Extended Family and Kinship Networks: Economic Insights and Evolutionary Directions

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Abstract. What do we know about extended families and kinship networks? What gaps in our knowledge most need to be filled? How can we best organize current work and identify priorities for future research? These questions are important for several reasons: households in developing countries depend on friends and relatives for their livelihood and sometimes their survival; help exchanged within extended families and kin networks affects the distribution of economic well-being, and this private assistance and exchange can interact with public income redistribution. Yet despite rapid recent progress there remain significant deficiencies in our understanding of the economics of extended families. Researchers confront a large and sometimes bewildering array of findings. We review and assess this literature by starting with an emphasis on standard economic concerns, most notably the possible interaction between government-provided social insurance and private kinship networks. Our review of the evidence suggests the specter of complete "crowding out," whereby introduction or expansion of public transfers merely supplants private transfers, is exceedingly remote, though not impossible. However, numerous studies do suggest partial—but nonetheless substantial—crowding out, on the order of a 20-to-30-cent reduction in private transfers per dollar increase in public transfers. But the range of estimated effects is exceedingly wide, with many studies suggesting little private transfer response at all. Reconciling and explaining these disparate findings is a priority for future research. Theorizing about the economics of families should move beyond its concentration on income effects. The empirical literature indeed indicates that non-economic variables, such as demographic factors, can have a powerful association with private transfers. We suggest that economists tap into the extensive non-economic literature that takes an evolutionary approach to the family. We show that this literature provides valuable guidance for modeling demographic effects in the interactions among extended family members. The evolutionary literature has much to offer economists interested in family behavior by proposing novel interpretations of existing findings and pointing out new and fruitful directions for future research. We encourage economists to pay more attention to this approach when studying kinship networks.

Keywords: Extended family; Kinship network; Private transfers; Remittances; Inter-household transfers; Crowding out; Risk sharing; Hamilton's Rule; Cultural norms

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I. Introduction

What do we know about kinship networks and extended families in developing countries? What do we wish we knew? This chapter organizes the rapidly growing, and sometimes unwieldy, economics literature on private transfers and risk sharing between households. We start by "viewing the glass as half full," by assessing the many contributions that economic research has made in recent years to our understanding of the behavior of kin networks and extended families. We end by "viewing the glass as half empty," by pointing out how research in this sub-discipline might be improved and expanded. We note in particular the potential for evolutionary thinking to inform future economic research on family behavior.

Extended families are important just about everywhere, but especially so in poor countries, where social safety nets are incomplete or nonexistent and households must cope with an unforgiving environment of severe poverty and shocks to economic and physical well-being. Autonomy is not a likely option for a household struggling to make ends meet in the face of looming disasters such as drought, flooding, pestilence or infectious disease—especially against a backdrop of inadequate formal credit and insurance markets and a minimal welfare state. In poor, laissez-faire economies ties to community, friends and relatives, both near and far, can make the difference between surviving and perishing.

We begin the Chapter by documenting the various economic roles that kinship networks and extended family have been shown to play – but also their limitations. Two questions arise from the literature: (1) what are the reasons for the limited effectiveness of kinship networks; and (2) are the services provided by kinship replaced by public provision.

The answer to the first question takes us to review succinctly the now extensive literature on limited commitment and asymmetric information. The answer to the second takes

us back to the debate on crowding out. For over three decades economists have been intrigued by the interplay between kinship ties and public-sector efforts to alleviate poverty and mitigate risk. Public safety net interventions can dilute incentives to maintain a private, informal coping network. Economists have long been cognizant of the specter of such "crowding out," an unintended consequence of public income redistribution that could, at least in principle, render the distribution of economic well-being impervious to the most ambitious plans for fighting poverty.

While the logic of crowding out was first proposed long ago (Becker 1974) and has gone through numerous variations and refinements, pertinent evidence was comparatively lacking at first. But nowadays, thanks to advances in data collection and econometrics, lower costs of computing and burgeoning interest among empirical researchers, there exists a large and rapidly growing empirical literature on inter-household transfers and risk sharing. This corpus of work enables us to take an initial stab at assessing of the economic importance of crowding out and other issues connected to networks of extended kin.

At the same time, our summary of the literature reveals a patchwork of disparate methods and focus. While the empirical literature has grown, it has not yet matured to the point of providing a consistent picture of extended families, and much work needs to be done to reconcile conflicting findings. For instance, though we have much more evidence about crowding out than we did 15 years ago, it is sometimes diffuse and often contradictory; estimates range from "extremely important" to "negligible," and compelling explanations for these disparities are frequently lacking. The literature is ripe for consolidation and reconciliation—much like, we believe, the empirical labor supply literature in the early 1980's.

We are reassured to find that work in this vein has recently begun, and we discuss it at the end of our survey.

After our assessment of what is *in* today's literature, we turn to a discussion of what remains *missing* from it. Much of the existing literature on private transfers and risk sharing between households is concerned, one way or another, with income effects: how private transfers respond to pre-transfer incomes of households, the extent to which risk sharing networks buffer consumption from income shocks, and the like. But our reading of the empirical literature suggests that demographic variables also figure importantly in kinship networks. Yet economics provides little theoretical guidance for understanding demographics *per se.*

We contend that evolutionary biology represents a fruitful avenue for addressing this gap. In the latter part of this chapter, we explain how insights from evolutionary biology inform and complement economic research on extended families by providing a framework for understanding, *inter alia,* age patterns in inter-household transfers, differences in the behavior of fathers and mothers, and differences in the treatment of sons versus daughters. We conclude that a biologically based approach has the potential to expand the economic literature on kinship in novel and useful directions.

The role of kinship networks in informal exchange and public good provision

We begin by providing a brief overview of the evidence regarding the role that kinship and extended family play in various forms of exchange and provision of public goods. There is a large literature documenting the exchange of services and the provision of public goods between households in informal, non-market ways. In fact, this literature so large that it is impossible to do it justice in a few pages. Here we limit ourselves to a few salient examples. We first

illustrate the many roles that kinship networks play before pulling some common threads on which we focus in the rest of the Chapter.

Much of the recent economic literature on kinship has focused on risk sharing. This follows a decade in which risk sharing between households attracted a lot of attention from economists (e.g. Mace 1991, Cochrane 1991, Townsend 1994). Empirical investigation of gifts and transfers between households has brought to light their role as risk sharing mechanisms (e.g. Rosenzweig 1988, Rosenzweig and Stark 1989, Fafchamps and Lund 2003). Researchers have also noted that most transfers between households take place between close relatives (e.g. Lucas and Stark 1985, Ellsworth 1989, Lund 1996, Fafchamps and Gubert 2004). Most papers, however, reject the hypothesis of "full" risk sharing in favor of "partial" risk sharing. A close look at the numbers also reveals that, while the signs of the coefficients are consistent with risk sharing, the magnitudes themselves can be quite tiny, as in Rosenzweig (1988) for instance. Why this may be the case is discussed in Section II.

Households do not just pool risk. Labor pooling is an institution commonly found in many developing countries. It takes many different forms, such as rotating arrangements and labor gangs. One of its purposes is to provide protection against health risk. Farming operations must be done in a timely manner. If a farmer is ill and cannot complete a critical task on time, the work of a whole season may be lost. Labor pooling enables farmers to seek assistance from their neighbors. In their discussion of labor pooling groups in rural Ethiopia, Krishnan and Sciubba (2004) point out the role that extended family and kinship play in facilitating the formation of these groups.

Fostering children from another family is a very common practice in many poor countries, particularly to enable children to attend a distant school (e.g. Akresh 2004a, Akresh

2004b). Child fostering also takes place in response to shocks, such as the death of one or both parents. Evans (2005), for instance, illustrate the role that child fostering plays in caring for AIDS orphans in Africa (see also Evans and Miguel (2005)). In all studies, child fostering takes place primarily between close relatives. In their work on South African pensioners, Case and Deaton (1998) for instance document how frequent it is for children to live with their grand parents. Evans (2005) finds the same for AIDS orphans. Not all children in need enjoy the benefits of fostering, however. A small minority ends up as street children. Many others remain in the care of parents who do not have the resources or wherewithal to provide them with the nutrition and schooling they need.

The extended family and kinship networks provide many forms of insurance and protection against external events. Those who flee drought and famine or roving bandits and lawless armies seek shelter among relatives and kin whenever possible. Migrants provide shelter and assistance to freshly arrived migrants, creating tightly knit migration networks linking village of origin and place of destination (e.g. Munshi 2003, Granovetter 1995a). Funeral societies are another illustration of insurance institutions that transcend the household. Dercon, Bold, De Weerdt and Pankhurst (2004) document the importance of funeral societies in rural Ethiopia and Tanzania as a way of dealing with funeral costs. While the funeral society is in many ways a formal institution with clearly defined regular contributions, the enforcement of contractual obligations often rests on extended family and kinship ties.

Other public goods require the pooling of resources to protect productive assets, such as the cleaning of irrigation canals or the preservation of communal resources. In these cases too, informal institutions play a paramount role (e.g. Wade 1988, Baland and Platteau 1995). But the form that collaboration must take depends on the distribution of occupations and assets, not

on family and kinship ties. For instance, farmers must cure the irrigation canal they share, whether they are related to each other or not. This makes collaboration more difficult, which probably explains why irrigation maintenance and the preservation of common property resources have received more attention in the literature than forms of collaboration in which households can choose each other freely. Indeed, when they can choose with whom to collaborate, households tend to select individuals related by blood or kin, probably because they anticipate things to go more smoothly.

Networks of blood and kin also serve to relay important information, such as information about job or business opportunities. Granovetter (1995b), for instance, documents the role that networks play in matching workers and employers. Montgomery (1991) proposes a model in which employed workers help their employer identify suitable recruits. In practice, these new recruits often are relatives and kin members (Barr and Oduro 2002). Munshi (2003) and Granovetter (1995a) provide evidence of how information about business opportunities circulates in family and ethnic networks.

Sometimes cooperation goes beyond the exchange of useful information, as when individuals pool resources together to create a new business. At the heart of many businesses a partnership can be found, and many partnerships are grounded in family and kin ties. Relatives for instance may pool their efforts into a larger farm, as in the case of vertically or horizontally integrated households (Binswanger and McIntire 1987). Individuals can also join their savings by creating rotating savings and credit associations (ROCSCAs) (e.g. van den Brink and Chavas 1997, Besley, Coate and Loury 1993). These associations often transcend family relationships, as for example when market traders form a ROSCA to reconstitute their working capital. This

probably explains why ROSCAs are rather formal, with clearly defined rules and obligations (e.g. Aryeetey and Udry 1997, Anderson and Baland 2002).

It has been argued that family and kin networks play a role in markets themselves, hinting that market transactions often take place between relatives and kin. Fisman (2001b), for instance, interprets evidence that supplier credit is preferentially given to members of the same ethnic group as evidence of family ties. Fafchamps (2001) argues that this is not in general the case: because they are embedded in long term relationships, exchanges between close relatives seldom take the form of a well defined market transaction. It is, however, possible to find examples of preferential hiring and of higher wages paid to employed relatives (Barr and Oduro 2002). There is, however, also evidence that entrepreneurs are reluctant to employ relatives because they are difficult to discipline. A much more common form of family involvement in the business is as unpaid help or partners. This ensures that profits are shared and is consistent with the long-term risk sharing relationship that typically binds extended family members.

Fafchamps and Lund (2003) demonstrate that risk is shared via gifts and transfers and via informal loans. They show that risk sharing takes place within networks that are made primarily of relatives and kin members. They also point out that while close relatives provide gifts, more distant relatives make informal loans. These loans are hybrid debt contracts whereby money is lent to those at zero interest in exchange for the promise of future repayment. As Udry (1994), Fafchamps and Gubert (2004) further show that repayment of such loans is contingent on shocks affecting both parties. They further show that contingent repayment takes place by letting borrowers in difficulty delay repayment and pay off part of the debt in labor.

As we have illustrated, family relations can be used for good things. They can also be used for bad. Fisman (2001a), for instance, provides empirical evidence that Indonesian

businesses headed by relatives of the Suharto family benefited from preferential treatment. Family and kin ties can be used for collusion and price fixing, or to cement efforts to exclude outsiders from jobs and market opportunities—perhaps the most despicable illustration of which is the Ku Klux Klan. Family ties can be used to attract and divert development aid, as discussed for instance by Platteau and Gaspart (2003). Ensminger (2004), for instance, provides a chilling account of how development aid directed at poor Kenyan herders was diverted by a family ring. Family ties can also be harnessed to ensure collaboration and enforce a law of silence among criminals and terrorists. Gambetta (1993) illustrates this in the case of the mafia. Others have discussed it in the case of terrorism (Krueger and Maleckova 2003).

What this brief overview of the literature shows is that family and kinship often fulfill part of the role that economists normally attribute to markets, namely the exchange of goods and services. The difference is that exchange does not rely on legal contracts, in contrast to market transactions.[1] Kinship networks can also help organize the provision of public goods, a role that normally falls upon the government. But they do so without the power to tax or mobilize resources. Rather the provision of public goods is organized as a form of exchange of favors between individuals and households.

In the absence of formal contracts, exchange typically takes the form of a sequence of unilateral transfers. There may be an implicit understanding that the exchange of favors is embedded in a long term relationship between individuals. But what cement this relationship is not entirely clear: Is it quid pro quo? Is it altruism, and if yes, where does altruism come from? The answer to these questions is important because it determines what we can reasonably expect

[1] Within households, patrimonial issues are often regulated by law – e.g., inheritance, child support, alimony. But relations between households typically fall outside the purview of patrimonial law.

the limitations to kinship exchange to be, and how we should expect it to interact with markets and government provision of public goods. To these issues we now turn.

II. The Logic of Private Inter-household Transfers

What is the economic logic that governs private transfers of money and other forms of assistance between households? What relationships in the data would we expect if donors were motivated by unvarnished altruism? How about if they gave in expectation of some *quid pro quo* or in response to pressure from potential recipients? Are the decisions of donor households best envisioned as unilaterally determined or as part of a bargaining process?

II-A. Why theory is important and what makes for good theory

Examining the logic of inter-household transfers and kinship ties among extended family members is important for several reasons. First, we seek *parsimony:* without some logic to narrow down the list of conceivable hypotheses, empirical investigations of inter-household transfers could veer toward disorganization and vagueness. To say without further elaboration that private transfers are governed by, say, "human nature" or "norms," for example, opens to door to haphazard, torturous empirical inquiry, exacerbated by the availability of ever-more-complex household surveys containing hundreds if not thousands of questions. Empirical work unmoored by parsimonious theory risks falling prey to a "curse of dimensionality," whereby partial correlations are ground out in conceivably limitless fashion. Such insidious combinatorics create fertile ground for any number of Type I errors.

Second, we seek the *counter-intuitive:* ideally, the logic of private transfer behavior should not just narrow the field of empirical relationships deemed interesting, it should illuminate non-obvious behavioral pathways. (Why bother theorizing if it just produces answers

anyone could have guessed *ex ante?)* Not all theories fare equally well on this front. For instance, a theory might posit that feelings of affection and closeness lead to transfers and assistance among family members. An empirical "test" might constitute correlating self-reported subjective feelings of closeness and actual assistance given. It would be surprising if such correlations did not turn out positive, yet such putative "theory" does little to impel us to think about family behavior in new and different ways.

Third, we seek the *falsifiable:* we want our theories to be bold enough that they dare empirical researchers to shoot them down. Non-testable assertions are devoid of predictive power.

Economic theories of inter-household transfers and kinship have done well in some respects but not in others. The successes have mostly to do with explaining the income effects of private inter-household transfers. The prominent approaches provide succinct, falsifiable and sometimes even surprisingly provocative hypotheses about the interplay between income endowments and private transfers. The remaining deficiencies have mostly to do with how demographic influences are conceptualized in the economics of family behavior. We suggest how these deficiencies might be remedied later. First, we point out where economic theory has succeeded to date.

We now discuss three major categories of explanations for the existence of exchange along kinship or extended family networks: altruism; quid pro quo; and bargaining.

II-B. The logic of family behavior begins with Becker's model of altruism

Without question, Becker's (1974) model of altruistic transfers provides the central conceptual benchmark for analyzing the behavior of extended families, and not just because it marks the beginning of modern economic analyses of the family. Becker's simple framework contains a

prediction of manifest significance for both the understanding of family behavior and for income redistribution policy—namely, the possibility that public transfers of income, instead of shuffling resources from rich to poor, might merely supplant private transfers, leaving the distribution of economic well being unchanged.

The argument is simple. Imagine two people, an altruistic donor, *d,* and a recipient, *r,* with endowed with incomes I_d and I_r. "Altruistic" here means utility interdependence; the donor's utility, *U,* depends on her own consumption, c_p, and the recipient's utility, *V,* which in turn depends on recipient consumption, c_k:

$$U = U(c_p, V(c_k)). \qquad (1)$$

The donor implicitly decides individual consumption levels by adding a private transfer, *T,* to the recipient's endowment of income I_r, in order to achieve a consumption pair $\{c_p, c_k\}$ that is most desirable from the donor's perspective. Joint consumption possibilities are determined by aggregate income, $I_d + I_r$; the donor's preferences in (1) pin down the optimal transfer T^*.

Now imagine a forced income transfer (a tax or subsidy, say) of $t < T^*$ from donor to recipient. Joint consumption possibilities remain unchanged, as do donor preferences. Hence each person's optimal consumption is likewise unchanged. What *does* change, then, is the private transfer, which must fall enough to exactly offset the transfer t.

The thought experiment of the forced transfer has become known as the "transfer derivative" (see, e.g., Altonji, Hayashi and Kotlikoff (1997)), expressed as $\partial T/\partial I_r - \partial T/\partial I_d = \partial T/\partial t$. Assuming pre-redistribution private transfers match or exceed t, the Beckerian transfer derivative is –1; public transfers completely "crowd out" private ones.

Adding administrative costs to public income redistribution generates a perverse outcome: by shrinking joint consumption possibilities, it hurts those it is presumably trying to help!

