Available online at www.sciencedirect.com
ScienceDirect
European Economic Review 51 (2007) 183–202
EUROPEAN ECONOMIC REVIEW
www.elsevier.com/locate/eer

On representative social capital☆

Charles Bellemare, Sabine Kröger*

Département d'économique, Université Laval, Canada

Received 8 April 2006; accepted 13 March 2006
Available online 19 May 2006

Abstract

This paper analyzes the behavior of subjects randomly drawn from the Dutch population who reveal their capacity to provide and sustain social capital by their propensity to invest and reward investments in an economic experiment. We find that heterogeneity in behavior is characterized by several asymmetries—men, the young and elderly, and low educated individuals invest relatively less, but reward significantly more investments. The age effects are found to corroborate existing findings, whereas those of gender and education do not. Higher expected levels of investments by others are found to have a positive and significant effect on own investments. Finally, a laboratory experiment with student subjects is found to provide a lower bound of the population level of social capital.

JEL classification: Z13; C90; C10; D39

Keywords: Social capital investments; Social norms; Experiments; Representative samples

1. Introduction

Most social relations are governed by implicit informal agreements rather than explicit contracts. Even in the later case, contracts are often incomplete, which gives rise to incentives to act against the interests of other parties. Institutional economists (e.g., North, 1990; Williamson, 1985) have argued that differences in the costs to enforce contracts

☆A previous version of this paper circulated under the title "On Representative Trust". Financial support from CentER and CentERdata is gratefully acknowledged.
*Corresponding author. Tel.: +1 418 6562131x12896.
E-mail addresses: cbellemare@ecn.ulaval.ca (C. Bellemare), skroger@ecn.ulaval.ca (S. Kröger).

0014-2921/$ - see front matter
doi:10.1016/j.euroecorev.2006.03.006

translate into cross-country differences in the performance of organizations and economic growth.

It is now well accepted that social capital rests, among other things, on the trust, trustworthiness, and altruism between individuals, and on self-enforcing norms of behavior, all of which allow trades between two agents to be completed informally, with lower transactions costs than required by complex contracts (e.g., Bowles and Gintis, 2002). Empirical evidence suggests that such factors may explain cross country differences in growth and efficiency of organizations (e.g., Zak and Knack, 2001; LaPorta et al., 1997).

In this paper we have a large representative sample of individuals drawn from the Dutch population play a computerized version of an investment game similar to that presented by Berg et al. (1995) (henceforth BDMc). In this game, a sender can pass (invest) part of his initial endowment to a responder. The amount invested is doubled and added to the responder's initial endowment. The responder then chooses how much of this to return to the sender. The structure of this game allows concerns for social efficiency and motives of trust, trustworthiness, positive reciprocity, and altruism to emerge from the investment and return decisions made by the relevant players (e.g., Cox, 2004).[1] The analysis of these decisions potentially improves on the analysis of survey questions intended to capture specific aspects of social capital (e.g., Alesina and La Ferrara, 2002), an approach which has recently been criticized on the basis that answers to such questions do not correlate well with observed behavior (see Glaeser et al., 2000; Gächter et al., 2004). Because we also collected answers to a popular survey question on trust towards others (taken from the World Value Survey (hereafter WVS)), we are able to document the extent to which revealed versus stated choice data can provide different inferences on individual behavior in the context at hand.

One of the main advantages of sampling from the entire Dutch population is that we can relate all the individual decisions in our experiment to a rich set of socio-economic characteristics. The decisions of senders are further related to their subjective social norms of behavior, norms believed to play an important role in many social settings (see Elster, 1989; Ostrom, 2000). To our knowledge, the relationship between subjective norms of behavior and social capital has not been addressed so far. We measure these social norms by directly asking senders to state their expectations about the average behavior of other senders. This approach allows for unrestricted heterogeneity in expectations across individuals, and does not impose a priori assumptions on how these norms are formed.[2] Eliciting subjective expectations raises endogeneity issues, as stated expectations may be jointly determined with decisions made during the experiment. We conduct exogeneity tests to investigate this possibility.

We also compare the behavior observed in our representative population to the behavior of student subjects who participated in the same experiment, this time in the laboratory. This provides a valuable insight on the capacity of the laboratory to provide information on the population level of social capital. This comparison can be more generally related to the literature assessing the external validity of lab experiments by comparing the play of

[1] In concentrating on these aspects of social capital, we do not mean to suggest that individual involvement in communities (e.g., Glaeser et al., 2002) or efforts to build and maintain social ties are unimportant in the larger scheme of social capital. Rather, they are not objects of our enquiry.

[2] One such assumption would be to assume that senders with the same observable characteristics have the same expectations, expectations which are consistent with their observed behavior in the experiment.

student subjects in the laboratory with the play in more heterogeneous populations, notably newspaper readers (e.g., Bosch-Domènech et al., 2002), professional traders, and CEO's (Haigh and List, 2005; Fehr and List, 2004). These studies typically find that the behavior of subjects in the special sample differs from the behavior in the student population. However, they either do not attempt to relate these differences to observable characteristics of subjects or report very few significant correlations (e.g., List, 2004; Gächter et al., 2004). Instead of focusing on a limited number of subgroups of a population, representative samples provide more of the necessary variation in observable characteristics to explain the behavior of interest across different sub-populations.[3]

Finally, our approach has the additional advantage over lab experiments of allowing to test for possible bias caused by using subjects selecting themselves in the experiment based on observable or unobservable characteristics which can be correlated with the decisions in the game, a topic on which very little is known. Eckel and Grossman (2000) report evidence suggesting the presence of participation bias in a classroom experiment. Their approach, however, compares responses of student volunteers to that of pseudo-volunteers who still partly self-select themselves in the experiment. Here, we test for participation bias using non-participants who truly select themselves out of the experiment.

Our results can be summarized as follows. First, we find substantial heterogeneity in investment and returns on investment behavior in the Dutch population. The heterogeneity we uncover is characterized by several unexpected asymmetries. In particular, women are found to invest significantly more than men, but reward investments significantly less; low educated individuals invest significantly less but reward investments significantly more than higher educated individuals; the relation between age and investment is inverted U-shape, with a maximum level of investment reached at 37 years of age, while the relation between reward behavior and age is convex and in many cases even U-shaped, reaching a minimum at the age between 35 and 38 years. When comparing our results to the existing literature, we find that the age-investment relationship we measure is consistent with previous findings. On the other hand, our gender and education effects are not in line with existing results. Second, we find that the behavior found in the laboratory provides a lower bound on the level of social capital estimated in the population as a whole. In particular, amounts invested and returned were substantially lower in the laboratory sample than in the representative sample. These differences are found to disappear when controlling for the composition effect of both samples, suggesting that economic and social characteristics of participants are sufficient to explain the differences across both samples. Third, social norms, i.e., higher prior subjective expectations about the investment levels of others, have a significant and positive effect on investment decisions. Fourth, inferences using a popular survey trust question as a proxy for social capital are significantly different from those using our experimental measure. Finally, we cannot find evidence of participation bias in any experimental decisions.

The remainder of the paper is organized as follows. Section 2 describes the design of the experiment, the experimental procedure, and our samples. Section 3 presents our

[3]Other experiments have recently been run with representative samples. Harrison et al. (2002) use a random sample of the Danish population to investigate the heterogeneity in individual discount rates. Hey (2002) uses a random sample of the Dutch population to analyze decisions under risk and uncertainty. Fehr et al. (2003) present results from a similar experiment to the one presented here using a random sample of the German population.

experimental results. Section 4 contrasts differences between investment and reward patterns across groups of the population. Section 5 summarizes our results and concludes.

