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- being part of a peasant organization is associated with an increase of 0.77 times the standard deviation of each dependent variable.
11. In many cases, more than one family member actively participates. Unfortunately, information on individual participation by household members is not available.
  12. This was calculated as (number of non-excludable activities - number of excludable activities)/number of total activities. This variable ranges from 1 to -1, with 1 representing the highest temptation to free-ride and -1 representing the lowest.
  13. A more complete discussion of the determinants of a peasant's decision to join a peasant organization is offered in Molinas (1998b).

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## 7. Measuring social capital: adding field experimental methods to the analytical toolbox

Jeffrey P. Carpenter\*

This chapter discusses the measurement of social capital using the method most often employed, standard survey techniques, and also using an underutilized method, economic experiments. The basic argument of the chapter is that investigators interested in social capital might increase the analytical power of their research by conducting experiments. More specifically, there are a number of experiments that, while designed by economists for other purposes, can also be usefully adapted for the field to validate survey results and measure social capital when one suspects that surveys may be biased by incentive and other problems.

The primitives of the concept of social capital are those characteristics of individuals including trust, trustworthiness, reciprocity, generosity and the propensity to cooperate in social dilemmas that make economic activity possible when contracts are hard to enforce. Presumably through some evolutionary process, behavioural conventions (for example, punishing free-riders or peer pressure) arise, allowing societies to accumulate social capital and steering them to more efficient outcomes. While social capital appears to be easily conceived of as a kind of interpersonal grease lubricating economic transactions, it is hard to measure and therefore the concept has not been widely adopted by policy makers.

The remainder of this chapter is organized as follows. The next section presents an argument for the use of economic experiments to measure social capital. The third section introduces four experiments that can be used to measure social capital (as defined above), while the fourth section reviews the results of the few studies that have used both methods to measure and study social capital. A final section concludes.

### THE LINK BETWEEN SURVEY AND EXPERIMENTAL MEASURES OF SOCIAL CAPITAL

The conventional approach to empirically studying social capital is to analyse survey data. Examples of this approach include Putnam (1995), Fukuyama

(1995), Molinas (1999), Glaeser *et al.* (2000), Isham and Kähkönen (2002, forthcoming), Grootaert *et al.* (Chapter 5 of the present volume) and Danriere *et al.* (Chapter 10, this volume). These studies typically test hypotheses concerning the effect of social networks or behavioural characteristics of individuals on economic outcomes using self-reported responses to survey questions. At the same time, there is also a smaller, but growing, literature interpreting the behavioural responses elicited in economic experiments as measures of social capital within a population (Fehr *et al.*, 1997; Berg *et al.*, 1995; Eckel and Grossman, 1996a) and between populations (Ockenfels and Weimann, 1999; Croson and Buchan, 1999; Roth *et al.*, 1991; Fershtman and Gneezy, 1998). Very few studies employ both methods to study the same population. As a result, complementarities between these two methods – such as the ease of gathering demographic data in a survey and the incentive compatibility of experiments – are rarely exploited. Further, any interesting relationships that may exist between the two types of data remain largely unexamined.

There are methodological reasons for simultaneously employing both methods. Survey-based research and experiments face a common fundamental methodological criticism: to what extent are the research methods used valid ways to answer important questions about economic behaviour? Research using self-reported survey techniques may be problematic because surveys do not use incentive-compatible mechanisms to ensure the truthful revelation of behaviour and, in some cases, respondents may perceive an incentive to lie (for example, 'Do you pay your taxes?'). Experiments are criticized because their external validity is open to question (for example, to what extent can we generalize the results of experiments conducted with students in a context-free laboratory setting?). As detailed below, this chapter argues that the weaknesses of each method may be largely offset by the strength of the other. The potential for complementarities between the two research methods suggests that a more complete understanding of social capital can be gained by combining experiments and surveys in field sites of particular interest.

### Some Problems with Surveys

Surveys are, probably, the best way to collect behavioural data when incentives are not an issue. There is no clear reason why the average respondent would misrepresent the truth when responding to questions such as 'How many social organizations do you actively belong to?' As such, demographic questions about behaviour are probably good measures of the depth of social networks. However, three concerns arise when using survey data based on behavioural questions: the 'hypothetical bias', the 'idealized persona bias' and 'incentive compatibility'.