It is no exaggeration that the specter of crowding out served as rocket fuel for the burgeoning literature on private transfer behavior. Not that it is the only reason to be interested in private transfers, which have been implicated in, *inter alia,* capital formation (Kotlikoff and Summers (1981)); human capital investment, inequality and intergenerational mobility (Becker and Tomes (1979)); insurance against income risk (e.g., Rosenzweig (1988)); migration (e.g., Lucas and Stark (1985)), and the alleviation of capital market imperfections (Ishikawa (1974), Cox (1990)). Nonetheless, crowding out is still routinely cited as a leading impetus for investigations of private transfers, and the measurement of transfer derivatives continues to figure prominently in empirical work.

Despite the simplicity of the model, some misconceptions about the logic of crowding out and altruism arise repeatedly in the applied literature. The most common one is this:

- *Misconception*—Testing for evidence consistent with altruistic preferences entails checking that the *sign* of the estimated value $\P T/\P I_r$ is negative.

Wrong: the *magnitude* of $\P T/\P I_r$ matters too. For the sake of the argument, imagine a regression equation—free from any specification problems—that produces a precisely estimated value of –0.02 for $\P T/\P I_r$. For the value of the transfer derivative $\P T/\P I_r - \P T/\P I_d$ to be consistent with altruistic preferences (i.e., equal –1) would require an implausibly large estimated value for $\P T/\P I_d$.[2] To be consistent with altruistic preferences, the empirical value of $\P T/\P I_r$ must generally be not just negative, but *negative and large in absolute value.* For instance, an altruist with Cobb-Douglas preferences who places equal weight on her own

[2] For more extensive discussions of tests for intergenerational altruism, see Cox and Rank (1992) and Altonji, Hayashi and Kotlikoff (1997).

consumption and that of the recipient would respond to a shortfall in recipient income by raising her transfers 50 cents per dollar shortfall—a far cry from the meager 2-cent response above.[3]

Moreover, while large transfer derivatives are necessary for the presence of altruistic transfer motives, they are not sufficient:

- *Caveat*—Finding large transfer derivatives does not necessarily imply altruistic preferences.

For instance, it is possible, at least in principal, for two completely selfish people to enter into a mutually beneficial co-insurance arrangement. They might decide, for example, to pool their incomes, setting each person's consumption equal to, say, half of their combined income. Such an arrangement can yield transfer derivatives that are identical to those implied by altruism. The reason has to do with the logic of shared budget constraints: as in the altruism case, any redistribution that keeps joint income constant must prompt equal and offsetting adjustments in private transfers in order to maintain the agreed upon allocation rule for consumption.

But tweak the rather implausible scenario above with just a bit of realism—say, the addition of moral hazard—and one can get vastly smaller transfer derivatives, as we will see in more detail in our discussion of non-altruistic transfer motives. The intuition is simple: like a market insurance company, I am concerned that if I protect a huge fraction of your income shortfalls, you will take less care to guard against preventable trouble. I act on this concern by requiring you to bear part of the consequence of any shortfall; hence, transfer derivatives would be smaller with moral hazard.[4]

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[3] Can the 2-cent response ever be consistent with altruism? Yes, as we will see below, but for this to happen requires stepping out of a single-period framework.

[4] A bit of logic associated with the altruism model that is rarely discussed, but potentially important for empirical work is this:

- *An overlooked attribute of the altruism model*—altruism generates a *linear* relationship between private transfers and income.

A further observation about the logic of altruism and transfer derivatives with possible consequences for empirical work is this:

- *Life-cycle considerations can matter*—the pronounced transfer derivatives predicted by the altruism model could well be a good deal *weaker* once life cycle considerations are taken into account.

Here is a simple illustration: suppose that donor and recipient live for 50 periods. Suppose also—and this is crucial—that each has access to perfect capital markets. For simplicity, abstract away from subjective rates of time preference or interest rates; each is zero, and desired consumption profiles are flat. Imagine a forced transfer that occurs in the first period only: the donor is taxed $100 to finance a one-shot subsidy for the recipient. The logic of crowding out still applies; the donor will reduce his transfers to the recipient by the same amount. Only now the *timing* of this reduction is no longer pinned down. The donor could reduce his private transfers immediately and all at once, but he could also spread out the reduction in $2 installments over 50 periods. If we were to observe only the first period, it would appear that transfer derivatives were rather tepid; when in fact over the life cycle they still attain the full value of –1 predicted by altruism.

Relax the perfect capital markets assumption and it is possible to restore the full value of the transfer derivative in the first period. Suppose that the recipient is credit constrained in the first period, and that the altruistic donor is currently transferring $150 to alleviate this constraint. The same $100 forced redistribution would now prompt an immediate $100 reduction in private

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The transfer derivative emanates from movement along a linear family budget constraint: taxing the donor and giving the proceeds to the recipient and the corresponding adjustment in private transfers are each movements along a linear constraint. All of the action in these comparative statics emanates from the budget constraint; the preferences themselves do not matter except to insure an interior solution for transfers.

Why is this linearity relevant for empirical work? Because a empirical test of altruism that regresses (say) the log of transfer receipts on the log of donor and recipient incomes may be getting the specification wrong from the very start. Altruism imposes a linear structure that comes straight out of the logic of the model. Whatever its other putative merits, a log-log specification is logically inconsistent with shared budget constraints.

transfers. Thus, life-cycle considerations and borrowing constraints can figure importantly into the logic of transfer derivatives in the altruism model.[5]

A final observation about altruism and transfer derivatives pertains to whether the donor values the act of giving *per se:*

- *The "purity" of altruism matters*—If an altruistic donor cares not just about the recipient but also about how much he or she gives, transfer derivatives will be weaker and crowding out less than complete.

In Becker's model altruism is "pure"; as long as the recipient is happy, the donor is happy, regardless of *how* the recipient's consumption is financed: be it a result of the donor's own largesse or someone else's. In contrast, if for some reason the donor also cares separately about his or her own giving—the so-called the "warm glow" or "impurely altruistic" model of Andreoni (1989)—then in the donor's eyes private and public transfers are no longer perfect substitutes. Returning to our example of income redistribution and crowding out, the impurely altruistic donor would respond by cutting private transfers T by less than the forced transfer t. The donor's reluctance to sacrifice "warm glow" generates this less than dollar-for-dollar response.

We have assigned a central role to transfer derivatives and crowding out in our discussion of the altruism hypothesis. It is the possibility of crowding out that, in our view, makes altruism the model to consider first and foremost, and the hub around which the rest of the theoretical literature on the extended family revolves. Because most of that other literature, with its emphasis on alternative motives for private inter-household transfers, refutes the prediction of crowding out, we think it makes sense at first pass to divide the logic of familial transfers into altruistic and non-altruistic approaches.

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[5] For a detailed discussion, see Cox (1990). One obviously important empirical issue is the extent to which poor households in developing countries face capital market imperfections. See Conning and Udry (2007) for a recent review of the myriad imperfections that beset rural credit markets in developing countries.

II-C. Quid pro quo

Despite its pre-eminence as a conceptual benchmark for family behavior, it is easy to imagine motivations for inter-household transfers that do not, at the margin, operate according to the altruistic framework pioneered by Becker. For instance:

- Private transfers might be given in *exchange* for a *quid pro quo* provided by the recipient: a migrant remits to his sister to compensate her for taking care of his property while he is gone; a parent lends money to his young adult child in exchange for old-age support later in life; a landowner conditions a bequest on the appropriate behavior of children.

- Private transfers might be part of an *informal insurance contract* among self-interested people.

A primary reason to care about these and other non-altruistic motives for private transfers is that they likely entail transfer derivatives that differ markedly from those implied by Beckerian altruism.

Exchange

For instance, it is unlikely that public transfers would crowd out private transfers if the latter were not altruistically motivated but instead part of a two-way *exchange* (Bernheim, Shleifer and Summers (1985), Cox (1987)).[6] Suppose that the donor uses private transfers, T, to compensate the recipient for the latter's provision of services, s. These "services" can be just about anything with less-than-perfect market substitutes, such as hours of care a wife provides to her mother-in-law. Suppose that providing services is costly and requires compensation. One can think of an implicit price that translates services into financial compensation:[7]

[6] While altruistic feelings might be intermingled as well—as in, for example, Lucas and Stark's (1985) eclectic approach—let's focus strictly upon exchange for the moment.

$$T = ps. \qquad (2)$$

Exchange-related transfer derivatives can differ dramatically from altruistic transfer derivatives. For instance, a rise in recipient income, I_r, would reduce the supply of services, raising *p* and reducing *s*. To a first approximation, *T* would rise or fall with I_r depending on whether the donor's demand for services were price inelastic or not. This result is obviously quite different from that of Beckerian altruism, where $\P T/\P I_r$ is unambiguously negative and plausibly large.

Mutual Insurance

The value of $\P T/\P I_r$ can likewise be markedly weaker than the "Beckerian benchmark" if private transfers are part of a self-interested system of mutual insurance (Kotlikoff and Spivak (1981), Kimball (1988), Coate and Ravallion (1993)). To illustrate, consider an example from Coate and Ravallion (1993): two self-interested parties play a non-cooperative 'insurance game' over an infinite horizon. What should be the rule for how transfers respond to income shocks, seeing how such a game can only be sustained if players do not have an incentive to defect? Coate and Ravallion show that the solution to this "implementability constraint" places a floor on transfers from the more fortunate to the less fortunate party. The floor serves to limit the more fortunate party's liability in order to prevent him from ducking an especially onerous transfer obligation through defection. Once this floor is reached, $\P T/\P I_r = 0$. This simple example can be extended to many forms of informal exchange based on quid pro quo. To this we now turn.

A theory of informal agreements with limited commitment

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[7] Implicit because few families would likely be so mercenary as to even use the "p"-word.

The theory of informal exchange with limited commitment start from the observation the enforcement of contracts by courts is not always feasible, however. Courts may be absent or unreliable, or the arrangement may be illegal or simply unprotected by law. In mutual insurance arrangements, writing a complete contract allowing for all contingencies may be too time consuming or simply impossible. Many transactions are too small to justify court action, or the parties too poor to recover anything in case of victory in court. This is particularly true in developing countries where many firms and market transactions are small and many people are too poor to be sued. In all these circumstances legal enforcement of contracts is problematic even though gains from exchange and public good provision may be relatively large. Informal enforcement mechanisms become necessary to enforce contracts, ensure contribution to public goods, and coordinate individual actions.

The literature has identified a variety of enforcement mechanisms that do not rely directly on legal institutions. Detailed discussions are provided in (e.g. Platteau 1994a, Platteau 1994b, Greif 1993, Fafchamps 1996). Economists have paid most attention to mechanisms that rely on rational self-interest. Borrowing from Evans-Pritchard's (1940) observation that it is scarcity not prosperity that makes the Nuer [in Southern Sudan] generous, Posner (1980) pointed out that informal arrangements can be built upon quid pro quo: I help you today because I expect you to help me tomorrow. Behavioral evidence supports the quid pro quo idea: individuals in experimental situations conditionally cooperate even in finitely repeated games. This point was made most forcefully by Axelrod (1984), who described tit-for-tat behavior in such experiments as 'brave reciprocity'. Axelrod's interpretation is that, when faced with somebody new, people often give them the benefit of the doubt and start by playing cooperatively. They continue playing cooperatively as long as the other person does. But if they

are cheated, they retaliate. The emergence of this human trait can be given an evolutionary interpretation, arguing that brave reciprocity makes it possible for human societies to achieve cooperation in a rapid and decentralized manner (see the literature on evolutionary games).

These insights were subsequently formalized with the help of repeated game theory to explain how contracts can be enforced in the absence of legal recourse. Early applications of this principle can be found in the literature on sovereign debt (e.g. Eaton and Gersovitz 1981, Eaton, Gersovitz and Stiglitz 1986, Kletzer 1984, Grossman and Van Huyck 1987). The successful application of repeated game theory to risk sharing by Kimball (1988), Fafchamps (1992) and Coate and Ravallion (1993) has been able to explain many empirical puzzles, notably the failure of informal risk sharing during times of great stress, the emphasis on quasi-credit rather than gifts, and asymmetric risk pooling between rich and poor – often referred to as patronage. Further extensions by Ligon, Thomas and Worrall (2001), Foster and Rosenzweig (2001) and Fafchamps (1999) have bridged the gap between gift exchange and quasi-credit of the kind described by Platteau and Abraham (1987), Udry (1994), and Fafchamps and Gubert (2002).

In repeated prisoner's dilemma, the threat of exclusion is the cornerstone of the enforcement strategy: breach of contract is deterred by threatening exclusion from future exchange. The cost of exclusion rises if an informal arrangement is embedded within a long-term multifaceted relationship: breaching an informal arrangement not only leads to the loss of further exchange within the arrangement, but possibly leads to the loss of other benefits associated with this relationship, such as socialization, participation in religious and social rituals, access to potential mates. This point was for instance made by Basu (1986) and many anthropologists. Blood relations are long lasting and generate multifaceted relations between

individuals, from physical exchange to moral support and camaraderie. Consequently, they provide the perfect environment for enforcing informal arrangements.

Repeated game theory has also found multiple uses in explaining market institutions (e.g. Greif 1993, Fafchamps 2004). What this body of work has brought to light is the importance of information sharing for informal enforcement. Such contract enforcement processes are typically called reputation mechanism or reputational contracts. Drawing inspiration from the way credit reference agencies operate, Kandori (1992) illustrates how sharing simple information about past behavior – e.g., a credit report – can be used to deter cheating in a repeated game setting. This point has been further expanded on by Taylor (2000) and Raub and Weesie (1990) to information sharing within networks. Market efficiency in general depends on the type and extent to which accurate information is shared, and on the inference economic agents draw from past action, a point made by Fafchamps (2002).

It follows that information-sharing networks play an important role in market efficiency, even when they do not directly enforce contracts, because they circulate information that is relevant to reputational mechanisms. Fafchamps (2000) and Fafchamps (2003), for instance, provide evidence that networks facilitate market exchange. Empirical evidence on the role of networks in enforcing contracts is provided, for instance, by Fafchamps and Minten (1999) and Fafchamps and Minten (2002). We have seen that family and kinship often circulate market relevant information, such as information about jobs, business opportunities, prices, goods for sale, house rentals, the quality of products and services, etc. So doing, they may be instrumental to market exchange. This point has been emphasized, for instance, by Granovetter (1985) who argues that all market transactions are embedded in a social context.

Emotions

Repeated game theory is not the only possible enforcement mechanism in informal arrangements. Emotions can also be enlisted to help enforce contracts, a point that has often been overlooked by economists and on which we focus in the remainder of this chapter.

The first emotion that is instrumental in enforcing contracts is guilt, that is, the capacity for an individual to feel bad for failing to fulfill a promise. Guilt has been studied by psychologists who have demonstrated that it critically depends on upbringing. Individuals who have been repeatedly abused during childhood tend to have a guilt deficit, psychopaths representing the extreme case. As a result, the capacity to feel guilty or not tends to be inherited across generations, at least in the statistical sense, because abused parents tend to abuse their own children. It is also likely that guilt is shaped by identity and religion and, as we will discuss more in detail later in this chapter, by family ties.

Another important emotion that can be harnessed to enforce informal arrangements is shame. Unlike guilt, shame is triggered by public exposure and disapproval and thus requires the sharing of information about one's actions. As Barr (2002) has illustrated, the capacity to resent shame varies from person to person. It may also vary across cultures. Identification with a group plays an important role in shaming. Individuals who choose to exclude themselves from the rest of the community often feel little or no shame transgressing community rules—or may even derive pride from it (Blume 2002).

Other emotions also play an enforcement role. In many circumstances, it is not rational to retaliate after having been cheated. This means that the threat of retaliation is not subgame perfect and hence not credible. In practice, human beings often become angry and irrational as a result of being cheated. Out of a sense of outrage, they often lash out at the culprit in ways that are self-damaging. Or they decide to sue simply to make a point, to be righted, in spite of the

fact that suing costs them money. Anger brings an element of irrationality into the situation that makes the threat of retaliation credible or, at least, possible. In his book Passions within Reason, Bob Frank makes arguments based on evolutionary games that traits like hard-wired vindictiveness can survive precisely because they allow for credible enforcement.

Altruism is another strong emotion—or combination of emotions—that can be harnessed for the enforcement of informal arrangements (e.g. Cox 1987, Cox, Hansen and Jimenez 2004, Ravallion and Dearden 1988). Altruism provides an emotional reward for doing the right thing, for helping others. As pointed out by Durlauf and Fafchamps (2005), a bit of altruism is often sufficient to eliminate free riding in prisoner's dilemma situations. Voluntary contribution to public goods is thus easier to achieve if parties are altruistic towards each other.

Identification

We have already discussed how family and kinship ties can be harnessed to circulate information important to reputation mechanisms and to increase the cost of exclusion. We now discuss briefly the relationship between emotions, family and kinship. Altruism has been found to be stronger among genetically related individuals. This may explain why family and kin ties facilitate the enforcement of informal arrangements. Shared genes thus raise the incentive power of altruism. Identification with the family or kinship group also facilitates guilt and shame. Given this, it is not surprising to find extended family and kinship networks to play a fundamental role in most non-market exchange—and in some forms of market exchange as well.

Identification with a group can also be created artificially by providing bonding experiences such as initiation ceremonies and other kinship activities. We suspect that bonding is strongest if it is accomplished at a young age, probably around puberty and in teenage years.

This tends to bond people of the same age together. Once the kin group has been socially engineered, it can serve many of the same functions as extended family.

Other social phenomena, such as religious sects and brotherhoods can also be used to generate strong bonds and engineer a family feel. Churches often seek to tap into the emotions triggered by family relationships by using titles such as "father," "brother," and "sister." The use of such titles demonstrates a desire to trigger the same emotional attachment as ideally found within an extended family.

