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Abstract

For the past two decades, studies measuring social preferences in developing settings have played an important role in building our understanding of economic development and poverty. This book chapter reviews lab-in-the-field experiments that measure social preferences, summarizes categories of social preferences, the standard experimental games that have been developed to test them, and why they are of interest to development economists. We describe experimental methodology adapted for developing contexts, give an overview of some recent advances in measuring social preferences in developing settings, we comment on the external validity of standard experimental games, and discuss unincentivized measures of social preferences. Finally, we review studies that explain variations in social preferences between and within individuals, with a focus on environmental factors. We comment on possible paths forward.

JEL-Codes: A330, B410, C900, D900, O120.

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Measuring Social Preferences in Developing Economies

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1 Introduction

For the past two decades, studies measuring social preferences in developing settings have played an important role in building our understanding of economic development and poverty. Social preferences are of particular interest to development economists, since communities in poor countries often have weak institutions and populations who live in small-scale communities that typically rely on social capital to enforce contracts and foster cooperation to a greater extent than in the developed world. Just as innovations in randomized control trials have informed provided evidence-based assessments of development policy, behavioural experiments have supplemented our knowledge of the poverty alleviation programs and other policy interventions.

Studies in the developing world have also brought unique insight into our understanding of social preferences in general. Environments where group identity, traditional power structures, informal punishment and local social networks play a major role in day-to-day life provide a rich setting to more cleanly test a range of theories that have universal applications.

The majority of experiments studying social preferences have been conducted with the standard subject pool of university students in Western countries.¹ Cross country studies that include subjects from developing countries have illuminated the diversity of social preferences and hinted at how the social and institutional environment may shape preferences.

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¹Henrich et al. (2010b) describes this group as "WEIRD": Western, Educated, Industrialized, Rich and Democratic.

The dominant paradigm for measuring social preferences in developing countries has been to adapt the protocols of lab experiments for use in the field with non-standard subject pools, for example, making them portable and easily understandable to non-literate subjects.²

Over the past two decades of behavioural experiments in the developing world, a set of best practices has emerged. Cardenas and Carpenter (2008) comprehensively review early lab-in-the-field experiments in development economics, including best practices and practical advice. In this chapter, we review and update these, adding new advances.³ In recent years, the field has come into its own, adopting a number of innovative techniques that would not be possible in university labs.

In this chapter, we begin with an overview of lab-in-the-field experiments that measure social preferences in Section 2. We summarize categories of social preferences, the standard experimental games that have been developed to test them, and why they are of interest to development economists. Next, in Section 3 we describe experimental methodology adapted for developing contexts. In Section 4, we give an overview of some recent advances in measuring social preferences in developing settings, such as field experiments and we comment on the external validity of standard experimental games. In Section 5, we discuss unincentivized measures of social preferences. While surveys were the first instruments used to measure social preferences, they were largely replaced by incentivized experiments. In recent years, however, they have made something of a comeback. In Section 6, we review studies that explain variations in social preferences between and within individuals, with a focus on environmental factors that are particularly relevant in the developing world. In Section 7, we conclude and comment on the way forward.

2 Measuring social preferences with behavioral games

The workhorse method of measuring social preferences in the field is a suite of incentivized behavioral games, including the dictator, ultimatum, third-party punishment, trust, and public goods games. These games were developed for use in the lab with university students, but have since been used with populations around the world. They can be combined or used individually, to separate and refine measurements of a particular social preference. In this section we review key social preferences and

²A note on terminology: we use lab-in-the-field to denote experiments using similar methods as lab experiments, but adapted to non-standard subject pools, conducted outside of university settings. This is similar to the term "artefactual field experiment" used by Harrison and List (2004).

³Kremer et al. (2019) also summarize recent advances in studying social preferences in development economics. Our study mainly focuses on measurement issues including applications as examples, they focus exclusively on applications. Readers interested in topics of moral attitudes and culture should refer to their study. We omit them for the sake of space.

associated behavioral games, with a focus on how each has been adapted to study topics related to economic development.

2.1 Altruism

Altruism, or a preference for helping others at a cost to oneself⁴ is most closely associated with the dictator game (DG) (Kahneman et al., 1986; Forsythe et al., 1994): the *dictator* is given an endowment, then chooses whether to transfer any portion of this to a passive *receiver*. In a typical DG, only the dictator is endowed, though in some experiments the receiver also begins with an endowment (e.g., List, 2007).⁵ Since there is no strategic motivation in the DG, the amount given by the dictator is interpreted as altruism. However, other motivations like inequality aversion and norm compliance might play a role as well. While selfish subjects should send nothing, numerous DG experiments have shown that the majority of subjects do send positive amounts (Engel, 2011).

Altruism is of particular interest in developing countries with weak institutions, as transfers to those who experience negative shocks can help mitigate risk in the absence of formal insurance. Evidence of this comes from a recent meta-study, which finds macro-economic development is negatively correlated with dictator-game offers (Cochard et al., 2021).⁶

Additionally, with high rates of poverty and low levels of publicly provided social services, interpersonal transfers can be crucial in alleviating poverty. And when public service jobs are paid below market rate, altruism is an important predictor of who self-selects into the sector (Lagarde and Blaauw, 2014; Banuri and Keefer, 2016).

2.2 Inequality aversion

The dominant framework for understanding inequality aversion is the Fehr and Schmidt (1999) model. The model posits that utility is decreasing in proportion to both advantageous inequality (in which the individual has more than someone else) as well as disadvantageous inequality (someone else has more than the individual). Aversion to the latter is assumed to be more extreme.

⁴Bowles and Gintis (2011), define altruism as an action in which helps one individual while "the helper would benefit in fitness or other material ways by withholding help (p.8)." Altruism might be motivated by "warm glow" Andreoni (1990), in which the helper gets utility from the act of giving, as well as distributional preferences, in which there are direct utility gains from others' consumption.

⁵For example, to measure inequality aversion as in Fehr et al. (2008).

⁶Since studies aggregated in Engel (2011) and similar meta-studies used different methodologies, it is difficult to make comparisons. E.g., Falk et al. (2018) show in a large representative sample survey that controlling for patience, social preference measures are no longer correlated with economic development.

The game most closely associated with measuring aversion to inequality is the ultimatum game (UG) (Güth et al., 1982).⁷ The *proposer* decides how much of an endowment to send to a *responder* (not endowed). The *responder* observes the proposed allocation and decides whether to accept the offer, in which case the sender's proposed allocation is realized. If the offer is rejected, both parties end the game with nothing. A responder who is purely interested in material gain will accept any positive offer. Anticipating this, the equilibrium prediction for a selfish proposer is to offer the smallest possible amount. However, this outcome is rare in practice, with most proposers making higher offers. Many responders reject low offers, which is evidence of aversion to (disadvantageous) inequality. This decision can be interpreted as paying a cost to punish the proposer.

A proposer who offers more than the minimum amount in the UG might do so out of self interest, anticipating that low offers will be rejected. In addition to this, they might be motivated by their own aversion to advantageous inequality or altruism.

In some versions, the *responder* is asked to make series of decisions conditional on a range of possible sender's actions without knowing, *ex ante*, which action the sender selected. This is called the strategy method (see Section 3.2.1). The threshold below which receivers reject offers is a measure of inequality aversion.