2. Experiment and samples

2.1. Survey experiment

The recruitment of our representative sample of participants was made by CentERdata, the survey research institute of Tilburg University in the Netherlands. The main activity of CentERdata is to manage and carry out panel surveys through the CentERpanel, a panel consisting of approximately 2000 households which is representative of the Netherlands. Every Friday, household members of the CentERpanel receive a questionnaire which they are asked to fill at home any time between Friday and Tuesday of the following week. This questionnaire is filled either on a computer or on a television set which is connected to a set-up box linking the household to the CentERdata server.[4]

Apart from providing access to a representative sample, there are many other reasons why the CentERpanel is an attractive medium to conduct experiments. First, because we communicate with participants via CentERdata, the experiment is double blind as participants were told that they will be anonymously matched and that their identities would not be revealed to the experimenters. Secondly, as CentERdata reimburses each panel member their weekly telephone costs for answering the questionnaire by crediting CentERpoints (1 CentERpoint= 0.01 Euro, hereafter CP) to their private bank accounts four times a year, our participants are familiar to payment in fictitious currency. This allows us to use CP as the experimental currency unit and reimburse our participants in a very convenient way.

Our design closely follows the investment game proposed by BDMc. In our game, a sender and a responder were both endowed with 500 CP.[5] The sender could send money to the responder from his endowment. We discretized the choice set of the sender to 11 investment possibilities $I \in \{0, 50, \ldots, 450, 500\}$. The amount sent was doubled by the experimenters and added to the endowment of the responder.

Responders made their decisions using the strategy method, by which they were asked to state how much they would return to senders for all 11 possible amounts they could receive. For each amount invested, the maximal amount responders could return was their endowment of 500 CP plus two times the amount invested. The amounts returned were not constrained to be discrete. The element of the vector of responses which corresponded to the actual investment of the sender was chosen to be the effective action and determined the payoff of both participants. The strategy method was chosen to overcome the difficulty of having members of the CentERpanel interact in real time, and to acquire the complete strategy plan for all 11 possible amounts received.[6] After all participants made their decisions, senders and responders were randomly matched. Payoffs of senders were the

[4]In order to keep the sample representative, low income households without a computer or a television set are given the necessary equipment in order to complete the weekly questionnaire. Children below 16 years of age as well as immigrants are not sampled by the panel.

[5]Computer screens of the original experiment (in Dutch) are available upon request. The translated text of all screens can be found at the following address: www.ecn.ulaval.ca/~skroger.

[6]There is no clear evidence that responses in games of proposal and response played in real time differ from responses collected using the strategy method. McLeish and Oxoby (2004) for example found that the choices in

endowment of 500 CP reduced by the amount they invested, plus the amount received from the responder. Responders got paid their initial endowment of 500 CP, the amount sent by the sender they are paired with multiplied by 2, less the amount they decided to return.

We elicit beliefs of players in our experiment with a series of questions, all of which were asked after players made their decisions in order to circumvent the possibility that eliciting beliefs affects the behavior in the experiment.[7] We measure social norms for senders by asking them for their prior subjective expectations of the average amount which will be sent in the experiment. Responders on the other hand were asked to state how much they thought of receiving from senders.

In order to control for past lifetime experiences, all players were then asked to answer the following question about their past experiences with trust

In the past, when you trusted someone, was your trust usually rewarded or usually exploited? (answers: (Always exploited) 1, ..., 7 (Always rewarded)).

Two weeks after the experiment, each participant received feedback information on the outcome of the game and their final payoff which was later credited to their CentERbank accounts. The experiment was conducted in two sessions, in the 31st and the 36th weeks of the calendar year 2002. Individuals contacted had to read an opening screen informing them that they were selected to participate in an experiment conducted jointly by a team of university researchers. 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 assigned to one of the roles and matched to another panel member. The role was revealed once a panel member had agreed to participate. We contacted 541 panel members from which 42 declined to participate. Of the 499 panel members who completed the experiment, 276 were senders and 223 were responders.[8]

2.2. The laboratory experiment

The laboratory experiment took place at the CentERlab of Tilburg University. Participants were recruited at the campus of Tilburg University and consisted primarily of undergraduates studying in economics and business administration. We have no information about prospective participants who heard about the laboratory experiment but decided not to participate, a clear disadvantage compared to our survey experiment. In total, 10 sessions were conducted at the end of 2002. In total 100 students participated in the experiment, of which 52 were senders and 48 responders.[9] Upon arrival, participants received a show up fee of 2 Euros and an identification (ID) code used throughout the experiment. In exchange of their ID-code, each participant collected a sealed envelope

(*footnote continued*)
the ultimatum game played in real time do not significantly differ from decisions made when the strategy method is used.

[7]Subjects were not rewarded based on the accuracy of their expectations. Existing empirical evidence does not suggest that paying for expectations affects the quality of the expectations collected in the investment game (see Ortmann et al., 2000; Friedman and Massaro, 1998).

[8]Note, that the number of senders exceeds the number of responders. In order to balance the unequal number of players in both roles, 53 responders were randomly assigned twice to a sender. Exactly like all other participants, those responders received payments resulting from only one matching, namely the first.

[9]The matching procedure used to deal with the imbalance between the number of senders and responders in the survey experiment was also used in the laboratory experiment.

Table 1
Descriptive statistics with standard deviations in parentheses

	Experiment					WVS	Variable description
	Survey participants			Laboratory participants		Survey participants	
	S mean	*R* mean	Not played mean	*S* mean	*R* mean	mean	
FEMALE	0.46	0.46	0.43	0.37	0.31	0.53	1 if female
AGE	42.66	43.98	53.79	21.25	21.46	45.96	Age
	(15.55)	(15.67)	(16.55)	(1.92)	(2.06)	(15.41)	
RETIRED	0.130	0.148	0.262	0.000	0.000	0.149	1 if retired
LOWEDUC	0.043	0.076	0.143	0.000	0.000	0.068	1 if low education level
SECONDEG	0.388	0.390	0.381	0.846	0.833	0.377	1 if secondary degree
TRAINDEG	0.449	0.448	0.357	0.077	0.063	0.443	1 if training degree
UNIVDEG	0.119	0.085	0.119	0.077	0.063	0.112	1 if university degree
WORK	0.591	0.511	0.429	0.019	0.042	0.572	1 if labor work
INCOME	1170.93	869.57	1069.52	477.21	566.56	962.31	Gross personal monthly
	(2783.24)	(1492.78)	(1626.47)	(373.96)	(477.99)	(1536.63)	income (in Euro)
CATHOLIC	0.297	0.349	0.309	0.308	0.292	0.310	1 if catholic religion
PROTEST	0.210	0.265	0.191	0.019	0.104	0.245	1 if protestant religion
OTHERS	0.493	0.386	0.500	0.673	0.604	0.445	1 if atheist or other religion
TRUSTEXP	3.16	3.45	–	4.75	4.81	3.96	1–7: 1 always disappointed, 7 always rewarded
N	276	223	42	52	48	2191	Number of observations

Standard deviations for binary variables and TRUSTEXP are omitted.

containing feedback information and his payoff (in Euros) two weeks after the experiment. This mirrors the delay of payment used with the representative sample. Participants were asked about their demographics and income in a post experimental questionnaire copied from the one used by CentERdata to collect information from its panel members, allowing us to gather information which is perfectly compatible with the one gathered for the representative sample.

