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Bellemare, C.; Kroger, S.

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ON REPRESENTATIVE TRUST

By Charles Bellemare, Sabine Kröger

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TILBURG ◆ ✿ ◆ UNIVERSITY

On Representative Trust*

Charles Bellemare

Tilburg University, CentER, The Netherlands

{c.bellemare@uvt.nl}

Sabine Kröger

Humboldt Universität zu Berlin, Germany

{skroeger@wiwi.hu-berlin.de}

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Abstract

Because of its relation to economic growth, there is a policy interest in measuring social capital and average trust as its currently most important proxy. In this paper we measure societal trust and trustworthiness by combining the virtues of laboratory experiments and survey data and present results from an economic experiment conducted using a representative sample of the Dutch population. By combining both types of data, we are able investigate the determinants of trust and trustworthiness at the population level, to contrast the inferences which can be made on trust propensity using stated and revealed measures, and to test for self-selection bias through voluntary participation in the experiment.

Our results can briefly be summarized as follows. Contrary to existing laboratory based experiments, we find that stated trust measures correlate with experimental trust. The effect of education and religion is shown to be depend enormously on the trust measure used. We find that the age and education profiles of experimental trust are the complete opposite of those of trustworthiness, which contrast with the findings of the social capital literature. Finally, we do not find evidence of a participation selectivity bias, which is a serious concern for laboratory experiments which rely almost exclusively on volunteer participants.

Introduction

It is increasingly argued that a nation's social capital can influence its economic performance. Although there is an ongoing debate over what constitutes social capital (Bowles and Gintis, 2002, Durlauf, 2002), there seems to be a consensus that both average societal trust and trustworthiness are two important components. The transactions cost paradigm remains the traditional way of thinking about the mechanism by which average trust affects economic performance as higher average societal trust leads to lower transactions costs which in turn improve both the functioning of organizations and governments which ultimately leads to better economic performance.

The research on social capital started with the influential work of Putnam (1993) who found a strong correlation between measures of civic engagement and government quality across regions in Italy. The association of social capital with growth started with the work of Knack and Keefer (1997) and Zak and Knack (2001) who both find that a one-standard deviation increase in the World-Value Survey (WVS) trust index increases economic growth by more than one-half of a standard deviation. La Porta et al. (1997) find that across countries, a one-standard deviation increase in the same measure of trust increases judicial efficiency by 0.7 of a standard deviation and reduces government corruption by 0.3 of a standard deviation. The WVS trust index on which these empirical facts rests is constructed by first drawing a random sample of participants who are asked to answer a series of questions. The wording of the trust question is

WVS trust question Generally speaking would you say that most people can be trusted or that you cannot be too careful in dealing with people?

1.) Most people can be trusted.

2.) You have to be very careful.

3.) I do not know.

The WVS reports for each country the percentage of responders who indicated that "Most people can be trusted".

Social scientists have argued that and individuals trust will be a function of his or her past experiences (Hardin 1996). These in turn may be affected by the individuals

age, education experience with trust. It becomes relevant for policymakers to correctly measure average trust and trustworthiness and to understand how they correlate with these characteristics for two reasons. First, simply because of the strong correlation between trust, economic growth and organizational performance. Secondly, it is well known that the age and income distributions of many Western societies evolve over time in very alarming ways (Gruber and Wise, 2001). How these structural changes translate into changes in social capital levels is still not well understood but deserves investigation.

Therefore, measuring the evolution of average trust and trustworthiness in a society requires a micro level analysis. There is very little empirical evidence on the determinants of trust and most of it relies on stated trust responses from WVS type questions. Allesina and La Ferrara (2002) use data on a sample of individuals in the United-States who have answered the WVS trust question. Their empirical results show that recent history of traumatic experiences, belonging to a group which historically felt discriminated and low levels of income and education have strong negative influences on trust. One drawback of this study is that it is based on the self-reported measure of trust which may be misreported by the individuals. This is particularly important since misreporting of a discrete dependent variable may lead to severely biased slope parameters even when the model correctly specifies the true unobserved categorical variable (see Bound et al., 2001, for a survey on the impact of this type of measurement error). Even though it is intuitively clear that self-reported answers to the WVS trust question may well be misreported, the empirical evidence suggesting that this is the case is scarce (see Bellemare and Kröger, 2003, for a first attempt to analyze the importance of misreporting in the WVS trust question).

Economic experiments, which collect data on revealed rather than stated preferences, is a potential alternative to survey trust measures. The seminal experiment of Berg, Dickhaut and McCabe (1995) (hereafter BDM) initiated an important follow-up literature and remains today the main experimental design used to assess the presence of trust and trustworthiness in a group of subjects. The BDM game is tailored to capture trust as defined by Coleman (1990) who defines an interaction as one involving trust when three contextual elements are present. i) a person S must place confidence

in the action of another person R which could lead S to experience a loss of welfare. ii) A trustworthy R behaves such that S is better (or at least not worse) off than he was before placing trust in R, iii) successful use of trust in transactions should make both S and R better off. The BDM design captures all three components of Coleman's definition. Numerous studies[1] have extended the BDM design to asses the robustness of the findings to different institutional and design settings.

Both survey and laboratory trust measures have important limitations. Laboratory experiments analyze behavior of subjects interacting with other participants in a controlled environment where decisions have real monetary consequences. This method has the virtue of measuring revealed trust intentions of individuals at the expense of generally doing so with homogenous student subject pools lacking the required variation in background characteristics to measure the parameters of interest. On the other hand, survey data like the World Value Survey observe responses in hypothetical situations from an heterogenous sample of a population at the expense of measuring stated preferences for trust.

The analysis of the determinants of trustworthiness is even less documented than trust, in part because there does not exists any satisfactory survey question which seems to embody the distinctive features of trustworthiness. Our current empirical knowledge has been provided by laboratory results which analyze responders behavior in trust games, the most famous of being the BDM game[2]. Overall, these studies have shown that trustworthiness, like trust, is robust to different institutional settings and designs (see the above references). Once again, the lack of heterogeneity in the subject pool prevents population inferences and complete measurement of its determinants. To our knowledge, there does not exists any micro level analysis of the determinants of trustworthiness in a society. This is troublesome as recent research has shown that

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[1]Glaeser et al. (2000) measure social capital among participants with the BDM game and additionally by analyzing the willingness to pay in a dropped envelope game. Gneezy, Güth, and Verboven (2001) investigate whether distributional considerations might explain the trustor's behavior by varying the range from which the trustee can send back. Burks, Carpenter, and Verhoogen (2003) study whether changing roles in a trust game gives rise to a more cooperative outcome as both parties experience all situations as trustor and trustee. Ortmann, Fitzgerald and Boeing (2000) find that the results of BDM are quite robust using different frames. All these papers find that the results of the BDM experiment are robust and lead to conclude that revealed trust is an important aspect of economic behavior.

[2]Other trust designs include Abbink et al. 2000, McCabe et al. 2002, and James Jr. 2002

trustworthiness could be "the" economically relevant component of social capital to understand the process of economic development (Francois and Zabojnik, 2002). If this were the case, then the lack of empirical evidence on the determinants of average societal trustworthiness is source of concern.

This paper makes two important contributions. First, we add to the scarce body of knowledge on the determinants of trust and measure age, education and other life experience effects of experimental trust responses from the BDM game. We do so by having a random sample of the Dutch population play a computerized version of the BDM game. A side product of our analysis is that we are able to contrast the inferences which can be achieved by using stated and revealed trust preferences. To do so, we propose a new minimum distance test statistic based on the distance between the underlying data generating processes of the stated and the revealed trust responses. Our evidence shows that the effect of some background characteristics can change dramatically when using the stated rather than the experimental trust responses.

Secondly, we shed a first look into the determinants of trustworthiness in a population. We follow the established trend of analyzing the choices of subjects assigned to the role of respondents in the BDM game. The crucial difference between our study and the existing literature is the heterogeneity of the responders background characteristics. To our knowledge, this is the first empirical assessment of the determinants of trustworthiness at the society level. We find that the education and age patterns to be the complete opposite of those of trust.

The paper makes some additional contributions. First, we depart from most trust experiments and elicit subjects' beliefs about their partner's action. We find that subjects' beliefs play a fundamental role in determining both trust and trustworthiness behavior.

Secondly, our paper allows to asses whether the inferences made using a random sample of the population differ substantively from those one could make using the more traditional student subject pools. The idea of using more heterogeneous subject pools in experimental economics is not new as testing the assumption of "parallelism" between the lab and the field has been a source pre-occupation for some time. The most popular mediums to reach more diversified participants have been newspaper ex-

periments (e.g. Bosch-Domènech et al., 2002, Güth, Schmidt and Sutter, 2002), and, more recently, the Internet (e.g. Lucking-Reiley, 1999). These papers generally find that newspaper and lab subjects have similar unconditional behavioral responses. One drawback of these studies is that one cannot make inferences on societies' average behavior as the subjects are not drawn randomly from the population of interest. Two noteworthy experiments have recently been run with representative samples. Harrison et al. (2002) setup an laboratory experiment to investigate the heterogeneity in individual discount rates. Their sample consists of a small random sample of the Danish population. Hey (2002) used a random sample of the Dutch population headed by the CentERpanel of Tilburg University and had them play an experiment on decision making under risk and uncertainty.

Thirdly, an inevitable consequence of most laboratory experiments is that they cannot control for the fact that subjects with specific unobserved characteristics such as high gambling predispositions and low risk-aversion self-select themselves to participate in the experiment. If these unobservable factors are related to the outcome that is measured in the lab, a familiar selection bias will be present (Heckman, 1978, 1990). Eckel and Grossman (2000) have attempted to investigate participation bias in laboratory experiments. Comparing student volunteers to student non-volunteers[3] they conclude that volunteers perform significantly differently. By giving our subjects the choice to participate in our experiment and by observing the characteristics of those who refuse to do so, we are in the unique position to test for participation bias in our experiment and control for its effect.

The remainder of the paper is organized as follows. Section 1 describes the design of the experiment in greater detail and summarize the experimental procedure. Section 2 describes the econometric models which we will use to analyze the trust and trustworthiness responses. Section 3 presents and discusses the empirical results of the experiment, analyzing in turn trust, trustworthiness and participation in the experiment. Section 4 concludes.

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[3]Those subjects did not deliberately decided to participate in an experiment. They were recruited in the course where the experiment was conducted immediately during the class.

1 The experimental design and procedure

The recruitment of our subjects was made by CentERdata, the survey research institute accommodated at Tilburg University, the Netherlands. The main activity of CentERdata is to manage and carry out panel surveys through a telepanel: the CentERpanel (CP). The CP is a panel of about 2000 representative Dutch households. The members of those households answer questions at home. We first give a brief description of the CP and then explain in detail our experimental design.

Every week households across the Netherlands are randomly drawn from the CP. These weekly questionnaires are delivered every Friday and some members of the selected households are asked to answer a questionnaire. They do this at any time between Friday and Tuesday of the following week, either on their computer or on a television set which is connected to a set-up box linking the household to the CentERdata server. In order to have a random sample of the Dutch population,[4] low income households without a computer or a television set where given the necessary equipment in order to complete the weekly questionnaire.

There are many reasons why the CP is an attractive device to conduct experiments. Apart from giving us access to a representative sample our design replicates as closely as possible the environment of a laboratory experiment as participants answer questions on a computer or a television set. Sample computer screens[5] (in Dutch) in the appendix of this paper show that they resemble typical laboratory screens. Furthermore, CP participants are acquainted to payments in fictitious currency as CentERdata reimburses the weekly telephone costs for answering the questionnaire in CentERpoints[6] which are automatically credited to each respondents private bank account four times a year. Finally, because participants interact with CentERdata, the experiment is double blind as participants were told that they will be matched anonymously and that their identities would not be given to the experimenters.

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[4]Immigrants are excluded from the panel as their language proficiency in Dutch makes it difficult for them to answer the questions on a weekly basis. For a description of the recruitment and sampling methods of CP members see: www.centerdata.nl .

[5]A translated version of all screens can be found in appendix B. All screen shots of the experiment in Dutch are available upon request at the authors.