Measuring inequality aversion with UGs is of interest to the study of economic development and poverty. First, market integration and UG offers are positively correlated, which may imply that the former involves bargaining on a regular basis, and thus a heightened concern for fairness (Henrich et al., 2010b). Despite this, Cochard et al. (2021) find that, in contrast to DG offers, UG offers do not vary with the level of economic development. Secondly, the UG is related to redistribution policies, which can affect institutional capacity and the provision of public services.

DGs have also been used to study inequality aversion. In particular, binary-choice DGs after Fehr et al. (2008), in which the dictator chooses between two allocations, with a trade-off between inequality and profit or inequality and efficiency.

Since altruism is potentially involved in both the proposer's and responder's decisions, the DG can be used in conjunction with the UG, using the former to control for altruism (Cox, 2004; Fershtman and Gneezy, 2001).

⁷Indeed, models of inequality aversion were devised, in part, to explain UG results (also see Bolton and Ockenfels, 2000; Charness and Rabin, 2002). Thousands of UGs, with many variants, have been run since Güth et al. (1982), which has over 6400 citations on Google Scholar as of writing, making it perhaps the most extensively studied experimental game in economics.

2.3 Trust and reciprocity

The literature on trust frequently quotes (Arrow, 1972, p. 357), who noted that "virtually every commercial transaction has within itself an element of trust." This may especially be the case in developing countries with weak institutions for enforcing contracts.

Reciprocity can be considered a type of conditional altruism: an individual is willing to behave altruistically, but only towards those who have treated them kindly in the past. Trust involves *expected reciprocity*: for example giving another individual use of a productive asset as part of an exchange that can potentially enrich both parties, but only if the *trustee* reciprocates by returning some of the surplus to the investor. This positive reciprocity in response to trust is termed *trustworthiness*.

The trust game (TG) (Berg et al., 1995) is designed to measure trust and trustworthiness. In the standard version of the game, both the *investor* and the *trustee* are endowed. The investor can send any portion of her endowment to the trustee. The amount passed is multiplied—typically tripled—by the experimenter. The trustee can either keep the money or pass some of it back to the investor, thus sharing the surplus. The amount sent by the investor is interpreted as trust. Other factors such as altruism, risk attitudes, distributional and efficiency preferences, and betrayal aversion might contribute to this decision.

The equilibrium prediction for selfish agents is that the trustee sends nothing back, and by backwards induction, the investor sends nothing to the trustee. However, a TG meta-study from around the world reports that 50% of the endowment is sent on average, and around 37% of the amount received by trustees is returned, making trust slightly profitable on average (Johnson and Mislin, 2011). The study included a number of developing countries as well. While there are some regional variations—African subjects sent less and returned less on average—subjects in all regions sent and returned significant amounts. TGs have been used in a variety of contexts to measure issues related to economic development. An early TG in the field conducted by Barr (2003) assessed trust and trustworthiness among resettled villagers in rural Zimbabwe, finding that villages with internally displaced populations were more likely to trust others in their village after accounting for expectations of trustworthiness, which she argues indicates a desire to build ties within new communities.

2.4 Cooperation

While not specific to any one social preference, willingness to cooperate in groups has been studied extensively, primarily through the public goods game (PGG) (Axelrod and Hamilton, 1981). A set of

subjects (typically three or more)⁸ begin the game with an endowment. The subjects chose how much to contribute to a group account (the public good) and keep the remaining amount. The contributions are typically simultaneous.⁹ The amount invested is multiplied by a factor greater than one but lower than the number of subjects. The amount is distributed evenly to all subjects, regardless of their own investments. The equilibrium for self-interested players is to invest nothing, while the socially efficient outcome is for all to contribute their entire endowment.

In most public good game experiments, subjects contribute positive amounts on average, but fall short of the socially efficient outcome.¹⁰ A variety of social preferences such as altruism, inequality aversion, and norms of fairness (alone or in conjunction) can explain this.

A common pattern in PGG experiments with multiple rounds is that contributions start high but taper off over time. This is due to a preference for *conditional cooperation*: individuals will cooperate as long as others in the group do (Fischbacher et al., 2001). Many individuals are *imperfect* conditional cooperators, who aim to contribute just below the group average. With each successive round, a group of conditional cooperators therefore contributes less and less. Martinsson et al. (2013) identifies the proportion of conditional cooperators in Vietnam and Columbia—both countries with low degrees of individualism. Despite much lower proportions of free riders. The proportion of conditional cooperators they observe is comparable to similar studies in the global North (Kocher et al., 2008, e.g.).

A common extension to the PGG is to add the option to punish others at a cost, which can lead to sustained cooperation (see next section).

Understanding the ability of individuals to solve local-level collective action problems and to engage in enforcement of cooperation is especially important in developing countries with weaker capacity of state in public goods provision.

2.5 Pro-social punishment

Punishing others, often at a cost, can be motivated by pro-social preferences, such as altruism or reciprocity. Altruistic punishment involves paying a cost to punish one individual to benefit others. In the PGG, adding the chance to punish others, after observing their contributions, can change the incentives for free-riders, such that cooperating becomes the dominant strategy. This can result in higher levels of cooperation (Fehr and Gächter, 2000). Altruistic punishment in this context can be

⁸When played with $n=2$ players, the PGG in reduced form is equivalent to a prisoner's dilemma.

⁹In some cases a "leader" goes first, setting an example for the rest of the group. If the group is composed of a significant proportion of conditional cooperators, then this can lead to higher contributions (see Section 6.5).

¹⁰A general meta-analysis of public good games does not include data for developing countries (Zelmer, 2003).

considered a second-order public good: by incurring a cost to punish, an individual makes others better off indirectly, by changing the incentives to encourage cooperation (Fehr and Gächter, 2002).

However, punishment in PGG can also be used against *high* contributors—presumably as revenge for punishment in previous rounds (Herrmann et al., 2008). Of particular note for the study of economic development is the finding that the frequency of anti-social punishment is greater in places with weaker rule of law.

The third-party punishment game (TPPG) (Fehr and Fischbacher, 2004) is a variant of the DG that measures aversion to inequality and indirect negative reciprocity. In contrast to the UG, the TPPG focuses on enforcement of fairness norms by materially uninterested third parties. The game has three players: a dictator, receiver, and punisher. The dictator is endowed, and given the opportunity to transfer any portion of her endowment to the receiver, who starts the game with nothing. The punisher observes the sender’s allocation, and may subsequently pay to reduce the payoff of the sender.¹¹ The decision of a third-party to punish is interpreted as enforcement of social norms, or indirect negative reciprocity (Fehr and Fischbacher, 2004). The sender’s allocation is driven by a combination of altruistic motives and of beliefs about punisher’s willingness to enforce sharing norms. The equilibrium prediction for selfish senders and punishers is for senders to send nothing and for punishers never to punish. Similar to the the results on UG, Henrich et al. (2010b) find a link between willingness to punish the unfair behavior of others and market integration in small scale societies.

