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Do Strong Family Ties Inhibit Trust?

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Non-technical Summary

We provide direct evidence that people with strong family ties have a lower level of trust in strangers than people with weak family ties, and argue that this association is causal rather than the result of selection. We also investigate the mechanisms that underlie this causal effect, and provide evidence that these revolve around the level of *outward exposure*: factors that limit exposure, of which strong family ties is one among others, limit subjects' experience as well as motivation to deal with strangers and learn from the interaction; by contrast, we find evidence that factors that promote exposure increase trust. Our findings are based on experimental data derived from a new design of the 'trust game' combined with panel survey data, both drawn from a near-representative sample of the British population. We consider trusters' decisions in a trust experiment with real monetary payoffs that concerns a simple financial transaction with anonymous trustees with whom we match the trusters.

Do Strong Family Ties Inhibit Trust?*

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Abstract

We provide direct evidence that people with strong family ties have a lower level of trust in strangers than people with weak family ties, and argue that this association is causal. We also investigate the mechanisms that underlie this effect, and provide evidence that these revolve around the level of *outward exposure*: factors that limit exposure limit subjects' experience as well as motivation to deal with strangers. Our findings are based on experimental data derived from a new design of the 'trust game' combined with panel survey data, both drawn from a near-representative sample of the British population.

Keywords: Trust, Family Ties

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According to the "emancipation theory of trust", developed by Yamagishi and his associates, trust in others in general and commitment formation are alternative solutions to the risk of being exploited in social interactions (Yamagishi and Yamagishi 1994; Yamagishi et al 1998). An important implication of this theory is that "strong and stable relations (such as family ties and group ties) promote a sense of security within such relations but endanger trust that extends beyond these relations" (p. 166). In other words, people with strong family and group ties (FGT) should have a lower level of trust in *strangers* compared with people with weak ties.[1] The evidence supporting this hypothesis is, as far as we know, only indirect, the grand example of which being that US citizens, members of a highly mobile society, are more trusting than the Japanese, who are assumed to belong to a more traditional and committed society that relies on family and groups.

Our first goal in this paper is to construct a direct test of this implication. Our second goal is to investigate whether this association is causal, and our third goal is to try and make progress in understanding the mechanism(s) by which individual variations in the strength of FGT affect trust in strangers. We pursue our aims by combining experimental data with panel survey data, both drawn from a near-representative sample of the British population. We consider trusters' decisions in a trust experiment with real monetary payoffs that concerns a simple financial transaction with anonymous trustees with whom we match the trusters.

We produce direct evidence that the strength of an individual's family ties is indeed inversely correlated with his trust in strangers. To understand the extent to which this finding captures a causal relation – our second goal – we think of this

[1] The idea that strong group ties are antithetical to trust with strangers is also found in Gellner (1988), and was taken up by Fukuyama (1995).

* We are grateful for comments on earlier versions of this paper to Ed Lawler, Tom Siedler and participants in a Royal Statistical Society Seminar.

hypothesis in terms of endogenous and exogenous components of trust and FGT. That is, on the one hand some people may 'inherit' strong FGT or be born in societies with higher social uncertainty which leads them to invest more in FGT, either way these conditions would in turn influence people's trust in strangers negatively. On the other hand, some people may, for personality or other idiosyncratic reasons, have a low level of trust in strangers, which encourages them to invest in FGT, which in turn discourages trust further.[2] For empirical purposes, one would like to identify the exogenous components of both trust and FGT. However, identifying a source of exogenous variation in individual trust levels is difficult, and we are not able to do so. Yet, we are able to estimate whether exogenous variations in the strength of FGT affect trust in strangers and we find that FGT seems a genuinely independent factor: our instrumental variable approach (using distance from family) attempts to mimic the exogenous component of family ties, and the evidence from this approach is consistent with the effect of family ties being causal.

With regard to mechanisms, Yamagishi et al (1998) contend that people with strong FGT are able to solve efficiently cooperation dilemmas because family and group members can easily monitor each other's behaviour and sanction members who 'free ride'. Being accustomed to monitoring and sanctioning, they would "feel insecure" in a social environment in which these mechanisms for achieving efficient collective outcomes are lacking. As a consequence they would cooperate less in social interactions involving strangers compared with people who are more accustomed to operating in such an environment. In particular, they would exhibit a lower level of trust in strangers. We could frame this situation as an *equilibrium* that binds people with strong FGT to persist in their commitments to FGT and feel reluctant to interact

[2] The ideas that higher social uncertainty leads people to form stronger commitments with family and group, and that low trusters are also more inclined in the same direction are presented and tested in

with strangers. If people venture out of the family den they would make more mistakes, which would lead them to trust strangers even less.

But why would people with *weak* FGT be more trusting? We can conceive of two mechanisms: one is *learning*, the other is *need.* They may operate independently of each other as well as interact, in the sense that need may encourage more trusting and this in turn may lead to more learning. With regard to learning, having to interact more with strangers can teach in two ways:

(1) to estimate more accurately the probability of trustworthiness; of course, this assumes that trust in strangers is more frequently rewarded than an individual with less experience would expect. If in the broader society people are not usually trustworthy, then more interaction with strangers would be associated with less trust in them;

(2) to become more adept at reading signs of (un)trustworthiness. Yamagishi et al. mention (2) as the important mechanism (1998; p.172, fn. 8). In our experiment, however, in which trusters do not observe or otherwise know anything about the trustees, (2) cannot possibly affect their trust decision, and so if learning has anything to do with their trust decisions it would be through (1).

