer decisions were made. They find that transfers under two-way identification were almost twice as big as under double anonymity.

More recent work has studied the interactions of other-regarding behavior with real-life social networks. Goeree et al. (2010) explore transfers among 5th and 6th graders in a Pasadena girls school. In advance of dictator games, they elicited the full social network of the students.[21] They find that dictator transfers increased dramatically with inverse social distance, with strangers receiving $\frac{1}{6}$ share and direct links receiving $\frac{1}{2}$. Their results suggest that social proximity is important for determining how resources are shared. Branas-Garza et al. (2010) elicited networks and played similar games with undergraduates. They find that measures of centrality (betweenness and degree) were positively related to giving.

Close friends and central individuals may make larger transfers for many reasons. First, the social network may parametrize altruism. Namely, individuals who are directly linked may exhibit greater directed altruism toward one another. Second, the social network may be correlated with motives of reciprocity or social sanctions. Individuals with stronger social connections are more likely to interact in the future and accordingly may have stronger incentives to transfer more in dictator games. Third, more generous people may simply be more popular, leading to more observed links between generous individuals.

Leider et al. (2009) conducted a careful online experiment with Harvard students to shed light on these different mechanisms. By experimentally concealing or revealing the identities of partners in a dictator game (similar to Bohnet and Frey 1999), Leider et al. (2009) controlled whether the directed altruism or future interaction motive (or both or neither) was present. They focus on how social network relationships interacted with the treatment conditions. They find that when dictator transfers were efficient (i.e., transferred resources grew in size), dictators were more generous to direct links relative to random strangers. When the dictator observed the identity of the other party in a single-blind fashion (directed altruism), social closeness increased transfers by 52%. When the identities of the participants were fully non-anonymized (enforced reciprocity + directed altruism), direct links received 24% more than strangers.

Ligon and Schechter (2012) conducted a similar experiment in a very different empirical context, 15 villages in rural Paraguay. However, they find a very similar

[21] With 95% sampling.

decomposition of the altruism effect as Leider et al. (2009). They estimate an average effect relative to the anonymous game of 8% when only the transfer recipient could identify the dictator, 6% when only the dictator could identify the recipient, and 17% when the identities were completely non-anonymized. In comparison, the effect sizes in Leider et al. (2009) were 14%, 10%, and 40%, respectively. Ligon and Schechter (2012) also find that individuals with higher degree appeared to exhibit less directed altruism and more reciprocity.

Goeree et al. (2010), Leider et al. (2009), and Ligon and Schechter (2012) all examine dictator play between individuals with preexisting social relationships and find surprisingly consistent results. In their settings, it is possible to carefully measure the altruism effect and to separate directed altruism from reciprocity or social sanctions. However, it is much harder to understand the role selection or other endogenous factors may play. Leider et al. (2009) find that individuals with stronger levels of baseline altruism had friends who were also more altruistic on average. They raise the question of whether individuals with altruistic preferences sort into friendships with like-minded people, or whether social relationships themselves shape an individual's preferences. To explain their result, Leider et al. (2009) also reason that high-degree individuals may act more reciprocally because of their network position. It is also possible that those with an intrinsic preference for reciprocity select into positions of network centrality. Goeree et al. (2010) find that personal traits predict links (in a homophilic fashion), which in turn predict behavior in the dictator game.

These discussions suggest how important it is to understand the network formation process when interpreting experimental findings. The state of the existing experimental literature on network formation and change is discussed in Section 16.7. Similarly, more work is needed to understand how the social network shapes an individual's underlying preferences.

Another important primitive studied in the experimental economics literature is trust.[22] Glaeser et al. (2000) conducted a laboratory experiment with 196 Harvard undergraduates. Participants were paired (often with friends who accompanied them to the experiment) and then played two different trust games. Comparing across pairs of participants, the authors find that both trusting behavior and trustworthiness increased with social proximity. Binzel and Fehr (2013) find similar results in a laboratory experiment in a Cairo slum.[23] Also see Breza et al. (2014) (described below) and Barr (2003) for related evidence.

