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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 sample of the British population.

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Do Strong Family Ties Inhibit Trust?*

1. Introduction

According to the "emancipation theory of trust", developed by Toshio Yamagishi and his associates (1994; 1998), trust in others in general and commitment formation are alternative solutions to the risk of being exploited in social interactions. 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" (1998: 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 so far only indirect, the grand example being that US citizens, members of a highly mobile society, are more trusting than the Japanese, who belong to a more traditional and committed society that relies on family and groups.

Our goals in this article are to construct a direct test of this implication; to investigate whether this association is causal, and make progress in understanding the mechanism(s) through which FGT affect trust in strangers. We pursue our aims by combining experimental data with panel survey data, both drawn from a 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 evidence that the strength of an individual's family ties is indeed inversely correlated with his trust in strangers. To understand whether rather than being the result of self-selection this finding captures a causal relation – our second goal – we use an instrumental variable approach (using distance from family), which

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enables us to estimate whether exogenous variations in the strength of FGT affect trust in strangers; the resulting evidence is consistent with the effect of family ties being causal.

With regard to mechanisms, Yamagishi et al. (1998) conjecture that people with strong FGT are able to solve efficiently cooperation dilemmas because family and group members can more 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 options are lacking, and would thus cooperate less with and exhibit a lower level of trust in strangers compared with people more accustomed to operating in such an environment. We could frame this situation as an *equilibrium* that binds people with strong FGT to persist in their commitments to FGT and refrain from interacting with strangers. If they 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 conjecture that they have both more *opportunities* and a stronger *motivation* to deal with strangers in order to fulfill their need for social interaction not satisfied by family or group. The motivation could work directly and lead them to take more risks in trusting strangers. But opportunities and motivation could also work indirectly, in the sense that interacting more with strangers can lead to *learning.* What we may call *outward exposure*, in other words, could teach people to be more trusting by improving their ability (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 at first. If people are not usually trustworthy, then

[1] The idea that strong group ties are antithetical to trust with strangers is also found in Gellner

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more interaction with strangers would be associated with less trust in them); (2) to select trustworthy partners by reading the signs of (un)trustworthiness more accurately. [2] In our experiment, however, trusters do not observe or otherwise know anything about the trustees, thus (2) cannot possibly affect their trust decision; if learning has anything to do with their decision it would be through (1).

If outward exposure matters, its effects should also be observable via other conditions that hamper or promote it even if these are unrelated to family ties. We measure some of these factors, illustrated in detail below, and find that their effect on trusting is consistent with our prediction. This suggests a simpler interpretation of the underlying mechanism: the negative effect of strong FGT on trust may not so much reflect the lower opportunities for 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.

Here we do not analyse the decision to be trustworthy, which is sharply different from the decision to trust. There is no reason to expect that it will be affected by conditions similar to those that affect the trust decision. In one-off financial transactions, exemplified in our experiment, one trusts if one expects to get more out of them, while being trustworthy implies getting less, and unlike when one decides to trust, one takes no risks in being (un)trustworthy. In addition, so diverse are the properties that sustain the propensity to be trustworthy – altruism, moral values, reciprocity, evolved or cultural dispositions, socialised expectations of rewards or punishment (for a discussion, see Bacharach and Gambetta 2001: 153) – that one cannot come up with just *one* theory for it. Furthermore, one can imagine that some such properties are indifferent to the intensity of one's family ties, while others may

(1988), and was taken up by Fukuyama (1995).

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be negatively affected and still others positively affected. So there is no way to construct for trustworthiness a parallel theory to that which we propose for trust.

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 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.

2. Trust and the components of a trust decision

We work with a notion of trust related to specific acts, which makes it easier 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'.

[2] Yamagishi et al. mention (2) as the important mechanism (1998; p.172, fn. 8).

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

Trust decisions involve three components. First, subjects consider the *returns* they would receive if trust is fulfilled relative to the costs they would incur if trust is unfulfilled. Next, trust decisions depend on the *expected probability* that the trustee will do X (Gambetta 1988; Barr 2003). This is the result of beliefs about other people's trust-warranting qualities with regard to doing X. 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 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.

Third, when the probability of trustworthiness is less than 1, subjects' *willingness to take the risk* of being exploited comes into play too. This willingness 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 we take some account of expectations and risk preferences. As well as each of these two dimensions having an independent effect on trusting decisions, they may interact. First, 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

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elicits more experiences and thus speeds up learning, while being reluctant to trust provides little new evidence.

