# RISK, GROWTH AND SOCIAL NETWORKS

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RISK, GROWTH AND SOCIAL NETWORKS

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This dissertation is an empirical investigation on the microeconomics of growth, focusing on the role of shocks and on the formation of credit networks. It uses original data, collected in Southern Ethiopia, an environment where nonlinear wealth dynamics that are at the root of persistent poverty were previously identified.

The first chapter briefly places this work in the wider context of the development economics literature. The second chapter explores the causal mechanisms behind the nonlinearities identified in earlier work. It focus on the role that not only climatic shocks but also ability play in shaping different accumulation patterns. It is found that herders of low ability are expected to converge to a unique dynamic equilibrium at a small herd size, while those with higher ability exhibit multiple stable dynamic wealth equilibria.

The third chapter of this dissertation validates a new approach to the collection of data on social relations that starts with a random sample of individuals and then randomly samples from the prospective relationships among sample respondents. Using original data from southern Ethiopia it is shown that this method yields estimates of the structure of social relations that are statistically indistinguishable from those generated by tracing respondents' local networks. Through the use of Monte Carlo simulation, it is also shown that introducing this second level of

sampling improves the accuracy of the inference on the determinants of network formation.

The last chapter explores the effect of herd dynamics on the formation of credit networks. It finds that the threshold at which wealth dynamics bifurcate serves as a focal point at which credit transfers are concentrated and that asset loans respond to recipients' losses as long as the recipients are not "too poor". These results suggest that, when shocks can have long term effects, asset transfers may aim to insure the permanent component of income generation, rather than the transitory component, as it is commonly assumed. The chapter also shows that the persistently poor are less likely to be known within their communities and less likely to receive transfers in response to shocks.

## BIOGRAPHICAL SKETCH

Paulo Santos was born in November 29, 1969. In 1994, he graduated from Universidade Técnica de Lisboa, in Lisbon Portugal, with a Licenciatura in Agronomy and in 1998 he received a Master of Science in Agricultural Economics and Rural Sociology also from the Universidade Técnica de Lisboa. He began the Ph.D. program at Cornell University in the Department of Applied Economics and Management in 2001.

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## TABLE OF CONTENTS

## LIST OF TABLES

## LIST OF FIGURES

# Chapter 1

# Introduction

Since Adam Smith, economics has focused on how to eliminate the poverty of nations. That is also a central theme in the field of Development Economics, with its focus on the recent experience of the less developed economies - those where poverty is, even today, widespread. A valuable synthesis of this intellectual engagement can be found, for example, in Lipton and Ravallion (1995). The breadth of such review is notable: the authors cover the history of the ideas on the poor, how to measure the extent of poverty, the characteristics of the poor and policy issues related to anti-poverty interventions. What is conspicuously absent in analyzes of poverty, even those dated as recently as the 1990s, is attention to questions such as "Are the poor always the same?" and "What are the mechanisms that cause poverty to persist?"

The empirical scrutiny of such questions has had to wait for the collection of suitable longitudinal data. We know now that there are substantial movements in and out of poverty (Grootaert and Kanbur, 1995, Baulch and Hoddinott, 2000, Hoddinott, 2003), supporting the interpretation of poverty as a stochastic phenomenon (Ravallion, 1988) and suggesting the distinction between transitory and chronic poverty. Nevertheless, the mechanisms that explain why some people are trapped in chronic poverty remain poorly understood.

The theoretical literature on this question has suggested several answers that usually rely on the confluence of two factors, technological indivisibilities and the presence of some imperfection in the capital–market(for example, Loury, 1977, 1981, Dasgupta and Ray, 1986, Azariadis and Drazen, 1990, Galor and Zeira, 1993, Banerjee and Newman, 1993). Together, they prevent the poor from investing, either through borrowing or through the gradual accumulation of assets.

There is a growing body of empirical evidence that checks the empirical validity of these explanations. Uninsured risk plays a key role in this discussion, acting to create or reinforce poverty in varied ways (Sinha and Lipton, 1999). One, more easily measured and directly observable in the wake of any severe disaster, is the destruction of individual wealth, either of the current generation (Dercon, 2004) or of the next one (Jacoby and Skoufias, 1997, Alderman, Hoddinott, and Kinsey, 2006). Perhaps less noticeable, risk may induce the choice of safe but unprofitable productive strategies by those individuals who are not wealthy enough to self-insure themselves from the potentially larger downside risk of more profitable livelihoods (Rosenzweig and Binswanger, 1993, Morduch, 1995). Clearly, the two are not separate as shown, for example, by Dercon (1998): in a risky environment, where investing in livestock (an indivisible investment) is the activity with higher returns, the wealth-differentiated capacity to deal with the effect of negative shocks leads the poor to select less profitable strategies, reinforcing initial poverty. The same point is made, with different approaches, by Zimmerman and Carter (2003) and by Elbers and Gunning (2003). The focus on "security" as one of the three pillars of a strategy of poverty reduction, in the most recent World Development Report devoted to poverty (World Bank, 2000), is perhaps the best expression of the consensus around the importance of the theme.

Risk also plays a role in the character of poverty for conceptual reasons. The same way that shocks can bring poverty to an ex ante wealthy individual, so can a succession of good draws lead those who start poor into "happiness". In that sense, initial poverty doesn't determine the final outcome or, in the words of Banerjee

(2001, pp.30–31), "There are good reasons not to take poverty traps [models] literally. The very lucky and the very talented among the poor will probably manage to escape their background, and some of the rich will surely manage to squander their patrimony. The robust implication of th[ese models] is rather that economic mobility will be slow [(...) and, with non-convexities, it will] come from those who are very talented or very lucky. In other words, it takes the form of large jumps by a relatively few people".

As a consequence, and once one accepts that risk is a part of life, poverty traps should not be checked by looking for the non-ergodicity of a dynamic system or, in other words, for an absolute lack of mobility of the poor (as, for example, in Easterly (2005)). This is why the identification of the mechanisms that slow the investment of the poor, in particular its indivisibility *and* failures in the financial markets, making poverty persist, is empirically important.

The identification of such mechanisms raises different types of questions. The first one is where to start, that is, what to consider as "assets". In environments (such as the one studied in this Dissertation) where people literally lived and died depending on whether they had or not enough of one asset (as it happened in East Africa with livestock (see Illife (1987) for the historical record on these societies), the choice is clear. In other, more diversified, contexts this task is not so easy, given that the mechanisms identified as possible sources of poverty persistence range from malnutrition to environmental degradation and geographical isolation. To this, one may add the possibility of fractal poverty traps, suggested by Easterly (2000) and by Barrett and Swallow (2006), that the dynamics at one scale (the region, for example) may influence the dynamics at another scale (the household, for example).

Together, they may rend inadequate a stricter focus on financial or productive capital, even if augmented with human capital. However, a more encompassing approach is not without problems. One is how to aggregate such different assets in one point in time. One possible solution, adopted by Barrett et al. (2006) and Adato, Carter, and May (2006) is the construction of asset indexes, where the appropriate aggregation weights are estimated through the relation between assets and income. The second, related problem is how to interpret changes in a composite index of assets, when each individual asset may have inherently different dynamics. This last point may be especially important if one admits the possibility that those assets with slow dynamics may act as bifurcators of the dynamics of the fast variables (Brock, 2001), in practice leading to the possibility of different accumulation paths and the formation of convergence clubs.

These difficulties have prompted a number of studies (recently reviewed by Carter and Barrett (2006)) that search for nonlinearities in the dynamics assets (or income), without necessarily exploring the mechanisms behind the identified behavior. Central to this literature is the existence (or not) of multiple equilibria, in particular the existence of a threshold at which accumulation dynamics bifurcate, such that with wealth below such a critical level one is *expected* to slide into poverty while, if above, one is *expected* to be able to accumulate more wealth. In spite of the importance of such idea in the public debate (most recently, Sachs (2005)) the empirical results have been mixed.[1]

[1]The lack of evidence on such thresholds may reflect its inexistence, that is not necessarily unexpected in diversified economies, with reasonably working safety nets and capital–markets. In those cases, chronic poverty may be statistically unimportant. It may, however, also reflect the empirical difficulties in its identification, that stem both from the short duration of most of the longitudinal studies from developing countries (that make the identification of different growth rates of income or assets for the same individual difficult to impossible) and from the

This dissertation, an investigation on the microeconomics of growth, focuses on the role of shocks and on the decision process underlying the formation of credit/insurance networks, through which the impact of shocks could be smoothed. It uses original data, collected in Southern Ethiopia, an environment where previous work (Lybbert et al., 2004, Desta, 1999) identified the type of nonlinear wealth dynamics that are conducive to make poverty a persistent phenomenon.

Although poverty is usually measured through income (or expenditure) data, the approach adopted in this work is asset-based. The advantages of such an approach to the analysis of poverty dynamics were recently summarized by Carter and Barrett (2006). Among others, of central value is the direct relation to the theoretic models of persistent poverty, which are built around the characteristics of investment (namely, its indivisibility) and individual behavior (namely, the role that access to credit and savings play in the accumulation process).

The next chapter explores the causal mechanisms behind the nonlinearities identified in earlier work. In particular, it focus on the role that not only luck (the climatic shocks that characterize this environment and make pastoralism the central livelihood) but also ability play in shaping different accumulation patterns, echoing Banerjee's earlier–cited observation. We find that those with lower ability are expected to converge to a unique dynamic equilibrium at a small herd size, while those with higher ability exhibit multiple stable dynamic wealth equilibria. These results underscore the criticality of asset protection against exogenous shocks in order to facilitate wealth accumulation and economic growth and the importance of incorporating indicators of ability in the targeting of asset transfers, as simulations

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lack of data in the neighborhood of the accumulation threshold (a central prediction of these models) that makes econometric inference harder, given the existing techniques, as discussed at length in Barrett (2005).

of alternative asset transfer designs demonstrate. They show also that pastoralists perceive the nonlinear long–term dynamics that characterize livestock wealth in the region.

More generally, this work suggests that wealth dynamics, even in a context that seems a priori to be relatively simple (dependence on only one asset, characterized by rapid biological accumulation and sensitivity to climate shocks), can be extraordinarily complex. Focusing on the possibility of convergence clubs, that take into account the differentiated role of specific assets (in this case, only two – livestock and "ability") may be a productive way of moving forward this area of research, specially so in more diversified environments.

In the absence of formal credit or insurance markets, the task of coping with the consequences of shocks falls to informal institutions. The study of such informal institutions is increasingly subsumed under the literature on "social capital". Social capital has received considerable attention in the recent literature as a candidate mechanisms of growth at both the micro and macro levels (Durlauf and Fafchamps, 2005). Despite that interest, and although "social capital is social networks", empirical analysis of the formation of social networks, i.e., how people accumulate such type of capital, remains quite limited.

One of the reasons, perhaps the central one, is the difficulty in collecting data on social relationships. The third chapter of this dissertation validates a new approach to the collection of data on social relations that is easy to accommodate with the usual sampling approaches of populations used by economists and other social scientists. After reviewing the growing literature that uses social networks as a method to analyze social context, paying special attention to how methods of sampling data on relationships affect inference with respect to the formation of social networks, the chapter uses original data from southern Ethiopia to demonstrate a new approach to collecting data on relationships. This new method starts with a random sample of individuals and then randomly samples from the prospective relationships among sample respondents. It is shown that this method yields estimates of the structure of social relations that are statistically indistinguishable from those generated using more expensive and time–consuming methods that trace respondents' social networks. Furthermore, and through the use of Monte Carlo simulation, it tests the value of this approach and shows that introducing this second level of sampling improves the accuracy of the inference on the determinants of network formation.

Having established that pastoralists accurately perceive the underlying asset dynamics that characterize their economic environment, and having validated a methodology to collect the data on social networks, the last chapter ties these two results through the analysis of the effect of herd dynamics on the formation of credit networks that could potentially provide informal finance against manage climate shocks. It finds that the threshold at which wealth dynamics bifurcate serves as a focal point at which credit transfers are concentrated and that asset loans respond to recipients' losses as long as the recipients are not "too poor". These results suggest that, when shocks can have long term effects, asset transfers may aim to insure the permanent component of income generation, rather than the transitory component, as it is commonly assumed. The chapter also shows that the persistently poor are less likely to be known within their communities and thus less likely to receive transfers in response to shocks, further reinforcing the lack of security of those who loose their assets.

As such, and more generally, this work also addresses concerns regarding the interactions between social networks, through which informal transfers flow, and public interventions. It shows that the possibility that public transfers may crowd out informal ones is remote in this context and, on the contrary, appropriately targeted interventions may even crowd in private transfers.

# Chapter 2

# Heterogeneous wealth dynamics: on the roles of risk and ability

## 2.1 Introduction

Contemporary policy debates are rife with discussion of "poverty traps".[1] There exist several theoretical models that combine some non-convex technology with some market failure to explain why "the poor stay poor and the rich stay rich".[2] But do poverty traps exist in the data? The empirical literature has mainly focused on searching for a threshold effect associated with multiple dynamic equilibria in the growth process, with one such equilibrium below a poverty line. The results of such studies remain quite mixed, with some studies (e.g. Dercon, 1998, Lybbert et al., 2004, Adato, Carter, and May, 2006, Barrett et al., 2006) finding support for the hypothesis while others (e.g. McKenzie and Woodruff, 2003, Lokshin and Ravallion, 2004, Antman and McKenzie, 2005, Jalan and Ravallion, 2004), find no evidence of such a threshold.

Nonlinear dynamics are sensitive to shocks that perturb their key variables. Not only it is possible to use this feature to test for the presence of growth thresholds (see Lokshin and Ravallion, 2004), but it is possible to conceive that a series of good draws from the distribution of states of nature can move some fortunate individuals above the threshold.[3] One contribution of this paper is to emphasize

[1]See, for example, Sachs (2005) or United Nations Millenium Project (2005).

[2]See Azariadis and Stachurski (2005) or Bowles, Durlauf, and Hoff (2006) for good reviews of the theoretical and early empirical literature on poverty traps.

[3]See Easterly et al. (1993) discussion of the effect of "good luck" on cross country growth and the micro evidence on the effects of favorable coffee price

how negative shocks may generate nonlinear dynamics associated with persistent poverty. In particular, we show that we only observe multiple dynamic wealth equilibria among our subject population in adverse states of nature.

This paper will also argue that risk is not the only factor shaping wealth dynamics. As the empirical literature on macroeconomic growth suggests, we argue that one needs to consider the possibility of "convergence clubs" based on intrinsic, unobservable characteristics such as time preferences, skills or disabilities.[4] Perhaps the talented can more easily escape poverty or perhaps the disabled are especially unlikely to do so, regardless of initial wealth. The role unobservable ability plays in determining earnings has long been recognized in, for example, studies of the private returns to education (Card, 1995) or in analysis of who becomes an entrepreneur (Evans and Jovanovic, 1989). Nevertheless, we know of no other study that explicitly considers the role of individual heterogeneity in shaping wealth dynamics.

These two explanations, risk and ability, may be closely related. It may be that all agents follow a path dynamic that converges towards a high-level equilibrium when faced with favorable states of nature and that low-level equilibria only arise because shocks routinely knock some backwards, before one's accumulated gains become sufficient to provide adequate self-insurance (Dercon, 1998). In that case, risk can be a source of persistent poverty not only because it induces ex ante risk management that causes the poor to choose lower expected return portfolios (Rosenzweig and Binswanger, 1993) but because differential ability to cope ex post

shocks on poverty in Uganda (Deininger and Okidi, 2003). See Acemoglu and Zilibotti (1997) for a theoretic model where growth is ergodic but poverty can persist.

[4]Baumol (1986), DeLong (1988) and Canova (2004) define and discuss the estimation of convergence clubs in macroeconomic growth data.

with shocks may distinguish high performers from their less fortunate counterparts. Thus, variation in welfare dynamics across states of nature may be central to understanding how both individual-level characteristics and initial conditions affect expected welfare dynamics.

Finally, the policy implications of the convergence club and threshold-based multiple equilibria mechanisms differ markedly. If poverty is a unique dynamic equilibrium because of immutable individual characteristics, ongoing social transfers may be the only available remedy for an unacceptably low standard of living. But if poverty results from initial asset holdings insufficient to clear a critical minimum endowment threshold and thereby follow a positive accumulation path, then asset transfers or changes to the productivity of existing assets can yield increases in wealth that move beneficiaries onto a different path dynamic, towards a higher-level equilibrium, thereby diminishing the need for ongoing transfers. If both processes are at play within a population, then effective targeting of appropriate interventions depends on identifying the relevant subpopulation to which a given poor household belongs. Sorting out the (potentially multiple) mechanisms that underpin persistent poverty is therefore enormously important in practical terms, but also quite difficult methodologically.

This paper explores these issues empirically. We unpack and extend the results of Lybbert et al. (2004), who analyzed wealth dynamics among Boran pastoralists, a poor population in southern Ethiopia. Cattle are the Borans major (in many cases, the only non-human) asset and herd evolution is characterized by boom-and-bust cycles determined by drought and biological reproduction. Using 17-year herd history data, Lybbert et al. find herd dynamics that follow an S-shaped curve with two stable dynamic equilibria (at roughly 1 and 35-40 cattle), separated by an unstable dynamic equilibrium, a threshold at 15-20 cattle. The authors conjecture that this threshold results from a minimum critical herd size necessary to undertake migratory herding to deal with spatiotemporal variability in forage and water availability. Those with smaller herds are forced to stay near their base camps, where pasture conditions soon get degraded, leading to a collapse of herd size towards the low-level stable equilibrium, while those with bigger herds can migrate in search of adequate water and pasture, enabling them to sustain far larger herds. We collected new data among the same population so as to explore the role of shocks and household-specific ability in shaping wealth dynamics.

The next section briefly explains the data. In section 2.3, we use data on pastoralists' expectations of herd size one year ahead, given different values of initial herd size, to simulate long-run equilibria that correspond closely with those identified in Lybbert et al. (2004). Pastoralists appear to perceive the dynamics reflected in herd history data. We disaggregate these dynamics as a function of respondents' expected rainfall states and find that multiple equilibria arise exclusively in adverse states of nature. Under favorable rainfall regimes, respondents' subjective perceptions suggest a smooth asset growth process towards a unique, high-level dynamic equilibrium. Given manifest variation in expected herd dynamics under adverse states of nature, section 2.4 explores the hypothesis that herder-specific ability, which we derive using stochastic frontier estimation methods, conditions wealth dynamics. This appears true in both the herders' expectations data and in herd history data. In Section 2.5 we apply this approach to the analysis of the (expected) evolution of the wealth of a sample of herders in this region. We find evidence that the incorporation of ability does make a difference in terms of expected wealth and inequality in this system. Section 2.6 concludes, stressing the policy implications of these findings with respect to complex wealth dynamics and the centrality of shocks and individual ability to understanding the existence of multiple equilibria in this system.

## 2.2 Data

We employ three data sets. The first is that used by Lybbert et al. (2004), originally collected by and described in Desta (1999), reflecting 17 years of herd histories for 55 Boran pastoralist households drawn from four communities (*woredas*) in southern Ethiopia (Arero, Mega, Negelle and Yabello). Because 16 of the sample households were formed within the 17 year period, this is an uneven panel of data, with 833 total observations. The data were collected using a stratified random sampling design, using detailed interviews held with entire extended families whose collective recall permitted the construction of reliable panel data on herd histories, including mortality, marketing, gifts and loans, slaughtering and calving.[5]

The second consists of household survey data collected from 120 randomly selected Boran pastoralist households in the same four communities of southern Ethiopia, although the respondent households differ from those Desta surveyed. These data were collected every three months, March 2000-June 2002, and then annually each September-October starting in 2003.[6] The data include rich detail on household composition, educational attainment,migration histories, changes in herds, shocks, etc.

The third data set consists of subjective expectations of herd dynamics we

[5]Prior studies have confirmed the reliability of herd history recall data collected among African pastoralists (Grandin, 1983, Assefa, 1990, Ensminger, 1992).

[6]The data were collected by the Pastoral Risk Management (PARIMA) project of the USAID Global Livestock Collaborative Research Support Program. Barrett et al. (2004) describe the location, survey methods and available variables.

elicited from the PARIMA survey households in 2004. The use of elicited expectations to study decision-making was recently reviewed by Manski (2004). Although the efficacy of elicited expectations for testing economic hypotheses has been well established, most such studies have taken place in high-income countries. Important exceptions are Delavande (2004) on the efficacy of contraceptive methods in Ghana, and Luseno et al. (2003) and Lybbert et al. (2006) on pastoralists' rainfall expectations in East Africa. Given the paucity of studies of low-income country respondents' subjective expectations, it is worth explaining in some detail how we elicited these data.

We started by randomly selecting four hypothetical initial herd sizes for each respondent, one from each of the intervals defined by the equilibria identified by Lybbert et al. (2004).[7] Respondents were then asked their expectations for rainfall next year (choosing between good, normal or bad [8]) and to assume a cattle herd of standard composition for the region (in terms of age and sex of the animals). After thus framing the problem, we asked each respondent to define the maximum and the minimum herd size they would expect to have one year later if they themselves started the year with the randomly assigned initial herd size. These bounds provide a natural anchor for the next step, in which we asked respondents to distribute, on a board, 20 stones among herd sizes between the minimum and the maximum previously elicited, thereby describing their subjective herd size distribution one year ahead conditional on the randomly assigned initial herd size.

[7] The intervals are [1,5), [5, 15), [15, 40) and [40, 60].

[8] Published rainfall forecasts, such as those disseminated by the regional Drought Monitoring Centre and government and nongovernmental organization extension officers, use precisely this sort of trinomial rainfall forecast, so it is familiar to respondents (Luseno et al., 2003, Lybbert et al., 2006). The data were collected well into the rainy season, hence these are not uninformed priors.

Finally, each respondent was asked if s/he had ever managed a herd approximately equal in size to the initial value provided as the random seed. The elicitation of the probability distribution function is an appropriate technique under these circumstances (Morgan and Henrion, 1990) and allows us to compute conditional distributions and their moments.

## 2.3 Expected herd dynamics in a stochastic environment

Figure 2.1 presents the scatter plot and kernel regression[9] relating expected herd size one year ahead and initial herd size, conditional on ever having had a herd with a similar size for our sample of 285 observations.[10] The solid 45° line from the origin represents the dynamic equilibria where herd sizes are equal across periods. Three points emerge immediately from comparing pastoralists' subjective expectations of one year-ahead herd dynamics (figure 2.1) with the dynamics revealed by Desta/Lybbert et al.'s herd history data (the dashed line in figure 2.2).

First, both exhibit multiple dynamic equilibria consistent with the notion of a poverty trap. Second, however, the equilibria identified by pastoralists appear to differ markedly from those apparent in herd history data, both with respect to

[9]We use the Nadaraya-Watson nonparametric regression, with the Epanechnikov kernel and bandwidth of 4.545. The value of bandwidth was selected using Silverman (1986) rule of thumb, as determined by the "bounds for Stata" package (Beresteanu and Manski, 2000). We apply the same bandwidth choice procedure in the remainder of this paper, unless otherwise noted.

[10]23 of the 464 total observations (116 respondents with four different starting values each) do not include a herd size prediction, either because respondents were unwilling to make predictions about rainfall or because they were unable to distribute the stones across the board. The latter problem occurred mainly for bigger initial herd sizes, when the difference between the maximum and the minimum was sometimes quite large. Of the remaining 441 observations, in 285 cases (64.6%) the respondents had prior personal experience managing a herd of comparable size.

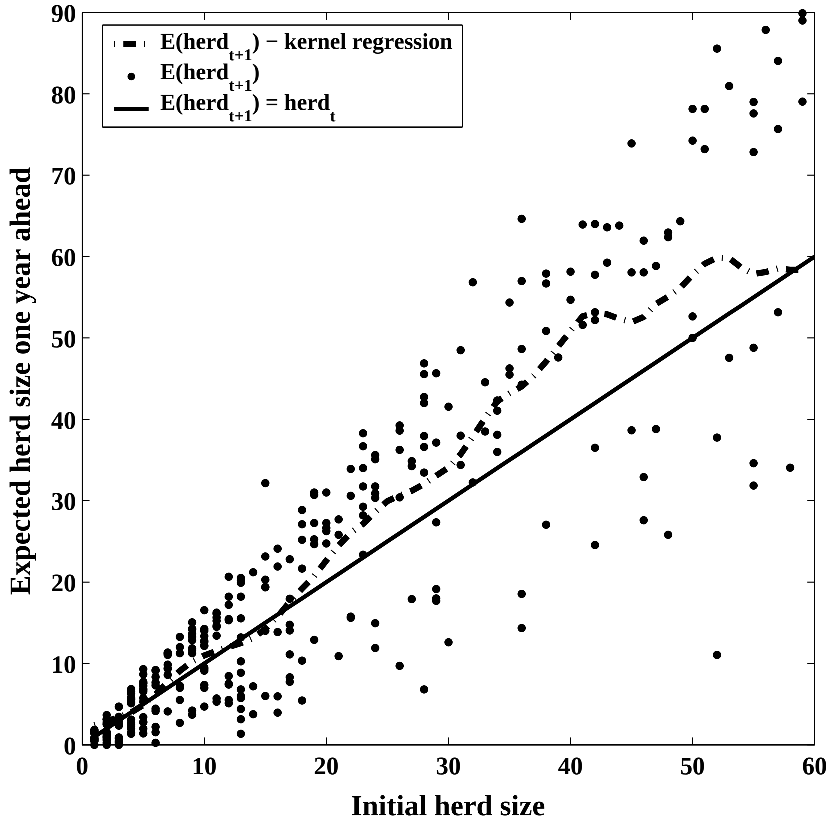


Figure 2.1: Herd dynamics based on respondent expectations

their location and stability. Notably, herd accumulation occurs for a wider range of initial herd sizes, while herd losses seem a relatively marginal occurrence. This would seem to suggest a different story from the one described by herd history data and detailed studies of the system (Coppock, 1994). Finally, there is considerable dispersion in pastoralists' expectations of herd dynamics conditional on a given starting herd size. If one interprets this variation as reflecting pastoralist-specific herding abilities  assuming each pastoralist accurately perceives his or her own herd dynamics given his or her individual aptitude for herding  then this suggests that ability plays a significant role in wealth dynamics.