[6]The exchange rate is 1 CenterPoint = 1 EuroCent (= 0.01 EURO).

In the trust game a sender S sends a share of his endowment to a responder R. R receives the amount plus interest (the amount is multiplied by a number greater than one) and has then the possibility to send back an amount to S. The payoffs for both players are as follows. S receives his initial endowment reduced by the amount sent plus the amount received from R. R earns additionally to her initial endowment the multiplied amount sent by S reduced by the amount R returns to S. Since the trust game described above is an interactive game involving two players and because panel members respond at different times of the day during the answer period, we chose to have the responders play the strategy method. The strategy method requires from responders to state contingent responses for each possible amount they might receive from their sender. The response which corresponds to the decision of their sender will be chosen to be the effective action and will determine the payoff of both participants. Apart from allowing us to have senders play with responders at different times, the strategy method has several advantages. First, it facilitates data acquisition as the complete strategy plan for all 11 possible amounts received is elicited. Secondly, as our game may seem complex to some subjects, the strategy method requires that people thoroughly familiarize themselves with the ramifications of all choices, so that we do not retrieve data from uninformed choices. Thirdly, reciprocity to trust in economic transactions might appear to be based on quick responses but seems rather a tough intensive process. People do not react inconsiderately but take rather time to think about the consequences of their actions, e.g., concerning self-perception, strategic and/or moral ventilation, and what they plan to do in each possible contingency they will face. As we attempt to understand responses to trust, it would seem more natural to employ the strategy method.[7]

In our design, senders S and receivers R are both endowed with 500 CentERpoints. By giving the same endowment to both players, we exclude the possibility that senders send amounts to the receivers for distributional motives such as fairness and inequality

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[7]According to usual beliefs in economics, the stationarity of preferences of individuals across different choices implies that the strategy method would yield the same decisions as the procedure involving observed actions. Nevertheless, it is still believed that some actions might trigger a stronger response in an immediate 'hot' environment. The evidences supporting this case are weak. Brandts and Charness (2000) find that the strategy method and the hot environment do not yield significant different responses in two simple sequential two player games.

aversion. We discretize the choice set of the senders to 11 amounts $50 \times y^E$ with $y^E \in \{0, 1, ..., 9, 10\}$. The amount sent is doubled[8] by the experimenters. Responders played the strategy method which means they had to state how much they are willing to give back to the sender for each of the 11 possible amounts that senders can send to them. After all participants made their decisions, senders and responders were randomly matched. Payments were subsequently made by taking the element of the strategy of each receiver that corresponds to what their matched sender player had actually sent to them.

Under the assumption that the objective of both players is own payoff maximization, the Nash equilibrium of the game is for the sender to send nothing to the receiver as the payoff maximizing receiver's dominant strategy is to return nothing to the sender. Hence, observing increasing positive amounts sent is interpreted as evidence that people increasingly trust others. Likewise, observing positive returned amounts is taken as evidence of the existence of reciprocity. By being a one shot game, the BDM design allows to measure a basic trust propensity which abstracts from reputation effects and punishment strategies which emerge in games of repeated interaction. Disentangling basic trust from reputation, punishment and experience effects in repeated trust games is difficult (see Anderhub et al. 1999 and Willinger et al. 2003?). These effects are deliberately excluded in the present design.

During the experiment participants could switch back and forth between the screens until completion of the experiment, after which they were not allowed to go back again. After players have entered their decisions, they are asked to answer some questions about the beliefs of their partner. We ask these beliefs after subjects have made their decisions in order to circumvent the possibility that belief elicitation induces non-cooperative behavior when asked before the play of the game (Croson, 2000). We elicit senders beliefs with two questions. The first question asks senders to state how much they think the receiver will return to them. The second question asks them to state what they think the average sender will send. The latter question will allow us to control for behavior to conform towards some social norm. Responders on the other

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[8]The choice of the multiplier usually varies between 2 (e.g. Glaeser et.al. 2000 and Gneezy et al. 2001) and 3 (BDM and Ortmann et al. 2000)

hand simply had to state how much units they think of receiving.

Subsequently, both senders and responders where asked to state their average experience with trust by answering the following question

Lifetime trust experience question   In the past, when you trusted someone or something, were you usually rewarded or usually disappointed?

(Always rewarded) 1 , 2 , 3 , 4 , 5 , 6 , 7 (Always disappointed).

In addition, we asked each participant to answer the WVS survey trust question mentioned in the introduction. This will allow to compare the inferences which can be made using stated and revealed decisions for the same subjects. The timing of the WVS trust question raises several methodological issues. As human behavior may change over time due to certain events or experiences, one would ideally want to collect both measures from the same person at the same point in time. However, it is important that the first measurement does not influence the other in a matter which cannot be controlled for.

One possible control strategy consists of running several experiments with different sequences of the tasks. For our experiment this would mean that some participants first interact in the experiment followed by answering the questions concerning trust experience and behavior. Other participants would face the reverse order, first answering the trust related questions and afterwards decide in the experiment. This design is undesirable as the terminology within the experiment is deliberately neutrally labelled to prevent framing effects, e.g., words related to the subject of study like "trust" are not mentioned at all. Asking the trust question beforehand would frame the whole experiment and the revealed data would be contaminated. This is likely to happen if participants generate specific anticipations concerning the behavior in the experiment which no longer under the control of the experimenter. Alternatively one ask the WVS trust question several weeks before the experiment is run. This is precisely what Glaeser et al. (2000) have done. In their design, the WVS trust question was asked 2 months before the experiment was undertaken. Their within subject comparison is valid under the strong assumptions that the trust propensity of subjects is stable over time and that subjects have in the meantime forgotten their answers and/or do not condition on it later in the experiment. Glaeser et al. do not make any attempt to verify

if the experimental result is corrupted in any way by having subjects answer first the WVS question, nor do they check is answers to the WVS are stable over time. One of the main results of the Glaeser et al. study is that WVS answers do not correlate with experimental trust. Abstracting from the possible non-stationarity in subjects trust propensities, we will show in the next section that that the "no-correlation" result of Glaeser et al. has in any case no clear behavioral interpretation.

Since our main contribution is to have a random sample reveal their trust and reciprocity preferences, we first had subjects play the experiment followed by the WVS trust question. We test for the presence of contagion effect in the WVS answers using appropriate econometric techniques which will be discussed in the next section.

Two weeks after their decision participants received de-briefing and feedback information summarizing the experiment, their decision, the decision of their matched partner and their final payoff which then was added to their CentER bank accounts. Despite all the advantages of using the CP, some elements are sources of potential concern. First, panel members were not constrained to complete the experiment in a limited amount of time and this could mean that some seek advice to answer their questionnaire. Explaining the procedure in greater detail in the remainder of this section we show that answering time for the experiment is rather low making such collusive behavior unlikely.

The experiment was conducted in two sessions, in the 31st and the 36th weeks of the calendar year 2002. 541 CP members were randomly contacted to participate in the economic experiment. Each individual contacted had to read an opening screen informing them that they were selected to participate in an experiment conducted jointly by a team of researchers from Tilburg University and Humboldt University Berlin. Anonymity was stressed through out the opening screen. A detailed description of the game followed with the mode of payments. Each person was informed that conditional on their participation, they would be randomly matched to one of the roles. The role is only revealed to panel members once they have agreed upon to participate. Out of the 541 panel members contacted, 42 panel members declined to participate, leaving us with 499 panel members who completed the experiment of which 276 were

S players and 223 were R players[9].

Table 2 gives the variable descriptions and descriptive statistics of the 541 household members contacted per role category. The means of most variables are relatively identical across non-participants, senders and responders. 63.7% of the persons contacted were heads of households and most players either had a secondary or vocational training degree. Catholics and protestants are the two most important religious communities in the sample and their relative weights in the three participation categories are very similar. Two notable differences across the three groups concern work propensity and age. Non-participants are on average 10 years older than both senders and responders. This age effect is also reflected in a higher labor market retirement frequency and lower labor work participation.

Figure 1 gives a graph of a nonparametric regression of the participation decision in the experiment on age. The participation probability remains close to 1 from the age of 16 to 40. From 40 onwards however, the participation probability significantly declines and reaches a low of 82% at the age of 60. Beyond 60 years of age, the participation probability increases again, although this rise is not statistically significant. As most non-participants did not mention any specific reasons for their decisions, we can offer some hypothetical explanations for the mid-life drop in participation. First, the natural explanation would be that non-participants have a higher opportunity costs of time. This interpretation is inconsistent with the descriptive evidence indicating that non-participants are both less involved in the labor market and do not have above average income. An alternative explanation would be that non-participants are more reluctant to get involved into cognitive demanding tasks. CentERdata keeps track of the time taken to answer any question they ask their members to answer. Table 1 displays some statistics on game completion time for all types of panel members contacted.

As expected, non-participants have the lowest participation time in the experiment at every point of the distribution, with a median time slightly greater than a minute. The distribution of time of play of R dominates that of S players at every quartile, a

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[9]Of the 42 non-participants, 14 of which had initially accepted to play but eventually backed out of the experiment after having observed the roles they were assigned to play. It is interesting to note that 11 out of the 14 panel members who declined to participate after having observed their roles were R players.

	25th	Median	75th	IQR	N
Not Played	0.73	1.28	3.36	2.63	42
S	2.40	3.29	4.30	1.89	276
R	4.10	5.93	8.15	4.05	223

Table 1: Percentiles of the minutes spent playing the experiment. The Inter-Quartile-Range (IQR) is defined as the difference between the 75th percentile and the 25th percentile of the distribution.

result primarily due to the fact that R players had to play the time-intensive strategy method. The interquartile range serves as a measure to judge inequality in a distribution. We find that R players not only took more time to play the experiment but that the distribution of their time of play is the most dispersed, followed by that of non-participants and that of S players. We mentioned previously that the unlimited answering time given to the participants allows for the possibility of group rather than individual decision making. Assuming that collective decision making takes time to complete, a high game time would be a source of concern. However, since the majority of players took less than 10 minutes to complete the experiment from the time they logged in the CentERdata network, it is unlikely that such collusive behavior is present.

2 The senders decision

2.1 Revealed versus stated trust behavior

We model trust as an unobserved continuous latent process. Let individual i's unobservable trust propensity $T_i^* \in \mathbb{R}$ be represented by the following linear single index form

$$T_i^* = \mathbf{x}_i^{\mathbf{t}\prime}\beta^{\mathbf{t}} + \varepsilon_i^{\mathbf{t}} \tag{1}$$

where $\mathbf{x}_i^{\mathbf{t}}$ ($K \times 1$) is a vector of realizations of the observed characteristics of proposer i plus a constant term, $\beta^{\mathbf{t}}$ ($K \times 1$) is the complete vector of unknown slope population parameters and $\beta^{\mathbf{t}*}$ will be referred to as the vector of slope parameters excluding the constant term. We observe two different discrete measures of trust for each individual, the $y_i^E \in Y^E$ comes from revealed preferences of the experiment and $y_i^{WVS} \in Y^{WVS}$

based on stated preferences based on answers to the WVS trust question. Both sets are defined as

$$\mathbb{Y}^E = \{0, 1, ..., C\}$$
$$\mathbb{Y}^{WVS} = \{0, 1\}$$

The categories correspond to the amount sent assuming that the individual trust propensity increases with higher amounts sent. We are interested in measuring the effects of background characteristics of trust. Both discrete measures can be thought of being generated from two distinct mappings from the real line to either $\mathbb{Y}^{WVS}$ or $\mathbb{Y}^E$. The ordered and binary response mapping functions (Maddala, 1983) yield the following choice probabilities

$$\Pr\left(y_i^E = j | \mathbf{x}_i^{\mathbf{t}}\right) = \Pr\left(\eta_{j-1} < T_i^* < \eta_j | \mathbf{x}_i^{\mathbf{t}}\right) \quad \text{for } j = 0, 1, ..., C \tag{2}$$

where $\{\eta_{j-1}\}_{j=0}^C$ are threshold parameters We make the following normalizations $\eta_{-1} = -\infty$ and $\eta_C = \infty$. The probabilities of stating that one trusts is given by

$$\Pr\left(y_i^{WVS} = 1 | \mathbf{x}_i^{\mathbf{t}}\right) = \Pr\left(T_i^* > \eta_0 | \mathbf{x}_i^{\mathbf{t}}\right) \tag{3}$$

In both models, η_0 is not separately identified from the constant, we normalize it to 0. Under the assumption that ε_i is normally distributed with variance σ^2 we obtain the familiar ordered and binary probit models. Allesina and La Ferrara (2002) use the binary probit to analyze individual responses to the WVS trust question. Implicit in their empirical model is the underlying latent trust propensity defined above. In parallel, the ordered probit model has been used to analyze experimental trust responses in the BDM game. Here again the latent trust propensity is implicit in the empirical model.