The need for cooperative social action that weak FGT people experience, and that is not satisfied by family or group, gives them a stronger motivation to trust others and also to make themselves trusted by others. That is, more interactions with others, which we call *outward exposure*, would lead to trusting not only through learning, but would also be sustained by greater motivation to take risks in trusting strangers. If outward exposure matters, its effects should be observable via other conditions that hamper or promote it and are unrelated to family ties. We try to

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Yamagishi et al. 1998.

measure some of these factors, which we will illustrate in detail below, and find that their effect on trusting is consistent with this view. In fact, we find that even frequent neighbourly interactions have a positive effect on trust in the experiment, and that it is only having strong ties with strict family members rather than with anybody else that decreases it. This suggests a simpler interpretation of the underlying mechanism: the negative effect of strong family ties on trust may not so much reflect the absence of monitoring and sanctioning outside the family, but, just like these other factors, strong family ties would limit both one's opportunities and motivation for outward exposure.

In the paper we first present the notion of trust we use, and describe the key components of a trust decision. Next, we describe the experiment and analyse the trust decision in the experiment. Then we describe the sample and the survey methods. After presenting some basic results, the next two sections examine the correlation between strong family ties and trust and between our measures of outward exposure and trust. We then present three multivariate models of the experimental trust decision, incorporating all of our key variables, followed by a discussion of issues of endogeneity and self-selection, and by our conclusions.

Trust and the components of a trust decision

We work with a notion of trust that relates trust 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, perform a given task as expected. The trust that we have in someone doing X does not necessarily extend to trust in that same person doing Y. 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 had she 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.

Trust decisions involve three components. First, subjects consider the returns to trusting if trust is fulfilled relative to the cost of trust being unfulfilled. Next, trust decisions depend on the expectation that the trustee will do X, framed in terms of a probability (Gambetta 1988; Barr 2003). The level of expectation is the result of beliefs about other people's trust-warranting qualities with regard to doing X. In most real life circumstances, these beliefs – which are at least in part derived from *learning through experience* – refer to specific persons or groups of persons whom we believe share certain trust-warranting properties. When interacting with anonymous strangers, trusters' beliefs are not *ad personam* but can be understood as being about the frequency of trust-warranting properties in the population of anonymous trustees – in our case the generic group consisting of people living in the UK and participating in the experiment.

In so far as beliefs are based on experience, past experience is important for people's decisions in experiments. The studies by Henrich *et al.* (2004), which used the same protocols in 'social dilemma' games (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 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]…" In order to measure trust and trustworthiness in British society accurately we want to tap into those experiences that subjects draw from real life trust situations. We do not want them to think of the experiment as a purely artificial exercise to be played as a board game. It is precisely their drawing on their social context that should give our experiment external validity. We believe that our design makes it highly likely that trustees' decisions reflect their habitual practices in these situations—their sense of reciprocity, fairness or sensitivity to obligations—and that the decisions of trusters reflect their expectations of such behaviour.

Third, in all cases in which the probability of trustworthiness is less than 1, subjects' willingness to take the risk of being exploited comes into play. The willingness to take the risk of exposure captures a preference or disposition relating to character traits and state of mind, and it could in addition reflect 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).

In our study we do not vary the monetary returns but are able to take some account of the second and third dimensions, expectations and preferences. We can expect each of these two dimensions to have an independent effect on trusting decisions: holding constant the level of expectation, trusters can choose to trust or not to trust depending on their willingness to take the risk of exploitation. Analogously, holding constant the willingness to take the risk we can expect that trust decisions will be positively associated with the level of expectation. The relationship between the two components is however richer. First, we cannot rule out that the willingness to take the risk of exploitation may colour the expectations via wishful thinking and cognitive dissonance reduction, with bolder trusters being also more inclined to be

optimistic. Next, the willingness to take risks may also affect the level of expectation via the speed and accuracy of learning about trustees' dispositions. Being open to trust elicits more experiences and thus speeds up learning, while being reluctant to trust provides little new evidence.

In the next section we describe our experiment, and in the following one we discuss the trust decision more explicitly in the context of our experiment.

The experiment

Our contribution combines the experimental method with both a near 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. Other studies have also used population samples, but ones that are less representative of the population than these two studies (e.g. Barr 2003 and Schechter 2007). In this section we first describe the experiment and in later secions the sample and methods we used.

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 us to obtain better measures of the concepts of trust and trustworthiness.

The game we presented to subjects, whom we shall call the truster (R, a 'she' for convenience) and the trustee (E, a 'he'), 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 her home; it is described as compensation for taking part in the interview *cum* experiment. R is told that she will have the opportunity to obtain £22 if she gives the money to another person (E) with whom she has been randomly matched and about whom she is told nothing. She is told that the experimenter will increase it by £30 and so E will receive £40. E, who is not paid for participating in the experiment,[3] is then given the choice to either pay back £22 to R, or keep all £40. R is informed that if she decides to give £10, she will know the outcome in about four weeks, and if E decides to pay back the specified amount she will receive a cheque for £22.[4] The procedures (see Appendix 1) ensure that 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. Again, the interviewer does not know his decision. Our procedures are thus, in a sense, 'double blind': the interviewer knows the subjects' name and address but not their decisions, while the researcher knows the subjects' decisions, but identifies them by their code numbers, not associating them to their name or address.

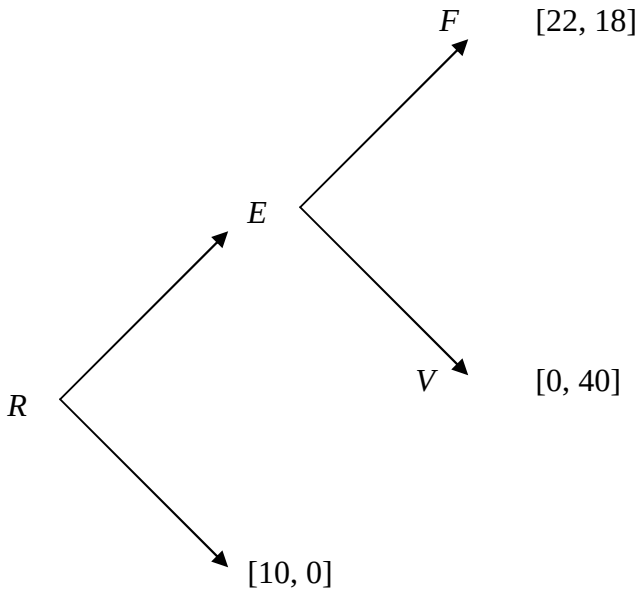
The payoffs we used in the experiment are illustrated in Figure 1. We have chosen the amount specified to be paid back in the case that E fulfils trust in a way

[3] We discuss this further below.