2.6 Spite and antisocial preferences

A number of studies show that some individuals gain utility by punishing others or otherwise paying a cost to lower the payoff of another. The joy of destruction game (JoD) (Abbink and Sadrieh, 2009; Abbink and Herrmann, 2011) involves two players, each of whom can pay to reduce the payoff of the other, with the absence of any strategic motivation. Inequity aversion (Fehr and Schmidt, 1999) does not affect choices due to the initial payoff symmetry. While the original JoD game does not involve inequality, adding inequality to the mix can exacerbate willingness to destroy others’ wealth (Kebede and Zizzo, 2015; Zeballos, 2018). These experiments show a negative correlation between willingness to destroy others’ resources and productivity outside of the lab, with implications for economic development (see Sanjaya (2021) for a review of experiments on anti-social behavior).

¹¹Alternatively, the 3rd party punisher might choose a threshold using the strategy method (Henrich et al., 2010a).

3 Lab-in-the-field methodology

In this section we summarize methods for running behavioural experiments in the field, including common methods of implementing treatments that are of interest to development economists.

In the decades since behavioural experiments have first been run in developing settings, a set of best practices has been established to supplement the guidelines for running a good lab experiment (Harrison and List, 2004). Based on early lab-in-the-field studies like Henrich et al. (2001), the practices for running experiments in developing countries have been refined. Cardenas and Carpenter (2008) give a summary of lessons learned up until that point. Here we review best practices, updated with some recent advances in the literature and reflections.

3.1 Experimental Treatments

Behavioural experiments might include treatments that change the incentives or structure of the game itself. However, particularly in studies on economic development the treatments involve manipulating the way that decisions are presented to subjects (framing), and the psychological mindset of subjects when making decisions (priming).

3.1.1 Framing

In all experiments the way that decisions are presented to subjects can affect results. In standard laboratory experiments with students, the convention is use neutral framing. This means that rather than labelling decisions "punishment" or "cooperation", the instructions refer to "deducting points" or contributions to "account A". This is done to isolate decisions based on preferences from out-of-lab norms. Generally, the best practice with field experiments is similar, although in some cases linking decisions to more familiar concepts might improve understanding.

In some cases varying the framing can be a treatment. A canonical example of framing effects in economics is Pillutla and Chen (1999) who show that contributions in a PGG are higher when the game is framed as a social event, rather than an investment.

One application for framing treatments is to link decisions in games with culture-specific practices. Cronk (2007) finds that framing a TG in terms of a concept of *osotua*, a Maasai expression for a gift-giving relationship, reduces trusting behavior of Maasai in Kenya, relative to a neutrally framed TG. Since different individuals in different settings may interpret the game in the context of different real-life frames, comparability of behavior across settings may be hampered. Understanding the contexts

within which subjects interpret abstract games may help with interpretation of results.

In other cases, explicit framing is used to elicit social norms, context-specific preferences and to proxy out-of-lab behavior. For example, researchers interested in studying cooperation in common pool resource games or in prisoner dilemmas often frame their games in terms of the resource extraction, while ultimately incentivizing decisions with money (e.g., Alpizar et al., 2011; D’Exelle et al., 2012; Cardenas et al., 2013b; de Melo and Piaggio, 2015; Polania-Reyes, 2015). This allows a researcher to measure attitudes and behavior with policy relevance that are difficult to elicit more organically.

3.1.2 Priming

Priming is a method pioneered in social psychology, in which experimenters study the effects of a phenomenon by exposing subjects to stimuli that trigger similar responses. Priming operates purely through psychological channels, by manipulating the salience of a desired theme.

One example particularly well suited to the study of economic development is to manipulate thoughts on poverty. Mani et al. (2013) ask subjects to consider scenarios in which they would face financial hardship, and consequently primes them to consider their own hardship. They show that the poverty prime reduces cognitive ability. Bartoš et al. (2021) use a similar method to show that poverty reduces patience in rural Uganda. We are unaware of any study studying a link between thoughts on poverty and social preferences.

Priming has been used in developing contexts to study how social identity, by making ethnic (Berge et al., 2020) and religious (Scacco and Warren, 2018) identity more salient.

Features of the decision-making environment may sometimes prime subjects unintentionally. Clist and Verschoor (2017) show how the language that the experiment is conducted in affects contributions to a PGG, among multi-lingual subjects in Uganda. This finding has implications for understanding how multi-lingual societies interact, but is also a lesson for designing experiments (see Section 3.2 below). Similarly, if subjects play multiple games, previous experience may prime behavior in the next game.

Several priming studies in psychology have failed to replicate, calling into question the reliability of the technique (Yong, 2012). Cohn and Maréchal (2016) discuss practical issues and concerns about use of priming following the failed replication controversy (Open Science Collaboration, 2015). They note that manipulation checks allow researchers to understand whether their prime works and to rule out alternative explanations.

For example, when manipulating severity of poverty related concerns, Bartoš et al. (2021) ask subjects to rate each scenario according to severity. When testing identity primes, strength of identity-related mental associations can be tested by comparing fractions of completed word-fragments with identity-related meaning versus other words (Cohn et al., 2014).

We believe cautious use of priming is an exciting area of future research for measuring social preferences in developing contexts. Priming makes it possible to randomize variables that would otherwise be impossible to manipulate experimentally. Because priming does not alter incentives, liquidity constraints do not confound the results. This is relevant when effects of extra income are theoretically well known, and the pure psychological effect is of interest. The downside is that priming does not identify the exact psychological mechanisms underlying the treatment effect.

3.2 Decision Environment

A good rule of thumb for any experimental design is to present subjects with the simplest possible decisions possible given the research question. This is particularly true in developing settings where many subjects might be illiterate or innumerate (Bruine de Bruin and Slovic, 2021), when subjects are potentially under stress, and where a number of distractions might be present at the time the experiment is being conducted.¹²

Over the years, researchers have developed simple procedures for working with less educated and low-literacy subject pools.¹³ For lab-in-the-field experiments, it is important to have consistent instructions. These might be presented in a group setting and/or individually just before making decisions. Clear visual aids with numerous examples help ensure good understanding of the task. The script will need to be translated to the local language. When researchers may not speak the local language, a common technique to check that this translation captures the meaning intended is to have a second translator "back-translate" the text into the original language.

While subjects may be uncomfortable with numbers, they are likely used to dealing with money. Hence simple calculations with cash come relatively naturally. Letting subjects make decisions physically, with either real money or with cards representing real banknotes, can make it easier for them to calculate and understand the decision.

Likewise, any experiment that relies on probabilities is best communicated using a physical appa-

¹²We have had experimental sessions interrupted by rain, wind and even livestock wandering through our "lab".

¹³For general scripts for basic behavioral games (DG, UG, TG, and TPPG) we refer to supplementary materials and methods sections of (Henrich et al., 2001; Barr, 2003; Bernhard et al., 2006; Bauer et al., 2014a).

ratus, such as flipping a coin or drawing a number from a bag. This also alleviates issues of mistrust. Using physical methods of representing probabilities can elicit quite sophisticated responses. For example, Delavande et al. (2011) offer tools to measure probabilistic beliefs asking subjects to distribute balls to (physical) frequency bins representing outcomes of interest.

Testing for subjects' comprehension prior their decisions is also advisable. Doing so can rule out any potential confounds that arise if a particular group (e.g., less educated, older, different language group) has a poorer grasp of the task or decision-making environment. For example, Jakiela (2015a) compares fairness norms of rural Kenyans and undergraduates from the US. Comparing these results requires that both groups equally understand the decisions they made.