II-D. Bargaining and other models of collective action

Useful insights on exchange within family networks have also been gained from the literature on collective bargaining. Indeed:

- Private transfers might be determined by a *bargaining* process. Even if they are partly altruistic, the logic of bargaining conceivably takes us far afield from the Becker framework and its attendant crowding out.
- Private transfers might be the result of *misanthropy* rather than altruism; perhaps a powerful relative extorts money from a less powerful one.

Nash Bargaining

A key assumption of Becker's altruism model is that the donor does the maximizing while the recipient passively reacts. One alternative to this "donor dominates" framework is cooperative bargaining between donor and recipient, and pioneered in the models of Manser and Brown (1980) McElroy and Horney (1981). Though these models have been used mainly to analyze *intra*-household allocation, they can just as well be applied to *inter*-household transfers as well. The key aspect of bargaining, as it impinges on crowding out, is this:

- With bargaining, transfer derivatives are no longer minus one; crowding out is not complete.

Recall that in Becker's model the donor dominates the decision making. With cooperative bargaining, both donor and recipient arrive at the transfer decision jointly, usually according to the model proposed by Nash (1950, 1953).

The easiest way to see how bargaining affects transfer derivatives is to consider a variant of a very simple model by Kotlikoff, Razin and Rosenthal (1990), in which private transfers are the outcome of Nash bargaining between donor and recipient. Formally, the optimal transfer, *T,* is the value that maximizes

$$N = [U(c_d, V(c_r) - U_0] \times [V(c_r) - V_o], \qquad (3)$$

where $U_0 = U(I_d, V(I_r))$ and $V_0 = V(I_r)$ are the respective "threat point" utilities of donor and recipient.[8] The solution to (3) has the following comparative statics properties: $\partial c_r/\partial I_r - \partial c_r/\partial I_d > 0$; $\partial c_d/\partial I_r - \partial c_r/\partial I_r < 0$. In this model, even though transfers are motivated by altruism, a forced redistribution from donor to recipient does not leave individual consumption unchanged, as in Becker's model. The reason has to do with Nash bargaining; the redistribution strengthens the recipient's bargaining position relative to the donor, thus raising the relative consumption of the recipient. It follows that the transfer derivative $\partial T/\partial I_r - \partial T/\partial I_d$ is less than unity in absolute value: Nash bargaining renders crowding out less than complete.

[8] We think that the simplicity of Kotlikoff, Razin and Rosenthal's application of Nash bargaining makes it the easiest way to illustrate how bargaining affects transfer derivatives. Pedagogy aside, however, one might question its relevance for actual family behavior. The authors argue that their approach accounts for the recipient's option to refuse any transfer offered by the donor. Fair enough, and we suppose that recent evidence from ultimatum games—where subjects opt for nothing rather than accept an unfair division of money (see, e.g., Fehr and Gachter (2000)) might be taken as supporting evidence. But it is one thing to turn down a 15 percent share of $100; it is quite another to walk away from a parcel of the family farm. Part of the problem is endemic to Nash bargaining; "threat points" notwithstanding, no threat is ever carried out in equilibrium. For an excellent, intuitive introduction to Nash bargaining and its drawbacks, see Kennan (1986); see also Chiappori (1988) for a critical perspective on Nash bargaining.

An alternative to Nash bargaining is the "separate spheres" bargaining model proposed by Lundberg and Pollak (1993). Their model, which like Nash bargaining models was designed to analyze intra-household allocation, can, like their Nash-bargaining counterparts, be applied to inter-household transfers. The key innovation of Lundberg and Pollak is to imagine that the alternative to cooperation is not complete estrangement but a non-cooperative equilibrium in which individuals revert to traditional roles that entail less-than-ideal contribution to the family. The Lundberg-Pollak approach re-defines the "threat point" utility that accrues to individuals if the cooperation falls apart. Applied to the generic "donor-recipient" framework above, the separate spheres model states that the recipient's threat-point need not be $V_0 = V(I_r)$ (which implies severed relations between donor and recipient) but the utility that accrues from a non-cooperative, perhaps dysfunctional, relationship with the donor. Despite the different characterization of threat points, the bottom line with respect to crowding out is unchanged: bargaining renders crowding out less than complete.

The Collective Model

Crowding out is likewise complete within the more general, "collective" model of household behavior proposed by Chiappori (1988, 1992). Though the model is primarily intended to describe within-household behavior, its logic can be applied to between-household transfers. Chiappori's model boils down to a consumption "sharing rule" that depends upon, *inter alia,* individual endowment incomes. The workings of the rule itself and the variables that influence it are left unspecified; all that is assumed is that the equilibrium allocations be efficient.

In the context of our simple example, we can specify the sharing rule $m = m(I_r, I_d)$, where m denotes the fraction of total household expenditures allocated to the recipient. Such a rule implies that, in general, $\partial c_r/\partial I_r - \partial c_r/\partial I_d \neq 0$ and $\partial c_d/\partial I_r - \partial c_r/\partial I_r \neq 0$. While there are variants

of the collective model that imply transfer derivatives consistent with income pooling—and therefore outcomes that are observationally equivalent to the crowding out implied by Becker's model—these transfer derivatives are not a *necessary* implication of the collective model (Browning, Chiappori and Lechene, 2004).

Mixed motives

Obviously, donors behavior can be governed by more than a single motive, a point forcefully underscored, for example, by Lucas and Stark (1985) who propose an eclectic model of "tempered altruism," or "enlightened self-interest," which "…views remittances as part of, or one clause in, a self-enforcing contractual arrangement between migrant and family". The underlying idea is that for the household as a whole it may be a Pareto-superior strategy to have members migrate elsewhere, either as a means of risk sharing or as an investment in access to higher earnings streams. Remittances may then be seen as a device for redistributing gains, with relative shares determined in an implicit arrangement struck between the migrant and the remaining family. The migrant adheres to the contractual arrangement so long as it is in his or her interest to do so. This interest may be either altruistic or self-seeking, such as concern for inheritance or for the right to return home in dignity." [p. 902]

To return to our recurring theme: How might mixed motives affect transfer derivatives? The short answer is "in myriad ways," since there is no end to the variety of eclectic, mixed-motives models that can be specified. Accordingly, and more pointedly, let's recast the question: "Is there a mixed-motives approach that encompasses Beckerian altruism and its predictions for crowding out?" This narrower question is not only tractable but more pertinent, in light of the importance of crowding out as a conceptual benchmark. What follows is a

synopsis of the "mixed motives" analysis put forth recently by Cox, Hansen and Jimenez (2004).

Before getting to the analytics, we provide the intuition for a particular "mixed motives" model, and a summary of its implications for empirical work. Imagine that, in addition to being motivated by Beckerian altruism given by (1), that the donor is also motivated by another, non-altruistic, consideration. For simplicity, and without losing anything essential, let's assume that this other motive is exchange. But before getting to exchange, consider the following example of unmitigated altruism.

Imagine that the recipient is victim of a flood. The donor, spared from the flood, provides transfers to the recipient in order to keep him alive: in technical terms, the altruist responds to the recipient's enormous, post-flood marginal utility of consumption.

To continue: the donor hears news of an impending food shipment from a relief agency. If the shipment gets through to feed the recipient, the donor will be happy and relieved. Should the shipment not arrive, the donor stands ready with large financial transfers. In the parlance of the altruism model, $\P T/\P I_r$ is negative and large in absolute value, where I_r includes the value of the food shipment. There is no exchange motive at the margin; in this matter of life and death, consideration of repayment is decidedly beside the point.

Fast-forward: The recipient has recovered fully from the flood, now long past. In terms of the model, I_r is at its pre-crisis level—a value too large, say, for the donor's altruistic transfer motive to be operative. Nevertheless, the donor still makes transfers to the recipient, but they are given in exchange for in-kind services that the recipient provides to the donor. The transfer derivatives associated with these are much less pronounced than the large negative one

associated with altruism. Indeed, as we saw above, $\P T/\P I_r$ might even be positive over some values of I_r.

The upshot is that with mixed motives the relationship between I_r and T need not be linear, even though Beckerian altruism is part of the mix. Indeed, the relationship need not even be monotonic. An illustration is provided in Figure 1, drawn for a fixed value of I_d. When recipient incomes fall below the cutoff $\bar{I}_r$, the donor's motive is altruistic; transfer derivatives are governed by Beckerian altruism and crowding out is complete. But with $I_r \geq \bar{I}_r$, the altruistic transfer motive is no longer operative. Transfer still take place but are now exchange motivated, with different transfer derivatives (Cox, Hansen and Jimenez (2004)). The primary empirical motivation of the mixed motives approach is that the estimation of transfer derivatives entail rather complex functional forms; the simple linear transfer function implied by Beckerian altruism alone is misspecified. Cox, Hansen and Jimenez (2004) find evidence supportive of the mixed-motives approach; a detailed discussion of their findings is deferred to section III .

Coercion

Private transfers have thus far been characterized either as altruistic *giving* or as part of a two-way *exchange*—how about *taking* as an alternative to giving or exchange? Udry (1996) cites the practice of domestic violence in West Africa as *prima facie* evidence against Pareto Optimality in household allocations; and Bloch and Rao (2002) present direct evidence of the role violence plays in such allocations in their case study of a potter community in South India.

Becker (1993) has worked out a simple model of coercion[9] that captures much of the essence of the problem. Consider the canonical donor-recipient framework. Imagine that despite being altruistic toward the recipient, the donor's transfer motive is *inoperative,* so that

[9] Which he calls "preference formation."

his intended transfers, *T,* are zero. Suppose that this "donor"—the word is now in quotes since private transfers will flow in the opposite direction—has the ability to extort a gift, *g,* from the "recipient" by spending resources, *x,* in order to make the latter feel guilty or fearful. Extortion works if these unpleasant feelings, which Becker appends to the recipient's utility function as $-G(x,g)$, are assuaged by increases in *g,* (i.e., $G_g < 0$) and if extortion intensifies this effect ($G_{xg} > 0$).

As one might guess, the income effect in this model is different from altruistic crowding out. For instance, an increase in I_r can act like a red flag in front of a bull for the extortionist, prompting an increase in *x.* [10] Of all the possible motivations for familial transfers, coercion is by far the least studied, likely because of the scarcity of information on things like violence or other forms of familial pressure; thus family conflict represents a potentially valuable area for further research.

Village-level risk-sharing

More than just two people can pool their resources, obviously; plus, there are many ways other than private transfers for people to cope with the vicissitudes of economic life. The so-called "village risk sharing" or "perfect markets" approach pioneered by Townsend (1994) adds these considerations to analyses of networks of family and friends.

What are the implications of the group-risk-sharing perspective with regard to crowding out? How do they relate to our starting point, the Beckerian altruism model? The two approaches share an important prediction, which is that *individual consumption depends not on individual income but on aggregate income*—only now the aggregation is over more than just

[10] Bloch and Rao indeed find that domestic violence against wives is fueled by the perpetrator's desire to extort money from his in-laws.

two people. But the approaches part ways when it comes to *transfer derivatives,* because private transfers are not the only means of redistributing incomes or coping with income risk. The perfect markets approach embodies the myriad ways that households can adjust to income fluctuations, *inter alia* (and in addition to private transfers): the use of formal and informal credit, adjustments to savings, changes in labor supply, the timing of durable purchases and asset sales, reliance on formal safety nets, and so forth.

As in the collective model, and unlike Becker's model, no single member of the group dominates decision-making: given the aggregate income of the group, individual consumption is determined by a "Pareto weight" analogous to the sharing rule in the collective model. The addition of degrees of freedom—more households, more ways to finance consumption—breaks the strict relationship between private and public transfers predicted in Becker's model. Consequently, much of the empirical attention in the risk-sharing literature has been focused not on private transfers but on the connection between individual consumption, individual income and the aggregate income of the risk sharing pool. The model's key prediction is that only income of the risk sharing pool, not individual income, should matter for individual household consumption. The result is a cross-sectional analogue of the implications of the life-cycle/permanent income hypothesis that permanent income – not current income – determines current consumption.

III. Empirical Evidence on Private Inter-household Transfers and Risk Sharing

III-A. Crowding Out

So much for theories of crowding out; what is the available evidence? There has been a boom in the number of empirical articles on private inter-household transfers in the past 15 years or

so. Much available evidence is consistent with partial crowding out, that is, transfer derivatives that are substantial enough that they probably should merit the concern of policymakers, but the typical study does not produce estimates large enough to be consistent with complete crowding out as predicted by Becker's model. Several studies estimate transfer derivatives in the range of 20 or 25 cents on the dollar. Empirical transfer derivatives, however, cover a wide range: a few are close to being consistent with complete crowding out while others suggest hardly any effect at all.

A complete accounting for differences in estimated transfer derivatives would be a daunting task, because so many things differ from one study to the next, *inter alia:* the details of how private transfer information is collected, how private transfers are defined, how transfer functions are specified, how much detail is available on the characteristics of potential recipient and donor households, the priorities given to the various econometric issues, and the institutional settings of the individual countries. Nonetheless, the surge of empirical work on private transfers in developing countries during the past decade and a half has contributed much to our understanding of crowding out, most importantly by demonstrating how rarely complete crowding out has appeared in the data. Additionally, this work enables us to have a much clearer picture of future research directions and needs.

So what does the recent empirical literature tell us about crowding out? Most of the work indicates that the necessary *background conditions* for crowding out to be a *possibility* are indeed in place in most developing countries. One necessary condition is that private inter-household transfers be widespread and large, for the simple reason that, were this not to be the case, there would be little to be crowded out. Another necessary condition is that private

transfers function like means-tested public income redistribution by flowing from better off to worse-off households. Recent work suggests that, in most places, both conditions hold.

III-A-1. The prevalence of private inter-household transfers

How widespread are private transfers between households? While there is no single number that captures the idea succinctly, we do our best to summarize, with caveats to follow. It is a safe bet that, across the spectrum of developing countries that have been studied, the modal percentage of households involved in private transfers in a given year (either as recipients, donors, or both) is somewhere around 40 percent. Some countries report much lower *involvement rates* (i.e. the fraction of households giving or receiving or both) and some report much higher rates: the minimum is perhaps around 10 percent, and the maximum at least 90 percent. So these numbers indeed suggest a great deal of private-transfer activity across households.

But having stuck our necks out by providing such a summary, we are compelled to qualify the above statement along several lines:

- There is no generally accepted, standardized way to collect information about private transfers. Hence, much of the conclusions about the prevalence of transfers (as we will see below) can merely be the consequence of how the data are collected. For instance, the more questions about private transfers a survey contains, the higher the survey's reported involvement rates.

- There is no generally accepted definition of what constitutes a private inter-household transfer. For instance, some surveys count informal loans between households as private transfers, others do not. Whether such loans should indeed be counted—and indeed how to distinguish a loan from a gift—entails subtle judgment calls that are not so easily resolved.

- There is no generally accepted definition of what "inter-household" means. Suppose someone who ordinarily lives in the household temporarily resides elsewhere, and remits a sum of money to that household. Typically this transfer is treated as an inter-household transfer, but one could imagine that in cases of extremely short absences it might be more appropriately categorized as an intra-household transfer.

- Even if the above problems were to someday be solved, there would no doubt remain further difficulties on several fronts, including cognition, culture and stigma. It is not clear what the optimal time frame would be for optimally efficient recall of private transfers, for example. Definitions of what constitutes a loan versus a gift would surely vary from one culture to the next. In some cultures there may be stigma attached to (for instance) receiving money from one's children; in others, there may be stigma attached to *not* receiving money from one's children!

- Nearly all surveys and studies of private transfers deal with *realized,* rather than *potential,* private transfers. But it is actually the former that determines, in the language of Barro (1974), whether the transfer motive is *operative.* Perhaps I have a brother who stands ready to help me in case of emergency, but that emergency never happens. Nonetheless I am insured. Such potential transfers are likely crucial, but are missed by standard surveys.

Cox, Galasso and Jimenez (2006) studied private inter-household transfers in a diverse cross section of developing countries for which nationally representative surveys with requisite information was available, in roughly comparable form (all surveys were from the World Bank's Living Standards Measurement Surveys (LSMS)). The cross section contains information reported between 1994 and 1998 from 11 countries from around the world: Albania, Bulgaria, Jamaica, Kazakhstan, the Kyrgyz Republic, Nepal, Nicaragua, Panama, Peru, Russia and Vietnam. Private transfers are defined as monetary gifts and the money value of in-kind transfers given and received by households (inter-household loans were excluded). Per LSMS definition, individuals absent from the household longer than three months during the past year were not counted as household members, nor were boarders or lodgers. Most countries (seven) queried respondents about transfers during the past 12 months, three asked about transfers during the last month, and one asked about transfers from the past three years.

Four of the 11 countries had involvement rates of 40 percent in private transfers; 8 of the 11 had involvement rates ranging between 30 and 50 percent. But the definition of private transfers makes a difference in these calculations. For instance, Vietnam's private-transfer

involvement rate based on gifts, calculated from that country's LSMS for 1998 was 37 percent, but the comparable figure adding in inter-household loans was 52 percent Cox (2004).

Further, it is not clear whether what a household calls a "loan" does not contain at least some element of a gift if, for example, it is given interest free. Conversely, what is reported as a gift might in fact be given in expectation of some future reciprocal help, in which case it might be more aptly conceptualized as a loan.

Several other recent studies of private transfers for which involvement rates are readily available indicate significant proportions of households involved with private transfers. These include: Amelina, Chirbuca and Knack (2004, Romania); Cox and Jimenez (1998, urban poor in Cartagena, Colombia); Cox, Jimenez and Okrasa (1997, Poland); Cox, Hansen and Jimenez (2004, Philippines); de la Briere, Sadoulet, de Janvry and Lambert (2002, the Dominican Sierra, Dominican Republic); Frankenberg, Lillard and Willis (2002, Indonesia); Hoddinott (1992, elderly in Western Kenya); Jensen (2004, South African "homelands"); Kazianga (2006, Burkina Faso); Kuhn and Stillman (2004, Russia); LaFerrara (2003, Ghana); Lee, Parish and Willis (1994, Taiwan); Lillard and Willis (1997, Malaysia); Maitra and Ranjan (2003, South Africa); McKernan, Pitt and Moskowitz (2004, Bangladesh); Miller and Paulson (2000, Thailand); Raut and Tran (2005, Indonesia); Udry (1994, Nigeria).