[4] 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%.

that makes the payoffs asymmetric. Symmetric payments may encourage fulfilling trust for reasons of fairness, rather than because E does what is expected of him (e.g. see Bacharach et al. 2001, Bohnet and Huck 2004 and Bohnet et al. 2005).[5]

Figure 1



(F='fulfill trust', V='violate trust'; R's payoffs listed first in brackets)

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

[5] McCabe *et al.* (2003) use binary trust games similar to that used here, in which R's expectation is known to E. Their results suggest that E's attribution of *intentions* to R is important in E's decision to fulfil trust or not. That is, 'outcome-based' preferences are not dominant in their data.

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'); and

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

The trust decision in the experiment

Obviously, whether the decision to keep or pass on her £10 amounts to a trust decision depends on R's preferences. Suppose there are three basic types in the population: the altruist, the egalitarian and the self-interested. If they were Rs in our game, their respective preferences over the outcomes are as indicated in Figure 2 (payoff of Rs appears first).

Figure 2

Self-interested	**Egalitarian types**		**Altruist**
	Outcome Egalitarian	**Norm Egalitarian**	
22;18	22;18	22;18	0;40
10;0	10;0	0;40	22;18
0;40	0;40	10;0	10;0
Pass on £10 conditional on expectation of R being trustworthy		*Pass on £10 unconditionally*	

If we ignore issues of betrayal (or risk) aversion, the factors shaping the choices of the self-interested and the altruist types are clear. The self-interested will pass on her £10 *if she expects* that the probability that E will return £22 is large enough, but will keep it otherwise. The altruist by contrast passes on £10 unconditionally, hoping that E will keep £40. The egalitarian is a more complicated

case for one can conceive of two subtypes. If she is an *egalitarian over the outcome*, what shapes her choice in this game is identical to that of the self-interested. Like the self-interested type, this type makes her decisions conditional on the probability R returns £22 (i.e. that the egalitarian outcome will be realised). Like the self-interested, such an egalitarian would be worse off if she does not receive £22 back than if she had kept the £10. Both types choose to pass on £10 in our game if and only if they *trust* Es. Thus, for these two types, behaviour in our trust game captures well the notion of trust introduced above.

There could, however, be a different type of egalitarian, one who has a *preference over her own actions,* and prefers to choose always the action which *may* lead to the egalitarian outcome regardless of whether the egalitarian outcome is realised. To add some narrative flesh to this type, we can imagine that this type has a strong moral norm, a sort of Kantian norm: 'do the right (egalitarian) thing, come what may'; if she complies with this norm she enjoys a 'warm glow' and/or she avoids a sense of guilt that she would incur if she breached it. This type chooses to pass on £10 regardless of whether or not she thinks the E will return £22. Our game does not capture the trust decision of this type, simply because, like the altruist, she does not make one.

The importance of the two types whose behaviour in our game does not constitute a trust decision, the altruist and the norm egalitarian, is an empirical question. The altruist type, which in this game would be quite extreme, seems by casual observation to be quite rare. For the norm egalitarian it is more difficult to say. One-fifth (n=14) of Rs who passed on £10 in our game stated that they did not think about whether or not they were going to get the £22 in return, and while these may be instinctive trusters, they could also be norm egalitarians or altruists. Everyone else

(80%) said that they weighed up the chances of getting the money back, thus indicating that they made a conditional decision and decided whether or not to pass on their £10 on trust (or distrust).

The characterisation of the trust decision is simplest for a risk neutral self-interested R, who maximises her expected return. For her, the two treatments in our experiment are identical in terms of their expected payoff. It 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. Ignoring discounting of future income, in either treatment, those who have a subjective estimate of P > 10/22 should trust.[6]

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, it 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' her money as compensation for taking part in the interview cum experiment and this is given to her in cash and before the interview begins. We expect

[6] 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).

that this should trigger an 'endowment effect' and make R treat as her own money, making her 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 she receives as a participation payment, we depart dramatically from the standard TGE, in which R can pass on any amount. In our view, the possibility of transferring any amount favours the intrusion of other motives such as 'let's risk part of it'. Karlan (2005) found that, among poor Peruvian women, the more that R passed on in the standard TGE, the lower her voluntary saving in a micro-credit program.[7] Because to save in such a program is to trust your peers to repay their loans, this association suggests that Rs who pass more are 'gamblers', namely people more willing to take risks rather than more trusting. 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, as argued in the preceding section, is the relevant consideration in R's decision to pass on £10 or not as long as she is self interested or an outcome egalitarian. While we cannot eliminate a gambling motive for R's decision in our design, by giving them a binary 'all or nothing' choice we increase the risk they need to take if they choose to pass on the money and, in addition, we use measures of willingness to take risks to examine the role that risk preferences may play in R's decision.

(c) In the standard TGE, E can return any amount to R. In our design, R knows in advance how much she can expect back from E if E fulfils trust, and E knows that this

[7] The program is called Foundation for International Community Assistance (FINCA). In the study, 868 members of FINCA played the standard trust game.

is an expectation of R when she makes her 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 herself in the situation of a person who has benefited from R's expectation that she will return £22. The study by Karlan (2005) indicated that Es in the standard TGE who returned a larger proportion are less likely to default on their loan, suggesting that the trust game behaviour indeed reflects individual-specific trustworthiness.