[22] The workhorse measure of trust is the game pioneered in Berg et al. (1995). Many have questioned whether the game really measures trust (see Schechter 2007 for a discussion). Such a debate is beyond the scope of this chapter. However, even if the Berg et al. (1995) game does not actually measure trust, it is still likely to measure the ability of a pair of individuals to successfully reach an efficient outcome in the absence of commitment. Karlan (2005), for example, shows that behavior in the game does predict loan repayment in Peru.

[23] The authors recruited pairs of friends and had subjects play trust and dictator games. In some treatments the friend pairs played together, while in others partners (who were likely to be strangers) were assigned. Using within-person variation, they find that trust was higher in the friend pairs.

As in the literature on altruism and social networks, most of these papers take the preexisting social network as fixed and vary with whom the trust games are played. Bearing in mind the same cautions about endogenous network formation and reverse causality, the lab-in-field literature suggests that social connections are likely to be quite important for sustaining trade and exchange.

16.5 Public Commitments, Peer Monitoring, and Enforcement

Examples of public commitments and peer monitoring are widespread in applications from labor to credit to going to the gym (i.e., Bandiera et al. 2010; Besley and Coate 1995; Babcock and Hartman 2010). Much theory is devoted to understanding how the network diffuses information and shares risk. However, some of these same network processes are also useful for peer monitoring and enforcement.

Breza et al. (2014) conducted non-anonymized trust games with third-party monitoring and punishment in 40 Indian villages and matched experimental outcomes with the Banerjee et al. (2013) networks data. They find evidence that when contracting parties could not call upon social proximity, peers could be used to improve outcomes. In their sample, 84% of the randomly assigned pairs in their experimental sessions were of social distance greater than two. If highly network central individuals were chosen to serve as the third party and were given a punishment technology, outcomes between these socially distant pairs were as good as those between socially close pairs without a third party. While network centrality was not randomly assigned, Breza et al. (2014) randomized both the identities of the players and the available contracting tools. They also find that no other demographic characteristics such as caste, elite status, or wealth could replicate the centrality effects. The result suggests that eigenvector centrality, which is usually thought to be important for diffusion, can be used in other capacities, namely enforcement.

Several field experiments have demonstrated the capacity for peer-based mechanisms to increase savings balances and loan repayment. In their field experiment studying different types of savings vehicles, Dupas and Robinson (2013b) find that social commitment increased savings and was more effective than their other individual-based treatments. Kast et al. (2012) further investigate this social commitment effect. Working with microfinance borrowing groups, they opened individual savings accounts for members, and some savers were randomly chosen to also receive monitoring by their microfinance groups. They find that those in the peer monitoring treatment saved almost twice as much as those in the control group. However, in a separate sample, they find that text-based SMS reminders were comparably effective and were not enhanced by the addition of a "Savings Buddy" who was chosen by the saver to monitor her performance.

Breza and Chandrasekhar (2015) ask whether such savings arrangements depend on the embedding of the savers and monitors in the social network. They conducted a field experiment in 60 villages in South India (also using the Banerjee et al. 2013 networks) with villagers who expressed a desire to save more. All savers were given a bundle of services including assistance with account opening, savings goal elicitation, and bi-weekly reminder visits by a surveyor. A randomly selected group of savers was also chosen to receive a monitor, who was updated about the saver's progress every two weeks. When savers were assigned to receive a randomly-chosen monitor, savings increased substantially (by 35% across all accounts) relative to the non-monitored group. The effects were even stronger when either the monitor had high eigenvector centrality or the saver and monitor were socially close. They also find suggestive evidence of a reputation-based mechanism, where monitors actively spread information about their savers to others. Sixty-three percent of monitors reported telling others about the saver's progress. Further, over a year later, other villagers were more likely to know if the saver exceeded her goal and to think that the saver was responsible if she had been randomly assigned to a more central monitor. When given the opportunity to choose their own monitors, savers picked monitors who were both closer and more central than average.

