

Measuring People's Trust*

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Summary

We measure trust and trustworthiness in British society with a newly designed experiment using real monetary rewards and a sample of the British population. The study also asks the 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 either 'comfortable' or 'difficult' compared with 'doing alright' or 'just getting by', 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', and more likely if they are divorced or separated. We also analyse the impact of attitudes toward risks on trust.

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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). Everyday examples include taking your car to a mechanic, hiring a baby-sitter and employing a research assistant. Even when performance monitoring is possible, it 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, trust is important for the smooth functioning of society; e.g. taxi drivers must trust that the passenger will pay the bill at the destination. Thus, opportunities for mutually beneficial transactions are lost in societies in which people cannot trust each other.

The primary aim of our research 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 even though there are no contractual obligations ‘trustees’ are clear about the trusters’ expectations. We also study how trust and trustworthiness vary by certain characteristics of the British population.

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 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. Nevertheless, this question has been used to measure trust 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).

Even if one thought that there was some value in asking this type of survey question 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, p.83ff., 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, are often carried out with limitations, one of which is that they are administered mostly to students, who are usually self-selected subjects and unrepresentative of the 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 investigate the individual-level correlates of both trust and trustworthiness.

In order to overcome those shortcomings our contribution follows a different approach. We combine the experimental method with both a sample from the general population *and* the survey method. 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. We know of only two other instances, neither of which in the UK, in which trust-game experiments have been carried out with a 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). Other trust studies have also used population samples (e.g. Barr 2003 and Schechter 2007), but ones that are less representative of the population than these two studies.

In addition, 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 trust experiments. We believe that the differences we introduce allow it to provide better measures of the concepts of trust and trustworthiness.

Our main aim is to measure levels of basic trust and trustworthiness in British society in interactions between strangers, including their variation with the population, and also to illustrate the methods that we use. While not novel in its broad outline, the particular constellation of methods that we adopt is original, and its interest may go beyond the study of trust as it could be applied to investigating other decisions. The paper proceeds as follows. First we clarify what we mean by ‘trust’ and then explain the experiment that we carry out. Next we discuss our sampling frame, procedures, survey methods and the external validity of our experimental

measure. The fourth section presents the main outcomes of the experiment. The fifth section compares behavioural and survey measures of trust and the sixth examines the impact of risk attitudes on trust. The seventh 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 than 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. What do we mean by ‘trust’?

We work with a notion of trust that relates it to specific acts and makes it easy to capture it empirically (Bacharach and Gambetta 2001). We trust when *we trust that someone will do X* – repay a loan, arrive on time, play fair, pay the fare, feed the cat, treat baby well, do his job as expected. The trust that we have in someone doing X does not necessarily extend to trust in that same person doing Y.

More precisely, we say that a person ‘trusts someone to do X’ if she acts on the expectation that he will do X when both truster and trustee know that two conditions obtain:

(i) if he fails to do X she would have done better to act otherwise – ‘if I knew he was a cheat I would not have lent him the money’. If trust is fulfilled the truster is better off than she would be if she had not trusted, but if trust is not fulfilled she is worse off.

(ii) her acting in the way she does gives him the opportunity to pursue a selfish reason not to do X – ‘if I hadn’t lent him the money he could not have cheated me’.

A trustworthy trustee is simply one who does X when those two conditions obtain.

The decision whether to trust or not involves three components. First, we can expect that subjects will consider the *returns* to trusting relative to the cost of trust being unfulfilled. Next, there is the *expectation* that the trustee will do X, framed in terms of a probability (Gambetta 1988; Barr 2003), and, thirdly, in all cases in which the probability is less than 1, a person’s willingness to take *the risk of being exploited* comes into play. In our study we do not vary the monetary returns and concentrate on the second and third dimensions only.

