

# Labor and Credit Networks in Developing Economies\*

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## Abstract

The past decade has witnessed a surge of interest in the economic analysis of networks. This chapter is concerned with the role played by labor and credit networks in shaping economic activity in developing countries. The problem of identifying network effects on economic outcomes is first discussed, followed by solutions to this problem that have been put forward in the literature. This chapter concludes by discussing the static and dynamic inefficiencies that can be associated with community-based networks as well as their effect on growth and mobility in developing countries.

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## Keywords

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Migration  
Growth  
Intergenerational mobility

## 1. INTRODUCTION

The past decade has witnessed a surge of interest in the economic analysis of networks. The starting point for this literature is the idea that the social ties that define a network can solve information and commitment problems in environments where markets are inefficient. Because developing economies are, by definition, associated with poorly functioning markets, it is not surprising that much of the empirical literature on social networks has been situated in these economies.

What roles do community-based networks play in developing economies? One role for networks is the provision of information about new technologies, which are constantly becoming available during the development process. Information can spread by word of mouth or individuals can learn from their neighbors actions and experiences with new technologies; a process that has been referred to as “social learning” in the economics literature. [Munshi \(2007\)](#) provides a review of the literature on information networks and social learning in developing countries.

A second role for networks is the provision of mutual insurance. Traditional agrarian economies are characterized by wide fluctuations in income across local areas and over time. In the absence of well functioning credit and insurance markets, kinship networks have historically served and continue to serve as the primary mechanism through which consumption is smoothed in developing countries. A review of the literature on kinship-based insurance networks is provided by [Fafchamps forthcoming](#) in this Handbook.

This chapter is concerned with a third role for social networks; providing jobs and credit for their members. Networks emerge in the labor market to solve information and commitment problems. These arrangements can provide information to their members about job openings. Alternatively, when a new worker’s ability cannot be observed by firms *ex ante*, incumbent workers can provide referrals for capable members from their network. Social ties solve the information problem in this case, while a desire to maintain a firm-specific reputation discourages incumbent workers from referring less capable members of their network. Along the same lines, when the worker’s effort cannot be observed and performance-contingent contracts are infeasible, a foreman can use his social connections and the threat of social sanctions to ensure that the members of his work-team do not shirk on the job.

Networks can also emerge in the credit market to solve information and commitment problems just as they do in the labor market. Bank credit is rarely available to small traders and manufacturers in developing countries. Firms must consequently rely on their family, community, and suppliers for fixed and working capital. Community

networks can screen entrepreneurs, channeling capital to those who are most able. Community networks can also punish members who renege on their business obligations, increasing the level of credit that can be received from the community and from suppliers, often without collateral, in equilibrium.

The most basic prediction from models of labor or credit networks is that individuals with access to stronger networks should have superior outcomes. Workers with a stronger labor network should be more likely to be employed and to have higher wages, conditional on being employed. Entrepreneurs with access to a stronger credit network should enjoy higher profits and their firms should grow faster. Although these predictions are simple and precise, it turns out to be extremely difficult to identify network effects in practice. For example, if we find that a businessman with closer ties to his suppliers achieves higher profits, it is certainly possible that his success derives from his access to a strong network. At the same time, we would expect more able individuals to develop stronger ties with their suppliers in equilibrium. The businessman's success in that case could be driven by his intrinsic talent rather than the connections he has established. Even if individuals are exogenously assigned to networks, changes in network strength over time or across communities could reflect variation in underlying economic opportunities, potentially giving rise to a spurious network effect once again. To take an example from the labor market, suppose that a migrant's employment outcome depends on the size of his network, measured by the stock of migrants from his origin community, at the destination. When conditions are favorable at the destination, individuals will have a greater propensity to migrate, giving rise to a larger network and more favorable outcomes even when network effects are absent.

[Section 2](#) of this chapter begins with a formal discussion on the identification of network effects, paying attention to the problems of selection and endogenous network size described above. Subsequently various approaches that have been taken to deal with these statistical problems will be discussed. Early attempts to estimate network effects used a limited set of individual characteristics and local economic conditions to control for selective entry into the network and endogenous network size (strength). Later attempts used panel data and fixed effects to control for individual heterogeneity, and natural variation in network size or even purposefully designed field experiments to more credibly establish a role for social networks in facilitating economic activity. Empirical research on networks has now moved beyond the identification of network effects to study how these arrangements can distort the allocation of resources, reduce investment in human capital, and reduce economic and social mobility. [Section 3](#) of this chapter begins with three examples, one from the capital market and two from the labor market, documenting the misallocation of resources when networks are active. Subsequently, the negative relationship between the strength of migrant networks and the educational attainment of their members, which has recently been documented in China and Mexico, will be discussed. Finally, the role played by traditional networks in restricting the mobility of their members

when new opportunities become available, as recently documented in the globalizing Indian economy, will be discussed. This chapter concludes in [Section 4](#) with a summary of the progress that has been made this far and suggestions for future research.

## 2. IDENTIFICATION AND ESTIMATION OF NETWORK EFFECTS

This section begins with a description of the network identification problem, drawing on the discussion in [Munshi \(2003\)](#). Although the labor market is the specific setting for this discussion, we will see that the main statistical problems that are identified, selectivity and simultaneity, would appear in an analysis of credit networks as well. Subsequently I review various attempts that have been made to solve these statistical problems.

### 2.1 The identification problem

Consider a migration model in which each individual works for two periods. At the beginning of his working life he makes an irreversible location decision: whether to remain in his origin community and work there or to move to the city. This decision will depend on the payoff at the origin and the destination over the next two periods.

Begin with the payoff at the destination, which will in general depend on the migrant's ability, the strength of his network, and economic conditions. Assume that there is a single occupation at the destination and normalize so that the wage in that occupation equals one. The migrant's (expected) payoff at the destination is then simply the probability of being employed. The employment outcome for individual  $i$  who migrates in period  $t$  can be expressed as,

$$\Pr(E_{it} = 1 | X_{it} = 1) = \beta X_{t-1} + \omega_i + C_t, \quad (1)$$

where  $E_{it} = 1$  if the individual is employed,  $E_{it} = 0$  otherwise.  $X_{it} = 1$  if the individual chooses to work at the destination,  $X_{it} = 0$  otherwise.  $X_{t-1}$  is the measure of migrants from the individual's origin community who moved to the destination in the previous period. We take it that individuals only provide referrals in the second year of their working life once they are established, regardless of their employment status at that point in time.<sup>1</sup>  $\omega_i$  measures the individual's ability and  $C_t$  measures time-varying economic conditions at the destination. Both these terms are unobserved by the econometrician and we will see in a moment that they will both be correlated with  $X_{t-1}$  in equilibrium, creating problems for consistent estimation of the network effect.

The corresponding expression for the individual's employment outcome in period  $t + 1$ ,  $\Pr(E_{it+1} = 1 | X_{it+1} = 1)$  is obtained by replacing  $X_{t-1}$  with  $X_t$ , and  $C_t$  with  $C_{t+1}$ .  $X_t$ ,  $C_{t+1}$  are unobserved by the individual when he chooses his location at the

<sup>1</sup> We could relax this assumption and only allow employed individuals to make referrals, without changing any of the results that follow.

beginning of period  $t$ , and we will see in a moment that this will complicate his migration decision slightly.

Turning to payoffs at the origin, we assume that individual  $i$ 's payoff at the origin in period  $t$ ,  $\Pi_{it}$ , depends on conditions in the rural home place  $Z_t$  but not the individual's ability,

$$\Pi_{it} = \Pi(Z_t). \quad (2)$$

The individual will locate at the destination if the expected return there over the next two periods, net of moving costs, is higher than the expected return at home. The complication that arises immediately is that the size of the network and, hence, the returns at the destination in period  $t + 1$  depend on the measure of migrants that moves with the individual in period  $t$ ,  $X_t$ , so there is a strategic element to the individual's location decision. Ruling out strategic uncertainty and assuming that an interior solution to the migration decision is obtained in each period, there exists a threshold ability  $\underline{\omega}$  such that all individuals with ability greater than  $\underline{\omega}$  choose to locate at the destination.

$$\begin{aligned} X_{it} &= 1 \text{ if } \omega_i \geq \underline{\omega}(X_{t-1}, C_t, E_t(C_{t+1}), Z_t, E_t(Z_{t+1})) \\ X_{it} &= 0 \text{ otherwise,} \end{aligned} \quad (3)$$

where  $E_t(C_{t+1})$  is the predicted employment shock in period  $t + 1$ , and  $E_t(Z_{t+1})$  is the predicted condition at the origin in that period.

Let the distribution of ability in any cohort be characterized by the function  $F$ . In that case, the measure of migrants in period  $t$  is given by the expression:

$$X_t = 1 - F(\underline{\omega}(X_{t-1}, C_t, E_t(C_{t+1}), Z_t, E_t(Z_{t+1}))). \quad (4)$$

Working back one period, the corresponding expression for  $X_{t-1}$  can be obtained as:

$$X_{t-1} = 1 - F(\underline{\omega}(X_{t-2}, C_{t-1}, E_{t-1}(C_t), Z_{t-1}, E_{t-1}(Z_t))). \quad (5)$$

Equation (5) makes clear the bias that can arise when estimating network effects in equation (1). Favorable conditions at the destination lower the ability threshold and, hence, increase migration. Thus high  $C_{t-1}$  in equation (5) is associated with high  $X_{t-1}$ . If economic conditions are (positively) serially correlated, then  $X_{t-1}$  will be positively correlated with  $C_t$  in equation (1), biasing  $\hat{\beta}$  upward.