These casual comparisons invite more careful analysis, especially as regards the

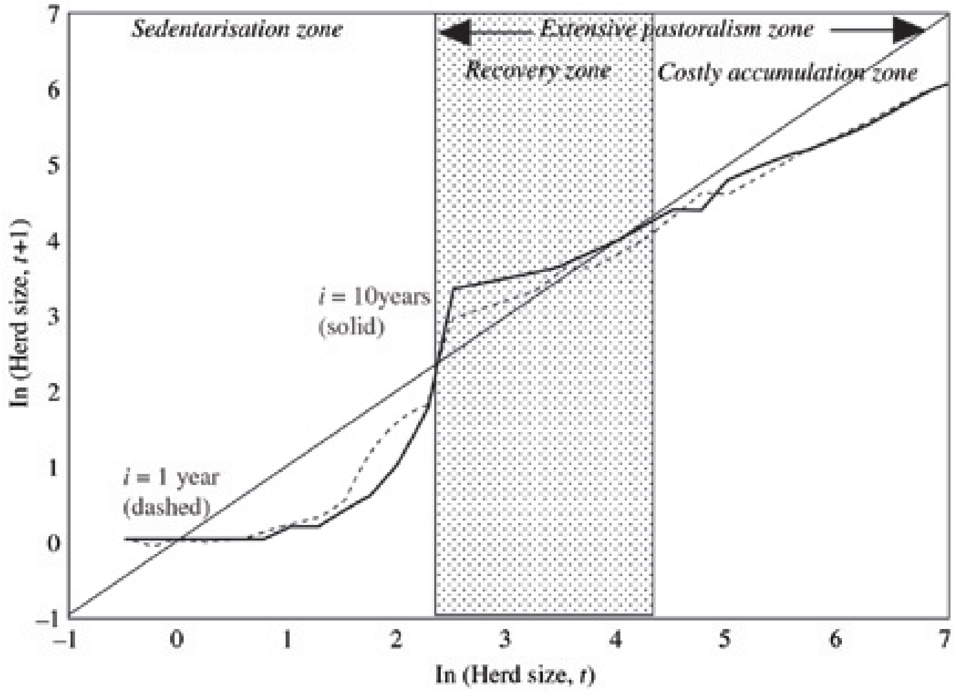


Figure 2.2: Herd dynamics based on herd history

intersection of rainfall conditions and herder ability. The pattern exhibited in the actual herd history data (figure 2.2) is the result of a mixture of environmental conditions over a period of 17 years.[11] Meanwhile, the data on herders' subjective assessments of herd dynamics (Figure 2.1) represent only the year-ahead expectation under necessarily more limited rainfall variability regimes. Put differently, the dashed line in figure 2 reflects herd dynamics conditional on rainfall across a varied mixture of states of nature while figure 2.1 reflects the union of the conditional dynamics with a more limited mixing. Figures 2.3 and 2.4 disaggregate herders' subjective herd dynamics, now conditioning on rainfall expectations.

[11]For example, Kamara, Swallow, and Kirk (2004) identify three major droughts (1984/85, 1991/92 and 1995/96) and two periods of excessive rains (1980/81 and 1997/98) in this region over the period covered by the Desta/Lybbert et al. data. To these natural disasters, one may add the generalized ethnic clashes between the Boran and the Gabra in 1992, following the fall of the Derg regime.

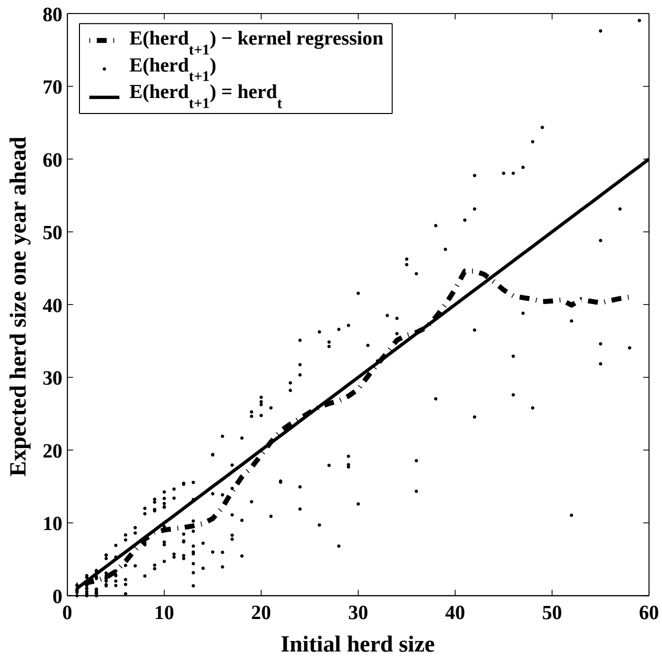


Figure 2.3: Expected herd dynamics under bad rainfall conditions

The difference is striking. The relation between expected and initial herd size is nonlinear and suggests multiple equilibria only in the case of bad rainfall conditions. Under good or normal climatic conditions (and perhaps unsurprisingly), herders expect herds to grow no matter the initial herd size. The dispersion around the expected values is also much bigger under conditions of bad rainfall than in a good or normal year. Herders exhibit far more heterogeneous beliefs about their ability to deal with adverse states of nature than with favorable ones. If we are correct in attributing this feature of the data to individual ability, then such differences seem to matter most when times are tough.

In order to simulate pastoralists' long run expectations of herd dynamics, we need data on the expected behavior under more extreme conditions, namely severe drought and very good years. To obtain such information, we used a second

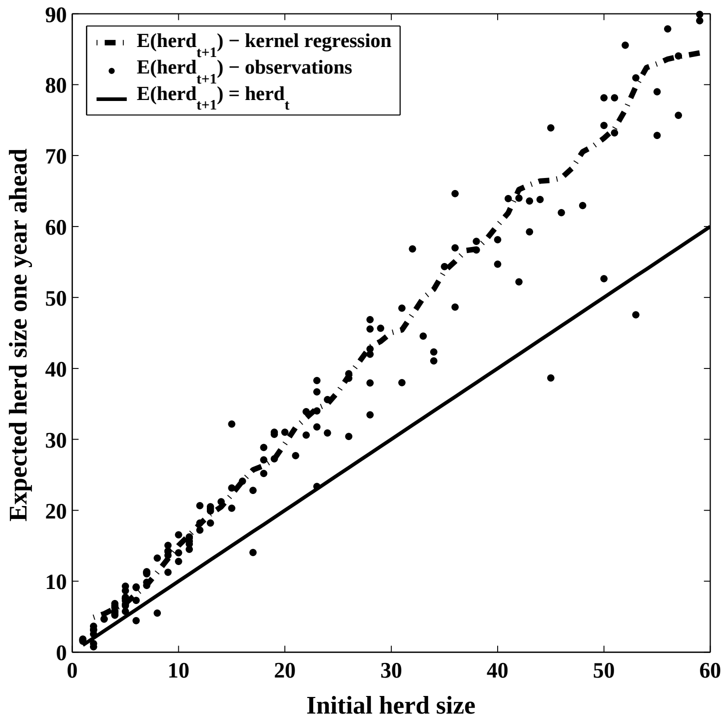


Figure 2.4: Expected herd dynamics under good rainfall conditions

questionnaire similar to the one described above except that we defined rainfall conditions in advance.[12] This instrument was fielded in only one of the four sites (Dida Hara). The results largely correspond with those already reported, showing an almost linear relation between expected and initial herd sizes in very good years and a highly nonlinear relation in cases of severe drought.[13]

In order to generate herders' subjective expectations of herd dynamics under a mixture of states of nature  corresponding to the solid line in figure 2, depicting ten year herd transitions in the Desta/Lybbert et al. data  we need to integrate

[12]In particular, we asked respondents to consider herd evolution "as if" in 1999, the last major drought, or "as if" in a very good year, which we asked them to define based on their own experience.

[13]To conserve space, we omit graphics reflecting these data and nonparametric regressions, although plots corresponding to Figure2.1 and Figures 2.3 and 2.4 are available upon request.

information on herd growth expectations conditional on rainfall (the elicited expectations data previously described) with historical information on rainfall data (in practice, monthly rainfall data for the 4 sites over the period 1991-2001).[14] With this information we can then simulate herd evolution over longer periods using. Since we must predict out-of-sample in simulating herd evolution for large values of initial herd size, we had to estimate the parametric relation between initial and expected herd sizes (hereafter, $\mathrm{herd}_1$ and $\mathrm{herd}_0$, respectively). Conditional on each of the four rainfall scenarios (drought, poor rainfall, normal/good rainfall, very good), we estimate this relation with a respondent fixed effect specification, $\alpha_i$, taking advantage of having repeated observations, r, across different herd size intervals on each individual. We thus estimate

$$\mathrm{h}_{1\ \mathrm{ir}} = \mathrm{f}(\mathrm{h}_{0\ \mathrm{ir}}) + \alpha_{\mathrm{i}} + \epsilon_{\mathrm{ir}} \tag{2.1}$$

where $\mathrm{f}(\mathrm{h}_{0\ \mathrm{ir}})$ is a polynomial function of initial herd size.[15]

Table 2.1 presents the estimates, which reflect the results displayed visually in figures 2.3 and 2.4: unambiguous, effectively linear expected growth under normal/good/very good rainfall conditions, but a nonlinear estimated relation between $\mathrm{herd}_1$ and $\mathrm{herd}_0$ only under conditions of poor rainfall (and drought), and

[14] Average rainfall was 490 mm/year, with a standard deviation of 152 mm/year. Given the skewness and the kurtosis of this distribution, we cannot reject the null hypothesis that rainfall follows a normal distribution. The minimum annual rainfall over the period was registered in 1999 (259 mm) and the maximum in 1997 (765 mm). The probability of such events is 0.064 and 0.035. Given these results, we assumed, for simulation purposes, a symmetric distribution, with a probability of extreme events (drought; or very good year) equal to 0.10.

[15] Besides the assumptions on the functional form of f(•), we also assumed that $\epsilon_{ir} \sim \mathrm{N}(0,\sigma^2)$. Other specifications, that replace the fixed effect with other regressors that could affect subjective expectations, such as gender, age, experience and migrant status, were considered, but none of those variables proved statistically significant, so we omit these results, which are available upon request. We omit higher order polynomial terms in the very good and good/normal year specifications because they added nothing given the good fit already achieved with a simple linear specification with fixed effects.

Table 2.1: Estimates of Expected Herd Dynamics Conditional on Rainfall

| Variable | Very Good | Good | Bad | Very bad |
|---|---|---|---|---|
| $herd_0$ | 1.293 | 1.477 | 0.528 | 0.246 |
| | [0.000] | [0.019] | [0.224] | [0.246] |
| $herd_0^2$ | | | 0.026 | 0.009 |
| | | | [0.010] | [0.010] |
| $herd_0^3$ | | | -0.00039 | -.00017 |
| | | | [0.0001] | [0.0001] |
| constant | 0.897 | 0.179 | 0.513 | -0.575 |
| | [0.448] | [0.416] | [1.185] | [1.083] |
| Number of observations | 61 | 96 | 192 | 61 |
| $R^2$ | 0.986 | 0.994 | 0.792 | 0.589 |

with considerable dispersion so that the precision of those estimates is far less than under favorable rainfall regimes. We then use these estimation results to simulate the expected evolution of herd sizes, properly calibrated to impose basic biological rules for livestock.[16] Figure 2.5 presents the basic structure of the simulation procedure we used.

| t-1 | t | t+1 |
|---|---|---|
| predict $herd_t$ | $\rightarrow$ rainfall draw | |
| ($herd_0$ given) | $\downarrow$ | |
| | call $h_{t+1}=f(h_t \mid rainfall)$ | |
| | $\downarrow$ | |
| | predict $h_{t+1}$ $\rightarrow$ | repeat as in t |

Figure 2.5: Scheme of simulation procedure

[16]More precisely, we do not allow for negative herds and impose that biological growth under good rainfall conditions is delayed in 2 years, i.e., enough for cows toreproduce. We also constrain the predicted values for initial herd sizes above 52 (poor rainfall) and 45 (drought) to be linear, with a slope of 0.03309 and 0.00913, preventing unbelievable predictions due to the parameter estimates at the boundaries of our sample.

Figure 2.6 presents the mean of 10-year ahead herd size for 500 replicates of this simulation with initial herd sizes between 1 and 60. The results are remarkably similar to the dynamics revealed by the herd history data (solid line in figure 2), both in the general shape of the curve and in the location of the different equilibria. While the one year ahead transitions predicted by the two data sets (figure 2.1 and the dashed line in figure 2) did not match because of the fundamentally different underlying states of nature, once one takes into account historical rainfall patterns and simulates the longer-term, decadal herd dynamics, it appears that Boran pastoralists have a remarkably accurate understanding of the nature of how their herds evolve. In particular, they expect that someone with a herd below approximately 15 cattle will eventually lose his wealth, collapsing into a destitute equilibrium with 1 animal.

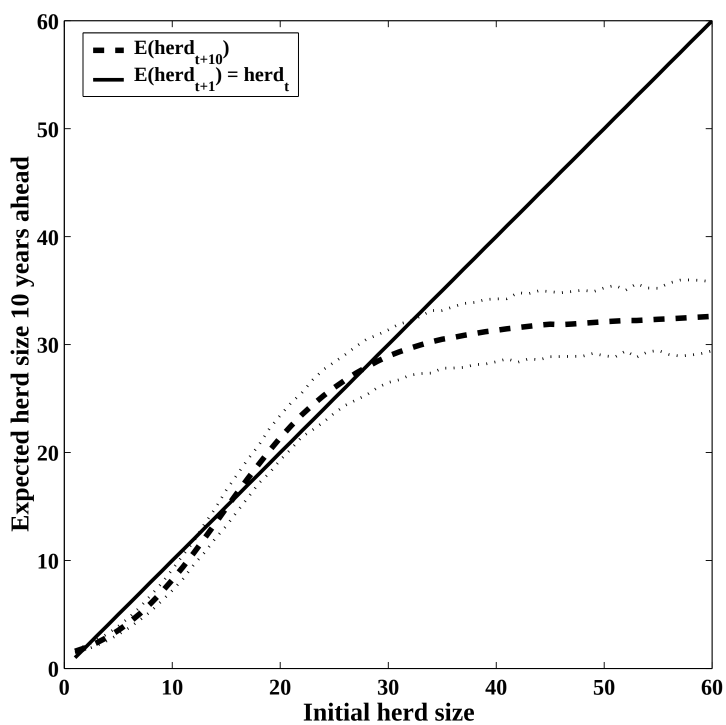


Figure 2.6: Simulated expected herd dynamics – all observations pooled

Can we be sure that multiple equilibria exist? The answer is "no"; the lower confidence band crosses the equilibrium line only once, from above, at the lower level equilibrium (1 animal). But as we show below, this merely reflects our current assumption that all herders follow the same growth path. Once we allow for the possibility of convergence clubs, the differentiated results become clearer.

Concentrating on our average estimates, do these nonlinearities lead to a poverty trap? The answer depends, in part, on what one means by a "poverty trap". In Table 2.2 we quantify the probability of moving between equilibria in a 10 year period given the stochastic nature of these shocks. There is a positive probability that a herder starting with a herd between 1 and 4 cattle will, 10 years later, have grown his herd. Indeed, he may even be above the accumulation threshold. The strictest interpretation of a poverty trap  that initial conditions totally determine future wealth and the system is non-ergodic (and thus the probability of growing is zero)  finds no support in our data. However, the probability of moving out of poverty is quite low (less than 12%), suggesting that, in this context, the idea of a poverty trap is better associated with a high probability (but not certainty) that agents will remain at lower levels of welfare, a weaker but perhaps more realistic interpretation of the concept, especially in stochastic environments (Azariadis and Stachurski, 2005).

Figure 2.7 synthesizes the discussion thus far by presenting the limiting distribution of this stochastic process. The system spends most of its time (78.9 %) with herd sizes below 4 cattle, a consequence of the asymmetric effects of rainfall conditions: the large losses suffered in periods of drought can only be fully compensated by a series of years of good rainfall.[17] With such a small probability of

[17]It is possible that this behavior reflects an underestimate of the true probability of remaining in the high welfare equilibrium identified by Lybbert et al. (2004), as

Table 2.2: Herd size transition matrix (10 year period)

| $\text{herd}_{t+10}$ / $\text{herd}_t$ | 0-4 | 5-14 | 15-39 | >40 |
|---|---|---|---|---|
| 1-4 | 0.879 | 0.113 | 0.009 | 0.000 |
| 5-14 | 0.575 | 0.262 | 0.133 | 0.030 |
| 15-39 | 0.204 | 0.280 | 0.255 | 0.261 |
| >40 | 0.136 | 0.230 | 0.291 | 0.342 |

being at the high welfare equilibrium (around 2%), compounded by the fact that such equilibrium is here defined as the residual interval of "herds larger than 40 cattle", we get a picture of a slow slide into generalized poverty that corresponds with others' description of the system (Coppock, 1994).

Summarizing the results so far, we find that Boran pastoralists accurately perceive long-term herd dynamics characterized by multiple wealth equilibria consistent with the notion of a poverty trap: shocks almost totally prevent wealth accumulation that would allow those herders at a low level of welfare from escaping poverty. However, these dynamics seem entirely the result of an asymmetry in growth rates under different rainfall conditions. Growth is universally expected in good years while S-shaped dynamics seem to result from wealth-differentiated capacity to deal with bad rainfall conditions.[18]

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a consequence of our assumptions regarding herd dynamics outside the range of data for which we have information. Recall that we assumed that, for herd sizes above a certain value and for conditions of poor rainfall or drought, growth rates were a linear function of initial herd size. As we show below, it is possible that that is not the case.

[18]This could explain why, for example, Mogues (2004) studying livestock accumulation in other regions of Ethiopia in the period 2000-03, with no major shocks in between, does not find evidence of such nonlinearities, and why Barrett et al. (2006) find evidence of an S-shaped curve for asset dynamics in the northern Kenya

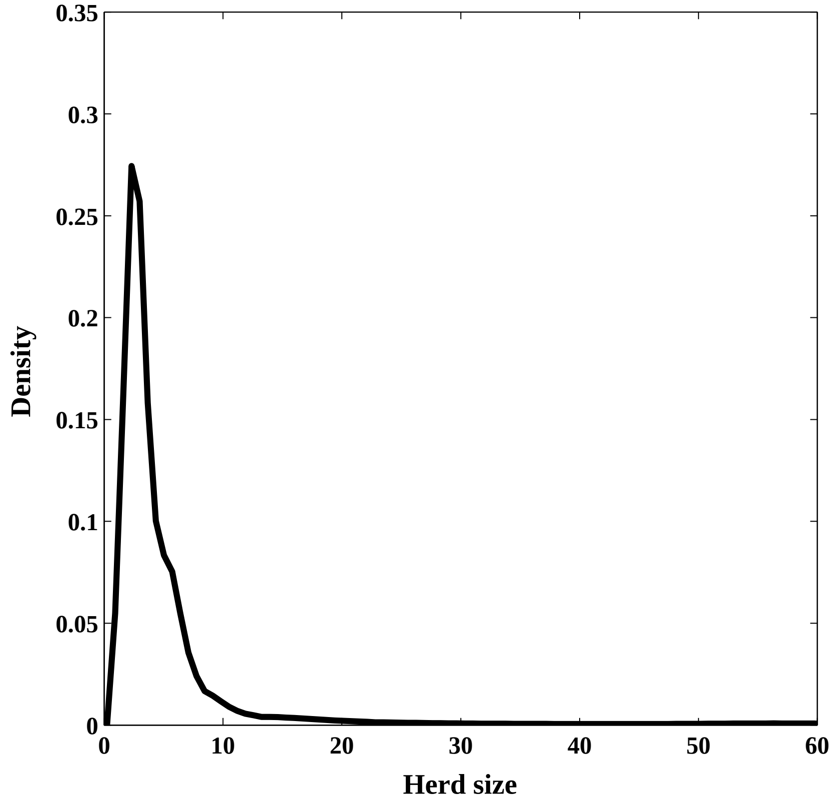


Figure 2.7: Limiting distribution

Our data also show that, even in bad years, not all herders expect their herds to shrink. The considerable interhousehold dispersion of beliefs about herd dynamics under adverse states of nature suggests that herder-specific characteristics, perhaps especially unobserved husbandry skills and related talents we summarize as "ability", may likewise play a central role in conditioning wealth dynamics among these Ethiopian herders. The next section investigates this hypothesis via two different methods.

PARIMA sample, which included a major drought ending in 2001.

## 2.4 Ability and expected herd dynamics

Herding is a difficult livelihood. One must know how to treat livestock diseases and injuries, protect cattle against predators, manage their nutrition, navigate to distant grazing and watering sites, assist in difficult calving episodes, etc. Not everyone learns and practices these diverse skills equally well. One would naturally expect more skilled herders to enjoy faster herd growth and to be less subject to adverse shocks to herd size than less skilled herders. Put differently, the herd dynamics explored in Lybbert et al. (2004) and in the previous section may ignore salient differences in herder ability. We explore the impact of differences in herding ability upon herd dynamics by using the PARIMA panel data on pastoralist households to estimate herder ability using stochastic parametric frontier estimation methods for panel data (Kumbhakar and Lovell, 2000). More precisely, we estimate the herd growth frontier conditional on household attributes and initial period herd size using a composed error term that includes a symmetric random component reflecting standard sampling and measurement error, $\psi$, and a one-sided term reflecting observation-specific but time invariant inefficiency, $\phi \geq 0$, which we assume follows a truncated normal distribution, $N^+(\mu, \sigma^2)$:

$$\mathrm{h_{it}} = \mathrm{f(h_{it-1})} + \beta \mathrm{X_{it-1}} - \phi_{\mathrm{i}} + \psi_{\mathrm{it}} \tag{2.2}$$

Since these households have been surveyed since 2000, we can take advantage of multiple observations for each herder to compute consistent herder-specific mean efficiency measures, i.e., each pastoralist's proximity to the herd growth frontier that provide at least a coarse proxy for herder-specific ability that is not otherwise directly observable. Table 2.3 presents estimates of the herd growth frontier based on 2000-1, 2001-2 and 2002-3 annual observations for the 113 households for which we have complete data on each of the covariates.[19] Table 2.4 defines these variables and presents the descriptive statistics.

Table 2.3: Stochastic parametric herd growth frontier estimates

| Variable | Coefficient | Std. Err. | P-value |
|---|---|---|---|
| herd size at t-1 × above threshold | 1.022 | 0.093 | 0.000 |
| herd size at t-1 squared × above threshold | 0.000 | 0.001 | 0.689 |
| herd size at t-1 × below threshold | 0.890 | 0.307 | 0.004 |
| herd size at t-1 squared × below threshold | -0.009 | 0.022 | 0.681 |
| no cattle at t-1 | -1.126 | 1.245 | 0.366 |
| labor × above threshold | -0.089 | 0.174 | 0.611 |
| labor × below threshold | 0.099 | 0.125 | 0.427 |
| land | 0.022 | 0.152 | 0.885 |
| sex | 1.333 | 0.702 | 0.057 |
| experience | 0.137 | 0.071 | 0.052 |
| experience squared | -0.002 | 0.001 | 0.174 |
| migrant | -0.605 | 0.998 | 0.544 |
| 2000-01 | -0.740 | 0.531 | 0.164 |
| 2001-02 | 1.553 | 0.525 | 0.003 |
| Dida Hara | 1.870 | 1.110 | 0.092 |
| Qorate | 0.026 | 1.229 | 0.983 |
| Wachille | 0.827 | 1.131 | 0.465 |
| constant | 13.012 | 195.554 | 0.947 |
| $\sigma^2 = \sigma_\phi^2 + \sigma_\psi^2$ | 18.847 | 1.657 | |
| $\gamma = \sigma_\phi^2 / \sigma^2$ | 0.229 | 0.104 | |
| Number of observations | 338 | | |
| Log-likelihood | -967.766 | | |

Notice that we use an exogenous switching regressions formulation to incorporate the possibility of two different growth paths, depending on whether the herder is above or below the 15 cattle threshold identified by Lybbert et al. (2004). The results indicate statistically significant (p-value = 0.053) differences in the asset dynamics above and below the threshold, with expected herd growth (collapse) above (below) the threshold. The estimated frontier is piecewise quadratic

[19]Because one of the households is the successor of an initial household, we only have data for the last two years. Hence, we're using an unbalanced panel, with 338 observations.

Table 2.4: Explanatory variables: definition and descriptive statistics

| Variable | Definition | Mean | Std Error |
|---|---|---|---|
| herd size at t-1×above threshold | Herd size in the previous period if greater than 15, 0 otherwise | 3.95 | 3.99 |
| herd size at t-1×below threshold | Herd size in the previous period if smaller or equal to 15, 0 otherwise | 4.17 | 12.08 |
| no cattle at t-1 | dummy variable, equal to 1 if the respondent has no cattle in the previous period, 0 otherwise | 0.185 | 0.389 |
| labor × above threshold | family size, if herd size in the previous period is greater than 15, 0 otherwise | 3.44 | 3.38 |
| labor × below threshold | family size, if herd size in the previous period is smaller or equal to 15,0 otherwise | 0.87 | 2.67 |
| land | land cropped in june 2000 | 1.12 | 2.25 |
| sex | dummy variable, equal to 1 if the respondent is male | 0.639 | 0.481 |
| experience | number of years since start of herd management | 20.26 | 14.07 |
| migrant | dummy variable, equal to 1 if the respondent migrated to the area where he currently lives | 0.210 | 0.408 |
| Dida Hara | dummy variable, equal to 1 if the respondent lives in Dida Hara | 0.25 | 0.43 |
| Qorate | dummy variable, equal to 1 if the respondent lives in Qorate | 0.25 | 0.43 |
| Wachille | dummy variable, equal to 1 if the respondent lives in Wachille | 0.25 | 0.43 |

in $\text{herd}_t$-$\text{herd}_{t-1}$ space, as higher order polynomial terms of lagged herd size have no statistically significant effect.[20] Household labor and land endowments have no effect at the margin on expected herd growth, signaling that these are not limiting in this environment for most households. Male-headed households enjoy significantly higher herd growth rates, which may partly capture household composition effects (with male-headed households having more men able to herd, holding labor availability constant). There exist statistically significant, albeit diminishing, marginal returns to herding experience. And there are marginally significant fixed effects associated with location and year (for 2001-2, the year of recovery after the severe 1999-2000 drought), the latter result reinforcing our earlier finding about state-dependent growth.

Using the predicted value of each herder's estimated technical inefficiency, we then divide our sample into two sub-samples: lower ability (those in the $4^{\text{th}}$ quartile of the inefficiency estimates, above 15.38) and a complementary category of higher ability herders. The distribution of the inefficiency estimates (with cattle as the units) is presented in figure 2.8,[21] allowing a visual analysis of the diversity within each sub-sample. The observations are concentrated within a limited range of inefficiency estimates, suggesting that there may be little value to further

[20] We also ran this regression using cubic and quartic terms, but none of the higher-order polynomials were statistically significantly different from zero and one could not reject the null hypothesis that the higher-order terms jointly have no effect on next periods herd size, once one allows for the threshold effect. The variable "no cattle at t-1" is included to control for the fact that the biology of herd growth is different when one has no cattle growth can then only occur through purchases or gifts, both of which are very infrequent (Lybbert et al., 2004) than when one has a positive herd size. Although the point estimate on this variable is statistically insignificantly different from zero, when we do not control for this effect, the estimated coefficients on lagged herd size and its various interactions become far more imprecise.