The level of expectation is the results of beliefs about other people’s trust-warranting qualities with regard to doing X. These beliefs are in turn based, at least in part, on *learning through experience*. In most real life circumstances these beliefs refer to specific persons or groups of persons whom we believe share certain trust-warranting properties. When interacting with anonymous strangers, trusters’ beliefs can be understood as being not *ad personam* but about the frequency of trust-warranting properties relating to doing X in the population of anonymous trustees – in our case the generic group consisting of people living in the UK and participating in the experiment. The willingness to take the risk of exposure captures a preference or disposition relating to the character traits and state of mind of the truster; in addition, this preference may be responsive to the nature of the trust situation itself: a person may be more willing to take risks in lending one’s car than in employing a baby-sitter.

2. Experimental design

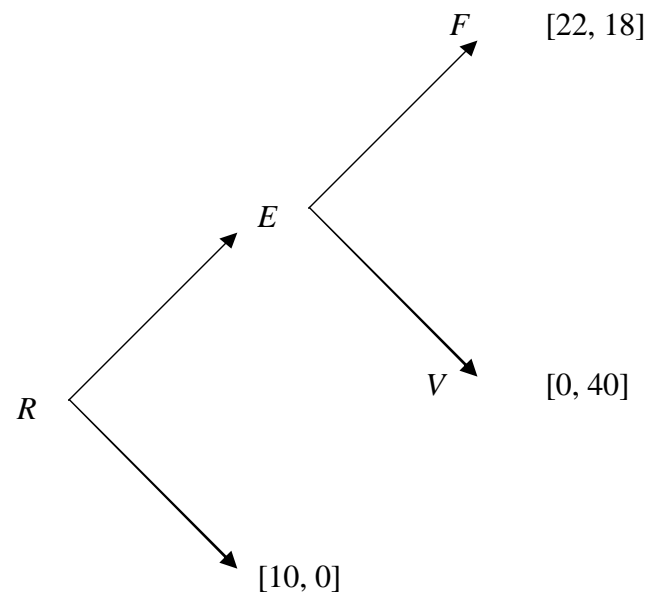
2.1 The experiment

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. R receives a £10 banknote at the onset of a professional interviewer’s visit to his home; it is described as compensation for taking part in the interview *cum* experiment. R is told that he will have the opportunity to obtain £22 if he gives the money to another person (E) with whom he has been randomly matched and about whom he is told nothing. He is told that the experimenter will increase it by £30 and so E will receive £40, who then will be offered 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 if E decides to pay back the specified amount he will receive a cheque for £22, else he will be left with nothing. (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%!)

R makes his choice by either putting the £10 banknote inside of a decision card or not, and then putting the card in an envelope and sealing it. The interviewer does not observe or otherwise know his decision. If R chooses to keep the £10 banknote, the game ends there. 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 his 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. Thus, our procedures are 'double blind' in the sense that neither the interviewer nor the researcher knows a particular subject's 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.

As in the definition of trust above, 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 decides to pass on the £10 it means he ‘trusts’ in the precise sense defined above: that he expects E to resist the pull of his ‘raw payoffs’ and return the £22.

For one-half of the sample we used a second treatment, in which R receives £12 in cash (a £10 note and a £2 coin) as compensation for taking part in the interview *cum* experiment. R is offered the binary choice of either keeping the whole £12 or giving £10 to E and keeping £2. The remaining steps are the same as in the previous treatment. There was no significant difference in behaviour between these two treatments, and so we will not distinguish between them in the rest of the paper.

The computer assisted personal interview (CAPI) script for the experiment is given in Appendix 1. The outcome variables of interest are: the probability that R passes £10 on (‘trusts’) and the probability that E pays back the £22 (‘fulfils trust’).

2.2 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 the standard one 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 more careful in parting with it, as he would be with his own money.

(b) We depart dramatically from the standard TGE, in which R can pass on *any* amount, 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. In our view, the possibility of transferring any amount favours the intrusion of other motives such as 'gift giving', or 'let's risk part of it'. The distribution of amounts passed on in standard TGE experiments is consistent with the operation of these other motives. It 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). Freedom to pass on any amount confounds these other 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). 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. Despite being strangers, E is in the analogous position of the agent to R's principal.