To characterize the bias due to the unobserved ability term  $\omega_i$ , note that  $E(\omega_i | X_{it} = 1) = \phi(\underline{\omega}(X_{t-1}, C_t, E_t(C_{t+1}), Z_t, E_t(Z_{t+1})))$ , where  $\phi$  is an increasing function of  $\underline{\omega}$ . An increase in  $X_{t-1}$  improves conditions at the destination, lowering  $\underline{\omega}$  and, hence, the migrant's ability in expectation. Selectivity biases  $\hat{\beta}$  downward, but note that this result is driven by the assumption that payoffs at the origin are independent of individual ability. Once this assumption is relaxed, the decision to migrate will depend on the ability differential  $\omega_i - \eta_i$ , where  $\eta_i$  is the individual's ability at the origin. As Borjas (1987) points out, the nature of the selection bias is ambiguous

in this case. If the ability differential is systematically larger for low- $\omega$  individuals, then they will be the first to migrate and unobserved selectivity will bias the network effect upward rather than downward as previously described.

The biases that could undermine the estimation of network effects in the labor market arise in the credit market as well. Consider a regression in which a businessman's level of trade credit is determined by the strength of his network, measured by the number of suppliers he interacts with socially. More capable businessmen might have better social skills and, hence, better relations with their suppliers. These businessmen could also independently receive higher levels of trade credit in equilibrium, giving rise to a potentially spurious network effect. Apart from such selectivity bias, the simultaneity bias associated with endogenous network strength that we discussed in the labor market context could arise in the credit market as well. Continuing with our simple example, the representative businessman might interact with more suppliers and (independently) receive higher levels of trade credit when economic conditions are favorable, giving rise to a spurious network effect once again.

The remainder of this section will describe various strategies that have been adopted to deal with these potential sources of bias. Much of the literature on labor market networks in developing countries has focused on job outcomes for migrants. Migrants are by definition newcomers in the labor market and so will be most susceptible to the information problems that generate a need for job referrals in the first place. Although this survey is primarily concerned with networks in developing countries, I will cover papers on internal migration as well as international migration, particularly the movement of labor from Mexico to the United States, which has received a great deal of attention in recent years. The literature on credit networks is mainly concerned with the relationship between small traders and manufacturers and their suppliers. Because the same statistical problems arise when estimating network effects in the labor or the credit market, applications from both markets will be discussed interchangeably in the discussion that follows.

## 2.2 Using controls to identify network effects

Looking back at [equation \(1\)](#), the key challenge when estimating network effects is to account for selectivity and simultaneity bias, associated with individual ability  $\omega_i$  and economic conditions  $C_t$ , respectively. One approach to deal with this problem is to include observed individual characteristics and measures of economic conditions as controls for those terms.

[Giles, Park and Cai \(2006\)](#) use data from urban China to study the effect of family networks, measured by the number of relatives living in the city, on the speed with which individuals who are involuntarily unemployed are re-employed. Controlling for individual characteristics such as age, education, the last occupation, access to public subsidies and pensions, characteristics of the neighborhood, and household demographic

characteristics, they find that individuals with larger networks are re-employed significantly faster.

Taking a similar approach, [Bian \(1994\)](#) and [Zhang and Li \(2003\)](#) study the effect of connections or *guanxi* on access to non-farm employment in China. The regressor of interest in both these studies indicates whether or not the respondent received help in securing a job. Bian includes individual characteristics such as age, education, party membership, and occupational history as controls, while Zhang and Li include, in addition, characteristics of the local rural community that the individuals are drawn from.

[Winters, de Janvry and Sadoulet \(2001\)](#) place more structure on the network effects by distinguishing between family and community networks, and between current and historical networks. Using data from rural Mexico, they find that family and community networks are substitutes; family networks have little impact on the individual's decision to migrate in communities with well established networks. Individual and household characteristics also have little effect on the migration decision in these communities, indicating that the development of community networks allows initially less favored individuals to migrate. As in the studies on migrants in China, Winters, de Janvry and Sadoulet include controls for individuals characteristics such as age and education, the demographic characteristics of the migrant's household, as well as asset ownership and community characteristics.

Early studies on business networks and trade credit also used controls to account for unobserved selectivity and potential simultaneity bias. [Fafchamps \(1996\)](#) uses data from a survey of 200 Zimbabwean manufacturing firms conducted by the World Bank in 1993 to study the relationship between the entrepreneur's ethnicity and trade credit. Controlling for the age and the size of the firm and the sector that it operates in, he finds that African owned firms receive and grant significantly less credit than non-African firms. A subsequent paper, [Fafchamps \(1999\)](#) uses data from a survey of 60 firms in Nairobi and 60 firms in Harare in 1993–94. Regressing the extent of supplier credit received by the firm on the owner's gender, ethnicity, and a set of firm characteristics, Fafchamps finds that African firms receive significantly less trade credit than Asian or European firms as in the previous study. To provide direct support for a network-based mechanism underlying these differences, Fafchamps experiments with an augmented specification that includes the extent to which entrepreneurs socialize with their suppliers as an additional regressor. Although the coefficient on this variables is positive and significant, as expected, ethnicity and gender continue to have a significant effect on access to trade credit.

[McMillan and Woodruff \(1999\)](#) adopt a similar strategy, using firm and industry controls to estimate the effect of business networks on access to trade credit in Vietnam. They hypothesize that higher levels of trade credit will be offered when (i) it is difficult for the firm to find an alternative supplier (outside options are weak), (ii) the supplier has prior information about the firm's reliability, and (iii) the firm belongs to a business network. Membership in a network is measured by whether

the supplier first heard about the firm through a social contact, whether the firm was managed by a friend or relative of the supplier at the time of first transaction, and the frequency with which the firm interacts socially with other firms in the market. The data support each of the hypotheses and all of the network variables with one exception, receiving information from a family member about the firm's reliability, have a positive and significant effect on the share of goods received by the firm that is paid after delivery. Moreover, these effects remain strong over time, indicating that the network is playing a continuing role sanctioning firms that renege on their obligations, rather than simply providing initial information about the firm's reliability.

Although the studies summarized above control for a number of individual characteristics and economic conditions that could be correlated with measures of the network, the obvious concern is that unobserved variables remain unaccounted for. Observed individual characteristics such as age, education, and occupational experience may not capture traits such as initiative and diligence that play a critical role in determining the individual's market outcomes. Empirical studies on migrant networks in the labor market, for example, use the number of friends or relatives in the city to measure the strength of the individual's network. If individuals with greater ability at the destination have a greater propensity to migrate, then social groups with higher ability will have larger migration rates. The number of friends and relatives in the city could then proxy for the migrant's unobserved ability, giving rise to a spurious network effect. Using received help or the extent of social interaction to measure the network suffers from potential selectivity bias as well, since we would expect more able individuals to receive more help and to be better connected in equilibrium. Indeed, when [Fafchamps \(1999\)](#) regresses bank credit on his measure of the network, the extent to which entrepreneurs socialize with their suppliers, the estimated network effect is positive and significant. While we would expect to see this effect with trade credit, as described above, the fact that the same result is obtained with bank credit as the outcome suggests that the network measure may be positively correlated with unobserved entrepreneurial ability.<sup>2</sup>

One approach to control for unobserved fixed heterogeneity is to use panel data. [Yamauchi and Tanabe \(2008\)](#) use data from the Thai Labor Force Survey 1994–96 to estimate the effect of labor networks, measured by the share of all migrants drawn from the individual's source region, on his employment outcome at the destination. Because the survey includes multiple rounds each year over multiple years, source region fixed effects, survey round dummies, and source region – year fixed effects can be included as regressors. Since the network is measured at the level of the source region, the region dummies capture all fixed heterogeneity, while the the interaction with year (but not survey round) captures changes over time in regional conditions

<sup>2</sup> There is also, of course, the possibility that bank and network credit are complements, although the usual assumption is that they are substitutes.

to some extent. Fisman (2003) similarly uses fixed effects to provide more robust estimates of business network effects than previous studies. His basic specification is

$$Pr(CREDIT_{ij} = 1) = X_{ij}\beta + \gamma T_{ij} + f_i,$$

where  $CREDIT_{ij}$  is a dichotomous variable taking the value one if entrepreneur  $i$  receives trade credit from supplier  $j$ ,  $X_{ij}$  is a vector of firm and relationship characteristics,  $T_{ij}$  equals one if  $i$  and  $j$  belong to the same ethnic group and zero otherwise,  $f_i$  is an individual fixed effect.

Using data from a World Bank survey of firms in several African countries, Fisman seeks to assess whether Asians and Europeans, who have a longer business history than Africans, have stronger business networks. What the fixed effects regression does is to allow for the possibility that Asian and European entrepreneurs are more capable than African entrepreneurs and so would receive more trade credit independently of their networks. With fixed effects, the  $\gamma$  coefficient effectively measures the increased access to credit for a given entrepreneur when a supplier belongs to his own ethnic group, which can be attributed to the role of his ethnic network. The hypothesis in this case is that  $\gamma$  should be larger for the Asian and European firms, which Fisman successfully verifies.