In the standard TGE it is common to pay Es for participating in the trust experiment to remove fairness reasons for Rs to pass on money to Es. This issue is, however, not so relevant in our design for the binary all-or-nothing choice of passing on £10 does not allow Rs to split their endowment with Es. As argued above, the decision to pass on £10 indicates *trust* for both self-interested and outcome egalitarian Rs. If the R were an altruist, then her behaviour in the experiment would not be altered by giving E an endowment. If R were a norm egalitarian, then their decision might be altered by giving E an endowment of say £10.[8]

As noted above, we introduce two payoff structures,. As shown above, this would make no difference if R aims to maximise her expected returns. Yet, we introduced this treatment because

(a) we expect that the treatment in which R has the draconian option of parting with all his money or keeping it all will *decrease* the chances that R will pass on £10. The

[8] There would also be significant additional operational and financial costs of paying an endowment to the Es. We would have had to draw a larger sample of Es to match them with the Rs who did not pass on £10, and we would have had to visit and pay even the Es who had nothing to do. The additional contact costs would have increased the costs of the experiment by about £10,000. Given the lack of compelling design reasons for paying the Es an endowment, such costs did not seem justified.

reason for this prediction is that we expect Rs to be more inclined to trust when the worse that can happen to them is that they are left with at least something. This may be due to a number of mechanisms, such as risk aversion, fear of loss being triggered more vigorously by the salience of the prospect involving the 'loss of everything' etc. Also face saving towards oneself and 'exploitation aversion' (i.e. R's fear of being taken advantage of by E) could be at work. Recent research suggests that the decision to trust is shaped by 'exploitation' or 'betrayal' aversion rather than by risk aversion (Bohnet and Zeckhauser, 2004; Bohnet et al 2008).

At the same time, however,

(b) we expect that the treatment in which R has the draconian option of parting with all her money or keeping it all, will *increase* the chances that E will pay his due back if R passes the money on.

The two hypotheses contradict one another for if the latter were true then the former cannot be: if R too makes the same hypothesis (b) as we make about E, he should be *more* rather than less inclined to trust. The reason we expect E to be less inclined to return the money in the non-draconian case is that we think that leaving R with a payoff of 2 will give E a way out, by making him feel less guilty about not fulfilling trust, approximately for the mirror mechanisms that make R more inclined to trust: E will feel that R does not suffer a total loss of face and that he is not leaving her with a worrisome and salient *nothing*.

The Sample and Survey Methods

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 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 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. Also, 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 and not to elaborate further.[9] We randomly matched E-players to the Rs 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). On the one hand, this is an advantage because it reinforces respondents' 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

[9] 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. Interviewers were also asked how worried the respondent seemed by being asked to take part in the

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. We carried out some tests (Ermisch et al. 2007), and even though we cannot rule out entirely the possibility that familiarity with the interviewer may encourage people to be more trusting and trustworthy, none of the correlations was statistically significant.

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. They then completed a short questionnaire with the interviewer (CAPI) and finally another confidential self-completion questionnaire, which included two questions about willingness to take risks. On average, the experiment took 10 minutes and the remainder of the survey about 15 minutes.

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%. Almost all of the non-response arose because of inability to locate the selected individual who had moved house (only one refusal). 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 (2009) 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, even after controlling for other variables that may affect trust or trustworthiness.

experiment; 95% reported them being 'not at all' worried, 4% 'somewhat' (6 Rs and 3 Es) and 1% very worried (1R and 2Es).

Thus, it appears that those who move more often do not behave differently in terms of trust and trustworthiness.

Our sample differs from the British population in a number of potentially important dimensions: it 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'.[10] At a minimum, our sample can be taken as representative of households with low to moderate income. The focus on lower income people may have its serendipitous advantages because the sums of money involved in the experiment may be more important to them.

Basic Results

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.[11]

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'.[12] These Rs were asked

[10] Analysis in Ermisch et al. (2007) strongly suggests that the percentages trusting and being trustworthy should be similar to those for a more fully representative sample of the population.

[11] 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 (this is discussed in Ermisch et al. 2007).

[12] Rs that weighed their chances were more likely to give £10 than those who did not (45% cf. 39%), but the difference was not statistically significant.

the follow-up question: *What did you think your chances of getting your money back were?* The responses to this question are shown in Table 1.

Table 1: Chances of Return and Trust

Behaviour	**Less than 50/50**	**50/50**	**More than 50/50**	*p-value*
% of Rs giving £10	23.6% N=72	69.8% N=53	80% N=5	0.000

Table 1 indicates that the person's expectation of the chances of return is strongly related to their trust decision, with more optimistic Rs being more likely to trust. This relationship and the fact that 80% of Rs weighed the chances of return are reassuring for our experimental measure of trust.

Rs' decisions do not differ significantly between the two treatments (i.e. R receives £10 or £12): see Table 2. By contrast, a somewhat larger percentage of Es return £22 in the all or nothing treatment as we expected, although the difference is not statistically significant at conventional levels. Still, we cannot rule out that the thought of R being left with £2 inclines E to be untrustworthy. This, as we mentioned, could explain the puzzling finding of many trust experiments whereby in many cases the second mover sends only a little money back but not enough to cover the sum sent over by the first mover – arguably a guilt allaying tip rather than a trustworthy act.