In our design, senders answered the WVS trust question after having chosen y_i^E. This contrasts with Glaeser et al. who asked the WVS two months before the experiment took place. As we discussed in the previous section, there are advantages and disadvantages to both approaches. The main disadvantage of our approach is that senders have the possibility to reinforce their experimental decisions with their answer to the WVS question rather than to state their true underlying trust propensity. This

would induce a spurious resemblance between both measures. Two cases are worth mentioning. First, an individual who sent a large amount in the experiment may be more inclined to state that he trust others when in fact he would have answered negative if the experiment had not taken place before hand. We denote the probability of this event as $\alpha_{10}\left(y_i^E\right)$. Secondly, some of the low senders may be more likely to state that they do not trust others when in fact they would have answered the opposite had the experiment not preceded the WVS question. Correspondingly, we denote the probability of this event as $\alpha_{01}\left(y_i^E\right)$. If either event occurs with positive probability, our WVS trust answers are potentially misclassified, where the magnitude and the direction of the misclassification would depend in part on y_i^E. To investigate this claim, we generalize the binary probit model (3) to allow for the possibility of misclassification. Using the law of total probability, we can rewrite the probability of answering positively to the WVS trust question as

$$
\begin{aligned}
\Pr\left(y_i^{WVS} = 1 | \mathbf{x}_i^{\mathbf{t}}, y_i^E\right) &= \Pr\left(y_i^{WVS} = 1 | T_i^* \geq 0, y_i^E\right) \cdot \Pr\left(T_i^* > 0 | \mathbf{x}_i^{\mathbf{t}}\right) \quad (4) \\
&\quad + \Pr\left(y_i^{WVS} = 1 | T_i^* < 0, y_i^E\right) \cdot \Pr\left(T_i^* < 0 | \mathbf{x}_i^{\mathbf{t}}\right) \\
&= \left[1 - \alpha_{01}\left(y_i^E\right)\right] \cdot \Pr\left(T_i^* > 0 | \mathbf{x}_i^{\mathbf{t}}\right) + \alpha_{10}\left(y_i^E\right) \cdot \Pr\left(T_i^* < 0 | \mathbf{x}_i^{\mathbf{t}}\right) \quad (5)
\end{aligned}
$$

Equation (5) shows that in the presence of misreporting, the probability that a sender states that he trusts others will be a weighted sum of the probability that he would really answer positively and that he would really answer negatively to the WVS question. In the absence of misclassification $\Pr\left(y_i^{WVS} = 1 | \mathbf{x}_i^{\mathbf{t}}, y_i^E\right) = \Pr\left(T_i^* > 0 | \mathbf{x}_i^{\mathbf{t}}\right)$, which corresponds to truthful statement of preferences. It is important to note that the experimental decision y_i^E only affects answers to the WVS trust question via their effect on the misreporting probabilities. Put another way, the experiment does not directly affect the trust propensity of an individual but it affects his probability of misreporting his true intentions. We make the following functional form assumptions on the misclassification probabilities

$$
\begin{aligned}
\alpha_{10}\left(y_i^E\right) &= \frac{\exp\left(\theta_0^{10} + \theta_1^{10} y_i^E\right)}{\exp\left(\theta_0^{10} + \theta_1^{10} y_i^E\right) + 1} \\
\alpha_{01}\left(y_i^E\right) &= \frac{\exp\left(\theta_0^{01} + \theta_1^{01} y_i^E\right)}{\exp\left(\theta_0^{01} + \theta_1^{01} y_i^E\right) + 1}
\end{aligned}
$$

All models are estimated my Maximum Likelihood. Some special cases are of interest.

If $\theta_1^{10} = \theta_1^{01} = 0$ misclassification is random in the population of senders[10] and is not affected by the preceding experiment. This hypothesis can be tested using usual Wald type tests (e.g. Ruud, 2000). When $(\alpha_{10} = \alpha_{01} = 0)$, senders truthfully answer the WVS question. We test for this last hypothesis using the log-likelihood ratio test approach of Andrews (2001)[11].

2.2   Comparing stated versus revealed preferences

A heavily debated topic is whether stated trust measures convey any information of the true underlying trust propensity of individuals. Glaeser et al. (2000) run a linear regression of y_i^E on y_i^{WVS} and a set of covariates and find that the coefficient of y_i^{WVS} is not statistically significant. They interpret this result as evidence that stated trust measures are unreliable as the result of individual misreporting of their true trust intentions. This result has since been referred to as convincing evidence against the use of stated trust responses (Bowles and Gintes, 2002) . We now show that the Glaeser et al. test explores whether or not the *unobserved* component determining y_i^{WVS} correlates with y_i^E. The proof of this result starts by writing down the following two linear probability models

$$y_i^E = \mathbf{x}_i^{\mathbf{t}\prime}\beta^E + \lambda y_i^{WVS} + \varepsilon_i^E \tag{6}$$

$$y_i^{WVS} = \mathbf{x}_i^{\mathbf{t}\prime}\beta^{WVS} + \varepsilon_i^{WVS} \tag{7}$$

The first equation corresponds to the linear probability model estimated by Glaeser et al. where the experimental results are regressed on the observed characteristics and answers to the WVS trust question while the second equation relates answers to the WVS trust question to the same vector of observed characteristics. Substituting (7) in

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[10]This is the model studied by Hausman et al. (1998).

[11]The parameter space of the misclassification probabilities is $[0,1]\times[0,1]$. Under the null hypothesis of no misclassification, $\alpha_{10} = \alpha_{01} = 0$ rests on the boundary of the parameter space. In this specific case, it is known that the distribution of the LR test statistic is no longer a standard chi-square distribution. Andrews (2001) proposed a technique to simulate the true critical values of the LR test. For a recent application of this method, the reader is referred to Dustmann and van Soest (2003).

(6)

$$
\begin{aligned}
y_i^E &= \mathbf{x}_i^{\mathbf{t}\prime}\beta^E + \lambda\left(\mathbf{x}_i^{\mathbf{t}\prime}\beta^{WVS} + \varepsilon_i^{WVS}\right) + \varepsilon_i^E \\
&= \mathbf{x}_i^{\mathbf{t}\prime}\left(\beta^E + \lambda\beta^{WVS}\right) + \lambda\varepsilon_i^{WVS} + \varepsilon_i^E \\
&= \mathbf{x}_i^{\mathbf{t}\prime}\beta^{E+} + \lambda\varepsilon_i^{WVS} + \varepsilon_i^E
\end{aligned}
\tag{8}
$$

where $\beta^{E+} = \beta^E + \lambda\beta^{WVS}$. Equation (8) clearly shows that testing $\lambda = 0$ amounts to testing whether ε_i^{WVS} correlates with experimental trust. Economically, it is unclear whether this correlation is of any interest. The literature comparing stated and revealed preference data has long recognized that the unobserved components of both types of data are likely to differ. Hence, the insignificant result found by Glaeser et al. can be difficultly interpreted. At a more fundamental level, it is more interesting to examine whether the inferences made on the joint effects of background characteristics on trust differ between both measures. This would require a test which compares the systematic part of the trust propensity across both models. The powerful feature of our empirical approach is to recognize that both experimental and WVS trust responses are modelled as being generated from the same underlying linear trust propensity (1). If all statistical assumptions of the model hold, Maximum Likelihood yields consistent estimates of $\beta^{\mathbf{t}}/\sigma = \left\{\frac{\beta_1}{\sigma}, \frac{\beta_2}{\sigma}, \frac{\beta_3}{\sigma}, ..., \frac{\beta_K}{\sigma}\right\}$. As we just discussed, comparing $\beta^{\mathbf{t}}/\sigma$ across both models is undesirable as σ^2 in both models are related to unobservable components whose difference have no clear behavioral interpretation. However, dividing $\beta^{\mathbf{t}}/\sigma$ by say the second component $\frac{\beta_2}{\sigma}$ yields a vector $\beta^{\mathbf{t}}/\beta_2 = \left\{\frac{\beta_1}{\beta_2}, 1, \frac{\beta_3}{\beta_2}, ..., \frac{\beta_K}{\beta_2}\right\}$ which is independent of σ. This suggests fixing the k'th slope parameter to 1 or -1 and recovering consistent estimates of $\xi = \beta^{\mathbf{t}-}/\beta_k$ (where $\beta^{\mathbf{t}-}$ is the subset of $\beta^{\mathbf{t}}$ which excludes the k'th slope parameter) both with the ordered and binary probit models.

Under the null hypothesis that the effects of background characteristics are the same, both sets of estimates would equal each other[12]. This forms the basis of a new minimum distance test which simply evaluates whether there are sufficient statistical differences between both sets of estimates. Our test statistic has the following familiar

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[12]The constant term parameter is generally not separately identified from the threshold parameters in both the binary and ordered probit models. Given their values are function of the ad hoc threshold assumptions, they are not used in computation of the test.

quadratic form

$$N \left(\xi^E - \xi^{WVS} \right)' \mathbf{W}^{-1} \left(\xi^E - \xi^{WVS} \right) \sim \chi^2 \left(K - 2 \right)$$

where ξ^E and ξ^{WVS} denote the vector of parameters of the experimental and stated trust models and $\mathbf{W}$ represents the covariance matrix of the difference vector $\xi^E - \xi^{WVS}$. A proof of the consistency, asymptotic distribution and details on the computation of the test can be found in the appendix of the paper.

2.3   The reciprocity decision

Responders were asked to play the strategy method by which they state how much they will give back for each of the 11 possible amounts they can receive from the sender. This design implies that we observe for each responder an eleven dimensional vector y^R

$$\mathbf{y^r} = \begin{cases} y_0^{\mathbf{r}} \in [0, 500] \\ y_{50}^{\mathbf{r}} \in [0, 500 + 2 \times 50] \\ y_{100}^{\mathbf{r}} \in [0, 500 + 2 \times 100] \\ \qquad \vdots \\ y_{500}^{\mathbf{r}} \in [0, 500 + 2 \times 500] \end{cases}$$

where $y_0^{\mathbf{r}}$ denotes the amount the responder is willing to return when receiving 0 CentERpoints from the sender, and so on. For any given individual, an increase in $y_a^{\mathbf{r}}$ as a increases may be due to the fact that the responder can return more and not because of the presence of genuine reciprocity. Since reciprocity is of primary interest, we remove this effect by dividing $y_a^{\mathbf{r}}$ by the upper bound of the width of category a, expressing the amounts returned as percentages of the total possible amount sent for each category. Note, that this transformation only normalizes all responses to lay between 0 and 1. We model increase reciprocity as the increase in proportion of possible money returned as a increases. Let $R_{ia} \in [0, 1]$ for all $a = 0, 1, 2, ..., 10$. Rescaling censors the data below at 0 and above at 1. We take this into account by first defining the "true" underlying reciprocity propensity index

$$R_{ia}^* = \mathbf{x}_i^{\mathbf{r}\prime} \beta^{\mathbf{r}} + \lambda \left(a \right) + \varepsilon_i^{\mathbf{r}} \tag{9}$$

where $\mathbf{x}_i^{\mathbf{r}}$ $(K \times 1)$ is a vector of background characteristics of the receiver including the receiver's age, income, level of education and his beliefs about the sender's action.