While this estimation strategy improves considerably on previous attempts to identify network effects, one limitation of the fixed effect regression is that it does not account for differences across suppliers. These results could also be obtained if Asian and European suppliers are more likely on average to provide goods on credit, as documented by Fafchamps (1996). Moreover, the fixed effects regression does not account for the possibility that the endogenous matching of firms to suppliers could vary across ethnicities. As discussed in Section 2.1, fixed effects will fully capture constant individual characteristics but will fail to account for unobserved factors that vary over time, or across business transactions in this case, and directly determine the individual's market outcomes.

### 2.3 Using statistical instruments to identify network effects

Given the limitations of using observed individual characteristics and economic conditions to control for  $\omega_i$  and  $C_i$  in equation (1), many recent studies have used instrumental variable regressions to estimate network effects. In a study of agricultural traders in Madagascar, Fafchamps and Minten (2000) regress two alternative measures of output, sales and value added (sales minus purchases), on the firm's working capital, the share of family members amongst its workers, the owner's gender, schooling, and experience, and the owner's social capital. This last variable is measured by the number of non-family traders known to the firm's owner. Recognizing that capital, labor, and social connections are determined endogenously, Fafchamps and Minten instrument for these inputs with the owner's age, place of birth, religion, number of siblings and children, parental characteristics, and history of informal lending and borrowing. Social capital is found to have a positive and significant effect on the firm's output, with and without instruments.

The key omitted variable in this regression is entrepreneurial ability, which would be correlated with the firm's working capital and the owner's social capital, while at the same time directly determining the firm's output. To satisfy the exclusion restriction, the instruments must be uncorrelated with entrepreneurial ability. This is unlikely to be the case for variables such as place of birth, religion, and parental characteristics and, in particular, the owner's history of informal lending and borrowing. Fafchamps and Minten add self-reported traits such as self-reliance and thrift as regressors, but then the limitation of adding a small number of imperfectly measured controls surfaces once again.

The same problem appears in a related paper on business networks and trade credit among agricultural traders in Benin and Malawi (Fafchamps 2003). The network is measured by the number of suppliers known to the trader, the number of clients known to the trader, the number of relatives in trade, and membership in a trade association. As expected, the network variables have a positive and significant effect on trade credit. However, these variables are also evidently (positively) correlated with the trader's unobserved ability, potentially giving rise to a spurious network effect. Fafchamps uses the firm's working capital, the number of clients, and the number of suppliers, all at the time of start-up, the number of relatives in trade, and average trade association membership among traders from the home district as instruments for social capital. To the extent that ability can be (imperfectly) observed in the market, higher ability entrepreneurs will have superior outcomes even at the time of start-up. If individuals with higher entrepreneurial ability select into trade and ability is correlated among members of the kinship group, then the number of relatives in trade will be correlated with the entrepreneur's own ability. By the same argument, trade association membership will be correlated with ability. Thus, each of the proposed instruments could be correlated with unobserved ability, reducing the credibility of the estimates.

Many recent empirical studies on networks, both in the labor and the credit market, focus on migrants. One advantage of these studies is that the origin community serves as the exogenous domain of the network, with the number of migrants from that community providing a convenient measure of the network's strength. In contrast, the business network studies discussed above use individual choices and outcomes to measure network access, giving rise to obvious statistical problems. A second advantage of working with migrant networks is that there is often an accidental aspect to their formation, in which case historical conditions serve as natural instruments for current networks.

Woodruff and Zenteno (2007) study the effect of international migration, via remittances, on investment in Mexican microenterprises, using data from the 1998 National Survey of Microenterprises (ENAMIN). Restricting attention to entrepreneurs who do not reside in their state of birth, Woodruff and Zenteno show that the firm's capital stock is positively associated with the migration rate in the entrepreneur's state of birth, net of migration in the state of residence. Additional controls include the entrepreneur's age and education, the age of the firm, and characteristics of the state of

birth and the state of residence. Robust specifications include state of residence fixed effects. To allow for the possibility that current migration rates in the state of birth could be correlated with contemporaneous entrepreneurial skill in the population or selective entry into (domestic) business, Woodruff and Zenteno instrument for current migration with distance to rail lines in the early twentieth century when migration to the United States was just commencing and networks were first being formed. Distance to rail lines, measured at the level of the state, is negatively correlated with historical and current migration. Using distance to rail lines as an instrument, greater migration in the entrepreneur's birth state is associated with higher investments and profits. To provide direct support for the causal mechanism underlying this relationship, Woodruff and Zenteno also show that receipt of remittances is increasing in the rate of migration in the entrepreneur's state of birth.

These instrumental variable estimates are robust to the presence of unobserved contemporaneous factors that jointly determine levels of international migration and entrepreneurial skill or selective internal migration from the state of birth. However, to the extent that historical migration has persistent effects, we might also expect historical migration to have given rise to persistent (entrepreneurial) traits in the population, undermining the validity of the instruments. In a companion paper, [Woodruff \(2006\)](#) attempts to deal with this problem by estimating the relationship between investment in capital stock and migration in the entrepreneur's state of birth over multiple ENAMIN rounds from 1992 to 1998. Remittances flowing into Mexico increased rapidly over the 1990s and so the expectation is that the estimated migration effect should be increasing over time if migration affects capital investments through remittances. Woodruff verifies that the estimated migration coefficient is indeed increasing monotonically over time.

[Luke and Munshi \(2006\)](#) take a similar approach in their study of labor market networks in urban Kenya, deriving and successfully testing additional predictions that increase confidence in the validity of their instruments. Urbanization in sub-Saharan Africa is a relatively recent phenomenon. Nevertheless, new networks based on traditional kinship ties have emerged in African cities, providing jobs, accommodation, and other forms of support for their members. The marriage institution is the linchpin of these kinship networks, widening their scope while at the same time increasing the individual's social obligations.

Marriage in much of sub-Saharan Africa is exogamous, in the sense that a man is not allowed to marry within his own clan or into any clan that has been designated as being related to his clan. In this kinship system, the man is born into a support network organized around his father's family and then subsequently acquires a new affine network, organized around his wife's family, when he marries. Luke and Munshi's basic empirical objective is to estimate the effect of marriage, and the new affine network it provides, on labor market outcomes for migrants in the city. The identification

problem that arises with such an estimation strategy is that a spurious marriage effect could be obtained if particular types of individuals show a greater propensity to enter the marriage institution. Suppose, for example, that higher-ability individuals who would have performed better on the labor market in any case are more likely to be married, at any given age. Marital status will then proxy for the individual's (unobserved) ability, and a spurious marriage effect could be obtained. Alternatively, if the selection pressures work in the opposite direction, then the estimated marriage effect will be biased downward.

Luke and Munshi's strategy to avoid this selection problem exploits traditional marriage rules among the Luo, an ethnic group that resides primarily in Kenya's Nyanza Province, to construct an instrument for marital status.<sup>3</sup> The Luo migrated south into Kenya from Uganda three to five hundred years ago. Areas lying directly in the path of the incoming migrants were settled by large numbers of unrelated clans, whereas more remote areas were settled later, often by related clans in a single wave. These patterns of historical migration gave rise to wide variation in the local level of clan relatedness across Nyanza Province today. Relatedness determines the efficiency of the matching process and, as expected, areas with a lower proportion of related clans are characterized by higher marriage prevalence at each age. Using data from a survey of male Luo migrants aged 21–45 conducted in Kisumu, the capital of Nyanza Province, in 2001, Luke and Munshi find that marriage significantly increases employment and income, after controlling for the migrant's age. This result is consistent with the presence of an underlying network organized around the marriage institution and is obtained with and without accounting for the endogeneity of marital status. The instrumental variable (IV) estimates are actually substantially *larger* than the OLS estimates, indicative of negative selection into marriage.

To see why this is the case, consider the regression equation that Luke and Munshi estimate:

$$\gamma_i = \beta M_i + \gamma X_i + \epsilon_i, \quad (6)$$

where  $\gamma_i$  is individual  $i$ 's labor market outcome, measured by income or employment.  $M_i$  measures marital status, taking the value one if the migrant is married, zero if he is single.  $X_i$  measures the migrant's age and  $\epsilon_i$  measures his unobserved ability. It is evident from the structure of the regression equation that  $M_i$  and  $\epsilon_i$  must be negatively correlated if the OLS estimate is smaller than the IV estimate.<sup>4</sup>

<sup>3</sup> A number of previous studies have used panel data and individual fixed effects to estimate the effect of changes in marital status on economic outcomes. The obvious drawback of this approach is that entry into marriage could be determined by unobserved economic factors that directly determine changes in outcomes before and after marriage. A spurious marriage effect could be obtained in this case.

<sup>4</sup> Measurement error could also bias the OLS estimates downward toward zero. However, this is unlikely to be a problem with current marital status.

To provide an explanation for this negative selection and to provide additional support for the link between marriage and networks, Luke and Munshi regress the share of the migrant's income remitted to the extended family in the origin location on his marital status and age. Marital status has a positive and significant effect on remittances, with and without instrumenting. Once again, the instrumental variable coefficient is substantially larger than the OLS coefficient. Replacing income or employment with remittances to the origin location as the dependent variable in [equation \(6\)](#), it follows that this result will be obtained if high-ability individuals (with larger  $\epsilon$ 's) remit a greater share of their income (the direct effect) *and* are less likely to be married as before. High-ability individuals benefit less from the kinship network in the city, because they have less use for the services it provides. The progressive *ability tax* identified above would only reinforce their natural propensity to defer entry into marriage.

