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Measuring people's trust

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Measuring People's Trust

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ISER Working Paper
2007-32

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NON-TECHNICAL SUMMARY

We measure trust and trustworthiness in British society with an experiment using real monetary rewards and a sample of the British population. The study also asks the most typical survey question that aims to measure trust, showing that it does not predict 'trust' as measured in the experiment. Overall, about 40% of people were willing to trust a stranger in our experiment, and their trust was rewarded one-half of the time. Analysis of variation in the trust behaviour in our survey suggests that trust is more likely if people are older, their financial situation is 'comfortable', they are a homeowner, or they are divorced or separated. Trustworthiness is less likely if a person's financial situation is perceived by them as 'just getting by' or difficult.

Trust is an important lubricant for social and economic transactions. Higher levels of trust and trustworthiness can reduce transaction costs by allowing the use of informal agreements instead of complex contracts and their costly enforcement. For example, in many instances, a 'principal' delegates tasks to an 'agent' whose objectives are different, and the principal cannot check completely the agent's performance (e.g. bad outcomes could arise from low effort or bad luck). Even when possible, performance monitoring is often too costly to devise a contract that fully accounts for all the possible contingencies of the relationship. Costly contracting may preclude beneficial transactions in the absence of effective informal agreements based on trusting others to fulfil their part of agreement and abiding by such agreements when others trust us to do so. Even when interacting with strangers, we often work under the assumption that they can be trusted; e.g. taxi drivers trust that the passenger will pay the bill at the destination. Thus, opportunities for mutual gain are lost in societies in which people cannot trust each other. Our main aim is to obtain measures of the extent of trust and trustworthiness in British society for simple trust situations involving strangers, in which stakes are relatively small and there are no contractual obligations.

A common approach is to try to measure trust in surveys, through responses to questions such as the following (from the World Values Survey, General Social Survey (US) and British Household Panel Study): 'Generally speaking, would you say that most people can be trusted, or that you can't be too careful in dealing with people?' Answers to questions such as this are difficult to evaluate, however, partly because they are attitudinal and partly because the questions themselves are too generic: the answers as a result do not reveal either the reference group or the types of action or the stakes respondents have in mind when making such an assessment; and variations in any of these respects could be large.

Even if one thought that there was some value in asking this type of survey questions about trust, attitudinal questions about a person's trustworthiness would be useless for obvious reasons. Everyone would reply "yes, of course, I am trustworthy!", and some will be telling the truth, but we would not know which ones. As a result, the ample trust literature that relies on survey data is inevitably silent on the extent to which people's trust in others is an idiosyncratic disposition, or a belief or a response to actual trustworthiness. Without some measure of trustworthiness we have no idea of whether the level of trust reflects the level of actual trustworthiness.

An alternative approach, which has spread in recent times, is to measure trust and trustworthiness through experiments that use real monetary rewards (see Camerer 2003 for a review). This method has the advantage of providing behavioural measures and of being clearer about the type of situation, the stakes and the reference group – usually anonymous subjects in the experiment. Experiments too, however, have limitations, one of which is that they are administered mostly to students, who are usually self-selected subjects and not representative of the general adult population (e.g. compare the distributions for the 'laboratory' and 'representative" samples in Bellemare and Kroeger 2007, Figure 1). Laboratory experiments also usually lack a sufficient range and variation of information on individual attributes needed to permit an analysis of the individual-level correlates of both trust and trustworthiness.

In order to overcome those shortcomings our contribution follows a different approach. First, we combine the experimental method with both a representative sample *and* the survey method. We know of only two other instances, neither of which is in the UK, in which trust-game experiments have been carried out with a representative sample of the population and combined with survey questions – one

was carried out in Germany (Fehr *et al.* 2002) and gave us the original inspiration, the other in the Netherlands (Bellemare and Kroeger 2007). This approach makes it possible to obtain sound and representative *behavioural* measures of both trusting and trustworthiness, and in addition it allows the gathering of data on individual attributes.

Next, we use a new experimental design developed by Ermisch and Gambetta (2006) that differs in a number of ways from the trust game used in most other trust experiments. We believe that the differences we introduce allow it to provide better measures of the concepts of trust and trustworthiness.

The main aim of the paper is to demonstrate our methodology, which could be usefully applied to measure different variables experimentally, and present our main results. It proceeds as follows. First we explain the experiment that we carry out. Next we discuss our sampling frame and survey methods. The third section presents the main outcomes of the experiment and discusses how they may be affected by sample selection and their external validity. The fourth section compares behavioural and survey measures of trust and the fifth presents our conclusions about measuring trust. Overall, we find that about two-fifths of people are willing to trust strangers by giving them £10 in the expectation of receiving £22 if the stranger is trustworthy. About one-half of strangers fulfil their trust, resisting the temptation of keeping £40 rather only £18. We also find that there is no correlation between trust behaviour as measured by our experiment and answers to survey questions about whether most people can be trusted.

1. The Experiment

The game we presented to subjects, whom we shall call the truster (R) and the trustee (E), is a basic 'one-shot' trust game in binary form. We first describe a treatment in

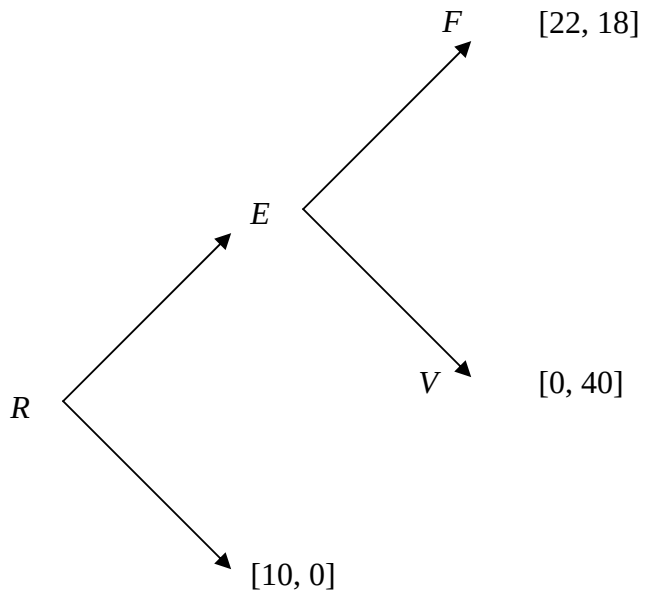
which R must decide whether or not to pass on the entire amount of money he receives, and then briefly describe a second treatment in which he can keep a small part of what he receives.