To illustrate the hypothetical bias, consider the following survey responses to

a question about adhering to a critical societal norm.<sup>1</sup> Of the survey respondents (all from the United States) 77 per cent said that they would intervene if a group of teenagers were harassing an elderly person. This seems plausible, but 35 per cent said they would intervene when a man threatened someone else with a weapon. This number seems high and we can easily understand why. Saying you would help in a hypothetical situation is costless; actually doing so is not.

The idealized persona bias can also be illustrated by an example. Consider the following phrases, often used to measure trust: 'When dealing with strangers, one is better off using caution before trusting them', or 'How often do you lend personal possessions to friends?' The data resulting from these questions may be problematic because people often reply as the person they think the researcher would like them to be or as the person they would like to be.

Finally, survey results may suffer from the lack of incentive compatibility when participants do not feel the need to take a survey seriously (Bertrand and Mullainathan, 2001). Although respondents are often paid for the completion of a survey, there is no way to control if they paid attention while responding. Without giving respondents an incentive to take the survey seriously, even well designed questions may elicit 'noisy' responses. In contrast, in economic experiments, participants are given an incentive to pay attention because the decisions they make determine how much they are paid; not taking the experiment seriously will be costly.

There is growing evidence from experimental economics that survey-based measures of social capital may indeed lead to misleading results. Glaeser *et al.* (1999) compare the results of two trust experiments – a letter drop experiment designed by the authors and a version of the trust game developed in Berg *et al.* (1995) – with the responses to a social capital questionnaire that had been administered among the same subjects. The standard trust game has two phases. First, a player, the sender, decides how much of their show-up fee they would like to pass to another participant, the recipient, who is anonymous. The experimenter triples any money sent before it reaches the second mover. Second, the recipient then decides how much of the tripled amount to send back to the sender. The theoretic prediction of this game is straightforward: because any money sent to the recipient will be kept for sure, senders should send nothing. When this experiment is conducted in the laboratory settings, participants tend to send about half of their show-up fee and recipients send back, on average, the amount that they were sent. The amount sent by the 'sender' can be understood as a measure of trust and the amount sent back as a measure of reciprocity.

Glaeser *et al.* (1999, 2000) suggest that trust and reciprocity are not correlated with participants' responses to survey questions designed to measure social capital, controlling for other possible factors. Consider the following question: 'Generally speaking, would you say that most people can be trusted or that you

can't be too careful in dealing with people?' One's response to this question does not predict trusting behaviour in experiments where real money is a stake.<sup>3</sup>

Similar results have been found in studies conducted by Ben-Ner and Patterman (1999) who design an experimental two-sided dictator game experiment to examine and measure reciprocity between participants. In the dictator game, a 'sender' is given a sum of money to split with 'a recipient' who has no say in how the money will be divided. If the sender allocates anything to the recipient, the amount can be understood as a measure of generosity. Ben-Ner and Patterman add a second stage to this game: recipients are given a sum of money that they can share with the sender. The authors argue that any money given back during this stage is a measure of reciprocity. They then compare responses to survey questions with experimental behaviour. They find that established social capital indicators (for example, number of times the individual and his/her family enters guests, the past charitable giving of the individual and his/her family) have little predictive power. Moreover, the marginal effects have counterintuitive signs (for example, they find that the more a person's family volunteers or gives to charity, the less the recipient sends back to the sender controlling for how much was sent to her).

Burks *et al.* (2000) also find little correlation between participants' demonstrated trust and reciprocity and their responses to survey questions designed to measure social capital. Most notably, the authors find no relationship between how much participants actually trust 'strangers' in experiments and how much they say they trust strangers in general; how much they trust when there is little or a lot at stake; or how often they lend possessions or money to friends. As for reciprocity, the results also suggest no relationship between behaviour and self-reported measures of trust.