In a few cases, however, the incidence of private transfers between households appears low. For instance, Cox, Galasso and Jimenez (2006) found only an 11 percent involvement rate for Albania, despite the fact that respondents there were asked to report any transfers received or given during the past three years. Albarran and Attanasio (2002) found that only 9-13 percent of their Mexican sample reported receiving transfers, and conjectured that the low percentages were due to the short reporting window of 30 days. Secondi (1997) found that only 6 percent of

a sample of rural Chinese households reported receiving transfers in 1989, but "transfers" carried the connotation of "financial support." When information about receipts of gifts is added, the rate of transfer inflows rises to 26 percent. These findings underscore the earlier points about the relevance of transfer definitions for assessing the pervasiveness of private transfer networks.

How about the actual money value of the transfers? Available evidence indicates that, in the modal case, there is indeed much in private transfers that could be crowded out. For instance, consider the modal four countries in terms of private transfer involvement rates (Kazakhstan, Kyrgyz Republic, Russia and Vietnam) from the 11-country study of Cox, Galasso and Jimenez (2006) referred to earlier. For these countries, the percentage of private transfer receipts in total household income ranges from 6 to 8 percent for all households, including non-recipients. Among sub-samples of recipient households, the percentage of private transfers in total household income range from one-quarter to one-third. So both the prevalence and size of private transfers can indeed be substantial.

Despite the substantial gains in knowledge about the scope of private transfer networks that recent data collection and analysis has provided, there remains a conceptual flaw (which we believe could be easily remedied) in how information about transfers is gathered. Nearly all surveys tend to focus on *realized* rather than *potential* transfers. Yet the latter are what might guide the household's behavior. Knowing that my brother stands ready to bail me out of a jam can affect my savings and investment decisions, and though it may turn out that trouble never finds me nonetheless I can depend on an operative transfer motive. Such potential transfers might function like precautionary savings, yet they may be invisible to researchers using standard survey tools.

Not that questions about potential transfers are entirely absent from existing surveys. For instance, the survey of urban poor in Cartagena, Colombia undertaken by Bamberger, Kaufmann, Velez and Parris (1992) and used in Cox and Jimenez (1998) asked respondents to report the number and financial status of network members, where a network was defined as "a set of individuals or households who regularly assist each other through the provision of money, goods, services or the provision of accommodation."[11] Such information is useful for identifying households who might rely on networks even if they have not received help of late.

Much more could be done, probably at low cost, to obtain higher quality information concerning the scope of operative inter-household transfers in developing countries. Consider the following simple survey question from the first wave of the United States Health and Retirement Survey (HRS) a household survey of persons approaching retirement age. Respondents were asked the following:

> Suppose you [and your (husband/wife/partner)] ran into severe financial problems in the future. Do you have relatives or friends who would be both willing and able to help you out over a long period of time?

A significant fraction of households *not* currently receiving private transfers nonetheless answered "yes" to this question, suggesting that a possibly large gap might exist between realized and potential transfers. So the question conceivably conveys valuable information about the extent of operative transfers. Further, it is rather simple and direct; it would seem rather straightforward and inexpensive to graft it onto, for instance, an LSMS survey module dealing with inter-household transfers.

III-A-2. Evidence on transfer derivatives and crowding out

[11] Bamberger, Kaufmann, Velez, and Parris (1992), p. 2-1.

While the existence of widespread private transfers is *necessary* for crowding out, it is far from *sufficient.* To cite a trivial example: if transfers were determined by strict cultural rules that specified fixed monetary amounts to be given irrespective of income or other events, crowding out would be nil even with ubiquitous private transfers. So we turn to empirical evidence on transfer derivatives. By way of preview: the modal study indicates that transfer derivatives are large enough that policymakers and academics should probably sit up and take notice. A rough guess would put the modal transfer derivative at somewhere around 20 to 25 cents on the dollar, which suggests that public transfers can indeed have a non-trivial, negative impact on private inter-household transfers. But the sort of complete crowding out envisioned by Becker (1974) and Barro (1974) is not found in the typical empirical study. Sightings of complete crowding out have occurred, and they are noteworthy but rare. (We discuss them later on below, and we also summarize much of the recent empirical findings in Table 1.)

Before getting into details, and to continue our broad-brush summary of the empirical literature on private transfers, we note that in many ways it is analogous to the empirical literature on labor supply in the late 1970's and early 1980's. Like the labor supply literature back then, researchers took diverse theoretical and empirical approaches to the data, and (perhaps not surprisingly) produced a spate of sometimes dramatically divergent estimates. Toward the late 1980's, labor economists turned their attention to unifying and resolving conflicting estimates in the literature, as in the classic paper of Mroz (1987), who addressed a variety of modeling and econometric issues with a single data set in order to understand what was driving divergent estimates. The empirical private transfers literature awaits such a full-scale unification. At the end of this section, we point out a recent paper that is noteworthy for attempting this for private transfers, and encourage further work along these lines. For now,

though, we note that the findings summarized below are generated from a variety of methods, and that some papers pay much more attention to certain econometric issues (e.g., endogeneity of income, selection bias, potential non-linearities) than others.

Since this section is primarily about income effects, we need to settle on convenient terminology. We will use the word "income" to denote "pre-private-transfer income," or what is sometimes referred to as "endowment income," that is, the value of household resources. Unless otherwise noted, "income" refers to current household income.

One rather coarse stylized fact about inter-household private transfers is that there is evidence that they act like means-tested public transfers, in the following sense: private transfers appear to flow from high- to low- income households in nearly every country for which such information is available. For instance, in their 11-country study of LSMS data, Cox, Galasso and Jimenez (2006) find that for 10 out of the 11 countries, the average incomes of private-transfer donors exceeds that of recipients—and usually by a wide margin.[12] And in most of the countries in their sample, the share of income accruing to the lowest quintile increases markedly, in percentage terms, after private transfers are figured into total household income.

So what does the existing recent literature tell us about the responsiveness of private transfers to a one-dollar increase in public transfers? Our reading of the literature suggests that a sensible estimate would be on the order of somewhere between 25 to 30-cent reduction in private transfers. That is an estimate that comes with several qualifications, but before getting to those, some detail on how the estimate was chosen.

[12] The only country in their sample that did not conform to this pattern was Albania.

Take the dozen papers for which it is straightforward to obtain the estimated value of the transfer derivative $\P T/\P I_r - \P T/\P I_d$ or the estimated partial derivative $\P T/\P I_r$.[13] A ranking of those estimates puts both the median and mode at the –0.25 to –0.30 range. Consider three studies from the middle of the distribution: Clarke and Wallsten (2003), Jensen (2004) and McKernan, Pitt and Moskowitz (2004).

Clarke and Wallsten (2003) avail themselves of a natural experiment—Hurricane Gilbert, which struck Jamaica in 1988—to investigate the impact of exogenous damage-related income shocks on inflows of remittances. Using panel data created from the Jamaican Survey of Living Conditions (an LSMS-style survey), they found that remittances increased 25 cents for every dollar's worth of damage inflicted by the hurricane. A potential problem with this episode is that the estimates might not provide much information about crowding out if private donors respond differently to hurricane-related shocks than they would to income changes from tax and public-transfer policies.

Jensen (2004) exploits a natural experiment generated from public policy—South Africa's dramatic post-apartheid expansion in public pension benefits—to estimate the response of private transfers to changes in public transfers, and finds that a one-rand increase in pensions is associated with a 0.25—0.30 rand reduction in remittances received from children living away from home.

The last "modal" study, by McKernan, Pitt and Moskowitz (2004) also uses policy-generated income variation—this time from credit programs targeted to the poor in Bangladesh—to investigate tradeoffs between program-provided credit versus informal credit-

[13] These are Albarran and Attanasio (2002), Clarke and Wallsten (2003), Cox, Eser and Jimenez (1998), Cox and Jimenez (1992), Cox, Jimenez and Okrasa (1997), Cox, Hansen and Jimenez (2004), Jensen (2004), Kazianga (2006), Lucas and Stark (1985), McKernan, Pitt and Moskowitz (2004), Raut and Tran (2005) and Secondi (1997).

plus-private-transfers. They find that a 100 Taka increase in female program credit reduces net private transfers and informal loans by 25 Taka; the equivalent figure for men is 31 Taka.

It would be misleading to conclude from these papers that any sort of consensus exists concerning crowding out; the actual range of estimates in the literature is exceedingly wide. For instance, a few of the dozen studies cited above (Cox, Eser and Jimenez (1998), Lucas and Stark (1985) and Secondi (1997)) estimate *positive* values for $\P T/\P I_r$. At the other end of the spectrum, some of the estimates of the transfer derivative $\P T/\P I_r - \P T/\P I_d$ estimated in Raut and Tran (2005) are exceedingly close to the value of –1 predicted by Becker (1974).

Nor is the current state of the art sufficiently developed to easily reconcile such differences (though recent developments suggest that the literature might be headed in this direction). For instance, Frankenberg, Lillard and Willis (2002) use the same data set as Raut and Tran (the 1993 Indonesian Family Life Survey) but obtain estimated income effects nowhere near as large as they do. A possible reason is that the latter authors use the Altonji-Ichimura method for controlling for sample selection bias (Altonji and Ichimura (2000), Altonji, Hayashi and Kotlikoff (1997)). This estimator accounts for the inherent nonseparability between incomes and preferences in the altruism model of transfers. Failing to account for such nonseparability can bias estimated transfer derivatives away from the strong effects implied by altruism. Intuitively, imagine that parents vary in their (unobservable) altruism toward their children. A child whose income is relatively large relative to that of his parents but nonetheless *still* receives a transfer is more likely to have especially generous parents, who accordingly would be prone to give especially large transfers. Failing to account for this nonlinear, nonseparable source of selection bias generates estimated values of $\P T/\P I_r - \P T/\P I_d$ that are biased toward zero, because they fail to control for the spurious effects generated by the

interplay between unobserved heterogeneity in parental altruism, on the one hand, and transfers and incomes, on the other.

Another source of nonlinearity, referred to in the earlier discussion of theory, is the possibility that more than one motive might govern private transfer behavior, and that large transfer derivatives might prevail only for households whose incomes are low enough to prompt assistance motivated by unvarnished altruistic sentiments (as opposed to, say, a desire for reciprocal assistance). Such a model implies, for example, that $\partial T/\partial I_r$ would be negative and large in absolute value for low values of I_r, but might well be negligible for higher values of I_r. Cox, Hansen and Jimenez (2004) estimate such a nonlinear model for the Philippines, and find rather pronounced nonlinearities consistent with a mixed-motives approach to private transfers. The nonlinear transfer function takes the form of a spline, where the knot point of the spline is itself a parameter to be estimated. They find striking differences in the estimated values of $\partial T/\partial I_r$ by I_r: about –0.40 for poorer households (29th percentile and below for urban households, 20th percentile and below for rural households) and negligible estimated values of $\partial T/\partial I_r$ for the others. They also show that failing to account for nonlinearities can generate misleadingly tepid estimates of $\partial T/\partial I_r$; a linear transfer function generates values between –0.02 and –0.03.

Maitra and Ray (2003) find corroborating evidence for South Africa, namely, that public pensions appear to crowd out private transfers among poor households, whereas the two forms of transfer appear to complement each other for the non-poor.

One problem with the study of Cox, Hansen and Jimenez (2004) and several related studies of inter-household private transfers, is that no matched information on transfer donors and recipients is available. This data deficiency can lead to potentially serious problems of

omitted variable bias, which tend to stack the cards against finding evidence for crowding out. For instance, in the case of altruistically motivated intergenerational transfers, positive correlation of incomes of parents and children would tend to bias estimated values of $\P T/\P I_r$ toward zero. In the case of the Cox, Hansen and Jimenez (2004) paper, the possibility of such bias makes the estimates that much more noteworthy, and the authors suggest that Becker's (1974) crowding out conjecture, so many times rejected empirically, should not yet be ruled out of court. Further they argue that, paradoxically, the most appropriate testing ground for crowding out might well be laissez-faire economies such as the Philippines, whose low rates of social spending have (perhaps) not yet rendered crowding out a *fait accompli.*

Another piece of evidence to suggest that income effects of private transfers might not be reduced to a single number comes from simple descriptive panel evidence for Vietnam, using LSMS data that initially surveyed households in 1993 and then re-surveyed them in 1998 (Cox (2004)). Indeed, there is evidence of extreme household-specific heterogeneity in transfer derivatives. The panel data were used to construct a simple, household-specific transfer derivative, $\Delta T/\Delta I$, where ΔT denotes the change in net transfer receipts (excess of inflows over outflows) between 1993 and 1998, and ΔI denotes the change in per-capita household income. The empirical distribution of $\Delta T/\Delta I$ spanned an exceedingly wide range, from –0.75 at the 10th percentile to 0.29 at the 90th percentile. The first figure is in line with received wisdom about private transfers, since changes in transfers act to offset changes in income. But the latter figure is at odds with that story, and indeed indicates that private transfers have a destabilizing effect on total household income.

So while we have learned much about crowding out in the past 15 years or so, mounting evidence points to disparities in estimated private-transfer income effects that are in need of

explaining—disparities between households within a country, disparities between countries, disparities between studies using the same data set for the same country, and so on.

With this thought in mind, we single out two papers in the literature that we think are exemplary for pushing the frontier of research on crowding out. The first is a recent study of private transfers in Burkina Faso by Kazianga (2006). He finds little evidence for crowding out in this country, but what is noteworthy here is not so much the findings themselves but the approach. What is attractive about this paper is that it takes rather ordinary data (surveys similar to the World Bank's LSMS) and applies a painstaking econometric approach that seeks to address a variety of estimation issues at once, including selection bias (making use of the Altonji-Ichimura estimator), potential endogeneity of income, and non-linearities in income effects. As such, the paper comes closest to what Mroz (1987) did for the empirical labor supply literature, which is to establish some benchmarks for econometric "best practice." As the literature on private transfers moves ahead, such benchmarks will prove increasingly valuable, so that researchers and policymakers wishing to see the big picture will be able to focus on differences in estimated crowding out attributable to fundamentals (such as country differences in social safety nets) as opposed to those due to modeling issues (such as the failure to properly model selection bias).

A second exemplary paper—a Romanian case study by Amelina, Chirbuca and Knack (2004)—pushes a distinctly different edge of the research frontier on crowding out by undertaking truly innovative and path breaking data collection. This effort, undertaken under the auspices of the World Bank in 2002, produced the Romanian Public/Private Transfers and Social Capital Survey. What this study—and survey—does that is different and valuable is that it goes beyond a narrow focus on private inter-household transfers to seek detailed information

about so-called “social capital” and civic life: that is, involvement formal private associations and clubs, and participation in collective action and local government decision making. In a nutshell, the study expands the purview of private transfers from the extended family into the neighborhood, and, from there, into local politics and beyond. The survey attempts to measure perceived corruption and trust in local government, for instance. Further, the survey supplements standard questionnaires by engaging in open-ended discussions with respondents in order to obtain more in-depth information about causes and consequences of private transfers and other coping mechanisms.

A basic insight that emerges from this work is that the canonical model of crowding out—whereby public transfers are exogenously bestowed like manna from heaven—needs to be rethought. The next generation of models of interactions between the public and private sectors should pay more attention to the subtle realities of how public transfers are allocated. How, for instance, interest groups must organize in order to elbow their way in to the public trough. Amelina, Chirbuca and Knack find that the Romanian poor are disadvantaged on many margins. They tend to be shut off from sources of formal private transfers such as clubs and associations. They tend to have a low level of trust in neighbors, strangers, and institutions, and in contrast to their richer counterparts they have trouble reaping gains dividends from social capital. Further, the authors find that, in the case of Romania, private familial transfers are of little help, since the poorest give about as much as they get, so that private transfers affect their economic position very little.

We hope that similar survey instruments and analyses will be implemented for other countries, and that the entrenched dichotomous view of public versus private transfers in the crowding out literature be replaced with a more nuanced approach that recognizes a continuous

spectrum of social safety nets that are not necessarily so easily pigeonholed. For instance, the Becker (1974) model demonstrates how endogenously determined private transfers respond to exogenous changes in public transfers. Yet it is just as easy to imagine that one might want to analyze a world in which public transfers are determined endogenously by private, grassroots networks that are partly familial, partly neighborhood-like and partly related to civic organizations. More cohesive private networks could be more adept at steering public benefits their way—"crowding in," rather than crowding out.

III-B. Demographic and other variables in empirical studies of private transfers

There can be little doubt that the specter of crowding out has been one of the largest—if not *the* largest—galvanizing force for motivating empirical work on private transfers. Hence researchers have devoted considerable attention to both the logic and evidence associated with income effects and private transfers. But it is equally obvious that other variables—such as demographic influences like age and gender—are also important determinants of private transfers.

One thing that distinguishes age effects from income effects is that the latter tend, at least roughly, to be the same from one country to the next, in the sense that the trend is almost always for private transfers to flow from high- to low-income households (e.g., as noted earlier in reference to the study of Cox, Galasso and Jimenez (2006)). In contrast, age patterns often differ dramatically between countries. For instance, Cox, Galasso and Jimenez (2006) find that transfers from young to old exceed those going from old to young in the Latin American countries in their sample (Jamaica, Panama, Peru and Nicaragua) and in Vietnam and Nepal, whereas the opposite is true for Russia and Bulgaria. While some of these effects are no doubt

attributable to differences in public pensions and crowding out, the inter-country differences in age patterns nonetheless persist even after controlling for household resources.