Credit is another financial product where some lenders have captured the power of peer enforcement. Many argue that peer monitoring is one of the reasons why microfinance is able to boast such high repayment rates. As discussed above, Bryan et al. (2015) conducted a clever field experiment to separately measure both peer screening and peer monitoring in credit markets. In their experiment, existing clients were asked to refer a friend to receive a loan and were randomly assigned into four different incentive treatments. They find very large monitoring effects, but no detectable screening effects. One could also speculate that the efficacy of such monitoring arrangements might depend on the network positions of the referrer and referred borrower.

These studies help to get inside of the black box of many informal financial products commonly observed in developing countries such as rotating savings and credit associations, self-help groups, and microfinance institutions, all of which incorporate groups of individuals from the same social network and rely on mechanisms that are likely to include mutual monitoring.

Another relevant application is public goods provision and the management of common pool resources.[24] However, there is surprisingly little experimental evidence on the topic. Carpenter et al. (2012) is one of the only laboratory experiments to consider how the graph structure when combined with social punishment can increase or limit the efficiency of public goods provision. Choi et al. (2011) consider the role networks can play in helping groups to coordinate on efficient levels of public goods. This area of study, especially in the field, represents a real research opportunity.

[24] A growing network theory literature (Bramoullé and Kranton 2007 and Elliott and Golub 2014 for example) exists on the topic as well as a large literature in political science that began with Ostrom (1992).

16.6 Risk-Sharing

Aside from social learning and diffusion, enabling the sharing of risk is one of the most important functions of social networks in economics. This feature of economic networks is well-theorized and has been the subject of many empirical, though often observational, studies (Kimball 1988; Coate and Ravallion 1993; Kocherlakota 2004; Ligon et al. 2000; Foster and Rosenzweig 2001; Udry 1994; and Townsend 1994). Because risk-sharing interacts with and encompasses many of the other mechanisms that have already been discussed, designing the "ideal" risk-sharing experiment is quite daunting. The ability for a decentralized group of individuals to share risk is shaped by many factors. Key components of risk-sharing include the sharing rule, the degree of commitment present, and opportunities to hide income or savings. Each of these is likely related to the social network through other-regarding preferences, peer enforcement, or information. Due to the complexity involved in studying risk-sharing, most of the existing experimental work comes from laboratory studies where it is much simpler to switch on and off mechanisms.

Barr and Genicot (2008) and Barr et al. (2012) analyze a multi-day lab experiment conducted in 14 villages in Zimbabwe. On the first day, individuals were invited to the lab to play a gamble choice game similar to Binswanger (1980). Participants were then invited back for a second day. They were allowed to form groups with other (non-anonymized) participants before they again played gamble choice games. Groups then shared their (risky) earnings equally under either external enforcement or limited commitment. In the limited commitment treatments, individuals could opt out of the group sharing after observing their own income realizations. Barr and Genicot (2008) show that full enforcement generated the largest groups and induced the most risk-taking by participants.[25] Barr et al. (2012) study the composition of the risk-sharing groups and find that participants tended to match by kinship and religious ties in the limited commitment treatments. However, matching by membership in a community-based organization only arose under full enforcement. They argue that only some types of ties were capable of meting out and withstanding the consequences of social sanctions.

Attanasio et al. (2012) conducted a similar exercise in 70 communities in Colombia and measured network connections between all participants in the lab. They find that close friends and relatives were more likely to form groups, and that they were also more likely to match assortatively on risk preferences. While only suggestive, these results highlight the scope for social proximity to facilitate risk-sharing under limited commitment.