Table 1 presents descriptive statistics of the players in the representative and laboratory samples, sorted by their role in the experiment. The means of most variables are relatively identical across all roles, within both samples. Two notable differences in the representative sample are the work propensity and age. Non-participants are on average 10 years older than both senders and responders and have relatively higher labor market retirement frequency and lower work participation.

As expected, participants in the laboratory experiment are younger, have lower income, lower probability of working, and higher levels of education than participants in the representative sample.[10] Catholics and protestants are the two most important religious communities in the representative sample while having no religious affiliation (being atheist) is the modal response in the laboratory sample.[11] The superior number of reported catholics in the laboratory sample is partly a consequence of the fact that Tilburg University is located in a predominately catholic area.

2.3. Participation decision

Because we observe the characteristics of individuals in our representative sample who decide not to participate in the experiment, we are able to test for the presence of participation bias. Participation bias could be present if, for example, participants have unobservable predispositions to gamble or systematically different levels of risk aversion which are correlated with their intrinsic propensity to invest and reward behavior. We estimated all our models with an auxiliary participation equation, allowing the unobservable random components of the equations of interest to be correlated with the unobservable random component of the participation equation, in a manner reminiscent of Heckman (1978). In this setup, unobserved characteristics common to both the experimental decisions and the participation decision would be picked up by the correlations between the unobservable components of the equations of interest and the participation equation.[12] We found that none of the correlations were statistically significant. This indicates that all our inferences will be representative of the population we sampled from.

[10]CentERdata distinguishes between nine different education degrees. After consulting CentERdata, we merged those to the following four categories: Low education (no education (yet), primary education, special education, other), secondary education (lower secondary education and higher secondary education), training degree (intermediate vocational training and higher vocational training), university degree (university education).

[11]Atheists are the predominant group subsumed in the variable OTHERS in both samples.

[12]As exclusion restriction, we use the proportion of questionnaires completed by panel members in the three months which preceded our experiment. This variable affects participation but does not directly affect the experimental measure used in this paper and captures, therefore, directly the participation propensity of subjects when participation is uncorrelated with financial outcomes (members of the CentERpanel are not paid to participate in the panel).

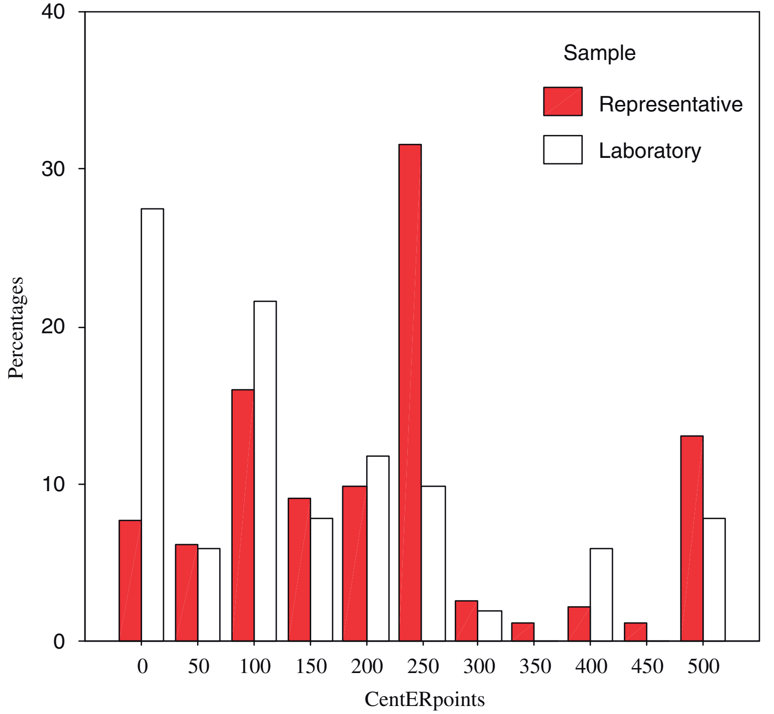

Fig. 1. Distribution of amounts invested in the representative and laboratory samples.

3. Experimental results

The sample distributions of investments is shown in Fig. 1. The distribution of investments in the representative sample is skewed to the left, with more than 30% of the sample investing half of their endowment (i.e., 250 CP). In contrast, the distribution of investments made in the laboratory sample has a mode at 0 CP, with more than 25% of subjects investing nothing, while less than 10% of subjects invest half of their endowment or more. A Pearson chi square test (p-value= 0.000) easily rejects the null hypothesis that both distributions are the same.

We measure the propensity to reward investments using the return ratio, defined as the amount returned divided by the maximal amount which can be returned, given an amount invested. For a given investment, the maximal amount which can be returned equals the amount invested multiplied by two, plus the experimental endowment of 500 CP. Because we use the strategy method, we observe a sequence $\{R_a \in [0,1] | a \in \{0, 50, \ldots, 500\}\}$ for each responder, where R_a denotes the return ratio when receiving an investment of a CP. The main advantage of the return ratio is that it is automatically scaled, which controls for the fact that receivers can send more simply because the total available amount to send from increases with a. We summarize the return ratio in Fig. 2, where the left and right columns present graphs for the representative and student samples, respectively. The curve with solid squares represents the sample median return ratio for all 223 responders of the representative sample and all 48 responders in the student sample for all possible levels of