3. The experiment

We combine the experimental method with both a population 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 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). Through this approach we can obtain behavioural measures of both trusting and trustworthiness, as well as rich data on individual attributes.[3] For reasons explained below, we use a new experimental design developed by Ermisch and Gambetta (2006) that differs from the trust game used in most trust experiments.

We present to subjects -- whom we shall call the truster (R, a 'she' for convenience) and the trustee (E, a 'he') -- 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, described as compensation for taking part in the interview *cum* experiment (the cash up front should trigger an 'endowment effect', making R treat it as her own money and careful in parting with it as a genuine trust decision requires). 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 and that E, who is not paid for participating in the experiment,[4] is given the choice to either pay back £22 to R, or keep all £40. R is informed that if she decides

[3] Other studies have used population samples, but ones that are less representative of the population than these two studies (e.g. Barr 2003 and Schechter 2007).
[4] See below for a discussion of this point.

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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.[5] The procedures (described in full in Ermisch et al. 2007) 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.

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 him (e.g. see Bohnet and Huck 2004 and Bohnet et al. 2005).

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

[5] 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%.

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previous treatment. The two outcome variables of interest are the probability that R passes £10 on ('trusts'), and the probability that E pays back the £22 ('fulfils trust').

3.1 The trust decision in the experiment

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, those who have a subjective estimate of P > 10/22 should trust.[6]

More generally, whether the decision to keep or pass on her £10 truly involves trust 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 1 (payoff of Rs appears first).

FIGURE 1 ABOUT HERE

The key 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 twofold case. If R 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

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£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. We can imagine that this type has a sort of Kantian moral norm: 'do the right (egalitarian) thing, come what may'. This type chooses to pass on £10 regardless of whether or not she thinks that 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.

How affected our experiment is by the presence of altruists and norm egalitarians is an empirical question. The rather extreme altruistic preference seems by casual observation to be quite rare. More importantly, we know that one-fifth (n=14) of Rs who passed on £10 stated that they did not think about whether they were going to get back the £22. They could be norm egalitarians or altruists. Everyone else said that they weighed up the chances of getting the money back, thus indicating that they made a conditional decision as to whether or not to pass on their £10 based on trust (or distrust).

3.2 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) and for two main reasons we believe it more precisely

[6] If R's rate at which he discounts future income is r per month, then the present value of the

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captures a clear notion of trust and trustworthiness and avoids confounding effects.[7]

First, by forcing R's exposure to a loss of the whole sum she receives as a participation payment (or 83% in the second treatment), we depart dramatically from the standard TGE, in which R can pass on any amount. The possibility of transferring any amount favours the intrusion of other motives such as 'let's risk part of it'.[8] These motives become confused with trust, which, as we argued above, is the relevant consideration in R's decision to pass on £10 only as long as she is self interested or an outcome egalitarian. While we cannot eliminate a gambling motive for R's decision, by giving her a binary 'all or nothing' choice we increase the risk R needs to take if she chooses to pass on the money (in addition, we also use measures of willingness to take risks to examine the role that risk preferences may play in R's decision).

Next, 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 is an expectation of R when she makes her decision. E thus knows that he cannot be just a 'little trustworthy'; he has to choose whether to be trustworthy or not. Thus, 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

£22 received in a month is 22/(1+r), and it is worthwhile to trust if P>(1+r)(10/22).

[7] See Ermisch and Gambetta (2006) for a detailed critique of the standard TGE.

[8] Karlan (2005) found that, among poor Peruvian women, the more the truster passed on in the standard TGE, the lower her voluntary saving in a micro-credit. Because to save in such a program is to trust your peers to repay their loans, this association suggests that trusters who pass more are 'gamblers', namely people who, rather than trusting, are more willing to take risks. The distribution of amounts passed on in standard TGE experiments is also consistent with the operation of these other motives. It usually covers the entire range, with a distinct modal value of about 50% of the truster'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).

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she will return £22.[9] 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 because it does not allow Rs to split their endowment with Es. In addition, as argued above, the decision to pass on £10 for both self-interested and outcome egalitarian Rs is based on trust.[10]

3.3 The treatment

As noted above, we introduce two payoff structures. This would make no difference if R aims to maximise her expected returns. Yet, we introduced this treatment because (a) we expect that when R has the draconian option of parting with all her money or keeping it all she will be *less* likely to pass on £10. The reason for this prediction is that we expect R to be more inclined to trust when at worst she is left with something rather than nothing. This could be due to a number of mechanisms: for instance, exploitation aversion', i.e. R's fear of being taken advantage of by E, or the fear of loss, including loss of face, which could be triggered more vigorously by the prospect

[9] 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.