3. The Sample and Survey Methods

3.1 The sample

The sample frame was households who were formerly members of the British Household Panel Survey (BHPS); they were dropped from the panel for technical and funding reasons in 2001 and were re-interviewed in 2003 for a special study (Jäckle et al 2004). 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 as 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, our subjects are used to receiving compensation (a voucher) for their participation.

3.2 Procedures

The experiment was carried out first. It and subsequent interviews were done 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 subjects understood what they were being asked to do in the experiment. The vast majority (94% of Rs and

83% of Es) understood 'easily' or 'very easily'. We randomly matched an E-player to each R who passed on the money.

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), and 31% had the same interviewer in 2003 and in the 2001 BHPS as well as in our experiment in 2007. 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, knowing the interviewer may subconsciously incline subjects to trust or be trustworthy in the experiment, 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). Those who had the same interviewer in 2001, 2003 and 2007 are more likely both 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 differed in a similar direction, but again the differences are not large enough to be significant.

3.3 Survey structure

After completing the experiment, the respondents filled out a short confidential self-completion questionnaire on their own that followed up their decision. They then completed a 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

(not previously asked in the BHPS) 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. On average, the experiment took 10 minutes and the remainder of the survey about 15 minutes.

3.4 Survey response

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%. Apart from one refusal, all of the non-responses arose because of inability to locate the selected individual. 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.

3.5 How representative is the sample?

Table 1 compares our sample with the 2005 BHPS on some key dimensions. Our sample over-represents women, people who are retired, older, divorced or separated, and those who describe their financial situation as ‘just getting by’ or ‘difficult’, while it under-represents homeowners and people whose financial circumstances make them feel they are ‘comfortable’ or ‘doing alright’. Being conservative, the sample can at least be taken as representative of households with low to moderate income. (The focus on lower income people may also have its serendipitous advantages because the

sums of money involved in the experiment may be more important to them.) Analysis in Ermisch et al. (2007) strongly suggests that the percentages trusting and being trustworthy should be similar to those for a more representative sample of the population.

3.6 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. Scrutiny by investigators in experiments may exaggerate pro-social behaviour relative to environments without such scrutiny. Absence of anonymity – whether between subjects or between subjects and interviewer (such as familiarity with interviewers which we discussed above) – 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). In addition, participants had no information at all about their counterparts, guaranteeing strict anonymity between subjects, and the investigators also do not know the identity of subjects.

It has been suggested that in games such as the trust game, financial concerns increase in prominence relative to pro-social behaviour as the stakes increase (Levitt and List 2007). 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 (1 Euro=0.8 British pounds). 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 (50 Euros) payoff to Es from not fulfilling trust is a substantial temptation to do so. On the other hand, compared to experiments with samples in poor countries (Barr 2003 and Schechter 2007), our stakes are relatively small. It is safe to say that inferences from our experiment may only be valid for real life trust situations with low to moderate stakes.

In so far as beliefs are based on experience, past experience is important for people's decisions in experiments. For example, the studies by Henrich *et al.* (2004), which used the so called 'ultimatum game' with the same protocols (including payoffs and description of the game) in 15 different small scale communities, found that the past experiences and social norms participants brought to the game influenced the outcomes. In the more technical language of Hoffman *et al.* (1996; p.655), '...subjects bring their ongoing repeated game experience and reputations from the world into the laboratory [the experiment]...' Because our objective is to measure basic trust and trustworthiness in British society we want to tap into those experiences etc. that subjects draw from real life trust situations. 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. (Laboratory experiments by McCabe *et al.* (2003) using binary trust games similar to that used here, in which R's expectation is known to E, suggest that E's attribution of intentions to R is important in E's decision to fulfil trust or not.)

4. Experimental Outcomes

Our basic figures are that 43% of Rs passed on £10 ('trusted'), and 50% of Es returned the specified £22 (were 'trustworthy'). These should give us a snapshot of the level of basic trust and trustworthiness in British society in simple trust situations involving strangers, in which stakes are relatively low and there is the knowledge that R's decision was taken on the expectation that £22 would be returned.

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 subjects 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.