[21] Estimated using the Epanechnikov kernel, with a bandwidth of 0.24697.

subdivision of the sample.[22]

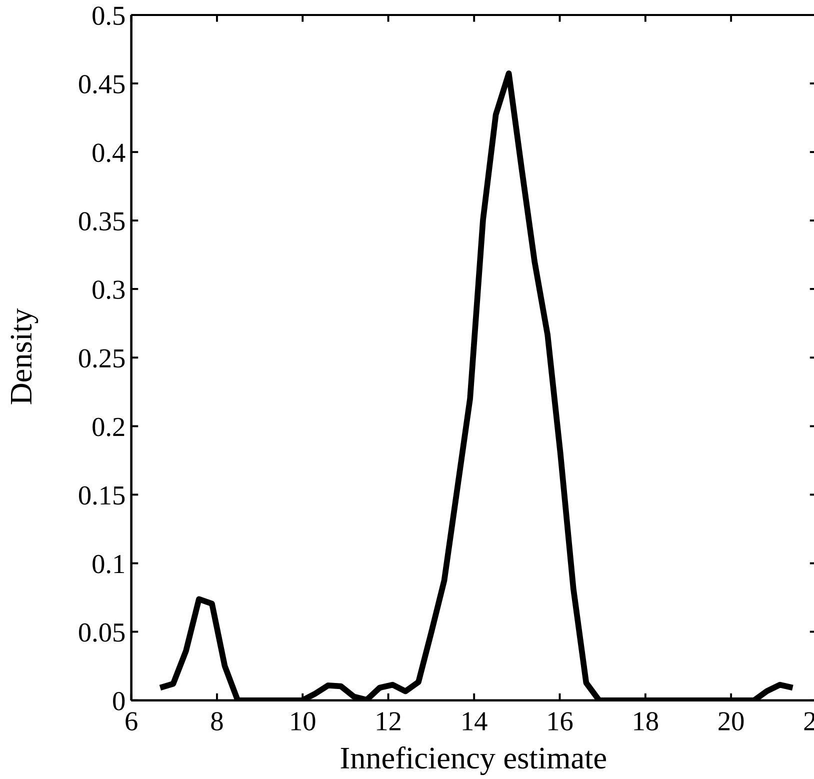


Figure 2.8: Empirical density function of inefficiency estimates

For each of these classes we re-estimated equation 2.1, obtaining estimates of the parametric models that relate expected and initial herd size for each sub-sample.[23] After calibration of these models we performed the same simulations as above. Figure 4.1 shows the mean of 10-year-ahead herd size obtained for 500 replicates with initial herd sizes between 1 and 60 for each ability class. The results

[22]In an earlier version of this paper, we did experiment with splitting the higher ability herders into two categories, those of highest ability (the 1st quartile of the inefficiency distribution) and a residual medium ability class (the 2nd and 3rd quartiles). The qualitative results are similar, so we present the simpler approach.

[23]These 8 parametric models (4 states of nature $\times$ 2 ability classes) are qualitatively similar to the ones presented in Table 2.1. To conserve space, we omit them here.

are easily summarized.

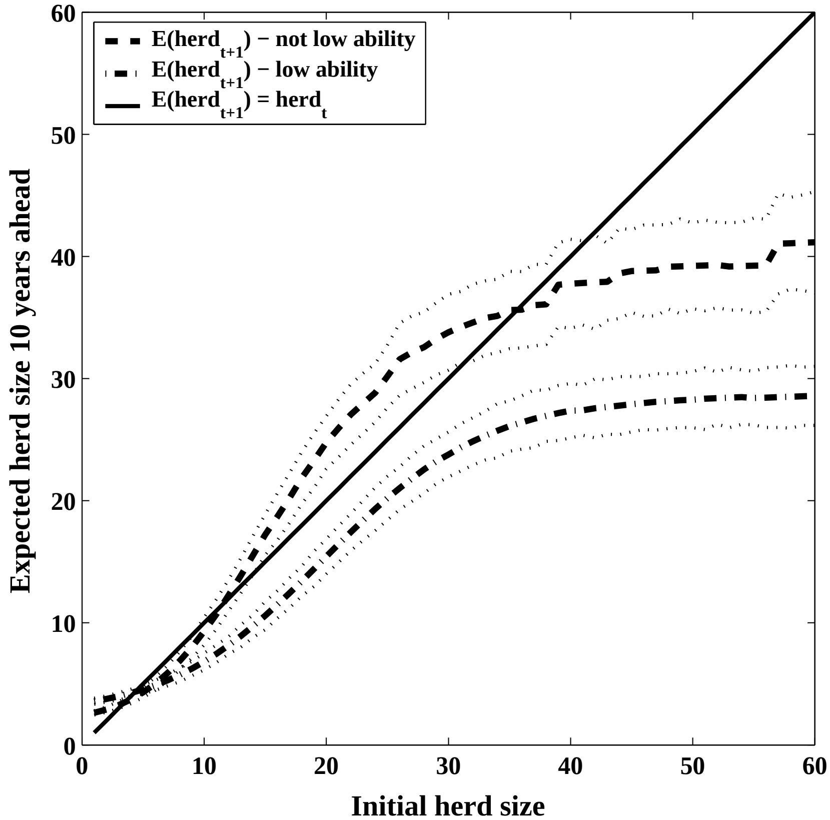


Figure 2.9: Simulated expected herd dynamics – the effect of ability

Although those in the lowest ability quartile exhibit S-shaped expected herd dynamics, these lie everywhere beneath the dynamic equilibrium line (the solid 45° line in figure 4.1). Thus, low ability herders are expected to converge towards the low level dynamic asset equilibrium of 1 or 2 head of cattle, just as Lybbert et al. (2004) found unconditional on ability. Higher ability herders likewise exhibit S-shaped expected herd dynamics. However, they face multiple dynamic equilibria, with a threshold (i.e., unstable dynamic equilibrium) at 12-17 cattle, similar to the threshold Lybbert et al. (2004) estimated in the herd history data. Notice also that, when we allow for different growth paths conditional on ability, we get much more precise estimates of the dynamics of this system. In particular, both confidence bands cross the equilibrium line in three intervals, two of which represent stable equilibria. The implication, reflected in figure 4.1, is that S-shaped herd dynamics characteristic of a poverty trap are not followed by all herders. In particular, low ability herders face a unique dynamic equilibrium at lower levels of welfare, giving rise to a different sort of poverty trap than that faced by herders with higher ability, who expect to accumulate wealth so long as they start with an adequate herd size. Figure 2.10 presents the limiting distributions of the wealth transitions for the two ability groups, reinforcing this point. Herders of higher ability enjoy a probability of holding herds above 55 cattle that is almost 5 times that for herders of lower ability.

These results clearly raise important practical questions with respect to any asset redistribution or transfer policy, as ability is not easily established, at least not by outsiders such as the governmental and nongovernmental agencies that typically provide transfers and public safety net programs.[24] Because of these critical policy implications, we sought to confirm this last result in the herd history data used by Lybbert et al. (2004). As before, we do that in two steps. First, we estimate a stochastic growth frontier, following equation 2.2, to obtain estimates of herder-specific, time-invariant inefficiency relative to the estimated growth frontier, and interpret these inefficiency estimates as a measurement of unobserved ability. Given the longer panel, here we use ten year transitions, rather than the annual transitions estimated in the more detailed PARIMA data. But the limited variables in this dataset restrict the controls we can include to site fixed effects and the number, in the previous decade, of years of bad rainfall and of good rainfall. As a

[24]Santos and Barrett (2007a) explore the effects of ability and multiple equilibria on private, interhousehold transfers among these pastoralist households.

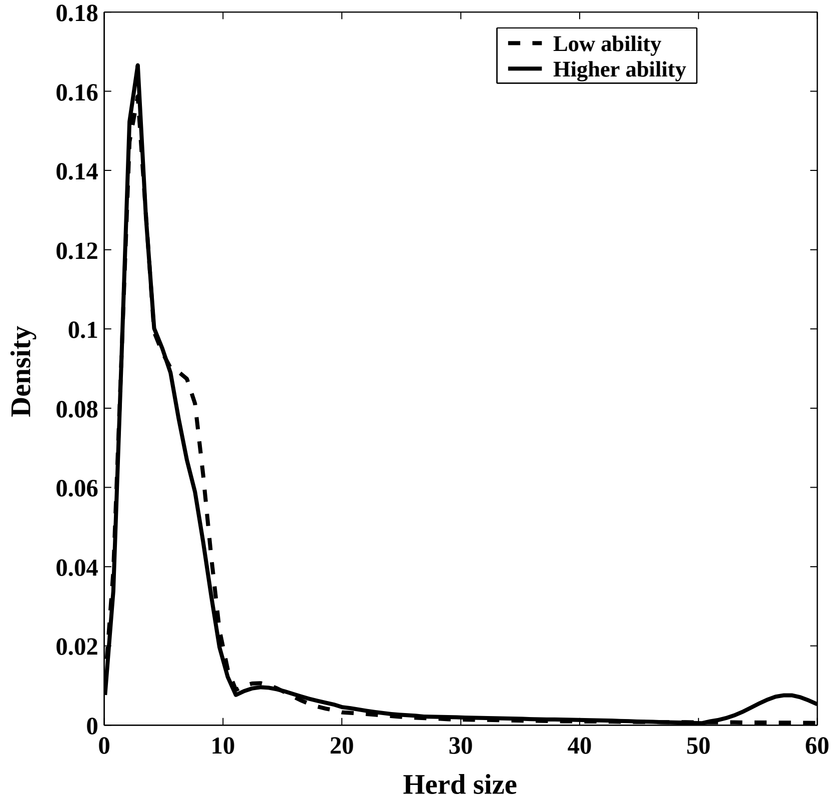


Figure 2.10: Limiting distribution - the effect of ability

consequence the interpretation of estimated inefficiency as ability is considerably less clear than in our previous results. Nevertheless, as a check on the robustness of the previous result, we think it is useful. Finally, because we are interested in comparing our results with the ones from the previous section, we restrict the estimation of this efficiency frontier to herd sizes within the same range as found in the PARIMA data, below 100 cattle.[25] Table 2.5 presents the estimation results.

The first observation concerns the statistical insignificance of the explanatory variables. The effect of past herd sizes (here, with a lag of 10 years) is better

[25]The smaller maximum herd sizes in the PARIMA data than in the Desta/Lybbert data reflect declining median herd sizes as well, reflecting what most observers perceive as deepening poverty in the region.

Table 2.5: Stochastic parametric herd growth frontier estimates

| Variable | Coefficient | Std. Err. | p–value |
|---|---|---|---|
| $\text{herd}_{t-10}$ | 0.141 | 0.501 | 0.779 |
| $\text{herd}_{t-10}$ squared | 0.001 | 0.011 | 0.914 |
| $\text{herd}_{t-10}$ cubed | -0.000 | 0.000 | 0.985 |
| good rainfall | 0.005 | 0.005 | 0.997 |
| bad rainfall | -1.907 | 1.416 | 0.178 |
| Mega | 0.613 | 13.239 | 0.963 |
| Arero | -5.009 | 13.632 | 0.713 |
| Negelle | -13.120 | 12.511 | 0.294 |
| Constant | 206.312 | 6978.869 | 0.976 |
| $\sigma^2 = \sigma^2_\phi + \sigma^2_\psi$ | 915.139 | 200.323 | |
| $\gamma = \sigma^2_\phi \ / \ \sigma^2$ | 0.869 | 0.032 | |
| Number of observations | 236 | | |
| Log Likelihood | -972.643 | | |

expressed through a cubic function and we cannot find evidence of a threshold at an initial herd size of 15 cattle, as we found in the PARIMA data analyzed above. These results can be explained both by the lack of detailed information on other covariates available in the PARIMA data, the much longer lag being explained and the overall differences between the two samples (for example, with respect to average herd size: 68.5 cattle in this sample versus 14.7 in the PARIMA data). As a consequence, not only are the inefficiency terms clearly different, they also explain a much larger share of total variation ($\gamma$=0.869 versus 0.229 in Table 2.3). Figure 2.11 graphs the empirical density function.[26]

With these estimates of herder-specific ability, we now explore the possibility of heterogeneous wealth dynamics within this sample using regression trees. This approach was used by Durlauf and Jonhson (1995) and more recently by Tan (2005) to study economic growth in national-level data. Regression trees is a non-parametric technique introduced by Breiman et al. (1984) that allows the

[26]Estimated using the Epanechnikov kernel, with a bandwidth of 6.9621.

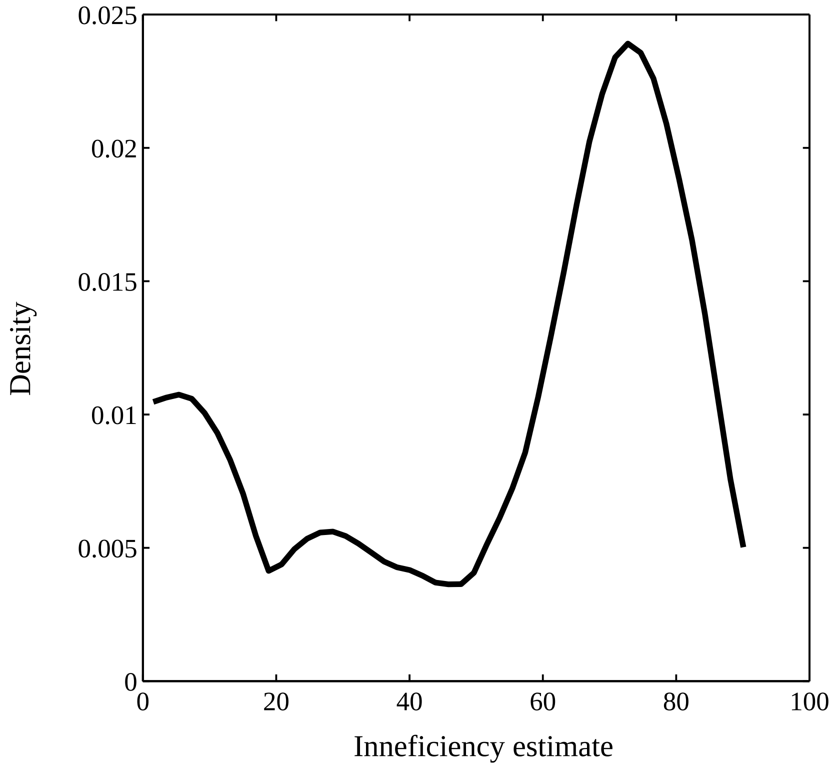


Figure 2.11: Empirical density function of inefficiency estimates

identification of an a priori unknown number of sample splits in order to maximize the fit of piecewise linear estimate of a regression function.[27] At each split, the estimator defines increasingly homogeneous subsets, without the need to determine exogenously the threshold variables and values that mark such divisions. Given the lack of theory on how to select such variables, this approach has the double advantage of eliminating much of the arbitrariness in the analysis and of providing results that are structurally interpretable, in the sense that they reveal the relative importance of particular determinants of the relation being explained. Although the results have been shown to be consistent Breiman et al. (1984), the limitation

[27] A very brief introduction to regression trees can also be found in Hardle (1990, chapter 10.1).

remains that there is no asymptotic theory to test the statistical significance of the number of splits identified.[28] In what follows we'll use the *Generalized, Unbiased Interaction Detection and Estimation* (GUIDE) algorithm, explained briefly in the Appendix and at length in Loh (2002).The result of this procedure is the regression tree shown in figure 2.12.

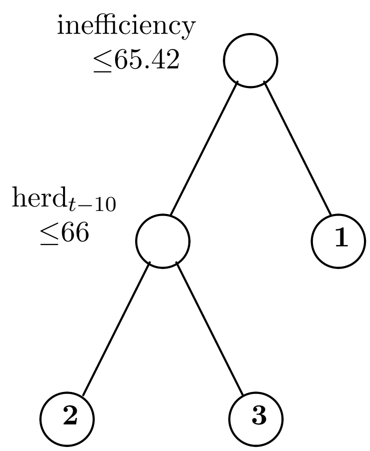


Figure 2.12: Regression tree: herd dynamics, ability and initial herd size

Empty circles indicate the splitting criteria while numbered circles represent terminal nodes that contain different subsamples. At each splitting point, the tree indicates the threshold variable and its value. Observations with a value smaller than the threshold value follow the left branch from the node; those with a greater value follow the right branch. Consistent with our findings to this point,

[28]Other approaches, such as the use of mixture models (Bloom, Canning, and Sevilla, 2003) can, in principle, overcome such problem but, given their computational cost, usually at the cost of reducing the number of admissible splits. Note also that the validity of the theory underlying the identification of thresholds through sample splitting proposed in Hansen (2000) is unclear when we consider more than one split of the original sample, as noticed by the author (p.588).

the first splitting variable is herder ability, which divides the sample into 164 observations on 24 lower ability herders (a much larger subsample than the lower quartile we arbitrarily imposed earlier) and 70 observations on 21 higher ability herders. Within the subsample of lower ability herders, there does not appear to be any threshold in the herd growth function, consistent with our earlier findings using other data from this region. Within the subsample of higher ability herders, however, a further split occurs, at the relatively high herd size of 66 head of cattle. The sample splitting generated by the regression trees method thus reinforces the finding of a unique equilibrium for lower ability herders and multiple equilibria for the rest.

Our estimates of the herd growth models associated with each terminal node appear in Table 2.6 and are graphed in figure 2.13.[29]Expected herd dynamics appear highly nonlinear in each regime. For the lower ability herders, however, the unique dynamic equilibrium occurs at a herd size of zero, qualitatively consistent with the earlier evidence of expected collapse into destitution. Interpretation of the higher ability herders' expected wealth dynamics is somewhat complicated by inevitable extreme behaviors in the tails of each subsample, due to the low-order polynomial, parametric model being fitted. But this too is qualitatively quite similar to our previous result. In particular, there appear multiple stable equilbria, in this case at 18-20 animals and around the sample splitting point of 66 head for those within the range of herd sizes comparable to our earlier results.

[29]The (perhaps counter-intuitive) lack of smoothness of these growth paths is a general result of the regression trees approach, given that splitting the data implicitly assumes that small changes in one variable lead to significant changes in behavior.

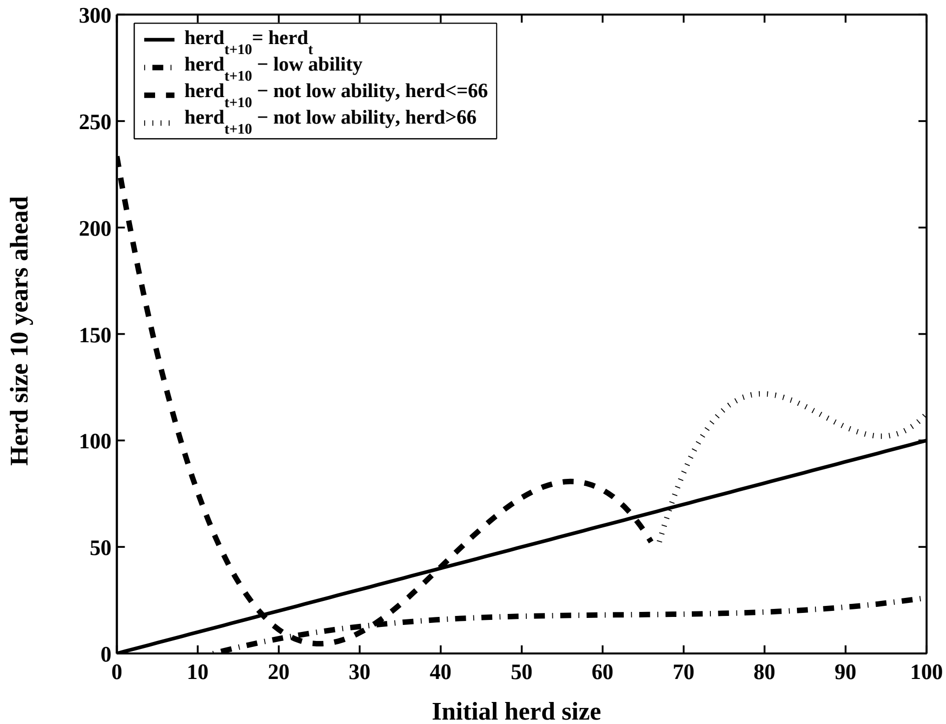


Figure 2.13: Predicted herd dynamics, conditional on ability and initial herd size

Table 2.6: Herd dynamics

| Terminal node | 1 | | 2 | | 3 | |
|---|---|---|---|---|---|---|
| Criteria | Inefficiency $> 65.45$ | | Inefficiency $\leq 65.45$ and herd$_{t-10} \leq 66$ | | Inefficiency $\leq 65.45$ and herd$_{t-10} > 66$ | |
| Variable | Coefficient | p–value | Coefficient | p–value | Coefficient | p–value |
| herd$_{t-10}$ | 2.162 | 0.000 | -6.739 | 0.007 | 268.360 | 0.020 |
| herd$_{t-10}$ squared | -0.0043 | 0.001 | 0.246 | 0.002 | -3.074 | 0.027 |
| herd$_{t-10}$ cubed | 0.00027 | 0.000 | -0.00231 | 0.001 | 0.0116 | 0.036 |
| good rainfall | 0.741 | 0.278 | -1.337 | 0.534 | -20.183 | 0.497 |
| bad rainfall | 2.036 | 0.007 | 2.607 | 0.208 | -18.317 | 0.002 |
| Yabello | 1.263 | 0.573 | 0.145 | 0.136 | 53.630 | 0.000 |
| Mega | 4.495 | 0.084 | -4.217 | 0.570 | 48.427 | 0.000 |
| Arero | 1.388 | 0.584 | -1.468 | 0.016 | | |
| Constant | -1.905 | 0.000 | 74.395 | 0.014 | -7604.675 | 0.016 |
| Number of observations | 164 | | 41 | | 29 | |
| $R^2$ | 0.28 | | 0.76 | | 0.71 | |

## 2.5 Expected growth and inequality among the Boran

We now apply this simulation approach to analyze the expected evolution of wealth and inequality in our sample of respondents. We use the same approach as above on the subsample of 97 households that had cattle in 2003.[30] Table 2.7 presents the results for expected average herd size 10 years ahead and for expected inequality, based on 500 runs of our simulation procedure, first when we disregard the effect of herder ability (column b), then when we incorporate it (column c).[31]

The results are simple to interpret. When we take into consideration the role individual heterogeneity plays in shaping wealth dynamics, we should expect both an increase in average herd size and a large increase in inequality over time, as low ability herders collapse into destitution. If we simulate the evolution of the wealth of this population with a simpler approach that neglects such differences, then still expect an increase in inequality (although somewhat smaller), but with a decrease in average wealth.

Finally, we explore the effectiveness of herd restocking in this system, as this is perhaps the most common form of post-drought assistance provided to pastoralists by donors and governments in the region. We simulate the effect of three different scenarios. In Scenario 1, all herds below 5 cattle (the Boran-defined poverty line)

[30]From our sample of 120 respondents, 5 were not interviewed in 2003 and 18 had no cattle. Given that we did not elicited the expectations about herd evolution for this situation and that, to the best of our knowledge, there are no reliable estimates of the rate of re-entry into pastoralism for herders who lost all their cattle, we dropped them from the simulation. Among those with no cattle in 2003, 5 households (or 27%) were classified as of being of low ability.

[31]Values in column (a) are for the 97 respondents in the PARIMA sample that had cattle in 2003. Values in columns (b) and (c) are the expected value of the statistics based on 500 replicates of our simulation procedure. Values within parentheses are standard errors. The standard error for the Gini coefficient was computed using the algorithm described in Karagiannis and Kovacevic (2000).

Table 2.7: Expected evolution of wealth and inequality among the Boran,

| | 2003<br>(a) | 2013<br>(disregarding ability)<br>(b) | 2013<br>(considering ability)<br>(c) |
|---|---|---|---|
| Average herd size | 12.76<br>(1.49) | 10.47<br>(3.59) | 14.59<br>(8.11) |
| Gini coefficient on herd size | 0.46<br>(0.05) | 0.66<br>(0.04) | 0.71<br>(0.07) |

are given animals to boost their herd to 5 head. In aggregate, that corresponds to a transfer of 36 cattle to 17 beneficiaries. In Scenario 2, we simulate the effects of transferring (approximately) the same number of cattle so as to compare mechanisms under a constant budget but now targeted not to the poorest first but rather in order to maximize expected herd growth from the transfer, assuming there exists no effective mechanism to elicit herder ability. Scenario 2 involves a fictive transfer of 35 cattle to 13 beneficiaries. In Scenario 3, we assume one can accurately identify herder by ability group and, as with Scenario 2, again target transfers so as to maximize asset growth. Scenario 3 involves transfers of 37 cattle to 16 high ability herders.

The main difference between these scenarios is evident in Figure 2.14, where we draw the expected herd growth associated with the transfer of 1 cattle. Given expected herd dynamics over the decade following the hypothesized transfer, the transfer is expected to generate herd growth, net of the 1 cattle transfer, only for recipients with ex ante herd size between 7 and 22 head. Those with the

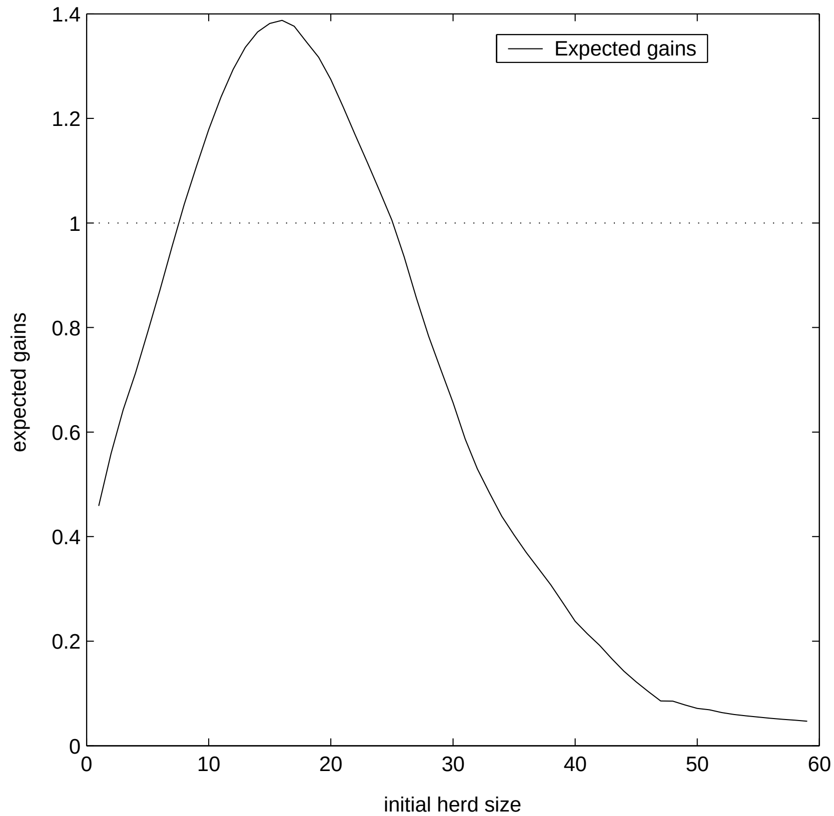


Figure 2.14: Expected gains from the transfer of 1 cattle

smallest (or largest) herds are expected to lose some of their post-transfer herd over the ensuing decade, signaling negative medium-to-long term growth returns on livestock transfers to the poorest (or wealthiest) herders. The expected herd gain is maximized for an ex ante herd size of 13 cattle, a significantly larger herd than is typical of restocking program participants, since such interventions are typically targeted following some wealth-sensitive process, like Scenario 1. Table 2.8 presents the results of a comparison among these three different scenarios for targeting herd restocking transfers.