While historical accident when the Luo first arrived in Kenya might have played a role in determining the relatedness patterns that we see today, it is possible that local conditions, such as climate and soil type, as well as the ability distribution among the arriving settlers, gave rise to particular local relatedness patterns. To the extent that some of these conditions are persistent, relatedness could be correlated with unobserved determinants of the migrant's outcomes in the city and would no longer be a valid instrument. Luke and Munshi verify that relatedness is uncorrelated with observed individual characteristics that are associated with ability, such as the migrant's inherited wealth and education. In addition, they propose a simple falsification test to verify the validity of the instrument. Relatedness should only affect marital status for migrants who arrive in the city as adults. Migrants who arrived in Kisumu as children, with their parents, have many unrelated partners to choose from in the city and so their marriage choices should be independent of relatedness in their ancestral locations. As expected, relatedness affects marital status, employment, income, and remittances among *late* migrants, who arrived as adults. In contrast, relatedness has no effect on marital status or any of the outcomes for the *early* migrants, who arrived before age 21. This useful result rules out the possibility that relatedness simply proxies for individual characteristics that are common to migrants from the same origin location and *independently* determine outcomes in the city. Relatedness appears to affect these outcomes exclusively through its effect on marital status – for late migrants only – satisfying the conditions for a valid instrument.

Although falsification tests of the sort described above provide useful support for historical conditions as determinants of current network strength, the cleanest strategy to identify network effects would exploit exogenous *shocks* at the origin as determinants of migrant-network strength at the destination. Recall from [equation \(5\)](#) that economic conditions at the origin in period  $t - 1$ ,  $Z_{t-1}$ , determined migration in that period, which in turn determined the strength of the network in the subsequent period,  $X_{t-1}$ . Any condition at the origin that determined the decision to migrate but was uncorrelated with conditions at the destination,  $C_t$ , would then serve as a valid instrument for  $X_{t-1}$ .

Munshi (2003) uses rainfall in Mexican origin communities as instruments for destination networks in the United States, merging data from the Mexican Migration Project (MMP) with historical rainfall data collected from each local community.

The MMP has been conducted jointly by researchers based in Mexico and the United States since 1982 (see Massey et al. [1987] for details of the study). In this project a small number of Mexican communities are surveyed each year. Each community is surveyed once only, and a retrospective history of migration patterns and labor market outcomes is obtained from typically 200 randomly sampled household heads. This permits the construction of a panel data set of individual location decisions and labor outcomes, from multiple communities, over a long period of time. The communities in the MMP are drawn from a region in Southwestern Mexico that has traditionally supplied between half and three-quarters of the Mexican migrants to the United States. Migration from this region tends to be recurrent: individuals move back and forth between Mexico and the United States, and only a small fraction settle permanently abroad. If the individual's network at the destination consists of other migrants from his origin-community, then this implies that both the size and the vintage of the network will be changing over time. Variation within the community over time rather than across communities can then be used to estimate network effects.<sup>5</sup>

The regression equation includes as the dependent variable the individual's employment outcome or the type of occupation, conditional on being employed, in the United States in period  $t$ . The most basic regression specification includes the size of the network, measured by the proportion of sampled individuals from the migrant's community who were present in the United States at that point in time, the migrant's unobserved ability ( $\omega_i$ ), and unobserved labor market shocks ( $C_t$ ) as determinants of his labor market outcome. Recall from the discussion in Section 2.1 that both  $\omega_i$  and  $C_t$  are correlated with network size,  $X_{t-1}$ . With panel data, individual fixed effects control for  $\omega_i$  under the assumption that ability does not vary over time. From the discussion above, rainfall in the Mexican origin community, which is included in  $Z_{t-1}$ , can be used as an instrument for the size of the network at the destination,  $X_{t-1}$ . Once fixed effects are included, rainfall shocks at the origin effectively serve as instruments for changes in network size at the destination.

All of the specifications in Munshi (2003) distinguish between recent migrants, who arrived at the destination in periods  $t - 1$  and  $t - 2$ , and established migrants who arrived in period  $t - 3$  or earlier. Instrumenting for the stock of recent migrants with recent-past rainfall (the  $t - 1$ ,  $t - 2$  average) and the stock of established migrants with distant-past rainfall (the  $t - 3$  to  $t - 6$  average), Munshi finds that a greater stock of established migrants significantly increases the probability that a migrant from that origin community

<sup>5</sup> The nature of the MMP survey leads to three data problems: measurement error in the network variable, recall bias due to the retrospective nature of the data, and missing migrants on account of the fact that some of the migrants might not have been present at the time of the survey. Munshi discusses each of these problems and verifies that the results are robust to each of them.

will be employed and will have a more remunerative non-agricultural job (conditional on being employed) in period  $t$ . In contrast, the stock of recent migrants has an insignificant effect on these outcomes.

In the corresponding reduced form regressions, recent-past ( $t - 1$ ,  $t - 2$ ) rainfall has little effect on labor outcomes at the destination, whereas distant-past ( $t - 3$  to  $t - 6$ ) rainfall is negatively correlated with these outcomes. Consistent with the proposed mechanism underlying these results, period  $t$  to  $t - 2$  rainfall has a positive and significant effect on employment at the *origin* in period  $t$ , but  $t - 3$  to  $t - 6$  has no effect. Low rainfall at the origin thus spurs migration, which improves labor market outcomes at the destination with a lag once the new arrivals have a chance to establish themselves and provide referrals for other members of the community. An alternative explanation for this result is that individual ability is not constant, as we have assumed, but grows with experience at the destination. The negative correlation between labor outcomes at the destination and distant-past rainfall could then simply reflect the fact that there are more established migrants around who are independently performing better. To rule out this possibility, Munshi restricts attention to recent migrants who arrived in period  $t$  and  $t - 1$  and shows that their outcomes are also improved when there are more established migrants at the destination (rainfall was relatively low in periods  $t - 3$  through  $t - 6$ ).<sup>6</sup>

In a recent paper, Beaman (2010) exploits the (conditionally) random assignment of refugees across locations in the United States to estimate the effect of social networks on employment and wages. The key insight from her theoretical analysis is that an increase in the size of the network can temporarily worsen labor market outcomes for its members by increasing competition for scarce referrals. The second prediction of the model is that the effect of this one-time increase in network size should be subsequently monotonically increasing over time as employment levels in the exogenously larger cohort and, hence, its ability to provide referrals grows. Including nationality and location fixed effects, and their interaction with time, Beaman verifies each of these predictions using data on refugees resettled by one agency over multiple years. While Munshi (2003) exploits variation at the origin to generate changes in the size of the network at the destination, Beaman uses exogenous variation in resettlement patterns across U.S. locations, conditional on the fixed effects and their interactions, to estimate network effects. Despite these differences in the source of exogenous variation, both studies find evidence that networks are active and that established members of the networks are most useful in improving labor market outcomes.

<sup>6</sup> The regression equation contains no individual characteristics other than the fixed effects. Most time varying characteristics, such as changes in family structure, that determine the individual's outcome at the destination would be uncorrelated with the rainfall instrument, measured many periods in the past. Apart from individual characteristics, the migrant's reservation wage or search intensity could also respond to rainfall at the origin. Although this alternative explanation generates higher employment among the migrants following a negative rainfall shock, it does not explain the long (four-year) delay before employment at the destination starts to rise. Moreover, low rainfall leads to improved occupational outcomes – a shift into nonagricultural jobs – among the migrants. A lowering of the reservation wage cannot explain this feature of the data.

## 2.4 Using indirect inference to identify networks

There are essentially two challenges associated with the empirical analysis of networks. First, networks are by their nature non-transparent and difficult to measure. Second, the composition and the size of the network may respond to (unobserved) changes in the economic environment that may directly determine the outcomes of interest. The studies described above use ethnicity or origin location to define the exogenous domain of the network and exploit various sources of variation to estimate network effects. In many situations, however, plausible instruments for the network will be unavailable or the network may be difficult to measure. In that case, an indirect approach may be used to demonstrate that networks are active.

An early example of this approach is [Banerjee and Munshi's \(2004\)](#) study of credit networks in the Indian knitted garment industry. 70% of India's exports in this sector are supplied by firms in the South Indian town of Tirupur. The textile industry in Tirupur was initially dominated by a local trading community. However, after a prolonged period of labor unrest in the 1960s it was taken over by the Gounders, a community whose previous economic activity had been confined to agriculture ([Swa-minathan and Jeyaranjan 1994](#)). For the next twenty years, the industry continued to be dominated by the Gounders and catered almost exclusively to the domestic market. Starting from the mid-1980s, however, the export of knitted garments from Tirupur started to grow extremely rapidly and by the early 1990s, the annual growth rate was above 50%. This generated an inflow of new entrepreneurs from outside Tirupur. In 1996, when Banerjee and Munshi conducted a survey of firms in the industry, about half of the exporters were Gounders while the rest belonged to traditional business communities drawn from all over the country. Banerjee and Munshi exploit this change in the sociological composition of Tirupur's production cluster to provide empirical support for the role played by community networks in providing credit for their members.