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The associated vector of unknown population parameters is $\beta^{\mathbf{r}}$ ($K \times 1$). Since all observed characteristics $x_i^{\mathbf{r}}$ are invariant across all a, any fixed effect type of estimation procedure would prevent us from estimating $\beta^{\mathbf{r}}$, our focus parameters. We must then assume that any individual specific effect constant across all alternatives is captured by the covariates $x_i^{\mathbf{r}}$. The parameter $\lambda(a)$ is an a-sender specific constant term, allowing for shifts in levels between categories. Monotone reciprocity would imply the chain of estimates $\widehat{\lambda}(0) \leq \widehat{\lambda}(1) \leq ... \leq \widehat{\lambda}(10)$. One could estimate the shift parameters by adding 10 fixed effects to the model. In the empirical application, we opt for a more parsimonious specification of the threshold model whereby $\lambda(a) = \gamma_1 a + \gamma_2 a^2$ which reduces the amount of parameters to estimate from 10 to 2 (γ_1, γ_2).[13] Given the continuous nature of the density of the unobservable latent variable and the observed mixture of discrete and continuous observations, the observation rule is given by

$$R_{ia} = \max\left\{0, \mathbf{x}_i^{\mathbf{r}\prime}\beta^{\mathbf{r}} + \lambda(a) + \varepsilon_i^{\mathbf{r}}\right\} \tag{10}$$

In this paper, we consider two ways to recover estimates of $\beta^{\mathbf{r}}$ and $[\gamma_1, \gamma_2]$. If $\varepsilon_i^{\mathbf{r}}$ is homoscedastic and normally distributed, consistent estimates of $\Xi = \{\beta^{\mathbf{r}}, \gamma_1, \gamma_2, \sigma^2\}$ can be obtained by maximizing the Tobit (1958) likelihood function. Discussion of the properties of this estimator can be found in Maddala (1983). However, the Tobit model is generally quite sensitive to the specification of the error distribution (Goldberger, 1983, Arabmazar and Schmidt, 1982), likelihood-based estimators are in general inconsistent when the assumed parametric form of the likelihood function is incorrect. Furthermore, heteroscedasticity of the error term can also cause inconsistency of the parameter estimates even when the shape of the error density is correctly specified, as Hurd (1979) as shown. Our second estimator of (10) is the Symmetrically Trimmed Least Squares estimator (STLS) of Powell (1986). Contrary to Tobit, the STLS estimator does not require normality and is robust to (bounded) heteroscedasticity of unknown form in $\varepsilon_i^{\mathbf{r}}$, hence it is semiparametric. Moreover, it does not require that moments of $\varepsilon_i^{\mathbf{r}}$ but simply that $\varepsilon_i^{\mathbf{r}}$ is symmetrically distributed. Under the null hypothesis of normality and homoscedasticity of the error term, both the Tobit and STLS estimators consistently estimate the conditional mean of the latent process R_{ia}^* al-

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[13]We have estimated both the extended and parsimonious parametrization of the shift parameters and found that both sets of results revealed the same monotonic paths. Results of the extended parametrization are available from the authors upon request.

though STLS will be relatively inefficient. Under the alternative that the error term is non-normal (but symmetric) and heteroscedastic, Tobit is inconsistent while STLS remains a consistent estimator of the model parameters. This will allow us to test the Tobit specification against non-normality and heteroscedasticity using a Hausman (1978) specification type test.

3 Empirical results

3.1 Trust

Figure 4 shows the distribution of revealed experimental responses for senders. The two distinctive features of this distribution are 1) the majority of subjects send positive amounts, contradicting self-interested behavior, 2) the distribution is heavily skewed to the left, with a mode at 5, the equal split category. The distributions of laboratory trust responses usually share both these characteristics, which implies at first glance that unconditional trust responses of our random sample of the Dutch population do not differ much from the unconditional distribution from samples drawn from student populations.

53% of our senders answered that most people can be trusted to the WVS trust question. To investigate whether senders who gave progressively more also tended to answer more positively to the WVS trust question, we plotted in each figure 4 the share of positive responses to the WVS trust question for each of the 11 categories sent. For individuals who have sent 0 or 1 unit, more than three quarters of them have reported negatively to the WVS trust question. This share progressively decreases from 2 units sent to 4 units sent. In category 5, i.e., sending half of the own endowment, senders are divided in their answers to the WVS trust question. This may be explained by being indifferent between giving a little or nothing and a lot or everything. In this case this category represents neutral actions as giving more is interpreted as signaling generosity while giving less is interpreted as making an unfair offer. This form of indecision could also explain the reported mix in answers. Finally, we find that 67% of those individuals who sent amounts above this indifference point have answered positively to the WVS trust question. These results combines indicate that there

seems to exist an unconditional relationship between stated and experimental trust.

We now turn to the regression results which are presented in table 3. Due to little observations we join categories 6 to 9 and estimate an ordered probit model with eight categories (0=0 CP sent, 1=50 CP sent, 2=100 CP sent, 3=150 CP sent, 4=200 CP sent, 5=250 CP sent, 6=300, 350, 400, 450 CP sent, 7=500 CP sent). The table presents results from the ordered and binary probit models, each with three specifications. We include a vector of standard background characteristics such as age, income, education and religion in all six specifications.

The first 3 columns of the table are the ordered probit estimates of the experimental response y_i^E. The first specification includes senders' expected amount returned by their responders (STHINK) and senders' estimate of the average amount that others will send (SMEANS) and the senders answer to the WVS trust question. The second specification omits the answer to the WVS trust question while specification 3 omits WVS and senders' beliefs. The last two columns are the probit regressions of the WVS answers. Specification 4 contains the standard background characteristics and specification 5 is the probit model with misclassification due to the experimental response.

In analyzing our results, we will frequently compare them with results found in Allesina and La Ferrara (2002), Glaeser et al. (2002), Putnam (2000) and La Porta et al. (1997). It is important to note that both studies rely on organization membership of subjects as a proxy for social capital. In fact, Glaeser et al. motivate their approach precisely by referring to the Glaeser et al. (2000) finding that WVS trust answers were flawed as they do not correlate with experimental trust. Both experimental trust and organizational membership are likely to capture different concepts. Nevertheless, as trust and organizational membership are both believed to be important components of social capital, a comparison of our results to these studies will shed light on the robustness of the findings to the choice of proxy for social capital.

Before comparing results using experimental and survey trust measures, we test whether having senders answer the WVS trust question after having played would of affected their trust behavior. Specification 5 presents regression results where senders

are allowed to misreport their true answer to the WVS trust question and the size and magnitude of the misclassification. Both θ_1^{10} and θ_1^{01} associated with senders experimental decision y_i^E are not significantly different from zero which indicate that senders experimental decision did not lead them to systematically misreport their true answer to the WVS trust question. There could still be random misclassification. The log-likelihood ratio test of Andrews (2001) based on specifications 4 and 5 has a value of 5.38 with 10% simulated critical value of 7.02, again not rejecting the null hypothesis of no-misclassification. These results seem to show that senders' survey trust answers were not systematically affected by the experiment which preceded. Moreover, they also seem to show the absence of any form of misclassification, be it random or related to the preceding experiment.

Gender, individuals retired from the labor force or not working and all do not correlate with either experimental or WVS trust responses. This contrasts with the earlier findings of Glaeser et al. (2002) who find that women are less involved in organizations while income has a positive effect on organizational membership.

The age effect captured is robust and of similar magnitude across both the ordered and binary probit specifications. Both age parameters are significant and of the expected sign reconfirming the concave life-cycle pattern usually found in the social capital literature (Putnam 2000, Glaeser et al. 2002).

The education profile differs remarkably between the experimental and the stated trust responses. Individuals with secondary and technical training are more likely to exhibit laboratory trust than subjects with either low education levels (the omitted category) and subjects with high levels of education. These findings tend towards an inverted U shape similar to that of age. Comparisons of specification 1 with specification 2 and 3 show that this relationship is robust. On the other hand, however, this education profile is not at all present in the survey trust probit estimates. It is important to note that Glaeser et al. (2002) also find that organizational membership increases with education but does not find presence of non-linearities.

The impact of religion on trust remains a controversial issue in economics. La Porta et al. (1997) find that country specific religion indicators negatively affect trust

in a panel of countries. They rationalize this finding in terms of social norms and hierarchies that these religions impose. On the other hand, Allesina and La Ferrara (2002) find that the religion does not explain micro level answers to the WVS trust question. One reason for this apparent conflict is aggregation. Our regression results show that the effect of religion on trust is very much driven by the measure used. In the case of experimental data, we find no evidence of any systematic religious differences; this result holds across the different specifications. However, religious effects come out very significant in the WVS probit regressions. We find that catholics and protestants are less likely to report trusting others than individuals reporting to have other not having any religious beliefs.[14] This finding is in line with those reported by La Porta et al. (1997).

Participants in the role of senders were asked to state their beliefs about the response they expect to receive (STHINK) and their evaluations of what they think the average sender will send (SMEANS). Both variables have positive effects on trust and are highly significant. Apart from being statistically significant, these two variables alone improve the predictive fit of the model substantially, with a log-likelihood ratio statistic of 232.12 and a 5% critical value of 5.99 for a test of the null of no joint significance. These results would imply that senders who expected to receive more also sent more and those who thought other persons in the same role were going to send more to the responder increased the own amount sent. The latter result can be interpreted as a social norm while the former result captures expectations of the subjects.

The effect of senders lifetime experience with trust (TRUSTEXP) (see section 1 above) was recorded on a seven point scale. Somewhat like religion, reported experience with trust does not predict well subjects experimental trust responses but correlated remarkably well with the WVS trust answers, with a positive and significant effect.

Finally, we discuss the robustness of the Glaeser et al. (2000) finding that answers to the WVS trust questions do not correlate with experimental trust. The first two columns of table 3 show the regression results where the dummy variable WVS is added as a regressor. We find this variable to be significant at the 5% level using both Wald

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[14]Other religious beliefs account with 11% rather less for this categogy.

and LR test procedures. On one hand, this result is not that surprising as we have seen in figure 4 that there was some form of parallelism between both measures, as those who have sent less in the experiment were relatively more likely to have answered negatively to the WVS. The notable differences between the Glaeser et al. study and the present one are that we asked the WVS-question after the experiment whereas Glaeser et al. weeks before. As we showed above, senders experimental decisions do not seem to have affected their truthful revelation of their survey trust answers. More importantly, we showed in section 2.2 that this correlation is difficult to interpret, as it implies that the unobservable factors explaining survey trust correlate with experimental trust, a correlation on which not much can be said.

The preceeding analysis has compared parameter estimates based on two different trust measures. We have found that the effect of religion and education may provide different information depending on the trust measure used. However, the similar age profiles and the similar insignificance of some other covariates imply that both trust measures may, for some covariates, provide the same information. In section 2.2, we introduced a new minimum distance test which tests for joint discrepancies between both measures. For our data, the test has a value of 6.78, with a 5% chi-square critical value of 22.36, clearly not rejecting the null hypothesis that the systematic part of the latent trust propensity is the same using both measures. This test result implies that the discrepancies between education and religion effects are not sufficient to overturn the similar age profiles and insignificant effects. This insignificance result could cause the possible low power of the test to detect some alternatives in a small sample. This issue is still a matter of investigation.

Before turning to the analysis of the responders choices, we briefly summarize the main points of this section. We made an effort to compare our findings to the existing social capital literature. This was primarily done to investigate robustness of the results to the proxy measure of social capital used. Our results show that experimental trust and organizational membership are likely to measure different behavioral characteristics of subjects. Nevertheless, as trust and organizational membership are both believed to be important components of social capital, our results suggest that estimation of gender, income and life cycle profiles will strongly be affected by which measure of

social capital used. This section also showed that contrary to Glaeser et al. (2000), the WVS trust answers seemed to be correlated rather well with our experimental measure of trust. Finally, we found that the presence of religion and education effects depend on whether or not we use experimental (revealed) or WVS (stated) trust measures but that those differences do not holt a jointly. Furthermore, subjects' beliefs are important determinants of their trust actions while lifetime trust experience is not.

3.2   Reciprocity

Figure 5 shows a boxplot of the average amounts returned for every possible unit the sender can choose to send. The two important elements of this figure are that amounts returned increase with the amounts sent and that the variability in the amounts returned increases with the amount sent. Figure 6 shows a boxplot of the normalized returns, i.e., the percentage returned out of the total possible amount of R sent back to S. The general pattern remains the same, as this percentage monotonically increases with the amounts received.