Table 2: All or Nothing Treatment Effect

Behaviour	**All or Nothing Treatment**	**£12 Treatment**	*p-value* *N*
% of Rs giving £10 **N**	39.5% 81	45.0% 80	0.48
% of Es returning £22 **N**	59.5% 42	44.7% 38	0.19

Family Ties and Trust

We now investigate whether individuals' ties to their families affect their trust behaviour. Our main hypothesis is that people with strong family ties exhibit a lower level of trust in strangers. To test it we exploit the sampling frame described earlier for our experiment, which provides 5 years of panel information for these individuals, covering the years 1997-2001. In particular, a battery of questions were asked in the 2001 BHPS about the numbers of relatives of particular types (e.g. mother, sons, brothers, grandchildren, grandparents) not living in the same household who were alive at the moment, about people's interactions with parents and adult children living elsewhere, how long it takes to visit them, and regular help given to/received from parents/adult children. The 'family ties' variable is based on how often the person sees their father, mother or adult child (the one with whom the parent has the most contact) living elsewhere. The possible responses are 'daily', 'at least once week', 'at least once a month', 'several times a year', 'less often' and 'never'. Thus, each person could have up to three possible 'frequencies of seeing' family members, and we take the highest among these as our 'frequency of seeing family'. We create a dichotomous variable called *Family* that is equal to unity for those who see that family member daily or at least once a week, and zero otherwise. Persons without a living child, father or mother living elsewhere are coded as zero.[13]

[13] We tested for a separate effect for this group of 18 persons, but they did not even approach being significantly different from the reference group—persons seeing a family member less frequently than once per week.

Table 3: Seeing Family at least once a week and Trust

Behaviour	**Less than once/wk**	**At least once/wk**	*p-value*
% of Rs giving £10	50.0%	34.4%	0.044
N	74	90	
% of Es returning £22	51.6%	48.9%	0.815
N	31	45	

Table 3 shows that trust (giving £10) is more likely for those who see their family less than once a week. Trustworthiness (returning £22) is not significantly associated with the *Family* variable.

While *Family* reflects behaviour 6 years prior to the experiment, we expect persistence in this behaviour. To check for it, we compare BHPS respondents in both 2001 and 2006. While these people are not in the experiment, the comparison should be indicative of persistence in our experimental sample. Comparison of BHPS respondents indicates that 76% of those who did not see a family member at least once a week in 2001 also did not do so in 2006. Similarly, of those who did in 2001, 83% also did in 2006. Thus, the 2001 responses for our experimental sample should be indicative of their usual behaviour in the years leading up to 2007. In any case, this possible misclassification of some subjects should *reduce* the association between *Family* and trust, thus making the finding in Table 3 all the more remarkable.

Yamagishi et al. include in their hypothesis not only family but also group ties, in other words ties that involve *non-strangers* to whom an individual feels some degree of commitment. Neighbours with whom one interacts frequently could be an instance of such group. The opportunities for monitoring and sanctioning neighbours, while not as ample as those one has with family members with whom one is closely connected, are still much higher than those one has with strangers. In this sense, the neighbourly contacts should be indicative of the underlying mechanism that Yamagishi et al have proposed.

To measure for it we draw on responses to questions about sample members' contact with neighbours that were asked annually in the BHPS between 1997 and 2001.[14] We create a dichotomous variable, *Neighbour*, that is equal to unity for those who spoke to their neighbours most days in 2001, and zero otherwise.[15]

Table 4: Speaking to Neighbours most days and Trust

Behaviour	**Most days**	**Otherwise**	*p-value*
% of Rs giving £10	47.8%	35.1%	.097
N	90	77	
% of Es returning £22	52.1%	47.1%	.654
N	48	34	

The association of the *Neighbour* variable with trust is *positive* and statistically significant at the 0.10 level (Table 4). Under the Yamagishi et al. hypothesis, if neighbours are identified as a group with which one has strong ties then we would expect, as in the case of *Family*, the correlation between trust and *Neighbour* to be negative. It seems, by contrast, that those who are exposed to interactions with non-family members are more likely to trust, even if these non-family members are as well known as neighbours are.

'Outward exposure' and trust

From the positive correlation of *Neighbour* and trust it seems that outward exposure to other than family members, even limited to non-strangers, encourages trusting behaviour. We now consider two further variables, one limiting and the other fostering outward exposure. The first is the extent of an individual's involvement in family care (primarily looking after children, but also other relatives in the

[14] The possible responses to the questions about how often the person speaks to neighbours are 'most days', 'once or twice week', 'once or twice a month', 'less than once a month' and 'never'.

[15] Speaking to neighbours most days is a relatively persistent attribute: taking pairs of consecutive years, 79% who spoke most days in the first year also did so in the second. Also, 68% of subjects who spoke most days in 1997 also did so in 2001.

household): the greater it is the more limited a person's opportunities for interaction with non-family members. Using the 1997-2001 panel data, we calculate the proportion of years in which a person reports 'family care' as best description of their current situation (cf. paid employment, retired, etc.), and denote it as *FamCare*. One-fifth of Rs spent at least one year doing family care during 1997-2001. Table 5 indicates that they are significantly less likely to trust (and also less likely to be trustworthy, but not significantly so). Interestingly, *FamCare* is not significantly correlated with the *Family* variable.[16]

Table 5: Family Care and Trust

Behaviour	**No years**	**At least one year**	*p-value*
% of Rs giving £10	46.3%	28.6%	0.059
N	134	35	
% of Es returning £22	52.5%	42.9%	0.448
N	61	21	

The second variable is divorce or separation. In one respect, this stressful and often depressing life event could be expected to induce a more prudent attitude to trust. At the same time, though, partnership separation tends to reduce the time spent with family and contacts with one's previous network. These give both a stronger motive and greater opportunities for people to seek new relationships outside their family or social network. This variable too is not significantly correlated with *Family*. Table 6 shows that divorced and separated people are indeed significantly more likely to trust, and they also tend to be more likely to be trustworthy.[17] There are no

[16] It is of course associated with whether the person is involved in family care in 2007, but there are only 10 such persons. Female Rs are more likely to have at least one year of family care than male Rs (30% cf. 6%), and the mean value of *FamCare* is 0.14 for women and 0.03 for men (among the Rs).

[17] Liz David Barrett, in a personal communication, suggested to us an interesting alternative mechanism to explain the positive effect of being divorce/separated on trust: "the reason that married and cohabiting people might appear less trusting could be that they regard the money that they receive as a common resource, owned by themselves and their partner (and possibly children/family unit).

significant differences among the remaining marital status categories—the difference is between the divorced/separated and the rest.