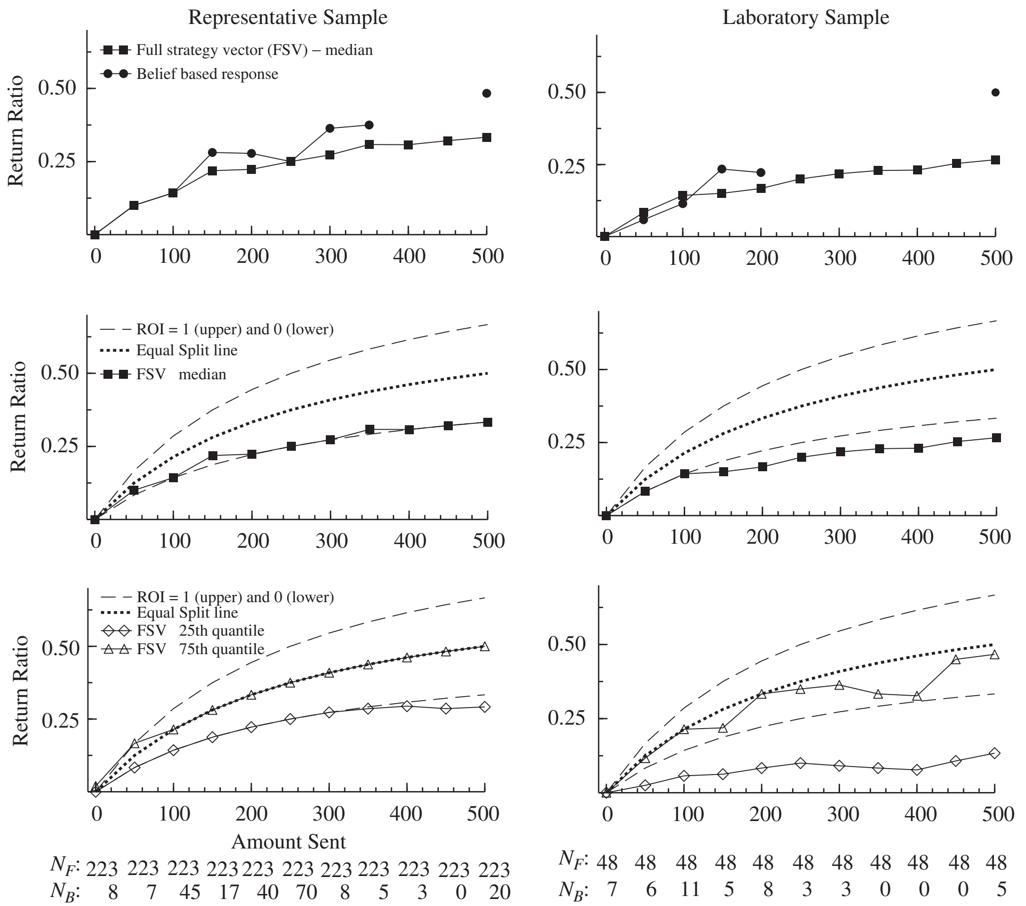

Fig. 2. Top row presents the median return ratio in the representative and the student samples for each possible amount received using the full strategy vector response data (square line). Circle line presents the median return ratio using only the element of the strategy vector corresponding to the amount responders believed they would be offered. Middle and bottom rows plot visual aids to understand response behavior in the representative and student samples. The lower (upper) dashed lines represent the return ratio providing senders with a return on investment ((Amount returned−Amount sent)/Amount sent) of 0 (1). The dotted lines represent the return ratio providing senders and responders with equal monetary payoffs. The middle row plots the median return ratio for both samples. The lower row plots the 25th and 75th quantiles of the return ratio for both samples. N_F denotes the sample size used to compute the return ratio for each outcome based on the full strategy vector, N_B denotes the sample size used to compute the return ratio for each outcome based on their expectations. The sum of N_B across all categories gives the sample size in both samples. In the later case, cells with less than five data points are not plotted.

investment. The important feature of these two panels is that the ratios monotonically increase and are concave in the amounts invested, a feature which has also been observed when both players interact in real time (see for example Croson and Buchan, 1999; Fehr and List, 2004). This suggests that monotonicity in return ratios is not strongly related to the method used to elicit responses. Further insights can be obtained by comparing the sample median return ratios using the full strategy vector data with the median return ratios using only the element of the strategy vector associated with the investment a responder believed would materialize in the play of the game. Because we are keeping only one element of the strategy vector for each responder, increases in return ratios across two consecutive levels of investment cannot be the result of having the same individuals reporting monotonically increasing return ratios. Results are graphed in the top panels as a curve with solid triangles.[13] We find that the shape of both curves are similar across all possible levels of investments in both samples, indicating that responders' decisions tend to increase monotonically with the amounts invested.

Does it pay to invest? In order to answer this question, all graphs in the bottom two rows of Fig. 2 plot a common set of visual aids. The lower and upper dashed lines of all four graphs plot the return ratios providing investors with a return on investment (ROI) of respectively, $ROI = 0$ (investors received exactly what they invested) and $ROI = 1$ (additionally to the amount sent, investors receive the entire surplus created) for all possible levels of investment I. The dotted line shows the return ratios providing proposers and responders with equal monetary payoffs.[14] The middle panels of Fig. 2 report the sample median return ratios for the representative and student samples. The median return ratio for the representative sample follows closely the line where $ROI = 0$ for the representative sample, but is below this line for investments in excess of 100 CP for the laboratory sample. This indicates that senders have an estimated 50% probability of making a loss when playing against a random responder from the Dutch population, as opposed to a significantly higher probability when interacting with a responder from the laboratory.[15]

In order to quantify differences in the spread or the return ratio distribution for both samples, we plot the 25th and 75th percentiles of both distributions in the lower panels of Fig. 2. We find that the return ratio distribution is more dispersed in the student sample than in the representative sample. In particular, the 25th percentile in the representative sample practically coincides with the sample median and with the line where $ROI = 0$, indicating that approximately 75% of the responders compensate investors exactly by returning to them at least their investment or more. On the other hand, the 25th percentile in the laboratory sample is both substantially lower than the sample median, and well below the line where $ROI = 0$.

3.1. Empirical results on investment behavior

In this section, we investigate the determinants of investments I, an ordinal and discrete variable. Define I^* as a proposer's unobservable latent propensity to invest. The mapping

[13] Outcomes associated with amounts sent, which were believed to occur by three responders or less, were not added to the graph.

[14] These ratios are computed as follows. By definition, the ROI_I for a level of investment I corresponds to $ROI_I = (A_I - I)/I$, where (A_I denotes the amount returned at that level of investment. The return ratio R_I is defined as $A_I/(2I + E)$, where $2I + E$ is the maximal amount which can be returned (i.e., twice the investment I plus the endowment E of 500 CP). Substituting A_I by $R_I(2I + E)$ in the ROI_I formula and rearranging, we get $R_I = I(ROI_I + 1)/(2I + E)$, representing the return ratio which corresponds to a return on investment of ROI_I. In our experiment, a return ratio resulting in equal monetary payoffs for both players will equal three quarters of the amount received, i.e., $A_I = 3/4 \cdot 2I$.

[15] Comparing the median return ratios of the representative sample to those of the student sample using a Mann–Whitney-U test results in p-values (for the null hypothesis that medians are the same) which are below 0.001 for all investment levels.

Table 2
Sender results

	Amount invested						WVS	
	Coeff.	t-value	Coef.	t-value	Coef.	t-value	Coef.	t-value
CONSTANT	1.373	14.158	−2.114	−3.118	−1.776	−2.487	−2.599	−9.012
LAB	−0.607	−3.459	−0.269	−0.957	–	–	–	–
FEMALE			0.208	1.988	0.267	1.993	−0.113	−1.985
AGE			0.064	2.333	0.061	2.116	0.037	3.065
AGE^2/1000			−0.811	−2.556	−0.802	−2.382	−0.424	−3.131
RETIRED			−0.204	−0.629	−0.199	−0.597	0.212	1.683
SECONDEG			0.677	2.235	0.716	2.332	0.029	0.233
TRAINDEG			0.672	2.193	0.643	2.167	0.103	0.817
UNIVDEG			0.858	2.389	0.805	2.208	0.204	1.402
WORK			−0.238	−1.278	−0.222	−1.147	0.063	0.734
LNINC			0.031	1.325	0.025	1.025	0.034	2.676
CATHOLIC			0.042	0.279	0.022	0.132	0.011	0.170
PROTEST			−0.042	−0.287	−0.043	−0.279	0.076	1.042
TRUSTEXP			0.060	1.067	0.044	0.716	0.508	17.748
SNORMS			0.555	10.812	0.546	9.900	–	–
Laboratory sample included	Yes		Yes		No		N/A	
Number of observations	328		328		276		2191	
Log-likelihood	−634.31		−526.47		−436.85		−1305.75	
B&C test [p-value]	$\chi^2_6 = 3.49$ [0.75]		$\chi^2_{84} = 91.91$ [0.26]		$\chi^2_{78} = 89.23$ [0.18]		–	