We now consider how the decisions in the experiment vary with personal characteristics. Appendix Table 1 provides descriptive statistics for the variables, and 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, are a homeowner, are divorced or separated or are active in organisation. (A potential problem with inclusion of the organisation variable is that those who are more disposed to trust may be more likely to be active in organisations.) Those who describe their financial situation as 'doing alright' or 'just about getting by' are less likely to trust than those are 'comfortable' or 'finding it very difficult', suggesting a U-shaped relation between trust and financial situation. The former may feel

comfortable enough to risk £10, while the latter may feel so poor that they may just as well risk £10 in the hope of gaining £22.

E's are less likely to be trustworthy if they find their financial situation is 'difficult' or they are 'just about getting by', suggesting that trustworthiness may be a 'luxury' that some cannot afford. E's are more likely to be trustworthy if they are divorced or separated.

In studying the impact of other personal attributes, we impose two statistically acceptable restrictions, because of our small sample size. One is that the reference group for marital status contains all but the divorced and separated, and the other is that the reference group for financial situation includes those who are 'comfortable' and 'finding it difficult', whom we find to be more trusting as opposed to those 'doing alright' and 'just about getting by', whom we find to be less trusting. In the first column of Table 3, we introduce three new variables compared to Table 2. These are factor scores for two of the 'big five' personality traits—'openness' and 'neuroticism'—and the third is an indicator of mental health—scoring 3 or more on the 12-point 'caseness' indicator taken from the GHQ questionnaire. There is no evidence that these personality traits affect trust, but those in poorer mental health are less likely to trust. The other variables have effects similar to those in Table 2, although regular activity in an organisation is no longer statistically significant. None of these three additional variables affect the odds of being trustworthy (results not shown).

Comparisons with the previous two studies using representative population samples (Fehr *et al.* 2002 and Bellemare and Kroeger 2007) are limited by different dependent variables (amounts sent and returned rather than 'trust or not' and 'be trustworthy or not'), use of the 'strategy method' by Bellemare and Kroeger (2007)

for studying trustee behaviour (statement of the amount they would return for all 11 possible amounts they could receive) and different covariates included in the analyses (e.g. questions about subjective beliefs about the behaviour of other players but not marital status or mental health). Within these constraints, there are some similarities and differences with our results. We do not find a U-shaped age effect for trust behaviour, and we find trust increasing with age, while their results have amounts sent declining with age in the neighbourhood of the mean age in our sample. We use a person's own perception of their financial circumstances rather than *personal income*, and we allow for and find a U-shaped effect of financial circumstances on trust. The other two studies do not make this allowance and find an insignificant income effect. Bellemare and Kroeger (2007) find that women send more money, and we too find that women are more likely to trust, but the impact is not statistically significant, unless we control for risk preferences (Table 7 below).

5. Survey questions vs. behavioural measures

Could we have short-circuited the experiment, at least for measuring trusting behaviour, by just using 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?”? Table 4 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)). In particular, Schechter (2007, p.281) finds that 'the share the player returns to the trustor when playing the role of trustee is highly correlated with amount he sends to the trustee when playing the role of trustor.'

In the second column of Table 3, we use the same variables to account for variation in trust measured by the positive response ('most people can be trusted') to the survey question. As in Bellemare and Kroeger (2007), the differences are remarkable. The variables that did well in predicting trust using the behavioural measure in the first column are not statistically significant, and people who are more open and less neurotic appear more likely to trust according to the survey measure. The only similarity is in the impact of mental health. Thus, it appears that the survey question is not helpful in analysing how trust varies within the population.

Laboratory evidence from University of Chicago MBA students provided by Sapienza *et al.* (2007) suggests that the standard survey question reflects only 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 5 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 5 indicates that the person’s expectation of the chances of return is strongly related to their experimental 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 4 indicates that there is either insufficient content about expectations in the survey question, or that there is another dimension that 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.

6. Risk preferences and trust

Few studies have studied the impact of risk attitudes on trust, despite the fact that the trust decision is basically a bet on the trustworthiness of a stranger (e.g. Eckel and Wilson 2004, Schechter 2007). The first risk-related question that we asked 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 is the same question that 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.