Table 6: Divorced/Separated and Trust

Behaviour	**Not Divorced/Sep.**	**Divorced/Sep.**	*p-value*
% of Rs giving £10	38.9%	60.0%	0.025
N	131	35	
% of Es returning £22	47.1%	66.7%	0.210
N	68	12	

These correlations further reinforce our suspicion that what affects trusting is any factor which either constrains people within the family circle or that gives them an opportunity and a motive to interact with others, whether neighbours or strangers. This suggests that the mechanism that could explain the negative effect of FGT is not that hypothesised by Yamagishi. Before drawing any conclusion however we need to take a few more steps.

Three models

To test our hypotheses that weaker family ties and more outward exposure increase trust in strangers we need to consider their association with trust in our experiment in the context of other personal attributes that may affect trust. We present three models in Table 7. In the first we control for sex and also for age, financial situation and homeownership because we know, through exploratory analysis we reported elsewhere (Ermisch et al. 2007), that they may be linked to trusting behaviour in our experiment (descriptive statistics are given in Appendix Table 1). In the second we additionally control for people's expectations about trustworthiness and in the third

They might therefore be more cautious about gambling with it or giving it away, since it is not just their own money to lose, but also someone else's. They might also anticipate scolding from the partner if they make the wrong choice, which might again make them less inclined to take the risk. Thus they would appear less trusting of strangers."

also for their willingness to take risks in trusting strangers. Before we discuss the results, we need to describe how we measure the two latter variables.

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

Variable	**(1)** **Give £10**	**(2)** **Give £10**	**(3)** **Give £10**
Age	0.028** (0.010)	0.029** (0.010)	0.036** (0.013)
Female	0.45 (0.36)	0.19 (0.39)	0.32 (0.37)
Financial situation: 'Comfortable' (*Ref. group*)			
• **Doing Alright**	-1.24** (0.61)	-1.17* (0.67)	-1.20* (0.72)
• **Just about Getting By**	-1.07* (0.60)	-1.30** (0.61)	-1.40** (0.61)
• **Finding it Difficult**	-0.378 (0.78)	-0.44 (1.21)	0.01 (1.51)
Homeowner	0.87** (0.36)	0.53 (0.40)	0.71 (0.48)
Divorced/separated	1.12** (0.41)	1.62** (0.45)	2.00** (0.58)
Family	-1.00** (0.41)	-0.97** (0.47)	-1.00** (0.50)
Neighbour	0.55 (0.42)	0.60 (0.50)	1.03 (0.63)
FamCare	-2.62** (0.91)	-2.39* (1.18)	-2.69* (1.21)
Expect '50-50 or more chance or return'	-	2.30** (0.43)	2.54** (0.48)
Does not weigh chances of return	-	1.00** (0.45)	1.55** (0.45)
Willingness to take risks in Trusting Strangers: Scale=6-10 cf. Scale 0-5	-	-	1.47** (0.50)
Constant	-1.987	-2.559	-4.172
N **Wald chi2 (df)** *p-value*	161 38.27 (10) 0.0000	157 89.07(12) 0.0000	150 90.06 (13) 0.0000

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

Our measure of expectations is based on the responses in Table 1, but we aggregate the top two categories of the 'chances of return' variable because of the small number in the top category. Fortuitously, the threshold of '50/50 or more' corresponds to the frequency of trustworthy responses in our experiment, making it a salient dividing line. This tool for measuring expectations is rather blunt: actual expectations may be more refined than this, and the variation in trust within these two categories, evident in Table 1, is consistent with this view. We also create a dichotomous variable that equals unity if the person did not weigh the chances of return and zero otherwise. Of course, in models 2 and 3 (Table 7), people with more optimistic expectations are much more likely to trust. Conditional on the other variables, people who do not weigh the chances of return are also more likely to trust.

Our measure of the risk component of trust decisions comes from a question in the self-completion part of the questionnaire, which is filled out at the end of the interview after the experiment. The question is: *Are you generally a person who is fully prepared to take risks in trusting strangers or do you try to avoid taking such risks?*' Subjects were then asked to tick a box ranging from 0 ('unwilling to take risks in trusting strangers') to 10 ('fully prepared to take risks in trusting strangers').[18] Rs who express more willingness to take risks *in trusting strangers* were more likely to

[18] Our question is identical to that asked in the 2004 wave of the German Socio-Economic Panel (GSOEP). We also asked a similar question about 'general willingness to take risks' (also from GSOEP), and find that it does not affect trust behaviour, a result consistent with Eckel and Wilson (2004), but not with Sapienza *et al.* (2007) or Schechter (2004). 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, which is different from the risk of losing in a game of chance or in the stock market.

pass on the £10 (Table 7), which suggests that there is some behavioural content in this question. This effect is confirmed in model 3 (Table 7). [19]

Table 8: Willingness to Take Risks in trusting strangers and Trust

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

The two trust components may not be independent: more risk-averse people tend to be more pessimistic, although not significantly so (Table 8). Furthermore, the exogeneity of the expectations and risk variables are, of course, suspect. The main reason for models 2 and 3 is to obtain some understanding of how the variables in the first model operate via expectations and risk.[20]

Table 9: Expectations of trustworthiness and willingness to take risks in trusting strangers

	Scale 0-5	**Scale 6-10**	*p-value*
Less than 50/50	56.0%	42.9%	0.222
50/50 or more	44.0%	57.1%	
N	91	28	

In all three models of Table 7, people who see their family frequently (*Family*) and those who spend more time doing family care (*FamCare*) are less trusting, while the divorced and separated are more trusting.[21] *Neighbour* has a positive coefficient,

[19] Similar results emerge if we use the 0-10 risk scale as a continuous variable. Its coefficient is 0.177, with a standard error 0.082.

[20] In a logistic regression for the expectation variable using the regressors in the first column of Table 9 other than expectations and not weighing the chances of return, only homeownership and *FamCare* are statistically significant, with homeowners more likely to believe the chances of return are 50/50 or more and those with higher values of *FamCare* being less likely to believe this. In a similar logistic regression for not weighing the chances, only *FamCare* is statistically significant, with a positive coefficient.