Their analysis is based on retrospective panel data over the 1992–1995 period collected from approximately 150 exporters with different levels of experience and belonging to different communities. Two stylized facts motivate the theoretical model and the strategy to identify the underlying networks. First, exports grow faster for the Outsiders than for the Gounders at all levels of experience. Second, the Gounders use roughly twice as much capital per unit of production (exports) as the Outsiders at all levels of experience. Let the export trajectory be determined by entrepreneurial ability and capital, and assume that these inputs are complements. If the export trajectory is steeper for the Outsiders despite having lower capital stock, they must then have higher ability. If ability and capital are complements, and the Outsiders have higher ability, then the Gounders will only invest more if the cost of capital is lower for them. The two stylized facts thus imply that the Outsiders must have higher ability on average *and* that the Gounders must face a lower cost of capital. The fact that different communities effectively

face different interest rates suggests that credit does not cross community lines and that networks must allocate resources within their respective communities. The Gounders are a wealthy landowning community and the garment export business was their first foray outside agriculture. Given that they have few alternative uses for their capital, unlike the Outsiders from well established and diversified business communities, it makes sense that community-specific (within-network) interest rates are lower for them.

The indirect approach just described does not estimate a network *effect*. What it does instead is to present a set of stylized facts and then derive conditions under which these facts are consistent with the presence of underlying networks. To complete an analysis of this sort it is necessary to rule out alternative non-network explanations for the stylized facts. In the current application, could the observed differences between communities be generated by differences in individual ability alone? The model tells us that this could be the case if ability and capital were substitutes rather than complements. The Outsiders, who are endowed with higher ability on average for historical reasons, could invest less in fixed capital and still end up with a steeper export trajectory. The Gounders with lower ability would invest more, even if both communities face the same interest rate, because ability and capital are substitutes. To rule out this explanation, Banerjee and Munshi look within the community. Among the Gounders and, separately, among the Outsiders, firms with a steeper export trajectory invest more, consistent with the hypothesis that ability and capital are complements. It is only when we look *across* communities that less capitalized firms grow faster, presumably because interest rates are positively correlated with ability at the level of the community.

Having provided empirical support for the key assumption of the model, Banerjee and Munshi proceed to rule out other differences between the communities as explanations for the empirical results. They consider differential access to labor and outsourcing, differential access to politically provided inputs, differential access to non-network capital, differential propensity to exit, and differential access to buyers, using the entire set of empirical results (within and across communities) to rule out each in turn.

More recently, [Munshi forthcoming](#) uses an indirect approach to provide theoretical and empirical support for the role played by community networks in supporting entrepreneurship. The starting point for his analysis is the observation that parental experience in business is an important determinant of entrepreneurship in many developing countries. Businessmen provide credit and connections for their sons, who are also instilled with traits in childhood that prepare them for careers in business. In economies where inherited wealth and family connections are so useful, first-generation entrepreneurs can only compete successfully with their established rivals with the support of fellow entrants from their community. The path to entrepreneurship in countries such as India has consequently been historically characterized, and continues to be characterized by the movement of entire groups into business ([Chandravarkar 1994](#), [Rudner 1994](#), [Damodaran 2008](#)).

Munshi develops a model of occupational migration that describes the process through which a community moves into a new occupation – business in a particular industry – over the course of a single generation. A sufficiently large influx of entrepreneurs is needed to set the network on a positive growth trajectory. Once the transition has been initiated, the main result of the model is that the new network will strengthen most rapidly in communities with *weak* outside options. As this network strengthens endogenously over time it will be able to support an increasing number of first-generation entrepreneurs, who are less competent businessmen on average, giving rise to relatively high inter-generational mobility in historically disadvantaged communities.

To test these predictions, Munshi takes advantage of a recent historical episode in which a traditionally disadvantaged Indian community made the transition from agriculture and industrial labor into the diamond business over a thirty-year period. Two traditional business communities – the Marwaris and the Palanpuris – dominated the Indian diamond trade from its inception in the mid-1960s, leaving the cutting and polishing of the rough stones to the Kathiawaris, a lower caste of agricultural laborers. The point of departure for Munshi's analysis is a supply shock that hit the world diamond industry in the late 1970s with the opening of Australia's Argyle mines. This allowed a relatively large number of Kathiawaris to move into business, satisfying the condition needed for the new network to form. These early entrants took advantage of this opportunity to bring other members of their community into the business, and the number of Kathiawari firms subsequently grew rapidly over time.

Once the first Kathiawaris had entered, the model predicts that the subsequent transition into business should have been relatively rapid in this historically disadvantaged community with weak options outside the diamond industry. Based on data collected from a survey of nearly 800 diamond export firms, Munshi finds that while there is a mild weakening in the inherited business background of the Marwaris and Palanpuris over time, there is indeed a particularly steep decline in the background of the entering Kathiawaris from the late 1970s onward. Although 70% of the Kathiawaris who entered the industry in 1970, before the supply shock, reported that their father was a businessman, this statistic declines steadily and drops below 20% by 2000.

Munshi's interpretation of this result is that a rapidly strengthening industry-specific network was able to support Kathiawari entrants with increasingly weak (family) business backgrounds over time. The critical step in the diamond production process is accessing rough diamonds on supplier-credit from the Antwerp market. Exporters who have established long-term relationships with select suppliers provide referrals (stand guarantor) for other members of their network, allowing individual exporters to draw from a wide range of suppliers from one period to the next. These referrals, linking many exporters over time, are difficult to measure. As in [Banerjee and Munshi \(2004\)](#), Munshi takes an indirect approach to identify the role played by the network in supporting entrepreneurship in his analysis. The key to his approach is the mismatch between

entrepreneurial characteristics and outcomes across communities. Despite the relatively steep decline in the business background of Kathiawari entrants over time, firm-level export data over the 1995–2004 period indicate that the Kathiawaris kept pace with their more established rivals. Indeed, once the compositional change in the industry is accounted for with firm fixed effects, the Kathiawari export trajectory is significantly steeper than the corresponding trajectory for the Marwaris and Palanpuris, precisely as predicted by the model, indicating that there is an underlying force at work boosting the performance of the Kathiawari firms. Although alternative non-network hypotheses such as declining outside options for the Kathiawaris can explain the observed changes in the business background of the Kathiawaris, they cannot explain the accompanying export results, with and without fixed effects, which go in the opposite direction.<sup>7</sup>

### 3. NETWORKS, GROWTH, AND EFFICIENCY

The studies discussed thus far have attempted to demonstrate that networks improve outcomes for their members. This section discusses results from recent studies that have focused instead on the negative consequences of networks; the static and dynamic inefficiencies that are sometimes associated with these institutions as well as the decline in the incentives of their members to invest in human capital.

#### 3.1 Networks and the misallocation of resources

While networks may facilitate economic activity within well-defined communities, an obvious limitation of these non-market institutions is that goods and services cannot be traded across community lines. [Banerjee and Munshi's \(2004\)](#) analysis of credit networks highlights this problem, which results in the more competent group – the Outsiders – investing less in fixed capital despite the fact that ability and capital are complements. This misallocation contrasts with the outcome in an efficient market where capital stock would be matched perfectly with entrepreneurial ability, irrespective of community affiliation.

Apart from restrictions on trade, favoritism can also give rise to inefficiencies in network-based economies. [Bandiera, Barankay, and Rasul \(2009\)](#) study the effect of social relationships between managers and workers on productivity on a British fruit-farm. The managers and the workers are university students from eight Eastern European countries who are hired for one fruit picking season and live on the farm-site for the duration of their study. Workers pick fruit, while managers are in charge of logistics, reassigning workers to new rows when they are finished and providing them with new crates when they are full.

<sup>7</sup> Kathiawari firms tend to specialize in small stones. If the supply of rough stones, and firm profits, were growing particularly rapidly in this niche over time, then Kathiawari firms with weaker business backgrounds could have entered the industry and retained their competitiveness over time. However, scrutiny of the data indicates that the small-stone segment was actually growing more competitive over time.

In 2003, the authors designed and implemented a field experiment in which managers were paid a fixed wage in the first half of the picking season and the fixed wage plus a daily performance bonus in the second half of the season. The performance bonus was specified to be increasing in the average productivity of the workers on the manager's field on that day. This experiment was designed to assess the effect of social connections between workers and managers on managerial effort and worker productivity, under each compensation scheme. For this purpose, social connections were measured by common nationality, residential proximity on the farm, and time of arrival.

To formally derive the effect of social relationships on managerial effort and worker productivity, consider a simplified version of the model developed by Bandiera, Barankay, and Rasul. In this model there are two types of workers with ability  $\theta_1$  and  $\theta_2 < \theta_1$ . Each manager is assigned to two workers, one of each type, each day. To begin with, ignore social affiliation and assume that worker ability and managerial effort are complements. To derive the efficient allocation, the Central Planner allocates effort  $m_1$  to worker 1 with ability  $\theta_1$  and effort  $m_2$  to worker 2 with ability  $\theta_2$  to maximize  $\theta_1 m_1 + \theta_2 m_2$ , subject to the constraint  $m_1 + m_2 \leq \bar{m}$ . The efficient allocation is evidently  $m_1 = \bar{m}, m_2 = 0$ , with the manager allocating all his resources to the more able worker.