[10] 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.

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of 'losing everything'.[11] At the same time, however, we expect that (b) the treatment in which R has the draconian option will *increase* the chances that E will pay his due back. The reason why we expect E to be less inclined to return the money in the non-draconian case is 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 two hypotheses may both be either true or false, but in a way they contradict one another for if R too makes the same hypothesis (b) as we make about E, she should be *more* rather than less inclined to trust when the option is draconian.

4. Sample and survey methods

The sample frame was an over-sample of low-income households in 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 of people who had previously participated in the BHPS and of employing the same survey organisation that carries out the BHPS is that they are more likely to believe that the field organisation and the managers of the survey can be trusted to act as they promise. Trust responses in a new survey are likely to be contaminated by varying degrees of distrust in the organisers of the experiment, in

[11] 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).

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addition to trust in their co-player. Also, these subjects are used to receiving compensation for their participation. The experiment was carried out face-to-face by professional interviewers at the subject's home in the spring of 2007. Interviewers were instructed to read only from the experimental script and not to elaborate further.[12] We randomly matched E-players to the Rs who passed on the money.[13]

The experiment was carried out first.[14] 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 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), 170 of 242 eligible cases were interviewed, a response rate of 70%. In the second stage (Es), 84 of 126 eligible cases were interviewed: a response rate of 67%.[15] Almost all of the non-response arose because

[12] 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'; among E-subjects this percentage was 83%.

[13] A consequence of using the same survey organisation that carries out the BHPS is that 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 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, 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 (Ermisch et al. 2007).

[14] The computer assisted personal interview (CAPI) script for the experiment is described in Ermisch et al. 2007.

[15] The sample targets for Rs and Es were based on the expectation (based on previous research) that about 40% of Rs would pass on the money. It turned out that there were 10 Es who were unmatched because we exhausted the sampling frame for Rs: As a consequence, we were not able to interview as many Rs as we wanted. As these 10 Es thought they were

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of inability to locate the selected individual, who had moved house. Since we know that the probability of moving in a particular year is inversely related to length of current residence (e.g. Morrison 1971 for an early study, and Belot and Ermisch 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. Thus, it appears that those who move more often do not behave differently in terms of trust and trustworthiness.

Our sampling procedure is an improvement on the 'self-recruited' samples common in the literature. Still, because of its original purpose 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'. 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. In our regression analysis we control for the financial circumstances of the respondents. While the over-sampling of poorer people may mean that the average proportion trusting is not representative of the British population, this does not necessarily bias our estimate of the relationship between family ties and trust.

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responding to Rs (and in good faith we thought they were too), it is still valuable to analyse their decisions.

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5. Basic Results

Overall, we find that 43% of people are willing to trust strangers by giving them £10 in the expectation of receiving £22 if the stranger is trustworthy. One-half of strangers fulfil their trust, resisting the temptation of keeping £40 rather than only £18.[16]

After the Rs had inserted their decision card 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'.[17] These Rs were asked the follow-up question: *What did you think your chances of getting your money back were?* The results indicate that a person's expectation of the chances of return is strongly correlated with their trust decision: 80% of those who thought their chances to get the money back were greater than 50% passed on the £10, 70% among those who thought their chances to be 50/50, and 24% of those who thought their chances were lower than 50%. These results 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). By contrast, a larger percentage of Es return £22 (60 vs. 45) 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 could explain the puzzling finding of

[16] 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).

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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.

6. Family Ties and Trust

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'. We focus on the family member they see most often and create a dichotomous variable called *Family* that is equal to unity if they 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.[18]

TABLE 1 ABOUT HERE

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

[18] 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.

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Table 1 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. An alternative definition of strong family ties might include those who speak to a family member on the telephone *daily* amongst those with strong ties. In fact, only 3 (of 74) trusters in our sample who see their family less than once a week spoke to a family member daily. Including these amongst those defined to have strong family ties has little effect on our results: 35% of those with strong ties trusting compared with 49% with weak ties (p-value=0.075). Also, in the regression in Table 2 below, adding a variable for daily telephone contact hardly affects either the coefficient of *Family* or its standard error, and the coefficient of the new variable has a p-value is 0.69.

While *Family* reflects behaviour six 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 1 all the more remarkable.

In the experiment, people who trust must wait 3-4 weeks to see whether £22 was returned to them. One could suspect that those with strong family ties might be less patient, and that this could account for the correlation in Table 1 . However, this does not seem to be the case because the survey shows that those with strong family

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ties are *more* like to save out of current income (44% cf. 35%, p-value=0.26)—that is, if anything they are more patient. This is confirmed by a logit regression for saving that includes the same other variables as in model 1 of Table 2, in which *Family* attracts a coefficient of 0.32 (p-value=0.44).