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’ or ‘betrayal’ aversion (Bohnet and Zeckhauser, 2004; Bohnet et al 2008): R’s aversion to being taken advantage of by E is different from the risk of losing in a game of chance or in the stock market.

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) or Schechter (2007). By contrast, Rs who express more willingness to take risks *in trusting strangers* were both more likely to respond that ‘most people can be trusted’ in the survey (second row of Table 6) and to pass on the £10 in the experiment (first row of Table 6). Thus, there appears to be some behavioural content in this question. Nevertheless, if we use the dichotomous risk variable in Table 6 to predict whether people trust in the experiment, we would still 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.

In Table 7, we test further whether the full 0-10 scale for willingness to take risks in trusting strangers affects trust as measured by either the experiment or the

survey question. In the first column, it is positively related to the experimental trust measure, although only statistically significant at the 0.11 level. Because this risk scale is negatively related to age and being female, but not significantly related to the other explanatory variables, there is an increase in the age effect and in the impact of being female on the odds of trusting relative to Table 3. The impact of the risk willingness scale on the survey trust response in the second column is large, and its inclusion reduces the impact of the personality traits, because more open people are more willing to take risks in trusting strangers and more neurotic persons less willing. This strong impact suggests that, contrary to Sapienza et al. (2007), the survey trust question contains information not only about expectations, but also willingness to take risks in trusting strangers. Indeed, risk aversion in trusting strangers significantly predicts responses to the survey question *within* each of the two main expectations groups: 25% cf. 60% ('less than 50-50') and 35% cf. 69% ('50-50 or more'). Despite this direct relationship between the risk willingness indicator and a positive response to the survey question, the latter does not predict experimental trust well because within each risk group of Table 6 nearly half of the subjects' survey response contradicts their experimental behaviour.

7. Conclusions

We conclude that behavioural responses to experiments like the one carried out here provide measures of trust and trustworthiness, which are both reliable and 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 (but not trustworthiness) 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 we cannot rule out a generational effect. Also, people who are homeowners are more likely to trust, while those in poorer mental health are less likely. Surprisingly, both people whose financial situation is 'comfortable' and those who are 'finding it difficult' are more likely to trust. Trustworthiness is less likely if a person's financial situation is perceived by them as 'just getting by' or 'difficult', suggesting that trustworthiness toward strangers is 'luxury' that some people cannot 'afford'. 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 their experiences 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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Table 1: Comparison of Trust Survey and 2005 BHPS Respondents*

Variable	Mean, Trust Survey	Mean, 2005 BHPS
Age	53	48
Female	0.61	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
• Just about Getting By	0.32	0.22
• 'Finding it quite or very difficult'	0.14	0.06
Divorced or separated	0.19	0.10
Active in organisation on regular basis	0.46	0.45

*BHPS weighted with cross-section weights for Great Britain

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	0.038** (0.012)	-0.014 (0.025)
Female	0.17 (0.28)	-0.03 (0.53)
Financial situation (ref. 'Comfortable'):	--	--
• Doing Alright	-1.29** (0.60)	-0.66 (0.62)
• Just about Getting By	-1.06* (0.61)	-1.24* (0.72)
• Finding it Difficult	-0.21 (0.78)	-1.84** (0.76)
Homeowner	0.62* (0.34)	0.51 (0.62)
Marital Status (ref. Married/cohabiting_	--	--
Widow(er)	-0.50 (0.53)	1.21 (0.90)
Divorced/separated.	0.93** (0.44)	1.92** (0.88)
Never married	0.45 (0.49)	0.25 (0.47)
Active in organisation on regular basis	0.59* (0.30)	0.35 (0.55)
Constant	-1.984	1.099
N	166	80
Wald chi2(10)	47.67	53.26
p-value	0.0000	0.0000

**Statistically different from zero at 0.05 level

*Significantly different from zero at 0.10 level.