Why the differences? One researcher who has devoted a large part of his career to pondering this issue is economic demographer Ronald Lee. He, along with anthropologist Hillard Kaplan, have proposed that age patterns in intergenerational transfers could be affected by the stage of economic development, as appreciated against a backdrop of evolutionary considerations. We will consider the latter in more detail in the next section, but for now, suffice it to say that our species has weathered the vicissitudes of the past 150,000 years or so (and, indeed, has flourished) in no small part due to our ingrained proclivity to nurture and support our young. The evolutionary baseline is that older generation members care about, and provide support for younger relatives more than *vice versa.* Kaplan's (1994) review of evidence from traditional (i.e. hunter-gatherer, or pre-agricultural) societies—which are thought to best reflect the evolutionary baseline—indeed supports this view: transfers from old to young predominate. The same pattern tends to hold for advanced industrial and post-industrial societies (Lee (1997)).

What then, are we to make of countries like Vietnam, where transfers from young to old, rather than old to young, tend to predominate? First, note that, in most of the developing world, social security consists of private old-age support from adult children. But probing more deeply beneath this proximate influence, Lee (1997) advances an intriguing hypothesis concerning agriculture, namely, that support of elder farmers can indirectly redound to the benefit of young children. How? Consider, as emphasized by Rosenzweig and Wolpin (1985) that idiosyncratic farm characteristics might exercise a heavy influence on agricultural productivity, lending primacy to the role of intergenerationally transmitted, farm-specific knowledge.

Intergenerational transfers from adult children to their elderly parents can benefit grandchildren and their forebears if they serve to perpetuate the valuable farm-specific human capital embodied in the elderly. There are, by now, a sufficient number of LSMS surveys from a diverse enough set of countries to test this hypothesis.

Unlike age patterns, which vary by country, patterns by gender do not: private transfers tend to be targeted to female-headed households. For instance, in each of the 11 countries studied by Cox, Galasso and Jimenez (2006), female-headed households were more likely to receive private transfers than male-headed households. Further, nearly all single-country studies of private transfers, be they developing or advanced economies, uncover this pattern: e.g., Lucas and Stark (1985, Botswana); Kaufmann and Lindauer (1986, El Salvador); Cox, Hansen and Jimenez (2004, Philippines); Guiso and Jappelli (1991, Italy); Cox (1987, United States).

One obvious potential explanation for the pattern has to do with migration, with wives receiving remittances from husbands temporarily absent from home. But Cox, Galasso and Jimenez (2006) find that the pattern holds up even if households with temporary migrants are removed from the samples. Another explanation is sex differentials in life expectancy, with old age support disproportionately targeted to widows. But again, the pattern holds up even controlling for age.

A different, and perhaps complementary, explanation advanced by Cox (1987) has to do with the exchange motivation for private transfers. There is abundant evidence from sociology and social psychology that women are more heavily involved in the provision of inter-familial services (e.g., caring for extended family members) that are predicted, under the exchange hypothesis, to be compensated for by inter-household transfers.

As with gender, several other of the covariates typically entered into the standard empirical transfer function are subject to multiple interpretations. Take education, for instance: it is correlated with lifetime resources; it may be correlated with past transfers, and hence be picking up the strength of parental altruism; it might be proxying the recipient's ability to reciprocate transfers they receive. Interpretations can differ with respect to hypothesized sign for the partial correlation of education and private transfers: for instance, the first story could predict a negative sign for educational attainment, the second and third predict a positive sign.

Even in a simpler world in which the latter two considerations were wiped away, the hypothesized sign of education need so be pinned down so simply. For instance, imagine a model in which private transfers are used to alleviate liquidity constraints, as in Cox (1990). Further, suppose in a regression of transfer receipts on education, that current income is being controlled for, and imagine that education, then, is picking up the permanent income of the potential recipient. With current income constant, more education implies higher permanent income and hence higher desired consumption. With current income constant then, more education implies a bigger gap between desired consumption and current resources. If private transfers alleviate liquidity constraints (and hence are used to fill this gap) predicted transfers should rise with education. One the other hand, however, if liquidity constraints were not important, so that private transfers were used to equalize, say, lifetime earning capacity, then education would be expected to enter the transfer function with a negative sign.

Indeed, the education effects produced in the empirical literature reflect these cross currents; some studies produce positive education effects, others produce negative ones, and usually there is little attempt to explicate the exact rationale for education's role in the transfer function. The same is true for other "controls" that are typically included in empirical studies of

inter-household transfers, such as household size, number and ages of children, ethnicity, and so forth.

Indeed, there exists a kind of double standard in the transfers literature with respect to the relationship between theory and empirics. Usually there is a painstaking discussion of the logic of income effects, often stemming from concerns about crowding out and a desire to understand underlying motivations for private transfers. Accordingly, the ensuing empirical work rests on a foundation that facilitates the interpretation of income effects. In sharp contrast, however, demographic and other variables are often merely entered as controls, and frequently they are given short shrift in the discussion—either described in and *ad hoc* manner or sometimes the results are suppressed altogether.

Yet demographic and other influences are at least as important as income for explaining variation in private transfers, and a major piece of unfinished business in the literature—something that we return to in the last section—is subjecting these influences to the same exacting theoretical scrutiny that has been directed toward income effects. Before venturing into those uncharted waters, however, we complete our survey of inter-household relationships by discussing the remaining major sub-discipline in the field—inter-household risk-sharing.

III-C. Risk Sharing

As a segue into our discussion of the risk sharing literature, we begin by noting a couple of additional findings from the private transfers literature not yet discussed. There is abundant evidence (perhaps not surprisingly) that private transfers appear responsive to adverse shocks experienced by households. For instance, in each of the countries studied by Cox, Galasso and Jimenez (2006) samples of households with someone sick enough to have to miss work or limit daily activities received transfers in greater numbers than healthy households. Fafchamps and

Lund (2003) found, for a sample of Filipino households, that gifts and informal loans were highly responsive to certain shocks to income and expenditures, such as the unemployment of the household head or spouse, or the onset of funeral expenses. These findings are consistent with a central premise of the so-called "risk sharing" literature, which is that households provide mutual insurance to one another in order to smooth their consumption in the face of risk.

But in some key aspects, the approach of the risk sharing literature is different from that of the private transfers literature. Most importantly, the variable that gets placed front and center in this literature—both conceptually and empirically—is not private transfers but consumption. The key question—first posed by Robert Townsend in his seminal 1994 paper—is this: as a rural household in a small village, facing risks of drought, pestilence, illness, and the like, what is it that determines my consumption in a given year? If I get sick or lose my job or my crops, will my family and I go hungry? Or might the appropriate "unit of consumption" extend beyond the walls of my home? Suppose that households in my village act as an extended family, pooling resources and consuming, as it were, from a common village pot. Such pooling would serve to lessen the sensitivity of my own consumption to fluctuations in my income; what would matter is the amount of resources in the entire village.

To a first approximation, the risk-sharing hypothesis can be thought of as a cross-sectional analogue of the life-cycle/permanent income (LC/PIH) hypothesis. (The analogy is not exact since there are time subscripts in the risk sharing model but it is pedagogically useful nonetheless.) Hall's (1978) pioneering test of the LC/PIH hinged in part upon the irrelevance of current income for consumption once permanent income had been controlled for. Townsend's test parallels that of Hall's, in that, likewise, with risk sharing, an individual household's current income should play only a minor role in determining its consumption.

Another attractive aspect of the risk sharing approach (and here is where we leave the cross-section analogy behind) is that, by focusing on consumption smoothing, the theoretical and empirical models can come to grips with *all* of the means by which households might cope with shocks in order to smooth consumption: not just with private transfer networks, but by using capital and credit markets, availing themselves of public transfers, private insurance, adjustments in the timing of discretionary durable purchases, and just about any other conceivable means of controlling consumption flows. Rather than seeking to parse out the individual mechanisms for smoothing, risk sharing studies focus on the bottom line: if consumption was smoothed, *some* combination of factors must have been at work to make it happen.[14]

In addition, the costs of collecting consumption data in developing countries—relative to advanced economies—is relatively low. Accordingly, there exist a wide variety of data sets amenable for testing the predictions of the risk sharing hypothesis, and, since the appearance of Townsend's (1994) paper, the literature has burgeoned considerably.

A consensus has emerged from this literature that parallels the literature on inter-household transfers, which is that, while there is evidence that households can mitigate the effects of shocks to their economic well being via risk sharing, such insurance is only partial, not complete. An early generation of tests, beginning with Townsend's own classic (1994) paper, followed by others such as Ravallion and Chaudhuri (1997), Townsend (1995) and many others, reported the extent to which the household's propensity to consume depended upon its own income after controlling for community resources. (To return to an earlier analogy: this

[14] Because kin-based and other inter-household transfers are but one element of the array of means by which households smooth consumption in the risk sharing framework, our treatment of this sub-literature in this Chapter is not as detailed as that of private transfers in earlier sections. For more detailed surveys, we refer readers to a variety of excellent papers, including Alderman and Paxson (1994), Morduch (1995, 1999), Townsend (1995), Fafchamps (1999), Dercon (2002) and Attanasio and Rios Rull (2003).

parallels the "excess sensitivity" tests of the LC/PIH in the macro consumption literature.) The estimated propensities are never zero, though some estimates are surprisingly low. (Again, the heterogeneity in estimates mirrors what has been found in the inter-household transfers literature for transfer derivatives: a range of estimates, with pronounced neutralization of income fluctuations the exception rather than the norm.)

A second generation of risk sharing tests, based upon variances rather than means, reinforces these earlier findings. Inspired by an early suggestion of Deaton and Paxson (1994), Attanasio (2002) and others have pioneered tests of risk sharing based upon comparisons of the variance of consumption versus the variance of income. The intuition for the test is rather straightforward: if households can avail themselves of various mechanisms for smoothing consumption, the variance of consumption should be less than that of income. An example of such a test is Attanasio and Szekely (2004), who find that Mexican households have difficulty insuring against wage shocks, and that negative shocks can cause cutbacks in purchases of goods related to human capital investment, thus possibly jeopardizing a household's future earning capacity.

The concept of income variances enters the risk sharing literature's perspective on crowding out, which is a bit different from that of the private transfers literature. The argument, as explicated by Attanasio and Rios Rull (2000), goes like this: public transfers reduce income variability, and the good news is that this can allow households to do a better job of consumption smoothing. A possible downside, however, is that, with incomes thus smoothed, households may no longer have sufficient incentive to band with others to form private risk sharing arrangements. Attanasio and Rios Rull (2000) go on to find supporting evidence: benefits from Mexico's PROGRESA program do indeed appear to partially crowd out private

transfers. Dercon and Krishnan (2003) find similar results for publicly provided food aid in rural Ethiopia.

The risk sharing literature has matured rapidly, both conceptually and empirically, in the sense that it is now accepted that problems of, *inter alia,* commitment and enforceability should be incorporated as standard fixtures in the modeling landscape. For instance, Foster and Rosenzweig (2001) propose a creative way of inferring problems of commitment (as well as the advantages of

familial altruism) by examining how past transfers affect current giving. The idea is that, all else equal (and with little altruism to impel continued generosity) having a long history of giving transfers should a household's propensity to make an additional transfer. The authors indeed find evidence to this effect, and they also find that problems of imperfect commitment do not appear so pressing in the presence of familial altruism. Ligon, Thomas and Worrall (2002) explicitly incorporate limited commitment into a model of household risk sharing, and find that this model empirically outperforms the simpler risk sharing model originally proposed by Townsend.

Another practical problem with inter-household risk sharing is that some risks will obviously be much easier to insure than others, and there is emerging evidence to support this idea. For instance, Gertler and Gruber (2002) find that Indonesian risk sharing networks can cope rather effectively with costs of ordinary illnesses, but not with severe ones that impair long-term health. Likewise, Fafchamps and Lund (2003) find that certain risks appear more insurable than others.

Townsend's original insight was to focus on the village as the unit of aggregation for the pooling of risk. This idea has much to recommend it, seeing how, for example, propinquity may be necessary for forming the bonds of trust needed to seal an implicit risk sharing agreement. People who live near one another have more opportunity to get to know one another and also have an easier time monitoring one another in order to police and mitigate moral hazard problems. But proximity entails problems too, not least of which is covariate risk. As Rosenzweig and Stark (1989) emphasize, one way to mitigate the problem of correlated risks is to forge links with far flung friends and relatives.

Recent work has attempted to move beyond the village-based risk sharing format. For instance, Grimard (1997) emphasizes how ethnic ties might play a role in the formation of risk sharing networks in Cote d'Ivoire. And Murgai, Winters, Sadoulet and de Janvry (2002) examine the role of transactions costs in determining the (endogenous) size and localization of the risk sharing group. This is an important issue to cultivate in future research on risk sharing. Too often, the literature takes a rather casual approach to the potential size of the risk sharing group,

and does not pay enough attention to problems of constraints on group size. We return to this issue in the next section.

IV. Moving Forward in an Evolutionary Direction

So much for the "half-full" part of the glass, what about the "half-empty" part? What *gaps* in the economic literature on extended families and kinship networks would we like to see filled? And how might researchers go about filling them? The considerable progress that economists

have made in the past 15 years has largely been concentrated in improving our understanding of forces that are central to the discipline: income effects, price effects, shared budget constraints, and the like. True, we have learned about other things along the way—demographic, cultural and geographic effects, for instance—but such influences are usually cast as adjuncts to economic issues or conceptualized in an *ad hoc,* purely descriptive manner.

Consider a typical regression from the empirical literature on private transfers: On the left-hand-side, a measure of private transfer receipts; on the right-hand-side, the household's income and/or wealth, including—data permitting—resources of potential donors. Education variables would likely be included, perhaps as indicators of permanent income. This canonical regression would likely also contain demographic variables, such as female headship, age, and marital status, number of children, household size, and the like. But as our discussion earlier in this chapter makes clear, while economists can draw upon a considerable body of theory for interpreting *income*-related variables, they have little guidance for thinking about the *demographic* variables, which often are just included as "controls."

We suspect that this is because economists lack a cogent framework for thinking about demographic influences *per se*. We contend that well-established insights from evolutionary biology can complement economic approaches to produce a more powerful model for understanding a fuller array of influences on family networks. Further, we argue that the approach is straightforward, easy to learn and parsimonious. It ties together diverse facets of behavior with just a few basic premises. And it is likely to look appealingly familiar to economists, entailing, as it does, maximization subject to constraints.

Before getting to details, and by way of motivation, we preview a sampling of predictions and insights that an evolutionary approach can provide:

- Mothers are expected to be more altruistic toward children than fathers. Relatedly, it may be highly useful to distinguish between *maternal* versus *paternal* grandmothers as sources of private inter-household transfers.

- Investigations of whether *sons* versus *daughters* tend to be favored with familial transfers might well pay attention to the parental family's wealth ranking in the relevant marriage market (and whether that market tends at least somewhat toward polygyny).

- Attention to biological basics helps to explain age patterns in the provision of assistance between extended family members and predicts that altruism of parents toward children should be stronger than that of children toward parents.

- Evolutionary theory predicts conflicts of interest can arise within families: children, for instance, will tend to want more than parents are willing to give to them, and interests of relatives from the husband's versus the wife's side of the family will not necessarily coincide.

- The theory advances clear-cut hypotheses regarding nepotistic behavior and transfers contingent on biological relatedness. Stepchildren, adopted children and foster children, for example, are expected to gain less from familial transfer networks than biological children.

The evolutionary approach unifies diverse phenomena in kinship networks, such as, *inter alia:* fetal development, health of the elderly, conflict between siblings over what constitutes fair treatment by parents, conflict between husbands and wives concerning quantity versus quality of children, conflict between in-laws, and the use of gifts versus loans in risk sharing networks (*gifts* are predicted to go to kin, *loans* to non-kin). To see how the approach works, we begin with its foundation, the so-called "Hamilton's Rule."

IV-A. Hamilton's Rule: The Evolutionary Cornerstone of Familial Altruism

Which should we expect to be stronger, a mother's altruism toward her young son, or an adult son's altruism toward his elderly mother? Might we expect mothers to be more solicitous toward their children than fathers? How about maternal versus paternal grandmothers? How much might we expect sons to be treated differently than daughters, purely because of their sex?

Should we expect siblings to be natural allies, or rivals who vie for scarce parental resources? Or perhaps we should expect they might be a bit of each?

Note that these questions are concerned with demographic effects *per se:* mothers versus fathers, sons versus daughters, old versus young. What is now known as Hamilton's rule was proposed by biologist William D. Hamilton over 40 years ago (Hamilton [1964]) and related theories, primarily those of Robert Trivers and his collaborators, form the basis for understanding the evolutionary basis for familial altruism. These theories make clear predictions about demographic influences within kinship networks. In addition to being falsifiable, the logic of Hamilton's rule is exceedingly compact, and its implications are sometimes far from intuitively obvious. As such, the biologically based approach shares strengths in common with the best of economic theory; it is parsimonious, counter-intuitive and falsifiable.

What is Hamilton's rule?

Hamilton's rule is a simple but far-reaching system of logic that contains the biological foundations for familial altruism. Acts of altruism, such as the honeybee's suicidal defense of its hive, seemed to contradict the Darwinian dictum of "survive and reproduce," the evolution-based objective of all living things including humans. Hamilton solved the problem of altruism by focusing on the *gene* rather than the *individual.* The honeybee's altruistic act could be optimal from the "gene's eye view": though the genetic code of the individual altruist is lost, even more of that same code, no longer imperiled, gets to prevail within the bee's rescued relatives. Richard Dawkins (1976) calls organisms 'survival machines,' *disposable* devices for protecting and disseminating *long-lived* genetic code.

Consider a hypothetical construct called a "helping gene," something that impels the individual to make sacrifices to help others. Hamilton asked: "What sort of helping genes might spread in the population?" Imagine, for instance, that my brother and I are soldiers, and that an enemy sniper has him in his sights. Suppose I could either cry out a warning to save him and draw the sniper's deadly fire toward myself, or I could remain silent. If I call out, I lose my own helping gene with certainty. What do I gain? Since my brother is a genetic relative, there is a 50-50 chance we share the same helping gene (that is, the 0.25 probability that we both inherit the gene from our mother plus the 0.25 probability we both inherit the gene from our father). Thus, in expected value terms, the benefit from calling out is half the value of my helping gene. From the "gene's eye view," then, the optimal policy is to remain silent. But suppose there were three brothers in the sniper's sights rather than one. Now there are net gains to being altruistic, since 1.5 helping genes (in expected value) are saved, a net gain of one-half. Thus, a gene that impelled an organism to issue a risky, even suicidal, warning cry could spread if such cries saved enough close relatives.