Next, consider a compensation scheme in which the manager receives a fixed wage, while the workers are compensated on the basis of their productivity ( $\theta_1 m_1, \theta_2 m_2$  respectively). Let the manager be socially connected to only one of the workers. Because he receives a fixed wage, the manager will allocate all of his effort to the worker with whom he is socially connected. This could be because he has other-regarding (altruistic) preferences towards that worker or because only socially connected workers can credibly commit to compensating managers who have supported them, *ex post*. If the manager is socially connected to worker 1, the respective worker productivities will be  $\theta_1 \bar{m}$  and 0, matching the efficient allocation. If the manager is socially connected to worker 2, the respective productivities will be 0 and  $\theta_2 \bar{m}$ , resulting in a decline in total surplus relative to the efficient allocation.

Finally, consider a compensation scheme in which the manager receives a performance bonus in addition to the fixed wage, while the workers continue to be rewarded on the basis of their productivity. The difference between this scheme and the fixed wage scheme is that the manager is now compensated directly by the firm when his workers are more productive. When the manager is socially connected to worker 1, his pecuniary incentives and his social incentives are aligned and he will certainly allocate all his effort to that worker. The productivity of the workers is  $\theta_1 \bar{m}$  and 0, respectively, matching the efficient allocation. When the manager is socially connected to worker 2, he will continue to allocate all his effort to worker 1 if the pecuniary incentive associated with the performance bonus dominates the social incentive.

This characterization of managerial incentives under alternative compensation schemes yields a number of testable predictions: First, workers of both types are more

productive when they are socially connected to the manager under the fixed wage scheme. Second, social connections have no effect on worker productivity when the performance bonus is introduced (under the maintained assumption that the pecuniary incentive dominates the social incentive for the manager). Third, restricting the analysis to worker-days when workers are socially connected to their managers, shifting from the fixed wage to the performance bonus scheme should result in a decline in productivity for low-ability workers but no change for high-ability workers. Fourth, restricting the analysis to worker-days when workers are socially unconnected to their managers, shifting from the fixed wage to the performance bonus should have no effect on the productivity of the low-ability workers but should increase the productivity of the high-ability workers.

The empirical analysis is based on productivity information over approximately 10 thousand worker-days. In practice, multiple managers and workers are assigned to a given field, so a worker is assumed to be socially connected on a given day if he has ties to any of the managers (the results are robust to using the probability of a social tie as well). High (low) ability workers are defined as those workers whose productivity is above (below) the median in the performance bonus regime when social affiliation is presumed to be irrelevant. If workers and managers were randomly assigned to fields from one day to the next, a simple comparison of means would be sufficient to test the predictions of the model. To allow for the possibility that assignments are non-random, Bandiera, Barankay, and Rasul verify that the results are robust to the inclusion of worker, manager, and field fixed effects. The identifying assumptions in this case are (i) that unobserved determinants of workers' allocation to managers are orthogonal to the compensation scheme in place, and (ii) that the effect of social connections on productivity unrelated to the managerial incentive scheme should remain unchanged over time.

The main finding from the empirical analysis is that when managers are paid fixed wages, the worker's productivity is 9% higher when he is socially connected to the manager. When managers are paid performance bonuses, being socially connected has no effect on worker productivity. The fact that managers change their behavior when their incentives are more closely aligned with the firm's implies that favoritism must lower the total surplus, as is evident from the simple model described above. In this particular context, social ties provide no productivity benefit, so this result cannot be generalized. Nevertheless, it does highlight one potential drawback of economic arrangements based on social connections.

Bandiera, Barankay, and Rasul (2005) describe and identify another source of inefficiency – collusion – associated with social networks, in the same research setting. In 2002, the authors designed and implemented a different field experiment in which the workers on the fruit-farm faced a relative incentive scheme in the first half of the picking season and a piece-rate scheme in the second half of the picking season. The worker's compensation per unit weight of fruit that he picks is fixed under the piece-rate scheme

and decreasing in the average productivity of all the workers on his field under the relative incentive scheme. Although the relative incentive scheme adjusts for differences in picking conditions across fields and over the course of the season, it also opens up the possibility of collusive behavior as shown below.

Consider a simplified version of Bandiera, Barankay, and Rasul's model with  $N$  workers on a field. Each worker exerts  $e_i$  units of effort, which maps one-for-one into his productivity (the amount of fruit that he picks). Under the relative incentive scheme, the compensation per unit of output is  $\gamma/\bar{e}$ , where  $\bar{e}$  is average productivity across all workers in the field (determined *ex post*). Workers have the same ability, with the cost of effort described by the convex function  $\theta e_i^2/2$ . When workers on the field are socially connected, worker  $i$  places weight  $\lambda \in (0, 1]$  on the earnings of his co-workers. When workers are unconnected,  $\lambda = 0$ . Worker  $i$  will then choose  $e_i$  to maximize,

$$\frac{\gamma}{\bar{e}} e_i + \lambda \sum_{j \neq i} \frac{\gamma}{\bar{e}} e_j - \theta \frac{e_i^2}{2},$$

which provides us with the following first-order condition:

$$\gamma \left[ \frac{1}{\bar{e}} - \frac{1}{N} \frac{e_i}{\bar{e}^2} \right] - \lambda \sum_{j \neq i} \frac{1}{N} \frac{\gamma}{\bar{e}^2} e_j - \theta e_i = 0.$$

Setting all worker outputs to be the same in the symmetric Nash equilibrium,

$$e^* = \left[ (1 - \lambda) \frac{\gamma}{\theta} \frac{N - 1}{N} \right]^{1/2}.$$

It follows immediately that output in the cooperative or collusive equilibrium  $e_C^*$  (with  $\lambda \in (0, 1]$ ) will be lower than output in the noncooperative equilibrium  $e_{NC}^*$  (with  $\lambda = 0$ ).

One strategy to identify such collusion would be to compare output on fields where workers were socially connected with output on fields where workers were socially unconnected, with the relative incentive scheme in place. Bandiera, Barankay, and Rasul do adopt this strategy as discussed below, but the obvious concern is that social connectedness is correlated with unobserved field or worker characteristics. An alternative strategy compares productivity under the relative incentive scheme with piece-rates.

Under the piece-rate scheme, the compensation per unit of output  $\beta$  is set *ex ante* and worker  $i$  chooses  $e_i$  to maximize

$$\beta e_i - \theta \frac{e_i^2}{2},$$

so that output in equilibrium  $e_{PR}^* = \beta/\theta$ . Output under the relative incentive scheme cannot be compared directly with output under piece-rates. However, if the compensation per unit output is lower *and* the output is higher under piece-rates, then there

must be collusion. To see why this is the case, note that the first condition implies that  $\beta < \left(\frac{\gamma\theta}{1-\lambda} \frac{N}{N-1}\right)^{1/2}$  and the second condition implies  $\left[(1-\lambda) \frac{\gamma}{\theta} \frac{N-1}{N}\right]^{1/2} < \frac{\beta}{\theta}$ . Combining the two inequalities and assuming  $N$  large, it follows that  $\lambda > 0$ . Bandiera, Barankay, and Rasul use this test to provide additional support for the presence of collusion on the fruit-farm.

To implement the test described above, Barankay, Bandiera, and Rasul compare productivity for 142 workers in 22 fields over 108 days, with the compensation switching exogenously from the relative incentive scheme to piece-rates midway through the season. They begin the analysis by verifying that compensation per unit of output was lower under piece-rates than under the relative incentive scheme. They then proceed to show that output is significantly greater with piece-rates, which implies from the discussion above that collusion was lowering output under the relative incentive scheme. This result is robust to the inclusion of worker and field fixed effects, a linear time trend, and measures of the worker's experience and the field's life-cycle. Additional robustness (placebo) tests verified that (i) there was no change in worker-productivity in the second half of the 2004 season when piece-rates were in effect throughout, and (ii) there was no change in productivity for workers and fields that did not switch incentive schemes in 2002. As a final direct test of collusion, Bandiera, Barankay, and Rasul verify that the worker's output is declining in the share of co-workers on the field who are his friends under the relative incentive scheme, but not with piece-rates. In the simple framework developed above, an increase in the share of co-workers who are friends is equivalent to an increase in  $\lambda$ . This would lower  $e_C^*$ , but would have no effect on  $e_{NC}^*$  (if there were no collusion) or  $e_{PR}^*$ .

### 3.2 Networks and investment in human capital

Migrants benefit disproportionately from labor networks in developing and developed economies. This is because migrants are, by definition, newcomers in the labor market and so the information problems that give rise to networks will be most severe for them. Migrants also typically end up in low-skill (blue-collar) occupations, which are disproportionately networked in most economies. As a community network strengthens at a particular destination, providing low-skill but relatively high-paying jobs for its members, the incentives for potential migrants at the origin to invest in human capital will decline. A number of recent papers have explored this negative relationship between migrant networks and human capital in the context of internal and international migration.