The comparisons of survey data and controlled experiments summarized here demonstrate the usefulness of running experiments to supplement surveys conducted in the field. Experimental methods will provide measures of social capital that are more representative of actual behaviour and therefore be a better basis for policy. At the same time, we stress that results from experiments in controlled laboratory settings do not provide definitive measures of social capital. Experimental economists are careful not to extrapolate from the behaviour they witness in the lab and do not assert that it can explain how things work in the more complicated real world.<sup>4</sup>

## EXPERIMENTAL MEASURES OF SOCIAL CAPITAL

In this section we introduce four economic experiments which, while designed primarily for other purposes (for example, testing the predictive nature of game theory), can also be used to measure aspects of social capital.

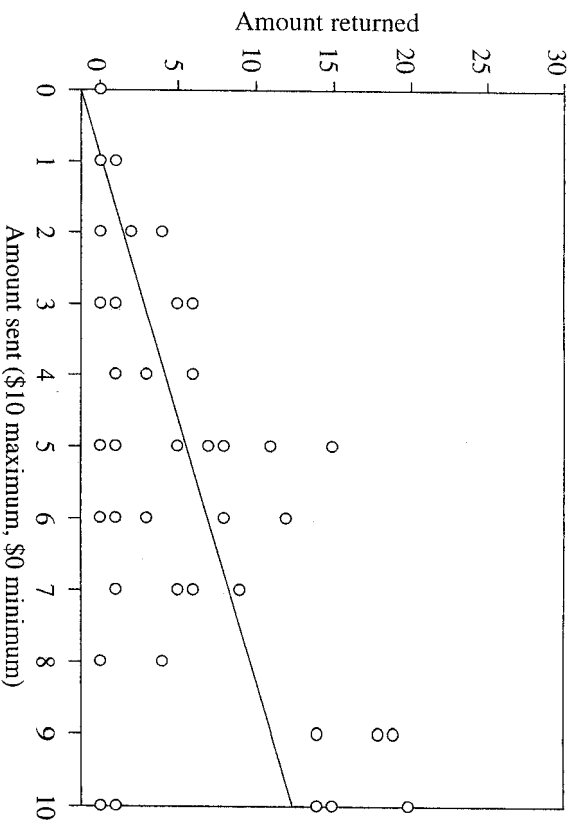


Figure 7.1 Student trust game results

### Trust Game

We begin with the 'trust' game mentioned above and developed first in Berg *et al.* (1995).<sup>5</sup> Remember that the theoretical prediction of this game is that the sender should send nothing, because the receiver, acting rationally, has no incentive to reciprocate. By contrast, in most laboratory settings, senders do send part of their money and recipients reciprocate by sending back about the same amount.<sup>6</sup>

For example, Figure 7.1 illustrates the results from an experimental study trust, among students at the University of Minnesota and Middlebury College.<sup>7</sup> Here, 91 per cent of the observations deviate from the game-theoretic prediction. In general, trust – the amount one sends – is rewarded by reciprocity – the amount that one receives. Each dollar sent is matched one to one, as illustrated by the slope of the linear regression line. This dynamic allows most of the participants to earn amounts that are significantly higher than predicted earnings. Hence the results of this experiment demonstrate how trust and reciprocity enhance efficiency.

### Ultimatum Game

The second game we will consider is the 'ultimatum game' first analysed in Gueth *et al.* (1982).<sup>8</sup> Like the trust game, the ultimatum game is also a

two-person, sequential game; however, the ultimatum game concerns the division of a set amount of money and therefore is well suited for measuring fairness norms.

The game proceeds as follows. The first mover, 'the proposer', is given the opportunity to propose a division of a set amount of money ('the pie'), and the second mover, 'the responder', then either accepts the proposal or rejects it. If the proposal is accepted, the pie is divided accordingly; if it is rejected, neither receives anything. This game also has a straightforward theoretic prediction. Assuming, again, that the responder is non-satiated in money, he should accept any proposal because rejecting a proposal yields a pay-off of zero. Anticipating this, the proposer should offer the smallest unit of account to the responder, being confident that he will then accept. The final theoretical division of the pie is 'e' for the responder and 'pie-e' for the proposer, where 'e' is the smallest unit of account.

Roth (1995) finds, in a study among students, that the modal offer is for half the pie (the mean is closer to 40 per cent), and offers of less than a quarter of the pie are routinely rejected. These results differ dramatically from the theoretical prediction, and it is reasonable to conjecture that the distance between the distribution of proposals and e is a measure of the strength of fairness norms operating in a population. The highest rejected offer may be seen as a measure of negative reciprocity.