[21] We also tested whether *Family* had different effects for those with different expectations, as measured by our dichotomous indicator, or for those who weigh the chances of return compared with those who do not. Neither of these interactions approaches statistical significance. With respect to *FamCare* the results are similar when we include the 2007 observation in computing the proportion of time spent in family care. When we do not control for *FamCare*, the impact of being female becomes

but is not statistically significant at the 0.10 level or less in any of the three models. Its effect is strongest, when we control for the risk component (model 3), because people who speak to neighbours most days are *less* willing to take risks in trusting strangers (e.g. compared with others they are less likely to score 6-10 on this risk scale (13% cf. 27%, p-value=0.023)).

In the third column, where we control for the person's willingness to take risks in trusting strangers, the main change to the coefficients is that the impacts of age and of not weighing the chances of the return of £22 become larger. This is because older people and those who do not weigh their chances are less willing to take such risks. The regressions also indicate that people are more likely to trust if they are older (this may be a generational effect) and if their financial situation is 'comfortable' or 'difficult', suggesting a U-shaped relation with financial situation.

Endogeneity and self-selection

Other studies have examined the impact of *civic participation* on people's trust (e.g. Li et al 2005), or taken it to be an indicator of social trust. In our survey, people were asked whether or not they are active in an organisation on a regular basis. Regularly active people are significantly more trusting: 51% of people who report that they are active in 2007 gave £10 in our experiment compared with 36% for inactive people (p-value=0.046). This is because regular activity in an organisation is associated with higher odds that the person believes that the chances that £22 will be returned are 50-50 or more. In the two models in Table 7 controlling for expectations, the coefficient of the organisational activity variable is virtually zero. We chose not to include the organisation variable in the models because we suspected an endogeneity problem,

much smaller (e.g. falling to 0.24 in the first model specification), reflecting the higher values of *FamCare* for women.

namely that those who are more disposed to trust may be more likely to be active in organisations.

With respect to the variables that we do include, we think that for *FamCare* the issue of endogeneity does not arise. A person's disposition to trust is not likely to be correlated with this variable. In other words, it is hard to conceive how life events such as the choice or the burden of taking care of one's children or relatives could be affected (inversely) by the disposition to trust strangers. If anything one could expect a positive effect.

There may be concern that the positive effect of being divorced or separated on trust does not reflect the mechanism we stress, but rather a greater propensity to take chances. That is, among people who are divorced or separated, a larger proportion may be inclined to take risks relative to those who stay married, and, as a consequence of their lack of prudence, their marriage would be more likely to fail. Comparison of the results in columns (2) and (3) in Table 7 suggests, however, that the risk explanation is unlikely to be the cause of the positive association with trust. While our risk measure is imperfect, the *increase* in size of the coefficient of divorced/separated when we control for willingness to take risks is not consistent with this explanation.

Where the issue of endogeneity could arise in a way that might affect our findings concerns *Family*: the negative coefficient in Table 7 could reflect a tendency for people who are less trusting to look to their family more for social interaction. In other words, family oriented people could be a self-selected group of low trusters. With a variable that produces exogenous variation in frequency of seeing family members we can test whether this endogeneity (or selection) is driving our results. A good candidate for such a variable is distance from the closest family member

(*Distance*), which is a strong predictor of the probability of seeing a family member at least once per week and hence of having strong family ties in our definition—see Table 10.

Table 10: Percent Seeing Family at Least Once a Week and Distance from Closest Family Member

Distance from closest family member	**Percent with *Family*=1**	**N**
Less than 15-minutes	86.6	67
15-30 minutes	64.7	34
30-60 minutes	60.0	15
60 minutes or more	3.6	28
All	62.5	144

p-value = 0.000

We argue for the validity of *Distance* as an instrument on a number of grounds. First, both parents and children are mobile relative to one another; that is, *Distance* depends partly on location decisions of people who are not in our sample. In nearly one-half of our sample, the relative who lives closest is an adult child, who is more likely to have moved in relation to the parent. Among sample members for whom a parent is the closest relative, 90% of them are aged above 30, making it likely that they have moved a few times since leaving home.

Using an exogeneity test suitable for dichotomous variables, explained in Appendix 2, we cannot reject exogeneity of the *Family* variable using *Distance* as an instrument, and so there is no evidence that the negative coefficient of *Family* in Table 7 reflects self-selection.[22] The failure to reject exogeneity is not because *Distance* is not a powerful instrument. An indicator of its power is a test for the

[22] The results are similar when we use additional instruments for *Family*: the length of residence in the present house and its square. As noted earlier, these are not significantly related to trust in the experiment, even after including control variables like those in the first column of Table 9 other than *Family*. The odds of seeing a family member at least once per week is positively related to residential tenure up to 31 years tenure.

exclusion of the *Distance* variables from the *Family* equation: the chi-square-test value (3 d.f.) is 36.2 (*p*-value=0.0000).[23]

Table 11: Percent Trusting and Distance from Closest Family Member

Distance from closest family member	% of Rs giving £10	N
Less than 15-minutes	41.8	67
15-30 minutes	23.5	34
30-60 minutes	46.7	15
60 minutes or more	53.6	28
All	40.3	144

p-value = 0.096

However, an objection to the validity of *Distance* as an instrumental variable for *Family* is that people who are less disposed to trust strangers do not move far from their family.[24] If this were the case, the 'reduced form' relationship between trust and *Distance* would be biased upwards: it would not only reflect the associations between *Distance* and *Family* and between *Family* and trust, but also a relationship between disposition to trust strangers and *Distance* per se. Table 11 shows a relatively weak relationship between *Distance* and trust, with those living 15-30 minutes away being the least likely to trust.[25] Further, substituting *Distance* for *Family* in model (1) of Table 7, we cannot reject the hypothesis that the coefficients of the *Distance* categories are jointly zero (*p*-value=0.29).