First three specifications are for amounts invested in the experiment and are estimated using an ordered probit. The Butler and Chatterjee test is for the null hypothesis that the distribution of the error term is correctly specified and that the regressors are jointly exogenous. Under this null, the statistic follows a chi-square distribution. The WVS specification is estimated using binary probit model.

between I^* and I is given by

$$I_i^* = \mathbf{x}_i'\boldsymbol{\beta} + \varepsilon_i, \tag{1}$$

$$I_i = k \quad \text{if } m_{k-1} < I_i^* \leqslant m_k, \;\; k = 0, \ldots, K. \tag{2}$$

The index i denotes the individual, $\mathbf{x}_i$ is a vector of explanatory variables including a constant term, $\boldsymbol{\beta}$ is the vector of parameters of interest, and ε_i is the error term, assumed to be normally distributed N(0, 1). The index k represents the discrete amount sent and K the total number of categories. We make the usual normalization $m_{-1} = -\infty$, $m_0 = 0$, and $m_K = \infty$. As can be seen from Fig. 1, there were little investments from 300 to 450 CP, which prevents identification of all threshold parameters. In our empirical application, we merge these categories and estimate the model with eight outcomes.

The first two specifications of Table 2 present estimates of the model combining the data of the representative and laboratory samples. The conditioning vector $\mathbf{x}$ in the first specification contains a constant term and a dummy variable LAB equal to 1 for observations in the laboratory sample and 0 otherwise. In line with the previous descriptive results, amounts invested are significantly lower in the laboratory sample. One explanation for the observed difference is that subjects in the laboratory sample differ from subjects in the representative sample in terms of observable characteristics. Another explanation is that participants in the lab, mostly economic and business students, may have learned through their courses or their past participation in similar experiments that it might not pay to invest in this game. This would be consistent with a negative and significant effect of being in the laboratory, an effect which would remain after we control for differences in observable characteristics.[16]

The second specification adds controls for observable characteristics of participants, previous life experiences when trusting others (TRUSTEXP), and the subjective beliefs about the expected investment made by other players (SNORMS).[17] The difference between the laboratory and the representative samples, captured by the LAB variable, is no longer significant, suggesting that differences in the background characteristics of players explain the better part of the difference in behavior in both samples. Related evidence taken from the social psychology literature is Van Lange et al. (1997), who measure social value orientations using a representative sample of the Dutch population surveyed by the 'Telepanel' (the predecessor of the CentERpanel), and compare individual answers to those of a sample of university students. In their survey, individuals were asked to choose between hypothetical payoffs for them and another person. These choices were used to classify individuals as having either prosocial, individualist, or competitive orientations. Prosocials' are defined as individuals who maximize outcomes for both of them and others and seek cooperation, and minimize differences between outcomes for themselves and others. Of the three social value orientations they identify, prosocials' are the most disposed to contribute and maintain social capital in the sense we investigate here. In line with our results, they find a greater proportion of prosocials in their representative sample than in their student sample. Moreover, they also find that the better part of the differences between both samples is attributable to differences in observable characteristics.[18]

Both the linear and quadratic terms in age (AGE and $AGE^2/1000$) are significant, indicating an inverted-U relation between age and investments in social capital, with the propensity to invest reaching its maximum at 37 years of age.[19] This reconfirms the inverted-U shape pattern usually found in the social capital literature (e.g., Putnam, 2000; Alesina and La Ferrara, 2002) and is also consistent with age profiles estimated using measures of community involvement (Glaeser et al., 2002) and prosocial value orientation (Van Lange et al., 1997).

Women (FEMALE) invest significantly more than men, a finding which departs from laboratory experiments using the investment game. Those studies find that men either invest relatively more than women (e.g., Buchan et al., 2004), or no significant differences in amounts invested between both sexes (Croson and Buchan, 1999). When restricting ourselves to the laboratory sample only (not reported in the table), average investments of women still exceed those of men, but the difference is no longer significant (p-value$=0.555$). Economic status, reflected through employment status (WORK) and personal income (LNINC), does not have a significant effect on investment decisions. We

[16]We do not have information allowing us to quantify past exposure to the investment game or past participation in experiments.

[17]We have experimented with specifications including cross-terms but none was found to be statistically significant.

[18]Gächter et al. (2004) find that the average contribution of students in a public good experiment conducted in Russia is significantly lower than the average contribution made in an heterogeneous sample of participants. They do not test whether differences between both samples are explained by differences in observable characteristic.

[19]Because we are using a cross-section, the age effect could also be a cohort effect, the two cannot be distinguished here.

do not find any effects of retirement status (RETIRED) on investment decisions, conditional on age.

Our results indicate that individuals with secondary (SECONDEG), technical training (TRAINDEG), and university degrees (UNIVDEG) are more likely to make higher investments than subjects with low education levels (the omitted category). These results differ to Alesina and La Ferrara (2002), where education effects seem to have a monotone increasing impact, and Fehr et al. (2003) who find no effects.[20]

We find no evidence that either catholics (CATHOLIC) or protestants (PROTEST) invest differently than atheists or individuals of other religions (the omitted category). These results are concordant to the Alesina and La Ferrara (2002) study, but in contrast to the Fehr et al. (2003) study who find a positive effect for catholics.

Interestingly, past experiences when trusting others (TRUSTEXP) do not have a significant effect on social capital investments.[21] This result does not support the prediction of the indirect evolutionary approach to adaptation through experience literature (for a recent survey see Ostrom, 2000) which suggest that social investment behavior is directly related to past experiences.

Individuals with higher expectations about the level of investments made by other senders (SNORMS) invest significantly more than senders with lower expectations, which corroborates the presence of social norms which increase efficiency. Endogeneity of our social norms variable is a potential concern if participants conditioned their subjective expectation statements on their investment decisions in an attempt to rationalize them. We test our model specification using the statistic proposed by Butler and Chatterjee (1997). Under the null hypothesis of normality of the error term ε_i and exogeneity of the model regressors $\mathbf{x}_i$, the Butler and Chatterjee test is distributed chi square, with degrees of freedom being a function of the number of outcomes of the categorical variable I, the dimension of the vector $\mathbf{x}$, and the number of model parameters (see Butler and Chatterjee, 1997, for details). Test values and associated p-values are reported at the bottom of Table 2. We find that the null hypothesis is not rejected at conventional levels, suggesting a correct model specification.

In order to check whether pooling both samples influences our estimates, we re-estimated the second specification removing the laboratory sample. Results of this exercise are presented in the third column of Table 2. We find that the signs and significance of the estimates are relatively insensitive to the inclusion of the laboratory sample, an indication that the relatively small sample size and the relatively small variation in observable characteristics of this sample do not contribute greatly to identify the model parameters.

3.1.1. *Inferences using WVS trust question as a proxy*

A more accessible alternative to experiments is to directly ask individuals questions on their intended behavior. The World Value Survey (WVS) trust question has been used by a number of researchers as proxy for social capital (e.g., Alesina and La Ferrara, 2002). We focus here on whether the effect of background characteristics found above can also be obtained by using answers to this question. To do so, the CentERpanel asked a large

[20]Alesina and La Ferara measure education by the number of years of education. Fehr et al. use a three level classification, distinguishing individuals with low, intermediate, and high levels of education.