Table 7.1 illustrates how the ultimatum game can be used to quantify differences in forms of social capital (in this case, fairness and negative reciprocity) between populations. The table is created by gathering data from four studies of the ultimatum game that used seven different participant populations. The first column reports the results of Gueth *et al.* (1982) who played the game with German students; the next four columns come from Roth *et al.* (1991). This experiment was conducted with students from Israel, Japan, Slovenia and the USA. The final two columns list data from non-student experiments done by Carpenter *et al.* (2001) with workers at a distribution centre in Kansas and by Henrich (2000) with Amazonian horticulturalists in Peru.<sup>9</sup>

There are a number of interesting contrasts across these studies. Note, in the first row, the differences in the mean offers. American, Japanese and Slovenian proposers make noticeably higher offers than German, Israeli and Peruvian proposers. The particularly low offer made by the Peruvians is notable: this set of participants, from the indigenous *Macheguenga*, who have the least contact with modern market society of the groups surveyed, behave the most like *homo economicus*! A plausible explanation for this result is that these Macheguenga, one-time nomadic people, have recently been resettled into villages; since they have been accustomed to dealing only with kin, they may not have developed fairness norms to deal with people outside of their

Table 7.1 A comparison of ultimatum game results

Study	Gueth <i>et al.</i> (1982)	Roth <i>et al.</i> (1991)	Roth <i>et al.</i> (1991)	Roth <i>et al.</i> (1991)	Roth <i>et al.</i> (1991)	Carpenter <i>et al.</i> (2001)	Henrich (2000)
Subjects	German students	US students	Slovenian students	Israeli students	Japanese students	US workers	Peruvian horticulturalists
Mean offer	35%	45%	44%	36%	45%	45%	26%
Number 50/50 offers	7 of 21	14 of 27	12 of 30	8 of 30	10 of 29	22 of 30	4 of 21
Highest rejected offer	20%	60%	50%	50%	55%	10%	10%
Pr(offer < e rejected)	29%	50%	67%	60%	57%	50%	10%

extended family (see Henrich, 2000). These results are consistent with suspicions that resettlements or other dislocating influences can dramatically alter forms of social capital.<sup>10</sup>

The frequency of fair offers (50/50) in each population is tallied in the second row. There are two outliers: the Peruvian horticulturalists on the low end and American workers on the high end. We have already discussed the low offers of the horticulturalists. About three-quarters of the proposals from the workers were for half of a \$100 pie. This mode is considerably higher than in the other studies. There are a number of contributing factors, which might explain this result. One explanation is that the workers deal intimately with each other on a daily basis; hence the social network existing among them is bound to be denser than those existing among anonymous students. As a result, the workers form a more cohesive 'in-group' which may contribute to a stronger fairness norm (Carpenter *et al.*, 2001).

Lastly, one can also compare the highest offers rejected, and probability of rejecting low offers to analyse differences in the propensity towards negative reciprocity across the populations. Beginning with the German students, American workers and the Peruvian horticulturalists, we see similarities in the highest rejected offer, but marked differences in the propensity to reject low offers. While the workers are quick to reject low offers, German students are more likely to accept low offers and the Peruvians accept almost all offers. Again, these differences might be explained by differences in the density of the social networks linking individuals. Members of a salient in-group are much more likely to reprimand other members and are less likely to be both-cred by the transgressions of outsiders (Bornstein *et al.*, 1996; Brewer, 1979; Kollock, 1996). Hence, from a social capital point of view, Table 7.1 can be viewed as summary statistics comparing the density of non-kin social networks in different societies. In this sample, workers in the USA appear to have very dense networks illustrated by the group sense of fairness and the propensity to punish deviations from fairness, while the Macheguenga, who have not internalized group fairness norms and do not punish greedy individuals, demonstrate weak social ties with others outside their kin group. An interesting analysis would include behavioural measures such as these as right-hand side variables in the standard social capital regression. On the evidence of Table 7.1, we might expect workers from the USA to go bowling with each other, but not the Macheguenga.