Chandrasekhar et al. (2014) take up this idea in laboratory experiments in 34 of the villages from Banerjee et al. (2013). To better understand the role of social networks in sustaining self-insurance, they played insurance games with pairs of participants. They

[25] The increase in risk-taking may arise both from the full enforcement and the increase in group size.

exogenously varied both the social connections between the players and the contracting environment. They find that socially close pairs reached more cooperative outcomes than distant pairs, especially when full enforcement was switched off. They also find less cooperation when the pairs had unequal centralities, especially in the absence of full enforcement. Because each individual played several games, the authors could include individual-specific fixed effects and use within-participant variation for identification. This helps to rule out bias from person-specific unobservables that could be correlated with the individual's position in the network.

One application where it is easy to observe active risk-sharing groups is microfinance. Fischer (2013) ran laboratory experiments with microfinance groups in India to study the role of joint liability contracts. He highlights the opposing incentives for risk-taking induced by risk-pooling and peer monitoring. To separate these incentives, he exogenously imposed five different group financing structures. When group members could not monitor perfectly, joint liability (risk pooling) created significant incentives to free ride and to take more risk. However, under both monitoring and risk pooling (which approximates microfinance contracts) borrowing groups were not able to take risk, even when efficient. Giné et al. (2010) find similar evidence in laboratory games in Peru. In their setting, joint liability increased risk-taking, but less so when individuals were able to choose their own groups.

Because of the difficulties outlined above, field experiments with risk-sharing are quite rare. Existing field work has taken the approach of observing how individuals in a network with preexisting risk-sharing relationships react when given the chance to take up formal financial products.

Mobarak and Rosenzweig (2012) conducted an experiment in India to measure the effects of marketing formal rainfall insurance to households that were already engaged in informal risk-sharing. They randomized offers of insurance to members of different subcastes (which proxied for risk-sharing groups) and also randomized the placement of rain gauges (and thus the amount of basis risk). Using survey data, they show that ex ante, some castes were able to insure aggregate risk and others could insure idiosyncratic risk. The authors find that the ability to informally insure aggregate risk and the presence of basis risk each decreased the demand for formal insurance. However, the negative impacts of basis risk were mitigated when the network could only insure idiosyncratic risk, suggesting complementarities between the two. Mobarak and Rosenzweig (2012) provide important field evidence on the relationship between formal and informal insurance, which raises a new set of questions. For example, when an individual is able to take formal insurance, what are the follow-on effects on the former risk-sharing partners? Moreover, what determines the scope of insurance against idiosyncratic and aggregate risks in a network?

In most situations, informal risk-sharing generally improves welfare for risk averse agents. However, there is potentially a dark side. Using a lab-in-the-field experiment in rural Kenya, Jakiela and Ozier (2015) investigate the conventional wisdom that individuals are often unable to consolidate windfalls due to unsolicited demands from friends and family. Participants were given endowments of different sizes, which they then divided between risky and safe investment opportunities. In some treatments, the risky investment could be observed by other participants. The authors report that women often took costly actions to hide income from neighbors and family. Further, men and women both were willing to pay up to 15% of their earnings to keep their incomes fully hidden. They argue that their evidence is consistent with the conventional wisdom and that pressure to make transfers erodes efficiency. Dupas and Robinson (2013a) also suggest that unwanted demands from social contacts is one mechanism that may make access to private bank accounts so valuable to poor households. Jakiela and Ozier (2015) provide experimental evidence that social pressure can reduce efficiency ex post. However, the policy implications of this result depend crucially on why this inefficiency arises. Under many models of informal insurance with limited commitment, the ability for individuals to exert social pressure ex post is efficiency enhancing from an ex ante perspective, even if there are ex post incentives to hide income. More work is needed to resolve this important issue.

16.7 Network Formation and Change

As discussed above, most empirical networks projects, experiments included, take the social network structure as given and exogenously overlay a treatment such as access to information or credit. In most cases, authors must rely on conditioning on observables or structural assumptions to make the case that causality runs through the observed social network. Because the social network encodes patterns of interactions between individuals in real life, it is often extremely hard, if not impossible, to find sources of exogenous variation in network structure. However, a small recent literature has begun using field experiments to do just this.