In more general terms, Hamilton's rule can be expressed as follows. Denote the *cost* of the altruistic act to the *donor* by *C,* and *benefits* of the act to the *recipients* by *B.* Let *r* denote the *coefficient of relatedness,* i.e., the chances that donor and recipient share the identical helping gene. Hamilton's rule stipulates that the donor provides help if

$$rB > C. \qquad (4)$$

In our example, *B* and *C* are counted in terms of lives saved. More generally, evolutionary biologists characterize these terms as *inclusive fitness,* which is defined as not only a person's own helping gene but the sum of any expected future progeny. Return to the warfare example

and imagine that I am, and will continue to be, childless, but that my brother has three children (for simplicity let's stop at the second generation). My brother's inclusive fitness is his helping gene plus the expected value of his helping gene in the children, or 1 + 0.5 + 0.5 + 0.5. The value of *rB* is 1.25, so Hamilton's rule predicts that I would sacrifice my life to preserve my brother's inclusive fitness. This example illustrates how Hamilton solved Darwin's dilemma of altruistic behavior among social insects, who are often sterile, and in fact Hamilton's rule quite accurately predicts the altruistic behavior of social insects as a function of their complex system of reproduction and relatedness (Trivers and Hare [1976]).

Sterility occurs in female humans as well, with the onset of menopause, and therein lies a prediction related to Hamilton's rule: the onset of menopause, all else equal, should spur increased altruistic behavior toward kin. Indeed, some behavioral ecologists have advanced the idea that menopause itself—rare among mammals—is an adaptation that encourages investment in young children (Hawkes, O'Connell and Blurton Jones [1997]).

Menopause is but one illustration of the built-in age-specific imbalances in altruistic sentiments that emanate from Hamilton's rule. Though relatedness members of our species is symmetric (*r* between grandmother and granddaughter is 0.25 from either's perspective) extended fitness is not, if the grandmother has passed her reproductive potential but the granddaughter has not. Family elders, therefore, would in general be expected to be more altruistic toward their younger kin than *vice versa*. Note that we have said nothing about income endowments; Hamilton's rule pertains to the sentiments embodied in the grandmother's utility function, not the money in her bank account. Another way to express this is that Hamilton's rule predicts that, between a granddaughter and grandmother, each of whom has

$100 to her name, one would expect it more likely for the grandmother to make transfers to the granddaughter than other way around.[15]

IV-B. Hamilton's Rule and Conflict in the Family

Hamilton's rule predicts several avenues for familial conflict: between parents and offspring, among siblings, between husbands and wives, and between in-laws. It is perhaps in this respect that the evolutionary approach differs most dramatically from the economic approach; until very recently, economists focused almost exclusively on Pareto Optimal solutions to economic problems in the family. For instance, Becker's (1974) "Rotten Kid" theorem implied that altruistic transfers from parent to child would obviate conflict, since no child would prefer to bite the hand that feeds him. Likewise, and as we saw in earlier sections, bargaining and collective models retain Pareto Optimal solutions.

In contrast, Trivers'(1974) model of parent-offspring conflict delineates conditions where a child might harm his mother, his siblings, or even himself to increase his share of parental transfers. Imagine a mother with two sons, Andy and Ben. Her relatedness to each is one-half, so if they are otherwise identical she would treat each equally according to Hamilton's rule. But neither son would be inclined to go along with this. While Andy's relatedness to Ben is one-half, his relatedness to *himself* is higher, namely, unity. Hence from Andy's perspective equal treatment does not go far enough; he would prefer to get more than Ben, and vice versa.

Sibling Rivalry—A Case Study

[15] Nor do such considerations of extended fitness always skew investments toward the youngest. A mother's altruism toward an unhealthy infant with slim chances of surviving to reproduce are predicted to be less than her altruism toward a healthy and mature child.

Consider the following East African case study of sibling rivalry and parent-offspring conflict from the 1950's. P. H. Gulliver (1961) studied the transition to cash farming among a group of subsistence farmers in Northern Tanzania (then Tanganyika). Traditional systems of inheritance were founded upon land abundance; a man's land typically was inherited by distant kin such as cousins or half brothers. Sons preferred to acquire land outside their natal village. But once land became scarce and valuable, inheritance laws quickly changed, so that land now passed from a father to his children (an outcome, by the way, predicted by Hamilton's rule, since parental altruism is stronger for sons than for more distant kin). The new system gave the eldest brother authority to allocate land between himself and among his younger siblings, with predictable results:

> At first, and as land grew more scarcer and more valuable, the eldest brother took the larger portion of the dead father's land, leaving his juniors to seek elsewhere as they could. But younger brothers quickly came to demand more nearly equal shares and a share for each, and in this they were supported by the local Nyakyusa courts. (Gulliver, p. 18)

Consistent with Trivers' hypothesis, the increase in land values fostered not only sibling rivalry but father-son conflict. Again, in Gulliver's words:

> A second locus of conflict is in the father-son relationship. Whereas formerly a son was not dependent on his father for agricultural or residential land (for he easily acquired land in the new village of his contemporaries), now he is primarily dependent upon his father. . . .[Sons] allege that a father expects too much work and subordination and gives too small shares in the joint enterprise. Fathers allege the exact reverse. (Gulliver, p. 19)

IV-C. Conflict between Fathers and Mothers

The male-female difference in reproductive biology—the enormous costs that reproduction imposes on a woman relative to a man, for instance—implies that mothers and fathers would disagree about quality/quantity tradeoffs in fertility: mothers favor quality; fathers, quantity.

Males and females differ in the size and number of sex cells (gametes) they produce. Indeed gamete size is what defines males and females. In humans, the former produce billions of abundant, cheap sperm (at the rate of about 3,000 per second!); the latter produce only 400 viable eggs in an entire lifetime. Female mammals invest more in offspring than do males, and this is especially true for humans. Owing to our extraordinarily large brains, childbirth is far more dangerous and painful for humans than for other primates. While a man can at least in principle "go forth and multiply," a woman can only "go forth and add."

Trivers (1972) was the first to argue that this sexual dimorphism in parental investment costs implies a conflict over quality/quantity tradeoffs between males and females. Women can advance their extended fitness by securing resources from their mates, friends and family for supporting their offspring. While men also have an interest in investing in their children, they can also advance their extended fitness by securing additional mates with which to have children. Total reproductive effort consists of investing in existing children and producing new ones (including effort to attract new mates). Men benefit more than women from the latter mode of investing.

Further, barring extreme events like maternity ward mishaps, a woman is always certain that her offspring is a biological relative, whereas, barring equally extraordinary circumstances a man might never be able to eradicate a small flicker of doubt concerning his child's relatedness. A straightforward adjustment of Hamilton's rule to reflect this uncertainty implies a lower value of paternal relative to maternal altruism.[16]

While prominent in biological analyses, these basic facts frequently get glossed over in economic models. While some early models of the economics of the family, notably Becker's

[16] For further discussion of theory and evidence pertaining to paternity uncertainty, see, e.g., Hrdy (1981) and Cox (2003).

(1981) analyses of the sexual division of labor, paid explicit attention to biological differences between men and women, later economic models of household behavior usually ascribed nothing special to being a father versus a mother; each may have well been "persons 1 and 2," and indeed are often referred to as such.

Such agnosticism about sex differences needlessly ties economists' hands, for each of these "biological basics"—sex differences in investment costs and paternity uncertainty—imply that mothers would be expected to behave more altruistically toward children than fathers. Indeed, this pattern has been found in dozens of studies of intra-household allocation (for instance see surveys by Strauss and Thomas (1995) and Haddad, Hoddinott and Alderman (1997)). What is rather astonishing is how the results are usually presented; there is generally little discussion about how it is always the mother who invests more. Instead, and in keeping with the standard "person 1—person 2" approach, economists merely note that the "preferences" of the spouses appear to "differ," and that the "unitary" model of household decision-making can be rejected. From a biological perspective, such verbiage is unduly circuitous, to say the least. But more important, economists could generate useful extensions of their approach to household bargaining by paying attention to biological basics, which relate the strength of mother-father conflict to things like cultural practices connected with paternity confidence, marriage and mating markets and a host of other variables pertinent to biological forces.

IV-D. Marriage and 'Mate Guarding'

A biologically based view of marriage differs markedly from most economics-based analyses, which emphasize gains from trade between husbands and wives, utility gains from pair bonding, the sharing of public goods, and the like. In contrast, and in raw form, the biological view is

that marriage is a system of "mate guarding" arranged by mutually suspicious spouses (especially husbands) and their relatives, to monitor the fidelity of each spouse (for a discussion of this view for various species, see, e.g., Birkhead [2000]). Since paternity is uncertain, husbands have an incentive to monitor their wives activities to insure that they are investing in children that are indeed biological relatives. In addition, since ovulation is hidden, husbands (so the theory goes) have to be more vigilant than, say, male chimpanzees, who are only interested in guarding females when they are in estrus, that is, the days when they display outward signs of being fertile.

Wives have an incentive to monitor husbands too, but for a different reason. Husbands who seek outside mating opportunities, perhaps producing offspring from them divert resources away from their spouse's offspring, toward people who are not their spouse's biological relatives. Further, the downside to the husband, in extended fitness terms, of his wife's infidelity generally exceeds the downside to the wife of her husband's infidelity. Cuckoldry, i.e., raising a child who one thinks is a biological relative but is really not, entails a potentially enormous waste of parental investment (again, from the strict perspective of extended fitness). In contrast, the fitness costs to a wife of her husband's philandering need not be so catastrophic. Hence, the infamous "double standard" pertaining to sexual fidelity that prevails in nearly all cultures, where female infidelity is punished more heavily and more strenuously guarded against than male infidelity. Such mate guarding takes the form of onerous restrictions in women's rights, sequestering, chaperoning, regulations on women's market work, and so forth. In extreme form, such guarding can be injurious to health and well being or even life threatening. For instance, female circumcision can be interpreted as an attempt to discourage female infidelity by reducing capacity for sexual pleasure, and domestic violence a weapon wielded by

husbands for controlling the social lives of their spouses. Thus mate guarding is implicated in extensive, worldwide public health problems. The World Health Organization (2000), for example, estimates that between 100 and 140 million women and girls in 28 countries have experienced some form of genital mutilation, including clitoridectomy (removal of the entire clitoris) and infibulation (sewing the vagina shut in order to insure virginity).

Evolutionary psychologists argue that jealousy is an emotion intimately related to mate guarding, and sex differences in mate guarding concerns have been found to play out with respect to corresponding differences in how jealousy is experienced. Buss, et. al., (1992) find that male jealousy tends to be triggered by the prospect of sexual infidelity on the part of their mate, whereas female jealousy tends to be ignited by emotional infidelity, that is, the prospect that their mate is cultivating serious romantic involvement elsewhere. This accords with sex differences in the costs of infidelity: while the worst-case scenario for the male is cuckoldry, the worst-case scenario for the female is desertion. Desertion is more costly, in extended fitness terms, than mere philandering because it presumably causes a larger reduction in paternal investments.

IV-E. In-Laws and Support for Grandchildren

Seldom do in-laws get mentioned in economic models of marital matching and gains from trade. Nor is there much concern about whether such matches occur ceremoniously or not. In contrast, a mate guarding perspective places in-laws and ceremony front and center. The public nature of marriage helps enlist extended kin, friends, and gossip networks of all description in the task of enforcing fidelity of the spouses. In all cultures, marriage is an exceedingly public event; elopement is generally quite rare.

One prediction about in-law altruism that emanates from considerations of mate guarding and paternity uncertainty is that relatives from the husband's side of the family might be expected to be more prone to condition their gifts and help upon their ability to monitor their child's spouse. Maternal grandmothers, for instance, are always certain that their grandchildren are biologically related to them, whereas paternal grandmothers might harbor some flicker of doubt. So financial transfers from maternal grandmothers might be less sensitive to her grandchild's geographic distance than financial transfers from paternal grandmothers, since paternal grandmothers who live close by would presumably face lower costs of monitoring their daughters-in-law.

Duflo (2003) finds empirical evidence consistent with differential altruism between maternal and paternal grandmothers in the context of an interesting natural experiment, South African pension reform. After the end of apartheid, in an attempt to address racial imbalances in pensions, the South African government increased cash transfers to the elderly (Case and Deaton, [1998]). Many South African households are multigenerational, with grandparents and grandchildren living under one roof. Duflo examined the impact of pension changes on nutrition indicators for grandchildren (weight for height and height for age) and found positive and significant effects in but one case—where grandchildren co-resided with their maternal grandmother.

Related evidence in a different context was found by Sear, et. al. (2002), who examined the relationship between the availability of kin and child mortality in rural Gambia. Among grandparents, only one—again, the maternal grandmother—stood out as significant for influencing child mortality. Indeed, the availability of the maternal grandmother was found to be more important for child survival that even the child's father, despite the fact that the villages

investigated were patrilocal. The absence of any kin from the father's side of the family did not matter for child mortality.

IV-F. The Trivers-Willard Hypothesis

Duflo's (2003) study of grandparental transfers and South African pension reform reports another intriguing demographic pattern, which is that grandmotherly largesse is directed at granddaughters not grandsons. Such a finding is arguably consistent with another biology-based theory of family behavior, the so-called Trivers-Willard hypothesis, named after Trivers and his mathematician co-author, Dan Willard, from a 1973 paper of theirs.

The Trivers-Willard hypothesis has to do with how parents might favor the production of, and investment in, sons versus daughters, and how such favoritism might vary with parental socioeconomic status. The argument goes like this: Imagine that (1) you were from the poorest family in your community and (2) you could only have one child and (3) you could choose the sex of that child and that (4) the marriage market in your community was somewhat polygynous. Finally, suppose you are just concerned about your extended fitness. Would you prefer a girl or a boy? If you had a boy, he might never have enough resources to attract a mate with whom to produce grandchildren. But even a daughter from a poor family would stand a good chance of reproducing, within either a monogamous or polygynous union. She might also stand a chance of advancing in socioeconomic status via marriage (so-called hypergamous behavior). Conversely, if you were from the richest family you would prefer a son since his wealth puts him in good stead to attract more than one mate, thus providing several high quality grandchildren by his many wives and concubines.

Though Trivers and Willard proposed their theory to explain sex ratios at birth, it can just as well be used to explain parental investments in children, a point made by Edlund (1999)

in one of the few papers in economics to make reference to the Trivers-Willard hypothesis. An anthropological study by Cronk (1989) supports the idea that low-status families bias their investments toward daughters. Cronk studied a small group of East African pastoralists, the Mukugodo of Kenya, who occupy the lowest reaches of the status hierarchy in the regional marriage market, a market which in turn is somewhat polygynous. The Mukugodo intermarry with their richer neighbors.

Cronk finds a pronounced pro-female bias in sex ratios at birth and among children aged 0-4; among the latter daughters outnumber sons 3 to 2. Moreover, daughters have higher reproductive success than sons; nearly all daughters reproduce, but many sons do not, and completed fertility is 25 percent higher for daughters compared to sons. Further, there is evidence of pro-daughter biased parental investments. Among children aged 0-4 taken to a nearby Catholic health clinic, Mukugodo daughters are over-represented relative to their proportion in the population (58 percent of the population but 64 percent of the visits). Among the non-Mukugodo children, the figures are reversed (daughters make up 49 percent of the population but only 45 percent of the visits to the clinic).

What about bias toward sons among the relatively wealthy? A case study from nineteenth-century northern India reported in Hrdy (1999) represents a possible example:

> Selective elimination of daughters first attracted attention in the West during the years of the British Raj. Nineteenth-century travelers visiting Rajasthan and Uttar Pradesh in northern India remarked on the rarity of seeing girls among any of the elite clans. It was assumed that as part of purdah the daughters of these proud descendants of warrior-kings were kept in seclusion. "I have been nearly four years in India and never beheld any women but those in attendance as servants in European families, the low caste wives of petty shopkeepers and [dancing] women," wrote Fanny Parks in her 1850 travelogue through northern India. It did not occur to the observer that *there were no daughters.*…Among the most elite clans such as the Jhareja Rajputs and the Bedi Sikhs—known locally as the "daughter destroyers"—censuses confirmed the near total absence of daughters; lesser elites killed only later born daughters. Overall, including

> lower-ranking clans who kept some or all daughters, sex ratios in the region were as high as 400 little boys surviving for every 100 girls. [p. 326]

Hrdy prefaces this account with an explanation along the lines of the Trivers-Willard hypothesis:

> In patriarchal social systems, a wealthy son finds himself in control of productive resources that women need. He will be in a position to attract multiple mates. In a stratified society such as Rajasthan's, families seeking social advancement compete among themselves to amass a dowry large enough to secure a place for their daughter in an elite household. This brings a prestigious alliance for parents along with the prospect of well-endowed grandsons. Should calamity strike, it is the only prospect for descendants surviving at all. Thus does son preference among elites lead to hypergamy, the custom by which women marry men of higher status. At the top of the hierarchy, however, hypergamy dooms daughters. There is no higher-ranking family for them to marry into. [p. 325]

We hasten to add that, first, there are several other (arguably dominant) factors that can lead to favoritism of sons over daughters (or vice versa), which have little to do with Trivers-Willard effects. On the first point, support from adult children is the predominant form of old-age support in the developing world (Nugent 1985) and for a farm family investment in sons may have higher returns than investment in daughters (see, e.g. Cain (1977)). In a completely different, but nonetheless related, vein, Oster's (2005) recent evidence on the relationship between the hepatitis B virus and male-biased sex ratios demonstrates that biological forces of a completely different sort can influence sex ratios.