### Dictator Game

The 'dictator game' was originally developed by Forsythe *et al.* (1994) to test whether people made modal offers for half the pie in the ultimatum game because of fairness considerations or fear of negative reciprocity. However, the

dictator game has been frequently used to measure the altruism of participants. The game is run as follows. As in the ultimatum game, there is a first mover who decides on a split of some fixed pie. However, in this game the second mover simply gets whatever is allocated to her by the first mover – the dictator – and is not given the chance to accept or reject. Hence whatever the dictator allocates to the anonymous second player is a measure of his altruism.

In the dictator game the theoretical prediction is clear: if the dictator is non-satiated in money, he should not allocate anything to the second player. Although the theoretic prediction happens when the game is played, it is not the only behaviour that is observed. The distribution of proposals is to the left of the distribution of the proposals of the ultimatum game, but it does not collapse onto zero (Forsythe *et al.*, 1994).

The first three columns of Table 7.2 compare the results of three different experiments, which demonstrate the general level of altruism among student participants. As mentioned above, the original dictator game was played in Forsythe *et al.* (1994) which resulted in an average allocation of 23 per cent of the ten dollar pie and two modes at giving 30 per cent and giving nothing (column 1 in this table). Compare these numbers to the comparable numbers in the next two columns. In Hoffman *et al.* (1994), the experimenters increased the social distance between participants by making the experiment double blind: not only were the subjects unable to see what each other did, the experimenter also could not assign a specific response to an individual. The result is as would be predicted by a theory of behaviour in which social networks and social distance matter: allocations to the second party fell significantly, to only 9 per cent of the ten dollar pie.

Instead of increasing the social distance between players, Eckel and Grossman (1996a) effectively reduce it by changing the characteristics of the recipient. Rather than an anonymous student in the second player's role, the experimenters allowed dictators to make anonymous contributions to the Red Cross. Under this treatment, the average allocation increased to 30 per cent of the pie and new modes arose at 20 per cent of the pie and at half of the pie.

Table 7.2 A comparison of dictator game results

Study	Forsythe <i>et al.</i> (1994)	Hoffman <i>et al.</i> (1994)	Eckel & Grossman (1996a)	Carpenter <i>et al.</i> (2001)
Treatment	single blind	double blind	recipient	subject pool
Mean allocation	23%	9%	30%	50%
Modes	0%, 30%	0%	0%, 20%, 50%	50%

One interpretation is that dictators in this treatment looked at the experiment as an opportunity to invest in social capital (that is, to do the right thing socially). By contrast, increasing social distance allows participants to rationalize not giving in terms of an anonymous recipient who may not deserve one's charity.

Finally, we can consider the results of Carpenter *et al.* (2001). Compared to the single blind student game of Forsythe *et al.* (1994), workers in Kansas display significantly more generosity and altruism. Returning to the question of why this might be important for social capital investigators, these results demonstrate the relationship between the density of social networks, the average social distance between members of some specific population, and the level of generosity. Specifically, where the density of social networks is proxied by the number and importance of interactions people have with each other on a daily basis, and social distance is measured by the relative anonymity of daily interactions, we find that workers who interact very frequently in intimate settings to produce jointly their livelihood demonstrate more altruism (towards each other) than do students who interact in larger, more anonymous populations and with little at stake. This phenomenon suggests that one can play the dictator game with various populations to gather empirical measures of altruism and generosity for each population. In turn, these data can be used either to control for differences in an empirical analysis or to enter as explanatory variables alongside more traditional survey responses.

#### Voluntary Contribution Mechanism Game

The last experiment we will consider can be adapted to understand and measure the propensity to cooperate in social dilemmas (that is, when individual and group incentives differ). The 'voluntary contribution mechanism game', VCM, developed in Isaac *et al.* (1984), simulates the provision of a public good. For each individual there is a dominant strategy to not contribute any of one's endowment. This leads, in theory, to an inefficient equilibrium in which all players completely free-ride. However, a dilemma arises because, if all the players contributed fully, they would collectively reach the Pareto-efficient outcome. This game is interesting because it provides an incentive-compatible measure of cooperation among a participant population. The data are incentive-compatible because each player can maximize his monetary rewards by following the dominant strategy. This fact implies that deviations from the equilibrium (controlling for errors by stationary replication of the stage game) measure the propensity to cooperate (that is, there is a monetary cost to not defecting).