1. R receives £10, described as compensation for taking part in the interview *cum* experiment. A £10 banknote is given to him by the interviewer at the onset of his visit. R is then notified that he will have the opportunity to give the money to another person (E) with whom he has been randomly matched and about whom he is told nothing. R is further told that if he does so, the experimenter will increase it by £30 and that E will thus receive £40.
2. R is then told that E will be given the choice to either pay back £22 to R, or keep all £40. R is informed that if he decides to give £10, he will know the outcome in about 4 weeks, and in that case he will receive a cheque for £22 if E decides to pay back the specified amount. (Of course £22 in four weeks is not necessarily worth the same to a person as £22 now. But for £10 now to be worth the present value of £22 in four weeks, R's *monthly* discount rate would need to be 120%, or an annual rate in excess of 1400%. Even loan sharks charge less interest than that.) R is also informed that E will have no knowledge of R, nor will E's interviewer know who R is.
3. R is then offered the binary choice of either to keep £10 or to give it to E. He either puts the £10 banknote inside of a decision card or not, and puts the card in an envelope and seals it; the interviewer does not observe or otherwise know his decision.
4. These payoffs imply that R would be better off not to give the money to E if E chooses to keep it all, but better off to pass it on if E chooses to pay back the specified amount. The problem is that E, in terms of his self-interested monetary

payoffs, can do better by keeping it all; i.e. by not fulfilling the trust placed in him by R. If R thinks that this is what E will do then he should keep the £10. If R decides to pass on the £10 it means he 'trusts' in the precise sense defined by Bacharach and Gambetta (2001): that he expects E to resist the pull of his 'raw payoffs' and return the £22.

5. If R chooses to keep the £10 banknote, the game ends there.
6. If R chooses to pass the £10 on, E is offered, by a different interviewer who visits him at his home, a binary choice of whether to pay back £22 and keep the difference or keep the whole £40 he received. E is told that R was informed that E would be making this choice before R decided to pass on £10. E is given two cheques made out in their name: one for £40 pounds and one for £18, either of which he can cash without delay. Depending on how he decides, he keeps the appropriate one, puts the other cheque in an envelope along with the decision form and seals it; the interviewer does not know his decision.

The payoffs we used in the experiment are illustrated in the following chart (F='fulfill trust', V='violate trust'; R's payoffs listed first in brackets):



We have chosen the amount specified to be paid back in the case that E fulfils trust in a way that makes the payoffs asymmetric. Symmetric payments may encourage fulfilling trust for reasons of fairness, rather than because E does what is expected of her (e.g. see Bacharach *et al.* 2001, Bohnet and Huck 2004 and Bohnet *et al.* 2005); for instance, an equal split may be a focal point. The asymmetric payoffs also discourage E from fulfilling trust because the 'temptation' to not fulfil is larger.

In the second treatment, the protocol changes in the following way.

1. R receives £12 in cash (a £10 note and a £2 coin) as compensation for taking part in the interview *cum* experiment. Once again the cash is given to him when the interviewer arrives.
2. R is offered the binary choice of either keeping the whole £12 or giving £10 to E and keeping £2. He either puts the banknote valuing £10 inside of a decision card or not, and puts the card in an envelope and seals it; the interviewer does not know his decision. If R does choose to pass on the £10, the money available to E is increased by £30, and E will be told that R has kept £2.

3. The remaining steps are the same as in the previous treatment.

In the tree diagram above, R's payoffs are simply increased by £2 in each branch.

Thus, the two treatments differ in the following way. In the first treatment, R will face a draconian choice of whether to pass on all the money he received or nothing. In the second treatment R can keep £2 even if he passes £10 on. However, they are identical in two important respects. First, the rate of return on the transfer that R may receive is the same in both treatments. Second, their expected payoff is £(22+k)P +£k(1-P) if they pass on the £10, and it is £(10+k) if they keep it, where k is either 0 or 2 and P is the probability of the second stage person (E) sharing the money in the specified manner, as perceived by R. Then, in either treatment, their expected return from trusting is £22P-10. Then, ignoring discounting of future income, in either treatment, those who have a subjective estimate of $P > 10/22$ should trust, assuming of course that they reason in terms of maximising expected returns (if R's rate at which he discounts future income is r per month, then the present value of the £22 received in a month is $22/(1+r)$, and it is worthwhile to trust if $P>(1+r)(10/22)$. Thus, in so far as Rs use the rate of return or the expected return when making their decisions, the two treatments make no difference.

The computer assisted personal interview (CAPI) script for the experiment is given in Appendix 1. The outcome variables of interest are:

(1) the probability that R passes £10 on ('trusts');

(2) the probability that E pays back the £22 ('fulfils trust').

1.1 Key features of our design

Our design differs from that of the standard trust-game experiment (TGE) (Berg et. al. 1995, Glaeser *et al.* 2000), also used by Fehr *et al.* (2002) and Bellemare and Kroeger (2007). Ermisch and Gambetta (2006) provide a detailed critique of the standard TGE, arguing that the standard TGE lacks the basic features of a trust situation *even in a one-shot case*, and, as a result, this blurs the link with clear notions of trust and trustworthiness *and* introduces confounding effects. The binary TGE outlined above is more realistic than previous games and more precisely captures a clear notion of trust and trustworthiness. This is because:

(a) R 'gains' his money as compensation for taking part in the interview *cum* experiment and this is given to him in cash and before the interview begins. We expect that this should trigger an 'endowment effect' and make R treat it more as his own money rather something that he can envisage passing over to E as a gift or because of a win-splitting sense of fairness; this should make R more careful in parting with it.