We now turn to the econometric results of the responders model. Table 4 shows the regression results of the Tobit and the STLS estimators for two specifications of the reciprocity propensity. In the first specification, reciprocity is modelled as a function of several background characteristics of the responder and their beliefs (RTHINK) about senders' possible actions. The second specification allows for the possibility that subjects' beliefs RTHINK, stated trust WVS and reported trust experience TRUST-EXP may vary with the age of the responders by including interaction terms. Both specifications were estimated using the parametric Tobit model and the semiparametric STLS model. In both cases, the Hausman test statistic does not reject the null hypothesis of normality and homoscedasticity of the error terms[15] on which the Tobit model rests. To compare the specification with and without interaction terms, we computed a standard log-likelihood ratio test. The test value of 19.9 and a 1% critical value of 11.34 clearly rejects the parsimonious specification in favor of the specification with interaction terms. Given the relative efficiency of Tobit over STLS under the null

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[15]For the two specification presented in table 4, the Hausman test statistic is respectively 3.107 and with bootstrap 5% critical values of 39.43 and based on 250 replications.

hypothesis and our specification tests, our analysis will focus on the results from the Tobit model.

The parameters of γ_1 and γ_2 correspond to the vector of units sent to the receiver and its square. They are indicative of the path of the shift parameters $\lambda(a)$. As could be seen from the raw data in figure 5, the estimates show that $\lambda(a)$ are increasing and progressively diminishing over a, a clear indication of monotonically concave reciprocity as responders give systematically more for each unit received. This is also reflected in the Tobit estimates. We find that the mean conditional reciprocity is increasing and concave in the amounts received.

The life cycle evolution of reciprocity is captured by the parameters of RETIRED, AGE, AGESQ and the three interaction terms. The change in the conditional mean return propensity which follows from a change in age is given by

$$\frac{\partial R_{ia}^*}{\partial \text{AGE}_i} = \underset{(0.0014)}{-0.0014} + \underset{(0.000017)}{0.000032}\,\text{AGE}_i - \underset{(0.0005)}{0.0014}\,\text{WVS}_i - \underset{(0.0000)}{0.0003}\,\text{RTHINK}_i \quad (11)$$

This indicates that participants tend to reciprocate less as they get older but at a decreasing rate. Additionally, those who report trusting others and who believe that they will receive more tend to return on average less as they get older. It is important to note, that this result only reports the average response. In a quantile regression analysis of the response behavior, Bellemare and Kröger (2003b) (Bellemare and Kröger 2003b) show that individuals returning the most increase their share returned as they get older.

These results differ remarkably from the life cycle patterns observed in the trust responses where trust was shown to be strictly decreasing and concave with age. The education pattern also seems to reflect an opposite picture than for trust. We find that educated subjects tended to return significantly less than non-educated subjects with no significant differences between secondary, training or university level education.

Contrary to the trust results documented earlier were gender did not appear to play an important role, we find that men return on average significantly less than women. There are nevertheless some areas where trust and reciprocity have concomitant effects. We do not find any relationship between income, work status, retirement status and

lifetime trust experience to correlate with reciprocity. Similarly to experimental trust results, religion effects seem to be absent. RTHINK measures the beliefs of responders what they expect their matched sender will eventually send to them. We find that those responders who believed they would receive more increased the proportion they are willing to return. Finally, we find that those who reported trusting more others were also more likely to return more to the senders. This fact supports the findings in Glaeser et al. (2000) were the same relationship was shown to exist.

The main conclusions of this section are the following. First, age and education profiles of reciprocity were shown to be at odds with those of the trust propensity. We find that higher educated subjects trust more but return less and that trust decreases with age while reciprocity does exactly the opposite. Second, we have found that men tend to reciprocate significantly less than women. Despite these differences, there are nonetheless similarities between both trust and reciprocity behavior. First, life time experience with trust, religion, income and work and retirement status do not correlate with reciprocity while subjects' beliefs are shown to be significant determinants of reciprocity. Furthermore, stated trust answers to the WVS question correlate with trustworthiness. This confirms previous findings in this literature.

3.3   Participation

Once contacted by CentERdata concerning our experiment panel members could decide whether or not to participate. The reasons for participating in laboratory experiments, especially the impact of financial rewards, have been analyzed in several meta studies (Smith and Walker, 1993?, Hertwig and Ortmann, 2001, Camerer and Hogarth, 1999). But also the impact of voluntary participation (Eckel and Grossmann, 2000) has been of interest for investigation. There has always been a fear that experimental subject pools are non-random samples of the population of interest. Amongst the most cited explanations of this non-randomness are that participants might be motivated intrinsically (Camerer and Hogarth), e.g., have above average taste for gambling and risk or higher cognitive abilities . The crucial question is whether or not non-randomness of participants based on these traits feeds itself into non-randomness in the experimental

outcomes.[16]

Usually investigation of participation bias is limited by two data-shortcomings. First, some of the traits affecting participation are mostly unobservable to the experimenter, often because they are difficultly measurable. Secondly, experimenters generally do not have any information on non-participants, preventing any possible control for their effects without further combinations of additional information and assumptions (see Manski, 1999). Because of these data limitations, tests of participation bias in laboratory-type experiments are scarce.

CentERdata uses a common method and selects CP members after certain criteria to ensure the representativeness of the Dutch population of the CP. The approached households are asked to participate in the panel (see section 1). Camerer and Hogarth mention that beside monetary incentives also intrinsic motivations play an important role to guide the attraction to participate in laboratory experiments, which is certainly the case for other scientific investigations like survey investigations. The incentives for Dutch persons to participate in the CP are not monetary. They receive a reimbursement for their telephone allowances while connected to CentERdata to answer the questionnaires. In this light, we probably face an already intrinsically highly motivated sample and thereby we might exhibit even a larger participation rates when asking to join and interact in an experiment with financial rewards than one would expect in the real field. Nevertheless, 7% of the panel members approached decided not to participate. In our experiment, non-participants are observable CP members with observable background characteristics. This allows us to test whether or not participants behaved in a non-random manner.

We test for participation bias in the framework developed by Heckman (1978)[17]. Let $d_i \in \{0, 1\}$ be an indicator of participation in the experiment and let the an individuals

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[16]The statistical dependance between the experimental outcome and the participation decision implies that $\Pr(Y|\mathbf{x}, d = 1) \neq \Pr(Y|\mathbf{x})$ where Y is the random outcome of an experiment and $\mathbf{x}$ are some conditioning characteristics. This inequality says that the probability distribution of observed outcomes of participants will differ from that of randomly assigned participants to the experiment, given $\mathbf{x}$. This result holds generally for the Ordered Probit model with the experimental measure $\left(Y = y_i^E, \mathbf{x} = \mathbf{x}_i^{\mathbf{t}}\right)$, the Binary Probit on the survey trust measure $\left(Y = y_i^{WVS}, \mathbf{x} = \mathbf{x}_i^{\mathbf{t}}\right)$ and the Tobit model of the responders decision $(Y = R_{ia}, \mathbf{x} = \mathbf{x}_i^{\mathbf{r}})$.

[17]Heckman (1990) gives an overview of more recent developments in selection models

unobserved latent propensity to participate.

$$d_i^* = \mathbf{x}_i^j \delta + \theta \text{RATIO}_i + \varepsilon_i^d \qquad \text{for } j = \mathbf{r}, \mathbf{t}$$

where $\mathbf{x}_i^j$ is the conditioning vector entering the trust and reciprocity models, ε_i^d is an unobservable $N(0,1)$ component driving participation and (δ, θ) are unknown population constants. A general feature of these models is the requirement of a valid exclusion restriction for nonparametric identification of the selection bias. In practical terms, we need a variable which affects participation but not directly the experimental and survey responses. We computed the variable RATIO, which is the number of questionnaires completed to the total number of questionnaires submitted to a CP member in the three months which preceded our experiment. The dependance between the experimental outcomes and the participation decisions is captured by the amount of correlation between ε_i^d and the unobservable components $\varepsilon_i^{\mathbf{t}}$ in trust propensity (for $\mathbf{t} = E$ and WVS, see equation (1)) and $\varepsilon_i^{\mathbf{r}}$ in the reciprocity propensity (see equation (9)). For the trust responses, we estimated separately both the ordered probit and binary probit models jointly with the selection equation. We estimated the Tobit model with selection for the reciprocity decisions.

The presence of participation bias can be determined by testing the statistical significance of all three correlation coefficients. In all three cases we did not find any correlation coefficients to be significantly different from zero, which implies the absence of selection bias in our data. Most of parameters entering the systematic part of the participation propensity were insignificant. One notable exception was income which came out with a positive and significant effect on participation. Absent the income effect, we conclude that participation in our experiment seems to be mostly driven by unobserved heterogeneity which does not correlate with the experimental decisions.

4 Conclusions

Up till now, the microeconometric analysis of trust has relied on one of two complementary methodologies. Survey methods on one hand collect responses of heterogeneous samples, at the expense of having to rely of hypothetical and self-reported measures.

On the other hand, laboratory experiments offer the possibility to collect data on actions deemed to reveal subjects preferences at the expense of collecting this data for a very special subset of the population of interest. This paper presented results from one of the first computerized trust experiments conducted on a random sample of the Dutch population. The main advantage of our approach is that we are able to combine the strengths of laboratory experiments and survey data collection methods without their weaknesses.

The paper makes two important contributions. First, we contrast the inferences which can be made using either stated or revealed trust measures by comparing responses of a representative subject pool which played a modified version of the BDM trust game and, additionally, answered the WVS trust question. The differences are stark. We find that education has an inverted-U shape relation with experimental trust while it does not correlate at all with answers to the WVS trust question. In contrast, we find that religion correlates well with answers to the WVS trust question but not with experimental trust. Although these education and religious effects differ depending on the measure used, this is not so for other variables. To paint a complete picture of the different inferences which can be made using either trust measure, we propose a new test statistic aimed at comparing the systematic part of each model. Even though there are clear differences between the significance of individual parameters across both survey measures, the differences do not hold jointly. We also find that answers to the survey trust question correlate well with experimental measures of trust, controlling for several observable characteristics, which contradicts the findings of Glaeser et al. (2000).

The second important contributions is that we shed a first look into the determinants of societal trustworthiness by analyzing responses of trustees in the BDM game. We find that the relation between trustworthiness and education is U-shaped while it is inverted-U shaped with respect to age. These relations are complete opposites of those we find for experimental trust responses. The age pattern of reciprocity is puzzling as trustworthiness, like trust, are both considered to be important components of social capital. The common belief in the social capital literature is that inverted-U shape age relationships are common features of all social capital components. This belief has

been reinforced by the existing evidence on organizational membership (Glaeser et al. 2002) and trust. To our knowledge, these are the first life-cycle and education profiles results on reciprocity to trust.

This paper makes four additional contributions. First, we investigate the importance of beliefs in determining trust and reciprocity responses. This is relevant as most trust experiments have ignored subjects beliefs in determining experimental outcomes. Our results clearly show that beliefs play a major role and that including beliefs improves dramatically the fit of the statistical model. The effects of background characteristics are shown to be robust to the inclusion of beliefs, which implies that beliefs are orthogonal to most population characteristics.

Secondly, we address the issue of the behavioral representativeness of student subject pools in trust experiments. The unconditional distributions of trust and trustworthiness in our experiment follow those found in the laboratory. Nevertheless, the age and education effects reported in this paper do suggest that university students, characterized by their below average age and above average level of education, behave differently in the experiment.

The paper also analyzes subject participation in an experiment and, more importantly, test whether or not participants in an experiment behave systematically differently than randomly selected subjects. Our results show that participation bias is not present in the data. This contrasts to some existing results and common beliefs which emphasized that risk-seeking gamblers and high cognitive individuals compose the bulk of participants subject pools.

Finally, we show that inferences on the effects of subjects' background characteristics on social capital may depend on the measure of social capital used. We showed that previous studies using organizational membership as a proxy for social capital find different age and gender patterns. An explanation of this fact is that trust and organizational membership are two distinct yet important contributors to social capital. Because of their distinctiveness, inferences on the effect of background characteristics are likely to differ depending on the social capital proxy used.