Table 7.3 provides a summary of the data from three different VCM experiments. The three experiments, Croson (1996b), Isaac and Walker (1988), and

Table 7.3 A comparison of VCM results (average contribution level)

Study treatment period	Croson (1996b)		Isaac & Walker (1988)		Carpenter (1999)*	
	Partners (%)	Strangers (%)	Communication (%)	No comm. (%)	Punishment (%)	No pun. (%)
1	56	41	100	54	55	49
2	53	40	88	44	57	54
3	46	36	96	35	68	50
4	50	32	92	28	67	49
5	50	28	100	15	69	41
6	48	33	93	23	65	37
7	39	27	100	17	68	36
8	30	20	100	16	67	41
9	36	21	100	8	75	45
10	19	10	100	13	71	38

Carpenter (1999), were chosen because they demonstrate three different methods that have been used to increase contributions to the public good.<sup>11</sup> As we will see, each manipulation of the standard game has implications for the study of social capital. The columns labelled 'Partners' and 'No comm.' represent two examples of the standard decay of contributions seen in VCM experiments. As one can see, contributions usually start, in period one of a ten-period game, at between 40 and 60 per cent of the endowment and then decay to between 10 and 20 per cent by the last period.

A number of explanations for this decay have been put forward and tested in subsequent experiments. We discuss two of the theories briefly and direct the reader to Ledyard (1995) and Andreoni (1988) for more detailed discussion of this topic. One theory explains the decay of contributions by learning. Initially, it is posited that participants have not learned the incentives of the game and when they do, near the end, they learn to free ride. Among other papers, Andreoni (1988) falsifies this theory in a clever experiment wherein participants play ten rounds and then groups are reshuffled and they immediately play another three rounds. Contributions leap from low levels to much higher levels at the beginning of the restart, which contradicts the learning hypothesis. The alternative theory has to do with reciprocity. As discussed in Fehr and Schmidt (1999) and Bowles *et al.* (2000), reciprocity requires that contributors retaliate against free-riders and the only way to do this in the standard VCM game is by withholding one's contributions.

Consider first the study by Croson (1996b). In this experiment participants are matched as 'partners' and remain in the same group for the entire ten periods or are matched as 'strangers' and are randomly reshuffled into new groups at the beginning of each period. The difference in the matching rule is interesting because it either fosters or dampens the possibility for shared norms to arise in repeated interactions. As one can see, the manipulation has a significant effect and cooperation is more sustainable when interactions are repeated. Next, the experiment by Isaac and Walker (1988), adds the possibility for communication between the participants of a group. Although communication is restricted so that whatever is said can only be interpreted as 'cheap talk' and therefore does not influence the standard prediction of complete free-riding, there is nevertheless a strong effect of communication. As one can see, in most periods the level of cooperation is above 90 per cent and often reaches the 100 per cent maximum. This study confirms what would be suspected from a social capital point of view: communication is key to sustainable cooperation. That is, communication allows the expression of shared norms and focuses the attention of the group on the appropriate norm – do not free-ride.

The last VCM experiment with implications for the study of social capital is the game designed by Fehr and Gächter (1999) and expanded on in Carpenter (1999). Here the standard game is changed so that group members

can monitor and punish free-riders at some personal cost. In the first stage, players choose contribution levels and, in the added, second stage, players see each other's contributions and have the ability to sanction each other. In effect, the new game adds a second-order public good to the first game. That is, because free-riders respond to punishment by increasing contributions in the future, the second-order public good is punishment. However, notice also that participants have to pay to punish someone else, which means no one should punish because it is costly to do so. Additionally, because the experiment uses the strangers' treatment (defined above) any benefits from getting a free-rider to contribute more in the next period will not accrue to the punisher because they will have been reshuffled into a different group.

Despite the theoretical prediction, considerable amounts of punishment are observed, which cause the level of cooperation to increase and be maintained. This result is seen by comparing the last two columns of Table 7.3. Note that this experiment is particularly interesting from the social capital point of view because the experimenter gathers data on both the propensity to cooperate and the propensity to punish free-riders.