(b) By forcing R's exposure to a loss of either all (or 83% in the second treatment) of the sum he receives as a participation payment, we depart dramatically from the standard TGE, in which R can pass on *any* amount. The distribution of amounts passed on in standard TGE experiments usually covers the entire range, with a distinct modal value of about 50% of R's initial endowment and short 'spikes' at zero and 100% (see Fehr *et al.* 2002; Bellemare and Kroeger 2007 (their 'representative sample') and Barr 2003). In our view, the possibility of transferring any amount favours the intrusion of other motives such as 'gift giving', 'let's risk part of it', 'I like to gamble' (a phrase we heard in our cognitive interview testing in preparing the experimental script). The actual distribution of R's transfers in many experiments is

consistent with the operation of these other motives. This confounds these motives with trust, which is a relevant consideration in the decision if and only if R's decision is driven by his self-interested aim to gain more if trust is fulfilled.

(c) In the standard TGE, E can return any amount to R. In our design, R knows in advance how much he can expect back from E if E fulfils trust, and E knows that this is an expectation of R when he makes his decision. E thus knows that he cannot be just a little more or a little less trustworthy; he has to choose whether to be trustworthy or not. Thus, in this binary TGE it is clear what trusting and trustworthiness are, as is common in real life (e.g. making and repaying a loan). Of course, in real life, those engaged in monetary transactions often make a contract concerning the terms of their exchange, whereas in our design E only agrees to be in the experiment and neither does he ask for any money nor of course does he promise to do anything in particular with it. Still, though falling short of the 'contract conditions' that often regulate trust situations, we believe that our design strongly encourages E to put himself in the situation of a person who has benefited from a rewarding investment arising from R's action and is expected to behave accordingly.

A variant of the TGE in Fehr and Rockenbach (2003) uses a solution that could also overcome the problems caused by the freedom of E to return any amount: R is required to specify a 'desired back-transfer' from E, and E is informed about it. In this way E knows also that R does have expectations and is not simply acting because of generosity. (Their paper does not report the percentage of Es who make a back-transfer at least as large as the one desired by R, which would be relevant for trustworthiness if paying such an amount constituted the trustworthy act, suggesting that when they introduced this variant the authors did not have this effect in mind.) However, we chose not to pursue this solution, instead leaving the task of specifying

the amount expected to us. This has a practical advantage in terms of simplifying the matching between R and E. Furthermore, it has the advantage of standardizing the amount expected in return so that all Es respond to the same condition. If one left R free to establish how much he wants to receive from E, too much variation would potentially be introduced, thus making it difficult to establish the extent to which trustworthiness was affected by the desired back-transfer established by R – greedy R could be punished by E and receive nothing or not as much as requested back.

2. The Sample and Survey Methods

The sample frame was households who were formerly members of the British Household Panel Survey; they were dropped from the panel for technical and funding reasons in 2001 and were re-interviewed in 2003 for a special study (see Appendix 2 for more information). We randomly selected one person from each household. An advance letter asking these persons whether they would agree to participate explained that we would be 'running an experiment on how people make simple financial decisions' as well as asking questions from a short questionnaire similar to earlier ones. An advantage of using a sample drawn from people who have participated in the BHPS for a number of years is that they are likely to believe that the field organisation and the managers of the survey can be trusted to carry out the experiment and payments in the way that they promise. Trust responses in a new survey are likely to be contaminated by varying degrees of trust in the organisers of the experiment, in addition to trust in their co-player. In addition, these subjects are used to receiving compensation (a voucher) for their participation.

The experiment and subsequent interviews were carried out face-to-face by professional interviewers at the subject's home. Interviewers were instructed to read only from the experimental script (see Appendix 1) and not to elaborate further. If the

subject had difficulty understanding, they were instructed to read that particular part of the script, or the whole script, again. At the conclusion of the full interview, interviewers were asked to report whether the subject understood ('easily', 'very easily', 'not easily' or 'not very easily') what they were being asked to do in the experiment. Among R-subjects, 94% understood 'easily' or 'very easily', but among E-subjects this percentage was only 83%; the difference is statistically significant. We find this pattern surprising because the R subjects had a more difficult and riskier decision than Es (Rs had to anticipate what E would do in response). Interviewers were also asked how worried the respondent seemed by being asked to take part in the experiment; 95% reported them being 'not at all' worried, 4% 'somewhat' (6 Rs and 3 Es) and 1% very worried (1R and 2Es).

We used the same survey organisation that carries out the BHPS. As a consequence, many of the respondents knew the interviewer from previous contact through the BHPS. For example, 63% had the same interviewer as they had in the last year they were interviewed (2003). On the one hand, this is an advantage because it reinforces trust that the experiment will be carried out as described. For example, Eckel and Wilson report that a large proportion of subjects participating in a one-shot trust game did not believe that they were matched with a real person (2004; p.458, fn.9). On the other hand, subjects may be more predisposed to trust and be trustworthy because they know the interviewer, despite the fact that we reiterated a number of times in the interview script that the interviewer would not know the subject's decision (see Appendix 1). We test for this below. Interviewers reported the respondent's cooperation was 'very good' or 'good' in 99% of the cases.

The experiment was carried out first. The two treatments were applied to one-half of the sampled Rs each. *After completing the experiment*, the respondents filled

out a short confidential self-completion questionnaire on their own that followed up their decision, which we analyse below. They then completed a short questionnaire with the interviewer (CAPI) and finally another confidential self-completion questionnaire containing the twelve questions from the General Health Questionnaire (GHQ), two questions about willingness to take risks and six questions for assessing two of the 'big five' personality traits—'openness' and 'neuroticism'. The CAPI questionnaire allowed us to update some basic information, such as current employment, marital status, homeownership and financial situation, at the time of the experiment. Finally, because running an experiment on a field sample is novel, the interviewers filled out a short questionnaire about how the experiment had gone; e.g. did the subject appear to understand it well (see above)? Were other people present? On average, the experiment took 10 minutes and the remainder of the survey about 15 minutes.