The results presented in this paper leave several questions unanswered. First, much more needs to be done to reconcile the age and education patterns which distinguish

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trust and reciprocity propensities. Secondly, the representativeness of student subject pools for other types of experiments is still open for question. Whether or not the effects of subject background characteristics presented in this paper can be generalized to other experimental settings remains a topic of future research.

A   Minimum Distance Test

We present here a minimum distance test for observable characteristics. Let $\xrightarrow{U}$ denote uniform convergence and $\xrightarrow{d}$ convergence in distribution. Further, E_N denotes expectation taken with respect to the empirical distribution and $\mathbf{E}$ expectation take with respect to the population distribution. We have an unknown population parameter vector θ_0 and let $\widehat{\theta}_1$ and $\widehat{\theta}_2$ be two $\sqrt{N}-$consistent estimators of θ_0 that satisfy

$$
\begin{aligned}
\widehat{\theta}_1 &= \arg\max_{\theta} \mathbf{E}_N\left[L^1(\theta)\right] \\
\widehat{\theta}_2 &= \arg\max_{\theta} \mathbf{E}_N\left[L^2(\theta)\right]
\end{aligned}
$$

where $L^1_\theta(\theta)$ and $L^2_\theta(\theta)$ are the scores of the concentrated log-likelihood functions of the probit and ordered probit models and $L^1_{\theta\theta'}(\theta)$ and $L^2_{\theta\theta'}(\theta)$ their associated matrices of second derivatives. Then, if follows that

$$
\begin{aligned}
\sqrt{N}\left(\widehat{\theta}_1 - \theta_0\right) &= -\left\{\mathbf{E}_N\left[L^1_{\theta\theta'}(\overline{\theta})\right]\right\}^{-1}\sqrt{N}\mathbf{E}_N\left[L^1_\theta(\theta_0)\right] + o_p(1) \\
\sqrt{N}\left(\widehat{\theta}_2 - \theta_0\right) &= -\left\{\mathbf{E}_N\left[L^2_{\theta\theta'}(\overline{\theta})\right]\right\}^{-1}\sqrt{N}\mathbf{E}_N\left[L^2_\theta(\theta_0)\right] + o_p(1)
\end{aligned}
$$

where $\overline{\theta} = \alpha_k\widehat{\theta}_1 + (1-\alpha_k)\theta_0$.

$$
\begin{aligned}
\sqrt{N}\begin{pmatrix} \widehat{\theta}_1 - \theta_0 \\ \widehat{\theta}_2 - \theta_0 \end{pmatrix} = -&\left\{\begin{pmatrix} \left\{\mathbf{E}_N\left[L^1_{\theta\theta'}(\overline{\theta})\right]\right\}^{-1} & \mathbf{0} \\ \mathbf{0} & \left\{\mathbf{E}_N\left[L^1_{\theta\theta'}(\overline{\theta})\right]\right\}^{-1} \end{pmatrix}\right. \\
&\left.\sqrt{N}\begin{pmatrix} \mathbf{E}_N\left[L^1_\theta(\theta_0)\right] \\ \mathbf{E}_N\left[L^2_\theta(\theta_0)\right] \end{pmatrix}\right\} + o_p(1)
\end{aligned} \tag{12}
$$

Under appropriate regularity conditions (see Newey and McFadden (1994)),

$$
\begin{aligned}
\mathbf{E}_N\left[L^1_{\theta\theta'}(\overline{\theta})\right] &\xrightarrow{U} \mathbf{E}\left[L^1_{\theta\theta'}(\theta_0)\right] \equiv \mathbf{H}_1 \\
\mathbf{E}_N\left[L^2_{\theta\theta'}(\overline{\theta})\right] &\xrightarrow{U} \mathbf{E}\left[L^2_{\theta\theta'}(\theta_0)\right] \equiv \mathbf{H}_2
\end{aligned}
$$

$$
\begin{aligned}
\mathbf{E}\left[L^1_\theta(\theta_0)\right] &= \mathbf{0} \\
\mathbf{E}\left[L^2_\theta(\theta_0)\right] &= \mathbf{0}
\end{aligned}
$$

which implies that (12) converges in distribution to

$$
\mathcal{N}\left(\begin{pmatrix} \mathbf{0} \\ \mathbf{0} \end{pmatrix}; \begin{pmatrix} \mathbf{H}_1^{-1} & \mathbf{0} \\ \mathbf{0} & \mathbf{H}_2^{-1} \end{pmatrix}\mathbf{E}\begin{pmatrix} \frac{\partial}{\partial\theta}L_1(\theta_0)\frac{\partial}{\partial\theta'}L_1(\theta_0) & \frac{\partial}{\partial\theta}L_1(\theta_0)\frac{\partial}{\partial\theta'}L_2(\theta_0) \\ \frac{\partial}{\partial\theta}L_2(\theta_0)\frac{\partial}{\partial\theta'}L_1(\theta_0) & \frac{\partial}{\partial\theta}L_2(\theta_0)\frac{\partial}{\partial\theta'}L_2(\theta_0) \end{pmatrix}\begin{pmatrix} \mathbf{H}_1^{-1} & \mathbf{0} \\ \mathbf{0} & \mathbf{H}_2^{-1} \end{pmatrix}\right)
$$

$$
= \mathcal{N}\left(\begin{pmatrix} \mathbf{0} \\ \mathbf{0} \end{pmatrix}; \begin{pmatrix} \mathbf{V}_{11} & \mathbf{V}_{12} \\ \mathbf{V}_{21} & \mathbf{V}_{22} \end{pmatrix}\right)
$$

It follows that

$$
\sqrt{N}\left(\widehat{\theta}_1 - \widehat{\theta}_2\right) \xrightarrow{d} N\left(\mathbf{0}; \mathbf{V}_{11} + \mathbf{V}_{22} - \mathbf{V}_{12} - \mathbf{V}_{21}\right)
$$

When estimator 2 is relatively efficient compared to estimator 1, $\mathbf{V}_{12} = \mathbf{V}_{21} = \mathbf{V}_{22}$ by the orthogonality projection lemma (see Ruud 2000). In the present case, none of the

estimators is relatively efficient. Hence, we need to compute consistent estimates of $\mathbf{V}_{11}$, $\mathbf{V}_{22}$ and $\mathbf{V}_{12}$. Consistent estimators of these elements follow by replacing population expectations with sample analogues and θ_0 by either $\widehat{\theta}_1$ or $\widehat{\theta}_2$, both equivalent under H_0. The Generalized Hausman test is given by

$$\mathcal{GHT} = N \left(\widehat{\theta}_1 - \widehat{\theta}_2 \right)' \left[\mathbf{V}_{11} + \mathbf{V}_{22} - \mathbf{V}_{12} - \mathbf{V}_{21} \right]^{-1} \left(\widehat{\theta}_1 - \widehat{\theta}_2 \right) \tag{13}$$

The limiting distribution of GHT is $\chi^2(K)$. In practice, all four unknown components of the covariance matrix can be replaced by consistent sample estimates $\widehat{\mathbf{V}}_{ij}$ and the test statistic is computed as

$$\mathcal{GHT} = N \left(\widehat{\theta}_1 - \widehat{\theta}_2 \right)' \left[\widehat{\mathbf{V}}_{11} + \widehat{\mathbf{V}}_{22} - \widehat{\mathbf{V}}_{12} - \widehat{\mathbf{V}}_{21} \right]^{-1} \left(\widehat{\theta}_1 - \widehat{\theta}_2 \right).$$

B Instructions

Panel members viewed the following screens on their home TV or Computer. Figures 1 and 2 present sample screens which show the decisions screens the senders and responders faced. In the following we present a translated text of the original Dutch screens. All original screens are available at the authors upon request. Participants could go forwards and backwards between screens during the experiment.

The first 3 screens of the experiment are the same for both, sender and responder. Italic notes in the translation are comments by the authors.

- *First screen:*
  This experiment is a research project of researchers from Humboldt University Berlin and Katholic University of Brabant.[18]
  With this experiment you can make real money in terms of CentERpoints.[19] You receive from the researchers additional CentERpoints (beside the usual telephone allowance).

- *Second screen:*
  During this experiment you will be matched with another member of the panel. You will not know who this person is, both of you will stay anonymous. Both of you receive 500 CentERpoints. Then the experiments starts.
  One of you has the possibility to send a share of this away. The amount of points sent will be doubled and given to the other person. The other person has then the opportunity to send a share of the own total amount back. The amount which is sent back will not be doubled.
  How many points you finally earn depends therefore on your decision and the decision of the person you are matched with. You will be randomly assigned your role.

- *Third screen:*
  We now give you the chance to indicate whether you want to participate. If you decide not to participate, the experiment will end immediately. You will receive the usual telephone reimbursement. If you continue you will receive the 500 CentERpoints.
  Do you want to continue?
  ○   Yes
  ○   No

Subjects who chose to participate were then randomly assigned to their roles. Senders and receivers had to read decision screens tailored to their role.

Sender:

---

[18]Now: Tilburg University. The Katholic University of Brabant changed its name after the experiment.

[19]Members of the CentERpanel answer the questionnaires on the TV with help of a set-top-box which is via the telephone connected with the CentERdata. Participating members receive an allowance to cover their telephone costs. This allowance is paid in CentERpoints where 1 CentERpoint = 1 EuroCent (= 0.01 EURO).

- *Fourth screen :*
  You have been matched with another member of the panel. Like you, this person received 500 CentERpoints. You can send a share of your 500 CentERpoints. The panel member with whom you are matched with receives the doubled amount of what you sent. Then, this person has the opportunity to send a share of the own total amount back (without knowing who you are). The amount which this person sends back to you will not be doubled.
  How many points do you want to give?
  [The sender could send one out of 11 possible amounts. For the original screen see figure (1).]
  0 the other person receives additionally nothing and has therefore 500 and you remain with 500 points.
  ...
  500 the other person receives additionally 1000 and has therefore 1500 in total and you remain with 0 points.

- *Fifth screen: (depending on the decision taken at the fourth screen, here as example 200)*
  You decided to send 200 points.
  The panel member you are matched with receives therefore 400 additional CentERpoints.
  He or she has therefore in total 900 CentERpoints.
  You remain with 300 CentERpoints.
  How many points do you think the other panel member with whom you are matched with will send to you?
  [Participants had to type in a number. In this example in the range of [0,900].]

- *Sixth screen:*
  This experiment is done with some panel members. Half of them interact in the same position as you. They can send a share from their 500 CentERpoints which is doubled and received by a person of the other position.
  What do you think, how many points those panel members have sent?
  [The sender could indicate one out of 11 possible amounts from 0 to 500.]

Responder:

- *Fourth screen:*
  You have been matched with another member of the panel. This person received as you 500 CentERpoints. This person is asked to send you a share from their own 500 CentERpoints. You will receive the doubled amount of those points the other person has sent.
  For exapmle if the other person has sent 100 CentERpoints you receive 200 CentERpoints. Together with the 500 points you have in total 700 CentERpoints. From this amount you can return a share. The amount you send will not be doubled.

- *Fifth screen:*
  As we do not know now how many CentERpoints the other panel member with

whom you are matched with has sent we present all possible amounts this person could send to you. Besides this amount it is written what you would receive. Please indicate in the last column what amount you would return for each possible amount sent.

After the real decision of the other person is known the amount you indicated for this particular decision will be realized. The amount you will return will be deducted from your total amount.

[The responder had to indicate for each of the 11 possible amounts the sender could send what he would return. For the original screen see figure (2). The table was designed as follows:]

If the other sends:	I receive:	In total with the 500 CentERpoints:	In this case I return:
0	0	500	_____
. . .	. . .	. . .	. . .
500	1000	1500	_____

- *Sixth screen:*
  How many points do you think the panel member with whom you are matched with has sent to you?
  [The responder could indicate one out of 11 possible amounts from 0 to 500.]