In a nice experimental design, Fafchamps and Quinn (2012) study connections between judges of business idea competitions in Ethiopia, Tanzania, and Zambia. By randomly assigning the judges to committees that met over the three-day competitions, Fafchamps and Quinn (2012) sowed the seeds for new business linkages to be formed. A few months after the competition, co-committee members were more likely to remember and to speak with one another. They find suggestive evidence of peer effects that are positive for low-risk and low-cost actions such as opening a bank account, but negative for high-cost and high-risk actions such as launching new products.[26]

Feigenberg et al. (2013) is one of the first to document the effects of an intensive social intervention. The authors randomized the frequency of microfinance group meetings between weekly (status quo) and monthly during clients' first 10 months of borrowing. They find that 16 months after the experiment's end, individuals assigned to the weekly group met with fellow group members 37% more than pairs in the monthly group. They then tie these stronger social connections to evidence of improvements in risk-sharing;

[26] Also see Fafchamps and Quinn (2013).

participants reached more efficient group-level outcomes in a risky lottery experiment administered 16 months after the end of the intervention, and borrowers in the weekly group were substantially less likely to default on subsequent loans.

While Feigenberg et al. (2013) focus on changes to the strength of ties through microfinance, Banerjee et al. (2014a) analyze persistent changes in the number of ties caused by randomized exposure to microfinance. In 2012, the authors returned to 104 neighborhoods in Hyderabad, India that were involved in the Banerjee et al. (2015) randomized roll-out of microfinance. Half of the households were exposed to microfinance in 2006, six years earlier (treatment group), and the other half were exposed in 2008 (control group).[27] The authors first show that longer access to microfinance had persistent positive effects on business, credit, and durable consumption for the subset of households who had started a business before microfinance was first made available. Because the experiment was conducted in dense urban neighborhoods, a full network elicitation was not possible. Instead, respondents were asked to name all of the other households in their neighborhood with which they participated in each of eight different financial and nonfinancial activities and indicate whether each link was supported.[28] Treatment households without a preexisting business reported 8% fewer network connections than the control (mean degree 5.9), and the number of supported links decreased by 16% (control mean 2.7). The non-entrepreneurs who had longer access to microfinance were not more likely to report links with whom they had shared a microfinance group in the past. Total informal borrowing also decreased. However, for preexisting entrepreneurs, there was no detectable change in number of overall links or number of supported links. This subgroup also borrowed more from informal sources. This evidence suggest that microfinance and informal risk-sharing served as substitutes for those without a preexisting business (and with relatively low demand for credit) and complements for those who selected into entrepreneurship before microfinance was available.

Comola and Prina (2014) study how transfers between households changed in response to a randomized savings intervention. The authors collected data on actual financial transfers between all households in 19 villages in Nepal before and after the intervention.[29] Treated households sent larger transfers and increased the number of recipients relative to the control, and the effects were strongest for wealthier households with better financial access. The authors then use structural methods similar to Bramoullé et al. (2009) to decompose expenditure peer effects arising from the intervention. They are particularly interested in the channel of peer effects that results from the intervention's impact on the network structure.[30]

[27] Due to political reasons, access to microfinance ended in all study areas in 2010.

[28] In other words, whether there existed a third person who did that activity with both the respondent and the friend (Jackson et al. 2012).

[29] Prina (2015) describes the intervention and its direct effects.

[30] One limitation of the Comola and Prina (2014) study is that their measured networks are quite sparse with an average degree in the baseline data of 0.64. While the estimated causal impact of formal

Binzel et al. (2015) study the impacts of the roll-out of rural bank branches on village networks in India and find evidence of substitution between formal and informal borrowing. They measure decreases in the amount respondents reported being able to borrow from their village network (borrowing capacity) and find declines in amounts transferred to financial network links in non-anonymized dictator games. The dictator games also suggest that households shifted transfers away from financial links and towards social links.