**Table 3: Logistic regression for the log odds of trusting
(standard error in parentheses, corrected for clustering on interviewer)***

Variable	(1) Experiment (Give £10)	(2) Survey Question
Age	0.032** (0.011)	-0.006 (0.008)
Female	0.32 (0.35)	-0.43 (0.30)
Financial situation:		
Doing Alright or Just About Getting By (ref. 'Comfortable' or 'Finding it Difficult'):	-1.25** (0.44)	-0.14 (0.31)
Homeowner	0.77** (0.32)	0.20 (0.36)
Divorced/separated (ref. all other)	1.01** (0.39)	0.28 (0.37)
Active in organisation on regular basis	0.35 (0.36)	-0.09 (0.39)
Openness	-0.16 (0.18)	0.53** (0.22)
Neuroticism	0.17 (0.27)	-0.59** (0.22)
'Poor' mental health (GHQ caseness>2)	-0.77* (0.42)	-0.62* (0.33)
Constant	-1.887	0.438
N	147	215
Wald chi2(9)	45.59	47.17
<i>p-value</i>	0.0000	0.0000

**Statistically different from zero at 0.05 level

*Significantly different from zero at 0.10 level.

Table 4: Attitudinal questions and Trust

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

Table 5: Chances of Return and Trust

Behaviour	Less than 50/50	50/50	More than 50/50	<i>p-value</i>
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
<i>N</i>	72	52	5	

Table 6: Willingness to Take Risks in Trusting Strangers and Trust

Behaviour	Scale 0-5	Scale 6-10	<i>p-value</i>
1. % of Rs giving £10	38.3%	54.8%	0.093
2. % who say 'most people can be trusted'	28.1%	64.5%	0.000
<i>N</i>	128	31	159

Table 7: Logistic regression for the log odds of trusting
(standard error in parentheses, corrected for clustering on interviewer)*

Variable	(1) Experiment (Give £10)	(2) Survey Question
Age	0.043** (0.014)	0.014 (0.012)
Female	0.55* (0.33)	-0.43 (0.30)
Financial situation:		
Doing Alright or Just About Getting By (ref. ‘Comfortable’ or ‘Finding it Difficult’):	-1.32** (0.44)	-0.12 (0.35)
Homeowner	0.88** (0.35)	0.26 (0.39)
Divorced/separated (ref. all other)	1.01** (0.45)	0.37 (0.46)
Active in organisation on regular basis	0.52 (0.35)	-0.28 (0.40)
Openness	-0.30 (0.22)	0.38 (0.28)
Neuroticism	0.27 (0.31)	-0.51* (0.29)
‘Poor’ mental health (GHQ caseness>2)	-0.91* (0.48)	-0.55 (0.36)
Willingness to take risks in trusting strangers	0.20 (0.12)	0.45** (0.08)
Constant	-3.464	-2.401
N	143	211
Wald chi2(10)	49.34	68.22
p-value	0.0000	0.0000

**Statistically different from zero at 0.05 level

*Significantly different from zero at 0.10 level.

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			
<input type="text"/>	<input type="text"/>	<input type="text"/>	<input type="text"/>	<input type="text"/>	<input type="text"/>	<input type="text"/>	<input type="text"/>

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} (if 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,

OR

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

OR

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 Table 1
Means and Standard Deviations

Variable	Mean (Std. Dev.)	N
Give £10	0.429	170
Return £22	0.500	84
Age	53.3 (16.9)	254
Female	0.61	254
Financial situation	--	--
○ Comfortable	0.21	
● Doing Alright	0.33	254
● Just about Getting By	0.32	254
● Finding it Difficult	0.14	254
Homeowner	0.44	254
Married/cohabiting_	0.46	246
Widow(er)	0.16	246
Divorced/separated.	0.19	246
Never married	0.19	246
Active in organisation on regular basis	0.46	254
'Poor' mental health (GHQ caseness>2)	0.31	224
Neurotic	0 (0.8)	222
Openness	0 (0.8)	221
Willingness to take risks in trusting strangers	3.4 (2.4)	234
General Willingness to take risks	4.4 (2.6)	233
% who say 'most people can be trusted'	38	254