After these screens the experiment was over. Nobody could go backwards and both senders and responders were asked the following post-experimental questions:

- *Seventh screen (Trust experience question):*
  The last two questions are about trust in general. This question is about your own trust experience.
  If you trust is your trust generally rewarded or exploited?
  Choose the number which is closest to your answer.
  always rewarded 1 2 3 4 5 6 7 always exploited.
  [Participants had to type in a number between 1 and 7.]

- *Eight screen (WVS trust question):*
  Generally speaking would you say that most people can be trusted or that you cannot be too careful in dealing with people?
  1.) Most people can be trusted.
  2.) You have to be very careful.
  3.) I do not know.

After having answered the post-experimental questions, participants were informed that they would receive the information about the decision the person they were matched with and the final results in two weeks. All potential participants had the possibility to express themselves in a comment box after the experiment or after the decision not to participate.

	S		R		Not played		
	Mean	StD	Mean	StD	Mean	StD	
GENDER	0.46	0.5	0.46	0.5	0.43	0.5	Dummy, 1 if Man
AGE	42.66	15.55	43.98	15.67	53.79	16.55	Age
GR_INCP	2580.38	6133.47	1916.28	3289.66	2356.90	3584.27	Gross Personal Montly Income (Guilders)
GR_INCH	4175.59	7526.39	3342.07	4313.76	4302.52	6593.33	Gross Household Montly Income (Guilders)
HSIZE	2.27	1.12	2.53	1.33	2.17	1.23	Household Size
NCHILD	0.55	0.92	0.75	1.16	0.4	1.06	Number of Children in household
PARTNER	0.72	0.45	0.78	0.42	0.76	0.43	Partner is living in the household
LOWEDUC	0.043	0.204	0.076	0.266	0.143	0.354	Dummy, 1 if Low education level
SECONDEG	0.388	0.488	0.390	0.489	0.381	0.492	Dummy, 1 if Secondary degree
TRAINDEG	0.449	0.449	0.448	0.499	0.357	0.485	Dummy, 1 if Training degree
UNIVDEG	0.119	0.325	0.085	0.279	0.119	0.329	Dummy, 1 if University degree
WORK	0.591	0.493	0.511	0.501	0.429	0.501	Dummy, 1 if Labor Work
RETIRED	0.130	0.337	0.148	0.356	0.262	0.445	Dummy, 1 if Retired
HHEAD	0.637	0.482	0.609	0.488	0.667	0.477	Dummy, 1 if Head of household
CATHOLIC	0.297	0.458	0.349	0.478	0.309	0.468	Dummy, 1 if Catholic religion
PROTEST	0.210	0.408	0.265	0.442	0.191	0.397	Dummy, 1 if Protestant religion
OTHERS	0.493	0.501	0.386	0.488	0.500	0.506	Dummy, 1 if Atheist or Other religion
WVS	1.5	0.56	1.6	0.58	-	-	Dummy, 1 if Yes to WVS trust question
TRUSTEXP	3.16	1.18	3.45	1.31	-	-	1-7, 1 if Always rewarded 7, Always disappointed
Nobs	276		223		42		

Table 2: Descriptive Statistics

Table 3: Sender Results.

| | Experimental response | | | | | Survey response (WVS) | | | |
| | (1) | | (2) | | (3) | | (4) | | (5) | |
	Coef.	Std.	Coef.	Std.	Coef.	Std.	Coef.	Std.	Coef.	Std.
CONSTANT	-1.828***	0.687	-2.018**	0.736	-0.314	0.681	-3.069***	0.838	-3.787	1.424
GENDER	0.217	0.160	0.182	0.133	0.229*	0.134	-0.186	0.181	-0.335	0.244
AGE	0.060**	0.031	0.067**	0.031	0.052*	0.029	0.067*	0.037	0.071	0.051
AGE_SQ/1000	-0.803**	0.349	-0.897**	0.367	-0.662**	0.335	-0.909**	0.435	-1.026*	0.589
RETIRED	-0.251	0.287	-0.193	0.374	0.271	0.293	0.528	0.411	0.707	0.535
SECONDEG	0.845***	0.318	0.908***	0.396	0.626*	0.308	0.518	0.422	0.504	0.578
TRAINDEG	0.761**	0.332	0.809**	0.404	0.589*	0.312	0.394	0.436	0.446	0.602
UNIVDEG	0.678	0.395	0.729	0.457	0.716*	0.361	0.522	0.499	0.337	0.678
WORK	-0.189	0.208	-0.219	0.193	-0.089	0.185	-0.290	0.243	-0.451	0.373
LN(GR_INCP)	0.019	0.039	0.024	0.026	0.007	0.028	0.034	0.038	0.044	0.053
NUMCHILD	-0.129	0.099	-0.118*	0.064	-0.043	0.066	0.105	0.102	0.121	0.146
CATHOLIC	0.149	0.171	0.101	0.170	0.002	0.165	-0.476**	0.211	-0.567*	0.290
PROTEST	0.042	0.208	-0.016	0.164	-0.109	0.149	-0.524**	0.227	-0.699**	0.355
TRUSTEXP	-0.032	0.082	0.045	0.059	0.090	0.065	0.665**	0.085	0.905***	0.205
STHINK	0.003***	0.000	0.003***	0.0001						
SMEANS	0.433***	0.044	0.423***	0.061						
WVS	0.362**	0.169								
θ_0^{10}									-3.467	1.621
θ_1^{10}									0.387	0.233
θ_0^{01}									-1.311	1.881
θ_1^{01}									-1.119	1.583
Log-Likelihood	-403.58		-406.42		-520.06		-150.8		-148.11	

Columns (1) to (3) refer to the ordered probit model, columns (4) and (5) to the binary probit model. The significance of parameters is based on robust standard errors and bootstrap empirical quantiles of the t-statistics (1000 repetitions).

Specification	(1)				(2)			
Model	Tobit		STLS		Tobit		STLS	
	Coef.	Std.	Coef.	Std.	Coef.	Std.	Coef.	Std.
CONSTANT	-0.042	0.032	-0.039	0.033	-0.076***	0.035	-0.042	0.036
γ_1	0.087***	0.004	0.079***	0.004	0.087***	0.004	0.079***	0.005
γ_2	-0.005***	0.0003	-0.004***	0.0004	-0.0045***	0.0003	-0.004***	0.0004
GENDER	-0.018***	0.007	-0.013**	0.007	-0.017***	0.0068	-0.014**	0.007
AGE	-0.002*	0.001	-0.001	0.001	-0.0014	0.0014	-0.0006	0.001
AGESQ/1000	0.033***	0.017	0.017	0.017	0.032*	0.017	0.009	0.018
RETIRED	-0.004	0.015	-0.002	0.017	0.0052	0.014	0.006	0.017
SECONDEG	-0.053***	0.012	-0.050***	0.016	-0.054***	0.012	-0.053***	0.015
TRAINDEG	-0.065***	0.012	-0.064***	0.016	-0.067***	0.012	-0.066***	0.015
UNIVDEG	-0.053***	0.015	-0.040***	0.018	-0.058***	0.015	-0.0469**	0.018
WORK	0.016*	0.009	0.012	0.008	0.019**	0.0089	0.0128	0.008
LN(GR_INCP)	0.0006	0.0011	0.000	0.001	0.0001	0.0011	-0.0005	0.001
CATHOLIC	-0.009	0.007	-0.014**	0.007	-0.0062	0.0073	-0.013	0.007
PROTEST	0.006	0.008	0.010	0.007	0.0051	0.0076	0.012	0.008
WVS	0.045***	0.007	0.042***	0.007	0.1035***	0.022	0.086***	0.020
TRUSTEXP	-0.003	0.003	-0.001	0.003	-0.017	0.009	-0.025**	0.009
RTHINK	0.013***	0.001	0.012***	0.001	0.0255***	0.0044	0.022***	0.0046
AGE×TRUSTEXP					0.0003	0.0002	0.0006**	0.0002
AGE×WVS					-0.0014***	0.0005	-0.0011**	0.0005
AGE×RTHINK					-0.0003***	0.0000	-0.0002**	0.0000
σ^2	0.144***	0.002			0.0207***	0.0006		
Log-Likelihood	936.52				946.47			
Hausman χ^2 test	3.107				2.928			

***: 1% Level. **: 5% Level. *: 10% Level

Table 4: Responder Results.

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ZAK, P. J., AND S. KNACK (2001): "Trust and Growth," *Economic Journal*, 111(4), 295–321.

U bent anoniem gekoppeld aan een ander panellid. Deze persoon krijgt net als u 500 CentERpunten. U kunt een gedeelte van uw 500 CentERpunten weggeven. Het panellid aan wie u gekoppeld bent ontvangt twee maal zoveel als u weggeeft. Daarna krijgt deze persoon de mogelijkheid om een gedeelte van zijn of haar punten terug te geven (zonder te weten wie u bent). Het aantal punten dat deze persoon terug geeft, wordt niet verdubbeld.

Hoeveel punten wilt u weggeven?

○   **0**   de ander krijgt dan niets extra en houdt dus   500   u houdt 500 punten over
○   **50**   de ander krijgt dan   100 extra, dus in totaal   600   u houdt 450 punten over
○ **100**   de ander krijgt dan   200 extra, dus in totaal   700   u houdt 400 punten over
○ **150**   de ander krijgt dan   300 extra, dus in totaal   800   u houdt 350 punten over
○ **200**   de ander krijgt dan   400 extra, dus in totaal   900   u houdt 300 punten over
○ **250**   de ander krijgt dan   500 extra, dus in totaal 1000   u houdt 250 punten over
○ **300**   de ander krijgt dan   600 extra, dus in totaal 1100   u houdt 200 punten over
○ **350**   de ander krijgt dan   700 extra, dus in totaal 1200   u houdt 150 punten over
○ **400**   de ander krijgt dan   800 extra, dus in totaal 1300   u houdt 100 punten over
○ **450**   de ander krijgt dan   900 extra, dus in totaal 1400   u houdt   50 punten over
○ **500**   de ander krijgt dan 1000 extra, dus in totaal 1500   u houdt   0 punten over

Kies "Verder" om verder te gaan of "Vorige" om terug te gaan.

Verder		Vorige

Figure 1: Computer screen shot of the decision screens faced by the senders.

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Omdat we nu nog niet weten hoeveel CentERpunten het panellid aan wie u gekoppeld bent weg geeft, leggen we u de 11 mogelijkheden voor die deze persoon had. Per regel staat steeds wat u kunt verdienen. Wilt u in de achterste kolom aangeven hoeveel u voor elke mogelijkheid terug zou geven?

Wanneer bekend is of en hoeveel punten de ander weggegeven heeft, wordt het dubbele bedrag bij uw 500 CentERpunten opgeteld. Het bedrag dat u daarnaast invult om terug te geven, wordt van uw saldo afgetrokken.

als de ander dit geeft:	dan ontvang ik:	samen met 500 punten is dit totaal:	Ik zou dan zoveel geven:
0	0	500	
50	100	600	
100	200	700	
150	300	800	
200	400	900	
250	500	1000	
300	600	1100	
350	700	1200	
400	800	1300	
450	900	1400	
500	1000	1500	

Kies "Verder" om verder te gaan of "Vorige" om terug te gaan.