By the same token, comparing outcomes across groups with different levels of stress and other cognitive load can be problematic. Studies that manipulate poverty-related stress through priming show an effect on cognitive ability (e.g., Mani et al., 2013) and patience (Bartoš et al., 2021). If, for example, default choices produce more selfish outcomes, comparisons across demographic categories may be confounded.¹⁴

3.2.1 Strategy method

To increase numbers of observations, to better understand choices under infrequent situations, and to get at causal effects of treatment on conditional choices in strategic games (as in Bartoš and Lively, 2021), researchers studying social dilemmas use a strategy method. In a strategy method, responders make conditional decisions for each possible choice set or for its subset. Brandts and Charness (2011) compare 29 studies that compare direct responses to strategy method. 16 studies find no difference, 4 find differences, and the remaining 9 comparisons are mixed. Reassuringly, when differences were detected, treatment effects were in the same direction using both methods. We are not aware of a study showing whether such comparison differs by level of subjects' sophistication. Strategy method may also be used to study subject's consistency and rationalizability of choices.

3.3 Deception and ethical concerns

Experimental economists are concerned about deceptive practices (Hertwig and Ortmann, 2001). Beyond aversion to lying on ethical grounds, the key practical concern is subject pool contamination. If

¹⁴For example, the correlation between cognitive ability and risk preferences seems to be a product of increased random decision making in lower cognitive ability agents (Andersson et al., 2016). Economic theory offers clear predictions in how rationality can be inferred from individual choices (Andreoni and Miller, 2002) and how deviations from rationality can be quantified (Afriat, 1972).

subjects do not believe the experimenters, their decisions may be distorted. This is most problematic if subjects are participating in experiments repeatedly, or in contact with potential subjects, such as subjects in university laboratories. In the field, this is typically not a major concern. As such, there is an ongoing debate as to how the field should treat deception in field settings (Charness et al., 2022).

Running experiments that involve cash payments and social interactions in small scale communities should be carefully considered, as visible inequalities might lead to adverse responses outside of laboratory, such as social pressure, jealousy, negative reciprocity, or shaming. Maintaining privacy during decision making and payouts, and allowing for plausible deniability, is thus imperative (e.g., Jakiela and Ozier, 2016).

Finally, while randomization is generally considered as a fair allocation principle that also allows researchers to study causal effects, such allocation mechanism may still lead to welfare loss: even though conducted in an abstract laboratory setting, Haushofer et al. (2019) document aggregate welfare loss due to randomization when initial endowments are unequal.

3.4 Incentives

The gold standard for the elicitation of social preferences is to use monetary incentives. When subjects make decisions about how to allocate real money, the decisions can be treated as *revealed* preferences, and thus good predictors of decision making outside of the lab. Alternative methods, such as survey questions and hypothetical choices (i.e. *stated* preferences) have typically been viewed as less reliable. When there is no cost to, for example, stating that one is altruistic, responses might be affected by social desirability bias and self-image concerns. Since effort is a costly mental process, incentives can also motivate individuals to think more carefully about their decisions.

A meta-analysis of DG results finds that hypothetical studies are not significantly different from incentivized DGs (Engel, 2011). While behavior in incentivized games is affected even when raising stake size up to 1000 times, it does not converge anywhere near selfish behavior (Slonim and Roth, 1998; Andersen et al., 2011). Section 5 focuses on unincentivized survey methods asking qualitative questions related to social preferences or eliciting hypothetical choices for behavioral games in detail, including a discussion of a survey-based measure of social preferences (Falk et al., 2018) validated with German undergraduate students.

Most evidence on the role of incentives comes from studies on risk preferences. Vieider et al. (2015) compare risky choices using incentivized methods and survey measures from 30 countries. They find

positive correlations between the two measures, however, with variation between countries. Using incentives, individuals seem to be more risk averse.

In general, an advantage to implementing incentivized behavioral games in low-income countries is that budgets allow for relatively high stake-sizes relative to subjects' income. Payments proportional to a days' wages or even higher are not uncommon (Cameron, 1999; Andersen et al., 2011). A possible motivation against incentivized experiments is the risk that more materially interested individuals would self-select into the sample, however evidence for this is missing (Falk et al., 2013). Another potential downside of using incentives is the time required to collect the data and extra logistics.

"Incentives" usually means cash payments, although in certain contexts, such as working with children (Bauer et al., 2014b, e.g.) in-kind payments might be more appropriate. Lively and van den Berg (2016) use in-kind incentives to measure household bargaining in Tanzania, since this forces subjects to make a choice during the experiment that cannot easily be redistributed later on. Cassar et al. (2016) vary the type of incentive by treatment among Chinese parents. They find that women are less willing to compete for cash, but the gender gap disappears when the prize is vouchers for books (which benefit their children). This is an important reminder that cash is not always a neutral and universal medium, and comes with cultural connotations that can influence the outcome of experiments.

3.5 Experimenter effect

The presence of an experimenter may influence the behavior of experimental subjects.¹⁵ While this is always an issue in experimental economics, it is of special concern when conducting experiments in developing countries where experimenters are often outsiders. In Sierra Leone, Cilliers et al. (2015) show that the presence of a white foreigner increases DG giving. On the other hand, among those with high exposure to non-governmental organizations (NGOs), the presence of a foreigner was correlated with less giving, as participants thought results might be used as means testing and affect future benefits. The lesson here is to maintain a clear separation between the research team and any NGO or government partners as much as possible, and to explicitly make clear that the results of the study will not be used to evaluate the community and determine future benefits (if true).

Experimenter effects are also of particular concern when working with illiterate subjects. When experiments are conducted one-on-one, it is reasonable to expect a larger degree of social desirability bias, particularly in regards to experiments that measure social preferences. Reduced anonymity affects

¹⁵This is an example of the Hawthorne effect, which describes how behaviour can change when observed.

prosocial choices (Hoffman et al., 1994; List et al., 2004; Oh, 2021). Increasing privacy when subjects make their decisions may mitigate these concerns.¹⁶

3.6 Beliefs in the supernatural

While the standard subject pool of university students generally interpret random events in the lab, such as a coin toss, to pure chance, subjects with a strong belief in supernatural forces might put more meaning into seemingly random outcomes (Platteau, 2015). Thus, researchers are advised to carefully consider the context and local beliefs when designing field experiments. For example, Jakiela (2015b) shows that rural Kenyans playing a DG do not differentiate between earned and unearned income, in contrast with a sample of UC Berkeley students. A potential explanation is that receiving an endowment by "chance" has a deeper meaning, which implies entitlement. This is in contrast to meritocratic theories of fairness (Konow, 2003).

Religion plays an important role in how individuals make social decisions. Studies priming religion have shown positive effects on DG giving (Shariff and Norenzayan, 2007), reduced third-party enforcement among religious individuals (Laurin et al., 2012), and increased in-group bias (Parra et al., 2016). Such effects may also play role when interpreting fairness norms.