Banerjee et al. (2014b) use their 75-village data set to study network change before and after microfinance. While again they do not have experimental variation, they use differences-in-differences to compare the network shapes of villages with or without access to microfinance, before and after the intervention. They find access to microfinance to be associated with lower link probabilities, lower levels of support, and less multiplexing (i.e., links with multiple dimensions of interaction). They further find that results are similar when looking at financial, social, and advice links.

To date, this line of inquiry has generally sought to understand how the shape of the network responds to external stimulus. The possibility of using exogenous network change to better understand causal links between network shape and other real outcomes is exciting. However, such an exercise would require that the underlying change to the network not be directly correlated with the outcome of interest (e.g., exclusion restriction). New developments in the modeling of network formation as discussed in “Econometrics of Network Formation” by Chandrasekhar in this volume will help advance this pursuit.

16.8 Conclusion and Open Issues

Experiments involving social networks both in the laboratory and the field have followed the broader growth of economic experiments. However, the addition of a network component requires new considerations ranging from identification to how to define and measure the network, how to measure spillovers, and how to conduct inference. Because of the recentness of this literature, there is still no firm consensus regarding how to approach these issues.

It is an exciting moment to reflect on the state of network experiments. This chapter reviews five interrelated strands of experimental networks research: diffusion and social learning, other-regarding preferences, peer monitoring and enforcement, risk-sharing, and network formation and change. In each of these areas, innovations in experimental techniques have permitted researchers to expand the scope of the study of peer effects from simple in- versus out-group spillover effects to incorporating more of the global network structure. Another recent trend in the literature has been to use experimental

savings accounts on realized transactions is identified given the experiment, the structural peer effects decomposition is likely quite sensitive to the way in which the network is specified.

variation to disentangle different network-based mechanisms underlying reduced form spillover effects.

Based on the large volume of research currently in progress, the next few years will mark an advance in the field's understanding of network-mediated phenomena. Further, as more experiments are completed, we will be able to better compare results across a range of different settings.

There are also many exciting directions for future research. One under-represented research area is the study of networks within and between firms. Information transmission is central for the spread of new productive technologies,[31] and understanding barriers to learning may help explain why productivity differences persist across firms. Supply chain networks may further have a credit or insurance component or may instead hasten the transmission of shocks (Cunat 2007; Garcia-Appendini and Montoriol-Garriga 2013; and Acemoglu et al. 2012). Pricing decisions may also evolve along networks (Gale and Kariv 2009), and there is scope for field research in this space.

Another application with great research promise is electronic payments and mobile networks. While observational studies have measured risk-sharing through mobile networks in response to shocks (Jack and Suri 2014 and Blumenstock et al. 2012), there has been little experimental work on the topic. Relatedly, Munshi (2003) and others have used non-experimental methods to demonstrate the importance of social networks in the migration decision and in migrant job search.[32] Revisiting these questions with new experimental network tools could also be fruitful.

Given the endogeneity concerns endemic to network field experiments, there may be high returns to pairing laboratory experiments with field experiments. While it is tremendously hard to randomly change the shape of real-world social networks, it is possible to randomly embed participants in communication networks in the lab. If researchers find suggestive evidence based on real-world network shapes, they can test those hypotheses in a controlled laboratory setting.

Finally, many of the applications discussed here have policy implications. For example, who should be given scarce information to maximize diffusion? Whose opinions should be elicited in order to target resources toward the poorest residents? How should firms organize themselves to minimize the effects of social comparisons? What is required to sustain informal insurance in underbanked communities? However, many of the implications are not quite ready for use by policy-makers, as they require detailed knowledge of network structure which is costly to collect. Therefore, advances in measurement techniques may help to move forward both the research and the applicability of its findings.

[31] Atkin et al. (2014) are diffusing new technologies to soccer ball firms in Pakistan and intend to measure spillovers. Menzel (2015) is measuring productivity spillovers across production lines in Bangladeshi factories.

[32] See "Community Networks and Migration" by Munshi in this volume. Also see the migration experiment by Bryan et al. (2014).

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