Verder		Vorige

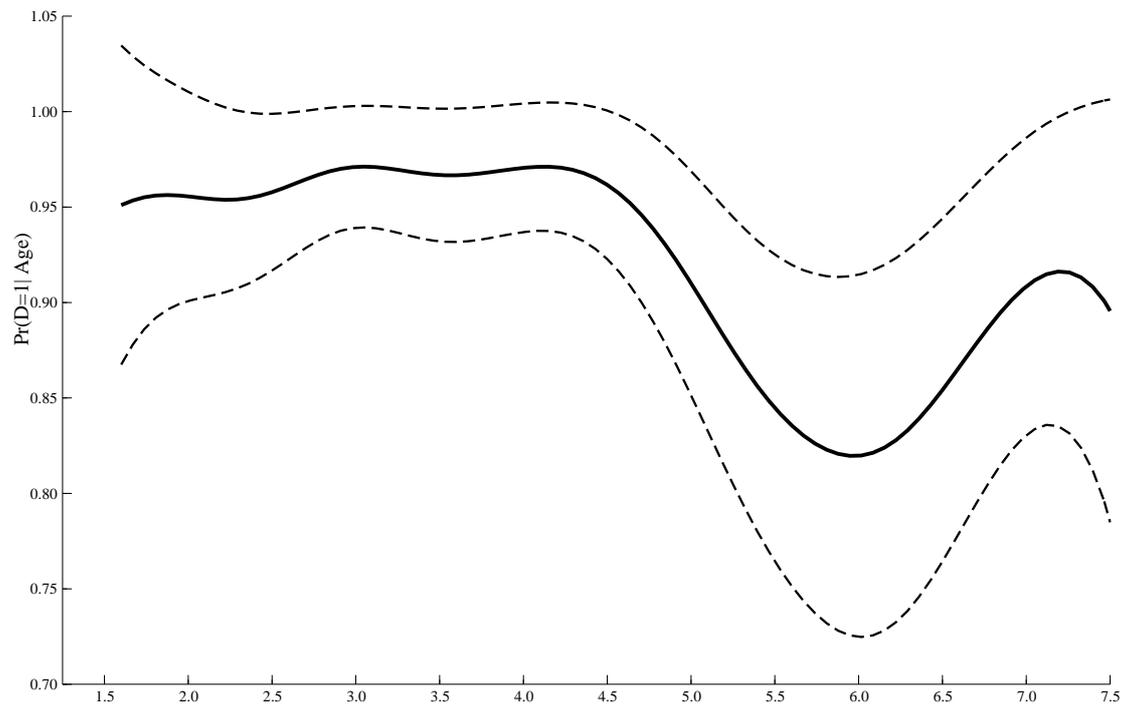Figure 2: Computer screen shot of the decision screens faced by the responders.

Figure 3: Nadaraya-Watson gaussian kernel regression of the participation decision on age. 95% uniform confidence bands are plotted. Bandwidth is chose by cross-validation
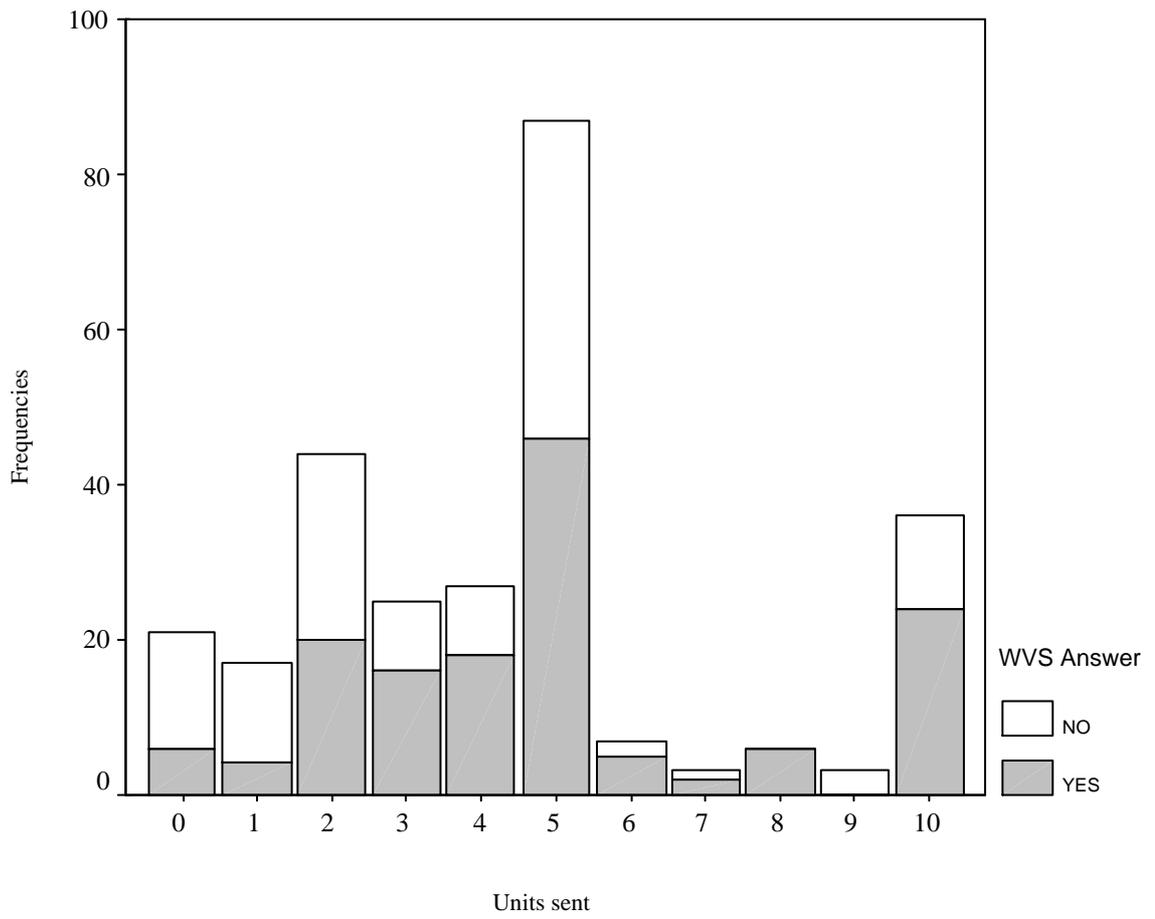
Figure 4: Distribution of experimental trust as a function of stated responses for senders.
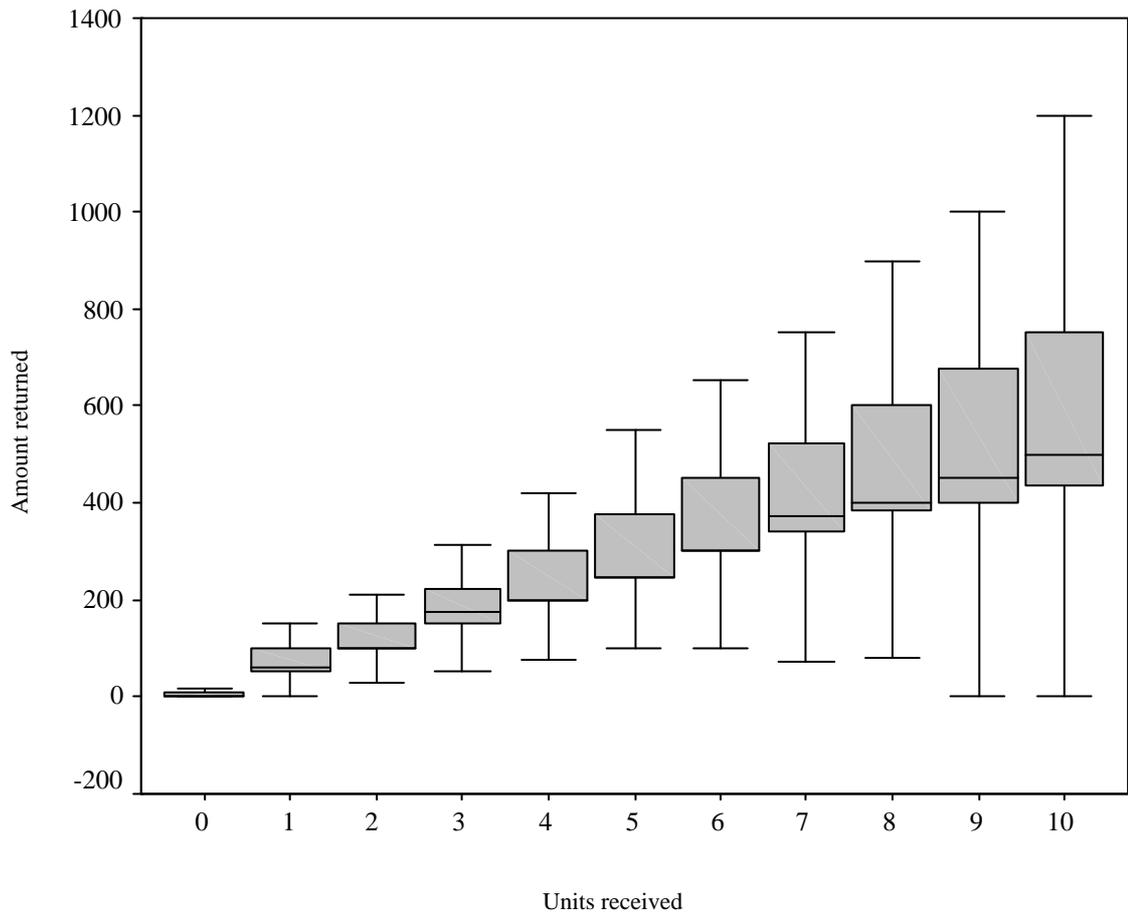
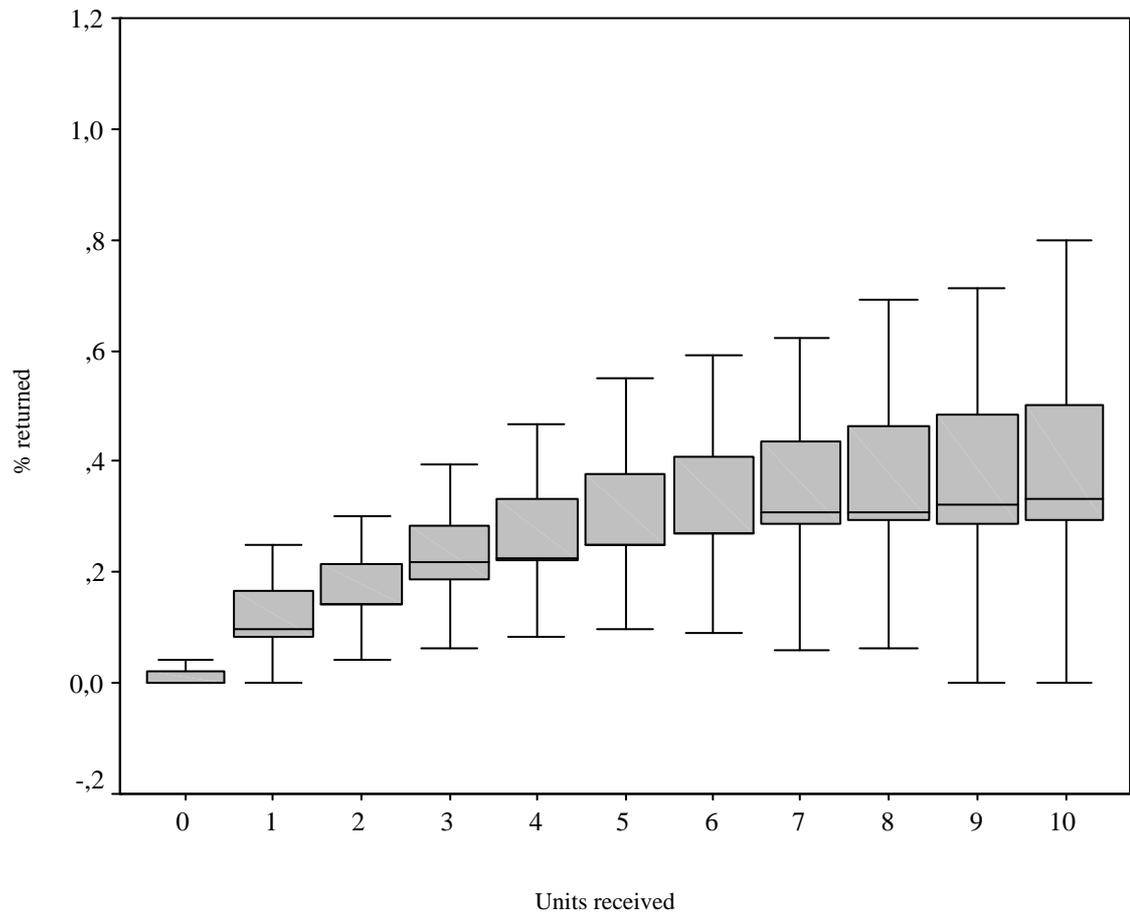Figure 5: Responder's distribution of gross amounts returned for each units received, strategy method.

Figure 6: Responder's distribution of relative amounts returned for each units received, strategy method