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.

We draw on responses to questions about sample members' contact with neighbours that were asked annually in the BHPS between 1997 and 2001.[19] 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.[20] The association of the *Neighbour* variable with trust is *positive* (48% cf. 35%, p-value=0.097). 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.

7. Outward exposure and trust

[19] 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'.

[20] 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.

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We consider two further variables related to outward exposure, one limiting and the other fostering it. The first is the extent of an individual's involvement in family care (primarily looking after children, but also other relatives in the 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 (rather than paid employment, retired, etc.), and denote it as *FamCare*. One-fifth of Rs spent at least one year doing family care during 1997-2001 and they are less likely to trust (29% cf. 46%, p-value=0.059).

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. On the other hand, 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. We find that divorced and separated people are indeed more likely to trust (60% cf. 39%, p-value=0.025).[21] There are no significant differences among the remaining marital status categories—the difference is between the divorced/separated and the rest.

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.

[21] Liz David Barrett, in a personal communication, suggested an alternative mechanism to explain the positive effect of being divorce/separated on trust: people in couples could be more cautious about giving the £10 because it is not just their own money to lose, but also someone else's.

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8. Testing the effects of family ties on trust

In Table 2 we present three logistic regression models that control for other personal attributes that may affect trust. 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 also for their willingness to take risks in trusting strangers

TABLE 2 ABOUT HERE

Our measure of expectations compares those who state their 'chances of return' as '50/50 or more' with those who rate them less than 50-50. The threshold corresponds to the frequency of trustworthy responses in our experiment, making it a salient dividing line. This tool for measuring expectations is, however, rather blunt: actual expectations may be more refined than this, and variation in trust within these two categories 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. In models 2 and 3 (Table 2), 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 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

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('fully prepared to take risks in trusting strangers').[22] Rs who express more willingness to take risks *in trusting strangers* (6-10 on the scale) were more likely to pass on the £10 (55% cf. 38%, p-value=0.093), an effect confirmed in model 3.[23]

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.

Our main findings are robust across the three models in Table 2: 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.[24] *Neighbour* has a positive coefficient, 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 model, 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

[22] 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 (2007). 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.

[23] 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.

[24] 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. When we do not control for *FamCare*, the impact of being female becomes much smaller (e.g. falling to 0.24 in the first model specification), reflecting the higher values of *FamCare* for women.

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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 their financial situation is 'comfortable' or 'difficult' -- suggesting a U-shaped relation with financial situation; in other words people would trust more when either they feel they would not lose much or when they have nothing to lose.

The results further show that people trust more as they get older. If nostalgia for bygone days in which most people are supposed to have been trustworthy, a belief common in the U.K., has a basis in facts, this effect could have a generational component. Still, this result could also strengthen our main argument. Drawing from developmental psychology, Sutter and Kocher (2007) -- who studied experimentally the age effect on trust behaviour from childhood onward -- hypothesise that the positive effect of age on trusting behaviour could depend on the age-related increase in personal contacts and encounters (p.368). Age, in other words, could be another measure of outward exposure. This surmise is reinforced by Sutter and Kocher's further finding – which matches what Fehr et al. (2002) and Bellemare and Kroeger (2007) also found – that the increase in trusting behaviour peaks "around the age of 30 and 40", while it decreases later, especially in retirement when social contacts often shrink (p.380).[25]

9. Endogeneity and self-selection

Concerns over endogeneity led us to exclude from our models *civic participation*, whose impact on people's trust has been examined by other studies (e.g. Li et al.

[25] If we include age-squared, the positive effect of age on trust peaks around age 65 and then turns negative (the average age of Rs in our sample is 53.5). The effect of age is hardly affected by the inclusion of *Family* in the regression models.

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2005). 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% cf. 36% p-value=0.046), but we suspect that those who are more disposed to trust may be more likely to be active in organisations.

Endogeneity, however, could affect also variables that we chose include. Given the focus of the paper, we are concerned that the negative coefficient for *Family* in Table 2 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. [26] 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. The best candidate for such a variable at our disposal 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—see the first column of Table 3.

TABLE 3 ABOUT HERE

It might be thought that *Distance* is not a valid instrument because those who are less disposed to trust strangers do not move far from their family. We argue for its validity 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.

[26] 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. .