As one would expect based on the growth dynamics in the system, restocking targeted to the lower levels of wealth (specifically, those below 5 cattle) fails to

Table 2.8: Expected effects of restocking under different targeting assumptions

| Scenario | | Number | Average transfer | Average herd size (2003) | Expected herd size (2013) | | Expected gains from transfer |
|---|---|---|---|---|---|---|---|
| | | | | | w/ transfer | w/out transfer | |
| 1 | Beneficiaries | 17 | 2.12 | 2.88 | 4.06 | 2.71 | 1.35 |
| | Non-Beneficiaries | 80 | 0 | 14.86 | 12.05 | 12.05 | |
| 2 | Beneficiaries | 13 | 2.69 | 12.54 | 14.63 | 11.48 | 3.15 |
| | Non-Beneficiaries | 84 | 0 | 12.80 | 10.25 | 10.25 | |
| 3 | Beneficiaries | 16 | 2.31 | 11.69 | 18.76 | 13.62 | 5.14 |
| | Non-Beneficiaries | 81 | 0 | 12.97 | 16.64 | 16.64 | |

promote growth among the poor. After 10 years, beneficiaries enjoy an expected gain of 1.35 cattle, but from an average transfer of 2.12 cattle. This implies a -4.4% compound annual return on investment in transfer resources, given expected herd losses below the critical herd size threshold. The growth-promoting impacts of herd restocking become more satisfactory in the other two scenarios. Under scenario 2, the average net returns to this policy after 10 years are 17% (1.6% annually). These more than double, to 37% (3.3% annually), under scenario 3, showing that the growth payoff to identification of a reliable mechanism for identifying herding ability is potentially considerable since ability seems to matter a great deal to wealth dynamics in this system.

## 2.6 Conclusions

Using unique data on household-level expectations of herd growth, collected through innovative empirical methods for eliciting subjective herd growth distributions, we find that southern Ethiopian pastoralists appear to understand the nonstationary herd dynamics that long-term herd history data suggest characterize their system, corroborating Lybbert et al. (2004) results using different data and methods. A poverty trap indeed seems to exist. Moreover, their responses enable us to unpack the herd history data, revealing that multiple dynamic equilibria arise purely due to adverse shocks associated with low rainfall years and for pastoralists of higher herding ability. Lower ability herders appear to converge towards a unique, low-level equilibrium herd size. Thus, the data suggest that even among a seemingly homogeneous population in an ethnically uniform region offering effectively only one livelihood option  livestock herding  there exist complex wealth dynamics characterized by distinct convergence clubs defined by individual ability and multiple dynamic equilibria for only a subset of those clubs.

These findings carry two very general policy consequences. First, the need for interventions to lift people out of or prevent their collapse into poverty traps, seems to depend on the nature of the adverse shocks, in particular whether their severity and frequency is such that growth under favorable states of nature is often and sharply reversed, making accumulation below a critical threshold unlikely, albeit not impossible. Risk mitigation to limit the frequency or magnitude of shocks may be as or more valuable than transfers to facilitate growth among the poorest. Second, the appropriate means of social protection in this stochastic environment depend very much on individual characteristics, perhaps including difficult-to-observe characteristics such as ability. Identifying ability may be operationally difficult, but failure to take such characteristics into account may lead to ill-conceived efforts and wasted scarce resources.

# Chapter 3

# Understanding the formation of social networks.

## 3.1 Introduction

A large and heterogeneous literature under the general label of social capital attempts to quantify the value of social embeddedness in terms of welfare improvements for households and individuals.[1] The concept of a social network plays a prominent motivational role, in that it is through the set of interpersonal links between individuals that the net benefits of social interaction are assumed to flow. In the words of Robert Putnam, an influential author in this literature, "My definition is: social capital is networks".[2]

This conceptual emphasis has not been matched by the use of social networks as a method to explore the effects of social context. Social capital has often been measured through the quantification of the density of membership in voluntary associations (sometimes referred to as "Putnam's instrument")[3] while the related literature on social interactions has largely followed a similar path, using easily available information on community or group membership (ethnicity, gender, geographic neighborhood,etc.) to proxy for social networks. Although this has moved the research on the importance of social context from "being a specialty for network sociologists" (Paldam, 2000, pp.636-7) into what Durlauf (2002, p.459) calls

[1]The literature on social capital was recently reviewed by Durlauf and Fafchamps (2005).

[2]Paldam (2000, p. 651, footnote 15).

[3]See, for example, Narayan and Pritchett (1997) for an early use of this type of variable in development economics.

"one of the most striking developments in social science over the last decade", the blurring of the distinction did not help solving the inferential problems on the analysis of social interactions initially pointed out by (Manski, 1993).[4]

It was the recognition of these problems and the need to have data on concrete interactions to overcome them (Manski, 2000) that led to the development, within economics, of a much smaller literature where social networks is not only a metaphor but also a method to characterize social context. The focus of this paper is on the development economics literature that aims at understanding the process underlying network formation, either as a question in itself or as a first step towards the quantification of the instrumental value of social connections.

Social networks are a set of individuals and the relationships among them. This joint focus is the source of differences from data collection strategies centered on the characteristics of individuals alone.[5] The relatively small literature that has collected both types of data is, nevertheless, diverse. Development economists have used a variety of sample designs, both for respondents (from census to random sample) and for relationships (from a complete enumeration to the selection of a pre-determined number of relations, from real to potential behavior). As interest in the empirical analysis of social networks grows and more researchers contemplate the possibility of collecting such data, it is important to understand the implications of these methodological choices.[6] That is the purpose of the next

[4]See also Brock and Durlauf (2001), Moffitt (2001). Both Soetevent (2006) and Blume and Durlauf (2005) present recent reviews of this literature

[5]This focus implies also that we consider only those studies where the characteristics of relationships were elicited. We leave outside of this analysis studies such as Bandiera and Rasul (2006) or Behrman, Kohler, and Watkins (2002), where the information on networks is limited to the number of contacts of each respondent.

[6]One strategy that seems not to have been used so far in development economics is "snowball" sampling (Goodman, 1961) where, starting with a set of initial respondents (seeds), one increases the sample by including those individuals named

section.

Ultimately, however, we want to probe the validity of one new approach that we introduce in Section 3.3 and label as *random matching*: individuals who are part of a random sample are randomly matched with other individuals from the same sample and asked about their willingness to establish a link with the random match, hence both individuals and relationships are randomly sampled. We do that in two steps. In section 3.4 we discuss whether the elicitation of the willingness to establish a relation allows us to understand the process underlying the formation of individuals' actual networks. We use data on the social networks of a random sample of individuals collected in two different ways – through direct elicitation and through random matching – and show that they yield results that are statistically indistinguishable. In Section 3.5 we demonstrate the importance of sampling relationships. Using Monte Carlo simulation, we compare the accuracy of the inference with respect to the determinants of network formation when data on relationships are collected in two different ways: random matching and the more frequent approach of relying on the set of links from a random sample of individuals as an accurate image of individuals' networks, which we label as *matches within sample*. Our results show that, for different models of network formation, the random matching approach is, in general, more accurate than using all matches within sample. Section 4.5 concludes the paper.

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by previous respondents. In this case the sampling of relationships and individuals (after the initial ones) is done simultaneously. Although well-suited for the sampling of "hidden populations", the respondents entering the sample after the seeds are not randomly selected which complicates inference about the population. See Heckathorn (2002) for a discussion of the conditions under which this problem can be solved and Heckathorn and Jeffri (2002) for an application to the analysis of jazz musician communities.

## 3.2 A review of current approaches

The analysis of networks requires data on both individuals *and* relationships. It is useful to review how the sampling of both units can and has been done.[7] As with every other survey, individuals are the source of information and the existing literature employs two strategies to identify them: a census of all individuals (as in DeWeerdt (2004), Dekker (2004) and, in one village, Goldstein and Udry (1999)) or, more commonly, a random sample of individuals from the population of interest. These lead to different network designs, commonly referred as global versus local network designs, respectively.[8] The pros and cons of each strategy are relatively obvious. Random samples are less expensive but they lead to a loss of information on the network structure as the information generated is essentially limited to dyads, leaving potentially interesting questions outside the range of possible analysis.[9]

Having decided how to sample individuals, the second level of sampling is done through the construction of a "name generator", a question that is used to elicit and identify relationships. If "[...] a network is defined by the links as much as the nodes" (Morris, 2004, p.10), this is a step as important as the selection of the individual respondents although perhaps less visible: "it happens in the questionnaire" (Morris, 2004, p.10). Name generators include two parts - the

[7]Much of the systematization that follows borrows from the clear exposition in Morris (2004). Several illustrations of the questions that we deal with in this paper can also be found there, but focusing specifically on the use of social networks to understand the epidemiology of HIV/AIDS.

[8]Global and local networks are also known, in the social networks literature, as sociometric and egocentric networks, respectively.

[9]This also means that much of the work developed within the field of social network analysis, directed to the analysis of complete networks (see Wasserman and Faust (1994) for an extensive treatment of such methods) cannot be directly applied to most of the data used by economists.

relation/behavior and a rule defining how many relations the researcher identifies.

As for the relationships among individuals, most of the studies by development economists look at potential relations, that is, those elicited through questions of the type "Who could you rely on to ...?" (DeWeerdt, 2004, Fafchamps and Gubert, 2007, Santos and Barrett, 2007a), while others focused on real relations through questions such as "From whom did you receive gifts?" (Dekker, 2004, Krishnan and Sciubba, 2005, Conley and Udry, 2005, Udry and Conley, 2005).

When looking at the motive for establishing the link, most studies focused on insurance, the exceptions being the analysis of information networks by Conley and Udry (2005) and Santos and Barrett (2005), and the analysis of the interpersonal relations through which information, credit, labor and land are transacted in Udry and Conley (2005), all building on the data collected and described by Goldstein and Udry (1999). Finally, concerning the "stopping rule", some studies have asked for all the relationships of the respondents (e.g. DeWeerdt, 2004, Goldstein and Udry, 1999) while others established a maximum number of links (e.g. Fafchamps and Gubert, 2007). This methodological diversity, which reflects both the relative novelty of the approach and the diversity of substantive questions for which such data was collected, is summarized in Table 3.1.

Several points arise. The first, and most obvious, is the extent of missing information, which is an issue regardless of whether we have a census or a random sample of individuals. For example, DeWeerdt (2004) reports that his analysis is limited to approximately two-thirds of the links identified by his respondents, as the remaining 1/3 were formed with individuals outside his census unit. Krishnan and Sciubba (2005, pp. 19-20), whose data on respondents were collected through a random sample, report a similar magnitude of missing information on the de-

Table 3.1: A summary of approaches to the study of network formation

| | Goldstein and Udry (1999) | DeWeerdt (2004) | Dekker (2004) | Krishnan and Sciubba (2005) | Fafchamps and Gubert (2007) | Santos and Barrett (2006) |
|---|---|---|---|---|---|---|
| Sampling of individuals | Random sample, census | Census | Census | Random sample | Random sample | Random sample |
| Sampling of relationships | Matches within sample, Random matching | Matches within sample | Matches within sample | Matches within sample | Matches within sample | Random matching |
| Link | Potential, real | Potential | Real | Real | Potential, strong | Potential |
| Instrumental value | Information, others | Insurance | Insurance | Insurance | Insurance | Insurance |
| Other references | Conley and Udry (2005), Udry and Conley (2005) Santos and Barrett (2005) | Dercon and DeWeerdt (2006) | | | Fafchamps and Lund (2002) | |

pendent variable,[10] while Fafchamps and Gubert (2007) have much higher values for the amount of information that is lost: of 939 network members identified by 206 households, 750 (or 79.9%) are not part of the sample and are disregarded in their analysis. Other studies, such as Udry and Conley (2005), also mention this problem, but less directly.[11]

An evaluation of the importance of these losses is beyond the scope of this paper as it would require data on complete networks in order to replicate the effects of missing information.[12] Nevertheless, one suspects that they are important, not only due to the extent of missing information but also because there may be non–random qualitative differences between the links that are left out and those that are identified. For example, even with complete networks (that is, when all individuals in a group are being sampled) well still miss the relationships with individuals outside the census unit. Yet these can be especially valuable if, for example, one is interested in the performance of informal insurance (as income shocks across

[10]The authors have data on "more than two-thirds" of the networks under analysis, reflecting the fact that "in most villages, over 30% of the village forms the sample and in some cases, about three-quarters of the village was surveyed" (Krishnan and Sciubba, 2005, p.19).

[11]In commenting on the graphical representation of the data used in their analysis of the determinants of link formation (Udry and Conley, 2005, Table10.4, p.257)these authors remark that "There are individuals in each village for each network who appear isolated in these graphs. That appearance is a misleading consequence of the strategy of constructing these graphs based on "ego-centric" data from a random sample of the population. In fact for each of these functional networks there is virtually no one in any of these villages who has no interactions with anyone. Virtually everyone in our sample has learning contacts, exchanges credit and/or gifts, hires labor, and has obtained land from someone. If none of those other parties happens to be in our sample, the individual appears isolated in the graphs."(Udry and Conley, 2005, p.250).

[12]The social network literature dealing with this problem (most recently, Kossinets (2006)), although not focusing on dyad formation, reports discouraging results regarding the reliability of the estimates of network statistics when information on nodes or links is missing.

villages are typically less correlated than within villages, increasing the scope for mutual insurance) or information flows (as outside links may provide access to information that is not easily accessed within the village).

If many relationships are not with individuals who also belong to the sampling unit, one way to diminish the importance of missing information would be to collect detailed information on the attributes of the network members for the sampled individuals. This information could then be used to explain observed patterns of network formation. While there is evidence that very specific details about links' activities may not be accurately known,[13] there seems to be no a priori reason to doubt the validity of information on readily observable attributes such as gender, ethnic affiliation, age (at least within some interval or by comparison with the respondent), migrant status, etc..

The second point that merits reference is the nature of the link that is surveyed. When limiting the number of relationships elicited from a respondent, as in Fafchamps and Gubert (2007), one risks eliciting an implicit ranking of the relationships as these authors recognize.[14] The same is true, although perhaps attenuated and less obvious, when one asks for a complete list of relationships. One can expect that those "closer" to the respondents will have a higher probability of

[13]For example, Goldstein and Udry (1999, p.20) report that, contrary to what is assumed in conventional models of social learning (where a group, such as a village, is assumed to be the network), farmers were not able to provide information about farm operations for a random sample of farmers in the villages they studied. This is further reinforced by Hogset and Barrett (2007), where a similar result is obtained when farmers are asked about details on agricultural practices of farmers that respondents indicated were in their information network.

[14]The authors mention that although they ask for a maximum of four relations per respondent, "In practice, respondents listed on average 4.6 individuals, with a minimum of 1 and a maximum of 8. This is because in a number of cases respondents *refused to rank individuals they regarded as equivalently close to them.* (Fafchamps and Gubert, 2007, p. 9, footnote 8, emphasis added).

being remembered and named (Brewer, 2000). In practice, one is leaving out weak ties, that is, those within the respondent's network who are socially more distant (Granovetter, 1974).[15][16]

Whether this emphasis on strong ties is a problem probably depends on the nature of the purpose for which data on networks are being collected (Sobel, 2002, Chwe, 1999). For some questions (for example, informal insurance), the Folk Theorem of repeated games would suggest that it is not a problem. In this case, the network is conceptualized as both a source of transfers and as a disciplining device that keeps the shadow of defection away; this last function requires proximity between everyone involved.[17] In other contexts (for example, information search), there seems to be less room for such an assumption as respondents may perceive those who are "more distant" as valuable sources of new information even if potentially less motivated to provide it (Santos and Barrett, 2005). In any case, and in general, it seems that relatively little attention has been given to the importance

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[15] In part, this is just a refinement of the previous point. Focusing on strong ties is on way of saying that information on weak ties is missing. See Kohler (1998) for an analysis of the effects of truncating the size of elicited networks on estimates of network density.

[16] In the original exposition of the hypothesis of the strength of weak ties, Granovetter (1974, p.1361) writes that "most intuitive notions of the "strength" of an interpersonal tie should be satisfied by the following definition: the strength of a tie is a (probably linear) combination of the amount of time, the emotional intensity, the intimacy (mutual confiding), and the reciprocal services which characterize the tie." In an early review of studies that tried to test this hypothesis, Granovetter (1982) identifies two major ways of operationalizing the concept of "strength of tie": (i) frequency of contact, used by Granovetter (1974), and (ii) the assumption that ties with different people (e.g., kin, friends, colleagues and acquaintances) have different strength. See Marsden and Campbell (1984) for a discussion.

[17] But see both Udry (1994), on the role for formal enforcers of such contracts, and Fafchamps (2002) for a discussion of the possibility of contracts when there is no evidence "of a single case of an agent being punished by others for dealing with someone who had previously breached a contract" (pp. 2-3).

of "weak ties" (Woolcock and Narayan, 2000, Ionnanides and Loury, 2004).

The distinction between potential and real links is potentially important.[18] Which is more appropriate probably depends on the purpose for which data on social interactions are being collected. Potential links may matter most when analyzing forward–looking behavior, as it is the perception that one can rely on a link, regardless of whether it has been previously used, that likely drives present decisions. Studying real links would perhaps be preferable when the objective is to study past behavior, for example to understanding how information networks have affected learning about and dissemination of a new technology.

Clearly, there does not have to be a perfect juxtaposition between the two. The set of real links will probably be a subset of the potential network as it is improbable that all potential relations are mobilized in a specific period. For example, the data collected by Goldstein and Udry (1999) show that, from the set of individuals who *could* be contacted when searching for information, only a small fraction *was* contacted in the past.

Finally, most analysis to date has implicitly assumed that "everyone knows everyone else in village settings". As a consequence, the possibility that some links are not formed because individuals do not know each other has rarely been raised.[19] How to test this assumption is not trivial. One obviously cannot ask a respondent for a list of individuals that she does not know and to ask for a list of those she knows seems both infeasible (due to respondent fatigue) and, ultimately, unconvincing because those not named could have been just momentarily forgotten, possibly just because of less frequent contact.

[18]See Harrison and Rutstrőm (2004) for evidence on concerns about hypothetical bias in nonmarket value elicitation research.

[19]Santos and Barrett (2005) and Santos and Barrett (2007a) are the exceptions.

The approach first used by Goldstein and Udry (1999) - to ask about social acquaintance between two randomly matched individuals belonging to a sample allows us to take a first look at this question.[20] Besides showing that not everyone knows everyone else, their data also show that knowing one's potential partner is a pre–condition for other interactions, providing support for the idea of embeddedness proposed by Granovetter (1985). Purposeful relations are formed from within a web of social relationships that are not necessarily constructed or maintained with a specific (instrumental) objective but that allow individuals to evaluate the costs and benefits of establishing a link with a specific purpose. The sequential nature of this process has consequences for the econometric model to be estimated (Maddala, 1983) as the analysis of the determinants of an instrumental network should be done using the subsample of those who know each other and not the full sample.[21]

To summarize, the empirical literature in development economics that has analyzed network formation is small, recent and diverse. The main substantive question about it pertains to the reliability of its conclusions when an important part of the network of interest is missing. In the next section we present an approach, random matching, that largely obviates this problem.

[20]Given that, it is not surprising that this approach shares some similarities with previous suggestions in the social networks literature, notably by Granovetter (1973). The main difference is that in the latter, respondents were presented with a roster of all individuals in the group (not a random sample) and asked whether they knew them or not. The results of the application of this approach in a small group are reported in Erickson, Nosanchuck, and Lee (1981) and Erickson and Nosanchuck (1983).

[21]Or, at least, the interpretation of the results should make clear that their validity also depends on the assumption of generalized inter-knowledge among the sampled individuals.

## 3.3 Random matching

The approach to the sampling of relationships that we validate was first used by Goldstein and Udry (1999). We label it *random matching*. Starting with a random sample of individuals from a population of interest, one elicits the willingness of each respondent to enter into some specific relation with a match that is randomly selected from the same random sample.[22] Random matching has three major advantages relative to alternative methods. First, it naturally fits into the sampling strategies commonly used to collect micro–level data. Second, by randomly presenting the respondents with different possible matches, one discourages neglect of "weak links". Finally, we know the characteristics of both the respondent and her prospective match, hence no information is lost because one of the nodes is unknown.

That said, it is important to notice the potential limitations and shortcomings of this approach to the sampling of relationships. In the approaches reviewed in the previous section, information loss occurs because, when free to choose from the population, respondents identified network members who were not in the random sample. With the random matching approach one relaxes the constraint of looking at existing links by imposing a new constraint: respondents must think about forming links with individuals who belong to the random sample. Why can random matching be trusted or even preferable to the matches within sample approach? It is easier to start answering this question by considering an example where random sampling of individuals, that underlies both random matching and matches within

[22]As we mentioned in the previous section, an important previous step made possible by this approach is to first establish whether the respondent is acquainted with the randomly selected match, allowing for an appreciation of the degree to which instrumental networks are embedded in a wider web of social connections.

sample, *should not* be used.

Consider patronage relations reviewed by Platteau (1995). In figure 3.1 we represent an extreme setting where only one individual (labeled A) is a suitable patron for the remaining ones (the clients, labeled by numbers). Clearly if the sample (represented by full circles) is formed only of clients (here, 1 and 2), who do not establish (and are unwilling to establish) links between themselves, both approaches – random matching and matches within sample – would fail in allowing us to understand the process underlying network formation. In the case of random matching, because all individuals, unwilling to establish a link with each other, would (falsely) appear isolated, given that the patron is outside the sample. As for the direct elicitation of (potential or real) links, the absence of survey information on the patron would prevent the use of the matches within sample approach, making it impossible to understand the decision underlying the formation of this link. Imagine now that another patron (labeled B) is available, although all clients still

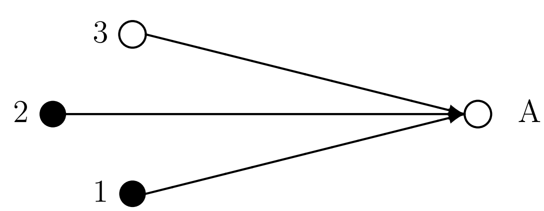


Figure 3.1: Sampling networks: without a prospective patron in the sample

establish their relationship only with A. The picture would be similar (see figure 3.2) and lets assume that, due to the sampling process, individual B is sampled but A is not. Direct elicitation of links would leave the researcher exactly in the same position as before: all individuals would still appear as isolates. Random matching, on the other hand, has the potential to reveal something about the link formation decision, as it is conceivable that clients would be willing to form a link with B even though, in practice, that link is dominated by that with A.

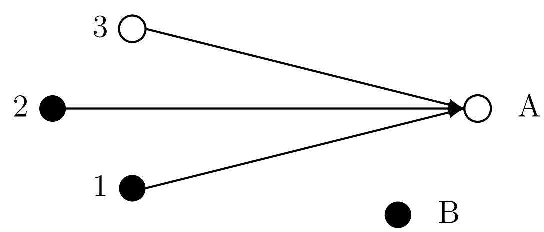


Figure 3.2: Sampling networks: with a prospective patron in the sample

Patronage as depicted here is an extreme example of a perhaps more general case, as suggested by Cox and Fafchamps (2006): individuals have limits to the number of relations that they can establish and maintain and, as such, social networks are bounded. It is therefore possible that links that were latent(perhaps because others were preferable or just because history and inertia led to a particular network configuration) may be "formed" during the questionnaire, allowing for inference that, in some cases (such as the one depicted in figures 3.1 and 3.1) would be impossible.

This potential advantage may come at a cost. If the relationships under analysis are the result of a thought experiment during which respondents are (implicitly) asked to reproduce the reasoning underlying the formation of social links but now facing a different set of partners it is not inconceivable, given the artificiality of the experimental setting, that cheap talk (or other noise) might generate connections that are uninformative about the characteristics of specific networks. It is therefore important to probe whether the links elicited following the random matching approach accurately reflect the decision processes underlying actual network formation. We do that in section 3.4.

Our second concern is that random matching involves the analysis of a subset of the possible relationships between the individuals in the random sample. Is this better than considering all relationships for which data exist, as in the matching within sample approach? In section 3.5 we show that for several models of network formation the answer is clearly "yes".

## 3.4 Can we trust data on hypothetical networks?

Although data on respondents' willingness to form a link has several advantages – it is forward-looking, it can identify feasible and attractive links that have not yet been activated, etc. – economists and other social scientists have a trained reticence to use data on hypothetical behaviors. In this section we ask whether we can trust that the data on hypothetical social network links form the basis for useful inference on the determinants of network formation. We address this question empirically, using household survey data collected in 2004 from 120 randomly selected pastoralist households in four communities of southern Ethiopia that have been repeatedly interviewed for several years as part of a study that provides rich background data on the respondents.[23]

We collected comparable social networks data from these households using two different approaches. The first is random matching. In each community we randomly matched each respondent with five other respondents that belong to the random sample from the same site. We then asked whether the respondent knew

[23] The original data were collected by the Pastoral Risk Management (PARIMA) project. Barrett et al. (2004) describe the location, survey methods and data.

the random match and whether the respondent would ask the match for a gift of one cattle. We subsequently asked our respondents to tell us how many people they could rely on to ask for cattle as a gift and asked for the names of up to five of those individuals, starting with the person they would ask first. These last two questions reveal the size of the respondents' relevant social network and the identities of their stronger links once we remove the random matching constraint that their potential links be with individuals belonging to the random sample.

In one site, we then interviewed as many as possible of the network members identified by the sample respondents, thereby providing a characterization of the respondents' local networks. In this site, one individual was not surveyed during this round. For three of our initial respondents we couldn't find any of the individuals mentioned while seven others were used as starting nodes of a different questionnaire and not subject to this exercise.

In one site, we then proceed to interview as many as possible of these individuals, providing a characterization of the respondents' local networks. In this site, one individual was not surveyed during this round. For three of our initial respondents we couldn't find any of the individuals mentioned while seven others were used as nodes of a different questionnaire and not subject to this exercise.[24] The analysis is therefore limited to the networks of 19 respondents, who named 70 people on who they could rely to ask for cattle as a gift. None of them was in the original sample, hence an analysis of the decision underlying the formation of these networks based on matches within the sample would be impossible. Of these

[24]Although we can recover the information on the identity of their network members and most of them were later found and interviewed, the differences in the survey instrument make these data imperfectly comparable. Thus we choose not to use them for this exercise.