Deciding whether to trust or not clearly involves risk, as it is basically a bet on the trustworthiness of a stranger (e.g. Eckel and Wilson 2004). The first risk-related question (not previously used in the BHPS, in contrast to the other questions) is: 'Are you generally a person who is fully prepared to take risks or do you try to avoid taking risks?' Please tick a box on the scale, where 0 means 'unwilling to take risks' and the value 10 means 'fully prepared to take risks.' This question was asked in the 2004 wave of the German Socio-Economic Panel (GSOEP). Dohmen *et al.* (2005) conduct a complementary field experiment and find that this measure is a good predictor of actual risk-taking behaviour. They also find that this 'general willingness to take risk' measure does better at predicting risk behaviours than a standard lottery question to measure risk preference. The second question also comes from the

GSOEP. It asks about willingness to take risks not in general but in *trusting strangers*, again on an 11-point scale.

In the first stage (i.e. Rs), 173 of 245 eligible cases were interviewed, a response rate of 71%. In the second stage (Es), 85 of 127 eligible cases were interviewed: a response rate of 67%. The sample targets of about 200 first stage interviews and about 80 second stage interviews were based on our expectation (based on previous laboratory research reported in Snijders and Keren 1999) that about 40% of Rs would pass on the money. Almost all of the non-response arose because of inability to locate the selected individual (only one refusal). We randomly matched E-players to the Rs who passed on the money. It turned out that there were 10 Es who were unmatched because we exhausted the sampling frame for Rs: only one person from each household was selected for interview and we were unable to locate a significant minority because they had moved house and we had no new address. As a consequence, we were unable to interview as many Rs as we planned. As these 10 Es thought they were responding to Rs (and at that stage we thought they were too!), it is still valuable to analyse their decisions.

In 81% of all interviews, no other people were present in the household during the experiment *cum* interview. Among the remainder, while 7 subjects (4 Rs and 3 Es) asked another household member for advice about their decision, in only one case did the interviewer report that another person influenced the answers during the experiment 'a fair amount' (an E-subject), and in one other case an R-subject was reported to have been influenced by another person during the experiment 'a little'. Five per cent of the subjects (4 Rs and 9 Es) asked the interviewer for advice, but it was of course not given. Thus, it appears that the experimental responses were individual ones.

We now investigate whether decisions vary with the interviewer. In the first instance, consider a simple decomposition of variance of latent variables for propensities to trust and be trustworthy, respectively, assuming they follow a logistic distribution. For trust (giving £10), 10% of the variance can be attributed to the interviewer, a proportion that is not significantly different from zero at the 0.05 level, but is so at the 0.10 level. For trustworthiness (returning £22), 12% of the variance can be attributed to the interviewer, but this is not significantly different from zero at the 0.10 level or less. In any case, such 'interviewer effects' may not arise because of the interviewer *per se*, but rather could be local area effects, because most interviewers operated in only one sample area and most areas had only one interviewer. Proportions of variance attributable to interviewer area are 8% and 22% for trusting and trustworthiness, respectively, neither of which is significant at the 0.05 level, but both are so at the 0.10 level.

Another possible source of variation with interviewer concerns the subject's previous experience (familiarity) with the interviewer. As noted earlier, 63% of the interviewers in the 2007 trust survey are the same as the person had in the 2003, and 31% had the same interviewer in 2003 and in the 2001 BHPS as well as in our experiment in 2007. Those who had the same interviewer in 2001, 2003 and 2007 are more likely to give £10 (51% cf. 40%) and to return £22 (57% cf. 46%), but the differences are not statistically significant (p-values of 0.18 and 0.36, respectively). Subjects who had the same interviewer in 2003 and 2007 are more likely to give £10 (46% cf. 38%) and more likely to return £22 (56% cf. 40%), but again the differences are not large enough to be significant (p-values of 0.33 and 0.17, respectively). Nevertheless, we should not discount entirely the possibility that familiarity with the

interviewer may encourage people to be more trusting and trustworthy, perhaps because it makes them feel they are ‘among friends’.

As Appendix 2 indicates, the sample is not representative of the general population—women, low-income households and older people are over-represented. Table 1 compares our sample with the 2005 BHPS. Our sample over-represents women, people who are retired, older, divorced or separated, those who describe their financial situation as ‘difficult’ and under-represents homeowners and people whose financial circumstances make them feel they are ‘comfortable’ or ‘doing alright’. The sample can probably be taken as representative of households with low to moderate income. The focus on lower income people may have its advantages because the sums of money involved in the experiment may be more important to them.

Table 1: Comparison of Trust Survey and 2005 BHPS Respondents*

Variable	Mean, Trust Survey	Mean, 2005 BHPS
Age	53	48
Female	0.60	0.53
Homeowner (outright or mortgage)	0.44	0.77
In paid employment	0.46	0.56
Retired	0.32	0.25
Financial situation:		
• 'Comfortable'	0.21	0.31
• 'Doing Alright'	0.33	0.41
• 'Finding it quite or very difficult'	0.14	0.06
Divorced or separated	0.23	0.10

*BHPS weighted with cross-section weights for Great Britain

3. Outcomes and Inferences

Our basic figures are that 43% of Rs passed on £10 ('trusted'), and 51% of Es returned the specified £22 (were 'trustworthy'). This should give us a snapshot of the level of trust and trustworthiness in British society in simple trust situations involving strangers, in which stakes are relatively low and there are no formal contractual obligations for E, but in which there is the knowledge that R's decision was taken on the expectation that £22 would be returned. The two treatments (i.e. R receives £10 or £12) did not produce significantly different outcomes, and so we no longer distinguish between them.

After the Rs had made their decision and inserted it in the sealed envelope, the interviewer gave the participant a short sheet of questions concerning how they made their decision. These were filled out in private and put in another sealed envelope so that the interviewer did not know how they replied. The first question was: *When you made your decision about whether to give £10, did you weigh up the chances of getting your money back?* Nearly 80% of Rs answered 'yes'. This is reassuring for

our measure of trust because 'trust' is by nature an expectation (Gambetta 1988; Barr 2003) and entails a self-interested aim to gain more if trust is fulfilled. There are, however, a number of issues that need to be addressed before concluding that these figures summarising subjects' behaviour in the experiment represent how, on average, they behave in real life trust situations.