4 Field measures of social preferences

A question often arising in discussion of behavioural games is whether they are meaningfully informative about the settings that economists are ultimately interested in. In other words, do abstract tasks in experiments using highly controlled, artificial settings predict behaviour in the "real world"? Firstly, it is important to keep in mind that the value of behavioral games is precisely their abstract nature. While donating to charity, for example, might be rooted in similar altruistic preferences as giving in a dictator game, many other motivations likely influence the decision as well, such as reputation and social pressure. When deciding whether to give money in "real-life"¹⁷, an otherwise generous individual might mistrust a particular charity or believe a cause is unworthy. Given this, we should not expect a perfect correlation between outcomes in behavioral experiments and out-of-lab actions. Indeed, some studies fail to find any correlation between lab and artefactual-field experiments with

¹⁶New methods have been devised to measure extent of experimenter demand (De Quidt et al., 2018).

¹⁷Some experimental economists have taken issue with describing out-of-lab settings as the "real world". While lab, artefactual and framed-field experiments have varying degrees of resemblance to everyday situations, decisions made by subjects nonetheless involve material trade-offs and are thus very much "real" examples of revealed preferences.

altruism, trust, or cooperation in natural settings (Voors et al., 2011; Sawada et al., 2013; Galizzi and Navarro-Martinez, 2019).

4.1 External validity and combining the lab and field

Nonetheless, a number of studies have demonstrated the external validity of lab-in-the-field methods by correlating outcomes in experiments with natural behaviours which can be considered relatively good measures of social preferences. DG allocations have been linked to charitable donations, self-selection into prosocial occupations, and prosocial behavior in general (Barr and Zeitlin, 2010; Kolstad and Lindkvist, 2013; Ligon and Schechter, 2012; Lagarde and Blaauw, 2014; Batista et al., 2015; Rao, 2019; Bartoš, 2021); and trusting behavior with participation in pro-social organizations (Serra et al., 2011; Cardenas et al., 2013a). Similarly, cooperation in PGGs is correlated with local levels of engagement in social organizations and voting (Rustagi et al., 2010; Fehr and Leibbrandt, 2011; Grossman and Baldassarri, 2012; Barr et al., 2014; Bluffstone et al., 2015).

Fehr and Leibbrandt (2011), find a particularly clean measure of cooperation among small-scale fishermen in rural Brazil: larger holes in nets allow bigger shrimp to escape and reach maturity, improving the (literal) common-pool of shrimp. They show that higher contributions in a PGG are correlated with the larger holes. Carlsson et al. (2014) correlate donations to physical public goods in a Vietnamese village (bridges and roads) to results from a PGG experiment with the same subjects, comparing results from a natural field experiment, a lab-in-the-field experiment, and two sets of observational data to demonstrate the stability of cooperative preferences over time.

In other cases, experimental measures of social preferences can be used to explain the nature of an out-of-lab outcome. Rustagi et al. (2010) study forest management in rural Ethiopia. They show that villages with a higher proportion of conditional cooperators—as measured in a PGG—spend more time monitoring forests to enforce limits on resource extraction, which in turn leads to healthier forests. While multiple cultural, economic and institutional factors might influence the intensity of monitoring in a particular village, the correlation with the experimental measure implies that social preferences are at least partially responsible for enforcement.¹⁸ Similarly, Beekman et al. (2014) employ a clever method of measuring corruption, in rural Sierra Leone, by precisely and surreptitiously counting and weighing supplies (such as seeds) intended for community distribution that were stored in village leaders' houses. They correlate the amount of goods that went missing from leaders' houses with PGG

¹⁸A follow-up study by Kosfeld and Rustagi (2015) shows that the villages with more conditional cooperators are led by more cooperative leaders.

data from villagers, and find that public goods contributions are higher in communities with honest leaders.

Rao (2019) is a particularly good example of how complementary measures of prosociality can be combined. He studies the effects of desegregation policies on social cohesion in India, exploiting a reform that mandated free access for poor students at some private schools—those that had been built on public land. He shows how contact with poor students increases generosity among the rich student, using administrative records of student charity volunteering. Using a DG, he is able to confirm this and link the result to higher aversion to inequality. Two field experiments measuring willingness to interact with poor students in relay races and play dates supplement this, showing that wealthy students in the treatment schools are also more willing to interact with poor students.

4.2 Field experiments

Natural field experiments (Harrison and List, 2004)—those in which the experimenter manipulates conditions in the field such that subjects encounter them as a normal part of their daily lives—can provide high-stakes, externally valid and convincing measures of social preferences as well.

One method that has been used to measure social preferences related to employment is to randomly expose potential job candidates to different job advertisements. Ashraf et al. (2020) use a field experiment with the government of Zambia to study the role of prosocial and extrinsic motivation on attracting talent on the labor market. The authors manipulate salience of career benefits to job ads for a new community health worker position, finding that indeed career incentives crowd out pro-social motivations (though this did not hold at the margins, and hence wages increases led to higher quality).

Breza et al. (2018) study the intersection of compensation and social preferences by manipulating wages to create inequality amongst small teams in a factory opened for the purpose of the study. They document that workers are less willing to work for unequal pay. The experiment allows them to hold individual productivity constant across treatments at the outset, thus eliminating concerns of self-selection by ability that would make this difficult to observe in a non-experimental setting.¹⁹

Lowe (2021) tests the theoretical predictions of contact hypothesis (Allport, 1954): that inter-group contact reduces prejudice if contact is between groups of equal status with common goals. Lowe introduces cricket tournaments in caste-segregated rural India, randomly manipulating team composition to

¹⁹As the random positive wage shock in the control condition closely mimics a gift-exchange game, this experiment also provides a high-stakes, out-of-lab test of external validity for this experimental design. They do not replicate the typical laboratory results. For more on the replicability of gift exchange results see Esteves-Sorenson (2018).

be caste-homogenous or caste-heterogenous. Willingness for cross-caste social interaction is measured in a self-reported survey, as well a field measure in which players are able to choose teammates: he shows that players are more likely to choose high ability, cross-caste teammates after collaborative contact. He supplements these measures with a TG.

Similarly, Mousa (2020) implements treatments that randomly vary the composition of football teams of Iraqi Christians and Muslims. She uses a variety of creative field measures. These include both decisions related to football—including willingness to register for a mixed team and voting for a "best newcomer" award—as well as "off the field" outcomes—willingness to attend a mixed dinner event, visit a restaurant in a predominantly Muslim city to redeem a voucher, and a DG-like measure of contributions to a in/out-group religious charity. While the treatment affected the football-related measures, there was no effect for the latter outcomes. This highlights the caveats of making too broad claims about the generalizability of treatment effects across tasks and domains.

5 Unincentivized measures of social preferences

Unincentivized survey modules are used to measure social preferences in large, often representative population samples. This complements the costly incentivized experiments in a few ways. First, while incentivized measures offer greater measurement reliability, the advantage of unincentivized survey methods is that their simplicity allows data collection on a large scale, even by phone- or internet-based online. Moreover, survey measures typically do not require extensive explanation, comprehension checks, and personal assistance through enumerators. This substantially reduces costs and time relative to the implementation of incentivized games. Since data and documentation from some large-scale surveys are freely available, they are a good option for researchers with limited budgets. Recent studies have revisited surveys and devised hypothetical behavioral games (Falk et al., 2018). This section discusses common instruments used, and potential issues with their use and interpretation.