70 people, we could trace and interview 46 (approximately two-thirds).[25] The difficulty we experienced in tracking down the identified network partners underscores the difficulties and costs associated with the characterization of local networks. If random matching generates results that are statistically equivalent to the actual networks, its simplicity would provide a good argument for its use.

Table 3.2 summarizes the network links established by these two different approaches for the 19 respondent households for whom we have both types of social networks data.[26] On the surface, the resulting network patterns seem quite different. The random matching approach yields 22.6% of the 93 matches as potential providers of a cattle transfer, while our characterization of the respondents' local networks suggests a far lower figure, only 5.7% of the possible matches (where possible matches are defined as the population of individuals named by at least one respondent as someone s/he would approach for a cattle transfer). Nonetheless, it seems hard to extract a conclusion about any behavioral difference from these values, given the differences in the ways that these relationship data were collected.

[25]These data represent the respondents' local networks subject to three caveats. First, it is clear that we effectively inquired about the identity of those who are socially closest to the respondent. Second, we assume that those individuals not named by our respondents are not part of their insurance networks. Of course, this may not be true. Perhaps some of them would be the $6^{th}$ or the $7^{th}$ person to be contacted in case of need but were omitted by our (arbitrary) rule limiting the insurance network to five individuals. An obvious consequence of this fact is that we are most probably underestimating the density of insurance dyads among this population, although this may not be a serious concern in this case as 10 out of the 19 respondents reported that they could rely on less than 5 individuals. Third, we cannot control for whether our respondents knew all the people named by other respondents and we neglect the possibility that insurance networks are embedded in a wider web of non-instrumental relations of friendship or social acquaintance.

[26]Some individuals were named by more than one of our respondents. We therefore have 50 links elicited among our 19 respondents and the 46 names they generated. Between these two sets of individuals there are 874 possible links, on which we only have direct information on 50. As mentioned in the previous footnote, we must assume that the other 824 links were not formed.

Table 3.2: Structure of insurance links: two approaches

| Link exists? | Yes | No | Total |
|---|---|---|---|
| Random matching[27] | 21 | 72[28] | 93 |
| Local network | 50 | 824[29] | 874 |
| Total | 71 | 896 | 967 |

[27] Data for the 19 respondents for who we found any of the insurance partners belonging to the local network.
[28] Elicited.
[29] Assumed.

We therefore test econometrically for the equivalence of the networks generated through random matching and direct elicitation, by estimating the model

$$\mathrm{Prob}\ (\mathrm{L_{ij}} = 1) = \Lambda(\gamma_1 \mathrm{X_{ij}}) \tag{3.1}$$

where the link variable ($\mathrm{L_{ij}}$) is a binary variable that equals one if a link between the respondent (indexed by $i$) and the match (indexed by $j$) is formed and is 0 otherwise and $\mathrm{X_{ij}}$ is the set of explanatory variables expressed as relative social distance, as in Santos and Barrett (2005) and Fafchamps and Gubert (2007), that we define and summarize in Table 3.3. Finally, we assume that the error term, $\varepsilon_{ij}$, follows the logit distribution, where $\Lambda(.)$ is its cumulative distribution function and we further assume that

$$\mathrm{E}(\varepsilon_{\mathrm{ij}}, \varepsilon_{\mathrm{ih}}) \neq 0 \text{ if} \mathrm{j} \neq \mathrm{h} \tag{3.2}$$

$$\mathrm{E}(\varepsilon_{\mathrm{ih}}, \varepsilon_{\mathrm{jh}}) = 0 \text{ if i} \neq \mathrm{j} \tag{3.3}$$

Taking advantage of having multiple matches for each respondent, we can then estimate equation 3.1 using a random effects specification of the logit model. One

Table 3.3: Variable definitions and descriptive statistics

| Variable | Definition | Random matching (1) | Local network (2) |
|---|---|---|---|
| Same clan | Dummy variable, equal to 1 if both $i$ and $j$ belong to the same clan | 0.279<br>(0.451) | 0.204<br>(0.403) |
| Same sex | Dummy variable, equal to 1 if both $i$ and $j$ are from the same sex | 0.473<br>(0.502) | 0.579<br>(0.494) |
| Bigger family | Absolute value of the difference in family size between $i$ and $j$ if $i$ has a bigger family than $j$, 0 otherwise | 1.882<br>(2.762) | 2.641<br>(3.263) |
| Smaller family | Absolute value of the difference in family size between $i$ and $j$ if $i$ has a smaller family than $j$, 0 otherwise | 2.452<br>(3.070) | 1.960<br>(3.362) |
| More land | Absolute value of the difference in land cropped between $i$ and $j$ if $i$ cultivates more land than $j$, 0 otherwise | 0.339<br>(0.730) | 0.143<br>(0.494) |
| Less land | Absolute value of the difference in land cropped between $i$ and $j$ if $i$ cultivates less land than $j$, 0 otherwise | 1.016<br>(1.263) | 5.989<br>(6.472) |
| More cattle | Absolute value of the difference in cattle owned between $i$ and $j$ if $i$ has more cattle than $j$, 0 otherwise | 2.290<br>(4.608) | 0.815<br>(2.885) |
| Less cattle | Absolute value of the difference in cattle owned between $i$ and $j$ if $i$ has less cattle than $j$, 0 otherwise | 9.720<br>(22.503) | 45.602<br>(90.659) |
| More experience | Absolute value of the difference in experience as herder between $i$ and $j$ if $i$ has more experience than $j$, 0 otherwise | 6.323<br>(11.239) | 6.547<br>(11.661) |
| Less experience | Absolute value of the difference in experience as herder between $i$ and $j$ if $i$ has less experience than $j$, 0 otherwise | 9.839<br>(13.949) | 10.029<br>(13.509) |
| Number of observations | | 93 | 874 |

alternative way of modeling the error term is to assume that,

$$\mathrm{E}(\varepsilon_{\mathrm{ih}}, \varepsilon_{\mathrm{jh}}) \neq 0 \text{ if i } \neq \text{ j} \tag{3.4}$$

that is, to incorporate the effect of matches' unobserved heterogeneity upon the link formation decision. Both (Udry and Conley, 2005) and Fafchamps and Gubert (2007) correct the variance matrix for the possible effect of matches' unobservables, using Conley (1999) estimator but do not find large differences due to this correction.[30]

We follow a different strategy, using a nonparametric permutation test known as Quadratic Assignment Procedure (QAP) (Hubert and Schultz, 1976, Krackhardt, 1987, 1988) to obtain correct p-values. The basic intuition behind this procedure is that the permutation of the data on the dependent variable must maintain its clustered nature. In practice, this means that the same permutation must be applied to respondents and matches. We can then estimate the above model when all correlation between dependent and independent variables is broken through resampling – that is, when the null hypothesis that all slopes equal zero is known to be true – and compare our first estimates with their empirical distribution obtained through the repetition of this exercise (in our case, 200 times), to generate a sampling distribution for the parameter estimates. Although we present both uncorrected and QAP–corrected p-values, we also find that this added control for unobserved heterogeneity across individuals yields no substantial difference in our results.

Table 3.4 presents the results of two models. Column (1) reports the parameter estimates when we consider the data obtained through random matching for the

[30]Although Fafchamps and Gubert (2007) mention that their Monte Carlo simulations support the importance given to this issue, as corrected standard errors can be much larger than uncorrected ones.

19 respondents for whom we could find any member of her local network. Column (2) presents the analogous regression estimates when we analyze the data on local networks. The qualitative results are quite similar: belonging to the same clan and being of the same sex have a positive effect on the likelihood of a transfer relationship, although there is considerable difference in the precision of these estimates, likely due in large part to the difference in sample size.

To understand if these two approaches produce results that are statistically similar, such that the random-matching approach can guide our understanding of how local networks form just as reliably as direct, unconstrained elicitation of social networks, we pool both sets of observations on links between individuals in this population and estimate the model

$$\text{Prob}\ (\mathrm{l^{ij}} = 1) = \Lambda(\gamma_1 \mathrm{X^{ij}}, \gamma_2(\mathrm{X^{ij}} \times \mathrm{RM})) \tag{3.5}$$

under the same assumptions as above. The dummy variable RM takes the value 1 if the observation was obtained through random matching and 0 otherwise. A test of the joint null hypothesis that $\mathrm{H_0}\colon \gamma_2=0$ then serves as a test for the statistical equivalence of the two methods at empirically identifying these insurance networks. Failure to reject the null hypothesis indicates that both approaches yield similar information about the structure of social networks.

Table 3.5 presents the regression coefficient estimates and p–values, as well as the Wald test of the null hypothesis that $\gamma_2=0$ for the slope terms (i.e., excluding the intercept, affected by the five name limit we imposed on respondents in reporting their prospective insurance partners).

The smallest p–value on a single parameter estimate in $\gamma_2$ exceeds 0.2 and the p–value on the joint null hypothesis is 0.858. Turning to the QAP–corrected p–values, we get similar results. In no case can we reject the null hypothesis at the

Table 3.4: Regression results

| Variable | Random matching (1) | | | Local network (2) | | |
|---|---|---|---|---|---|---|
| | Coefficient | p–value | QAP p–value | Coefficient | p–value | QAP p–value |
| same clan | 1.170 | 0.220 | 0.280 | 1.231 | 0.000 | 0.040 |
| same sex | 1.451 | 0.108 | 0.220 | 0.110 | 0.747 | 0.440 |
| bigger family | -0.012 | 0.960 | 0.530 | 0.007 | 0.908 | 0.450 |
| smaller family | 0.065 | 0.727 | 0.460 | -0.007 | 0.886 | 0.560 |
| more land | 0.025 | 0.634 | 0.470 | 0.251 | 0.364 | 0.300 |
| less land | -0.037 | 0.304 | 0.440 | -0.002 | 0.936 | 0.440 |
| more cattle | -0.223 | 0.241 | 0.340 | -0.053 | 0.456 | 0.360 |
| less cattle | -0.001 | 0.968 | 0.510 | 0.001 | 0.787 | 0.330 |
| more experience | 0.232 | 0.815 | 0.110 | 0.004 | 0.788 | 0.500 |
| less experience | 0.417 | 0.257 | 0.070 | -0.004 | 0.789 | 0.460 |
| constant | -3.579 | 0.033 | 0.470 | -3.263 | 0.000 | 0.270 |
| Number of Observations | 93 | | | 874 | | |
| Number of Respondents | 19 | | | 19 | | |

Table 3.5: Testing the equivalence between different approaches

| Variable | Coefficient | p–value p–value | QAP |
|---|---|---|---|
| same clan | 1.228 | 0.000 | 0.100 |
| same clan × RM | -0.192 | 0.783 | 0.410 |
| same sex | 0.130 | 0.714 | 0.550 |
| same sex × RM | 0.538 | 0.410 | 0.320 |
| bigger family | 0.010 | 0.861 | 0.470 |
| bigger family × RM | -0.091 | 0.588 | 0.340 |
| smaller family | -0.006 | 0.905 | 0.450 |
| smaller family × RM | 0.062 | 0.605 | 0.340 |
| more land | 0.263 | 0.352 | 0.350 |
| more land × RM | -0.104 | 0.847 | 0.440 |
| less land | -0.002 | 0.934 | 0.520 |
| less land × RM | 0.324 | 0.205 | 0.050 |
| more cattle | -0.055 | 0.444 | 0.450 |
| more cattle × RM | -0.142 | 0.323 | 0.280 |
| less cattle | 0.001 | 0.785 | 0.410 |
| less cattle × RM | -0.010 | 0.516 | 0.230 |
| more experience | 0.004 | 0.787 | 0.420 |
| more experience × RM | -0.025 | 0.434 | 0.590 |
| less experience | -0.004 | 0.763 | 0.510 |
| less experience × RM | -0.012 | 0.669 | 0.450 |
| constant | -3.252 | 0.000 | 0.030 |
| constant × RM | 1.607 | 0.069 | 0.010 |
| $H_0$: $\gamma_2$=0 (not including constant) | | | |
| Wald statistic | 5.470 | 0.858 | |
| $\sum \mid \gamma_2(X_{ij} \times RM) \mid$ | 1.507 | | 0.975 |
| Number observations | 967 | | |
| Number respondents | 19 | | |

usual levels of statistical significance, although in one case (the variable "less land $\times$ RM") we are clearly at its limit. This does not change our conclusion regarding the joint null hypothesis, tested through the statistic

$$\sum \mid \gamma_2(\mathrm{X_{ij}} \times \mathrm{RM}) \mid \tag{3.6}$$

that generates a measure of how distant the sum of all slopes is from zero. This test statistic equals 1.507 (Table 3.5) and has a QAP–corrected p–value of 0.985. We clearly cannot reject the null hypothesis that random matching provides a method of identifying the structure of respondents' social networks that is statistically equivalent to direct elicitation following standard methods. Random matching does indeed seem to provide useful inference about the structure of local networks.

One way to overcome, at least partially, the fact that we may be looking at variables that are slightly different is to look at the other piece of information we have about these networks: the number of links that each respondent thinks can be mobilized in case of need, this time without any limit imposed by the interviewer. We have information on this variable for the respondents in the four sites. Does a model such as the one from equation 3.1 yield predictions of network size that are accurate enough to give us a good idea of the extent of the respondent's network?

To answer this question we re–estimate the model from equation 3.1 using the data from the four sites. The estimation results are presented in Table 3.6.[31] We then use these results to predict (out of sample) the probability that each respondent would ask for cattle from any of the 29 potential matches in each village, hence generating a 30 x 30 matrix of predicted values of probability of a link.[32] Assuming that a link is formed if such probability is above some arbitrary

[31]Because we only use these results to predict out of sample we skip the presentation and discussion of QAP-corrected p-values.

[32]By convention, links with oneself do not exist.

Table 3.6: Asking for gifts

| Variable | Coefficient | p–value |
|---|---|---|
| same clan | 1.947 | 0.000 |
| same sex | -0.026 | 0.810 |
| bigger family | 0.015 | 0.821 |
| smaller family | 0.007 | 0.920 |
| more land | -0.054 | 0.662 |
| less land | 0.081 | 0.505 |
| more cattle | -0.002 | 0.825 |
| less cattle | 0.006 | 0.505 |
| more experience | 0.011 | 0.538 |
| less experience | -0.016 | 0.258 |
| village 1 | -0.209 | 0.747 |
| village 2 | -0.436 | 0.479 |
| village 3 | 1.343 | 0.008 |
| constant | -2.208 | 0.000 |
| N | 551 | |

threshold (here, 0.5), we can construct a square matrix of links. Finally, summing across the columns of this matrix we can obtain an estimate of the number of individuals that each respondent could ask for a transfer.

How does this estimate correlate with the number of people that could be asked for gifts, as reported by the respondents themselves? Quite highly. The Pearson correlation coefficient equals 0.337 (with a p-value of 0.002).[33] We interpret this result as additional supporting evidence that the random matching approach yields data that accurately reflect the behavior underlying the formation of these networks.

These are not necessarily surprising results. An extensive literature on stated choice methods suggests that when properly contextualized, elicitation of hypo-

[33] The coefficient of rank correlation may even be a better indicator of the fit between the predictions of the model and the elicited values given that the maximum number of predicted links in each village is constrained to the size of the village sample and no such constraint was imposed when eliciting the size of the network. The Spearman $\rho$ is 0.525 and also statistically significant (p-value=0.000).

thetical behaviors can provide an accurate view of actual behaviors (Arrow et al., 1993, Carson and Hanemann, 2005). As a concrete example of this equivalence, Barr (2003) shows that her experimental results, intended to understand how people form insurance networks in villages in Zimbabwe, were mirrored by reality in that the networks of risk pooling contracts constructed during the experiment and the networks existing in real life were significantly correlated.

## 3.5 Monte Carlo evaluation of different approaches to network sampling

Having shown empirically that randomly matched data on willingness to establish a link can guide the inference on the determinants of network formation, we now turn to our second core question: How reliable are inferences about social network structure based on different approaches to sampling data on individuals and relationships? We answer this question through the use of Monte Carlo simulation so that we can know (by construction) the underlying network formation process and then test which sampling methods generate data that permits accurate inference of that process.

We start by constructing an artificial village of 200 households that mimics, in terms of the distribution of the different variables (clan, gender, cattle ownership, etc.), the data to be used in section 3.4 (and described in Table 3.3, column 1). We then consider three models of link formation. In the first, which we call Random Links, these variables play no role in explaining the relationships between individuals, which originate purely through a random process. Although we do not believe this reflects actual behavior underlying the formation of instrumental networks, it provides a useful benchmark with which to compare the performance of the different sampling strategies, as it helps us establishing whether particular sampling designs might be predisposed to suggest structure where none really exists.

In the second model of link formation, which we call Structured Links, the propensity to form a link is a linear function of the variables included in the characterization of the village, similar to the one that we estimated for the set of all respondents in the previous section (presented in Table 3.6). When this propensity is above a certain threshold (here, 0) a link is formed. Our third and final model is a minor variation on the Structured Links model, in which we limit the number of links an individual may form. We call this process Limited Links. Again, a threshold in the propensity to form a link has to be crossed for a link to be formed (the threshold remains 0) but an individual cannot form more than a limited number of links. For those who would surpass the limit, links are randomly deleted down to the imposed (and common, within the village) limit. We obviate this admittedly mechanical way of capping the number of links in a network by considering the effect of different limits (10, 20 and 30 links).

After specifying the structural process of social link generation, we then estimate, in the population, the same logit model from equation 3.1 (repeated here for convenience),

$$\mathrm{Prob}(\mathrm{L_{ij}} = 1) = \Lambda(\gamma_1 \mathrm{X_{ij}})$$

where the variables have the same meaning as above: $\mathrm{L}_{ij}$ is a binary variable that is equal to one if a link between $i$ and $j$ is formed, $\mathrm{X}_{ij}$ is the set of explanatory variables expressed as relative social distance and $\Lambda(.)$is the logit cumulative distribution function. In table 3.7 we present the population estimates of this model, the "true" relation between the links and the explanatory variables for each of the three network formation models under consideration.

Table 3.7: Logit estimates of the link formation decision

| | Random Links | Structured Links | Limited Links | | |
|---|---|---|---|---|---|
| | | | 10 | 20 | 30 |
| Same clan | 0.0338 | 2.2467 | 0.3478 | 0.4939 | 0.6817 |
| Same sex | 0.0182 | 0.4027 | 0.0074 | 0.4230 | 0.6005 |
| More experience | -0.0006 | 0.5565 | -0.1211 | -0.0271 | 0.0581 |
| Less experience | 0.0003 | -0.5605 | -0.1528 | -0.2428 | -0.1174 |
| More land | 0.0582 | 1.4182 | 1.3666 | -0.4339 | -1.2254 |
| Less land | 0.0136 | -1.2401 | -1.3031 | 0.4746 | 0.0010 |
| More cattle | -0.0002 | -0.6689 | -0.0485 | -0.0401 | -0.0422 |
| Less cattle | 0.0000 | -0.0847 | -0.0065 | -0.0235 | -0.0263 |
| Bigger household | -0.0110 | -1.7549 | -0.0089 | 0.1164 | 0.0586 |
| Smaller household | -0.0065 | 0.3423 | 0.3200 | 0.3446 | 0.0593 |
| Constant | 0.3256 | 4.5544 | -2.0324 | -1.8256 | -1.8109 |

In the remainder of this section we analyze how well one can recover the underlying structure of network formation through the use of two different sampling strategies. The first randomly samples individuals and then considers all the links among these individuals, the commonplace *matches within sample* approach. The second is the *random matching* approach, which, as explained above, randomly samples relations among randomly sampled individuals. While the first approach is perhaps easy to understand (we sample individuals and consider *all* the links between them), the second involves a second level of random sampling, as we just consider *some* of the possible links formed by the randomly selected individuals.

Given that we're interested in understanding which approach gives us a more accurate representation of the link formation process in the population (known by construction), we mainly focus in tests of the hypothesis

$$\mathrm{H}_0 : \gamma^{\text{sample}} = \gamma^{\text{population}} \tag{3.7}$$

where $\gamma^{population}$ represents the parameter vector for each underlying model of network formation and is given in Table 3.7. For each sampling method – matches within sample and random matching, the latter with 5, 10 or 15 random matches – and for each of four different sampling ratios (0.33, 0.50, 0.66 and 0.90) we generate 100 samples and estimate the logit equation 3.1 each time. Table 3.8 reports the frequency with which we fail to reject null hypothesis (equation 3.7), i.e., the frequency with which the resulting sample generates inferences consistent with the true underlying data generating procedure.[34] The Stata code used to generate the village characteristics, the links between individuals, the sampling procedures and how we evaluate their consequences is presented in the Appendix.

This Monte Carlo analysis yields four main results. First, inference based on matches within sample, the most commonly used approach for analyzing local networks, seems valid only when links are formed randomly, an unlikely and uninteresting case, as it would signal that no intentional behavior is present. For other models of network formation, matches within sample seem to perform well only when the sampling ratio is quite high. Under the "structured links" and different "limited links" models, the matches within sample approach is virtually incapable of revealing the structure of link formation for sampling ratios as high as 2/3. This calls into question the reliability of inference about social network formation patterns based on data collected using the matches within sample method.

Second, as a rule, the random matching approach beats the matches within

[34]Percentage of failure to reject $\mathrm{H}_0 : \gamma^{\text{sample}} = \gamma^{\text{population}}$, based on 100 replicates. "Limited links (10)" refers to the link formation process where we imposed that no individual established more than 10 links; "Limited links (20)" and "Limited links (30)" have similar interpretations. "Random matching: 5 relations" refers to presenting each respondent with 5 randomly sampled from matches within the sample; "Random matching: 10 relations" and "Random matching: 15 relations" have similar interpretations.

Table 3.8: Monte Carlo evaluation of two sampling approaches:Matches within sample vs. Random matching

| *Sampling ratio (individuals)* | *33* | *50* | *66* | *90* |
|---|---|---|---|---|
| Random Links | | | | |
| Matches within sample | 92 | 99 | 100 | 100 |
| Random matching: 5 relations | 96 | 96 | 96 | 94 |
| Random matching: 10 relations | 98 | 94 | 95 | 99 |
| Random matching: 15 relations | 96 | 100 | 95 | 95 |
| Structured Links | | | | |
| Matches within sample | 0 | 0 | 0 | 92 |
| Random matching: 5 relations | 25 | 29 | 63 | 69 |
| Random matching: 10 relations | 11 | 26 | 47 | 73 |
| Random matching: 15 relations | 1 | 15 | 48 | 78 |
| Limited Links (10) | | | | |
| Matches within sample | 4 | 2 | 4 | 60 |
| Random matching: 5 relations | 73 | 83 | 91 | 93 |
| Random matching: 10 relations | 68 | 70 | 86 | 93 |
| Random matching: 15 relations | 58 | 57 | 82 | 92 |
| Limited Links (20) | | | | |
| Matches within sample | 2 | 1 | 4 | 44 |
| Random matching: 5 relations | 74 | 79 | 91 | 95 |
| Random matching: 10 relations | 52 | 70 | 79 | 96 |
| Random matching: 15 relations | 38 | 58 | 74 | 97 |
| Limited Links (30) | | | | |
| Matches within sample | 0 | 1 | 3 | 30 |
| Random matching: 5 relations | 74 | 84 | 92 | 94 |
| Random matching: 10 relations | 51 | 68 | 77 | 91 |
| Random matching: 15 relations | 38 | 57 | 66 | 93 |

sample approach. Especially in the “limited links” models, the performance of the random matching model is far better than that of the matches within sample approach, albeit still imperfect. Indeed, this is not to say that random matching is adequate under all circumstances. In particular, if social links are formed according to what we termed “structured links”, i.e., without limits to the size of networks, then this approach can still perform quite poorly, even if it remains clearly superior to the “matches within sample” approach under standard sampling ratios (i.e., below 90%).

Third, our capacity to accurately describe the link formation decision decreases as we increase the number of relations sampled, emphasizing the importance of sampling relations after sampling individuals, reflecting the double nature of social networks. Given that in the limit, when each respondent in a sample is presented with all possible matches, the two procedures are identical this is a plain consequence of the already discussed superiority of the random matching approach when compared to the matches within sample. This is especially evident in the more interesting models, when links are not randomly formed, and for sampling ratios below 90%.

Finally, we notice that the results regarding the adequacy of the random matching approach under the Limited Links model does not change much with the maximum number of links allowed (and, consequently, with the density of links in the population). Random matching appears slightly more accurate the lower the limit on the number of links formed in the population. But what really seems to matter most is the existence of such a limit.

## 3.6 Conclusions

This paper makes a methodological contribution to the growing literature that aims at understanding how social networks are formed, typically as a first step toward analysis of social networks' role in explaining individual behavior and outcomes. We validate a new approach to the collection of data on network structure – which we label "random matching" – where individuals from a random sample are allowed to form links with randomly matched individuals from the same sample. The central advantages of this approach are two: the ease with which it can be integrated into the surveys that economists commonly conduct and use and the fact that both respondent and match are part of the sample.

We compare the determinants of individuals' decision to link or not to link with a random match with the determinants of directly elicited local networks and conclude that these two data collection processes generate statistically identical results with respect to the correlates of social network structure. Furthermore, the size of the predicted network generated by the random matching data is highly correlated with the size of the local network directly elicited from survey respondents. Finally, we demonstrate, via Monte Carlo methods, the superiority of this random matching approach relative to the more conventional method of using all the links between individuals in a random sample.

The way in which we established the relation between the elicited size of the respondent's network and its predicted size at the end of section 3.4 also suggests how we believe researchers might usefully employ the random matching approach to sampling social networks. In addition to providing a statistically valid means of eliciting data for analysis of social network structure, which may be interesting in its own right, one can also use the resulting parameter estimates to predict respondents' networks and subsequently perform analyzes based on those predicted networks. This is similar, in spirit, to the analysis by Woittiez and Kapteyn (1998), who estimate a latent variable model to infer the unobserved reference groups of respondents, after which the means of behaviors within such groups (in their case, hours of work and labor force participation rate) are used as explanatory variables for individual decisions (in their case, the labor market behaviors of Dutch women). In doing this, one must recognize, however, that we start from simple local rules and aim at the complete structure. Although some evidence exists on the utility of such approach for some questions,[35] more work is probably needed before the validity of these generated variables is reasonably established.

This paper by no means resolves questions of how to identify the structure of social networks of all sorts and under all conditions. Our results reflect only data from insurance networks in just one location, and it is also obvious that the utility of asking questions about potential links is limited in some cases.[36] But, if the validity of the random matching approach to collecting data on social networks is confirmed in other settings, it could help establish a statistically valid and cost-effective method for generating data for social networks analysis to respond to burgeoning questions about the role and importance of social connectivity in processes of economic development, free of some of the key inferential problems that presently plague this literature.

[35]See the discussion in Morris (2003) and an application to the epidemiology of HIV in Kretzschmar and Morris (1997)

[36]It is hard to imagine, for example, how and why one would ask a respondent about his/her willingness to form a sexual network, even if cultural norms made such questions permissible.Yet understanding such networks is essential to study the epidemiology of sexually transmitted diseases.