Second, the Trivers-Willard hypothesis is not uncontroversial. For instance, Freese and Powell (1999) find little support for Trivers-Willard effects in data on parental investments in adolescents in the United States. Then again, the hypothesis is one of extremes, which is presumed to hold in the context of at least a somewhat polygynous marriage market, so that the setting examined by these authors might not be one in which we would be expected to find

much support. There is definitely more potential for exploring further the possibility of Trivers-Willard effects in developing countries.

Recently, Norberg (2004) found a slight but precisely measured difference in sex ratios at birth favoring the production of boys when the mother was living with a spouse or partner at time of conception or birth. This is consistent with the Trivers-Willard hypothesis since, all else equal, fathers' presence would be correlated with resources for investment in the child. But sex ratio at birth is only a small component of the Trivers-Willard hypothesis, since parents can and do make decisions about how much to invest in children once they are born.

Parents in some places nowadays can also practice sex-specific abortion, and infanticide and neglect were always available as a means to control the sex composition of families. The famous problem of the "100 million missing women," actively publicized by Amartya Sen, is evidence of the leeway that parents have for influencing sex ratios. While much of this bias is no doubt caused by preference for the old-age support that sons provide in patrilineal, agrarian societies, Sen himself has recently emphasized that sex preference is not always biased toward boys. He finds, for example, significant variation in sex ratios and sex-specific child mortality across individual Indian regions and states, variation that is, in his view, puzzling: "The pattern of contrast does not have any obvious economic explanation. The states with anti-female bias include the rich states. . . as well as poor states. . ." (Sen [2001], p. 40). But such a pattern could be conceivably be explained as an outcome of the countervailing forces of Trivers-Willard effects and the need for old-age support. But if Sen is indeed aware of the Trivers-Willard hypothesis, he makes no reference to it. Neither does Esther Duflo in her (2003) study discussed above. But favoritism toward girls could be interpreted in light of Trivers-Willard effects. From the perspective of poor families, the ending of Apartheid, and possibilities for

decreased social stratification could open up new opportunities for female hypergamy. There are only a couple of economic studies that refer to Trivers-Willard effects. One is a paper by Edlund (1999), which points out that such effects might generate pernicious long-run effects on the status of women, once household bargaining effects are taken into account. Assuming, as much evidence indicates, that a wife's power within marriage is influenced by her parent's wealth, a system whereby low-income families provide brides for the sons of high-income families will perpetuate low bargaining power of wives. If in addition such diminution of power limits a wife's ability to provision daughters (as, for example, the empirical work of Thomas (1994) appears to indicate) then Trivers-Willard effects could help perpetuate the low status of women.

The economic development literature has a long tradition of investigating the treatment of sons versus daughters in the family (for examples of careful and thorough reviews, see Behrman [1997] and Strauss and Thomas [1995]). But there is little work on how such favoritism could interact with the constellation of variables pertinent to the Trivers-Willard hypothesis, including familial socioeconomic status within the marriage market, the inherent polygynousness of that market, and sex-specific patterns in exogamy and inheritance of status.

IV-G. Evolutionary Perspectives on Interactions with Non-kin, Boundedness of Human Groups and Risk Sharing

As we have seen, when it comes to the analysis of *kin* relations, economics and evolutionary biology have often been like two ships passing in the night. Sadly, cross-fertilization of ideas has been lacking, to the detriment of economics of the family especially, and we hope that this chapter will help bridge the two disciplines. In contrast, however, when it comes to analysis of

non-kin relations there has already been profitable trade between the two fields, with biologists borrowing useful concepts from economics and vice versa.

Analyses of problems of cooperation between non-kin (or between, say, firms or nations) was already well underway in economics and political science before biologists broached the subject. Economists had been using insights from game theory—the prisoner's dilemma in particular—long before biologist Robert Trivers published his seminal work on reciprocal altruism in 1971. Trivers posed a question similar to Hamilton's (1964) query, but with a twist: "Can a gene that impels someone to assist a non-relative prevail under natural selection?" The answer, at least in principle, is of course a qualified "yes," as long as some form of fitness-enhancing payback is prompted by such altruistic acts. Ten years later, the interdisciplinary efforts of a biologist (Hamilton, again) and a political scientist (Robert Axelrod) produced a landmark study of the problem of cooperation among non-relatives using a repeated prisoner's dilemma framework (Axelrod and Hamilton, [1981]).

Biologist John Maynard Smith borrowed insights from game theory starting in the early 1970's (e.g., Maynard Smith [1974]), added to the theory, then economists, starting with Daniel Friedman (1991), began borrowing and adding to Maynard Smith's framework. The result of this cross-fertilization, evolutionary game theory has of course become a vibrant discipline all its own.

Early analyses of prisoner's dilemma games concentrated on individual choice of strategies, where homogenous players decided whether to cooperate with one another or not. An insight added by Maynard Smith was to imagine heterogeneous, fixed "types," say, "hawks" and "doves," who were born to defect or cooperate, respectively. Consider random pairings, where two doves enjoy the fruits of cooperation, two hawks muddle through with mutual

defection, and hawk-dove pairings generate plunder for hawks and crumbs for doves. Imagine too that hawks and doves leave descendants who tend to inherit their traits, and that the richer the parent, the more offspring it leaves.

Since defection is the dominant strategy in a prisoner's dilemma, hawks would eventually drive doves to extinction. But suppose there were some marking that honestly signaled whether someone was a hawk or dove. No sane dove would pair off with a hawk; they would seek out each other to enjoy the cooperative life. That would leave hawks the relatively meager rewards of mutual defection and eventually it would be hawks who would be driven extinct, leaving a society of doves living in cooperative peace.

Imagine, though, that one day a mutant appears who shatters the idyll: a hawk disguised a dove. He and his descendants would go marauding through the population of doves until no true dove were left—only hawks in dove's clothing, living the Hobbesian life of mutual defection. An alternative to this scenario, however, might be that, though every bird looks the same, for a price one could get a glimpse into its soul to verify whether it was truly hawk or dove. Such conditions could support a heterogeneous population of hawks and doves, with equilibrium proportions determined by the costs versus benefits of screening. To mix metaphors, this cat-and-mouse game involving signaling, screening, and concerns about cheating provided fertile ground for the work of evolutionary psychologists Leda Cosmedes and John Tooby, who argue that human mental modules have evolved with the express purpose of detecting cheaters and signaling cooperativeness (Cosmedes and Tooby, [1992]). Cosmedes and Tooby argue that the ubiquity of prisoners dilemma problems and the high stakes associated with success or failure with them would have led, over the many thousands of years of human evolution, to dedicated, and finely honed, cognitive tools dedicated to navigating the potentially

treacherous waters of social life. Their "mental module" approach can be likened to the dedicated language acquisition modules in the brains of toddlers. Linguist and evolutionary psychologist Steven Pinker argues that language is just too important an adaptation to be left to be learned from scratch; hard-wired language acquisition modules that facilitate the absorption of complex grammatical and syntactical processes give individuals an advantage for surviving and reproducing (Pinker, [1994]).

Likewise, Cosmedes and Tooby argue that similar mental modules exist for solving problems of social exchange, such as the detection of cheaters. Perhaps their best known experiment involves the effects of content on the ability to comprehend the nuances of logical problems. Their idea is that people are a lot smarter at solving problems expressed in the very concrete and pressing terms of detecting cheaters than they are at solving logically identical problems that are expressed without the cheater-detection backdrop. These results indicate, in their view, that though human minds are somewhat poorly equipped to handle abstract problems concerning necessary and sufficient conditions, they are in contrast naturally adept at solving problems concerning social contract.

Indeed, some evolutionary psychologists have advanced the hypothesis that possessing the cognitive wherewithal to succeed in the practice of social intrigue conferred distinct adaptive advantages and that intelligence and language are human adaptations for social exchange. This proposition is known as the 'Machiavellian intelligence' hypothesis (Humphrey, [1988]). Why might these ideas from evolutionary psychology matter for networks of mutual support? A key reason has to do with the subtleties of "cheater detection" modules. Presumably, since these adaptations are likely to have evolved in small groups, cues obtained from face-to-face contact are likely to have played a significant role in social exchange among non-kin. Casual

acquaintance "A" proposes a cooperative venture with non-relative "B." "B" listens and watches intently for cues connected with dishonesty: sweating, failure to maintain eye contact, dryness of mouth and hoarseness of voice, excessive blinking, etc. If detection of cheating matters, the formation of far-flung support networks with non-kin is predicted to be far dicier than the formation of support networks with kin (since the dictates of Hamilton's rule can at least partially facilitate the latter). Accordingly, we would expect that geographic propinquity (and perhaps middlemen) would play a more significant role in non-kin support networks. We would also expect to see a higher prevalence of non-kin support (relative to kin support) in places with higher population densities.

Human Groups for Risk Sharing and Production

A key function of cooperation among rural households is the sharing of idiosyncratic risks that can befall families. The response of private transfers to income fluctuations and calamities caused by things like droughts and pestilence has occupied much of the literature dealing with support networks. How large might we expect the typical risk-sharing network to be? Might there be limits on the size of networks? How might opportunities for increased division of labor in production affect risk-sharing networks? We argue below that evolutionary considerations can provide fresh insight into comparatively neglected problems in economic analyses of group behavior.

Much of the existing empirical literature on risk-sharing in economics pays little attention to the size of informal risk-sharing groups. For example, these groups have been alternately envisioned as: the extended family (Altonji, Hayashi and Kotlikoff [1992]); the village (Townsend [1994]); subsets of states in the United States (Asdrubali, Sorensen and

Yosha [1996]); the entire United States (Mace [1991]); even the whole world (Lewis [1996]). The lack of attention to group size in this literature stems from its emphasis on the complete set of possible means by which households deal with risk—not just the use of informal groups, but borrowing, drawing from savings, sales of durable goods, and so forth. The tide is beginning to change, however, and economists are beginning to give increased attention to inherent limits in network and group size (see, for example, Fafchamps and Quisumbing, this volume). Still, economists can avail themselves of useful evolutionary insights on the limitations of network size. We begin by first recognizing that many activities besides risk-sharing—including work, leisure, defense and governance—take place within groups. Second, we start with a motivating example of a natural experiment that illustrates how limitations on group size can conceivably constrain production.

One problem in determining the effectiveness of group size in production is that all we can usually observe are endogenously determined, equilibrium values. Lin's (1990) study of collectivization in China and agricultural output is less prone to this problem because group size was to a large extent exogenously determined. After the communist takeover in 1949, small, family-run farms were liberated from their corrupt landlords and family farm work was consolidated in various forms of cooperatives, where labor and other inputs were pooled among households. Cooperative schemes ran the gamut from the "mutual aid team"(4 or 5 households), the "elementary cooperative"(20-30 households), to the "advanced cooperative"(150-200 households). Collectives were allowed to coalesce voluntarily. Later on we will discuss the potential evolutionary significance of the maximum values 150-200.

Lin reports that the early stages of collectivization, from 1952 through 1958, saw a substantial gain—over 25 percent—in agricultural output. Further consolidation was mandated

by the Great Leap Forward, initiated in 1958, and the average commune size ballooned to 5,000 households, and agricultural output collapsed. The sharp reduction in productivity is consistent with a binding network constraint, in which the cohesiveness of the production group is destroyed.[17] On the risk-sharing front as well, too large a group can thwart the objective of harmonious consumption from a common pot. Witness the failed utopian societies of the nineteenth century, or the spate of defunct hippie communes from the 1960's. Usually, effective risk-sharing requires a small group. For example, Lomnitz's ethnography of reciprocal networks in a Mexico City shantytown indicates a maximum size of 6 households, with an average size of 3.65 families per network. In their study of risk-sharing in the Philippines, Lund and Fafchamps (2003) find that mutual insurance, primarily provided in the form of informal loans between households, takes place not at the village level, but instead among much smaller groups of friends and relatives. The costs of maintaining group cohesiveness is likely to increase with the size of the group. In the realm of both risk-sharing and teamwork, groups can be beset with the problem of free-riding. What can be done to mitigate the problem? There are basically three options: (1) the group can try to screen out those likely to cheat, (2) it can attempt to alter individual preferences to make them less prone to moral hazard, or (3) the group can invent incentives and systems of monitoring that make cheating less likely. Economists have devoted the most attention to the third option. For example, Kimball (1988) and Coate and Ravallion (1993) investigate trigger strategies that can help keep reciprocal relationships together. Coate and Ravallion consider an infinite horizon, repeated, non-cooperative game in

[17]Lin argues that the unwieldy size of the communes was not the root cause of the output collapse, however. He points to a rule change implemented during the Great Leap Forward, which eradicated previous rights to withdraw from a commune. Lin argues that this rule change ruined work incentives, and points to evidence that agricultural productivity did not recover once communes returned to their smaller size but retained their compulsory membership rules.

which two individuals attempt to insure one another from random shocks to income. As discussed in earlier sections of this Chapter, they emphasize the implementability constraint—a condition that insures that utility from immediate defection is always less than utility from continued cooperation. The prediction from the Coate and Ravallion model is that mutual aid will only be responsive to income shortfalls up to a point, since the requirements of extremely large contributions would violate the implementability constraint. As a result, private transfers follow a non-linear relationship with the earnings of the potential recipient. They are at first responsive to income shortfalls, then flatten out.

Kimball limits his investigation to full, rather than partial, risk-sharing, but considers the possibility of more than just two risk-sharers and the implications of increased group size. The larger the risk-sharing group, the bigger the gains from cooperation and the larger the penalties from defection, attributes which favor big groups. But once formed, larger groups are harder to maintain, since defection would be relatively attractive for those with lucky group members, who would otherwise have to share their windfalls with too many others. Fafchamps (1992) presents a detailed treatment of a variety of features of mutual insurance systems in pre-industrial society in a unified framework that emphasizes findings form the theory of repeated games. He and others have also applied game theoretic considerations to incentive problems in work teams. For example, a partnership in which output is split among n workers would be expected to be beset with free-rider problems, since each worker would reap only one-of the fruits of his or her efforts. Fafchamps shows how subsistence insurance can generate better incentives than full income pooling. Repeated games can lead to self-enforcing agreements and help overcome the moral hazard problem (Telser [1980], Radner [1986]). Becker (1992) has recently argued that the problems of commitment emphasized in game-theoretic approaches to

strategic interactions, such as those described above, are exaggerated because they ignore the possibility that habits of commitment and loyalty can be deliberately inculcated. If we recognize the prospect that risk-sharers and teammates can engage in "bonding," that is, activity which enhances the functionality and cohesiveness of a small group, then the relationship between group size constraints and functionality becomes more transparent. Rotemberg (1994) pursues the idea that bonding can affect performance in the workplace. He cites evidence from the "Hawthorne experiments," a classic study in organizational behavior from the 1930's, which investigated worker behavior in Western Electric's Hawthorne plant. In one group of experiments, increased time for worker socializing was linked to increased productivity. Increased friendliness among workers was cited as the reason for the increased output. If there are costs to cultivating feelings of altruism toward an individual, as envisioned by Mulligan (1997) and Rotemberg, then the costs of developing a cohesive group will increase with its size.[18] Platteau (1991) cites a different example of bonding, in the context of risk-sharing, among the Kung San, hunter-gatherers who live in the Kalahari. The Kung San practice hxaro, a system of hunger insurance that is characterized by sharing with far-flung kin, both fictive and real. The initiation of a hxaro relationship is highly ritualized and time-consuming, involving a staggered gift exchange between two persons for a year or longer. The ceremonial gifts are intended to inculcate bonds of friendship. Stack's (1970) ethnography of low-income Blacks living outside Chicago documents the same principle, called "swapping": Since an object is offered with the intent of obligating the receiver over a period of time, two individuals rarely simultaneously exchange things. Little or no premium is placed upon immediate compensation;

[18] A countervailing argument is advanced by Kandel and Lazear (1992), however. They argue that peer pressure might be more effective in larger groups, because shirking can potentially arouse the ire of more persons. They do acknowledge though, that after some point increases in group size would undermine the quality of interpersonal relationships and the strength of peer pressure.

time has to pass before a counter-gift or a series of gifts can be repaid. While waiting for repayments, participants in exchange are compelled to trust one another."[p. 41] Similar patterns have been documented in Lomnitz's (1977) study of networks in a Mexican shantytown and in Mauss' (1990 [1950]) comparative study of gift giving and exchange.

The Boundedness of Groups—Evolutionary Considerations

There are many considerations that can lead to the boundedness of groups that is mostly ignored in the economics literature is cognitive limitations. Introduce more characters into the story, and the plot thickens, until it becomes impossibly Byzantine, at which point cognitive overload sets in. A corollary of the Machiavellian-intelligence hypothesis discussed earlier is that larger groups are more intellectually demanding, and limitations in intelligence would imply corresponding limitations on group size.

A detailed treatment of the Machiavellian-intelligence hypothesis was recently proposed by Robin Dunbar, a psychologist who studies primate and human behavior. Dunbar (1993, 1996) argues that a species' optimal group size is determined by exogenous factors—such as advantages in defense against predators, or increased feeding efficiency given the distribution of food—but once nature chooses this size, behaviors and capacities evolve to enable individuals to function within, and maintain, the cohesion of the group. He argues that cognitive limitations deter-mined by the size of a species' neocortex (roughly speaking, the problem-solving part of the brain) set effective limits on maximum group size. Groups beyond a certain size would tax an animal's cognitive capabilities. Group living entails soap-opera-like intrigue with sometimes nasty consequences; larger groups could entail increased harassment from other group members. As a result, it behooves individuals to forge a set of close alliances, in the form

of a "primary network" containing a few close friends. In primate societies, these friendships are maintained by social grooming.