Hanson and Woodruff (2003) use the 2000 Mexican census to show that migration increases accumulated schooling (number of school grades completed) for 10–15 year olds. However, McKenzie and Rapoport (2005, 2006) show that a negative relationship between migration and education appears once the analysis is extended to older ages. Their analysis is based on the 1997 National Survey of Demographic Dynamics

(ENADID). A household is defined as having a migrant if any member aged 19 and over has ever been to the United States to work or has moved to the United States in the last five years for any other reason. Historical state-level migration rates (from 1924) are used as instruments for migration status to estimate the effect of migration (networks) on education. McKenzie and Rapoport find no effect of migration on the educational attainment of 12–15 year olds, but a strong negative effect on 16–18 year olds. Partitioning the sample by gender, they find a negative effect on school attendance for 16–18 year old boys and girls, as well as for 12–15 year old boys.

De Brauw and Giles (2007) study the effect of new migrant opportunities on school attendance in rural China. Primary education is almost universal in China and the critical decision is whether to enroll in high school. Opportunities to migrate to urban areas are determined by the availability of identity (ID) cards, which were provided to communities at different points in time. Using data from household and village surveys conducted in 52 villages in 2004, DeBrauw and Giles study the effect of the availability of ID cards on rural-urban migration and educational attainment.

Their survey collected information on educational attainment, birth year, occupation, and work and migration histories of all current and former residents of the sampled households. Based on this information and the year of issue of the ID cards, they find that migration levels are roughly unchanged over time prior to the availability of the ID cards and then *increase* steadily, suggesting that urban networks may be strengthening and attracting more migrants over time. The probability of being enrolled in high school also demonstrates no trend prior to the availability of the ID cards, and then subsequently *declines* steadily over time. Although the time of issue of the ID cards across villages is potentially endogenous, the key to DeBrauw and Giles' empirical strategy is the discontinuous trend-break in education and migration *within* the village immediately following the issue of the ID cards.

While the studies discussed above are concerned with the effect of migration (networks) on *investments* in education, McKenzie and Rapoport (*forthcoming*) study how migrant *selection* by level of education varies with the strength of the network, measured by the level of migration in the community. The model of selective migration that they develop is based on the idea that the cost of migration is declining with individual education and that this negative relationship is weaker in communities with stronger migrant networks. In addition, McKenzie and Rapoport assume lower returns to education in the United States than in Mexico. These two assumptions, taken together, imply that negative selection will be observed in communities with strong networks, where migration costs are effectively independent of education levels. In contrast, positive selection could be observed in communities with weak networks if the migration-cost effect dominates.

Munshi's forthcoming model of occupational migration can be extended to generate positive and negative selection using variation in the returns to education alone,

without making assumptions about the relationship between education and migration costs. Let individual heterogeneity now be measured by education rather than by ability, and consider the process of geographical rather than occupational migration. Let the returns to education in Mexico be constant across communities. Because migrant networks tend to supply their members with relatively high-paying but low-skill jobs, as described above, migrants from communities with strong networks are more likely to end up in such jobs. The returns to education in the United States are consequently declining in the strength of the migrant network, although the payoff in levels may be higher in communities with stronger networks. If the returns to education are steeper in the United States than in Mexico when networks are absent, but shallower when networks are strong, then there exists a threshold network strength above (below) which negative (positive) selection on education will be observed among the migrants.

To test this prediction, McKenzie and Rapoport regress migrant status on educational attainment and the interaction of education with network strength, including community fixed effects to estimate the selection patterns within the community. The ENADID data are utilized, as in their other work discussed above. An individual is defined to be a migrant if he is aged 15–49 and worked in the United States for the first time in 1996–1997, just prior to the survey, although a more appropriate measure would be whether the individual had ever migrated. The strength of the network is measured by the proportion of the community that has ever migrated, with state-level migration in 1924 used as an instrument for the potentially endogenous measure of network strength. The coefficient on the uninteracted education variable is positive and significant and the coefficient on the interaction of education and network strength is negative and significant, precisely as predicted by the model.

### 3.3 Networks and mobility

The preceding section described results from recent studies indicating that strengthening migrant networks, with the low-skill but relatively high-paying jobs they provide, can lower investments in human capital among potential migrants. This decline in human capital does not have negative welfare consequences unless there are externalities associated with investments in human capital or if communities get locked into low human capital equilibria. [Munshi and Rosenzweig \(2006\)](#) explore this last possibility, documenting the presence of dynamic inefficiencies in the traditionally heavily networked Mumbai labor market.

Schooling in Mumbai can be either in English or Marathi, the local language. Marathi schooling effectively channels the child into blue-collar jobs, traditionally dominated by caste-based networks, whereas more expensive English schooling substantially increases the probability that the child will obtain a high-wage white-collar job in the future. The starting point of Munshi and Rosenzweig's analysis is the economic and financial liberalization of the Indian economy in the 1990's. This resulted

in an increase in the returns to English schooling. Based on a survey of the parents of students who entered school (first grade) over the 1982–2001 period, Munshi and Rosenzweig uncover an accompanying increase in the proportion of children sent to English schools over the course of the 1990's. However, this shift into English schools varied substantially by caste and by gender within caste. High caste boys and girls in the oldest cohort in the sample (who entered first grade in 1982) are much more likely to be schooled in English than lower castes in the same cohort, reflecting the fact that upper castes historically had access to administrative and professional jobs, while lower castes were concentrated in blue-collar jobs. While these caste differences persist over the next 10 cohorts, the caste-gap narrows dramatically for the girls in the 1990's, coinciding with the restructuring of the economy. However, there is no convergence for the boys. The key question is why the lower-caste boys seemingly fail to take advantage of these new economic opportunities.

Munshi and Rosenzweig develop a simple model of schooling choice to explain these patterns. Consider a population with a continuum of individuals. Each individual  $i$  is endowed with a level of ability  $\omega_i \in \{0, \frac{1}{2}, 1\}$ . We can think of ability in this context as pre-school human capital. He lives for three periods, studying in the first period and working in the remaining periods. Schooling choice is restricted to instruction in English or Marathi. Occupational choice is restricted to white-collar and working class jobs. Education in English is required to obtain a white-collar job, but is more expensive than Marathi education, which is assumed for simplicity to be costless. Occupational choice is based on the wage that the individual will receive in the white-collar and the working class job, net of the pecuniary cost of schooling. Each individual then makes his schooling decision based on the type of job that he (correctly) anticipates he will occupy in the subsequent period. If he prefers to hold a white-collar job then he will study in English, if not he will study in Marathi which is less costly.

Each individual is born into a community or caste. There is a large number of communities in this economy, and we normalize so that the measure of individuals in each cohort or generation of a community is equal to one. To simplify and highlight the role of network externalities in intergenerational occupational persistence assume that the distribution of pre-school human capital does not vary over generations or across communities.<sup>8</sup> Within each community-generation there is a measure  $P_L$  of low types (with ability  $\omega = 0$ ) and a measure  $P_M$  of medium types (with ability  $\omega = 1/2$ ).

On the demand side of this labor market, firms operate competitively in both the working class and the white-collar sectors. The white-collar worker's ability, and hence his productivity, can be observed perfectly and so the white-collar wage (net of schooling costs) is specified to be  $\theta\omega$ . Here  $\theta$  represents the returns to ability in the white-collar

<sup>8</sup> This assumption is relaxed in the empirical work described below by allowing for heterogeneity in ability across communities.

job, which in our set up also reflects the returns to English education. In contrast, the nature of the production technology prevents working class firms from directly observing their employees' ability before they commence work. If the firm is unable to specify a performance-contingent wage contract, it will use referrals from its incumbent workers to hire new employees, generating a role for the network in the working class jobs alone. The expected working class wage over the individual's working life is specified to be the proportion  $P$  of the previous generation (three-year olds) in the community that are employed in the working class job when he enters the labor force.

We now proceed to derive the different occupational distributions, and hence schooling equilibria, that can be sustained across communities with the same ability distribution. Each individual chooses the occupation, and hence the language of instruction, that maximizes his net return. This return depends on his own ability, as well as the proportion of his community in the previous generation employed in the working class occupation. Under conditions specified below, with three levels of ability, three distinct schooling equilibria can be sustained within communities: (1) only low types choose Marathi education, (2) low and medium types choose Marathi education, (3) everyone in the community chooses Marathi education.

**Condition 1:**  $P_L < \frac{\theta}{2}$

**Condition 2:**  $\frac{\theta}{2} < P_L + P_M < \theta$

**Condition 3:**  $\theta < 1$ .

It is easy to verify that once a community is exogenously assigned a particular occupational distribution, this distribution will persist unchanged over many generations when the conditions above are satisfied.<sup>9</sup> This intergenerational state dependence is a consequence of the network externality associated with the working class occupation. It implies, in turn, that the probability that any individual  $i$  drawn randomly from *jati*  $j$  will be schooled in English ( $E_{ij} = 1$ ) is related to the proportion of men in the previous generation employed in the working class job,  $P_j$ :

$$Pr(E_{ij} = 1) = 1 - P_j. \quad (7)$$

This expression will serve as the starting point for the empirical analysis described below, where the relationship between schooling choice in the current generation and the occupational distribution in the previous generation will be examined, to identify the presence of an underlying community-based network.