[21]The effect of past experiences measured here differs from the effects measured in multiple period investment games who relate investments to past outcomes in preceding experimental periods (see Engle-Warnick and Slonim, 2004).

sample of panel members who did not participate in the experiment to answer the 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? (answers: (1) Most people can be trusted, (2) You have to be very careful, (3) I do not know).

We collected answers to this question from 2191 panel members in October 2002. Descriptive statistics of this new sample (the column "WVS survey participants" of Table 1) indicate that the composition of this new sample is relatively identical to that of the sample of individuals who participated in our experiment. We follow Alesina and La Ferrara (2002) and group in a single category individuals reporting not knowing whether they trust with individuals who do not report to trust others. Based on this aggregate measure, 50.5% of respondents stated not to trust others. The last two columns of Table 2 report the results from a probit regression of answers to this survey question on background characteristics of the panel members. The differences with the experimental estimates are quite remarkable. First, we do not find any effect of education on survey trust, while low educated individuals invest relatively less in the experiment. A second difference concerns gender, with women being less likely to state that they trust others. Finally, positive past experiences with trust have a positive and significant effect on the probability of stating to trust others, while no such effects were found in the experiment. Finally, the inverted-U shape effect of age is still present using survey trust, while economic status variables such as income and work status do not correlate with stated trust.

3.2. *Empirical results on rewards to investment*

The individual level analysis of the return ratio R_{ai} is based on the following Tobit model

$$R^*_{ai} = \gamma_0 + \gamma_1 a + \gamma_2 a^2 + \mathbf{z}_i'\boldsymbol{\eta} + (a \cdot \mathbf{z}_i)'\boldsymbol{\alpha}_1 + (a^2 \cdot \mathbf{z}_i)'\boldsymbol{\alpha}_2 + \varepsilon_{ai}, \tag{3}$$

$$R_{ai} = R^*_{ai} \quad \text{if } R^*_{ai} > 0 \tag{4}$$

$$= 0 \quad \text{if } R^*_{ai} \leqslant 0, \tag{5}$$

where Eq. (3) describes an individual's latent propensity to reward investments, and Eqs. (4) and (5) describe the censoring rule which allows responders with extremely low propensities to return nothing with positive probability. Like for the case of investments, the return propensity is modelled as a function of the amount invested by the sender, i.e., $a \in \{0, 50, \ldots, 500\}$, background characteristics $\mathbf{z}_i$, a vector of parameters $\boldsymbol{\eta}$, and an unobservable component $\varepsilon_{ai} \sim \mathrm{N}(0, \sigma^2)$. The quadratic form in a is added to capture the monotone increasing shape of proportions returned.[22] We interact the quadratic form of a with the observable characteristics to allow return ratio vectors to differ across individuals both in terms of levels and slopes.

We estimated two specifications grouping both the representative and the lab samples. The first included only the dummy variable LAB for decisions made in the laboratory and its interaction with the quadratic form in a. The second specification added controls for background characteristics, with all variables interacted with the quadratic form in a. In

[22] We have estimated a less restrictive specification with dummy variables for each a category. Results were numerically identical to those presented above.

Table 3
Responder results—Tobit estimator

	Return ratio					
	Coef.	*t*-value	Coef.	*t*-value	Coef.	*t*-value
CONSTANT	−0.063	−4.607	0.257	3.128	0.247	2.833
a	0.088	17.759	0.011	0.477	0.008	0.320
a^2	−0.005	−11.844	−0.001	−0.597	−0.001	−0.582
LAB	−0.096	−3.258	−0.068	−1.547	–	–
LAB×a	0.001	0.101	−0.007	−0.449	–	–
LAB×a^2	−0.0002	−0.275	0.0002	0.191	–	–
FEMALE			−0.044	−1.947	−0.051	−1.861
AGE			−0.013	−4.718	−0.011	−4.207
AGE^2/1000			0.161	5.374	0.146	4.829
RETIRED			−0.013	−0.775	−0.006	−0.376
SECONDEG			−0.104	−1.980	−0.098	−1.891
TRAINDEG			−0.081	−1.823	−0.072	−1.789
UNIVDEG			−0.141	−2.456	−0.136	−2.201
WORK			0.017	2.129	0.011	1.356
LNINC			0.002	1.479	0.002	1.615
CATHOLIC			−0.002	−0.456	−0.009	−1.224
PROTEST			0.002	0.231	0.003	0.478
TRUSTEXP			−0.011	−1.033	−0.014	−1.157
RTHINK			0.003	0.728	0.003	0.662
AGE×a			0.002	5.633	0.003	6.295
AGE^2/1000 × a			−0.028	−5.472	−0.032	−6.116
AGE×a^2			−9.37E-5	−9.783	−0.0001	−11.217
AGE^2/1000 × a^2			0.001	4.719	0.001	5.329
σ^2			0.021	23.334	0.021	22.11
Laboratory sample included	Yes		Yes		No	
Number of observations	2981		2981		2453	
Log-likelihood	862.13		1036.80		939.83	

The *t*-statistics are based on robust standard errors. Only the significant interactions of variables with a and a^2 are reported in the second and third specifications. Other interaction terms were included in the regressions. They were not significant and are omitted from the table.

light of the relatively high number of parameters, and that most variables did not have a significant effect on the slope of the return ratio vectors (their corresponding elements of $\boldsymbol{\alpha}_1$ and $\boldsymbol{\alpha}_2$ were not statistically different from zero), Table 3 reports the regression results keeping only the interaction terms which were significant.

Results from the first specification confirm the descriptive analysis of the previous section, namely that the average return ratio vector is increasing and concave in a, and that return ratios are significantly lower in the laboratory sample compared to the representative sample. The insignificance of both interaction terms of the LAB variable with the quadratic form in a suggest that differences in average return ratios between both samples are relatively constant over all possible levels of investments. Like for the case of investments, we find that the magnitude of the LAB variable diminishes and is no longer significant once we add controls for background characteristics, suggesting again that differences in observable characteristics explain most of the discrepancies between both

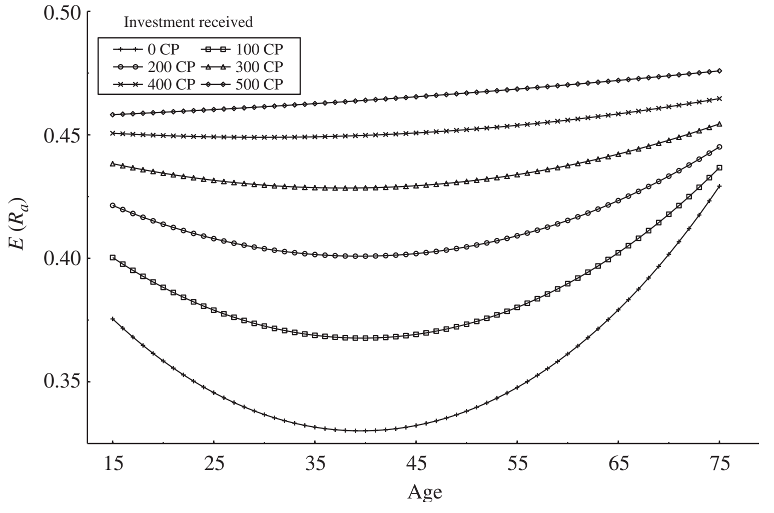

Fig. 3. Predicted expected return ratio's by age and levels of investments received. Expected returns are evaluated at the sample means of all observable characteristics (except age).

samples. A log-likelihood ratio test ($\chi^2_{29} = 349.34$, p-value$= 0.000$) clearly prefers the model with controls for observable heterogeneity in the population.