3.1 Self selection

The first issue, which may invalidate inferences, is *self-selection*. Unlike in laboratory experiments in which self-selection occurs because students volunteer, in our experiment people did not volunteer; they had the option to refuse to participate, but only one did. In our case self-selection only arises indirectly from non-response, which occurred almost entirely because we could not locate target sample members who had moved house. Since we know that the probability of moving in a particular year is inversely related to length of current residence (e.g. Morrison (1971) for an early study and Belot and Ermisch (2006) for evidence from the BHPS), we check whether non-response may have affected our inferences by relating the odds of giving the £10 (among Rs) and returning the £22 (among Es) to length of residence. We found no significant effects. Thus, it appears that those who move more often do not behave differently in terms of trust and trustworthiness.

A second issue is *our sample frame* and its ability to represent the British population. Table 1 showed that it differs from the British population in a number of potentially important dimensions, and so it is interesting to explore whether trust and trustworthiness vary along these. Table 2 presents logistic regressions for the odds that Rs give £10 and the odds that Es return £22. The regressions suggest that Rs are more likely to trust if they are older, their financial situation is 'comfortable', they are

a homeowner, or they are divorced or separated, while E's are less likely to be trustworthy if they find their financial situation is 'difficult' or they are 'just getting by', and more likely to be trustworthy if they are divorced or separated. We use estimated regression coefficients like those in Table 2 to create an artificial world that looks like 2005 Britain along these dimensions. We generate predictions based on the assumption that the mean values of age, sex, financial situation, homeownership, marital status in the British population in 2005 (estimated from the BHPS) are given (non-stochastic) and that a model like those in Table 2 is correct, using bootstrapping (100 replications) to obtain the standard error of the predictions. For trusting the mean predicted percentage is 41%, with a 95% confidence interval of 28% to 54%. For trustworthiness, the mean prediction is 57%, with a confidence interval of 39% to 74%. Thus, not surprisingly, our overall estimates fall well within the confidence intervals for predictions based on the regression models and the covariate means for a representative sample of the British population.

Table 2: Logistic regression for the log odds of trusting and trustworthiness (standard error in parentheses, corrected for clustering on interviewer)*

Variable	(1) Give £10	(2) Return £22
Age	0.031**	-0.000
	(0.010)	(0.016)
Female	0.19	-0.19
	(0.36)	(0.44)
Financial situation (ref. 'Comfortable'):	--	--
• **Doing Alright**	-1.30**	-0.64
	(0.56)	(0.62)
• **Just about Getting By**	-0.99	-0.91
	(0.62)	(0.56)
• **Finding it Difficult**	0.11	-1.61**
	(0.76)	(0.67)
Homeowner	0.79**	0.48
	(0.29)	(0.52)
Divorced/separated.	0.81**	1.13*
	(0.41)	(0.60)
Constant	-2.086	0.636
N	170	84
Wald chi2(8)	31.86	11.18
p-value	0.0000	0.1310

**Statistically different from zero at 0.05 level
*Significantly different from zero at 0.10 level.

3.2 External validity

Even if our results were perfectly representative of what the British population would do when participating in an experiment such as ours, it still does not follow that their behaviour in the experiment carries over to real life trust situations. Levitt and List (2007) analyse how behaviour in experiments may be influenced by a number of factors, and how these influences can affect inferences about 'pro-social behaviour' such as trust and trustworthiness. We consider the factors they mention in turn.

3.2.1 Scrutiny and Anonymity

Scrutiny by investigators in experiments is likely to exaggerate pro-social behaviour relative to environments without such scrutiny. Absence of anonymity between

subjects and interviewer, such as familiarity with interviewers (discussed above), and between subjects tends to work in the same direction. In our experiment, we used procedures that ensured that interviewers did not know the participant's decision (e.g. the sealed envelopes), and we clearly pointed this out to participants in the interview script (Appendix 1). Of course, participants were aware that someone was going to know their decision, but also that 'that someone' would not know who they are and would have no connection to them or their network. In addition, participants had no information at all about their counterparts, guaranteeing strict anonymity between subjects. The procedures in our experiment substantially reduce the bias due to experimental scrutiny or absence of anonymity, and should certainly do better in this regard than most laboratory experiments, in which students know they are interacting with another student. Not only is the other student likely to be similar to them in many respects, there is also a good chance that, while they do not know who their co-player is, they know that the person could be someone they know, in part because friends sign up for the experiment together. In our case subjects might have been aware of playing with other survey subjects living in Britain, but in all other respects their co-players were perfect strangers.

3.2.2 Stakes

Levitt and List (2007) suggest that in games like the trust game, financial concerns increase in prominence relative to pro-social behaviour as the stakes increase. We did not vary the stakes, but our payoffs are relatively large compared to most other TGEs, in both laboratories or with representative samples. In Fehr *et al.* (2002), the initial stake was 10 Euros and in Bellemare and Kroeger (2007) it was 5 Euros, and this was 'virtual money' to be settled later, compared to £10 cash in our study. In both of these studies, the amount passed on by R was doubled, making the stake for Es at

most 10-20 Euros (plus a 10 or 5 Euro endowment, respectively) and much less than this for the typical transfer by R. In our study, the £40 payoff to Es from not fulfilling trust is a substantial temptation to do so. Nevertheless, inferences from our experiment may only be valid for real life trust situations with low to moderate stakes.

3.2.3 Social context

Social context matters for people's decisions in experiments, and the experimental researcher does not have complete control over the context within which the subject makes decisions. The studies by Henrich *et al.* (2004), which used the same protocols (including payoffs and description of the game—the so called 'ultimatum game' was used) in 15 different small scale communities, provides a good example. The context participants brought to the game, such as past experiences and social norms, influenced the outcomes. This is a problem only to the extent to which one wants to keep those influences out of the experiment. However, since our objective is to measure trust and trustworthiness in British society we want to tap into those experiences etc. that subjects draw from real life trust situations. We do not want them to think of the game as a purely artificial exercise to be played as a board game. It is precisely their drawing on their social context that should give the experiment external validity. We believe that our design makes it highly likely that Es' decisions reflect their habitual practices in these situations – their sense of reciprocity, fairness or sensitivity to obligations and the Rs' decisions reflect their expectation of such behaviour.