5.1 World Values Survey

The World Values Survey (WVS) is a global representative survey that contains questions on "social, political, economic, religious and cultural values values." The WVS has been conducted with nationally representative samples in 120 countries, representing over 90 percent of the world's population, over 7 waves since 1981. The WVS includes questions on generalized trust, trust in institutions and

comparative trust across groups. Civic norms questions include the justifiability of cheating on taxes, bribe acceptance, and claims of government benefits by those not eligible among others. Responses to these questions have been linked to economic (Keefer and Knack, 2007) and financial development (Guiso et al., 2004), and government regulation (Aghion et al., 2010). The large representative samples even allow to study regional differences (La Porta et al., 1997; Tabellini, 2010). Similar questions are included on regional surveys. For example, (Nunn and Wantchekon, 2011) use the data on trust from the Afrobarometer survey to show how the slave trade has an effect on present day levels of trust.²⁰

5.2 The Global Preferences Survey (GPS)

A measure that has recently gained substantial influence is the GPS (Falk et al., 2018). Conducted with a representative sample from 76 countries and about 80 thousand subjects, the GPS measures fundamental economic preferences using a simple survey instrument. They developed short questions to measures positive and negative reciprocity, and altruism and include a novel measure of trust. The GPS includedes both qualitative and quantitative questions. For the latter, respondents allocate hypothetical amounts of money. Falk et al. (2023) validate the survey measures in an incentivized laboratory experiment with undergraduate students in Germany. The survey preference measures are predictive of a range of self-reported prosocial behaviors, such as charity donations, volunteering, and helping others. The representative sample allows the authors to show geographic differences in prosociality. For example, a follow-up paper documents that preferences of men and women diverge with economic development and gender equality (Falk and Hermlle, 2018). This has substantial implications for changing gender-specific preferences and needs to accommodate to them on the path of economic development.

5.3 Validating survey measures of social preferences and measurement issues

A potential downside to survey measures of social preferences is the interpretation of responses. Recent evidence by Bauer et al. (2020b), using questions from the GPS with low-income individuals in Kenya, shows that there may be limits to how universally valid the GPS is. While the quantitative parts of the survey predicted responses in an incentivized game, this was not true of the qualitative questions.

²⁰The Afrobarometer currently covers 36 African countries, with 8 rounds since 1999. Similar surveys include: Arab barometer (14 Arab countries, 6 waves since 2003), Asian barometer (currently 16 Asian countries, 5 waves since 2001), and Latinobarometro (18 Latin American countries, 22 waves since 1995).

It may be that the qualitative questions are context specific.

There is also some evidence of a correlation between the WVS survey-based trust measures and incentivized trust (Glaeser et al., 2000) and PGGs (Gächter et al., 2004; Bellemare and Kröger, 2007; Thöni et al., 2012). Sapienza et al. (2013) show that while a trustor's behavior in an incentivized TG captures both preferences and beliefs, the WVS trust measure mostly captures the belief component of trust. Most of this micro-level evidence comes from high or upper-middle income countries, however Ashraf et al. (2006) and Banerjee (2018) find evidence of survey and experimental cross-validation in Russia, South Africa, and India.

While surveys are easy to administer, they are potentially more susceptible to demand effects and social desirability bias.²¹ Moreover, it may be difficult to compare responses across settings and groups as similar questions may be interpreted differently in different contexts. In general, the jury on the use of incentives is still out. While both GPS and WVS survey measures exhibit many intuitive patterns previously documented in the experimental literature, a more systematic cross-country validation exercise would be welcome.

6 Variability and formation of preferences

While the traditional approach to modelling economic preferences is to assume stability over time (e.g., Stigler and Becker, 1977), in the medium to long term, peer effects, environment and life experiences often shape a person's social preferences. Moreover, preferences might be "state-dependent", varying according to the relationship between individuals, their social identity, social environment, or the institutional setting in which decisions are made. In this section we review research on these topics, with a focus on applications to economic development.

6.1 The formation of social preferences.

There is evidence that basic social preferences, such as inequality aversion, start to develop very early in life (Schmidt and Sommerville, 2011), and are potentially shaped by conditions in utero and infancy, such as disease burden (d'Adda and Lively, 2016; Cecchi and Duchoslav, 2018; Brañas-Garza et al., 2019). Family background matters as well. Evidence from developed countries shows that children from lower socioeconomic status backgrounds are less pro-social and that parents' preferences predict

²¹List experiments (or item count technique) implemented in surveys are a popular tool for eliciting honest answers to sensitive questions (e.g., Blair et al., 2020). The downside of this method, however, is a lack of individual-level statistics.

children’s (Dohmen et al., 2012; Bauer et al., 2014b; Falk et al., 2021). Notably, a recent study by Chowdhury et al. (2022) measure the intergenerational transmission of social, risk and time preferences using incentivized experiments among a large sample of families in rural Bangladesh. They find a robust relationship between parents’ and children’s preferences. Interestingly, after controlling for parents’ social preferences, socio-economic status and even parenting style no longer explain children’s preferences.

While early experiences can shape social preferences later in life, this does not imply that they are static. A number of studies have compared social preferences across age groups. The most popular method used in these studies is a set of simple binary DGs after Fehr et al. (2008), measuring spite, inequality aversion and altruism, which can be implemented with children as young as 3 years old.²² Studies comparing social preferences across age groups broadly show that social preferences emerge at an early age, with children becoming more parochial and inequality averse until adolescence, at which point they become more altruistic and concerned with efficiency (Fehr et al., 2013; Almås et al., 2010; Cobo-Reyes et al., 2020).²³

A recent study from Cadsby et al. (2020) highlights the complexity of combining these factors to understand the effects of economic development. They study children in rural China who are left behind when their parents migrate to cities for work. While the authors expected the absence of parents to correlate with reduced altruism, they find the reverse. Children with absent parents were more pro-social, behaving more like older children, and moreover, have preferences that more closely match those of rural children. Likely, these results are connected to a variety of factors including parental transmission of preferences, economic resources in the household, or time spent with other adults. We also see very little research on the effect of early childhood education on social preferences in developing settings, with Dean and Jayachandran (2020) as the only exception we’re aware of.

6.2 The effect of conflict on social preferences

Exposure to conflict has emerged as a particularly influential environmental factor involved in shaping preferences. The general conclusion of studies—mostly using lab-in-the-field experiments—is that people exposed to violence behave more cooperatively, even long after the conflict ends. Yet, the increase in prosocial is typically specific to the ingroup (Bauer et al., 2016). The results may partially explain the observed rapid post-conflict recovery in many countries. But the parochial nature of the

²²Bauer et al. (2014b) extended the set of games to differentiate between spiteful and selfish individuals.

²³In contrast, Khachatryan et al. (2015) document relative stability of altruism between the ages of 7-16 in Armenia.

effect can also explain vicious cycles of conflict across groups.

The positive relationship between exposure to violence and prosocial behavior in behavioral games has been documented in a range of settings: increased altruism in Burundi, Georgia, and Sierra Leone (Voors et al., 2012; Bauer et al., 2014a; Cecchi et al., 2016), increased trust in Nepal and Uganda (Gilligan et al., 2014), increased PGG contributions in Nepal (Gilligan et al., 2014), and increased punishment of non-cooperators in Israel (Gneezy and Fessler, 2012).²⁴ Bauer et al. (2018b) compare the trustworthiness of ex-child soldiers in Uganda with their peers using a TG, and show that the former are—contrary to public stereotypes—actually more trustworthy than their peers. Parents of former child soldiers, who are presumably knowledgeable about this effect, send more to them in the trust game and expect higher back-transfers. The authors note that abduction in this context was more or less random, and thus the effect of soldiering on preferences can be interpreted causally.