## STUDIES THAT COMBINE SURVEYS AND EXPERIMENTS

To date, few studies have used both the standard survey method and the field experimental method for gathering data on the evolution of social capital within specific populations. In this section we will briefly review the work of three research groups which have used both techniques and arrived at interesting results.

The core of the first group consists of Edward Glaeser and David Laibson, who have produced two examinations of the microfoundations of social capital (Glaeser *et al.* 1999; Glaeser *et al.*, 2000). In the initial study, discussed above, the researchers used both methods to quantify trust and discovered a difference between trusting behaviour reported in a survey and the behaviour demonstrated in trust experiments. In addition, when combining their data into a meta analysis of trust, the investigators found the following results: (1) while behaviour in their experiments does not correlate with self-reported measures of trust, the data from the two experiments do correlate; (2) demographic data from their survey do seem to correlate with the behaviour in their experiments. Together, these facts argue for the use of both techniques to further understand how social capital is developed and maintained. As the authors concluded,

Finally, we believe that this paper shows the value of using experiments and surveys together. Experiments can measure personal attributes much more convincingly



than surveys. By connecting the two forms of evidence, we can determine the socioeconomic correlates of hard-to-measure personal attributes, and test the validity of survey measures of these attributes. (Glaeser *et al.* 1999, pp. 33–4)

In Burks *et al.* (2000), the researchers examine trust and trustworthiness. As mentioned earlier, they are also unable to predict trusting behaviour by survey responses. However, differently from Glaeser *et al.* (1999), this study also employs the Machiavellian Personality Profile first developed in Christie and Geis (1970). The 'mach scale' is a series of assertions derived from Machiavelli's *The Prince* with which respondents can either agree or disagree. The scale was constructed to identify instrumental behaviour a component of which is the willingness to violate social norms for one's own benefit and, consequently, the prior hypothesis of the researchers was that instrumental behaviour would correlate (negatively) with trust and, in particular, trustworthiness. If so, then social capital could be measured in a less direct, but much cheaper, manner using the mach scale because people who exhibited Machiavellian tendencies would be less trusting and trustworthy.

As it turns out, there is a relationship between observed trusting behaviour and the mach scale, but it is not as strong as originally hypothesized. After controlling for demographic characteristics of the participants, the mach scale significantly predicts the amount someone sends as a first mover in the trust game. More specifically, as people become 'higher machs' (that is, more instrumental), they send less to their counterpart and therefore appear to trust less. This relationship does not hold, however, for trustworthiness. Here higher machs are no more or less trustworthy after controlling for personal characteristics and the amount sent. Hence these results are an example of how to calibrate or modify the study of trust. Instead of asking direct questions about trusting behaviour, which do not seem to measure trust very well, one might use the mach scale as a proxy for trusting behaviour.

To conclude our analysis of studies which employ both data-gathering techniques we consider the work of Juan-Carmilo Cardenas, which is probably the most integrated example of social capital research because he not only utilizes an extensive survey and runs experiments, but he does so in the field with specific populations of participants. Cardenas has done an extensive study of cooperation in rural Columbia (Cardenas *et al.* 2000; Cardenas, 1999; also see Chapter 8 of this volume). In both papers, Cardenas examines the propensity of peasants to cooperate in a game similar to the VCM, called 'the common-pool resource game'.<sup>12</sup> In this game participants cooperate by refraining from overextracting a commonly held resource (such as a fishery or forest). In addition, he compares behaviour in the game with demographic characteristics of the individual participants (for example, sex, age and wealth) to find intuitive and interesting results.

Cardenas *et al.* (2000) examine the level of cooperation in villages where the participants actually face social dilemmas in their daily lives. In one region, participants gather resources from a local mangrove, which is held collectively. In another region, participants make handicrafts from fibres collected in a neighbouring, collectively held, forest. The authors show that peasants are able to restrict endogenously the consumption of a commonly held resource and the effect becomes much stronger when communication is allowed. In contrast, when the experimenter simulates a governmental solution to overextraction (that is, probabilistic monitoring by the experimenter who takes on the role of the government), the level of extraction increases significantly after an initial decline. Here Cardenas provides strong evidence of the implications of policy on endogenously maintained social capital. When government monitoring is introduced, the institution crowds out participants' inherent propensity to cooperate and the result is even more dramatic when compared to the near Pareto-optimal levels of extraction present when participants can regulate themselves using cheap talk.