4. Survey questions vs. behavioural measures

Could we have short-circuited the experiment, at least for measuring trusting behaviour, by just using a survey question? The common question about trust which has been run in the BHPS and many other surveys – "Generally speaking, would you

say that most people can be trusted, or that you can't be too careful in dealing with people?" – has been used in around 500 papers that analyse the economic effects of trust (according to Sapienza *et al.* 2007), and in particular it has been employed to study trust in Britain as a function of individual attributes and measures of neighbourhood attachment, social networks and civic participation (Li et al 2005). Table 3 shows that this question has absolutely no predictive power for people's trusting behaviour as measured in our experiment. It weakly suggests, however, that those who answer that most people can be trusted are more likely to be *trustworthy* in the experiment. Although the differences are not significant for the Es either, the pattern of results matches the findings by Glaeser *et al.*. (2000) and Sapienza *et al.* (2007). This suggests that some of those who say that others are mostly to be trusted may be inferring from, as it were, a sample of one, namely from their own behavioural inclinations. This result matches other experimental findings. ('In a succession of experimental studies exploring the circumstances surrounding cooperation in n-person prisoner's dilemmas, we have collected data about subjects' expectations of others behaviour. One of our most consistent findings throughout these studies – a finding replicated by other's work – is that *cooperators expect significantly more cooperation than do defectors.* . . . [there are further] findings consistent with expectations being dependent on the actor's own behaviour . . ." (our emphasis, Orbell and Dawes 1991: 519)).

Table 3: Attitudinal questions and Trust

Behaviour	**Most people can be trusted**	**Can't be too careful**	**Other, depends**	*p-value*
% of Rs giving £10	43.1%	42.7%	44.4%	0.995
N	*58*	*103*	*9*	*170*
% of Es returning £22	55.3%	43.6%	57.1%	0.547
N	*38*	*39*	*7*	*84*

Laboratory evidence from University of Chicago MBA students provided by Sapienza *et al.* (2007) suggests that the standard survey question reflects people's expectations about other's trustworthiness. We can test this hypothesis with our data using the follow-up questions after Rs had made their decision. The 80% of Rs who said that they had weighed the chances of getting their money back were asked the follow-up question: *What did you think your chances of getting your money back were?* The first row of Table 4 shows that those who were more optimistic about receiving £22 in return were more likely to say that 'most people can be trusted', suggesting that the survey question captures to some extent the expectations component of trust decisions.

The second row of Table 4 indicates that the person's expectation of the chances of return is strongly related to their trust decision, with more optimistic Rs being consistently more likely to trust, which is also reassuring for our trust measure. Nevertheless, the failure of the survey question to predict trusting behaviour in the experiment evident in Table 3 indicates that there is either insufficient content about expectations in the survey question, or that there is another dimension which plays an important role in addition to expectations, or both. This second dimension could be the disposition or preference individuals have to take risks, namely to allow themselves to be exposed to the potential opportunism of others.

Table 4: Chances of Return and Trust

Behaviour	**Less than 50/50**	**50/50**	**More than 50/50**	*p-value*
1. % who say 'most people can be trusted'	27.8%	39.6%	80%	0.036
2. % of Rs giving £10	23.2%	69.2%	80%	0.000
3. Willingness to take risks *in trusting strangers*, Scale=0-5	81.8%	75%	40%	0.087
4. General willingness to take risks, Scale=0-5	65.2%	56.9%	40%	0.406
N	72	52	5	
(*N*, row 3/4)	(66/69)	(52/51)	(5/5)	

Few studies have studied the impact of risk attitudes on trust. Our risk measures show that, similar to the German data (GSOEP), the two risk scales are correlated (r=0.46), but respondents are less willing to take risk in trusting strangers than they are willing to take risk in general. This is evidence that, when the outcome depends on the behaviour of others, risk is perceived differently, and may reflect 'exploitation aversion' (Bohnet and Zeckhauser, 2004): R's aversion to being taken advantage of by E, which is different from the risk of losing in a game of chance or in the stock market. The third row of Table 4 indicates that willingness to take risks in trusting strangers varies with a person's perception of the chances of return, with more risk-averse people being more pessimistic. While the general willingness to take risks varies in the same way, the relationship is not statistically significant.

We test whether the 11-point 'willingness to take risks' scales affect trust and trustworthiness. There was no evidence that 'general willingness to take risks' affected either type of behaviour, a result consistent with Eckel and Wilson (2004) for trust behaviour, for which risk is relevant, but not with Sapienza *et al.* (2007). Table 5 shows that Rs who express more willingness to take risks *in trusting strangers* were

more likely to pass on the £10. Thus, there appears to be some behavioural content in this question.

Table 5: Willingness to Take Risks in Trusting Strangers and Trust

Behaviour	**Scale 0-5**	**Scale 6-10**	*p-value*
% of Rs giving £10	38.3%	54.8%	0.093
N	128	31	159

When this dichotomous indicator for taking risk in trusting strangers (scale 6-10=1) is included in a regression with the other variables in the first column of Table 2, its coefficient of 1.01 is significant at the 0.04 level. Using the full 0-10 risk scale as a continuous variable, its significance level is 0.089. Because this risk scale is negatively related to age, but not significantly related to the other variables in Table 2, there is an increase in the age effect on the odds of trusting when we control for willingness to take risks in trusting strangers.

Nevertheless, if we used the dichotomous risk variable in Table 5 to predict whether people trust in the experiment, we would be wrong for 38% of the cases who are 'risk averse' according to this measure (scale 0-5), and for those who are not risk averse, we would be wrong for 45% of the cases. Overall, we would be wrong for 40% of the subjects.

5. Conclusions

We conclude that behavioural responses to experiments like the one carried out here provide reliable measures of trust and trustworthiness, which are superior to the common survey questions regarding trust. The cost of running these experiments is low relative to the cost of making contact with participants. Thus, the marginal costs of these measures are low when a representative survey is being undertaken for other

purposes. Our results also suggest that a question concerned with willingness to take risks in trusting strangers is a rough but cheap way to monitor changes in propensity to trust over time in repeated surveys.