# Chapter 4

# Persistent poverty and informal credit

## 4.1 Introduction

Risk is a central feature of life in rural areas of developing countries and therefore has appropriately attracted much attention in the economics literature. The focus of much of this literature has been on how households smooth consumption in the face of idiosyncratic variations in income, either by analyzing how specific actions – most commonly, credit, insurance or savings – contribute to that objective,[1] or by asking how well the complete set of available instruments performs in stabilizing consumption.[2] The consumption smoothing literature uniformly starts, however, from a key assumption that shocks have only transitory consequences, in other words that the income generation process is stationary. Coate and Ravallion (1993, p.4), for example, justify their focus on symmetric insurance arrangements with the assumption that "either player could end up 'rich' or 'poor' in any period" with equal probability. However, the assumption that income generation processes are stationary and thus that all poverty is transitory does not easily square with the empirical evidence, which suggests that a substantial share of poverty in many low–income countries is persistent (Baulch and Hoddinott, 2000, Barrett, Carter, and Little, 2006) and that rates of intergenerational earnings transmission are high even in high income countries (Solon, 2002).

As is widely recognized, uninsured risk and persistent poverty may be linked,

[1] See Alderman and Paxson (1994), Besley (1995) or Lim and Townsend (1998) for useful reviews.

[2] Deaton (1992) and Townsend (1994) are key contributions in a large literature that tests for the presence of full insurance or risk pooling in developing countries.

either because poorer individuals choose safer investment portfolios that prove, on average, less profitable (Rosenzweig and Binswanger, 1993, Morduch, 1995, Dercon, 1996, Bardhan, Bowles, and Gintis, 2000), or because negative shocks have a disproportionately detrimental impact on poor people's investments, perhaps especially with respect to the formation of human capital (Jacoby and Skoufias, 1997, Dasgupta, 1997, Alderman, Hoddinott, and Kinsey, 2006, Carter et al., 2007). Whether due to the former, ex ante effects or the latter, ex post ones, risk and shocks may have long-lasting effects on welfare status. But while the link from risk to persistent poverty has been probed extensively, the link from persistent poverty back to risk management options remains underdeveloped. This paper aims to contribute to filling that void.

Theoretical models in which poverty is a stable dynamic equilibrium suggest two key conditions under which short-term shocks can have longer-term consequences. First, a non–convexity in some technology generates a critical threshold, an unstable dynamic equilibrium at which wealth dynamics bifurcate. This causes the mapping from current to future wealth to exhibit multiple stable dynamic equilibria. When at least one of them associated with consumption below the poverty line, a (stochastic) poverty trap emerges. Second, some sort of market imperfection, on average, prevents those initially below the unstable dynamic equilibrium from moving themselves above the threshold so as to jump onto a path that converges on a higher welfare level. In such a world, even a transitory shock associated with a stationary stochastic process can have permanent effects. This has been well-recognized in the literature (Azariadis and Stachurski, 2005, Carter and Barrett, 2006). What has not yet been recognized is that the first condition above – the existence of an unstable dynamic equilibrium wealth level – might induce the market imperfection that is the second condition for risk to cause persistent poverty. Multiple equilibria might lead to credit (and insurance) rationing that excludes the poorest, those who most need financial instruments to manage risk. In this paper we empirically explore this possibility that nonconvex wealth dynamics might induce exclusion of the very poor from informal credit markets that might facilitate their escape from poverty.

The extensive literature on equilibrium credit rationing focuses largely on adverse selection and moral hazard may cause the poor to be disproportionately rationed out of credit markets.[3] Poverty matters because it leads to "desperation" (Banerjee, 2000): the poor are not creditworthy because, having too little to loose, it may be prohibitively costly for a lender to punish them in case of default (see also Banerjee and Newman (1993)). This paper suggests an additional plausible explanation. If the lender has an interest in the results of the project because informal loans bundle an insurance or equity element with the loan – as Udry (1994) finds in studying northern Nigeria – then the presence of non–convexities may turn the unstable dynamic equilibrium (or its neighborhood) into a focal point for loans, since this is the point at which the expected gains to the borrower are greatest. In this context, those who are not too poor (the "middle class") become preferred borrowers, while both poorer individuals and the very rich are excluded from such credit arrangements. Observing such behavior in informal credit arrangements would reinforce a key long-recognized policy implication of non–convexities: small transfers can have large, long-term welfare impacts if they lift an ex ante poor recipient onto a path of sustained accumulation towards a higher level equilibrium.

The remainder of the paper proceeds as follows. Section 4.2 introduces the

[3]See Stiglitz and Weiss (1981) or Carter (1988) for early contributions to this literature and Banerjee (2001) for an excellent recent synthesis.

setting we study and the data we use, collected from Boran pastoralists in southern Ethiopia, drawing partially on previous work (Lybbert et al., 2004, Santos and Barrett, 2006) that has documented nonlinear wealth dynamics and the presence of an unstable dynamic wealth equilibrium in this system and explained the apparent sources of this structure. In this paper, we take the existence of such phenomena as given in order that we can focus on the implications of prospective multiple equilibria on informal lending relationships. In section 4.3 we study how informal credit networks form among Boran pastoralists. We find that the decision to extend credit (in kind) to an individual is better explained by the expected gains due to the transfer than by the recipient's expected capacity to repay the loan. This result is robust to a series of additional controls for individual ability, correlation in asset returns between borrower and lender, and the ex ante network of the lender. These findings imply a "middle class" bias in informal lending of the sort we study, in which the poorest members are rationed out of informal credit markets in equilibrium due to the existence of an unstable dynamic wealth equilibrium. In section 4.4 we then study patterns of social acquaintance (hereafter, social networks) and find that wealth plays a role in explaining who is known within a community. Being destitute (i.e., having no wealth in cattle) has a strong, negative impact on the probability of being known within the community. And since credit networks are nested within social networks, social invisibility further reinforces the exclusionary process associated with credit rationing. Finally, section 4.5 discusses the policy implications of our findings.

## 4.2 Nonlinear wealth dynamics: evidence from southern Ethiopia

Lybbert et al. (2004) analyze wealth dynamics among Boran pastoralists, a poor population in southern Ethiopia. Using herd history data for 55 households over a 17 year period, they show that herd dynamics follow a S-shaped curve with two stable equilibria (at approximately 1 and 35–40 cattle), separated by an unstable threshold (at 12–16 cattle), consistent with stylized poverty traps models. Drawing on prior ethnographic research and extensive direct field observation, the authors suggest that this threshold results from a minimum critical herd size necessary to undertake migratory herding to deal with spatiotemporal variability in forage and water availability. Those with smaller herds are forced to stay near their base camps, where pasture conditions soon get degraded, leading to a collapse of herd size towards the low-level stable equilibrium, while those with bigger herds can migrate in search of adequate water and pasture, enabling them to sustain far larger herds.[4]

These authors present two other findings that help motivate the present paper. First, they show that asset risk is predominantly idiosyncratic. This creates conditions conducive to the implementation of welfare-improving insurance or lending contracts among pastoralist households. Nevertheless and second, inter-household gifts and loans of cattle are conspicuously limited.[5] The purpose of this paper is

[4]During migration only part of the household moves, mainly young men, who are physically strong enough to undertake arduous, long treks to move herds between distant water points and to protect them against (human and animal) predators. Hence the need for a sufficiently large herd that can be split and still feed both the migrant herders and the remaining (largely child, aged, infirm and female) members of the household who are left at the base camp.

[5]Several recent studies from semi-arid African systems confirm the relatively small importance of gifts and loans, both of income (Lentz and Barrett, 2004) and

to understand whether such paucity of prospectively welfare-improving informal financial transactions might be a direct consequence of the apparent poverty trap faced by these pastoralists.

In order to answer that question, in 2004 we collected new data on expected wealth dynamics and on bilateral credit relations within the same communities (but not the same individual respondents) studied by Lybbert et al. (2004). This effort took place within a larger research project that has repeatedly surveyed these same households since 2000, generating a data set that includes rich detail on household composition, migration histories and herd changes, among other relevant characteristics.[6] The data on expected wealth dynamics are discussed and analyzed in detail in Santos and Barrett (2006). Here we only briefly present key elements of that discussion that are necessary to understand our two key explanatory variables: borrowers' expected gains from a loan and their expected future wealth.

We first asked each respondent about his/her expectation regarding weather conditions for the coming year. We then assigned each respondent four initial (hypothetical) herd sizes, randomly selected from the interval 1–60 animals, and then elicited their subjective herd size distribution one year ahead, given the state of nature just elicited and the seed herd size. These data equipped us to model the relation between initial and expected future wealth - herds are the lone non-human form of wealth in the study area - for each of the four states of nature considered (drought, bad year, good year, very good year). Combined with meteorological information on rainfall histories, these estimates were enabled us to

assets (McPeak, 2004, Kazianga and Udry, 2006, McPeak, 2006)

[6]The data were collected by the Pastoral Risk Management (PARIMA) project of the USAID Global Livestock Collaborative Research Support Program. Barrett et al. (2004) describe the location, survey methods and available information.

simulate the empirical distribution of herd size several periods (up to ten years) ahead. Motivated by the large dispersion of expected herd size under conditions of bad rainfall, we then investigated how latent ability to deal with shocks affected wealth dynamics.[7] Using the estimated herding ability for each respondent, we classified respondents into two categories: low ability (those in the $4^{\text{th}}$ quartile) and a residual higher ability category. We then redid the herd growth simulations described above to establish the relation between expected wealth and initial herd size for each state of nature and each ability category. The results, presented in figure 4.1, suggest a complex growth mechanism that combines both club convergence (defined by herders' ability) and, for the higher–ability club, multiple equilibria.

Our data on the willingness to extend credit to an individual follows an approach introduced by Goldstein and Udry (1999). We randomly matched each respondent with other respondents from the sample and asked two types of questions. The first about (real) social networks, through the question "Do you know (the match)?". The other on the possibility of transferring cattle as a loan if the match asked for it.[8] The latter question provides information on potential credit networks and is the subject of study in the next section. Our approach to data collection offers one major advantage relative to previous studies of informal transfers. Because we know the characteristics of both lender and borrower, we

[7] More precisely, we used the 2000-3 panel data to estimate a herd growth function frontier using a composed error term that includes a symmetric random component reflecting standard sampling and measurement error and a one-sided term reflecting herder-specific, time-invariant inefficiency, which we assumed follows a truncated normal distribution. The results are available upon request and are discussed at length in Santos and Barrett (2006).

[8] We asked also about the possibility of transferring cattle as gifts but the pattern of answers is virtually identical and loans and gifts seem empirically indistinguishable. Out of 561 matches, in only 13 (2.3%) does the decision differ between loans and gifts. We therefore concentrate on transfers deemed "loans" in what follows.

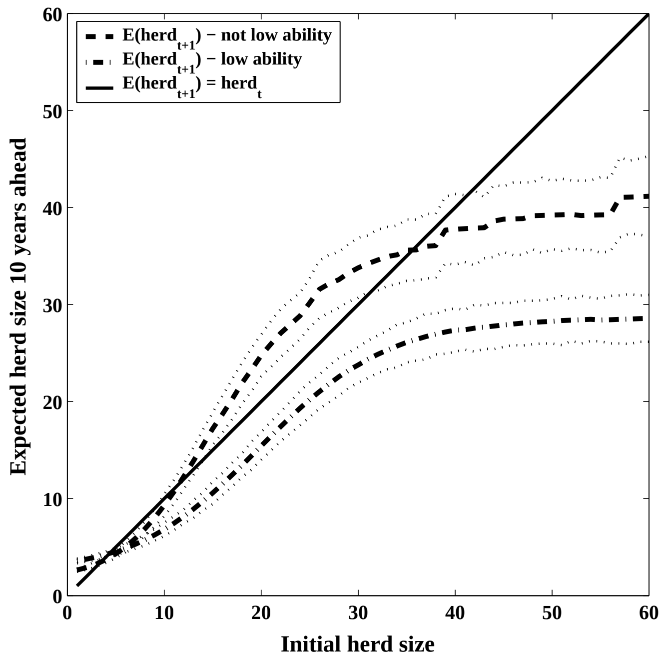


Figure 4.1: Heterogeneous wealth dynamics

can avoid concerns of biased estimates due to lack of knowledge about one end of this relation (Rosenzweig, 1988, Cox and Rank, 1992).

However, there are two prospective problems with this approach. First, by studying links between individuals rather than the transfers themselves, one risks errors due to excessive discretization. This does not seem to be a problem in our data because informal asset transfers among Boran pastoralists are quite small. In our sample, over the period 2000–03, there were 15 such transfers, out of which 12 (i.e., 80%) were of 1 or 2 cattle.[9] For that reason, and with only a slight abuse of language, we use the terms "credit network formation" and "loans" interchangeably in what follows.

[9]A separate survey of cattle transfers motivated by shocks, conducted in 2004, in the same geographical area but with different respondents, suggests even greater dominance of small transfers: out of 112 transfers, 102 (or 91%) were of 1 animal, 8 (or 7%) were of 2 cattle and the remaining less than 2% were more than 2 cattle.

Second, one might reasonably wonder how well potential credit networks elicited in this manner reflect the decision process underlying the formation of real credit networks. In a separate paper Santos and Barrett (2007b) we show that the inferred determinants of insurance networks derived from the approach used in this paper closely match those obtained from analysis of real insurance relations among the same population. The appeal of using randomly matched respondents thus seems to outweigh the prospective pitfalls of using discrete data on hypothetical transfers.[10]

## 4.3 Nonlinear wealth dynamics and credit networks

The basic pattern of answers to the credit link questions is described in Table 4.1. Three key facts emerge clearly. First, not everyone knows everyone else, even in this rural, ethnically homogeneous setting in which households pursue the same livelihood and there is very little in– or out–migration. Although most people know the random match presented to them, almost 14% of the matches were unknown by the respondent. Second, social acquaintance is, for our respondents, clearly a necessary condition for willingness to make a loan: in only 2/69 cases did a respondent indicate that they would be willing to lend livestock to someone they did not know. The sequential structure of these answers carries consequences for our econometric strategy – in particular, it leads us to estimate the determinants of insurance networks only on the subsample of those who know their matches (Amemyia, 1975, Maddala, 1983) – and raises the additional question of identifying

[10]The benefits of using experimental data in the study of social capital (a concept closely related to that of social networks) is emphasized by Durlauf and Fafchamps (2005). Barr (2003) also concludes that experimental evidence is mirrored by reality.

Table 4.1: Knowing and lending: a sequential process

| Know \ Lend | Yes | No | Total |
|---|---|---|---|
| No | 67 | 2 | 69 |
| Yes | 367 | 144 | 511 |
| Total | 434 | 146 | 580 |

the correlates of exclusion from social networks, one that we explore in section 4.4. Finally, knowing people is by no means a sufficient condition for pastoralists to be willing to transfer animals to a match. In just under one quarter of the cases where the respondent knew the match was he or she willing to lend an animal to the match. The acquaintance between lender and borrower seems therefore to be necessary but insufficient for obtaining credit.

### 4.3.1 Understanding exclusion from credit contracts

The intuition behind the analysis of these responses is that respondents evaluate the expected benefits and costs of each potential link/loan, answering "yes" if their evaluation of the benefits exceeds the costs. Two motives may enter this calculus: the possibility that the borrower may not repay the loan and the value of the compensation for parting with an animal.

The first motive is the one usually emphasized in the literature that explores the relation between wealth and exclusion from contracts (see Banerjee (2001) for a review), usually concluding for a monotonically positive relation between borrower's wealth and its creditworthiness. If informal credit were strictly a textbook debt instrument, this might be the end of the story.

In our setting, however, as in many developing country settings, loans often come bundled with quasi–insurance (Udry, 1994) or an element of equity investment. Among the Boran, as we show below, lending is overwhelmingly in response to shocks, thus it functions much like insurance. Furthermore, informal lending traditions in this culture hold that the loan of a cow (even money to be used to buy animals, which is becoming less rare) entitles the lender not only to the original animal (the conventional loan component) but also to its male offspring, with female calves kept by the borrower. This introduces a second channel through which a borrower's wealth may matter: the borrower's expected herd growth drives the expected returns to the lender.

Clearly, these motives are non–exclusive and we can think that they jointly lead Boran pastoralists (indexed by $i$) to make lending decisions as if maximizing the net expected returns (ER) on a loan of one cattle to another herder (indexed by $j$):

$$\mathrm{ER_{ij}} = \sigma \mathrm{EG_j} \times \mathrm{r(EW_j)} - 1 \tag{4.1}$$

Here, $\mathrm{EG_j}$ stands for j's Expected Gains from a loan, $\sigma$ stands for the lenders' share in the gains from the loan, which is set by social convention (the male offspring hence, on average, half the gains), and r(EW) is the repayment function, which we assume, following the extant literature, to be a monotonically strictly positive function of borrower's Expected Wealth (EW), both evaluated at some relevant horizon T. Let

$$\mathrm{EW} \equiv \mathrm{E_0}\ \{\sum_{t=0}^{T} \mathrm{F(W_{jt} + l_{ij})} \times \theta_{jt} |\ \phi(\theta), \mathrm{W_0}, \alpha_j\} \tag{4.2}$$

where $\mathrm{F}(\bullet)$ is a growth function, $\mathrm{W_{jt}}$ is borrower's wealth at time t, $\mathrm{l_{ij}}$ is the binary decision reflecting the lender's decision regarding the loan, $\phi(\theta)$ is the distribution function of the production shocks, $\theta$, and $\alpha_j$ defines borrower's ability. Before we discuss the characteristics of the growth function, we define Expected Gains as

$$\mathrm{EG} \equiv (\mathrm{EW}|\, \mathrm{l_{ij}} = 1) - (\mathrm{EW}|\, \mathrm{l_{ij}} = 0) \tag{4.3}$$

Clearly, both EG and EW are a function of the same variables, namely borrower's ability and initial wealth, raising important empirical questions regarding the identification of the importance of each motive.

The growth function, F(•), in its most general form incorporates two possibilities, identified in earlier work in this environment (Santos and Barrett, 2006) and represented in figure 4.1. First, household characteristics (e.g., intrinsic ability, $\alpha_j$) may sort cross–sectional units into distinct cohorts or clubs, c. Second, within each club, agents might face nonlinear dynamics – in particular, the possibility of a critical threshold value, $\gamma_c$, at which the welfare dynamics bifurcate, with one path, subscripted $l$ , leading to a low-level equilibrium and another, subscripted $h$, leading to a high-level equilibrium. These possibilities imply that for each individual j, belonging to the club c=1, . . . , C, the growth function can be written as[11]

$$\mathrm{F^c_t} = \begin{cases} \mathrm{F^c_l(W_{jt} + l_{ij})} & \text{if } \mathrm{j} \in \mathrm{c},\ \mathrm{W_{jt}} \le \gamma_\mathrm{c} \\ \mathrm{F^c_h(W_{jt} + l_{ij})} & \text{if } \mathrm{j} \in \mathrm{c},\ \mathrm{W_{jt}} > \gamma_\mathrm{c} \end{cases} \tag{4.4}$$

The empirical relevance of the different variables has important implications

[11]Borrowing the terms from the growth literature, this specification can be simplified into a club convergence approach (as in Quah (1997)) if there are no asset thresholds at which asset dynamics bifurcate (that is, $\gamma_c$=0, ∀ c), or into a threshold model (as in Azariadis and Drazen (1990)) if there is only one club (that is, C=1). In the more standard case, one would assume that F(•) is concave, and that there are no convergence clubs or thresholds. Several approaches have been recently suggested to identify convergence clubs (for example, Canova (2004)) and thresholds (for example, Hansen (2000)) but not, to our knowledge, both. In the empirical section we'll build on previous work (**?**Santos and Barrett, 2006) to identify both convergence clubs and accumulation thresholds.

for our understanding of informal bilateral credit relations and for related policy interventions. If only matches' expected wealth drives credit access, it would signal that, although persistent poverty plays a role, the wealth threshold *per se* does is not important. In this case, we would expect the wealthiest herders to be the primary beneficiaries of these loans.

Given the small size of these loans, expected growth, even after the loan, is low or even negative for those in the vicinity of the stable equilibria (that is, the wealthiest or the poorest members of the community). On the other hand, and in expectation, they enable those below and "sufficiently close" to the unstable equilibrium to recover onto a growth path leading to a higher level equilibrium.[12] If expected gains guide the allocation of transfers, it might then induce a bias that favors herders of intermediate wealth. Such a pattern, if it exists, would suggest that the effects of informal lending (or equivalent insurance arrangements) in the presence of nonconvexities might be best understood as a mechanism to prevent participants from falling into persistent poverty.

Although in the empirical part of this paper we'll mainly focus on the analysis of

[12]Given the standard transfer of one animal from one household to another, individual transfers can clearly serve this safety net purpose only for those herders quite close the unstable equilibrium. One needs to recognize, however, that this limitation is purely an artifact of the two person, dyadic model we employ. Anecdotal evidence from a survey of life histories collected during fieldwork suggests that coordinated transfers are commonly sought and obtained, raising the potential for transfers to perform such a role over a wider herd size range although, unfortunately, not so wide as to catch the very poor or the destitute: the maximum size of a transfer such as this was 5 cattle. This is further corroborated by anthropological work among the Boran (Dahl, 1979, Bassi, 1990) on the functioning of *busa gonofa*, an institution through which such coordination is achieved. Similar institutions have been analyzed among other east African pastoralist societies (for example, Potkanski (1999)). Coordination of transfers raises a separate set of questions – e.g., how are the obvious free rider problems resolved? – that cannot be pursued here.

these two considerations – expected wealth with its standard effect on likelihood of repayment and expected gains – several other explanations of rationing of credit or insurance contracts merit attention. The closest study, empirically, to our analysis is McPeak (2006). The author explores different motives for livestock transfers in an environment quite similar to ours (the rangelands of Northern Kenya) and finds that transfers are targeted to wealthier pastoralists, which he interprets as reflecting differential capacity to reciprocate the original transfer, essentially our r(EW) function. More surprisingly, he finds support for an interpretation of asset transfers as a form of precautionary savings (his term) as transfers do not seem to be triggered by recent wealth shocks. We differ from this study in that we analyze the formation of credit networks through which such transfers occur and can condition our analysis on expected gains thanks to our analysis of the wealth dynamics. Omission of this term from McPeak (2006) could explain the difference in our results.

Hoff (1997) analyzes the relation between insurance arrangements, the erosion of investment incentives and the persistence of poverty, and predicts matches along wealth levels. Individuals with high enough expected wealth may not invest in insurance relations because the expected benefits may not compensate for expected net contributions to the insurance pool. This result implicitly depends on the lack of convergence in incomes between agents (i.e., some have higher expected income than others) and relies heavily on the impossibility of separating insurance from redistribution due to egalitarian sharing rules, an environment quite different from the one that we study. In the empirical section we test Hoff's model as well, since we use data from both sides of the credit contract and control for the lender's wealth.

Given that informal transfers can insure only against idiosyncratic shocks, asset covariance between potential insurance partners should matter to contracting choices, as the literature on peer selection in micro-credit arrangements suggests (Ghatak, 1999, Sadoulet and Carpenter, 1999). Agents might therefore rationally opt out of insurance contracts with those whose wealth covaries strongly with their own wealth. We'll address this possibility below as well, as an additional check on our results.

Finally, Murgai et al. (2002) suggest that the costs of establishing insurance links may limit the domain of equilibrium contracting. Genicot and Ray (2003) likewise suggest that insurance groups may be bounded because risk-sharing arrangements need to be robust to deviations by sub–groups. Although these authors do not explicitly model wealth as a source of friction that might prevent insurance links from forming, they offer complementary explanations for the behavior that we observe. In our empirical work, we therefore control also for covariates that may reflect differences in the degree of enforcement of such contracts or of monitoring of other agent's activity and, less perfectly, for the degree of alternative insurance ex ante of the link formation decision.[13]

[13]Unlike Genicot and Ray (2003), we address network formation rather than group formation. Groups differ from networks because the latter lack common boundaries. If A establishes a link with B, the fact that B already has a link with C does not mean that A will also have a (direct) link with C. Hence considerations about sub-group deviations may be less of a concern here than in more formalized institutions such as, for example, the funeral insurance groups studied by Bold (2005).

### 4.3.2 Econometric model

We study respondents' decision (to lend or not) using a model that nests the different explanations/motives for asset transfers under the reduced form

$$\mathrm{Prob}(\mathrm{l_{ij}} = 1) = \Lambda(\mathrm{EG_j}, \mathrm{EW_j}, \mathrm{L_j}, \alpha_\mathrm{j}, \mathrm{W_i}, \mathrm{X_{ij}}) \tag{4.5}$$

where $\mathrm{l_{ij}} = 1$ denotes that a credit link is formed between $i$ (the respondent) and $j$ (the match), $\mathrm{EG}_j$ is the match's expected gains from the loan of 1 animal, $\mathrm{EW}_j$ is the match's expected wealth after the same transfer, $\mathrm{L}_j$ indicates whether the match lost cattle in the recent past (in practice, the period 2000/03 for which we have data), $\alpha_j$ is a classification of the respondent as being of low or higher ability, $\mathrm{W}_i$ is the respondent's wealth and the $\mathrm{X}_{ij}$ vector captures a range of covariates describing the distance, in both the physical and socio-economic space, between $i$ and $j$. Finally, $\Lambda$ is the logit cumulative distribution function and we assume that relations are nested within respondents:

$$\mathrm{E}(\varepsilon_{\mathrm{ij}}, \varepsilon_{\mathrm{ih}}) \neq 0 \text{ if } \mathrm{j} \neq \mathrm{h} \tag{4.6}$$

$$\mathrm{E}(\varepsilon_{\mathrm{ih}}, \varepsilon_{\mathrm{jh}}) = 0 \text{ if } \mathrm{i} \neq \mathrm{j} \tag{4.7}$$

where $\varepsilon_{ij}$ is the error term of the regression. Taking advantage of having multiple matches for each respondent, we can then estimate equation 4.5 using a random effects specification of the logit model. Three issues need to be addressed before we present our estimates: (1) the way we construct our two central variables, expected gains from a loan and expected wealth, and how is identification obtained, (2) the way we express the distance between respondent and match (the vector $\mathrm{X_{ij}}$), which differs from the formalization of social distance presented in Akerlof (1997) and used in much of the empirical work, and (3) how to address the inferential problems that may arise if, contrary to our assumptions regarding the error term, unobserved heterogeneity across individuals is important for the network formation decision.