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To test for the exogeneity of *Family* we estimate a bivariate probit model for the probabilities of trusting and having *Family*=1, in which *Distance* appears in the *Family* equation but not in the trust equation. *Family* is in the trust equation and the other variables in both equations are as in the first model of Table 2. The exogeneity test is a test for the independence of the error terms of the two equations. We find that their correlation is only -0.10 (SE=0.28).[27] Thus, we cannot reject the exogeneity of *Family* using *Distance* as an instrument, which is evidence against the hypothesis that the negative coefficient of *Family* in Table 2 reflects self-selection.[28]

A word of caution, however, is required. On the one hand, an indicator of the power of the *Distance* variables suggests that they are not particularly weak: if we exclude them from the *Family* equation the chi-square-test value (3 d.f.) is 36.2 (*p*-value=0.0000) instruments. On the other hand, the power of the exogeneity test may be limited by the small size of our sample (n=142): *Distance* may not generate sufficient variation in *Family* relative to the sample size to produce a powerful exogeneity test or to estimate the effect of *Family* very precisely. That this may be the case is suggested by three considerations: (i) the instrumental variable (IV) estimate (of -0.54; std. error=0.38) includes both zero and the estimate in Table 2 (-1.00) in its 95% confidence interval; (ii) there is a relatively weak relationship between *Distance* and trust (second column of Table 3) (iii) substituting *Distance* for *Family* in model 1

[27] 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*.

[28] 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 2 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.

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of Table 2, we cannot reject the hypothesis that the coefficients of the *Distance* categories are jointly zero (*p*-value=0.29).

If we accept the results of the exogeneity test the estimates in model 1 of Table 2 are preferable to the IV estimates on efficiency grounds.[29] But even if we suspect that *Distance* is not a sufficiently powerful instrument or is an 'imperfect' one, the conclusion is the same and the estimates of model 1 are still preferable to the IV ones.

A problem similar to that posed by *Family* could be posed by *Neighbour*: 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 potentially endogenous *Neighbour* from the models in Table 2 hardly affects the coefficients of *Family* and the other outward exposure variables.

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 in the group that display matrimonial failures to take chances. However, we are reassured by the fact that the *increase* in size of the coefficient of divorced/separated when we control for willingness to take risks is not consistent with this explanation (cf. models 2 and 3 in Table 2).

10. Conclusions

The present study uses a behavioural approach to measuring trust in strangers and its correlates, undertaking an experiment with a British sample for which we have panel

[29] For those who remain sceptical about the validity of *Distance* as an instrument, recent analysis by Nevo and Rosen (2008) is relevant. They 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.

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data for the past and contemporary data about socio-economic circumstances, expectations and willingness to take risks. The design of our experiment aims to tap into those experiences that subjects draw from real life trust situations. According to Hoffman et al. (1996; p.655), "…subjects bring their ongoing repeated game experience and reputations from the world into the laboratory [the experiment]…" 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.

Overall, about 40% of people were willing to trust a stranger in our experiment, and their trust was rewarded one-half of the time by trustees. In particular, our results support the hypothesis that people with weaker links with their family are more likely to trust strangers.[30] 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 the

[30] Interestingly, in a recent paper Alesina and Giuliano (2009, table 2) also found that "family ties appear to be a substitute for generalized trust, rather than a complement to it"; they come to this conclusion by using survey data in a number of countries, and by measuring the effects of a measure of strength of family ties on trust attitudes, as measured by the standard survey question.

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difference in trust levels, between those with strong and those with weak family ties, is explained by the latter's greater outward exposure: trust is positively affected by *any* factor that promotes the experience of the behaviour of others beyond one's family circle. People who interact more with strangers and who have stronger motives to take risk with strangers appear to be more likely to trust because, if their experiences are predominantly positive, their expectation that people will be trustworthy is higher.[31] 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. In contrast, those with strong family ties sustain an equilibrium with limited interactions with strangers by their strong commitments to other family members.

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APPENDIX TABLE 1 HERE

[31] 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.

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Table 1: 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	

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Table 2: 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.

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Table 3: Percent Seeing Family at Least Once a Week/Percent Giving £10 and Distance from Closest Family Member

Distance from closest family member	**(1) Percent with *Family*=1**	**(2) % of Rs giving £10**	**N**
Less than 15-minutes	86.6	41.8	67
15-30 minutes	64.7	23.5	34
30-60 minutes	60.0	46.7	15
60 minutes or more	3.6	53.6	28
All	62.5	40.3	144
p-value	0.000	0.096	

p-value = 0.000

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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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Figure 1: R's Preferences over Outcomes

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*	

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