Overall, about 40% of people were willing to trust a stranger in our experiment, and their trust was rewarded one-half of the time. Analysis of variation in the trust behaviour in our survey suggests that trust increases with age, although this could be a generational effect. Also, people whose financial situation is comfortable are more likely to trust, as are homeowners. Trustworthiness is less likely if a person's financial situation is perceived by them as 'just getting by' or difficult. Taken together with the results for trusting, this suggests that trust in strangers and trustworthiness toward them are 'luxuries' that some people cannot 'afford'. To our surprise, people who are divorced or separated are both more likely to trust and more likely to be trustworthy.

Should the experiment be carried out repeatedly in panel surveys like the BHPS and the new UK Household Longitudinal Study, as was done by Fehr *et al*. (in a study not yet published) with three consecutive waves of the German Socio-Economic Panel (2003-2005; they sampled about 1500 persons from the 12,000 households in the Panel)? Although the case is not entirely straightforward, we think there are very good reasons for doing so. The potential problem inherent in panel repetition is that subjects might start treating the experiment as a 'game' rather than tapping into those experiences which subjects draw from real life trust situations in one-shot games. On the other hand, it would be interesting to study how subsequent play of Rs is affected by having trust rewarded or violated, although perhaps unclear about how to make inferences about real life trust situations from this behaviour.

Panel repetition can also be valuable for assessing causal influences as it can allow for unobserved individual heterogeneity as well as dynamics.

One way that we could use experiments in panel surveys to monitor changes in trust behaviour over time, while avoiding the potential problem arising from panel repetition, is to run the experiment on different small subsets of the panel sample each year. In other words, create a series of repeated cross-sections from within the panel. This would also be relatively cheap because the payment arrangements (cash and cheques) need only be set up for this sub-sample each year. Having repeated measures of trusting and trustworthiness would give us a simple yet precise sense of the relative health of social relations in the country.

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Appendix 1: Experiment Script (CAPI)

[This is a printout of the CAPI computer screens, familiar to our interviewers]

LIVING IN BRITAIN

SPRING 2007

QUESTIONNAIRE

D0a. DATE OF INTERVIEW

DAY		MONTH		YEAR			

FOR INTERVIEWER REFERENCE

(T1 = 0 & T2 = 0)
"This respondent is ineligible for interview."

(T1 = 1 & T2 = 1)
"This is a first-stage respondent. You will be using **PINK** coloured materials for this respondent.

Please ensure that this respondent has received the card with the **£10 note**. Before you start, make sure the respondent has opened it and seen the money."

(T1 = 1 & T2 = 2)
"This is a first-stage respondent. You will be using **PINK** coloured materials for this respondent.

Please ensure that this respondent has received the card with the **£10 note and £2 coin**. Before you start, make sure the respondent has opened it and seen the money."

(T1 = 2)
"This is a second-stage respondent. You will be using the CREAM coloured materials for this respondent.

Please hand over the two checks for £40 and £18 at the appropriate point in the experiment."

THE FOLLOWING STATEMENT MUST BE READ TO ALL RESPONDENTS:

This interview is completely voluntary -- if we should come to any question that you don't want to answer, just let me know and we'll go on to the next question.

For R-subjects

SC1 CHECK
IF T1 = 1 & T2 = 1 GO TO SC1 (£10 OPTIONS)
ELSE IF T1 = 1 & T2 = 2 GO TO SC1 (£12 OPTIONS)
ELSE IF T1 = 2 GO TO SC5

SC1 ***INTERVIEWER NOTE***
THIS RESPONDENT SHOULD HAVE RECEIVED {£10} (IF T2 = 1) / {£12} (IF T2 = 2) IN CASH.
PLEASE MAKE SURE YOU HAVE HANDED OVER THE CARD WITH THE {£10 NOTE} *(IF T2 = 1)* **{£10 NOTE PLUS £2 COIN}** *(IF T2 = 2)* **BEFORE YOU START AND MAKE SURE THE RESPONDENT HAS OPENED IT AND SEEN THE MONEY**

READ OUT
As you know, we are running an experiment on how people make simple financial decisions. In this experiment we randomly matched you with another person. The other person does not know your identity or anything about you and I know nothing about the other person. I will have no contact with this person. They will be interviewed by another interviewer following your interview.

Let's start by explaining a bit more about the experiment and the choice you need to make. We have given you {£10} (if T2 = 1) / {£12) (if T2 = 2). This money is yours to keep as a "Thank you" for participating in this survey. In this experiment, however, we are giving you the opportunity to give {this £10} (if T2 = 1) / {£10 of this} (if T2 = 2) to the person we have randomly matched you with. If you give {£10} (it T2 = 1) / {£10 of this} (if T2 = 2) to the person we matched you with, we will add £30, so that the other person receives £40. We will then ask them to decide

whether to return £22 of this to you and keep £18,
<u>OR</u>
whether to keep the £40

The other person is absolutely free to choose either option.

Your decision needs to be made in private so please do not tell me now, even if you know immediately what you are going to do.. I will never know what you decided.

HAND LAMINATED SHOWCARD SIDE 1 TO RESPONDENT
READ OUT

You must decide whether or not to give £10 to the other person.

If you decide **not to give the £10**, your participation in the experiment ends. We will just finish off the rest of the interview.

If you decide to give the £10, you may receive £22 back, or nothing. You will find out about the other person's decision and receive payment, if any, in about four weeks.

In this experiment both you and the other person are free to decide what you want to do. There is no 'correct' decision.

CONTINUE ☐

SC2 **HAND THE RESPONDENT THE ENVELOPE AND ASK THEM TO TURN OVER THE LAMINATED CARD TO SIDE 2 FOR THE INSTRUCTIONS**

READ OUT

If you decide NOT to give the £10 to the other person, you should put the empty card in the envelope.

If you decide to give the £10 to the other person, put the £10 note inside the card and put it in the envelope.

Please seal the envelope before you hand it back to me. Someone else will open the envelope and I will never know your decision.

Can you please make your decision now. I will leave the room / turn my back so you can make your choice in private. Please take as long as you need to make your decision.