Both the expected gains and expected wealth variables were created following the simulation procedure briefly described above and in detail in Santos and Barrett (2006). We define borrower's "expected wealth" as the probability that future herd size ten years hence, post transfer of one animal, will be larger than a specified value – 30 cattle – given actual (2003) herd size. [14] We define "expected gains" of a loan as the difference in expected herd size, 10 years ahead, due to the transfer of 1 cattle given actual herd size. Both variables are graphically represented in Figure 4.2, with expected wealth the solid line (read against the righthand vertical axis) and expected gains the dashed line (read against the lefthand vertical axis). Two features merit particular attention. First, the probability that a recipient's herd size will reach the high-level asset equilibrium (more than 30 cattle) is S-shaped, with values less than 1% below 7 head and reaching a plateau in the 35-45% range beginning roughly at 22 head. Second, that initial herd size interval of 7-22 cattle – the neighborhood of the threshold at which wealth dynamics bifurcate – is the only asset range over which expected gains exceed the 1 cattle transfer.[15]

[14] Other herd sizes (10, 15, 20, 25, 35) lead to similar conclusions. We also experimented with the *change* in the probability of having a herd size above 30 due to the transfer of one animal. The results are qualitatively similar to the ones discussed below.

[15] Because our simulation procedure only considers initial herd sizes between 1 and 60 cattle, we face a problem in assigning values to these variables outside of that interval. We chose not to assign any values to these variables when herd size in 2003 is bigger than 60 given that we only lose 9 of 463 observations and the degree of arbitrariness in that decision would be unacceptable. The decision on what values to assign to the case when the match has no cattle is much more straightforward. For expected wealth, we assumed that Pr(herd size10 years ahead $\geq$ 30| match has no cattle, gift of 1 cattle)= Pr(herd size10 years ahead $\geq$ 30 | match has 1 cattle) = 0. For expected gains, we assumed that (expected herd size after 10 years | match has no cattle, gift of 1 cattle) = (expected herd size 10 years

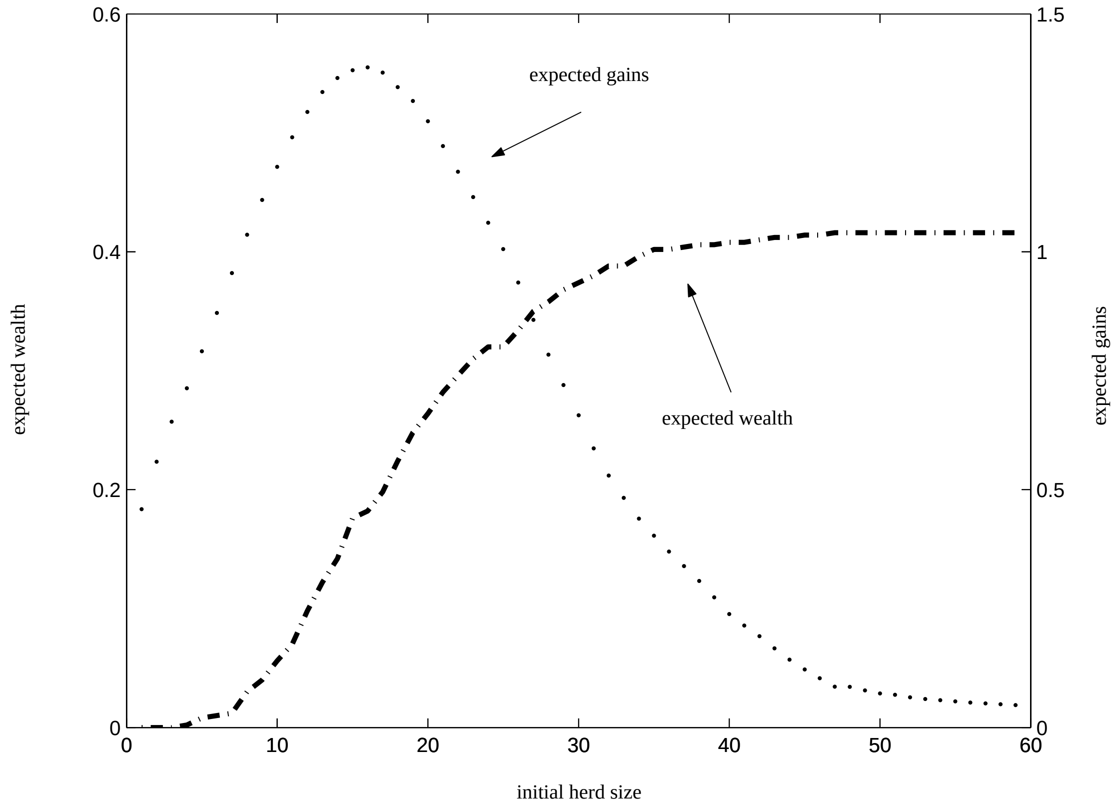


Figure 4.2: Expected consequences of a loan of 1 cattle

The elements of the X–vector – clan membership, gender, age, land holdings, and household size – are expressed not as the Euclidean distance between the pair but rather using a measure of distance that allows for ordinal differences in the relative position of the respondent and match to play a role in explaining the respondent's decision. To be more concrete, consider the case of a categorical variable such as gender. If the match and respondent share the same gender we can either control for a dummy variable "same gender" - implicitly imposing that the effect of a female–female match is the same as that of a male–male one – or we can consider the set of all possible matches (female–female, female–male, male–female

ahead | match has 1 cattle) = 1.612, and that, in case they receive no gift, 10 years ahead their herd size will remain 0.

and male–male) and incorporate a dummy variable for each specific combination. *Mutatis mutandis*, the same reasoning applies to continuous variables.[16]This approach offers an intuitively more appealing interpretation of the effects of social and economic distance than the more conventional Euclidean measure of social distance that (implicitly) imposes symmetry in the effect of these variables upon the dyad formation decision.

One alternative way of modeling the error term is to assume that,

$$\mathrm{E}(\varepsilon_{\mathrm{ih}}, \varepsilon_{\mathrm{jh}}) \neq 0 \text{ if i } \neq \text{ j} \tag{4.8}$$

that is, to incorporate the effect of matches' unobserved heterogeneity on the link formation decision. Just as we assume in equation 4.6 that (unobserved) lender's capacity do resist demands on his/her assets may drive observed credit access decisions, it may be reasonable to think that (unobserved) borrower's persistence or trustworthiness might play a role in the pattern of answers that we analyze. Both Udry and Conley (2005) and Fafchamps and Gubert (2007) correct the covariance matrices of their estimates for the possible effect of matches' unobservables, using Conley (1999) estimator. Neither study finds large differences due to this correction.[17]

We follow a different strategy for addressing the possibility reflected in equation 4.8, using a nonparametric permutation test known as Quadratic Assignment Procedure (QAP) (Hubert and Schultz, 1976, Krackhardt, 1987, 1988) to obtain correct p–values. The basic intuition behind this procedure is that the permutation of the data on the dependent variable must maintain its clustered nature. In

[16]With a different formalization, the same idea is captured in Fafchamps and Gubert (2007).

[17]Fafchamps and Gubert (2007) mention that their Monte Carlo simulations support the importance given to this issue, as corrected standard errors can be much larger than uncorrected ones.

practice, this means that the same permutation must be applied to respondents and matches. We can then estimate the above model when all correlation between dependent and independent variables is broken through resampling – that is, when the null hypothesis that all slopes equal zero is known to be true – and compare our first estimates with their empirical distribution obtained through the repetition of this exercise (in our case, 200 times), to generate a sampling distribution for the parameter estimates. Contrary to previous studies, we find that this added control for unobserved heterogeneity across individuals indeed matters to our results with respect to the formation of credit networks. For that reason, and although we'll present both uncorrected and QAP–corrected p–values, we'll focus the discussion on the last, more general results.

### 4.3.3 Estimation results

Table 4.2 presents descriptive statistics of the regressors used in the regressions we now discuss.

Table 4.2: Variable definitions and descriptive statistics

| Variable | Definition | Mean (SD) |
|---|---|---|
| $EW_j$ (Expected Wealth) | Probability that the mach will have a herd bigger than 30 cattle, 10 years after receiving a loan of one cattle, given current (2003) herd size | 6.5 (0.10) |
| $EG_j$ (Expected Gains) | Difference in match's expected herd size, 10 years after receiving a loan of one cattle, given current (2003) herd size | 1.063 (0.327) |
| $L_j$ (Loss) | Dummy variable, equal to 1 if the match lost cattle in the period between September 2000 and September 2003 | 0.21 (0.40) |
| Match has no cattle | Dummy variable, equal to 1 if the match has no cattle in September 2003 | 0.15 (0.36) |

*Continued on next page...*

*... table 4.2 (continued)*

| Variable | Definition | Mean (SD) |
|---|---|---|
| Physical distance | Absolute value of the distance between respondent and match, in kilometers | 37.07 (55.78) |
| Same clan | Dummy variable, equal to 1 if both respondent and match belong to the same clan | 0.190 (0.39) |
| Both male | Dummy variable, equal to 1 if both respondent and match are male | 0.41 (0.49) |
| Male, female | Dummy variable, equal to 1 if respondent is male and the match is female | 0.24 (0.43) |
| Female, male | Dummy variable, equal to 1 if the respondent is female and the match is male | 0.22 (0.41) |
| Older | Absolute value of the age difference between respondent and match if the respondent is older than the match, 0 otherwise | 8.48 (12.92) |
| Younger | Absolute value of the age difference between respondent and match if the respondent is younger than the match, 0 otherwise | 8.18 (12.91) |
| More land | Absolute value of the difference in land cropped be– tween the respondent and match if the respondent cultivates more land than the match, 0 otherwise | 0.39 (1.27) |
| Less land | Absolute value of the difference in land cropped be– tween the respondent and match if the respondent has less land than the match, 0 otherwise | 0.37 (1.11) |
| Bigger family | Absolute value of the difference in family size (in persons) between the respondent and the match if the respondent has a bigger family than the match, 0 otherwise | 1.59 (2.40) |
| Smaller family | Absolute value of the difference in family size (in persons) between the respondent and the match if the respondent has a smaller family than the match, 0 otherwise | 1.66 (2.50) |
| Positive correlation | Absolute value of the correlation in asset levels, be– tween the respondent and the match, if the corre– lation is positive, 0 otherwise | 0.26 (0.29) |
| Negative correlation | Absolute value of the correlation in asset levels, be– tween the respondent and the match,if the corre– lation is negative, 0 otherwise | 0.12 (0.21) |
| Number of brothers | Number of brothers of the respondent | 3.04 (2.08) |

*Continued on next page...*

*... table 4.2 (continued)*

| Variable | Definition | Mean (SD) |
|---|---|---|
| No cattle since 2000 | Dummy variable, equal to 1 if the match has no cattle since 2000 | 0.04 (0.20) |
| Poor since 2000 | Dummy variable, equal to 1 if the match manages a herd size that is smaller than 5 cattle (but strictly positive) since 2000 | 0.05 (0.21) |
| Not poor but below threshold, since 2000 | Dummy variable, equal to 1 if the match has a herd of intermediate size but below the threshold (i.e., between 5 and 14 cattle) since 2000 | 0.22 (0.41) |
| Above thresh– old, not wealthy, since 2000 | Dummy variable, equal to 1 if the match has a herd of intermediate size but above the threshold (i.e., between 15 and 39 cattle) since 2000 | 0.01 (0.09) |
| Wealthy since 2000 | Dummy variable, equal to 1 if the match manages a herd that is larger than 40 cattle since 2000 | 0.01 (0.11) |

Table 4.3 then reports the estimates of the random effects logit regression when the dependent variable is the decision to lend cattle to the match if he/she requests a loan. Before we discuss the effects of our core covariates of interest – the respondent's expected wealth and expected herd growth – let us first note a few results with respect to the X variables, defining relational characteristics between $i$ and$j$. These results reflect possible frictions and associated costs of establishing a credit relation, analogous to the effect of physical distance in driving localized insurance Murgai et al. (2002).

The propensity to lend cattle is strongly and positively influenced by belonging to the same clan, which may reflect closer affinity or, less altruistically, the interest in keeping one's "strength in numbers" when competing with individuals from other clans for the control of natural resources (especially water in this setting). Variables that measure social distance in terms of gender are clearly asymmetric. Men are more willing to lend cattle (either to women or to other men) than are

Table 4.3: Logit estimates of loan giving patterns

| Variable | Coefficient | p-value | QAP p-value |
|---|---|---|---|
| $L_j$=0 × $W_j$=0 | 2.038 | 0.071 | 0.035 |
| $L_j$=0 × $EW_j$ | 0.031 | 0.076 | 0.055 |
| $L_j$=0 × $EG_j$ | 0.313 | 0.569 | 0.275 |
| $L_j$=1 × $W_j$=0 | -2.340 | 0.129 | 0.070 |
| $L_j$=1 × $EW_j$ | -0.206 | 0.153 | 0.175 |
| $L_j$=1 × $EG_j$ | 2.006 | 0.084 | 0.055 |
| Respondent's wealth | 0.030 | 0.176 | 0.065 |
| Physical distance | 0.001 | 0.691 | 0.255 |
| Same clan | 3.506 | 0.000 | 0.000 |
| Both male | 1.172 | 0.174 | 0.050 |
| Respondent is male, match is female | 1.187 | 0.172 | 0.035 |
| Respondent is female, match is male | 0.514 | 0.514 | 0.145 |
| Respondent is older than match | 0.009 | 0.609 | 0.145 |
| Respondent is younger than match | 0.010 | 0.543 | 0.085 |
| Respondent has more land than match | -0.144 | 0.563 | 0.440 |
| Respondent has less land than match | -0.140 | 0.607 | 0.305 |
| Respondent has a bigger family than match | -0.211 | 0.048 | 0.085 |
| Respondent has a smaller family than match | -0.243 | 0.025 | 0.030 |

Note: Village–specific dummies and a constant were included in the estimation but are not reported. $W_j$=0: Match has no cattle. $L_j$=0: Match did not loose wealth in the period 2000/03. $L_j$=1: Match lost wealth in the period 2000/03. $EW_j$ : Match's expected wealth. $EG_j$ : Match's expected gains from a loan

women. Respondents are slightly, but statistically significantly, more willing to lend cattle to matches' who are older than themselves. Differences in household size decrease the probability of a loan, signaling a propesnity to establish links with those in a similar stage of the life–cycle. Physical proximity has no statistically significant effect on credit access patterns in these data, as is perhaps unsurprising among a population that has mobility at the center of its livelihood. Finally, we notice that Hoff (1997) suggestion that wealthier givers would be less interested in entering into such contracts does not seem to find support in these data. The probability of extending an informal loan is modestly increasing in respondent's wealth.

We now turn to the core hypotheses of interest: the relation between credit access and the match's wealth and shocks, holding the respondents' wealth constant. The first point to notice is that our estimates are generally imprecise: after controlling for the effect of unobserved heterogeneity across individuals, only having no cattle and having suffered no loss since 2000 is statistically significant at the conventional five percent significance level, although a few other variables are significant at the ten percent level.

Second, having suffered losses in the recent past (that is, the period 2000/03, for which we have data) seems to be important in defining the selection criteria of who is creditworthy. Expected gains are important (with a p–value of 0.055) when the borrower lost cattle, while only expected wealth matters (likewise with a p–value of 0.055) for the sub–sample of those who suffered no loss in the recent past.

The identification of the effect of a prospective borrower's wealth on the probability of being given credit requires us to take into account the combined effect of three variables – expected wealth, expected gains and a dummy that accounts for possible discontinuities due to the fact that the borrower has no wealth. This combined effect is graphed in Figure 4.3 for the "average link" (that is, one characterized by the average value of all other variables), taking into consideration the differences between those who suffered a loss and those who did not.

Credit seems to respond to losses only for those herders who, having cattle, are not "too poor", that is, those with wealth in the neighborhood of 7-10 animals, while those with wealth above 15 animals receive no loans in response to shocks. Recall that the unstable equilibrium is in the neighborhood of 12–16 animals.

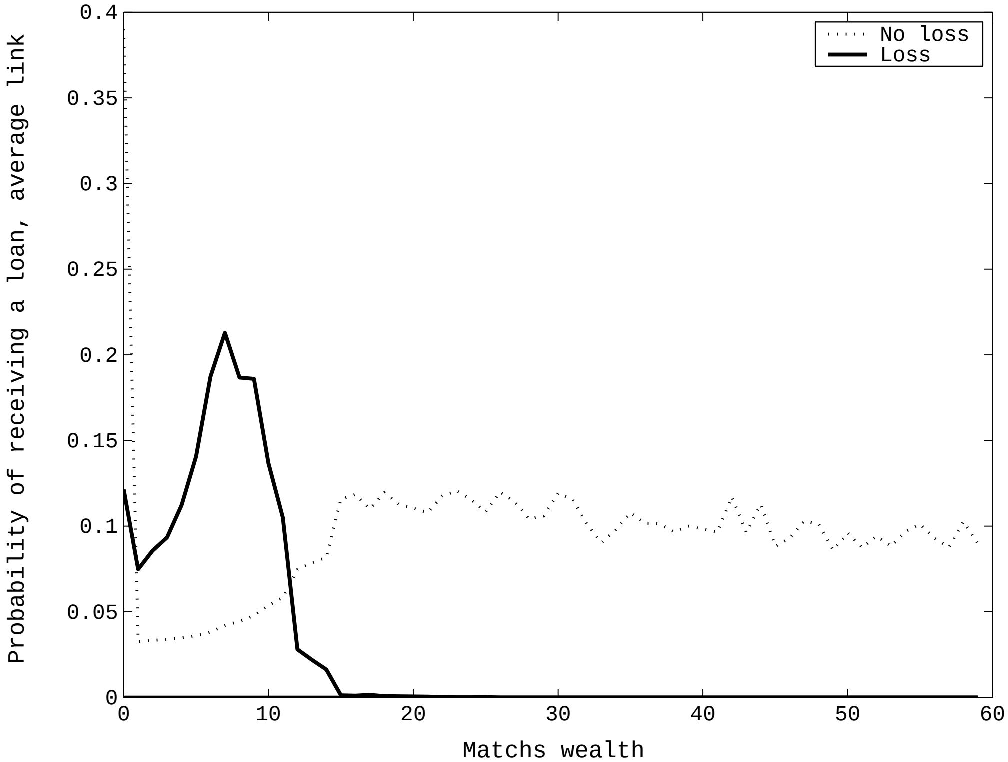


Figure 4.3: Probability of establishing a credit link: the effect of match's wealth

This suggests that asset transfers may insure the permanent component of income generation (that is, a wealth level that allows them to remain mobile herders able to grow toward the higher herd size equilibrium), rather than the transitory component. Given our earlier discussion, this appears a direct consequence of how gains from informal credit are shared, creating an incentive for lenders to extend credit to prospective borrowers in the neighborhood of the threshold at which wealth dynamics bifurcate. The social convention behind informal lending in this setting seems evolved to provide an effective safety net against collapse into the pastoral poverty trap.

Those herders who did not suffer losses in the recent past seem to be evaluated under different criteria: expected capacity to repay seems to matter most and wealthier herders are preferred borrowers. Here again a wealth level of 15 animals seems to play a role: above this value, the probability of receiving credit does not seem to change much, signaling that all herders above the accumulation threshold seem to be seen as equally desirable/viable, but those with smaller (but non-zero) herd sizes are significantly less likely to receive a loan if they have not suffered a loss.

Finally, those herders who were destitute at the time of our survey had a higher probability of receiving cattle as credit, possibly reflecting the fact that, as with gifts, there is room here for altruism, generating the sharp nonlinearities in the transfer function identified by other authors (Cox, 1987, Cox, Hansen, and Jimenez, 2004). This seems to contradict the historical record, which underscores that cutting off the destitute has traditionally been a standard response to dire poverty among East African pastoralists (Illife, 1987, Anderson and Broch-Due, 1999). In section 4.4 we revisit this point and show that the exclusionary behavior identified by anthropologists and historians may occur at another level, that of social networks from which credit networks are activated. Conditional on having established social ties, extreme destitution, in the form of stocklessness, appears to induce the highest probability of receiving a loan when the prospective borrower has not experienced a shock, suggesting some space for altruism in informal lending patterns among the Boran.

One must notice see also that the expected probability of giving credit is never above 0.5: in other words, under no conditions is the "average link" expected to correspond to a situation where the lender effectively gives credit to the borrower. Of course, the average link is an abstraction and, given the estimates from Table 4.3, a good candidate to insert some realism into the analysis of the kind of links that lead to credit being given is to look at links between individuals belonging to the same clan. The results are graphed in figure 4.4 and can be quickly summarized: credit, now restricted to the situation when both lender and borrower are from the same clan, does not seem to be given to those who didn't loose wealth in the recent past unless they are destitute (that is, with no cattle), while credit within members of the same clan may function as insurance only for those that are not "too poor" – in practice, with wealth below 11 animals.

One must note also that the expected probability of giving credit never exceeds 0.5. In other words, under no conditions is the "average link" expected to correspond to a situation in which the prospective lender extends credit to the borrower. Of course, the average link is an abstraction. Given the estimates reported in Table 4.3, a good candidate scenario for increased realism is to look at links between individuals belonging to the same clan. Those results are graphed in figure 4.4. Now restricted to prospective intra–clan lending, the basic pattern of limited credit access in the absence of recent asset losses remains. The destitute enjoy a high probability (greater than 0.8) of receiving an informal loan – reinforcing the impression of altruistic lending to the very poorest – and those with non–zero herd sizes have little prospect for receiving a loan although that probability is increasing in wealth up to the wealth threshold, beyond which it is effectively constant at 0.20-0.25. Intra–clan credit responds robustly to shocks as de facto insurance only for those who are not "too poor" – with wealth of 6–12 cattle – and not at all to shocks for those with herd sizes beyond the critical wealth threshold. The strength of the safety net mechanism remains even within clans, reinforcing the sense that informal lending is directed chiefly toward those who will gain the most from the loan because it tips them into the more desirable basin of attraction, toward the

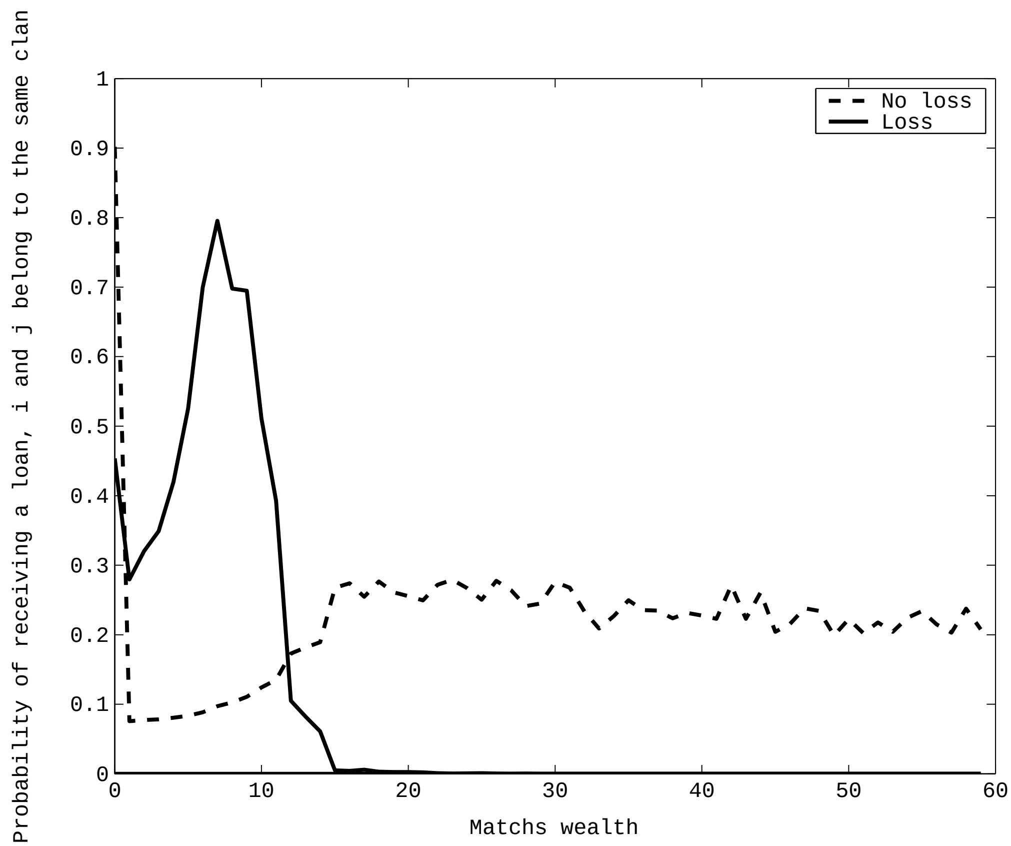


Figure 4.4: Probability of establishing a credit link: the effect of match's wealth and clan membership

high-level herd size equilibrium.

### 4.3.4 The effect of borrower's ability

In section 4.2 we suggested the possibility that wealth dynamics may be characterized by club convergence as well as by multiple equilibria. Differences in herding ability seem to affect expected herd dynamics; in particular lower ability herders do not exhibit multiple equilibria and are expected to fall into the low–level equilibrium regardless of the herd size with which they start. Transfers to low ability herders are thus ineffective insurance against the permanent effects of shocks irrespective of ex post herd size. By contrast, higher ability herders exhibit multiple stable equilibria and thus positive expected gains from a transfer when made to a borrower in the neighborhood of the unstable equilibrium that appear to drive informal credit patterns in the absence of controls for borrower herding ability.

Assuming herders rank the ability of their matches similarly to our estimates, a match's ability should therefore matter to a respondent's likelihood of extending credit to a match if this behavior is indeed heavily influenced by borrowers' expected long–run herd gains. This is effectively what we find in Table 4.4. As

Table 4.4: Logit estimates of loan giving patterns: the effect of herder's ability

| Variable | Coefficient | p-value | QAP p-value |
|---|---|---|---|
| Match is of low ability × $L_j$=0 × $W_j$=0 | 0.030 | 0.989 | 0.320 |
| Match is of low ability × $L_j$=0 × $EW_j$ | 0.033 | 0.272 | 0.150 |
| Match is of low ability × $L_j$=0 × $EG_j$ | 0.989 | 0.204 | 0.110 |
| Match is of low ability × $L_j$=1 × $W_j$=0 | -4.073 | 0.411 | 0.185 |
| Match is of low ability × $L_j$=1 × $EW_j$ | -14.180 | 1.000 | 0.045 |
| Match is of low ability × $L_j$=1 × $EG_j$ | 3.263 | 0.325 | 0.130 |
| Match is of high ability × $L_j$=0 × $W_j$=0 | 2.496 | 0.070 | 0.020 |
| Match is of high ability × $L_j$=0 × $EW_j$ | 0.026 | 0.179 | 0.140 |
| Match is of high ability × $L_j$=0 × $EG_j$ | 0.242 | 0.675 | 0.320 |
| Match is of high ability × $L_j$=1 × $W_j$=0 | -2.963 | 0.086 | 0.050 |
| Match is of high ability × $L_j$=1 × $EW_j$ | -0.224 | 0.125 | 0.185 |
| Match is of high ability × $L_j$=1 × $EG_j$ | 2.370 | 0.050 | 0.020 |
| Respondent's wealth | 0.029 | 0.157 | 0.065 |

Note: Other covariates presented in table 4.3 were used in the estimation but are not presented here.

a rule, low ability borrowers' wealth plays no statistically significant role in explaining the decision to give credit. This is true even when the borrower has no cattle. Even seemingly altruistic lending behavior seems to discriminate between those with low and higher ability, as the latter enjoy a sharply and statistically significantly higher probability of receiving a loan when they have not suffered a recent loss. When our estimates are precise enough to guide some conclusion, as in the case of the variable "low ability $\times$ loss $\times$ expected wealth", the results do not easily square with conventional models that focus on wealth as guarantee of no default, as our estimate has the "wrong" sign. Credit does not function as insurance to these wealthier herders, who are left to insure themselves. Those who are expected to suffer the greater long-run herd declines (the wealthiest low ability herders) are the least likely to receive informal loans.