Bauer et al. (2016) conduct a meta-analysis of 16 studies that contain some measures of prosociality and for which data are available. They standardize outcomes to ensure comparability across studies, even those employing different empirical strategies. Results from behavioral games show the strongest effects. While none of the studies pins down exact mechanisms thorough which the effect operates, it does not seem to be driven by differences in economic constraints, which is a topic that deserves future study.

6.3 Shocks and stability of social preferences.

Lives of the poor are frequently affected by natural disasters and their coping strategies are generally more limited. Cassar et al. (2017) study effects of the 2004 tsunami in Thailand, and find that those living in affected villages had higher trust (as measured in a TG) five years later, potentially due to mutual support in the aftermath of the disaster. Andrabi and Das (2017) find that those living closer to the epicenter of the 2005 Pakistani earthquake had higher (self-reported) trust in foreigners, possibly due to the overwhelming inflow of aid.

Several studies examine how preferences respond to changes in response to seasonal variation in income. This is of particular relevance among farmers with little access to savings and credit. Aksoy and Palma (2019), Bartoš (2021) and Boonmanunt and Meier (2020) all use DG among farmers in Peru, Afghanistan and Thailand, respectively. None find a significant difference in sharing behavior

²⁴In Tajikistan, Cassar et al. (2013) find increased exposure to violence is correlated with *lower* trust towards individuals from their village compared to outsiders, possibly due to the localized nature and intra-group fighting that characterized that conflict.

with other village members before and after the harvest. However, Bartoš (2021) finds that scarcity lowers willingness to engage in the costly punishment of those who violate sharing norms. Aksoy and Palma (2019) and Boonmanunt and Meier (2020) and also manipulate group identity. The former show that in-group bias in two domains—DG allocations and cheating for the benefit of a partner—are limited to times of relative abundance. Boonmanunt and Meier (2020) do not replicate this finding in the DG, however they do find that in-group bias in punishment in a PGG is lower with scarcity. In sum, while scarcity does not seem to affect underlying sharing preferences, enforcement of sharing norms may be lower in times of scarcity. Long-term studies on this topic are, however, lacking.²⁵ One notable exception is Prediger et al. (2014), who document a higher prevalence of antisocial behavior in a JoD game among Namibian pastoralists with long-term exposure to scarcity.

Agneman et al. (2020) show that priming participants on their current food situation aggravates the negative effect of food scarcity on cooperation, but the effect of the experimental manipulation is not precisely estimated.

Documenting a causal link between scarcity and envy or interpersonal trust is of importance in our understanding of underlying reasons behind the negative correlation between trust and economic development (Algan and Cahuc, 2010). Relatedly, it can provide empirical support for theoretical models of behavioral poverty traps (Ghatak, 2015).

In addition to being an important topic of study in its own right, understanding how seasonal changes in agricultural income or other economic shocks affect social preferences is an important methodological concern. Researchers working in areas affected by such variations are advised to carefully consider how it might affect measurements over time and to balance treatments accordingly.

6.4 Group identity

There is a rich history of research on group identity in laboratory experiments, beginning with psychology literature, in which arbitrary group identity was created in the lab (Tajfel, 1979). Similar techniques have been incorporated and used extensively by economists to study the role of group identity in mediating social preferences (e.g., Chen and Li, 2009; Hargreaves Heap and Zizzo, 2009). A number of lab-in-the-field experiments have expanded this literature through the use of organic group identity, such as ethnicity, to measure the difference between preferences towards one's *ingroup* and

²⁵In contrast to these studies from developing societies, evidence from developed countries on income shocks have shown changes in preferences: Barr et al. (2016) find that unemployed subjects in Spain are less likely to recognize the difference between earned and unearned income when making distribution decisions, while Fisman et al. (2015) find that Americans exposed to the great recession were more selfish and less concerned with equality.

outgroup. These lab-in-the-field experiments have provided micro-level evidence for the negative effect of ethnic or other group cleavages on economic outcomes observed in cross-country studies (Alesina and Ferrara, 2005).

In an early lab-in-the-field experiment on ethnic discrimination, Fershtman and Gneezy (2001) study trust and trustworthiness among Ashkenazi and Eastern Jews in Israel, using a combination of the TG, DG and UGs. The DG has also been used on its own to test for *parochial altruism* (Choi and Bowles, 2007; Whitt and Wilson, 2007).

A series of experiments have examined how punishment is applied across social groups. Bernhard et al. (2006) show how willingness to punish norm violators in a TPPG is greater when the victim is an in-group member. In a similar vein, Alexander and Christia (2011) find that multi-ethnic groups in Bosnia-Herzegovina cooperate less, but adding the the ability to punish free-riders narrows this gap. There is also evidence that punishment has a different effect when applied across and between ethnic groups (Bartoš and Lively, 2021). Habyarimana et al. (2007) run DG and PGG in Kampala, Uganda, exploring the underlying causes of lower cooperation across ethnic lines. While they find no evidence of ingroup bias in preferences, they find that co-ethnics are able to better cooperate through, in part, a better ability to sanction free-riders.

While most experiments on group identity in developing countries focus on ethnicity, Scacco and Warren (2018) manipulate the religious identity of DG recipients to study effects of an integration program in Nigeria.

A practical concern is how to communicate identity without making subjects aware of the intentions of the experimenters, and thereby triggering social desirability bias or a demand effect. Fershtman and Gneezy (2001) communicate subjects' surnames (maintaining anonymity), while Bauer et al. (2018a) and Scacco and Warren (2018) use first names. Berge et al. (2020) communicate the partner's hometown, which in their context (Kenya), is a strong indicator of ethnic identity. Alternatively, pictures might be used: Habyarimana et al. (2007) use photos with portions blacked out to maintain anonymity, Blouin and Mukand (2019) use realistic drawings, Doleac and Stein (2013) use photos of hands, Bauer et al. (2021) use group photos. Voice has been used to indicate gender (Beaman et al., 2009), and a similar technique could be used to study differences in accents. In some settings, it might even be possible to communicate ethnic identity directly without it seeming too conspicuous (Bartoš and Lively, 2021).

Most studies discussed here focus on individual behavior. Social signalling literature, literature on

third party punishment, and studies of networks document the role of social influence. Behavior in groups (as opposed to individual behavior) seems to increase selfishness (Charness and Sutter, 2012; Kugler et al., 2012). More worryingly for the study of hostility, emergence of conflicts, and study of organized crimes, groups in Uganda also exhibit increased costly antisocial behavior, possibly due to reduced perception of individual responsibility for harmful acts (Bauer et al., 2020a).

6.5 Social environment

A number of experiments have shown how social environment through social networks, peer effects, and social norms can play a pivotal role in spreading technology and shaping behaviour. However, social pressure may also discourage investment and productivity.

For example, BenYishay and Mobarak (2019) show how small-scale farmers are more likely to adopt technology when they hear about it from a peer farmer, who shares similar characteristics. Similarly, Beaman et al. (2021) map social networks in Malawian villages by soliciting who respondents consulted when making agricultural decisions. They use this to show that targeting farmers central to the network is more effective.