Interviewer code:

Envelope with card returned ☐

SC3 **ONCE THE RESPONDENT HAS HANDED BACK THE SEALED ENVELOPE WITH THE CARD PASS THEM THE SINGLE PINK PAGE SELF-COMPLETION QUESTIONNAIRE TO COMPLETE TOGETHER WITH ANOTHER ENVELOPE**

READ OUT

We would next like you to answer a few questions concerning your decision. Can you please complete the questions on the sheet and then seal it in the envelope before you hand it back to me. I will turn my back again so that you can complete the questions in private.

Interviewer code:

Questionnaire returned ☐

SC4 **READ OUT**

Thank-you. We will process your decision and, if you gave £10 and the person we paired you with returns £22, we will send you a cheque for £22. In any case, we will notify you about the outcome. This should take about four weeks.

That is the end of the experiment but I just have a few other questions I'd like to ask you. This will take only 5 or 10 minutes.

CONTINUE ☐ **GO TO H0**

For E-subjects

SC5 **READ OUT**

As you know, we are running an experiment on how people make simple financial decisions. In this experiment we randomly matched you with another person. The other person does not know your identity or anything about you. I myself have had no contact with this person. They have already been interviewed by another interviewer.

Let's start by explaining a bit more about the experiment and the choices you can make about what to do with £40. This £40 has been made available to you because of the decision made by the other person when they were interviewed. This is what has happened so far.

The other person received {£10} (IF T2 = 1) / {£12} (IF T2 = 2) from us for taking part in the experiment.

We told them that they would have the opportunity to receive £22 if they chose to give you £10. They were told that if they gave you £10, we would add £30 to make £40, which is the amount you now have available.

The other person made their decision knowing that you would be asked to decide whether to keep £18 and return £22 to them or keep all £40.

They decided to give you the £10 knowing this was the choice you would be making.

Your decision needs to be made in private so please do not tell me now, even if you know immediately what you are going to do. I will never know what you decided.

HAND OVER THE CREAM SHOWCARD SIDE 1
READ OUT

We now ask you to decide whether you want to …

Keep the £40
<u>OR</u>
Keep the £18 and return £22

Let me stress that you are absolutely free to choose either option and that the other person knew you would be free to choose. There is no 'correct' decision.

CONTINUE ☐

SC6 **HAND THE RESPONDENT THE ENVELOPE WITH THE CHEQUES AND ASK TO OPEN**
ASK TO REFER TO THE CREAM SHOWCARD SIDE 2
HAND THE RESPONDENT THE CREAM DECISION CARD AND ENVELOPE

READ OUT

Here are two cheques made out to you, one for £18 and the other for £40.

Can you look at this card and decide whether you want to keep £40 OR keep £18 and return £22 to the person who made the £40 available to you.

Put both the card with your decision and cheque in the envelope and seal it before handing it back to me. Someone else will open the envelope and I will never know your decision.

Let me stress again that you are absolutely free to choose either option. There is no 'correct' decision.

So that you can make your decision in private, I will leave the room/turn my back. Please take as long as you need to make your decision..

Interviewer code:

Envelope with card returned ☐

SC7 **ONCE THE RESPONDENT HAS HANDED BACK THE SEALED ENVELOPE WITH THE DECISION CARD PASS THEM THE SINGLE CREAM COLOURED SELF-COMPLETION QUESTIONNAIRE TOGETHER WITH ANOTHER ENVELOPE**

READ OUT

Next, we would like you to answer a few questions. Can you please complete the questions on this sheet, then seal it in the envelope before you hand it back to me. I will turn my back again so that you can complete the questions in private.

Interviewer code:

Questionnaire returned ☐

SC8 **READ OUT**

Thank-you. You can cash the cheque that you kept immediately.

That is the end of the experiment but I just have a few other questions I'd like to ask you. This will take only 5 or 10 minutes.

CONTINUE ☐ **GO TO H0**

Appendix 2 – BHPS sample

Our experimental study aimed to draw a sample of about 300 individuals from the 'low income' sub-sample of the UK part of the European Community Household Panel Survey (ECHP). This sample was interviewed annually from 1994 to 2001 – on eight occasions in total. Since 1997 it was administered by the Institute for Social and Economic Research (ISER) and undertaken jointly with the British Household Panel Survey (BHPS) activities. The sub-sample was selected in 1997 from the ECHP Great Britain panel. Selection was based on characteristics associated with low income – direct income data was not available at that time. Households were eligible if all adult members had been interviewed in the previous wave, and one of the following applied: household reference person unemployed currently or in the last year; household reference person receiving lone parent benefit; rented housing; receipt of means-tested welfare benefits.

Funding for the ECHP expired in 2001, but respondents who participated in the survey in 2001 were interviewed once more in early 2003 for purely methodological purposes (see Jäckle et al 2004). Interviews were sought with all ECHP sub-sample members who responded at the 2001 BHPS, that is, with 1,163 individuals in 781 households. New entrants and those not interviewed at the previous wave were not interviewed; eligible movers were followed to their new address. Interviews were completed with 88.8% of the 1,163 eligible adults. Non-interviews were due to untraced moves (2.8%), refusals either by the individual (3.2%) or the entire household (3.5%), or other non-coded reasons (1.6%).

The 2003 sample is not representative of the GB resident population in 2003, because low-income households are over-represented. Additionally, the sample is affected by attrition between 1994 and 2003. But the sample still contains a wide

range of population groups, and the existence of a number of earlier waves of data for this sample provides a very rich background of information on these individuals. To quote Jäckle et al (2004), their 2003 sample "over-represents the older population and under-represents younger age groups compared to population estimates. Employment rates and educational qualifications are lower in the survey sample, as are the proportions of owner-occupiers and consumption indicators such as the number of cars in the household. As to the causes of these differences, the age distribution is clearly affected by attrition: younger sample members are less likely to complete the interview. The survey also seems disproportionately to lose men, individuals with intermediate qualifications (A or O-levels, CSE), self-employed or unemployed, and those receiving zero benefits. For other employment and benefit characteristics the differences between respondents and non-respondents are not significant. Differences compared to the population estimates might therefore be attributed solely to the initial sampling process. However, the small number of employees may be masking differences caused by attrition."