The state dependence at the level of the community derived above is obtained under the assumption that the parameters of the model,  $P_L$ ,  $P_M$ ,  $\theta$  remain stable over time. To explore the effect of the increase in the returns to English ( $\theta$ ) in the 1990s,

<sup>9</sup> It is merely necessary to show that no individual wishes to deviate from the occupation, and hence schooling choice, assigned to his type in his community in the previous generation, for each of the schooling equilibria. An equilibrium in which everyone chooses English could also be sustained but this can be ruled out by assuming that a single individual employed in the working class occupation earns  $\epsilon$  above zero.

we now allow for multiple cohorts within each generation. If  $\theta$  remains constant within a generation, then the results derived above follow through without modification for all cohorts. However, if  $\theta$  increases across successive cohorts, holding  $P_j$  constant, then schooling choice within a community could change over the course of a single generation. When  $\theta$  just crosses one, high ability boys belonging to communities that were traditionally in equilibrium 3 switch to English. When  $\theta$  subsequently reaches  $2(P_L + P_M)$ , medium ability boys in communities that were traditionally in equilibrium 2 or equilibrium 3 switch to English, at which point schooling choice across all communities will converge.

Although the network externality described above can explain the persistence of traditional occupational patterns within the community over many generations, and hence the initial caste-gap, it cannot by itself explain the absence of convergence over the 1990s among the boys as the returns to English grew. To explain this absence of convergence, Munshi and Rosenzweig consider the possibility that heavily networked (working class) communities might have put restrictions on occupational mobility, and hence schooling choice, in place to preserve the viability of the community network. Note that women historically did not participate in Bombay's labor market and so did not benefit from the caste networks.

To understand why restrictions on mobility might emerge, define a social welfare function that places equal weight on all members of the community. Now the welfare in a community situated in equilibrium 3, in which everyone studies Marathi, is simply the unweighted average of all the payoffs from the working-class occupation,  $W = 1$ . When  $\theta$  just crosses one, in a given cohort, all high types in the community can expect to earn more in the white-collar sector than in the "traditional" working-class occupation and will thus switch to English schooling. Welfare from that cohort onward is then  $W = (P_L + P_M)^2 + (1 - P_L - P_M)$ . The new welfare level is a weighted average of  $P_L + P_M < 1$  and 1, and so community-level welfare must unambiguously decline when schooling choice, and hence the occupational distribution, shifts. There was intense competition for scarce working-class jobs historically in Bombay. Because larger numbers improve the community's competitiveness, and increase the working class wage in general, it is easy to see why social restrictions on occupational mobility could emerge endogenously. Moreover, the fact that the lower-caste girls do not display a similar resistance to change can be attributed to the gender-specific nature of these job networks.

Social restrictions on occupational mobility can be welfare-enhancing for small and medium changes in  $\theta$ , as noted above. But they could give rise to substantial inefficiencies if they continue to persist when  $\theta$  grows large. For example, it is easy to verify that the social restrictions described above for equilibrium 3 will be inefficient once  $\theta$  reaches  $1 + (P_L + P_M)$ . While Munshi and Rosenzweig conjecture that restrictions on occupational mobility might be in place in the heavily networked *jatis*, no direct

evidence of their presence in Bombay is available. They can, however, test one important implication that is consistent with the presence of these restrictions; the relationship between schooling choice  $E_{ij}$  and the occupational distribution within the *jati* in the previous generation  $P_j$  must not weaken over successive cohorts in the current generation, even as the returns to English grow. This stability in intergenerational state dependence would then explain the wedge between high caste and lower caste schooling choices for boys that was observed at the aggregate level through the 1990s.

To test for the intergenerational state dependence in occupational choice implied by the model when networks are active, Munshi and Rosenzweig estimate an augmented version of [equation \(7\)](#) derived above:

$$Pr(E_{ij} = 1) = \alpha P_j + X_{ij}\beta + \omega_j \quad (8)$$

where  $E_{ij} = 1$  if individual  $i$  belonging to community  $j$  is schooled in English,  $E_{ij} = 0$  otherwise.  $P_j$  is the share of fathers in the previous generation who held working-class jobs.  $X_{ij}$  is a vector of household characteristics that determine schooling choice. These characteristics include the child's cohort and sex, each parent's language and years of schooling, and household income.  $\omega_j$  measures (unobserved) preschool human capital, which directly determines schooling choices and is now allowed to vary across communities. Although  $\alpha$  should be  $-1$ , based on the simple framework described above, more generally this coefficient should be negative and significantly different from zero with intergenerational state dependence at the level of the community.

The obvious threat to identification is that  $P_j$  and  $\omega_j$  could be negatively correlated if children born in working-class communities are endowed with lower preschool human capital. A spurious negative correlation could then be obtained even if networks play no role in current occupational choices. To deal with this potential concern, Munshi and Rosenzweig exploit the fact that caste networks historically provided jobs to men alone. Estimating [equation \(8\)](#) separately for boys and girls, the  $\alpha$  coefficient is negative and statistically significant for the boys, whereas it is small in magnitude and insignificant for the girls. To provide a more flexible test of the hypothesis that networks, and occupational persistence, apply to the boys alone, Munshi and Rosenzweig proceed to pool the boys and girls, including a gender dummy and community fixed effects in the schooling regression. The specification of the pooled regression is

$$Pr(E_{ij} = 1) = (\alpha - \tilde{\alpha})P_j \cdot B_{ij} + X_{ij}\tilde{\beta} + X_{ij} \cdot B_{ij}(\beta - \tilde{\beta}) + \gamma B_{ij} + f_j \quad (9)$$

where the fixed effect  $f_i \equiv \omega_i + \tilde{\alpha}P_i$ ,  $\alpha, \beta$  refer to the coefficients when [equation \(8\)](#) is estimated for the boys and  $\tilde{\alpha}, \tilde{\beta}$  refer to the corresponding coefficients when that equation is estimated for the girls, and  $B_{ij}$  is a boy-dummy. We can no longer estimate  $\alpha$ , the coefficient measuring occupational persistence for the boys, but under the assumption that girls are unaffected by the traditional network,  $\tilde{\alpha} = 0$ , the coefficient

on the  $P_j \cdot B_{ij}$  interaction term should coincide with the  $\alpha$  coefficient for the boys. Munshi and Rosenzweig proceed to verify that the estimated  $(\alpha - \tilde{\alpha})$  coefficient in the pooled regression (equation (9)) is indeed very similar to the  $\alpha$  coefficient for the boys estimated with equation (8).

Munshi and Rosenzweig subsequently estimate the  $(\alpha - \tilde{\alpha})$  coefficient separately for five cohort-groups of equal size over the 20-year sample period. As the model makes clear, we would expect to see a decline in the  $(\alpha - \tilde{\alpha})$  coefficient over time, especially in the last ten years as the returns to English grew, if the restrictions on mobility were absent. Instead, the estimated coefficients remain significant and stable over the entire period, actually increasing slightly in absolute magnitude over time. It is this community effect, identified in the data at the individual level, that presumably sustains the gap in schooling choice between low caste and high caste boys observed at the aggregate level over the course of the 1990s, even as the returns to English grew.

In a related paper, Magruder (2010) studies the relationship between parental support in the labor market and intergenerational occupational mobility. As he notes, the scope of the network is crucial to understanding changes in the distribution of wealth and economic opportunity across generations. If parents play an important role in securing jobs for their children, we can infer that the scope of the network is limited, with implications for growth and inequality in the future.

Using a panel data set of young adults in Cape Town, South Africa, Magruder finds that sons are more likely to work if their fathers are living in the same province and employed in an industry that is growing (hiring more workers). His estimates indicate that 10% growth in the father's industry increases the probability that the son is employed by 4%. To emphasize the job referral channel and rule out family wealth effects as the source of the increase in children's employment, Magruder also studies the effect on employment when the father resides in a different province, finding a *negative* effect in this case.<sup>10</sup> As an additional test, he also studies the effect of growth in the mother's industry on her children's employment, finding no effect on boys or girls. The gender-specific nature of intergenerational occupational transmission and the particularly important link between fathers and sons broadly matches the results in Munshi and Rosenzweig (2006). Although Magruder does not separately estimate the role of the extended family or community network, the fact that parental effects are so strong suggests that the scope of the network and, hence, patterns of inter-generational mobility may be importantly determined by the underlying social structure in practice.<sup>11</sup>

<sup>10</sup> The longitudinal nature of the data allow him to control for the fact that particular types of individuals will reside away from their fathers. His interpretation of the negative effect is that an increase in family wealth lowers the labor supply of the children.

<sup>11</sup> In India, where caste networks are well developed, we would expect family effects to be relatively unimportant. Munshi and Rosenzweig include father's occupation in the schooling choice regression as a robustness test, but find that it has little effect on the estimated caste-level coefficient.

## 4. CONCLUSION

Great strides have been made in the empirical analysis of networks in recent years. There are two features of this emerging literature that are worth mentioning: First, a great deal of emphasis has been placed on the endogeneity of networks and selective entry into these institutions. Second, recent contributions have avoided the use of self-reported networks, based on lists of friends or contacts, using exogenously determined communities based on kinship or spatial boundaries as the basis for their analyses instead. This careful attention to network measurement and the related statistical concerns of endogeneity and selection have given this area of research much credibility.

Moving beyond the identification of network effects, the next task for development economists would be to understand how these community-based institutions shape and are shaped by the forces of economic change. The basic questions that development economics must answer is why behavior is often so slow to change and why valuable resources are so often misallocated. An economic analysis of the social structure that networks and other community-based economic institutions are embedded in, which characterizes its interaction with the market and the dynamic process through which it evolves over time, would help answer both these questions.

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