On the other hand, the likelihood of granting an informal loan is sharply and statistically significantly increasing in the borrower's expected herd gains for herders of higher ability who lost wealth. They are the most credit worthy members of the community. Introducing borrower ability thus reinforces the patterns already observed: informal credit is concentrated overwhelmingly on those near the threshold who have suffered a wealth loss, thus serving as a safety net, and on high ability destitute herders who have not suffered a loss, signaling altruistic transfers.

### 4.3.5 Alternative explanations of exclusion from credit contract

Finally, we check whether our central results are robust to the inclusion of additional controls suggested by the alternative models identified at the close of the section 4.3. We already addressed the concerns of Hoff (1997) and Murgai et al. (2002) in Tables 4.3 and 4.4. In Table 4.5 we include, as additional controls, the correlation between asset levels of our respondents and their random matches in the nine survey rounds for which we have data. As with other covariates, we allow for the possibility of different effects upon the propensity to transfer cattle as a loan depending on whether this correlation is positive or negative.

Table 4.5: Logit estimates of loan giving patterns: the effect of correlation in wealth dynamics

| Variable | Coefficient | p-value | QAP p-value |
|---|---|---|---|
| Match is of low ability × $L_j$=0 × $W_j$=0 | -4.749 | 0.305 | 0.500 |
| Match is of low ability × $L_j$=0 × $EW_j$ | 0.008 | 0.854 | 0.130 |
| Match is of low ability × $L_j$=0 × $EG_j$ | 1.769 | 0.171 | 0.050 |
| Match is of low ability × $L_j$=1 × $W_j$=0 | -21.732 | 0.280 | 0.035 |
| Match is of low ability × $L_j$=1 × $EW_j$ | -32.236 | 1.000 | 0.040 |
| Match is of low ability × $L_j$=1 × $EG_j$ | 6.354 | 0.269 | 0.040 |
| Match is of high ability × $L_j$=0 × $EW_j$ | 0.028 | 0.139 | 0.045 |
| Match is of high ability × $L_j$=0 × $EG_j$ | 0.137 | 0.799 | 0.410 |
| Match is of high ability × $L_j$=1 × $W_j$=0 | -5.721 | 0.064 | 0.000 |
| Match is of high ability × $L_j$=1 × $EW_j$ | -0.324 | 0.105 | 0.100 |
| Match is of high ability × $L_j$=1 × $EG_j$ | 2.660 | 0.042 | 0.015 |
| Respondent's wealth | 0.027 | 0.177 | 0.170 |
| Negative correlation in wealth | -0.517 | 0.635 | 0.175 |
| Positive correlation in wealth | 1.466 | 0.065 | 0.245 |

Note: Other covariates presented in table 4.3 were used in the estimation but are not presented here. "Match is of high ability × $L_j$=1 × $W_j$=0" was dropped due to multicollinearity.

Although these additional controls are not statistically significant, their inclusion does change our results in one important way. It is no longer true that belonging to the low ability category leads to being excluded from these credit networks, signaling that either matches' ability is not correctly understood by our respondents or that our own classification is somewhat flawed. The likelihood of lending is now statistically significantly increasing in the expected gains from the transfer even for low ability herders who have suffered a loss. Hence, one of our core results remains: informal lending appears concentrated around the unstable wealth equilibrium in response to asset shocks, serving as a safety net against collapse into a poverty trap.

This is likewise true when we include the respondent's number of brothers and its square as a proxy for the size of the ex ante insurance network (Table 4.6).

But just as when we control for correlation in wealth between respondent and match, we now find that expected gains from a transfer post-shock appear to drive informal lending irrespective of the borrower's herding ability.

Table 4.6: Logit estimates of loan giving patterns: the effect of ex ante credit networks

| Variable | Coefficient | p-value | QAP p-value |
|---|---|---|---|
| Match is of low ability × $L_j$=0 × $W_j$=0 | -5.539 | 0.230 | 0.510 |
| Match is of low ability × $L_j$=0 × $EW_j$ | 0.001 | 0.973 | 0.435 |
| Match is of low ability × $L_j$=0 × $EG_j$ | 2.019 | 0.125 | 0.065 |
| Match is of low ability × $L_j$=1 × $W_j$=0 | -24.201 | 0.263 | 0.030 |
| Match is of low ability × $L_j$=1 × $EW_j$ | -31.701 | 1.000 | 0.040 |
| Match is of low ability × $L_j$=1 × $EG_j$ | 7.052 | 0.253 | 0.030 |
| Match is of high ability × $L_j$=0 × $W_j$=0 | 2.500 | 0.134 | 0.000 |
| Match is of high ability × $L_j$=0 × $EW_j$ | 0.028 | 0.157 | 0.470 |
| Match is of high ability × $L_j$=0 × $EG_j$ | 0.233 | 0.674 | 0.390 |
| Match is of high ability × $L_j$=1 × $W_j$=0 | -5.197 | 0.104 | 0.065 |
| Match is of high ability × $L_j$=1 × $EW_j$ | -0.313 | 0.116 | 0.325 |
| Match is of high ability × $L_j$=1 × $EG_j$ | 2.434 | 0.068 | 0.000 |
| Respondent's wealth | 0.027 | 0.159 | 0.475 |
| Number of brothers | -0.437 | 0.340 | 0.000 |
| Number of brothers squared | 0.063 | 0.256 | 0.080 |

Note: Other covariates presented in table 4.3 were used in the estimation but are not presented here.

## 4.4 Nonlinear wealth dynamics and social exclusion

The fact that the poorest members of the community are less likely to receive transfers than those near the accumulation threshold suggests a process of social exclusion. If, as Santos and Barrett (2006) claim, multiple dynamic equilibria arise because of asset shocks, then insurance against asset shocks is critical to maintaining a viable livelihood for those of medium and high herding ability. Yet if the asset poor cannot get transfers, either as gifts or as transfers, their ability to climb out of poverty is negligible. The results reported in the preceding section may even understate this effect because they are based only on credit decisions relating to the subsample of random matches with whom respondents were already acquainted. Given that social acquaintance seems to precede the establishment of a credit network, as shown in table 4.1, this section explores the possibility of wealth–dependent "social invisibility", which could reinforce the credit rationing mechanism identified in the previous section.

Table 4.7: Logit estimates of social acquaintance networks

| Variable | Coefficient | p-value | QAP p-value |
|---|---|---|---|
| Match is destitute since 2000 | -1.106 | 0.025 | 0.070 |
| Match has less than 5 cattle since 2000 | -0.145 | 0.736 | 0.391 |
| Match has between 5 and 14 cattle since 2000 | -0.127 | 0.639 | 0.379 |
| Match has between 15 and 39 cattle since 2000 | -0.581 | 0.558 | 0.485 |
| Match has more than 39 cattle since 2000 | -1.297 | 0.287 | 0.284 |
| Match lost cattle since 2000 | 0.203 | 0.466 | 0.356 |
| Respondent has more cattle than match | -0.014 | 0.009 | 0.096 |
| Respondent has less cattle than match | 0.040 | 0.001 | 0.043 |
| Distance | -0.007 | 0.323 | 0.201 |
| Same clan | 0.743 | 0.015 | 0.033 |
| Both male | 0.684 | 0.081 | 0.118 |
| Respondent is male, match is female | 0.177 | 0.671 | 0.359 |
| Respondent is female, match is male | 0.618 | 0.084 | 0.121 |
| Respondent is older than match | -0.026 | 0.013 | 0.005 |
| Respondent is younger than match | -0.000 | 0.971 | 0.515 |
| Respondent has more land than match | 0.143 | 0.215 | 0.193 |
| Respondent has less land than match | 0.482 | 0.001 | 0.013 |
| Respondent has a bigger family than match | 0.042 | 0.499 | 0.264 |
| Respondent has a smaller family than match | -0.097 | 0.088 | 0.111 |

Note: Village–specific dummies and a constant were included in the estimation but are not reported here. Being from Qorate predicts being known perfectly – the variable was dropped and 300 observations were not used.

We use the same logit estimation approach from equation 4.5 to examine patterns of social acquaintance among the individuals in our sample, now using the “know” variable from table 4.1 as the dependent variable. Because this variable is certainly the result of past processes, we incorporate the effect of past dynamics (in practice, herd size transitions between 2000 and 2003) and not the variables that we previously interpreted as a measure of future herd size or expected gains from a loan. The results are presented in table 4.7.

Being from the same clan and having less assets (cattle and land) than one’s match increases the probability of knowing the random match, while having more cattle and being older have a negative impact, a clear demonstration of the asymmetric effects of wealth and status on the structure of social networks. This effect is even clearer when we consider the effect of a match being destitute, i.e., having no cattle. Destitution is strongly associated with exclusion from social networks, as reflected in a large, negative, and statistically significant coefficient estimate. A herd size consistently at the low–level equilibrium appears associated with greater likelihood of social invisibility that, recall from Table 4.1, seems to prevent one from entering into dyadic credit relationships. Informal credit arrangements cannot function for the poorest members of a society if they are not part of the social networks from which credit networks are drawn.

The nature of the channels through which this process operates are not entirely clear, although the anthropological literature on the Boran offers some suggestions. Dahl (1979), for example, mentions that the participation in the social and political life of the Boran is hardly compatible with the daily management of the herd: wealthy herders, who usually occupy these traditional (and highly visible) offices, quite often delegate these tasks to someone else. Lybbert et al. (2004) hypothesize that multiple herd size equilibria result from the involuntary sedentarization of the destitute while those with viable herds migrate. Seasonal migration might thereby create sufficient physical separation and differences in lifestyle that the poorest become invisible to those who remain as herders. Regardless of the precise causal mechanisms by which the greater social invisibility of the poor arises, what seems clear from historical accounts is that exclusion generated by persistent poverty is not something new. For example, Illife (1987, p.42) notes that "[t]o be poor is one thing, but to be destitute is quite another, since it means the person so judged is outside the normal network of social relations and is consequently without the possibility of successful membership in ongoing groups, the members of which can help him if he requires it. The Kanuri [in the West African savannah] say that such a person is not to be trusted". Closer to our study site, a Somali proverb states that "Prolonged sickness and persistent poverty cause people to hate you" (World Bank, 2000, p.16).

We should note, however, that the evidence that we find for the importance of social invisibility in this environment is weakened once we use the QAP to obtain correct p–values for the variables in our model. In particular, persistently having no cattle is no longer significant at the 5% level (although the p–value increases only to 0.07) and the asymmetries in the effects of difference in wealth become less precisely estimated. There are two possible explanations for this. First, knowing one's match may be a less "rational" process than is choosing a loan recipient, leading to a greater role for unobserved heterogeneity for both respondent and match. Second, even if we are using all the relevant variables to eliminate the two–way unobserved heterogeneity concern, we only observe them for a relatively short period and there can be no presumption that the process from destitution to social invisibility takes effect immediately. For example, moving to a larger urban center as a consequence of utter destitution is not quickly or easily undertaken.

This raises the theoretically and empirically interesting question of describing the dynamics of these networks, a topic that unfortunately we cannot address with these data.

## 4.5 Conclusions and policy implications

This paper presented a simple conceptual model of the implications of multiple wealth equilibria for patterns of informal credit and established that data from a population among which poverty traps have been previously identified support the hypothesis that informal credit conforms to this model. Livestock loans among these herders appear to function largely as safety nets, triggered by herd losses so long as those losses leave the prospective transfer recipient not ”too poor” so that the expected gains to the borrower from the loan – and thus to the lender – are relatively high, as compared to loans to poorer or richer prospective borrowers. For the poorest, stockless herders, their destitution induces prospective partners to rationally exclude them from credit–cum–insurance networks, even though they know each other, although informal credit does flow to the destitute altruistically, especially between members of the same clan.

This effect of credit rationing that leaves out poorer (if not necessarily the poorest) members of the community is compounded by the fact that the poor are less socially visible than their somewhat wealthier neighbors. Because being known is, in our sample, a necessary condition for receiving transfers, the greater social invisibility of the destitute compounds their rational exclusion from informal financial transactions effected through social networks, leaving them vulnerable to shocks and largely without credit networks to fall back on in times of need.

The existence of multiple wealth equilibria and the focal role played by the dynamic wealth threshold that we identify in this setting have profound implications for public policies to address problems of persistent poverty and asset loss in a setting characterized by poverty traps. Because transfers can have, literally, life or death consequences in contexts such as the rangelands of southern Ethiopia, it is perhaps unwise to derive conclusions about optimal redistributive policies simply from our econometric results (Cohen-Cole, Durlauf, and Rondina, 2005). Nevertheless, our results speak to the concern that external transfers from governments, donors or international nongovernmental organizations may crowd out existing informal arrangements. Boran pastoralists seem to act in such a way that clearly marginalizes those who are trapped in dire poverty. In this context, worries about the crowding out effect of public interventions seem misplaced, as the poorer members are clearly left uninsured with distressingly high probability. In fact, our empirical results suggest that, up to some wealth level, public transfers may even lead to the crowding-in of private transfers, as a recent analysis of private transfers in the Philippines likewise suggests (Cox, Hansen, and Jimenez, 2004). This result is no surprise in a context where transfers are risk-sharing mechanisms motivated by exchange/reciprocity considerations, in which case there may be a positive correlation between the welfare of the recipient and a private transfer because better-off recipients will be better placed to reciprocate a transfer in the future.

# Appendix A
# Regression tree analysis

This Appendix describes the construction of a regression tree using **G**eneralized, **U**nbiased, **I**nteraction **D**etection and **E**stimation (GUIDE). Loh (2002) is the central reference, while Loh (2007) explains how to use the program and how to interpret the output. The program is freely downloadable from www.stat.wisc.edu/ loh/.

We start by considering four categories of variables, as a function of their type (numerical(N)/ categorical(C)) and their role in the model (fit the model(F)/ split the tree(S)/ both):

| | Fit | Split | Fit + Split |
|---|---|---|---|
| Numerical | F | S | N |
| Categorical | F | C | N |

The algorithm proceeds in three steps: 1) choice of the splitting variable at each node of the tree; 2) choice of the splitting value and finally, 3) cost–complexity pruning. Steps 1) and 2) construct two mutually exclusive subsets at each node, starting with the set of all observations and stopping when the number of observations in the subsets falls below a predetermined (chosen) value. To avoid over–fitting the data, the tree is pruned back using a cost–complexity algorithm.

The choice of the split variable proceeds as follows:
1) obtain the residuals from the regression on the N and F variables;
2) for each numerical variables used to split the sample (either S or N), divide the data into 4 groups at the sample quartiles; construct a $2 \times 4$ contingency table with the signs of the residuals (positive/ non-positive) as rows and the groups as columns; count the number of observations in each cell and compute the $\chi^2$ statistic and its p–value from the $\chi^2_3$ distribution;
3) do the same for each categorical variable used to split the sample (either C or N), taking the categories of the variable as the columns; omit those columns with zero column totals;
4) to detect interactions:
4.1) between pairs of variables, divide the space formed by them into 4 quadrants by splitting each in two at the sample median; construct a 2×4 contingency table (with residuals as rows and each quadrant as columns); compute the $\chi^2$ statistic and its p–value;
4.2) do the same for each S variable;
4.3) use the value pairs of the C variables to divide the sample space; construct a 2×(c1×c2) contingency table, where c1 and c2 are the number of unique values of each variable; compute the $\chi^2$ statistic and its p–value, omitting those columns with zero column totals;
4.4) compute the $\chi^2$ statistic and its p–value for each pair (N,C) from a contingency table with 2×(2×c1) dimensions, omitting those columns with zero column totals;

4.5) do the same for each pair (S, C);
4.6) do the same for each pair (S, N), following 4.4);
5) if the smallest p–value comes from one of the sets generated by steps 2) or 3), the associated variable is selected to split the node;
6) if the smallest p–value comes from one of the sets generated by step 4), then use the following rules to select which, from among the interaction variables, is the splitting variable:
6.1)if only one of these variables is a N–variable, choose the other one;
6.2) if neither is a N–variable, choose the one with the smallest p-value, as computed from step 3);
6.3) if both are N–variables, split the node along the sample mean of each variable and choose the variable whose split yields the smaller total SSE.

After this step, the split value for that variable has to be determined. This is done using the next algorithm:
1) define the partitions $P_1(v)$ and $P_2(v)$ as:

$P_1(v) = \{(y,X) \mid x_j \leq v\}$

$P_2(v) = \{(y, X) \mid xj > v\}$

where $x_j \in X_j$ and $X_j$ is the chosen split variable;
2) regress y on X separately for each partition and obtain the residuals of these regressions ($r_1$ and $r_2$, respectively);
3) choose v to be the value of the split variable that minimizes the sum of squared residuals:

$1/n_1 \times r_1^2 + 1/n_2 \times r_2^2$

where $n_1$ and $n_2$ are the number of observations in each partition.

Finally, once the most extensive tree is constructed, the algorithm "prunes" it to avoid over–fitting the data. This is done using cost–complexity pruning, where a penalty is put on overly complex trees: formally, the cost complexity criterion is expressed by

(A.1) $C_\alpha(T_b) = \sum_{n=1}^{b} \sum_{(x_i,y_i)\in n} (y_i\text{-}\beta_n x_i)^2 + \alpha\times b$

where $\alpha$ is the penalty parameter ($0 \leq \alpha \leq \infty$) and $T_b$ represents a tree with b nodes. The objective of the algorithm is to identify the tree that minimizes $C_\alpha$. It proceeds in two steps: the construction of the optimal tree for each value of $\alpha$ (denote it by $T^*(\alpha)$) and the choice of the optimal $\alpha$ (denote it by $\alpha^*$). Denote by $T_0$ the tree originated when splits were costless (that is, $\alpha = 0$).
1) Start with $T_0$ and increase $\alpha$.
2) Remove any terminal splits in $T_0$ whose elimination reduces the value of equation (A.1), producing a new tree. This is done by merging the observations in these terminal nodes in a new terminal node.
3) Increase $\alpha$ by a chosen increment.
4) Repeat Steps 2) and 3) until the nodes of tree have a unique element (by analogy with our previous notation, denote the resulting tree by $T_\infty$).
5) For each $T^*(\alpha)$, produce a V-fold cross validated estimate of the squared sum of residuals (SSR) in equation (A.1).
6) Choose $T^*(\alpha)$ that minimizes the SSR.

Breiman et al. (1984) show that each of the trees in the (finite) sequence between $T_0$ and $T_\infty$ is unique and it must contain $T^*(\alpha)$. The concept of V-fold cross-validation is explained in detail in Hastie, Tibshirani, and Friedman (2001, section 7.10).

# Appendix B
# Monte Carlo simulation code

This is the main structure of the Stata code used to generate the results presented in Table 3.8. Its use requires small adaptations and extensions (to get different sampling ratios, to allow for other models of network formation, etc) that are duly signaled.

```
*START CODE
drop _ all
set obs 200
set seed 12345
gen clan=uniform()
replace clan=1 if clan≤0.20
replace clan=2 if clan≤0.2333
replace clan=3 if clan≤0.30
replace clan=4 if clan≤0.40
replace clan=5 if clan≤0.7667
replace clan=6 if clan≤0.90
replace clan=7 if clan≤0.9667
replace clan=8 if clan≤1.00
set seed 12345
gensex=uniform()
replace sex=1 if sex≤0.633
replace sex=0 if sex>0.633 & sex!=1
set seed 12345
gen hhsize=invnorm(uniform())
```

```
replace hhsize=(hhsize*3.59)+7.5

replace hhsize=int(hhsize)

replace hhsize=1 if hhsize≤0

set seed 12345

genexp=invnorm(uniform())

replace exp=(exp*14.94) + 23.2

replace exp=int(exp)

replace exp=0 if exp<0

set seed 12345

gen land=invnorm(uniform())

scalar a=1.48

scalar b=1.37

replace land=ln(a)+sqrt(ln(b))*land

replace land=exp(land)

set seed 12345

gen ind=uniform()

set seed 12345

gen cat1=invnorm(uniform())

scalar a=5.444

scalar b=4.255

replace cat1=ln(a) + sqrt(ln(b))*cat1 if ind≤0.90

replace cat1=0 if ind>0.90

set seed 12345

gen cat2=invnorm(uniform())

replace cat2=67.333+37.647*cat2 if ind>0.90
```

```
replace cat2=0 if ind≤0.90
gen cattle=cat1 + cat2
replace cattle=0 if cattle<0
replace cattle=int(cattle)
drop ind cat1 cat2
gen name=[_n]
tempfile namev1
save “‘namev1’”
foreach var in clan sex hhsize exp land cattle{
    ren ‘var’ ‘var’1
}
ren name match
tempfile matchv1
save “‘matchv1’”
sort match
save, replace
use “‘namev1’”
sort name
expand 200
sort name
gen match=.
replace match=[_n] if [_n]≤200
forvalues x = 2 (1) 200{
    quietly replace match=match[_n-200] if _n>(‘x’-1)*200 & _n≤‘x’*200
}
```

```
save, replace
sort match
merge match using "`matchv1'"
drop _merge
gen sclan=(clan==clan1)
gen ssex=(sex==sex1)
foreach var in exp land cattle hhsize {
    gen m`var'=`var'-`var'1
    replace m`var'=0 if `var'<`var'1
    gen l`var'=abs(`var'-`var'1)
    replace l`var'=0 if `var'>`var'1
}
drop clan* sex* hhsize* exp* land* cattle*
save ...\village.dta", replace
* RANDOM LINKS
sort name match
set seed 123456
gen link=uniform()
replace link=0 if name==match
centile link, c(58.4375)
scalar cut=r(c_1)
replace link=(link<cut)
logit link sclan ssex mexp lexp mland lland mcattle lcattle mhhsize lhhsize
```

```
save "...\villageRL.dta", replace
* STRUCTURED LINKS
use "...\village.dta", clear
gen link=1.206*sclan + .071*ssex - .029*msize +.007*lsize +.335*mland
    - .024*lland - .071*mcattle -.001*lcattle - .001*mexp -.008*lexp
replace link=0 if name==match
replace link=(link>0)
logit link sclan ssex mexp lexp mland lland mcattle lcattle mhhsize lhhsize
save "...\villageS.dta", replace
* LIMITED LINKS
use "...\villageS.dta", clear
sort name match
by name, sort: gen slink=sum(link)
replace link=0 if slink>10
logit link sclan ssex mexp lexp land lland mcattle lcattle mhhsize lhhsize
save "...\villageSL.dta", replace
/* Simulating the MATCHES WITHIN SAMPLE approach when links are randomly formed*/
program define networkstructure,rclass
    version 8.0
    drop _all
    set obs 200
    gen u=uniform()
    centile u, c(33)
    scalar r=r(c_1)
```

```
replace u=(u≤r)
gen name=_n
sort name
tempfile name
save "`name'", replace
ren name match
tempfile match
sort match
save "`match'", replace
use "...\villageR.dta", clear
sort name
merge name using "`name'"
drop _merge
ren u sample1
sort match
merge match using "`match'"
drop _merge
ren u sample2
keep if sample1==1
keep if sample2==1
scalar bsclan=.0338991
scalar bssex=.0182271
scalar bmexp=-.0006444
scalar blexp=.0003125
scalar bmland=.0582165
```

```
    scalar blland=.0135889
    scalar bmcattle=-.0002283
    scalar blcattle=.0000456
    scalar bmsize=-.0110378
    scalar blsize=-.0065319
    scalar bcons=.3256091
    logit link sclan ssex mhhsize lhhsize mland lland mcattle lcattle mexp
      lexp
    testnl_b[sclan]-bsclan==_b[ssex]-bssex==_b[mhhsize]-bmhhsize==
      _b[lhhsize]-blhhsize==_b[mland]-bmland==_b[lland]-blland==
      _b[mcattle]-bmcattle==_b[lcattle]-blcattle==_b[mexp]-bmexp==
      _b[lexp]-blexp==_b[_cons]-bcons==0
    return scalar test=r(p)
end
set seed 23456
tempfile structure_R33RSI
simulate "networkstructure" testRRSI33=r(test), reps(100)
      saving("`structure_RRSI33'")
program drop networkstructure
gen N=_n
sort N
save, replace
/*this program has to be repeated for the remaining sampling ratios (50%, 66%,
90%) and for the remaining models of network formation*/
merge N using structure_R33RSI'
```

```
drop _merge
sort N
save, replace
merge N using `structure_R50RSI'
drop _merge
sort N
save, replace
merge N using `structure_R66RSI'
drop _merge
save, replace
foreach var in testR33RSI testR50RSI testR66RSI testR90RSI {
	count if `var'>.05 & `var'!=.
}
/* Simulating the RANDOM MATCHING approach when links are randomly formed*/
program define networkstructure, class
	version 8.0
	drop _all
	set obs 200
	gen u=uniform()
	centile u, c(33)
	scalar r=r(c_1)
	replace u=(u≤r)
	gen name=_n
	sort name
```

```
tempfile name
save “‘name’”, replace
ren name match
tempfile match
sort match
save “‘match’”, replace
use“...\villageR.dta”,clear
sort name
merge name using “‘name’”
drop _merge
ren u sample1
sort match
merge match using “‘match’”
drop _merge
ren u sample2
keep if sample1==1
keep if sample2==1
gen sample3=uniform()
sort name sample3
replace sample3=1
by name, sort: gen sum3=sum(sample3)
keep if sum3≤5
scalar bsclan=.0338991
scalar bsamesex=.0182271
scalar bmexp=-.0006444
```

```
    scalar blexp=.0003125
    scalar bmland=.0582165
    scalar blland=.0135889
    scalar bmcattle=-.0002283
    scalar blcattle=.0000456
    scalar bmsize=-.0110378
    scalar blsize=-.0065319
    scalar bcons=.3256091
    logit link sclan ssex mhhsize lhhsize mland lland mcattle lcattle mexp
      lexp
    testnl _b[sclan]-bsclan==_b[ssex]-bssex==_b[mhhsize]-bmhhsize==
      _b[lhhsize]-blhhsize==_b[mland]-bmland==_b[lland]-blland==
      _b[mcattle]-bmcattle==_b[lcattle]-blcattle==_b[mexp]-bmexp==
      _b[lexp]-blexp==_b[_cons]-bcons==0
    return scalar test=r(p)
end
set seed 23456
tempfile structure_R33RSR5
simulate "networkstructure" testR33RSR5=r(test), reps(100) saving
    ("`structure_R33RSR5'")
program drop networkstructure
/* this simulation has to be repeated for the remaining sampling ratios, different
models of network formation and number of relations to be sampled (10 and 15)*/
```

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