Several variables have a significant impact on the level of the return ratio vector but not on its slope. Individuals who work (WORK) return significantly more. We find that women (FEMALE) return relatively less than men, and low educated individuals (the omitted category) return relatively more, most of these differences being either significant or close to being significant at the 5% level. These patterns are opposite to results for investment behavior, where all these variables had a significant but opposite effect on investment decisions.

Age is the only variable with both a significant effect on the level (AGE) and the slope ($\text{AGE}^2/1000$) of the expected return ratio vector. We comment on their effects by plotting the predicted expected return ratio conditional on age for regularly spaced investments of 0, 100, 200, 300, 400, and 500 CP.[23] We fix the level of the expected return ratio vector by setting all characteristics of the return propensity equation (3), except age, at their sample average. Fig. 3 presents the predicted age profiles. There are two striking aspects to this graph. First, there is strong U-shape relation between the propensity to reward and age for investments between 0 and 200 CP, with the expected return ratios reaching a minimum in the range of 35 and 40 years of age, close to the turning point of the *inverted U* relation between investments and age discussed in the previous section. The U-shape pattern flattens out progressively as levels of investment increase but remains convex over the age domain.

The impact of responders' beliefs about the investment they thought they would receive (RTHINK) does not have a significant effect on the return ratio. As in the case of the investment decision, personal gross income (LNINC) do not correlate significantly with

[23]We make use of the fact that the conditional expectation of R_a, which is censored from below at zero, is equal to $\mathbf{E}(R_a|\mathbf{z}, a) = (\mathbf{E}(R_a^*|\mathbf{z}, a) + \sigma \cdot \lambda(\mathbf{z}, a)) \cdot \Pr(R_a^* \geqslant 0|\mathbf{z}, a)$, where $\lambda(\mathbf{z}, a)$ denotes the inverse Mills ratio.

the return decision. Religion (CATHOLIC and PROTEST) and past experiences with trust (TRUSTEXP) also do not have significant effects on the response behavior. Finally, the last two columns of Table 3 report estimates of the second specification using only the representative sample data. Again, we find very little differences in either sign or significance of parameter estimates. One exception is whether an individual works or not (WORK), which no longer enters significantly when we exclude the laboratory sample from our regression.

4. Discussion of asymmetries

The previous section has provided evidence that the heterogeneity in investment and reward behavior is significantly related to observable characteristics of participants. This relation was found to be characterized by some surprising asymmetries. In particular, keeping other factors constant, men and low educated individuals make significantly lower investments, but reward investments significantly more. This pattern was also found to hold for the young and the old, at least for investments representing less than half of the senders' endowment.

Because the present paper is one of the first systematic exploration linking background characteristics of a random sample of a population with their experimental behavior, it is difficult to relate the age and education asymmetries to other existing results. By far the easiest and most documented asymmetry we can relate to is that of gender. Croson and Buchan (1999) note that gender differences in the investment game tend to be conditional on the level of risk present in the experiment. In decisions where risk is present, such as the investment decision in our experiment, they find that the behavior of women and men does not systematically differ. On the other hand, for decisions involving no risk, such as responders in our game, women return significantly more. Our results do not support this explanation, as we find that women invest more than men despite the riskiness of the situation, while men return significantly more than women in the risk free role of responders, suggesting that gender effects are unlikely to be universal.

Another explanation for the gender asymmetry is that subgroups of the population react differently in the roles of senders and responders because the benefits and costs of sending money to the other party, possibly out of pure altruism, differ between senders and responders. In our experiment, senders need only to forego 50 CP to transfer 100 CP to the other party, as investments were multiplied by two, while responders had to tradeoff one to one to transfer back to investors. Andreoni and Vesterlund (2001) for example find that men are more altruist when the costs of altruism is low, while women are more altruist when the cost of being altruist is high. Quite to the contrary, our gender effects indicate that women send significantly more than men when cost of altruism are relatively low (in the investment decision), but men send relatively more when the cost of altruism are relatively high (in the responders decision).[24]

Some support for the hypothesis that experimental parameters may affect asymmetries in behavior can be obtained by comparing our results to those of Fehr et al. (2003). In their design, both amounts invested and amounts returned were doubled, thus in effect

[24] Our game differs from dictator games used by Andreoni and Vesterlund to measure altruism because trust and trustworthiness are also intrinsic and perhaps the most prevalent motive for investing and rewarding investments in our experiment.

equalizing the costs of sending money to the other party across both roles.[25] Their regression results do not support the presence of significant asymmetries in investment and reward to investment behavior across the German population.[26] These and our results suggest that asymmetry in the incentive structure of the game may be an important aspect in explaining asymmetries in investment and reward behavior.

5. Conclusion

Societies are composed of heterogeneous individuals who differ in terms of their socio-economic characteristics and also possibly in terms of their propensities to trust, to be trustworthy, or to be altruist. This study analyzed data on investment and reward behavior in social relations for a representative sample of individuals drawn from the Dutch population. The different inferences we obtained using answers to a widely used survey question to capture specific aspects of social capital indicate the importance of combining experimental and survey methods in order to allow heterogeneous participants to reveal rather than state their propensities to provide and sustain elements of social capital. Furthermore, we found no participation bias in the experiment, suggesting that our results are representative of the population we sampled from.

The heterogeneity in propensities to invest and to reward investments was found to be characterized by several asymmetries. Men and low educated individuals invest significantly less, but reward significantly more investments than women and individuals with average and high levels of education. Our gender asymmetry is inconsistent with previous results, which tend to find an opposite asymmetry. The effect of education in the investment game, much less documented than the effect of gender, does not corroborate recent studies.

Furthermore, the young and the old were found to make significantly lower investments, but reward significantly more investments than middle aged individuals. Our estimated relationship between age and investments corroborated existing findings and appears to be robust. One possible reason for this robustness is that the nature of social interactions changes over the life-cycle, with middle age individuals being involved in more interactions which require helping others than young and old individuals (Van Lange et al., 1997). The exposure to such interactions is believed to promote pro-social behavior, a behavior defined as the propensity to choose socially efficient and egalitarian outcomes. Investing in our experiment creates a social surplus and is, therefore, socially efficient. Investments are highest for middle aged individuals, which is consistent with the pro-social hypothesis. Because amounts returned are not multiplied in our experiment, differences in concerns for social efficiency cannot explain differences in returns. This could explain why we find an asymmetry in amounts returned, with middle aged individuals returning relatively less. More generally, we conjecture that this asymmetry, and those we found for gender and education, can be the result of differences in experimental designs—differences in initial endowments of senders and responders, and/or different multipliers used to send and return money—which

[25]Our design differs from theirs in several other ways. First, their experiment is performed after face-to-face interviews conducted for the German Socio-Economic Panel. Second, they use an environment of immediate response rather than the strategy method to collect the reward decisions of responders.