Members of primate societies spend a great deal of time grooming, i.e., removing articles such as parasites and burrs from one another's fur. Grooming is hygienic, pleasurable, and time-consuming, taking up to 20 percent of waking hours in some species. Most importantly, grooming is also conducive to the formation of bonds. There is evidence that grooming is related, in many primate species, to the formation of alliances (Dunbar [1996], Walters and Seyfarth [1986]). For example, one of the studies reviewed in Walters and Seyfarth indicates that, among the vervet monkeys of Kenya, if an animal hears a cry from someone it has recently groomed it is more likely to go to its aid. Grooming signals commitment, and may function like an "entrance fee," much in the same way that Carmichael and MacLeod (1997) characterize a gift given at the beginning of a relationship. They argue that such a system can prevent problems of free-riding in relationships of mutual assistance by raising the price of defection, since the defector must give another gift to initiate a new relationship. Dunbar (1996) also notes that grooming builds trust, since the "groomee" is in a vulnerable state during the process. What about humans? Dunbar argues that human groups are too large for relationships to be maintained by grooming, since time requirements would be too demanding. He speculates that time spent in conversation is analogous to grooming, and more efficient as well, since one speaker can simultaneously "groom" more than one person. But conversation too is subject to cognitive limits. For example, a conversational clique tends to splinter when it grows beyond four participants, and this occurs for psychophysical reasons (Dunbar [1993]). Dunbar uses the estimated relationship between the relative size of the neocortex and group size to predict a maximum human group size of about 150, and gathers outside evidence that appears to support

this figure as an upper bound on the size of a group that can be sustained by close contact. Dunbar cites several examples of groups that hover around 150: (1) Brigham Young divided his group of 5,000 into subgroups of 150 during their trek from Illinois to Salt Lake City; (2) the Hutterites, a fundamentalist farming community, deliberately subdivides when group size reaches 150; (3) the maximum size of military fighting units ranges from 100 to 225; (4) guidelines on maximum church congregation promulgated by the Church of England stipulate a range of 150-200.

The constraint on group size is likely to vary depending on function. For example, Mancur Olsen (1965) cites evidence that the optimal size of "action" groups (i.e., those which must produce output) is about 7, half that of "non-action" groups (e.g., an advisory committee). Christian Buys and Kenneth Larson (1979) attempted to estimate the size of a "sympathy group" by asking survey respondents to list the number of persons whose death would cause them anguish or the number with whom they have close emotional ties, and found a mean sympathy group size of 11 with a standard deviation of about 7. On the other hand, if all that is needed is for group members to be able to attach a name to a face, the size can be much larger.

Group Size Constraints Can Create Tradeoffs between Risk-Sharing and Specialization in Production

Carol Stack's (1970) ethnography of low-income Black households from a community just outside of Chicago documents the demands that risk-sharing networks make on their participants, increasing the costs of participating in other social spheres, such as work life. Resources devoted to the maintenance of ties within the sharing network leave little room for relationships outside of the network, making it difficult for network members to straddle the demands of the network and those of a life outside the network:

> Marriage and its accompanying expectations of a home, a job, and a family built around the husband and wife have come to stand for an individual's desire to break out of poverty. It implies the willingness of an individual to remove himself from the daily obligations of his kin network. People in The Flats recognize that one cannot simultaneously meet kin obligations and the expectations of a spouse. [Stack, p. 113]

Horne (1918) and Jevons (1918) discuss the difficulty that family networks posed for Indian industrial development in the early twentieth century. Horne notes that a leading cause of labor scarcity in urban jute mills was the return of workers to their homes to look after their "domestic affairs."

In a completely different context, a recent study by Berman (2000) documents the tremendous influence of participation in religious schooling (Yeshiva) among Ultra-Orthodox Jews in Israel. Ultra-Orthodox communities practice mutual insurance to an extreme degree. They also have pathologically low rates of rates of labor force participation of prime-aged males, which is indicative of a tradeoff between risk-sharing and production. Berman argues that the poverty is a sacrifice used to insure that those with insufficient commitment to the religious community are screened out, following the logic of a recent model of religious behavior proposed by Iannacone (1992). This example highlights the potential tradeoff between the size of a production team and the size of a risk-sharing clan.

Considerations of limitations of group size and possible tradeoffs between producing and risk-sharing have novel implications for the impact of public income distribution on productivity. The standard argument is that public safety nets are antithetical to productivity, since, for example, income guarantees can sap incentives to work. In contrast, the considerations of group size above suggest that these safety nets, by obviating the need to form risk-sharing networks, can allow people to concentrate their limited group management resources on the problem of team production. If the production technology exhibits increasing returns in the number of

workers, and public safety nets make it possible to field larger work teams, production and incomes rise. Group size limitations could play a significant role in the transition from agriculture to manufacturing. The number of workers per establishment is an order of magnitude larger in manufacturing than in either agriculture or services. Seen in this light, public safety nets might help facilitate industrialization. For example, Mokyr (1985) conjectures that the early existence of public safety nets in England may have contributed to its industrialization:

> Indeed, it could be maintained that the Poor Laws, despite their obvious flaws (in particular their non-uniformity), may have had some overall positive effects on the Industrial Revolution. A comparison of Ireland, which had no formal system of poor relief prior to 1838, bears this out. . . . The social safety net provided by the Poor Laws allowed English individuals to take risks that would have been imprudent in Ireland where starvation was still very much a possibility. In societies without such laws, self-insurance in the form of large families and liquid assets were widely held. [p. 14]

The approach also provides an explanation for the policy focus on state-provided redistribution during the process of rapid transition from agriculture to manufacturing, as that which occurred in the Soviet Union during the middle part of the twentieth century. The state usurps the duties of the clan, so that limited capacity for group formation can be concentrated within the realm of the work team.

Group Size Constraints and Group Lending

Group lending schemes, such as the Grameen Bank, could be modeled in a similar manner to that of risk-sharing. The key idea is that the emotional and intellectual resources necessary to sustain a viable group loan compete with other activities, such as production, which also require these resources. One puzzle in the literature is the nearly exclusive targeting of group lending to women. Typical explanations are usually concerned with the incidence of poverty and liquidity

constraints, but these indicators cannot explain the pronounced gender divide in group lending that is usually observed. For example, 94 percent of Grameen Bank borrowers are women (Pitt and Khandker [1998]). Group lending started in rural Bangladesh, where female work for wages is rare and women tend to be secluded in accordance with Islamic law. Such seclusion could contribute the success of group lending since in such isolated settings constraints on network size are unlikely to be binding.

V. Conclusion

A survey is supposed to take stock of a literature and point out fruitful future directions. All along in writing this Chapter we have assumed (perhaps pretended is a better word) that our reader is a novice in the sub-discipline—a graduate student, perhaps, or someone who has just switched into the field. What would we recommend to such a person in order to make the most of his or her research efforts? We will now go out on a limb and attempt to give some advice to such readers, with the proviso that all research prospects are at least somewhat risky, and that our advice may not be suitable for all and accordingly that other opinions should be sought out.

With those caveats in mind, we think that research on income effects in inter-household transfers is beginning to hit sharply diminishing returns. The specter of complete crowding out, which galvanized the empirical literature during the past few decades, appears to be fading as a policy concern and an intellectual problem. Not that private behavioral responses of transfer networks can be safely ignored by policymakers; far from it. It is just that the marginal value of an additional case study of income effects from a standard data set, such as the LSMS, is likely to be relatively low.

Instead, the current focus on income effects should give way to an intensified scrutiny of

all of the other variables that researchers typically consider—but rarely think very hard about—in studies of inter-household transfers, particularly demographic effects. For instance, we need to understand better why it is that in some countries intergenerational transfers are used primarily for old-age support, whereas in others, they are targeted primarily to younger households. The question matters, for example, for reasons of economic growth: the more resources are directed toward the young, in the form of human capital investments, the better are the prospects for growth.

We also need to have a better understanding of gender differences in kinship relations and support. Too often economic models are gender blind, populated with generic parents and children and "spouses 1 and 2" rather than husbands, wives, fathers, mothers, sons and daughters. This modeling choice is in part a legacy of the nature of economics, which has little to say about gender in and of itself—such as the nature of motherhood versus fatherhood. But as we argue in section IV, evolutionary biology *does* have a lot to say about these things, and that combining insights from that discipline, in order to refine our notions of familial utility functions, could open new doors for understanding demographic influences in inter-household transfers.

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Country and Segment of Population	Year	GDP per capita (2000 USD)	Percentage of Households		Average Transfer Amount as a percentage of Average Income		Transfer Responsiveness to Income	Source
			Receiving	Giving	Receiving	Giving		
Albania	1996	983	8.8	2.5	70.4*	15.4*	-	Cox, Galasso & Jimenez, 2006
Bangladesh	1998/99	346*					-	McKernan, Pitt, Moskowitz, 2005
Gifts			14.6	1.7	48.3 *	-		
Informal loans			12.7	5.0	57.6 *	-		
Bulgaria	1995	1567	16.3	14.4	20.9*	19.0	-	Cox, Galasso & Jimenez, 2006
China *(rural hh cross-China)*	1988	347	29.9	-	9.3	-	0.011*	Secondi, 1997
Columbia *(Cartagena's poor)*	?		46	52	9.1	6.3	-	Cox and Jimenez 1998
El Salvador (*remittances)*	1997	2028						Cox Edwards & Ureta, 2003
Rural			14		49*	-	-	
Urban			15		37*	-	-	
Ghana (*Rural and semi-urban informal borrowing)*	1988/89	212*	32	33	4.4 *	-	-	LaFerrara, 2003
India *(Rural informal borrowing in Northern Uttar Pradesh)*	1981-1982	234*	17	-	-	-	-	Kochar, 1997
Jamaica	1997	3140	52.9	17.5	17.7*	11.1*	-	Cox, Galasso & Jimenez, 2006
Kazakhstan	1996	1021	27.2	20.3	29.9*	20.2*	-	Cox, Galasso & Jimenez, 2006
Kyrgyz Republic	1996	240	33.2	15.6	32.2*	23.3*	-	Cox, Galasso & Jimenez, 2006
Malaysia *(Parent/child transfers)*	1988	2230					-	Lillard, Willis 1997
Parents			61.5	23.6	7.8*	-	-	
Children			18.5	54.3	-	9.2*		
Nepal	1996	218	23.4	10.1	38.2*	17.6*	-	Cox, Galasso & Jimenez, 2006
Nicaragua	1998	736	20.3	1.1	29.7	7.8	-	Cox, Galasso & Jimenez, 2006
Nigeria	1988-1989	320*	65	75	8.5*	-	-	Udry, 1994

Country and Segment of Population	Year	GDP per capita (2000 USD)	Percentage of Households		Average Transfer Amount as a percentage of Average Income		Transfer Responsiveness to Income	Source
			Receiving	Giving	Receiving	Giving		
(Rural villages informal borrowing)								
Panama	1997	3726	38.2	17.1	9.8*	4.5*		Cox, Galasso & Jimenez, 2006
Peru	1985-1986	2188*	25	-	4	-	Low inc 0.140* High inc -0.013	Cox, Eser and Jimenez 1997
Peru	1994	1852	35.4	13.5	14.1*	8.5*		Cox, Galasso & Jimenez, 2006
Philippines – (*remittances)*	1993	877	17	-	8*	-	-	Rodriguez, 1996
Philippines – *(Rural villages)*	1994-1995	916*	100	100	23.4*	11.1*	-	Fafchamps & Lund, 2003
Philippines	1988	882						Cox, Hansen, Jimenez 2004
Rural			89	50	13.0	1.1	Low inc. -0.4* High inc -0.03	
Urban			82	44	14.3	0.8	Low inc –0.39 High inc –0.01	
Poland	1987	3053*	49	29	9.4	2.7	-0.054 *	Cox, Jimenez and Okrasa, 1997
(Worker households)	1992	2894	53	28	4.2	2.8	-0.031	
Romania *(gifts, loans, other informal transactions)*	2003	1963	37.2 *	59.6 *	8.5	12.4	3.7 e(-8)*	Amelina, Chiribuca, Knack, 2004
Russia *	1994-2000	1591*					-0.1 (elderly hh only) *	Kuhn and Stillman 2004
Rural			18	22	10*	9*		
Urban			25	24	9	6		
Russian Federation	1996	1564	24.4	23.4	40.9*	30.3*	-	Cox, Galasso & Jimenez, 2006
South Africa*	1994	2846	21.9	3.5	-	-	Earned Income: Above poverty level: 0.00 Below poverty level: -0.07* Public Pensions: Above poverty level: 0.04 Below poverty	Maitra & Ray, 2003

Country and Segment of Population	Year	GDP per capita (2000 USD)	Percentage of Households		Average Transfer Amount as a percentage of Average Income		Transfer Responsiveness to Income	Source
			Receiving	Giving	Receiving	Giving		
							level: -0.09*	
South Africa *Remittances going to pensioners in Venda province (low income)*	1989 1992	3131 2842	68 70	- -	25 12	- -	Women: -0.30 Men: -0.26 (responsiveness of remittances to pension increase between 89 & 92)*	Jensen, 2004
Taiwan *Exchanges of support between sons, daughters and parents>50*	1989	-	Sons: 14 Daughters: 21	Sons: 79 Daughters: 70	- -	- -		Lee, Parish, Willis, 1994
Thailand (*remittances)*	1988	1185	21.6	15.6	33.2*	16.4*	-	Miller & Paulson, 1999
United States	1988	27362	20.2	13.3	5.7	6.2	-.013*	Schoeni, 1997
United States *Mexican Americans* *Mexican Immigrants (support to/from relatives only)*	1989	28062	 4.8 3.1	 10.1-13.3 14.5-24.7	 - -	 - -	 - -	Glick, 1999
Vietnam	1997-98	364*	25.9	18.9	31.6*	14.7*	-	Cox, Galasso & Jimenez, 2006

- Percentage of households receiving or giving transfers in the past year, unless otherwise noted

- Average transfer amounts as percentage of average income are calculated as average transfer amount for those who are recipients/donors divided by average post-transfer income for total sample, unless otherwise noted.

- Transfer Responsiveness to Income: Answers the question, if income increases by 1 unit, by how many units do private transfers increase or decrease? See country notes to determine if TRI calculated over entire sample or for recipients only

- GDP per capita from World Bank World Development Indicators, 2000 USD.

Albania

*May include loans as loans are not asked about explicitly
*Average transfer amounts as percentage of average income are calculated as noted above for **net** recipients and **net** givers only.

Bangladesh
*GDP per capita from 1999
*Average of gift or loan as percentage of total household income is calculated using average post-transfer household income for recipients of gifts and recipients of loans, not average income overall. From Table 7.

Bulgaria
*May include loans as loans are not asked about explicitly
*Average transfer amounts as percentage of average income are calculated as noted above for **net** recipients and **net** givers only.

China
*TRI calculated by OLS for those families reporting a transfer. Includes transfers from families and gifts. Transfers from family members alone has a responsiveness of .033 (t-value 2.319).

El Salvador
*Transfer amount as percentage of income uses average **pre-remittance** household income.

Ghana
*GDP per capita from 1989
*Informal loan amount as percentage of average household **expenditure**, not income.

India
*GDP per capita from 1982

Jamaica
*Average transfer amounts as percentage of average income are calculated as noted above for **net** recipients and **net** givers only.

Kazakhstan
*May include loans as loans are not asked about explicitly

*Average transfer amounts as percentage of average income are calculated as noted above for **net** recipients and **net** givers only.

Kyrgyz Republic

*Average transfer amounts as percentage of average income are calculated as noted above for **net** recipients and **net** givers only.

Malaysia

*Receiving figure is mean percentage of parents' hh post-transfer income received as a gift from children, for **entire** sample of eligible parents(not just recipients).

*Giving figure is percentage of child's hh income given to parents when both husbands' and wives' parents are alive, for the **entire** sample of eligible children (not just donors).

Nepal

*Average transfer amounts as percentage of average income are calculated as noted above for **net** recipients and **net** givers only.

Nicaragua

*Average transfer amounts as percentage of average income are calculated as noted above for **net** recipients and **net** givers only.

Nigeria

*GDP per capita from 1989

*Amount borrowed reported as percentage of household WEALTH, not income (income not available)

Panama

*May include loans as loans are not asked about explicitly

*Average transfer amounts as percentage of average income are calculated as noted above for **net** recipients and **net** givers only.

Peru (Cox, Eser and Jimenez, 1997)

*GDP per capita from 1986

*TRI based on splines approach dividing sample into low income and high income households (Table 1 p. 69)

Peru (Cox, Galasso and Jimenez, 2006)

*Average transfer amounts as percentage of average income are calculated as noted above for **net** recipients and **net** givers only.

Philippines (Rodriguez, 1996)
*Represents 8% of Philippines household income calculated on a national, not individual household, basis

Philippines (Fafchamps & Lund, 2003)
*GDP per capita from 1995
*Transfers include gifts only (not gifts and loans) and the percentage is calculated as the percentage of **pre-transfer** income. Percentage of households receiving/giving includes gifts and loans.

Philippines (Cox, Hansen & Jimenez, 2004)
*TRI based on splines estimation with estimated cutoff points

Poland
*GDP per capita from 1990 (1987 not available)
*TRI is calculated at sample means (see pg. 203)

Romania
*Percentages are of **net** recipients and donors, not gross
*TRI based on tobit analysis reported in Table B1, evaluated at the mean.

Russia
*All values are for the month prior to the interview
*GDP per capita for 1997
*Transfers as percentage of average income are overall average transfer/overall average income for each urban/rural category (Table 1 p.140)
*TRI results from OLS regression which only includes income from elder pensions, so responsiveness of transfers to income only given for single-generation elderly households (Table 5 p. 152)

Russian Federation
*May include loans as loans are not asked about explicitly

*Average transfer amounts as percentage of average income are calculated as noted above for **net** recipients and **net** givers only.

South Africa (Maitra & Ray)

*South African time period covers last 30 days, not last year

*TRI results from 3SLS regressions

South Africa (Jensen)

* TRI figured in a differences in differences estimation, where men and women were estimated separately – from pages 104 and 105

Thailand

* Transfers (Remittances) as percentage of income recorded for previous month, not previous year

United States

*TRI from Table 6 – tobit estimates of transfers received.

United States (Mexican American and Mexican Immigrant populations)

*The ranges for giving are based on the numbers reporting giving to different categories of relatives, from Tables 4 and 5

Vietnam

*GDP per capita from 1998

*Average transfer amounts as percentage of average income are calculated as noted above for **net** recipients and **net** givers only.