A problem similar to that posed by *Family* could be posed by *Neighbour*, in that people who are more disposed to trust strangers could also have more frequent contact with their neighbours. It is more difficult to find a credible instrument for *Neighbour* than for *Family*. All that we can say in defence is that the exclusion of the

[23] Similar results emerge if we assume linear probability equations for trust and *Family*. The partial R-squared of the excluded instruments (*Distance*) is 0.43 and the F-test for the relevance of the excluded instruments is 32.74 (p-value=0.0000). The p-value of the Sargan test of over-identifying restrictions is 0.25. Also, a Hausman test cannot reject the hypothesis of exogeneity of *Family*.

[24] As noted in Appendix 2, if this is the case the estimates in the first column of Table 7 may be preferable to the instrumental variable estimates based on the 'imperfect instrument' *Distance*.

[25] Within this distance category, 18% with *Family*=1 trust compared with 33% with *Family*=0; thus, there is considerable 'within distance category' co-variation between *Family* and trust.

potentially endogenous *Neighbour* from the models in Table 7 hardly affects the coefficients of *Family* and the other outward exposure variables.

Conclusions

The present study uses a behavioural approach to measuring trust in strangers and its correlates, undertaking an experiment with a near representative British sample for which we have panel data for the past and contemporary data about socio-economic circumstances, expectations and willingness to take risks. Overall, about 40% of people were willing to trust a stranger in our experiment, and their trust was rewarded one-half of the time. These data support the hypothesis that people with weaker links with their family are more likely to trust strangers. This suggests, counter-intuitively, that a decline in family connections typical of modern societies could not so much make for a more trusting society directly, but encourage people to take risks and discover through experience the real level of trustworthiness in their community, which if higher than they thought would raise their trusting expectations and their trust in strangers.

What the mechanism is that explains this effect we cannot be certain. Yamagishi's hypothesis, that this is because people with strong family ties feel more insecure in a social environment lacking mutual monitoring and sanctioning of social interactions, does not seem to be compatible with our results. These suggest that trust is positively affected by *any* factor that promotes exposure to experiencing the behaviour of others beyond one's family circle. People who have more experience of interacting with strangers and who have stronger motives to take risk with strangers appear to be more likely to trust if their experiences are predominantly positive

because their expectation that people will be trustworthy is higher.[26] In this sense, the expectations of people with more outward exposure should better reflect the level of trustworthiness in their 'community' outside the family. This suggests that people with weak family ties are in an equilibrium sustained by their better knowledge of others' trustworthiness and by being better able to read the signs of trustworthiness in people. In contrast, those with strong family ties sustain an equilibrium with very limited interactions with strangers by their strong commitments to other family members.

[26] Our experimental result that one-half of Es returned £22 is broadly consistent with the assumption that trust in strangers is more likely than not to be rewarded in British society, because in most day-to-day encounters Rs and Es have more information about each other than in our experiment.

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,
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 2

A test for the exogeneity of family ties

Let T be a latent variable indicating a person's propensity to trust, and let F^* be a latent variable representing the frequency of seeing family members. Define F as a dichotomous variable indicating that a person sees a family member at least once per week, where $F=1$ if $F^*>0$ and $F=0$ otherwise (i.e. $F=Family$). Let D_i indicate distance from the closest family member (i.e. $D_i=Distance$). Then we assume, for person i, that

$$T_i = \delta' X_i + \gamma F_i + v_i \quad (1)$$

$$F^*_i = \delta' X_i + \delta D_i + u_i \quad (2)$$

where v_i and u_i have a joint standard normal distribution. That is, we have a bivariate probit model, with the probability of trusting being given by $\Phi(\delta' X_i + \gamma F_i)$, where $\Phi(.)$ is the standard normal distribution function.

If v_i and u_i are independent, then F_i is exogenous, and we can estimate its effect (γ) on trust more efficiently by estimating equation (1) on its own. We estimate the trust model corresponding to the first column of Table 7, thereby excluding the possibly endogenous expectations' variable, and find that the correlation between v_i and u_i is only -0.10 (SE=0.28). Thus, we cannot reject the hypothesis that these two equations are independent. It is therefore appropriate, in Table 7, to estimate (1) on its own, assuming a logistic rather than a normal distribution for v_i, but the conclusions are the same when we assume normality.

If *Distance* is an 'imperfect instrument' for *Family* (i.e. correlated with v_i), the estimates in Table 7 may be preferable to those from the joint estimation of (1) and (2). In recent analysis of a linear model, Nevo and Rosen (2008) show that even if an imperfect instrument exhibits a weaker correlation with the equation error term than

the endogenous variable for which it is used, it is possible that the IV estimator using that instrument does not offer an improvement over treating the endogenous variable (e.g. *Family*) as exogenous.

Appendix Table 1
Means and Standard Deviations for Trust Regressions

Variable	Mean (Std. Dev.)	N
Give £10	0.429	170
Return £22	0.500	84
Age	53.5 (16.7)	170
Female	0.61	170
Financial situation	--	--
• **Comfortable**	0.22	170
• **Doing Alright**	0.33	170
• **Just about Getting By**	0.34	170
• **Finding it Difficult**	0.12	170
Homeowner	0.44	170
Married/cohabiting_	0.46	166
Widow(er)	0.16	166
Divorced/separated	0.17	166
Never married	0.19	166
Active in organisation on regular basis	0.54	170
Willingness to take risks in trusting strangers	3.3 (2.4)	159
General Willingness to take risks	4.3 (2.6)	161
Family	0.55	164
FamCare	0.10	169
Neighbour	0.54	167
Expect '50-50 or more chance or return'	0.35	162
Does not weigh chances of return	0.22	164
Willingness to take risks in Trusting Strangers: Scale=6-10 cf. Scale 0-5	0.19	159

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