[26]They find that individuals of 65 years and older invest relatively less but reward investments relatively more than individuals below 35 years of age, while immigrants invest relatively more but reward investments relatively less than natives. Both asymmetries are, however, only significant at the 10% level.

may affect how individuals trade-off own and others well-being. The extent to which differences in design parameters are sufficient to reconcile the asymmetries reported here and in other studies is a question requiring further research. Reconciling education effects across studies will be further hampered by the comparability of the measures of education used. The classification of individuals at either high, intermediate and low levels of education is partly country specific, and partly a choice of the empirical analyst.

A second result of the paper is that social norms were found to play an important role in determining investment behavior in our experiment, with senders who expected higher investments from other senders significantly sending more themselves. Also, whether subjects have good or bad past lifetime experiences when trusting others had no significant impact on their investment and reward behavior in our experiment.

Finally, the behavior observed in our laboratory sample differed from that in our representative sample, with laboratory participants making significantly lower investments, and rewarding significantly less investments than participants in the representative sample. We showed that differences in the observable composition of both samples account for most of these differences. Our laboratory sample was nevertheless informative of the behavior in the Dutch population in the sense of providing a lower bound on the levels of investments and of amounts returned. Similar lower bounds were found for public good games and for experiments in social psychology. These and our results indicate that trust, trustworthiness and other social preferences, once identified in the lab, are likely to be present, with greater intensity, in the population as a whole.

Acknowledgments

The authors thank the Canada Chair of Research in the Economics of Social Policies and Human Resources for support. We thank Hanneke Dam, Marcel Das and Corrie Vis of CentERdata for their support throughout the experiment. We are grateful to Oliver Kirchkamp, Arthur van Soest, Eric van Damme, Jan Magnus, Pierre-Carl Michaud, Wieland Müller, Karim Sadrieh, Jan Potters, Matthias Sutter, Ernst Fehr, Martin Dufwenberg, James Cox and Yann Bramoullé and a referee for helpful comments. Useful comments were made by participants at the NAKE Research Day in Amsterdam, the ENDEAR Workshop in Jena, the ESA European Meeting in Strasbourg and Erfurt, the Max-Planck-Institute in Jena, the CIRANO in Montréal, the DIW in Berlin, the ENTER Jamboree 2003 meeting, the Social Capital Workshop in Tilburg, the Spring Meeting of Young Economists in Leuven, the Economics seminar in Maastricht, Zürich, and Québec. The second author gratefully acknowledges financial support by the Deutsche Forschungsgemeinschaft, SFB Transregio 15, ("Governance and Efficiency as Economic Systems"), Humboldt Universität zu Berlin, and the EU-TMR ENDEAR Network Grant (FMRX-CT98-0238). The usual disclaimer applies.

References

Alesina, A., Ferrara, E.L., 2002. Who trust others. Journal of Public Economics 85 (2), 207–234.

Andreoni, J., Vesterlund, L., 2001. Which is the fair sex? Gender differences in altruism. Quarterly Journal of Economics February, 293–312.

Berg, J., Dickhaut, J., McCabe, K., 1995. Trust, reciprocity, and social history. Games and Economic Behavior 10, 122–142.

Bosch-Domènech, A., Montalvo, J.G., Nagel, R., Satorra, A., 2002. One, two, (three), infinity,...: Newspaper and lab beauty-contest experiments. American Economic Review 92 (5), 1687–1701.
Bowles, S., Gintis, H., 2002. Social capital and community governance. The Economic Journal 112 (483), F419–F436.
Buchan, N., Croson, R., Solnick, S., 2004. Trust and gender: An examination of behavior, biases, and beliefs in the investment game. University of Wisconsin.
Butler, J.S., Chatterjee, P., 1997. Test of the specification of univariate and bivariate ordered probit. The Review of Economics and Statistics 79 (2), 343–347.
Cox, J.C., 2004. How to identify trust and reciprocity. Games and Economic Behavior 46, 260–281.
Croson, R., Buchan, N., 1999. Gender and culture: International experimental evidence from trust games. American Economic Review Papers and Proceedings 89, 386–391.
Eckel, C., Grossman, P., 2000. Volunteers and pseudo-volunteers: The effect of recruitment method on subjects' behavior in experiments. Experimental Economics 3, 107–120.
Elster, J., 1989. Social norms and economic theory. Journal of Economic Perspectives 3 (4), 99–117.
Engle-Warnick, J., Slonim, R.L., 2004. The evolution of strategies in a repeated trust game. Journal of Economic Behavior and Organization 55, 553–573.
Fehr, E., List, J.A., 2004. The hidden costs and returns of incentives—trust and trustworthiness among CEO's. Journal of the European Economic Association 2, 743–771.
Fehr, E., Fischbacher, U., Rosenbladt, B.V., Schupp, J., Wagner, G., 2003. A nation-wide laboratory examining trust and trustworthiness by integrating behavioral experiments into representative surveys. Working Paper 141, University Zurich.
Friedman, D., Massaro, D.W., 1998. Understanding variability in binary and continuous choice. Psychonomic Bulletin and Review 5, 370–389.
Gächter, S., Hermann, B., Thöni, C., 2004. Trust, voluntary cooperation, and socio-economic background: Survey and experimental evidence. Journal of Economic Behavior & Organization 55, 505–531.
Glaeser, E.L., Laibson, D., Scheinkman, J.A., Soutter, C., 2000. Measuring trust. Quarterly Journal of Economics 115 (3), 811–846.
Glaeser, E.L., Laibson, D., Sacerdote, B., 2002. An economic approach to social capital. The Economic Journal 112 (483), F437–F458.
Haigh, M., List, J., 2005. Do professional traders exhibit myopic loss aversion? Journal of Finance 60 (1), 523–534.
Harrison, G.W., Lau, M.I., Williams, M.B., 2002. Estimating individual discount rates in Denmark: A field experiment. American Economic Review 92 (5), 1606–1617.
Heckman, J.J., 1978. Sample selection bias as a specification error. Econometrica 47 (1), 153–162.
Hey, J.D., 2002. Experimental economics and the theory of decision making under risk and uncertainty. Geneva Papers on Risk and Insurance Theory 27 (1).
LaPorta, R., de Silanes, F.L., Shleifer, A., Vishny, R.W., 1997. Trust in large organizations. American Economic Review 87 (2), 333–338.
List, J.A., 2004. Young, selfish and male: Field evidence of social preferences. The Economic Journal 114, 121–149.
McLeish, K.N., Oxoby, R., 2004. Specific decision and strategy vector methods in ultimatum bargaining: Evidence on the strength of other-regarding behavior. Economics Letters 84 (5), 399–405.
North, D., 1990. Institutions, Institutional Change and Economic Performance. Cambridge University Press, Cambridge, UK.
Ortmann, A., Fitzgerald, J., Boeing, C., 2000. Trust, reciprocity, and social history: A re-examination. Experimental Economics 3, 81–100.
Ostrom, E., 2000. Collective action and the evolution of social norms. Journal of Economic Perspectives 14, 137–158.
Putnam, R., 2000. Bowling Alone: The Collapse and Revival of the American Community. Simon and Schuster, New York.
Van Lange, P.A., Otten, W., Bruin, E.M.N.D., Joireman, J.A., 1997. Development of prosocial, individualistic, and competitive orientations: Theory and preliminary evidence. Journal of Personality and Social Psychology 73 (4), 733–746.
Williamson, O., 1985. The Economic Institutions of Capitalism. Free Press, New York, USA.
Zak, P.S., Knack, S., 2001. Trust and growth. Economic Journal 111 (4), 295–321.