In the second paper, Cardenas (1999), the author compares cooperative behaviour with survey demographics. He finds that actual wealth and occupation explain cooperation in addition to a constructed measure of social distance. More specifically, the results suggest that the participants who are more willing to cooperate have less real wealth, and are employed in occupations which revolve around commons dilemmas. Lastly, he also shows that, as the average social distance increases, participants cooperate less. These results are interesting and important because they contradict commonly held beliefs that influence policy. In particular, these results falsify the claim that the poor are more likely to destroy a commons (perhaps because of lower discount factors – another hypothesis that could be tested experimentally).

## CONCLUSION

Recent results from experimental economics indicate that research on social capital should be the domain of both survey and field experimental methods because the two complement each other. Surveys are perhaps the best method for collecting demographic data, but behavioural questions on surveys should be calibrated by experiments and experiments should be the primary source of information when incentives are important. Further, it is important to stress the external benefits of conducting both surveys and experiments. Once enough data has been collected on the relationship between true (that is, experimental) behaviour and self-reports of behaviour, it will be possible to write new surveys that are much less costly to conduct than field experiments and provide data that can be linked to and calibrated against real behaviour.

We have identified four experiments that are (relatively) easily adaptable for use in the field: the trust game which measures trust, the ultimatum game which measures the strength of norms of reciprocity, the dictator game which measures altruism and the voluntary contribution game which measures the propensity to cooperate. In fact, a number of these experiments have already been used in the field. Cooperation has been studied by Cardenas *et al.* (2000), trust has been studied by Barr (1999), and norms of fairness, reciprocity and generosity have been examined in Henrich (2000) and Carpenter *et al.* (2001). However, only a few studies have used both survey and experimental methods, suggesting that our understanding of social capital will continue to deepen as investigators increasingly measure behaviour, behaviourally.

## NOTES

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1. This survey, conducted in the USA is available at [www.planeproject.com](http://www.planeproject.com).
2. This problem has also plagued the contingent valuation approach to valuing environmental quality and resources (Harrison and Rutstrom, forthcoming).
3. Another reason survey measures may not predict behaviour is that respondents frame or generalize the questions asked differently than the researchers who collect the data. This is a framing issue that also can be addressed experimentally. Credit belongs to Tom Kelly for pointing this out.
4. One reason for the lack of external validity is that, while students are a nice cross-section of the general population (that is, the cheapest and most accessible cross-section), they may or may not have experience in the kind of social dilemma situations in which social capital is thought to be relevant. For example, students may consider free-riding on other group members when it comes time to write a collective term paper, but this is a much different decision than whether or not to defect in dilemma situations such as the provision of clean water in an urban slum or to restrain from overharvesting a common source of fish or some other regenerating, but fragile, resource.
5. The trust game is also used in Barr (1999), Cox (1999) and Croson and Buchan (1999).
6. The question of stakes often arises. While some experimentalists treat the stakes issue as a solved problem, citing the numerous ultimatum bargaining studies that have been run for as much as three months' wages which show very robust results (Cameron, 1998). The question is still open for other games such as the dictator game (see Carpenter *et al.*, 2001).
7. Figure 7.1 is generated by pooling the data from Berg *et al.* (1995) and Burks *et al.* (2000), which use mostly identical procedures.
8. The ultimatum game is perhaps the most studied bargaining experiment. Interesting variations of the standard game are Blount (1995), Croson (1996a), Eckel and Grossman (1996b) and Prasinjar and Roth (1992). Also consider Camerer and Thaler (1995), Roth (1995) and Davis and Holt (1993) for reviews.
9. Table 7.1 should be used for rough comparisons only because of protocol and stakes differences. One should also note that the Roth *et al.* (1991) data are from the first round of a ten-round game.
10. For another field experimental study making a similar point about the dislocating effects of resettlement, the reader should see Barr (1999) who uses the trust game.
11. Again, note that Table 7.3 is only useful for rough comparisons because specifics of the experiments often differ. Additionally, note that the data listed under the Croson (1996b) columns have been estimated from a graph in the original paper.
12. This game is studied extensively in Ostrom *et al.* (1992) and Ostrom *et al.* (1994).

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