Social network data has also been combined with lab-in-the-field experiments: Breza et al. (2014) conduct a modified trust game with third-party monitors in Indian villages. They find that monitors with higher network centrality are more effective at enforcing cooperation, especially when they have the ability to punish, but interestingly, simply observing the interaction is enough to increase back-transfers.

A related literature examines how leaders encourage cooperation. Laboratory experiments have shown that simply setting a positive example, for instance making the first contribution to a PGG, can influence others' decisions (Güth et al., 2007; Rivas and Sutter, 2011). Several studies have extended this idea to the field, examining the relationship between cooperative behaviour and "real-life" leaders in small communities. Jack and Recalde (2015) compare randomly chosen leaders with local authorities. Community leaders are more likely to make a high contribution to a public good (local school) when observed by others, though randomly chosen leaders have nearly as much influence.

However, social environment may also have a darker side. While preferences and norms for sharing resources and redistributing wealth are generally conducive to economic development (Kosse and Tinicani, 2020) by producing public goods and can serve as an important social safety net in the absence of formal institutions. However, as noted by Platteau (2015), excessive pressure from kin to redistribute

can lead to under-investment and social losses.

Jakiela and Ozier (2016) provide empirical evidence for how social pressure from family and friends can lead to inefficiencies, using a lab-in-the-field experiment in rural Kenya. Subjects choose how much to invest in a risky but profitable asset. The experiment consists of two treatment dimensions: whether the amount invested can be observed by others in the session and the size of the initial endowment. Since the amount not invested is private, subjects who are assigned a high endowment in the observable investment treatment can conceal their earnings from other participants by investing less—thus earning less on average, but potentially avoiding social pressure to share with family. They find that this strategy is adopted in particular by women, and more so when they have relatives present during the experimental session. Similar findings that individuals take costly actions to hide income from others has also been experimentally documented elsewhere (Ashraf, 2009; Beekman et al., 2015; Goldberg, 2017; Fiala, 2018; Boltz et al., 2019).

Carranza et al. (2022) provide similar evidence from a high-stakes field experiment that redistributive pressure functions as a social tax with distortionary effects. They offer factory workers in Cote d’Ivoire an option to save extra earnings, then vary by treatment whether those savings are observable by the workers’ social networks. They find that effort increases in the treatment with private savings, presumably because workers whose savings are public face pressure to share.

While many innovative studies have examined the interaction between social preferences and networks, these studies treat networks as static. Understanding the role that social preferences play in network formation would be informative. Relatedly, mapping social networks and their dynamics may be useful in understanding preference endogeneity, peer effects, and social norms.

6.6 Preferences and institutions

Social preferences can also be *state-dependent* and vary according to situation (Bowles and Polania-Reyes, 2012). One particularly well studied example of this is how material incentives, such as fines, can crowd out pro-social motivation (Gneezy and Rustichini, 2000; Falk and Kosfeld, 2006). Fehr and Rockenbach (2003) use a modified TG, in which the investor can request a desired back-transfer, and in one treatment, can also impose a small fine on the trustee if they fail to meet the request. In their experiment, with German undergraduates, they find that imposing the conditional fine is counter-productive, as the presence of the fine crowds out pro-social motivations. Fehr and List (2004) replicate this finding among students and CEOs in Costa Rica. In Bartoš and Lively (2021) we use

a similar design in Afghanistan, and introduce an ethnic group treatment. In contrast to previous studies, we do not observe that the fine decreases back-transfers in either treatment. One explanation for this is that the subjects have very little experience with formal sanctioning institutions, and thus have weaker reactions to the presence of sanctions. Rather, the fine increases back-transfers in the out-group treatment and has no effect on transfer in the in-group treatment, suggesting that the crowding-out of pro-social motivation is sensitive to group identity. Developing countries typically have weak formal institutions, which creates a reliance on voluntary cooperation. If the introduction of formal institutions crowds out pro-social motivations, this could lead to worse outcomes in the short run, and is thus a topic that deserves further attention.

Experiments have been used to study corruption and its relationship to cultural norms and economic development (Banerjee et al., 2022). Barr and Serra (2010) develop a laboratory game in which "citizens" can pay bribes for corrupt services to "officials", which is mutually beneficial if both consent, but has a negative externality for a third group of subjects. Similar to Fisman and Miguel (2007)'s natural experiment on parking fines, they are able to predict corrupt behaviour among a group of international undergraduates living in England using the prevalence of corruption in their home countries. They find that the correlation decreases with time spent in the UK. Similarly, (Gächter and Schulz, 2016) use a die-rolling experiment to measure honesty (Fischbacher and Föllmi-Heusi, 2013), with university students in 23 countries around the world. They show a correlation between willingness to lie in the experiment and corruption at the country level when the students were born.

7 Discussion and path forward

Understanding the role of social preferences matters for the setting of developing countries with relatively lower level of formal institutional capacity. Social capital then plays increased role in business transactions, labor markets, provision of credit, insurance and social safety nets, commons management, intergroup relations, and kin and intrahousehold relations. Whenever these are less regulated by a well functioning state with reliable enforcement mechanisms, social preferences gain more importance.

In this chapter, we have discussed recent advances in the literature of measuring preferences in developing countries. While the core experimental games (DG, UG, TPPG, TG, and PGGs) remain the workhorses to study preferences, we have witnessed new developments on a range of fronts. First, new behavioral games were developed that help us understand a broader range of social preferences, including measures of antisocial preferences or of social pressure. Also, a range of methodological con-

tributions increased research credibility. Second, a set of unincentivized measures of social preferences have been developed and validated. These measures can be employed to measure preferences in large samples at relatively low cost. Third, new measures of social preferences using alternative measures and using treatment variations have been developed.

We also describe applications in which we have seen developments over the past decade. These range from intergroup conflict, evolution and stability of preferences, social learning, role of institutions, kinship ties, and the role of social networks. These applications span settings from labor markets, agriculture, financial markets, family economics, public policy, voting, public goods, common pool resource management, natural disasters, financial shocks, and beyond.

While a tremendous amount of work has been done, we see several pathways in which the literature may evolve. More thorough validations of survey measures from different settings may be necessary to understand if they are universally valid or whether there are major differences across cultures in how subjects understand and interpret questions. The role of incentives when studying social preferences should be studied more systematically, as has been the case for risk preferences (Vieider et al., 2015). We also need better explanations of when our preferred measures of social preferences correlate with relevant field behavior and when it is confounded by other factors. Existing studies that find correlations between behavioral game measures and real life behavior typically fail to explain the large degree of heterogeneity. A more systematic approach may help. Relatedly, understanding whether established findings from laboratory experiments hold across domains (e.g., Mousa, 2020) is another fruitful avenue. While, as we argue, behavioral games capture underlying preferences well, their relevance for policy may be limited if other factors overrule the role of preferences. Rao (2019) outline a pathway of evaluating the effects of policies based on "softer" economic outcomes, such as social preference. More studies can follow this path. Finally, targeting individuals with specific social preferences using tools such as manipulating career incentives in job postings (Ashraf et al., 2020) could be examined in settings beyond labor markets, for example in financial markets or in common resource pool management to improve their efficiency.

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